

THE EFFECTS OF SCHOOL DESEGREGATION ON CRIME

Draft date: November 2, 2008

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The authors contributed equally to this paper and are listed in reverse alphabetical order. This research was supported by grants from the Spencer Foundation and the National Science Foundation (SES-0820033). Thanks to Jonathan Guryan and Sarah Reber for sharing their programs and data, to Laurel Beck, Michael Corey, Heather Harris, Daniel Stenberg and Jake Ward for excellent research assistance, and to Elizabeth Ananat, Josh Angrist, Pat Bayer, Hoyt Bleakley, Liz Cascio, Kerwin Charles, Charles Clotfelter, Philip Cook, David Deming, David Figlio, Jack Greenberg, Jonathan Gruber, John Horton, Steve Levitt, Erzo Luttmer, Derek Neal, Robert Sampson, Elizabeth Vigdor, Jacob Vigdor, Ofer Malamud, Tom Miles, Michael Tonry, Jeff Wooldridge and seminar participants at Brown University, the Brookings Institution, Duke University, the University of California at Berkeley, the University of Chicago Law School and Graduate School of Business, Harvard University, the University of Maryland, the University of Wisconsin, and the annual meetings of the American Economic Association, Association for Public Policy Analysis and Management, and National Bureau of Economic Research for helpful comments. All opinions are of course our own and do not necessarily represent the views of the Federal Reserve Board of Governors or its staff.

THE EFFECTS OF SCHOOL DESEGREGATION ON CRIME

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 districts subject to such orders. For blacks, we find that homicide victimization rates decline by around 25 percent when court orders are implemented, and that homicide arrests also decline significantly. These effects are quite persistent – attending a desegregated school appears to reduce homicide offending into adulthood. White homicide victimizations also decline, due perhaps in part to a decline in the rate at which blacks kill whites.

Abstract word count: 100

JEL codes: I2, J15, J18, K42

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I. INTRODUCTION

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.” The decision launched what former Solicitor General Walter Dellinger has called “the most important legal, political, social and moral event in twentieth-century American domestic history” [Williams, 1998, p. 400]. 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].

The *Brown* ruling assumed that school desegregation would improve the life chances of black children. Whether this belief is correct remains of great policy interest. While residential segregation by race has been declining over the past several decades, school segregation has not [Vigdor and Ludwig, 2008]. Only around half of blacks and one-quarter of whites believe the U.S. should do more to integrate schools [Public Agenda, 1998] and there are signs of growing dissent about school desegregation even within the NAACP [Hanley, 1995]. The majority of Supreme Court decisions about school desegregation since 1973 have gone against Civil Rights groups [Kahlenberg, 2001], and during the 1990s, a large number of local school desegregation plans were terminated [Clotfelter, Ladd and Vigdor, 2005; Lutz, 2005]. Most recently, in 2007 the Supreme Court issued two 5-4 decisions striking down voluntary desegregation plans in Seattle and Louisville, which are sure to generate additional litigation.¹

A large body of research over the half-century since *Brown* has tried to understand the effects of court-ordered school desegregation on children, with most studies focused on the consequences for academic outcomes.² Yet impacts on *non-academic* outcomes might be at least as important for social welfare. One of the most important non-academic outcomes is crime, given the enormous social costs –

¹ In the two decisions announced by the Supreme Court on 6/28/07, *Meredith v. Jefferson Co. Board of Ed.* and *Parents Involved in the Community Schools v. Seattle School District No. 1*, Justice Kennedy’s controlling opinions left the door open to more narrowly targeted plans. As one lawyer told the *New York Times*, “The decision leaves unanswered questions about when race may be considered, and unanswered questions lead to more litigation” [Lewin, 1997]. These decisions do not impact districts under court order to desegregate, only those that had initiated desegregation efforts on their own.

² A few studies have focused on labor market outcomes; see for example Vigdor [2006], Ashenfelter, Collins and Yoon [2005], Boozer, Krueger and Wolkon [1992], Grogger [1996], and Rivkin [2000].

perhaps as much as \$1 or \$2 trillion per year [Anderson, 1999; Ludwig, 2006]. In the Perry Preschool program, 70 percent of the benefits came from reduced crime [Belfield et al., 2006]. The substantial cross-sectional and time-series variation in crime suggests this behavior might also be particularly susceptible to social or policy influences. For example, from 1984 to 1993 the homicide rate tripled for black males 14-24, then dropped by fully half over the next seven years [Levitt, 2004, p. 180].

Theory yields ambiguous predictions about how court-ordered school desegregation might affect crime. Desegregation orders could reduce crime for purely mechanical reasons – by incapacitating youth on long bus rides during the high-crime hours after school. Desegregation orders also seem to improve schooling outcomes for blacks [Guryan 2004, Lutz 2005], which, all else equal, would be expected to reduce criminal involvement [Becker, 1968, Lochner and Moretti, 2004], and may also change attitudes about the fairness and opportunities available in American society. On the other hand, desegregation orders could increase crime at least in the short term by exacerbating racial tensions, which was the concern expressed by Justice Hugo Black during conference discussion of *Brown* [Klarman, 2004, p. 294]. The net effect on crime is ultimately an empirical question.

In this paper we present the first estimates for the effects of court-ordered school desegregation on crime that use a plausibly exogenous source of identifying variation.³ In many ways our basic research design is similar to that employed by Guryan [2004] and Reber [2005]. We exploit the fact that most of the largest school districts in the U.S. were slow to desegregate after the *Brown* ruling, and so wound up being forced to desegregate by local Federal courts. Many local desegregation orders were the result of litigation by the NAACP, which seems to have filed cases strategically in places when and where they

³ Only one previous study we know of has examined this question. 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.

were most likely to win, rather than to generate the greatest immediate benefit to minority children. As a result, differences in the timing of court orders among districts ever subject to such orders are plausibly orthogonal to other determinants of youth crime, and serves as our source of identifying variation. Within the set of districts ever subject to such orders, we examine 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 large share of the total social costs of crime [Ludwig, 2006].

We find that for blacks who were of school age when desegregation orders were enacted, these court orders reduce homicide victimization rates by around 25 percent, and generate even larger proportional declines in homicide arrests. These are large effects, consistent with Guryan's [2004] finding that court desegregation orders cause a 25 percent decline in dropout rates – another outcome that, like crime, is 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, Figlio, and Sellin, 1972; Tracy, Wolfgang and Figlio, 1990]. School desegregation would need to change the behavior of just a small share of high-risk youth to generate large changes in crime. We also find homicide victimization and offending rates seem to decline by smaller proportional amounts among blacks who were already adults at the time of the court orders.

The schooling gains from court school-desegregation orders estimated by Guryan [2004] must be part of the explanation for why homicide offending rates decline among black youth. Consistent with this mechanism, our estimates for offending impacts are about as large for summer months as for the school year (suggesting incapacitation does not explain our results), and offending impacts on school age youth seem to persist as these cohorts age into adulthood. But something else must also be at work, given that homicide arrests also declined among black adults after court school-desegregation orders are enacted.

Our findings for a behavioral response among adults are consistent with the hypothesis from Rossell [1978] that court school-desegregation orders change community-wide attitudes. Consistent with this hypothesis, Rossell finds these court orders reduce racial intolerance among whites by 10 percent, while Guryan [2004] finds black dropout rates decline by large amounts even in the first year after the court orders, even though blacks would not have been exposed for very long to different schools or peers.

Homicide victimization rates also declined for whites following enactment of court desegregation orders. The reductions in white victimizations seem due in part to a decline in the rate at which blacks kill whites. Whether desegregation orders reduce white homicide offending is less clear in our data.

The key question for our study is whether these patterns reflect a real behavioral response to court-ordered school desegregation, or instead reflect non-randomness in the timing of when these court orders are enacted. Some empirical support for the validity of our research design comes from the fact that we do not see any pre-existing homicide trends for black or white youth or adults in the years *before* these court desegregation orders are enacted.⁴ In addition, court school-desegregation orders appear to be unrelated to the political affiliation or ideology of the federal judges in each district, and do not seem to have any impact on youth or adult mortality from disease, an outcome that should logically not be affected by school desegregation. Moreover, we find black homicides seemed to decline the most in those districts that experienced the largest declines in school segregation levels, while white homicides seemed to decline the most in areas where local governments responded to desegregation orders with the largest increases in spending on schools and police. Our suggestive evidence that effects may be largest in areas that experienced the largest “treatment dose” provides additional support for our research design.

⁴ Guryan [2004] shows districts that desegregated at different times have similar trends in socio-economic outcomes between the 1960 and 1970 decennial censuses. Because we have annual data on victimization rates from the VS, we can provide an even sharper test of this identifying assumption.

Since we rely on county-level data, a different concern is that desegregation orders might affect population migration patterns. However, this does not seem to be a major problem in practice. One potential issue is measurement error in the denominator of our homicide rates, but we obtain similar results using data just from decennial census years when we have very accurate population counts. Alternatively, we might confound behavioral responses to desegregation orders with changes over time in county population characteristics. But we find little evidence that court orders cause migration across county boundaries or that they impact the socio-demographic composition of the counties in our analytic sample. Furthermore, analyses using larger geographic units (MSAs or bordering county groups) are qualitatively similar to our main results.

This topic is of interest in part because of contemporary debates about school desegregation. Do estimates for the effects of school desegregation orders implemented decades ago have any relevance for understanding the social consequences of current desegregation plans? While there is no way to definitively answer this question, we note that there is little evidence that crime impacts differ between desegregation orders implemented earlier versus later in our study period. We also find that voluntary plans, which provide students with some choice over where to attend school and are the approach most commonly used in recent years, have similar impacts to involuntary plans.

Our findings for blacks suggest the potential for crime prevention through human capital interventions, a hypothesis implied by Becker's [1968] economic model of crime but for which there have not been many good examples in practice [Donohue and Siegelman, 1998, Farrington and Welch, 2007]. Our findings also help explain several unresolved puzzles in national crime statistics, including the convergence in black-white homicide rates beginning in the late 1960s.

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

In 1955, a year after *Brown*, the Supreme Court issued the *Brown II* decision indicating school districts should desegregate “with all deliberate speed” (349 U.S. 294, 1955). What this meant in practice was not specified, and details were left to be determined by the lower Federal courts. Thurgood Marshall tried to be optimistic, claiming that “those white crackers are going to get tired of having Negro lawyers beating ‘em every day in court” [Williams, 1998, p. 239]. Yet in practice, 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., 2007]. However, large districts were slower to desegregate. Since *Brown* only bound the school boards in five cases [Klarman, 2004], most of the nation’s largest districts wound up having 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 the process through which these orders came about. 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]. As a result, a large share of desegregation lawsuits were filed by the NAACP, which, given resource constraints, was selective in deciding where and when

to litigate. The NAACP used a strategy starting well before *Brown* of filing lawsuits focused on establishing a series of favorable legal precedents, rather than maximizing short-term social welfare gains.

The legal strategy for the NAACP during its earliest years was to attack the principle of “separate but equal” established by *Plessy v. Ferguson* (1896) by challenging discrimination in graduate and professional schools.⁵ The primary motivation for focusing first on post-graduate education, rather than on K-12 schooling, was the perception of a greater probability of winning – even if the number of students who would be affected by desegregating post-graduate schools would be orders of magnitude smaller. Many states that refused to admit blacks to post-graduate and professional programs in their public universities did not have a separate segregated option. The NAACP sought to force states to either develop separate and equal options for minorities, which they doubted states could afford, or else to integrate their graduate programs [Williams, 1998, p. 76, 94, 174]. This strategy led to several key victories, which laid the groundwork for the *Brown* challenge (see Appendix A). Another benefit of focusing on graduate schools rather than on K-12 was to “bypass the inflammatory issue of ‘race-mixing’ among young children” [NAACP, 2004, p. 9].

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. 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].

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

⁵ This is based on the excellent discussion in NAACP [2004] and www.naacp.org/legal/history/index.htm.

did not achieve much desegregation (see Appendix A). 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. Our focus is mostly on these major local Federal court decisions following *Green*, which we show below helped desegregate schools.

Over time the process through which desegregation lawsuits were filed seems to have become even more idiosyncratic and decentralized (see Appendix A). 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 at least in part on the probability of success in the local Federal courts. There seems to have been considerable variability across lower Federal courts in how desegregation cases were handled [Klarman, 2004]. The belief that districts were “cherry picked” for desegregation lawsuits on the chances of winning seems to be widely held by lawyers even today.⁶

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,⁷ and suggests the importance of adequately controlling for region-specific trends in crime outcomes over time in our empirical analysis below.

III. CONCEPTUAL FRAMEWORK

We discuss the mechanisms through which court desegregation orders might affect crime within the framework of the “market for criminal offenses” from Ehrlich [1981] and Cook [1986]. The supply of

⁶ 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” (*NY Times*, Sunday, May 6, 2007, “Liberal Case for Gun Rights Sways Courts, p. A1, A18, by Adam Liptak).

⁷ 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 *de jure* segregation. As a result, desegregation became more viable in school districts outside of the south in which *de facto* segregation was present.

offenses increases as the “price” of crime (loot minus expected punishment) increases [Becker, 1968]. The “demand for offenses” slopes downward because crime prevention activities seem to increase with the crime rate [Ehrlich and Becker, 1972, Clotfelter, 1978, Philipson and Posner, 1996], which reduces the returns to crime. The model highlights several key points: it is hard to predict how court school-desegregation orders will influence either the supply or demand for offenses; effects on either supply or demand could be at least partially offset by behavioral responses on the other side of this “market”; and as a result, changes in crime rates may understate the social gains (or losses) from desegregation orders.

A. Supply-side factors

Court desegregation orders could reduce crime for purely mechanical reasons: By incapacitating children on long bus rides during the high-crime hours after school. More interesting is the possibility that school desegregation orders might change criminal behavior by exposing to children to higher-quality schools or peer groups and so improve their cognitive or non-cognitive skills.⁸ Guryan [2004] finds court-ordered desegregation reduces black dropout rates by 3 percentage points, or about 25 percent [see also Lutz, 2005 and Rivkin and Welch, 2006]. Lochner and Moretti [2004] suggest a schooling impact of this magnitude might reduce black homicide arrests by around 20 percent.⁹ These two studies taken together suggest that for every 100,000 black youth, court ordered school-desegregation should lead to 3,000 fewer dropouts and 6 fewer homicides. This is a large implied effect. By way of comparison, in 2005 the homicide rate in the U.S. was 6.1 per 100,000, and for blacks 15-19 was equal to 34.6.

⁸ The fact that many studies do not find large differences in measurable school inputs between blacks and whites does not rule out potentially important differences in school quality, given the weak correlation between most school characteristics and rates of student achievement. See also Boozer, Krueger and Wolkon [1992].

⁹ Dahl and Lochner [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.

While Guryan and Lutz do not find detectable changes in the dropout rates of whites, it is nonetheless possible that desegregation orders may still have improved their school quality. Some local Federal judges required school districts to increase overall school spending as part of their desegregation plans. Desegregation orders could also have led to “white flight” into higher-quality private schools, or into nearby public school districts that were not subject to desegregation orders.

Impacts of desegregation orders on non-cognitive outcomes could be at least as important as cognitive skills in understanding why criminal behavior may change. Better school environments for children could improve self control, anger management, or patience. Given the black-white gap in criminal offending, desegregation could reduce crime by minority youth by exposing them to less criminally involved peers. Even if there is limited social interaction between blacks and whites within schools [Clotfelter, 2002], desegregation could increase social cohesion within both race groups.¹⁰

Equally important are the potential effects of school desegregation on community-wide attitudes. School desegregation could have profound effects on the perceptions of self worth among minority children, a possibility suggested by Kenneth Clark’s [1947] famous “doll study” cited by the Supreme Court in *Brown*,¹¹ or influence perceptions among blacks of all ages about their opportunities for economic or social advancement in America. A large body of research in social psychology suggests that on net, intergroup contact between minorities and whites reduces intergroup prejudice and anxiety about

¹⁰ Consider reports from black students in Chesapeake, VA following desegregation in 1971-2: “They [whites] were always on one side of the hallway, like we were criminals or something.” As a result of this separation, black students decided they had to “support each other” and act as “support mechanisms” [Fowler, 1997, p. 98]. One white student recalls that the “blacks stood off by themselves, I guess they were as scared as we were” [Fowler, 1997, p. 99], while another student reported “everyone had their territory” [p. 124-5]. Black students would travel in groups to provide both “safety in numbers” and a “comfort zone” [p. 106]. White students “were careful about leaving [themselves] unprotected” [p. 124-5].

¹¹ As Clark later remarked: “What was surprising was the degree to which the children suffered from self-rejection, with its truncating effect on their personalities, and the earliness of the corrosive awareness of color. I don’t think we had quite realized the extent of the *cruelty* of racism and how hard it hit” [NAACP, 2004, p. 39, emphasis in original]. Clark talked about a boy in Arkansas he had tested, “when I asked [which doll he liked], he pointed to the white doll. And I asked him which one don’t you like, and he pointed to the brown-skinned doll. He was brown-skinned.” [Williams, 1998, p. 197].

intergroup interactions.¹² Court-ordered school desegregation also seems to have reduced self-reported racial intolerance among whites [Rossell, 1978]. Large Southern school districts desegregated mostly in the 1968 to 1972 period (see Figure 1), 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, which include measures of attitudes toward school desegregation,¹³ 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. Although we lack comparable measures of racial attitudes for blacks, it is reasonable to presume that the factors reducing white racial intolerance over this period may have had a similar effect on blacks. Moreover, the reduction in white prejudice may have itself altered black attitudes toward whites and about society more generally. Rossell [1978] notes that a few years after school desegregation in Boston, Charlotte-Mecklenberg, and Louisville, anti-busing candidates for the school boards were voted out of office, while blacks were voted in – despite the fact that blacks were a minority of voters in these areas.¹⁴

¹² The “contact hypothesis” from Allport [1954] claims intergroup contact should have positive effects on attitudes when there is equal group status within the situation around which the two groups are interacting, the two groups have common goals, there is intergroup cooperation, and the intergroup contact is supported by the authorities or custom [see also Pettigrew, 1998]. A recent meta-analysis of the available empirical results “clearly indicate that intergroup contact typically reduces intergroup prejudice” [Petigrew and Tropp, 2006, p. 766], and that the conditions emphasized by Allport increase the magnitude of the effect but are not necessary for the effect to arise. The meta-analysis is careful to account for publication bias, and notes that this evidence is not simply an artifact of the selection of racially tolerant blacks and whites into more integrated settings, since similar results have been found in cases where people have relatively little choice about their social setting, such as the desegregation of the Merchant Marine in 1948, the Philadelphia police department, or housing projects in New York City.

¹³ The racial intolerance measure is based on responses to five questions: (1) Do you think white students and Negro students should go to the same schools or to separate schools? (2) How strongly would you object if a member of your family wanted to bring a Negro friend home to dinner? (3) White people have a right to keep Negroes out of their neighborhoods if they want to and Negroes should respect that right; (4) Do you think there should be laws against marriages between Negroes and whites? (5) Negroes shouldn’t push themselves where they are not wanted. See Rossell [1978], p. 170, footnote 130.

¹⁴ These findings are also consistent with case studies of local school desegregation orders. For example, with the desegregation of public schools in Chesapeake, Virginia, the initial suspicion and hostility associated with school desegregation was mixed in over time with instances of new understanding across groups – within three to four years the school became “culturally mixed” [Fowler, 1997, p. 92-94]. One black female recalled that the first time white classmates visited her house they seemed “shocked that it was so nice,” and white students generally seemed to report positive impressions of the academic performance of their new black classmates [p. 159].

Perhaps the major concern with court school-desegregation orders is that they might increase racial tensions and violence between race groups, at least in the short term. Surveys conducted after *Brown* found that 15-25 percent of Southern whites endorsed the use of violence if necessary to preserve racially segregated schooling [Klarman, 2007, p. 192]. And in fact many cities experienced riots following their attempts to desegregate public schools.¹⁵

B. Demand-side factors

The net effects of school desegregation on crime will depend on the behavioral responses of potential victims as well as potential offenders. Consider, for example, Panel A of Figure 2. Suppose desegregation orders reduce the propensity of blacks to commit crimes, shifting inward the supply of offenses schedule from S_1 to S_2 . Potential victims may respond by reducing their own crime-prevention activities (i.e. the demand for offense schedule is downward sloping), which offsets part of the supply side effect. In the extreme, if the public adjusts behavior to preserve some target level of crime, any beneficial effects of desegregation on youth behavior need not change the crime rate at all (Panel B). The potential for offsetting behavior is symmetric: victim responses could also mute any *adverse* impact of court orders from increased racial tensions (i.e., from S_1 to S_3). 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].

Desegregation orders could shift the demand for offense schedule in either direction. School desegregation could reduce the net returns to criminal opportunities facing minority youth (shift the

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 Kentucky the governor ordered the prevention of desegregation with tanks, "taken along for the proper psychological effect" [Greenberg, 1994, p. 227]. 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] In 1970 in Lamar, SC, "buses filled with black children being transferred to previously all-white schools were met by a mob of white adults armed with ax handles, chunks of cement, and chains. The mob clashed with state troopers, and managed to turn two of the buses over, injuring several children and troopers" [Rodgers and Bullock, 1972, p. 92]. See Greenberg [1994] for other examples.

demand schedule inward, from D_1 to D_2) by increasing the amount of time they spend in parts of town that have higher-quality policing services, or where victims are more likely to report crimes to the police. Whether this change in the returns to schooling would translate into a reduction in the local crime rate depends on the degree to which youth crime responds to changes in incentives (Panel C versus Panel D). The same issue is relevant to whether the extra “loot” that might be available in the more affluent areas in which minority children now attend school (shifting demand out from D_1 to D_3) leads to more crime.

IV. 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, listed in Appendix Table A1 together with the year of their court desegregation order. 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.¹⁶ We seek to identify the effect of court-ordered school desegregation on youth crime in these districts.

Our main data sources are the Vital Statistics (VS) system of the U.S. and the FBI’s Supplemental Homicide Reports (SHR), aggregated to the county level. We focus on homicide because this is widely regarded as the most reliably measured crime, and accounts for a disproportionate share of the total social costs of crime [Ludwig, 2006]. We have also examined other types of crimes using data from the FBI’s Uniform Crime Reports (UCR). But these data are known to have a great deal of measurement error at

¹⁶ In 1968 these districts accounted for 45 percent of minority enrollment in the U.S. Because of the historically strong relationship between city size and per capita crime rate [Blumstein, 2000], these districts also account for a large share of all crime. In 1968 the counties containing our school districts accounted for nearly half of all homicides to blacks in the US as a whole, and just over one-third of all homicides to whites.

the county level [Maltz, 1999], and so in practice 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. We devote considerable 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 Figure 1 shows a large share of these orders were enacted by that time. But the SHR data extends through 2002, and so can be used to examine the effects of school desegregation orders on long-run homicide offending behavior. The SHR data show the race and age of homicide offenders is positively, but imperfectly, correlated with that of victims. For black 15-19 year old offenders, around one-fifth of victims were other blacks aged 15-19, one-fifth of victims were blacks 20-24, and a third of victims were blacks 25+. Around one-fifth of victims were

white.¹⁷ A reduction in the propensity of black youth to commit homicide will therefore likely reduce victimization rates for both older blacks and whites.

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. In this case, mismeasured white flight during inter-censal years would lead us to understate homicide rates for whites following desegregation (similarly, in-migration of blacks sparked by desegregation would lead us to overstate the homicide rate). Below we demonstrate this does not seem to be much of a concern in practice. With the SHR we calculate our offending rates two ways, first using the Census population data from the Vital Statistics as the denominator (which is the default for our results shown below), and then using data from the UCR on just the population living in jurisdictions that reported UCR crime data to the FBI. Both sets of results are qualitatively similar.

Table 1 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

¹⁷ For white offenders ages 15-19, around one-quarter of victims were also white 15-19 year olds, one-fifth were white 20-24 year olds, and over 40 percent were whites 25 and older. Most of the remaining victims were whites under 15 years of age; during our time period, whites rarely killed blacks, at least according to the national SHR data.

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, 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).

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

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 more pro-social peers or higher-quality schools, might have effects that depend on duration of exposure. In addition, court desegregation orders in some districts were phased in

gradually.¹⁸ Finally, some of the factors which might lead to an increase in 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.¹⁹ 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 average county. However the results are similar when we estimate the effects on the average juvenile instead, by re-estimating (1) weighting by each county's juvenile population.

We initially estimate equation (1) using OLS in levels, and calculate standard errors that are clustered at the county level to account for arbitrary forms of serial correlation [Bertrand et al., 2004]. It is not certain, however, that this is the correct functional form. As shown on Figure 3, we see substantial differences across counties in the cross section in homicide rates, especially for black youth, which might suggest a proportional effects model. But a standard log linear specification is complicated by the fact that many counties record no homicides to youth in some years.

¹⁸ The average school district in our sample phased in their initial court-ordered desegregation plan in approximately 1.5 years. Some districts had plans phased in over as long a period as 3 or 4 years. Twenty percent of the districts had a second court-ordered plan put in place after their initial plan.

¹⁹ 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.

In order to estimate a proportional response model using OLS, we first employ 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. While the method allows for estimation of a proportional response using a linear model, it is biased because the dummy variable is endogenous. We refer to this as the “log linear dummy 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 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. (The computer code to estimate this model is available upon request).

We also experiment with re-estimating (1) including county-specific linear trends, as well as a model which 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:

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

where X_i is the vector of time-invariant county characteristics measured as of the 1960 Census and λ_i 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.²⁰

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. Something happened that is worth studying, although we note that changes in school racial composition are only one mechanism through which these desegregation orders might potentially influence criminal behavior. Our empirical evidence suggests that these court orders on net reduce homicide victimization rates for blacks by around 25 percent. We estimate large reductions in homicide offending among black youth and adults that seem to persist over time. We find white homicide victimizations also decline by nearly 25 percent, although whether there is a change in white offending behavior is less clear.

A. Impacts on School Segregation

Court desegregation orders were intended to reduce the degree of public school racial segregation, with the hope that these changes would improve life outcomes for minority children. At the very least these court orders were successful in accomplishing the first objective.

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 composition.²¹ The figure plots the regression coefficients on our indicator variables for years before and

²⁰ 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.

²¹ The dissimilarity index is defined as:

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

after desegregation orders go into effect (the year before is the reference period), using OLS to estimate equation (1) with the dissimilarity index as the dependent variable and 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.²² 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 .28.²³ 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]. In any case, these results suggest there was an interesting “treatment dose” that resulted from desegregation orders.

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.

²² To see why, consider an extreme example in which virtually every white child in a school district moved out, leaving a single white child in each school – the dissimilarity index would in this case drop to zero, but black students would have almost no contact with white students.

²³ The extent of interracial contact within a school district is measured directly by the exposure index:

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

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.

B. Homicide Victimization

The results shown in Table 2 suggest black homicide victimization rates declined substantially following implementation of court school-desegregation orders. These results come from estimating a parsimonious version of equations (1) and (2) using homicide data from the Vital Statistics as the dependent variable of interest. The key explanatory variables 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 an order goes into effect. The first column of Panel A shows that for black youth of high school age (15-19), homicide victimization rates declined by 5.9 per 100,000 over the first five years following these court orders, which is equal to around 20 percent of the mean homicide victimization rate to blacks in this age range in our sample (equal to 29 per 100,000, Table 1). The coefficient on the indicator for 6+ years after the court orders is of about the same magnitude – 6.5 per 100,000 – suggesting the effect persists.²⁴

The results from proportional response models (columns 4 to 6 in Table 2) are probably more appropriate for our application, given the skew in homicide rates in our sample, and are more precisely estimated. In our OLS log dummy specification (column (6)) we estimate declines in homicide victimization rates to blacks 15-19 of around 8 percent during the first 5 years after these court orders go into effect, and 15 percent thereafter. Our preferred QML count model (column (4)) suggests larger proportional effects, equal to 17 percent the first 5 years after the court orders, and 27 percent thereafter.

As noted in Section IV, school-age homicide offenders often kill older people, and so in Panel B we expand our focus to black victims 15-24. Compared to the results for black victims 15-19, the estimated effects for victims 15-24 are larger in absolute terms (between 9 and 11 per 100,000 over the long term) but are roughly similar in size or slightly smaller in proportional terms, given the baseline

²⁴ A caveat to this conclusion should be noted. The final coefficient in the post-vector is identified from an unbalanced set of counties. Counties which desegregated early contribute more observations to its identification than do counties which desegregated later. The coefficient estimate may therefore partially reflect sample composition issues.

homicide rate for blacks is much higher for those 15-24 than 15-19 (45.2 vs. 29.0). Panel C of Table 2 shows that for blacks ages 25-34 victimization rates declined by 15 percent during the first five years after the court orders and by 29 percent thereafter (column 4). For blacks 35-44, victimization rates decline by a smaller proportional amount – 12 percent in the first 5 years and 16 percent thereafter (Panel D, Table 2). Part of the reduction in victimization rates to black adults is due to a decline in offending by black youth, since as noted above a large share of youth killers have adult victims, but part of this estimated effect also seems to reflect a decline in offending behavior by adults as well, as discussed further below.

Note that all of our estimates in Table 2 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 (see Table 2, columns (2), (5) and (3)).

Table 3 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, homicide victimization rates for whites 15-19 decline by around 2 per 100,000, approximately one-fifth of the control mean. We also see some signs that homicide victimization rates might have declined for older whites as well (victims ages 25-34 and 35-44), although for these older age 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 shows that for blacks

there is very little evidence of any pre-existing trend in homicide rates before desegregation orders go into effect for any of our age groups. When the desegregation orders are implemented, we see a break in trend. This is true for each of the age groups we examine. 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 somewhat less precisely estimated, are in the appendix. Our findings are generally similar whether we use our OLS levels and QML count models (shown), or the OLS log dummy model (unreported).

As shown in Figure 6, we also do not see any evidence of pre-existing trends in white homicide victimizations for any of our age groups before these desegregation orders go into effect. Compared to the results for blacks, for whites there appears to be more of a delay in when homicide 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. And of course the amount of exposure people of all ages have to court desegregation orders will increase with time.

C. Homicide Offending

Victimization data are only partially informative about the behavioral responses by blacks and whites to court school-desegregation orders. To examine this issue directly we examine data on homicide offenders from the SHR, which has the drawback of only providing information on offenders when the police identify a suspect or make an arrest. Another drawback is the SHR data are available only back to 1976, and so estimates for short-term effects will not use data from the roughly 75 percent of districts in our sample that enacted court orders before 1977 (Figure 1). We have greater statistical power to detect longer-term impacts on homicide offending.²⁵

²⁵ For example Figure 1 shows that most desegregation orders go into effect around 1968 or later. This means that if we examine homicide offending behavior for whites and blacks measured 10 years after court orders go into effect, then our estimates would use data on almost all of the districts in our sample.

With these qualifications in mind, Table 4 provides evidence for a decline in homicide offending by high-school aged blacks (15-19) after court desegregation orders go into effect. (To see the results of truncating the panel, in column 1 we replicate our VS victimization results using data from just 1976 forward). OLS estimates show a reduction in black homicide arrests of nearly 7 per 100,000 during the first five years following the court orders, and a decline of nearly 13 per 100,000 thereafter (Panel A, column 2). By way of comparison, recall that the findings of Guryan [2004] and Lochner and Moretti [2004] taken together suggest the decline in black dropout rates caused by these court orders would be predicted to lead to 6 fewer homicides per 100,000 youth.

Our QML count model also implies large reductions in homicide arrests to black youth, equal to 37 during the first 5 years after the court orders and 59 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. 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. In any case the effect among the one-quarter of our sample that desegregates after 1977 are of about the same size as those from the Moving to Opportunity experiment, which found that moving low-income minority youth to less disadvantaged neighborhoods reduced violent crime arrests by 38 percent the first five years after random assignment [Kling, Ludwig and Katz, 2005].²⁶ Ludwig and Kling [2007] find that

²⁶ Kling, Ludwig and Katz [2005] find that while MTO moves cause a reduction in violent crime arrests for youth in the mobility treatment versus control groups, for both male and female youth, 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

racial composition seems to be the most important neighborhood attribute in affecting violent-crime arrests in MTO. These are large changes, but then, large changes in this outcome are not uncommon. As noted above, homicide arrest rates to blacks 14-24 nationwide tripled from 1984-1993 and then dropped by half the next seven years [Levitt, 2004, p. 180].

Column (3) of Table 4 suggests the rate at which blacks commit murder against whites may also have declined, despite the fact that desegregation increases the proximity and interactions of blacks and whites. While the desegregation effects for black-on-white offending rates are somewhat imprecisely estimated, they are nonetheless large relative to our estimated declines in white homicide victimization. To improve our statistical power, we expand the age range we are considering to examine homicides committed by blacks ages 15 to 24 (columns (4) – (6)). The main OLS results for the effects of desegregation orders on black homicide arrests in years 1-5 are smaller for the 15-24 age range (column 5) than 15-19 year olds (column 2), consistent with the idea that 20-24 year olds are mostly untreated during the first five years after these court orders. But the added power from expanding the age range to 15-24 in the period 6+ years after the court orders, when most of this group would now have spent time in desegregated schools, improves our ability to detect some reduction in black homicide offenses against white victims. The 95 percent confidence interval around our estimate raises the possibility that a large share of the reduction in white homicide victimizations could be due to reductions in offending by blacks.²⁷ Evidence for a short-term effect on white homicide offending is not very compelling in Table 4.

costs of violent crime is so much higher than for property offending that even if there is some offsetting effect to increase property crimes, holding distributional considerations aside, MTO shows that on net the effect is to substantially reduce the social costs of youth crime offending.

²⁷ Focusing on offenders ages 15 – 24 for simplicity, column (5) of Table 4 shows an estimate for the effect on homicides by blacks 15-24 against whites 6+ years after the court orders equal to -4.4 per 100,000, with a standard error of 2.3. The average county in our sample contains 20,834 blacks 15-24 years of age (Table 1), so our point estimate suggests -0.9 fewer homicides by black teens and young adults against whites in the typical county, while the confidence interval includes an impact as large as around -1.9. The reduction in homicide victimizations to whites 15-24 is around 2 per 100,000 (Table 3), and the average county in our sample has around 99,149 whites 15-24 years old (Table 1), so on average desegregation orders cause about 1.8 fewer white homicides per county.

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. Our QML count model suggests that within the first five years after desegregation orders are enacted, homicide arrests declined by 26 percent for blacks age 25-34 and by 31 percent for blacks age 35-44, and for the latter group there seems to be a decline in offending against whites as well. These effects seem to persist, as suggested by the fact that the coefficients for the period 6+ years after desegregation orders are about as large as the year 1-to-5 effects. These findings are consistent with Rossell's [1978] claim that school desegregation generates community-wide social changes, discussed above and also further below.

Table 6 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 in the first three columns 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 attended desegregated schools (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. This comparison captures just the direct effect of attending desegregated schools – the indirect effect of court orders in changing community attitudes will influence both the “treatment” and “comparison” observations and is differenced out. Column (3) shows that for blacks of school age when the court orders were enacted, the QML count model provides evidence for a decline in homicide offending at age 35-44 of 14 percent

(Panel A).²⁸ The OLS models are imprecise, but the large number of zero counts²⁹ leads us to strongly prefer the count model. The count model also suggests white homicide arrests decline 16-18 percent.

Column 4 of Table 6 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 the idea that any indirect community-wide effect on homicide is net out when comparing cohorts who were already adults when desegregation orders were enacted in their districts. 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 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, since 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. It is also interesting to note that the estimated reduction in Table 6 in homicide offending among 35-44 year olds who attended

²⁸ One minor complication with these estimates is that 2.5% of the county-year observations in the SHR from 1976 to 2002 have not yet been subject to a desegregation order and hence have not been exposed to the society wide treatment (what we are calling the “indirect effect”). This potential issue is handled through unreported results which limit the sample to those districts that were subject to school desegregation orders in 1976 or before. The sample restriction increases the QML point estimate by around 1/3 for both blacks and whites, and the estimates remain precise.

²⁹ Zeros account for around twenty percent of the black 35-44 homicide offending observations. The mean number of homicides for this age group is 5.4 per 100,000.

desegregated schools, -14 percent, is almost exactly equal to the difference in Table 5 between the estimated reduction in homicide arrests to 15-24 year olds 6+ years after the court orders are enacted minus the estimated effect during years 1-5. It may be that the short-term reduction in homicide offending among black youth in Table 5 largely captures the impact from a change in community attitudes. The fact that the coefficients for the year 1-5 effect in Table 5 are roughly similar in size supports this interpretation, as does Guryan's [2004] finding of a large reduction in black dropout rates even during the first year after court school-desegregation orders are enacted. Factors associated with actually attending a desegregated school, such as better classroom instruction or higher-achieving or more affluent peers, would be expected to take time to influence behavior.

D. Robustness and Falsification tests

Are the results that we estimate for homicide victimization 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 have also shown that our results are not very sensitive to conditioning on base-year demographic characteristic-year interactions, or county-specific linear trends. 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.³⁰

As another check on omitted variables concerns, we examined whether there is any relationship between the politics of the local federal judges in each district³¹ 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 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 it might lead to gains, which would lead us to understate black homicide reductions).

To address this concern, in Table 7 we re-calculate our estimates for homicide victimization rates from the Vital Statistics data, but now restrict ourselves to the decennial census years 1960 through 1990. The results for blacks are qualitatively similar to those we see in our full county-year panel, but obviously

³⁰ 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.

³¹ 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

much less precisely estimated. The OLS results for whites are qualitatively similar to the results from the full data while the QML estimates are smaller in absolute value using data from just decennial census years compared to the full panel.

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 8 we estimate equation (1) using as the dependent variables the log of the county population of 15 to 19 year old whites or 15 to 19 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. The point estimates are small and not statistically significant.

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 census region. 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*³² – as opposed to the *counties* used in this paper.^{33,34} Presumably much of the in-migration by blacks into urban school districts in BSL must be coming from inner suburbs within the same counties.

As another check on whether our findings are driven by compositional changes in county population, we use decennial Census data from 1960, 1970, 1980 and 1990 to estimate the impact of

³² There is a high degree of overlap between the school districts in the BSL sample and the school districts used by this paper to identify desegregated counties.

³³ 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).

³⁴ 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.

desegregation on county demographic characteristics (Table 9). 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, 2007].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 a final check on the possibility that our findings are driven by cross county migration, we recalculate our estimates using MSA-year as the unit of observation (Table 10).³⁶ If our main results shown above 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. The 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 is to examine whether school desegregation orders have an “effect” on outcomes that should logically not be affected, which also of course serves as a more general robustness check of our results against the possibility of omitted variable bias. Table 11 presents the results from such a falsification exercise. We estimate the effect of

³⁵ The demographic variables chosen and the use of the non-white category (as opposed to 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 resides in a desegregated county and the remainder resides in other counties within the MSA. For whites age 15 to 19, the comparable figure is 75 percent.

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

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 12. We use data on government spending from the Census of Governments for the years 1972, 1977, 1982, and 1987, and then estimate equations (1) and (4) using OLS where the dependent variable is 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 (1999 dollars) following court desegregation orders (Panel A), about 6 percent of the sample mean of \$2,750. The implications for criminal behavior from this sort of change in school spending are not entirely clear.³⁸

In contrast 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

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).

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.

could be effective [Sherman, 2002], although we have no way to test this hypothesis. McCrary [2007] finds that court-ordered changes in the racial composition of police departments does not seem to have much impact on crime, so that does not seem like a serious counter-explanation for the results presented here. To show the education spending result is not simply an artifact of some spurious correlation between desegregation orders and overall local public spending, in panel C we show that the estimate for spending on fire protection is never significant and always a small share of the mean of \$55 per capita.

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. 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. 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 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 13 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 desegregation order) with changes in the exposure and dissimilarity indices at the same time, the former seems to be driving the result.³⁹ The fact that we observe the largest impacts on black homicide where we observe the largest “treatment dose” from desegregation orders provides additional support for the credibility of our research design. For whites (Table 14), the largest decline in victimizations occur in counties where spending on schools or police increased most.^{40,41}

Since our estimates rely on studying desegregation orders that went into effect during the period from the late 1960s through early 1980s, there is naturally a question of whether and 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 time heterogeneity in desegregation treatment effects (unreported).

Another way to explore this issue is to see whether the specific design features of the plan influence the effect on crime. Welch and Light [1987] provide a useful typology of the types of desegregation 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

³⁹ 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.

⁴⁰ As noted above, our results are not precise enough to rule out a significant increase in police spending. Given the violence associated with court-ordered desegregation discussed in Section III, it is quite plausible that desegregation led to an increase in police spending in some locations.

⁴¹ Reber [2007] studies desegregation plans in Louisiana and finds that increased school spending seems to be a more important factor in explaining improved black student outcomes than does increased exposure to white students. But the pattern she finds in Louisiana – where 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, where we see no detectable relationship between minority composition and changes in school spending.

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.

Alternatively it is possible that the effects of school racial desegregation depend on the disparity in socio-economic status between race groups. 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 have 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.

A direct way to test whether our results are due to busing, or to the fact that youth are spending more time in different types of neighborhoods around their new schools, is to use SHR data on month-of-offense to examine effects on homicides over the summer months versus during the academic year. Table 15 shows that the estimated effects are about as large for homicides that occur over the summer compared to when school is in session. These results, together with those presented above suggesting long-term effects on offending even into adulthood for cohorts of students exposed to school desegregation orders, suggest that desegregation effects on persistent cognitive and non-cognitive skills must be an important part of the explanation for our results.

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 (see Table 8), 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 16

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.

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 2002, and Baum-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 14, 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 16).

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. These estimated effects are in line with Guryan's [2004] estimates of large proportional effects of these court orders on dropout. In fact, Guryan's estimates help explain a large share of our estimated reduction in homicide arrests to black youth, based on Lochner and Moretti's [2004] findings on how increased schooling reduces criminal behavior. Consistent with this mechanism, we find that the effects on homicide arrests for black youth are about as large during the summer months as during the school year, and seem to persist as cohorts exposed to desegregated schools age.

Homicide victimization and offending also seems to decline shortly after desegregation orders are enacted among black adults. The fact that we see no pre-existing trends in homicide for black adults, and that other causes of death among adults such as from disease are not affected by school desegregation orders, makes us believe that these are real behavioral effects. Community-wide attitudinal changes seem to be the most plausible hypothesis, consistent with evidence that school desegregation orders seem to reduce racial intolerance [Rossell, 1978] and with Guryan's [2004] finding that dropout rates declined by very large amounts even within the first year following a court order.

White homicide victimization rates decline by 15 to 20 percent as well as a result of court desegregation orders, which likely reflects at least in part a reduction in the rate at which blacks kill whites and seems to be correlated with increased school and police spending.

The size of our estimates, particularly for blacks, raises the natural question: Could desegregation orders 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 some implications for understanding why trends in black criminal offending 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 black youth 15-24 by around 13 percent and lowered the rate to white youth 15-24 by around 7 percent, and might account for around one-quarter of the convergence in black-white homicide rates from 1970 to 1980.⁴⁴ It is possible that the termination of many school desegregation orders could have contributed to the halt of the massive American crime drop of the 1990s [Levitt, 2004, Clotfelter, Ladd and Vigdor, 2005, Lutz, 2005].

These findings also have implications for contemporary debates about efforts to reduce the amount of racial segregation in our public schools. Although the Supreme Court recently 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" (see Appendix A). Legislatures and other policymakers may also consider various education or housing policies that could accomplish racial desegregation of schools through a variety of means, which might be guided in part by benefit-cost considerations.

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. In any case, 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. Our estimates

⁴⁴ Table 1 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 2 shows that court-ordered desegregation reduced homicide rates to blacks 15-24 by around 11 per 100,000, and our counties account for around half of all black homicides nationwide. Table 3 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.

thus imply benefits of nearly \$1,000 per black student and nearly \$200 per white student from reductions in homicide. These are large changes relative to, say, the average total per-pupil spending in the public schools in our sample, which as noted above equaled \$2,750. Of course a proper benefit-cost analysis should consider other impacts as well, such as the consequences of changes in residential patterns within American metropolitan areas [Baum-Snow and Lutz, 2008]. But our findings at least 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.

Our findings also reinforce the general potential for social policy as a tool for crime control. As noted above, our results are consistent with the violent crime impacts from the MTO residential mobility experiment. Moreover the present study can help to at least indirectly answer a question that research on MTO movers cannot – namely, what happens to the behavior of those youth already in the destination schools and neighborhoods. Our results suggest that if there are any adverse effects on violence by youth who were living in the receiving areas, they seem to be outweighed by the beneficial effects on the movers. Our results are also consistent with those from the early childhood literature suggesting that effective human capital interventions can prevent criminal behavior [Garces et al., 2002, Schweinhart et al., 2005]. 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.

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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. One of the first 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 *de jure* segregation. 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 even more 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.”⁴⁵ 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 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.⁴⁶ 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

⁴⁵ Personal communication, Jens Ludwig with Jack Greenberg, July 5, 2007.

⁴⁶ www.civilrightsproject.ucla.edu/policy/legal_docs/cover.pdf.

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.⁴⁷

⁴⁷ 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 black 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 Lutz 2005 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 2002.

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, and are linearly interpolated for the intercensal years with some adjustments made by the Census Bureau for migration and births and deaths. 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. 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 the population sub-groups affected by school desegregation, i.e. youth, 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.⁴⁸

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.

⁴⁸ 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 12, 13 and 14) 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 9) 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):

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

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).

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

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 $\psi=1$, 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 p th year of school desegregation.⁴⁹ 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.

⁴⁹ 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 recalculate 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 10. 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 quite similar to the standard estimates, 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 $t = 0$

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

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

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

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 has means in all periods equal to the control group, $i = 0$

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

then

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

and

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

Figure 1
Desegregation Implementation Dates

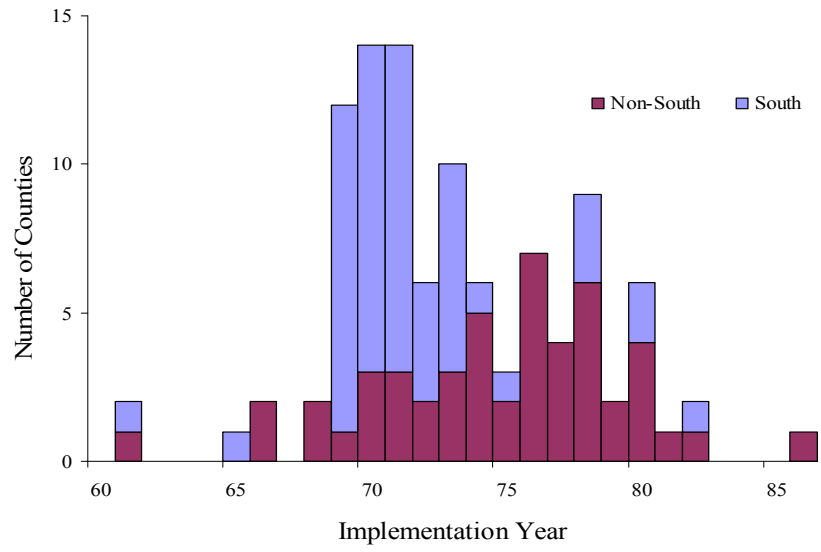


Figure 2

Potential effects of court-ordered school desegregation on “supply” and “demand” schedules in the “market for crime”

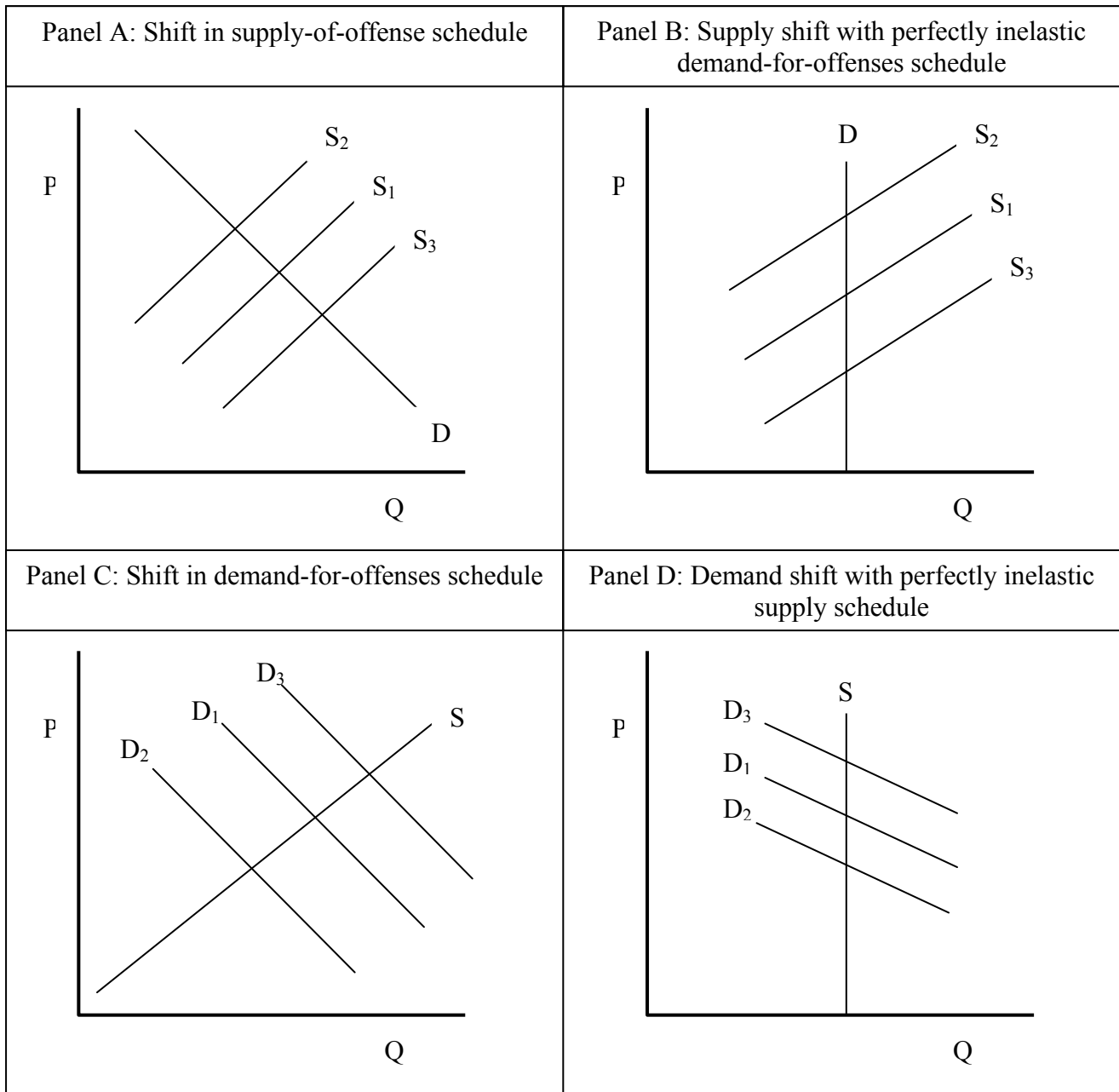
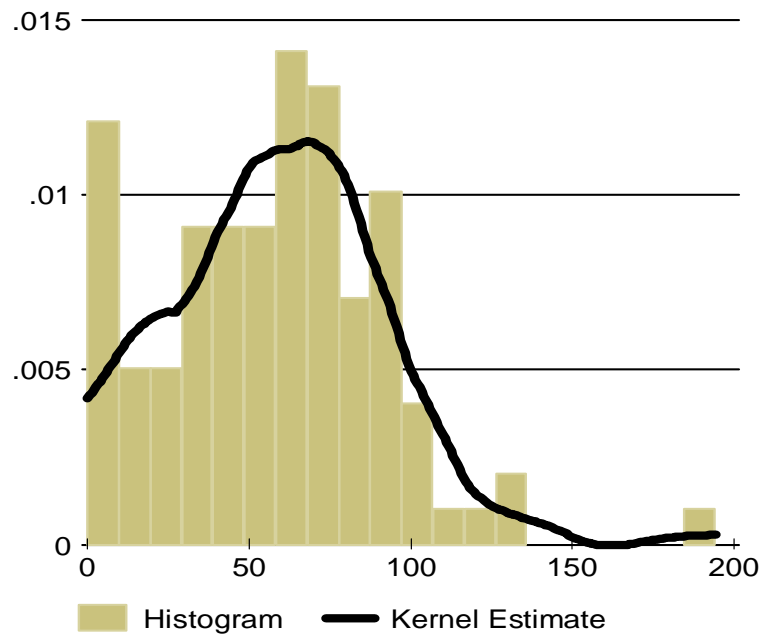
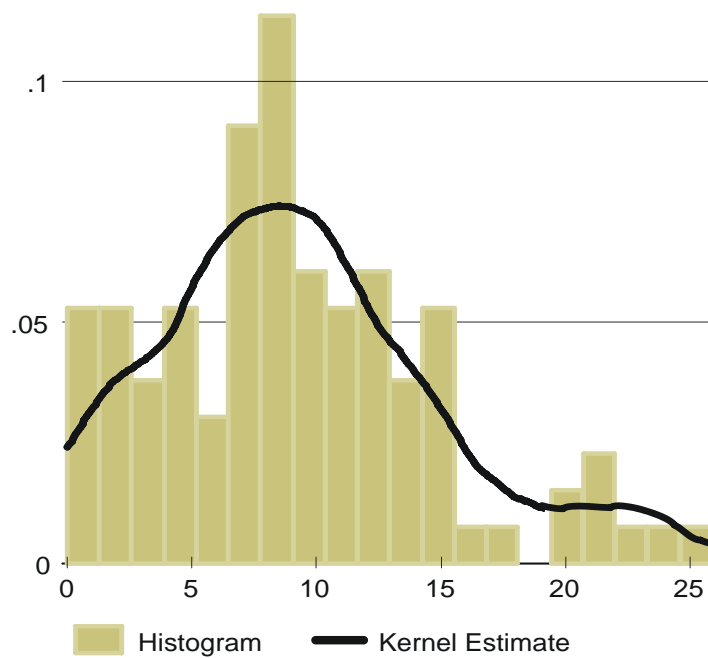


Figure 3

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



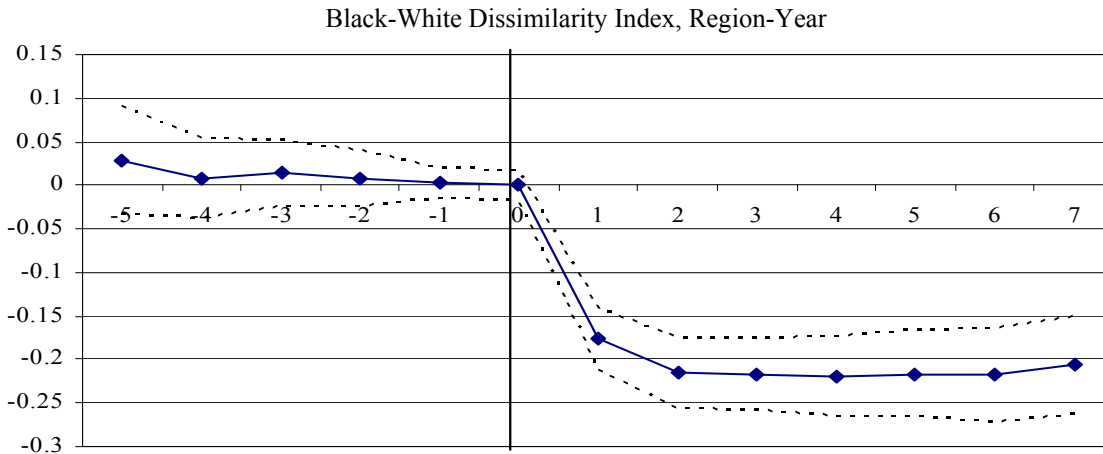
B. Distribution of 1975 White Age 15 – 24 Homicide Rates per 100,000



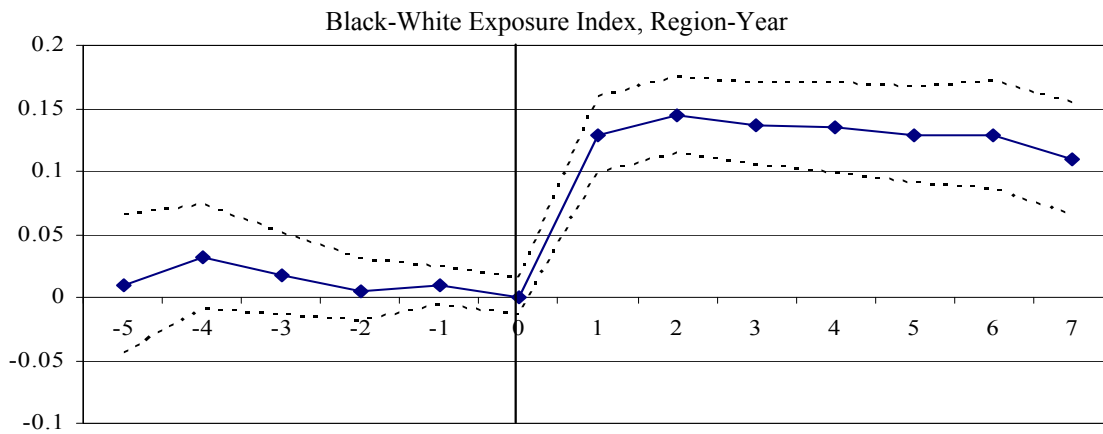
Note. The figures displays histogram and kernel density estimates of the 1975 age 15 – 24 homicide rate per 100,000. The kernel density estimate uses a Epanechnikov function and a bandwidth of 1.2. The sample is restricted to the counties in the Welch and Light (1987) sample with a major desegregation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

Figure 4: Effects of court desegregation orders on school segregation indices

Panel A:



Panel B:



Panel C:

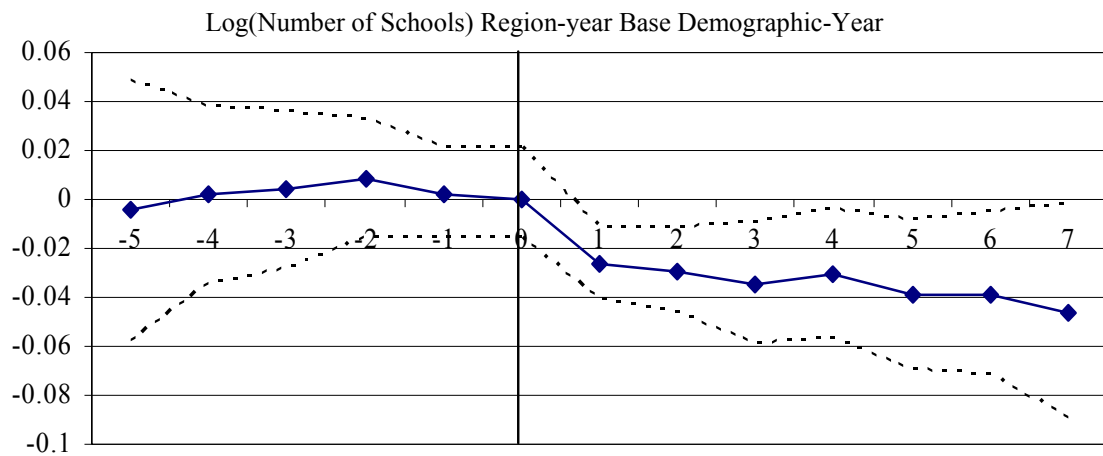
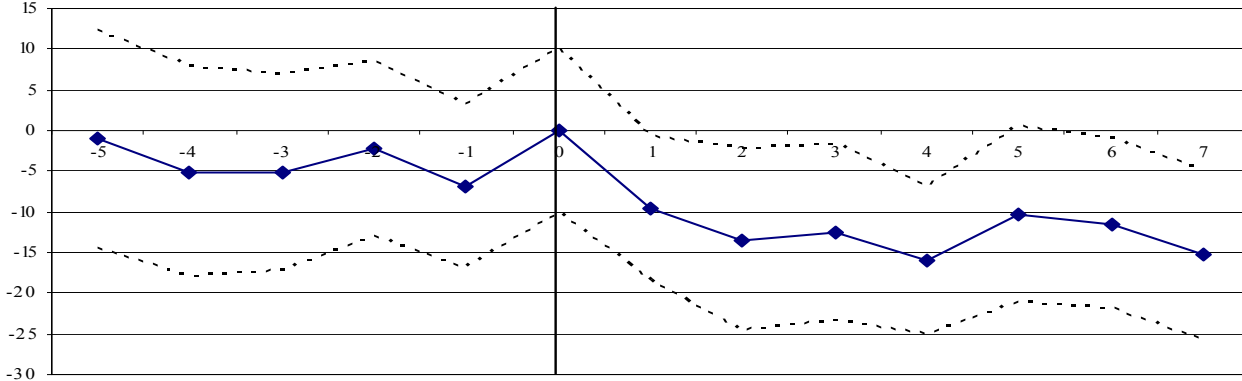
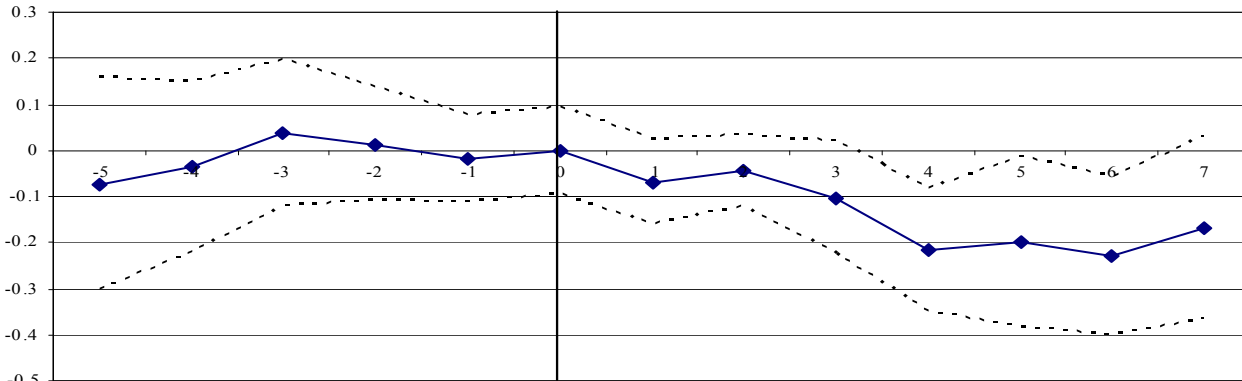


Figure 5: School Desegregation & Black Homicide Victimizations

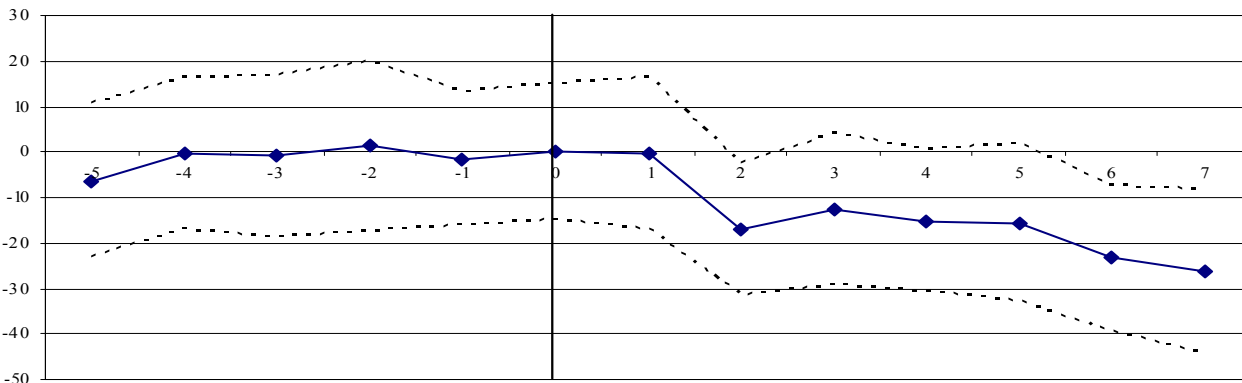
Panel A: Age Cohort 15-24 OLS level



Panel B: Age Cohort 15-24 QML count



Panel C: Age Cohort 25-34 OLS level



Panel D: Age Cohort 25-34 QML count

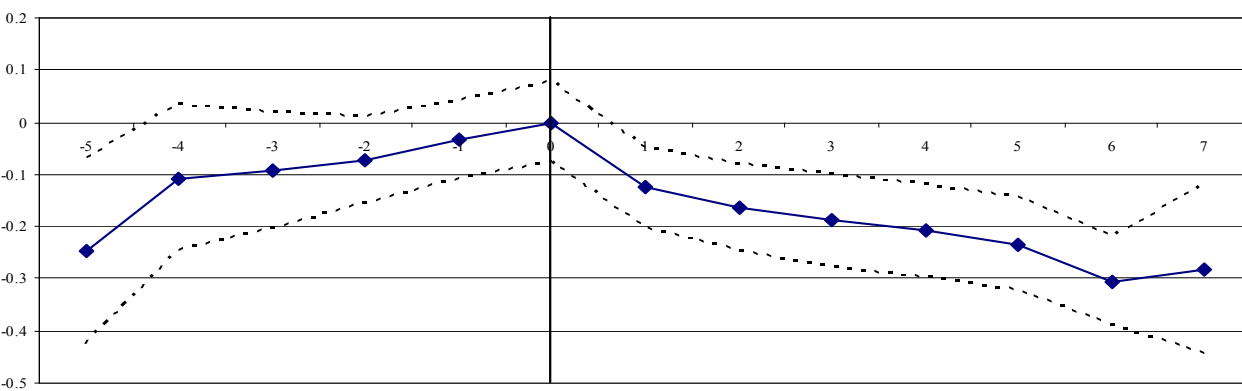
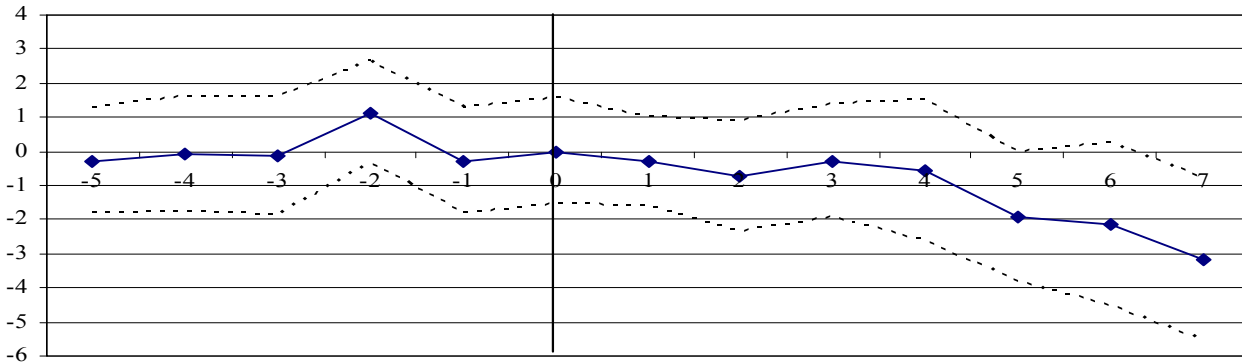
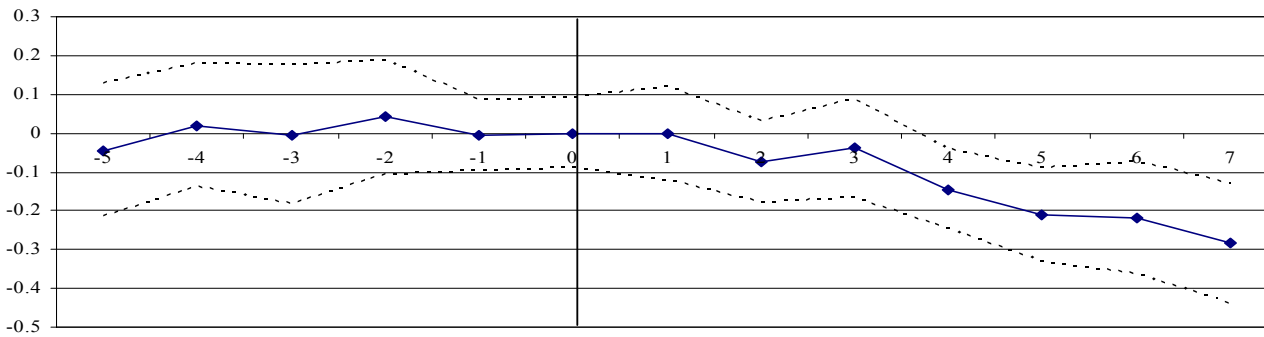


Figure 6: School Desegregation & White Homicide Victimizations

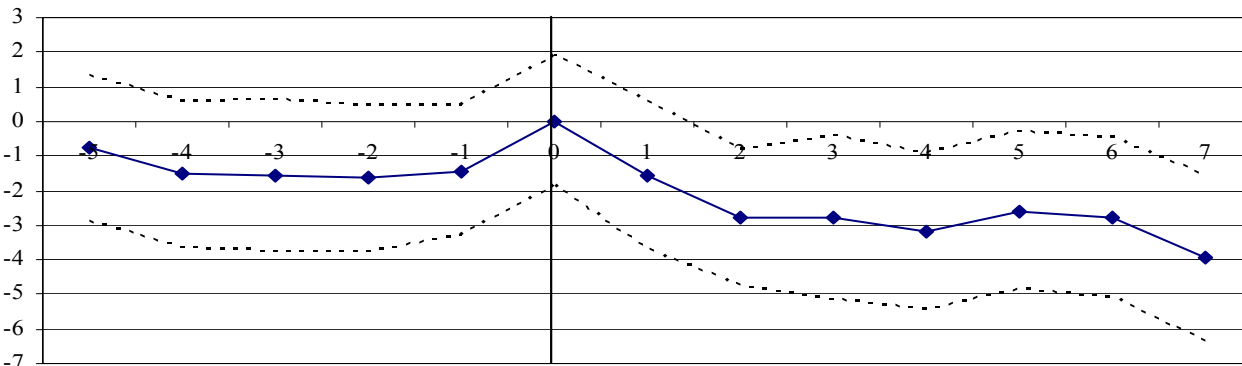
Panel A: Age Cohort 15-24 OLS level



Panel B: Age Cohort 15-24 QML count



Panel C: Age Cohort 25-34 OLS level



Panel D: Age Cohort 25-34 QML count

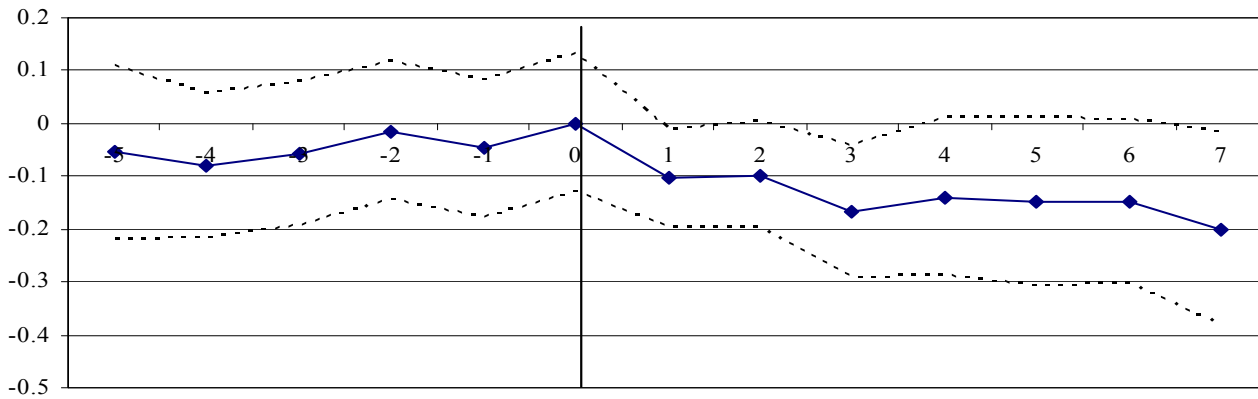
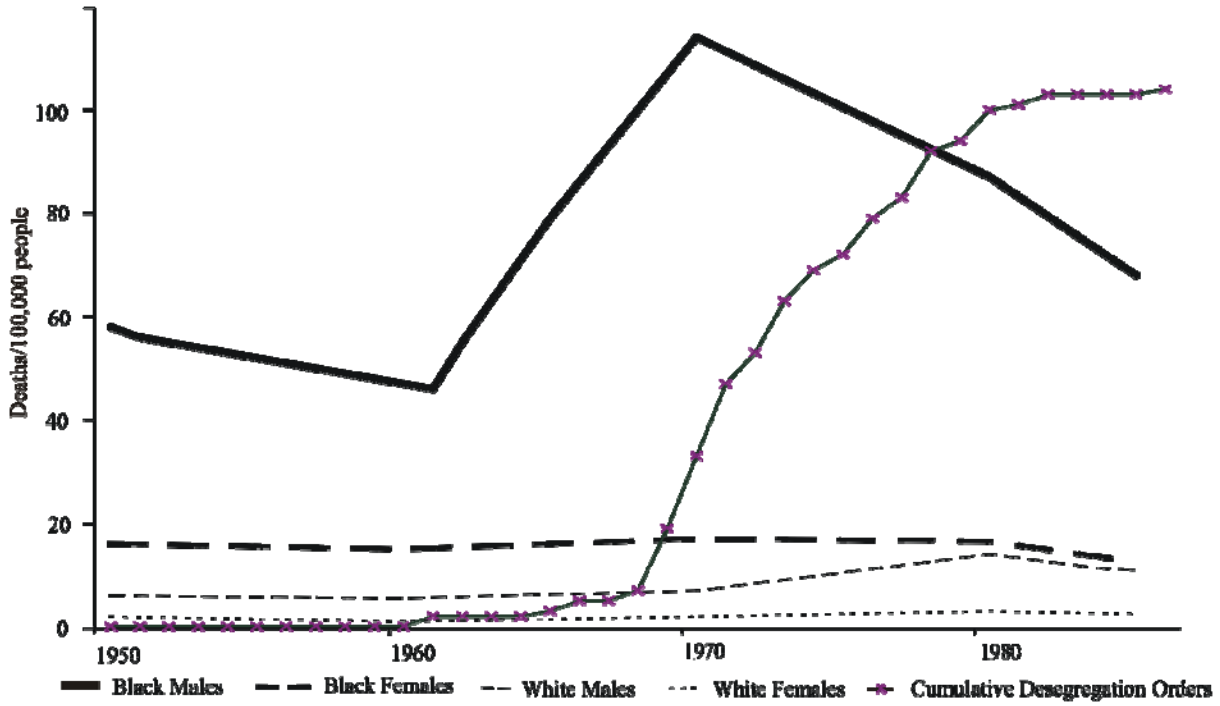
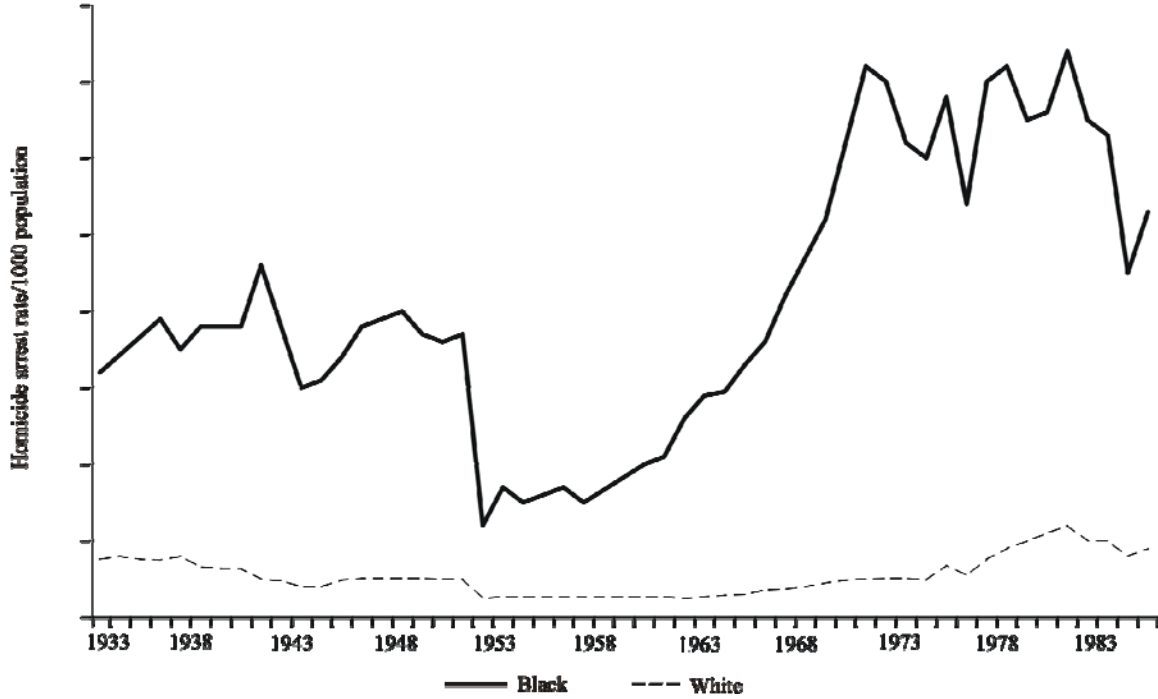


Figure 7 Historical Homicide Trends

Panel A: Homicide rates for people aged 15-24



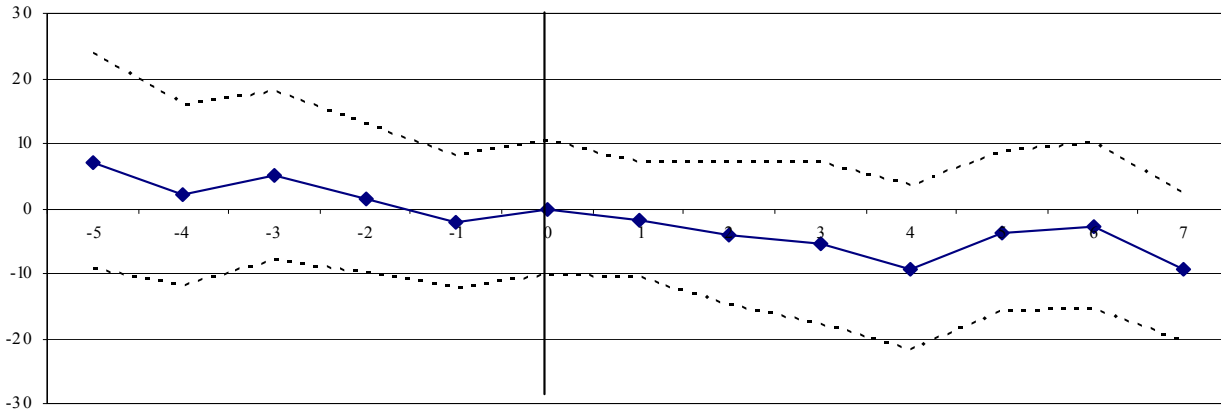
Panel B: Homicide arrest rates, by race, 1933-1985



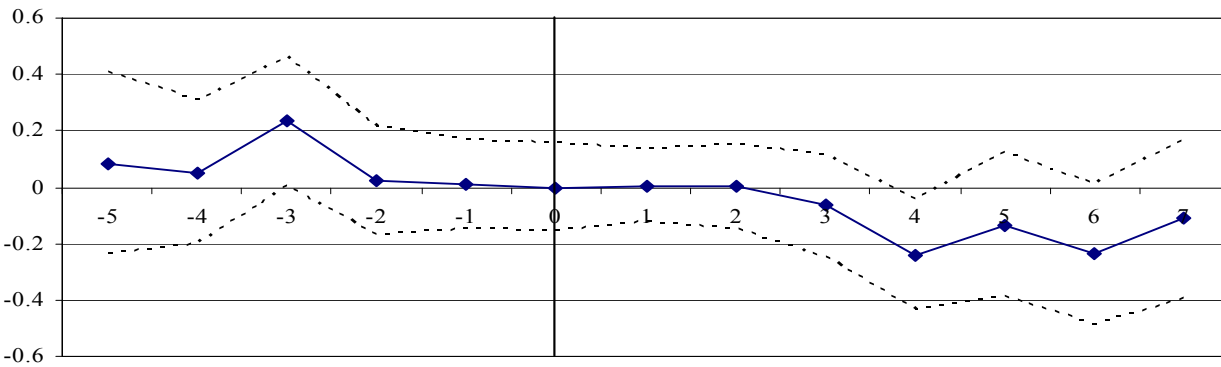
Source: Jaynes and Williams (1989), pp. 458-9.

Figure A1 School Desegregation & Black Homicide Victimizations

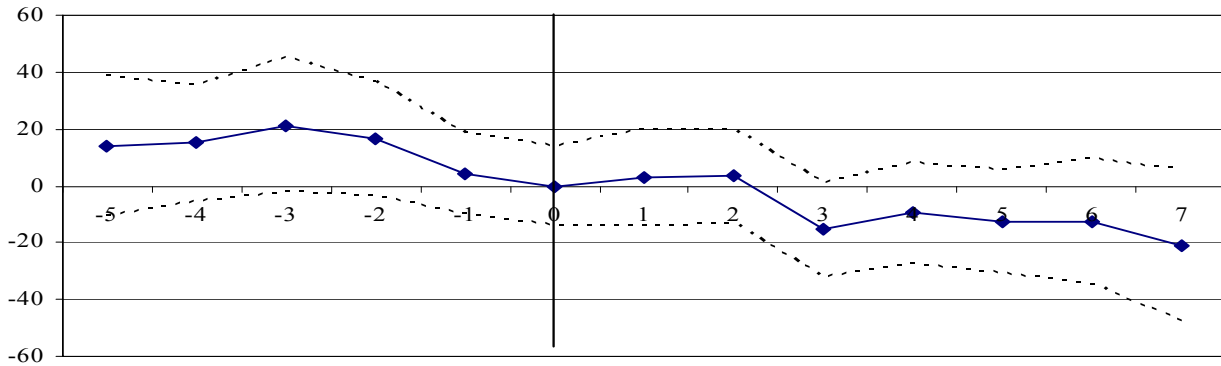
Panel A: Age Cohort 15-19 OLS level



Panel B: Age Cohort 15-19 QML count



Panel C: Age Cohort 35-44 OLS level



Panel D: Age Cohort 35-44 QML count

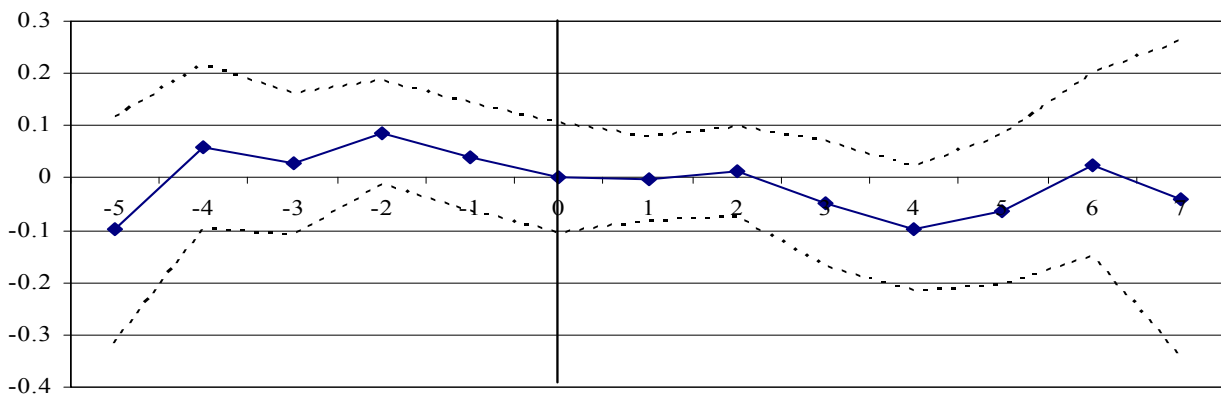
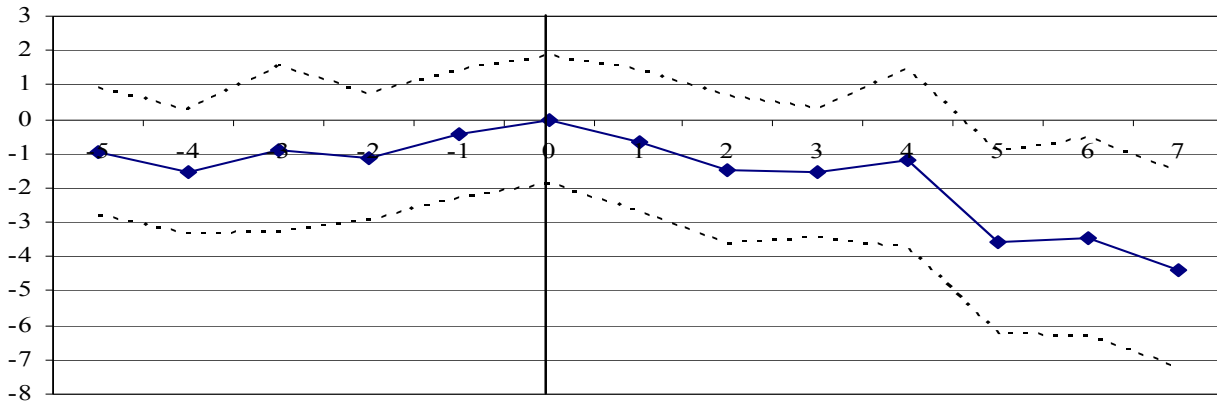
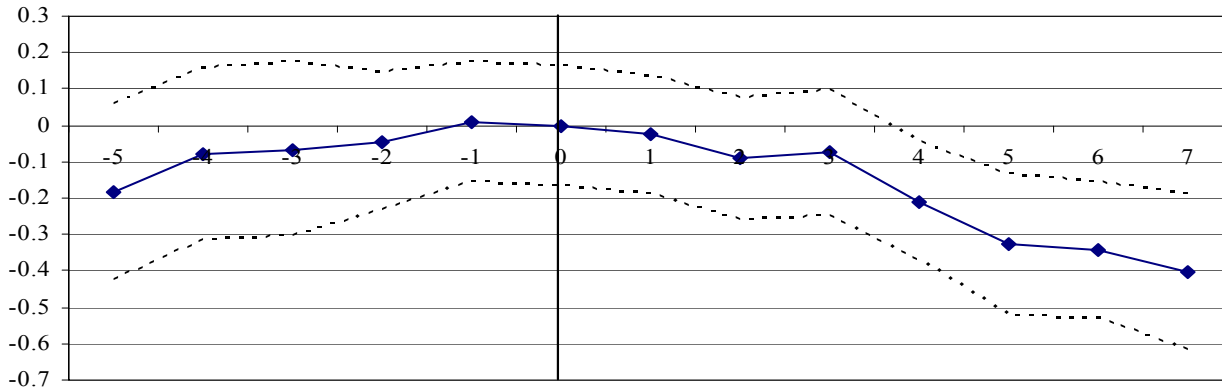


Figure A2 School Desegregation & White Homicide Victimizations

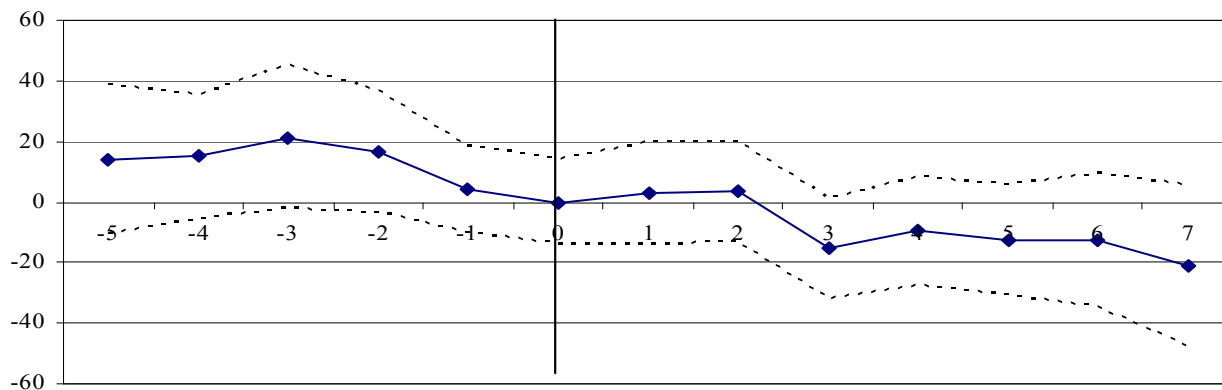
Panel A: Age Cohort 15-19 OLS level



Panel B: Age Cohort 15-19 QML count



Panel C: Age Cohort 35-44 OLS level



Panel D: Age Cohort 35-44 QML count

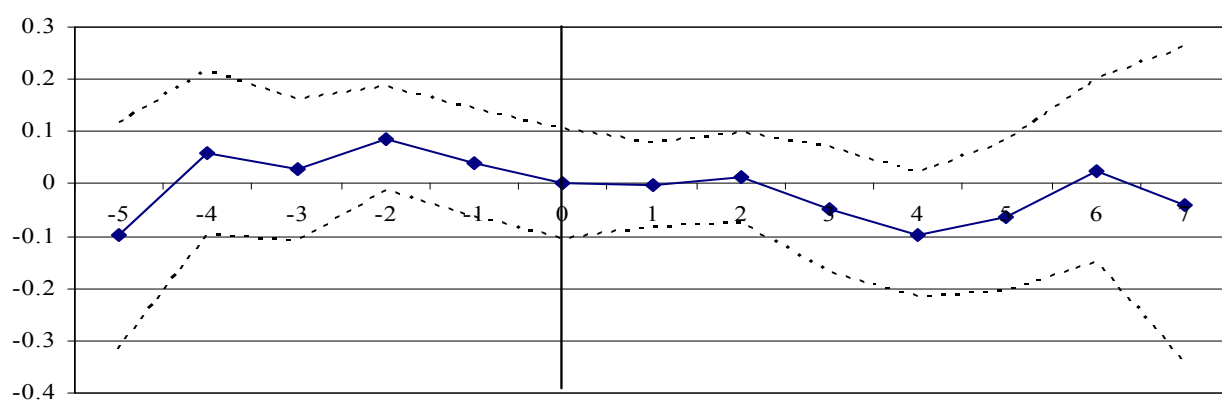


Table 1
Descriptive Statistics

	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 2
Black Homicide Victimization

	Levels			Proportional Response		
	OLS			QML Count		OLS Log Dummy
	(1)	(2)	(3)	(4)	(5)	(6)
A. Age 15 - 19						
Post Desegregation Years 1 - 5	-5.89 (2.86)	-5.05 (2.84)	-5.14 (3.01)	-0.17 (0.07)	-0.16 (0.07)	-0.08 (0.05)
Post Desegregation Years 6+	-6.52 (3.93)	-5.71 (3.87)	-6.26 (4.00)	-0.27 (0.09)	-0.28 (0.09)	-0.15 (0.07)
B. Age 15 - 24						
Post Desegregation Years 1 - 5	-8.91 (2.76)	-7.45 (2.58)	-8.59 (2.85)	-0.14 (0.04)	-0.11 (0.04)	-0.13 (0.05)
Post Desegregation Years 6+	-10.55 (3.81)	-9.32 (3.58)	-11.27 (3.69)	-0.23 (0.06)	-0.21 (0.06)	-0.19 (0.08)
C. Age 25-34						
Post Desegregation Years 1 - 5	-10.90 (4.90)	-9.54 (4.88)	-9.68 (5.21)	-0.15 (0.04)	-0.11 (0.03)	-0.09 (0.05)
Post Desegregation Years 6+	-23.61 (6.30)	-21.68 (6.36)	-21.54 (6.97)	-0.29 (0.05)	-0.21 (0.04)	-0.18 (0.07)
D. Age 35-44						
Post Desegregation Years 1 - 5	-12.28 (4.82)	-12.37 (4.86)	-10.04 (4.86)	-0.12 (0.05)	-0.12 (0.05)	-0.10 (0.05)
Post Desegregation Years 6+	-20.47 (9.10)	-20.52 (7.92)	-15.74 (8.26)	-0.16 (0.10)	-0.15 (0.08)	-0.16 (0.07)
Number of observations	3039	3039	3039	3039	3039	3039
Region * Year Effects	X	X	X	X	X	X
1960 County Charact. * Year Effect		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 rate per 100,000 in columns (1) - (3) and (7) and the homicide count in columns (4) and (5).

Table 3
White Homicide Victimization

	Levels			Proportional Response		
	OLS			QML Count		OLS Log Dummy
	(1)	(2)	(3)	(4)	(5)	(6)
A. Age 15 - 19						
Post Desegregation Years 1 - 5	-0.48 (0.50)	-0.38 (0.51)	-0.49 (0.53)	-0.05 (0.06)	-0.01 (0.05)	-0.07 (0.04)
Post Desegregation Years 6+	-2.22 (0.82)	-2.24 (0.80)	-2.23 (0.87)	-0.23 (0.09)	-0.20 (0.08)	-0.24 (0.07)
B. Age 15 - 24						
Post Desegregation Years 1 - 5	-0.49 (0.41)	-0.52 (0.42)	-0.43 (0.40)	-0.05 (0.04)	-0.02 (0.04)	-0.07 (0.05)
Post Desegregation Years 6+	-2.20 (0.72)	-2.22 (0.66)	-1.97 (0.68)	-0.18 (0.06)	-0.15 (0.06)	-0.24 (0.07)
C. Age 25-34						
Post Desegregation Years 1 - 5	-1.07 (0.59)	-1.04 (0.61)	-1.01 (0.62)	-0.04 (0.05)	-0.01 (0.05)	-0.10 (0.05)
Post Desegregation Years 6+	-1.57 (0.76)	-1.47 (0.73)	-1.33 (0.83)	-0.06 (0.07)	-0.03 (0.06)	-0.14 (0.07)
D. Age 35-44						
Post Desegregation Years 1 - 5	-0.29 (0.68)	-0.50 (0.60)	-0.18 (0.73)	-0.06 (0.05)	-0.05 (0.05)	0.00 (0.05)
Post Desegregation Years 6+	-1.27 (0.74)	-1.59 (0.72)	-0.97 (0.85)	-0.12 (0.06)	-0.11 (0.06)	-0.06 (0.06)
Number of observations	3040	3040	3040	3040	3040	3040
Region * Year Effects	X	X	X	X	X	X
1960 County Charact. * Year Effect		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 rate per 100,000 in columns (1) - (3) and (7) and the homicide count in columns (4) and (5).

Table 4
Supplemental Homicide Report Data: Homicide Offenders

	Age 15 - 19			Age 15 - 24		
	Vital Statistics	Supplemental Homicide Report		Vital Statistics	Supplemental Homicide Report	
	Victim	Offenders	Offenders Against Opposite Race	Victim	Offenders	Offenders Against Opposite Race
	(1)	(2)	(3)	(4)	(5)	(6)
A. Black OLS						
Post Desegregation Years 1 - 5	-0.74 (4.91)	-6.77 (6.81)	-1.03 (2.41)	-7.05 (3.69)	-3.09 (6.43)	-1.24 (2.04)
Post Desegregation Years 6+	-3.34 (5.62)	-12.85 (8.35)	-1.60 (2.79)	-9.75 (4.64)	-8.63 (6.93)	-4.40 (2.29)
Number of observations	1363	1333	1333	1363	1333	1333
B. Black QML Count						
Post Desegregation Years 1 - 5	-0.27 (0.16)	-0.37 (0.16)	-0.04 (0.12)	-0.15 (0.12)	-0.30 (0.14)	-0.03 (0.10)
Post Desegregation Years 6+	-0.43 (0.20)	-0.59 (0.19)	-0.18 (0.17)	-0.26 (0.15)	-0.43 (0.17)	-0.17 (0.11)
Number of observations	1363	1333	1333	1363	1333	1333
C. White OLS						
Post Desegregation Years 1 - 5	-2.98 (1.22)	-0.46 (1.33)	0.34 (0.46)	-2.02 (0.70)	-0.04 (1.18)	0.22 (0.28)
Post Desegregation Years 6+	-4.80 (1.60)	0.70 (2.08)	0.29 (0.62)	-3.82 (1.08)	0.46 (1.55)	0.34 (0.37)
Number of observations	1363	1333	1333	1363	1333	1333
D. White QML Count						
Post Desegregation Years 1 - 5	-0.15 (0.07)	-0.19 (0.11)	0.04 (0.16)	-0.12 (0.05)	-0.18 (0.10)	0.01 (0.13)
Post Desegregation Years 6+	-0.28 (0.11)	-0.11 (0.15)	-0.10 (0.23)	-0.22 (0.08)	-0.16 (0.11)	-0.01 (0.12)
Number of observations	1363	1324	1213	1363	1333	1333
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 sample runs from 1976 through 1988. For panels A and B, the "Offenders Against Opposite Race" column refers to blacks who offend against whites; for panels C and D it refers to whites who offend against blacks.

Table 5
Supplemental Homicide Report Data

	QML Count					
	Black Offender			White Offender		
	Age 15-24	Age 25-34	Age 35-44	Age 15-24	Age 25-34	Age 35-44
	(1)	(2)	(3)	(4)	(5)	(6)
A. Offenders						
Post Desegregation Years 1 - 5	-0.30 (0.14)	-0.26 (0.13)	-0.31 (0.12)	-0.18 (0.10)	-0.16 (0.09)	-0.10 (0.10)
Post Desegregation Years 6+	-0.43 (0.17)	-0.32 (0.14)	-0.24 (0.12)	-0.16 (0.11)	-0.14 (0.10)	-0.01 (0.10)
B. Offenders against Opposite Race						
Post Desegregation Years 1 - 5	-0.03 (0.10)	0.10 (0.08)	-0.25 (0.10)	0.01 (0.13)	-0.11 (0.10)	-0.10 (0.11)
Post Desegregation Years 6+	-0.17 (0.11)	-0.01 (0.11)	-0.31 (0.14)	-0.01 (0.12)	-0.11 (0.11)	0.01 (0.11)
Number of observations	1326	1326	1326	1363	1333	1333
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 sample runs from 1976 through 1988. In panel B, "Offenders Against Opposite Race" refers to blacks who offend against whites in columns (1) - (3) and whites who offend against blacks in columns (4) - (6).

Table 6
School Desegregation and Long Run Homicide Offending - Age 35 - 44

	Levels		Proportional Response	
	OLS	OLS Log Dummy	QML Count	
	(1)	(2)	(3)	(4)
	A. Black			
Post Desegregation Years 25+	0.55 (3.47)	-0.04 (0.06)	-0.14 (0.07)	
Post Desegregation Years 20 - 24				-0.03 (0.04)
Post Desegregation Years 25 - 29				-0.17 (0.08)
Post Desegregation Years 30+				-0.18 (0.13)
Number of Observations	2659	2659	2643	2643
	B. White			
Post Desegregation Years 25+	-0.65 (0.54)	-0.11 (0.06)	-0.16 (0.08)	
Post Desegregation Years 20 - 24				0.01 (0.05)
Post Desegregation Years 25 - 29				-0.16 (0.09)
Post Desegregation Years 30+				-0.10 (0.13)
Number of observations	2659	2659	2659	2659
<u>Region * Year Effects</u>	X	X	X	X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable in columns (1) is the rate of homicide offending per 100,000 and in columns (2)-(4) is the count of homicide offenders. In column (5), the dependent variable is the log of the homicide rate. The sample runs from 1976 - 2002, the years for which the SHR data is available.

Table 7
Homicide Victimization, Sample Restricted to Decennial Census

	Level		Proportional response	
	OLS	OLS Log Dummy	QML Count	
			Standard	Two-Year Average*
	(1)	(2)	(3)	(4)
A. Black 15 - 19				
Post Desegregation Years 1 - 5	-17.570 (8.634)	-0.175 (0.106)	0.025 (0.150)	-0.046 (0.124)
Post Desegregation Years 6+	-25.112 (11.568)	-0.300 (0.158)	-0.385 (0.203)	-0.399 (0.149)
B. Black 15 - 24				
Post Desegregation Years 1 - 5	-15.929 (9.481)	-0.250 (0.126)	0.103 (0.100)	0.009 (0.086)
Post Desegregation Years 6+	-20.225 (12.528)	-0.175 (0.163)	-0.128 (0.145)	-0.253 (0.113)
C. White 15 - 19				
Post Desegregation Years 1 - 5	0.632 (1.173)	-0.031 (0.095)	0.122 (0.156)	-0.055 (0.126)
Post Desegregation Years 6+	-3.362 (1.668)	-0.282 (0.141)	-0.006 (0.144)	0.022 (0.136)
D. White 15 - 24				
Post Desegregation Years 1 - 5	0.734 (1.219)	-0.063 (0.126)	0.017 (0.118)	-0.075 (0.087)
Post Desegregation Years 6+	-2.486 (1.747)	-0.351 (0.155)	-0.120 (0.115)	-0.119 (0.099)
Number of observations	420	420	420	420
Region * Year Effects	X	X	X	X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The sample is restricted to 1960, 1970, 1980 and 1990. The dependent variable is the homicide rate in columns (1), the transformed log of the crime rate in column (2) (see the text), and the homicide count in columns (3) and (4). * Column (4) uses the average of the homicide count in years t and t+1 (i.e. for the 1960 observation, the average homicide count from 1960 and 1961 is used) in order to increase precision.

Table 8
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 (0.034)	-0.044 (0.030)			0.053 (0.033)	0.035 (0.031)		
Post Desegregation Years +6	-0.011 (0.045)	-0.022 (0.040)			0.074 (0.050)	0.051 (0.046)		
Post Desegregation			-0.033 (0.034)	-0.043 (0.030)			0.054 (0.033)	0.036 (0.031)
B. South Interaction Specifications								
Post Desegregation Years 1 - 5	0.021 (0.039)	-0.007 (0.040)			0.016 (0.053)	0.017 (0.043)		
Post Desegregation Years +6	0.041 (0.055)	-0.006 (0.056)			0.068 (0.083)	0.051 (0.067)		
Post Desegregation Years 1 - 5 * South	-0.088 (0.062)	-0.063 (0.062)			0.061 (0.068)	0.032 (0.062)		
Post Desegregation Years +6 * South	-0.077 (0.088)	-0.004 (0.085)			-0.029 (0.102)	-0.019 (0.089)		
Post Desegregation			0.022 (0.039)	-0.008 (0.040)			0.018 (0.053)	0.018 (0.043)
Post Desegregation * South			-0.087 (0.062)	-0.057 (0.062)			0.056 (0.067)	0.029 (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 - 1990.

Table 9
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 (0.017)	-0.012 (0.018)	-0.016 (0.009)	-0.007 (0.009)	-0.005 (0.005)	-0.003 (0.004)
Post Desegregation Years 6+	-0.015 (0.028)	-0.011 (0.029)	0.010 (0.012)	0.017 (0.014)	-0.007 (0.007)	-0.006 (0.007)
	B. Whites					
Post Desegregation Years 1 - 5	0.001 (0.009)	0.001 (0.009)	0.006 (0.004)	0.006 (0.005)	0.005 (0.005)	0.005 (0.004)
Post Desegregation Years 6+	-0.017 (0.016)	-0.011 (0.017)	0.009 (0.006)	0.009 (0.006)	0.004 (0.007)	0.004 (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 10
Homicide Victimization: MSA Sample

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

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

Table 11
Falsification Test, Death From Illness

	Age 15-24			Age 25-34			Age 35-44		
	Level	Proportional response		Level	Proportional response		Level	Proportional response	
	OLS Level	QML Count	OLS Log Dummy	OLS Level	QML Count	OLS Log Dummy	OLS Level	QML Count	OLS Log Dummy
	(1)	(2)	(3)	4	(5)	(6)	7	(8)	(9)
A. Black									
Post Desegregation Years 1 - 5	-0.32 (1.74)	-0.04 (0.04)	-0.01 (0.03)	-0.35 (6.25)	0.07 (0.04)	0.05 (0.04)	-10.85 (15.05)	0.04 (0.03)	-0.02 (0.04)
Post Desegregation Years 6+	2.49 (2.92)	0.04 (0.05)	0.04 (0.05)	-0.48 (9.84)	0.15 (0.09)	0.04 (0.06)	-21.60 (24.88)	0.08 (0.06)	-0.07 (0.06)
Number of observations	3039	3039	3039	3040	3040	3040	3040	3040	3040
B. White									
Post Desegregation Years 1 - 5	-0.67 (0.48)	-0.06 (0.03)	-0.03 (0.04)	0.02 (1.01)	-0.03 (0.03)	-0.01 (0.04)	0.22 (3.33)	0.00 (0.03)	-0.02 (0.03)
Post Desegregation Years 6+	-0.23 (0.72)	-0.04 (0.04)	-0.01 (0.07)	0.68 (1.32)	-0.01 (0.04)	0.02 (0.05)	-0.96 (5.12)	0.01 (0.04)	-0.07 (0.05)
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 rate of death from illness per 100,000 in the OLS specifications, is the count of deaths from illness in the QML Count specifications, and is the log of the rate of death from illness per 100,000 in the OLS Log Dummy specifications.

Table 12
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 (88.4)	164.9 (83.6)		
Post Desegregation Years 1 - 5			173.5 (85.7)	163.8 (81.6)
Post Desegregation Years +6			163.0 (89.1)	155.9 (88.7)
B. Ratio of Police Expenditures to Population				
Post Desegregation	2.5 (2.7)	2.4 (2.7)		
Post Desegregation Years 1 - 5			1.9 (2.8)	1.7 (2.8)
Post Desegregation Years +6			-2.3 (4.4)	-2.9 (4.2)
C. Ratio of Fire Department Expenditures to Population				
Post Desegregation	-0.2 (1.8)	0.0 (1.8)		
Post Desegregation Years 1 - 5			-0.1 (1.9)	0.1 (1.9)
Post Desegregation Years +6			0.3 (2.8)	0.4 (2.9)
Number of Observations	419	419	419	419
Region * Year Effect	X	X	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 for each of the two panels is given in the panel title. The dependent variables 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 13
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 (0.05)	-0.04 (0.05)	-0.07 (0.06)	-0.09 (0.06)	-0.07 (0.07)	-0.12 (0.07)
Post Deseg. Years 6+	-0.13 (0.08)	-0.08 (0.10)	-0.11 (0.12)	-0.18 (0.08)	-0.20 (0.09)	-0.17 (0.08)
Post Deseg. Years 1 - 5 * Δ Exposure Index	-0.54 (0.20)		-0.53 (0.35)			
Post Deseg. Years 6+ * Δ Exposure Index	-0.88 (0.29)		-0.71 (0.50)			
Post Deseg. Years 1 - 5 * Δ Dissimilarity Index		0.29 (0.11)	0.00 (0.22)			
Post Deseg. Years 6+ * Δ Dissimilarity Index		0.56 (0.22)	0.14 (0.43)			
Post Deseg. Years 1 - 5 * Δ Ed. Expend. Per Pupil					-0.04 (0.11)	
Post Deseg. Years 6 + * Δ Ed. Expend. Per Pupil					0.10 (0.14)	
Post Deseg. Years 1 - 5 * Δ Police Per Pop.						2.35 (2.36)
Post Deseg. Years 6 + * Δ Police Per Pop.						-4.00 (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 number of black 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, except in columns (4) - (7). 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.

Table 14
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 (0.06)	-0.05 (0.06)	-0.01 (0.07)	-0.10 (0.05)	-0.07 (0.06)	-0.09 (0.06)
Post Deseg. Years 6+	-0.09 (0.08)	-0.05 (0.09)	-0.12 (0.08)	-0.28 (0.11)	-0.19 (0.11)	-0.20 (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 number of white 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 (4) - (7). 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 spans 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. Δ % white in deseg school refers to the change in the percent of the white school aged county population which is enrolled in the desegregated district.

Table 15
 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 (0.18)	-0.30 (0.13)
Post Desegregation Years 6+	-0.58 (0.21)	-0.61 (0.19)
Number of observations	1317	1317
Region * Year Effects	X	X

Note. Standard errors clustered by county in parentheses.

Table 16
Effect of Desegregation Plan on Percent of Children Attending the Desegregated School District

	White		Black	
	(1)	(2)	(3)	(4)
	Ratio of Enrollment at Desegregated School District to Children in the Country			
Post Desegregation Years 1 - 5	-0.054 (0.012)	-0.032 (0.012)	-0.005 (0.015)	0.000 (0.013)
Post Desegregation Years 6+	-0.064 (0.015)	-0.039 (0.016)	0.011 (0.019)	0.014 (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 dependent variable for each of the panels is given in the panel title. The unit of observation is county-year in Panel A. 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

Table A2
Black and White Homicide Victimization, Weighted by Population

	Black			White		
	Levels	Proportional Response		Levels	Proportional Response	
	OLS	QML Count	OLS Log Dummy	OLS	QML Count	OLS Log Dummy
	(1)	(2)	(3)	(4)	(5)	(6)
A. Age 15 - 19						
Post Desegregation Years 1 - 5	-7.82 (2.81)	-0.21 (0.05)	-0.18 (0.06)	-0.40 (0.65)	-0.03 (0.04)	-0.11 (0.06)
Post Desegregation Years 6+	-11.79 (4.02)	-0.16 (0.11)	-0.34 (0.10)	-2.51 (1.14)	-0.16 (0.10)	-0.35 (0.15)
B. Age 15 - 24						
Post Desegregation Years 1 - 5	-9.61 (3.05)	-0.18 (0.03)	-0.13 (0.04)	-0.64 (0.71)	-0.06 (0.04)	-0.09 (0.05)
Post Desegregation Years 6+	-14.93 (4.13)	-0.21 (0.05)	-0.22 (0.07)	-2.54 (1.13)	-0.17 (0.07)	-0.27 (0.08)
C. Age 25 - 34						
Post Desegregation Years 1 - 5	-12.92 (3.77)	-0.18 (0.03)	-0.14 (0.04)	-0.54 (0.67)	-0.05 (0.05)	-0.09 (0.05)
Post Desegregation Years 6+	-25.02 (4.65)	-0.28 (0.04)	-0.28 (0.07)	-1.04 (0.89)	-0.09 (0.07)	-0.15 (0.07)
D. Age 35 - 44						
Post Desegregation Years 1 - 5	-7.78 (3.63)	-0.06 (0.05)	-0.10 (0.06)	-0.48 (0.44)	-0.08 (0.05)	-0.04 (0.04)
Post Desegregation Years 6+	-11.28 (6.43)	0.06 (0.14)	-0.16 (0.10)	-1.14 (0.71)	-0.19 (0.06)	-0.12 (0.07)
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-year. The dependent variable is the homicide rate per 100,000 in columns (1) - (3) and (7) and the homicide count in columns (4) and (5). All specifications are weighted by the relevant total age-race population count for the panel.

Table A3
Homicide Victimization: Bordering County Sample

	Levels: OLS			Proportional Response: QML Count		
	Bordering County Sample Estimate	Implied County Estimate Assuming No Migration	Actual County Sample Estimate (Tables 2 & 3)	Bordering County Sample Estimate	Implied County Estimate Assuming No Migration	Actual County Sample Estimate (Tables 2 & 3)
	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	-4.53 (2.31)	-8.20	-8.91	-0.05 (0.04)	-0.09	-0.14
Post Desegregation Years 6+	-5.59 (3.32)	-10.13	-10.55	-0.11 (0.05)	-0.21	-0.23
B. White Age 15 - 24						
Post Desegregation Years 1 - 5	-0.01 (0.31)	-0.01	-0.48	0.01 (0.04)	0.01	-0.05
Post Desegregation Years 6+	-0.66 (0.57)	-1.20	-2.22	-0.07 (0.06)	-0.12	-0.23
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 rate per 100,000 in column (1) and the homicide count in columns (2) and (3). δ equals the percent of the bordering county group population which resides in the treated counties - see the Appendix for more details.

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 (3.13)	-2.28 (3.61)	-3.01 (3.54)	-3.85 (4.07)	-0.34 (5.67)	3.11 (5.18)
Post Deseg. Years 6+	-4.07 (4.18)	-3.54 (4.43)	-3.76 (4.39)	-6.33 (6.18)	-5.51 (6.67)	-2.92 (6.93)
Post Deseg. Years 1 - 5 * Δ Exposure Index	-28.02 (15.52)		-24.01 (19.47)			
Post Deseg. Years 6+ * Δ Exposure Index	-27.29 (14.61)		-23.65 (19.31)			
Post Deseg. Years 1 - 5 * Δ Dissimilarity Index		19.54 (11.47)	3.82 (13.14)			
Post Deseg. Years 6+ * Δ Dissimilarity Index		18.95 (10.54)	3.50 (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 black 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, except in columns (4) - (7). 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.

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 (0.54)	-0.51 (0.62)	-0.39 (0.54)	-1.28 (0.75)	-1.38 (1.02)	-1.22 (1.10)
Post Deseg. Years 6+	-1.76 (0.96)	-1.30 (1.07)	-2.17 (0.79)	-4.06 (1.66)	-3.32 (1.69)	-3.36 (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 white 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 (4) - (7). 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.