Black Lives: The High Cost of Segregation*

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Abstract

Exploiting the arrangement of railroad tracks in northern cities, we explore the extent to which segregation impacts homicide victimization by race. Our results reveal a robust positive relationship between segregation and non-white homicide victimization. In addition, we find that the lack of public provisions is likely driving our results, as highly segregated locations generate fewer revenues and have lower public expenditures. Our findings suggest that white flight and segregation deplete the local tax base, leading to urban decay and higher crime, resulting in the loss of non-white lives.

JEL Classification: I31, J15, K42, R23

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1 Introduction

Homicide is the leading cause of death for young Black men in the United States.[1] In 2013, young Black males represented 40 percent of all homicide victims, while young white males accounted for 16 percent. Young Black men and Black Americans, in general, tend to live in more disadvantaged, racially segregated neighborhoods, which restricts their upward mobility (Chetty et al., 2014; Andrews et al., 2017; Bayer et al., 2021) while also exposing them to higher levels of violence (Firebaugh and Acciai, 2016). Although the non-white U.S. population share has grown from 10 percent to roughly 40 percent over the last 70 years, U.S. neighborhoods remain highly segregated. The average Black citizen in a metro area resides in a neighborhood that is more than 50 percent non-white (Glaeser and Vigdor, 2001; Frey, 2018). Massey and Denton (1989) find that Black segregation is unlike that of any other racial or ethnic group; they describe it as *hypersegregation*.

Outside of the South, much of the residential segregation that persists today originated during the first wave of Black migration, peaking at the end of the second wave in 1970 (Cutler et al., 1999; Logan and Parman, 2017). Between 1910 and 1970, approximately 6 million Black Americans migrated to northern and western cities in hopes of better economic opportunities, drastically altering the racial composition of America's largest cities. New migrants sorted into urban neighborhoods established before WWI (Shertzer and Walsh, 2019). Federal policies, local covenants, threats of violence, and white flight trapped northern Blacks in select communities, stymieing the economic progress of native northern Blacks (Boustan, 2010; Derenoncourt, 2021). Ultimately, this social exclusion led to a high concentration of poverty and depletion of the tax base, resulting in lower educational attainment and earnings, as well as higher rates of single motherhood among Black Americans (Cutler and Glaeser, 1997).

In particular, the second wave of Black migration in northern cities led to a reduction in upward mobility for third-generation descendants of these migrants, largely due to changes in environmental factors rather than selection (Derenoncourt, 2021). Communities that experienced an increase in Black migration between 1940 and 1970 faced higher levels of racial segregation (changes in the neighborhoods' racial composition), greater crime rates, greater incarceration

[1] We define young as under the age of 45. For statistics, see CDC – Leading Cause of Deaths for Non-Hispanic Males.

rates, and increased expenditures on public safety, resulting in increased contact between the police and Black residents in destination cities (Derenoncourt, 2021). The strong correlation between Black migration, racial segregation, and higher murder rates post-1970 is consistent with prior research investigating the relationship between racial segregation and violent crime (Peterson and Krivo, 1993; Massey, 1995; Shihadeh and Maume, 1997; Bjerk, 2006), which finds a positive correlation between the two. However, research has yet to find a causal relationship between segregation and homicide victimization by race.[2]

In this paper, we explore the degree to which residential segregation influences homicide victimization by race. We focus on homicides because of the significant racial disparities that exist in victimization. Evans et al. (2018) find that the introduction of crack cocaine dramatically increased homicides for younger men, disproportionately affecting younger Black males.[3] Yet, homicides were the leading cause of death of young Black males before and after the arrival of crack cocaine. The violence attributed to illegal drug markets was likely exacerbated by pre-determined environmental factors associated with segregation and isolation. Therefore, segregation in and of itself is an important phenomenon plausibly contributing to racial disparities in homicide victimization.

Residential racial segregation not only isolated Black Americans geographically, but also separated them from social and economic opportunities as jobs and resources moved from cities to suburbs. Andrews et al. (2017) find that historical racial segregation is correlated with diminished contemporary economic mobility. Therefore, concentrated poverty and the myriad of problems associated with it, including drugs and crime, followed the residential racial segregation experienced by Black Americans. Massey (1995) purports that it is the interaction between high levels of poverty and residential segregation that has led to high rates of violence and victimization within Black communities. Violence and victimization become endemic to this type of environment as residents adopt violent behavioral responses to deter potential criminals and lower their own risk of victimization (Massey, 1995; O'Flaherty and Sethi, 2007, 2010; Evans and Kotowski, 2021). In addition, these communities have poor social control of crime due to disruptions in social bonds (e.g., family disruption, joblessness) stemming from poverty and isolation. Moreover, city officials

[2]The work of Bjerk (2006) is closely related. Two-stage least square results reveal a positive relationship between segregation and robbery and aggravated assaults; they reveal no relationship between segregation and property crime (burglary, larceny, motor vehicle theft). Nonetheless, Bjerk (2006) does not explore murder/manslaughter or victimization, leaving this, as well as explanations for potential mechanisms, an open question.

[3]Younger refers to those ages 15-24.

and businesses may target racially segregated communities for disinvestment in public and private resources, creating further disadvantages. Such disinvestments could lead to greater crime in segregated communities (if, for example, police are inadequately funded), or decreases in crime, specifically property crime, due to a decrease in the supply of available goods to steal. In this sense, residential segregation not only leads to social disadvantage but may also lower the political efficacy of the isolated group (Ananat and Washington, 2009).

However, it is not clear who bears the burden of segregation. Krivo et al. (2009) hypothesize that residential segregation not only increases violence within the segregated communities but also increases violence in all neighborhoods in more segregated cities. Whites in highly segregated cities may experience higher rates of violent crime because the lack of shared residential spaces breaks down the inter-group coalitions needed to solve pressing social and institutional problems. Segregation allows for the proliferation of distinct beliefs regarding the nature of social problems, which creates barriers to addressing these challenges. For example, Blacks tend to think Black-white inequality results from structural problems, while whites place more emphasis on behavior (Krivo et al., 2009). Thus, if these differing views make it politically challenging to implement structural solutions to address community violence, this could lead to greater violence in the city, possibly increasing white victimization. Other studies, however, do not find that the harmful effects of segregation spill over into the broader community. For instance, Ananat (2011) finds that segregation increases Black poverty and inequality while at the same time decreasing white poverty and inequality. There is likely considerable heterogeneity across white communities, resulting in varied responses to segregation. Chyn et al. (2021) find that segregation reduces intergenerational mobility for Black children regardless of their parents' relative income rank, while segregation negatively impacts the economic mobility of white children with parents from the bottom half of the income distribution. Given the literature, the burden of the violence associated with segregation remains unresolved.

We examine the violent consequences of segregation within a causal framework. We focus our analysis on the post-Great Migration period, between 1970 and 2010. During this period Blacks left northern and western cities and returned to the south, reversing migration patterns. Moreover, during this period there was a reversal in crime and prison admission rates. By examining this period, we can rule out general trends in crime-related outcomes as confounding

effects. To deal with endogeneity concerns, we follow Ananat (2011) and exploit the arrangement of railroad tracks in the nineteenth century. Because railroad tracks acted as distinct boundaries for neighborhoods, they represent a specific technology for segregation and a useful instrument for identifying a causal relationship between racial segregation and crime-related outcomes. We confirm that northern cities, partitioned into a relatively greater number of neighborhoods by railroads, experienced higher segregation levels. As in Ananat (2011), we also run a series of falsification tests that indicate that prior to the Great Migration, socio-economic factors did not influence railroad tracks' arrangement within an area.

We use the Vital Statistics Multiple Cause of Death files to measure victimization by race. Our regressions results reveal a strong positive relationship between segregation and non-white homicides. We find no evidence that segregation influences white homicide victimization. For non-white homicides, we identify a relatively large positive relationship post-1970; however, the marginal effect decreases after 1990 but remains statistically significant. We find that in 1990, a one-standard-deviation increase in our segregation measure results in 14 additional non-white deaths due to homicide. Our results hold when we include other contemporary local characteristics such as the share of the population being Black, poverty, inequality, labor market characteristics, and educational attainment.[4] As a falsification check, we examine the impact of segregation on suicides and find no effect. Suicides can serve as a falsification test, as there are no direct linkages between suicides and segregation, although there are several plausible indirect channels (Caron, 2021; Xiao et al., 2021).

Our main finding of increased homicides is substantiated with strong evidence of a positive relationship between violent crime and segregation. We find a robust causal relationship between segregation and the murder rate. An important takeaway from our analysis is that general violent crime statistics mask heterogeneity in victimization by race. Due to structural and environmental factors associated with segregation, the brunt of violence is borne by non-white residents. In addition, the marked increase in homicides is driven by city centers, as we find no effect of segregation on murder or violent crime in non-principal cities across MSAs.

Additionally, we explore the impact of segregation on another form of victimization: police-

[4]Descriptive work by Shihadeh and Maume (1997) explores the relationship between segregation and Black homicide victimization, finding a weak but positive relationship. Further analysis reveals that the relationship breaks down when accounting for labor market characteristics, poverty, and educational attainment.

related fatalities. In 2013, the onset of the #BlackLivesMatter social movement, Black males accounted for 26 percent of police-related fatalities, while white males accounted for 37 percent.[5] Black males are 2 to 3 times more likely to be killed by the police and significantly more likely to be killed while unarmed (Plant and Peruche, 2005; Edwards et al., 2018, 2019). However, it is unclear whether racial disparities in police-related fatalities are driven by factors associated with socio-economic conditions, location, and segregation, or police organization and culture. Several studies have linked police killings to policing activity, police organizational structure, and police culture;[6] however, we lack evidence on the environmental factors that may contribute to police killings or racial disparities in police-related fatalities.[7] We attempt to fill this void by exploring the relationship between police killings and segregation. Our results reveal little evidence that segregation influences police-related fatalities of white or non-white civilians post-1970. This is true across various specifications and when we use alternative data sources for police-related fatalities.

We attempt to identify plausible mechanisms through which segregation impacts non-White victimization. First, we find that segregation lowers local government revenue driven by lower property tax revenue which reduces police expenditures per capita in 1990 and 2000. Interestingly, we also find that segregation leads to a shift in funding towards policing. So although less funding is available, the share of funding devoted to public safety is greater in segregated MSAs. However, lower revenues and less overall spending on public safety result in lower levels of police employment per capita. According to our estimates, the inability to adequately employ police officers accounts for approximately 56 percent of non-white homicides attributed to segregation. Moreover, the lack of public safety is reflected in lower arrest rates for violent and property crimes.

Further analysis reveals that segregation decreases school spending on average, and erodes state and federal policy designed to increase school spending for black neighborhoods. We argue that our IV estimates suggest the school funding structure in segregated cities relies more heavily on property tax revenues (as opposed to state revenues), despite the popularity of state finance

[5]There is a stark difference when comparing across age groups: police killings of Black males are comprised mostly of young Black males (244 of 282), while young white males are a smaller share of white male victims (275 of 403). See Mapping Police Violence.

[6]A few examples of recent work in this area: Montiel Olea et al. (2021); Cox et al. (2021); Cunningham and Gillezeau (2019); Cunningham et al. (2021); Weisburst (2019); Ba et al. (2021); Holz et al. (2019); Hoekstra and Sloan (2020); Stashko and Garro (2021).

[7]There are several descriptive papers, such as Jacobs (1998), Ross (2015), and Gray and Parker (2020).

reforms during our sample period. As a whole, the school finance results are important because education has been shown to have sizable crime reducing benefits, even as it relates to murder (Lochner, 2020). Specifically, Weiner et al. (2009) find that court-ordered school desegregation significantly decreases homicide victimization among Black youth by 25 percent. Thus, the de facto segregation that occurred as a result of Black migration, led to a decrease in public expenditures in two categories with important crime fighting benefits, policing and education, ultimately leading to increases in non-White homicides.

At the same time, we provide weak evidence that Black prison admissions rates increase and strong evidence that Black imprisonment rates significantly increase. We find no impact on white admissions or imprisonment rates. This finding is in alignment with research finding that prison rates increased due to changes in policies that led to more punitive approaches to less serious offenses and longer state prison sentences (Raphael and Stoll, 2013), as well as research that ties this desire for a more punitive approach to mainstream societies desire to deal with "inner city" (i.e., Black) crime with incarceration versus social programming (see Hurwitz and Peffley (2005). It is likely that local actors respond to higher violent crime rates with a more punitive criminal justice system (Feigenberg and Miller, 2021), reflected by higher rates of Black incarceration. This is important because the cost of more incarceration is paid by the state and not local municipalities.

Our results support prior theoretical and descriptive work finding a positive association between segregation and violent crime (Peterson and Krivo, 1993; Massey, 1995; Shihadeh and Maume, 1997; Bjerk, 2006; Krivo et al., 2009). However, we find that Black residents bear the burden of the violence associated with segregation, as we find no effect on white victimization. Our work is closely related to Ananat (2011), but we also contribute to the larger literature on the effects of the Great Migration. In addition, our work complements previous studies on the impact of segregation on mobility and inequality. Our results highlight that segregation significantly influences the number of Black males contributing to greater society, as they are likely the victims of violent crime and a more punitive criminal justice system (Wolfers et al., 2015). Lack of Black males impacts family structures and, therefore, intergenerational mobility—resulting in long-lasting and persistent inequality in Black communities.

Our findings also suggest that some of the structural and environmental factors that influence homicides may not affect other forms of victimization, such as police killings. Therefore,

policy recommendations that address segregation and upward mobility may impact violent crime and homicides but plausibly have a negligible effect on police-related fatalities. Similarly, police reform may reduce racial disparities in police killings but have little effect on racial differences in homicide rates.[8] Therefore, separate targeted policies should be implemented to address racial disparities in homicides and police-related fatalities. Lastly, our study illustrates the high cost of segregation and structural racism, where minorities are locked out of opportunity and receive sub-par funding for public amenities, paying for these disadvantages with their lives.

2 Data

We use several data sources to examine the impact of segregation on victimization. We focus our analysis on homicides by implementing a two-stage-least-squares (2SLS) strategy similar to Ananat (2011), though we examine the effects of segregation over several census years, 1970-2010. In addition, we complement our analysis by examining changes in local government finances, particularly public safety and education expenditures, as possible mechanisms for our main findings. Finally, we explore changes in crime and incarceration. Our analysis will focus on northern MSAs identified in Ananat and Washington (2009) and Ananat (2011). Due to the fact that many of our outcomes are not reported consistently over time, we create two samples. The first sample, the county sample, constructs outcome variables of interest from county-level observations aggregated up to the MSA. The second sample focuses on the central city (principal city) of the MSA. We have complete and consistent reporting over time for our primary outcome interest, homicide victimization; this is not the case for other outcomes, which we will discuss below.

2.1 Dissimilarity Index

Our main independent variable of interest is residential segregation. To measure segregation in each census year, we use the commonly employed index of dissimilarity (Duncan and Duncan, 1955; Cutler and Glaeser, 1997). The dissimilarity index captures the degree to which Blacks are located in certain census tracts relative to whites within an MSA. The index provides the share of the Black population that would need to move to other census tracts to be fully integrated within a city or MSA. Thus, we can capture relative separation or integration across all neighborhoods

[8] Cunningham and Gillezeau (2019), Cunningham et al. (2021), and Cox et al. (2021) provide examples of policies and events that influence police-related fatalities while having little to no effect on crime and homicide victimization.

within the city or metropolitan area in our data.[9] The following equation constructs our measure of segregation:

$$\text{Index of dissimilarity} = \frac{1}{2}\sum_{i=1}^{N}\left|\frac{\text{Black}_i}{\text{Black}_{total}} - \frac{\text{non-Black}_i}{\text{non-Black}_{total}}\right| \times 100 \quad (1)$$

where i represents a census tract in an area. We follow Ananat and Washington (2009) to create our measure of segregation by constructing an index of dissimilarity using census counts from the Census of Population and Housing for 1970, 1980, 2000, and 2010. For 1990, we use the index of dissimilarity from Cutler et al. (1999). From census data, we create the index based on counties that belong to each MSA for locations outside of New England. City and towns, rather than counties, identify MSAs in New England. Therefore, we calculate the index of dissimilarity using census tracts in the cities or towns that belong to a particular MSA in New England. In addition, we impute measurements of segregation for MSAs with insufficient or missing census tract data.[10] As documented by Ananat and Washington (2009), not every county or location had defined census tracts before 1990. Therefore, we use a predicted dissimilarity index value using segregation levels in 1990, change in the Black population, poverty rates, graduations rates, and employment rates as independent variables.

Table 1 reports summary statistics from our final samples. The index of dissimilarity reported in row 1 is from the county sample, and row 2 is from the central city sample. For the county sample, we are comforted by the fact that our index of dissimilarity closely matches Ananat and Washington (2009) in the census years that overlap. The average for the index of dissimilarity is roughly .564, with a standard deviation of .166. An important point to note is that segregation has decreased significantly over the last 50 years. In our sample, the least segregated MSA in 1970 was Brockton, MA (index of .393), while the most segregated was Oklahoma City, OK (index of .987). In turn, during the last year of our sample, 2010, the most segregated was Detroit, MI (index of .736), while the least segregated remained Brockton, MA (index of .142). An example of an MSA with an average level of segregation in 1970 is Lorain, OH, and in 2010 Portland, OR. By 2010, the dissimilarity index was 39 percent lower than in 1970, the end of the second wave of the Great

[9] For additional explanation and examples, see https://www.censusscope.org/about_dissimilarity

[10] It is important to note that we use the 1990 definition of MSAs in each census year to construct our measure of segregation.

Migration.

2.2 Victimization

To construct victimization rates between 1970 and 2010, we use mortality information from the Vital Statistics Multiple Cause of Death Files (US DHHS and ICPSR 2007). The Vital Statistic files are publicly available at the ICPSR from 1959 to 1988. We obtain the restricted detail mortality files for 1989 to 2016 directly from the Center for Disease Control and Intervention National Vital Statistics System (NVSS).

2.2.1 Homicides

We construct measures of victimization—homicides by race (deaths per 1,000 civilians).[11] Appendix Figure **??** plots the U.S. fraction of homicides between 1970 and 2010 for white and non-white individuals, respectively. Homicides are reported per 100,000 individuals in each racial group. The figure shows that the homicide trends are similar over time. However, note that the axes differ, with non-white homicides occurring at a much higher rate as a proportion to their population, regularly five times or higher during this period. We can also see this trend in the summary statistics in Table 1, which reports the share of homicides by each census year used in our analysis. In 1970, the number of non-white homicides per 100,00 non-white residents was .22, which is seven times that of white residents. The number of homicides for both groups reached its peak around 1980. However, since then, the number of homicides for both groups has decreased substantially over time; non-white homicides in 2010 were .12 per 100,000 non-white residents. Still, there is a disparity in this number, with the rate of non-white homicides being four times larger in 2010.

2.2.2 Suicides and Police-Related Fatalities

We consider the role segregation plays in suicides by race (deaths per 1,000 civilians). As with the homicide death data, we collect information on suicides from the NVSS. We identify the relevant ICD codes to construct the number of suicides for whites and non-whites in each county between 1970 and 2010.[12] We also consider an additional measure of victimization—police-related

[11] Population counts by race and year are available after 1967 from the Surveillance, Epidemiology, and End Results Program (SEERs).

[12] For the years 2000 and 2010, we use the following ICD-10 codes to classify a death by suicide: X60-X84, Y870, U030. For the years 1980 and 1990, we use the following ICD-9 codes: E960-E969. Finally, for the year 1970, we use the

fatalities (deaths per 100,000 civilians). However, we acknowledge that the Vital Statistics and other government sources of police-related fatalities report roughly half of the deaths recorded in non-governmental sources (Barber et al., 2016; Krieger et al., 2015). Therefore, we supplement our analysis with the Fatal Encounters data.[13] Although Vital Statistics data is an under-count of police killings, national time-series strongly correlate with Fatal Encounters.[14]

2.3 Crime, Police Contact, and Imprisonment

Data on other crime-related outcomes come from the UCR "Offenses Known and Clearance by Arrest". The data on crime includes monthly information on the number of unfounded offenses, actual offenses, offenses cleared by arrest, and offenses cleared involving individuals under 18. We focus on the number of violent offenses (murder and manslaughter, rape, robbery, and assault) reported and the number of homicides reported (murder and manslaughter). In addition, we obtain the number of arrests by offense from the UCR's "Arrests by Age, Sex, and Race" files, which we use to construct arrest rates by race for violent and property crime and drug-related offenses. Due to the infrequent reporting of crime data, we focus our analysis on the principal city in the sample. Many local municipalities fail to produce a consistent series of crime data over time. We overcome missing data using crime counts from the most recent reporting year within five years of a census year. As a result, we have crime statistics for each principal city in every census year or close to every census year; this is not true for every city in an MSA. For robustness, we compare our results when we include cities outside the MSA in our sample as well as when we only use non-principal cities in the MSA.

As shown in Table 1, both total crime and arrests by race increased until the 1990s and then fell dramatically. Between 1990 and 2010, crime decreased by 38 percent. However, the violent crime rate remains relatively flat after 1990. The violent crime rate in our sample dropped by 3 percent after 1990, but the murder rate declined by 18 percent. Similar to crime, we see an increase and then a decrease in arrest rates over time. Since the 1990s, the Black arrest rate fell by over 54 percent. Significant drops in arrests occur for both violent and property crimes (larceny burglary,

following ICD-8 codes: E960-E969.

[13] Fatal Encounters data is publicly available at fatalencounters.org.

[14] Appendix Figure **??** plots the average number of police-related fatalities in a county between 2000 and 2016, the years that are available for both the Vital Statistics files and Fatal Encounters. We restrict the Fatal Encounter data to fatalities when any police force is reported. It is clear that the Vital Statistics data is consistently half of the Fatal Encounter series, but they follow very similar trends over time.

and motor vehicle theft) as well as for drug sales. White arrests also decrease over time, but the decline is much smaller (14 percent) and is driven by property crime arrests.

Information on prison admission and population by race comes from the Vera Institute of Justice. We focus on the county's contribution to the state prison population. The Vera Institute of Justice compiles county counts of individuals sentenced to state prison. The primary source for county prison information is the Bureau of Justice Statistics National Corrections Reporting Program (NCRP), which began in 1983.[15] We primarily focus on prison admission and the prison population. It is important to note that the prison custody population is not available until 1999. The Vera Institute report estimates the prison population from 1983 to 1998. The calculations are based on prison admissions and release information by year and county. Unfortunately, NCRP does not provide admission and release information for the same person. The Vera Institute goes through great effort to check the validity of prison and jail records.[16]

We use prison data to construct race-specific admission rates (admissions per 1,000 civilians) and imprisonment rates (number of individuals in prison per 1,000 civilians). In our sample, both the imprisonment and admission rates peaked in 2000, after the crime drop. By 2010, imprisonment and admission rates decrease for Black civilians but not for whites.

2.4 Local Government Finances and Public Safety

We consider various local finance outcomes as potential mechanisms for any effects resulting from segregation. Data on local government operations, revenue, and expenditures are from the Census of Governments and the Annual Survey of Governments. These sources provide employment counts, which we use to supplement the police employment analysis as well as revenues and expenditures across various categories. We focus our analysis of government finances on total revenue, property tax revenue, and public safety expenditures (police and fire). Total revenue and expenditure values are reported per capita (in the year 2000 dollars). Table 1 shows that over the time of our sample, average MSA spending on police more than doubles from $88 per person to $224 per person. There is also a large increase in fire safety expenditure and a more than 30 percent increase in property tax revenue. To complement our analysis on public safety expenditures, we construct police employment rates (police per 1,000 residents) using employment counts from

[15] Participation in the NCRP is voluntary and therefore does not contain information for every state or every year.
[16] For more information, see Kang-Brown et al. (2018) and Hinds et al. (2018).

the Uniform Crime Reporting's (UCR) Law Enforcement Officers Killed and Assaulted (LEOKA) files. As with police expenditures, the number of officers increases over time. The number of sworn police officers per 1,000 residents increases by 28 percent between 1970 and 2010.

Given that school segregation and school spending has been associated with improvements in educational attainment Jackson (2020); Johnson (2011); Ananat and Washington (2009), and school desegegration and education have been shown to reduce violent crime (Lochner, 2020; Weiner et al., 2009) we also consider the effects of segregation on public school expenditures. We supplement our analysis with school district data from the National Center for Education Statistics (NCES) national school finance survey.[17]

2.5 Final Sample

Our analysis considers two primary samples; the first sample includes 99 MSAs constructed from county-level observation plus 11 counties from New England. MSAs from New England are comprised of towns that are distributed regionally across multiple counties. We take a weighted average across counties to produce a measure of segregation for the New England counties. The Appendix includes an analysis that compares the results when we restrict our sample to non-New England counties (see Appendix Figure B8). Our results are robust to the exclusion of New England from our sample. The second sample focuses on crime-related outcomes from the principal city of 121 MSAs. Given the voluntary nature of UCR reporting, many of the cities within an MSA do not consistently report over time. As a specification check, we conduct an additional analysis including surrounding cities within an MSA. The inclusion of these cities does not change the main findings but decreases the precision of our estimates. These results are also available in the Appendix (see Appendix Figure B4 and Appendix Figure B5).

The summary statistics in Table 1 show that segregation decreases over time. The decrease in segregation occurs in the most and least segregated locations. Appendix Figure A2 shows that segregation decreases over time through the entire distribution. Therefore, we can rule out changes in segregation in any one location driving the analysis. Similarly, we see a decline in non-white homicide victimization rates. The decrease in homicides, however, does not follow changes in crime rates. As noted in literature from criminology and sociology, crime rose through

[17] There are roughly 2,500 districts that we can map to the list of MSAs in our sample. We compute measures for median per-pupil spending for the median school district in an MSA.

the early 1990s and then decreased. By 2010, crime rates were slightly higher than in 1970, but lower property crime rates drive the decrease in crime from the 1990 peak. Relatedly, we see a rise in admissions and imprisonment rates until the year 2000 and then they decrease.

These trends in crime-related outcomes are important for our analysis. Segregation and crime are co-determined. However, we see very different trends in the two series. Segregation mainly decreases due to a decline in overt discrimination, a rise of civil rights litigation in employment and housing, and Black flight as integration expands neighborhood choices (Cutler et al., 1999). However, crime and imprisonment rates increase until the 1990s, and decarceration begins in the late 2000s. Our sample years also capture various federal interventions into local policing. We see an increase in drug arrests for both Black and white residents in the 1990s. The low rates of drug arrests in 1980 reflect the lack of emphasis on drug-related crimes by local police agencies prior to the Reagan Administration escalation of the War on Drugs (Cox and Cunningham, 2021). We also capture changes in sentencing guidelines and more policing in urban communities through the Violent Crime Control and Safe Streets Act (VCCA) of 1994 and the Community Oriented Policing Servicing (COPS) grants. Although crime and homicide rates decrease during this period, the size of police departments is more prominent than in pre-VCCA years, reflected in increasing expenditures devoted toward public safety over time. In 1970, roughly 10 percent of expenditures are devoted to policing; and by 2000 the share increases to 12 percent.

Figure 1 highlights the relationship between segregation and our primary outcome of interest, homicide victimization rates. Panels (a) and (b) plot 1990 homicide rates by race against segregation (measured by the index of dissimilarity in 1990). It is clear that non-white homicide rates are positively correlated with segregation, but there is no relationship with white victimization rates. Panels (c) and (d) show a similar relationship for violent crime and murder, respectively. As the index of dissimilarity increases, violent crime and murder rates also increase. Nonetheless, Appendix Figure A3 shows a decrease in non-white homicides over time across the entire distribution of the index of dissimilarity. While the top and bottom quartiles experience decreases in non-white homicides, white homicides show little movement over time.

Although segregation decreases over time, the relationship between non-white homicides and segregation is quite robust. Appendix Figure A4 plots the relationship between segregation and homicides by decile and over time. In each census year, higher levels of segregation are associ-

ated with higher levels of non-white homicides. This provides suggestive evidence of a consistent positive relationship between segregation and non-white homicides. However, this relationship does not exist for white homicides. In many cases, higher deciles of segregation are associated with lower levels of white homicide. As stated before, segregation and crime-related outcomes are co-determined, and therefore descriptive analyses cannot identify a causal relationship. It is possible that homicides drive white flight and increase measures of segregation. White flight also depletes the local tax base and funding for public safety, resulting in higher homicide and violent crime rates for non-white residents.

3 Methodology

To examine the effect of residential segregation on homicides and other crime-related outcomes, we follow Ananat (2011) and use MSA railroad configurations to capture exogenous variation in the dissimilarity index. Below we describe the instrumental variable, along with our empirical strategy.

3.1 Railroad Division Index

To address the endogeneity concerns, we follow Ananat (2011) and use the configuration of railroad tracks as an instrumental variable. The configuration of railroad tracks within a city or MSA provides distinct geographical markers or boundaries that can be used to define neighborhoods. These distinct boundaries create more easily identified neighborhoods: they act as physical markers for segregated communities. From city maps, Ananat (2011) constructs the railroad division index (RDI), akin to a Herfindahl index, to exploit the configuration of railroad tracks that create subdivisions within a given location. The RDI is defined as:

$$\text{RDI} = 1 - \sum_i \left(\frac{\text{Area}_i}{\text{Area}_{total}} \right)^2 \tag{2}$$

where i defines a neighborhood created by railroad track configurations and Area_{total} is the total area of the city. As the number of neighborhoods created by railroad tracks increases, the RDI gets closer to one. Conversely, if a city has no divisions and a single neighborhood comprises the entire city area, the RDI would be zero. For RDI to serve as a valid instrument, it cannot be co-determined or correlated with unobserved propensities of homicide victimization post-Great

Migration. The majority of railroad tracks were laid prior to WW1; therefore, they are not historically determined with post-migration outcomes. Ananat (2011) shows that RDI is not correlated with pre-Great Migration city characteristics. In addition, in panel (e) of Figure 1, we show that there is no relationship between RDI and 1910 homicide rates.[18] Panel (f) plots the relationship between RDI and 1990 segregation levels. RDI is highly correlated with post-Great Migration segregation. Put another way, we confirm that the additional neighborhoods created by railroads are strongly correlated with the dissimilarity index (i.e., segregation).[19]

3.2 Two-Stage Least Squares

We start with the following naive ordinary least squares (OLS) model:

$$Y_i = \beta_0 + \beta_1 D_i + \beta_2 X_i + \eta_i \tag{3}$$

where Y_i is the outcome variable of interest in MSA i. The independent variable of interest, D_i, is the index of dissimilarity. The vector X_i includes the total length of railroad tracks in MSA i. Additional specification checks will control for percent of the population being Black, manufacturing share, educational attainment, and measures of well-being (inequality and poverty). Our main specification includes minimal controls, because segregation is co-determined and influences many of the control variables of interest. For example, a high degree of segregation is a consequence of white flight and Black migration patterns. Therefore, controlling for percent of Black population will understate the impact of segregation on crime-related outcomes. Thus, our main specification focuses only on segregation, and then we supplement our analysis by including additional covariates (See Table A2).

Given that omitted MSA characteristics can confound our measure of segregation, we implement a two-stage least squares approach to overcome this endogeneity concern. We estimate the following first-stage regression:

$$D_i = \alpha_0 + \alpha_1 RDI_i + \alpha_2 X_i + \varepsilon_i \tag{4}$$

[18] Data comes from Table 4 of the 1910 Vital Statistics, which is made available by the CDC. Violent deaths by race is only available for a handful of cities in 1910.

[19] In panel (d) of Figure A3 and panels (k)-(o) of Figure A4, we show that the relationship between segregation and RDI is robust over time.

where RDI_i allows us to capture and use exogenous variation in the dissimilarity index independent of any confounding effects. Table 2 shows the first-stage estimates for the relationship between RDI, segregation, and 1910 homicide rates. RDI is positively associated with the index of dissimilarity and has no relationship with 1910 homicide rates. The effective F-statistic for the first-stage regression in 1990 is 15.45, larger than the traditional threshold of 10. The estimation of equation (4) provides exogenous predictions, $\widehat{RDI_i}$, that can be used in the second stage

$$Y_i = \beta_0 + \beta_1 \widehat{D_i} + \beta_2 X_i + \eta_i. \tag{5}$$

Appendix Table A1 reproduces the Ananat (2011) falsification tests, where we find no effect of RDI on a host of 1910 and 1920 characteristics. These include demographics and measures of economic well-being such as population, ethnic dissimilarity, Black population, literacy rates, and labor force participation.

4 Results

We follow Ananat and Washington (2009) and show our results from equation (5) by decade, 1970–2010. We standardize our regression results for easy comparison across years and outcomes; therefore, our estimates are reported in standard deviations. Panel (a) of Figure 2 reports our first-stage estimates by year. The solid circle marker reports our estimate of α_1, and the line with an open circle reports the 95 percent confidence intervals constructed from heteroskedastic robust standard errors. Similar to Ananat and Washington (2009), RDI has a strong and positive relationship with the index of dissimilarity over multiple census years. The relationship was the weakest in 1970, but the point estimates increase over time. The weaker relationship in 1970 is likely a byproduct of estimating the index of dissimilarity for several MSAs due to incomplete or missing census tract identifiers. Ananat and Washington (2009) also report a smaller relationship between the index of dissimilarity and segregation in 1970.

4.1 Segregation and Victimization

We focus on homicide victimization due to the fact that traditional violent crime and murder rates mask the heterogeneity in victimization by race. As highlighted previously, segregation has been linked to violent crime (Peterson and Krivo, 1993; Massey, 1995; Shihadeh and Maume, 1997;

Bjerk, 2006). However, it is not clear who actually bears the cost. It is possible that segregation only impacts Black residents. Ananat (2011) finds that white inequality and poverty rates decrease with segregation, while Black inequality and poverty rates are positively correlated with segregation. Relatedly, Derenoncourt (2021) uncovers a positive relationship between murder and the Great Migration. However, it is unclear if this relationship exists due to migration patterns, segregation, or racial animus in response to new arrivals.

Panel (b) of Figure 2 shows the effect of segregation on victimization rates by race. For whites, there is no effect of segregation on homicide victimization rates: the coefficients are near zero and statistically insignificant. This is not surprising, considering the mixed evidence in the literature. For instance, using the National Neighborhood Crime Survey, Krivo et al. (2009) finds a positive relationship between segregation and violent crimes in white neighborhoods. However, Light and Thomas (2019) uncovers a negative relationship using a weighted least squares model with fixed effects.[20] Using OLS, in Table 4 we find that segregation and white victimization is statistically significant and positively correlated in 1970 and negatively correlated in 1990 and 2010. However, 2SLS estimates are statistically indistinguishable from zero in every census year and negative in 4 out of 5 census years.

For non-whites, we see a relatively large and statistically significant effect of segregation and homicides in 1970. The coefficients remain large and statistically significant but eventually decrease after 1990. Nonetheless, in every census year, the estimated impact of segregation on non-white victimization is positive and statistically significant. Moreover, 2SLS estimates in Table 3 are larger than OLS estimates, implying that previous descriptive work on the impact of segregation on Black homicides underestimated the true effect. Using the coefficient in 1990, we find that a one-standard-deviation increase (.137) in the dissimilarity index increases non-white homicides by .98 standard deviations, or 86 percent. In terms of actual lives, a one-standard-deviation decrease in the dissimilarity index would save 14 non-white lives; if an MSA moved from the top quartile (.69) to the bottom quartile (.46) of the index of dissimilarity, 23 non-white homicides would have been averted.

[20] Light and Thomas (2019) followed Cutler and Glaeser (1997) and used the number of municipal governments and the share of local revenue as instruments in a robustness check for their finding of decreased white victimization. They did not do this for Black victimization. Also, the relationship is only statistically significant when using population weights, although point estimates are similar.

Appendix Figure B1 plots the impact of segregation on the racial difference in homicides. We follow Cox and Cunningham (2021) and stack the data so that each MSA is characterized by their white or non-white homicide rates. We then regress homicides on the level of segregation and an interaction term that captures the impact of segregation on racial differences in homicides.[21] As expected, segregation increases the racial gap in homicides. However, the relationship weakens over time as both homicides and segregation decrease. Yet, the estimates are positive and statistically significant in every census year. Moreover, the change in the racial gap in homicides reflects the estimated effects from panel (b) of Figure 2, as the impact of segregation on non-whites decreases over time.

The size of our 2SLS estimates implies a relatively high cost of segregation for non-white residents. As an additional check on the validity of our estimates, we examine the impact of segregation on suicide deaths. We do this because there are racial differences in suicide rates; white suicide rates are higher than non-white rates. In our analysis, suicides can serve as a falsification test, as there are no direct linkages between suicides and segregation. However, one can fathom that economic deprivation and increased homicides can have deleterious effects on mental health and lead to higher suicide rates. Ananat (2011) finds that segregation decreases poverty and income inequality for whites but increases poverty and inequality for Blacks. In this case, it would be possible that suicides are positively related to segregation for non-whites and negatively related to segregation for whites. In Figure B2 of the Appendix, we find no relationship between segregation and suicides for non-white civilians; this is not surprising given that suicide rates are lower among Blacks relative to whites.

In Table A2, we test the robustness of our results in panel (b) of Figure 2. We follow Ananat (2011) and control for local characteristics that are correlates of urban decay and crime. We first examine how the racial composition of the population influences our results. Our IV is able to disentangle the effects of segregation from migration patterns; *all things equal*, sorting in northern MSAs will vary by pre-Migration configuration of neighborhoods, defined by the placement of railroad tracks. Appendix Figure B3 shows that segregation is positively related to the share of the population being Black. Therefore, it is reasonable to assume that our IV is positively corre-

[21] To estimate the racial differential in homicide, we estimate the following equation: $Y_{i,r} = \beta_0 + \beta_1 Black_r + \beta_1 \widehat{D_i} + \beta_1 (D_i \times \widehat{Black_r}) + \beta_2 X_{i,r} + \eta_i$. The binary variable, $Black_r$, is equal to one when referencing non-white homicides and zero for white homicides. We instrument for D_i using the RDI_i and $(D_i \times Black_r)$ with $RDI_i \times Black_r$.

lated with the share of the population being Black, which is the case in panel (b). However, this relationship functions entirely through segregation. The relationship between our IV and Black population shares goes away when we account for the index of dissimilarity (Appendix Table A4). However, the relationship between our IV and segregation still persists when we account for Black population share (Appendix Table A4). The second row in Table A2 shows that our results are robust to controlling for the total population and the non-white population share. In addition, our results are robust to accounting for local educational attainment and labor force participation. Lastly, we check to see if our results for non-white homicides are driven by poverty rates or inequality. We find no evidence that the inclusion of these measures of well-being impact our results.[22]

With regards to other measures of victimization, panel (c) of Figure 2 reports 2SLS results for the impact of segregation on violent crime and murder per 1,000 residents. For both violent crime and murder, the coefficients for each decade are positive. The relationship between segregation and victimization is stronger for murder than for violent crime, which includes murder, rape, robbery, and assaults. According to the point estimate in 1990, if our measure of segregation increases by one standard deviation, violent crime increases by .436 standard deviations, or about eight violent crimes per 1,000 residents. Our sample's average number of violent crimes in 1990 was 27 per 1,000 residents, implying a 30 percent increase in violent crime. Our result implies that going from the top segregation quartile (.67) to the bottom quartile (.46) decreases violent crime by roughly 44 percent, averting 12 violent crimes per 1,000 residents.

The impact of segregation on violent crime, although positive, is statistically significant (95% CI) in 1990 and 2010; it is marginally statistically significant in 2000. In contrast, the relationship between segregation and murder is positive and statistically significant in 1980, 2000, and 2010; it is marginally statistically significant in 1990. According to our estimates for 1990, a standard-deviation increase in segregation increases the murder rate by .41 standard deviations or .045 murders per 1,000 residents (roughly 41 percent).

Appendix Figure B5 replicates the analysis presented in panel (c) of Figure 2 when we include all cities in a given MSA that report crimes in the census year (or a year close to the census year).[23]

[22] Our results bolsters Peterson and Krivo (1993), providing further evidence that residential segregation is the most important explanatory variable for understanding Black homicides.

[23] Due to inconsistent reporting, many local municipalities fail to produce a consistent series of crime data over time.

For violent crime, point estimates when using all cities in an MSA are positive but not statistically significant in any census year. However, when we focus on the relationship between the murder rate and segregation, we find a robust, statistically significant relationship. For the principal city analysis, the coefficient for segregation is positive and statistically significant at the 90 percent or 95 percent confidence interval after 1970, while estimates are marginally statistically significant in every census year for the full MSA analysis. This is likely due to the high concentration of homicides in Black communities.

Panel (d) of Figure 2 examines the impact of segregation on police killings of civilians by race. There is little to no effect of segregation on police killings of white civilians—point estimates are close to zero in every census year except for 2010 and never statistically significant. For Black deaths, the coefficients are positive, except in 1970, but only statistically significant in 1980. Given the rarity of police killings of civilians in any given location, it is not surprising that we find weak evidence that segregation influences police killings of civilians.

We test several specifications for the impact of segregation on police killings of civilians in Appendix Table A6. Using counts of non-white deaths instead of mortality rates produces positive but statistically insignificant coefficients. Also, we find no evidence that segregation influences police killings when using data from Fatal Encounters. It is important to note that segregation is positively correlated with police killings and that the coefficients are statistically significant when not accounting for endogeneity. Given these results, we find little evidence that segregation influences police killings of civilians and that not accounting for endogeneity could lead to erroneous conclusions.[24]

4.2 Mechanisms: Public Spending for Safety and Education

We find a positive and robust causal relationship between crime and non-white homicide rates. These findings align with Ananat (2011), which links segregation to lower educational attainment and higher poverty rates for Black residents. Relatedly, Cutler and Glaeser (1997) link segregation to lower earnings and higher rates of single motherhood. Derenoncourt (2021) shows that local responses to the influx of Black migrants and persistent segregation limited economic

We overcome missing data using crime counts from the most recent reporting year within five years of a census year.

[24] Due to heterogeneity in reporting across jurisdiction, it is generally not recommended to conduct cross-location analysis using Vital Statistics data on police-related fatalities. See Loftin et al. (2003).

mobility for Black residents in northern cities. Lastly, Boustan (2010) reports lower property values in northern cities that experienced Black migration and white flight. Lower income and property values should result in less revenue and, therefore, fewer resources or public goods. However, Alesina et al. (1999) find that diverse cities and metros spend more on police but less on fire, education, and sanitation.

We explore how government finances are influenced by segregation in Figure 3. Panel (a) focuses on revenue per capita, while panel (b) reports the impact of segregation on public safety expenditures. According to our results, segregation is negatively related to general revenue and property tax revenue per capita. Lower general revenue is consistent with lower housing values found in Boustan (2010). When accounting for endogeneity, Boustan (2010) finds that the value of owner-occupied homes decreased between 1950-1970 and attributes the decline in property value to white flight as Blacks often paid more for housing units (Cutler et al., 1999) and migrated to locations with high labor demand and housing costs. However, according to our results, by 1970, highly segregated cities generated less revenue per capita, and the effects of segregation are stable across census years. According to panel (a), in 1990 a one-standard-deviation increase in the dissimilarity index decreases general revenues by $655 per resident or .86 standard deviations. Half of the decrease in general revenue is due to lack of revenue from property taxes. A one-standard-deviation increase in segregation decreases property tax revenue by $272 to $370 per resident between 1970 and 2010.

Lower government revenues suggest that a likely mechanism driving our results is lack of resources, in particular lack of the public goods that deter crime such as public safety and education expenditures. Panel (b) highlights that segregation is also negatively related to police expenditures and fire safety spending per capita. Initially, there is little to no difference in spending on police associated with segregation. Still, by 1990 and 2000, there exists a robust negative relationship between the dissimilarity index and public safety expenditures.[25] According to our 1990 estimate, a one-standard-deviation increase in the dissimilarity index decreases spending on police by .64 standard deviations or $66 per resident. We find a similar relationship between fire safety and segregation. Less spending on public safety suggests that higher homicide rates are

[25] Although the relationship is not statistically significant in 2010, when we include all cities in the MSA, we find a marginally statistically relationship between police expenditures and segregation (see Appendix Figure B5d).

driven by a lack of the resources to deter violent crime.

Moreover, residential segregation across the broader housing market is more responsible for school segregation than sorting across schools within a district (Reardon 2016). Given the recent explosion of causal evidence that school spending improves education and labor market outcomes (Jackson Johnson Perisco; Baron; Lafortune and Rothstein), and that education reduces violent crime and murder (Lochner, 2020; Weiner et al., 2009), heterogeneity in school resources may explain differences in victimization outcomes within the same MSA. Complicating the study of racial differences in school resources is the proliferation of state finance reforms taking place during the sample period, aimed at reducing gaps between the richest and poorest districts. To that end we argue for the segregation IV operating independent of state finance reforms. Looking beneath the surface of MSA-level results, we find evidence that local segregation counteracts state efforts to spend more on schooling black residents.

The first school finance results are obtained by estimating equation 5 with MSA median levels of expenditures, state and property tax revenues taken as the outcome. The OLS estimates in Table A7 show a time pattern consistent with the idea that state finance reforms increased observable levels of spending in segregated MSAs. Prior to the first wave of reforms the OLS coefficient is negative in 1970, then turns positive and increases steadily for the next three decades. This bias is driven by the fact that nearly 60% of the districts in our sample have been exposed to reforms by the year 2000. The historical timing of the IV captures variation in segregation that is related to school financing, but orthogonal to reform-induced variation in district budgets. The 2SLS results in Table A7 show that median school spending is in fact decreasing in segregation. Further, the 2SLS results in Appendix Tables A8 and A9 reveal the funding structure of segregated MSAs relies more heavily on local property taxes than state revenues.

With headline victimization results that reveal startling differences in outcomes for blacks and whites in the same housing market, we want to credibly identify how segregation affects school financing for black households relative to whites in the same MSA. Using contiguous tract-level demographic data matched to 1990 school district boundaries, we model within-MSA differences using MSA fixed effects μ_i in the regression

$$Y_{ji} = \alpha_0 + \alpha_1 MarjorityBlack_j + \alpha_2 MarjorityBlack_j \times \widehat{D_i} + \alpha_3 X_{ji} + \mu_i + \epsilon_{ji}. \tag{6}$$

Y_{ji} is per-pupil spending of the school district geographically zoned to census tract j in MSA i. Majority black neighborhoods are defined as census tracts where blacks comprise the largest racial share. With over 16,000 census tracts divided over 106 MSAs, this measure gives us rich local variation in school financing. We once again instrument for endogenous segregation using the railroad index, and by employing MSA fixed effects we are able to credibly recover heterogeneity in school spending for black neighborhoods: $\hat{\alpha}_1$ and $\hat{\alpha}_2$.

Evidence of within-MSA heterogeneity for black neighborhoods is presented in Table A10[26], where we estimate our original model then incorporate MSA fixed effects. The positive coefficient for majority black, $\hat{\alpha}_1$, implies that black neighborhoods receive higher relative school spending in the absence of segregation. The byproduct of a reform environment targeting historically underfunded schools is the implicit increase in expenditures for black schools districts. For local segregation to be considered innocuous in this process, the interaction term coefficient $\hat{\alpha}_2$ must be a precisely estimated null effect. The fixed effect estimates in Table A10 show a sharp, precise decline in school spending for black neighborhoods as segregation rises.

It is beyond the scope of this paper to tie school spending to non-white victimization directly. Notwithstanding, there is reason to believe that the cumulative effects of lessor funded schools for blacks create a path to violent crime and victimization through a cycle of poverty. Figures B9 and B10 descriptively highlight that poverty rates for blacks increase with segregation across all years, while white poverty rates largely decline with segregation. Taking estimates from the literature, we can compute the effect of reduced spending on poverty[27]. In 1970, a 1 standard deviation increase in segregation decreased school expenditures 0.44×\$1,467 = \$645 or 11.4% from the mean (Table A7). A similar number is obtained for 2010, in which segregation decreased school spending by 0.43×\$3,030 = \$1,303 or 10.6% from the mean. If we take estimates from Jackson, Johnson and Persico, poverty rates are between 3.3 and 3.7 percentage points higher on average due to foregone spending on education. Our results suggest the resource deficits in black neighborhoods lead to even higher black poverty in segregated areas, which has a direct tie to violent crime and homicides.

[26] We have consistent neighborhood race data beginning in 1970

[27] The Jackson, Johnson and Persico paper finds that a 10% increase in school spending decreases adulthood poverty incidence by 3.2 percentage points

4.3 Mechanisms:Policing, Arrests and Imprisonment

The lack of public provisions may reflect different preferences in using resources across demographic groups or the inability to coordinate the use of public goods effectively (Ananat and Washington, 2009; Alesina et al., 1999). In conjunction with limited resources, this coordination problem could explain the high rates of homicides for non-white residents. Indeed, our public spending results highlight the contrast between racial segregation and income segregation or inequality. Seminal papers such as Alesina et al. (1999) present evidence that income diverse communities spend more per resident than more homogeneous populations, and Boustan et al. (2013) link income inequality to higher levels of expenditure per capita on police and fire safety. Moreover, Tabellini (2020) finds that the first wave of the Great Migration had little impact on the share of spending devoted toward policing, education, sanitation, and fire, despite decreases in public spending and tax revenues.

Chalfin et al. (2020) find that more resourced cities benefit from additional police. According to their finding, a one-percent increase in law enforcement reduces Black homicides by 1.1 to 2.5 percent. Panel (c) of Appendix Figure B5 shows that segregation is negatively correlated with police department size. We find that in 1990, a one-standard-deviation increase in segregation decreases the number of police by .39 standard deviations or .41 per 1,000 residents. Our estimates imply that a one-percent decrease in the dissimilarity index correlates with a .8 percent increase in police per 1,000 residents. Using estimates from Chalfin et al. (2020), this would lead to a .9 to 2 percent decrease in non-white homicides. Comparing this estimate to the results in panel (b) of Figure 2, the lack of investment in policing explains approximately 56% of non-white homicides attributed to segregation.

Although we find significant decreases in police expenditures per capita, these decreases are only statistically significant in two of the five census years we examine. The evidence is much stronger, however, for police presence. Segregated MSAs have fewer officers. Point estimates are negative in every census year we examine, and statistically significant in three of the five census years. Even so, according to Appendix Figure B6, segregation is positively correlated with the share of expenditures devoted to policing. Therefore, segregated cities with less revenue increase their share of spending on public safety. This is counter to Tabellini (2020) finding that the first

wave of the Great Migration did not cause a change in spending shares toward policing. Instead, our result aligns with the finding in Alesina et al. (1999) that more diverse cities spend more (as a share) on public safety. But even though segregated cities reallocate funding toward police, they cannot overcome losses in property tax revenue to adequately provide police and fire safety. Moreover, segregated cities disinvest in other public goods in order to provide additional public safety, which may lead to additional increases in crime, therefore, putting additional strain on public safety.

To complement our crime-related outcomes, we also examine the effect of segregation on arrests. Figure B7 plots the point estimates for arrests by type of crime. Panel (a) shows the results for violent crime arrests. The effect is negative for each census year for both white and Black arrests, with the largest effect in 1990. We also see statistically significant decreases in violent crime arrests in 1980 and 2010 (for whites). For example, a one-standard-deviation increase in the dissimilarity index decreases violent crime arrests by .56 standard deviations or 1.7 arrests per 1,000 residents for white residents and .68 standard deviations or six arrests per 1,000 residents for Black residents. Panel (b) of Figure B7 presents the results for property crime arrests. Here, we see a similar decrease across all census years for both white and Black arrests, though the only statistically significant effect is for Black drug arrests in 1980. Panel (c) plots the RDI estimates when drug possession arrests are the outcome. Here, the results are similar to the violent crime findings; each census year has a negative effect, with statistically significant decreases for white and Black arrests in 1980 and 1990. Lastly, panel (d) reports the results for drug sale arrests. Although none of the point estimates are statistically significant, starting in 1990 all point estimates are negative. Together, the arrest results coincide with the previous findings on crime and public expenditures for community protection. Fewer resources result in additional crimes (Chalfin et al., 2020) and fewer arrests. Figure 3 indicates that increased residential segregation decreases police expenditures, and Figure B5 suggests a decrease in the number of officers. Together, these constraints may inhibit highly segregated areas from deterring violent crime and arresting perpetrators.

To complement our arrest analysis, we also examine whether segregation affects criminal justice-related outcomes. Figure 5 panel (a) plots the point estimates for admissions rates, and panel (b) plots imprisonment rates. Both figures present results by race. The results indicate no effect of segregation on admission rates. However, we do see a disparate impact on imprisonment

rates. For each census year between 1980 and 2010, we see an increase in Black imprisonment rates—there is no such effect for whites. For instance, in 1990, a one-standard-deviation increase in the dissimilarity index increases the Black imprisonment rate by 30 percent, amounting to about 167 additional Black civilians in prison.

Given the isolation from economic opportunity, the disinvestment in public resources, and the increase in non-white homicides (which, because of criminal justice reforms such as "truth in sentencing" laws over this period, was accompanied by an increase in time served) in racially segregated communities, this finding is not surprising. Prior research finds that the increase in incarceration rates during this period were largely due to changes in public policy rather than other common explanations such as changes in criminal behavior, deinstitutionalization of the mentally ill, or the crack-cocaine epidemic (Raphael and Stoll, 2013; Neal and Rick, 2016). However, racial residential segregation likely leads to a bifurcation along racial lines of the public's beliefs about the root causes of crime, especially urban crime: whites tend to believe that crime results from cultural deficits, while Blacks believe that crime is due to structural reasons (e.g., poor employment and educational opportunities) (Piquero and Brame, 2008). In addition, Feigenberg and Miller (2021) find that criminal justice systems are more punitive in more diverse communities or when there is more political competition along racial lines. Hurwitz and Peffley (2005) find that whites prefer to use incarceration over social programming for crime committed by those in the "inner city" (i.e., Black). However, it is not clear if a more punitive criminal justice system is rooted solely in divergent views of the underlying causes of crime; Forman Jr (2017), provides anecdotal evidence in which Black-elected officials in majority Black municipalities chose more punitive approaches to combat violent crime.

5 Conclusion

In this paper, we examine the relationship between residential racial segregation and violent crime using a causal inference approach. Overall, we find evidence that residential racial segregation leads to an increase in homicides of non-white residents. This is largely consistent with prior literature establishing a positive association between residential segregation and violent crime (Peterson and Krivo, 1993; Massey, 1995; Shihadeh and Maume, 1997; Bjerk, 2006). While we find no effect of residential segregation on white homicides, it may be that racial residential segregation

acts as an economic protectionist policy for white Americans, ensuring for them lower levels of economic uncertainty (Massey, 1995; Light and Thomas, 2019). Nonetheless, we uncover a causal link between segregation and non-white homicide victimization, providing new evidence that the detrimental effects of segregation impose a high and lasting cost on Black lives.

Though our research is closely related to Derenoncourt (2021), a few key differences distinguish our analysis. First, the featured phenomenon driving our results is segregation, not Black migration. As shown in Appendix Table A2, we are able to isolate the effects of segregation from Black Migration patterns. Secondly, our analysis focuses on MSAs and central cities rather than commuting zones. In particular, our main spending results come from the central city sample, though we find similar results in the MSA sample. We find that the geographic unit of analysis is an important distinction, as a large share of Black migrants moved to city centers. Therefore, the division of neighborhoods in these locations matter for sorting: thus our emphasis on segregation.

In addition, our sample focuses on select northern cities for which we have the railroad division index. Although Ananat and Washington (2009) found similar OLS results for northern MSAs when including non-RDI locations,[28] our results lack external validity. Therefore, the reader should interpret our results with this caveat in mind.[29] Nonetheless, we find deleterious effects of de facto segregation on communities of color. Segregation is strongly associated with higher homicide rates of non-white civilians and higher imprisonment rates. The lack of provision of public goods is a likely mechanism, reflected in less spending on public expenditures that deter crime such as public safety and education. Our results not only highlight the role of local institutions, but also complement findings by Chyn et al. (2021) linking segregation to intergenerational mobility, or the lack thereof.

Unfortunately, the rate of homicide victimization is still a concern for Black Americans. Likely channels for partially explaining elevated homicide rates are the loss in local revenue, depleted tax base, and decreased public expenditures associated with segregation, resulting in insufficient public goods. This is consistent with theories and research that find that racial residential segregation allows communities to be targets for disinvestment by governments and businesses (Massey, 1995;

[28] See Table 4 of Ananat and Washington (2009).

[29] For instance, Bayer et al. (2021) find higher levels of Black-white neighborhood inequality in northern and midwestern MSAs, the MSAs in our sample. It is plausible, that our findings may weaken in southern MSAs with lower levels of neighborhood inequality.

Alesina et al., 1999; Ananat and Washington, 2009; Ejdemyr et al., 2018; Gordon, 2020), which is reflected in fewer police officers, less spending on public safety, and lower school expenditures per pupil. Ultimately, segregation undermines pluralist politics through the formation of coalitions across racial lines, allowing politicians to make budget cuts in Black neighborhoods either because they expect minimal political fallout or to maintain the privileged status of white communities (Krivo et al., 2009; Ananat and Washington, 2009).

Furthermore, segregation and white flight disincentivize collective action where all groups have a vested interest in fighting crime in Black communities (Massey, 1995; Shihadeh and Maume, 1997; Krivo et al., 2009; Trounstine, 2016). Massey (1995) argues, "[i]f [B]lacks are segregated across municipal as well as neighborhood boundaries [W]hites minimize their exposure to crime and other social problems, but to a large extent they can also avoid paying the costs" (p. 1227). Thus, Blacks not only experience social and physical isolation, but also fiscal isolation. However, it would be ill-advised to recommend policies that only increase funding toward public safety. Residential segregation is multifaceted and a result of a deep-rooted and complex racial history where structural racism has trapped Black Americans in a permanent underclass. Therefore, targeted policies that improve socio-economic conditions and increase opportunities for upward mobility would have long-lasting and persistent effects, decreasing non-white homicides in the long-run.

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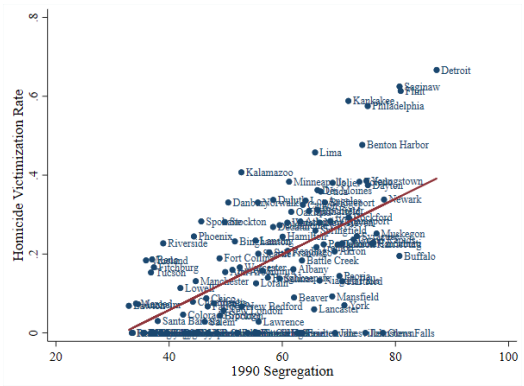
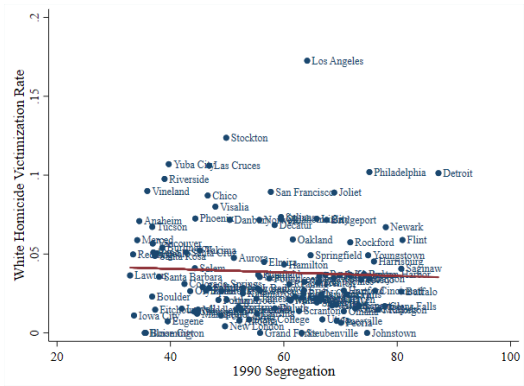
Figure 1: Relationship Between Segregation, Crime, and RDI

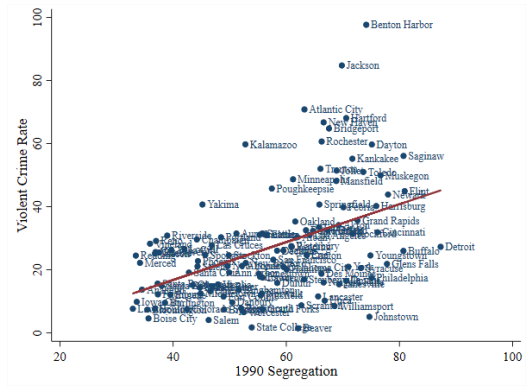
(a) Non-white Homicides



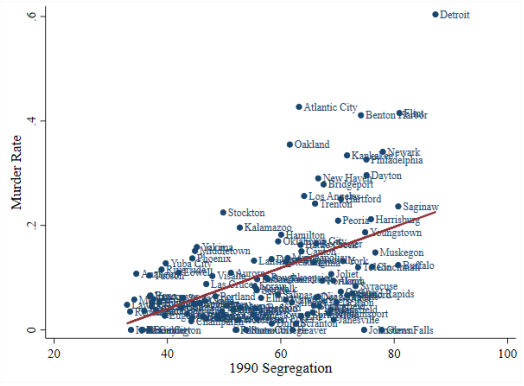
(b) White Homicides



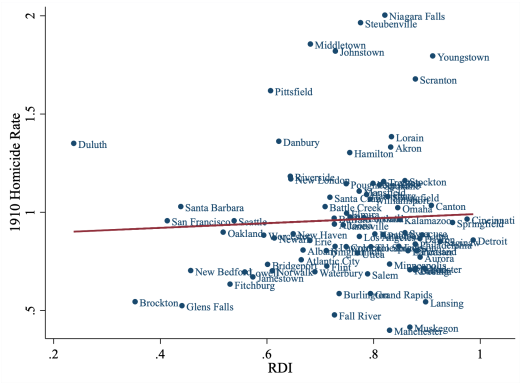
(c) Violent Crime



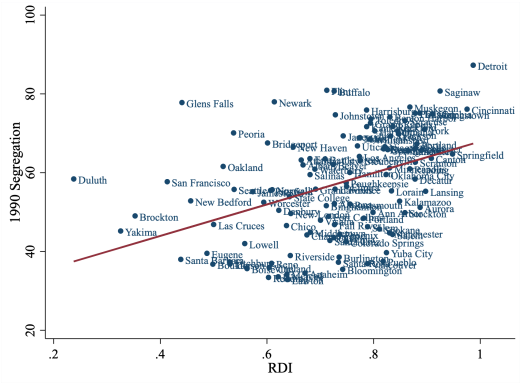
(d) Murder



(e) RDI & Homicides, 1910



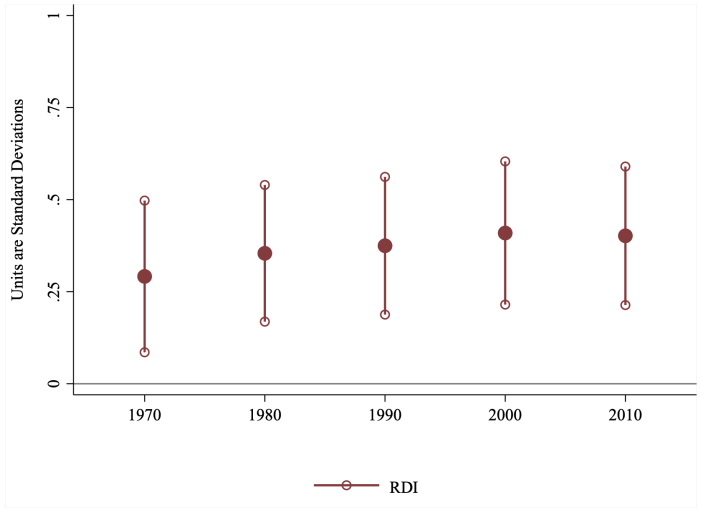
(f) RDI & Segregation



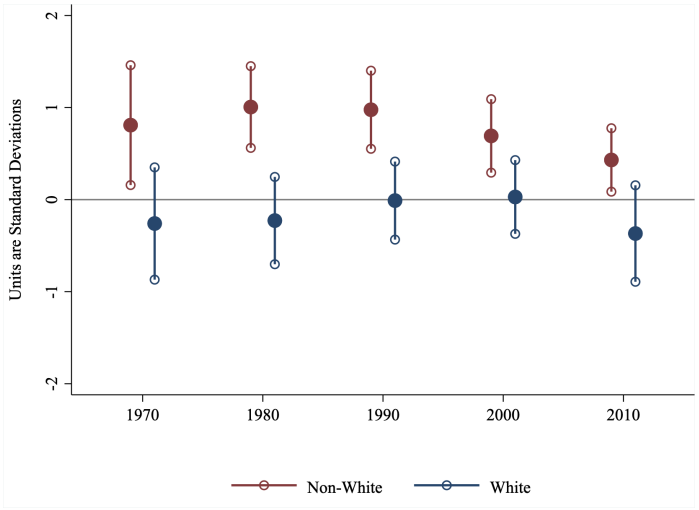
Notes: Figure panels (a) - (d) show the relationship between crime and segregation in 1990. Panel (e) shows the relationship between RDI and the homicide rate in 1910. The slope of the line is 0.12 (0.25). Panel (f) plots the relationship between RDI and segregation in 1990. Segregation is measured by the index of dissimilarity.

Figure 2: Two-Stage Least Squares Results: Segregation, RDI, and Victimization

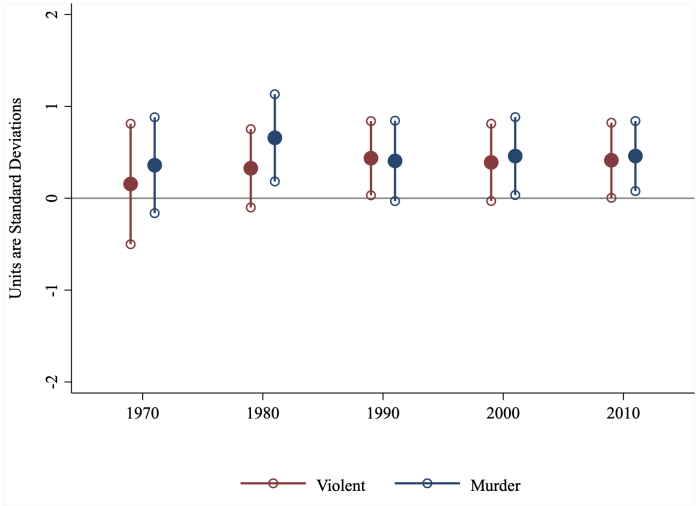
(a) First Stage, RDI & Segregation



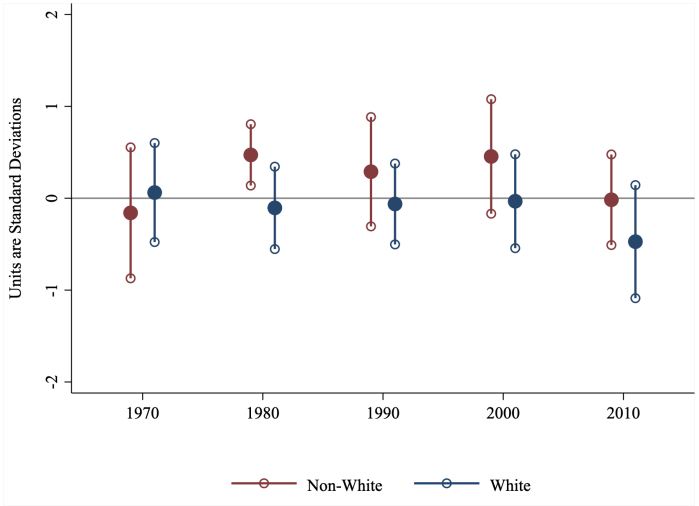
(b) Homicides Per 1,000 Residents



(c) Violent Crime Per 1,000 Residents



(d) Police Killings Per 100,000 Residents

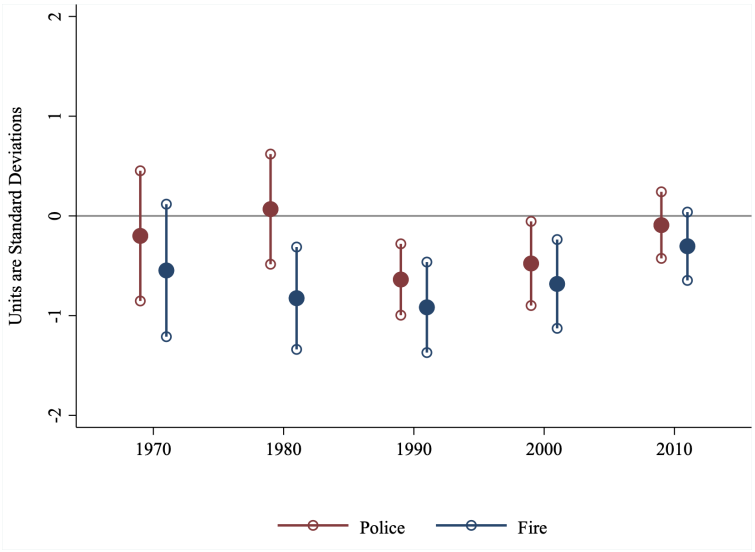


Notes: Figure plots regression estimates from a two-stage least squares analysis of the impact of segregation on crime and victimization by census year. Panel (a) plots estimates from the first-stage regression of the RDI on the index of dissimilarity by census year. Confidence intervals are constructed from heteroskedastic robust standard errors.

Figure 3: Two-Stage Least Squares Results: Segregation, Government Finances, and RDI

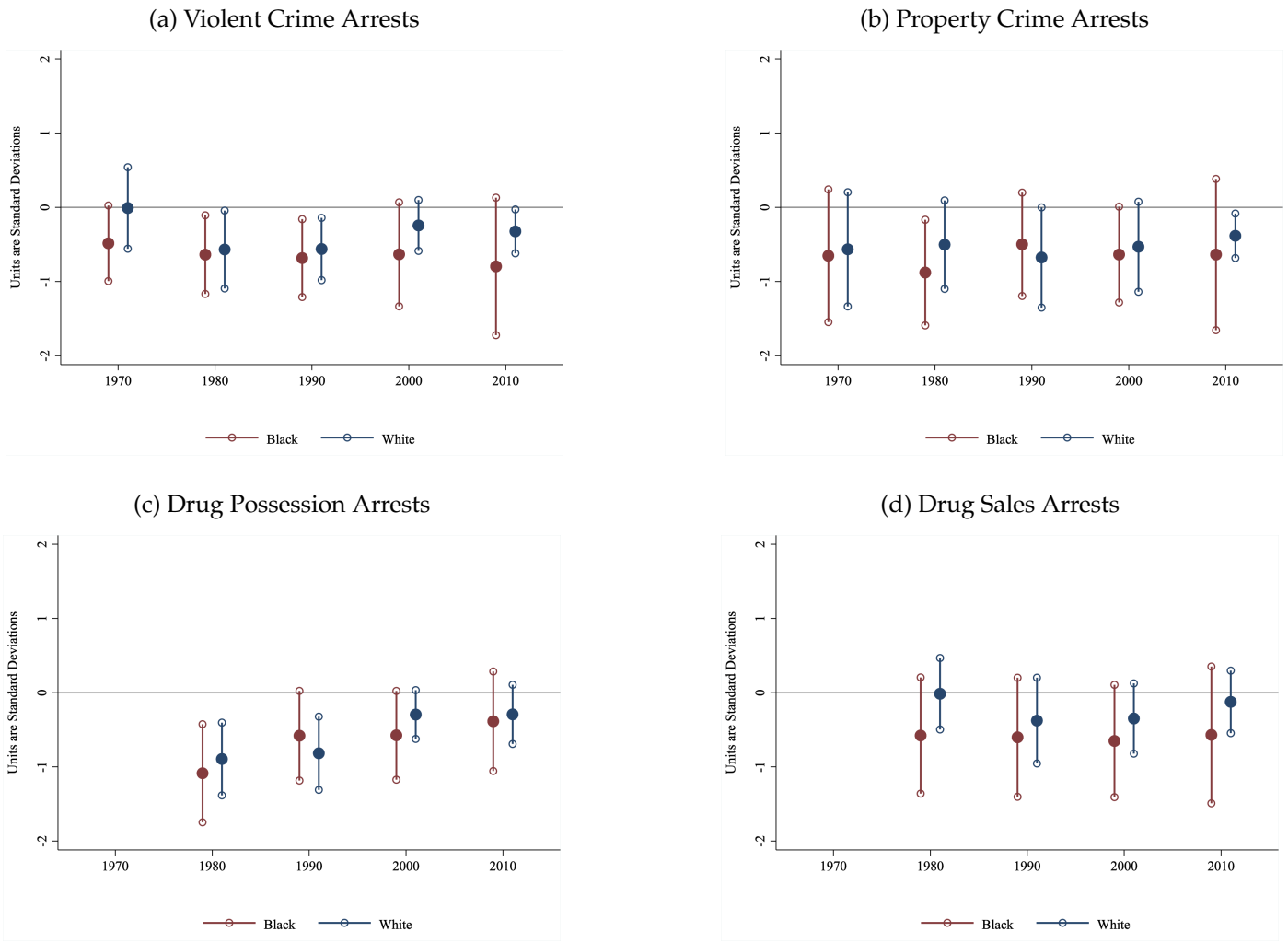
(a) Revenue Per Capita

(b) Expenditures Per Capita



Notes: Figure plots regression estimates from a two-stage least squares analysis of the impact of segregation on local government expenditures by census year. Confidence intervals are constructed from heteroskedastic robust standard errors.

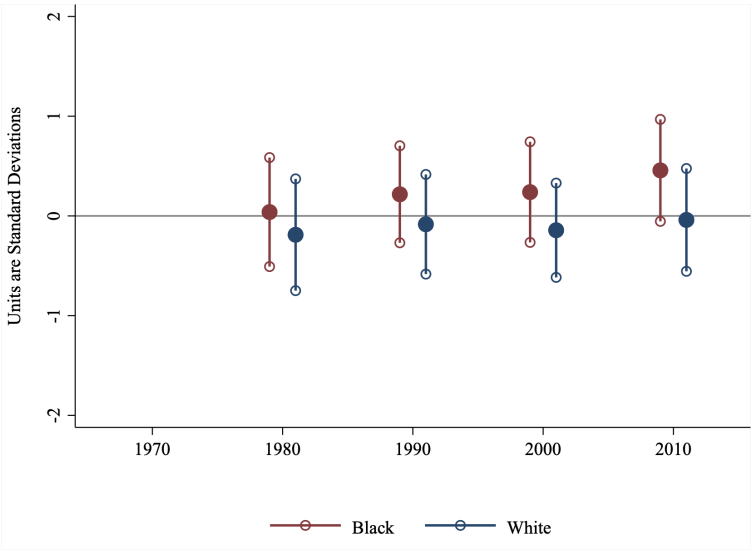
Figure 4: Two-Stage Least Squares Results: Segregation, Arrests, and RDI



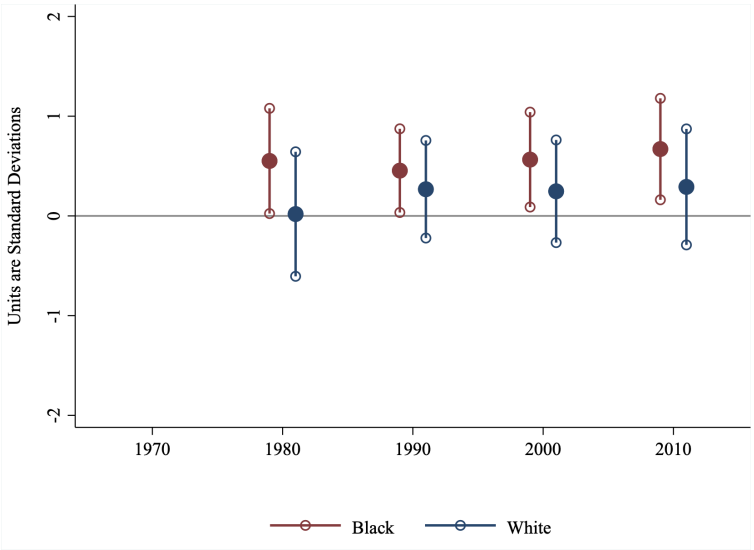
Notes: Figure plots regression estimates from a two-stage least squares analysis of the impact of segregation on arrests by census year. Confidence intervals are constructed from heteroskedastic robust standard errors.

Figure 5: Two-Stage Least Squares Results: Segregation, Imprisonment, and RDI

(a) Admission Rate



(b) Imprisonment Rate



Notes: Figure plots regression estimates from a two-stage least squares analysis of the impact of segregation on criminal-justice-related outcomes by census year. Confidence intervals are constructed from heteroskedastic robust standard errors.

Table 1: Summary Statistics by Decade

	1970	1980	1990	2000	2010
Index of Dissimilarity (County Sample)	0.71	0.59	0.58	0.50	0.45
Index of Dissimilarity (Central City Sample)	0.70	0.57	0.57	0.48	0.43
Homicide Victimization (per 1,000 residents)					
Non-white	0.22	0.19	0.17	0.10	0.12
White	0.03	0.05	0.04	0.03	0.03
Suicides (per 1,000 residents)					
Non-white	0.06	0.06	0.07	0.06	0.06
White	0.12	0.12	0.13	0.11	0.14
Police-Related Fatalities (per 100,000 residents)					
Non-white	0.48	0.81	0.27	0.09	0.22
White	0.05	0.08	0.07	0.06	0.08
Crime Per 1,000 Residents					
Total	50.50	89.00	102.74	76.54	64.97
Violent	0.96	2.50	3.08	2.95	2.99
Murder	0.10	0.10	0.11	0.08	0.09
Arrests Per 1,000 Residents					
Black	34.87	42.80	48.00	27.47	21.84
White	7.20	12.12	14.31	12.07	12.37
Violent Crime Arrests Per 1,000 Residents					
Black	8.42	12.09	14.77	9.96	8.55
White	0.82	2.20	3.16	3.74	3.82
Property Crime Arrests Per 1,000 Residents					
Black	26.46	30.71	33.23	17.51	13.29
White	6.37	9.92	11.15	8.33	8.55
Drug Possession Arrests Per 1,000 Residents					
Black		4.11	16.91	17.44	14.54
White		2.06	4.68	7.78	8.46
Drug Sales Arrests Per 1,000 Residents					
Black		0.84	8.85	6.82	4.84
White		0.45	1.90	1.80	1.56
Imprisonment Rates (per 1,000 residents)					
Black		1.38	3.07	3.48	2.92
White		0.25	0.48	0.62	0.63
Admission Rates (per 1,000 residents)					
Black		3.99	5.84	7.72	6.62
White		0.50	0.78	1.16	1.22
Employment Per 1,000 Residents					
Sworn Officers	1.73	2.05	2.08	2.33	2.22
Local Government Finances Per Capita					
General Expenditures	886.51	1,292.14	1,411.40	1,689.93	1,869.13
Property Tax Revenue	368.14	352.01	368.97	406.49	482.42
Police Expenditures	88.20	126.56	157.28	196.03	223.84
Fire Safety Expenditures	77.02	99.31	113.77	127.27	140.63
School Expenditures (per-pupil)	5628	6072	8627	10018	12208
Number of MSAs in County Sample	110	110	110	110	110
Number of MSAs in Central City Sample	121	121	121	121	121

Note: Data comes from the Census of Population and Housing, the Census of Governments, Vital Statistics Multiple Cause of Deaths Files, Uniform Crime Report, the Vera Institute, and the Annual Survey of Governments. Imprisonment and admission rates means are calculated for 1983 instead of 1980 due to data availability.

Table 2: First-Stage Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First-Stage						Homicide Rate
	All	1970	1980	1990	2000	2010	1910
RDI	0.367***	0.216***	0.374***	0.359***	0.458***	0.427***	0.151
	[0.0907]	[0.0779]	[0.1000]	[0.0914]	[0.111]	[0.102]	[0.245]
Track Length	19.12*	12.27**	21.12**	17.84*	23.23*	21.15*	-15.37**
	[10.29]	[6.102]	[10.54]	[10.45]	[12.89]	[12.15]	[6.894]
Observations	550	110	110	110	110	110	86
Effective F-Stat	16.35	7.719	13.97	15.45	17.12	17.71	–
Mean DV	0.564	0.705	0.588	0.576	0.499	0.453	0.962

Note: Robust standard errors in brackets. ***1%, **5%, and *10%.

Table 3: Relationship Between Segregation and Non-White Homicide

	(1)	(2)	(3)	(4)	(5)	(6)
DV: Non-White Homicides	All	1970	1980	1990	2000	2010
	Panel (a): OLS					
Dissimilarity Index	0.515***	0.254***	0.570***	0.574***	0.527***	0.518***
	[0.0446]	[0.0840]	[0.0819]	[0.0883]	[0.0876]	[0.0871]
	Panel (b): 2SLS					
Dissimilarity Index	0.844***	0.809**	1.002***	0.977***	0.694***	0.429**
	[0.171]	[0.328]	[0.222]	[0.214]	[0.202]	[0.172]
Observations	550	110	110	110	110	110
Robust Conf. Int.	[.5519, 1.325]	[.3288, 2.094]	[.6584, 1.715]	[.5961, 1.579]	[.3176, 1.199]	[.0814, .8321]
AR p-value	0	0.0024	0	0.0001	0.0019	0.0213
Effective F-Stat	16.35	7.719	13.97	15.45	17.12	17.71
Mean DV	0.163	0.225	0.195	0.175	0.100	0.119
Std. Dev.	0.155	0.196	0.168	0.150	0.0927	0.109

Note: The table reports regression estimates for the impact of segregation on non-white homicides by census year. Panel (a) reports the OLS estimates, see equation (3). Panel (b) reports the two-stage least squares estimates, see equation (5) Robust standard errors in brackets. ***1%, **5%, and *10%.

Table 4: Relationship Between Segregation and White Homicide

DV: White Homicides	(1) All	(2) 1970	(3) 1980	(4) 1990	(5) 2000	(6) 2010
			Panel (a): OLS			
Dissimilarity Index	-0.0300	0.185**	-0.0117	-0.208**	-0.0780	-0.246***
	[0.0562]	[0.0815]	[0.0935]	[0.0923]	[0.0855]	[0.0870]
			Panel (b): 2SLS			
Dissimilarity Index	-0.173	-0.260	-0.228	-0.0105	0.0288	-0.369
	[0.202]	[0.308]	[0.238]	[0.213]	[0.202]	[0.264]
Observations	550	110	110	110	110	110
Robust Conf. Int.	[-.6281, .2669]	[-1.466, .2881]	[-.7840, .2911]	[-.3906, .5554]	[-.3947, .4842]	[-.9844, .1637]
AR p-value	0.400	0.350	0.346	0.961	0.886	0.162
Effective F-Stat	16.35	7.719	13.97	15.45	17.12	17.71
Mean DV	0.0332	0.0287	0.0458	0.0377	0.0268	0.0273
Std. Dev.	0.0246	0.0185	0.0299	0.0282	0.0184	0.0203

Note: The table reports regression estimates for the impact of segregation on non-white homicides by census year. Panel (a) reports the OLS estimates, see equation (3). Panel (b) reports the two-stage least squares estimates, see equation (5) Robust standard errors in brackets. ***1%, **5%, and *10%.

Supplementary Appendix

Black Lives: The High Cost of Segregation

Robynn Cox, Jamein Cunningham, Alberto Ortega, and Kenneth Whaley

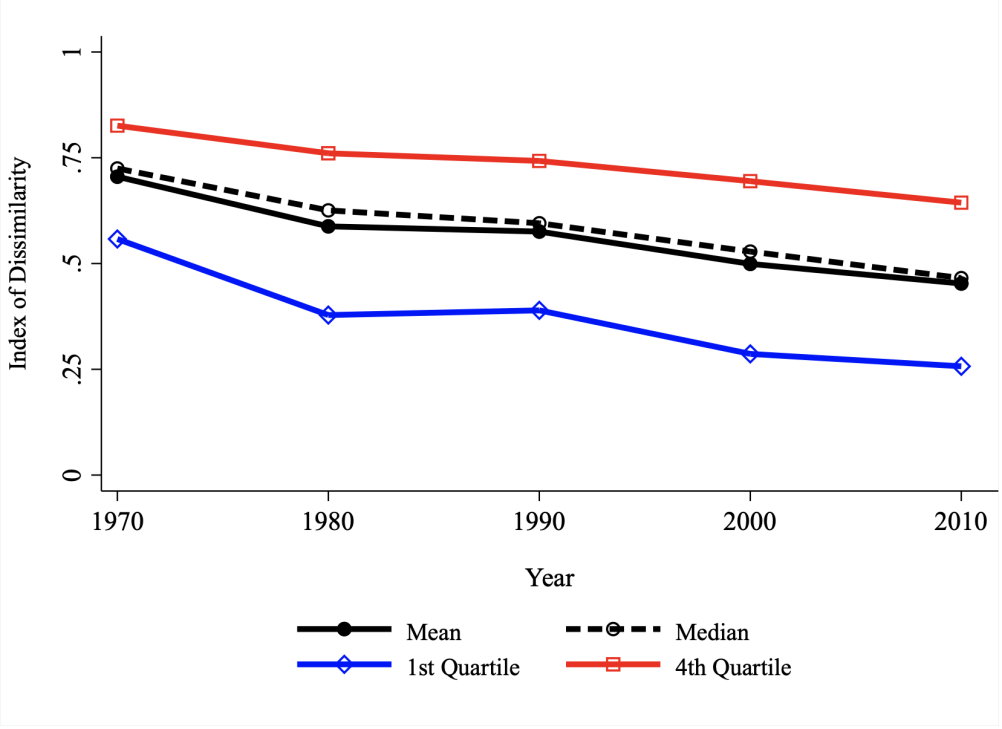
A Data Appendix

This paper relies on various data sources to estimate the effect of residential segregation on victimization, public safety, and related outcomes. Below we described the data used in the paper to obtain our results.

A.1 Dissimilarity Index

To measure segregation, we rely on the commonly used dissimilarity index (Duncan and Duncan, 1955; Cutler and Glaeser, 1997). The index provides the share of the Black population that would need to move to other census tracts to integrate a city or MSA fully.[30] For 1990, we use the dissimilarity index from Cutler et al. (1999). To construct the dissimilarity index for the other census years, we rely on population counts from the Census of Population and Housing for the Census years between 1970 and 2010. We use the 1990 definition of MSAs in each census year to calculate the dissimilarity index. We derive the index from counties belonging to each MSA outside of New England. Since New England MSAs are comprised of cities and towns, we calculate the dissimilarity index using the census tracts in the cities or towns within each MSA. We impute the dissimilarity index for MSAs with insufficient or missing census tract data. Figure A2 plots the dissimilarity index for MSAs in the first and fourth quartile in 1970. The figure also plots the mean and median dissimilarity index over time. Figure A2 shows that segregation is decreasing for all MSAs and rules out the possibility of any one or few MSAs driving our findings.

Figure A1: Index of Dissimilarity Over Time



Notes: Figure plots the index of dissimilarity between 1970 and 2010.

[30] For additional explanation and examples, see https://www.censusscope.org/about_dissimilarity.

A.2 Railroad Division Index

To capture the plausible exogenous variation in our measure of segregation, we use the same railroad division index (RDI) from (Ananat and Washington, 2009). The RDI is similar to a Herfindahl-Hirschman Index and exploits the arrangement of railroad tracks that create subdivisions within a city. See Figure 1 in (Ananat and Washington, 2009) for two example cities of this natural experiment.

A.3 Vital Statistics Data

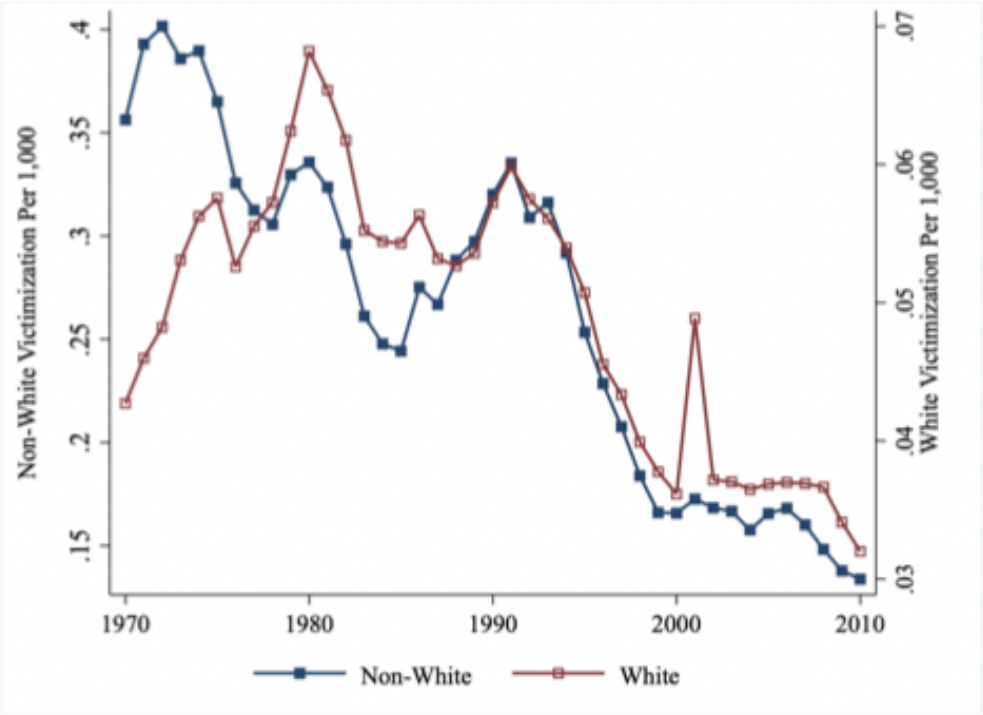
Our main measures of victimization are collected from the public-use (1970, 1980) and restricted-use (1990,2000,2010) Vital Statistics Multiple Cause of Death Files made available by the Center for Disease Control and Intervention National Vital Statistics System (NVSS).

A.3.1 Homicides

Our primary outcomes are homicides by race. We use the following ICD-10 codes in the NVSS: *U01-*U02, X85-Y09, Y87.1. For homicide deaths from 1970 to 1998, we use the following ICD-9 and ICD-8 codes:E960-E969 . We focus our analysis on white and non-white homicides because the NVSS data does not collect detailed information on race or ethnicity in the earlier years. Figure **??** plots white and non-white homicide rates over time and shows that homicides have decreased between 1970 and 2010. Although the trends are similar, the vertical axes show that non-white homicides have often been three to seven times higher than white homicides.

A.3.2 Annual Survey of School System Finances

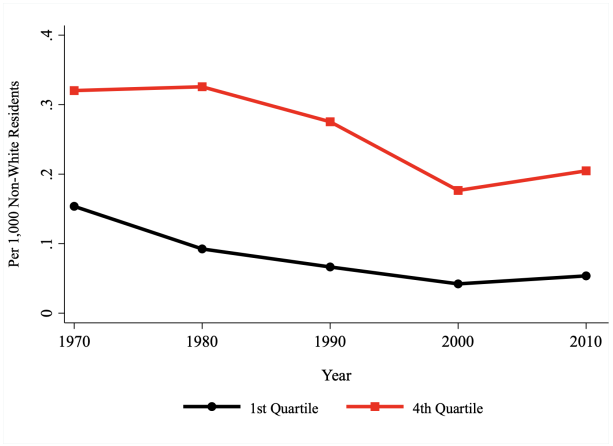
Figure A2: Homicides Over Time



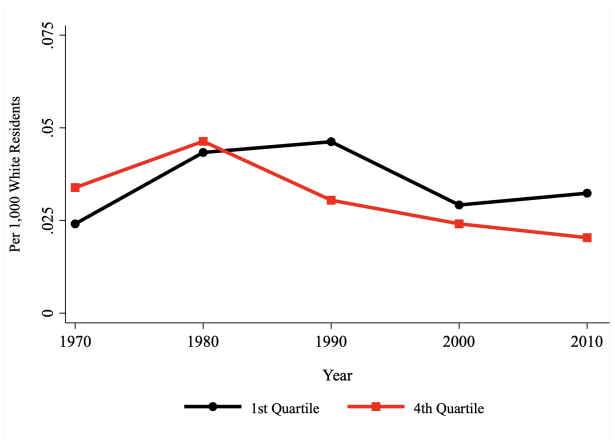
Notes: Figure plots number of homicides per 100,000 residents between 1970 and 2010 for white and non-white individuals using data from the National Vital Statistics System.

Figure A3: Homicides and RDI by Index of Dissimilarity Quartiles

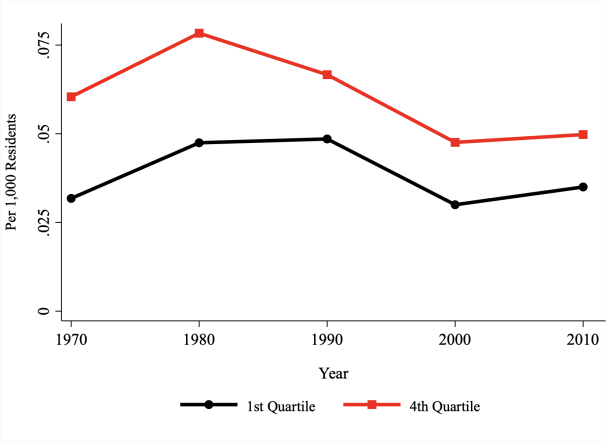
(a) Non-White Homicides



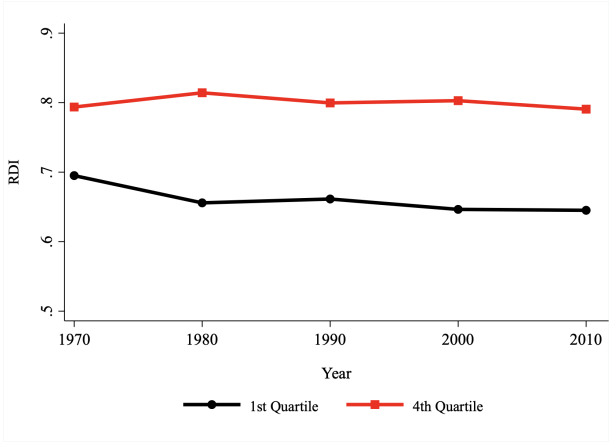
(b) White Homicides



(c) All Homicides

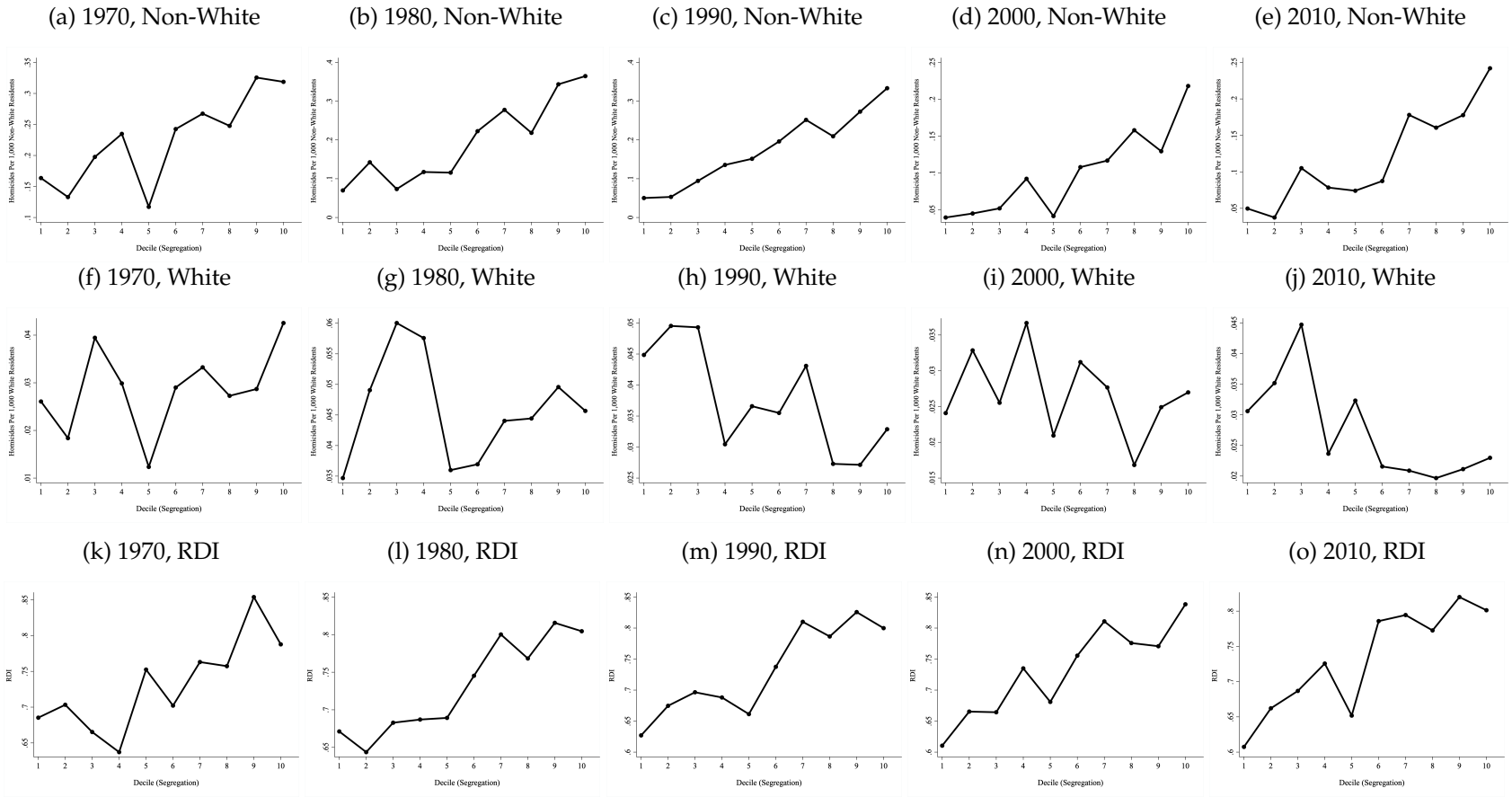


(d) RDI



Notes: Figure plots homicide rates by race and railroad division index for the top and bottom quartile by census yearâ panels (a) and (b) plot non-white and white homicides per 1,000 individuals, respectively. Panel (c) plots all homicides per 1,000 individuals. Panel (d) Plots the railroad division index.

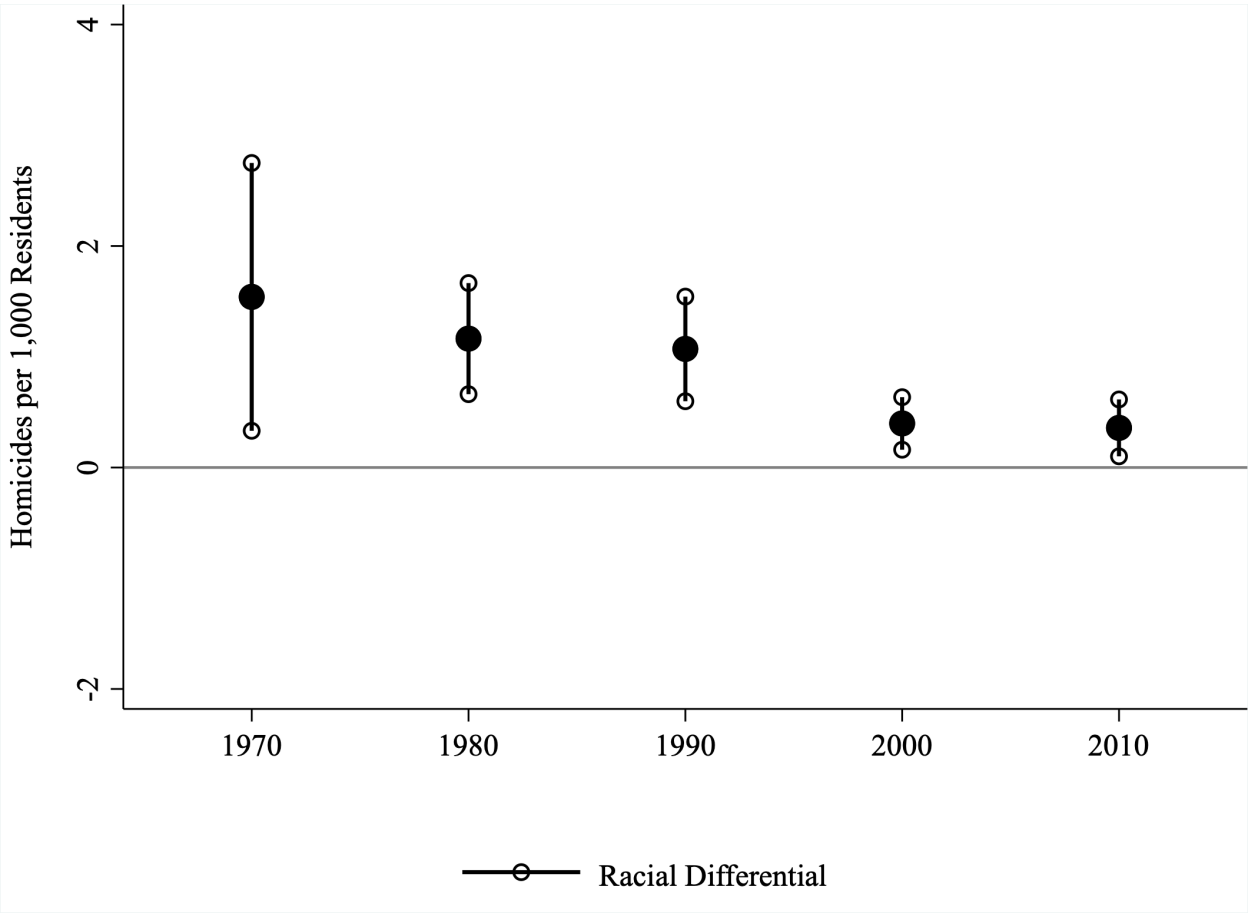
Figure A4: Homicides and RDI by Index of Dissimilarity Deciles



Notes: Figure plots homicides and railroad division index by decile. Panels (a)-(e) plots non-white homicides per 1,000 individuals for census years between 1970-2010, Panels (f)-(j) plots non-white homicides per 1,000 individuals for census years between 1970-2010, and panels (k)-(o) plots the railroad division index for census years between 1970-2010.

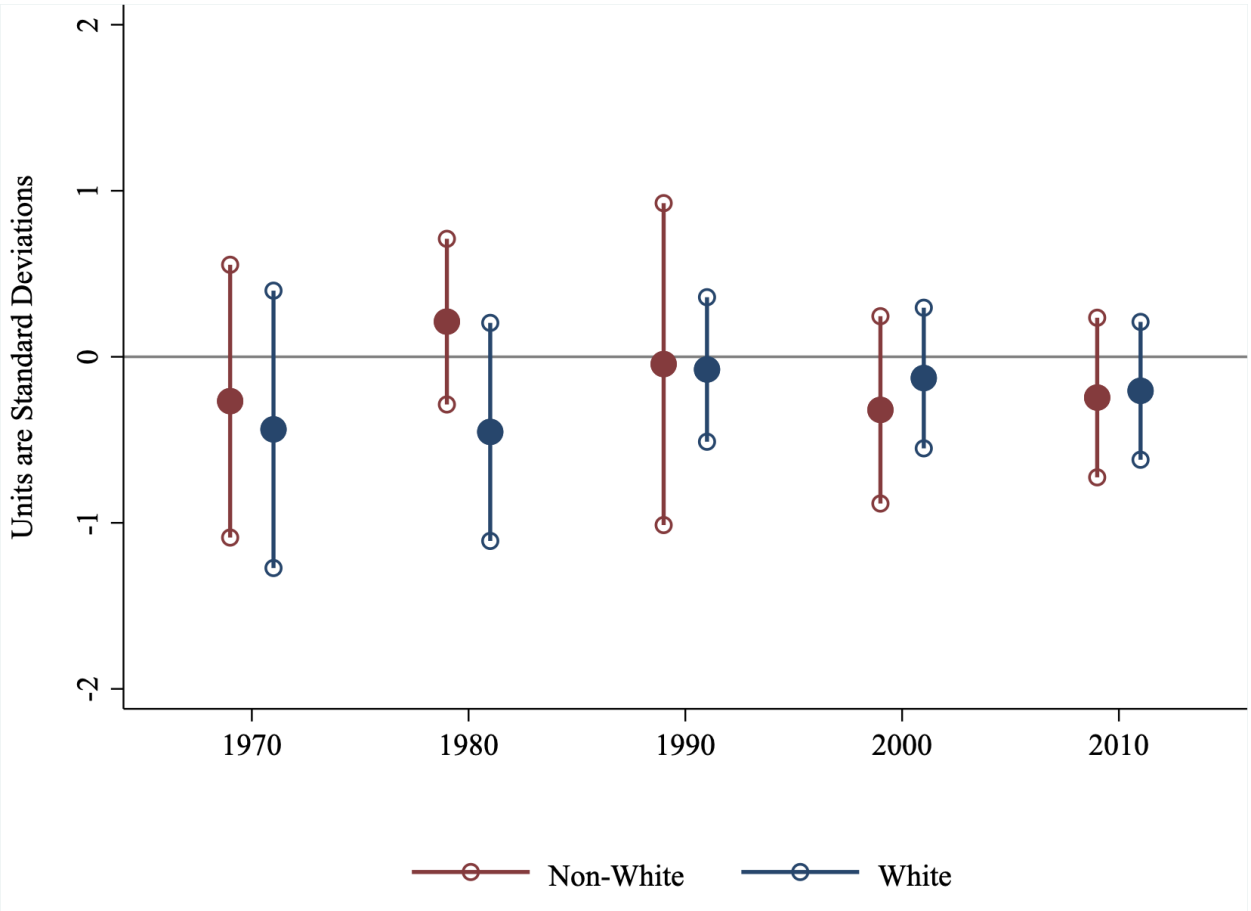
B Appendix Figures and Tables

Figure B1: Two-Stage Least Squares Results: Segregation, Racial Differential, and RDI



Notes: Figure plots regression estimates from a two-stage least squares analysis for the impact of segregation on the racial homicide differential by census year. The confidence intervals are constructed from heteroskedastic robust standard errors. To estimate the racial differential in homicide we estimate the following equation $Y_{i,r} = \beta_0 + \beta_1 Black_r + \beta_1 \widehat{D_i} + \beta_1 (D_i \widehat{\times Black_r}) + \beta_2 X_{i,r} + \eta_i$. The binary variable, $Black_r$, is equal to one when referencing non-White homicides and zero for White homicides. We instrument for D_i using the RDI_i and $(D_i \times Black_r)$ with $RDI_i \times Black_r$.

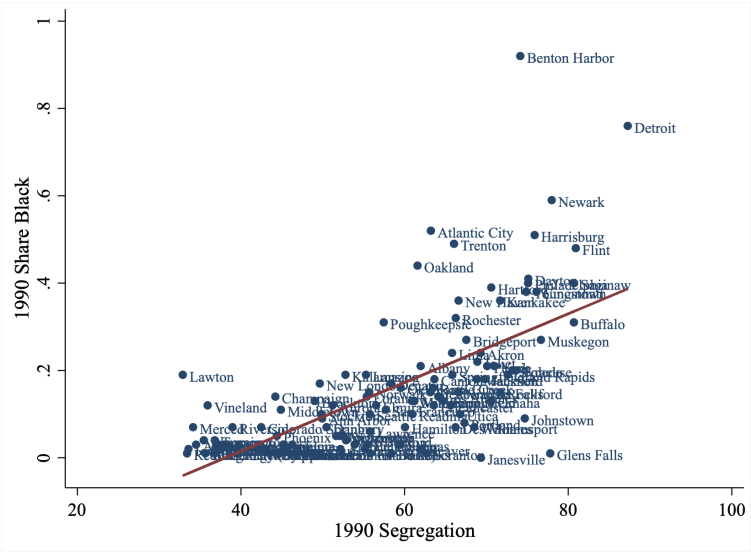
Figure B2: Two-Stage Least Squares Results: Segregation, Suicides, and RDI



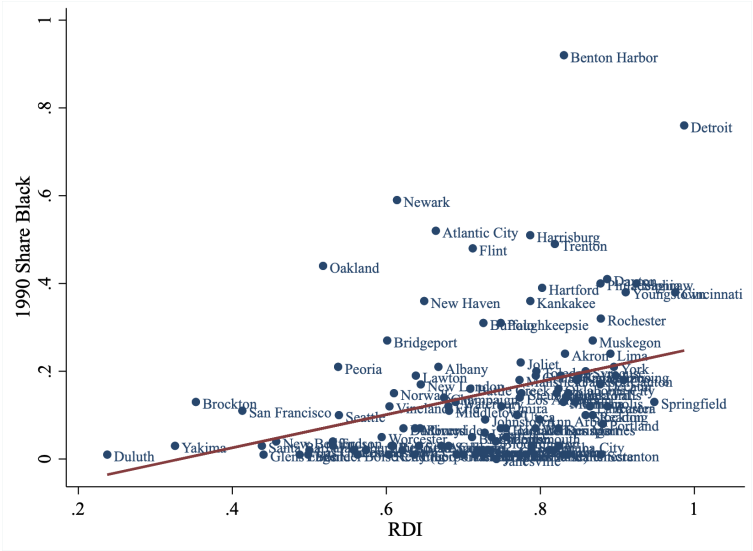
Notes: Figure plots regression estimates from a two-stage least squares analysis for the impact of segregation on suicide by race and census year, see equation (5). Confidence intervals are constructed from heteroskedastic robust standard errors.

Figure B3: Relationship Between Black Share of the Population, Segregation, and RDI

(a) Index of Dissimilarity



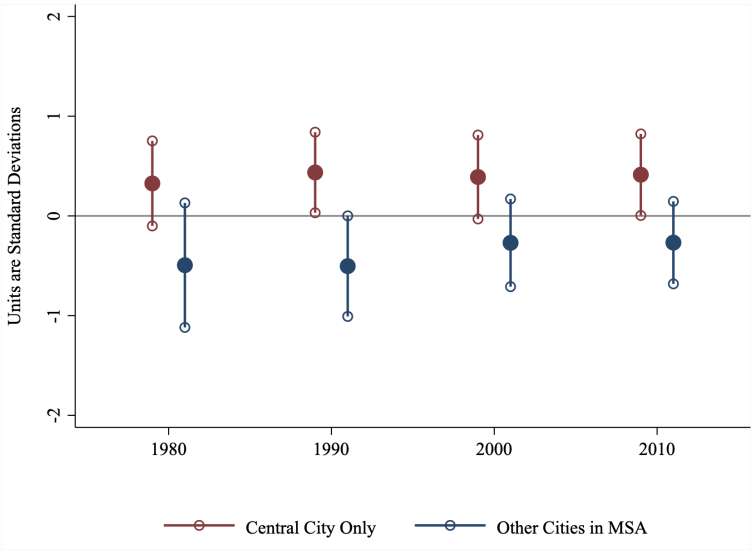
(b) RDI



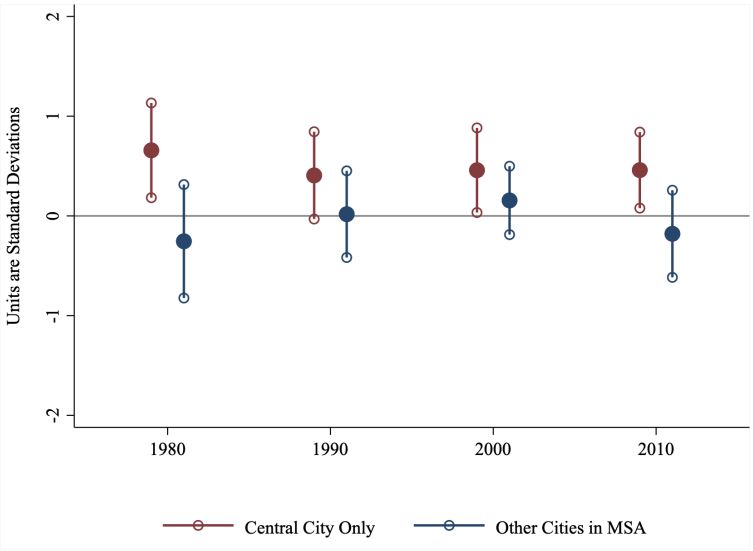
Notes: Panel (a) plots the relationship between the 1990 dissimilarity index and the share of individuals in an MSA that is Black. Panel (b) plots the relationship between the railroad division index and the 1990 Black share of individuals in an MSA.

Figure B4: Two-Stage Least Squares Results: Other Cities in MSA vs Central City

(a) Violent Crime

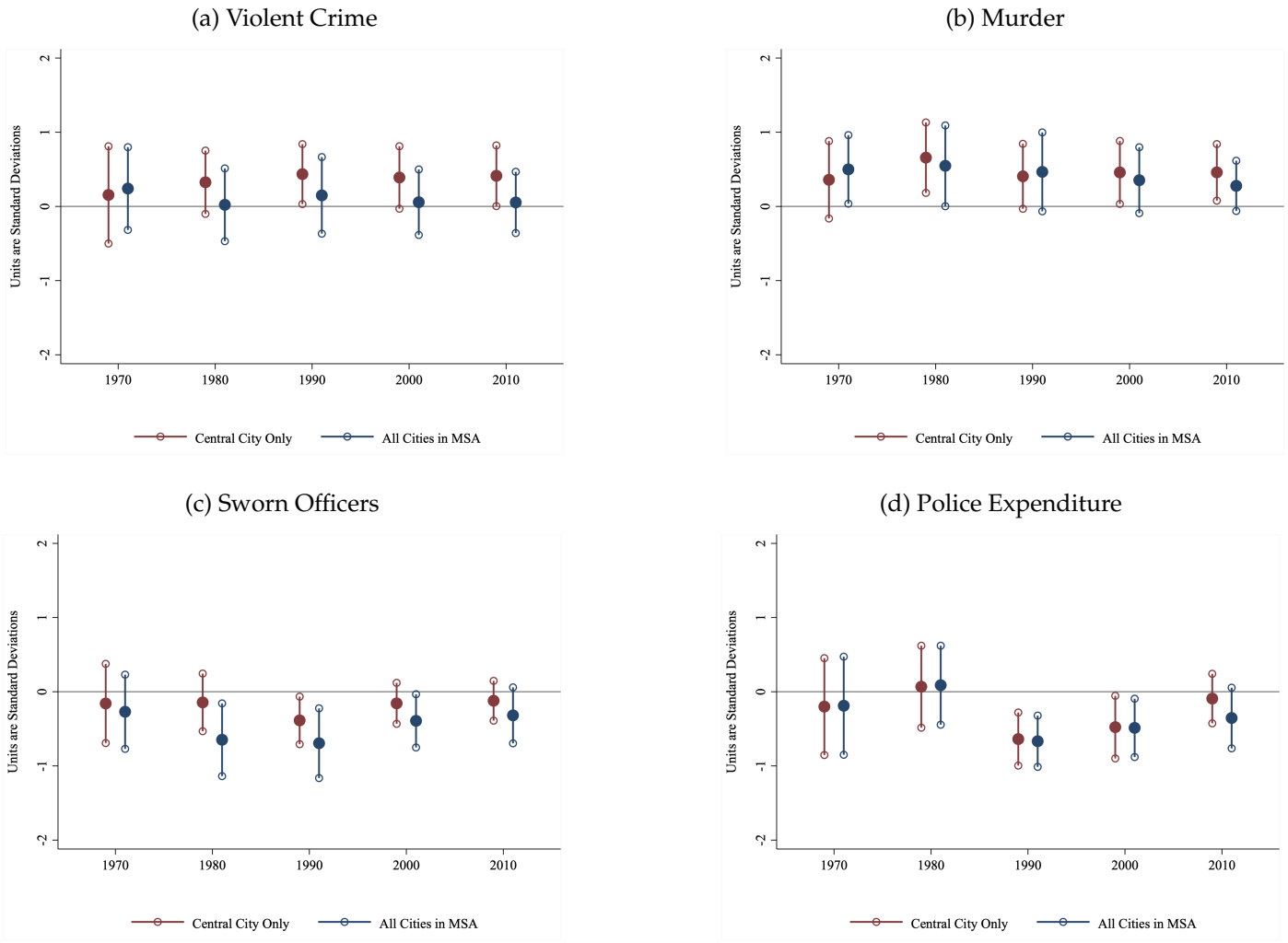


(b) Murder



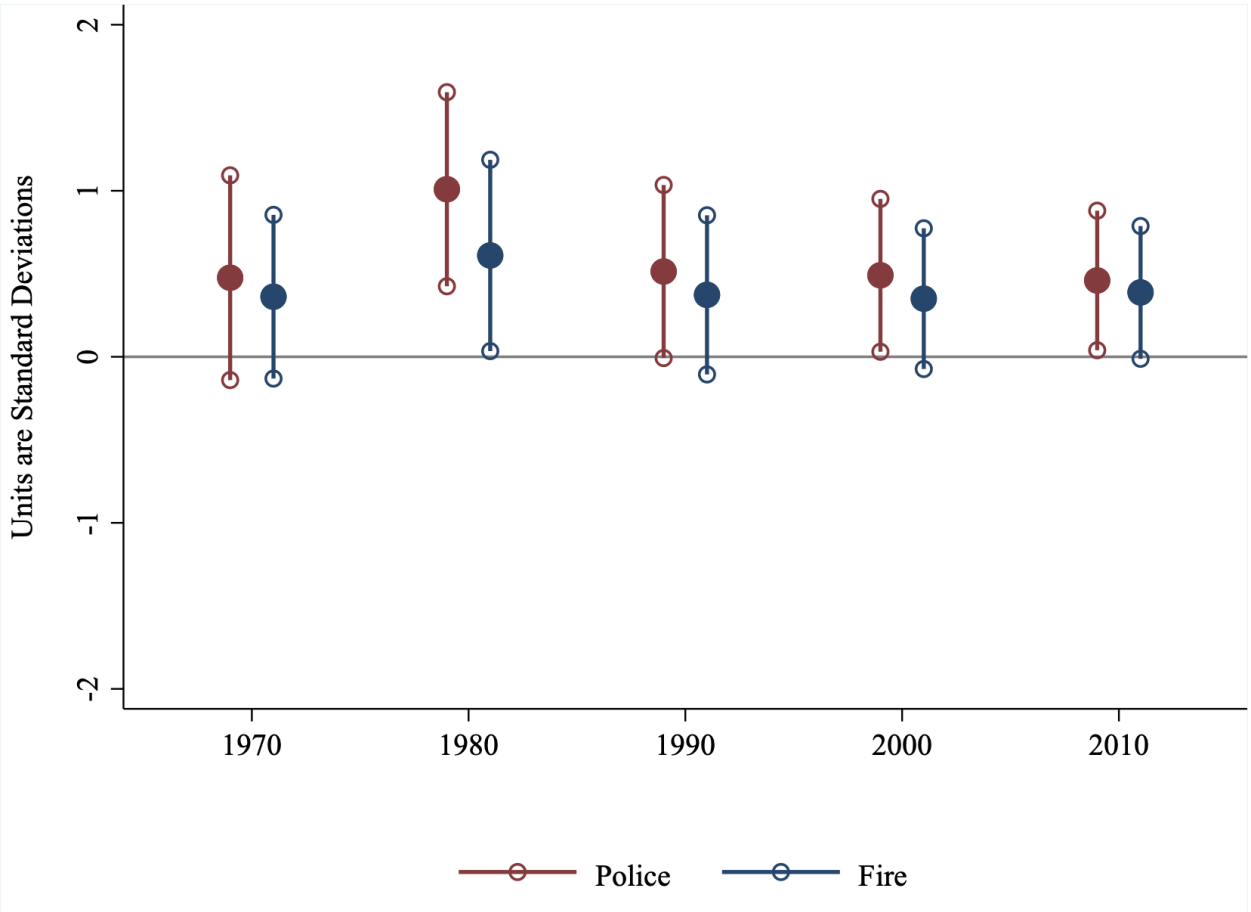
Notes: Figure plots regression estimates from a two-stage least squares analysis for the impact of segregation on violent crime (panel (a)) and murder (panel (b)) by census year, see equation (5). The estimates plotted are for either the central city within an MSA or the surrounding towns. Confidence intervals are constructed from heteroskedastic robust standard errors.

Figure B5: Two-Stage Least Squares Results: MSA vs Central City



Notes: Figure plots regression estimates from a two-stage least squares analysis for the impact of segregation on violent crime (panel (a)), murder (panel (b)), number of sworn officers (panel (c)), and police expenditures (panel (d)) by census year, see equation (5). The estimates plotted are for either the central city within an MSA or the entire MSA, including the central city. Confidence intervals are constructed from heteroskedastic robust standard errors.

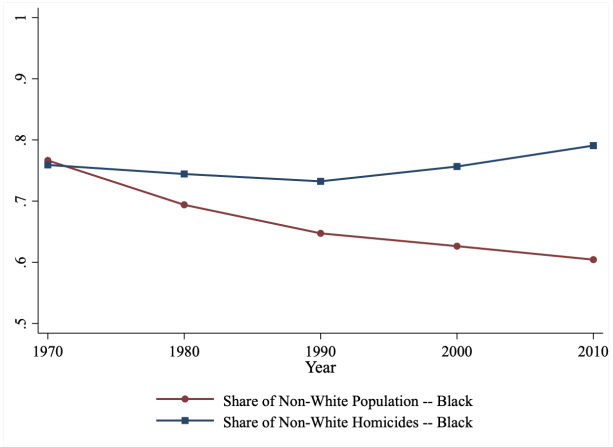
Figure B6: Two-Stage Least Squares Results: Segregation, Spending Shares, and RDI



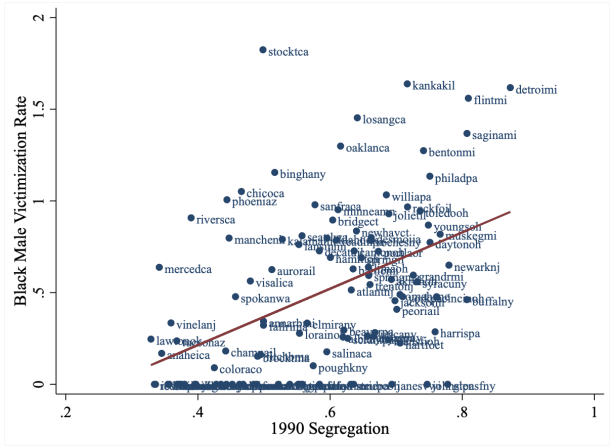
Notes: Figure plots regression estimates from a two-stage least squares analysis for the impact of segregation on the share of public safety expenditures by census year, see equation (5). Confidence intervals are constructed from heteroskedastic robust standard errors.

Figure B7: Black vs Non-White Homicides

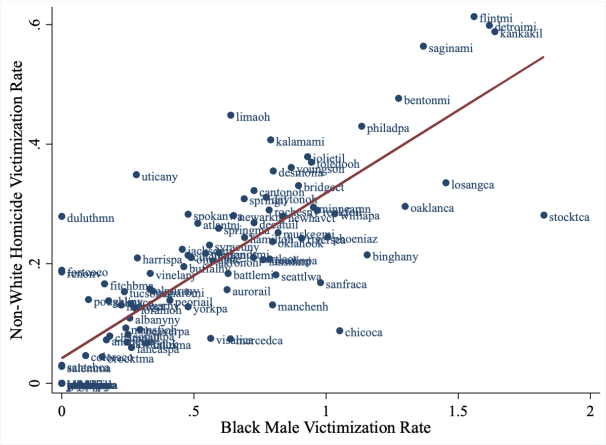
(a) Share of Non-White Population – Black



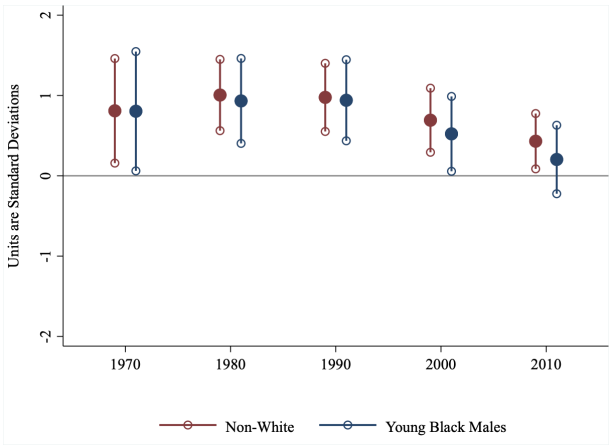
(b) Black Male Homicides



(c) Non-White vs. Black Male Homicides

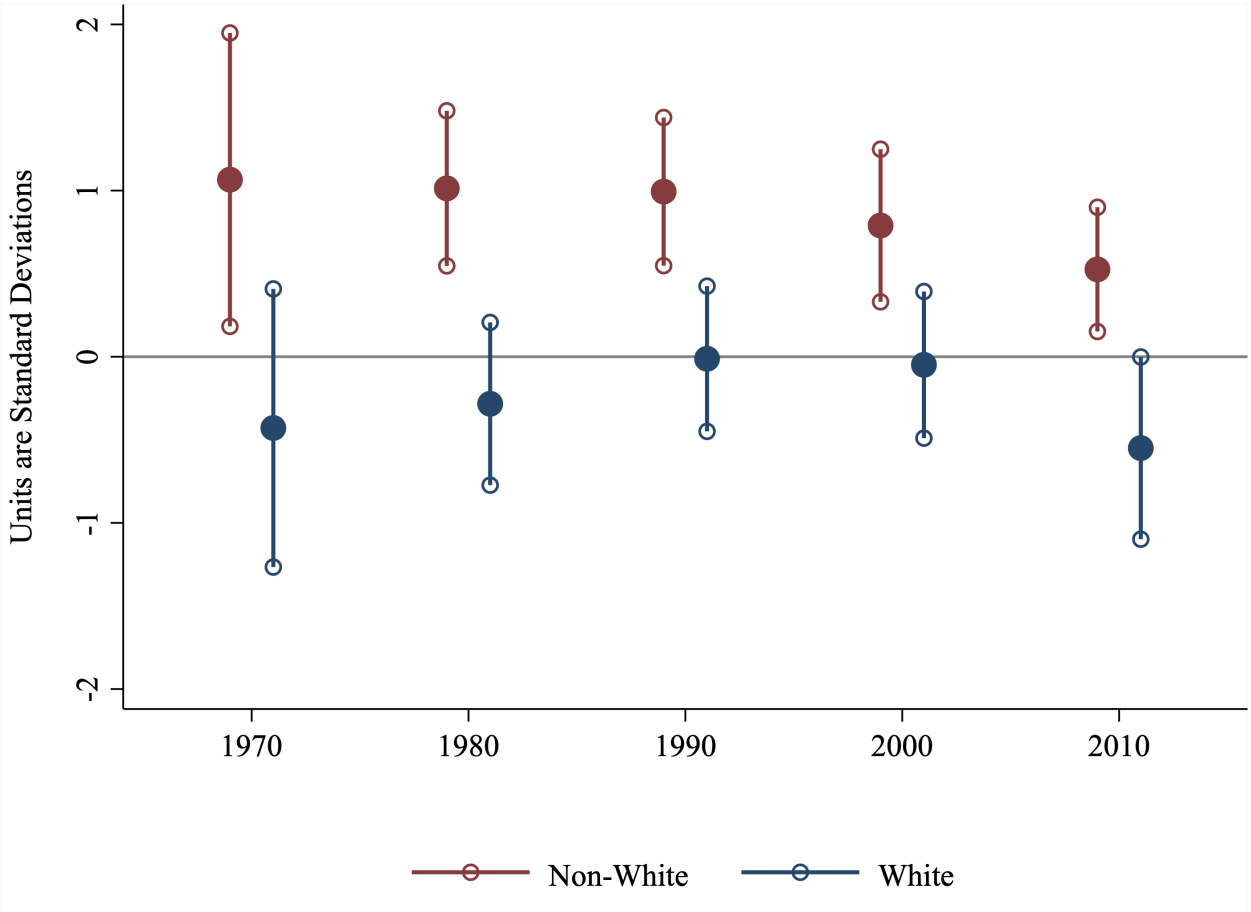


(d) Two-Stage Least Squares Results



Notes: **[Panel (a)]** plots the relationship the share of Non-white population that is Black over time. **[Panel (b)]** plots the relationship between segregation and Black male homicide rates in 1990. **[Panel (c)]** plots the relationship between non-white and Black male homicide rates. Lastly, **[Panel (d)]** plots two-stage least squares estimates for the impact of segregation on Black male homicide rates.

Figure B8: Two-Stage Least Squares Results: Homicides Excluding New England MSAs

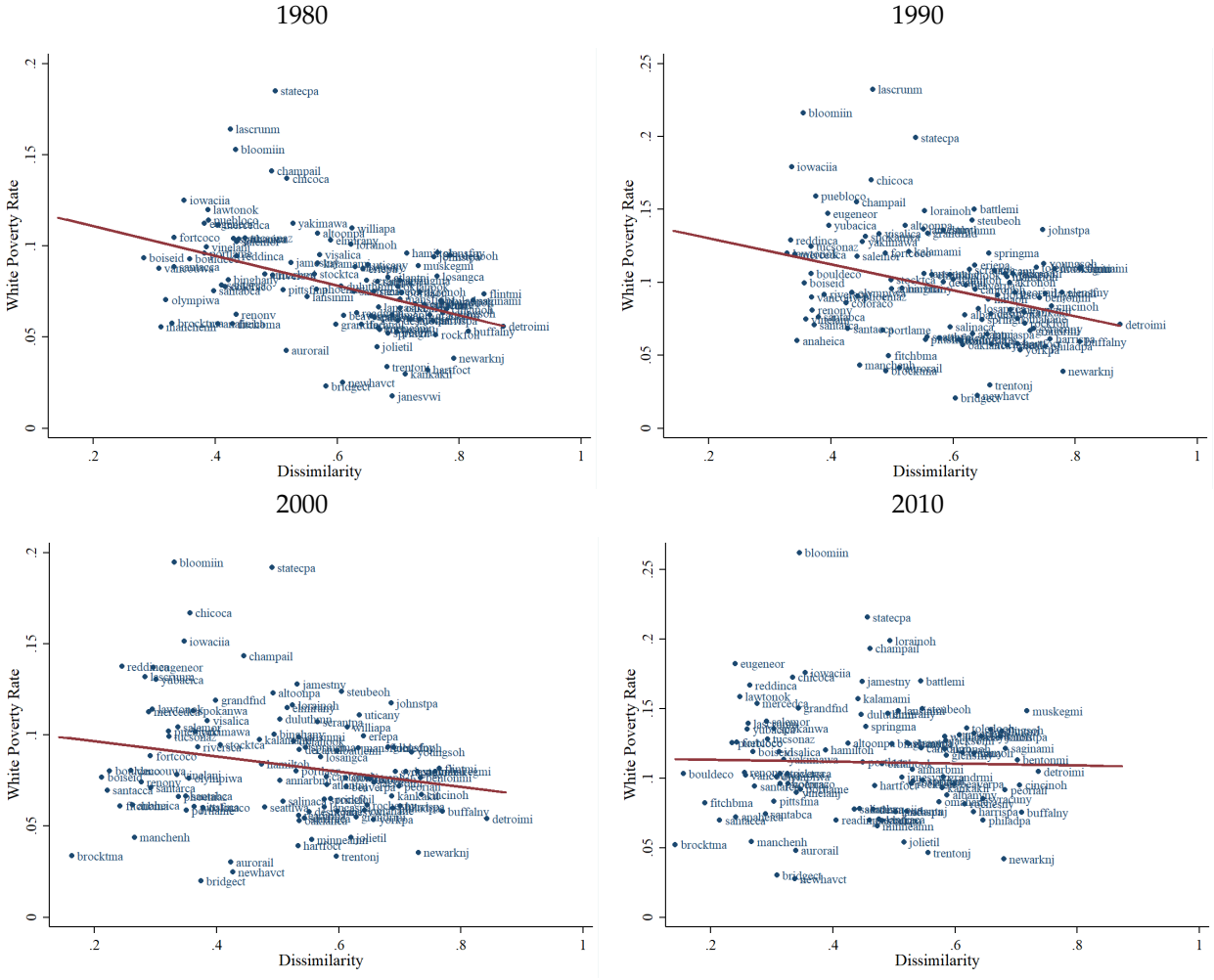


Notes: Figure plots regression estimates from a two-stage least squares analysis for the impact of segregation on homicides by race and census year, see equation (5). The plotted estimates exclude MSAs in the New England. Confidence intervals are constructed from heteroskedastic robust standard errors.

Figure B9: Black Poverty Rates By Decade

Notes:

Figure B10: White Poverty Rates By Decade



Notes:

Table A1: Testing RDI as an Instrument

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	First Stage	Falsification Checks					
		1910 Characteristics					
Outcome:	1990 Index of Dissimilarity	Physical Area	Population	Ethnic Dissimilarity Index	Ethnic Isolation Index	Percent Black	Street Cars Per Capita
RDI	0.357***	-3,993	665.8	0.0765	0.0267	-0.000633	-132.1
	[0.0878]	[11,986]	[1,363]	[0.185]	[0.0702]	[0.00998]	[183.2]
Track Length	18.51*	-574,401	75,553	15.34	-12.44	9.236***	3,361
	[10.73]	[553,669]	[134,815]	[53.25]	[17.29]	[0.650]	[20,507]
Observations	121	58	121	49	49	121	13
Mean DV	0.569	14626	1527	0.311	0.0554	0.0144	179

	Falsification Checks						
	1920 Characteristics						
				Percent of Employment in:			
Outcome:	Percent Black	Percent Literate	Labor Force Participation	Trade	Manufacturing	Railroads	1990 Income Segregation Index
RDI	0.0132	0.0526*	0.0284	-0.0803	0.191	-0.0738	0.0323
	[0.00905]	[0.0303]	[0.0240]	[0.0937]	[0.137]	[0.0681]	[0.0322]
Track Length	9.119***	0.180	-3.427**	-0.152	18.40*	1.592	-2.504
	[0.615]	[0.880]	[1.500]	[2.910]	[10.91]	[2.428]	[1.626]
Observations	121	121	121	121	121	121	69
Mean DV	0.0156	0.959	0.419	0.0577	0.462	0.00316	0.217

Note: This table replicates Table 1 from Ananat (2011). The table regresses RDI and railroad track length on 1910 and 1920 demographic and MSA characteristics. Column (1) reports the first stage estimate that regresses RDI on the 1990 dissimilarity index, see equation (4). Robust standard errors in brackets. ***1%, **5%, and *10%.

Table A2: Robustness Check - 1990 Segregation and Characteristics

	(1) Homicides	(2)
	Non-White	White
Original	0.9764***	-0.0105
	[0.2165]	[0.2162]
Population Controls	0.9526***	-0.1137
	[0.2265]	[0.2031]
Education Controls	1.3883***	0.4363
	[0.3352]	[0.3474]
Labor Force Participation	0.9435***	0.0797
	[0.1836]	[0.2070]
Black Poverty Rate	1.0312***	0.0578
	[0.2439]	[0.2317]
Black Gini Index	1.0266***	0.0438
	[0.3023]	[0.3216]
All	1.2677**	0.5101
	[0.5252]	[0.5684]

Note: Units are standard deviations. Robust standard errors in brackets. ***1%, **5%, and *10%.

Table A3: Robustness Check - Historical Characteristics

	(1) Homicides	(2)
	Non-White	White
Original	0.9764***	-0.01050
	[0.2165]	[0.2162]
Historical Population Controls	1.1024***	0.17930
	[0.3114]	[0.2813]
Historical Labor Market Controls	1.1051***	-0.03580
	[0.2996]	[0.2750]
All Historical Controls	1.2677***	0.19140
	[0.3781]	[0.3109]

Note: Units are standard deviations. Robust standard errors in brackets. ***1%, **5%, and *10%.

Table A4: Relationship Between Black Share of Population, Segregation, and RDI

	(1)	(2)	(3)	(4)
	Share Black		Index of Dissimilarity	
RDI	0.249***	-0.0228	0.357***	0.240***
	[0.0838]	[0.0761]	[0.0878]	[0.0691]
Track Length	39.82***	27.23***	18.51*	-5.194
	[9.784]	[3.843]	[10.73]	[5.577]
Index of Dissimilarity		0.709***		
		[0.0994]		
Share Black				0.576***
				[0.0584]
Mean DV	0.144	0.144	0.569	0.569

Note: The table reports estimates from the relationship between black share of MSA population, segregation, and RDI. Robust standard errors in brackets. ***1%, **5%, and *10%.

Table A5: Impact of Segregation on Non-White Homicides Over Time

	(1) Non-White Homicides per 1,000
Observed	
a. 1970	0.225
b. 2010	0.119
c. Change	-0.106
Counterfactual	
d. $\widehat{2010}$	0.197
e. $\widehat{Change}$(d-a)	-0.028
How much did changes in segregation change homicides compared to the counterfactual?	
$\frac{b-d}{d} \times 100$	40%
How much of the change in homicides can changes in segregation explain?	
$\frac{c-e}{c} \times 100$	74%

Note: To calculate the counterfactual we use our estimates to obtain the non-white homicide rate at the 1970 level of segregation.

Table A6: Robustness Check - Specification Check: Police-Related Fatalities

	(1)	(2)	(3)	(4)	(5)
	Non-White Police-Related Fatalities				
	1970	1980	1990	2000	2010
	Panel A: Two-Stage Least Squares				
Mortality Rate (Original)	-2.226	3.708***	1.146	0.912	-0.104
	[2.918]	[1.345]	[1.323]	[0.646]	[0.767]
Count	4.013	4.515	2.095	2.127	0.300
	[6.059]	[2.735]	[1.887]	[1.965]	[2.157]
Poisson Count	-4.247	14.72	7.276	3.512	0.150
	[21.50]	[12.67]	[11.81]	[11.30]	[9.097]
Fatal Encounters (Rate)				0.318	-0.594
				[1.457]	[1.355]
	Panel B: No Instrument				
Mortality Rate	-0.572	1.379*	-0.185	0.758***	-0.173
	[1.344]	[0.728]	[0.521]	[0.246]	[0.358]
Poisson (Count)	11.76***	16.86***	3.802	8.111***	1.476
	[3.685]	[3.792]	[2.439]	[2.629]	[2.132]
Number of MSAs	99	99	99	99	99

Note: The table reports regression estimates for the impact of segregation on non-white police-related fatalities by census year. Panel (a) reports the two-stage least squares estimates, see equation (5). Panel (b) reports the OLS estimates, see equation (3). Robust standard errors in brackets. ***1%, **5%, and *10%.

Table A7: Effect on Segregation on School District Expenditures

DV: School District Expenditures PP ($2015)	(1) All	(2) 1970	(3) 1980	(4) 1990	(5) 2000	(6) 2010
	Panel (a): OLS					
Dissimilarity Index	-0.3159***	-0.0987*	0.0053	0.1317**	0.1607***	0.1200
	[0.0451]	[0.0553]	[0.0296]	[0.0641]	[0.0554]	[0.0946]
	Panel (b): 2SLS					
Dissimilarity Index	-0.2680*	-0.4437**	-0.1552*	-0.2215	-0.0992	-0.4323*
	[0.1465]	[0.2084]	[0.0834]	[0.1645]	[0.1355]	[0.2539]
Observations	523	98	107	107	104	107
Robust Conf. Int.	[-.691369, -.03023]	[-1.2441,-.121896]	[-.415969,-.026512]	[-.683853, .058571]	[-.469433, .142238]	[-1.16621,-.020084]
AR p-value	0.0296	0.00947	0.0220	0.135	0.436	0.0416
Effective F-Stat	18.22	9.630	14.68	15.58	16.54	17.76
Mean DV	8553	5628	6072	8627	10018	12208
Std. Dev.	3176	1467	907	2048	1979	3030

Note: Panel (a) reports the OLS estimates. Panel (b) reports the two-stage least squares estimates. The outcome variable is median per-pupil expenditure in each MSA, standardized across all MSA level observations for a given year. Robust standard errors in brackets. ***1%, **5%, and *10%.

Table A8: Effect of Segregation on School District Property Tax Revenues

DV: Property Tax Revenue PP ($2015)	(1) All	(2) 1970	(3) 1980	(4) 1990	(5) 2000	(6) 2010
	Panel (a): OLS					
Dissimilarity Index	0.0629	-0.0186	0.1217*	0.2442**	0.2911***	0.3322**
	[0.0601]	[0.0826]	[0.0638]	[0.0933]	[0.1037]	[0.1475]
	Panel (b): 2SLS					
Dissimilarity Index	0.3712*	-0.1748	0.4604***	0.7111**	0.3573*	0.3455
	[0.1966]	[0.3352]	[0.1588]	[0.2799]	[0.2126]	[0.3035]
Observations	523	98	107	107	104	107
Robust Conf. Int.	[-.010182, .845997]	[-1.3028, .422375]	[.189946, .932003]	[.256709, 1.54218]	[-.071914, .853921]	[-.291388, 1.03045]
AR p-value	0.0634	0.583	0.00164	0.00305	0.104	0.263
Effective F-Stat	18.22	9.630	14.68	15.58	16.54	17.76
Mean DV	2746	2539	1991	2857	2817	3509
Std. Dev.	1920	1264	1134	1955	1902	2614

Note: Panel (a) reports the OLS estimates. Panel (b) reports the two-stage least squares estimates. The outcome variable is median per-pupil property tax revenues per-pupil in each MSA, standardized across all MSA level observations for a given year. Robust standard errors in brackets. ***1%, **5%, and *10%.

Table A9: Effect of Segregation on School District State Revenues

DV: State Revenue PP ($2015)	(1) All	(2) 1970	(3) 1980	(4) 1990	(5) 2000	(6) 2010
	Panel (a): OLS					
Dissimilarity Index	-0.3122***	-0.0301	0.0065	0.1381**	0.1562***	0.1023
	[0.0444]	[0.0556]	[0.0275]	[0.0631]	[0.0564]	[0.0958]
	Panel (b): 2SLS					
Dissimilarity Index	-0.2924*	-0.4245	-0.1748**	-0.3038*	-0.1575	-0.4021*
	[0.1543]	[0.2910]	[0.0845]	[0.1747]	[0.1433]	[0.2326]
Observations	523	98	107	107	104	107
Robust Conf. Int.	[-.010182, .845997]	[-1.3028, .422375]	[.189946, .932003]	[.256709, 1.54218]	[-.071914, .853921]	[-.291388, 1.03045]
AR p-value	0.0634	0.583	0.00164	0.00305	0.104	0.263
Effective F-Stat	18.22	9.630	14.68	15.58	16.54	17.76
Mean DV	2746	2539	1991	2857	2817	3509
Std. Dev.	1920	1264	1134	1955	1902	2614

Note: Panel (a) reports the OLS estimates. Panel (b) reports the two-stage least squares estimates. The outcome variable is median state revenue per-pupil in each MSA, standardized across all MSA level observations for a given year. Robust standard errors in brackets. ***1%, **5%, and *10%.

Table A10: Local Effects of Segregation on School District Expenditures

DV: School District Expenditures PP ($2015)	(1) 1980	(2) 1990	(3) 2000	(4) 2010
2SLS				
Dism	0.0054	0.0786	0.2485**	0.0286
	[0.0704]	[0.1069]	[0.1054]	[0.2529]
Majority Black	0.556	1.796	1.588*	3.770**
	[0.572]	[1.640]	[0.843]	[1.868]
Majority Black × Dism	-0.0818	-0.4217	-0.4077*	-0.8799*
	[0.1287]	[0.3589]	[0.2169]	[0.4764]
2SLS w/ MSA Fixed Effects				
Majority Black	0.747***	0.260*	1.218***	2.009***
	[0.169]	[0.152]	[0.227]	[0.220]
Majority Black × Dism	-0.1147***	-0.0552	-0.3070***	-0.4489***
	[0.0388]	[0.0350]	[0.0578]	[0.0596]
Tracts	17,360	17,736	17,296	17,595
MSA Fixed Effects	105	106	106	106

Note: Majority black is defined as a census tract where the share of black residents is higher than white and hispanic population shares. The outcome variable is the per-pupil expenditure for the school district geographically assigned to each census tract, standardized across all school districts in the sample. Robust standard errors in brackets. ***1%, **5%, and *10%.