Essays In Housing And Urban Economics

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Essays In Housing And Urban Economics

Abstract
This dissertation examines how housing and location choice decisions contribute to spatial and social inequality. The first chapter studies the financial burdens of property taxes on homeowners. Exploiting a reform in Philadelphia that generated changes in property taxes without changing the provision of public goods and services, I measure how sensitive homeowners are to increases in their property tax bills. I find that a $100 increase in property taxes increases property tax delinquency by 3.9% after one year and 7.7% after two years. Home sales also increase by 4.1% after two years. There is no effect on house prices. Further, the financial burdens of property taxes vary considerably by owner race and occupancy status. White owners are more likely to recover from delinquency and sell their homes than Black and minority owners. Owners who live in their houses are also more likely to sell than landlords. The second chapter studies how the time spent commuting to work have evolved over the last four decades for White and Black commuters. In 1980, Black commuters spent 50.3 more minutes commuting per week than White commuters; by 2019, that difference declined to 22.4 minutes. Two factors account for the majority of this decline: Black workers are more likely to commute by transit, and Black workers make up a larger share of the population in cities with long average commutes. Increases in car commuting by Black workers account for nearly one quarter of the decline in the racialized difference in commute times between 1980 and 2019. Today, commute times have mostly converged (conditional on observables) for car commuters in small- and mid-sized cities. However, persistent differences in commute times still remain today in large, segregated, congested, and—especially—expensive cities, revealing the limits of cars in overcoming entrenched racialization of other factors of commuting.

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ABSTRACT

ESSAYS IN HOUSING AND URBAN ECONOMICS

Ellen Fu

Fernando Ferreira

This dissertation examines how housing and location choice decisions contribute to spatial and social inequality. The first chapter studies the financial burdens of property taxes on homeowners. Exploiting a reform in Philadelphia that generated changes in property taxes without changing the provision of public goods and services, I measure how sensitive homeowners are to increases in their property tax bills. I find that a $100 increase in property taxes increases property tax delinquency by 3.9% after one year and 7.7% after two years. Home sales also increase by 4.1% after two years. There is no effect on house prices. Further, the financial burdens of property taxes vary considerably by owner race and occupancy status. White owners are more likely to recover from delinquency and sell their homes than Black and minority owners. Owners who live in their houses are also more likely to sell than landlords. The second chapter studies how the time spent commuting to work have evolved over the last four decades for White and Black commuters. In 1980, Black commuters spent 50.3 more minutes commuting per week than White commuters; by 2019, that difference declined to 22.4 minutes. Two factors account for the majority of this decline: Black workers are more likely to commute by transit, and Black workers make up a larger share of the population in cities with long average commutes. Increases in car commuting by Black workers account for nearly one quarter of the decline in the racialized difference in commute times between 1980 and 2019. Today, commute times have mostly converged (conditional on observables) for car commuters in small- and mid-sized cities. However, persistent differences in commute times still remain today in large, segregated, congested, and—especially—expensive cities, revealing the limits of cars in overcoming entrenched racialization of other factors of commuting.
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1.1. Introduction

Property taxes are an essential source of revenue in the U.S. Annually, they generate one-third of total state and local government revenues—over $547 billion in 2018—which are used to fund local public goods and services, such as public schools. However, they are also a substantial cost and tax burden to homeowners. On average, property taxes account for 12% of annual homeownership costs for households with mortgages and 28% for households without mortgages (Bradley, 2017). People report disliking the property tax more than any other tax, even though they also report that property tax revenues are better spent than other tax revenues (Cabral and Hoxby, 2012). Property taxes are also collected in lump-sum payments annually at the beginning of each fiscal year, and often increase every year, further making them unpopular with homeowners. Households have actively voiced their opposition towards property taxes and have pushed for tax reforms to reduce their yearly property tax burdens. To date, residents in 46 states have pressured their local governments to adopt some form of property tax limitations (Anderson, 2006).

These stylized facts suggest that homeowners, especially those with constant incomes, may be sensitive to large increases in their property tax bills, and some may not be able to afford the increases. However, this margin—the financial burdens on homeowners when their property taxes changes—is not well studied in the property tax literature. Standard economic theories have mainly focused on the legal and economic incidence of property taxes, discussing the capitalization of taxes into prices. While capitalization is important, it is only part of the full incidence story. There are other consequences that come with changes in property taxes that are critical to examine in order to fully understand what

happens when property taxes change. To investigate what are the financial burdens on homeowners are when property taxes change, I examine two specific empirical questions: (1) can homeowners afford to pay their tax bills and to stay in their homes when their property taxes increase?; and (2) do the financial burdens of property taxes fall equally on different types of homeowners?

To answer these empirical questions, I exploit a unique natural experiment in the city of Philadelphia where a property tax reform generated quasi-random variation in property taxes while the provision of local public goods and services stayed constant. Prior to this reform, Philadelphia did not have a reassessment schedule, and consequently was taxing homeowners using property assessments that were far below the current fair market values. The reform, implemented in 2014, initiated a reassessment of all pieces of real estate in the city and resulted in a massive increase in property assessments: the average property assessment on a single-family property rose from $16,575 in 2013 to $140,886 in 2014. At the same time, the city lowered the property tax rate by almost 90% so that the total tax revenue after the reform would be the same as before the reform; this meant that local public goods and services did not change during this time period. This “revenue neutrality” condition is key to my identification strategy because it allows me to difference out any changes in public goods and services due to the taxes, and to cleanly isolate the causal effects from changes in property taxes.

The reform resulted in large variation in individual property tax bills, raising taxes for some homeowners and lowering taxes for other homeowners. The average change in tax bill is only around $100, while the standard deviation is $770. Additionally, the reform updated the city’s property assessment algorithm. In this new algorithm, census tracts are the main determinants of the size of the reassessments, which results in discontinuities in the changes in property tax bills across census tract boundaries. Given this spatial variation, I

\[\text{The city lowered the real estate tax rate from 9.771% in 2013 to 1.34% in 2014 with the intention of offsetting the assessment increases so that total real estate tax revenues would remain the same before and after the reform, see: https://www.phila.gov/media/20211217105117/Historic-Tax-Rate-PDF-Template-update-December-2021.pdf}\]
use a border discontinuity design to compare properties in “treated” tracts—census tracts that received larger reassessments (and subsequently larger increases in property taxes) than their bordering neighbors—with nearby properties in “untreated” tracts that received lower reassessments. To measure the financial burden of changes in property taxes on households, I examine the following outcomes: property tax delinquency, home sales, and house prices.

I look at these outcomes of interest—delinquencies, sales transactions, and house prices—because they allow me to directly measure how sensitive homeowners are to property tax changes and what the magnitudes of their financial burdens are. There are real consequences to being delinquent in Philadelphia, and property tax delinquency helps me understand if homeowners can afford to pay their tax bills. Homeowners that are delinquent on their taxes face the risk of foreclosure, which means it is unlikely that homeowners would choose to become delinquent unless they cannot pay their bills. Thus, increases in delinquencies following the reform would indicate that homeowners do not have the financial means to pay their outstanding tax bills. Similarly, sales transactions help me understand if homeowners can afford to stay in their current homes. Because there are large costs associated with moving and selling their homes, homeowners will not choose to move out of their current homes unless the financial burdens of property taxes exceed the moving costs. Thus, an increase in home sales after the reform would indicate that their homes are no longer affordable due to the property taxes. I also look at house prices to investigate the capitalization of property taxes. By examining how prices respond after the reform, I can determine if households value the implied added benefits that come with higher taxes. Finally, I look at heterogeneity in these outcomes by homeowner characteristics, which provides me with insights into the distributional consequences of changes in property taxes.

I find empirical evidence that changes in property taxes do not affect prices, but do lead to more property tax delinquencies and home sales, which confirms the motivating hypothesis that property taxes do have consequences beyond just price capitalization. I find that home-

See Appendix A1 for a more thorough explanation as to why I chose these outcome variables.
owners that received larger increases in their tax bills are more likely to undergo property tax delinquencies and sales transactions than homeowners that received smaller increases in their tax bills, providing evidence that, on average, homeowners struggle to pay when their property taxes go up. In particular, a $100 increase in property taxes, which is about a 6% increase in the average annual property tax bill, increases tax delinquencies by 3.9%. This increase in delinquency happens immediately, and increases to 7.7% by the second year of the reform. Home sales do not change immediately, but they increase by 4.1% in the second year, which is suggestive of a latency effect, perhaps because homeowners do not immediately internalize the full cost of their higher property tax bills. Interestingly, I find no price capitalization. Altogether, these findings suggest that homeowners are financially burdened when their property taxes increase. However, given that the magnitude of the home sales elasticity is smaller than the magnitude of the delinquency elasticity, only a subset of homeowners fully adjust their housing consumption in response to property tax increases.

Moreover, I find heterogeneity in outcomes across different types of homeowners. I find that white owners on average receive larger tax increases and are twice as likely to experience delinquency following increases in property taxes than black and minority owners (4.4% compared to 2.3%). Intriguingly, I also find that home sales increase by 4.9% for White owners but exactly 0% for Black and minority owners. Taken at face value, these results suggest that black and minority owners are more committed to staying in their homes, even conditional on being delinquent. One possible explanation for this could be because they face higher moving costs or fewer outside housing options, which consequently results in them bearing more of the economic burden compared to white owners when their property taxes increase. I also find that landlord and owner-occupied properties have similar delinquency propensities, but landlords are twice as likely to sell their homes (2.6% compared to 1.0%).

My paper contributes to the existing literature in several ways. First, I offer a better empirical setting for studying property taxes because I can directly control for the provision
of public goods and services. In particular, in the property tax capitalization literature, there is an active discussion about whether increases in taxes are offset by the additional benefits from public goods. However, the empirical findings so far have been mixed (see, e.g. Yinger (1982); Russo (2008); Oates (1969); Bartolomé and Rosenthal (1999); Elinder and Persson (2014)). Previous research papers are not well-identified because changes in property taxes are usually accompanied by changes in tax revenues and spending, making it difficult to disentangle the effects of the latter from the former. As a result, the estimated degree of capitalization varies depending on the data, setting, and assumptions used (Hilber, 2017; Lillywhite, 1994; Sirmans et al., 2008; Lutz, 2008, 2015; Lang and Jian, 2004). Using Philadelphia as my setting, I am able to overcome these empirical challenges and, thereby, keep tax revenues and spending completely fixed. With my empirical setting, I find a null result in price capitalization, in contrast to previous papers that have found partial capitalization.

In addition to price effects, I also examine delinquency effects. My findings on tax delinquency and home sales align with what other papers in this space have found, which reaffirms the importance of this financial burden channel. Hayashi (2019) and Wong (2020) both find positive relationships between property tax hikes and mortgage delinquencies, similar to my results for property tax delinquency. The magnitudes of my results are larger though—Wong (2020) finds that a monthly tax increase of $50, which equates to an annual tax increase of $600, increases mortgage delinquency by 9%, whereas a $600 increase would generate a 23.4% increase in property tax delinquency according to my estimates. Similarly, Anderson and Dokko (2008), focusing on subprime mortgages, show with an event study that payment shocks from property tax bills contribute to greater numbers of missed mortgage payments and foreclosures, Atuahene and Berry (2018) find that residential properties with higher tax assessment ratios sold after 2009 are more likely to experience a subsequent tax foreclosure, and Campbell et al. (2011) find that foreclosures lead to lower house prices. My paper also extends the consumption commitment literature to the context of property taxes: homeowners exhibit excess smoothness in their consumption (they do not adjust
their housing consumption and instead incur losses, i.e., undergo delinquency) when their property tax shocks are small, but they adjust (i.e., sell their homes) when their tax shocks are large (Chetty and Szeidl, 2016; Shore and Sinai, 2010; Sinai and Souleles, 2013).

Lastly, I help extend the growing literature on the distributional effects of property taxes by providing causal evidence that the financial burdens of property taxes vary by homeowner race and occupancy status (owner-occupied or landlord). My findings build upon previous papers that have documented disparities and inequities in who bears the tax burdens. Amornsiripanitch (2020) shows that property taxes are regressive, with owners of cheap houses paying almost 50% higher effective tax rates than owners of expensive houses. Löffler and Siegloch (2021) find that property taxes are fully passed through to renters when housing supply is inelastic, which suggests that landlords are better shielded from tax hikes. Additionally, Avenancio-Leon and Howard (2019) highlight the existence of racial inequalities in property tax assessments, documenting that black and Hispanic residents face higher tax burdens for the same bundle of public services than white residents. Bayer et al. (2017) reaffirm the existence of racial disparities by showing that black and Hispanic homebuyers in their four cities of interest pay a premia of around 2% on average. There is also a growing sub-literature on the relationship between property taxes and gentrification (Singh et al., 2019; Fraenkel, 2021; Pennington, 2021). In particular, Ding et al. (2016) and Ding and Hwang (2020) use the same setting of the Philadelphia property tax reform as me, and, using a difference-in-differences strategy, they find that property taxes are higher in gentrifying areas compared to already gentrified areas. While my paper does not identify the precise mechanisms driving my heterogeneity results, other papers have also explored the role of behavioral biases (e.g., inattention, tax salience, and loss aversion) in explaining disparities in property taxation (Chetty et al., 2009; Bucks and Pence, 2008; Chetty et al., 2014; Chirico et al., 2016, 2019; Finkelstein, 2009; Myers, 2019; Bradley, 2017; Bradley et al., 2013; Cabral and Hoxby, 2012; Jones, 2020).

The rest of the paper proceeds as follows. Section 2 provides background on the Philadelphia
property tax reform. Section 3 discusses the data sources. Section 4 outlines the spatial variation that motivate the empirical strategy. Section 5 describes the border discontinuity design. Section 6 presents the main regressions results. Section 7 investigates heterogeneity in outcomes by homeowner race and home occupancy, suggesting possible explanations and avenues for further research.

1.2. Philadelphia Property Tax Reform

In the U.S., property taxes are levied by state and local governments, which means that each jurisdiction has full control over how and how much to tax its residents. By definition, property taxes are determined by two factors: the property assessment value and the property tax rate, also known as the millage rate. In Philadelphia, property assessments are determined on a citywide basis and independently from when the properties were sold. Whenever the city decides to conduct reassessments, it does so for all real estate properties simultaneously: it first sends appraisers to physically examine and collect data on all the properties, and then it uses a proprietary valuation model to calculate the new property assessment values. Homeowners are typically notified of their property assessments by mail in May, before the start of the fiscal year, and have until October of that same year to appeal the reassessments if they believe the updated values are incorrect. In a separate process, City Council sets the millage rate every fall based on the city’s budget plan for the new fiscal year. The city of Philadelphia conveniently only has one county and one school district, which means the millage rate is uniform across all properties in the city limits. Homeowners are informed of their property tax bills in December, and they have until March 31st of the following year to pay their taxes.

For years, Philadelphia’s property tax system has been criticized as outdated and inaccurate. Pennsylvania is one of nine states that does not have any standard assessment method or a reassessment timetable, and, before 2014, Philadelphia had not conducted a comprehensive reassessment in decades. This meant that property taxes were based on outdated property assessments that were far below their current market values. As a result, property taxes
were very low compared to taxes in other nearby jurisdictions. In 2011, the property tax per capita was $729 in Philadelphia compared to $2,799 in Washington, D.C., $2,213 in Boston, and $1,191 in Pittsburgh. Recognizing the issues surrounding its property tax system, the city established the Office of Property Assessment (OPA) in 2010 with the intention of improving and streamlining its assessment valuation process. Then in 2012, Mayor Michael Nutter proposed the Actual Value Initiative (AVI) to completely overhaul the property tax system. The reform was passed in spring 2013 and implemented starting in tax year 2014 (i.e., effective for tax bills due in 2014).

The AVI overhauled property taxation in three major ways: (1) it mandated the reassessment of all residential and commercial real estate properties in Philadelphia for the 2014 tax year to their current fair market value; (2) it updated the property assessment algorithm to improve the accuracy of the property assessment valuations; and (3) it introduced tax exemptions—namely the Homestead exemption, which offered a $30,000 in value exempt from taxation for owner occupied properties, and the Longtime Owner Occupants Program (LOOP), which provided tax reductions for qualified low-income longtime residents—to help offset anticipated increases in property tax bills. In additional to these three changes, the city also lowered the millage rate to preserve revenue neutrality.

The citywide reassessment led to a huge spike in property assessments. Table 1.1 shows the average assessment went from $16,575 in 2013 to $140,886 in 2014, and the average reassessment amount was $124,402. However, even though assessments skyrocketed for all properties in the city, property taxes did not dramatically increase for every homeowner because the city lowered the millage rate by 90%, from 9.771% in 2013 to 1.34% in 2014. The city did this to maintain revenue neutrality, meaning that it wanted the projected 2014 tax revenues to be the same as the 2013 tax revenues. Figure A.2.1 shows the total property taxes of Philadelphia.

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4 For more on Philadelphia’s property tax system, see: https://www.pewtrusts.org/-/media/legacy/uploadedfiles/wwwpewtrustsorg/reports/philadelphia_research_initiative/philadelphiapropertytaxespdf.pdf

5 The AVI imposed separate assessment algorithms for assessing commercial and residential properties; this paper focuses on residential properties.
Table 1.1: Property Assessments by Year

<table>
<thead>
<tr>
<th></th>
<th>2012</th>
<th>2013</th>
<th>2014</th>
<th>2015</th>
<th>2016</th>
<th>2017</th>
<th>2018</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean</td>
<td>16,386</td>
<td>16,575</td>
<td>140,886</td>
<td>141,816</td>
<td>143,145</td>
<td>146,253</td>
<td>148,450</td>
</tr>
<tr>
<td>Median</td>
<td>12,512</td>
<td>12,576</td>
<td>113,700</td>
<td>114,200</td>
<td>114,500</td>
<td>115,900</td>
<td>117,400</td>
</tr>
<tr>
<td>Std. Dev.</td>
<td>18,767</td>
<td>19,936</td>
<td>134,233</td>
<td>135,984</td>
<td>137,701</td>
<td>143,160</td>
<td>146,721</td>
</tr>
<tr>
<td>No. of Obs.</td>
<td>488,233</td>
<td>489,723</td>
<td>483,999</td>
<td>484,963</td>
<td>481,619</td>
<td>476,551</td>
<td>465,712</td>
</tr>
</tbody>
</table>

|                  | 2012    | 2013    | 2014    | 2015    | 2016    | 2017    | 2018    |
|Δ Assessments     |         |         |         |         |         |         |         |
| Mean             | 130     | 148     | 124,402 | 1,456   | 1,143   | 2,600   | 895     |
| Median           | 0       | 0       | 100,200 | 24,290  | 24,660  | 24,682  | 24,681  |
| Std. Dev.        | 4,304   | 5,746   | 117,834 | 24,290  | 24,660  | 24,682  | 24,681  |
| No. of Obs.      | 485,914 | 486,596 | 482,544 | 483,495 | 480,176 | 474,945 | 464,544 |

Assessment values and one year change in assessments of all residential single-family properties in Philadelphia. New construction properties that were not present during the full sample period were dropped, as were properties that had zero as their assessment values (such as properties that received the 10-year tax abatement).

tax revenue collected by the city and the millage rate from 2010 to 2016. The vertical red line at 2014 denotes when the reform went into effect. Tax revenue and spending did not increase after the reform, as seen by the flat tax revenue line in 2014 and 2015. This revenue neutrality condition—a key feature in this empirical setting—allows me to cleanly identify the causal effects from changes in property taxes. This condition only holds in the post reform period from 2014 to 2015 though, because the city increased the the millage rate (and consequently collected more tax revenues) in 2016.

Figure 1.1 and Figure 1.2 show the distribution of property tax bills in Philadelphia. Figure 1.1 shows the distribution of individual property tax bills in 2013 and 2014. The distributions did not change much, due to the revenue neutrality condition. Figure 1.2 shows the distribution of the dollar change in property tax bills after the reform. The distribution is centered around zero, with many homeowners experiencing no changes in their tax bills, but there remains substantial variation in the property tax changes. Over half the homeowners received increases in their tax bills, with some as high as $1,000-$2,000.
1.3. Data

This empirical paper relies on several data sources. First, I use publicly available property data from the Philadelphia Office of Property Assessment (OPA), which includes all real estate properties in Philadelphia from 2006 through 2018. Observations are at the individ-
ual property level, identified by a unique parcel number. The panel contains information on property assessment values, property tax liabilities, housing characteristics, year built, location (longitude and latitude), owner name, and any applicable tax exemption codes. For my analyses, I restrict the sample to only residential, single-family properties that were built before the reform took effect in 2014, so that I can compare outcomes before and after the reform for each property. My sample covers years 2011 through 2015. I don’t include data after 2015 because the revenue neutrality condition does not hold after 2015.

Data for my three outcome variables—property tax delinquency, home sales, and house prices—come from two additional sources. For property tax delinquency, I use confidential property tax payments data from the Philadelphia Department of Revenue, which contains detailed records of real estate payments associated with each residential property listed in the OPA property assessment data. It includes variables such as: the year, month, and day that the homeowner submitted tax payments to the city; the total and outstanding (unpaid) property tax balance; the dollar amount of each payment; any late fees incurred; and the owner’s bankruptcy status. From these data, I construct delinquency indicators based on the outstanding tax balance in each fiscal year and the timing of the tax payments. I consider homeowners to be delinquent if they are more than one year late on their property tax bills, which is how Philadelphia formally defines tax delinquency. Thus, in my sample, homeowners are delinquent if they have not paid the full amount of their property taxes one year after the March 31st deadline.

For home sales and house prices, I use transaction level data from Corelogic, which has variables for sales price, owner and seller name, occupancy (owner-occupied or absentee/landlord) at time of sale, and mortgage information if applicable. I generate a home sale dummy variable that equals one if a property transacted at a sales price greater than $1 and the current owner’s name is different from the seller’s name. I exclude outliers in house prices from my sample, including properties where the sales price is $1 or less, or greater than $10 million.
Finally, I merge all three of my datasets together using the unique parcel number, which is available in all three datasets.

1.4. Spatial Variation from Assessment Algorithm

One of the key features of the AVI reform was that it updated the property assessment algorithm. Before the reform, property assessments were extremely outdated and it was unclear what the city even did to determine the assessment values. With the reform, the city adopted an entirely new algorithm for determining reassessments. This change in the algorithm was plausibly exogenous, since the sole motivation for the change was to develop a more accurate valuation model.

The new algorithm for residential properties calculates assessments using standard hedonic variables that are in the OPA property assessment data. The full set of variables are: property type, age, condition, bedrooms, bathrooms, square footage, year built, central air, garage, street parking, view, and location, which the city measures using census tracts.\(^6\)

The residential property assessment algorithm (there is a separate algorithm for commercial properties) in 2014 is represented by the following standard hedonic equation:

\[
\ln(\text{assessment}_i) = X_{i} + location_i + \epsilon_i
\]

I regress the log assessment of house \(i\) on the vector of variables \(X\) and location, as measured by the census tract location, using the 2014 property assessments data. The \(R^2\) of the full hedonic model is 1. Additionally, I also regress each regressor in the hedonic model separately to determine how much each individual regressor explains the data. The \(R^2\) from the individual regressors are shown in Table A.2.1.

\(^6\)This list of variables was given to me by the OPA’s Deputy Chief Assessment Office in March 2018. I had a few phone calls with him in early 2018 to learn more about the assessment algorithm. While he was not able to share all the details to the assessment process with me, he graciously emailed me the variables used in the algorithm, which I used to replicate the city’s model. He also told me that the location variable in the algorithm is measured by census tract. The reassessments that I predict using my hedonic regression are similar to the actual reassessments from the city’s assessment algorithm.
I find that the location variable by itself, as measured by census tract fixed effects, is the main determinant in the assessment algorithm, explaining 99% of the variation in reassessments. The reason for this feature is the OPA uses other properties in the same census tract to inform the assessment values of each individual house. This feature means that different census tracts received different reassessments, which leads to discontinuities in property assessments and property taxes between one census tract and a neighboring census tract.

Figure 1.3: Average Change in Property Assessments By Census Tracts

Figures 1.3 and 1.4 show the average changes in property assessments and property taxes, split into sextiles, by census tracts respectively. In Figure 1.4, the tracts shaded in blue colors actually had lower property taxes after the reform. As these figures show, there are stark discontinuities in property assessments and property taxes across census tracts. In many cases, adjacent census tracts received very different level changes to their property taxes. My empirical strategy relies on this unique quasi exogenous variation from census tract discontinuities.
1.5. Empirical Strategy

1.5.1. Border Discontinuity Design

Exploiting the spatial variation across census tracts, I use a border discontinuity design where I compare properties that effectively have the same housing characteristics and location, but happen to fall on different sides of a census tract border (See e.g., for more on regression discontinuity methods: Aaronson et al. (2021), Bayer et al. (2007), Black (1999), Keys et al. (2010)). I get cross-sectional variation by comparing properties in the treated and control groups, which I define in Section 1.5.2. I also get temporal variation by comparing the pre-reform period (2011-2013) and the post-reform period (2014-2015). I use the size (in dollars) of the property tax changes as my continuous treatment variable.

I estimate the following equation for property $i$ in year $t$ and border $b$:

$$ y_{i,t} = \beta_0 + \sum \beta_t \left( year_t \ast \left[ treated_i \ast TaxGap_{b(i)} \right] \right) + year_t + property_i + \mu_{i,t} \quad (1.1) $$
where \( y_{i,t} \) is the outcome variable of interest, \( treated_i \) is the treated group indicator, \( TaxGap_{b(i)} \) is the difference in average tax bills between the treated and control groups (i.e., the normalized average tax bill shock of the treated group; I define this variable in Section 1.5.2), \( year_t \) are the year fixed effects, \( property_i \) are the property fixed effects, and \( \mu_{i,t} \) is the error term. \( \beta_{2014} \) is the coefficient of interest and represents the causal effect of a $100 increase in property taxes on the outcome variables. I also include individual property fixed effects, which allows me to control for census tract and border pair fixed effects as well as property level housing characteristics. I cluster standard errors at the zip code level.

The outcomes of interest are tax delinquency, home sales, and house prices. For tax delinquency, I regress Equation (2.2) on the delinquency indicator that equals 1 if a homeowner is delinquent and 0 if a homeowner is not delinquent. Similarly, I regress Equation (2.2) on the sales transaction indicator that equals 1 if a property transacted and 0 if a property did not transact. For house prices though, I measure price capitalization by comparing the actual sales prices of homes that transacted before and after the reform. In the house prices regression, I omit properties that did not have transactions before and after the reform, which greatly reduces the number of observations available. However, this repeat sales approach allows me to control for unobservable property characteristics and estimate a more precise \( \beta_{2014} \) for house prices.

1.5.2. Treated and Control Groups

I define my treated and control groups in the following manner. First, for every residential property in my estimation sample, I identify its nearest neighboring census tract and the corresponding border segment using its geographical coordinate points. All the properties in the sample are within 0.1 miles away from another census tract. For properties that have multiple neighboring census tracts within 0.1 miles, I use the census tract that is closest. Each border segment borders exactly two census tracts. I assign border pair dummy variables to each unique border segment.
Then, for each unique border pair, I calculate the average change in property taxes for each census tract in the pair. The census tract with the larger change in property taxes of the two is the one that received a larger treatment. Within a given border pair, I assign the properties located in the census tract with the larger change in property taxes to the treated group, and properties located in the census tract with the smaller change in property taxes to the control group. Lastly, I demean the differences in property tax changes between the two census tracts in each border pair; I refer to this difference as the tax gap.

Figure 1.5 illustrates how I assign properties into the treated and control groups. In this example, House 1 and House 2 are both located in Tract B. For each house, I identify the nearest border segment. In this case, House 1 is closest to the blue border segment, which contains tract pair A and B, and House 2 is closest to the red border segment, which contains tract pair B and C. The average change in property taxes in Tract A, B, and C are $250, $300, and $500 respectively. For each border segment pair, I assign properties in the tract with the larger change in tax bills to the treated group, and properties in the tract with the smaller change in tax bills to the control group. This means that House 1 is assigned to the treated group with a tax gap of $50, and House 2 is assigned to the control group with a tax gap of $200. Even though both houses are part of tract B which experienced an average change in tax bill of $300, House 1 and 2 are assigned to different groups and have different tax gaps.

Figure 1.5: Illustration of Property Assignments to Treated and Control Groups

House 1 is assigned to treated group. Its tax gap is $50
House 2 is assigned to control group. Its tax gap is $200
1.5.3. **Unconditional Event Study Plots**

I create unconditional event study plots of the data to further motivate the empirical strategy. Figures 1.6–1.9 show the unconditional differences in property tax bills, delinquencies, home sales, and house prices between the control and treated groups, at each year of the estimation period. In Figure 1.6 and Figure 1.7, there is a noticeable increase in property taxes and delinquencies respectively when the policy reform was implemented in 2014. The treated group initially had lower average tax bills than the control group, but it had on average $50 more in taxes after the reform. Similarly, there were no differences in delinquencies between the two groups prior to the reform, but the difference widens to over 1% after 2014. For home sales, there is no immediate jump in volume after the reform, but the number of home sales in the treated group increases slightly in the second year of the post-reform period. For house prices, there is no significant change in prices over this time period.

Figure 1.6: Differences in Property Taxes Between Treated and Control Groups

1.6. **Results**

Table 1.2 reports estimates of $\beta_t$ from Equation (2.2) for each of the outcomes of interest. The results show that properties in tracts that received larger changes in property taxes have
higher tax delinquencies and home sales, providing evidence that on average homeowners are financially burdened when property taxes go up. Specifically, Column (1) shows that a $100 increase in property taxes leads to an immediate 0.9 percentage point increase in delinquencies. Dividing by the baseline delinquency rate in 2013, that estimate translates to an elasticity of 3.9%.

The tax delinquency results are striking because delinquency rates in Philadelphia were actually declining before the reform. As Table A.2.2 shows, the delinquency rate had
decreased by 1.4 percentage points between 2012 and 2013. However, once the reform took
effect, delinquency increased back to around the 2012 level. The magnitude of $\beta_{2014}$ for
delinquency is also relatively large, given that a mere $100$ increase in property taxes—
which is only about a 6% increase in the average property tax bill—increases delinquency
by 3.9%. These results indicate that homeowners are indeed financially burdened when their
property taxes increase. Moreover, the coefficient for $\beta_{2015}$ is also positive and significant,
which suggests that the financial burdens from property taxes persist even after one year.
By 2015, property tax delinquency doubles to 7.7%, even though property taxes did not
change between 2014 and 2015.

Column (2) shows the home sales results. The $\beta_{2014}$ coefficient for home sales is precisely
zero, which means property taxes have no immediate effect on home sales. This finding is not
surprising, because increases in property taxes have to be very large in order for homeowners
to sell their homes and move. However, the $\beta_{2015}$ coefficient shows that home sales increase
by 0.001 percentage points, which equates to a 4.1% from the baseline. In other words, by
the second year of the reform, property taxes cause some homeowners to no longer be able
to afford to live in their homes. This one-year lag in home sales seems reasonable because
it takes time to sell a house. Moreover, this finding suggests that homeowners do not
Table 1.2: Estimated Effects from Property Taxes

<table>
<thead>
<tr>
<th></th>
<th>Delinquency (1)</th>
<th>Home Sales (2)</th>
<th>House Prices (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \beta_{2011} )</td>
<td>0</td>
<td>0</td>
<td>-0.021*</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>( \beta_{2012} )</td>
<td>0.001*</td>
<td>0</td>
<td>-0.005</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>( \beta_{2014} )</td>
<td>0.009***</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>( \beta_{2015} )</td>
<td>0.009***</td>
<td>0.001***</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>( \beta_{2016} )</td>
<td>0.005***</td>
<td>0</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.628</td>
<td>0.174</td>
<td>0.758</td>
</tr>
<tr>
<td>( \text{Obs.} )</td>
<td>2,233,478</td>
<td>2,289,619</td>
<td>19,405</td>
</tr>
</tbody>
</table>

Notes: * \( p < 0.05 \), ** \( p < 0.01 \), *** \( p < 0.001 \).

immediately internalize the full costs associated with their higher property tax bills. This could be attributable to homeowners’ inattention to property taxes or expectations of their future property tax bills. Since the reform was completely unprecedented and unexpected, perhaps homeowners did not believe their property taxes would permanently increase.

Altogether, these findings suggest that homeowners are financially burdened when their property taxes increase. However, the magnitudes of the home sales coefficients are smaller than the magnitudes of the delinquency coefficients. This means that only a subset of homeowners who received property tax increases (and subsequently, became financially burdened) decided to move out of their homes. This aligns with the consumption commitment theory, which states that households will not adjust their housing consumption unless the costs (from property taxes) are very large.

Column (3) of Table 1.2 shows the estimated coefficients for house prices. Unlike the delinquency and home sales results, \( \beta_{2014} \) for house prices is exactly 0 and statistically insignificant, which implies no price capitalization effects. This means that changes in property taxes do not affect house prices at all, which is the opposite of what the price capitalization literature predicts. In the standard model of property tax capitalization, the
current house price $P$ is expressed as the following, per Summers (1985):

$$P = \sum_{n=0}^{N-1} \frac{R - T}{(1 + r)^n}$$

where $r$ is the constant discount rate, $N$ denotes expected house lifetime, $R$ is revenues from potential rents, and $T$ is total tax payments, which can be decomposed into the effective property tax rate ($\tau$) times the taxable property assessment value ($V$). A change in $T$, ceteris paribus, should lead to a change in $P$. One possible explanation for my precise null finding is that households did not internalize the changes in their property tax bills. The AVI reform was completely unexpected and unprecedented for everyone, so it is reasonable that households might not have viewed the changes in their tax bills in 2014 as a permanent change in $T$. Additionally, given that the overall distribution of property taxes did not change from 2013, it is also possible that the changes in individual property taxes only led to a marginal change in $T$, which therefore would not result in any significant changes in house prices.

1.7. Heterogeneity

While the preceding results clearly show that homeowners face financial burdens when their property taxes increase, these results may vary across different homeowners. I am able to add in additional homeowner characteristics to my estimation sample, and use them to examine heterogeneity by homeowner race and occupancy status.

1.7.1. Imputing Owner Race and Occupancy

I do not direct observe owner race in my data, so I use a two-step imputation procedure based on homeowners’ names and census tracts, per Diamond et al. (2019). In the first step, I use “NamePrism”, a non-commercial ethnicity classification tool intended to support academic research (Ye et al., 2017), to calculate baseline probabilities of race for each homeowner based on his or her first and last name. In the second step, I apply Bayes’ Rule to update
the name-based probabilities using the local racial distribution at each owner’s place of residence.

In the first step, I input the first and last names of all the owners in my estimation sample into the “NamePrism” online tool, and obtain baseline probabilities for the six ethnic categories defined by the U.S. Census Bureau: non-hispanic white, non-hispanic black, non-hispanic Asian and Pacific Islander, non-hispanic Native American, and non-hispanic multi-racial. “NamePrism” assigns each owner a probability, ranging from 0 to 1, of belonging to each of these six groups. For each name, the probabilities across the six groups sum up to one. The probability of name \( n \) belonging in race \( r \) is denoted as \( Pr(r|n) \).

In the second step, I update each owner’s baseline racial probabilities using the racial characteristics associated with the census tract that he or she lives in. In particular, from the American Community Survey, I obtain the proportion of the population in census tract \( t \) that belongs to race \( r \), denoted as \( Pr(t|r) \). Applying Bayes’ rule gives the probability that an owner with name \( n \) living in census tract \( t \) belongs to race \( r \):

\[
Pr(r|t, n) = \frac{Pr(r|n)Pr(t|r)}{\sum_{r' \in R} Pr(r'|n)Pr(t|r')}
\]

where \( R \) is the set of 6 race categories. This Bayesian updating procedure requires that the probability of living in a given census tract, given one’s race, is independent of one’s name.

Finally, I assign each owner name to the race that corresponds to the maximum of the six posterior probabilities. I only assign a race to the owner name if the maximum probability is 0.8 or higher. I also simplify the race variable into four categories: White, Black, Hispanic, and Asian. Table 1.3 shows the predicted race assignments for owners in my estimation sample. Compared to the actual race percentages from the 2013 Census, this imputation method overpredicts White and underpredicts Black.

I also add in owner occupancy status (i.e., owner occupied or landlord) to my estimation
Table 1.3: Racial Distribution of Homeowners

<table>
<thead>
<tr>
<th>Predicted Race</th>
<th>Freq.</th>
<th>Share</th>
<th>Share of Residents</th>
<th>Share of Homeowners</th>
</tr>
</thead>
<tbody>
<tr>
<td>White</td>
<td>175,165</td>
<td>62.2%</td>
<td>41.3%</td>
<td>45.7%</td>
</tr>
<tr>
<td>Black</td>
<td>71,406</td>
<td>25.3%</td>
<td>43.5%</td>
<td>42.8%</td>
</tr>
<tr>
<td>Hispanic</td>
<td>18,763</td>
<td>6.7%</td>
<td>6.4%</td>
<td>4.4%</td>
</tr>
<tr>
<td>Asian</td>
<td>16,444</td>
<td>5.8%</td>
<td>5.9%</td>
<td>5.2%</td>
</tr>
</tbody>
</table>

Predicted race from the two-step imputation procedure. Each owner is assigned to one race category. The share of residents and homeowners are calculated using the 2013 Census data.

sample using the Homestead tax exemption code. The Homestead exemption gives owners a $30,000 reduction in their property assessment value, and are only available for owner occupied properties. Owners have to submit an application to receive the exemption, but all owner-occupied properties quality for the exemption, and all completed applications are automatically approved by the OPA. I infer whether properties in 2014 and 2015 are owner occupied based on whether they received the Homestead exemption, and I fill in the occupancy status for years 2011-2013 by looking at whether the owner names in those years were the same as the 2014 owner names. While this proxy is not a perfect measure of occupancy status because owners have to actively apply for the exemption, the measurement error is low. I validate this by comparing the proxy with the occupancy status indicator from Corelogic at time of sale as well as the census tract level owner occupancy rates from the ACS, and they match perfectly.

1.7.2. Triple Difference

Appendix Figures A.2.4–A.2.11 show the unconditional changes in tax bills, delinquencies, home sales, and house prices for white homeowners and black homeowners between the treated and control groups. Figure A.2.4 and Figure A.2.5 show that White homeowners on average face larger increases in their tax bills than black homeowners. Figure A.2.6 shows that delinquencies for white owners increase to 1.5% immediately following the reform, and then drops back to the initial baseline level of 0.5%. In contrast, Figure A.2.7 shows that black homeowners experience a more progressive and persistent rise in delinquencies in the
post-reform years. Both Figures A.2.8 and A.2.9 show no discernible increases in home sales after the reform because the number of sales overall is very low, but white owners have a larger confidence interval around their home sales points. Lastly, Figures A.2.10 and A.2.11 show that house prices do not change for either groups. Overall, these figures support the need to examine heterogeneity in outcomes.

Building upon the border discontinuity design, I investigate the heterogenous effects using a triple-difference framework, where $Group_i$ denotes the more sensitive group:

$$y_{i,t} = \beta_0 + \sum \beta_{1,t}(year_t \ast [treated_i \ast TaxGap_{b(i)}]) + \sum \beta_{2,t}(year_t \ast [treated_i \ast TaxGap_{b(i)}] \ast Group_i) + year_t + year_i \ast Group_i + property_i + \mu_{i,t}$$

Drawing from findings in the literature, I assign black homeowners as the more sensitive group because black households tend to have lower incomes than white households, which likely means they would be more sensitive to their housing costs. Similarly, owners would also be more sensitive because they have to bear the full economic incidence of property taxes, whereas landlords can pass their taxes onto renters.

1.7.3. Heterogeneity Results

Table 1.4 shows the heterogeneous effects of increases in property taxes by homeowner race. White owners have similar delinquency effects as the overall sample, unsurprising given that they represent the majority of the overall sample. Black owners, on the other hand, are 0.3 percentage points more likely than white owners to become delinquent in 2014, and the magnitude of $\beta_{Black}$ continues to increase in 2015 and 2016. However, dividing by white and black owners’ respective baseline delinquency rates, these estimates translate to delinquency elasticities of 4.4% for White owners and 2.6% for Black homeowners, which indicate that White owners are twice as likely to face delinquency than minority or black owners. Interestingly, while both groups face persistent delinquency after 2014, Black owners are more
likely to undergo delinquency in 2015 and 2016. This suggests that it is easier for White owners to recover from delinquency, while some mechanism is making it more difficult for Black owners to recover.

Column (2) shows the estimates for home sales. Interestingly, $\beta_{2014}$ is positive for White owners, but precisely zero for Black owners. In fact, by 2015, home sales increase by 4.9% for White owners, which is slightly higher than the 4.1% estimated for the overall sample. In contrast, Black and owners do not sell their homes. Taken at face value, these results suggest that Black owners are more committed to staying in their homes. One possible mechanism for this could be that minority households face higher moving costs or have fewer housing options, which make them more willing to stay in their homes even when it is no longer affordable to stay. Another possible mechanism could be due to differences in attention or information. White owners could be more informed about their property taxes and pay attention when their property taxes change, whereas Black owners might not be aware of changes in their property taxes. For example, households with mortgages are generally less aware of their property taxes because their escrow accounts manage the taxes for them (Cabral and Hoxby, 2012). These results are robust even when accounting for all minority owners, not just Black owners.

Similar to the overall results, there are no differences in price capitalization for either of the two groups.

Table 1.5 shows the results by occupancy. Landlord and owner occupied properties have similar delinquencies (4.3% and 4.1% respectively). However, landlords do not sell their homes, while owners who live in their homes are 3.6% more likely to sell. One possible explanation could be that the relative cost from property taxes is lower for landlords, likely because they have other streams of income. Therefore, increases in property taxes are not large enough to induce them to sell. However, these point estimates are not statistically significantly different between the two groups.
1.8. Conclusion

The city of Philadelphia implemented its Actual Value Initiative with the sole intention of updating the previously outdated property tax system. While the reform did bring property assessments closer to current market values and make the system more fair, this paper finds that the reform also resulted in some unintended financial and distributional consequences. I find that a $100 increase in property taxes leads to a 3.9% increase in delinquencies. This increase happens immediately, and increases to 7.7% by the second year of the reform. Meanwhile, home sales do not change immediately, but they increase to 4.1% in the second year. Moreover, the effects of the reform also varies depending on the homeowner’s race. White owners experience a 4.4% immediate increase in delinquency, while Black owners only experience a 2.6% increase in delinquency. However, Black owners are more likely to

<table>
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<tr>
<th></th>
<th>Delinquency</th>
<th>Home Sales</th>
<th>House Prices</th>
</tr>
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<tbody>
<tr>
<td>$\beta_{White, 2011}$</td>
<td>0</td>
<td>0</td>
<td>-0.03</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>$\beta_{White, 2012}$</td>
<td>0</td>
<td>0</td>
<td>-0.016</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>$\beta_{White, 2014}$</td>
<td>0.007***</td>
<td>0</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.031)</td>
</tr>
<tr>
<td>$\beta_{White, 2015}$</td>
<td>0.008***</td>
<td>0.001*</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.023)</td>
</tr>
<tr>
<td>$\beta_{White, 2016}$</td>
<td>0.003***</td>
<td>0</td>
<td>-0.002</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>$\beta_{Black, 2011}$</td>
<td>0.002</td>
<td>0</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.022)</td>
</tr>
<tr>
<td>$\beta_{Black, 2012}$</td>
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<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.078)</td>
</tr>
<tr>
<td>$\beta_{Black, 2014}$</td>
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<tr>
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<td>(0.00)</td>
<td>(0.055)</td>
</tr>
<tr>
<td>$\beta_{Black, 2015}$</td>
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<td>-0.001</td>
<td>-0.041</td>
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<tr>
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<td>(0.00)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>$\beta_{Black, 2016}$</td>
<td>0.009**</td>
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<td>-0.013</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.00)</td>
<td>(0.045)</td>
</tr>
</tbody>
</table>

$R^2$ 0.67 0.227 0.784
Obs. 1,191,471 1,221,272 3,016

Notes: * p<0.05, ** p<0.01, *** p<0.001.
Table 1.5: Heterogeneity By Occupancy Status Estimates

<table>
<thead>
<tr>
<th></th>
<th>Delinquency</th>
<th>Home Sales</th>
<th>House Prices</th>
</tr>
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<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>( \beta_{\text{Landlord,2011}} )</td>
<td>0</td>
<td>0</td>
<td>-0.042</td>
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<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.027)</td>
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<tr>
<td>( \beta_{\text{Landlord,2012}} )</td>
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<td>-0.001</td>
<td>-0.016</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.040)</td>
</tr>
<tr>
<td>( \beta_{\text{Landlord,2014}} )</td>
<td>0.013***</td>
<td>0</td>
<td>-0.058</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.00)</td>
<td>(0.045)</td>
</tr>
<tr>
<td>( \beta_{\text{Landlord,2015}} )</td>
<td>0.005**</td>
<td>0</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>( \beta_{\text{Landlord,2016}} )</td>
<td>0.004**</td>
<td>0</td>
<td>0.009</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.034)</td>
</tr>
<tr>
<td>( \beta_{\text{OwnerOccupied,2011}} )</td>
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<td>0</td>
<td>0.014</td>
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<tr>
<td></td>
<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>( \beta_{\text{OwnerOccupied,2012}} )</td>
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<td>0.006</td>
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<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.039)</td>
</tr>
<tr>
<td>( \beta_{\text{OwnerOccupied,2014}} )</td>
<td>-0.002</td>
<td>0.001</td>
<td>0.042</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.001)</td>
<td>(0.045)</td>
</tr>
<tr>
<td>( \beta_{\text{OwnerOccupied,2015}} )</td>
<td>0.007***</td>
<td>0.001</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.036)</td>
</tr>
<tr>
<td>( \beta_{\text{OwnerOccupied,2016}} )</td>
<td>0.001</td>
<td>0</td>
<td>-0.013</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.029)</td>
</tr>
</tbody>
</table>

\( R^2 \) | 0.629 | 0.203 | 0.766  
Obs. | 2,233,478 | 2,289,619 | 3,709  

Notes: * \( p<0.05 \), ** \( p<0.01 \), *** \( p<0.001 \).

remain delinquent after the first year. White owners are also 4.0% more likely to sell their homes, while Black and minority owners do not sell their homes.

These results shed light on the financial burdens of property taxes, finding that prices do not change when property taxes change. Instead, the results show that some homeowners struggle to pay their property tax bills. This margin is often overlooked by the taxing entities, but is important to consider since cities need homeowners to stay current on their property tax bills in order to generate enough revenues for public goods and services. As property taxes increase each year, it becomes more important to understand how incremental changes in property taxes will affect the homeowners who bear the legal incidence.

This paper enriches the burgeoning literature on the distributional consequences of property taxes. My heterogeneity results, particularly the findings by race, provide convincing evi-
dence that the financial burdens from changes in property taxes are not distributed evenly across homeowners. While White owners on average received larger increases in their property taxes, they are able to recover from delinquency more quickly, perhaps because they are more able and quicker to adjust their housing consumption. In contrast, Black homeowners will stay delinquent for years, perhaps because they are less informed and unable to adjust their housing consumption even when faced with large losses. These changes in property taxes may even be contributing to the homeownership gap in Philadelphia, where the majority of the population is Black but the majority of homeowners are White.

While this paper is unable to identify the specific mechanisms driving these heterogeneous results, these findings offer important insights into the full economic incidence of property taxes, and suggest some possible mechanisms that future papers to investigate. As we become more cognizant of the unintended consequences that come with property taxation, we can work towards reducing those racial disparities.
CHAPTER 2 : The Problem Has Existed Over Endless Years: Racialized Difference in Commuting, 1980–2019

2.1. Introduction

In 1955, Rosa Parks and five other Black women physically desegregated buses in Montgomery, AL when they refused to give up their seats to White passengers. Parks was arrested, but her arrest ignited the local Black community, brought local leaders together to form the Montgomery Improvement Association (MIA), and motivated them to lead a boycott of the buses until a more just solution was achieved.\(^7\) The year-long boycott involved many Black bus commuters: in the 1960 Census, only 36% of commuters in the most segregated Black census tracts of central Montgomery commuted by car.\(^8\) In addition to coordinating carpooling services for the many Black bus commuters, the MIA organizers faced a myriad of other challenges. Montgomery was very segregated, with Black residents heavily concentrated in neighborhoods away from the mostly White neighborhoods that were closer to the jobs in the city center. Black women in particular were likely to work in domestic service, which entailed commuting to White households scattered throughout the segregated city. Meanwhile, the police sought to intimidate carpool drivers and boycott leaders by pursuing early versions of “driving while Black” policing strategies (Harris, 2010; Rice and White, 2010).

The challenges faced by the MIA highlight how home location, work location, and the means of getting between the two collectively shape the time a worker spends commuting each day. These three factors have changed significantly since the 1960s. Residential segregation, as measured by the Black-White dissimilarity index, has declined somewhat after peaking in 1970, with some Black families now having access to a wider array of neighborhoods (Blair, 2010).

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\(^7\)In fighting for their civil rights within transportation, the women entered a longstanding battleground. The landmark Supreme Court case enshrining segregation, Plessy v. Ferguson, was filed by Homer Plessy over segregated railcars (Plessy v. Ferguson, 163 U.S. 537 (U.S. Supreme Court 1896)). Fights in this arena have continued and expanded, with the Los Angeles Bus Riders Union filing suit over heavy investment in White suburbs relative to the communities of color in Los Angeles proper and adjacent neighborhoods (bus).

\(^8\)By contrast, 90% of commuters in the most segregated White tracts commuted by car (see Appendix).
Occupational segregation has likewise declined; Black workers have greater opportunities in a wider array of occupations and industries (see, e.g., Bahn and Cumming, 2020). Lastly, 85% of Black workers now commute by car, a far cry from the transit and walking dependence of Black commuters in 1950s Montgomery. With more Black workers having access to homes in a wider array of neighborhoods, jobs in a wider variety of occupations and industries, and cars, are commuting outcomes in American cities today equitable by race?

The short answer is no. While the racialized difference in commute times has declined from 50.3 minutes per week in 1980, Black commuters today still spend 22.4 more minutes per week commuting than White commuters. In this paper, we investigate the factors behind this partial convergence and examine the mechanisms operating on individual commuters, on neighborhoods, and on cities that collectively obligate Black commuters into spending more time commuting between home and work.

We develop a decomposition framework to quantify the racialized difference in commute times and determine what portion of its evolution worked through channels observable in our data. Two factors explain more than half of the difference in commute times: Black workers are more likely to live in cities with longer average commutes and to commute by transit. Black workers also hold demographic and job characteristics associated with shorter commutes; these differences partly offset the other factors and lower the racialized difference by 3% in 1980 and by 22% today. Income does not explain the racialized difference either. Income is positively correlated with commute time, and while the racialized difference in commute times is larger among those with low incomes and among transit users, it persists even for those with high incomes and who commute by car.

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9Throughout this paper, we use the language of “racialized difference” to refer to the longer journeys to work reported by Black commuters relative to White commuters. We use this wording—rather than a passive term like “gap”—to highlight that this material outcome is a manifestation of social processes of racialization, the “process that naturalizes social difference” (Chun and Lo, 2015).

10We use “channels” to describe the role of observable characteristics in the manifestation of a racialized difference in commuting. These characteristics are not ‘controls’ that must be accounted for to uncover the effects of racism because the labor and housing markets underlying these characteristics are themselves racialized (Bayer et al., 2017; Neumark, 2018).
Of the total decline in the racialized difference in commute times from 1980 to 2019, we attribute 24% to changes in travel mode and 13% to changes in industry, occupation, and income. Much of the aggregate convergence from travel mode is attributable to an increase in car usage among Black commuters. In 1980, 88% of White commuters and 76% of Black commuters used an automobile. These shares increased to 92% of White commuters and 85% of Black commuters by 2019. Intriguingly, demographics and CZ of residence play almost no role in the decline. The remainder of the overall decline (63%) flows through other channels besides observable commuter-level characteristics.

To go further, we compare patterns of persistence in the racialized difference across cities by investigating segregation and related spatial factors (like job-residence mismatch and low travel speed) that heighten its effect on commute times (Kain, 1968). The extent and commuting implications of segregation vary across CZs. High segregation in cities like Chicago might create long commutes for Black workers who live far from major job centers. Even car ownership may not ensure an easy commute for Black residents of segregated neighborhoods. By contrast, Birmingham, AL, is nearly as segregated, but its small extent (and many freeways) may offer drivers easy access even to jobs across town. We describe this confluence of factors as spatial stratification: the organization of a city whereby segregated Black neighborhoods feature higher travel costs to jobs than do segregated White neighborhoods.

We estimate the residual racialized difference (RRD)—the average commute time difference that does not arise through observable channels—for each city and decade. The RRD has declined since 1980 in most cities. The remaining portion is strongly correlated with city population, suggesting that a large population is now necessary (but insufficient) for a city to generate a racialized difference in commute times. We investigate city-level ingredients for spatial stratification that may contribute to these patterns. We find that segregation and differential access to employment centers both play a role in the persistently high RRD in large cities today. Similarly, infrastructural ingredients of spatial stratification—less freeway
construction and expanding transit, indicators of slower travel speeds—are associated with a larger RRD. Lastly, high housing price growth is a significant driver of persistent positive RRD, a result consistent with spatial stratification. Indeed, had housing prices remained at their 1980 levels, the racialized difference would today be 40% smaller.

Racialized commuting outcomes were a pervasive feature of U.S. geography 40 years ago, present across much of the country regardless of city size or travel mode. The dramatic decline since 1980 belies heterogeneous experiences that are increasingly city specific: for car commuters in small- and mid-sized cities, there has been almost complete convergence, conditional on observed characteristics. Today, the racialized difference in commute times arises primarily in large cities with the ingredients of spatially stratified job access, and among transit commuters everywhere. The evolution of the racialized difference in commuting reflects both meaningful gains for many Black workers and durable barriers to continued convergence.

This paper offers several contributions to literatures within urban economics and inequality. First, we comprehensively quantify the Black-White difference in commute times for all U.S. commuting zones (CZ) and describe its evolution over the last 40 years. This updates prior work that focused on the 1970s and 1980s in a subset of cities (Gabriel and Rosenthal, 1996; Pettitte and Ross, 1999; Taylor and Ong, 1995). Johnston-Anumonwo (1997), McLaugherty (1997), and Johnston-Anumonwo (2001) also study specific cities using 1980 and 1990 commuting data. An often integrated literature shows that mode influences differences in labor market outcomes between Black and White workers. Poor labor market outcomes for Black workers are associated with lack of automobile access (Ong, 2002; Raphael and Stoll, 2001; Ong and Miller, 2005; Gobillon et al., 2007; Gautier and Zenou, 2010), and automobile use plays an outsized role in the reduction of commute times for Black commuters (Johnston-Anumonwo, 1997, 2001; Taylor and Ong, 1995).11 A related and growing

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11The increase in automobile use by Black commuters, though, has expanded the potential for unequal treatment by law enforcement; see, e.g., Feigenberg and Miller (2021). Indeed, Martin Luther King, Jr., faced his first arrest for purportedly driving 5 miles over the speed limit (King, 2010).
literature also examines gendered differences in commuting (see, e.g., Black et al. (2014); Gutierrez (2018); Liu and Su (2020); Hu (2021)).

We extend decomposition methods used in the literature on gender and race wage differences to study individual and city-level explanations of the difference in commuting times (Altonji and Blank, 1999; Blau and Kahn, 2017; DiNardo et al., 1995; Chamberlain, 2016). Like this literature, we account for the role that observable individual demographic and occupation characteristics play in explaining racialized or gendered difference. Blau and Kahn (2017) find that individual characteristics explain very little of the gender wage gap in more recent years, and Altonji and Blank (1999), in a summary of the racial wage gap literature, note that the convergence of individual characteristics over time contribute to the decrease in the gap. The unexplained portion of the gap is traditionally interpreted as a measure of discrimination; however, it may also account for unmeasured productivity or compensating differentials (Blau and Kahn, 2017). Importantly, discrimination may influence observable individual characteristics as well (education, travel mode, residential location, occupation, etc.).

We hypothesize that spatial stratification within cities provides a basis for racialized commuting differences to arise. Existing work on neighborhood sorting contextualizes commuting differences, arguing that transportation rather than housing prices dictate urban patterns of income sorting (Glaeser et al., 2008; LeRoy and Sonstelie, 1983). Lee and Lin (2018) explore how the persistence of neighborhood-level income sorting relates to natural amenities. Aliprantis et al. (2019) observe that in cities without high-income Black neighborhoods, high-income Black households locate in Black neighborhoods with socioeconomic status similar to low-income White neighborhoods. In large cities with large Black populations and high-income Black neighborhoods, this result does not hold. They find that race alone—through possible channels of psychological costs and benefits, white flight, and racial discrimination—and not financial constraints (wealth, housing prices) is driving income and racial neighborhood sorting.
Lastly, we complement a growing literature on Black suburbanization and neighborhood change as it relates to the spatial organization of Black and White households within cities (Wiese, 2005; Card et al., 2008; Blair, 2017) and the related literature on sorting in schools (e.g., Caetano and Maheshri, 2017). Two recent papers are particularly relevant. Bartik and Mast (2021) document some convergence in the neighborhood income levels and poverty rates experienced by White and Black households, a change coming largely from the migration of some Black households to suburban neighborhoods (and not rising incomes in mostly Black central-city neighborhoods). Indeed, about one-third of African Americans lived in the suburbs before 1980; by 2000, nearly two-thirds did (Wiese, 2005). Miller (2018) determines that job suburbanization has decreased Black employment, showing that Black workers are less likely to work in jobs further from city centers even among relocating firms.

The rest of the paper proceeds as follows. Section 2 describes our data. Section 3 showcases the main descriptive statistics that motivate our empirical analyses. Section 4 discusses the methodology used to construct the decomposition, and develops several explanatory variables used to investigate the residual racialized difference by city. Section 5 presents the main regressions and decomposition results. Section 6 investigates spatial stratification using the residual racialized difference by CZ.

2.2. Data

We study commuting time in the United States from 1980–2019 as reported in response to the Census Journey to Work questionnaire. Beginning in 1980, the Census asked long-form respondents to give their usual travel time and primary mode for the one-way journey from home to work in the prior week. Our primary data source is the IPUMS Census and American Community Survey (ACS) public use microdata from 1980, 1990, 2000, and 2005–2019 (Ruggles et al., 2021). We limit our sample to commuters, i.e., those in the labor force actively working outside the home. For a limited set of descriptive variables on mode share, we also draw from 1960 and 1970 Census microdata.
We use slightly modified 1990 commuting zones as our base geography, following Autor and Dorn (2013) and Dorn et al. (2019) to assign observations to commuting zones. We combine five pairs of commuting zones that reflect larger metropolitan areas. Denoted by their largest constituent cities, they are: New York City and Newark, Dallas and Fort Worth, Philadelphia and Wilmington, Charlotte and Gastonia-Rock Hill, and Hickory and Morganton. We adjust observation weights so that the sum of weights is equivalent to the average employed population for each of the following groups of years (year bins): 1980, 1990, 2000, 2005–2011, and 2012–2019. For 2000 and later, we use the Census public use microdata areas (PUMAs) to control for residential location in some specifications (pre-2000 PUMAs do not provide much additional geographic resolution).

We normalize key variables to ensure consistency over time. We top-code travel time to the minimum top-coded value of 99 minutes. To consistently reflect changes in the classifications of transportation modes over time, we use the following mode categories: Walking (walked only), Bicycle, Bus (bus or streetcar), Subway (includes elevated), Railroad (typically commuter rail), Auto (includes motorcycles, taxi, and carpooling), and Other. For nominally denominated variables, we adjust to real using the CPI. We also use a variety of other individual covariates from Census/ACS data; we introduce these as needed below and provide details in the Appendix.

We rely on the definitions of race available in the data. These have evolved over time, though our results are not overly sensitive to these details. For our primary analyses, we denote as “Black” those observations that are recorded as “Black alone or in combination”. However, prior to 2000, the Census did not record responses on multiple races, and so Black is assigned only to those who list Black as their primary race. The share of respondents who list Black along with other races increases substantially after 2010. As a comparison group, we use respondents whose primary race is listed as White or White alone.

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12Travel time is only reported for about one-half of eligible respondents in the 1980 Census, so weights are doubled. The year bins 2005–2011 and 2012–2019 respectively include seven and eight years of a 1% sample of the population, and are thus downweighted by a factor of seven and eight.

13We experimented with using the entire non-Black commuting population as a comparison group; this
We supplement these data with various other data sources that we use to construct the variables included in the CZ-level specifications. This is primarily tract-level data taken from the IPUMS National Historical Geographic Information System (NHGIS) (Manson et al., 2021) corresponding to decennial Census data (1980, 1990, 2000), ACS data (2006-2010, 2014-2018), and Zip Code Business Pattern data (1994, 2000, 2010, 2018).

2.3. Descriptive Statistics

Figure 2.1 graphs the unconditional average one-way commute times in each sample year since the Census began asking questions about travel time. In 1980, the average commute among Black workers was just over 26 minutes, while the average commute among White workers was just over 21 minutes. There was some convergence over the next decade, as average commutes rose to 22 minutes among White workers while commutes fell to about 25.5 minutes among Black workers. After 1990, average commutes trended upward together. By 2019, the average commute among White workers was almost 26 minutes while makes little difference in our main results. When we use CZ-level aggregates (e.g., commuting population), we calculate them from the entire commuting population regardless of race.
the average commute among Black workers was just over 28 minutes.

We also show the full distribution of commute times for White and Black commuters in 1980 and 2012–19 in Figure 2.2 and Figure 2.3, broken into 5-minute wide bins. In 1980, there were substantially more Black commuters in the 30, 45, and 60 minute commute time bins than White commuters, and substantially fewer between 0 and 15 minutes. This pattern of difference is still visible in the 2012–19 histogram, though the distributions are somewhat closer together. Also of note, there are substantially more Black than White commuters with commutes of 90 minutes or longer in both time periods.

Mode is a key determinant of travel time. To provide context, we establish a few facts about commutes on different modes. Figures 2.4–2.6 report the share of commuters that use each mode in each sample year. For this variable, we can extend our window of study to 1960. The solid lines denote the share for White commuters and dashed lines the corresponding share for Black commuters. Figure 2.4 shows the rise of automobile commuting. About 76% of White commuters used private vehicles in 1960, rising to 88% in 1980 and 92% in 2019. Among Black commuters, the share of drivers in 1960 was only about 50%, rising to 76% in 1980 and to just over 85% in 2019. The Black-White difference in commuting by private automobile thus declined by nearly three-quarters since 1960, from 26 percentage points (pp) in 1960 to 12pp in 1980 and about 7pp today.

The increase in automobile share came at the expense of transit share, in particular for buses and streetcars. Figure 2.5 shows the decline in the share of commuters using buses and streetcars, falling from about 8% of White commuters in 1960 to 3.5% in 1980 and just 2% in 2019. For Black commuters, there was an even greater decline, from 24% in 1960 down to just over 12% in 1980 and about 6% in 2019. There was a slight uptick in subway usage among White commuters over the last 40 years (after falling between 1960 and 1980) and a slight decline for Black commuters.

There was also a large decline in the share of commuters that walk to work, as shown in
Figure 2.2: Distribution of Commute Times by Race in 1980

Figure 2.3: Distribution of Commute Times by Race in 2012–2019
Figure 2.4: Unconditional Auto Share

Figure 2.5: Unconditional Transit Share

Figure 2.6: Unconditional Nontransit Share
Figure 2.6. In 1960, nearly 17% of Black commuters and 11% of White commuters walked to work. By 1980, walking had mostly converged to about 6% for both Black and White commuters, and fell further to about 3% for both groups by 2019. Conversely, bicycle use increased slightly, as did the ‘Other’ category, which includes commutes via modes not elsewhere categorized (this residual category includes bicycles before 1980). These large shifts in commute share reflect substantial suburbanization over the latter half of the 20th century largely driven by expansion of the Interstate Highway System (Baum-Snow, 2007), which also had the effect of spatially separating residential location and place of work (Baum-Snow, 2020).

Differences in commute times for Black and White commuters persist when examining specific modes and are not decreasing uniformly. Figure 2.7 reports the evolution of average commute times by mode from 1980 to 2019, with 2005–2011 and 2012–2019 binned together. Solid blue lines denote average travel time for White commuters and dashed red lines show the corresponding time for Black commuters. All three transit modes have longer average commutes than driving, while bicycling and walking show shorter average commutes (other is a bit longer on average than driving). Travel times are generally trending upward for most modes, with the possible exception of subway.

For most modes, Black commuters face longer travel times than White commuters (the only exceptions are railroad and other, which contain about 3% of the employed population). Times for private automobiles evolved similarly to overall times, showing some degree of convergence between 1980 and 2019. For transit modes, however, differences in average commute times have been static or increasing. This divergence is particularly notable for subway commuters, as average times for White subway commuters have fallen since 1980 while they have risen for Black subway commuters.
2.4. Methodology

Our baseline measure of the racialized difference in commute times between Black and White workers is a simple regression of log commute time in minutes on race. For commuter $i$ in commuting zone $c$ in year bin $t$, we specify:

$$\ln(\tau_{ict}) = \beta_t \mathbb{I}[\text{Black}_{ict}] + \lambda_t + \epsilon_{ict}$$

(2.1)

where $\tau$ is the log reported travel time for a one-way commute, $\lambda_t$ are year dummies, $\epsilon$ is the error term, and the subscript $t$ on coefficients indicates that they are time varying across year bins. We cluster by commuting zone throughout the paper. The $\beta$ coefficient corresponds to the overall racialized difference, $\Delta_t = \beta_t$.

We extend the baseline model to account for selection into different commuting zones, variation across time, and a variety of individual characteristics. The purpose of this exercise is to observe how the coefficient on $\mathbb{I}[\text{Black}]$ changes when these various controls are included.
in the following specification:

$$\ln(\tau_{ict}) = \beta_t^* \mathbb{I}[\text{Black}_{ict}] + x'_{ict} \mu_t + \lambda_{ct} + \epsilon_{ict} \quad (2.2)$$

where \(x\) are individual and job characteristics and \(\lambda_{ct}\) are commuting zone-by-year bin fixed effects. We denote the coefficient on \(\mathbb{I}[\text{Black}]\) as \(\beta^*\) to differentiate from \(\beta\) in Equation (2.1).

We group variables into four sets based on the information they contain:

- **Demographics and Education**: sex; indicators for education (less than high school, high school, college graduate, and masters or higher); a quadratic in age; marital status; head of household; and indicators for numbers of children (zero, one or two, and three or more).

- **Transportation Mode**: indicators for each mode, including private motor vehicle (including motorcycle, taxi, and carpool); bus or streetcar; subway or elevated; railroad (commuter rail); bicycle; walked only; and other.

- **Work and Income**: an indicator for zero income; log income (set to 0 if zero income); indicators for industry and for occupation (1990 IPUMS basis).

- **Commuting Zone**: fixed effects for each commuting zone of residence.

When specifications report year-bin-specific estimates of \(\beta^*\), controls are also interacted with year bins to allow for time-varying correlation with commute time.

The coefficients \(\beta_t\) and \(\beta_t^*\) provide unconditional and conditional regression-based measures of the racialized difference in commute times. However, it is important to note that there are two significant caveats in their interpretation. The first challenge is conceptual: which estimate (\(\beta\), \(\beta^*\), or one in between) should we take as being the ‘truest’ measure of the racialized difference? The values of these covariates may themselves be determined in part by other manifestations of structural racism, in which case these covariates may lead to collider bias in the estimation of the racialized difference. Alternatively, we interpretation
the response of the estimates to these covariates as a way to understand the varied channels through which the racialized difference manifests.

The measure may also reflect selection into the workforce and into employment, as we do not observe commute times for those who do not commute for work. There is conflicting evidence about how adjusting for labor force participation might impact $\beta$ and $\beta^*$. Gabriel and Rosenthal (1996) use plausibly excludable household income variables to control for selection into labor force participation; however, such controls seem to matter little for their results. On the other hand, Raphael and Stoll (2001) find that car ownership can be important for closing differences in employment levels by race, and Black et al. (2014) show that women are less likely to work in long commute cities, suggesting that commuting mode (and time) impact the marginal worker’s entry decision. We acknowledge this may be an important margin for adjustment, and control for a wide variety of individual and city characteristics to limit such concerns. The likely consequence is that our results underestimate the true difference. That is, accounting for the entry would likely produce larger estimates of the racialized difference.

2.4.1. Decomposition

We now describe how to interpret the coefficients $\beta_t$ and $\beta^*_t$ in a decomposition framework (Kitagawa, 1955). Consider a model with heterogeneous coefficients by race:

\[
\begin{align*}
\ln(\tau_{ict}) &= \alpha^W_t + x'_{ict}\mu^W_t + \lambda_{ct} + \epsilon^W_{ict} & \text{if } I[\text{Black}_{ict}] = 0 \\
\ln(\tau_{ict}) &= \alpha^B_t + x'_{ict}\mu^B_t + \lambda_{ct} + \epsilon^B_{ict} & \text{if } I[\text{Black}_{ict}] = 1
\end{align*}
\]

where $B$ indexes the sample and coefficients if $I[\text{Black}_{ict}] = 1$, and $W$ indexes the sample and coefficients if $1[\text{Black}_{ict}] = 0$. The overall racialized difference can be decomposed as
follows, per Fortin et al. (2011a):

\[
\Delta = \left( (\alpha^B - \alpha^W) + \bar{x}^B' (\mu^B - \mu^W) \right) + \left( (\bar{x}^B - \bar{x}^W) \mu^W + \sum (p^B_c - p^W_c) \lambda_c \right)
\]

where \(\bar{x}^k\) is the group-\(k\) average of \(x\) and \(p^k_c\) is a the share of the overall population of \(k\) that lives in \(c\); the time-varying coefficient notation is suppressed for brevity. \(\Delta^{\text{Explained}}\) is the portion of the racialized difference that operates through channels associated with observed characteristics, and \(\Delta^{\text{Unexplained}}\) is the portion that operates through unobserved channels.

Fortin (2008) describes a ‘regression-compatible’ variant of this decomposition framework that we adopt to simplify estimation and exposition. It assumes that the coefficients estimated from a single-regression model like Equation (2.2) provide a valid counterfactual for conditional commuting times. Equivalently, this requires that \(\mu\) capture the relevant conditional effect regardless of race (i.e., \(\mu^B = \mu^W = \mu\)). Under this assumption:

\[
\Delta = (\alpha^B - \alpha^W) + (\bar{x}^B' - \bar{x}^W') \mu + \sum (p^B_c - p^W_c) \lambda_c
\]

\[
\Delta = \beta^* + \Delta^{\text{Explained}}
\]

and \(\beta^* = \Delta^{\text{Unexplained}}\) is the portion of the racialized difference unexplained by observables. The decomposition identifies the role of each channel in determining \(\Delta_t^{\text{Explained}}\):

\[
\Delta_t^{\text{Explained}} = \Delta_t^{\text{Demographics & Education}} + \Delta_t^{\text{Transit Mode}} + \Delta_t^{\text{Work & Income}} + \Delta_t^{\text{Commuting Zone}}
\]

We follow Gelbach (2016) to avoid bias from inferring the shares of \(\beta\) explained from the sequential inclusion of controls.

2.4.2. City-Level Heterogeneity

We use a two-step approach to explore CZ-level factors associated with heterogeneity in the racialized difference in commute times. The first step is to estimate CZ-by-year-bin-specific
models to produce a panel of CZ-specific racialized difference. As these are conditional on observables, we call them estimates of the residual racialized difference (RRD). The second step is to regress the RRD on city-level characteristics:

\[
\ln(\tau_{ict}) = \beta^*_ct \mathbb{I}[\text{Black}_{ict}] + x'_{ict}\mu_{ct} + \lambda_{ct} + \epsilon_{ict} \tag{2.3}
\]

\[
\hat{\beta}^*_ct = z'_{ct}\gamma + D_c + T_t + e_{ct}. \tag{2.4}
\]

The first equation is similar to eq:main except in that we estimate a separate \(\beta^*\) for each CZ and year-bin combination, allowing for local heterogeneity in the role that individual controls play. The second equation lets us study the role of CZ-level factors on the racialized difference in commute times.\(^{14}\) In some specifications, we include CZ and year-bin fixed effects in the second stage to further limit the role of unobserved factors.

Our selection of CZ-level measures to include in the second stage is motivated by the desire to describe how city-level characteristics contribute to the spatial stratification of people into longer commutes. Our hypothesis is that more sorting (larger racialized difference) is more likely to occur in cities that have longer commutes in general but still retain some variation in commute length so observable sorting can take place. Thus, some measures listed in Section 6 attempt to describe the general commuting environment while others are mechanisms that may contribute to longer commutes in general. These variables consider spatial and aspatial aspects of city-level population, employment, and urban form. While many of these variables may be endogenous with respect to the RRD, our intent is generally to document and describe.

2.5. Results

We now estimate the racialized difference in commute times, describe its relation to observable characteristics, and explore its evolution over the last forty years. We refer to observable

\(^{14}\)This two-step approach is equivalent to adding CZ-level controls to Equation 2.2, and so the portion of \(\Delta_{(t)}\) explained by this second step is conceptually a subset of \(\Delta_{(t)}^{\text{Unexplained}}\). See Appendix ?? for discussion.
features like commute mode, residential location, and demographic and job characteristics as channels. They are not controls accounting for alternative, non-racial explanations. Racialization, the process by which social difference is naturalized (Chun and Lo, 2015), permeates the markets and policies underlying all of these determinants of commute time. For example, labor markets feature direct discrimination resulting in lower wages for Black workers (Neumark, 2018). Of course, wage differentials are only partly accounted for by discrimination, with “pre-market” factors like educational attainment accounting for a substantial portion of the remainder (Bayer and Charles, 2018)—but schooling itself remains heavily segregated (Erickson, 2016; Logan and Burdick-Will, 2016). No factor is necessarily upstream of racialization.

2.5.1. The Role of Observable Individual Characteristics

Table 2.1 reports estimates of $\beta_t$ and $\beta_t^*$ that correspond to Equations (2.1) and (2.2), respectively. Column 1 includes only year-bin dummies and provides baseline measures of the racialized difference in commute times, $\Delta_t$. The 1980 difference of 26.3 log points implies a 30.1% longer unconditional average commute for Black commuters than White commuters. The difference declines consistently over the observed time period, falling to 12.4 log points (13.2%) in 2012–19. The majority of this partial convergence occurs before 1990.

Column 2 introduces CZ-by-year-bin fixed effects to assess the role of the differential distribution of the Black and White commuting population across commuting zones with longer (e.g., New York) and shorter (e.g., Salt Lake City) average commutes. In this specification, the estimate of $\beta_t^*$ only compares people living in the same commuting zone at the same time. Accounting for this first-order channel reduces the estimates to 18.0 log points (19.7%) in 1980 and 4.6 log points in 2012–19 (4.7%). Again, the majority of the partial convergence occurs between 1980 and 1990.

The next columns introduce individual, commute, and job-related characteristics. These
Table 2.1: Estimates of the Racialized Difference in Commute Time

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$1^{[Black]} \times t_{1980}$</td>
<td>0.263***</td>
<td>0.180***</td>
<td>0.198***</td>
<td>0.129***</td>
<td>0.139***</td>
<td>0.136***</td>
</tr>
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<td>(0.022)</td>
<td>(0.015)</td>
<td>(0.016)</td>
<td>(0.008)</td>
<td>(0.008)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>$1^{[Black]} \times t_{1990}$</td>
<td>0.191***</td>
<td>0.106***</td>
<td>0.126***</td>
<td>0.062***</td>
<td>0.076***</td>
<td>0.079***</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.020)</td>
<td>(0.022)</td>
<td>(0.009)</td>
<td>(0.010)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>$1^{[Black]} \times t_{2000}$</td>
<td>0.178***</td>
<td>0.091***</td>
<td>0.110***</td>
<td>0.056***</td>
<td>0.071***</td>
<td>0.078***</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.019)</td>
<td>(0.020)</td>
<td>(0.010)</td>
<td>(0.011)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>$1^{[Black]} \times t_{2005–11}$</td>
<td>0.150***</td>
<td>0.069***</td>
<td>0.090***</td>
<td>0.034***</td>
<td>0.051***</td>
<td>0.061***</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.018)</td>
<td>(0.019)</td>
<td>(0.010)</td>
<td>(0.010)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>$1^{[Black]} \times t_{2012–19}$</td>
<td>0.124***</td>
<td>0.046***</td>
<td>0.070***</td>
<td>0.018*</td>
<td>0.037***</td>
<td>0.049***</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.017)</td>
<td>(0.017)</td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.009)</td>
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<table>
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<tr>
<th>Year Bin×CZ FEs</th>
<th>-</th>
<th>Y</th>
<th>Y</th>
<th>Y</th>
<th>Y</th>
<th>Y</th>
</tr>
</thead>
<tbody>
<tr>
<td>Controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Demog. &amp; Edu.</td>
<td>-</td>
<td>-</td>
<td>Y</td>
<td>-</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Trans. Mode</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Work &amp; Income</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>Y</td>
<td>Y</td>
</tr>
</tbody>
</table>

Data: All commuters in the Census (1980, 1990, 2000) and ACS (2005–2019) with race Black alone or in combination or White alone. Each column is for a different specification; in each, the number of observations is 48,767,398. The dependent variable is log travel time top-coded at 99 minutes. Demographics include sex, educational attainment, age, marital and household status, and number of children in household. Work and income controls are log income, and indicator for zero income, and indicators for industry and occupation. Controls are interacted with year bin. Observations weighted by adjusted person sample weights. Standard errors clustered by commuting zone. + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

columns estimate $\beta^*_t$ in Equation (2.2), and the estimated coefficients capture the unexplained racialized difference arising through channels other than those that are observed. Column 3 adds in demographic and education characteristics, Column 4 instead adds transportation mode, and Column 5 adds in both demographic and education characteristics and transportation mode. Column 6 further adds work and income characteristics. Accounting for travel mode substantially reduces the estimate of $\beta^*_t$, suggesting that mode is a central factor in the production of the racialized difference in commuting. All other controls have relatively little effect, or even increase the estimate of $\beta^*_t$.

Figure 2.8 graphically depicts estimates of $\beta_t$ and $\beta^*_t$ before (black line) and after (colored
Figure 2.8: Estimates of the Racialized Difference in Commuting Time

![Figure 2.8: Estimates of the Racialized Difference in Commuting Time](image)

lines) conditioning on the same observable characteristics. Figure 2.8 highlights that the relative ordering of the specifications shown in Figure 2.1 are relatively stable over time, with a notable exception: conditioning on demographic and job characteristics does not alter the estimate of $\beta_t^*$ much in 1980, whereas the same exercise increases $\beta_t^*$ substantially in later years.

Next, we use the decomposition described in Section 2.4 to precisely discuss the relative contribution of the different channels. We first replicate Columns 1 and 6 of Table 2.1 as Columns 1 and 2 of Table 2.2, respectively. These correspond to $\Delta$ and $\Delta^{Unexplained}$. The remaining columns of tab:decomp characterize the contribution of the various groups of characteristics to the explained portion of $\Delta$. Because we follow the partial decomposition method proposed by Gelbach (2016) to avoid sequential bias, the estimates in Columns 2–6 of each row of tab:decomp conveniently sum to the estimate in Column 1. tab:decomp includes a Components of Change calculation that presents the portion of the change in $\Delta$

---

15These results are similar but not identical to tab:main: each year beginning in 2005 is estimated with single-year coefficients instead of multi-year bins.
between 1980 and 2012–19 that is explained by each group of characteristics.

Table 2.2: Decomposing the Racialized Difference in Commute Time Due to Observable Individual Characteristics

<table>
<thead>
<tr>
<th>Decomposition</th>
<th>$\Delta_t$</th>
<th>$\Delta_{t}^{\text{Unexplained}}$</th>
<th>$\Delta_{t}^{\text{Explained}}$</th>
<th>$\Delta_{t}^{\text{Demog.}}$</th>
<th>$\Delta_{t}^{\text{Tr. Mode}}$</th>
<th>$\Delta_{t}^{\text{Work/Inc.}}$</th>
<th>$\Delta_{t}^{\text{CZ}}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>1[Black] $\times t_{1980}$</td>
<td>0.263***</td>
<td>0.136***</td>
<td>-0.008***</td>
<td>0.073***</td>
<td>-0.001</td>
<td>0.062***</td>
<td>(0.022)</td>
</tr>
<tr>
<td>1[Black] $\times t_{1990}$</td>
<td>0.191***</td>
<td>0.079***</td>
<td>-0.009***</td>
<td>0.063***</td>
<td>-0.007***</td>
<td>0.065***</td>
<td>(0.029)</td>
</tr>
<tr>
<td>1[Black] $\times t_{2000}$</td>
<td>0.178***</td>
<td>0.078***</td>
<td>-0.008***</td>
<td>0.050***</td>
<td>-0.011***</td>
<td>0.069***</td>
<td>(0.027)</td>
</tr>
<tr>
<td>1[Black] $\times t_{2005-11}$</td>
<td>0.150***</td>
<td>0.061***</td>
<td>-0.009***</td>
<td>0.049***</td>
<td>-0.014***</td>
<td>0.063***</td>
<td>(0.027)</td>
</tr>
<tr>
<td>1[Black] $\times t_{2012-19}$</td>
<td>0.124***</td>
<td>0.049***</td>
<td>-0.008***</td>
<td>0.040***</td>
<td>-0.019***</td>
<td>0.063***</td>
<td>(0.025)</td>
</tr>
</tbody>
</table>

Components of Change

\[
\frac{\Delta_{t}^{k} - \Delta_{t}^{K} - 19}{\Delta_{t}^{k} - \Delta_{t}^{K} - 19} \times 100\%
\]

| Computed as (\Delta_{t}^{k} - \Delta_{t}^{K})/((\Delta_{t}^{k} - \Delta_{t}^{K}) - 19) | - | 62.6% | 0.0% | 23.7% | 12.9% | -0.7% |

Data: All commuters in the Census (1980, 1990, 2000) and ACS (2005–2019) with race Black alone or in combination or White alone. The number of observations is 48,767,398. Column 1 is the unconditional racialized difference in commute time. Columns 2–6 report the contribution of a group of variables to the level and the share of $\Delta_t$. The specification corresponds to Column 6 of tab:main. Demographics include sex, educational attainment, age, marital and household status, and number of children in household. Work and income controls are log income, and indicator for zero income, and indicators for industry and occupation. Standard errors clustered by commuting zone. Components of change are calculated as $(\Delta_{t}^{k} - \Delta_{t}^{K})/((\Delta_{t}^{k} - \Delta_{t}^{K}) - 19)$ for each group of variables $k$. + $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Transportation mode plays an important role in accounting for the racialized difference in every year, and is the largest observed factor in its decline over time. It accounts for about 28% of the racialized difference in 1980 and about 33% in 2018, though in levels $\Delta_{t}^{\text{Tr. Mode}}$ falls by nearly half. Figures 2.4, 2.5, and 2.6 indicate substantial but incomplete convergence in the modes used by Black and White commuters. Despite its central role
among the observable characteristics, the partial convergence in mode explains only about one quarter (24%) of the overall decline in the racialized difference in commute time.

A disproportionate share of Black workers continue to live in commuting zones with relatively long commutes, and this factor explains a substantial share of the overall difference in commuting times. But there is essentially no convergence on this front: CZ of residence does not explain any of the decline in racialized difference in commuting since 1980. As the unconditional racialized difference fell, the measured contribution of CZ of residence increased from 24% in 1980 to 50% in 2012–19.

Job-related factors (including income) do not matter very much in 1980 but are an increasingly important factor over time, accounting for -15% of the difference in unconditional commute times by 2012–19. As shown in Figure 2.8, the negative sign means that differences in income and work characteristics increase the estimate of $\beta^*_t$ differences in commute time. Of the variables that drive $\Delta_{t}^{\text{Work/Inc.}}$, the contribution of log-income declines in magnitude from -0.009 in 1980 to -0.003 in 2012–19. In contrast, occupation accounts for 0.012 in 1980, but only -0.005 by 2012–19. Altogether, Black commuters today hold jobs and earn incomes that are associated with relatively short commutes. Divergence in job-related factors has supported the partial convergence in commute times: changes in work and income covariates explain about 13% of the decline in $\Delta$ since 1980. Lastly, other observable demographic characteristics like age and education account for very little of the difference in each year-bin, and none of the decline over time.

Unobserved factors account for 39%–52% of the racialized difference in each year, and changes to these factors account for the majority (nearly 63%) of its decline since 1980. While we later investigate the role of urban spatial processes like residential segregation in accounting for the decline of $\Delta_{t}^{\text{Unexplained}}$, we first examine aggregate patterns of heterogeneity in the racialized difference by income and mode. These ensure that our results are not clouded by assumptions of linearity or averaging across heterogeneous experiences, and are also of interest in their own right.
2.5.2. Heterogeneity in Racialized Difference by Income

While we included income as a control in the preceding results, the production of the racialized difference may vary across income levels. To study this heterogeneity, Figure 2.9 plots estimates of $[\text{Black}]$ interacted with twenty equally sized bins along the income distribution. Across income groups, Black commuters face substantially longer commutes. The black lines represent 1980, and the blue lines 2012–2019. Solid lines include just commuting zone fixed effects (like Column 2 of Table 2.1). Dotted lines also include individual, transportation mode, and job-related characteristics (like Column 6 of Table 2.1).

The difference is widest at the lower end of the income distribution; it is unconditionally nearly 36 log points (43%) at the 10th income percentile in 1980. Roughly one third of this difference is generated through channels captured by observable characteristics—accounting for these, the difference is 23 log points (26%) at the 10th income percentile in 1980. Workers in this income range likely face greater challenges in covering the expense of a car, potentially accounting for the relatively large role that observable characteristics
play among low-income Black workers. Both the conditional and unconditional estimates of the racialized difference decline slowly across the middle part of the income distribution. At high incomes (above the 90th percentile), the racialized difference in 1980 is still present, but is typically less than 10 log points.

This pattern persists in 2012–2019, although overall levels are lower. The difference is unconditionally about 17 log points (19%) at the 10th income percentile and 10 log points (11%) conditional on observables, substantially reduced from 1980—again, in line with the the convergence in car-commuting rates and the role of mode in overall convergence. The difference declines by about half up to the middle of the income distribution, where it then levels out before increasing slightly at the top of the income distribution.

While income plays a role in shaping commuting possibilities, our finding of a large racialized difference in commute times cannot be fully explained by the racialized differences in income.\textsuperscript{16} The relationship between income and commute time is potentially complex: “short commutes” may be a normal good, and higher wages may incentivize workers to pursue short commutes. Indeed, estimates of the value of time suggests that it is increasing, creating more incentive for sorting into short-commute locations (Su, 2019). On the other hand, long commutes may come bundled with attractive amenities that the rich value more than a short commute. In line with this latter possibility, we find a positive correlation between income and commute time in our data. In our estimates of eq:main, the coefficient on income varies between 0.051–0.058.\textsuperscript{17} The differential findings here—White workers have relatively short commutes, but richer workers have relatively long commutes—highlight the importance of investigating racialization per se.

2.5.3. Differences by Mode

Mode is a central determinant of commute times. As shown in Table 2.2, mode explains 28%–33% of the unconditional racialized difference in commute times, 24% of its decline

\textsuperscript{16}Lacking data, we cannot investigate the role of wealth, itself a site of even greater racialized difference between Black and White individuals (Kuhn et al., 2020).

\textsuperscript{17}In specifications with CZ of residence but no other controls, the coefficient is even larger: 0.105–0.128.
from 1980–2019, and as much 66% of the difference conditional on CZ. In this section, we estimate mode-specific models of the racialized difference in commute times to investigate heterogeneity in the roles of observable characteristics across mode. This approach implicitly allows mode-specific coefficient estimates, reducing the concern that, e.g., differences in mode-specific fixed effects between cities as different as New York City and Houston are confounding the aggregate difference.

Figure 2.10 shows the racialized difference for commuters using private automobiles (inclusive of carpooling), motorcycles, or taxis. Given the high share of commuters that use automobiles, this figure is broadly similar to Figure 2.8. Controlling for just CZ and year bin, the difference declines from 13 log points in 1980 to zero by 2019. However, once demographics and job characteristics are included, a positive and significant difference is once again present in recent years. This suggests patterns in residential and workplace locations lead to longer commutes for Black workers with similar observable characteristics and income as White workers, even when all drive to work.

The difference for Black and White bus commuters, however, barely declines between 1980 and 2019. Figure 2.11 shows that the racialized difference in bus commute times falls somewhat between 1980 and 1990, but then increases substantially from 1990 to 2013 before decreasing again through 2019. In addition to differential patterns in residential and workplace location, this may reflect a decline in quality of bus service for Black commuters relative to White commuters (McKenzie, 2013). Given the large declines in bus share among Black commuters (and smaller declines among White commuters) shown in Figure 2.5, the difference may also indicate poorer service to increasingly marginalized commuters.

Among subway commuters, the racialized difference in commute times increases substantially during earlier decades. Figure 2.12 reveals a clear divergence in subway commute times through 2006, with less patterned movements since then. As very few cities have subways, the role of CZ of residence is greatly diminished in these regressions: all CZs with subways have long average commutes. Unlike for the other modes or the aggregate
Figure 2.10: Racialized Difference Conditional on Mode = Car

Figure 2.11: Racialized Difference Conditional on Mode = Bus & Streetcar
results, controlling for demographic and job characteristics actually decreases the difference in commute time for subway riders.

Conditioning on subway ridership restricts the sample to CZs with subway systems, and to residence/workplace pairs with subway stations. These CZs are largely expensive coastal cities, and the neighborhoods and workplaces served are largely central; about 44% of subway commuters today live in the five boroughs of New York City and commute to Manhattan.\textsuperscript{18} Neighborhoods with subway access have a distinct racial and class geography that have been heavily shaped by gentrification. In New York, the Black communities of Bed-Stuy and Harlem have experienced gentrification, pushing many Black residents to more-distant neighborhoods. Gentrification may account for both distinctive features of subway commuters: first, the Black commuters displaced are those with lower incomes, while the White residents of the most central areas have high incomes and short commutes. Second, gentrification during our sample period may help account for the growth of the racialized difference among subway commuters. Conditioning on subway commuters brings these within-CZ spatial factors to the fore, and we explore them more directly in the next two sections.

2.5.4. Finer Controls for Residential Geography

We now turn to the role of within-CZ urban spatial processes. Census microdata limit the residential geographic resolution available. Nevertheless, we provide two additional exercises to determine whether residential location explains differences in commute times by race. First, we build on the above individual-level models but use somewhat finer geographic areas than CZs, and second, we use tract-level regressions to investigate even finer spatial variation (at the cost of individual-level data). These geographic controls capture part of the unexplained variation under some conditions, but data limitations prevent us from drawing strong conclusions about the role of urban spatial processes.

Starting in 2000, the Census provides PUMAs that are of a fine enough spatial scale to approximate subregions of urban commuting zones. Incorporating PUMA fixed effects controls for meso-scale regional differences and sorting within larger CZs.\(^\text{19}\) Because PUMA vintages prior to 2000 contain much less geographic resolution, we do not report results that include them. By construction, PUMAs contain at least 100,000 residents. The ability of PUMAs to differentiate sub-CZ spatial patterns thus depends on CZ size. In smaller CZs, PUMAs may be able to broadly distinguish central cities from surrounding areas, or distinguish between polycentric towns. By contrast, PUMAs in the largest CZs contain dozens of PUMAs that can capture relatively nuanced distinctions within cities, as well as across suburbs.

Table 2.3 reports aggregate and mode-specific measures of \(\beta_i^*\) from eq:main. Panel A excludes the PUMA fixed effects, while Panel B includes them. All the specifications condition on observable demographic and job characteristics (and transportation mode in Column 1), and so are similar to Column 6 in Table 2.1. Column 1 of Table 2.3 shows a clear downward trend in \(\beta_i^*\) over time, both with and without PUMA fixed effects. The similarity between the estimates of \(\beta_i^*\) across the panels indicates that accounting for residential geography at the PUMA level does not explain the racialized difference in commute times over the period 2000–2019 among commuters as a whole. To the extent that PUMAs capture internal urban spatial processes, like the movement of many Black households to suburbs over the last forty years (Bartik and Mast, 2021; Wiese, 2005), our results suggest that these processes are not a primary driver of the decline in \(\beta_i^*\).

Columns 2–5 of Table 2.3 repeat this exercise but condition the sample by mode (as in Section 2.5.3). The results for car commuters in Column 2 are qualitatively and quantitatively similar to the overall results: the estimate of \(\beta_i^*\) trends down over time, and controlling for PUMA of residence does not make a notable difference.

\(^{19}\)As an example, Los Angeles County contains over half the population of its CZ and features 60–70 PUMAs during the period 2000–2019. We do not geo-normalize PUMAs across years.
### Table 2.3: Racialized Difference in Commute Time by Mode and with Residential PUMA Controls

<table>
<thead>
<tr>
<th></th>
<th>All Modes</th>
<th>Car</th>
<th>Bus</th>
<th>Subway</th>
<th>Walk</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>A. Year-Specific Estimates</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$1[Black] \times t_{1980}$</td>
<td>0.136***</td>
<td>0.134***</td>
<td>0.089***</td>
<td>0.036***</td>
<td>0.299***</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.012)</td>
<td>(0.016)</td>
<td>(0.011)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>$1[Black] \times t_{1990}$</td>
<td>0.079***</td>
<td>0.069***</td>
<td>0.061***</td>
<td>0.040***</td>
<td>0.279***</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.012)</td>
<td>(0.015)</td>
<td>(0.011)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>$1[Black] \times t_{2000}$</td>
<td>0.078***</td>
<td>0.066***</td>
<td>0.085***</td>
<td>0.091***</td>
<td>0.291***</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.011)</td>
<td>(0.018)</td>
<td>(0.018)</td>
<td>(0.022)</td>
</tr>
<tr>
<td>$1[Black] \times t_{2005-11}$</td>
<td>0.061***</td>
<td>0.047***</td>
<td>0.102***</td>
<td>0.114***</td>
<td>0.208***</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.009)</td>
<td>(0.016)</td>
<td>(0.019)</td>
<td>(0.023)</td>
</tr>
<tr>
<td>$1[Black] \times t_{2012-19}$</td>
<td>0.049***</td>
<td>0.035***</td>
<td>0.104***</td>
<td>0.102***</td>
<td>0.172***</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.008)</td>
<td>(0.016)</td>
<td>(0.019)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>$N$</td>
<td>48,767,398</td>
<td>45,071,097</td>
<td>770,058</td>
<td>397,298</td>
<td>1,743,047</td>
</tr>
</tbody>
</table>

B. Year-Specific Estimates, with year-bin $\times$ PUMA FEs (2000 and later only)

<table>
<thead>
<tr>
<th></th>
<th>All Modes</th>
<th>Car</th>
<th>Bus</th>
<th>Subway</th>
<th>Walk</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>$1[Black] \times t_{2000}$</td>
<td>0.076***</td>
<td>0.069***</td>
<td>0.069***</td>
<td>0.022***</td>
<td>0.255***</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.006)</td>
<td>(0.012)</td>
<td>(0.007)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>$1[Black] \times t_{2005-11}$</td>
<td>0.060***</td>
<td>0.053***</td>
<td>0.079***</td>
<td>0.036***</td>
<td>0.196***</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.006)</td>
<td>(0.007)</td>
<td>(0.010)</td>
<td>(0.013)</td>
</tr>
<tr>
<td>$1[Black] \times t_{2012-19}$</td>
<td>0.043***</td>
<td>0.034***</td>
<td>0.071***</td>
<td>0.033***</td>
<td>0.153***</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.008)</td>
<td>(0.009)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>$N$</td>
<td>37,362,675</td>
<td>34,765,319</td>
<td>528,659</td>
<td>302,729</td>
<td>1,161,492</td>
</tr>
</tbody>
</table>

Data: All commuters in the Census (1980, 1990, 2000) and ACS (2005–2019) with race Black alone or in combination or White alone. Columns 2–5 further restrict the sample based on commute mode. Each column in each panel is for a different specification. The dependent variable is log travel time top-coded at 99 minutes. Each column includes demographic controls and work and income controls interacted with year bin, as well as commuting-zone-by-year-bin fixed effects. Column 1 of both panels includes transit mode controls (Panel A Column 1 replicates Column 6 of tab:main). Panel B includes residential-PUMA-by-year-bin fixed effects and so only uses data from 2000 and later because pre-2000 PUMAs are too geographically coarse. Observations weighted by adjusted person sample weights. Standard errors clustered by commuting zone. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Column 3 in Panel A again shows a slight increase in the racialized difference for bus commuters, as seen in Figure 2.11; it is at 10.4 log points (10.9%) as of 2012–19. Controlling for PUMA of residence leads to a moderate decrease in the estimate of $\beta_t^*$ in Panel B; it is at 7.1 log points (7.4%). This suggests that geography plays more of a role in determining differential commute times by race for bus commuters than for automobile commuters.

Subway (and elevated rail) commuters are in Column 4. Here we see a sizeable increase in the estimate of $\beta_t^*$ over time, from 3.6 log points in 1980 to 10.2 log points in 2012–19.
Among subway commuters, Panel B reveals that residential geography plays a very substantial role in determining the difference. Controlling for PUMA of residence, the difference is a positive but small 3.3 log points in 2012–19. For subway (and elevated) commuters, residential geography is central to the racialized difference in commute times. As described above, restricting our sample to subway commuters also restricts our sample to very large cities where PUMAs are small enough (relative to the city) to capture meaningful spatial variation.

Finally, Column 5 examines the subset of commuters who use the second most popular mode: walking. This mode shows a large estimate of $\beta_t^*$ that declines somewhat over the last forty years, but remains sizeable relative to other modes. The difference of 29.9 log points (34.8%) in 1980 fell to a still large 17.2 log points (18.8%) by 2012–19. Controlling for PUMA of residence lowers the point estimate of $\beta_t^*$ by a sizeable (1.2–3.6 log points) amount. Nevertheless, the racialized difference remains substantial: 15.3 log points (16.5%) in 2012–19.

As noted above, PUMAs are a more meaningful geographic control in large cities: Manhattan alone has more than 10 PUMAs, while small CZs may have a single PUMA for the entire center city and inner suburbs. The large role played by PUMAs among subway commuters might not be due to subways per se; instead it might follow from restricting the sample to large cities. To investigate this possibility, we also estimate the models shown in Table 2.3 on three major subsets of cities: big transit CZs, big non-transit CZs, and all other CZs. Results, shown in tab:modespumascities, suggest an important (but not determinative) role for spatial processes within CZs.

Across all years, baseline estimates of $\beta_t^*$ are largest in big transit CZs, followed by big non-transit CZs; this also holds among car commuters. For other CZs and in recent years,

---

20 Big transit CZs are those with sizeable heavy rail ridership: New York City, Boston, Chicago, Philadelphia, Washington, D.C., San Francisco, Atlanta, and Los Angeles. These cities contain about 95% of all subway and elevated commuters observations in our data. Big non-transit CZs are Dallas-Fort Worth, Houston, Miami, Phoenix, Seattle, Detroit, San Diego, and Minneapolis-St. Paul.

21 We do not condition on subway and elevated ridership in big non-transit and other CZs.
the estimates of $\beta_t^*$ are very small, and nearly zero in some specifications: the racialized difference in commute times within these CZs is mediated nearly entirely through observable channels. Indeed, PUMA of residence tends to raise the estimates of $\beta_t^*$—although, as noted, PUMAs in smaller CZs may be less spatially informative.

Within large cities, PUMAs capture meaningful variation in the racialized difference in commute times. Furthermore, conditioning on PUMA of residence appears to matter particularly in large transit CZs, relative to other large CZs. Among car commuters, PUMAs lower the estimates of $\beta_t^*$ by 2.7–4.0 log points in big transit CZs, but negligibly (or not at all) in non-transit large CZs and other CZs. Among bus commuters in large transit and non-transit CZs, our estimates of $\beta_t^*$ are relatively similar and relatively stable, while the corresponding estimates in other CZs trended upward to converge with the estimates for large CZs. PUMAs lower the estimates of $\beta_t^*$ by a similar amount (2–4 log points) among large CZs as well as for other CZs in recent years. For walkers, the residual difference is smaller in big transit CZs and biggest in non-transit and especially other CZs, perhaps reflecting selection into walking due to limited or unreliable transit options for non-car commuters in these cities.

Lastly, we analyze census tract-level average commute times and Black residential population shares to investigate whether finer-scale residential location explains differences in commute times. This analysis, which we detail in Appendix A.6, is not directly comparable to the other results presented in this section. However, it allows for tract-level fixed effects, which flexibly control for time-invariant tract-level factors that might drive commuting differences. Results are shown in Table A.7.3. Unconditional results accord closely with coefficients in Column 1 of Table 2.1, providing assurance that tract-level Black population share is a reasonable proxy for individual race in this setting. Models that include tract fixed effects and control for transit share show a significant racialized difference between 4.3–8.7 log points; these results do not exhibit a clear trend over time. While a bit smaller than $\Delta^{\text{Explained}}$ in Table 2.2, this measure’s significance suggests that residential location, at
least as measured by census tracts, cannot entirely explain differences in commuting time.

We take several insights from these results, in particular relating to the persistence of the racialized difference in commute times. First, the relatively slow rate of conditional convergence in big transit CZs suggests that factors distinct to large, transit-dependent cities may play a role in the overall persistence. This insight is reinforced by the near-complete conditional convergence among car commuters in our other-CZ subsample of smaller cities. Second, the relatively large effect of PUMA of residence on conditional convergence in big transit CZs suggests a potential role for internal spatial processes in this persistence. For example, the conditional divergence among subway riders since 1980 is nearly entirely accounted for by PUMA of residence. PUMA of residence remains a relatively coarse measure of residential location, and does not speak to changing geographies of employment (see, e.g., Miller, 2018, for how the suburbanization of jobs disproportionately impact Black employment). Nor do these approaches account for investments in transportation infrastructure that would mediate any relationship between segregation and racialized difference in commuting outcomes. We tackle these questions next.

2.6. City-Level Heterogeneity and Spatial Stratification

Convergence in the share of Black and White commuters driving to work accounts for the majority of the explained convergence in commute times. But in some cities—large, segregated, and congested or transit-dependent—a car may be insufficient to ensure a fast commute. Further, high land costs may price a car (and parking) beyond the reach of many households; these attributes may be a barrier to a car-based convergence. In this section, we explore CZ-level variation in the constellation of attributes that we term spatial stratification, and evaluate whether this process may help account for patterns of persistence and decline in the racialized difference in commute times.

We understand spatial stratification as the organization of a city whereby segregated Black neighborhoods feature higher travel costs to major job sites than do segregated White
neighborhoods. This equilibrium outcome reflects the confluence of several ingredients, including residential segregation, employment sites that are closer to segregated White neighborhoods, and long commutes or slow travel speeds. Residential segregation is a facet of essentially all U.S. cities to varying degrees, although levels of segregation have declined in some places since 1980. The co-location of employers and White neighborhoods has been a concern of urban economists at least since Kain (1968), and Miller (2018) documents its continued persistence. However, in small cities—or “fast cities”, whereby long distances can be traversed quickly, e.g., due to freeway investment—patterns of segregation and unequal job access may be overcome.

We relate measures of the ingredients of spatial stratification to the racialized difference in commute times by CZ. Using CZ-by-year-bin models that condition on the same observable controls used in Column 6 in Table 2.1, we estimate CZ-specific measures of the residual racialized difference; this is $\beta^*_ct$ in Equation 2.3. For brevity, we refer to this measure as ‘RRD’. Because the RRD values are estimates, we exclude commuting zones with small numbers of total workers and small numbers of Black commuters to limit noise.\(^{22}\) We weight all statistics and models by the number of Black commuters in that CZ and year bin to account for heteroskedasticity.

We next examine patterns of persistence [carto]graphically. Cross-city persistence in the RRD is especially visible in large, coastal cities—precisely those where the ingredients for spatial stratification are likely to be prominent. We identify and construct several panel measures of these factors to test the hypothesis. Lastly, several of the ingredients to spatial stratification—for example, a large city suffering from congestion, with unequal access to jobs—are also features that may reflect (or induce) high average house prices. Using house prices as a potential indicator of spatial stratification, we test the hypothesis that within-city stratification plays a key role in the evolution and persistence of the RRD.

\(^{22}\)Specifically, we only consider commuting zones that satisfy two criteria in all five of the year bins: (i) Census data indicate there are at least 1,000 total employed persons, and (ii) there are greater than 50 unique Black commuter respondents.
Table 2.4: Summary Statistics of the Residual Racialized Difference (RRD) and CZ Characteristics

<table>
<thead>
<tr>
<th>Characteristics</th>
<th>Years</th>
<th>N</th>
<th>Mean</th>
<th>SD</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>Residual Racialized Difference (RRD)</td>
<td>1980</td>
<td>341</td>
<td>0.131</td>
<td>0.072</td>
<td>-0.339</td>
<td>0.485</td>
</tr>
<tr>
<td></td>
<td>1990</td>
<td>341</td>
<td>0.070</td>
<td>0.072</td>
<td>-0.326</td>
<td>0.246</td>
</tr>
<tr>
<td></td>
<td>2000</td>
<td>341</td>
<td>0.068</td>
<td>0.077</td>
<td>-0.412</td>
<td>0.247</td>
</tr>
<tr>
<td></td>
<td>2005-11</td>
<td>341</td>
<td>0.053</td>
<td>0.073</td>
<td>-0.384</td>
<td>0.220</td>
</tr>
<tr>
<td></td>
<td>2012-19</td>
<td>341</td>
<td>0.032</td>
<td>0.070</td>
<td>-0.257</td>
<td>0.230</td>
</tr>
<tr>
<td>Employed Population All</td>
<td>All</td>
<td>1705</td>
<td>1,581.252</td>
<td>2,258,153</td>
<td>3,420</td>
<td>8,511,690</td>
</tr>
<tr>
<td>Black Share of Employed Population</td>
<td>All</td>
<td>1705</td>
<td>0.204</td>
<td>0.110</td>
<td>0.004</td>
<td>0.598</td>
</tr>
<tr>
<td>Dissimilarity Index</td>
<td>All</td>
<td>1684</td>
<td>0.568</td>
<td>0.148</td>
<td>0.000</td>
<td>0.908</td>
</tr>
<tr>
<td>Employment Concentration (Black)</td>
<td>1990–2019</td>
<td>1363</td>
<td>0.572</td>
<td>0.178</td>
<td>0.041</td>
<td>0.996</td>
</tr>
<tr>
<td>Employment Concentration (White)</td>
<td>1990–2019</td>
<td>1363</td>
<td>0.452</td>
<td>0.085</td>
<td>0.071</td>
<td>0.658</td>
</tr>
<tr>
<td>Centrality All</td>
<td>All</td>
<td>1685</td>
<td>-0.022</td>
<td>0.075</td>
<td>-0.255</td>
<td>0.862</td>
</tr>
<tr>
<td>Miles of Highway</td>
<td>1980–2000</td>
<td>786</td>
<td>241</td>
<td>208</td>
<td>0</td>
<td>999</td>
</tr>
<tr>
<td>Transit Mode Share</td>
<td>All</td>
<td>1705</td>
<td>0.051</td>
<td>0.083</td>
<td>0.000</td>
<td>0.342</td>
</tr>
<tr>
<td>Average Travel Time (Auto)</td>
<td>All</td>
<td>1705</td>
<td>24.466</td>
<td>3.325</td>
<td>10.653</td>
<td>35.944</td>
</tr>
<tr>
<td>Average House Price</td>
<td>All</td>
<td>1705</td>
<td>223,698</td>
<td>122,847</td>
<td>74,165</td>
<td>842,038</td>
</tr>
<tr>
<td>Corr(Commute Time, House Price)</td>
<td>All</td>
<td>1684</td>
<td>-0.118</td>
<td>0.217</td>
<td>-1.000</td>
<td>1.000</td>
</tr>
</tbody>
</table>

Estimates of the Residual Racialized Difference (RRD) in commute time and CZ-level summary statistics. RRD values are estimated for each CZ in each year bin as explained in sec:methods. RRDs are only reported for CZs with at least 1,000 total employed persons and with greater than 50 unique Black commuter Census respondents in all five year bins. Summary statistics pool data from all available years. Observations weighted by the number of Black commuters in each CZ-by-year-bin cell.

2.6.1. Patterns of Persistence

Table 2.4 reports summary statistics by year bin of the RRD across CZs. The weighted mean difference in 1980 is 13.1 log points, which falls to 3.6 log points by 2012–19. Minimum and especially maximum values both narrow. Despite this, however, the dispersion of the RRD does not decrease notably.\textsuperscript{23}

Figures 2.13–2.16 show the spatial distribution of the RRD in 1980, 2000, and 2012–19. Red indicates a positive RRD, white corresponds to zero, and blue indicates a negative RRD. The bottom right panel presents the change in RRD from 1980 to 2012–19 using the same scale. The RRD is positive and pervasive across most of the nation in 1980. Throughout much of the Northeast and the South, as well as in the West, most places show large and

\textsuperscript{23}Mean RRD values in Table 2.4 are similar to $\Delta^{\text{unexplained}}$ estimated with heterogeneous effects of characteristics by CZ (see Appendix for details), but differ because they refer to a restricted set of CZs and weight by Black commuting population instead of total commuting population. $\Delta^{\text{unexplained}}$ estimated with heterogeneous effects of characteristics by CZ is 0.105 in 1980 and 0.038 in 2012–19.
positive RRDs. Within the South, the Black Belt of counties with larger Black populations appears to have elevated RRDs, and rural counties elsewhere—as well as Chicago—are positive outliers. Only a few predominately rural areas concentrated in the Midwest and non-coastal Upper South experience a negative difference. In 2000, the previously positive RRD in parts of the South and Midwest begin to fade, a trend that continues through 2012–19. Large cities generally see smaller changes. Positive RRD remains visible in major cities across regions, with the Northeast Corridor and West Coast showing particularly elevated levels.

The cities with notable persistence are suggestive of the same factors identified above as inputs to spatial stratification: large cities with many neighborhoods far from job centers. The largest U.S. cities are all notable for their visible persistence. The autocorrelation in CZ-level RRD between 1980 and 2012–19 is relatively high, at 0.57 (see Figure A.7.2). However, this is driven primarily by CZs with larger populations. The magnitude of correlation between the RRD and population nearly doubles over the same period, and the variation in the RRD explained by population alone more than triples from 17% to 59% (see Table A.7.2). In contrast, the correlation of CZ-level Black share of the commuting population drops substantially over the same period. However, panel models that rely only on within-CZ changes see smaller and insignificant coefficients on population and Black population share. These findings together suggest an increasing role for features that vary strongly with city size in determining the RRD. However, because the within-city effect of population growth over time is only weakly associated with larger RRD, the mechanism is likely related to large cities but is not necessarily driven by changes in city population per se.

We relate these patterns of persistence to several potential ingredients of spatial stratification in Appendix Figures A.7.3–A.7.12, which plot CZ-level RRD for two year-bins (usually 1980 and 2012–2019, depending on data availability) against measures of these ingredients. Among other findings, Figure A.7.5 shows that in 1980, residential segregation across cities was relatively uncorrelated with the RRD. By contrast, a clear positive relationship de-
veloped in more recent years among larger CZs. Figure A.7.7 shows a similar evolution between the RRD and our measure of job access for Black households: CZs with a larger GINI index—signifying little overlap between employment centers and the neighborhoods that Black workers reside—have larger values of the RRD. In the next section, we further explore the links between spatial stratification and the generation of persistent racialized difference in commuting.

2.6.2. Ingredients of Spatial Stratification

We develop measures of the ingredients of spatial stratification, including population characteristics and urban form, and investigate how these relate to the RRD across large CZs and over time. Our measures are guided by the following intuition: the racialized difference in commute time must reflect a difference in residential location, in workplace location, or in the mode and speed of travel between them. The spatial extent of larger cities implies the potential for longer-distance (and more-variable-distance) commutes, a feature amplified by the slower speeds of bigger cities (Couture et al., 2018). But even in big cities, some locations are close to workplaces. Our measures of spatial stratification aim to test whether more-stratified places, or places with slower speeds, can account for the correlation between city size and persistent RRD.

While there are many candidate measures of urban form, we focus on a few observable (and constructable) time-varying measures that reflect combinations of residential location, workplace, or travel speed by race. We present this generally as suggestive (and not causal) evidence of mechanisms that point towards future avenues of research.24

Table 2.5 presents panel estimates of regression correlates of the city-level residual difference. We consider CZs with populations over 200,000. Because many measures are highly dependent on city size, we provide unconditional estimates (Panel A) and estimates in which we control for log population (Panel B); results are largely consistent across panels.

24Our approach is not exhaustive: There may be other factors that play a role in the large (but incomplete) decline of the RRD, such as transit provision, that we do not investigate.
Figure 2.12: Racialized Difference Conditional on Mode = Subway & Elevated

Figure 2.13: Map of Residual Racialized Difference in 1980 by CZ
Figure 2.14: Map of Residual Racialized Difference in 2000 by CZ

Figure 2.15: Map of Residual Racialized Difference in 2012-19 by CZ
Estimates include CZ fixed effects, which control for the average level of the measure as well as for time-invariant features of the CZ, and year-bin fixed effects, which remove aggregate average changes in the measure. These estimates therefore reflect the correlation between the changes of the measure and changes in the RRD. Column labels indicate explanatory variables; in all cases the dependent variable is the RRD.

Columns 1–3 suggest that the spatial patterning of Black and White commuters plays a role in persistent RRDs, and perhaps in its decline. First, we investigate segregation using a dissimilarity index; higher values indicate higher levels of segregation. Cities with declining levels of segregation tend to also see faster declines in the RRD (Column 1), hence the persistence of segregation can help account for the persistence in the RRD.\footnote{Similar results using the Hutchens’ Square Root Index are shown in the Appendix. The Appendix also includes details on variable construction as well as a discussion of the shortcomings of aspatial measures of segregation.} Next, we use an aspatial measure of job access for Black and White commuters to investigate whether differential changes to job access across cities can account for the persistence in the RRD. We calculate a jobs-to-people balance measure using Lorenz Curves that is akin to the measure proposed by Bento et al. (2005), but we calculate it separately for Black and White workers. A larger value implies that jobs and residences are more unequally distributed across zip
Table 2.5: Two-Way Fixed Effects Estimates of CZ-Level Correlates of the RRD

<table>
<thead>
<tr>
<th></th>
<th>Dis-</th>
<th>Black</th>
<th>White</th>
<th>Central-</th>
<th>Log</th>
<th>Transit</th>
<th>Ave.</th>
</tr>
</thead>
<tbody>
<tr>
<td>simi-</td>
<td>Conc.</td>
<td>Conc.</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>larity</td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
</tr>
<tr>
<td>Panel A. No Controls</td>
<td>Measure</td>
<td>0.2448*</td>
<td>0.2379**</td>
<td>-0.2927+</td>
<td>0.0098</td>
<td>-0.0791**</td>
<td>0.4587**</td>
</tr>
<tr>
<td></td>
<td>(0.1160)</td>
<td>(0.0707)</td>
<td>(0.1692)</td>
<td>(0.0801)</td>
<td>(0.0285)</td>
<td>(0.1716)</td>
<td>(0.0032)</td>
</tr>
<tr>
<td>Panel B. Controlling for Log Population</td>
<td>Measure</td>
<td>0.2863*</td>
<td>0.2282**</td>
<td>-0.2392</td>
<td>0.0404</td>
<td>-0.0710**</td>
<td>0.4604**</td>
</tr>
<tr>
<td></td>
<td>(0.1147)</td>
<td>(0.0731)</td>
<td>(0.1559)</td>
<td>(0.0696)</td>
<td>(0.0245)</td>
<td>(0.1570)</td>
<td>(0.0033)</td>
</tr>
</tbody>
</table>

Data: Estimated RRDs and CZ-level characteristics for CZs with at least 1,000 total employed persons, greater than 50 unique Black commuter Census respondents, and at least 200,000 total commuters in all five year bins. Each column in each panel is for a different specification. The dependent variable in each specification is the estimated RRD for each CZ-by-year-bin cell. The column title indicates the which CZ-level characteristics (“Measure”) is being used as the independent (right-hand-side) variable. All models include two-way fixed effects by CZ and year bin. Panel B further includes log commuting population as a control. Models are weighted by the Black commuting population in the CZ-by-year-bin cell. Standard errors clustered by commuting zone. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Column 2 shows that cities where Black workers increasingly tend to reside in zip codes with relatively few jobs also exhibit increases in the RRD, while Column 3 shows the reverse for White workers.

Together, these results suggest that segregation and job access play a role in the persistently high RRDs in large cities today. Increasingly limited access to centers of employment for Black workers may represent city-level patterns of job suburbanization over time. While our data are not suited to directly measure job suburbanization, Miller (2018) finds that Black workers are less likely than White workers to work in suburbs and that job suburbanization led to declines in Black employment rates between 1970 and 2000 period. For those workers that remain in the labor market, efforts to relocate to job-proximate suburban locations may be met by significant barriers associated with discrimination in housing and mortgage

---

\(^{26}\)For more details, see the Appendix. We omit 1980 from the analysis as zip code unemployment data is not available for 1980.
Column 4 presents a spatial phenomenon that could have mattered, but seems not to: the centrality of the residential population within the CZ.\textsuperscript{27} Cities that become more decentralized over time—more sprawling—may be locations of increasing commuting times in general. Conversely, increasing centralization may indicate increasing levels of congestion and slower travel speeds. Coefficients in Column 4 are positive, indicating increasing centrality is associated with increases in the RRD; however, they are highly insignificant.\textsuperscript{28} Centralized cities (like Chicago) and decentralized cities (like Dallas) are both capable of producing large RRDs.

Columns 5–7 suggest that slower cities have larger RRDs. All else equal, cities with features consistent with slower travel speeds (i.e., those with fewer freeway miles or more transit commuters) will have more variable job access across residential locations. This variation creates the possibility of a spatial origin to the RRD.\textsuperscript{29} To assess the relationship between highway investment and the RRD, we use city-level highway mileage data from Baum-Snow (2007). In line with this hypothesis, cities that add more freeway miles see larger declines in the RRD (Column 5). Similarly, CZs with faster declines in transit commuting see larger declines in the RRD (Column 6). Lastly, cities that see larger increases in the average travel time of all car commuters also see larger increases in the RRD (Column 7). All of these results are consistent with the idea that large cities may have large RRDs because travel in those cities became slower over time.

Lastly, Figure A.7.1 in the Appendix shows time trends of the RRDs for 16 large cities. These panels display three aspects of heterogeneity worth noting: the level of the RRD, its change over time, and the role of PUMA of residence. The RRD estimates for Chicago,

\textsuperscript{27}The measure of centrality is adapted from Galster et al. (2001) and can be interpreted as to what degree a population is more centrally located than would be expected on average—larger values indicate greater population centrality with respect to the city’s central business district (CBD). For details on construction, see Appendix.

\textsuperscript{28}We also investigated relative centrality of Black and White commuters; these results are insignificant.

\textsuperscript{29}See Couture et al. (2018) for a discussion of the determinants of travel speed; freeway miles are correlated with faster travel throughout their specifications.
Philadelphia, and Los Angeles are all over 0.2, while the fast-growing cities of Phoenix and Seattle are under 0.1 for most of the sample period. Chicago, Houston, and Detroit saw steep declines while New York City, San Francisco, and Boston saw relatively flat levels of RRD; Phoenix and Seattle saw growth from low levels. The RRD estimates for several cities—notably, including many with subway systems—are meaningfully smaller after accounting for PUMA of residence: New York, Chicago, Washington, San Francisco, Philadelphia, and Los Angeles. The contribution of PUMA of residence to the RRD is smaller but growing in Dallas, Houston, and Atlanta.

2.6.3. Housing Prices and Stratification

As we conceptualize it, spatial stratification arises from the confluence of residential segregation, easier access to job sites from segregated White neighborhoods, and factors like congestion or transit dependence that cause slower travel speeds. Several of these ingredients are also associated with high house prices: inelastic housing supply, whether due to land availability or regulation, pushes new construction to an urban fringe that is relatively far from jobs, inducing higher house prices in more job-accessible regions. Expensive cities thus feature greater internal variation in job access relative to cheap cities. This underlying variation contributes to an economic landscape around which socioeconomic forces may orient the racial geographies of the city.

We argue, therefore, that high house prices are a useful indicator of spatial stratification.\footnote{Lens and Monkkonen (2016) test the link directly, showing that restrictive land-use regulations are associated with income segregation for high- and middle-income households. Relatedly, Hanson et al. (2012) reports “drive-til-you-qualify” behavior, wherein credit constrained households sort further from central cities.}

\footnote{This idea is similar to the relationship identified by Lee and Lin (2018), who show that cities with high internal variation in the presence of natural amenities (rivers, hillsides, coastlines, etc.) feature relatively stable internal distributions of income, with the rich clustering in high-amenity locations. Here, we highlight the variation in job access, which might be thought of as a “second-nature” amenity (Cronon, 1991), and the spatial distribution of racialized groups.}

\footnote{This relation arises within a classic system-of-cities model with internally monocentric cities, like Henderson (1974). Cities with more productive industries (or region-wide consumer amenities) will grow spatially larger, producing longer average commutes as well as greater variation in commute times. Internal spatial equilibrium will in turn drive up house prices in relatively central portions of productive cities, raising average house prices relative to less-productive cities (which, in equilibrium, are smaller and feature shorter average commutes).}
Empirically, as shown in fig:maps, the patterns of persistence suggest a potential link. However, reverse causality could drive this relationship. Cities with high estimated RRDs due to stratification may be more desirable if there is a preference for segregation, driving up housing prices.

To rule out reverse causality, we adopt an instrumental variable (IV) approach. We employ the local sensitivity instrument of Guren et al. (2021), who develop a time-varying proxy for local housing supply elasticity to use as an instrument for housing price (as an alternative to, e.g., Saiz, 2010; Mian et al., 2013). The instrument is comprised of estimates from:

\[
P_{cdt} = \delta_c \bar{P}_{(-c)dt} + \psi_0 \bar{\beta}_{ct} + \psi_1 m_{cdt} + \phi_c t + D_c + \epsilon_{cdt}
\]  

(2.5)

where \( P_{cdt} \) is log mean housing price in CZ \( c \) in Census division \( d \) in year-bin \( t \), \( \bar{P}_{(-c)dt} \) is the leave-c-out log mean housing price in the Census division, \( \psi_0 \bar{\beta}_{ct} \) controls for any effect of RRD and \( \psi_1 m_{cdt} \) for share Black. CZ-specific time trends and fixed effects are included as \( \phi_c t \) and \( D_c \), respectively. \( \epsilon_{cdt} \) is the error term. The estimates \( \hat{\delta}_c \bar{P}_{(-c)dt} \) are then used as a time-varying instrument for price in Equation 2.4.\(^{33}\) The \( \hat{\delta}_c \) are CZ-specific proxies for local housing supply elasticities, akin to Saiz (2010). Thus, the interacted term \( \hat{\delta}_c \bar{P}_{(-c)dt} \) provides a measure of the local response to regional price shocks. This approach infers the effect of housing prices on the RRD from the differential response of cities to regional housing trends.

We are agnostic as to whether housing prices per se or some downstream channel that responds tightly to changes in housing prices are most at play, as we cannot delineate housing price changes from downstream channels. This suggests viewing housing price as a cluster of mechanisms in our setting, rather than the more direct consumption-wealth channel discussed in Guren et al. (2021). Identification requires that there is no unobserved

\(^{33}\)We differ in implementation from Guren et al. (2021) by using more granular geographies (CZs instead of core-based statistical areas and Census divisions instead of regions) and by estimating eq:proxy in levels rather than differences (though we retain CZ-specific time trends). First-stage point estimates are slightly smaller but roughly in line with Guren et al. (2021).
factor correlated with changes in CZ-level housing prices that differentially affects CZs more sensitive to cross-sectional housing price variation (conditional on included controls)—that is, if housing prices respond to regional shocks differently according to factors separate from but correlated with housing supply elasticities. For example, if housing prices capitalize property tax expense, then identification is threatened if locations with inelastic housing supply systematically change property tax rates in response to regional housing demand shocks differently than elastic housing supply locations.

Table 2.6 shows estimates of the relationship between housing prices and the RRD. OLS estimates with year and CZ fixed effects indicate that a 10pp increase in housing prices is correlated with an increase in the RRD by about 0.7pp. Panel B shows first-stage estimates; the instruments are not weak and are highly correlated with CZ-level housing prices. The IV estimates are a bit smaller than the OLS results, but still find that a 10pp increase in housing prices leads to a 0.5pp increase in the RRD. These results are robust to the inclusion of controls for (log) commuting population and the share of workers in the CZ who are Black.34

High housing costs undo some of the partial convergence in the racialized difference in commute times, and these results are economically significant. As a counterfactual exercise, suppose that house prices were held to their 1980 (real) values. Using the IV estimate in Column 4, the average conditional racialized difference in 2012–19 would be 0.028 log points instead of the 0.049 log points we observe in Table 2.1. Said differently, aggregate RRD would be 43% lower today if real housing prices were flat over the last 40 years. High housing costs—indicative of spatial stratification—appear to be a key feature of observed patterns of persistence in the RRD.

Columns 5 and 10 provide an alternative test of stratification by comparing the relationship between neighborhood-level commute times and housing prices across CZs. We compute

34We prefer specifications without controls: city population is likely a bad control, as population and house price are jointly determined by common underlying demand and supply features.
Table 2.6: Two-Way-Fixed-Effect and IV Estimates of Housing Price Effect on RRD

<table>
<thead>
<tr>
<th></th>
<th>All Cities</th>
<th>Cities with &gt;200k</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS (1)</td>
<td>OLS (2)</td>
</tr>
<tr>
<td></td>
<td>OLS (3)</td>
<td>OLS (4)</td>
</tr>
<tr>
<td></td>
<td>IV (5)</td>
<td>IV (8)</td>
</tr>
<tr>
<td></td>
<td>IV (9)</td>
<td>Sort. (9)</td>
</tr>
<tr>
<td></td>
<td>Sort. (10)</td>
<td></td>
</tr>
<tr>
<td><strong>A. Estimates</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( P_{cdt} )</td>
<td>0.068***</td>
<td>0.066***</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>( \ln(\text{Pop}) )</td>
<td>0.025</td>
<td>0.029</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.022)</td>
</tr>
<tr>
<td>% Black</td>
<td>0.163</td>
<td>0.154</td>
</tr>
<tr>
<td></td>
<td>(0.136)</td>
<td>(0.138)</td>
</tr>
<tr>
<td>( \rho_{ct}(P, \tau) )</td>
<td>-0.050*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td></td>
</tr>
<tr>
<td><strong>B. First Stage</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>( \hat{\delta}<em>{c} P</em>{(c)d}\tau_{c} )</td>
<td>0.627***</td>
<td>0.614***</td>
</tr>
<tr>
<td></td>
<td>(0.122)</td>
<td>(0.132)</td>
</tr>
<tr>
<td>F-stat, CD</td>
<td>1285.9</td>
<td>1244.6</td>
</tr>
<tr>
<td>F-stat, KP</td>
<td>26.6</td>
<td>21.8</td>
</tr>
<tr>
<td>N</td>
<td>1705</td>
<td>1705</td>
</tr>
</tbody>
</table>

Data: Estimated RRDs and CZ-level characteristics for CZs with at least 1,000 total employed persons and greater than 50 unique Black commuter Census respondents in all five year bins. Each column is for a different specification, Panel B presents the first-stage results corresponding to Panel A. The dependent variable in each specification is the estimated RRD for each CZ-by-year-bin cell. All models include two-way fixed effects by CZ and year bin. Columns 1–5 use all CZs that are not too noisy; Columns 6–10 use only CZs with at least 200,000 commuters in all five year bins. Columns 1, 2, 6, and 7 provide OLS estimates of the correlation between CZ-level housing prices and RRD, whereas Columns 3, 4, 8, and 9 use the local sensitivity instrument, \( \hat{\delta}_{c} P_{(c)d}\tau_{c} \). Columns 5 and 10 show the effect of CZ-by-year-bin specific correlation between tract-level average housing prices and commute times on RRD. CD and KP refer to Cragg-Donald and Kleibergen-Paap tests, respectively. Models are weighted by the Black commuting population in the CZ-by-year-bin cell. Standard errors clustered by commuting zone. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.
the simple correlation between tract-level average commute times and median home values within a given city. We expect that cities where neighborhood commute times and housing prices are negatively correlated (diverging) will have greater RRDs. This hypothesis holds true with marginal significance.\textsuperscript{35}

These results are consistent with the causes and effects of housing price increases in the literature. Van Nieuwerburgh and Weill (2010) show increasing dispersion of house prices in the U.S. between 1975 and 2007, driven in part by the flow of workers to the most productive metropolitan areas. Guerrieri et al. (2013) in turn document substantial variation in housing price growth within cities and provide a model of neighborhood housing price dynamics in response to a citywide housing demand shock. Their model captures a channel of spatial gentrification, wherein lower-income neighborhoods near higher-income neighborhoods shift to being higher income. These neighborhoods are often those with a high degree of job access. Finally, Gyourko et al. (2013) show that high housing prices tend to crowd out lower income households even from municipalities within the same metropolitan area.

Evolving job access and time use preferences, as described by Su (2019) and Edlund et al. (2021), provide a partial basis for such shifts. These papers relate rising wages and working hours (respectively) among high-paid workers to gentrification. These forces make commuting more costly, and so these workers respond by moving to center-city neighborhoods and pushing up house prices there. Via the mechanisms in Guerrieri et al. (2013) and Gyourko et al. (2013), this then spills out in equilibrium, reducing affordability in high-access neighborhoods. We note that gentrification in these papers is one manifestation of spatial stratification. Our approach likely includes related processes, including racialized patterns of suburbanization.

\textsuperscript{35}Construction details for this measure are provided in the Appendix.
2.7. Conclusion

The Montgomery Bus Boycott lasted 382 days, ending after the Supreme Court ordered the buses of Montgomery to be integrated. The ensuing dozen years saw renewed federal commitment to the civil rights of Black Americans, including the Civil Rights Act of 1964 and the Fair Housing Act of 1968. In the aftermath of these hard-fought battles, the production of the racialized difference in commute times was transformed: whereas Black workers spent 50.4 minutes per week longer commuting than White workers in 1980, the difference was 22.4 minutes by 2019. The patterns of persistence point towards meaningful roadblocks to continued convergence: the racialized difference in commute times persists even when looking narrowly at commuters who drive, it persists across the income spectrum, and it persists particularly in large, segregated, congested, and expensive cities.

About 37% of the decline in the racialized difference in commute times arises from a partial convergence in observable characteristics, especially car use. Notably, the difference in automobile use between Black and White commuters declines from 12pp to 7pp over the last four decades. Job characteristics increasingly interact with race to determine commute time: controlling for occupation, industry, and income together increases the conditional racialized difference, especially in recent years. However, 63% of the decline is not accounted for by changes to the individual characteristics that we observe. Some observable characteristics that contribute to the level of the racialized difference play no role in its decline: throughout our study period, Black workers are disproportionately likely to live in commuting zones with long commutes. Likewise, our measure of residential location is not a central channel of decline, although the measure is fairly coarse.

We turn to CZ-level heterogeneity to investigate the determinants of persistence, especially the different roles that spatial stratification plays across cities. Large and congested cities may be relatively impervious to car-based convergence. High land prices make cars expensive and slow travel speeds transform extant racial and employment segregation into a
racialized commute differential. As many of the ingredients for spatial stratification also reflect high housing costs, we use high housing prices to indicate spatially stratified cities. Increasing house prices at the CZ level are associated with a larger residual racialized difference in commute times. This effect is quantitatively important: were all cities held at their 1980 housing price levels, the average racialized difference in 2012-19 would be 0.028 log points instead of the 0.049 log points we observe.

Our results enrich the literature on changing racialized residential and workplace patterns by refocusing on commuting itself as an outcome of interest (Aliprantis et al., 2019; Bartik and Mast, 2021; Miller, 2018). The 21st century continues to see suburban growth of both jobs and Black communities (and other communities of color), but these processes do not necessarily overlap spatially (Kneebone and Holmes, 2015a). Job growth is often concentrated in particular suburbs that may not overlap with the suburbanization of communities of color; indeed, the two may be on opposite ends of the city, as in Dallas-Forth Worth or Washington, DC. Time spent commuting represents a real cost to households: time spent in traffic or on the bus is time unavailable for other pursuits. The persistent production of the racialized difference in commute times is an ongoing process of spatial inequality whose costs are born by Black commuters and their families.

36Car-based convergence has other problems too: while cars can ease travel, they do nothing to address underlying segregation or the racism at its root (see, e.g., Steil and Charles, 2020). Car commuters are subject to anti-Black policing strategies (Harris, 2010; Rice and White, 2010), and car-based commuting plays a non-negligible role in carbon emissions (e.g., Brand et al., 2021).
A.1. The Financial Burdens of Property Taxes: Outcomes of Interest

According to the “benefits view” of property taxation, property taxes are simply fees for services provided by the local government that levies the tax (Oates and Fischel, 2016). Homeowners pay property taxes in exchange for access to a bundle of local public goods and services, and they sort into jurisdictions with their preferred combination of local taxes and amenities (See, e.g. Tiebout (1956); Oates (1969); Hamilton (1976); Brueckner (1979); Ross and Yinger (1999)). When property taxes increase, the corresponding local amenities also increase, and thus the homeowners will receive more benefits in return for paying more taxes. Under this view, any homeowners that dislike the new combination of higher taxes and more amenities will leave and resort into jurisdictions with lower taxes (and subsequently, fewer amenities). This partial equilibrium outcome, however, depends on a few important assumptions that do not hold in practice. First, it ignores homeowners’ ability to pay when their property taxes increase. Homeowners initially sorted into their homes at a price point and user cost that was affordable to them given their incomes and budget constraints. When property taxes increase, the user costs subsequently increase and housing becomes a more expensive component of homeowners’ budget constraints. As a result, homeowners who used to have the financial means to live in their houses before might not be able to anymore. Even if those homeowners want the additional goods and services that come with higher taxes, they will still get priced out of the housing market if they cannot pay their tax bills. The inability of homeowners to pay their taxes would result in property tax delinquencies.

Second, the partial equilibrium outcome also assumes that all homeowners value the added benefits that come with property tax increases, but some do not. For homeowners who

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37 Kaplan and Violante (2014) find that many households hold little or no liquid wealth despite owning sizable quantities of illiquid assets such as housing. This supports the conjecture here that increases in property tax bills, which are expenses that must be paid, would be problematic for households who do not have cash or other liquidity assets.

38 For example, Johnson and Walsh (2009) focus on owners of vacation homes in their empirical study
do not value the added benefits, the cost of property taxes exceeds their added utility from those additional tax revenues, and thus exceeds their willingness to pay to stay in their homes. If the increase in property taxes is large enough to offset the cost of moving, then there will be an increase in home sales, as those homeowners sell their homes and move to another place with a more desirable combination of taxes and services. Similarly, if current homebuyers—who are the future homeowners—do not value the added benefits that come with higher taxes, then there will be price capitalization; if they do value the benefits, then there will not be any changes in house prices following the property tax increases.

A.2. The Financial Burdens of Property Taxes: Additional Results

Figure A.2.1: Tax Revenue and Millage Rate from 2010 to 2016

because they argue that those owners do not receive any benefits from public goods funded by property tax changes. Similarly, Ferreira (2010) shows that the willingness to pay for amenities varies across homeowners, which goes against the assumption that all homeowners value the benefits from property taxes.
Table A.2.1: $R^2$ of Variables in the Assessment Algorithm

<table>
<thead>
<tr>
<th>Regressor</th>
<th>$R^2$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Full Model</td>
<td>1</td>
</tr>
<tr>
<td>Zipcode and Census Tract</td>
<td>0.999</td>
</tr>
<tr>
<td>Zipcode</td>
<td>0.994</td>
</tr>
<tr>
<td>Census Tract</td>
<td>0.995</td>
</tr>
<tr>
<td>Bedrooms</td>
<td>0.715</td>
</tr>
<tr>
<td>Bathrooms</td>
<td>0.719</td>
</tr>
<tr>
<td>Sq. Feet</td>
<td>0.484</td>
</tr>
<tr>
<td>Living Sq. Feet</td>
<td>0.893</td>
</tr>
<tr>
<td>Number of stories</td>
<td>0.768</td>
</tr>
<tr>
<td>Number of rooms</td>
<td>0.686</td>
</tr>
</tbody>
</table>

The $R^2$ values are calculated by regressing the standard hedonic equation, and each individual regressor in the equation by itself, on the 2014 property assessments. The goodness of fit from the model comes entirely from the census tract fixed effects, which explain 99% of the variation in reassessments. I also use zip code as a measure of location, but census tracts explain more of the variation from the data because they are smaller than zip codes.

Table A.2.2: Delinquency Rates by Year

<table>
<thead>
<tr>
<th>Delinquenties</th>
<th>2012</th>
<th>2013</th>
<th>2014</th>
<th>2015</th>
<th>2016</th>
<th>2017</th>
<th>2018</th>
</tr>
</thead>
<tbody>
<tr>
<td>90 day Late</td>
<td>27.5%</td>
<td>25.9%</td>
<td>27.8%</td>
<td>26.8%</td>
<td>24.7%</td>
<td>20.9%</td>
<td>17.8%</td>
</tr>
<tr>
<td>1yr Late</td>
<td>24.5%</td>
<td>23.1%</td>
<td>24.0%</td>
<td>23.0%</td>
<td>21.5%</td>
<td>17.5%</td>
<td>13.8%</td>
</tr>
<tr>
<td>Share Paid on Time</td>
<td>77.8%</td>
<td>78.5%</td>
<td>79.4%</td>
<td>79.4%</td>
<td>81.6%</td>
<td>82.0%</td>
<td>85.3%</td>
</tr>
<tr>
<td>Share Paid in Year</td>
<td>88.4%</td>
<td>90.1%</td>
<td>91.5%</td>
<td>92.6%</td>
<td>94.0%</td>
<td>95.2%</td>
<td>99.3%</td>
</tr>
</tbody>
</table>

N: 530,230 530,345 524,645 526,513 524,412 520,630 510,158

Delinquency rates among all homeowners in Philadelphia. 1 year late refers to the delinquency rate, which is formally defined as one year after the deadline. Share Paid on Time refers to the share of the outstanding tax bill that was paid by the March 31st deadline. Share Paid in Year refers to the share of the outstanding tax bill that was paid one year after the March 31st deadline.
Figure A.2.2: No Pre-Trends in Housing Characteristics

Event studies of the four most important variables in the assessment algorithm after census tracts show that differences in housing characteristics between the control and treated groups did not change after the property tax reform. This confirms that the two groups follow parallel trends.
Event studies of the four race categories, imputed using homeowner names, show that the differences in racial compositions between the control and treated groups did not change after the property tax reform. This confirms that the two groups follow parallel trends.
Figure A.2.4: Differences in Property Taxes Between Treated and Control Groups, White Owners

Figure A.2.5: Differences in Property Taxes Between Treated and Control Groups, Black Owners
Figure A.2.6: Differences in Delinquency Between Treated and Control Groups, White Owners

Figure A.2.7: Differences in Delinquency Between Treated and Control Groups, Black Owners
Figure A.2.8: Differences in Home Sales Between Treated and Control Groups, White Owners

Figure A.2.9: Differences in Home Sales Between Treated and Control Groups, Black Owners
Figure A.2.10: Differences in House Prices Between Treated and Control Groups, White Owners

Figure A.2.11: Differences in House Prices Between Treated and Control Groups, Black Owners
A.3. The Problem Has Existed Over Endless Years: Additional Derivations for Two-Step Decomposition

Footnote 8 argues that the two-step approach in Equations 2.3 and 2.4, which allows both $\beta^*$ and all individual covariates to vary at the CZ level, contributes to decomposing the subset of $\Delta_{\{t\}}$ that is captured by $\Delta_{\{t\}}^{\text{Unexplained}}$. The following two subsections show that this is true under some additional assumptions.

**CZ-specific control heterogeneity contributes to $\Delta_{\{t\}}^{\text{Unexplained}}$**

First, rewrite differential outcomes by race to allow city-specific coefficients:

\[
\ln(\tau_{ict}) = \alpha^W_{ct} + x_{ict}'\tilde{\mu}^W_{ct} + \tilde{\lambda}_{ct} + \epsilon^W_{ict} \quad \text{if } \mathbb{I}([\text{Black}_{ict}] = 0)
\]

\[
\ln(\tau_{ict}) = \alpha^B_{ct} + x_{ict}'\tilde{\mu}^B_{ct} + \tilde{\lambda}_{ct} + \epsilon^B_{ict} \quad \text{if } \mathbb{I}([\text{Black}_{ict}] = 1).
\]

Define $\tilde{\mu}^k_{ct} = \tilde{\mu}^k - \tilde{\mu}^k_{ct}$ for $k \in \{B, W\}$. Substituting in:

\[
\ln(\tau_{ict}) = \alpha^W_{ct} + x_{ict}'(\tilde{\mu}^W_{ct} - \tilde{\mu}^W_{ct}) + \tilde{\lambda}_{ct} + \epsilon^W_{ict} \quad \text{if } \mathbb{I}([\text{Black}_{ict}] = 0)
\]

\[
\ln(\tau_{ict}) = \alpha^B_{ct} + x_{ict}'(\tilde{\mu}^B_{ct} - \tilde{\mu}^B_{ct}) + \tilde{\lambda}_{ct} + \epsilon^B_{ict} \quad \text{if } \mathbb{I}([\text{Black}_{ict}] = 1).
\]

Again following Fortin (2008), we set $\tilde{\mu}^k = \tilde{\mu}$ and $\tilde{\mu}^k_{ct} = \tilde{\mu}_{ct}$ for $k \in \{B, W\}$ to retain regression compatibility. The difference in expected outcomes in a particular city $c$ is (suppressing time variation):

\[
\tilde{\Delta}_c = (\alpha^B_c - \alpha^W_c) + (x^B_c - x^W_c)(\bar{\mu} - \tilde{\mu}_c).
\]

The overall difference between the two expected outcomes is now given by the sum of the weighted average of the city-specific differences and the weighted average of city-specific
FEs (again suppressing time variation):

\[ \Delta = \sum p_c \tilde{\Delta}_c + \sum (p_c^B - p_c^W) \tilde{\lambda}_c \]

where \( p_c \) is the share of the total population in \( c \) and \( p^k_c \) is as before.

Substituting \( \tilde{\Delta}_c \) into \( \Delta \), we get:

\[ \Delta = \sum p_c (\bar{x}_c^{B'} - \bar{x}_c^{W'}) (\bar{\mu} - \mu_c) + \sum p_c (\alpha_c^B - \alpha_c^W) + \sum (p_c^B - p_c^W) \tilde{\lambda}_c. \]

Noting that

\[ \sum p_c (\bar{x}_c^{B'} - \bar{x}_c^{W'}) \bar{\mu} = (\bar{x}_c^{B'} - \bar{x}_c^{W'}) \bar{\mu} + \sum \left( s_W (p_c^W - p_c^B) \bar{x}_c^{B'} - s_B (p_c^B - p_c^W) \bar{x}_c^{W'} \right) \bar{\mu}, \]

where \( s_k \) are the overall share of \( k \) in the population, we see that

\[ \Delta = (\bar{x}_c^{B'} - \bar{x}_c^{W'}) \bar{\mu} + \sum (p_c^B - p_c^W) \tilde{\lambda}_c \]

\[ + \sum \left( s_W (p_c^W - p_c^B) \bar{x}_c^{B'} - s_B (p_c^B - p_c^W) \bar{x}_c^{W'} \right) \bar{\mu} - \sum p_c (\bar{x}_c^{B'} - \bar{x}_c^{W'}) \mu_c \tilde{\Delta}_{\text{Explained, City Avg.}} \]

\[ + \sum p_c (\alpha_c^B - \alpha_c^W) \tilde{\Delta}_{\text{Unexplained}} \]

City-level heterogeneity in non-race individual controls is represented by \( \tilde{\mu}_c \), and thus its contribution to \( \Delta \) is captured by \( \tilde{\Delta}_{\text{Explained, City Avg.}} \). This component also reflects the differential distributions of group-specific population characteristics.

To relate these to the decomposition in the Section 2.4, we make additional assumptions to allow us to compare adding CZ-heterogeneous controls sequentially after those in the main paper (in contrast to Gelbach (2016)). Specifically, suppose that \( \bar{\mu} = \mu \) and \( \tilde{\lambda}_c = \lambda_c \) (that is, assume that including CZ-heterogeneous controls does not change the values of these estimates). Then \( \tilde{\Delta}_{\text{Explained, Aggregate}} = \Delta_{\text{Explained}} \) and

\[ \Delta - \Delta_{\text{Explained}} = \Delta_{\text{Unexplained}} = \tilde{\Delta}_{\text{Explained, City Averages}} + \tilde{\Delta}_{\text{Unexplained}}. \]
Thus, ignoring changes in $\mu$ and $\lambda$, CZ-level heterogeneity is a subset of $\Delta^{\text{Unexplained}}$.

**Contribution of Second Step to $\Delta_{\{t\}}$**

Define the CZ-specific RRD as $\tilde{\Delta}_{c}^{\text{RRD}} = \alpha_{c}^{B} - \alpha_{c}^{W}$ (recall that RRD is residual racialized difference). Suppose this has a linear representation, such that:

$$
\tilde{\Delta}_{c}^{\text{RRD}} = \alpha_{c}^{B} - \alpha_{c}^{W} = a_{0} + \gamma z_{c} + e_{c}
$$

Recall that $\sum p_{c} \tilde{\Delta}_{c}^{\text{RRD}} = \tilde{\Delta}^{\text{Unexplained}}$, so we can quantify how any variable (or vector of variables) $z_{c}$ contributes to $\tilde{\Delta}^{\text{Unexplained}}$ as:

$$
\begin{align*}
\tilde{\Delta}^{\text{RRD Explained}}(z_{c}) &= \sum p_{c} \gamma z_{c} \\
\tilde{\Delta}^{\text{RRD Unexplained}}(z_{c}) &= \sum p_{c} \left( \tilde{\Delta}_{c}^{\text{RRD}} - \gamma z_{c} \right)
\end{align*}
$$

where naturally $\tilde{\Delta}^{\text{RRD Explained}}(z_{c}) + \tilde{\Delta}^{\text{RRD Unexplained}}(z_{c}) = \tilde{\Delta}^{\text{Unexplained}}$ for any $z_{c}$ and $\gamma$.

As before, when $\tilde{\mu} = \mu$ and $\tilde{\lambda}_{c} = \lambda_{c}$, $\tilde{\Delta}^{\text{Unexplained}}$ is itself a subset of $\Delta^{\text{Unexplained}}$, so its subcomponents $\tilde{\Delta}^{\text{RRD Explained}}(z_{c})$ and $\tilde{\Delta}^{\text{RRD Unexplained}}(z_{c})$ are as well.\(^{39}\)

This $\tilde{\Delta}^{\text{RRD Explained}}(z_{c})$ embeds a differential response to a city-level variable, as we can expand $\tilde{\Delta}_{c}^{\text{RRD}}$ with race-specific coefficients:

$$
\tilde{\Delta}_{c}^{\text{RRD}} = \alpha_{c}^{B} - \alpha_{c}^{W} = (a_{0}^{B} - a_{0}^{W}) + (\gamma^{B} - \gamma^{W}) z_{c} + (e_{c}^{B} - e_{c}^{W}),
$$

where $\gamma^{B} - \gamma^{W} = \gamma$ is the value identified from our estimation model. This is not a difference in ‘endowments’ or characteristics, but rather represents a differential response to aggregate variables. This is does not “explain” the RRD in the same sense as individual covariates, but rather highlights channels through which racialized difference may arise. For this reason, we typically do not report magnitudes of $\tilde{\Delta}^{\text{RRD Explained}}(z_{c})$ (with the exception

---

\(^{39}\)Note, however, that an additional difference may arise between OLS estimates of $\Delta^{k}$ and average $\sum p_{c} \Delta_{c}^{k}$, because OLS estimates are variance weighted rather than weighted by population (Gibbons et al., 2018). We ignore this concern to maintain simplicity of calculation and exposition.
of housing prices, for which we have a plausibly causal estimate).

A.4. The Problem Has Existed Over Endless Years: City-Level Heterogeneity Measures

Below we describe the full set of measures considered. Note that not all appear in the main text. We include all measures and variations on measures here for clarity and transparency.

**Population centrality**

Centrality measures the population weighted average distance from census tract centroid to the commuting zone central business district (CBD). Given the variation in commuting zone total area, the population weighted average distance is standardized with respect to the average distance from all census tracts to the center. Centrality of a commuting zone is calculated as follows:

\[
Ctr = \frac{\sum_{n=1}^{N} \frac{d(n, CBD)}{i_n/I} \cdot d(n, CBD)}{\sum_{n=1}^{N} \frac{i_n}{I}} - 1
\]  

(A.1)

where \(d(n, CBD)\) is the distance from the centroid of census tract \(n\) to the CBD and \(i_n/I\) is the weight assigned to tract \(n\) based on the proportion of population of type \(i\) in tract \(n\) with respect to the total population of type \(i\) within a given commuting zone. A number larger than zero indicates a population is more centrally located than would be expected on average. We consider the total population as well as Black and White populations separately.

Central business district longitude and latitudes are based on downtown location derived from Google Maps (Manduca, 2021). This is a similar methodology to Holian and Kahn (2015), but with full coverage of all commuting zones considered. Population counts and census tract centroids are retrieved from the Decennial Census (1980, 1990, 2000) and the American Community Survey (2006-2010, 2014-2018) via NHGIS.
Population segregation

We consider two measures of segregation: Dissimilarity Index (Duncan and Duncan, 1955; Massey and Denton, 1988) and the Square Root Index (Hutchens, 2001). Such aspatial measures have shortcomings. Namely, they do not account for patterns of spatial organization that occur at multiple scales (Arcaya et al., 2018; Reardon et al., 2008). We acknowledge these shortcomings but present results in the main text using the Dissimilarity Index for ease of interpretation.

The Dissimilarity Index and the Square Root Index for a given commuting zone are constructed as follows:

\[
\text{Dissimilarity} = \frac{1}{2} \sum_{i=1}^{N} \left| \frac{w_i}{W} - \frac{b_i}{B} \right| \quad (A.2)
\]

\[
\text{SquareRoot} = 1 - \sqrt{\frac{1}{N} \sum_{i=1}^{N} \left( \frac{w_i}{W} \cdot \frac{b_i}{B} \right)^{1/2}} \quad (A.3)
\]

where \( w_i \) and \( b_i \) represent the White and Black population count in tract \( i \). \( W \) and \( B \) represent the total White and Black population in the commuting zone. Larger values for both indexes indicate more White and Black separation. Population counts from the Decennial Census and ACS are used to construct both indexes.

Balance of jobs versus housing

Following Bento et al. (2005), we construct a measure that indicates how evenly distributed jobs are relative to population. This measure is akin to Massey and Denton’s Gini coefficient. We consider the relationship between jobs and the total population of employed people as well as employed White and Black people separately. The Gini coefficient is the area between the Lorenz curve and the 45 degree line. To produce the curve, zip codes in each commuting zone are ordered from smallest number of jobs to largest number of jobs and plotted against
the cumulative percent of employed population for those zip codes.

For employment counts, we use Zip Code Business Patterns data (ZCBP) for 1994, 2000, 2010, and 2018 (Manson et al., 2021). Unfortunately data for 1980 and 1990 is unavailable. We thus match 1994 ZCBP to 1990 Census data. Zip code level Decennial Census (1990, 2000) and ACS (2006-2010, 2014-2018) data provide population counts. Note that the annual ZCBP data is produced using zip codes, where as Census data relies on zip codes for 1990 then uses Zip Code Tabulation Areas (ZCTAs) for remaining years. ZCTAs are generalized representations of zip code boundaries constructed by the Census Bureau.\textsuperscript{40}

While the majority of zip codes are stable over time and do coincide with ZCTAs, combining these two datasets presents some challenges. First, the number of zip codes that do change over time is large enough to introduce measurement error into subsequent analysis. Zip codes may be decommissioned, merged, or split in any given year. Second, some zip codes in the ZCBP represent large postal customers (e.g. a large company in one building) or PO boxes. Thus, they do not have associated spatial boundaries and are merely points in space. These zip codes do not have corresponding ZCTAs as ZCTAs represent spatial boundaries with positive residential population. Third, zip codes with positive employment and associated geography (not a large postal customer or PO box) that do not contain residential population (e.g. commercial office park) will not be contained within the Census data. This makes it difficult to know whether a zip code in fact does not have residential population, or it is not properly crosswalked to consistent zip code or ZCTA boundaries, a method which we describe below. We drop from the dataset ZCBP zip codes and Census zip codes/ZCTAs that we are unable to merge via the methods described below. This works out to 1,056, 50, 0, 0 ZCBP zipcodes for 1994, 2000, 2010, 2018 respectively. From the Census data we drop 212, 386, 0, 0 for 1990, 2000, 2006-2010, 2014-2018 respectively. Note that for the 2006-2010 and 2014-2018 ACS all ZCBP zip codes merge so we set employment in the unmerged ZCTAs to zero thus do not drop any ZCTAs.

\textsuperscript{40}More details on the construction of ZCTAs can be found here https://www.census.gov/programs-surveys/geography/guidance/geo-areas/zctas.html.
We use a national zip code crosswalk spanning 1990-2010 to create geographically stable “zip code clusters” over 1990-2000 and 2000-2010 (Bailey and Suppan Helmuth, 2020). This crosswalk facilitates the majority of merges between the ZCBP and Census datasets. To account for large customers and PO boxes, we use the 2020 UDS Mapper zip code to ZCTA crosswalk (Snow, 2020). For large postal customers or PO boxes this amounts to spatially joining the latitude and longitude of these zip codes to the enclosing ZCTA. For older data with decommissioned zip codes, this 2020 dataset is less helpful. Further, as stated by the creators of the crosswalk, not all large customers and PO box latitude and longitudes correspond to the location of the actual customers. We do not observe when this is the case and acknowledge potential for measurement error here. For ZCBP zip codes that remain unmerged, we attach longitudes and latitudes and spatially join to ZCTAs shapefiles for their respective years. Longitudes and latitudes are provided by https://www.unitedstateszipcodes.org (Zip Code Database, 2021). These longitudes and latitudes are associated with current zip codes; thus, older zip codes from the ZCBP that we are not able to account for using other methods may remain unmerged if not contained within the longitude/latitude database.

Commute time and housing value

We consider two measures to account for the spatial relationship between housing values and commute time. The first is a simple correlation between the average one-way commute time in minutes and the median housing value within a commuting zone using census tracts. The second measure is based on the absolute difference in percentile rank of commute time and housing value. Specifically, we rank tracts within a commuting zone by longest commute time to shortest commute time (worst commute to best commute) and rank tracts from lowest median housing value to highest median housing value. We then average the tract absolute difference between the two rankings for each commuting zone.

Both measures are computed for the following Decennial Census and ACS years using
census tract level data retrieved from NHGIS: 1980, 1990, 2000, 2006-2010, 2014-2018. Note that for the ACS 5-year surveys, aggregate commute time is missing for roughly 25% of the tracts. We require the aggregate value to calculate average commute time. However, counts for binned commute times are available for all tracts. We impute the missing aggregate values by regressing the observed aggregate values on the set of binned counts along with commuting zone fixed effects. Coefficient estimates are used to construct the missing aggregate values. The R2 is 0.99 for the regression.

A.5. The Problem Has Existed Over Endless Years: PUMA Use

We use Public Use Microdata Areas (PUMAs) to control for residential location. PUMAs provide a more coarse geographic resolution than ideal, but do allow for some heterogeneity within major cities. In large CZs, residential PUMAs divide a larger area into smaller areas of roughly 100,000 people each, subject to data disclosure rules. This means that, at least within cities, there is some resolution into where people live in our data.

However, these are not constant over time. In the 1980 Census, residential PUMAs were based on county groups, and provide little additional resolution beyond CZs. After 1990, these became a bit more refined, however, 1990 residential PUMAs do not divide within census-designated places—this means that they do not distinguish areas within municipal boundaries. This is especially impactful in big cities where many of the survey respondents in our data live.

Differences over time are why we restrict analysis to 2000 and later for PUMA-enabled models. The table below gives the number of unique residential in each year bin.
### A.6. The Problem Has Existed Over Endless Years: Tract-level Analysis

We provide additional analysis of (geonormalized) census-tract level average commuting times. This has the advantage of allowing us to include census-tract fixed effects to control for time-invariant factors that determine commute times, like distance to the CBD. However, a disadvantage is that relatively few controls are available, and we can only include tract-level shares and averages.

Specifically, we index census tracts by \( a \) and estimates variants of:

\[
\ln(\bar{\tau}_{act}) = \beta_t r^{Black}_{act} + \bar{x}_{act}'\mu + \xi_a + \lambda_{ct} + u_{act},
\]

where \( \bar{\tau}_{act} \) is the average commute time in \( a \), \( s^{Black}_{act} \) is the Black residential population share in \( a \), \( \bar{x}_{act} \) are tract-level averages functioning as controls (we use transit share), and \( \xi_a \) are tract fixed effects. CZ-by-year-bin-specific differences and changes in commute times are captured by \( \lambda_{ct} \). Results are shown in Table A.7.3 on both observed tract-level travel times, and tract-level travels times augmented with imputed values for missing tracts as discussed in Appendix A.4.
A.6.1. Montgomery, AL commute mode statistics

Statistics regarding the mode choice of commuters in extremely segregated census tracts of 1960 Montgomery were compiled using Social Explorer. First, we identified census tracts where the racial composition of residents is at least 95% Black or 95% White. For these tracts, we tallied the number of total workers as well as the number listing their means of transportation to work as car, bus, or walking.\textsuperscript{41} We then summed employment as a total and by mode across mostly-Black and mostly-White tracts, respectively to produce the figures shown in the text.

Tract 53, in the northeast of Montgomery, appeared to be an outlier: it was 96% White, but only 14% of commuters used a car. The next lowest share in a mostly-white county was 86%. Upon further examination, the site is a military installation, likely explaining the different commuting patterns. We report totals with and without this tract.

Maps from which this data was derived are available at https://www.socialexplorer.com/6323c92504/view.

A.7. The Problem Has Existed Over Endless Years: Additional Results

\textsuperscript{41}Technically, the category is “bus or streetcar”, but Montgomery did not operate a streetcar at the time, see https://web.archive.org/web/20081204163028/http://www.montgomerytransit.com/history.html
Figure A.7.1: Evolution of Residual Racialized Difference (RRD) in 16 Big Cities
Figure A.7.2: Persistence of Residual Racialized Difference (RRD) Across Cities

Note: Circle size indicates the size of the Black commuting population in 2012–19. Regression slope is estimated weighting each CZ by its Black commuting population in 2012–19, standard errors are robust to heteroskedasticity.

Figure A.7.3: Racialized Difference in Commute by CZ and Population from 1980 to 2012–2019
Figure A.7.4: Racialized Difference in Commute by CZ and Black Share of Commuters from 1980 to 2012–2019

Figure A.7.5: Racialized Difference in Commute by CZ and Dissimilarity from 1980 to 2012–2019
Figure A.7.6: Racialized Difference in Commute by CZ and Centrality from 1980 to 2012–2019

Figure A.7.7: Racialized Difference in Commute by CZ and Black employment concentration (GINI) from 1980 to 2012–2019
Figure A.7.8: Racialized Difference in Commute by CZ and White employment concentration (GINI) from 1980 to 2012–2019

Figure A.7.9: Racialized Difference in Commute by CZ and Log Miles of 2- and 3-digit Highways from 1980 to 2012–2019
Figure A.7.10: Racialized Difference in Commute by CZ and Transit Mode Share from 1980 to 2012–2019

Figure A.7.11: Racialized Difference in Commute by CZ and Log Average House Value from 1980 to 2012–2019
Figure A.7.12: Racialized Difference in Commute by CZ and Corr(Neighborhood Commute, House Value) from 1980 to 2012–2019
Table A.7.1: Racialized Difference in Commute Time by Mode and CZ Type and with Residential PUMA Controls

<table>
<thead>
<tr>
<th></th>
<th>Big Transit CZs</th>
<th>Big Non-Transit CZs</th>
<th>Other CZs</th>
</tr>
</thead>
<tbody>
<tr>
<td>1[Black] × t1980</td>
<td>0.184***</td>
<td>0.210***</td>
<td>0.108***</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.026)</td>
<td>(0.029)</td>
</tr>
<tr>
<td>1[Black] × t1990</td>
<td>0.145***</td>
<td>0.151***</td>
<td>0.080**</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.024)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>1[Black] × t2000</td>
<td>0.161***</td>
<td>0.161***</td>
<td>0.095**</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.015)</td>
<td>(0.035)</td>
</tr>
<tr>
<td>1[Black] × t2005–11</td>
<td>0.138***</td>
<td>0.131***</td>
<td>0.109***</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.007)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>1[Black] × t2012–19</td>
<td>0.124***</td>
<td>0.117***</td>
<td>0.107***</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.007)</td>
<td>(0.029)</td>
</tr>
</tbody>
</table>

Observations 6,491,943 5,314,304 317,202 377,870 256,602 3,432,918 3,205,900 91,234 81,198 38,842,537 36,550,893 361,622 1,405,247

B. Year-Specific Estimates, with year-bin × PUMA FE (2000 and later only)

<table>
<thead>
<tr>
<th></th>
<th>Big Transit CZs</th>
<th>Big Non-Transit CZs</th>
<th>Other CZs</th>
</tr>
</thead>
<tbody>
<tr>
<td>1[Black] × t2000</td>
<td>0.125***</td>
<td>0.128***</td>
<td>0.072**</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.009)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>1[Black] × t2005–11</td>
<td>0.106***</td>
<td>0.104***</td>
<td>0.079**</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.007)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>1[Black] × t2012–19</td>
<td>0.082***</td>
<td>0.077***</td>
<td>0.068**</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.004)</td>
<td>(0.016)</td>
</tr>
</tbody>
</table>

Observations 4,730,009 3,884,881 212,557 287,120 173,408 2,639,666 2,473,849 66,125 55,198 29,993,000 28,406,589 249,977 932,886

Data: All commuters in the Census (1980, 1990, 2000) and ACS (2005–2019) with race Black alone or in combination or White alone. Columns 1–5 consider ‘Big Transit Cities’, CZs with sizable heavy-rail transit: New York City, Boston, Chicago, Philadelphia, Washington, D.C., San Francisco, Atlanta, and Los Angeles. Columns 6–9 consider ‘Big Non-Transit Cities’: Dallas-Fort Worth, Houston, Miami, Phoenix, Seattle, Detroit, San Diego, and Minneapolis-St. Paul. Columns 10–13 consider all other CZs. Technically, Miami has a heavy-rail transit system; its scale, ridership, and/or ridership per mile are relatively small compared to the other cities. Columns 2–5, 7–9, and 11–13 further restrict the sample based on commute mode. Each column in each panel is for a single speciﬁcation. The dependent variable is log travel time top-coded at 99 minutes. Each column includes demographic controls and work and income controls interacted with year bin, as well as commuting-zone-by-year-bin ﬁxed effects. Columns 1, 6, and 10 of both panels include transit mode controls. Panel B includes residential-PUMA-by-year-bin ﬁxed effects and so only uses data from 2000 and later because pre-2000 PUMAs are too geographically coarse. Observations weighted by adjusted person sample weights. Standard errors clustered by commuting zone. + p < 0.10, * p < 0.05, ** p < 0.01, *** p < 0.001.
### Table A.7.2: Correlations between CZ-Level Population and Share Black and RRD

<table>
<thead>
<tr>
<th></th>
<th>1980</th>
<th>2012–19</th>
<th>Panel</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Ln(Pop)</td>
<td>0.0188***</td>
<td>0.0251***</td>
<td>0.0334**</td>
</tr>
<tr>
<td></td>
<td>(0.0056)</td>
<td>(0.0066)</td>
<td>(0.0126)</td>
</tr>
<tr>
<td>% Black</td>
<td>0.3325***</td>
<td>0.2358**</td>
<td>0.1146***</td>
</tr>
<tr>
<td></td>
<td>(0.0552)</td>
<td>(0.0736)</td>
<td>(0.0280)</td>
</tr>
</tbody>
</table>

Cities: All

CZ & Year FEs: -

N: 341

R²: 0.171

Data: Estimated RRDs and CZ-level characteristics for CZs with at least 1,000 total employed persons and greater than 50 unique Black commuter Census respondents. Columns 1–3 only consider 1980, Columns 4–6 only consider 2012–19, and Columns 7–9 use all years. Columns 3, 6, and 9 only consider CZs with at least 200,000 total commuters in all five year bins. Each column is for a different specification. The dependent variable in each specification is the estimated RRD for each CZ-by-year-bin cell. Columns 7–9 include two-way fixed effects by CZ and year bin. Models are weighted by the Black commuting population in the CZ-by-year-bin cell. Standard errors in Columns 1–6 are robust to heteroskedasticity, and in Columns 7–9 are clustered by commuting zone. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.
### A. Observed Tract-level Travel Times

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Share Black in Tract × (t_{1980})</td>
<td>0.245(***)</td>
<td>0.129(***)</td>
<td>0.032</td>
<td>0.032</td>
<td>0.064(***)</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.024)</td>
<td>(0.030)</td>
<td>(0.029)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>Share Black in Tract × (t_{1990})</td>
<td>0.179(***)</td>
<td>0.040</td>
<td>-0.046(*)</td>
<td>-0.042(*)</td>
<td>0.021</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.031)</td>
<td>(0.021)</td>
<td>(0.019)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Share Black in Tract × (t_{2000})</td>
<td>0.197(***)</td>
<td>0.073(*)</td>
<td>0.005</td>
<td>0.018</td>
<td>0.087(***)</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.035)</td>
<td>(0.024)</td>
<td>(0.023)</td>
<td>(0.013)</td>
</tr>
<tr>
<td>Share Black in Tract × (t_{2006−10})</td>
<td>0.116(***)</td>
<td>-0.023</td>
<td>-0.047</td>
<td>-0.026</td>
<td>0.059(***)</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
<td>(0.027)</td>
<td>(0.027)</td>
<td>(0.025)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>Share Black in Tract × (t_{2014−18})</td>
<td>0.100(**)</td>
<td>-0.026</td>
<td>-0.047</td>
<td>-0.025</td>
<td>0.065(***)</td>
</tr>
<tr>
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<td>(0.037)</td>
<td>(0.025)</td>
<td>(0.024)</td>
<td>(0.022)</td>
<td>(0.013)</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>294,906</td>
<td>294,741</td>
<td>294,741</td>
<td>294,741</td>
<td>294,686</td>
</tr>
</tbody>
</table>

### B. Observed and Imputed Tract-level Travel Times

<table>
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<tr>
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</tr>
</thead>
<tbody>
<tr>
<td>Share Black in Tract × (t_{1980})</td>
<td>0.245(***)</td>
<td>0.129(***)</td>
<td>0.032</td>
<td>0.032</td>
<td>0.063(***)</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.024)</td>
<td>(0.030)</td>
<td>(0.029)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>Share Black in Tract × (t_{1990})</td>
<td>0.179(***)</td>
<td>0.040</td>
<td>-0.046(*)</td>
<td>-0.042(*)</td>
<td>0.021</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.031)</td>
<td>(0.021)</td>
<td>(0.019)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Share Black in Tract × (t_{2000})</td>
<td>0.197(***)</td>
<td>0.073(*)</td>
<td>0.005</td>
<td>0.018</td>
<td>0.086(***)</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.035)</td>
<td>(0.024)</td>
<td>(0.023)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Share Black in Tract × (t_{2006−10})</td>
<td>0.132(**)</td>
<td>0.014</td>
<td>-0.038</td>
<td>-0.019</td>
<td>0.043(***)</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
<td>(0.035)</td>
<td>(0.025)</td>
<td>(0.024)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>Share Black in Tract × (t_{2014−18})</td>
<td>0.112(*)</td>
<td>-0.004</td>
<td>-0.048</td>
<td>-0.029</td>
<td>0.044(***)</td>
</tr>
<tr>
<td></td>
<td>(0.049)</td>
<td>(0.038)</td>
<td>(0.029)</td>
<td>(0.028)</td>
<td>(0.012)</td>
</tr>
<tr>
<td><strong>N</strong></td>
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<td>346,522</td>
<td>346,522</td>
<td>346,522</td>
<td>346,478</td>
</tr>
</tbody>
</table>

Year Bin×CZ FEs: - Y Y Y Y

Controls
Share Transit in Tract: - - Y Y Y
Distance to CBD: - - - Y -
Tract FEs: - - - Y

Data: Average observed and imputed travel times, share Black, and share commuting by transit in 1980, 1990, 2000 Census data and 2006–10 and 2014–18 5-year ACS, from NHGIS, geonormalized to 2010 geographies. Imputation of travel time is described in Appendix ??, and the model is described in Appendix ???. Each column in each panel is for a different specification. The dependent variable is log average travel time in a census tract. Central Business District (CBD) locations is derived from Google Maps as in (Manduca, 2021). Controls are interacted with year bin. Standard errors clustered by commuting zone. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.
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