Segregation and Upward Mobility: Evidence from Neighborhood Tipping

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Abstract

I estimate the effect of residential segregation on upward mobility using estimated tipping points as instruments for neighborhood racial composition. I find that low-income children who grew up in neighborhoods which experienced white flight and minority in-migration – forces of segregation – have substantially lower household incomes as adults. The results indicate that a 10 percentage point increase in neighborhood minority share resulting from segregation reduces the average low-income child's adulthood rank in the household income distribution by four percentiles. This corresponds to a decrease in the adulthood annual household income for the average low-income child of $4,500, or nearly 12 percent. Perhaps surprisingly, growing up in a neighborhood which experiences these segregationary forces is more harmful to the upward mobility of white children than the upward mobility of black children, even though the absolute degree of mobility is higher for whites than for blacks. A potential explanation is that spatial proximity to a racially similar peer group has some mitigating effects for black children who experience segregation.

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To my parents, for their love and support.

Contents

Chapter 1

Introduction

Racial residential segregation is a defining and persistent characteristic of the United States. In the early 1900's, cities used zoning laws to enforce black-white segregation (Silver 1997). After the Great Depression, the redlining of minority neighborhoods – a practice in which individuals were denied access to credit due to the racial composition of their neighborhood – caused increases in the degree of segregation by race that continued through the 1970's (Aaronson, Hartley, and Mazumder 2017). While overall segregation has declined somewhat since its peak in the 1970's, American cities remain divided on racial lines. In 1980, the average white person lived in a neighborhood that was 88 percent white and only five percent black, while the average black person lived in a neighborhood that was 61 percent black and 31 percent white. In 2010, the average white person lived in a neighborhood that was 75 percent white and only eight percent black, while the average black person lived in a neighborhood that was 45 percent black and 35 percent white (Logan and Stults 2011).

There is a striking negative correlation between segregation and a variety of measures of health, education, and income. More segregated cities have, on average, higher infant mortality, higher crime rates, more boarded-up housing, and higher poverty rates (Denton and Massey 1993). Wilson (1987) and Denton and Massey (1993) argue that segregation creates an "underclass" in America, a large class of people, mostly minorities, who are left out of the nation's prosperity. Descriptive empirical evidence supports the theory that segregation reduces upward social mobility—defined as children's adult outcomes conditional on their parent's economic status. Growing up in a commuting zone with one standard deviation higher racial segregation is associated with a 5.2 percentile reduction in income rank (in the national distribution of adulthood income) for children who grew up in families at the 25th income percentile (Chetty and Hendren 2018b). However, people do not live in random neighborhoods.The endogeneity of neighborhood selection and the association between segregation and a host of other social ills means that correlational studies do not identify the causal relationship between segregation and upward mobility. It is difficult to separate the sorting of individuals into different neighborhoods and the resulting demographic compositions of those neighborhoods from other neighborhood characteristics that affect children's outcomes.

Little is known about the causal effect of residential racial segregation, particularly at the neighborhood-level, on upward mobility. Understanding this relationship can inform policies that seek to promote racial equity and integration while lifting people out of poverty.

In this paper, I estimate the causal effect of segregation on upward mobility through a novel two-stage least squares (2SLS) approach derived from the theory of neighborhood tipping. I illustrate how the neighborhood tipping model predicts that residential racial segregation can arise due to social interactions in white preferences for neighborhood demographic composition. Once the minority share in a neighborhood exceeds a critical "tipping point," the neighborhood will undergo white flight and minority in-migration, causing a discontinuity in minority population growth. I study how these segregationary changes in neighborhood composition affect the prospects for upward mobility for those who grow up in these "tipped" neighborhoods.

I draw from the work of Card, Mas and Rothstein (2008), henceforth referred to as "CMR," who provide empirical evidence that neighborhoods in the United States exhibit tipping dynamics. CMR estimate the tipping points for large Metropolitan Statistical Regions (MSAs) across the United States for the decades from 1970 to 2000 using decennial Census data from the Neighborhood Change Database (NCDB). I merge these MSA-level estimated tipping points with other Census tract-level extracts from the NCDB and tract-level measures of upward mobility for children born from 1978 to 1983. These measures of upward mobility were produced by the Opportunity Insights project (Chetty et al. 2018) from restricted-access federal income tax returns. The measures of upward mobility are disaggregated by race and gender, allowing for the estimation of differential effects of segregation on the upward mobility of different racial and gender groupings.

To overcome the endogeneity of neighborhood composition, I instrument for the neighborhood's minority share using a regression discontinuity design around tracts' estimated MSA tipping points. The intuition behind the identification strategy is that neighborhoods in an MSA that have a minority share just above the MSA's tipping point should be comparable to neighborhoods in the MSA that have a minority share just below the tipping point. Given the timing of childhood for the 1978 to 1983 birth cohorts observed in the upward mobility data, I instrument for the tract's 1990 minority share with the regression discontinuity model around the 1980 tipping point and then estimate a structural equation that models the tract's upward mobility as a function of the 1990 minority share.[1] In addition to isolating plausibly exogenous variation in neighborhood composition, the 2SLS

[1] Chetty and Hendren (2018a, 2018b) and Chetty et al. (2018) assign children observed in the tax data to the tract where they reside in 1995, further supporting this choice of timing.

estimates are interpretable as the effect of segregation on upward mobility, even though tract-level minority shares are not necessarily good proxies for tract-level segregation (both a minority share of zero and 100 percent would indicate segregation). The 2SLS model estimates the Local Average Treatment Effect (LATE), the average treatment effect for tracts impacted by the tipping dynamics. Thus, I estimate the effect of an increased minority share for tracts whose higher minority share was the result of white flight and avoidance–forces of segregation.

I find that segregation caused by neighborhood tipping reduces the upward mobility of children who grow up in neighborhoods that experienced white flight. I estimate that every 10 percentage point increase in neighborhood minority share reduces the adulthood rank in the distribution of household income of the average poor child from the neighborhood by four percentiles. A four percentile reduction corresponds to an annual loss in income of nearly $4,500. This is a substantial reduction in adulthood annual income for low-income children; $4,500 is 12 percent of the mean predicted household income for individuals who grew up in poor families.

The results also indicate that growing up in a neighborhood that experienced white flight is worse for the upward mobility of white children whose families did not leave the neighborhood– "white stayers" – than for the upward mobility of black children who grew up in the neighborhood. This is consistent with the minority enclave hypothesis put forward by Cutler and Glaeser (1997) and Wilson and Portes (1980). They note that if people are better off when surrounded by a peer group that is similar to them, then segregation may have some positive impacts on the individual outcomes for minorities. My findings indicate that black children may benefit from an increasingly minority peer group after their neighborhood tips, mitigating some of the harmful effects of segregation, although segregation is still found to decrease black upward mobility. However, the estimated effect on white upward mobility should be interpreted with caution. As segregation resulting from neighborhood tipping is mostly driven by white flight from the neighborhood, the estimated effect on white upward mobility may reflect the composition of white stayers. If the white stayers were those with lower underlying potential outcomes, this selected out-migration would imply that my results overstate the harmful effect of segregation on white upward mobility.

Robustness checks for the identification assumptions indicate that neighborhoods that have minority shares below the tipping point have significantly lower pre-tipping average household income than those with minority shares past the tipping point, even after controlling for linear trends in minority share and absorbing cross-MSA variation. These differences suggest that tipping may not isolate wholly exogenous variation in neighborhood demographic composition. Using Oster's (2019) method, I estimate bounds around the true causal effect of segregation on upward mobility. I find that a four percentile reduction in upward mobility from a 10 percent increase in minority share due to segregation is a conservative estimate for the harmful effects of segregation on upward mobility. My estimate is on the lower end of the bounding interval, and the negative impact of segregation on upward mobility could well be larger.

The paper is structured as follows. Chapter 2 briefly reviews the literature on segregation, neighborhood effects, and upward mobility, and describes my contribution to these bodies of work. Chapter 3 outlines a model of neighborhood tipping that informs the causal identification. Chapter 4 provides background on the data sources and presents descriptive statistics. Chapter 5 details the instrumental variables empirical strategy. Chapter 6 presents the results of estimating the main empirical models. Chapter 7 presents robustness analyses. Chapter 8 concludes.

Chapter 2

Literature Review

In this chapter, I review three categories of previous studies which I build upon in this paper. First, I summarize work on the causes and consequences of residential segregation in the United States. Next, I briefly discuss theories of neighborhood tipping (which are further detailed in Chapter 3). I conclude with a synopsis of the literature on neighborhood effects, specifically focusing on empirical evidence on the impact of neighborhoods on upward mobility. In each section, I highlight how this paper contributes to the body of knowledge on the subject.

2.1 Residential Segregation

Most directly, this paper contributes to the extensive social science literature on the causes and consequences of residential segregation in the United States. I set out a theoretical model of how segregation occurs, and present a novel empirical strategy for estimating the effect of residential segregation on the long-term outcomes of children who grew up in segregated neighborhoods. Sociological scholarship on residential segregation is forcefully presented in Wilson (1987) and Massey and Denton (1993), who argue that racial residential segregation has had devastating impacts on black inner-city communities and that such segregation has created an "underclass" of American society. The economics literature has followed these important works in seeking to explain why segregation occurs and the implications of segregation for individuals and society at large. As Boustan (2011) provides a comprehensive review of this literature, I include only a summary here.

Cutler, Glaeser, and Vigdor (1999) characterize three possible causes of black-white residential segregation. The first possible cause is termed "self-segregation," which would arise from blacks preferring to live near other blacks. This explanation explains little of the observed segregation (Krysan and Farley 2002; Ihlanfeldt and Scafidi 2002). The second possible cause of "collective action," is segregation arising from organized white efforts to exclude blacks from certain areas through legal or extra-legal methods. The redlining of black neighborhoods after the Great Depression was one form of "collective action" (Massey and Denton 1993). More recently, such efforts may have been implemented through discrimination. Turner et al. (2013) find that while the incidence of housing discrimination has declined since 1989, in 2012, people of color looking for places to live were still informed by realtors of fewer homes and apartments than whites.

The third potential cause arises without coordinated actions; if large numbers of individual whites leave areas as minorities move in, these "individual actions" lead to residential racial segregation in the aggregate. The empirical evidence supports the hypothesis that individual actions, at least during the period of the Great Migration, drove some of the observed segregation. Boustan (2010) develops an instrumental variable strategy to identify exogenous patterns of southern black migration to Northern cities, allowing her to demonstrate that postwar suburbanization from 1940-1970 was driven by white flight in response to black in-migration. In viewing out-migration as individual decisions, theories of neighborhood formation have emphasized the externalities such moves impose on other residents through the resulting change in neighborhood demographics. Due to such externalities, neighborhoods have been shown to exhibit tipping behavior–the rapid neighborhood transition from majority white to majority minority.

Estimating the consequences of segregation is difficult, as segregation is often correlated with many unobservable characteristics of neighborhoods and individuals. Furthermore, as Cutler and Glaeser (1997) make clear, the formation of racial and ethnic enclaves has a theoretically ambiguous effect on their minority residents. Wilson and Portes (1980) find evidence that ethnic enclaves of Cuban immigrants in Miami provided economic returns and encouraged higher investment in human capital among those within the community.

To address the concerns about omitted variable bias and reserve causality, researchers have primarily looked for exogenous variation in segregation driven by city history or geography. Using a variety of instruments for segregation, including the number of rivers and streams passing through a metropolitan area, Cutler and Glaeser (1997) find substantial adverse effects of segregation on a variety of short-term socioeconomic outcomes for African Americans. Ananat (2011) uses the configuration of a city's railroad tracks–measuring the subdivision of the city into neighborhoods by railroads–as an instrument for segregation. She finds that higher degrees of segregation increases racial inequality, and causes cities to have African American populations with higher poverty rates and white populations with lower poverty rates. The results I present here extend this work to further understand the long-run effects of segregation. Narrowing the geography of interest from cities to neighborhoods allows for a more granular understanding of the impacts of segregation on those who grow up in the "ghetto." In this sense, I test the hypothesis of Wilson (1987) and Massey and Denton (1993) that segregation creates an "underclass" with poor prospects for upward mobility.

2.2 Neighborhood Tipping

The identification strategy for this paper builds upon the foundation of previous work on neighborhood tipping. In this paper, I propose that tipping points–threshold values for minority share beyond which whites flee the neighborhood–can be used as an instrumental variable for later segregation, a framework which extends this literature towards the estimation of the causal effects of segregation. The canonical Schelling (1969, 1971, 1972) model of residential segregation showed that residential segregation can emerge in long-run equilibria through tipping point dynamics, even if there are only weak individual preferences for living near people of the same group. When a neighborhood's minority share moves beyond a threshold value, that neighborhood's demographic composition rapidly transitions–tipping to be heavily minority. A large body of literature has advanced these models; I summarize and reference some works in Chapter 3, where I outline a brief model of a local housing market to illustrate the dynamics of tipping. Additionally, these dynamics have been empirically tested. Caetano and Maheshri (2017) estimate tipping points for Los Angeles public schools from 1995 to 2012. CMR (2008) estimate tipping points for large MSA's in each decade from 1970 to 2000. They find that neighborhoods exhibit tipping-like behavior, with a distribution of tipping points ranging from a 5 percent to 20 percent minority share.

A closely related paper within the literature on neighborhood tipping is Böhlmark and Willén (2020), which studies the effect of ethnic segregation between natives and immigrants in Sweden on education and labor market outcomes. Böhlmark and Willén follow the methodology of CMR (2008) to estimate tipping points in the ethnic composition of Swedish neighborhoods between natives and immigrants. They use these tipping points in a regression discontinuity framework to identify how living in a neighborhood which tips affects individual outcomes. The identification of causal effects from within-city deviations in neighborhood composition around an estimated tipping threshold is identical to the identification strategy I employ here. Böhlmark and Willén present only reduced form estimates for the effect of tipping on outcomes; they do not employ tipping points as an instrument for later neighborhood composition. I put the reduced form specifications they develop into a full 2SLS framework, thereby identifying the structural effect of racial residential segregation on upward mobility in the United States. Furthermore, Böhlmark and Willén find no evidence of adverse impacts of tipping on labor market earnings for natives and immigrants in Sweden. In contrast, my results indicate that neighborhood tipping in the United States has important implications for those who grow up in tipped neighborhoods.

2.3 Neighborhood Effects and Upward Mobility

This paper also contributes to the broader literature on the effect of childhood neighborhoods ("neighborhood effects"). I show that residential racial segregation is an important mechanism by which childhood neighborhoods can inhibit upward mobility. Graham (2018) thoroughly reviews the literature on neighborhood effects, with a focus on measuring the effect of neighborhood racial composition on outcomes. The Moving to Opportunity (MTO) project is perhaps the best-known empirical study of neighborhood effects. The MTO program provided randomly assigned rent subsidies to over 4,600 families in public housing to leave for lower poverty neighborhoods between September 1994 and August 1998. These families were assigned to one of three groups: 1) the MTO Low-Poverty Voucher Group, which received rent subsidies usable only in low-poverty neighborhoods, 2) the Traditional Voucher Group, which received unrestricted rent subsidies, and 3) the Control Group, which received no treatment but remained eligible for all programs they already qualified for (Sanbonmatsu et al. 2011). Katz, Kling, and Liebman

(2007) find that the relocations caused substantial short-run reductions in crime and improvements in mental health. Chetty, Hendren, and Katz (2016) revisit the MTO and examine the long-run effects of the relocations. Moving to lower-poverty neighborhoods significantly improved college attendance rates and earnings for children who were below the age of 13 at the time of the move. These children also lived in better neighborhoods as adults and were less likely to become single parents.

Recently, studies of neighborhood effects have sought to understand the impact of neighborhoods on the inter-generational elasticity (IGE) of income and inter-generational mobility. Measuring the IGE of income and inter-generational mobility is the subject of an extensive literature (Black and Devereux 2011; Davis and Mazumder 2019; Mazumder 2005; Hilger 2015; Lee and Solon 2009). But until recently, data constraints have limited the ability of researchers to precisely measure these elasticities for smaller geographic regions. Chetty et al. (2014) use administrative data to estimate the IGE of income and the level of inter-generational mobility for every neighborhood in the United States. Using federal income tax data from 1996 to 2012, they characterize the distribution of parent and child income across the US. Additionally, they estimate a measure of upward mobility at the county, commuting zone (CZ), and MSA level and report correlations between these mobility measures and observable characteristics of CZs. Residential segregation is found to be highly correlated with upward mobility; children who grew up in CZs which are more racially segregated are less upwardly mobile. Chetty and Hendren (2018a) use Census Bureau data to construct these measures of upward mobility at the county level. Chetty et al. (2018) construct the measures of upward mobility at the tract level. In a second paper, Chetty and Hendren (2018b) use a "movers" design to estimate the causal effect of each county and CZ in the United States on children's incomes in adulthood. Using these estimates, they show that growing up in a CZ with one standard deviation higher segregation is associated with (but not causally related to) a 5.2 percentile reduction in children's rank in the national income distribution for families in the 25th income percentile.

Others have begun to build on these geographic measures of opportunity. Abramitzky et al. (2019) study immigrant assimilation and intergenerational mobility by place-of-origin. Rothstein (2019) examines the mediators of intergenerational mobility of income, showing that variation in these measures is better explained by job networks and the structure of local labor markets than by the quality of the educational system. Chetty et al. (2018) study the racial and ethnic disparities in income and inter-generational mobility, finding that differences in family characteristics explain little of the black-white income gap. Derenoncourt (2019) ties together the literature on demographic change and segregation and upward mobility in estimating the effect of the Great Migration on upward mobility at the CZ level. To overcome neighborhood selection issues, Derenoncourt uses the shift-share instrument of Boustan (2010) for urban black population increases during the Migration. She shows that higher black population in CZ that resulted from the Great Migration is associated with significantly reduced upward mobility in those CZs today—as measured by Chetty et al. (2014). As potential mechanisms for this effect, Derenoncourt presents evidence that the Great Migration is associated with decreases in white public school enrollment and urban residence within the commuting zone; higher local government expenditures on police and higher murder rates; and increased rates of incarceration. Her results suggest that the causal effect of the Great Migration explains nearly half of the racial gap in upward mobility.

Chapter 3

Theoretical Framework of Neighborhood Tipping

3.1 A Model of Residential Dynamics

To understand how tipping can be used to identify the causal effect of segregation, it is important to consider how tipping arises. In this chapter, I develop a model in which neighborhoods exhibit tipping dynamics even when whites prefer to live in a neighborhood with some minority neighbors. I adapt the model in Banzhaf and Walsh (2013), which demonstrated the importance of neighborhood public goods in determining tipping. I tie the Banzhaf-Walsh framework to the aggregate one-sided tipping model presented in CMR (2008).

The model consists of two neighborhoods $j \in \{1, 2\}$ which are populated by two types of individuals $g \in \{b, w\}$. Each neighborhood is composed of an set of fixed-size housing stock with measure 0.5. While the housing units are identical, the neighborhoods may have different physical densities–the housing units may be more or less spread out within a neighborhood. In this context, the two groups can be considered blacks and whites. Blacks are the minority group; type b has measure $\beta < 0.5$. Type w (whites) therefore has measure $(1-\beta) > 0.5$. Each individual consumes one unit of the homogeneous housing stock.

The utility function for an individual i of group g when living in neighborhood j is assumed to be a function of (1) non-housing consumption, (2) an exogenous neighborhood amenity level, and (3) endogenous neighborhood demographic composition. Consumption is measured as income (Y) less the price of housing (p_j) in the neighborhood.

Each group values neighborhood amenities differently. For example, blacks may receive a proportionally smaller service flow from the neighborhood amenities than whites due to discrimination. Such a scenario reflects evidence that individual blacks receive less benefit from public goods and neighborhood amenities due to racism and structural inequalities. For example, blacks are more likely to experience non-lethal force in police encounters, thus mitigating the benefits from larger police presences (Fryer 2016, Hoekstra and Sloan 2020). However, it could be the case that whites value neighborhood amenities less than blacks. For example, if whites are more likely to send their children to private school because they tend to have higher incomes, they will receive less utility from living in a neighborhood with high-quality public schools.[1] Finally, whites are assumed to have bliss point preferences with respect to minority share in their neighborhood. Whites prefer to live in neighborhoods with some individuals of each type rather than segregated neighborhoods, but prefer that the minority share of the neighborhood not go beyond a certain percentage. Whites have a baseline preferred minority share $\alpha \in [0, \frac{\beta}{.5}]$, but are sensitive to the density of the housing stock in the neighborhood, with lower densities associated with higher preferred minority shares for a given α. To keep the model simple, blacks have no preference over the demographic composition of their neighborhood.

[1] The value of high-quality public schools could be capitalized into home prices, but that is abstracted away in this model of uniform housing quality.

I model these utility functions as:

$$u_{i,j}^{b} = Y_i - p_j + \nu_b G_j \tag{3.1}$$

$$u_{i,j}^{w} = Y_i - p_j + \nu_w G_j - \rho(\alpha - \psi_j m_j)^2 \tag{3.2}$$

Where $\nu_g \in (0,1)$ is the amenity value parameter, G is the base service level of the neighborhood amenity, and ρ measures the sensitivity of white utility to demographic composition. The minority share in neighborhood j is denoted m_j, and can be defined as $\frac{\mu(S_{bj})}{.5}$, where $\mu(S_{bj})$ is the measure of the set of black individuals locating in neighborhood j. As $m_1 = \frac{\beta}{0.5} - m_2$, the utility functions can be defined in terms of m_2. $\psi_j \in [0,1]$ measures the density of neighborhood j (higher ψ_j implies a denser neighborhood). p_1 is the numeraire, and is normalized to zero to close the model. A white resident has increasing utility with a more diverse neighborhood until a point, and then declining utility. Because neighborhood interactions occur less frequently in sparser neighborhoods, whites have a higher preferred minority share in such neighborhoods. Mathematically, $\frac{\partial u_{i,j}^{w}}{\partial m_j}$ is positive for $m_j \in [0, \frac{\alpha}{\psi_j}]$ and negative thereafter. Because the white share of the neighborhood, w_j, equals $1 - m_j$, white willingness to pay for a higher white share is positive for $w_j \in [0, 1 - \frac{\alpha}{\psi_j})$ and negative for $w_j \in (1 - \frac{\alpha}{\psi_j}, 1]$.

Within each group, income is distributed according to the cumulative distribution functions $F_g(Y)$. I impose two conditions on the income distributions for black and white (type b and w) individuals.

$$F_w(Y) \leq F_b(Y) \; \forall Y \tag{3.3}$$

$$Y_w^{Min} < F_w^{-1}[\frac{.5(1-2\beta)}{1-\beta}] < Y_b^{Max} \leq Y_w^{Max} \tag{3.4}$$

The first condition requires that the population of w individuals has higher income than the population of b individuals in the sense of first-order stochastic dominance. The second condition ensures that there are some individuals of type b who are in the upper half of the pooled income distribution. These conditions are both imposed to have the model better reflect reality and to highlight the interesting cases. The first condition restricts the model to cases in which the minority group will consume lower levels of the public good, on average. The second condition ensures that income alone does not drive segregation in the model; if the neighborhoods stratified only by income, some blacks would live in the higher income neighborhood. Equivalently, if preferences depended only on consumption and the level of the public good, there would be at least some positive measure of type b individuals in the higher public good neighborhood. Without some preferences over demographic composition, segregation would not occur.

Equilibrium in the model is characterized by allocations of individuals across the two neighborhoods and the price of housing in neighborhood 2, p_2. In equilibrium:

1. Housing markets clear. In each neighborhood there is a set of individuals of measure 0.5 which reside in the community.

2. All individuals reside in their preferred community. Formally, for all individuals in neighborhood j, $u^g_{i,j} \geq u^g_{i,-j}$ $\forall i, g$.

Studying the equilibria of such models can be reduced to studying the incomes and willingness to pay of the marginal individuals (between the neighborhoods) in each group. Banzhaf and Walsh (2013) show that a model with the above framework exhibits stratification by income within types. For each type, there is a boundary individual with income $\bar{Y}_g$ such that all individuals of type g with incomes above $\bar{Y}_g$ reside in the community which that type views as more desirable. These boundary incomes implicitly define (for each group separately) the boundary individual's willingness to pay to live in neighborhood 2 rather than neighborhood 1. These marginal bid variables, $Bid_{\bar{Y}_g}$, are the price levels in neighborhood 2 which make the boundary individual for each group indifferent between the two neighborhoods. Equations (3.5) and (3.6) present the implicit definitions of $Bid_{\bar{Y}_g}$, which are generated by setting the utility of the boundary individual (with income $\bar{Y}_g$) from living in neighborhood 1 equal to the utility of that boundary individual from living in neighborhood 2.

$$\bar{Y}_w + \nu_w G_1 - \rho(\alpha - m_1)^2 = \bar{Y}_w - Bid_{\bar{Y}_w} + \nu_w G_2 - \rho(\alpha - m_2)^2 \tag{3.5}$$

$$\bar{Y}_b + \nu_b G_1 = \bar{Y}_b - Bid_{\bar{Y}_b} + \nu_b G_2 \tag{3.6}$$

As CMR (2008) note, at an integrated equilibrium with m_1 and $m_2 \in (0, 1)$, the boundary white individual and the boundary black individual must have the same willingness to pay to live in neighborhood 2. Otherwise, the individual with the higher willingness to pay will bid up the price of housing in neighborhood 2 and establish a new demographic equilibrium. In other words, at an integrated equilibrium, $Bid_{\bar{Y}_b} = Bid_{\bar{Y}_w}$. Solving for $Bid_{\bar{Y}_w}$ and $Bid_{\bar{Y}_b}$ in equations (3.5) and (3.6) implies that at an integrated equilibrium, the following condition holds:

$$\nu_w(G_2 - G_1) + \rho(\alpha - m_1)^2 - \rho(\alpha - m_2)^2 = \nu_b(G_2 - G_1) \tag{3.7}$$

Plugging in $\frac{\beta}{0.5} - m_2$ for m_1, the equilibrium condition can be defined in terms of the endogenous m_2 variable and a set of exogenous variables.

This setup forms a microeconomic foundation for the one-sided tipping model presented in CMR (2008). Studying the shape of the marginal bid functions with respect to m_2 allows us to characterize the demographic dynamics of the two neighborhoods. There are a variety of values of $\{G_1, G_2, \psi_1, \psi_2, \rho, \alpha, \nu, \beta\}$ under which this model exhibits tipping dynamics. I present one such example below and explore the implications for the empirical strategy.

Consider the following schedule of exogenous variables: $\{G_1 = 1, G_2 = 1.1, \psi_1 = 0.5, \psi_2 = 1, \rho = 5, \alpha = 0.15, \nu_b = 0.6, \nu_w = 1, \beta = 0.25\}$. These values for ρ and α are consistent with survey evidence that suggests that whites prefer a neighborhood with a minority share around 10 percent and strongly avoid neighborhoods where the minority share is greater than 25 percent. See Clark (1991) and Farley et al. (1993) for examples which cover the time period studied in this paper. The minority share of the population, $\beta = 0.25$, reflects the Atlanta, Georgia MSA in 1980 (author's calculation from NCDB). Neighborhood 1 is assumed to be half as dense as neighborhood 2. With these values, the marginal bid functions are, after some algebra:

$$Bid_{\bar{Y}_w} = 0.0375 + m_2 - 3.75(m_2)^2 \tag{3.8}$$

$$Bid_{\bar{Y}_b} = .06 \tag{3.9}$$

The first panel of Figure 1 plots these marginal bids as a function of m_2, the minority share in neighborhood 2. The quadratic function is $Bid_{\bar{Y}_w}$ and the line at 0.06 is $Bid_{\bar{Y}_b}$. This figure is the equivalent of Figure II in CMR. The first point where the marginal bid functions are equal (at approximately $m_2 = 0.025$) is a stable integrated equilibrium. If m_2 is perturbed lower from that point, the marginal black bidder outbids the marginal white bidder to move into neighborhood 2, raising m_2 until it returns to the equilibrium. If the minority share is perturbed higher, the marginal white bidder enters the neighborhood until the neighborhood returns to the equilibrium m_2. The second point (at approximately $m_2 = 0.24$) is an unstable integrated equilibrium. If the m_2 is perturbed higher, the marginal black individual moves into the neighborhood, further increasing m_2. As $Bid_{\bar{Y}_b} > Bid_{\bar{Y}_w}$

past this point, blacks continue to move into neighborhood 2 and whites continue to leave the neighborhood until all the black individuals live in neighborhood 2. Neighborhood 2 experiences white flight and minority in-migration which drives segregation. Note that both equilibrium points are feasible. At $m_2 = 0.025$ and $m_2 = 0.24$, the measure of the set of black individuals in each neighborhood is greater than or equal to zero, and less than $\beta = 0.3$.

Panels 2 through 4 of Figure 1 illustrate how changes in the marginal bid functions create tipping dynamics. Panel 2 considers a scenario in which decreasing discrimination against blacks increases ν_b, the black amenity value parameter, thereby increasing $Bid_{\bar{Y}_b}$. Panel 3 then considers a subsequent decrease in ν_w, the white amenity value parameter, that brings down the white marginal bid function until $Bid_{\bar{Y}_b}$ is tangent to the maximum of $Bid_{\bar{Y}_w}$. Denote the the neighborhood 2 minority share which maximizes $Bid_{\bar{Y}_w}$, the white marginal bid function, as m^*. That implies, for $m \in [m^* - d, m^* + d]$ where d is small, the marginal white buyer has a (weakly) lower willingness to pay than the marginal minority buyer. That value, m^*, is thus a "tipping point"; as shown in Panel 4, any further increase in minority demand leaves only the segregated equilibrium. Meanwhile for a city with only slightly lower $Bid_{\bar{Y}_b}$, there would exist a locally stable equilibrium to the left of m^*.

There are two implications of the model which can inform empirical identification. First, the location of m^* depends on the distaste of whites for minority neighbors. In the model, this is represented in the parameters ρ and α. For example, if $\alpha_j = .15$ and thus white demand decreases sharply beyond a minority share of 15 percent, the tipping point will not be much beyond this level of a modest minority share. However, if a city has more tolerant whites– with either a higher α_j or lower ρ_j– the tipping point will be at a higher minority share. Differences across cities in white preferences toward an increasingly diverse demographic composition is what drives variation in tipping points. While small differences in white preferences lead to small differences in tipping points, small differences in relative minority demand (or white demand) can lead to significant differences in the equilibrium demographics for different neighborhoods. That is, small differences in the relative height of the $Bid_{Y_{\bar{w}}}$ and $Bid_{Y_{\bar{b}}}$ (the marginal bid) curves can generate substantial neighborhood tipping. That these small differences between neighborhoods can lead to such divergent outcomes forms the basis of the causal identification for this paper.

To further illustrate how Schelling tipping points are a possible source of random variation in residential segregation, consider two neighborhoods (1 and 2) in city k which are identical in all dimensions (including the shape of their relative demand curves) except neighborhood 1 exists at an integrated equilibrium with minority share m_1 slightly less than m^* and neighborhood 2 exists at an integrated equilibrium with minority share m_2 slightly greater than m^*. These equilibria exist due to the concave and linear shapes of $Bid_{Y_{\bar{w}}}$ and $Bid_{Y_{\bar{b}}}$ outlined above (there are two points of intersection so long as $Bid_{Y_{\bar{b}}}$ has not risen above $Bid_{Y_{\bar{w}}}$). Suppose each neighborhood undergoes the same small relative demand shock at time T which slightly increases relative minority demand to live in the neighborhood. If this shock is small, neighborhood 1 will move to a new equilibrium point m_1' in year T + 10, where $m_1' - m_a$ is small.[2] However, after the shock, the marginal bidder for homes in neighborhood 2 will be a minority, thus increasing the minority share. As the minority share increases, this further increases the gap between the minority bid function and the white bid function, leading to further minority in-migration and white flight. This continues until B arrives at the fully segregated equilibrium $m_b' = 1$. The neighborhood has tipped.

In summary, two neighborhoods (A and B) which are identical in all dimensions, but B has a minority share slightly above the tipping point while A has a

[2] Implicitly assuming that neighborhoods equilibrate over a 10 year period.

minority share slightly below the tipping point, will experience drastically different demographic dynamics. A's demographics will remain fairly stable, while B will experience majority flight, resulting in a neighborhood with high minority share. In this setup, having a minority share beyond the tipping point can potentially be used in a regression discontinuity design as a valid instrument for the neighborhood's future minority share.

3.2 Limitations and Other Tipping Models

This model has the benefit of simplicity while still conveying the key points of the analysis. Nonetheless, it is limited in a number of respects. First, this model considers only a two-neighborhood city. The implications for larger aggregations of neighborhoods are not explored. Furthermore, the algebra of the model implies that tipping arises from the assumption that blacks and whites receive different service flows from the neighborhood amenities. Finally, as the willingness to pay to live in an area depends on the level of public goods in that area, the dynamics of neighborhood tipping depend on the relative levels of public goods.

CMR (2008) find that there is significant variation in estimated tipping points across cities. The model above suggests that some of this variation may be due to differences in public investments in schools, parks, libraries and other civic institutions. While explaining some of the variation in CMR's estimated MSA tipping points, this conclusion also casts doubt upon CMR's assumption that all neighborhoods within a given city have the same tipping point. Differences in public goods between neighborhoods within the same city would lead to different tipping points for those neighborhoods. In the description of the empirical strategy, I explain why the violation of this assumption does not restrict the instrumental variables identification under the interpretation of these differences in tipping points across neighborhoods within cities as random measurement error in the estimated tipping points.

A number of other papers further develop social interaction models which examine neighborhood dynamics and find the possibility for tipping, including: Becker and Murphy (2000), Pancs and Vriend (2007), and Sethi and Somanathan (2004). Additionally, Zhang (2011) develops a general equilibrium model which incorporates the key elements from Schelling's (1969, 1971, 1972) spatial proximity and bounded neighborhood frameworks in a multi-neighborhood setting. He shows that residential segregation could emerge from tipping dynamics and persist even if: "(1) There exists no racial discrimination of any type; (2) both blacks and whites prefer to live in 50–50 neighborhoods; and (3) the socioeconomic disparities between blacks and whites are completely eliminated." Reassuringly, his conclusions broadly match those drawn from the model above.

Chapter 4

Data

The data for this paper are obtained from two sources. Estimated MSA tipping points and Census tract characteristics are drawn from the replication archive for CMR (2008). These are combined with data made available by the Opportunity Insights project that measure upward mobility by Census tract (Chetty et al. 2018). In this chapter, I describe each source in turn, and detail how the datasets were combined to form my final analysis sample. I define key variables and present summary statistics. I conclude with observations on some of the correlates of upward mobility and descriptive evidence on the empirical relevance of tipping points.

4.1 Tipping Points and Tract Characteristics

The data underlying the CMR (2008) estimates are from the 2000 edition of the Neighborhood Change Database (NCDB).[1] The NCDB is a panel of Census tracts from 1970 to 2000 that maps the earlier years' data onto the tract boundaries from the 2000 Census. Census tracts are areas which generally cover between 2,500 and 8,000 people, with boundaries that seek to encompass homogeneous neighborhoods (as best as possible). Census tract usage has increased over time; it

[1] This section relies heavily on CMR (2008).

was not until the 2000 Census that tract-level data was collected for the entirety of the United States. Thus, the CMR sample only includes data on tracts within metropolitan areas, and further excludes cities with fewer than 100 tracts. As residential segregation is generally an urban-suburban phenomenon (but not rural), focusing on large metropolitan areas is appropriate for the research design. In total, 114 MSAs are observed in the CMR sample, comprising 40,462 Census tracts. For each tract, the NCDB records the demographic composition of the tract, from which CMR calculate the white (white non-Hispanic) and minority shares of the population. Additionally the NCDB contains a number of economic measures, such as the employed and unemployed working-age populations, average household income, the poverty rate, and information about the housing stock and prices in the tract.

CMR use this data to estimate tipping points (the point at which the minority share becomes large enough so that the neighborhood tips) for each MSA for the 10 year periods of 1970-1980, 1980-1990, and 1990-2000. As the theoretical model above indicates, the shape of the marginal inverse bid functions for whites and minorities results in neighborhoods beyond some m^* experiencing white flight and minority in-migration. CMR model such changes in the white population within a tract as a function of the minority share. The goal is to identify if and where there is a negative discontinuity in the 10-year change in the white population as a function of the base-year tract minority share. That is, does the white share decline suddenly at some point as the minority share increases. They examine whether neighborhoods in the late 20th century developed as the tipping model predicts, and if so, at what minority share is each city's tipping point? If neighborhoods with a minority share just beyond some m^* experience significantly lower white population growth or larger declines in white population relative to those in the same city with minority share below m^*, this is interpreted as evidence of tipping.

This is the sense in which tipping is a discontinuity.

To empirically identify the tipping points, CMR use a "fixed point" procedure as their preferred approach. Following CMR's notation, let $W_{ck,t}$, $M_{ck,t}$ and $P_{ck,t}$ denote the number of whites, minorities and total population in tract c of city k in year t. $D_{ck,t}$, the ten year change in the white population as a share of the base-year population, equals $\frac{W_{ck,t}-W_{ck,t-10}}{P_{ck,t-10}}$, and the minority share $m_{ck,t}$ equals $\frac{M_{ck,t}}{P_{ck,t}}$. CMR observe that smoothed approximations to $E[D_{ck,t}|c, M_{ck,t-10}]$ have a consistent shape across many different cities. At low minority shares (low $m_{ck,t-10}$), the function is positive and relatively flat, but then the function declines sharply at a minority share which varies by city. After falling, it levels off again until a minority share near 60 percent, at which point it increases toward zero. This sharp decline is interpreted as tipping, and the tipping point is thus a "fixed point." Neighborhoods with minority shares above the tipping point will experience lower white population growth than the city average, while those below the tipping point will experience higher white population growth than average. This empirical approach has the advantage of allowing for secular city-wide demographic trends while still estimating tipping points using the variation from the mean.

Formally, if there is a tipping point for city k at m_k^*, then:

$$E[D_{ck,t}|k, M_{ck,t-10} = m_k^* + \epsilon] > E[D_{ck,t}|c, m_k^*] > E[D_{ck,t}|c, M_{ck,t-10} = m_k^* - \epsilon)] \text{ , } \forall\, \epsilon > 0 \tag{4.1}$$

Thus, a consistent estimator for the tipping point of city k is the root of the continuous approximation for $E[D_{ck,t}|k, M_{ck,t-10}] - E[D_{ck,t}|k]$. CMR use a two-step procedure to smooth the data. For each city separately, $D_{ck,t} - \bar{D}_{ck,t}$ is fit as quartic polynomial of $m_{ck,t-10}$ using all tracts with $m_{ck,t-10}$ below 60 percent. Then, selecting the lowest root of this function, $\tilde{m}_k$, $E[D_{ck,t}|k, M_{ck,t-10}] - E[D_{ck,t}|k]$ is again fit as a quartic polynomial of $m_{ck,t-10}$ using all tracts with $m_{ck,t-10}$ within 10 percentage points of $\tilde{m}_k$. The lowest root of this function, $\hat{m^*}_k$, is the estimated tipping point for city k. The idea is to smooth the data to reveal the point at which it is not smooth.[2] This procedure is performed on a randomly selected two-thirds subsample of tracts within each MSA (the ”search sample“). As these estimated tipping points are then used to identify the magnitude of tipping, splitting the sample avoids specification search bias that would arise from identifying a structural break and testing its magnitude from the same data.[3] For this reason, my analysis sample for this paper is restricted to the ”estimation sample,“ the one-third of tracts which were not used to identify the tipping point locations.

4.2 Upward Mobility

The Opportunity Atlas made available by Chetty et al. (2018) estimates the average adulthood income percentile rank in the national income distribution for children who grew up in nearly every Census tract in the country.[4] These “mean predicted outcomes” are also reported after conditioning on parent income, with the mean predicted outcome for children whose parents were in the 25th percentile of the income distribution being of particular interest in measuring upward mobility. All outcomes are further broken down by race, which allows for studying differential effects of segregation across racial groups.

To construct the Atlas, Chetty et al. combine three sources of anonymized microdata from the Census Bureau: (1) the Census 2000 and 2010 short forms; (2) federal income tax returns in 1989, 1994, 1995, and 1998-2015; and (3) the Census 2000 long form and the 2005-2015 American Community Surveys (ACS). Children from the 1978-1983 birth cohort are linked to their parents based on the first adult

[2]Tipping points are omitted for cities where this polynomial has no root.
[3]A $\frac{2}{3} - \frac{1}{3}$ split is used because the procedure for locating the tipping points is data intensive.
[4]This section draws heavily from Chetty et al. (2018).

to claim the child as their dependent on the 1040 tax form. Children are assigned to 2010 Census tracts by where they lived until the age of 23 (as observed in the tax data). Children who grew up in multiple tracts are assigned to those tracts proportionally to the time they spent there. Excluding children for whom address information is not available leads to a sample of nearly 20.5 million individuals. Parental income is measured as the mean of household income over five years: 1994, 1995, and 1998-2000 (tax records for 1996 and 1997 are unavailable). The children's income is measured as the average of 2014 and 2015 incomes, when the children are between the ages of 31 and 37. These measures of income are transformed into ranks in the national distribution of income for each birth cohort (those born in the same year). Race and gender are observed in the data for both parents and children.

The Atlas seeks to estimate a child's expected income rank, conditional on growing up in tract c with parental household income rank p, racial group r and gender g. That is:

$$\bar{y}_{prgc} = E[y_i|P(i) = p, R(i) = r, G(i) = g, C(i) = c] \; \forall \; p, r, g, c \tag{4.2}$$

However, there are not enough observations to non-parametrically estimate this in each percentile by tract-race-gender cell. Chetty et al. first use a lowess regression of average child income within a percentile-race-gender cell, $\bar{y}_{prg}$, on p, parent income rank, for each race by gender grouping at the national level to generate a transformation of p_i which renders the relationship between p_i and y_i linear on the national level. Child income, y_i is then regressed on the predicted values from this regression, $f_{rg}(p_i)$, for each tract-race-gender cell:

$$y_i = \alpha_{crg} + \beta_{crg} f_{rg}(p_i) + \epsilon_i \tag{4.3}$$

Using the estimated coefficients in this regression, they construct the mean predicted outcomes for children conditional on parent income rank for each race by gender by tract cell. For example, $\hat{\alpha}_{crg} + \hat{\beta}_{crgrg}(25)$ is $\hat{y}_{25crg}$, the mean predicted outcome for a child of race r and gender g who grew up in tract c with parents at the 25th percentile of household income. These mean predicted outcomes are also generated for tract by parental income rank by: pooled gender by race, pooled race by gender, and pooled gender by pooled race. Chetty et. al. (2018) consider the $p = 25$ mean predicted outcomes to be the preferred measure of upward mobility, capturing an estimate of how well children from low-income families are able to move up the socioeconomic ladder. The 25th percentile of the parental income distribution corresponds to a family income of roughly $27,000 (in 2015 dollars). With this interpretation in mind, I use the term ″upward mobility“ to describe the $p = 25$ mean predicted outcomes in this paper.

The mean predicted outcomes for each tract are publicly available for a set of parental income ranks, including $p = 25$ and $p = 50$. Due to federal disclosure standards, Opportunity Insights did not release estimates for cells (tract by race by gender) with 20 or fewer children. Furthermore, independent, normally distributed errors are added to the estimates to protect privacy. The size of the ″noise“ depends on the sensitivity of the estimates to a single individual's data, following the literature on statistical disclosure limitation. In addition to the mean predicted outcomes, the Atlas reports the number of observations used to estimate each cell, and the mean household income rank (parent income percentile) for each tract by race-gender cell.

Chetty et al. (2018) document the wide variation in these mean predicted outcomes. Pooling across all racial groups, there is a standard deviation for upward mobility of 6.2 percentiles. Upward mobility is highly varied across geographies, with 32 percent of the variation in pooled upward mobility accounted for at the

CZ level, while within-CZ county differences explain 13.5 percent of the variation. The remaining 54.5 percent of the variation is explained by within-county differences between tracts, indicating the importance of neighborhood characteristics, such as segregation, in determining upward mobility.

4.3 Analysis Sample

The final analysis sample for this paper comprises over 12,000 Census tracts, which are defined and identified according to the 2000 Census tract boundaries and FIPS codes. Each tract is also identified as being located within an MSA. For each tract, I observe a set of measures of upward mobility for various race-gender cells drawn from the Opportunity Atlas. Additionally, I observe a series of tract economic and demographic characteristics (for example: minority share, average household income, and the poverty rate) from 1970, 1980, and 1990. These tract characteristics are extracted from the NCDB.[5] Finally, each tract has a corresponding estimated tipping point, which is drawn from CMR (2008). As these tipping points are estimated at the MSA level, tracts within the same MSA have the same estimated tipping point.

To construct the analysis sample, I begin by merging the data sources. Both the CMR and Opportunity Atlas datasets are available at the tract level with FIPS identifiers, but the CMR data is based on the 2000 Census tract boundaries while the Opportunity Atlas reports mean predicted outcomes for the 2010 Census tract boundaries. To merge the sources, I first crosswalk the Opportunity Atlas to the 2000 Census tract boundaries using a crosswalk algorithm based on the Longitudinal Tract Data Base constructed by Logan et al. (2014). This crosswalk produces estimates of the mean predicted outcomes (including the measures of upward mo-

[5] The dataset is not a panel. Each observation corresponds to one tract, with separate variables for the 1970, 1980, and 1990 characteristics.

bility) that can be merged with the tipping points and demographic data from the 2000 NCDB in the CMR replication archive. The algorithm crosswalks the mean predicted outcomes for each 2010 tract to the corresponding 2000 tract(s), allocating ″count“ variables (e.g. population) to the various 2000 tracts proportionally to the share of the population of the 2010 tract residing in the 2000 tract, and weights ″mean“ variables (e.g. mean predicted outcomes) proportionally to user inputted weights.[6]

I perform the crosswalk back to the 2000 tracts, rather than forward to the 2010 tracts, because the Opportunity Atlas data provides well-documented variables for the number of people used to calculate each mean predicted outcome which are easily used as weights in the crosswalk algorithm. There are nearly 25,000 tracts for which the Opportunity Insights reports mean predicted outcomes but that are not included in the CMR data. There are only 23 tracts which are observed in the CMR data but not in the Atlas. This is consistent with the sample definitions. The Atlas contains all tracts for which there are more than 20 children in an outcome cell, a criteria which would be met by tracts in smaller metropolitan areas which are omitted from the CMR sample. It is important to note that the crosswalked variables are estimates, rather than one-to-one transformations. Some of the resulting observations are clearly incorrect. For example, there are predicted income percentiles, which should be measured from zero to 100, which are very negative or in the millions after the crosswalk. For the analysis sample, I restrict the observations to those tracts for which the upward mobility estimates are between zero and 100 (otherwise the regression functions below reflect obvious outliers).[7] In the summary statistics below, I explore how this restriction affects the composition of the sample. The main results of the paper are robust to minor changes in the

[6]See Logan et al. (2014) for additional information on the construction of these shares.

[7]I restrict the observations to those tracts for which the $p = 25$, $p = 50$ and mean household rank variables are between zero and 100.

boundaries of the sample restriction.

As the Opportunity Atlas observes the 1978-1983 birth cohorts, those who would have been in the middle of childhood in 1990 (seven to twelve years old), I study the effects of living in a segregated tract in 1990, and instrument for 1990 segregation with tipping that occurred from 1980 to 1990. The key right-hand side variables are the minority share in 1990 ($m_{c,90}$) in Census tract c (which is used as the proxy for segregation), the minority share in 1980 ($m_{c,80}$) and the estimated tipping point for the MSA k which the tract is in ($\hat{m^*}_k$). I construct a variable, Dev_c, measuring the tract's deviation in minority share from its tipping point, such that $Dev_c = m_{c,80} - \hat{m^*}_k$. A Tract was past the tipping point in 1980 if Dev_c is positive, the tract was below the tipping point in 1980 if Dev_c is negative. $T_c \in \{0, 1\}$ is an indicator variable for a tract beyond the tipping point. The tipping point is thus normalized to zero (in terms of Dev_c) for all MSAs. The outcomes of interest (the left-hand side variables) for the analysis are the upward mobility measures from the Atlas.

Table 4.1 presents summary statistics for the upward mobility measures and Table 4.2 presents summary statistics for the minority share, tipping point and tract characteristics variables. In each table, column one calculates the statistics over the entire sample, including those tracts with upward mobility measures that are not between zero and 100. Column two calculates the statistics including only the tracts with upward mobility measures between zero and 100. Column three further restricts the sample to only those tracts which were not used to identify the location of the tipping points. This corresponds to the analysis sample. The means of the upward mobility measures in the full sample (as reported in column one) clearly reflect the noise introduced in the crosswalk. For example, the mean overall upward mobility would imply that the average tract's mean predicted income rank for low-income ($p = 25$) children is the 785th percentile.[8] Furthermore, there is a standard deviation on that variable of over 1,196 percentiles. The data on upward mobility for black females is the most egregiously skewed by crosswalk errors, with a mean of the 140,000th percentile and a standard deviation of over 167 million percentiles. Reassuringly, these distributions are the result of crosswalk errors in a relatively small fraction of observed tracts. Restricting the sample to tracts with upward mobility measures between zero and 100 involves dropping fewer than 4,000 tracts, and all MSAs are still observed in the sample. As column 2 of Table 4.2 indicates, the characteristics of the tracts in the sample are similar on average after the restriction. Furthermore, the mean and variance of the upward mobility measures mirrors the findings of Chetty et al. (2018). The average overall upward mobility for low-income children is the 43rd percentile of the household income distribution, with a standard deviation across tracts of 7.6 percentiles. Whites are generally more upwardly mobile than blacks, with whites from the average tract reaching the 47th percentile and blacks on average reaching only the 35th percentile. Black men are the least upwardly mobile group on average, with the average tract's mean predicted outcome for low-income black men at the 32nd percentile; this represents an average generational advancement in the income distribution of only seven percentiles.

The summary statistics for the analysis sample are presented in column 3 of the two tables. For the analysis sample, I restricts the data to the random one-third subsample of tracts which were not used by CMR to identify the location of the tipping points. This leaves an analysis sample of over 12,200 tracts across the United States. Given the random assignment of tracts to the search and estimation samples, it is not surprising that the characteristics of these tracts (in column 3) are,

[8]The Atlas measures income percentiles from 0 to 1 (i.e. a mean predicted outcome of 0.5 corresponds to the 50th percentile). For clarity, I have multiplied these values by 100 so that, in my dataset, a mean predicted outcome of 50 corresponds to the 50th percentile.

Table 4.1: Summary Statistics: Upward Mobility

UM: Race, Gender	(1) Full Sample	(2) UM: Zero to 100	(3) Analysis Sample
UM: Pooled, Pooled	785.23	43.27	43.30
	(119,685.69)	(7.67)	(7.65)
UM: Pooled, Male	778.55	41.34	41.39
	(115,853.54)	(8.02)	(8.04)
UM: Pooled, Female	845.57	45.28	45.30
	(128,015.43)	(8.39)	(8.33)
UM: White, Pooled	2,766.37	47.14	47.24
	(215,982.02)	(7.69)	(7.67)
UM: Black, Pooled	954,075.10	34.76	34.68
	(123,827,962.75)	(6.95)	(6.90)
UM: White, Male	19,109.12	45.67	45.77
	(2,915,213.05)	(8.26)	(8.23)
UM: Black, Male	852,665.06	31.66	31.43
	(96,626,911.37)	(6.97)	(6.81)
UM: White, Female	21,338.29	49.12	49.20
	(3,098,861.30)	(8.93)	(8.83)
UM: Black, Female	1,479,454.65	36.79	36.69
	(167,036,579.58)	(7.03)	(6.84)
Observations	40,327	37,781	12,240
MSAs	114	114	113

Notes: Means; sd in parentheses. Each observation is one Census tract. Data from Opportunity Insights Opportunity Atlas (Chetty et al. 2018). Converted to 2000 Census tract boundaries using crosswalk from Logan et al. (2014). UM refers to upward mobility. Upward mobility denotes the mean predicted percentile rank in adult income distribution for children in 25th percentile of family income. Column 1 reports summary statistics for the entire sample after the crosswalk. Column 2 drops observations which reflect obvious crosswalk errors. Specifically, all observations with any upward mobility measure below zero or above 100 (the range of possible true predicted values for percentile rankings) are excluded. Column 3 restricts the sample in Column 2 to only those tracts which are in the estimation sample from CMR.

Table 4.2: Summary Statistics: Tipping and Tract Characteristics

	(1) Full Sample	(2) UM: Zero to 100	(3) Analysis Sample
Past TP (T_c)	0.47	0.47	0.46
	(0.50)	(0.50)	(0.50)
Deviation from TP (Dev_c)	7.88	7.61	7.69
	(27.87)	(27.56)	(27.62)
Tipping Point ($\hat{m}_k^*$)	14.00	14.03	14.07
	(9.63)	(9.63)	(9.66)
Minority Share 1980	23.38	23.16	23.24
	(29.23)	(28.99)	(29.04)
Minority Share 1990	29.00	28.88	29.28
	(30.97)	(30.77)	(30.89)
Avg. Household Income 1970	12,142.32	12,128.39	12,114.53
	(4,534.86)	(4,415.42)	(4,177.61)
Avg. Household Income 1980	24,714.71	24,697.37	24,680.39
	(9,551.05)	(9,289.55)	(9,168.30)
Avg. Household Income 1990	47,263.66	47,081.61	47,151.00
	(23,952.73)	(23,352.66)	(23,277.41)
Unemployment Rt 1970	4.31	4.29	4.29
	(2.80)	(2.71)	(2.67)
Unemployment Rt 1980	6.57	6.52	6.52
	(4.63)	(4.47)	(4.43)
Unemployment Rt 1990	6.86	6.78	6.79
	(5.73)	(5.41)	(5.43)
Poverty Rt 1970	10.33	10.19	10.14
	(9.31)	(9.06)	(9.05)
Poverty Rt 1980	11.36	11.15	11.13
	(11.12)	(10.74)	(10.70)
Poverty Rt 1990	12.50	12.29	12.27
	(12.92)	(12.46)	(12.52)
Observations	40,439	37,893	12,273
MSAs	114	114	113

Notes: Means; sd in parentheses. Data from 2000 NCDB extracts. Average household income variables are reported in nominal US dollars. Column 1 reports summary statistics for the entire sample after the crosswalk. Column 2 drops observations which reflect obvious crosswalk errors. Specifically, all observations with any upward mobility measure below zero or above 100 (the range of possible true predicted values for percentile rankings) are excluded. Column 3 restricts the sample in Column 2 to only those tracts which are in the estimation sample from CMR on average, similar to the overall sample (column 2). While I still observe most of the large metropolitan areas from the CMR data, the Lexington, Kentucky MSA is no longer observed in the final analysis sample. This is not a major concern for the validity of the results, as the empirical strategy identifies the parameters off within-MSA variation.

In the analysis sample, there is a wide distribution of tipping points across MSAs. The average tipping point across the 113 observed MSAs is a minority share of 13.8 percent; the minimum non-zero tipping point is a minority share of 1.3 percent, estimated for the Jackson, Mississippi MSA; the maximum tipping point is a minority share of 45.7 percent, estimated for the Stockton-Lodi, California MSA. Below, I briefly summarize the correlations between city characteristics and estimated tipping points. Developing a better empirical understanding of the sources of the variation in the location of tipping points across MSAs is an interesting topic for future research, but is not necessary for understanding the empirical strategy of this paper. Here, I estimate parameters from within-MSA variation, specifically exploiting the deviation of tract minority shares from their estimated MSA tipping point. On average, tracts had minority shares 7.7 percentage points beyond their MSA's tipping point in 1980. Averaging masks a skewed distribution; the standard deviation on Dev_c (the tract's minority share as a deviation from the tipping point) is nearly 30 percentage points, and while the average Dev_c is positive, 54 percent of tracts have minority shares which are below their $\hat{m}^*{}_k$ (the tract's MSA estimated tipping point). Over half (6,441 out of 12,273) of the tracts are within a 10 percentage point bandwidth around their tipping point.

4.4 Correlates of Upward Mobility and Tipping Points

In this section, I examine the historical (1970-1990) neighborhood characteristics that are correlated with upward mobility and the correlations between city characteristics and estimated tipping points. Understanding the observational associations between neighborhood characteristics and upward mobility is an important first step in any work which seeks to identify what creates good neighborhoods for low-income families. Chetty et al. (2018) correlate upward mobility with neighborhood characteristics drawn from a variety of data sources; this section serves as a complement to that work. Similarly, analyzing associations between city characteristics and the CMR estimated tipping points provides additional context for the data.

Columns 1 through 3 of Table 4.3 present the linear correlations between tract characteristics from 1970 to 1990 and measures of upward mobility for the tracts in the analysis sample. For brevity, I report only the correlations for the upward mobility measures decomposed by race and pooled across genders. The direction of the correlations are in line with reasonable *a priori* expectations. Children from low-income families who grow up in neighborhoods with lower average household, higher unemployment, and more poverty tend to have worse outcomes in terms of income rank in adulthood; these relationships hold across racial groups. The correlations increase over time, with the strongest relationships between the 1990 variables and upward mobility. This likely reflects the importance of a child's environment in adolescence on long term-outcomes. Additionally, the correlations with neighborhood characteristics are strongest for the overall (pooled) upward mobility measure. One possible explanation is that black and white upward mobility are also influenced by (unobserved) race-specific factors that vary across tracts in ways that are not well correlated with these neighborhood characteristics, leading to weaker linear correlations with neighborhood characteristics.

Table 4.3: Correlates of Upward Mobility (UM) and Tipping Points

	UM:Pooled	UM:White	UM: Black	Tipping Point
Minority Share 1990	-0.45	-0.23	-0.12	0.44
Minority Share 1980	-0.44	-0.22	-0.13	0.45
Avg. Household Income 1990	0.55	0.49	0.31	0.05
Avg. Household Income 1980	0.52	0.45	0.27	-0.03
Avg. Household Income 1970	0.47	0.41	0.23	-0.04
Unemployment Rt 1990	-0.46	-0.35	-0.16	0.12
Unemployment Rt 1980	-0.42	-0.32	-0.18	0.02
Unemployment Rt 1970	-0.22	-0.19	-0.12	0.16
Poverty Rt 1990	-0.52	-0.39	-0.23	0.18
Poverty Rt 1980	-0.47	-0.33	-0.19	0.20
Poverty Rt 1970	-0.41	-0.27	-0.16	0.24
Observations	12,240	11,184	7,078	110
MSAs	113	113	113	110

Notes: Each cell reports the (unweighted) linear correlation between each pair of variables. Column 1-3: Each statistic calculated at the tract level for tracts in the analysis sample. Column 4: Correlations reported at the MSA level because tipping points are estimated by MSA. Row variables are aggregated to the city level by taking within-city averages weighted by tract-population.

Minority share is strongly correlated with less upward mobility; the correlation coefficient between the tract's 1990 minority share and overall upward mobility is -0.45. Insofar as the 1990 minority share is a proxy for tract-level segregation, this indicates that neighborhood segregation is associated with lower upward mobility. Two cautionary remarks are necessary here. First, in this context, minority share is a poor proxy for segregation. Both a minority share of zero and 100 would indicate high levels of segregation– the tract is all-white or all-minority. Second, this observational relationship is almost certainly not a valid causal estimate. The tract's 1990 minority share is not exogenously determined, but rather reflects sorting into neighborhoods in a manner which is correlated with the potential outcomes for the children who grow up in those neighborhoods. We can observe some of this bias in the dataset; the 1990 minority share is negatively correlated with the tract's average household income in 1990, which is positively correlated with upward mobility. This would lead to negative bias in an OLS regression of upward mobility on 1990 minority share, overstating the detrimental impact of higher minority share on upward mobility. In addition to being related to tract characteristics that are observed in the data, neighborhood sorting is likely correlated with unobservable characteristics of the individuals in those neighborhoods. This discussion derives a well-established difficulty in this literature: to estimate the causal effect of segregation on outcomes, it is necessary to identify sources of exogenous variation in neighborhood composition.

Column 4 of Table 4.3 reports linear correlations between the MSA-level estimated 1980 tipping points and MSA characteristics. The MSA characteristics are constructed from population-weighted averages of the tract characteristics. The 1980 tipping points are weakly associated with city economic characteristics. The highest correlation (in absolute value) is between tipping points and the poverty rate in 1970. In contrast, the location of the tipping point is associated with the city's minority share. The correlation between 1980 MSA minority share and the tipping point is 0.45, and the correlation between the 1990 MSA minority share and the tipping point is 0.44. This relationship is consistent with the tipping model outlined in Chapter 3. As cities with more minorities may be more likely to have whites who are less intolerant on racial issues, their tipping points may also be higher. Further understanding what drives the variation in tipping points across MSAs is a fruitful topic for future research. However, such cross-MSA variation does not bias the empirical results below. The identification strategy of this paper includes MSA fixed effects to absorb variation between MSAs. Coefficients are estimated from differences between tracts within the same MSA– and these are tracts which have the same estimated tipping point.

4.5 Empirical Relevance of Tipping Points

Neighborhood tipping is a plausibly exogenous source of variation in neighborhood demographics because tracts directly on either side of the tipping point should be similar socioeconomically, but will likely experience different demographic dynamics over the following ten years. The empirical strategy employs a regression discontinuity design around the tipping point to instrument for 1990 minority share in examining the effect of segregation on upward mobility. For this approach to work, having a minority share above or below the tipping point must be empirically relevant for both the tract's 1990 minority share and upward mobility measures. That is, being past the tipping point in 1980 must be correlated with both the 1990 minority share and upward mobility. Preliminary descriptive evidence below suggests that tipping matters for neighborhood composition and upward mobility. Below, I discuss how this evidence informs the empirical specification of this paper.

Tables 4.4 and 4.5 present covariate balance tables for the two subsamples of the analysis sample with $T_c = 0$ and $T_c = 1$ (1980 minority share less than the tipping point and 1980 minority share past the tipping point). Table 4.3 summarizes the upward mobility measures across the subsamples, while Table 4.4 summarizes the tract demographics and economic measures. Tracts that are past their tipping point in 1980 have significantly higher average minority shares in 1990, consistent with the model of neighborhood tipping in which "tipped" neighborhoods experience white flight and minority in-migration. The average 1990 minority share in tracts past their tipping point is over 51 percent, while the average 1990 minority share in tracts that are not past their tipping point is only 10 percent. Furthermore, tracts that are past their tipping point in 1980 have significantly lower upward mobility for every race-gender grouping. Tracts that are past their tipping point have an average overall upward mobility nearly five percentile ranks lower than those

Table 4.4: Comparing Upward Mobility (UM) Across the Tipping Point

Variable	(1) $T_c = 0$	(2) $T_c = 1$	(3) Diff
UM: Pooled, Pooled	45.57	40.65	-4.92***
	(6.55)	(7.99)	(0.00)
UM: Pooled, Male	43.79	38.58	-5.21***
	(7.01)	(8.26)	(0.00)
UM: Pooled, Female	47.47	42.76	-4.71***
	(7.53)	(8.50)	(0.00)
UM: White, Pooled	48.16	45.92	-2.24***
	(6.78)	(8.61)	(0.00)
UM: Black, Pooled	35.56	34.07	-1.49***
	(7.27)	(6.56)	(0.00)
UM: White, Male	46.50	44.59	-1.91***
	(7.53)	(9.11)	(0.00)
UM: Black, Male	32.35	30.95	-1.40***
	(7.27)	(6.50)	(0.00)
UM: White, Female	49.96	47.97	-2.00***
	(8.10)	(9.79)	(0.00)
UM: Black, Female	37.22	36.41	-0.82***
	(6.71)	(6.89)	(0.00)
Observations	6,605	5,668	12,273

Notes: Data from Opportunity Insights Opportunity Atlas, converted to 2000 Census tract boundaries using crosswalk from Logan et al. (2014). Data is restricted to observations in the analysis sample. Column 1 restricts sample to tracts which have minority share lower than their estimated tipping point in 1980. Column 2 restricts sample to tracts which are past their estimated tipping point in 1980. In Columns 1 and 2, standard deviations are in parentheses Column 3 reports difference in means between the columns, with robust p-values in parentheses.

Table 4.5: Comparing Tract Characteristics Across the Tipping Point

Variable	(1) $T_c = 0$	(2) $T_c = 1$	(3) Diff
Deviation from TP	-8.59	29.95	38.54***
	(8.23)	(29.26)	(0.00)
Tipping Point	14.75	13.13	-1.62***
	(10.24)	(8.73)	(0.00)
Minority Share 1980	6.16	43.15	36.99***
	(6.07)	(32.36)	(0.00)
Minority Share 1990	10.34	51.36	41.01***
	(10.42)	(32.17)	(0.00)
Avg. Household Income 1970	13,267.97	10,819.59	-2,448.38***
	(4,276.29)	(3,653.48)	(0.00)
Avg. Household Income 1980	27,764.54	21,080.49	-6,684.04***
	(9,229.19)	(7,661.52)	(0.00)
Avg. Household Income 1990	53,665.04	39,540.91	-14,124.13***
	(23,800.41)	(20,141.10)	(0.00)
Unemployment Rt 1970	3.67	4.99	1.31***
	(2.10)	(3.05)	(0.00)
Unemployment Rt 1980	5.19	8.06	2.87***
	(2.90)	(5.33)	(0.00)
Unemployment Rt 1990	4.59	9.34	4.75***
	(2.35)	(6.73)	(0.00)
Poverty Rt 1970	6.88	13.79	6.91***
	(4.93)	(11.01)	(0.00)
Poverty Rt 1980	6.38	16.67	10.29***
	(4.60)	(12.89)	(0.00)
Poverty Rt 1990	6.67	18.81	12.15***
	(5.68)	(14.92)	(0.00)
Observations	6,605	5,668	12,273

Notes: Data from NCDB. Data is restricted to observations in the analysis sample. Column 1 restricts sample to tracts which have minority share lower than their estimated tipping point in 1980. Column 2 restricts sample to tracts which are past their estimated tipping point in 1980. In Columns 1 and 2, standard deviations are in parentheses Column 3 reports difference in means between the columns, with robust p-values in parentheses.

which are not past their tipping point. Notably, growing up in a tipped tract seems to be worse for low-income whites than low-income blacks. Growing up in a tract past the tipping point is associated with a 2.2 percentile decrease in income rank for white children at $p = 25$, compared to a 1.5 percentile decrease for black children at $p = 25$; a similar pattern exists for males and females. These statistics suggest (at least preliminarily) that tipping is an empirically relevant phenomenon. Tipping into segregation matters for the lives of the children living in the neighborhoods as they tip.

While illustrative, such descriptive statistics cannot be interpreted as the causal effect of growing up in a tract with a 1980 minority share past that tract's tipping point on either the 1990 minority share or on upward mobility. Simple tests of differences in means across these subsamples may be comparing a tract in one city with a minority share of 7 percent and tipping point of 8 percent to a tract with a minority share of 40 percent and tipping point of 35 percent. Insofar as these places are different in ways that may be correlated with whether or not a tract is beyond the tipping point, such correlational tests do not identify causal effects. Indeed, tracts that are past their tipping point have significantly lower average household income, higher unemployment rates, and higher poverty rates in 1970 and 1980– the ”pre-treatment“ period. Additionally, these tests compare neighborhoods in places like New York and California with neighborhoods in places like Jackson, Mississippi; it is doubtful that these are ”apples-to-apples“ comparisons. To estimate causal effects, it is important to absorb cross-MSA variation and compare tracts within the same MSA.

Chapter 5

Empirical Strategy

I estimate the effect of segregation on upward mobility using variation in neighborhood composition caused by the tipping of neighborhoods from majority-white to high minority shares after passing a threshold minority share (the tipping point). My approach compares the upward mobility measures for Census tracts that experienced white flight and minority in-migration due to tipping with the upward mobility measures of Census tracts whose demographics remains stable, restricting these comparisons to tracts within the same MSA. The identification strategy assumes that, within cities, tracts have minority shares on one side of the tipping point or the other in a manner which is otherwise unrelated to their potential for upward mobility and assumes that changing neighborhood composition is the only mechanism through which tipping affects upward mobility. I examine these assumptions by analyzing the distribution of other pre-tipping (1980) and post-tipping (1990) tract characteristics around the tipping point.

Using the notation from Chapter 4, let c index Census tracts, k index MSAs, and $t \in (80, 90)$ denote the decade. Additionally, let $r \in (pooled, white, black)$ and $g \in (pooled, male, female)$ denote the race and gender cell of the upward mobility measures y_{crg}. The estimated tipping point for MSA k is $\hat{m}^*{}_k$, the deviation of tract c's 1980 minority share from its tipping point is Dev_c, and T_c is an indicator for if this deviation is greater than or equal to zero. The variable $m_{c,90}$ is tract c's minority share in 1990. The causal parameter I seek to estimate is the effect of tract segregation on upward mobility (y_{crg}); the structural equation for an OLS regression of upward mobility on the proxy for segregation, 1990 minority share, would be:

$$y_{crg} = \alpha^{OLS} + \beta^{OLS} m_{c,90} + \epsilon_c^{OLS} \tag{5.1}$$

However, as discussed in Chapter 4, $m_{c,90}$ is almost certainly correlated with ϵ_c^{OLS}, biasing estimates of β^{OLS}. Given that tracts with higher minority shares tend to have lower average incomes, higher poverty rates and higher unemployment, OLS estimates are likely to be biased downwards, overstating the negative effect of a higher minority share on upward mobility.

To overcome this issue, I exploit the quasi-experimental variation in demographic composition induced by neighborhood tipping. My reduced form specification for outcome y_{crg} takes the form:

$$y_{crg} = \delta T_c + \theta_1 Dev_c + \theta_2 Dev_c \times T_c + \lambda_{k(c)} + \epsilon_c \tag{5.2}$$

Where $\lambda_{k(c)}$ are MSA fixed effects. In the terminology of regression discontinuity, Dev_c is the "running variable," and $Dev_c = 0$ is the "threshold" value. The parameter of interest, δ, captures the discontinuity in upward mobility at the tipping point; that is, the average treatment effect of having a minority share past the tipping point for neighborhoods near the tipping point. I calculate heteroskedasticity-robust standard errors clustered at the MSA-level.

The intuition behind this approach is that individuals living in tracts just above the tipping point should be similar to individuals living in tracts within the same city just below the tipping point. However, the neighborhoods past the tipping point will experience a different change in neighborhood demographic composition from those tracts that have not tipped. Formally, the key identification assumption is that treatment assignment (being on either side of the tipping point) within cities is as good as random around the tipping point; that $E[T_c\epsilon_c|k] = 0$. While this assumption cannot be tested directly, I present a series of robustness checks that test for differences in observable characteristics for tracts on either side of the tipping point. Additionally, for δ to be a valid causal estimate, the functional form of equation (5.2) must correctly specify the relationship between the conditional mean of tract upward mobility (the outcome variable) and the deviation from the tipping point (the running variable).

The parameter δ in equation (5.2) measures the impact of having a minority share past the tipping point in 1980 on upward mobility. To study the effect of segregation on upward mobility, I estimate an instrumental variables (IV) model where the discontinuity in tract characteristics around the 1980 tipping point is used as an instrument for 1990 minority share. Specifically, I estimate:

$$m_{c,90} = \gamma T_c + \theta_1^m Dev_c + \theta_2^m Dev_c \times T_c + \lambda_{k(c)}^m + \epsilon_c^m \tag{5.3}$$

$$y_{crg} = \beta m_{c,90} + \theta_1^y Dev_c + \theta_2^y Dev_c \times T_c + \lambda_{k(c)}^y + \epsilon_c^y \tag{5.4}$$

Where equation (5.3) is the "first stage" equation that relates 1980 tipping to 1990 minority share, and equation (5.4) shows the "second stage" equation that relates 1990 minority share to upward mobility. Both equations include the linear controls for Dev_c on either side of the tipping point and MSA fixed effects. I calculate heteroskedacticty-robust standard errors clustered at the MSA level.

For β to estimate a causal effect, the discontinuity at the tipping point must be a valid instrument for 1990 minority share. The four conditions for instrument validity in this context are:

1. Randomness – conditional on the controls, having a tract minority share on one side or another of the tipping point is uncorrelated with the error term in the first stage ($E[T_c \epsilon_c^m] = 0$).

2. Satisfies the exclusion restriction – the only mechanism through which tipping affects upward mobility is through changes in demographic composition.

3. Not a weak instrument – there is a discontinuity in 1990 minority share at the tipping point ($\gamma \neq 0$).

4. The response function of upward mobility to minority share as tracts are farther from their tipping point is well approximated by the linear controls for Dev_c.

While assumptions 1 and 2 cannot be directly tested, in addition to the robustness checks for discontinuities in other pre-tipping (pre-treatment) tract characteristics around the tipping point, I present a series of tests for discontinuities around the tipping point in post-tipping characteristics other than 1990 minority share to examine assumption 2. Assumption 3 can be directly tested by examining the first-stage F statistic. Finally, assumption 4 is less plausible for observations further from the discontinuity. As $|Dev_c|$ gets larger, local linear approximations to possible non-linearity in the response functions on either side of the tipping point break down. To ensure that the results are not driven by observations far from the discontinuity, I re-estimate the models, restricting the sample to tracts with a minority share within a bandwidth around their estimated tipping point. These robustness checks are presented in Chapter 7.

In addition to detailing the conditions under which β, the coefficient on 1990 minority share in the second-stage equation, estimates a causal effect, the interpretation of β also merits explanation. The 2SLS estimator with valid instruments captures the average treatment effect for individuals whose treatment was influenced by the instrument (Angrist and Imbens 1994). Understanding the estimated β as this local average treatment effect (LATE) is fundamental for the empirical strategy. In the 2SLS specification above, "treatment" refers to the tract's 1990 minority share. The coefficient β estimates the effect on upward mobility of a higher minority share for those tracts whose higher minority shares were the result of neighborhood tipping from a mostly white tract to a higher concentration of minorities. Tracts that tipped after 1980 experienced a combination of white flight and minority inflows– residential segregation – which is measured by the minority share in 1990. Under the LATE interpretation of this 2SLS model, the 1990 minority share is an apt proxy for measuring residential segregation because the coefficient on it measures the impact on upward mobility of experiencing the segregationary forces of white flight and minority clustering.

Finally, the estimated $\hat{\beta}$ from the IV model will be a consistent estimator for the true β even if the estimated tipping points include random measurement error. Consider the tipping model of Banzhaf and Walsh (2013), in which they show that differences in public goods across neighborhoods within cities imply that different neighborhoods within a city can have different tipping points. In this case, the estimated tipping points from CMR, $\hat{m}_k^*$, would reflect the median tipping point within an MSA and each tract would have its own tipping point $m_c^* = \hat{m}_k^* + \tau_c$. Thus, when determining whether a tract had tipped or not, we may observe a tract that has a minority share less than the estimated tipping point ($m_{1980} - \hat{m}_k^* < 0$) but past its true tipping point ($m_{1980} - m_c^* > 0$), and assign it $T = 0$ even though it had tipped. And under the assumption that these errors happen randomly (for example, if τ_c is randomly assigned to each tract), the error in T_c is termed misclassification error (Aigner 1973).

While such misclassification error will bias the first stage and reduced form coefficients toward zero, the 2SLS estimates will be unaffected so long as the observed instrument (which includes the misclassification error) is not a weak instrument. The intuition for this result is as follows: Assume that the misclassificaton error in the instrument is randomly assigned, and that the true (unobserved) instrumental variable is a valid instrument. The observed instrumental variable is still uncorrelated with the error because it is the sum of the true valid instrument and an independent random variable. Thus, the observed instrument will still satisfy the randomness and exclusion restriction conditions. The misclassication error will weaken the instrument, as it will bias the first-stage parameter (γ) toward zero. However, the weak instrument condition can be easily tested empirically. Additionally, the measurement error will introduce additional variability into the asymptotic distribution of the IV estimates, reducing the power of the model. The bias and consistency of the estimator are unchanged. A formal proof of this result is presented in Appendix A.

Chapter 6

Results

6.1 Reduced Form Results

Figures 2 and 3 present graphical evidence for the effect of neighborhood tipping on upward mobility. In each plot, the horizontal axis denotes the deviation of the tract's 1980 minority share from its city's estimated tipping point (Dev_c). The dashed line at zero separates tracts which have 1980 minority shares past the tipping point ($T_c = 1$) from those which do not ($T_c = 0$). The dots depict local averages across tracts of the mean predicted outcomes for children from the 25th percentile, grouped into 50 quantiles of Dev_c. The solid lines are linear regressions of the mean predicted outcome variable on Dev_c, fit separately on either side of the tipping point. The reduced form effect of tipping on upward mobility is captured by the difference between the two linear fits at the discontinuity. While these plots do not absorb cross-MSA variation, they are informative initial visual representations of the reduced form relationship.

Figure 2 studies the reduced-form relationship for upward mobility pooled across all children. The graph clearly depicts the negative relationship between growing up in a tract with higher minority share relative to the tipping point and upward mobility. Averaging within the "bins," children from the 25th percentile who grew up in a neighborhood with a 1980 minority share which is approximately 10 percentage points below their tipping point are predicted to reach the 45th percentile of the income distribution. Meanwhile, children from the 25th percentile who grew up in neighborhoods with a 1980 minority share 30 percentage points beyond their tipping point have mean predicted outcomes below the 40th percentile. There is evidence of a discontinuity in upward mobility at the tipping point, as there is a gap of approximately three percentiles between linear regressions at the tipping point. While the underlying scatter plot appears somewhat continuous through the tipping point, there seems to be a sharp negative slope that occurs just past the tipping point, evidence supporting the decision to model tipping as a discontinuity. Figure 3 presents separate graphs for black and white upward mobility, pooling across genders within race. Upward mobility is declining in Dev_c for both white and black children, with a small discontinuity of about one percentile at the tipping point for both groups.

Table 6.1 presents the results of estimating the reduced-form model from equation (5.2) for the pooled, black, and white upward mobility outcomes. After absorbing variation from differences across MSA's and controlling for linear trends on either side of the tipping point, having a minority share past the tipping point is associated with a statistically significant decline in upward mobility. A 1980 minority share past the tipping point reduced the overall upward mobility of children who grew up in that neighborhood by over 2.2 percentiles in the income distribution. White upward mobility is more negatively affected from having a minority share past the tipping point than black upward mobility; the difference between the coefficients on T_c in columns 2 and 3 is significant at the 10 percent level ($|t| = 1.81$).

The reduced form intent-to-treat estimates imply that growing up in a neigh-

Table 6.1: Reduced Form Estimates

	(1) Pooled	(2) Black	(3) White
T	-2.229***	-1.018***	-1.729***
	(0.335)	(0.283)	(0.272)
Dev	-0.201***	-0.048*	-0.139***
	(0.036)	(0.021)	(0.025)
T x Dev	0.088*	0.015	0.061*
	(0.035)	(0.023)	(0.026)
MSA FE	Yes	Yes	Yes
N	11,404	6,639	10,572
R-Squared	0.451	0.137	0.279

Notes: Dependent variables are OI measures of upward mobility, which measure mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. The models are estimated on the tracts in the analysis sample, excluding tracts with missing OI measures or whose OI measures reflect crosswalk errors and the two-thirds tracts used to identify the location of the tipping points. MSA clustered standard errors in Parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

borhood which had a minority share past its tipping point negatively impacts poor children's prospects for socioeconomic advancement. Converting the difference in percentile ranks into dollar differences further illustrates this effect.[1] The average mean predicted outcome for children at $p = 25$ for tracts that are not past their tipping point ($T_c = 0$) is the 46th percentile, corresponding to an annual household income of $37,600. The 44th percentile of income corresponds to an annual household income of $35,400. The two percentile reduction in income rank from growing up in a neighborhood which was past its tipping point translates to a reduction in annual adulthood income of more than $2,000. Given that any misclassification error in the tipping point indicator (T_C) would bias the reduced form estimates towards zero, these estimates may be a lower bound on the true intent-to-treat effect.

[1] Chetty et al. (2018) provide a crosswalk from the percentile ranks in the children's adult household income distribution to 2015 dollars.

Changes in neighborhood characteristics from tipping, including change in demographic composition, are associated with effects that limit the upward mobility and reduce the adult income of children who grew up in tipped neighborhoods.

6.2 IV Estimates

The IV estimation strategy requires that the instrument have a significant effect on the endogenous variable; there must be a discontinuity in the relationship between 1990 minority share and the deviation of the tract's 1980 minority share from its tipping point (Dev_c) at the tipping point. Figure 6 presents graphical evidence for the first-stage effect. The horizontal variable is again the deviation of tract's 1980 minority share from its tipping point. The dots represent local averages of tract 1990 minority share within 50 quantiles of the distribution of Dev_c and the solid lines are linear regressions of 1990 minority share on Dev_c fit on either side of the tipping point. Indeed, there is a clear gap between the regressions on either side of the tipping point. Column 1 of Table 6.2 shows the results of estimating equation (5.3), the first-stage specification. Having a minority share past the tipping point in 1980 is associated with a 5.7 percentage point increase in the neighborhood's minority share in 1990. The first-stage F-statistic is nearly 90, far beyond the cutoff at which there would be concern about a weak instrument. That tipping points are strongly associated with changing demographic composition is consistent with CMR's conclusion that neighborhood dynamics exhibit tipping patterns.

Columns 2 through 4 of Table 6.2 present the IV estimates of the effect of 1990 minority share on upward mobility, instrumenting for 1990 minority share with the tipping point regression discontinuity. Under the LATE interpretation of the estimates, a higher minority share driven by tipping dynamics is associated with a significant reduction in upward mobility. A 10 percent increase in minority share is estimated to reduce the mean predicted outcomes of children at $p = 25$ (children whose parents were in the 25th percentile of household income) by almost 4 percentiles. Again, consider the effect of such segregationary forces on the average low-income child who otherwise would have had a mean predicted outcome of the 46th percentile. A household income in the 42nd percentile corresponds to $33,100 per year. A 10 percentage point increase in childhood neighborhood minority share from segregation is associated with a $4,500 average reduction in annual household income as an adult. When estimating the model using black children's upward mobility as the outcome, a 10 percentage point increase in minority share is associated with a $2,000 reduction in annual household income (for an individual who would otherwise be upwardly mobile to the 46th percentile). In the model with white upward mobility as an outcome, the reduction in annual household income is over $3,300.

These are sizable reductions in income for households in the bottom half of the income distribution. For the low-income child who otherwise would have a mean predicted outcome of the 46th percentile, a 10 percentage point increase in neighborhood minority share driven by segregation is estimated to decrease annual household income by 12 percent. Offsetting this loss would require an amount equivalent to nearly 70 percent of the maximum EITC credit.[2]

It is clear that there are other dimensions of welfare for which segregation is more detrimental for blacks. This analysis does not capture these effects, considering only the impact on adulthood household income. The IV estimates imply that segregation is associated with a larger reduction in upward mobility for white children than black children. This difference is both statistically significant ($|t| = 2.14$) and economically consequential. When considering only the dimension of the impact on future earnings, segregation is estimated to be more harmful for white

[2]The maximum credit for a household with three or more qualifying children in tax year 2019 is $6,557.

Table 6.2: 1990 Minority Share and Upward Mobility: 2SLS

	(1) First-Stage	(2) 2SLS: Pooled	(3) 2SLS: Black	(4) 2SLS: White
Minority Share 1990		-0.389***	-0.182***	-0.346***
		(0.057)	(0.050)	(0.058)
T	5.681***			
	(0.599)			
Dev	1.113***	0.230**	0.134*	0.255**
	(0.048)	(0.084)	(0.065)	(0.083)
T x Dev	-0.217***	0.005	-0.007	-0.011
	(0.057)	(0.046)	(0.029)	(0.035)
MSA FE	Yes	Yes	Yes	Yes
N	11,436	11,404	6,639	10,572
R-Squared	0.927	0.438	0.117	0.265
First Stage F-Stat	89.94			

Notes: Column 1: Dependent variable is 1990 minority share. Columns 2-4: Dependent variables are OI measures of upward mobility, which measure mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. The models are estimated on the tracts in the analysis sample, excluding tracts with missing OI measures or whose OI measures reflect crosswalk errors and the two-thirds tracts used to identify the location of the tipping points. MSA clustered standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

children who grow up in neighborhoods that experience white flight and minority in-migration than for black children in those same neighborhoods. This differential effect is consistent with the minority enclave hypothesis described in Cutler and Glaeser (1997) and Wilson and Portes (1980). They suggest that the impact of residential segregation on individual outcomes may be ambiguous due to the positive peer effects that result from clustering. If people are better off when they are surrounded by those like them, then segregation may have a positive impact on children's upward mobility. While these results indicate that overall, segregation is detrimental, blacks in tipped neighborhood that experienced white flight and avoidance may have experienced some offsetting benefit from these clustered peer effects. Meanwhile, whites who remained in these neighborhoods were surrounded by neighbors who were increasingly of a different racial group than them. Thus, white children received no mitigating benefits from the segregation which might counteract some of the negative effects. These results may also reflect some selection, in that whites who remain in tipped neighborhoods are different from those who do not. Given that changes in neighborhood composition after tipping are mostly driven by white flight and avoidance (Boustan 2010), considering how differences between those who stay and those who leave may impact empirical results is particularly important when studying white upward mobility. In Chapter 7, I further examine possible concerns about the identifying assumptions.

6.3 Heterogeneity by Gender

Neighborhood tipping and segregation may have different effects on the upward mobility of males and females. Figures 4 and 5 examine heterogeneity in the reduced form relationship between tipping and upward mobility for different race-gender groups of children. In Figure 4, there is evidence of only a small discontinuous effect of tipping on upward mobility for black females, and a slightly larger effect for white females. In Figure 5, tipping appears to be more relevant for male upward mobility, with both black and white male upward mobility reduced by approximately one percentile at the tipping point. These graphical results are not readily interpretable as evidence for a strong reduced form relationship between neighborhood tipping and upward mobility for any group. Nonetheless, there is *some* negative discontinuity for each group's upward mobility at the tipping point. Additionally, as noted in Chapter 4, controlling for cross-MSA variation in deviation from tipping points and upward mobility through the fully specified reduced-form model is important for robust inference.

Appendix tables B.1 and B.2 explore gender heterogeneity in reduced form effects in more detail. The point estimates suggest that the reduction in male upward mobility after the tipping point are larger than the reduction in female upward mobility for the pooled (across races) and black outcome variables. For whites, living in neighborhood with a minority share past the tipping point is associated with a slightly larger decline in female upward mobility than male upward mobility. However, the differences across genders between these estimated effects are not statistically significant. While the estimated reductions in overall and white upward mobility by gender are highly significant, the relationship between black upward mobility by gender and the tipping point is weaker. For black female upward mobility, we cannot reject the possibility that having a minority share past the tipping point has a minor, positive impact on upward mobility.

Appendix tables B.3 and B.4 present the results of estimating the IV models separately on male and female upward mobility. For pooled and black upward mobility, the point estimates imply that segregation has a more negative effect on male upward mobility than on female upward mobility. For whites, segregation is associated with a larger decrease in female upward mobility. This pattern is consistent with the gender heterogeneity in the reduced form estimates. While the estimated effect of segregation on black upward mobility is significant when upward mobility is pooled across genders, this result is not robust to examining male and female upward mobility separately. The IV coefficient on 1990 minority share is not statistically significant at the five percent level in the models for both black male and black female upward mobility. Nonetheless, the point estimates remain negative, consistent with the conclusion that segregation is harmful for upward mobility.

Chapter 7

Robustness Analyses

In this chapter, I present robustness tests for the main results from Chapter 6. First, I conduct a sensitivity analysis to investigate the robustness of my main findings to changes in data definitions. I then present a series of analyses to examine the validity of the identification assumptions that underpin the empirical strategy. The first set of robustness checks ensure that the results are not driven by observations far from the discontinuity (of the tipping point). The second set concern the exclusion restriction, testing for possible mechanisms through which tipping may affect upward mobility other than segregation.

The third set consider that the 1980 minority share of a tract may not be randomly assigned around the tipping point. Here, I find evidence that tracts on either side of the discontinuity have significantly different 1980 characteristics, even after conditioning on the MSA fixed effects and linear controls in the 1980 minority share. A 1980 tract minority share past the tipping point is associated with significantly lower average household income in 1980–a proxy for lower socioeconomic status. This result suggests that the IV estimates may be affected by some omitted variables which are not orthogonal to the instrument. As these omitted variables are likely comprised of some observed tract characteristics and some unobserved factors, including all observed variables as controls in the model will not necessarily eliminate all omitted variable bias from the IV estimates. While the bias cannot be eliminated, Oster (2019) proposes a method to bound the causal effect of an explanatory variable under the threat of omitted variable bias. Following Bevis and Barret's (2020) implementation of Oster's method, I find that the causal effect of segregation on upward mobility is bounded negatively away from zero. That is, I can reject that segregation has no effect (or a positive effect) on upward mobility. Even more promising for the robustness of my results, the IV estimates above are on the higher (less negative) end of the Oster bounds, implying that my estimates are conservative estimates for the negative effect of segregation. In other words, the true causal effect of segregation on upward mobility may be more detrimental than I estimate, contrary to *a priori* expectations.

7.1 Sensitivity to Sample Definition

In building the analysis sample, I had to decide how to treat observations with upward mobility measures which were clearly the result of errors from the crosswalk from 2010 census tract boundaries to 2000 census tract boundaries. For the primary analysis, I imposed the natural cutoffs of zero (for the lowest percentile) and 100 (the highest percentile) for these variables. Appendix tables C.1 and C.2 examine the robustness of the main results to expanding these cutoffs to negative 50 and 200. Though the point estimates change slightly, the main conclusions are robust to the alternative sample definition.

7.2 Bandwidth Tests

The regression discontinuity design relies on the model approximately controlling for the conditional expectation functions of the potential outcomes on either side of the discontinuity. Misspecification of these functions can cause a non-linear continuous function to be misinterpreted as a discontinuity, or can overfit the data and cause a discontinuity to be misinterpreted as a non-linear continuous function. A standard approach in estimating regression discontinuity models – which I have adopted here – is to use linear controls on either side of the tipping point, relying on the local approximation of higher order polynomials to linear functions. However, such approximations break down when they are no longer "local." When a regression discontinuity model is estimated over the entire range of the running variable, the linear functions may poorly approximate the conditional expectation functions for observations far from the discontinuity, causing the results to be invalid.

To ensure that my results are not driven by the use of linear controls in the running variable (Dev_c), I re-estimate the main models with a series of bandwidth restrictions. Under a bandwidth restriction, observations with values of the running variable outside a bandwidth b around the discontinuity are excluded from the estimation sample.[1] Appendix tables C.1, C.2, and C.3 present the results of estimating the first-stage and IV models (equations 5.3 and 5.4) with bandwidths of 40, 30, and 20, respectively. The results are robust to the bandwidth restrictions. Indeed, restricting the bandwidth actually strengthens the negative relationship between segregation-driven increases in minority share and overall upward mobility. After restricting the bandwidth to 20, a 10 percentage point increase in minority share is associated with a nearly eight percentile reduction in the mean predicted adulthood income rank for children whose parents were in the 25th percentile of income ($p = 25$). Consider again the example of the child at $p = 25$ who otherwise would have had a mean predicted outcome in the 46th percentile of household income. A

[1] Tract c is included in the estimation sample if $|Dev_c| =< b$.

reduction to the 38th percentile corresponds to a loss in annual household income of over $8,700 dollars.[2] However, restricting the bandwidth does reduce the significance of the coefficients in the model of black upward mobility. The coefficient on 1990 minority share is not significant at the 10 percent level after the bandwidth is restricted to 20. The reduction in power may be due to the lower number of tracts for which black upward mobility is reported in the Opportunity Atlas; the point estimates remain consistent with the unrestricted results.

7.3 Exclusion Restriction Tests

For tipping points to be a valid instrument, The exclusion restriction requires that tipping only affects upward mobility through changes in minority share. If this is the case, other determinants of upward mobility in the post-treatment period (after 1990) should have no significant discontinuity at the tipping point. Here, I use average household income and the unemployment rate in the tract as proxies for socioeconomic determinants of upward mobility. For clarity, I transform the income variable to a logarithmic scale; coefficients are interpreted as percentage increases. Figure 7 presents graphical evidence of the relationship between the 1990 tract characteristics and the deviation of the tract's minority share from its tipping point in 1980. While there is essentially no discontinuity at the tipping point in the plot for 1990 unemployment rate, there is an evident discontinuity in the plot for 1990 log-average household income. To quantify these discontinuities and evaluate their statistical significance, I estimate the reduced form specification from equation (5.2) with the 1990 tract covariates as the left-hand side variables. Columns 1 and 2 of Table 6.3 present the results of these regressions. After absorbing cross-MSA variation and controlling for linear trends on either side of the

[2] 2015 dollars.

tipping point, having a 1980 minority share past the tipping point is associated with a 12 percent decrease in the average household income and a 0.5 percentage point increase in the unemployment rate for the neighborhood in 1990. Both coefficients are statistically significant at the five percent level. Given that the average 1990 unemployment rate in the analysis sample is 6.8 percent, this estimate corresponds to an increase in the unemployment rate of 7.8 percent of the average unemployment rate.[3] These discontinuities indicate that tracts past their 1980 tipping points have worse 1990 economic conditions. As the economic conditions of childhood neighborhoods likely impact upward mobility, the exclusion restriction appears to be a somewhat tenuous assumption.

Table 7.1: Testing Identification Assumptions

	(1) Log(Avg. HH Inc) 90	(2) U-Rate 90	(3) Log(Avg. HH Inc) 80	(4) U-Rate 80
T	-0.120***	0.534**	-0.088***	0.234
	(0.019)	(0.173)	(0.015)	(0.138)
Dev	-0.012***	0.061***	-0.010***	0.045**
	(0.002)	(0.015)	(0.002)	(0.016)
T x Dev	0.004	0.087***	0.003*	0.057***
	(0.002)	(0.019)	(0.002)	(0.017)
MSA FE	Yes	Yes	Yes	Yes
N	11,419	11,436	11,424	11,436
R-Squared	0.468	0.564	0.459	0.538

Notes: Dependent variables are denoted in the column headers. Unemployment measured in percentage points, from zero to 100. The models are estimated on the tracts in the analysis sample, excluding tracts with missing OI measures or whose OI measures reflect crosswalk errors and the two-thirds tracts used to identify the location of the tipping points. MSA clustered standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

While these results raise concerns about the validity of the exclusion restriction, they do not provide definitive evidence that it is violated. Average household income in 1990 may decrease and the unemployment rate may increase in

[3]The arithmetic for this calculation is: $100 \times \frac{\text{Effect of Tipping on Unemployment Rt}}{\text{Avg. Unemployment Rt}} = 100 \times \frac{0.5}{6.8} = 7.8$ percent.

tracts that are past their tipping point as a result of the demographic dynamics of tipping. Given that tipping is a demographic phenomenon, the mechanism underlying empirical associations between tipping and tract economic characteristics may be the increased minority share due to segregation in tracts that are past their tipping point. White flight from these neighborhoods may have disrupted the remaining residents' social capital and labor market networks. These networks have been shown to have important impacts, particularly for low income workers, and there is evidence that spatial residential proximity matters for the strength of these networks (Hellerstein, Kutzbach and Neumark, 2014; Hellerstein, Kutzbach, and Neumark, 2019). If changing minority share is the only mechanism through which tipping affects tract characteristics, then the exclusion restriction is still valid even though there are significant associations between tipping and post-treatment neighborhood characteristics. Furthermore, even if the exclusion restriction does not hold, the reduced form estimates are still valid so long as having a minority share on either side of the tipping point is random (conditional on the controls).

7.4 Randomness Assumption Tests

If the randomness assumption holds, we should expect no significant differences in pre-treatment (1980) characteristics between neighborhoods which are past the tipping point and neighborhoods which have minority shares lower than the tipping point (after conditioning on the controls). I again use the log-average household income and unemployment rate as proxies for a host of neighborhood characteristics, this time studying their levels in 1980. Figure 8 shows the binned scatter-plots of the 1980 tract characteristics on the deviation of the 1980 minority share from the tipping point. The solid red lines are linear regressions fitted on either side of the tipping point. There is a visible discontinuity in the graph for log-average household income, but no obvious discontinuity in the graph for unemployment rate. Estimating these models using the tipping point regression discontinuity framework of equation (5.3) confirms these conclusions. Columns 3 and 4 of Table 6.3 report the estimates from these regressions. Having a minority share past the tipping point is associated with a 8.8 percent decrease in average household income in the neighborhood. That this estimate is highly statistically significant is strong evidence of a difference between tracts on either side of the tipping point, even when comparing neighborhoods within the same city and after controlling for linear trends in minority share. In contrast, there is no significant discontinuity in 1980 unemployment rate at the tipping point. However, this may be due to the noisiness of the unemployment data at the tract level, since there can be substantial variation across tracts within the same MSA. The standard deviation of 1980 unemployment rate is more than two-thirds of the sample mean, whereas the standard deviation of 1980 average household income is only one-third of the sample mean.

The existence of a significant discontinuity in average household income at the tipping point is concerning for the validity of the randomness assumption. If tracts on either side of the tipping point are different (after absorbing the controls) in ways that impact the upward mobility of children from those neighborhoods, tipping is not an exogenous source of variation in neighborhood composition. Not only would tipping be an invalid instrument, but the reduced form regression discontinuity estimates would be biased. The decline in average household income after the tipping point suggests that tipping is correlated with non-demographic pre-treatment characteristics which negatively affect upward mobility. This would imply that both the reduced form and IV estimates are biased downward, overstating the negative effect of segregation on upward mobility. However, there may also be omitted relevant variables which positively bias the coefficient estimates.

From these results alone, both the magnitude and direction of the bias are not clear, though the evidence suggests that the bias tends downward (negative).

7.5 Oster's Bound for the Causal Effect

It is difficult to ensure that there are no unobserved confounding variables in a non-experimental setting such as neighborhood tipping. While the tipping instrument is likely orthogonal to many of these confounders, it may not perfectly isolate exogenous variation in segregation. Oster (2019), extending the theory presented in Altonji, Elder and Taber (2006), develops a method to bound the causal effect of an explanatory variable under the threat that there are possible unobserved relevant variables (i.e. the explanatory variable is not exogenous). Empirical studies have begun to utilize Oster's method; Bevis and Barrett (2020) use Oster's bounds to study the inverse relationship between farm size and productivity in developing countries.[4] The method uses a key assumption about the relative correlations between the potentially endogenous explanatory variable (here, 1990 minority share) and relevant observables, and that variable and unobservables. Below, I translate Oster's framework to the context of this paper to bound the causal effect of segregation, as proxied by 1990 tract minority share ($m_{c,90}$), on upward mobility (y).

Following Oster (2019), consider the following data generating process for tract-level upward mobility:

$$y = \beta m_{90} + \psi W_1 + W_2 + \epsilon \tag{7.1}$$

Where β is the causal effect of 1990 minority share on upward mobility, W_1 is a vector of observed tract characteristics correlated with 1990 minority share, W_2 is a vector of other unobserved tract characteristics correlated with 1990 minority

[4]The overview of Oster's method below follows Bevis and Barret's (2020) summary.

share, and ϵ is the random mean zero error. The regression of y on m_{90} results in the biased estimate $\mathring{\beta}$ and R-squared $\mathring{R}$. Including W_1 in the regression results in the less biased estimate $\tilde{\beta}$ and R-squared $\tilde{R}$. If we were able to observe W_2, this hypothetical regression would have an R-squared of R_{max}, which would be less than one if ϵ has a non-zero sample variance.

These regressions have analogues among the models detailed in the empirical strategy above (Chapter 5). The regression of Y on m_{90} is the OLS model from equation (5.1). The regression of y on m_{90} and W_1 is nearly identical to the ”second stage“ model in equation (5.4), except that the tract's 1980 log-average household income, unemployment rate, and poverty rate are also included in W_1 with the MSA fixed effects and the deviations of minority share on either side of the tipping point.[5]

The key assumption for the validity of the Oster bound is that the relative contribution of each variable in W_1 to Y is the same as the relative contribution of that variable to X. While this relationship is unlikely to hold with equality when W_1 contains more than one element (as it does here), the Oster bound provides a consistent approximation so long as the deviations from this condition are not ”extremely large“ (Oster 2019). Under this assumption, the true causal effect is between $\tilde{\beta}$ and β^*, where:

$$\beta^* = \tilde{\beta} - \varsigma[\mathring{\beta} - \tilde{\beta}]\frac{R_{max} - \tilde{R}}{\tilde{R} - \mathring{R}} \tag{7.2}$$

ς measures the relative proportion of m_{90} which is explained by the elements of W_2 vs. W_1– the fraction of the explanatory variable of interest which is explained by the unobservables in comparison to that explained by the observables. Therefore ς is always greater than or equal to zero (assuming that the explanatory vari-

[5]Thus, $\tilde{\beta}$ and $\tilde{R}$ are the estimates of the regression: $y = \tilde{\beta} m_{c,90} + \tilde{\theta}_1 Dev_c + \tilde{\theta}_2 Dev_c \times T_c + \varphi X_c + \tilde{\lambda}_{k(c)} + \tilde{\epsilon}$. X is a vector which includes the tract's 1980 log-average household income, unemployment rate, and poverty rate.

able is endogenous). $\varsigma = 1$ would imply that the observable tract characteristics and the unobserved tract characteristics are equally important in explaining the 1990 minority share. Thus, the Oster bound requires two parameters, $\mathbb{R}_{max}$ and ς, which cannot be estimated from the data. Oster suggests that $\varsigma = 1$ and $R_{max} = min\{1.3\tilde{R}, 1\}$ are appropriate values in most cases. I calculate the Oster bounds for the coefficient on 1990 minority share (the proxy for segregation) in the models for overall, black, and white upward mobility under the suggested values, and validate that the conclusions hold under a range of values for ς and R_{max}.

Appendix table E.1 presents the results of the OLS models (without controls), estimating $\mathring{\beta}$ and $\mathring{R}$ for the models of the effect of segregation on pooled, black, and white upward mobility. For the pooled upward mobility model, $\mathring{\beta} = -0.113$ and $\mathring{R} = 0.207$. In all three models, the OLS estimated coefficient on 1990 minority share is higher (less negative) than the IV estimate, implying that instrumenting for segregation using the tipping regression discontinuity and absorbing MSA-level variation eliminates positive omitted variable bias. Appendix table E.2 presents the results of estimating $\tilde{\beta}$ and $\tilde{R}$, regressing upward mobility on the 1990 minority share, a vector of tract characteristics as controls, and MSA fixed effects. After including the controls, the estimated coefficient on 1990 minority share becomes more negative and the R-squared increases substantially in all three models.

Using these estimates, I calculate Oster's bound on the causal effect of 1990 minority share on upward mobility. For example, β^* for the pooled model is calculated with $\mathring{\beta} = -0.113$, $\tilde{\beta} = -0.158$, $\mathring{R} = 0.207$, and $\tilde{R} = 0.550$. Appendix table E.3 shows the estimated β^* and the bounding intervals for the effect on pooled, black, and white upward mobility using $R_{max} = min\{1.3\tilde{R}, 1\}$ and $\varsigma = 1$ as Oster (2019) suggests.

The bounding intervals display four qualities which are reassuring for the robustness of the main estimates. First, the coefficients are bounded away from zero, indicating that the true effect of segregation on upward mobility is negative– consistent with the main estimates. Second, the IV estimates for the effect of segregation on upward mobility fall squarely within the Oster bounds for all three models. Third, the ordering of the interval bounds is consistent with the ordering of the IV coefficient estimates. The IV estimates imply that segregationary white flight is more detrimental for the upward mobility of white children who remain in the segregated neighborhoods than for the upward mobility of black children from those neighborhoods. Similarly, the Oster bounding interval for the coefficient on 1990 minority share in the model for white upward mobility is more negative than the interval for the coefficient in the model for black upward mobility. Finally, the IV estimates are on the high end of the Oster bounding interval for all three models. Substantial portions of the intervals are more negative than the IV point estimates, implying that segregation may be far more harmful for upward mobility than my estimates suggest.[6]

I also relax the assumptions of $\varsigma = 1$ and $R_{max} = 1.3\tilde{R}$ to ensure that the reassuring qualities of the Oster bounds are not driven by these choices. I calculate the Oster bound for every combination of $\delta \in [0, 2]$ (increasing the value by 0.1 in each iteration) and $R_{max} \in [\mathbb{R}_{max}, 1]$ (increasing the value by 0.05 in each iteration) for all three models. For all three models, the bounding interval for the coefficient on 1990 minority share contains values greater than or equal to zero for fewer than four percent of combinations over the set of values for $\{\varsigma, R_{max}\}$. Although neighborhood tipping is not a randomized controlled trial, using tipping points to instrument for segregation provides credible estimates for the effect of segregation on upward mobility.

[6]Oster's method does not specify the probability distribution of the true causal effect within the bounds; this claim imposes some limited distributional assumptions.

Chapter 8

Conclusion

In this paper, I study the effect of residential racial segregation on upward mobility, using neighborhood racial tipping points to isolate plausibly exogenous variation in neighborhood demographics. I construct a model of neighborhood residential dynamics which demonstrates how tipping arises from individual decisions. The key insight from the model is that otherwise similar neighborhoods whose minority shares are on opposite sides of a threshold value will experience vastly different changes in demographic composition, as the neighborhood past the tipping point undergoes white flight and minority in-migration. From this observation, I use tipping as the basis of a novel instrumental variables model to overcome the endogeneity of neighborhood sorting which makes the identification of causal effects in such work so difficult. I combine data on estimated tipping points for large metropolitan areas across the United States from Card, Mas, and Rothstein (2008) with detailed tract-level estimates of upward mobility from Opportunity Insights (Chetty et al. 2018). Using this data, I estimate the effect of white flight and minority in-migration during the period of 1980 to 1990 on the upward mobility of low-income children who grew up in a neighborhood which experienced these segregationary forces.

The empirical results show that segregation has a substantial adverse effect on the upward mobility of poor children. Measuring upward mobility as the mean predicted rank in the adulthood household income distribution for children who grow up in the 25th percentile, I find that a 10 percentage point increase in neighborhood minority share which is driven by segregationary white flight and minority in-migration causes a four percentile reduction in upward mobility. Translating this effect into dollars, this increase in segregation reduces the adulthood annual household income of the average poor child by nearly $4,500, or 12 percent of annual household income.

Modeling black and white upward mobility separately, segregation has a statistically significant effect on the upward mobility of children of both races, and is more harmful to the upward mobility of white children who grow up in segregated areas. For a poor black child, a 10 percentage point increase in neighborhood minority share driven by segregationary white flight and minority in-migration causes a two percentile reduction in upward mobility. The same segregation is associated with a four percentile reduction in upward mobility for a poor white child from that same neighborhood (and the difference between races is statistically significant). This finding is consistent with the theory that the formation of minority enclaves provide some offsetting benefit for members of those communities. Poor whites who remain in these ”tipped“ neighborhoods do not receive these benefits which serve to offset some of the harmful effects of segregation for black children. A potential avenue for future research would be to use individual-level data to examine whether selection in the composition of the whites who remain in the neighborhood affects these results.

These results depend on the validity of the identification assumptions for the tipping discontinuity instrument. Robustness checks indicate that neighborhoods that were past their 1980 tipping points also had significantly worse economic characteristics in 1980. This finding suggests that the tipping discontinuity instrument may not control for all relevant omitted variables. Nonetheless, estimated bounds around the causal effect of segregation on upward mobility confirm that segregation is detrimental to the upward mobility of children from both racial groups and suggest that the tipping instrument produces conservative estimates for the true negative effects.

Having estimated the causal effect of neighborhood-level segregation on intergenerational upward mobility for the first time, this paper has important implications for policymakers. My results strongly suggest that there would be substantial long-term returns to policies that directly reduce residential segregation. Such programs may include providing housing vouchers for low-income minority families to move into predominantly white neighborhoods; constructing more affordable housing in segregated neighborhoods; or funding better public schools in neighborhoods with high concentrations of minorities to encourage integration and improve outcomes for children from those neighborhoods. An important avenue for future research is to evaluate the efficacy and efficiency of such policies in promoting racial integration and increasing upward mobility.

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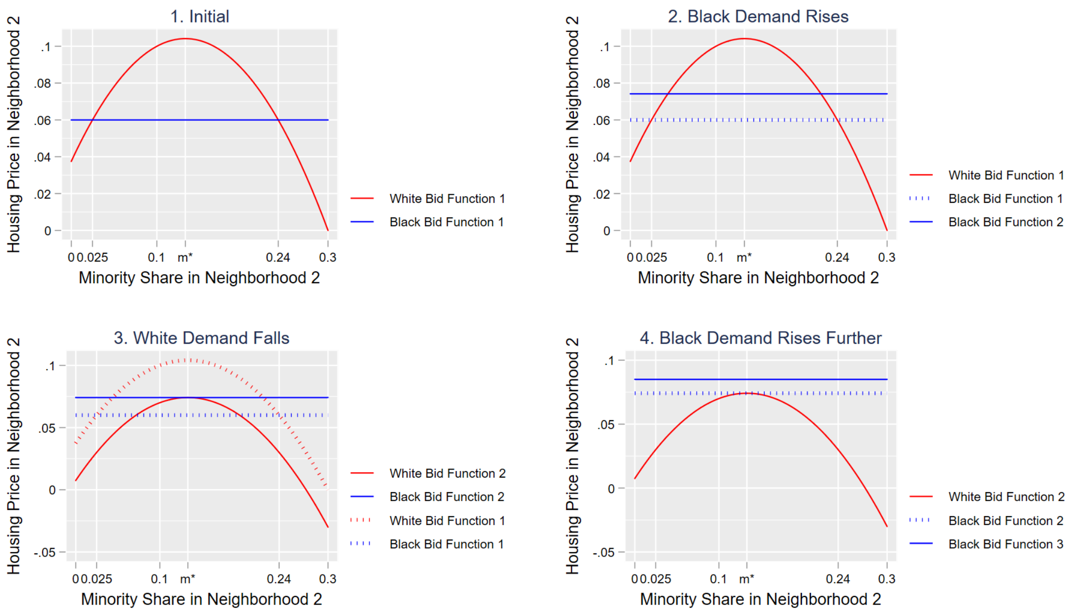
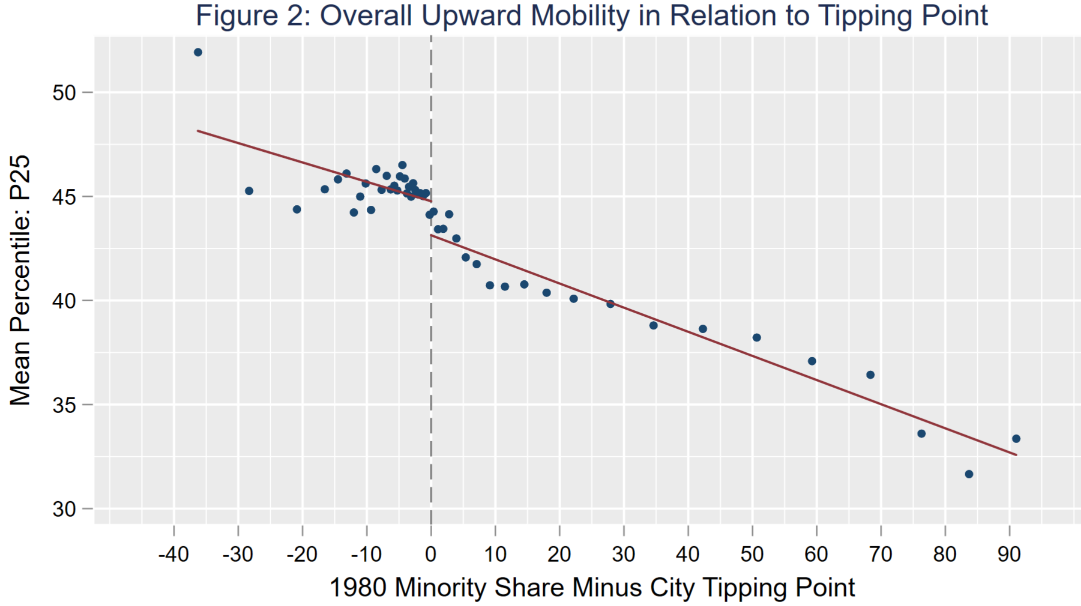
Figure 1: A Progression of Marginal Bid Functions



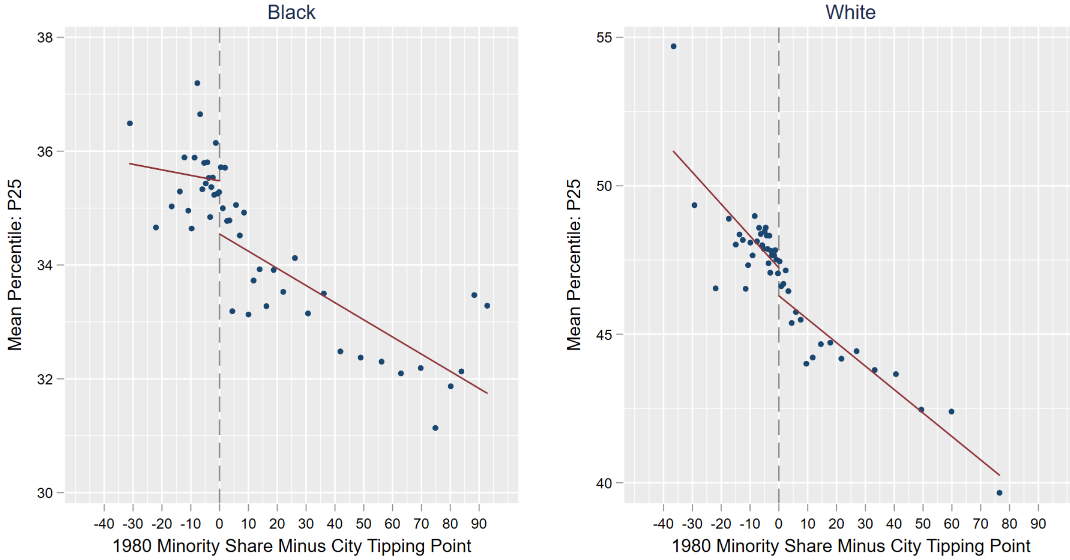
Notes: Graphs depict marginal bid functions under a series of values for the public good valuation parameters. Housing price in neigbhorhood 2 is relative to the price in neighborhood 1, which is normalized to zero.

Figure 2: Overall Upward Mobility in Relation to Tipping Point



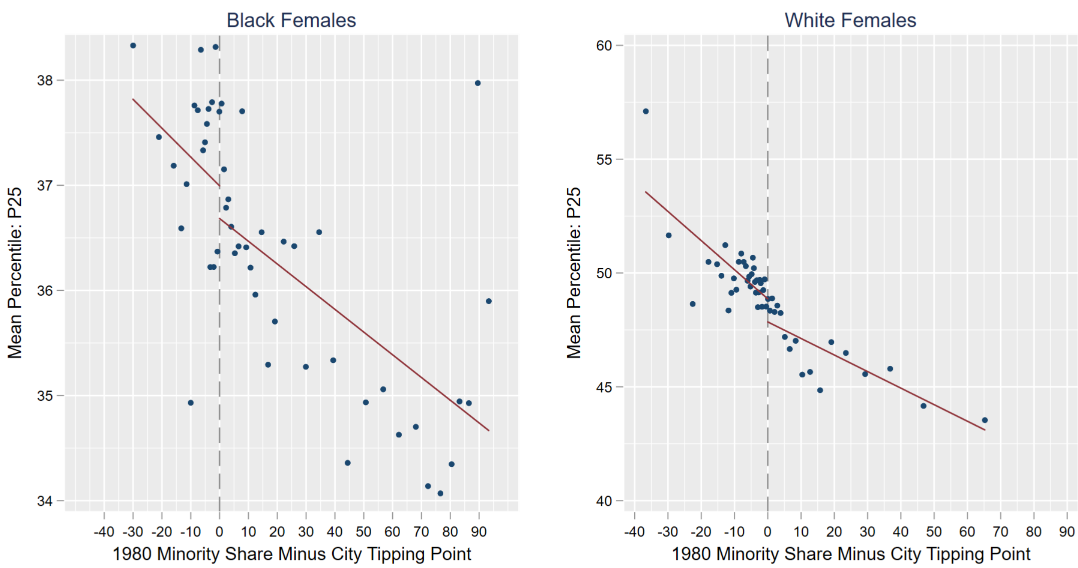
Notes: Dots show mean adult income percentile for children from the 25th percentile of household income grouping tracts into 50 quantiles by deviation of 1980 minority share from the tract's tipping point. Red lines are linear regressions fit on either side of tipping point. Only the one-third of the tracts not used for identifying the location of the tipping points are used for these visual depictions.

Figure 3: Upward Mobility in Relation to Tipping Point by Group



Notes: Dots show mean adult income percentile for children from the 25th percentile of household income grouping tracts into 50 quantiles by deviation of 1980 minority share from the tract's tipping point. Red lines are linear regressions fit on either side of tipping point. Only the one-third of the tracts not used for identifying the location of the tipping points are used for these visual depictions.

Figure 4: Upward Mobility in Relation to Tipping Point by Group

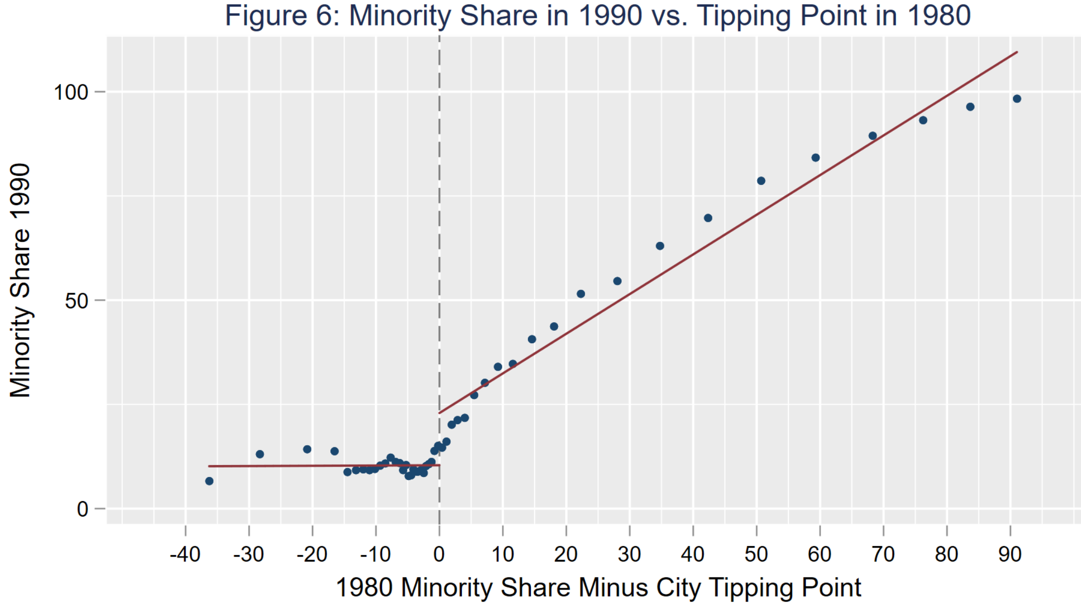


Notes: Dots show mean adult income percentile for children from the 25th percentile of household income grouping tracts into 50 quantiles by deviation of 1980 minority share from the tract's tipping point. Red lines are linear regressions fit on either side of tipping point. Only the one-third of the tracts not used for identifying the location of the tipping points are used for these visual depictions.

Figure 5: Upward Mobility in Relation to Tipping Point by Group

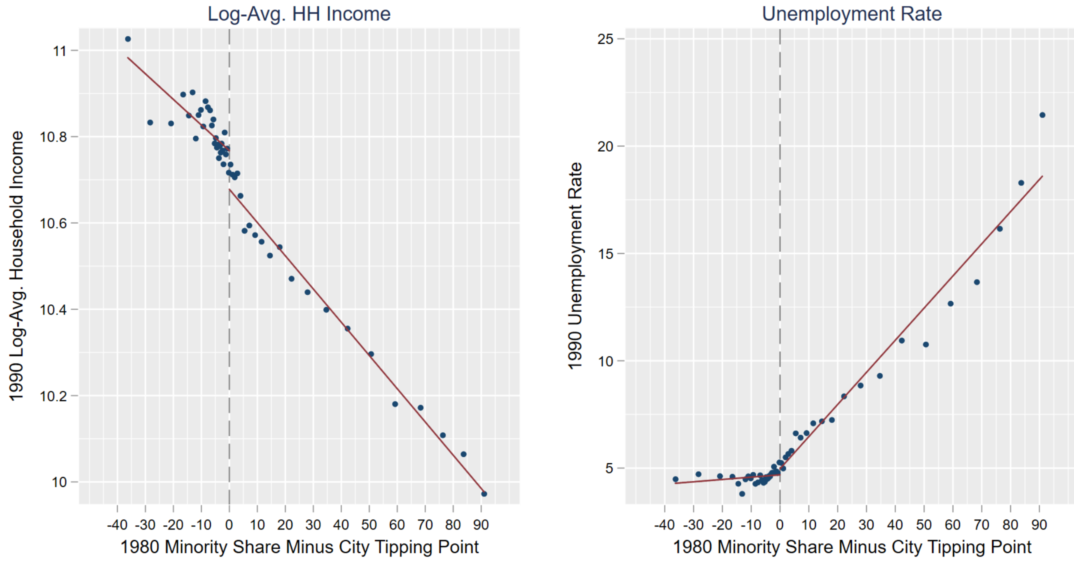
Notes: Dots show mean adult income percentile for children from the 25th percentile of household income grouping tracts into 50 quantiles by deviation of 1980 minority share from the tract's tipping point. Red lines are linear regressions fit on either side of tipping point. Only the one-third of the tracts not used for identifying the location of the tipping points are used for these visual depictions.

Figure 6: Minority Share in 1990 vs. Tipping Point in 1980



Notes: Dots show 1990 minority shares, grouping tracts into 50 quantiles by deviation of 1980 minority share from the tract's tipping point. Red lines are linear regressions fit on either side of tipping point. Only the one-third of the tracts not used for identifying the location of the tipping points are used for these visual depictions.

Figure 7: 1990 Covariates in Relation to Tipping Point



Notes: Dots show averages of Y-variables, grouping tracts into 50 quantiles by deviation of 1980 minority share from the tract's tipping point. Red lines are linear regressions fit on either side of tipping point. Only the one-third of the tracts not used for identifying the location of the tipping points are used for these visual depictions.

Figure 8: 1980 Covariates in Relation to Tipping Point

Notes: Dots show averages of Y-variables, grouping tracts into 50 quantiles by deviation of 1980 minority share from the tract's tipping point. Red lines are linear regressions fit on either side of tipping point. Only the one-third of the tracts not used for identifying the location of the tipping points are used for these visual depictions.

Appendix A

Misclassification Error in a Binary Instrument

Denote the true (unobserved) instrumental variable as $T \in \{0,1\}$ and the observed instrument as $\tilde{T} \in \{0,1\}$. In this context, measurement error in $\tilde{T}$ is defined as follows: (1) for a random fraction ν of the population, $\tilde{T} = 1$ while $T = 0$, and (2) for a random fraction η of the observations, $\tilde{T} = 0$ while $T = 1$. The fraction of the sample for which (1) holds is $\hat{\nu}$ and the fraction for which (2) holds is $\hat{\eta}$. Clearly $\hat{\nu}$ and $\hat{\eta}$ converge in probability to ν and η. This is not classical measurement error (when there are i.i.d. normal errors added to the true variable) but rather misclassification error. Following Aigner (1973), I derive some of the properties of 2SLS estimates which use $\tilde{T}$ as the instrument, under the assumption that T is a valid instrument.

Using the definitions above, let $\tilde{T} = T + u$, where u is the random misclassification error. Suppose that T is distributed Bernoulli with parameter P (and let $Q = 1 - P$). Clearly, $\tilde{T}$ is also distributed as a Bernoulli random variable, and denote the parameter of this distribution $\tilde{P}$ (and $1 - \tilde{P} = \tilde{Q}$). It can be shown that $(\tilde{T}, u)$ is jointly distributed:

$$(\tilde{T}, u) = \begin{cases} (0,-1), \text{ with } P(0,-1) = \eta\tilde{Q} \\ (0,0), \text{ with } P(0,0) = (1-\eta)\tilde{Q} \\ (1,0), \text{ with } P(1,0) = (1-\nu)\tilde{P} \\ (1,1), \text{ with } P(1,1) = \nu\tilde{P} \end{cases} \tag{A.1}$$

Thus, we have that $E(u) = \nu\tilde{P} - \eta\tilde{Q}$, $V(u) = \eta\tilde{P} + \nu\tilde{Q} - (\eta\tilde{P} - \nu\tilde{Q})^2$, and $cov(\tilde{T}, u) = (\eta + \nu)\tilde{P}\tilde{Q}$. Let $\hat{\tilde{Q}}$ and $\hat{\tilde{P}}$ denote the sample analogues of $\tilde{Q}$ and $\tilde{P}$. The difference from the case of classical measurement error is apparent; u does not have mean zero and is not uncorrelated with the true variable (T).

Now, consider a simplified version of the 2SLS model above, omitting the control variables so that there is only one instrument and one endogenous variable (the simplification is made for clarity in the algebra). Assume the usual random sampling and finiteness assumptions hold, that T_c is a valid instrument ($E[T_c\epsilon_c^y] =$

0), and that u is uncorrelated with ϵ_c^y and ϵ_c^x. The model we want to estimate is specified as:

$$x_c = \gamma T_c + \epsilon_c^x \tag{A.2}$$
$$y_c = \beta x_c + \epsilon_c^y \tag{A.3}$$

However, as we only observe T_c, the actual model we estimate is:

$$x_c = g\tilde{T}_c + \epsilon_c^x = g(T_c + u_c) + \epsilon_c^x \tag{A.4}$$
$$y_c = b x_c + \epsilon_c^y \tag{A.5}$$

Where g and b are the first and second stage population parameters. The goal is to understand the asymptotic behavior of the estimated $\hat{b}$ with respect to β.

Consider the distribution of the 2SLS estimator for $\hat{b}$ under this form of non-classical measurement error. Denote the population covariances of the variables i and j as $\Sigma_{i,j}$, for each pair of variables within $\{x, y, T, \tilde{T}, \epsilon\}$, and let $S_{i,j}$ correspond to the sample analogue. Furthermore, let $g_c = \epsilon_c^y T_c$ and $\tilde{g}_c = \epsilon_c^y \tilde{T}_c$; let $\bar{g}$ and $\bar{\tilde{g}}$ denote the corresponding sample means. Similarly, $\Sigma_{g,g} = E[gg] = V(g)$ and $\Sigma_{\hat{g},\hat{g}} = E[\tilde{g}\tilde{g}] = V(\tilde{g})$. This implies that the 2SLS estimator for b is:

$$\hat{b} = S_{x,\tilde{T}}^{-1} S_{y,\tilde{T}} = b + S_{x,\tilde{T}}^{-1} S_{\tilde{T},\epsilon_c^y} \tag{A.6}$$
$$= b + (S_{x,T} + S_{x,u})^{-1}(\bar{g} + S_{u,\epsilon_c^y}) \tag{A.7}$$

From the assumption that T is a valid instrument, $E[g] = E[\epsilon_c^y]E[T] = 0$. Hence, $\bar{g} \xrightarrow{p} 0$ by the law of large numbers. Furthermore, we assumed that u was uncorrelated with ϵ_c^y; so S_{u,ϵ_c^y} also converges in probability to zero. This implies that $\hat{b} \xrightarrow{p} b$; the 2SLS estimator is still consistent if there is random misclassification error in the instrument.

The misclassification error will increase the asymptotic sampling variance of the estimator. Without the error, $\sqrt{n}(\hat{b} - b)$ would converge in distribution to a normal with mean 0 and variance $\Sigma_{T,x}^{-1}\Sigma_{g,g}\Sigma_{x,T}^{-1}$. However, from (A.7):

$$\sqrt{n}(\hat{b} - b) = (S_{x,T} + S_{x,u})^{-1}(\sqrt{n}\bar{g} + \sqrt{n}(S_{u,\epsilon_c^y})) \tag{A.8}$$
$$\xrightarrow{d} N(0, V_1) + N(0, V_2) = N(0, V_1 + V_2) \tag{A.9}$$

Where $V_1 = (\Sigma_{x,T} + \Sigma_{x,u})^{-1}\Sigma_{g,g}(\Sigma_{x,T} + \Sigma_{x,u})^{-1}$ and $V_2 = (\Sigma_{x,T} + \Sigma_{x,u})^{-1}V[u\epsilon_c^y](\Sigma_{x,T} + \Sigma_{x,u})^{-1}$. The covariance between x and u is non-zero. And while the expectation of $u\epsilon_c^y$ is zero, the variance does not. Therefore, $V_1 + V_2 > \Sigma_{T,x}^{-1}\Sigma_{g,g}\Sigma_{x,T}^{-1}$.

These results imply that the misclassification error reduces the power of the 2SLS models. The confidence intervals on the coefficients are larger due to the misclassification error in the instrument. Thus, if we can reject the null hypothesis that a coefficient is zero at the 95 percent level when estimating the model with the instrument which includes the misclassification error, this inference is valid for determining the statistical significance of the estimated $\hat{b}$.[1]

[1]In a large sample, the difference in the estimated standard errors caused by the misclassification error is likely to be minimal.

Appendix B

Gender Heterogeneity

Table B.1: Reduced Form Estimates- Female

	(1) Pooled	(2) Black	(3) White
T	-2.207***	-0.402	-1.900***
	(0.365)	(0.371)	(0.345)
Dev	-0.228***	-0.056*	-0.179***
	(0.033)	(0.024)	(0.028)
T x Dev	0.124***	0.030	0.114***
	(0.033)	(0.026)	(0.028)
MSA FE	Yes	Yes	Yes
N	11,383	5,057	10,132
R-Squared	0.387	0.137	0.235

Notes: Dependent variables are OI measures of upward mobility, which measure mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. The models are estimated on the tracts in the analysis sample, excluding tracts with missing OI measures or whose OI measures reflect crosswalk errors and the two-thirds tracts used to identify the location of the tipping points. MSA clustered standard errors in Parentheses.

* $p < 0.05$, ** $p < 0.01$, ***

Table B.2: Reduced Form Estimates- Male

	(1) Pooled	(2) Black	(3) White
T	-2.317***	-0.921*	-1.791***
	(0.332)	(0.365)	(0.287)
Dev	-0.180***	-0.024	-0.116***
	(0.041)	(0.026)	(0.033)
T x Dev	0.058	-0.006	0.053
	(0.040)	(0.027)	(0.034)
MSA FE	Yes	Yes	Yes
N	11,387	5,043	10,184
R-Squared	0.420	0.127	0.201

Notes: Dependent variables are OI measures of upward mobility, which measure mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. The models are estimated on the tracts in the analysis sample, excluding tracts with missing OI measures or whose OI measures reflect crosswalk errors and the two-thirds tracts used to identify the location of the tipping points. MSA clustered standard errors in Parentheses.

* $p < 0.05$, ** $p < 0.01$, ***

Table B.3: 2SLS Estimates- Female

	(1) 2SLS: Pooled	(2) 2SLS: Black	(3) 2SLS: White
Minority Share 1990	-0.384***	-0.069	-0.411***
	(0.059)	(0.063)	(0.075)
Dev	0.197*	0.010	0.292**
	(0.083)	(0.076)	(0.101)
T x Dev	0.042	0.023	0.023
	(0.041)	(0.030)	(0.036)
MSA FE	Yes	Yes	Yes
N	11,383	5,057	10,132
R-Squared	0.376	0.144	0.204

Notes: Dependent variables are OI measures of upward mobility, which measure mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. The models are estimated on the tracts in the analysis sample, excluding tracts with missing OI measures or whose OI measures reflect crosswalk errors and the two-thirds tracts used to identify the location of the tipping points. MSA clustered standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.4: 2SLS Estimates- Male

	(1) 2SLS: Pooled	(2) 2SLS: Black	(3) 2SLS: White
Minority Share 1990	-0.404***	-0.165*	-0.388***
	(0.060)	(0.065)	(0.070)
Dev	0.269**	0.133	0.329**
	(0.091)	(0.081)	(0.104)
T x Dev	-0.029	-0.022	-0.030
	(0.052)	(0.032)	(0.047)
MSA FE	Yes	Yes	Yes
N	11,387	5,043	10,184
R-Squared	0.403	0.109	0.168

Notes: Dependent variables are OI measures of upward mobility, which measure mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. The models are estimated on the tracts in the analysis sample, excluding tracts with missing OI measures or whose OI measures reflect crosswalk errors and the two-thirds tracts used to identify the location of the tipping points. MSA clustered standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix C

Sensitivity to Analysis Sample Definition

Table C.1: Reduced Form Estimates- Overall

	(1) Pooled	(2) Black	(3) White
T	-1.728***	-0.726*	-1.442***
	(0.332)	(0.333)	(0.344)
Dev	-0.212***	-0.065*	-0.137***
	(0.037)	(0.028)	(0.030)
T x Dev	0.124**	0.060	0.100**
	(0.037)	(0.032)	(0.032)
MSA FE	Yes	Yes	Yes
N	11,572	6,762	10,710
R-Squared	0.216	0.053	0.142

Notes: Dependent Variables are OI measures of upward mobility, which measures mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. Models estimated on all observations with OI mean predicted outcomes between negative 50 and 200, excluding the two-thirds tracts used to identify the location of the tipping points. MSA clustered standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.2: 2SLS Sensitivity Analysis

	(1) First-Stage	(2) 2SLS: Pooled	(3) 2SLS: Black	(4) 2SLS: White
Minority Share 1990		-0.300***	-0.129*	-0.285***
		(0.060)	(0.060)	(0.071)
T	5.702***			
	(0.587)			
Dev	1.112***	0.121	0.064	0.188
	(0.048)	(0.088)	(0.080)	(0.102)
T x Dev	-0.217***	0.060	0.044	0.039
	(0.057)	(0.047)	(0.038)	(0.043)
MSA FE	Yes	Yes	Yes	Yes
N	11,604	11,572	6,762	10,710
R-Squared	0.928	0.229	0.054	0.153
First Stage F-Stat	94.31			

Notes: Column 1: Dependent variable is 1990 minority share. Columns 2-4: Dependent Variable is OI measure of upward mobility, which measures mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. Models estimated on all observations with OI mean predicted outcomes between negative 50 and 200, excluding the two-thirds tracts used to identify the location of the tipping points. MSA clustered standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix D

Bandwidth Restrictions

Table D.1: Bandwidth = 40 Robustness Check

	(1) First-Stage	(2) 2SLS: Pooled	(3) 2SLS: Black	(4) 2SLS: White
Minority Share 1990		-0.570***	-0.257*	-0.460***
		(0.109)	(0.112)	(0.099)
T	3.230***			
	(0.521)			
Dev	1.163***	0.438**	0.220	0.396**
	(0.051)	(0.150)	(0.129)	(0.130)
T x Dev	-0.059	0.049	0.012	0.006
	(0.066)	(0.050)	(0.032)	(0.035)
MSA FE	Yes	Yes	Yes	Yes
N	9,879	9,854	5,334	9,814
R-Squared	0.825	0.227	0.077	0.190
First Stage F-Stat	38.45			

Notes: Column 1: Dependent variable is 1990 minority share. Columns 2-4: Dependent variables are OI measures of upward mobility, which measure mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. The models are estimated on the tracts in the analysis sample, excluding tracts with missing OI measures or whose OI measures reflect crosswalk errors and the two-thirds tracts used to identify the location of the tipping points. Observations restricted to those tracts with minority shares within 40 percentage points of tract's estimated tipping point. MSA clustered standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.2: Bandwidth = 30 Robustness Check

	(1) First-Stage	(2) 2SLS: Pooled	(3) 2SLS: Black	(4) 2SLS: White
Minority Share 1990		-0.732***	-0.271*	-0.575***
		(0.151)	(0.132)	(0.133)
T	2.629***			
	(0.559)			
Dev	1.195***	0.675**	0.252	0.566**
	(0.057)	(0.202)	(0.155)	(0.172)
T x Dev	-0.027	0.030	-0.011	-0.012
	(0.078)	(0.054)	(0.039)	(0.041)
MSA FE	Yes	Yes	Yes	Yes
N	9,265	9,244	5,013	9,219
R-Squared	0.801	0.025	0.073	0.079
First Stage F-Stat	22.12			

Notes: Column 1: Dependent variable is 1990 minority share. Columns 2-4: Dependent variables are OI measures of upward mobility, which measure mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. The models are estimated on the tracts in the analysis sample, excluding tracts with missing OI measures or whose OI measures reflect crosswalk errors and the two-thirds tracts used to identify the location of the tipping points. Observations restricted to those tracts with minority shares within 30 percentage points of tract's estimated tipping point. MSA clustered standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table D.3: Bandwidth = 20 Robustness Check

	(1) First-Stage	(2) 2SLS: Pooled	(3) 2SLS: Black	(4) 2SLS: White
Minority Share 1990		-0.794***	-0.219	-0.582**
		(0.234)	(0.209)	(0.210)
T	1.732***			
	(0.452)			
Dev	1.259***	0.743*	0.174	0.555
	(0.060)	(0.321)	(0.259)	(0.282)
T x Dev	0.015	0.067	-0.004	0.021
	(0.088)	(0.069)	(0.049)	(0.046)
MSA FE	Yes	Yes	Yes	Yes
N	8,558	8,538	4,538	8,524
R-Squared	0.776	-0.016	0.112	0.084
First Stage F-Stat	14.67			

Notes: Column 1: Dependent variable is 1990 minority share. Columns 2-4: Dependent variables are OI measures of upward mobility, which measure mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. The models are estimated on the tracts in the analysis sample, excluding tracts with missing OI measures or whose OI measures reflect crosswalk errors and the two-thirds tracts used to identify the location of the tipping points. Observations restricted to those tracts with minority shares within 20 percentage points of tract's estimated tipping point. MSA clustered standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix E

Oster's Causal Bounds

Table E.1: OLS: No Controls

	(1) Pooled	(2) Black	(3) White
Minority Share 1990	-0.113***	-0.027**	-0.072***
	(0.012)	(0.008)	(0.010)
Constant	46.596***	35.720***	48.908***
	(0.472)	(0.354)	(0.536)
Observations	12240	7078	11184
R^2	0.207	0.016	0.053

Notes: Dependent variables are OI measures of upward mobility, which measure mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. The models are estimated on the tracts in the analysis sample, excluding tracts with missing OI measures or whose OI measures reflect crosswalk errors and the two-thirds tracts used to identify the location of the tipping points. MSA clustered standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E.2: Controlling for Observables

	(1) Pooled	(2) Black	(3) White
Minority Share 1990	-0.158***	-0.049***	-0.120***
	(0.011)	(0.009)	(0.010)
Dev	0.013	0.007	0.072**
	(0.038)	(0.025)	(0.025)
T x Dev	0.055	0.019	0.015
	(0.038)	(0.022)	(0.024)
Log Avg. Household Income 1980	7.830***	5.983***	9.638***
	(0.515)	(0.598)	(0.493)
Unemployment Rt 1980	-0.175***	-0.029	-0.239***
	(0.035)	(0.033)	(0.032)
Poverty Rt 1980	0.083**	0.075*	0.097***
	(0.027)	(0.032)	(0.021)
MSA FE	Yes	Yes	Yes
N	11,402	6,639	10,571
R-Squared	0.550	0.170	0.413

Notes: Dependent variables are OI measures of upward mobility, which measure mean predicted rank in the adulthood income distribution for children in the 25th percentile. The percentiles are scaled from zero to 100. The models are estimated on the tracts in the analysis sample, excluding tracts with missing OI measures or whose OI measures reflect crosswalk errors and the two-thirds tracts used to identify the location of the tipping points. MSA clustered standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E.3: Oster's Bounds for the Causal Effect of Segregation

	(1) Pooled	(2) Black	(3) White
β^*	-3.116	-1.549	-1.699
Bounding Interval	[-0.158, -3.116]	[-0.049, -1.549]	[-0.120, -1.699]
N	11,402	6,639	10,571
R_{max}	0.715	0.220	0.536

Notes: β^* calculated using formula from Oster (2019) as detailed equation (7.2). ς is set to 1 and $R_{max} = min\{\tilde{R}, 1\}$ as recommended in Oster (2019). $\mathring{\beta}$ and $\mathring{R}$ for each column from corresponding column of table E.1. $\tilde{\beta}$ and $\tilde{R}$ for each column from corresponding column of table E.2.