THE EFFECTS OF SCHOOL DESEGREGATION ON CRIME

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One of the most striking features of crime in America is its disproportionate concentration in disadvantaged, racially segregated communities. In this paper we estimate the effects of court-ordered school desegregation on crime by exploiting plausibly random variation in the timing of when these orders go into effect across the set of large urban school districts ever subject to such orders. For black youth, we find that homicide victimization declines by around 25 percent when court orders are implemented and homicide arrests also decline significantly, which seem to be due at least in part to increased schooling attainment. We also find positive spillover effects to other groups, with beneficial changes in homicide involvement for black adults and perhaps whites as well. Our estimates imply that imposition of these court orders in the nation's largest school districts lowered the homicide rate to black teens and young adults nationwide by around 13 percent, and might account for around one-quarter of the convergence in black-white homicide rates over the period from 1970 to 1980.

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I. INTRODUCTION

One of the most striking features of crime in America is its disproportionate concentration in disadvantaged, racially segregated communities. For example in 2003 the homicide rate in Hyde Park, the racially and economically mixed home of the University of Chicago, was 3 per 100,000. In the directly adjacent neighborhood of Washington Park, where over half of residents are poor and 98 percent are African-American, the homicide rate was 26 times as high (78 per 100,000).[1] Nationwide, homicide is the leading cause of death for African-Americans ages 15-24, responsible for more deaths in 2006 than the 9 other leading causes of death *combined*. Because homicide disproportionately affects young people, nearly as many years of potential life are lost among blacks from homicide as from the nation's leading killer, heart disease (314,253 versus 329,638),[2] with the large majority of these deaths to black men.

This paper examines the effects on crime from what former Solicitor General Walter Dellinger called "the most important legal, political, social and moral event in twentieth-century American domestic history" [Williams, 1998, p. 400] – court-ordered school desegregation. In 1954 the U.S. Supreme Court ruled unanimously in *Brown v. Board of Education of Topeka* (347 US 483) that racial segregation in the public schools "denies to Negro children the equal protection of the laws guaranteed by the Fourteenth Amendment." At the time *Brown* was announced, the Harlem *Amsterdam News* declared it "the greatest victory for the Negro people since the Emancipation Proclamation" [Williams, 1998, p. 231].

A large body of research over the half-century since *Brown* has tried to understand the effects on children from court-ordered school desegregation, most of which has focused on academic outcomes.[3] For example Guryan [2004] exploits plausibly random variation across school districts in the timing of local court desegregation orders and finds that such orders generate large declines in school dropout rates

[1] http://www.cchsd.org/cahealthprof.html
[2] http://www.cdc.gov/injury/wisqars/index.html
[3] A few studies of school desegregation policies have focused on labor market outcomes; see for example Vigdor [2006], Ashenfelter, Collins and Yoon [2005], Boozer, Krueger and Wolkon [1992], and Rivkin [2000].

for blacks (around 25 percent), with no detectable impacts on whites.[4] These findings have good external as well as internal validity, since the analytic sample consists of almost all of our largest school districts. A companion literature has found that moving children to more economically or racially mixed schools and neighborhoods through school choice or housing voucher programs seems to have mixed effects on academic outcomes, but may reduce anti-social behavior.[5] One limitation of looking only at what happens to movers is the possibility that re-sorting children across social settings might be a zero sum game. In addition, the policies that are enacted to try to re-sort children across settings may themselves have additional system-level effects that impact everyone (for better or worse), for example by changing people's attitudes about themselves and American society, or by changing local public spending priorities.

The question of how court-ordered school desegregation impacts crime is important in part because from the very beginning the U.S. Supreme Court, and in particular Justice Hugo Black, was concerned that any academic gains from court-ordered desegregation might be offset by increased racial tensions and even violence [Klarman, 2004, p. 294]. Ignoring any impacts on crime could distort, perhaps significantly, the social welfare implications of this policy. For example with the influential Perry Preschool program, 70 percent of the monetized benefits come from reduced crime [Belfield et al., 2006].

This question is also of more than just academic interest. While residential segregation by race has been declining over the past several decades, school segregation has not [Vigdor and Ludwig, 2008]. Only

[4] Guryan [2004] find that court-ordered desegregation increased educational attainment of blacks both in and outside the south, and Reber [2007a] finds a similar effect in Louisiana. Lutz [2005] finds the termination of court-ordered desegregation reduces black educational attainment, but only outside of the south. These studies are not necessarily in conflict in part because the phase-out of desegregation studied by Lutz occurred in a very different environment from the one in which these orders were implemented. Residential segregation has decreased significantly [Glaeser and Vigdor 2003], funding is more equalized across school districts [Card and Payne 1998; Murray, Evans and Schwab 1998; Hoxby 2001] and attitudes toward race have changed dramatically [Schuman, Steeh and Bobo 1985; Quillian 1996]. In addition, desegregation may have caused permanent changes that outlive the formal end of court involvement. Finally, Lutz [2005] presents evidence that southern policy makers may take compensatory actions to help mitigate any negative impact from terminating a desegregation plan.

[5] Examples of public school choice studies include Cullen, Levitt and Jacob [2006], Hastings, Kane,Staiger and Weinstein [2007], and Deming [2009]. See Neal [2002] on private school vouchers, and Kling, Ludwig and Katz [2005], Sanbonmatsu et al. [2006], and Kling, Liebman and Katz [2007] on housing vouchers.

around one-half of blacks and one-quarter of whites believe the U.S. should do more to integrate schools [Public Agenda, 1998]. The majority of Supreme Court decisions about school desegregation since 1973 have gone against Civil Rights groups [Kahlenberg, 2001]. And in 2007 the Supreme Court issued a 5-4 decision striking down voluntary desegregation plans in Seattle and Louisville, which is sure to generate additional litigation [Lewin, 2007].

Our analysis exploits the fact that most of America's largest school districts were slow to desegregate after *Brown*, and so most districts wound up being forced to desegregate by local Federal courts in response to lawsuits by the NAACP. Variation across districts in the timing of these orders is our source of identifying variation, as in Guryan [2004] and Reber [2005], and is plausibly orthogonal to other determinants of youth outcomes given the NAACP seems to have filed cases strategically when and where they were most likely to win, rather than to maximize short-term social benefits.

Our findings suggest that court-ordered school desegregation on average reduces homicide victimization rates by around 25 percent among school-age blacks, and generates even larger proportional declines in homicide arrests.[6] These results come from using data just on the set of districts ever subject to such court orders, and comparing crime trends before and after the orders go into effect using data on homicide victimizations from the Vital Statistics (VS) and on offending from the FBI's Supplemental Homicide Reports (SHR). We focus on homicide because this is the most reliably measured crime, and accounts for a disproportionate share of the social costs of crime [Ludwig, 2006]. These crime impacts seem to be due at least in part to improved schooling outcomes, since the estimated effects are about as large in the summer as during the school year and persist into adulthood. Moreover the long term effects

[6] Only one previous study we know of has examined the issue of how school segregation in general is related to crime. In concurrent work (we became aware of the then-unpublished paper after we had begun work on this study), LaFree and Arum [2006] ask whether people brought up in different states, with different levels of school desegregation, are differentially likely to be incarcerated as adults, holding state of residence in adulthood constant. However their study may be susceptible to bias if the propensity of people with different levels of crime risk to move out of state are related to levels or changes in school segregation, or if omitted state policies or other social factors are correlated with levels or changes in school segregation.

appear to be quite different for cohorts that were born just a few years apart but differ in whether they would have been of school age vs. already out of school when court desegregation orders were enacted.

Unlike with many criminal justice interventions or private crime-prevention activities that may generate negative spillovers by displacing criminal activity, court-ordered school desegregation seems to generate positive spillovers: By making school-age youth less criminogenic and victimogenic, desegregation orders seem to lead to fewer homicides by school-age blacks against black adults and vice versa, and perhaps fewer homicides across race lines as well. It is possible that court orders also generate positive spillovers by changing community attitudes and freeing up law enforcement resources that would have gone towards investigating offenses by black youth, but we have no direct evidence on these points.

The key question is whether these patterns represent a real behavioral response or instead simply reflect non-randomness in the timing of when court orders are enacted. Some support for the validity of our research design comes from the fact that there is little evidence of pre-existing homicide trends for blacks or whites (for either school-age youth or adults) in the years *before* these court desegregation orders are enacted.[7] In addition, desegregation orders do not have any impact on mortality to either youth or adults from disease, an outcome that should logically not be affected by desegregation. Moreover, we find black homicides declined the most in districts that experienced the largest declines in school segregation levels. The fact that homicide declines most in areas with the largest "treatment dose" provides some additional support for our research design.

Since we rely on longitudinal data measured at the county level, a different concern is that desegregation orders might affect population migration patterns. However, we do not find any effect of court desegregation orders on migration across county lines, or on county socio-demographic

[7] Guryan [2004] shows districts that desegregated at different times have similar trends in socio-economic outcomes between the 1960 and 1970 censuses. Because we have annual data we can provide an even sharper test of this identifying assumption.

composition, and we obtain qualitatively similar results when we use larger geographic units (MSAs or bordering county groups). Our results also do not seem to be driven by measurement error in the denominator of our homicide rates.

Our findings suggest that the benefits from court-ordered school-desegregation may be far larger and widely distributed than previous research suggests. Our findings also have the potential to help explain several unresolved puzzles in national crime statistics, including why black-white homicide rates began to converge so substantially starting in the late 1960s [Cook and Laub, 1998] – just as school desegregation began in earnest in America's biggest cities.

The remainder of the paper is organized as follows. Section II provides some history behind the court orders we study, which is important to our claim that the timing of these orders is plausibly orthogonal to trends in other determinants of youth outcomes. A framework for thinking about how these orders might influence crime is presented in Section III. Our data and methods are presented in Sections IV and V, results are in Section VI, and implications are discussed in Section VII.

II. BACKGROUND

Shortly after the landmark 1954 *Brown* decision, the Supreme Court declared that school districts should desegregate "with all deliberate speed" (*Brown II;* 349 U.S. 294, 1955). What this meant in practice was not specified, and details were left to the lower Federal courts. Thurgood Marshall tried to be optimistic, claiming "those white crackers are going to get tired of having Negro lawyers beating 'em every day in court" [Williams, 1998, p. 239]. Yet few districts saw much desegregation for many years. Smaller districts, particularly in the South, began to desegregate in the 1960s after the Federal government threatened to withhold Title I funds [Cascio et al., 2008, 2010]. Large districts were slower to desegregate. Since *Brown* only bound five school boards [Klarman, 2004], most of the nation's largest districts had to be ordered to desegregate as a result of individual cases filed in local Federal court.

Our key identifying assumption is that among the set of large school districts ever subject to court desegregation orders, the timing of when these orders went into effect is unrelated to trends in other determinants of youth outcomes. This assumption seems plausible given that a large share of desegregation lawsuits were filed by the NAACP, which, given resource constraints, was selective in deciding when and where to file. The NAACP used a strategy starting well before *Brown* of filing lawsuits to establish a series of favorable legal precedents, rather than maximize short-term welfare gains. Guryan [2004] provides a model showing this is optimal in a legal system like that in the U.S. that assigns great importance to precedent.

The NAACP's initial legal strategy was to attack the principle of "separate but equal" established by *Plessy v. Ferguson* (1896) by challenging discrimination in graduate and professional schools [see NAACP, 2004]. The primary motivation for focusing first on post-graduate education, rather than K-12 schooling, was the perceived increased probability of winning – even if the number of students affected by desegregating post-graduate schools would be orders of magnitude smaller.[8] This strategy led to several key victories, which laid the groundwork for the *Brown* challenge (see Appendix A).

The NAACP's focus on litigating with an eye towards strategic legal considerations, rather than maximizing short-term social welfare gains, is evident in the *Brown* case itself. The NAACP focused on Kansas in part because race differences in school quality there were not as pronounced as in other states, which meant that the gains in school quality for blacks from desegregation in Kansas would be smaller than in other states. But focusing on Kansas had the strategic advantage of focusing the court on the issue of segregation itself, rather than on whether facilities in segregated schools were equal [NAACP, 2004].

[8] Many states that refused to admit blacks to post-graduate programs in public universities did not have separate segregated options. The NAACP sought to force states to either develop separate and equal options, which they doubted states could afford, or else to integrate graduate programs [Williams, 1998, p. 76, 94, 174]. Another benefit of focusing on graduate schools was to "bypass the inflammatory issue of 'race-mixing' among young children" [NAACP, 2004, p. 9].

Following the *Brown* and *Brown II* decisions, many large school districts enacted "freedom of choice" plans that ostensibly gave minority students the option to attend different schools, but in practice did not achieve much desegregation. These placement plans were prohibited by the Supreme Court in 1968 in *Green vs. New Kent County, Virginia* (391 U.S. 430), which in turn led to a surge of litigation activity in the lower Federal courts. Our focus is mostly on these major local Federal court decisions following *Green*, which, as we demonstrate below, actually helped desegregate schools.

Over time the process through which desegregation lawsuits were filed seems to have become even more idiosyncratic and decentralized. When and where cases were filed seemed to depend in part on the decisions of individual plaintiffs and attorneys to file cases (and funders to support these suits), which presumably depended in part on the probability of success in court. Federal courts seem to have varied considerably in how they handled desegregation cases [Klarman, 2004]. The belief that districts were "cherry picked" for desegregation lawsuits seems to be widely held by lawyers even today.[9]

While this history suggests that the timing of local Federal court desegregation orders is plausibly orthogonal to trends in local social conditions, Southern districts do seem to have been disproportionately likely to be subject to court orders earlier in the period (see Figure 1). This regional patterning is itself the product of the evolution of legal doctrine,[10] and suggests the importance of adequately controlling for region-specific trends in crime outcomes over time in our empirical analysis below.

III. CONCEPTUAL FRAMEWORK

[9] For example, consider why the plaintiffs against Washington DC's handgun ban filed there in the *Heller* case: "The gun law there is one of the most restrictive in the nation, and questions about the applicability of the Second Amendment to state laws were avoided because the district is governed by federal law. 'We wanted to proceed very much like the NAACP,' Mr. Levy said, referring to that group's methodical litigation strategy intended to do away with segregated schools" [Liptak, 2007].
[10] Prior to 1973, court-ordered desegregation could only occur in districts proved to have engaged in *de jure* segregation. The 1973 *Keyes v. Denver School District* decision (413 U.S. 189) ruled court-ordered desegregation could proceed in areas that had *de facto* segregation resulting from past state action, which made desegregation lawsuits more viable outside of the South.

Court school-desegregation orders could reduce crime for purely mechanical reasons, by incapacitating youth on long bus rides. But we expect the primary mechanism through which court orders might affect crime is through improved schooling outcomes of minority youth, which may increase both the opportunity costs of crime and the cognitive, socio-emotional, and behavioral skills that enable youth to stay out of trouble.[11] Desegregation may elevate educational attainment by exposing black students to higher quality schools [12] and more developmentally productive or pro-social peer groups.

This schooling effect on crime might be quite large. As noted above, Guryan [2004] finds desegregation orders reduce black dropout rates by around 25 percent, which the estimates from Lochner and Moretti [2004] suggest should in turn black homicide arrests by around 20 percent.[13] Put differently, for every 100,000 black youth, court ordered school-desegregation would lead to 3,000 fewer dropouts and 6 fewer homicides. By way of comparison, in 2005 the homicide rate was around 6 per 100,000 for the U.S. as a whole and 35 per 100,000 for blacks ages 15-19. Lochner and Moretti's [2004] study also seems to suggest that the main effect of schooling is to act on the extensive margin of criminal involvement (offending rate) rather than on the intensive (severity) margin, since they do not seem to find evidence of substitution from more to less serious crimes.

We believe that the effects of school desegregation on attitudes may also be quite important, for example by changing perceptions of self worth among minority children, as in Kenneth Clark's famous "doll study" [Clark, 1950], by reducing prejudice and anxiety about intergroup interactions, as suggested by the large body of research in social psychology on the "contact hypothesis" [Allport, 1954, Petigrew,

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[11] Oreopoulous and Salvanes [2009] present evidence that schooling instills positive behavioral traits such as patience and a reduced tendency to engage in risky behavior.

[12] The fact that many studies do not find large black-white differences in school inputs does not rule out important differences in school quality, given the weak correlation between school attributes and student achievement [see also Boozer et al., 1992].

[13] Lochner and Moretti [2004] suggest that a 10 percentage point increase in high school graduation rates would reduce overall violent crime arrest rates for blacks by 25 percent [see their footnote 36]. Their Table 11 shows that for blacks and whites pooled together, the estimated effect of dropout rates on murder specifically is about 2.66 times as large as the effect on the overall violent crime rate. If the ratio of effects on murders versus all violent crimes is the same for blacks and whites, then a 10 percentage point increase in graduation rates would reduce murder arrests for blacks by two-thirds.

1998, Pettigrew and Tropp, 2006], and by changing fundamental attitudes about American society.[14] Some additional support for the potential importance of these attitudinal mechanisms comes from Guryan's [2004] findings that black dropout rates seem to have declined even in the very first year after a court desegregation order, despite the fact that black students would not have been exposed for long to different schools or peer groups [see also Lutz, 2005 and Rivkin and Welch, 2006].

The net impact of any changes in criminal offending propensities by black youth will also depend on the response of potential victims, as suggested by Ehrlich [1981] and Cook's [1986] model of the "market for criminal offenses." In panel A of Figure 2 improvements in the schooling outcomes of minority youth shift inward their "supply of offenses" schedule, which we expect to be increasing in the "price" of crime (loot plus non-monetary gains from crime, minus expected costs of punishment) [Becker, 1968]. But if the "demand for offenses" is downward sloping, as suggested by evidence that crime prevention activities by potential victims increase with the crime rate [Ehrlich and Becker, 1972, Clotfelter, 1978, Philipson and Posner, 1996], then part of the benefits from the decline in the supply of offenses by minority youth will be realized by reductions in costly protective behaviors by victims.

One of the concerns of the opponents of school desegregation had to do with negative spillovers. If court desegregation orders simply re-sort black and white children across existing school options within a district, then any gains in school quality for blacks might be exactly offset by declines among whites. Similarly, if attending school with relatively higher-achieving, more affluent whites is developmentally productive for African-American children, then in principle desegregation might lead to adverse peer

[14] Some research from political science supports the "contact hypothesis," in that court-ordered school desegregation seems to have reduced self-reported racial intolerance among whites [Rossell, 1978]. Furthermore, large Southern school districts desegregated mostly in the 1968 to 1972 period, and school segregation declined far more in the South over this time period than in the North. From 1970 to 1972 survey measures of white racial intolerance declined by around 16 percent in the South compared to only about 5 or 6 percent in the North, suggesting a simple difference-in-difference estimate for the effect of court-ordered school desegregation on racial intolerance of around -10 percent. Note that this easing of racial intolerance apparently took time to manifest itself, as racial tensions rose in many districts at the time of desegregation – see footnote 14.

effects for white students. Whether the overall youth crime rate in a district increased or decreased after desegregation would depend in part on whether there are non-linearities in peer influences on behavior, or differences between black and white youth in how their behavior responds to peers and school quality. Concerns about these negative spillovers themselves seem to have led to a different type of negative spillover: white hostility. Surveys after *Brown* found 15-25 percent of Southern whites endorsed the use of violence if necessary to preserve racially segregated schooling [Klarman, 2007, p. 192], and of course many cities experienced riots following their attempts to desegregate public schools.[15]

But court-ordered school desegregation could generate positive spillover effects as well. Any beneficial change in the behavior of minority youth may reduce victimizations to other race and age groups, given that a non-trivial amount of homicide offending occurs across race and age lines [Cook and Laub, 1998]. Table 1 shows that for black homicide offenders ages 15-24, over half of victims were 25 and over, and nearly one out of five victims was white.[16] School desegregation could reduce the homicide *offending* behavior among people of different age or race groups as well, by making black youth less victimogenic. Far and away the most common motivation to commit homicide is an altercation [FBI, 2007, Chicago PD, 2008]. Research in criminology suggests that victims often help initiate or sustain violent events [Wolfang, 1958, 1967], and in fact the characteristics of homicide offenders and victims are quite similar, with the vast majority of both groups having a prior arrest record [Chicago PD, 2008, Schreck et al., 2008]. Table 1 shows that among black homicide offenders ages 35+, 16 percent of victims

[15] Arkansas Governor Orval Faubus threatened that if efforts to desegregate Little Rock's Central High in 1957 were successful, "blood will run in the streets" [Williams, 1998, p. 263]. In Birmingham in 1973, "an orgy of mob violence resulted from a court order desegregating a number of previously all-white schools … rioting whites killed at least three blacks. During this troubled period, a black church was bombed – killing four little girls at Sunday School and injuring 23 others." [Rodgers and Bullock, 1972, p. 73] See Greenberg [1994] for other examples.

[16] Table 1 is calculated using data for our sample of counties through the year 1988 from the FBI's Supplemental Homicide Reports; these data are discussed in detail below. To simplify the table we exclude figures for the small number of offenders and victims under age 15, as well as victims and offenders of other race groups, both of which are rare in our study sample.

were under age 25. More pro-social behavior by youth may shift in the "demand for offense" schedule facing adult offenders (Panel B, Figure 2), which would reduce offending by adults.

A second potential source of positive spillover effects may arise if court-ordered school desegregation induces changes in public good provision. For example, some local Federal judges required school districts to increase overall spending as part of their desegregation plans.[17] Law enforcement spending could also have changed. For example when Boston's attempts at desegregation in 1974 led to riots by whites and fighting between students, there was an aggressive police response, or as one policymaker put it, "a cop for every kid" [HGSE News, 2000]. Even if total police spending is unchanged, any decline in criminal behavior among youth would free up the amount of police resources available to investigate adult offenses, which would basically be what Kleiman [1993] calls "enforcement swamping" but in reverse. This would shift in the demand-for-offense schedule facing adults (Panel B, Figure 2).

IV. DATA

Our study focuses on the set of large school districts subject to court orders that were included in a dataset compiled by Welch and Light [1987] for the U.S. Commission on Civil Rights; the districts and the year of their court desegregation order are listed in Appendix Table A1. These data cover all districts that in 1968 were 20 to 90 percent minority with enrollments of 50,000+, and a random sample of districts that were 10-90 percent minority with enrollments of between 15,000- 50,000. This sample is not necessarily representative of all districts in the U.S., but is still of great interest given it accounts for such a large share of minority students – and crime – in America.[18] We seek to identify the effect of court-ordered school desegregation on youth crime in these districts.

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[17] The *Milliken II* decision (433 U.S. 267, 1977) permits judges to order increased educational spending [Orfield and Eaton 1996; Lindseth 2002]. The funding is provided by the state government in some cases.

[18] In 1968 these districts accounted for 45 percent of minority enrollment in the U.S.; the counties containing these districts accounted for nearly half of all homicides to blacks in the U.S., and just over one-third of all homicides to whites.

Our main data sources are the Vital Statistics (VS) and the FBI's Supplemental Homicide Reports (SHR), aggregated to the county level. We focus on homicide because this is the most reliably measured crime, accounting for a disproportionate share of the social costs of crime [Ludwig, 2006]. We have also examined other types of crimes using data from the FBI's Uniform Crime Reports. But these data have a great deal of measurement error, particularly at the county level [Maltz, 1999], and so our results for these other crimes are imprecisely estimated and ultimately not very informative (see Appendix B).

One complication of working with county-level crime data is that the Welch and Light dataset is at the level of the school district. For 37 percent of the districts in our sample, the school district boundary follows the county boundary. This figure is higher in the South (65 percent). We believe the county should be the preferred unit of analysis even if homicide data were available at the district level, because county data are less susceptible to problems from "white flight" in response to court orders. So long as whites stay in the county, movement to nearby school districts or private schools will not generate any mechanical change in homicide rates (this is also true for blacks). We devote substantial attention below to showing our results are not due to compositional changes in the populations living in our counties.

The VS provides a census of all deaths and enables us to measure homicide victimization rates by county and year to separate age-race groups over the period from 1959 through 1988. Starting in 1976 we can use the SHR to capture information on homicide victims, and when police have made an arrest, homicide offenders. The fact that SHR data on offenders is available only starting in 1976 limits our ability to measure short-term effects of desegregation orders, since a large share of these orders were enacted by that time (Figure 1). But the SHR data extends through 2003, and so can be used to examine the effects of school desegregation orders on long-run homicide offending behavior.

County population data come from the Census and the VS interpolations for inter-censal years. Measurement error for county population could in principle lead to systematic biases with our estimates if one consequence of court-ordered desegregation is to increase “white flight” to other counties, but we show below this does not seem to be a concern in practice. Our main estimates use Census / VS population data to construct offending and victimization rates, but we find that offending rates are similar when we instead use population data just for those jurisdictions that report SHR data to the FBI.

Table 2 provides some general background on our analytic sample. These are large counties, with a mean population of around 677,000 over our study period. Around 17 percent of county residents are African-Americans. Homicide victimization rates to white youth 15-19 increase dramatically from 1960 to 1980, from 2.3 to 9.7 per 100,000, while victimization rates to black youth 15-19 start off much higher (20.3 per 100,000), almost double from 1960 to 1970, and then decline over the 1970s. This convergence in black and white youth homicides continues through the mid-1980s [Cook and Laub, 1998, p. 44].

V. METHODS

Our basic empirical approach is to examine how homicide victimization rates for white or black youth in county *i* in year *t*, y_{it}, change in response to court school desegregation orders. Our key explanatory variables are a set of indicators $D_{p,it}$ equal to one if in calendar year *t*, district i had a desegregation plan implemented p years beforehand, and equal to 0 otherwise. In most models we use the year before desegregation plans are implemented as our reference point. We define indicators for the period 6 or more years before the orders go into effect, for each of the five years individually before orders are enacted, for each of the six years individually after orders are enacted, and then the period 7 or more years after the orders are implemented, although we estimate more parsimonious specifications as well. We condition on a set of county and region-year fixed effects, γ_i and $\delta_{t,r}$, the latter being particularly important given that Figure 1 shows some regional pattern to the timing of desegregation orders within our sample of counties. Our main estimating equation is given by (1).

(1) $$y_{it} = \alpha + \sum_{p \in \Psi} \beta_p D_{p,it} + \gamma_i + \psi_{t,r} + \varepsilon_{it}$$

The coefficients of interest, the β_p vector, are identified under the assumption that, in the absence of the desegregation plans, homicide rates would have trended similarly in districts which had desegregation plans implemented at different times. The vector of pre-desegregation coefficients provides a partial test of this assumption. Our flexible specification also allows for effects of desegregation on crime that are either immediate or gradually unfold over time, which is important because it will take several years for all of the individuals in a given age cell to have been "treated" following a court order. More generally, many of the mechanisms through which desegregation orders could impact crime, such as higher-quality schools or peers, might have effects that depend on duration of exposure. In addition, court desegregation orders in some districts were phased in gradually.[19] Finally, some factors that might increase violence following desegregation, such as inflamed racial tensions, may dissipate with time.

It is important that the entire β_p vector be identified from the same set of counties, to avoid confusing the time path of how areas respond to desegregation with changes in the composition of counties in our analytic sample. We therefore restrict our sample to counties that contribute to each of the first six points in the post-desegregation vector and at least four of the last five years in the pre-desegregation vector.[20] This removes around 8 percent of the county-year observations from the sample. Estimates produced using the full sample are similar to those from the restricted sample.

In our main set of estimates, we treat the individual counties as the observational unit and estimate equation (1) without weighting by county population, to estimate the effect of school desegregation on the

[19] The average district in our sample implemented their initial court-ordered plan in around 1.5 years, although some phased in their plans over as long a period as 3 or 4 years. Twenty percent of districts had a second plan put in place after the initial plan.
[20] Note that we lack reliable Vital Statistics data for 1967. A large number of school districts desegregated between 1968 and 1972. Requiring counties to contribute to all of the last five points of the pre desegregation vector would result in the loss of a significant percent of the sample. We therefore require that each county contribute to the identification of 4 of the last 5 pre vector coefficients, instead of contributing to all 5.

average county. However the results are similar when we estimate the effects on the average juvenile instead, by re-estimating (1) using each county's juvenile population as weights.

We initially estimate equation (1) using OLS in levels, and calculate standard errors clustered at the county level to account for serial correlation [Bertrand et al., 2004]. This might not be the right functional form since there is substantial cross-sectional variation in homicide rates, particularly for black youth (Figure 3), which might suggest a proportional effects model. But a standard log linear specification is complicated by the fact that many counties record no youth homicides in some years. In order to estimate a proportional response model using OLS, we use the method from Pakes and Griliches [1980]. The homicide rate is transformed by replacing zero values with ones, and then we log this transformed variable. A dummy variable, equal to one for all instances in which the true homicide rate equals zero, is included as an explanatory variable. This "log linear dummy model" allows for estimation of a proportional response using a linear model but is biased because the dummy variable is endogenous.

In order to estimate a proportional response model that does not suffer from the bias inherent to the log linear dummy model, we also estimate a fixed-effect Poisson count model using a quasi-maximum likelihood (QML) approach [Wooldridge, 1999; see Appendix C for details]. This estimator maximizes the same log-likelihood function as the standard fixed-effect Poisson model, but rather than assuming mean-variance equality, relies on a robust standard error calculation instead. The model is fully robust to distributional misspecification. We use total homicide counts for the relevant age-race group of interest as the dependent variable, and control for the county population in that age-race group as the exposure variable.[21] (The computer code to estimate this model is available upon request.)

[21] For estimates that use as the dependent variable the number of homicide offenses that occur across race and class lines, there is a question about whether we should use the offender population or the victim population as the exposure variable. In the results shown below, we use the offender population as the exposure variable but the results are quite similar in either case.

We also experiment with re-estimating (1) including county-specific linear trends, and a separate model specification that controls for trends in crime associated with county demographic characteristics measured at the start of the sample period. This "base demographic model" is given by:

$$(2) \qquad y_{it} = \alpha + \sum_{p \in \Psi} \beta_p D_{p,it} + \gamma_i + \psi_{t,r} + \lambda_t X_i + \varepsilon_{it}$$

where X_i is the vector of time-invariant county characteristics measured as of the 1960 Census and λ_t is the vector of time varying coefficients on these characteristics. This model controls, in a flexible manner, for trends in crime associated with socio-demographic attributes such as median household income, percent of population over age of 25 with a high school degree, the percent of employment in manufacturing, and percent non-white. Time-variant demographic variables are not included in the model because they may be endogenous to desegregation.[22]

VI. RESULTS

We begin by replicating results from Reber [2005] and Guryan [2004] showing that the court orders we study did in fact succeed in reducing levels of racial segregation in schools, although we note that changes in school racial composition are only one mechanism through which these court orders might influence criminal behavior. Our estimates suggest desegregation orders reduce homicide victimizations for black youth and adults by around 25 percent, and reduce offending among both groups as well. White homicides also decline, although whether white offending rates are affected is less clear in our data.

A. Impacts on School Segregation

The top panel of Figure 4 shows that following court desegregation orders, there is a sharp drop in the dissimilarity index, which ranges from 0 to 1 and is the percent of black students who would need to be reassigned to a different school for perfect integration to be achieved given the district's overall racial

[22] We have experimented with including a time-varying measure of non-school desegregation race riots (such as the 1965 Watts Riot in Los Angeles), which has no effect on our results.

composition.[23] The figure plots the regression coefficients on our indicator variables for years before and after desegregation orders go into effect (the year before is the reference period), using OLS to estimate equation (1) conditioning on county and region-year fixed effects. We see little evidence of pre-existing trends in our counties in the years prior to the court orders, followed by a large drop in the dissimilarity index in the first two years after the court order, consistent with a decrease in school segregation in these areas. Within two years the impact is 0.2, which is a large share of the 1968 mean of 0.71 in our sample.

Note that a decline in the dissimilarity index need not imply that blacks are attending schools with proportionately more whites, if there is a change in the share of the district that is white. We therefore also examine the exposure index, which reflects the percent of white students in the average black student's school. A decrease in segregation is reflected by an *increase* in the exposure index, which is clearly what happens following court orders (Figure 4, Panel B) – the impact of 0.15 within two years is again large relative to the 1968 mean value in our sample of .28.[24] Our findings that desegregation orders produce a sharp and persistent decline in racial segregation are similar to those in Reber [2005].

In Panel C we present some original results showing that court-ordered desegregation reduces the number of schools within a district by 3 to 5 percent. Predominantly minority schools seem to have been most likely to be closed [Hamilton, 1968, Butler, 1974, Orfield 1975, Haney 1978], which raises the

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[23] The dissimilarity index is defined as:

$$D_t = \frac{1}{2} * \sum_{i=1}^{n} \left| \frac{b_{it}}{B_t} - \frac{w_{it}}{W_t} \right|,$$

where b_{it} and w_{it} refer to the number of black and white students, respectively, at school *i* at time *t* and B_t and W_t refer to the total number of black and white students, respectively, in the school district.

[24] The exposure index is defined as:

$$E_t = \frac{1}{B_t} \sum_{i=1}^{n} b_{it} * \frac{w_{it}}{t_{it}},$$

where t_{it} is the total number of students in school *i*. It is interpretable as the percent of white students in the average black student's school. For a given district, it ranges from 0 to the percent of white students in the district as a whole. It can be viewed as a measure of the extent of contact between the two races.

possibility that average school quality in these districts could have increased. In any case, these results taken together suggest there was an interesting “treatment dose” that resulted from desegregation orders.

B. Homicide Victimization

The results shown in Table 3 suggest black homicide victimization rates in the Vital Statistics declined substantially following implementation of court school-desegregation orders. These results come from estimating a parsimonious version of equations (1) and (2) where the key explanatory variables of interest are indicators for whether the county-year observation falls within the first five years after a desegregation order is imposed, or 6 or more years after such an order.

Our preferred QML count model suggests that for black youth of high school age (15-19) homicide victimization rates declined by 17 percent the first 5 years after the court orders.[25] The effect seems to persist, with an estimated decline of 27 percent in the period 6+ years after the court orders, although we note that this coefficient is identified from an unbalanced set of counties.[26] Estimates from OLS in levels or the log dummy model are slightly smaller in proportional terms but qualitatively similar.

As noted above, school-age offenders often kill older people, and so in the other panels of Table 3 we expand our focus to older victims as well. Compared to the results for black victims 15-19, the estimated effects are of about the same size in proportional terms for victims ages 15-24 and 25-34, and are slightly smaller for victims ages 35-44. In what follows we will typically show results for both the 15-19 and 15-24 year old groups; the advantage of the former is almost everyone in that age range is clearly “treated,” while the advantages of expanding the age range include bringing more data to bear and accounting for offending by teens against young adults. In addition, by 6 years after the desegregation orders, most 15-24 year olds would have been spent time in school after the orders were enacted.

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[25] Both the log dummy coefficients and the QML count model coefficients can be interpreted as semi-elasticities of the homicide rate with respect to the year of desegregation (see Appendix C).

[26] Counties which desegregated early contribute more observations to its identification than do counties which desegregated later, and so the estimate may therefore partially reflect sample composition issues.

Note that all of our estimates in Table 3 condition on region-year fixed effects to account for the regional pattern in the timing of court desegregation orders shown in Figure 1. Our results are also robust to re-estimating our OLS levels model and our QML count model controlling for interactions of baseline county socio-demographic characteristics and year effects, as in equation (2), or to re-estimating our OLS model in levels controlling for county-specific linear trends.

Table 4 shows that desegregation orders seem to reduce homicide victimizations to whites as well. We generally do not see any statistically significant impacts of desegregation orders on white homicides during the first five years after these orders go into effect. But 6 or more years after these orders are in effect, victimizations to whites 15-19 decline by 23 percent. We also see signs that homicide victimization rates might have declined for older whites as well (ages 25-34 and 35-44), although for these older groups the results are somewhat sensitive to our choice of model specification and estimation approach.

The key identifying assumption behind our study is that the timing of when these desegregation orders go into effect is unrelated to trends in other determinants of youth homicide. To explore this issue we estimate the time path of homicide victimization rates using equation (1), which includes a full set of indicators for the years before and after these court orders go into effect. Figure 5 presents the results for black victims ages 15-24 and 25-34; results for 15-19 and 35-44 year olds (which are less precisely estimated) are in the appendix. For blacks there is very little evidence of any pre-existing trend in homicide rates before desegregation orders go into effect for both age groups. When the desegregation orders are implemented, we see a break in trend. Our findings are generally similar whether we use our OLS levels and QML count models (shown), or the OLS log dummy model (unreported).

Figure 6 shows that there is no evidence of pre-existing trends in white homicide victimization rates either, although compared to the results for blacks, there appears to be more of a delay in when white victimization rates decline following desegregation orders. The gradual impact of desegregation orders on white and to some extent black homicide victimizations might reflect the fact that the share of prime-age offenders exposed to school desegregation orders increases over time.

C. Homicide Offending

Victimization data are only partially informative about behavioral responses by specific age or race groups given the amount of cross-group offending documented in Table 1. To examine offending directly we use data from the SHR, which has the drawback of only providing information on offenders when the police identify a suspect or make an arrest. Another very important drawback is that the SHR data are available only back to 1976, and so estimates for short-term effects of desegregation orders will not fully use data from the nearly 75 percent of districts in our sample that enacted court orders before 1977 (Figure 1). We have more power with the SHR to detect longer-term impacts on offending.[27]

With these qualifications in mind, Table 5 provides evidence for a decline in homicide offending by high-school aged blacks (15-19) after court desegregation orders go into effect. In order to see the results of truncating the panel, in column 1 we replicate our main victimization results using the VS data from just 1976 forward. Our QML count model implies large reductions in homicide arrests to black youth, equal to 33 percent during the first 5 years after the court orders and 55 percent 6+ years out. The offending impact for our full sample of counties might be somewhat smaller than in the one-quarter of counties that desegregated after 1977 (i.e., the ones we rely on to estimate offending impacts with the SHR), as suggested by the fact that the *victimization* impacts seem to be somewhat smaller in the full sample.[28] There is no clear evidence that offending by whites changed as a result of these orders.

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[27] For example Figure 1 shows most desegregation orders go into effect around 1968 or later. If we examine offending measured, say, 10 years after the court orders, our estimates would use data on almost all of the sample districts.

[28] For example, for blacks 15-19 years old, the victimization effect during the first five years after desegregation orders is 17 percent in the full sample versus 27 percent among the post-1977 desegregators; in the period 6+ years after the court orders are enacted, the effects are 27 percent versus 43 percent, respectively. Note, however, that when the sample is divided in half by date of desegregation, we find no evidence that the desegregation impact differs for "late" desegregators in the VS sample.

The estimates imply very large declines in black youth homicide offending rates, although it is very important to keep in mind that the SHR data underlying Table 5 are quite thin (since we are only using data from the one-quarter of school districts in our sample that had court orders enacted after 1976) so the magnitudes of the estimates should be interpreted cautiously. The fact that the estimates point in the direction of potentially large effects seems consistent with other evidence that criminal behavior is very sensitive to environmental influences, evidence that includes the massive time-series variation that we observe in crime rates. For example between 1984 and 1992 the homicide arrest rate to blacks 14-24 nationwide *tripled,* and then dropped by *half* over the next seven years [Levitt, 2004, p. 180]. As noted above, the estimated effect of desegregation orders on black dropout rates by Guryan [2004] combined with Lochner and Moretti's [2004] estimates for the effects of schooling on crime would predict that desegregation orders would reduce homicide rates by 6 per 100,000. This is a sizable share of our estimates from OLS in levels, which imply effects of 6 per 100,000 in years 1-5 after court orders and 13 per 100,000 thereafter. As another point of comparison, our estimates suggest black youth homicide arrests decline by 33 percent in years 1-5 after desegregation orders, almost the same size as the decline in violent-crime arrests from moving to less distressed neighborhoods in the 5-year follow up study of the MTO randomized mobility experiment [Kling, Ludwig and Katz, 2005].[29] Ludwig and Kling [2007] find racial composition may be the most important neighborhood attribute in affecting violent crime.

Homicide offending seems to have declined among black *adults* as well as youth in the years right after these school desegregation court orders are implemented, as shown in Table 5 (columns (5) – (8)).

[29] Kling, Ludwig and Katz [2005] find that while MTO moves cause a reduction in violent crime arrests for both male and female youth in the mobility treatment versus control groups, by three or four years after random assignment, treatment group boys experience more arrests for property offending than those in the control group. We do not know whether there is an offsetting increase in property offending induced by court desegregation orders, given the limitations of the UCR county-level data on offenses besides homicide, discussed in the appendix. But from a social welfare perspective the social costs of violent crime is so much higher than for property offending that the net effect of the MTO intervention (setting distributional considerations aside) is to substantially reduce the social costs of youth crime offending.

Our QML count model suggests that within the first five years after desegregation orders are enacted, homicide arrests declined by 22 percent for blacks age 25-34 and by 27 percent for blacks age 35-44.. These effects seem to persist, given that the coefficients for 6+ years after desegregation orders are about as large as the year 1-to-5 effects.

Table 6 shows that there are large declines in offending rates across age groups among blacks following court desegregation orders. The magnitudes of these estimates should be interpreted even more cautiously than those in Table 5, since we are now dividing up data from just one-quarter of our analytic sample into very detailed offender-victim cells. We focus on our 10-year age groupings (15-24, 25-34, 35-44) to help address the thinness of the data, and lump together data on white victims of all ages for the same reason. The results shown in Table 6 suggest that the rate at which black offenders 15-24 killed older adults as well as other 15-24 year olds declined following court desegregation orders. The data provide at least suggestive evidence that the rate at which adults offended against younger people may also have declined following desegregation orders, although the standard errors around these point estimates are quite large. Our results provide suggestive support for the hypothesis that desegregation orders may have reduced victimization and offending among older blacks by making younger blacks less criminogenic and victimogenic

We also look at the degree to which desegregation orders may have changed offending rates across race groups in the short term (column (4)), although again our statistical power with the SHR is limited because only one-quarter of the districts enacted court orders after 1976 when the SHR begins. While none of these point estimates are statistically significant, the standard errors are so large that we cannot rule out either a zero effect or a very large effect.[30]

[30] In some instances the SHR contains missing values for the race and/or age of the victim and/or offender. Fox and Swatt [2009] produce a version of the SHR that imputes this missing data using multiple imputation methods. The imputed data is not perfectly suited for our analysis because the age ranges into which offenders and victims are broken into are broader than those

Table 7 provides evidence that the impact of court school-desegregation orders on youth homicide offending persists into adulthood. The outcome here is homicide arrest rates to people ages 35 to 44. Our key explanatory variable is an indicator equal to one if a desegregation order went into effect 25+ years before the calendar-year in which the SHR homicide offending data are measured, which captures whether a desegregation order was in place when those ages 35-44 were 19 or younger. Note that by the time that even the earliest desegregating district in our sample reaches the 25th year after its court order is enacted, almost all of our districts will have had court desegregation orders implemented. This means that we are basically comparing homicide arrest rates for 35-44 year olds who were of school age when desegregation orders were enacted (i.e., their district's court order was implemented 25+ years ago) to 35-44 year olds who were already adults when their district's court order was put into place. So this comparison captures just the direct effect of attending desegregated schools – spillover effects are differenced out. Column (1) shows that for blacks of school age when court orders were enacted, the QML count model provides evidence for a decline in offending at age 35-44 of 14 percent (Panel A).

Column (2) of Table 7 seems to support a causal interpretation of these estimates by showing that there is a sharp difference in the effect on cohorts that were born fairly close together in time but differ in whether they actually attended desegregated schools. Our key explanatory variables are now indicators for whether a district desegregated 20-24 years ago (most 35-44 year olds would have been too old to have attended desegregated schools), 25-29 years ago, or 30+ years ago; county-year observations that fall within 20 years of enactment of a court order are the omitted reference group. The coefficient for whether the district desegregated 20-24 years ago is close to zero, consistent with any spillover effects being differenced away. In contrast, the coefficients for whether the district desegregated 25-29 years ago or 30+ years ago, which are identified by comparing people who did versus did not attend desegregated

used in our analysis. Furthermore, the imputed data is most appropriate for national or state-level analysis. However, where we can replicate our estimates with the imputed data, the results are qualitatively similar to those shown on Table 7.

schools, are much larger. The fact that the estimated effect differs so much among birth cohorts born close together in time but who simply differ in whether they actually attended desegregated schools seems to support a causal interpretation of these results as the "direct effect" of attending desegregated schools. Any confounding influences from changing county demographics or social policies would not be expected to have such sharply different influences on cohorts born just a few years apart.[31]

Finally, the bottom panel of Table 7 suggests there was a substantial long-term decline in the rate at which blacks kill whites. Note we have much better power to detect long-term as opposed to short-term impacts on cross-race offending, because all of the districts in our analytic sample contribute information to the estimation of these long-run impacts as opposed to just one-quarter of districts contributing towards our short-term impact estimates. Table 8 examines whether there is any evidence for changes in long-term homicide offending behavior by whites, and the rate at which whites kill blacks. While many of the point estimates are negative and large from an economic perspective, they are fairly imprecisely estimated.

D. Robustness and Falsification tests

Are the results that we estimate really due to school desegregation orders, or to some other factors that might happen to be changing around the same time these court orders go into effect? The fact that we do not see systematic differences between desegregating and other counties' homicide rates in the immediate years *before* these court orders go into effect provides some partial reassurance against a story focused on omitted variables bias. We've also shown our results are not very sensitive to conditioning on interactions of year effects with base-year demographic characteristics, or county-specific linear trends.

[31] It is interesting that the estimated effect on homicide arrests among 35-44 year olds who attended desegregated schools, -14% , is almost exactly equal to the difference between the estimated reduction in homicide arrests to 15-24 year olds 6+ years after the court orders are enacted and the estimated effect during years 1-5 (Table 5, panel A, column (4)). It may be that the short-term reduction in homicide offending among black youth largely captures the impact from changes in perceptions of opportunities available in society, the local policing environment or community attitudes. The fact that the coefficients for the years 1-5 effect are roughly similar in size across the age cohorts supports this interpretation. Factors associated with actually attending a desegregated school, such as better classroom instruction or higher-achieving peers, would be expected to take time to influence behavior.

The appendix shows that we obtain similar results when we weight by the relevant age-race population count in each county, rather than calculate un-weighted estimates (Table A2). The pre/post vector approach (displayed on Figures 5 and 6) produces similar results when the full sample of county-year observations are used – that is, when we include the 8% of districts which do not meet our requirement of contributing a sufficient number of points pre- and post-desegregation. (The results from the truncated model with points for 1-5 and 6+ years after the court orders, displayed in all of our tables, always include the full sample.) Our SHR offending results are qualitatively similar when we construct our rates using counts of people living in jurisdictions within the county that report to the UCR system, rather than the Census-based county population estimates.[32]

As another check on omitted variables concerns, we examined whether there is any relationship between the politics of the local federal judges in each district[33] and the timing of when court school-desegregation orders are enacted. We first estimate a cross-section regression and find that the baseline political composition of each federal judicial district is unrelated to the average year when court school-desegregation orders go into effect for the school districts in our sample located within each judicial district. We also found that changes in the political composition of these judicial districts over time are unrelated to the likelihood that a school district is subject to a desegregation order (available on request).

Perhaps the main threat to inference with our study, aside from omitted variables, is the possibility of cross-county population migration in response to school desegregation orders. One way this could affect our results is through measurement error in our county population variable. If the imputed Census

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[32] Because the UCR system only provides total populations living in reporting jurisdictions, we construct the ratio of UCR-reporting jurisdictions to total county population and then multiply our Census-based age-race specific population counts by the ratio of UCR population to total population.

[33] One measure of the politics of the local federal judges in each district is the party of the president who appointed the judge. A different measure is the "common space scores" for judicial ideology from Poole and Rosenthal [1997], which range from -1 for the most liberal judges to +1 for the most conservative. We constructed these measures for each federal judge who was seated during the period from 1968 to 1982. Data from: http://voteview.ucsd.edu/dwnomin_joint_house_and_senate.htm

population figures for inter-censal years fail to capture some population loss in our counties, our estimates would overstate (in absolute value) any reductions in homicide. This is mostly a concern for the white estimates, as desegregation would not be expected to produce black population loss. (Indeed, school desegregation might lead to gains, which would lead us to understate black homicide reductions.)

To address this concern, in Table 9 we re-calculate our estimates for homicide victimization rates from the Vital Statistics data, but now restrict ourselves to decennial Census years 1960 through 1990. We also show results using data from the the year before, year of, and year after each Census, which might add some measurement error compared to using just census years but obviously triples our number of observations. The results seem qualitatively similar to our main findings but less precisely estimated.

A different concern is that population migration could lead us to confound behavioral responses by county residents with compositional changes in the county population over time. To explore this issue, in Panel A of Table 10 we estimate equation (1) using as the dependent variables the log of the county population of 15 to 24 year old whites or 15 to 24 year old blacks. The sample is restricted to the decennial Census years of 1960, 1970, 1980 and 1990 to avoid issues with measurement error. There is no evidence that desegregation induced migration across county boundaries for either whites or blacks.[34] This also provides further assurance against the possible concern that measurement error in the denominator of the homicide rate is responsible for our results.

[34] At first blush these results might seem inconsistent with those in Baum-Snow and Lutz [2008], henceforth BSL, who find evidence of black migration into desegregated central city schools, but only outside of the south. Panel B therefore allows the desegregation effect to vary by region, and shows that there is no evidence of cross county migration in or outside of the South. Our results are easily reconciled with those of BSL by noting that BSL find in-migration into desegregated school *districts* – as opposed to the *counties* used in this paper. This migration was likely intra-county because non-southern school districts tend to be smaller than the counties in which they are located. This hypothesis is supported by the results in Panel B. County-wide school districts would perhaps have been more likely to have experienced cross county migration as the result of desegregation. Estimates which allow the desegregation effect to vary by the presence of a county-wide school district provide no evidence of migration (unreported). The same thought process applies for whites as well: Although there is strong evidence that whites exited desegregated school districts [e.g. Reber 2005], our evidence suggests that they did not leave the county, but instead moved to nearby alternative public districts or went to private schools. Presumably much of the in-migration by blacks into urban school districts in BSL must be coming from inner suburbs within the same counties (Boustan [2009] finds that in areas close to school district boundaries, desegregation caused both whites and blacks to migrate)

We can also check whether our findings are driven by compositional changes in county population by using decennial Census data from 1960, 1970, 1980 and 1990 to estimate the impact of desegregation on county demographic characteristics (Table 11). For blacks, the point estimates for median family income and the probability that an adult had finished high school or college are all small, statistically insignificant, and negative, suggesting that if anything the county black population is becoming more, not less, crime prone [see Jacob and Ludwig, 2009].[35] While there is some evidence that the percent of whites finishing high school increased, the estimate is only marginally significant and is small in magnitude, suggesting around a 1 percentage point increase.

As another check on the possibility that our findings are driven by cross county migration, we re-calculate our estimates using MSA-year as the unit of observation (Table 12).[36] If our results were simply due to population migration across nearby county lines in response to desegregation orders, we would not expect any impact on homicide when the analysis is conducted at the level of the MSA. But the MSA-level estimates are quite similar to our main findings, suggesting endogenous migration does not explain our results. In principle people could be migrating out of the MSA entirely, but when we replicate our results using larger geographic areas still (bordering county groups), our results, discussed in Appendix D, again do not seem to support an endogenous migration story.

A final way to address the possibility of bias from population migration and other forms of omitted variable bias is to examine whether school desegregation orders have an "effect" on outcomes that should logically not be affected. Table 13 presents the results from such a falsification exercise. We

[35] Our choice of demographic variables and use of the non-white category (vs. black) are dictated by data availability for 1960.
[36] We use 1990 MSA definitions. Raleigh County, WV is omitted from the MSA sample because it is not located within an MSA. There are 96 MSAs in the sample, as compared to 105 counties in the county sample. Eight of the MSAs contain two desegregated counties. In these cases, the year of desegregation is defined as the earlier of the two desegregation dates. Within the MSA sample, an average of approximately 85 percent of blacks age 15 to 24 reside in a desegregated county and the remainder reside in other counties within the MSA. For whites age 15 to 19, the comparable figure is 75 percent.

estimate the effect of school desegregation orders on mortality rates from major illnesses,[37] which should not be affected by the school or peer quality or community attitudinal changes that we hypothesize drive our estimated effect of court school-desegregation orders on homicide. Whether we use our OLS levels, QML count or log dummy model, the estimated "effects" of desegregation orders on mortality from illness are much smaller in magnitude than what we see for homicide victimization rates and are never statistically significant for blacks or whites in any of our age groups (15-24, 25-34, or 35-44).

E. Mechanisms

It is possible that the estimated changes in violence from school desegregation orders is simply the mechanical result of incapacitating youth on long bus rides during the high-crime hours after school, or, relatedly, simply the result of having black youth spend more time in the communities around their new schools where policing quality may be higher. We test this hypothesis by using SHR data on month-of-offense to examine effects on homicides over the summer months versus during the academic year. Table 14 shows the estimated effects are about as large for homicides over the summer as in the school year.

Increased racial integration of the public schools is not the only change induced by court desegregation orders – overall education spending also seems to have increased, as suggested by the estimates in Table 15. We use data on government spending from the Census of Governments for the years 1972, 1977, 1982, and 1987, and estimate the truncated version of equation (1) by OLS with the dependent variable specified as the ratio of total public education spending to children ages 5 to 19 in each county. We find that education spending per child increases by around $175 per pupil (1990 dollars) following court desegregation orders (Panel A), about 6 percent of the sample mean of $2,750. Previous research does not provide any clear prediction for what size change in criminal behavior we should expect

[37] Specifically we look at the effect of desegregation on mortality from the following seven illnesses: septicemia, neoplasms (cancer), respiratory (bronchitis, pneumonia, influenza, asthma, etc), circulatory (heart disease, hypertension, etc), anemias, digestive and meningitis. The mortality rate from illness in our sample for those aged 15 to 19 is similar to what we see for homicides (13.0 versus 10.7 per 100,000).

from an increase in school spending of this magnitude.[38] (This result does not seem to be spurious since we see no estimated effect on fire protection, shown in panel C of Table 15.)

We find no systematic evidence that on average police spending per capita is affected by desegregation orders (Panel B), although the sample mean for this variable is around $99 and we cannot rule out an increase of up to 8 percent. It is possible that police respond to desegregation by changing deployment patterns, which randomized "hot spot" patrol experiments in criminology suggest could be effective [Sherman, 2002], although we have no way to test this hypothesis. It is possible that some police forces could have responded to court school-desegregation orders by integrating their police department, but this does not seem like a counter-explanation for our findings since McCrary [2007] finds little impact on crime from changes in the racial composition of the local police force.

We can provide some indirect evidence on what behavioral mechanisms might matter most by interacting changes in our measures of school segregation and public spending with our indicators for implementation of court orders.[39] We note that these findings are at best suggestive, since those counties that experience particularly large changes in any one of our candidate mediators may also experience large changes in other potential mediating mechanisms not captured by our data. The fact that there is no evidence of pre-existing trends in homicides before the court orders are enacted means unmeasured mediators are probably not biasing our outcome estimates, but our ability to determine the specific mediators that are driving our observed homicide impacts is somewhat limited. The interactions for our

[38] Dating back to the Coleman report [Coleman et al., 1966], the literature has provided weak support that increased school spending improves student outcomes. More recently Figlio [1997] and Guryan [2001] suggest an increase in school spending of 6 percent might increase student test scores by up to .2 standard deviations, concentrated among the bottom of the distribution. But little is currently known about the relationship between achievement test scores and criminal behavior.

[39] The changes in the segregation indices are defined as the changes from one year prior to desegregation to four years after desegregation, while the changes in government spending are defined as the five year change in spending between Census of Government years (i.e. years ending in 2 or 7) that spans the year of desegregation.

spending variables are further limited by the fact that we only have those measures starting in 1972, which means that only districts desegregated in 1973 or later contribute to the estimates.

With these caveats in mind, Table 16 shows that homicide victimization rates declined the most for blacks in districts where exposure of blacks to whites in the public schools increased the most. These results come from estimating our preferred QML model; as shown in the appendix (Tables A4 and A5), OLS results are usually qualitatively similar but less precise, particularly for our results on spending interactions where our sample of county-years is roughly cut in half. When we include interactions of our "treatment" indicators (years post court desegregation order) with changes in the exposure and dissimilarity indices at the same time (column (3)), the former seems to be driving the result.[40] The fact that we observe the largest impacts on black homicide in places with the largest "treatment dose" from court orders provides additional support for the credibility of our research design. For whites (Table 17), the largest decline in victimizations are in counties where spending on schools or police increased most.[41]

Finally, there is another potential mechanism that would be relevant only for whites – migration out of the desegregated school district. While there is no evidence of "white flight" out of the counties, there is evidence that whites move from school districts subject to desegregation orders to other districts *within the same county* that are not subject to court-ordered school desegregation. Table 18 shows that the ratio of white enrollment in districts subject to court orders to the total number of white school-age children in the county declines by between 4 and 6 percentage points after these court orders go into effect – around a 15 percent decrease relative to the sample average of 0.39 [see also Reber 2005, and Baum-

[40] Recall that the dissimilarity index is coded the reverse of the exposure index, and so the signs of the interactions for the exposure and dissimilarity indices shown in Table 13 point in the same direction although the exposure index interactions are much larger absolutely and compared to the standard errors.

[41] Reber [2007a,b] studies desegregation plans in Louisiana and finds that increased school spending seems to be more important in explaining improved black student outcomes than does increased exposure to white students. But the pattern she finds in Louisiana – schools with a higher share minority experience the largest gains in school spending – does not seem to hold in our sample of large urban school districts.

Snow and Lutz 2008]. These results, together with our finding of no decline in the overall number of school-age white children in our counties, imply that some white families must be moving to other public school districts (and, according to BSL, private schools for whites outside of the South) within the same county to avoid court-ordered desegregation. If these new districts or private schools are less criminogenic than the districts subject to desegregation orders, this could provide another mechanism driving our result. One suggestive data point against this hypothesized mechanism comes from Table 17, column (3), which shows that the impact of desegregation orders on white homicide victimizations do not appear to be larger in desegregating districts with the largest change (i.e. decline) in the percent of white children in the county enrolled in the desegregated school district (i.e. the measure explored on Table 18).

F. Generalizability

Since our estimates rely on studying desegregation orders that went into effect through the early 1980s, there is naturally a question of whether or how our estimates might be relevant for the effects of current desegregation efforts. One imperfect way to address this question is to examine whether the estimated effects of desegregation orders vary between those enacted early versus late during our study period. We find no evidence for this sort of heterogeneity in desegregation treatment effects (unreported).

Another way to explore this issue is to see whether the design features of the desegregation plan influence the effect on crime. Welch and Light [1987] provide a useful typology of the types of plans that were implemented, which include several different types of "voluntary" plans such as magnet programs that provide students some choice over where they attend school and are similar to those plans used most commonly today. "Involuntary" plans include rezoning of school catchment boundaries and pairing-clustering plans that integrate groups of schools by grade, and are thought to involve the greatest amount of busing among the different plan types.[42] When we re-estimate our main specifications including interactions between time since desegregation order and plan type, we do not find any evidence for heterogeneity in treatment effects by plan type.

Another reason that the effects of school desegregation could change over time is if a key mechanism underlying our results was the mixing of students from different socio-economic backgrounds. If that were true, then we might expect the effects of desegregation orders to decline over time, since the black-white difference in poverty rates has declined.[43] Yet we find no evidence that our estimated impacts vary according to the black-white difference in median family income in each county.

VII. CONCLUSIONS

Our estimates suggest that the court school-desegregation orders enacted starting in the late 1960s in the largest districts in the U.S. reduced homicide victimization rates to black youth by around 25 percent and also generated large declines in homicide offending among blacks as well. Criminal offending is heavily concentrated in the left tail of the behavioral distribution. Studies in criminology consistently find that around 6 percent of each birth cohort is responsible for 50-60 percent of crime committed by that cohort [Wolfgang et al., 1972; Tracy et al., 1990]. Desegregation orders would need to change behavior by just a small share of high-risk youth to generate large proportional changes in crime.

Unpacking the mechanisms through which court school-desegregation orders affect crime is complicated, because these orders not only re-sort children into new schools and peer environments, but may also change people's attitudes as well as affect the level or allocation of local public goods. We do not find any evidence that changes in police spending drive the declines in offending or victimization

[42] Welch and Light [1987, p. 27] explain: "Pairing and clustering involves reassigning students between a pair or group of schools, usually via grade restructuring, … [that] may have either contiguous or noncontiguous attendance zones. For example, a (predominantly) white school and a (predominantly) black school, both offering grades K-6, could be paired by converting one into a lower elementary school (grades 1-3) and the other into an upper elementary school (grades 4-6)."

[43] The poverty rate for blacks was 41.8% in 1965, 29.3% in 1995, and 24.9% in 2005. The poverty rate for whites in each of these years was 13.3%, 11.2% and 10.6%, respectively.

among blacks. Some evidence that exposure to more developmentally productive schools or peer groups is an important part of the story comes from the fact that the long-term changes in homicide offending among blacks are larger for those birth cohorts who were of school age when their local court order was enacted, compared to the impact on blacks born just a few years earlier who were already out of school.

We hypothesize that the impacts of school desegregation orders on people's attitudes may also play a role, judging in part from the "doll studies" by Kenneth Clark and the meaning so many people seemed to have placed on policy efforts to racially desegregate public schools. As a *Chicago Defender* editorial said the day after *Brown*: "This means the beginning of the end of the dual society in American life and the … segregation which supported it." These types of reactions are not simply limited to the original *Brown* decision. For example, in response to a 2000 court decision regarding school segregation one Southern parent noted, "Louisiana is still ignorant to the equality of all humans" [Caldas et al., 2002]. Some more systematic empirical support for this hypothesis comes from Guryan's [2004] finding that dropout rates declined by large amounts even in the first year after a court order, despite the fact that black youth would not have been exposed for very long to different schools, peers, or public goods.

Whatever the underlying mechanisms, changes in schooling outcomes is a likely proximate cause for a sizable share of these crime impacts, as suggested in part by the fact that declines in homicide arrests to black youth are about as large in the summer as the school year and persist as youth age into adulthood. By combining the estimated 25 percent decline in black dropout rates from Guryan [2004] with the estimated effect on schooling on crime from Lochner and Moretti [2004], we can explain a reasonable share of our estimated impacts on criminal behavior.

Unlike with many law enforcement interventions where the concern is with negative spillovers from displacement of crime to other geographic areas, our findings suggest that there were positive spillover effects to other groups from court-ordered school desegregation. We find homicide victimization and offending seems to decline among black adults shortly after desegregation orders are enacted. The fact that we see no pre-existing trends in homicide for black adults, that other causes of death among adults such as from disease are not affected by school desegregation orders, and the fact that our data provide some evidence for large changes in offending by black adults against school-age youth and vice versa, increases our confidence that these changes in adult behavior capture real behavioral responses to school desegregation orders. In addition to changes in offending rates across age groups, part of the impacts on black adults could also be due to changes in community-wide attitudes, and from redirection of law enforcement resources towards investigating crime by adults when youth offending declines.

Despite the concerns of many white parents at the time of these desegregation orders, we find no evidence that white homicide offending or victimization rates *increased* after these orders were implemented. In fact our results suggest that white victimization rates declined by around 15-20 percent by 6 years after the court orders went into effect. If there are any adverse peer effects from re-sorting low-income African-American children into more affluent, white schools, they appear to be outweighed by the beneficial effects on the black children who are entering into new schools together with whatever public goods or attitudinal changes are generated by court school-desegregation orders.

The size of our estimates, particularly for blacks, raises the natural question: Could desegregation orders really have caused such large declines in crime without anyone having noticed? It seems quite possible, given the staggered timing of when these orders went into effect across cities and the fact that the period in which these court orders went into effect – the late 1960s to early 1980s – was one in which homicide rates experienced dramatic secular trends [Cook and Laub, 1998, Levitt, 2004].

Our findings may have implications for ongoing debates about school desegregation policies, since our results suggest that existing research focused on just academic outcomes or adult earnings may understate – perhaps substantially – the benefit side of the ledger from policy efforts to desegregate the public schools. Note that our estimated crime impacts may understate social welfare gains if part of the effect of desegregation orders on the supply of crime is undone by reductions in costly victim avoidance behaviors. A full benefit-cost analysis would also need to value other impacts as well, such as changes in residential patterns [Baum-Snow and Lutz, 2008]. In any case our estimates imply benefits of nearly $1,000 per black student and nearly $200 per white student from reductions in homicide, which are both large relative to the average per-pupil spending level of schools in our sample of around $2,750.[44] Although the Supreme Court recently issued a 5-4 decision striking down school desegregation plans in Seattle and Louisville, Justice Kennedy's controlling opinion leaves open the possibility for more narrowly-targeted policies such as strategic site selection for new schools or re-drawing attendance zones.

Our findings may also help us better understand why crime rates for African-Americans have changed over time. Cook and Laub [1998] note that the ratio of black to white homicide arrests for people under 18 declined steadily and dramatically starting in the late 1960s, for reasons that are poorly understood. More generally, for blacks, homicide victimization rates to young people (15-24) and overall arrest rates peaked in the late 1960s and then started to decline, as shown in Figure 7 – just as the set of large urban districts we study, which account for a large share of all minority crime in the US, began to implement school desegregation orders. The large urban counties in our district accounted for nearly half of all black homicides in the US as a whole in 1968 and over one-third of white homicides. Our estimates imply that over our study period desegregation orders in our counties lowered the *nationwide* homicide rate to blacks 15-24 by 13 percent and lowered the rate to whites 15-24 by 7 percent, and might account for around one-quarter of the convergence in black-white homicide rates from 1970 to 1980.[45]

[44] Our preferred count model estimates imply something on the order of 10 fewer homicides per 100,000 black youth, and 2 fewer homicides per 100,000 white youth. Cohen and colleagues [2004] estimate that the social costs per homicide equal around $9.7 million in current dollars.

[45] Table 2 shows that between 1970 and 1980 the difference in homicide rates for blacks and whites ages 15-24 declined by 19.5 per 100,000. Table 3 shows that court-ordered desegregation reduced homicide rates to blacks 15-24 by around 11 per

Our findings also reinforce the general potential for social policy as a tool for crime control, and in particular social policies that affect the level of racial and economic segregation in America. Our results are consistent with evidence from the randomized Moving to Opportunity (MTO) residential mobility experiment and from a more recent randomized wait-list lottery employed as part of the city of Chicago's regular housing voucher program, both of which suggest declines in violent-crime arrests for minority youth who move into less distressed neighborhoods [Kling, Ludwig and Katz, 2005; Jacob et al. 2009]. It is possible that some of our most cost-effective crime policies might not have anything at all to do with the criminal justice system.

100,000, and our counties account for around half of all black homicides nationwide. Table 4 suggests these court orders reduced white homicide rates by 2 per 100,000, and around one-third of white homicides occur in the counties we study.

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APPENDIX A: A BRIEF HISTORY OF SCHOOL DESEGREGATION DECISIONS

This section provides a brief overview of some of the key Supreme Court decisions relevant to school desegregation. A very large share of these key decisions resulted from litigation filed by the NAACP, given the limited involvement of the U.S. Department of Justice in litigating in this area. Following *Brown*, President Eisenhower refused to authorize his Attorney General to file lawsuits on behalf of black parents to require districts to desegregate [Klarman, 2007, p. 112-3]. This changed in 1964, but federal enthusiasm for litigation in this area waned again with the election of President Nixon in 1968 [Greenberg, 2004, p. 413-4].

One of the first relevant Supreme Court decisions was *McLaurin v. Oklahoma* (1950), in which the court ruled that the University of Oklahoma's decision to force a 68 year old African-American law student to sit apart from other students, separated by a rope, and eat lunch at a different time from whites, did not constitute an equal educational experience to that of white students. In *Sweatt v. Painter* (1950) the Supreme Court decided that the three-room law school for blacks that Texas developed in the basement of a petroleum company building was not equal to the University of Texas Law School. After the *Sweatt* decision was announced, Thurgood Marshall declared that he had plans to "wipe out … all phases of segregation in education from professional school to kindergarten." But as Marshall's biographer notes: "The militant attitude in public statements from Marshall and the lawyers, however, was quite different from their private discussions. Marshall was still deeply concerned that a direct attack on all school segregation could be time-consuming and, even worse, ultimately lead to defeat. Integrating law schools, professional schools, and even colleges with adult students might not have been hard. But racial integration of boys and girls in grade schools, Marshall suspected, was going to provoke the strongest possible backlash" [Williams, 1998, p. 195].

Following *Brown II* in 1955, pupil placement laws were adopted by all of the Southern states and allowed schools to place students on the basis of a wide range of ostensibly racially neutral factors, which as Klarman (2004, p. 119) notes “helped insulate the system from legal challenge because of the difficulty of providing that a multifactor decision was racially motivated.” The fact that these plans claimed to treat students as individuals helped rule out class action litigation, since plaintiffs would then have difficulty showing “sufficient commonality of circumstance” (Klarman, 2004). These placement plans were prohibited by the Supreme Court in 1968 in *Green vs. New Kent County, Virginia* (391 U.S. 430), which in turn led to a surge of litigation activity in the Federal courts.

Prior to 1973, court-ordered desegregation could only occur in school districts proved to have engaged in *de jure* segregation. The 1973 *Keyes v. Denver School District* decision (413 U.S. 189) ruled that court-ordered desegregation could proceed in areas that had not practiced *du jure* segregation, but in which segregation existed by virtue of past state action. As a result, desegregation became more viable in school districts outside of the south in which *de facto* segregation was present.

Some other important desegregation cases include *Milliken v. Bradley* in 1974 (418 US 717), which struck down an inter-district desegregation plan in Detroit but specified the conditions under which this approach would be allowed. *Newburg Area Council, Inc. v. Board of Education of Jefferson County* in 1975 (521 F.2d 578, 6th Circuit) ordered the first inter-district remedy that met the Milliken requirements. The “Milliken II” case, *Milliken v. Bradley* 1977 (433 US 267) approved remedies that involved increased educational resources in predominantly black schools. *Swann v. Charlotte-Mecklenburg Board of Education* in 1972 (402 US 1) allowed for busing to be used to remedy racial imbalance in the schools, even if this imbalance was due only to the geographic distribution of students of different races across areas.

Over time, the process generating local Federal lawsuits to desegregate schools seems to have become increasingly decentralized and idiosyncratic. As described by Jack Greenberg, director of the NAACP's Legal Defense and Educational Fund from 1961 to 1984: "Ours was not a regimented or even somewhat controlled operation as to sequence and, indeed, other matters. Local groups, usually although not always NAACP, and local lawyers just filed cases … To the extent to which we had influence it was because during early days the number of civil rights lawyers in the south was limited (black lawyers only took such cases and there weren't many black lawyers during early days) and there were more or less close personal relationships. … Also cases needed funding and we exercised some control when groups came to us for money, if not expertise, but cases cropped up on their own, particularly in the North where civil rights lawyers were more abundant during early years."[46] See also Greenberg [1994] and Klarman [2004].

Most recently in June 2007, the U.S. Supreme Court issued two 5-4 decisions striking down school desegregation plans in Seattle and Louisville. Justice Kennedy's controlling opinion leaves open the possibility for more narrowly-targeted desegregation policies such as strategic site selection for new schools or re-drawing school attendance zones. Race-conscious policies are subject to "strict scrutiny" by the courts, which requires that they be "narrowly tailored" but also that there be a "strong basis in evidence" that the relevant policy serves a "compelling government interest."

The Harvard Civil Rights Project has a useful summary of how the courts have interpreted these terms of art in previous cases. The courts generally find that policies to remedy the effects of past discrimination, or "remedial interests," meet the test for a compelling government interest, but have been more divided over "non-remedial" interests such as promotion of educational diversity (the focus by Justice Powell in *Regents of the University of California v. Bakke*) or reducing racial isolation, and have

[46] Personal communication, Jens Ludwig with Jack Greenberg, July 5, 2007.

rejected the use of race-conscious policies to remedy general societal discrimination or to provide role models for racial minorities. The "narrow tailoring" test examines the "fit" between the policy and the objective, where courts often strike race-conscious policies that achieve ends where race-neutral policies would also be an option.[47] As the Civil Rights Project notes, "[school] choice plans that consider multiple factors could be upheld with appropriate educational justification. … Permissible options may [also] include race-conscious efforts that do not single out any one student on the basis of his or her race such as siting schools in areas that would naturally draw students from a mixture of racial / ethnic backgrounds or magnet schools that have special programs that draw students from different backgrounds." It is also important to note that the Louisville and Seattle decisions do not affect districts that are under court order to desegregate, only those that initiated desegregation efforts on their own.[48]

[47] www.civilrightsproject.ucla.edu/policy/legal_docs/cover.pdf.
[48] www.civilrightsproject.ucla.edu/policy/court/voltint_joint_full_statement.php

APPENDIX B: DATA

Our study focuses on the set of large school districts subject to court orders that were included in a dataset compiled by Finis Welch and Audrey Light [1987] for the U.S. Commission on Civil Rights. These data cover all districts that in 1968 were 20 to 90 percent minority with enrollments of 50,000+, and a random sample of districts that were 10-90 percent minority with enrollments of between 15,000-50,000.

Our main data sources are the Vital Statistics (VS) system of the United States, which enables us to measure homicide victimization rates by county and year to separate age-race groups, and the FBI's Supplemental Homicide Reports (SHR), which we use to construct homicide offending rates to age-race groups by county and year.

The VS is administered by the CDC and provides a census of all death certificates in the U.S. These death certificates are completed by physicians, medical examiners and coroners across the country and include information about the decedent's year and cause of death (coded using a standardized system, either the International Classification of Diseases version 8 or 9 system depending on the year), as well as their state and county of residence, age, race / ethnicity, gender, and in some cases educational attainment and marital status as well. We have assembled an annual Vital Statistics dataset that captures death rates from homicide and other causes by different age groups for the period 1959 through 1988.

Data for 1968 through 1988 come from the Compressed Mortality Files (CMF), which provide VS death counts by cells defined at the county level for different combinations of cause-of-death and decedent characteristics. While the data for most years comes from a census of death certificates, for 1972 the data are a 50 percent sample and so are weighted up by a factor of 2. For years before 1968, we use micro-mortality records and aggregate up to the level of the county, cause-of-death and decedent category ourselves. The sample ends in 1988 for most of our analyses because at least 3 districts were dismissed from their orders in 1989-1990 and then in 1991 the legal environment for court-ordered desegregation changed radically with the first of three Supreme Court decisions (see Clotfelter, Ladd, and Vigdor [2006], Lutz [2005], Orfield and Eaton [1996] and references therein). However, for the runs in which we only have decennial census data, we include 1990 in order to increase sample sizes.

The SHR is compiled by the FBI from homicide data that is voluntarily provided by local and state police agencies. Because the VS provides a more reliable measure of homicide victimization rates than does the SHR, we use the SHR primarily to learn something about homicide offenders, about whom the VS is entirely silent. Of course the SHR will only provide information on offender characteristics in cases where there is an arrest. We use the SHR data to construct annual homicide offending rates for age-race groups at the county level for the period 1976 to 2003.

The key explanatory variable for our analysis is the date that school districts were subject to local court orders to desegregate, which we take from Welch and Light [1987]. One complication for our study is that the Welch and Light dataset has the school district as the unit of analysis, while the VS and SHR data are available only at the level of the county. Some of the school districts in the Welch and Light sample include the entire county, while others are in counties with multiple school districts. There are four counties in our sample that contain more than one desegregated school district. We handle this issue by estimating our results classifying these counties initially as "desegregators" when the first district within the county is subject to a desegregation order and then re-calculating our estimates defining the county's desegregation date as the last date that any district in the county is subject to a desegregation order. The results are not substantially different in either case. For instance, Jefferson County in Alabama contains two school districts: Birmingham district, with a desegregation year of 1970, and Jefferson County district with a desegregation year of 1971. We first estimate our results counting Jefferson County as if it desegregates in 1970, and then redo our analysis Jefferson County as a 1971 desegregator. This approach gets complicated for Los Angeles County, which contains five school districts, although a single district – Los Angeles School District – enrolls around 611,228 of the total 760,690 students in the county as a whole (figures are as of 1973, the mean year a district in LA County was subject to a desegregation order). In this case we always assign LA County to have the LA School District's year of desegregation orders.

To construct homicide victimization and offending rates we also require some data on annual county population counts by age and race. For our VS analysis, population data for 1960, 1970, 1980 and 1990 come from the decennial census. For the inter-censal years for the 1968-88 period the CMF provides population figures that are calculated by the Census Bureau that begin by linearly interpolating population from the decennial censuses, and adjusting for data on births and deaths in each county. The CMF reports data for the 1968-88 period that was released before the 1990 Census data were available. The Census Bureau in this case estimated across-county population migration and growth using data on changes and trends in changes for the 1970s. For the period 1961-7 we conduct our own linear interpolation between the 1960 census data and the 1968 county population figures reported by the CMF, and for 1959 we estimate values using the linear trends in population changes observed for each county from 1960-68. For the period before 1968 we are forced to use the 1960 census information on "non-whites" as our measure of the black population within our counties.

The primary source of information about other types of crime besides homicide is the FBI's Uniform Crime Reporting (UCR) system, through which local and state police departments voluntarily report to the FBI citizen complaints of crime. These UCR data will miss crimes that are not reported to the police, which is of some concern in part because some of the major policy "treatments" of interest in crime research may affect the propensity of victims to report crimes as well as the volume of actual criminal activity. Of particular concern for this study, desegregation may have altered the reporting behavior of both victims and authorities, potentially making any resulting measurement error non-classical in nature. Homicide is less subject to this problem because of the common view within criminology that most homicides eventually become known to the authorities.

The propensity of police agencies to report, or report accurately, also varies across areas and over time; see for instance Maltz [1999] for a detailed discussion, with a focus on how measurement error with the UCR is particularly severe at the unit of observation for our study – the county. UCR data are noisy particularly at the county level because of inconsistent reporting practices by local police agencies that are not well documented in the UCR [Maltz, 1999]. Police may also classify events into different crime categories differently over time. For example police practices for determining what counts as an aggravated versus simple assault seem to have changed sharply over time, as evidenced in part by the fact that UCR data show a substantial increase over our study period in aggravated assault rates, while victim reports to the National Crime Victimization Survey (NCVS) show flat trends [Blumstein, 2000]. The other limitation of the UCR is that to identify offenses committed by population sub-groups we must rely on arrest data, and the fraction of offenses (aside from homicide) that result in arrest is quite low. Even the "clearance rate" for homicide itself is surprisingly low. Given these UCR data problems, it is not surprising that most of our results from analyzing the UCR are very imprecisely estimated.[49]

The NCVS is unfortunately not a useful data source for our study because the sampling frame is intended to yield nationally but not locally representative samples, and because in any case geographic identifiers are not made available for NCVS data.

[49] Among the numerous UCR outcomes we examined the only statistically significant pattern we see (other than for a drop in UCR murder rates, consistent with our Vital Statistics and SHR results) is an increase in aggravated assault, which we find difficult to interpret given the classification concern mentioned above. Our view is that this is likely to be an artifact of law enforcement practices rather than a real behavioral response by potential offenders, given the fact that aggravated assault and murder rates usually move together, since the latter is often a byproduct of the former, and yet we do not see an increase in murder rates following desegregation orders using the Vital Statistics victimization data, which are widely regarded as quite accurate.

The data on government spending (Tables 15, 16, 17, A4 and A5) are obtained from the *Census of Government* (COG) for the years 1972, 1977, 1982 and 1987. We use the version of the COG contained in the Historical Database on Individual Government Finance -- a longitudinally consistent version of the COG produced by the Census Bureau. The COG data are organized at the level of the individual government and include municipalities, counties and other forms of local government. We convert this data into county-level observations by taking the *direct* expenditures on a given category of public expenditure (e.g. education spending) and summing them to the county level. These data should capture most school, police and fire spending, the main expenditure categories we examine in our tables. We do not examine other types of social program spending because so much of that is accounted for by higher levels of government not captured by our COG data.

The demographic data (used on Table 11) are obtained from the 1960, 1970, 1980 and 1990 decennial censuses. We use versions of the census data summarized at the geographic level of the county. The 1960 data were obtained from hardcopy versions of Census of Population: 1960, Vol. 1, *Characteristics of the Population*. The 1970, 1980 and 1990 data were obtained in electronic format from the National Historic Geographic Information System (NHGIS) maintained by the Minnesota Population Center, University of Minnesota.

APPENDIX C: ADDITIONAL ESTIMATION DETAILS

I. QML Count Model

In order to estimate a proportional response model that does not suffer from the bias inherent to the log linear dummy model, we also estimate a fixed-effect Poisson Count model as in equation (A1):

$$\text{(A1)} \qquad E(y_{it} \mid \bar{D}_{it}, \gamma_i, \delta_{t,r}, pop_{it}) = \exp(\alpha + \sum_{p \in \Psi} \beta_p D_{p,it} + \gamma_i + \delta_{t,r} + \psi\, pop_{it})$$

where y_{it} is the count of homicides for a given age/race cohort in county i at time t, $\bar{D}_{it} = \sum_{p \in \Psi} D_{p,it}$ and pop_{it} is the size of the age/race cohort. Equation (A1) is transformed to remove the county fixed-effect terms, γ_i, because the nonlinearity of the equation precludes their consistent estimation (Hausman, Hall and Griliches, 1984).

$$\text{(A2)} \qquad E(y_{it} \mid \bar{D}_{it}, \gamma_i, \delta_{t,r}, pop_{it}, \bar{y}_{it}) = \frac{\exp(\alpha + \sum_{p \in \Psi} \beta_p D_{p,it} + \delta_{t,r} + \psi\, pop_{it})}{\sum_{t=1}^{T} \exp(\alpha + \sum_{p \in \Psi} \beta_p D_{p,it} + \delta_{t,r} + \psi\, pop_{it})} \bar{y}_{it}$$

where $\bar{y}_{it}$ is the count of homicides in county i over the entire sample period ($\bar{y}_i = \sum_{t=1}^{T} y_{it}$). Equation (A2) is estimated by quasi-maximum likelihood (QML). We refer to this as the QML count model, which has good consistency properties relative to other count models; the conditional mean assumption, equation (A1), is sufficient to ensure consistency. The parameter estimates remain consistent even in the case of distributional misspecification (i.e. the assumption that the distribution of y given x is Poisson fails to hold) and there is no need to make assumptions about over or under-dispersion or, more generally, to specify the conditional variance, as must be done for many count models (Wooldridge 1999).

By imposing the constraint that ψ=1, the pop_{it} variable controls for "exposure". The parameters of interest, β_p, can therefore be interpreted as semi-elasticities of the homicide rate with respect to the year of school desegregation — i.e. they estimate the percent change in homicides rates associated with a county being in its *pth* year of school desegregation.[50] We calculate standard errors using the robust variance estimator proposed by Wooldridge (1999). These standard errors account for arbitrary forms of serial correlation in the model's error term. The computer code for generating these estimates is available from the authors upon request.

[50] The β_p coefficients can also be interpreted as semi-elasticities in the linear log dummy variable model.

APPENDIX D: BORDERING COUNTY GROUP ANALYSIS

In order to explore the possibility that our findings are driven by endogenous migration, we re-calculate our estimates by expanding each county observation to include all counties which border it – an exercise similar in spirit to the MSA estimates presented on Table 12. Unlike the MSAs, where a substantial majority of the population lives within a desegregated county, within the "bordering county groups," a substantial fraction of the population resides in non-desegregated counties. Specifically, 55 percent of blacks age 15 to 24 reside in desegregated counties and the remainder reside in counties which border a desegregated county. For whites age 15 to 19, the comparable figure is 44 percent. If our main findings represent a true causal relationship, then the bordering county group treatment effect, $\hat{\beta}_{BCG}$, divided by the average percent of the population residing in desegregated counties (as opposed to bordering counties), δ, should equal the standard, county-based treatment effect, $\hat{\beta}$: $\frac{\hat{\beta}_{BCG}}{\delta} = \hat{\beta}$ (this equality is derived below). We therefore expect the adjusted bordering county group estimate, $\frac{\hat{\beta}_{BCG}}{\delta}$, to range between $\hat{\beta}$ and 0, with $\hat{\beta}$ in the case of no endogenous migration and 0 in the case where our results solely reflect endogenous migration. The bordering county group estimates, $\hat{\beta}_{BCG}$, are presented in columns (1) and (4) of Table A3, the adjusted estimates, $\frac{\hat{\beta}_{BCG}}{\delta}$, in columns (2) and (5) and, for comparison, the standard county-based estimates, $\hat{\beta}$, in columns (3) and (6). The adjusted bordering county group estimates are similar to the standard estimates, particularly for the black results, suggesting endogenous migration does not explain our results.

II. Simple Derivation of the Relationship between the Bordering County Group DD Estimator and the County DD Estimator under Assumption of No Migration

County DD estimator
$i = 0$: never desegregated
$i = 1$: county desegregated at time $t = 1$, segregated at time $t = 0$

$$\hat{\beta} = E[y \mid i = 1, t = 1] - E[y \mid i = 1, t = 0] - [E[y \mid i = 0, t = 1] - E[y \mid i = 0, t = 0]]$$

Bordering County Group DD Estimator assuming no migration
The treatment group can be seen as being composed of two sub-groups – the desegregated counties (same as above; i=1) and the counties not subject to court-ordered desegregation, but located in the same bordering county group as a desegregated county (i=2)

$i = 2$: not desegregated

The conditional expectation for the treatment group is a weighted average of the conditional expectations of the two sub-groups. The weights for each of the sub-groups are equal to their percentage of the treatment group population. The DD estimator becomes

$$\hat{\beta}_{BCG} = \delta * [E[y \mid i = 1, t = 1] - E[y \mid i = 1, t = 0]] + (1 - \delta) * [E[y \mid i = 2, t = 1] - E[y \mid i = 2, t = 0]] -$$
$$[E[y \mid i = 0, t = 1] - E[y \mid i = 0, t = 0]]$$

where δ =percent of treatment group that resides in the desegregated counties (i.e. that is part of sub-group i=1)

Assume there is *no migration*. Type $i = 2$ is untreated – these counties have not been desegregated – and therefore have means in all periods equal to the control group, $i = 0$

$$E[y \mid i = 2, t = a] = E[y \mid i = 0, t = a] \quad \forall a$$

then

$$\hat{\beta}_{BCG} =$$
$$\delta * [E[y \mid i = 1, t = 1] - E[y \mid i = 1, t = 0]] + (1 - \delta) * [E[y \mid i = 0, t = 1] - E[y \mid i = 0, t = 0]] -$$
$$[E[y \mid i = 0, t = 1] - E[y \mid i = 0, t = 0]] =$$
$$\delta * [E[y \mid i = 1, t = 1] - E[y \mid i = 1, t = 0] - [E[y \mid i = 0, t = 1] - E[y \mid i = 0, t = 0]]] =$$
$$\delta * \hat{\beta}$$

and

$$\frac{\hat{\beta}_{BCG}}{\delta} = \hat{\beta}$$

Figure 1
Desegregation Implementation Dates

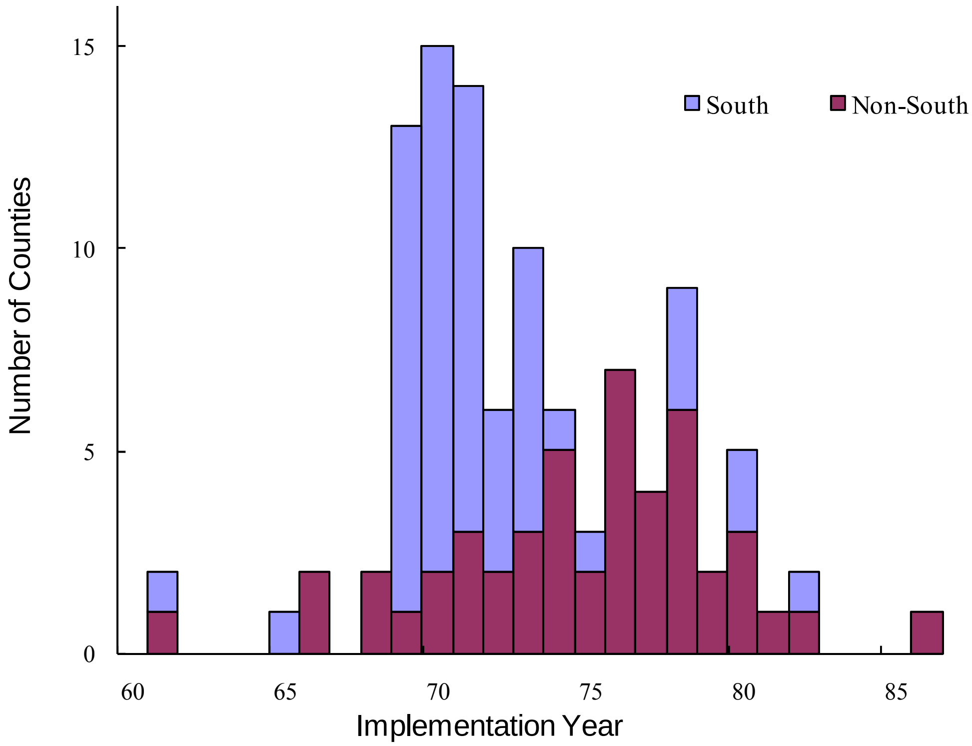


Figure 2
Potential effects of court-ordered school desegregation on "supply" and "demand" schedules in the "market for crime"

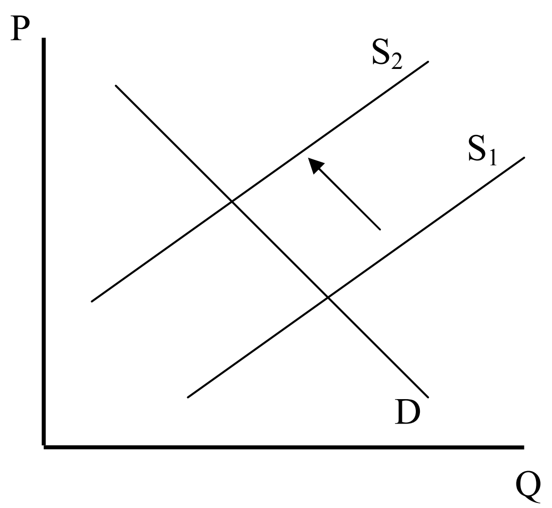
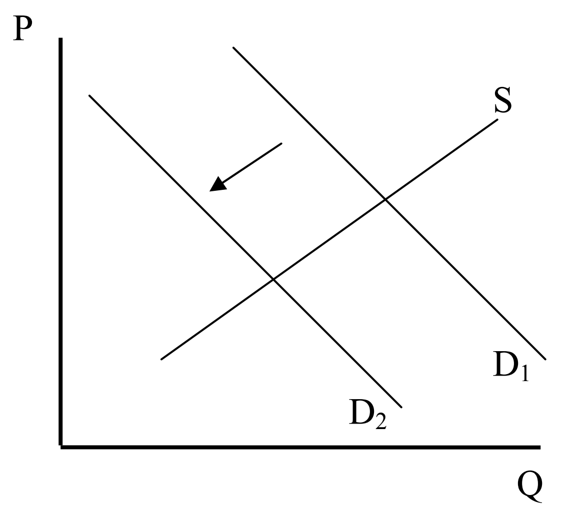
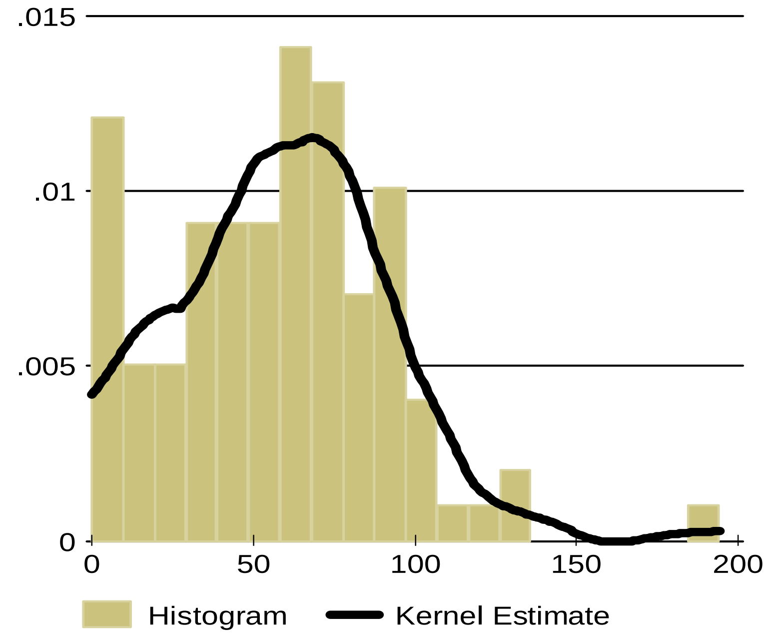
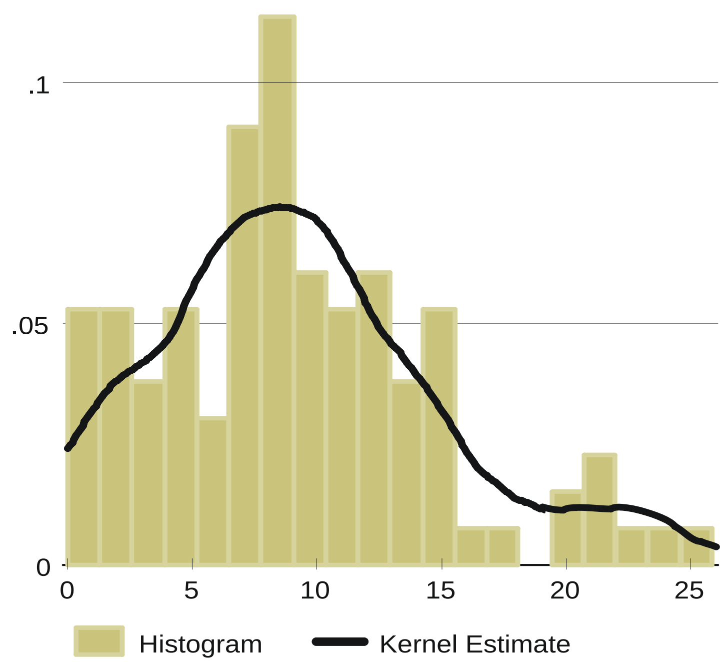
Panel A: Shift in Juvenile Supply-of-Offense Schedule	Panel B: Shift in Adult Demand-for-Offense Schedule
	

Figure 3

A. Distribution of 1975 Black Age 15 – 24 Homicide Rates per 100,000



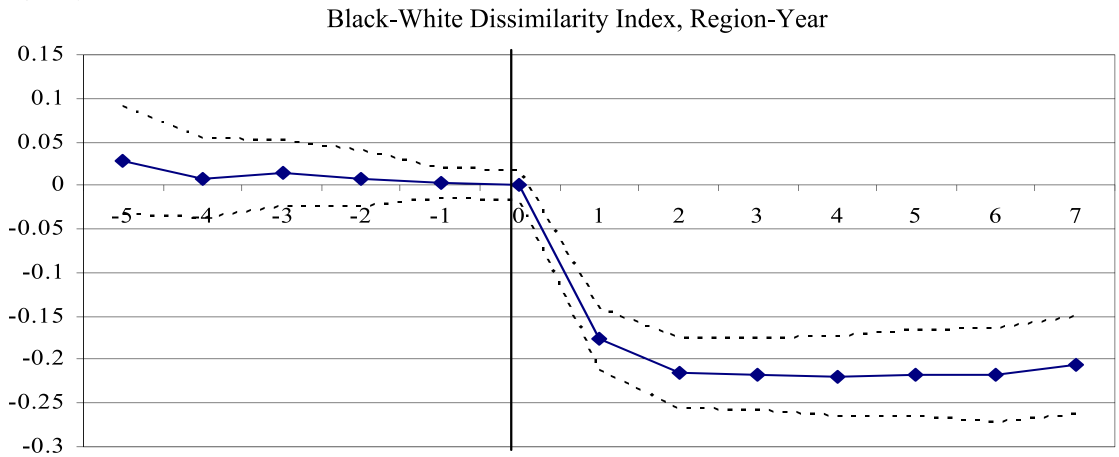
B. Distribution of 1975 White Age 15 – 24 Homicide Rates per 100,000



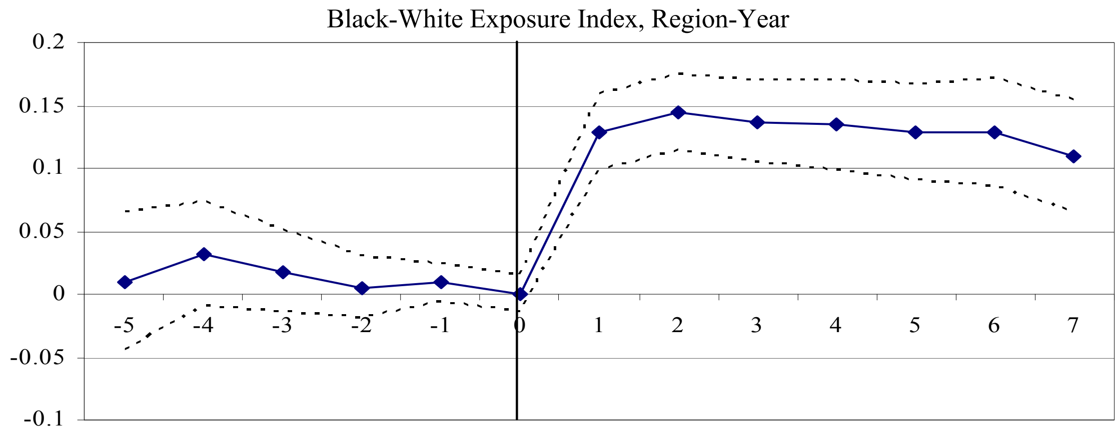
Note. The figures displays histogram and kernel density estimates of the 1975 age 15 – 24 homicide rate per 100,000. The kernel density estimate uses a Epanechnikov function and a bandwidth of 1.2. The sample is restricted to the counties in the Welch and Light (1987) sample with a major desegregation plan.

Figure 4: Effects of Court-ordered Desegregation on Segregation and Number of Schools

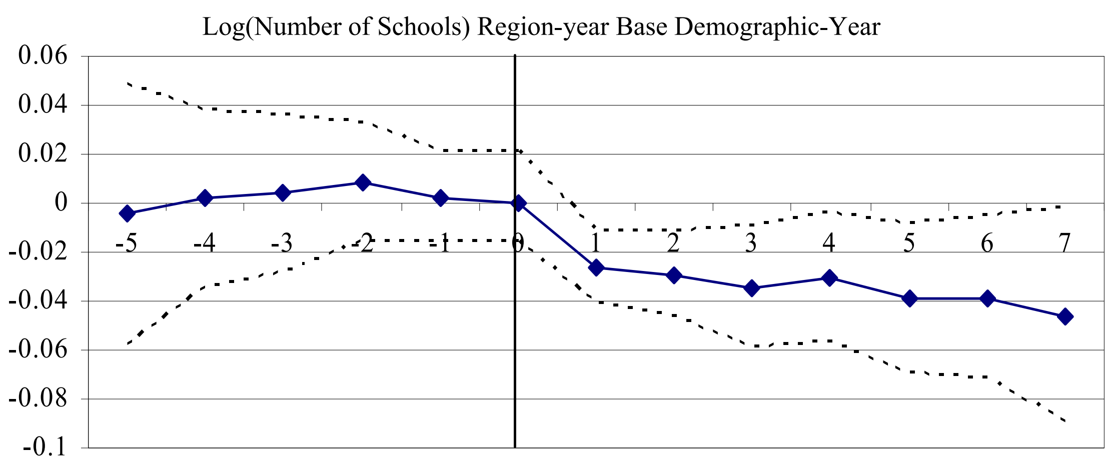
Panel A:



Panel B:



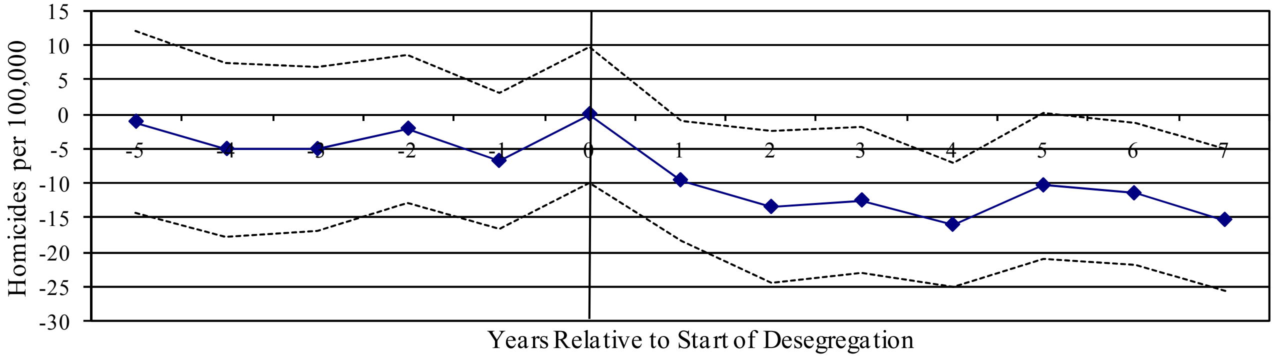
Panel C:



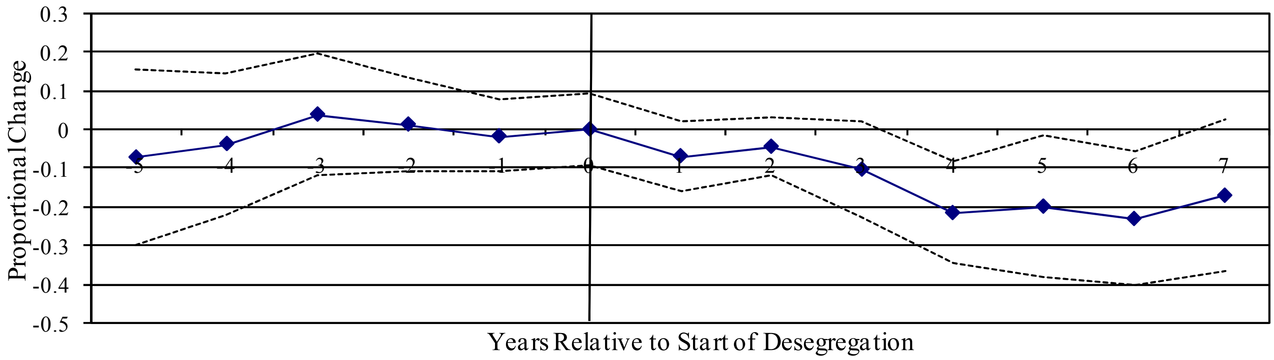
Note. The solid points display coefficient estimates and the dashed lines display the 95% confidence intervals around these estimates. The vertical axis displays the magnitude of the coefficient estimate. The horizontal axis displays years relative to the implementation of desegregation. Year "0" is the year immediately prior to the start of desegregation.

Figure 5: School Desegregation & Black Homicide Victimizations

Panel A: Age Cohort 15-24 OLS Level

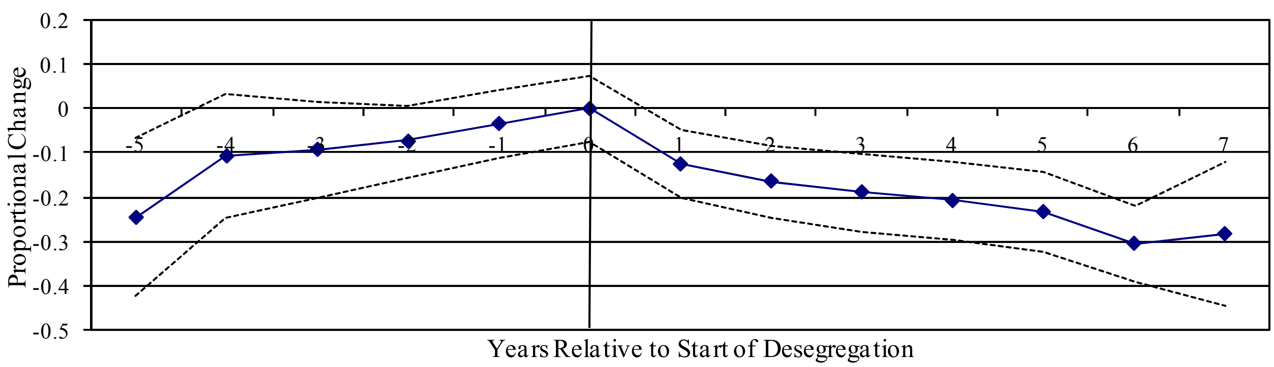


Panel B: Age Cohort 15-24 QML Count



Panel C: Age Cohort 25-34 OLS Level

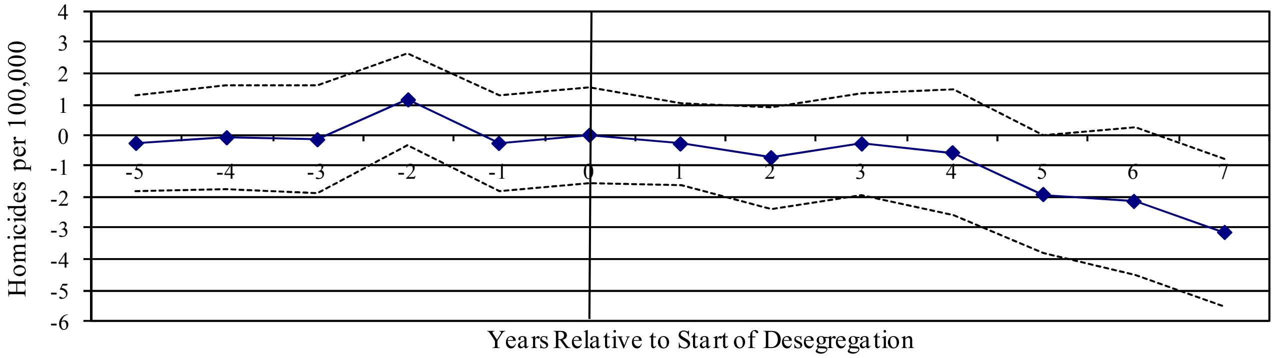
Panel D: Age Cohort 25-34 QML Count



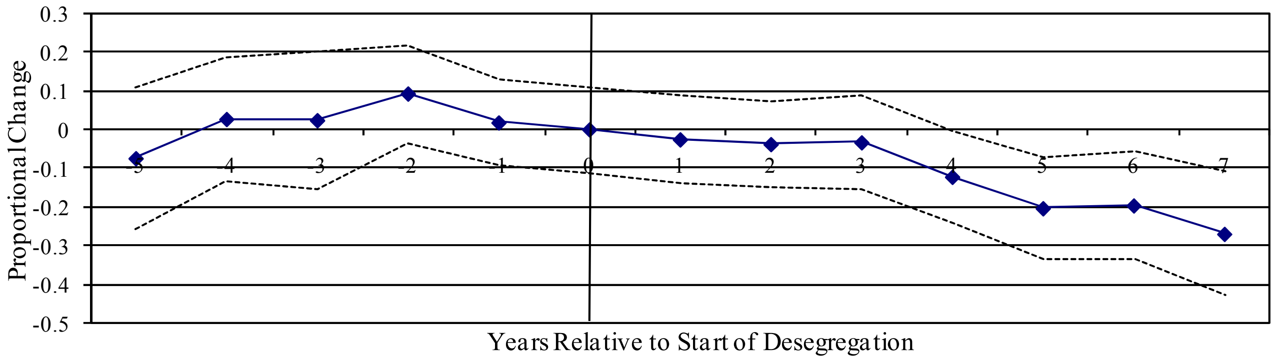
Note. The solid points display coefficient estimates and the dashed lines display the 95% confidence intervals around these estimates. Year "0" is the year immediately prior to the start of desegregation.

Figure 6: School Desegregation & White Homicide Victimizations

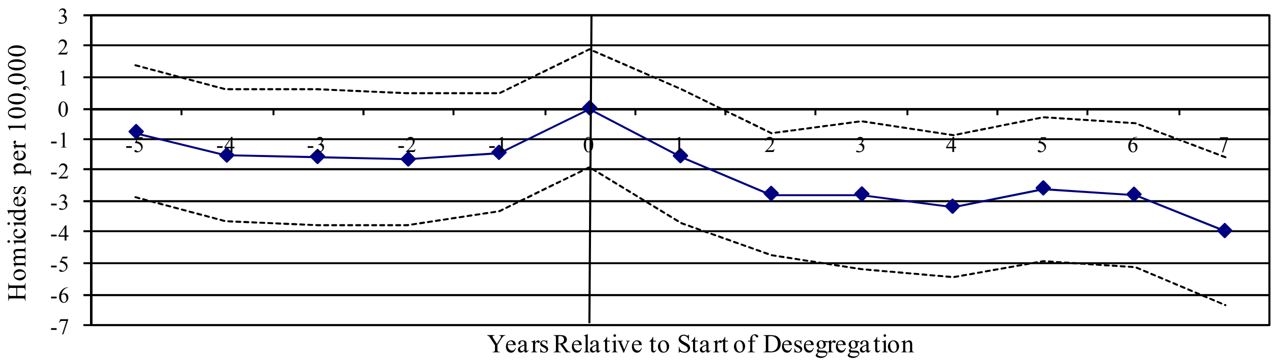
Panel A: Age Cohort 15-24 OLS Level



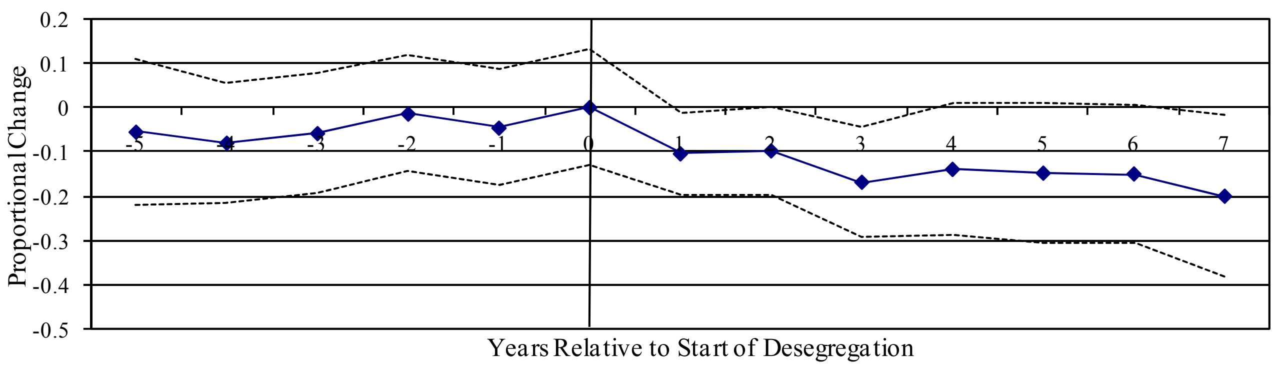
Panel B: Age Cohort 15-24 QML Count



Panel C: Age Cohort 25-34 OLS Level

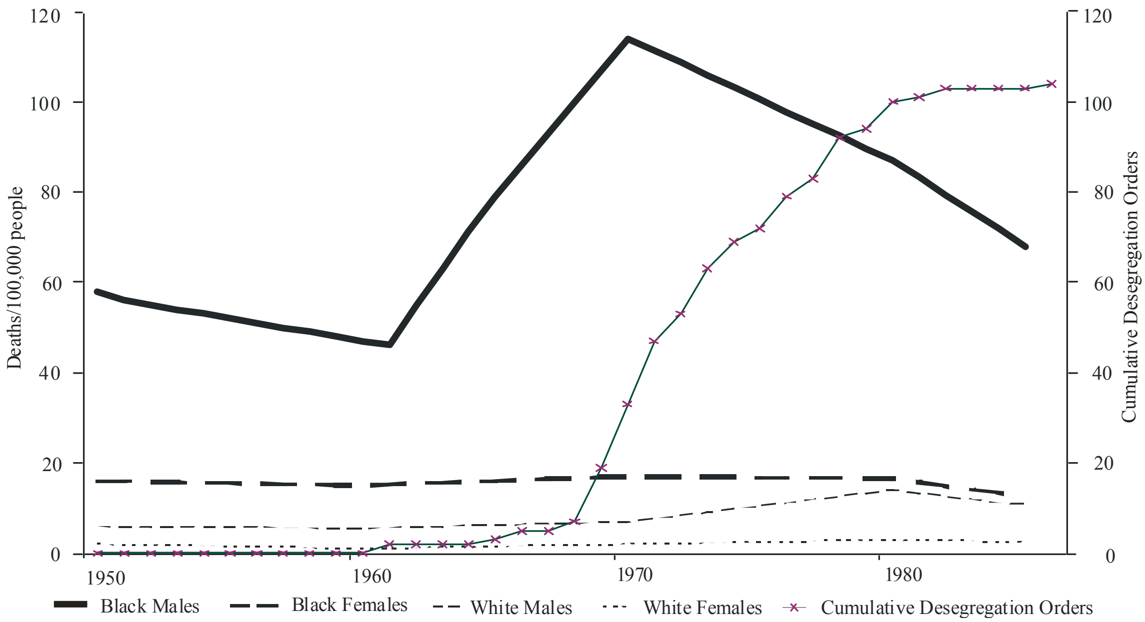


Panel D: Age Cohort 25-34 QML Count



Note. The solid points display coefficient estimates and the dashed lines display the 95% confidence intervals around these estimates. Year "0" is the year immediately prior to the start of desegregation.

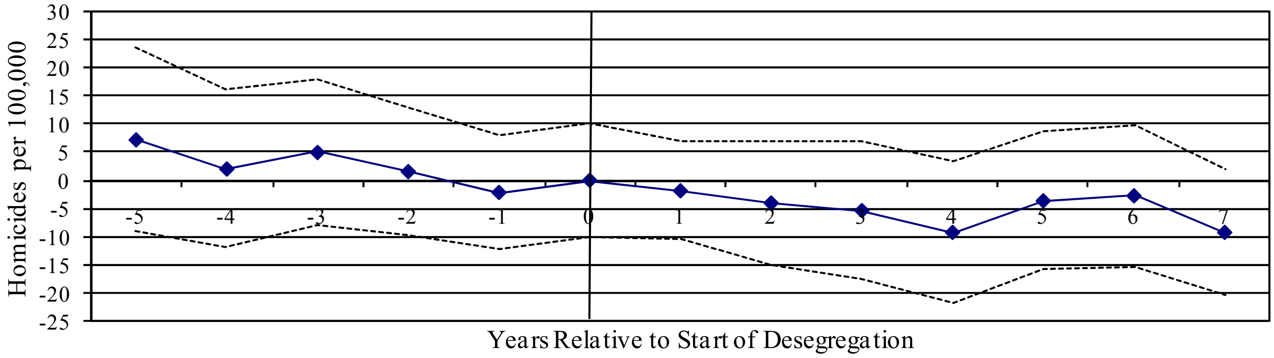
Figure 7: Historical Homicide Rates for Individuals Aged 15-24



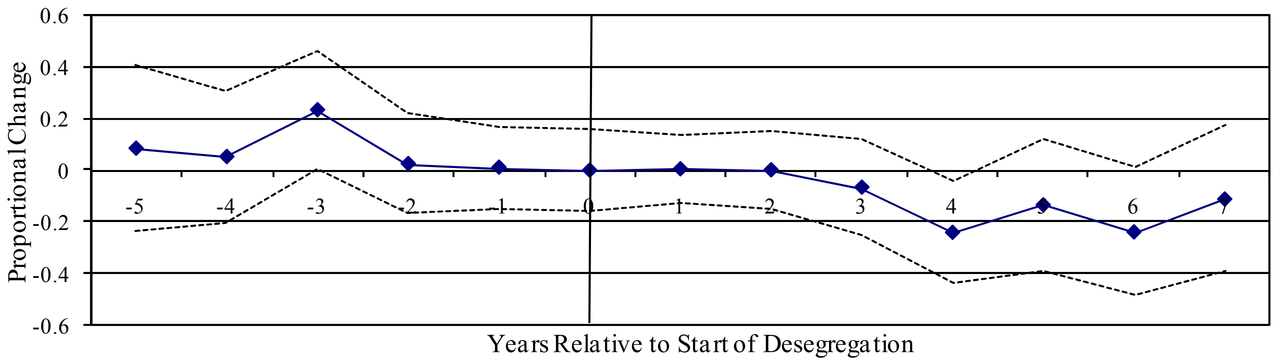
Source: Jaynes and Williams (1989), pp. 458-9.

Figure A1 School Desegregation & Black Homicide Victimizations

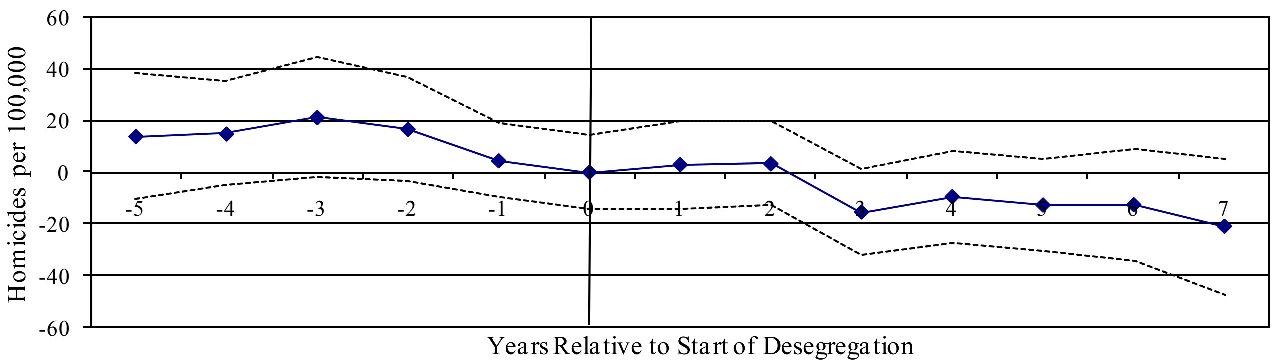
Panel A: Age Cohort 15-19 OLS Level



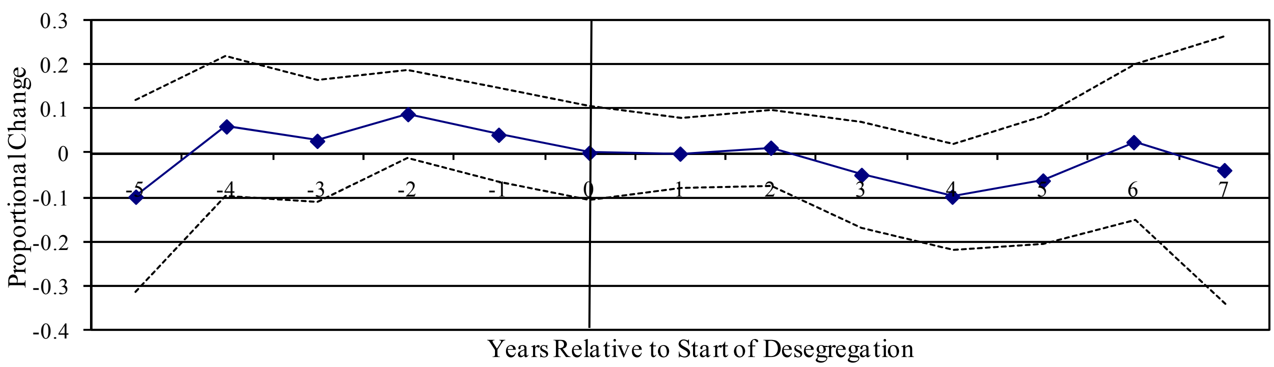
Panel B: Age Cohort 15-19 QML Count



Panel C: Age Cohort 35-44 OLS Level



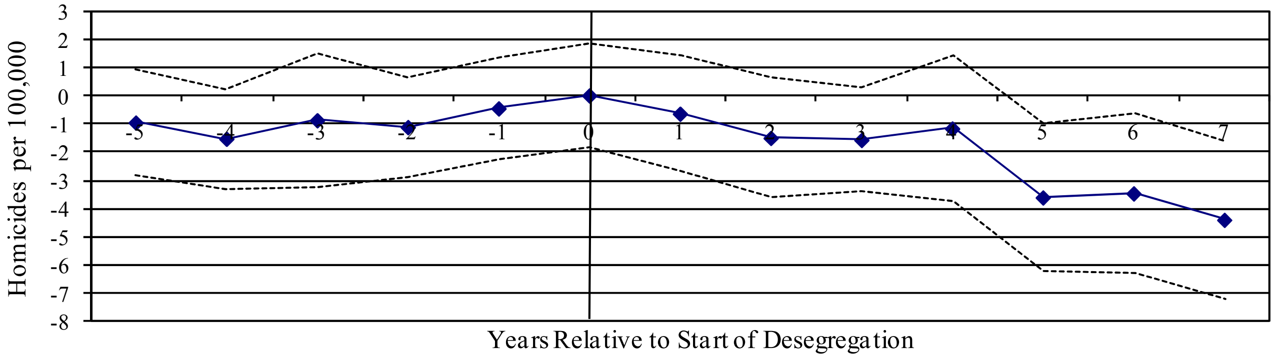
Panel D: Age Cohort 35-44 QML Count



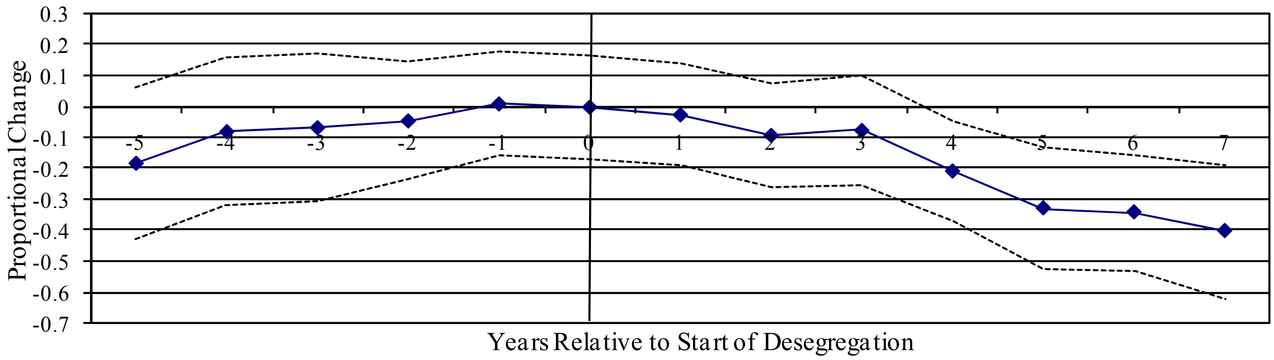
The solid points display coefficient estimates and the dashed lines display the 95% confidence intervals around these estimates. Year "0" is the year immediately prior to the start of desegregation.

Figure A2 School Desegregation & White Homicide Victimizations

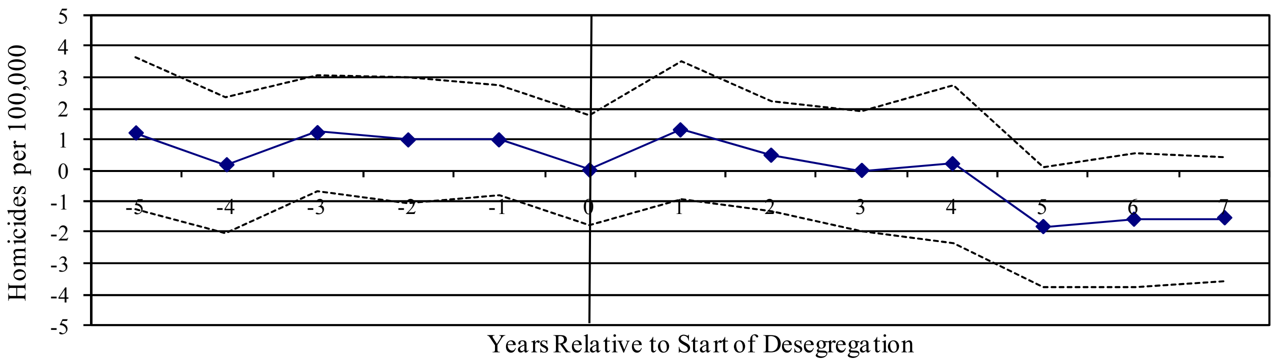
Panel A: Age Cohort 15-19 OLS Level



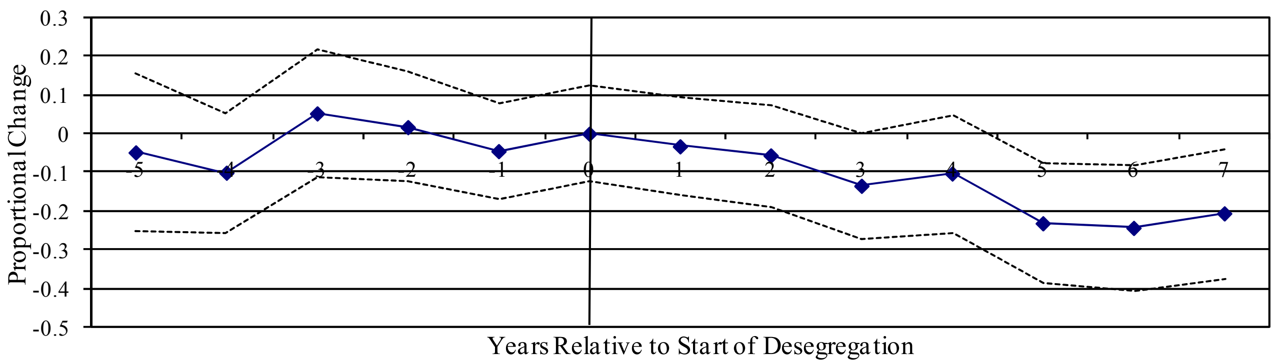
Panel B: Age Cohort 15-19 QML Count



Panel C: Age Cohort 35-44 OLS Level



Panel D: Age Cohort 35-44 QML Count



The solid points display coefficient estimates and the dashed lines display the 95% confidence intervals around these estimates. Year "0" is the year immediately prior to the start of desegregation.

Table 1
Homicide Offending

Offender	Victim						
	Black 15-24	Black 25-34	Black 35+	White 15-24	White 25-34	White 35+	Total
Black 15-24	8448	5190	4125	1158	961	2161	22043
	(.38)	(.24)	(.19)	(.05)	(.04)	(.10)	(1.00)
	{.52}	{.28}	{.22}	{.09}	{.07}	{.11}	{.22}
Black 25-34	3763	7256	4995	497	715	1094	18320
	(.21)	(.40)	(.27)	(.03)	(.04)	(.06)	(1.00)
	{.23}	{.39}	{.27}	{.04}	{.05}	{.06}	{.18}
Black 35+	2386	4474	8431	324	433	953	17001
	(.14)	(.26)	(.50)	(.02)	(.03)	(.06)	(1.00)
	{.15}	{.24}	{.45}	{.02}	{.03}	{.05}	{.17}
White 15-24	517	366	266	6324	3528	3833	14834
	(.03)	(.02)	(.02)	(.43)	(.24)	(.26)	(1.00)
	{.03}	{.02}	{.01}	{.47}	{.25}	{.20}	{.15}
White 25-34	506	627	480	3051	4958	4034	13656
	(.04)	(.05)	(.04)	(.22)	(.36)	(.30)	(1.00)
	{.03}	{.03}	{.03}	{.23}	{.36}	{.21}	{.14}
White 35+	481	556	425	2012	3301	6939	13714
	(.04)	(.04)	(.03)	(.15)	(.24)	(.51)	(1.00)
	{.03}	{.03}	{.02}	{.15}	{.24}	{.36}	{.14}
Total	16101	18469	18722	13366	13896	19014	99568
	(.16)	(.19)	(.19)	(.13)	(.14)	(.19)	(1.00)
	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}	{1.00}

Note. The cells display the total number of homicides in our sample of counties over the years 1976 to 1988 for offenders of the given age and race against victims of the given age and race. The data is from the Supplemental Homicides Report (SHR). Row percents are in parentheses and column percents are in brackets.

Table 2
Descriptive Statistics

	Full Sample	1960	1970	1980
	A. County Population Means			
Total	676517	573534	663642	709841
Total white	551253	490995	550597	564368
Total black	111646	82539	104269	125932
White 15-19	44782	33536	48789	48808
Black 15-19	10909	5648	10629	13706
White 15-24	92149	63904	96071	104377
Black 15-24	20834	11129	19098	26690
White 25-34	84733	64893	70071	96926
Black 25-34	17114	11956	13030	20757
White 35-44	67789	69536	63387	63523
Black 35-44	12799	11038	11589	13183
	B. Homicide rates per 100,000			
Total	10.8	6.6	11.3	14.0
Total white	5.9	3.1	5.7	8.6
Total black	34.4	27.1	40.1	37.5
White 15-19	5.7	2.3	5.0	9.7
Black 15-19	29.0	20.3	37.1	25.8
White 15-24	7.6	3.4	5.8	12.4
Black 15-24	45.2	29.2	60.0	47.1
White 25-34	9.7	4.8	10.3	13.5
Black 25-34	75.3	77.1	86.4	86.3
White 35-44	8.8	4.6	8.5	11.6
Black 35-44	63.1	50.2	80.2	56.4

Note. The cells display county means. The data is restricted to counties with a desegregated school district identified in the Welch and Light (1987) study. The "Full Sample" column contains data from 1959 - 1988.

Table 3
Black Homicide Victimization

	Proportional Response			Levels		
	QML Count		OLS Log Dummy	OLS		
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Age 15 - 19			
Post Desegregation Years 1 - 5	-0.17	-0.16	-0.08	-5.89	-5.05	-5.14
	(0.07)	(0.07)	(0.05)	(2.86)	(2.84)	(3.01)
Post Desegregation Years 6+	-0.27	-0.28	-0.15	-6.52	-5.71	-6.26
	(0.09)	(0.09)	(0.07)	(3.93)	(3.87)	(4.00)
			B. Age 15 - 24			
Post Desegregation Years 1 - 5	-0.14	-0.11	-0.13	-8.91	-7.45	-8.59
	(0.04)	(0.04)	(0.05)	(2.76)	(2.58)	(2.85)
Post Desegregation Years 6+	-0.23	-0.21	-0.19	-10.55	-9.32	-11.27
	(0.06)	(0.06)	(0.08)	(3.81)	(3.58)	(3.69)
			C. Age 25-34			
Post Desegregation Years 1 - 5	-0.15	-0.11	-0.09	-10.90	-9.54	-9.68
	(0.04)	(0.03)	(0.05)	(4.90)	(4.88)	(5.21)
Post Desegregation Years 6+	-0.29	-0.21	-0.18	-23.61	-21.68	-21.54
	(0.05)	(0.04)	(0.07)	(6.30)	(6.36)	(6.97)
			D. Age 35-44			
Post Desegregation Years 1 - 5	-0.12	-0.12	-0.10	-12.28	-12.37	-10.04
	(0.05)	(0.05)	(0.05)	(4.82)	(4.86)	(4.86)
Post Desegregation Years 6+	-0.16	-0.15	-0.16	-20.47	-20.52	-15.74
	(0.10)	(0.08)	(0.07)	(9.10)	(7.92)	(8.26)
Number of observations	3039	3039	3039	3039	3039	3039
Region * Year Effects	X	X	X	X	X	X
1960 County Charact. * Year		X			X	
County-Specific Linear Trends						X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the homicide count in columns (1) and (2), the log of the transformed homicide rate per 100,000 in column (3) and the homicide rate per 100,000 in columns (4) - (6).

Table 4
White Homicide Victimization

	Proportional Response			Levels		
	QML Count		OLS Log Dummy	OLS		
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Age 15 - 19			
Post Desegregation Years 1 - 5	-0.05	-0.01	-0.07	-0.48	-0.38	-0.49
	(0.06)	(0.05)	(0.04)	(0.50)	(0.51)	(0.53)
Post Desegregation Years 6+	-0.23	-0.20	-0.24	-2.22	-2.24	-2.23
	(0.09)	(0.08)	(0.07)	(0.82)	(0.80)	(0.87)
			B. Age 15 - 24			
Post Desegregation Years 1 - 5	-0.05	-0.02	-0.07	-0.49	-0.52	-0.43
	(0.04)	(0.04)	(0.05)	(0.41)	(0.42)	(0.40)
Post Desegregation Years 6+	-0.18	-0.15	-0.24	-2.20	-2.22	-1.97
	(0.06)	(0.06)	(0.07)	(0.72)	(0.66)	(0.68)
			C. Age 25-34			
Post Desegregation Years 1 - 5	-0.04	-0.01	-0.10	-1.07	-1.04	-1.01
	(0.05)	(0.05)	(0.05)	(0.59)	(0.61)	(0.62)
Post Desegregation Years 6+	-0.06	-0.03	-0.14	-1.57	-1.47	-1.33
	(0.07)	(0.06)	(0.07)	(0.76)	(0.73)	(0.83)
			D. Age 35-44			
Post Desegregation Years 1 - 5	-0.06	-0.05	0.00	-0.29	-0.50	-0.18
	(0.05)	(0.05)	(0.05)	(0.68)	(0.60)	(0.73)
Post Desegregation Years 6+	-0.12	-0.11	-0.06	-1.27	-1.59	-0.97
	(0.06)	(0.06)	(0.06)	(0.74)	(0.72)	(0.85)
Number of observations	3040	3040	3040	3040	3040	3040
Region * Year Effects	X	X	X	X	X	X
1960 County Charact. * Year		X			X	
County-Specific Linear Trends						X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the homicide count in columns (1) and (2), the log of the transformed homicide rate per 100,000 in column (3) and the homicide rate per 100,000 in columns (4) - (6).

Table 5
Supplemental Homicide Report Data: Homicide Offenders

	Age 15 - 19		Age 15 - 24		Age 25-35		Age 35 - 44	
	VS: Victim	SHR: Offender	VS: Victim	SHR: Offender	VS: Victim	SHR: Offender	VS: Victim	SHR: Offender
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. Black QML Count				
Post Desegregation Years 1 - 5	-0.27	-0.33	-0.15	-0.26	-0.16	-0.22	-0.10	-0.27
	(0.16)	(0.14)	(0.12)	(0.12)	(0.06)	(0.12)	(0.07)	(0.11)
Post Desegregation Years 6+	-0.43	-0.55	-0.26	-0.38	-0.25	-0.26	-0.09	-0.19
	(0.20)	(0.19)	(0.15)	(0.16)	(0.08)	(0.13)	(0.09)	(0.12)
				B. Black OLS				
Post Desegregation Years 1 - 5	-0.74	-5.99	-7.05	-2.24	-19.09	-4.94	-11.29	-5.40
	(4.91)	(6.32)	(3.69)	(5.79)	(11.66)	(6.32)	(6.11)	(5.50)
Post Desegregation Years 6+	-3.34	-12.14	-9.75	-7.40	-24.55	-5.81	-13.50	-4.03
	(5.62)	(8.01)	(4.64)	(6.40)	(13.66)	(7.95)	(9.67)	(6.05)
				C. White QML Count				
Post Desegregation Years 1 - 5	-0.15	-0.21	-0.12	-0.19	-0.03	-0.14	0.01	-0.10
	(0.07)	(0.12)	(0.05)	(0.10)	(0.07)	(0.09)	(0.07)	(0.11)
Post Desegregation Years 6+	-0.28	-0.12	-0.22	-0.17	-0.02	-0.11	-0.05	0.02
	(0.11)	(0.16)	(0.08)	(0.11)	(0.09)	(0.10)	(0.08)	(0.11)
				D. White OLS				
Post Desegregation Years 1 - 5	-2.98	-0.36	-2.02	0.21	-0.62	-0.21	0.95	1.06
	(1.22)	(1.23)	(0.70)	(1.06)	(0.77)	(1.00)	(1.61)	(1.41)
Post Desegregation Years 6+	-4.80	1.14	-3.82	0.70	-1.04	-0.58	0.28	1.73
	(1.60)	(1.98)	(1.08)	(1.45)	(1.15)	(1.12)	(1.70)	(1.36)
Number of Obs.	1363	1347	1363	1347	1363	1347	1363	1347
Region * Year	X	X	X	X	X	X	X	X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The sample runs from 1976 through 1988. The dependent variable is the homicide count in panels A and C and the homicide rate per 100,000 in panels B and D.

Table 6
Across-Age & Across-Race Homicide Offending

		QML Count Model			
		Victim			
	Offender	Black 15-24	Black 25-34	Black 35-44	White
		(1)	(2)	(3)	(4)
Post Desegregation Years 1 - 5	Black 15-24	-0.51	-0.24	-0.32	-0.01
		(.13)	(.21)	(.13)	(.12)
Post Desegregation Years 6+		-0.74	-0.24	-0.45	-0.08
		(.18)	(.26)	(.20)	(.15)
Post Desegregation Years 1 - 5	Black 25-34	-0.17	-0.37	-0.25	0.15
		(.15)	(.11)	(.15)	(.16)
Post Desegregation Years 6+		-0.18	-0.41	-0.13	0.09
		(.19)	(.12)	(.16)	(.19)
Post Desegregation Years 1 - 5	Black 35-44	-0.40	-0.09	-0.33	-0.06
		(.14)	(.18)	(.14)	(.16)
Post Desegregation Years 6+		-0.21	-0.01	-0.32	-0.08
		(.21)	(.21)	(.15)	(.19)
Number of observations		1336	1336	1222	1323
Region * Year Effects		X	X	X	X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The sample runs from 1976 through 1988. The estimates are produced using the QML count model. The dependent variable is the count of homicides by the black age-group identified in the "Offender" columm"against the group identified in the "Victim" columns. The expsoure variable is set equal to population count of the offender group. The number of observations refers to the black 15-24 row.

Table 7
School Desegregation and Long-Run Black Homicide Offending: Age 35 - 44

	Proportional Response			Levels
	QML Count		OLS Log Dummy	OLS
	(1)	(2)	(3)	(4)
	A. Black Age 35 - 44 Offending			
Post Desegregation Years 25+	-0.14		-0.05	-0.40
	(0.06)		(0.05)	(3.12)
Post Desegregation Years 20 - 24		-0.06		
		(0.05)		
Post Desegregation Years 25 - 29		-0.19		
		(0.08)		
Post Desegregation Years 30+		-0.22		
		(0.12)		
	B. Black Age 35 - 44 Offending Against Whites			
Post Desegregation Years 25+	-0.33		0.04	-0.81
	(0.12)		(0.04)	(0.74)
Post Desegregation Years 20 - 24		-0.14		
		(0.12)		
Post Desegregation Years 25 - 29		-0.47		
		(0.16)		
Post Desegregation Years 30+		-0.38		
		(0.21)		
Number of observations	2778	2778	2778	2778
Region * Year Effects	X	X	X	X
1960 County Charact. * Year Effect				

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the count of homicide offenders in columns (1)-(2), the log of the transformed homicide rate in column (3) and the homicide rate in column (4). The sample runs from 1976 - 2003, the years for which the SHR data are available.

Table 8
School Desegregation and Long-Run White Homicide Offending: Age 35 - 44

	Proportional Response			Levels
	QML Count		OLS Log Dummy	OLS
	(1)	(2)	(3)	(4)
	A. White Age 35 - 44 Offending			
Post Desegregation Years 25+	-0.15		-0.11	-0.57
	(0.08)		(0.06)	(0.52)
Post Desegregation Years 20 - 24		0.00		
		(0.05)		
Post Desegregation Years 25 - 29		-0.16		
		(0.09)		
Post Desegregation Years 30+		-0.09		
		(0.12)		
	B. White Age 35 - 44 Offending Against Blacks			
Post Desegregation Years 25+	-0.23		0.02	-0.16
	(0.16)		(0.04)	(0.10)
Post Desegregation Years 20 - 24		0.01		
		(0.11)		
Post Desegregation Years 25 - 29		-0.21		
		(0.18)		
Post Desegregation Years 30+		-0.26		
		(0.24)		
Number of observations	2778	2778	2778	2778
Region * Year Effects	X	X	X	X
1960 County Charact. * Year Effect				

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the count of homicide offenders in columns (1)-(2), the log of the transformed homicide rate in column (3) and the homicide rate in column (4). The sample runs from 1976 - 2003, the years for which the SHR data are available.

Table 9
Homicide Victimization, Sample Restricted to Decennial Census

	Proportional Response				Levels	
	QML Count		OLS Log Dummy		OLS	
	Census Years	3-Years Around Census	Census Years	3-Years Around Census	Census Years	3-Years Around Census
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Black 15 - 19			
Post Desegregation Years 1 - 5	0.03	-0.13	-0.18	-0.18	-17.57	-11.83
	(0.15)	(0.09)	(0.11)	(0.07)	(8.63)	(4.96)
Post Desegregation Years 6+	-0.38	-0.41	-0.30	-0.32	-25.11	-18.16
	(0.20)	(0.13)	(0.16)	(0.11)	(11.57)	(7.22)
			B. Black 15 - 24			
Post Desegregation Years 1 - 5	0.10	-0.11	-0.25	-0.25	-15.93	-15.58
	(0.10)	(0.07)	(0.13)	(0.08)	(9.48)	(5.14)
Post Desegregation Years 6+	-0.13	-0.33	-0.17	-0.30	-20.23	-20.85
	(0.14)	(0.12)	(0.16)	(0.11)	(12.53)	(7.33)
			C. White 15 - 19			
Post Desegregation Years 1 - 5	0.12	-0.10	-0.03	-0.10	0.63	-0.88
	(0.16)	(0.10)	(0.10)	(0.07)	(1.17)	(0.91)
Post Desegregation Years 6+	-0.01	-0.08	-0.28	-0.24	-3.36	-3.16
	(0.14)	(0.11)	(0.14)	(0.09)	(1.67)	(1.52)
			D. White 15 - 24			
Post Desegregation Years 1 - 5	0.02	-0.13	-0.06	-0.07	0.73	-0.99
	(0.12)	(0.07)	(0.13)	(0.08)	(1.22)	(0.82)
Post Desegregation Years 6+	-0.12	-0.15	-0.35	-0.27	-2.49	-3.17
	(0.11)	(0.08)	(0.16)	(0.10)	(1.75)	(1.41)
Number of observations	420	1258	420	1258	420	1258
Region * Year Effects	X	X	X	X	X	X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the homicide count in columns (1)-(2), the log of the transformed homicide rate in columns (3)-(4), and the homicide rate in columns (5)-(6). The sample is restricted to 1960, 1970, 1980 and 1990 in columns (1), (3) and (5). The sample is restricted to 1959, 1960, 1961, 1969, 1970, 1971, 1979, 1980, 1981, 1989, 1990, and 1991 in columns (2), (4) and (6).

Table 10
Effect of Desegregation Plan on County Population

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(White Age 15 - 24)				Log(Black Age 15 - 24)			
				A. Base Specifications				
Post Desegregation Years 1 - 5	-0.035	-0.044			0.053	0.035		
	(0.034)	(0.030)			(0.033)	(0.031)		
Post Desegregation Years +6	-0.011	-0.022			0.074	0.051		
	(0.045)	(0.040)			(0.050)	(0.046)		
Post Desegregation			-0.033	-0.043			0.054	0.036
			(0.034)	(0.030)			(0.033)	(0.031)
				B. South Interaction Specifications				
Post Desegregation Years 1 - 5	0.021	-0.007			0.016	0.017		
	(0.039)	(0.040)			(0.053)	(0.043)		
Post Desegregation Years +6	0.041	-0.006			0.068	0.051		
	(0.055)	(0.056)			(0.083)	(0.067)		
Post Desegregation Years 1 - 5 * South	-0.088	-0.063			0.061	0.032		
	(0.062)	(0.062)			(0.068)	(0.062)		
Post Desegregation Years +6 * South	-0.077	-0.004			-0.029	-0.019		
	(0.088)	(0.085)			(0.102)	(0.089)		
Post Desegregation			0.022	-0.008			0.018	0.018
			(0.039)	(0.040)			(0.053)	(0.043)
Post Desegregation * South			-0.087	-0.057			0.056	0.029
			(0.062)	(0.062)			(0.067)	(0.061)
Number of Observations	420	420	420	420	420	420	420	420
Region *Year Effect	X	X	X	X	X	X	X	X
1960 County characteristics *Year Effect		X		X		X		X

Note. Standard errors clustered by county in parentheses. The dependent variable for each of the panels is given in the panel title. The unit of observation is county-year. The estimation sample includes the years 1960, 1970, 1980, and1990.

Table 11
Effect of Desegregation Plan on Demographic Characteristics of County

	Log(Median Family Income)		Percent Age 25+ w/ High School Degree*		Percent Age 25+ w/ College Degree	
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Non-Whites			
Post Desegregation Years 1 - 5	-0.011	-0.012	-0.016	-0.007	-0.005	-0.003
	(0.017)	(0.018)	(0.009)	(0.009)	(0.005)	(0.004)
Post Desegregation Years 6+	-0.015	-0.011	0.010	0.017	-0.007	-0.006
	(0.028)	(0.029)	(0.012)	(0.014)	(0.007)	(0.007)
			B. Whites			
Post Desegregation Years 1 - 5	0.001	0.001	0.006	0.006	0.005	0.005
	(0.009)	(0.009)	(0.004)	(0.005)	(0.005)	(0.004)
Post Desegregation Years 6+	-0.017	-0.011	0.009	0.009	0.004	0.004
	(0.016)	(0.017)	(0.006)	(0.006)	(0.007)	(0.006)
Number of Observations	420	420	420	420	420	420
Region *Year Effect	X	X	X	X	X	X
1960 County characteristics *Year Effect		X		X		X

Note. Standard errors clustered by county in parentheses. The dependent variable is given in the column headings. The unit of observation is the county-year. * "Percent age 25+ w/ high school degree" refers to the percent with a high school degree, but without a college degree. The estimation sample includes the years 1960, 1970, 1980 and 1990.

Table 12
Homicide Victimization: MSA Sample

	Proportional Response: QML Count	Levels: OLS
	(1)	(2)
	A. Black Age 15 - 24	
Post Desegregation Years 1 - 5	-0.11	-6.30
	(0.05)	(2.75)
Post Desegregation Years 6+	-0.20	-8.08
	(0.07)	(3.67)
	B. White Age 15 - 24	
Post Desegregation Years 1 - 5	-0.05	-0.47
	(0.05)	(0.36)
Post Desegregation Years 6+	-0.14	-1.45
	(0.08)	(0.58)
Number of observations	2779	2779
Region * Year Effects	X	X

Note. The unit of observation is MSA-year. Standard errors clustered by MSA in parentheses. The dependent variable is the homicide count in column (1) and the homicide rate per 100,000 in column (2).

Table 13
Falsification Test, Death From Illness

	Age 15-24			Age 25-34			Age 35-44		
	Proportional Response		Level	Proportional Response		Level	Proportional Response		Level
	QML Count	OLS Log Dummy	OLS Level	QML Count	OLS Log Dummy	OLS Level	QML Count	OLS Log Dummy	OLS Level
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				A. Black					
Post Desegregation Years 1 - 5	-0.04	-0.01	-0.32	0.07	0.05	-0.35	0.04	-0.02	-10.85
	(0.04)	(0.03)	(1.74)	(0.04)	(0.04)	(6.25)	(0.03)	(0.04)	(15.05)
Post Desegregation Years 6+	0.04	0.04	2.49	0.15	0.04	-0.48	0.08	-0.07	-21.60
	(0.05)	(0.05)	(2.92)	(0.09)	(0.06)	(9.84)	(0.06)	(0.06)	(24.88)
Number of observations	3039	3039	3039	3040	3040	3040	3040	3040	3040
				B. White					
Post Desegregation Years 1 - 5	-0.06	-0.03	-0.67	-0.03	-0.01	0.02	0.00	-0.02	0.22
	(0.03)	(0.04)	(0.48)	(0.03)	(0.04)	(1.01)	(0.03)	(0.03)	(3.33)
Post Desegregation Years 6+	-0.04	-0.01	-0.23	-0.01	0.02	0.68	0.01	-0.07	-0.96
	(0.04)	(0.07)	(0.72)	(0.04)	(0.05)	(1.32)	(0.04)	(0.05)	(5.12)
Number of observations	3040	3040	3040	3040	3040	3040	3040	3040	3040
Region * Year Effects	X	X	X	X	X	X	X	X	X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the count of deaths from illness in columns (1), (4) and (7), the log of the transformed rate of death from illness per 100,000 in columns (2), (5) and (8), and the rate of death from illness per 100,000 in columns (3), (6) and (9).

Table 14
Supplemental Homicide Report Data: Homicide Offenders

	Proportional Response: QML Count	
	School Year	Summer
	(1)	(2)
	Black 15 - 19	
Post Desegregation Years 1 - 5	-0.40	-0.30
	(0.18)	(0.13)
Post Desegregation Years 6+	-0.58	-0.61
	(0.21)	(0.19)
Number of observations	1317	1317
Region * Year Effects	X	X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the count of homicides.

Table 15
Effect of Desegregation Plan on Local Public Good Provision

	(1)	(2)	(3)	(4)
	A. Ratio of Education Expenditures to Pop. Age 5 - 19			
Post Desegregation	175.0	164.9		
	(88.4)	(83.6)		
Post Desegregation Years 1 - 5			173.5	163.8
			(85.7)	(81.6)
Post Desegregation Years +6			163.0	155.9
			(89.1)	(88.7)
	B. Ratio of Police Expenditures to Population			
Post Desegregation	2.5	2.4		
	(2.7)	(2.7)		
Post Desegregation Years 1 - 5			1.9	1.7
			(2.8)	(2.8)
Post Desegregation Years +6			-2.3	-2.9
			(4.4)	(4.2)
	C. Ratio of Fire Department Expenditures to Population			
Post Desegregation	-0.2	0.0		
	(1.8)	(1.8)		
Post Desegregation Years 1 - 5			-0.1	0.1
			(1.9)	(1.9)
Post Desegregation Years +6			0.3	0.4
			(2.8)	(2.9)
Number of Observations	419	419	419	419
Region * Year Effect	X	X	X	X
1960 County characteristics * Year		X		X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variables given in the panel titles are from the Census Bureau's *Census of Governments* and are measured in 1990 dollars. The sample includes the following years: 1972, 1977, 1982 and 1987.

Table 16
Black Homicide age 15 - 24 Victimization Interactions

	QML Count					
	Δ Segregation Interactions			Δ Public Expenditure		
	(1)	(2)	(3)	(4)	(5)	(6)
Post Deseg. Years 1 - 5	-0.07	-0.04	-0.07	-0.09	-0.07	-0.12
	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)	(0.07)
Post Deseg. Years 6+	-0.13	-0.08	-0.11	-0.18	-0.20	-0.17
	(0.08)	(0.10)	(0.12)	(0.08)	(0.09)	(0.08)
Post Deseg. Years 1 - 5 *	-0.54		-0.53			
Δ Exposure Index	(0.20)		(0.35)			
Post Deseg. Years 6+ *	-0.88		-0.71			
Δ Exposure Index	(0.29)		(0.50)			
Post Deseg. Years 1 - 5 *		0.29	0.00			
Δ Dissimilarity Index		(0.11)	(0.22)			
Post Deseg. Years 6+ *		0.56	0.14			
Δ Dissimilarity Index		(0.22)	(0.43)			
Post Deseg. Years 1 - 5 *					-0.04	
Δ Ed. Expend. Per Pupil					(0.11)	
Post Deseg. Years 6 + *					0.10	
Δ Ed. Expend. Per Pupil					(0.14)	
Post Deseg. Years 1 - 5 *						2.35
Δ Police Per Pop.						(2.36)
Post Deseg. Years 6 + *						-4.00
Δ Police Per Pop.						(3.14)
Region * Year Effects	X	X	X	X	X	X
Desegregated after 1972				X	X	X
Number of observations	2693	2693	2693	1449	1449	1449

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the count of homicides. Δ refers to the change in the variable from one year prior to the implementation of desegregation to the fourth year after desegregation implementation, expect in columns (5) and (6). In these columns it refers to the five year change in spending between Census of Government years (i.e. years ending in 2 or 7) which span the year of desegregation. In these columns the sample is restricted to those counties desegregated in 1973 or later because the change in spending can only be calculated for these districts. Government spending is measured in thousands of 1990 dollars.

Table 17
White Homicide age 15 - 24 Victimization Interactions

	QML Count					
	Δ Segregation Interactions			Δ Public Expenditure		
	(1)	(2)	(3)	(4)	(5)	(6)
Post Deseg. Years 1 - 5	-0.05	-0.05	-0.01	-0.10	-0.07	-0.09
	(0.06)	(0.06)	(0.07)	(0.05)	(0.06)	(0.06)
Post Deseg. Years 6+	-0.09	-0.05	-0.12	-0.28	-0.19	-0.20
	(0.08)	(0.09)	(0.08)	(0.11)	(0.11)	(0.11)
Post Deseg. Years 1 - 5 * Δ Exposure Index	0.20					
	(0.31)					
Post Deseg. Years 6+ * Δ Exposure Index	-0.26					
	(0.36)					
Post Deseg. Years 1 - 5 * Δ Dissimilarity Index		-0.09				
		(0.18)				
Post Deseg. Years 6+ * Δ Dissimilarity Index		0.31				
		(0.25)				
Post Deseg. Years 1 - 5 * Δ % white in deseg school			0.16			
			(0.73)			
Post Deseg. Years 6+ * Δ % white in deseg school			-0.24			
			(0.77)			
Post Deseg. Years 1 - 5 * Δ Ed. Expend. Per Pupil					-0.14	
					(0.11)	
Post Deseg. Years 6 + * Δ Ed. Expend. Per Pupil					-0.31	
					(0.08)	
Post Deseg. Years 1 - 5 * Δ Police Per Pop.						-3.11
						(2.77)
Post Deseg. Years 6 + * Δ Police Per Pop.						-8.46
						(3.02)
Region * Year Effects	X	X	X	X	X	X
Desegregated after 1972				X	X	X
Number of observations	2694	2694	2694	1449	1449	1449

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the count of homicides. Δ refers to the change in the variable from one year prior to the implementation of desegregation to the fourth year after desegregation implementation, expect in columns (5) and (6). In these columns it refers to the five year change in spending between Census of Government years (i.e. years ending in 2 or 7) which span the year of desegregation. In these columns the sample is restricted to those counties desegregated in 1973 or later because the change in spending can only be calculated for these districts. Government spending is measured in thousands of 1990 dollars.

Table 18
Effect of Desegregation Plan on Percent of Children Attending the Desegregated School District

	White		Black	
	(1)	(2)	(3)	(4)
	Ratio of Enrollment in Desegregated School District to Children in the Country			
Post Desegregation Years 1 - 5	-0.054	-0.032	-0.005	0.000
	(0.012)	(0.012)	(0.015)	(0.013)
Post Desegregation Years 6+	-0.064	-0.039	0.011	0.014
	(0.015)	(0.016)	(0.019)	(0.019)
Number of Observations	306	306	306	306
Region * Year Effect	X	X	X	X
1970 School characteristics * Year Effect		X		X
1960 County characteristics * Year Effect		X		X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the ratio of enrollment in the desegregated school district to the number of children in the county. The sample includes 1970, 1980 and 1990.

Appendix Table A1
Counties and School Districts in Sample and Year of Desegregation

County	Desegregated School District Name	State	Desegregation Date
Jefferson	Birmingham	AL	1970
Jefferson	Jefferson County	AL	1971
Mobile	Mobile	AL	1971
Pulaski	Little Rock	AR	1971
Pima	Tucson	AZ	1978
Alameda	Oakland	CA	1966
Contra Costa	Richmond	CA	1969
Fresno	Fresno	CA	1978
Los Angeles	Long Beach	CA	1980
Los Angeles	Los Angeles	CA	1978
Los Angeles	Pasadena	CA	1970
Sacramento	Sacramento	CA	1976
San Bernardino	San Bernardino	CA	1978
San Diego	San Diego	CA	1977
San Francisco	San Francisco	CA	1971
Santa Clara	San Jose	CA	1986
Solano	Vallejo	CA	1975
Denver	Denver	CO	1974
Fairfield	Stamford	CT	1970
Hartford	Hartford	CT	1966
New Castle	Wilmington County (Wilmington)	DE	1978
Brevard	Brevard County (Melbourne)	FL	1969
Broward	Broward County (Fort Lauderdale)	FL	1970
Duval	Duval County (Jacksonville)	FL	1971
Hillsborough	Hillsborough County (Tampa)	FL	1971
Lee	Lee County (Fort Meyers)	FL	1969
Miami-Dade	Dade County (Miami)	FL	1970
Orange	Orange County (Orlando)	FL	1972
Palm Beach	Palm Beach County (West Palm Beach)	FL	1970
Pinellas	Pinellas County (St Petersburg)	FL	1970
Polk	Polk County (Lakeland)	FL	1969
Volusia	Volusia (Daytona)	FL	1969
Dougherty	Dougherty County (Albany)	GA	1980
Fulton	Atlanta	GA	1973
Muscogee	Muscogee County (Columbus)	GA	1971
Cook	Chicago	IL	1982
Winnebago	Rockford	IL	1973
Allen	Fort Wayne	IN	1971
Marion	Indianapolis	IN	1973
St. Joseph	South Bend	IN	1981
Sedgwick	Wichita	KS	1971
Wyandotte	Kansas City	KS	1977
Fayette	Fayette County (Lexington)	KY	1972
Jefferson	Jefferson County (Louisville)	KY	1975
Caddo	Caddo Parish (Shreveport)	LA	1969
Calcasieu	Calcasieu Parish (Lake Charles)	LA	1969
E. Baton Rouge	East Baton Rouge Parish	LA	1970
Jefferson	Jefferson Parish	LA	1971
Orleans	New Orleans Parish	LA	1961
Rapides	Rapides Parish (Alexandria)	LA	1969
Terrebonne	Terrebonne Parish	LA	1969
Bristol	New Bedford	MA	1976
Hampden	Springfield	MA	1974
Suffolk	Boston	MA	1974

Baltimore City	Baltimore	MD	1974
Harford	Harford County	MD	1965
Prince George's	Prince Georges County	MD	1973
Ingham	Lansing	MI	1972
Kent	Grand Rapids	MI	1968
Wayne	Detroit	MI	1975
Hennepin	Minneapolis	MN	1974
Jackson	Kansas City	MO	1977
St. Louis City	St. Louis	MO	1980
Cumberland	Fayetteville/Cumberland County	NC	1969
Gaston	Gaston County (Gastonia)	NC	1970
Mecklenburg	Mecklenburg County (Charlotte)	NC	1970
New Hanover	New Hanover County (Wilmington)	NC	1969
Douglas	Omaha	NE	1976
Essex	Newark	NJ	1961
Hudson	Jersey City	NJ	1976
Clark	Clark County (Las Vegas)	NV	1972
Erie	Buffalo	NY	1976
Monroe	Rochester	NY	1970
Cuyahoga	Cleveland	OH	1979
Franklin	Columbus	OH	1979
Hamilton	Cincinnati	OH	1973
Lucas	Toledo	OH	1980
Montgomery	Dayton	OH	1976
Summit	Akron	OH	1977
Comanche	Lawton	OK	1973
Oklahoma	Oklahoma City	OK	1972
Tulsa	Tulsa	OK	1971
Multnomah	Portland	OR	1974
Allegheny	Pittsburgh	PA	1980
Philadelphia	Philadelphia	PA	1978
Charleston	Charleston	SC	1970
Greenville	Greenville County	SC	1970
Richland	Richland County	SC	1970
Davidson	Nashville	TN	1971
Shelby	Memphis	TN	1973
Bexar	San Antonio	TX	1969
Dallas	Dallas	TX	1971
Ector	Odessa	TX	1982
El Paso	El Paso	TX	1978
Harris	Houston	TX	1971
Lubbock	Lubbock	TX	1978
McLennan	Waco	TX	1973
Potter	Amarillo	TX	1972
Tarrant	Fort Worth	TX	1973
Travis	Austin	TX	1980
Arlington	Arlington County	VA	1971
Norfolk City	Norfolk	VA	1970
Pittsylvania	Pittsylvania County	VA	1969
Roanoke City	Roanoke	VA	1970
King	Seattle	WA	1978
Pierce	Tacoma	WA	1968
Milwaukee	Milwaukee	WI	1976
Raleigh	Raleigh County (Beckley)	WV	1973

Appendix Table A2
Black and White Homicide Victimization, Weighted by Population

	Black			White		
	Proportional Response		Levels	Proportional Response		Levels
	QML Count	OLS Log Dummy	OLS	QML Count	OLS Log Dummy	OLS
	(1)	(2)	(3)	(4)	(6)	(5)
			A. Age 15-19			
Post Desegregation Years 1 - 5	-0.21	-0.18	-7.82	-0.03	-0.11	-0.40
	(0.05)	(0.06)	(2.81)	(0.04)	(0.06)	(0.65)
Post Desegregation Years 6+	-0.16	-0.34	-11.79	-0.16	-0.35	-2.51
	(0.11)	(0.10)	(4.02)	(0.10)	(0.15)	(1.14)
			B. Age 15-24			
Post Desegregation Years 1 - 5	-0.18	-0.13	-9.61	-0.06	-0.09	-0.64
	(0.03)	(0.04)	(3.05)	(0.04)	(0.05)	(0.71)
Post Desegregation Years 6+	-0.21	-0.22	-14.93	-0.17	-0.27	-2.54
	(0.05)	(0.07)	(4.13)	(0.07)	(0.08)	(1.13)
			B. Age 25-34			
Post Desegregation Years 1 - 5	-0.18	-0.14	-12.92	-0.05	-0.09	-0.54
	(0.03)	(0.04)	(3.77)	(0.05)	(0.05)	(0.67)
Post Desegregation Years 6+	-0.28	-0.28	-25.02	-0.09	-0.15	-1.04
	(0.04)	(0.07)	(4.65)	(0.07)	(0.07)	(0.89)
			B. Age 35-44			
Post Desegregation Years 1 - 5	-0.06	-0.10	-7.78	-0.08	-0.04	-0.48
	(0.05)	(0.06)	(3.63)	(0.05)	(0.04)	(0.44)
Post Desegregation Years 6+	0.06	-0.16	-11.28	-0.19	-0.12	-1.14
	(0.14)	(0.10)	(6.43)	(0.06)	(0.07)	(0.71)
Number of observations	3039	3039	3039	3039	3039	3039
Region * Year Effects	X	X	X	X	X	X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the homicide count in columns (1) and (4), the log of the transformed homicide rate per 100,000 in columns (2) and (5), and the homicide rate per 100,000 in columns (3) and (6). All specifications are weighted by the relevant total age-race population count for the panel.

Appendix Table A3
Homicide Victimization: Bordering County Sample

	Proportional Response: QML Count			Levels: OLS		
	Bordering County Sample Estimate	Implied County Estimate Assuming No Migration	Actual County Sample Estimate (Tables 3 & 4)	Bordering County Sample Estimate	Implied County Estimate Assuming No Migration	Actual County Sample Estimate (Tables 3 & 4)
	β_c	$\frac{\beta_c}{\delta}$	β	β_c	$\frac{\beta_c}{\delta}$	β
	(1)	(2)	(3)	(4)	(5)	(6)
A. Black Age 15 - 24						
Post Desegregation Years 1 - 5	-0.05	-0.09	-0.14	-4.53	-8.20	-8.91
	(0.04)			(2.31)		
Post Desegregation Years 6+	-0.11	-0.21	-0.23	-5.59	-10.13	-10.55
	(0.05)			(3.32)		
B. White Age 15 - 24						
Post Desegregation Years 1 - 5	0.01	0.01	-0.05	-0.01	-0.01	-0.49
	(0.04)			(0.31)		
Post Desegregation Years 6+	-0.07	-0.12	-0.18	-0.66	-1.20	-2.2
	(0.06)			(0.57)		
Number of observations	3040	3040	3040	3040	3040	3040
Region * Year Effects	X	X	X	X	X	X

Note. Standard errors clustered by county in parentheses. The unit of observation is county group-year, where a county group is a county listed on Appendix Table A1 *plus* all counties which border it. The dependent variable is the homicide count in column (1) and the homicide rate per 100,000 in column (4). δ equals the percent of the bordering county group population which resides in the treated counties - see Appendix D for details.

Appendix Table A4
Black Homicide age 15 - 24 Victimization Interactions

	OLS Level					
	Δ Segregation Interactions			Δ Public Expenditure		
	(1)	(2)	(3)	(4)	(5)	(6)
Post Deseg. Years 1 - 5	-3.42	-2.28	-3.01	-3.85	-0.34	3.11
	(3.13)	(3.61)	(3.54)	(4.07)	(5.67)	(5.18)
Post Deseg. Years 6+	-4.07	-3.54	-3.76	-6.33	-5.51	-2.92
	(4.18)	(4.43)	(4.39)	(6.18)	(6.67)	(6.93)
Post Deseg. Years 1 - 5 * Δ Exposure Index	-28.02		-24.01			
	(15.52)		(19.47)			
Post Deseg. Years 6+ * Δ Exposure Index	-27.29		-23.65			
	(14.61)		(19.31)			
Post Deseg. Years 1 - 5 * Δ Dissimilarity Index		19.54	3.82			
		(11.47)	(13.14)			
Post Deseg. Years 6+ * Δ Dissimilarity Index		18.95	3.50			
		(10.54)	(12.37)			
Post Deseg. Years 1 - 5 * Δ Ed. Expend. Per Pupil					-9.96	
					(7.42)	
Post Deseg. Years 6 + * Δ Ed. Expend. Per Pupil					-0.49	
					(5.83)	
Post Deseg. Years 1 - 5 * Δ Police Per Pop.						-81.28
						(188.54)
Post Deseg. Years 6 + * Δ Police Per Pop.						-243.13
						(176.69)
Region * Year Effects	X	X	X	X	X	X
Desegregated after 1972				X	X	X
Number of observations	2693	2693	2693	1449	1449	1449

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the homicide rate per 100,000. Δ refers to the change in the variable from one year prior to the implementation of desegregation to the fourth year after desegregation implementation, expect in columns (5) and (6). In these columns it refers to the five year change in spending between Census of Government years (i.e. years ending in 2 or 7) for the years which include the year of desegregation. In these columns the sample is restricted to those counties desegregated in 1973 or later because the change in spending can only be calculated for these districts.

Appendix Table A5
White Homicide age 15 - 24 Victimization Interactions

	OLS Level					
	Δ Segregation Interactions			Δ Public Expenditure		
	(1)	(2)	(3)	(4)	(5)	(6)
Post Deseg. Years 1 - 5	-0.63	-0.51	-0.39	-1.28	-1.38	-1.22
	(0.54)	(0.62)	(0.54)	(0.75)	(1.02)	(1.10)
Post Deseg. Years 6+	-1.76	-1.30	-2.17	-4.06	-3.32	-3.36
	(0.96)	(1.07)	(0.79)	(1.66)	(1.69)	(1.82)
Post Deseg. Years 1 - 5 * Δ Exposure Index	1.57					
	(2.45)					
Post Deseg. Years 6+ * Δ Exposure Index	-2.24					
	(3.28)					
Post Deseg. Years 1 - 5 * Δ Dissimilarity Index		-0.52				
		(1.85)				
Post Deseg. Years 6+ * Δ Dissimilarity Index		2.89				
		(2.45)				
Post Deseg. Years 1 - 5 * Δ % white in deseg school			-0.75			
			(3.85)			
Post Deseg. Years 6+ * Δ % white in deseg school			-2.73			
			(4.30)			
Post Deseg. Years 1 - 5 * Δ Ed. Expend. Per Pupil					0.11	
					(1.54)	
Post Deseg. Years 6 + * Δ Ed. Expend. Per Pupil					-2.38	
					(1.06)	
Post Deseg. Years 1 - 5 * Δ Police Per Pop.						-10.44
						(49.23)
Post Deseg. Years 6 + * Δ Police Per Pop.						-52.05
						(38.93)
Region * Year Effects	X	X	X	X	X	X
Desegregated after 1972				X	X	X
Number of observations	2693	2693	2693	1449	1449	1449

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the homicide rate per 100,000. Δ refers to the change in the variable from one year prior to the implementation of desegregation to the fourth year after desegregation implementation, expect in columns (5) and (6). In these columns it refers to the five year change in spending between Census of Government years (i.e. years ending in 2 or 7) for the years which include the year of desegregation. In these columns the sample is restricted to those counties desegregated in 1973 or later because the change in spending can only be calculated for these districts.