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THE EFFECTS OF SCHOOL DESEGREGATION ON CRIME

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THE EFFECTS OF SCHOOL DESEGREGATION ON CRIME

This paper studies the impact of school desegregation on youth homicide offending and victimization. The direction of this effect is theoretically ambiguous, and could change the welfare implications of this policy implied by previous studies of academic outcomes or earnings. Our research design exploits the fact that since the Supreme Court's 1954 *Brown* decision, the majority of the nation's largest school districts were subject to mandatory, court-ordered desegregation plans. The timing of when these plans went into effect is idiosyncratic and plausibly exogenous to other determinants of youth outcomes across jurisdictions, as suggested by the fact that using annual county-level homicide data we show there is no evidence of any pre-existing trends before these court orders went into effect. Our results suggest school desegregation reduces homicide victimization for both blacks and whites by around 25 percent, and reduces long-term homicide offending as well. The decline in homicide for blacks is largest in districts where blacks experience the largest increase in exposure to white students, while homicide declines for whites are largest in districts where school spending increased the most following desegregation orders. These effects seem to persist well into adulthood for students exposed to these orders.

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I. INTRODUCTION

In 1954 the U.S. Supreme Court ruled 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 one of the most important social policy changes of the 20th century, or in the NAACP's words "one of those incandescent moments that immediately divide history into Before and After" (NAACP, 2004, p. 6). The *Brown* ruling assumed that school desegregation would improve the life chances of black children. Whether this belief is correct remains of great interest given the Supreme Court's recent rulings against school racial desegregation plans in Seattle and Louisville, and the possibility of additional litigation to clarify the Court's decision,[1] signs of declining support for desegregation among African-Americans, and re-segregation of the public schools in recent years, due perhaps in part to the widespread termination of court-ordered desegregation plans beginning in the early 1990s.[2]

Given the importance of court-ordered school desegregation it is striking that most of the social science research over the past 50 years has focused on how

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[1] In the two decisions announced 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 sided with the 5-4 majority but wrote a separate opinion leaving 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).

[2] For a discussion of black support for desegregation in the decades after Brown, see Jaynes and Williams (1989), and, more recently, Lewin and Herszenhorn (2007). See Lutz (2005) for a discussion of the Supreme Court decisions in the early to mid 1990s that have led to many desegregation plans being terminated. Regarding trends in school segregation, the fraction of black students attending majority non-white schools increased in the late 1980s and early 1990s (Clotfelter, 2004), although this seems to be driven in part by a general increase in the share of public school students who are non-black and non-white (Logan, 2004, Clotfelter, Ladd and Vigdor, 2005). Other measures of school desegregation are relatively constant over this period.

desegregation orders influence *academic* outcomes,[3] despite the fact that impacts on *non-academic* outcomes could easily change the social welfare implications of this policy. Particularly important among these is crime, in part because social science theory is ambiguous about whether school desegregation should increase or decrease criminal activity. The costs of crime are so large – perhaps as much as $2 trillion per year (Ludwig, 2006) – that taking such impacts into account could change the cost-benefit analysis or distributional consequences of desegregation orders.[4] To the best of our knowledge only one previous study examines the effects of school desegregation on crime (LaFree and Arum, 2006), although this earlier study relies on a research design that may have difficulty isolating the causal effects of desegregation on crime.[5]

The most obvious way in which court desegregation orders might affect crime is through beneficial peer effects on the schooling outcomes of African-American children, which in turn would increase the opportunity costs of criminal activity (Becker, 1968, Lochner and Moretti, 2004). This mechanism receives support from

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[3] 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).

[4] For example in the Perry Preschool program fully two-thirds of the program benefits are estimated to come from reductions in crime (Belfield et al., 2006). The Job Corps is another noteworthy example of the importance of considering impacts on crime (Burghardt et al., 2001, Schochet et al., 2003).

[5] LaFree and Arum (2006) use a research design that follows Card and Krueger (1992, 1996) and examines whether incarceration rates are higher in the 1970, 1980 and 1990 decennial censuses for people who were born in states with more racially segregated schools, holding adult state of residence constant. They find that blacks brought up in states where schools were relatively more segregated have higher incarceration rates as measured by the census, and that these associations increase in magnitude over time for more recent birth cohorts. However their identification comes from comparing people who move out of state between birth and adulthood. But these mobility decisions could themselves be influenced by segregation in schools or other public institutions. The more general concern is that across-state and over-time variation in school segregation is potentially related to a wide range of other public policies that influence outcomes for racial minorities, including crime.

evidence that court desegregation orders seem to reduce dropout rates for blacks, with little impact on the dropout rates of white children (Guryan, 2004, Lutz, 2005).

However it is important to note that peer effects on academic outcomes are neither necessary nor sufficient for court-ordered school desegregation to reduce crime, since a variety of other behavioral mechanisms could increase – or decrease – experiences with crime for both blacks and whites. For example some federal courts required additional school spending as part of their desegregation orders, which could reduce criminal activity by both blacks and whites. It is also possible that localities respond to court desegregation orders with increased spending on law enforcement.

But school desegregation could also potentially *increase* crime, even if these orders improve schooling outcomes for children, given the possibility of increased racial tension from court-ordered desegregation. For example during the Supreme Court's internal discussions about the *Brown* case, Justice Hugo Black predicted "violence if [the] court holds segregation unlawful," (Klarman, 2004, p. 294). School racial desegregation could also cause crime to go up by increasing the exposure of low-income youth to more affluent youth (and hence more valuable "loot"), given average differences across race groups in family income (see Kling, Ludwig and Katz, 2005).

The contribution of the present paper is to provide what we believe are the first estimates of the effects of court-ordered school desegregation on crime for blacks and whites using a plausibly exogenous source of identifying variation.[6] The key to our

[6] To the best of our knowledge only one previous study (Lafree and Arum, 2006) has examined how court desegregation orders impact crime. Their paper examines whether people brought up in different

research design is that most of the large school districts in the U.S. were slow to desegregate after the landmark *Brown* ruling, and so wound up eventually being forced to desegregate by local court order. Within the set of districts subject to court order the timing of when the orders are executed is plausibly random: As we discuss below, up through at least the time of *Brown* the NAACP seems to have strategically filed lawsuits in places where and when they were most likely to win, rather than where a favorable ruling would generate the greatest benefit to minority children, and over time the process generating lawsuits seems to have become if anything more random. This difference in the timing of desegregation orders among districts ever subject to such orders is the identifying variation we use to estimate desegregation effects on crime.

We specifically focus 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, which are listed in Appendix Table A1 together with the year of their court desegregation order.[7] The districts in our study are not necessarily representative of all districts in the U.S., but they are nonetheless of substantial interest in their own right given they account for such a large share of minority students in America.[8] We seek to identify the effect of desegregation orders on youth crime in these districts, which as discussed below may differ from the effects

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states, with different levels of school desegregation, are differentially likely to be incarcerated as adults holding adult state of residence constant. As we discuss below their study may be susceptible to bias from selection effects of school desegregation on who moves out of state.

[7] 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 15,000- 50,000.

[8] In 1968 these districts accounted for 45 percent of minority enrollment in the U.S. Importantly they also account for a disproportionately large share of all crime in the country, given the strong positive relationship historically between city size and per capita crime rate (Blumstein, 2000).

of naturally occurring variation in racial composition but is more directly relevant for questions about whether to use policy levers to force schools to desegregate.

Within the set of districts subject to court desegregation orders, we compare crime victimization and offending rates for blacks and whites in the years before and after these court orders go into effect. Our victimization measure comes from annual mortality records from the Vital Statistics (VS) from 1959-88, while we measure offending using data from the Federal Bureau of Investigation's Supplemental Homicide Reports (SHR), which begins in 1976 and so given that desegregation orders became much more prevalent starting in 1968 gives us more power to examine long-term than short-term impacts. Given these two data sources our study necessarily focuses on homicide, which is of considerable interest given that homicide accounts for a disproportionately large share of the total social costs of crime[9] and is usually thought to be measured more reliably than other types of crime. Moreover both the VS and SHR have the great advantage of providing us with annual data, which enables us to examine with some precision how crime changes in the years immediately before and after court desegregation orders go into effect.

We find that school desegregation orders reduce homicide victimization rates for both black and white youth by around 25 percent. We also estimate that black and white adults who were exposed to court-ordered desegregation when they were in school are about 15 percent less likely to commit homicide. These are proportionately

[9] Ludwig (2006) estimates that the victimization costs from street crime in the U.S. in 2004 or 2005 equal around $694 billion. The costs for murder alone equal around $156.5 billion.

large effects, about the same size as those reported by Guryan (2004) for dropout rates – another outcome that, like crime, is concentrated in the left tail of the behavioral distribution. One question raised by these findings is whether desegregation orders could really generate such large changes in crime without anyone having noticed. But note that the period in which these court orders went into effect – mostly the late 1960s and 1970s – was one in which homicide rates nationwide experienced pronounced secular trends (Cook and Laub, 1998, Blumstein, 2000, Levitt, 2004).

The other central question for our study is whether the timing of when court school-desegregation orders go into effect among the set of districts subject to such orders is in fact essentially random. 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 homicide victimization rates from the VS, we can provide an even sharper test of this identifying assumption by showing there are no differential trends across counties in homicide rates in the individual years *before* court school-desegregation orders go into effect. In addition we find no relationship between court school-desegregation orders and *other* youth health outcomes that should not be causally affected by desegregation orders. We also show that our results are not driven by measurement error in the denominator of our homicide rate calculations, or more generally by changes over time in the composition of whites and blacks living in our sample of counties.

Also of interest is to know something about the behavioral mechanisms that are responsible for these crime impacts, given that at least in principle all of the candidate

mechanisms discussed above besides peer interactions could be modified by policymakers without having to re-sort children across schools. Our study suggests that the mechanisms responsible for changes in homicide offending and victimization differ for black and white youth. Specifically, the estimated effect of desegregation orders on homicide for blacks is largest in those school districts where these orders generate the largest measured changes in racial composition of the public schools. In contrast, the impacts on homicide for whites are largest in those districts where court orders were accompanied by the largest increases in school spending.

The next section provides additional background on court desegregation orders. Section III discusses the ways in which school desegregation orders might affect crime as well as previous evidence, Sections IV and V present our data and methods, Section VI presents our main results, specification checks, and examination of behavioral mechanisms, while Section VII discusses limitations and policy implications.

II. BACKGROUND

In 1955, a year after the *Brown* decision, the Supreme Court issued the *Brown II* decision indicating school districts were expected to desegregate "with all deliberate speed" (349 U.S. 294, 1955), although what this meant in practice was not specified and details were left to be determined by lower Federal courts. Many smaller districts, particularly in the South, began to desegregate in the 1960s after the Federal government threatened to withhold Title I financial assistance to districts that continued to discriminate by race (Cascio et al., 2007). However large school districts

were much slower to desegregate, and as a result most of the nation's largest school districts wound up being ordered to racially desegregate by local Federal courts.

The key to our study is the identifying assumption that among the large school districts subject to court desegregation orders, the timing of when these orders went into effect is plausibly unrelated to trends in other determinants of youth outcomes. This assumption is plausible since most of the desegregation lawsuits were filed by the NAACP (Klarman, 2004), which adopted a strategy well before *Brown* of filing lawsuits to establish a series of favorable legal precedents (rather than maximizing short-term social welfare gains), and starting in the late 1960s the process generating school desegregation lawsuits seems to have become if anything even more random.

Consider for example that from 1936 to 1950 the NAACP strategically focused on lawsuits to desegregate graduate and professional schools, rather than K-12 schooling where the social benefits would presumably have been much greater, because the organization believed the probability of a favorable court ruling was enhanced in cases that "would bypass the inflammatory issue of 'race-mixing' among young children" (NAACP, 2004, p. 9). The NAACP took a case from Kansas for *Brown* itself in part because differentials in school quality between blacks and whites were not as pronounced there as in many other states. The specific benefits from desegregating schools would thus be less pronounced than from favorable rulings in other states, but focusing on Kansas had the strategic legal advantage of focusing the Supreme Court squarely on the issue of desegregation itself (NAACP, 2004).

In 1968 the Supreme Court prohibited local school desegregation plans that gave minority students the choice to attend different schools but in practice did not effectively desegregate the public schools (*Green vs. New Kent County, Virginia*, 391 U.S. 430, 1968), which led to a surge of litigation activity. Jack Greenberg, director of the NAACP's Legal Defense and Educational Fund from 1961 to 1984, describes the process through which school desegregation lawsuits were filed as becoming increasingly decentralized and dependent in part on the willingness of local parents to sign on to lawsuits against the local schools and for lawyers and funders to adopt the cases (see also Greenberg, 1994 and Klarman, 2004). The willingness of individual plaintiffs, lawyers and funders to support litigation would presumably have depended in part on the probability of success in the local Federal courts, and there does indeed seem 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 basis of the probability of winning a favorable ruling seems to be widely shared among lawyers even today.[10]

However there is at the very least something of a regional effect on the timing of when these Federal court orders were implemented (see Figure 1), which is the product of the evolution of legal doctrine. Prior to 1973, court-ordered desegregation could only occur in school districts proved to have engaged in *de jure* segregation.

[10] For example the plaintiffs in the case against DC's handgun ban selected that jurisdiction for strategic reasons: "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).

The 1973 *Keyes* decision (*Keyes v. Denver School District*, 413 U.S. 189), ruled that court-ordered desegregation could proceed in areas which had not practiced *du jure* segregation. As a result, desegregation became more viable in school districts outside of the south in which *de facto* segregation was present. Figure 1 shows that Southern districts were disproportionately likely to be subject to court orders to desegregate earlier in the period. This pattern suggests the importance of adequately controlling for region-specific trends in crime outcomes over time in our empirical analysis below.

III. CONCEPTUAL FRAMEWORK

Why might court school desegregation orders affect crime victimization or offending? In this section we discuss four candidate mechanisms: changes in peer influences; improvements in school quality; increased police spending; and changes in the population composition of counties in response to court orders.

A. Peer Effects

One mechanism through which court desegregation orders could reduce the propensity of minority youth to engage in crime comes from changes in the peer environments to which they were exposed, which the Supreme Court itself assumed in the *Brown* decision were important. Attending a racially segregated school could influence the emotional development and perceptions of self worth among minority children, a possibility suggested by Kenneth Clark's (1947) landmark "doll study" that was cited in the *Brown* decision.[11] Segregated schools could cause black children to

[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

conclude they have limited opportunities to succeed in America. And because whites are on average more affluent than blacks with higher test scores and lower rates of crime involvement, school desegregation could change criminal behavior by minority youth through the usual "contagion" mechanisms widely discussed in the literature.[12]

The Coleman Report (1966) found that school racial composition was weakly correlated with student achievement, and that having more affluent schoolmates was instead a much stronger predictor of individual student test scores. Recent studies that also use cross-section variation typically reach similar conclusions (Mayer, 1991, Rivkin, 2000, Rumberger and Palardy, 2005, Card and Rothstein, 2006).[13] Recent research that uses different research designs typically finds stronger evidence that school racial composition affects the achievement test scores of black students, and perhaps white children as well (Hoxby, 2000; Hanushek, Rivkin and Kain, 2004).

Yet the effects on student outcomes identified using naturally occurring variation in school or classroom racial composition are not necessarily reflective of the impacts we would expect from exogenous changes in school racial composition induced by policies such as court orders. For instance the likelihood that students of different races interact with one another may depend on whether the school has

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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).

[12] Attending a more racially integrated school with more affluent, higher-achieving children who are at lower risk for criminal activity could change the social stigma associated with crime, information about the returns to schooling or other pro-social activities, or the availability of teacher time for instruction rather than dealing with disruptive students, or opportunities to form study groups with higher-achieving classmates (Jencks and Mayer, 1990; Manski, 2000). Similar types of peer mechanisms may be at work for academic outcomes, which would then increase the opportunity costs of criminal behavior.

[13] The discussion in this section draws in part on Ludwig and Vigdor (2006).

reached a given level of racial integration voluntarily rather than as a result of an external mandate such as a court order (Jencks et al., 1972).

In this sense, particularly important for policy purposes are Guryan's (2004) findings that court-ordered school desegregation plans reduce black dropout rates by 2-3 percentage points, with no detectable effect on whites. Guryan focuses on the same set of districts that we examine using data from the decennial census in 1960 to 1980. Lutz (2005) finds qualitatively similar effects when he examines the impact of termination of many of these desegregation plans during the 1990s.

However any peer effects induced by court desegregation orders could cause criminal behavior to increase among whites through the reverse of the peer-effect mechanisms that might lead desegregation to reduce crime by blacks.[14] Peer effects could also lead desegregation orders to increase crime by blacks as well as whites, since as Jencks and Mayer (1990) note some youth may have adverse reactions to more competitive academic environments or being surrounded by more affluent peers (see also Luttmer, 2005, Kling, Ludwig and Katz, 2005, and Ludwig and Kling, 2007). And desegregation orders could increase racial tensions, as Justice Black had feared. For example early efforts to desegregate Little Rock High School in 1957 led to "mob violence," and eventually required federalized National Guard troops to carry out (NAACP, 2004, p. 7). There were riots in Kentucky as well, and the governor even

[14] Note that any deleterious peer influences on crime by whites could arise even if there are beneficial or neutral impacts on schooling outcomes for whites, since some peer influences on crime operate through different channels than do those that might affect schooling outcomes. For instance increased criminal behavior by one's schoolmates would all else equal reduce the probability that the marginal offender is apprehended and punished, which might be material for youth decisions to participate in crime but presumably less directly relevant for many academic outcomes.

ordered the prevention of desegregation orders 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)[15]

B. School Quality

The findings by Guryan (2004) and Lutz (2005) that desegregation orders improve schooling outcomes for blacks do not necessarily mean that peer influences are the operative mechanism. Desegregation orders also change the specific schools attended by black students and in some cases whites as well, so changes in outcomes for blacks and whites could be due to changes in school quality rather than peers.

Documenting disparities in the quality of schools attended by black and white children was the main motivation for the massive data collection effort that led to the famous Coleman Report (1966). The fact that Coleman did *not* find large differences between predominantly black versus white schools in measurable school inputs does not by itself rule out the potential importance of this mechanism, given the weak correlation between most measurable school inputs and student outcomes.

[15] 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) or any history of school desegregation for numerous other examples around the country.

Even if there are no baseline differences in average school quality between blacks and whites, in the *Milliken II* decision (Milliken v. Bradley, 433 U.S. 267, 1977) the Supreme Court explicitly permitted federal courts to order additional education spending as part of desegregation plans. Many local federal courts used this authority to require increased spending by local districts or state governments. This additional spending could improve the overall school quality for black and white children in a district and reduce crime by increasing formal labor market prospects. This extra spending could be used to improve academic outcomes, or instead for shorter-term gains from increased social control, for example by increasing the number of school security guards or staff available for lunchroom and hallway monitoring.

C. Police Spending

An alternative possibility is that school desegregation leads to an increase in spending on "social control," including stepped-up spending on the criminal justice system (Cook and Ludwig, 2006; Kinsler, 2006). For example after Boston's attempts to desegregate the schools in 1974 led to riots by whites opposed to desegregation and inter-racial fighting between students there was an aggressive police response, or as one policymaker put it, "a cop for every kid" (HGSE News, 2000).

D. Population Changes

The unit of analysis for our study is the county and so in principle another way in which court desegregation orders could change the homicide offending or victimization rates is by affecting the socio-demographic composition of the people who live in the county. Previous research provides ample evidence that white families

are concerned about the effects of black in-migration on both schools and crime. "White flight" out of counties that contain districts subject to court desegregation orders could affect homicide rates for mechanical reasons.

Other forms of white flight that could arise include moving to another school district within the same county that is not subject to court-ordered desegregation, or transferring from public schools to private schools (known colloquially in the South as "white flight academies"). So long as whites stay within the county this sort of movement across districts within counties or into private schools will not generate any mechanical change in our homicide rates, since we are focusing on counties.

IV. DATA

Our main data sources are the Vital Statistics (VS) system of the United States and the FBI's Supplemental Homicide Reports (SHR). We provide a brief review of both data sources here and more details in the Data Appendix.

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. The SHR comes from voluntary data reported by local and state police to the FBI, which we use to construct homicide offending rates to age-race groups by county and year. 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. 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.

One limitation with the SHR for our purposes is that these data are available starting only in 1976. Figure 1 shows a considerable share of districts in our analytic sample were subject to desegregation orders before this time, so we will have limited power to detect impacts on homicide offending during the first few years after these orders are in place. But the SHR data go through 2002, and so can be used to examine the effects of school desegregation orders on long-run homicide offending behavior.

County population data for constructing victimization and offending rates come from the Census and the VS interpolations for intercensal 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 intercensal years would lead us to understate homicide rates for whites following desegregation. In practice this does not seem to be much of a concern, as we demonstrate below.

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. In cases where a county includes multiple school districts we count the entire county as desegregating at the time the first district is subject to a court order, although the results are not sensitive to alternative approaches.

V. METHODS

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

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

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" by desegregation 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 over time.[16]

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 sample composition changes. We therefore restrict our sample to counties which 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,[17] which 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 we show the results are qualitatively similar when we estimate the effects on the average juvenile instead by estimating equation (1) using county population as weights.

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[16] 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.

[17] 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.

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 2, we see substantial differences across counties in the cross section in homicide rates, especially for black youth, which at first glance would suggest that a log linear specification that estimates proportional effects from school desegregation orders may be preferable to a linear model that assumes constant absolute effects. However it is also the case that many counties record no homicides to youth in some years, especially for black youth (this can be seen in the histogram displayed on Figure 2). The log linear specification is problematic because observations equal to zero are undefined when the log transformation is taken.

In order to estimate a proportional response model using OLS, we employ the method proposed by Pakes and Griliches (1980). The homicide rate is transformed by replacing any zero values with ones. The log of this transformed variable is used as the dependent 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. For ease of exposition, this model will be referred to as the log linear dummy variable specification.

In order to estimate a proportional response model with does not suffer from the bias inherent to the log linear dummy model, we also estimate a fixed-effect Poisson count model as in (2):

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

where y_{it} is the count of homicides for a given age/race cohort in county *i* at time $t, \bar{D}_{it} = \sum_{p \in \Psi} D_{p,it}$ and pop_{it} is the size of the age/race cohort. Equation (2) 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).

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

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 (3) 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 (2), is sufficient to ensure consistency. The parameter estimates remain consistent even in the case of distributional misspecification (i.e. the assumption that the distribution of y given x is Poisson fails to hold) and there is no need to make assumptions about over or under-dispersion or, more generally, to specify the conditional variance, as must be done for many count models (Wooldridge 1999)

By imposing the constraint that ψ=1, the pop_{it} variable controls for "exposure". The parameters of interest, β_p, can therefore be interpreted as semi-elasticities of the

homicide rate with respect to the year of school desegregation — i.e. they estimate the percent change in homicides rates associated with a county being in its *pth* year of school desegregation.[18] 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).

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:

$$y_{it} = \alpha + \sum_{p \in \Psi} \beta_p D_{p,it} + \gamma_i + \delta_{t,r} + \lambda_t X_i + \varepsilon_{it} \quad (4)$$

where X_i is the vector of time-invariant county characteristics measured as of the 1960 Census[19] and λ_t is the vector of time varying coefficients on these characteristics. This model controls, in an extremely flexible manner, for trends in crime associated with the characteristics. For instance, median family income is included in the X_i vector. The model controls for the possibility that low income communities may have increasing

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

[19] Ideally, these characteristics would be measured as of the first year of the sample, 1959, but we are forced instead to use the 1960 census data contained in the County and City Databook.

rates of crime over time. Time-variant demographic variables are not included in the model because they are likely endogenous to desegregation.[20]

VI. RESULTS

Table 1 provides some general background on our analytic sample of counties in the Welch and Light dataset. As noted above these are unusually large counties, with a mean population of around 677,000 over our entire study period, of which around 17 percent are African-Americans. Homicide victimization rates to white youth 15-19 in these counties increase dramatically from 1960 to 1980, from 2.3 to 9.7 per 100,000, while homicide 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 during the 1970s. This convergence in black and white youth homicides starting in 1970 continues at least through the mid-1980s (Cook and Laub, 1998, p. 44).

We begin by demonstrating that court desegregation orders actually did change various measures of school racial segregation and increased total education spending in these counties but not local spending on police. Our empirical evidence suggests that on net the changes in social conditions induced by court desegregation orders reduce homicide offending and victimization for both black and white youth. Reductions in youth homicide victimization rates for both blacks and whites in the VS data are proportionally quite large (20 to 25 percent for both groups) and precisely estimated. Our results for youth homicide offending are more complicated because our power to

[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 the results presented below.

detect short-term changes in homicide offending by youth is limited by the fact that the SHR data start only in 1976, and so miss implementation of many of the early desegregating districts. But looking at long-term changes in homicide offending by both black and white birth cohorts exposed to desegregation orders suggests changes in behavior that persist into adulthood for both groups.

Sorting out the specific mechanisms through which these impacts arise is necessarily more complicated and subject to some uncertainty. With that caveat in mind, we present some evidence suggesting peer effects or hard-to-measure aspects of school quality may be the most important mechanisms behind changes in black homicide offending and victimization, since impacts on these outcomes are largest in school districts that experience the largest declines in school segregation. On the other hand increased school spending seems to be an important driver for whites.

A. Impacts on Segregation and Public Goods

Court desegregation orders were intended to reduce the degree of public school racial segregation, with the hope that these changes would in turn improve life outcomes for minority children. At the very least these court orders were successful in accomplishing the first of these objectives, as seen in Figure 3. The top panel shows that following court desegregation orders there is a sharp drop in the dissimilarity index, which is a measure of how students are sorted across schools.[21] The

[21] The dissimilarity index is defined as:

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

dissimilarity index ranges from 0 to 1, with 1 denoting complete segregation, and reflects the percent of black students who would need to be reassigned to a different school for perfect integration to be achieved given the districts overall racial composition. Panel A of Figure 3 comes from estimating equation (1) above with OLS using the dissimilarity index as the dependent variable of interest, in a model that conditions on county and region-year fixed effects. We plot the regression coefficients on our indicator variables for years before and after the court orders go into effect, with year 0 set equal to the year *before* these orders go into effect. We see very little evidence of any 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.

Note that a decline in the dissimilarity index need not imply that blacks are attending schools with proportionately more whites. 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. We therefore also examine a measure of interracial context within the school district, the exposure index, which reflects the percent of white students in the average black student's school. An increase in segregation is reflected by a *decrease* in the exposure

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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.

index, which is clearly what happens following court orders (Figure 3, Panel B).[22] Our findings that desegregation orders produce a sharp and persistent decline in racial integration measured by both indices are very similar to those in Reber (2005).

However 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 2. 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 0.17 following implementation of a court desegregation order (Panel A, Table 2), equal to about 6 percent of the sample mean of 2.8. In contrast we find no systematic evidence that police spending per capita is affected by desegregation orders (Panel B, Table 2).

The key question for our study is whether these changes in how students are sorted across schools and in total education spending increase, decrease, or leave unaffected crime by and against blacks and whites. We turn to this question next.

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[22] 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

We begin by examining whether school desegregation orders affect for black and white youth their rates of homicide victimization, since the VS data available to measure victimization are available for a much longer period of time (back to 1959) compared to our SHR data on homicide offending (back to 1976).

Table 3 shows that black homicide victimization rates declined substantially following implementation of court school-desegregation orders. These estimates come from estimating a more parsimonious version of equations (1) and (4) where the two key explanatory variables of interest are indicators for whether the county-year observation falls within the first five years after a desegregation order is imposed, or is 6 or more years after a court desegregation order goes into effect. 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 (29 per 100,000, as shown in Table 1). The coefficient on the indicator for 6+ years after these court orders went into effect is of about the same magnitude – 6.5 per 100,000 – suggesting that the effect of desegregation on crime is persistent.[23] However not all of the coefficients here are very precisely estimated.

Because of the skew in homicide rates in our sample, a proportional response model might be more appropriate. In our OLS log dummy specification we estimate

[23] 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.

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 suggests somewhat larger proportional effects, equal to 17 percent during the first 5 years after the court orders and 27 percent thereafter.

Note that all of our estimates in Table 3 condition on region-year fixed effects to account for the regional pattern in the timing of court desegregation orders (Figure 1). To further examine the robustness of the results, we re-estimate our OLS levels model and our QML count model controlling for interactions of baseline county socio-demographic characteristics and year effects, as in equation (4),[24] and re-estimate our OLS levels model controlling for county-specific linear trends. Table 3 shows that our findings are fairly robust to conditioning on these additional variables.

The bottom panel of Table 3 tries to improve the precision of our estimates by expanding the age range of black victims that we consider, since school-age homicide offenders often have older victims,[25] and so in Panel B of Table 3 we expand our focus to include black homicide victims ages 15-24. Compared to the results for black victims ages 15-19, the estimated effects for black victims 15-24 are even larger in absolute terms (equal to between 9 and 11 per 100,000 over the long term) but are

[24] The base year demographic characteristics, allowed to influence crime in a time-variant manner (see equation (2)) are: 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.

[25] Cook and Laub (1998) examine SHR data from the period 1985-95 and find that around three-quarters of killers age 13-17 were younger than their victims, and around two-thirds were three or more years younger than their victims. These results suggest that homicide *offending* by school-age youth should affect victimizations to young adults as well.

roughly similar in size or slightly smaller in proportional terms, given the baseline homicide rate for blacks is much higher for those 15-24 than 15-19 (45.2 vs. 29.0).

Table 4 shows that court 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 homicide victimizations during the first five years after these orders go into effect. By 6 years after the court orders are implemented homicide victimization rates decline by around 2 per 100,000, or a proportional impact of roughly one-fifth. These findings are generally consistent across model specifications in Table 4 and for models that examine victimizations to people ages 15-19 or expand the focus to examine those 15-24.

To learn more about the time path of how desegregation orders affect homicide victimization we estimate a version of equation (1) that includes an expanded set of indicators for the years before and after these court orders go into effect. Figure 4 shows that the results are fairly imprecise when we use data just on victimizations to blacks 15-19, but when we take advantage of the additional statistical power that comes from looking at black victims age 15-24 we see evidence for some break in trend around the time the desegregation orders go into effect (Figures 5 and 6). In general homicide victimization rates are relatively stable in the years before these desegregation orders go into effect, but seem to decline thereafter. Five or six years after desegregation implementation, black youth homicide has declined by about 10 per 100,000 (Figure 5), or a proportional change of about 20 percent (Figure 6). Looking across the panels we see the results are qualitatively similar when we also

condition on base-year demographic characteristics (panel B of Figures 5 and 6) or control for county-specific linear trends (panel C of Figure 5).

For whites ages 15-19 there appears to be more of a delay in when homicide victimization rates decline following desegregation orders, when estimated using either OLS in levels (Figure 7) or proportional response models (Figure 8). Again there is very little evidence for any pre-existing trend before the desegregation orders go into effect. The gradual impact of desegregation orders on white and to some extent black homicide victimization rates might reflect the fact that the share of prime-age offenders exposed to school desegregation orders increases over time.[26] More generally the amount of exposure that people of any given age will have to desegregated schools will increase with time since the court order goes into effect. Below we return to the question of what the mechanisms at work in more detail.

C. Specification Tests for Homicide Victimization

The results that we have presented up to this point are un-weighted. Appendix Figure A1 displays results for models including the full pre and post vectors which are weighted by the black juvenile county population. The results are similar to those discussed above. Appendix Figure A2 presents results which use the full sample – that is, these estimates now include the 8% of districts which do not meet our earlier requirement of contributing a sufficient number of points to the pre and post desegregation vectors. Again, the results are similar to those above. The results for

[26] For example, if most high school seniors are 18 years old then people age 20 will not have attended desegregated schools until at least 2 years after these court orders go into effect, even ignoring that it usually takes a few years to fully implement these desegregation plans (Figure 3).

whites are also similar when we either weight by the county population of white youth or use the full sample.

Are the results that we estimate for homicide victimization really due to school desegregation orders, or to other factors that are changing coincident to these court orders? The fact that we do not see systematic differences between desegregating and other counties in the immediate years before these court orders go into effect provides some partial reassurance against a counter-explanation that rests on some omitted variables story. Our findings are also not very sensitive to conditioning on base year demographic characteristic-year interactions or county-specific linear trends.

Another way to address this issue is to examine whether we see impacts of school desegregation orders on youth outcomes that should logically not be affected. Table 5 presents the results from such a falsification exercise, where we estimate the "effect" of school desegregation orders on mortality rates to black and white youth from major illnesses,[27] which should not be affected by the same school quality, peer influence or other mechanisms hypothesized to drive desegregation impacts on academic and non-academic behavioral outcomes. The mortality rate from illness in our sample for those aged 15 to 19 is 13.0 per 100,000, compared to a rate of 10.7 for homicides. We estimate this separately for whites 15-19 and blacks 15-24 (the group for which we have more statistical power to detect homicide impacts, as shown above), and use our three separate estimation approaches – OLS levels, the QML count model,

[27] 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.

and the OLS log dummy model – on the parsimonious, two-point post vector model. As shown in Table 2, the estimated "effects" of desegregation orders on mortality from illness are both much smaller in magnitude than what we see for homicide victimization rates and are generally quite imprecise.

A more specific concern with our estimates comes from the possibility of measurement error in our county population variable. As discussed above, if the imputed census population figures for inter-censal years miss population loss in our counties, our estimates will be downwardly biased. This is primarily a concern for the white estimates, as desegregation would not be expected to produce black population loss (indeed it might lead to black population gain which could lead to *upward* bias in the black estimates). One reason to believe our homicide estimates do not suffer from such bias is the lack of any impact of desegregation on death from illness, since our measure for that cause of death uses the same denominator data as our homicide rates.

In order to further address this concern in Table 6 we re-calculate our estimates restricting our sample to the decennial census years 1960 through 1990, which should be measured fairly accurately. The estimated impacts of desegregation orders on white homicide victimization 6+ years after the orders go into effect from the OLS models are at least as large as in our full county-year panel, but obviously much less precisely estimated given we lose about 90% of our county-year observations by focusing just on census years. The QML count model estimates are smaller than when the full panel is used. Estimates of a larger magnitude, however, are obtained with minor changes to the specification. Restricting the sample to 1960 – 1980 in column (4) – almost all of

the districts were desegregated by 1980 (Figure 1) – produces larger estimates. Altering the first post-desegregation variable such that it includes only years 2 through 5 of desegregation (and the first year is included in the omitted category) also produces larger estimates. This alteration is justified by the fact that there is little evidence of an effect on white homicides in the first year of desegregation (Figures 7 and 8).[28] The black estimates are generally of a similar magnitude as those using the whole sample.

D. Homicide Offending

The results presented so far suggest that court desegregation orders reduce homicide victimization rates to both blacks and whites. Our finding of a beneficial effect for whites might at first glance seem surprising, given that previous research finds no detectable impact of these court orders on dropout rates for whites (Guryan, 2004, Lutz, 2005). But the reduction in white homicide victimizations could in principle be driven entirely by behavioral changes among blacks.

To examine whether there is a behavioral response by blacks and whites we use data on homicide offenders from the SHR. One drawback from the SHR is that these data are available only back to 1976. As a result we will not be able to estimate the effects of court orders on youth homicide offending during the first few years after these orders go into effect for the relatively large share of districts in our sample where court orders were imposed before 1976 (see Figure 1). We will obviously have more statistical power to detect impacts on longer-term changes in people's propensity

[28] When the data is restricted to census years, each district contributes only a single observation to the identification of the 1 – 6 years of desegregation coefficient. It is therefore not overly surprising that the estimates are sensitive to such a minor change in the specification.

towards homicide offending.[29] All of our SHR results are subject to the important qualification that data on offenders are available only in cases where there is a known suspect and local law enforcement officials choose to report these data to the FBI as part of the (voluntary) SHR program.[30]

With these caveats in mind, the SHR data provide at least suggestive evidence for reductions in homicide offending by both blacks and whites. Column (2) of Table 7 provides some evidence for a decline in homicide offending by blacks ages 15-24 after court desegregation orders go into effect. (Column 1 replicates our VS victimization results over the period 1976+ for comparison). Estimates from the QML count model are much more precise than those from OLS and suggest a 30 percent reduction in black homicide offending rates within the first 5 years after the court orders go into effect, and a decline of 43 percent 6+ years out. Interestingly, column (3) in Table 7 suggests that the rate at which blacks commit murder against whites also declines, despite the fact that school desegregation increases the proximity and interactions between blacks and whites (although the estimate in panel B is imprecise).

Table 8 provides more mixed evidence for short-term changes in homicide offending by whites, with the QML count models suggesting reductions in white

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[29] 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.

[30] Another concern with our SHR results is that we are using population data from the VS to calculate denominators for homicide offending rates, even though not all police agencies within our counties will be voluntarily submitting data to the FBI. This would only bias our results if there were some systematic relationship between local police data reporting and the timing of when desegregation orders goes into effect. In any case in the next version of the paper we will explore the sensitivity of our results to re-estimating the SHR homicide offending data using UCR data on the population living in just those areas whose local police report to the FBI.

offending rates of around 10 percent although these are not statistically significant. However as noted above these SHR estimates for short-term homicide offending patterns are subject to the important qualification that we omit data from the large share of districts that desegregated before 1976.

E. Mechanisms

As discussed above, one potential mechanism behind our results is cross-county migration. Even if population migration does not introduce measurement error into the denominator of our homicide rate variable, migration sparked by desegregation could produce a county population with a lower propensity for crime. We would most likely expect out-migration for whites, i.e. "white flight," while for blacks we might expect in-migration as black families seek to obtain the improved educational environment produced by desegregation plans. The estimates we have presented above could in principle simply pick up the effects of changing population characteristics in our counties rather than any actual behavioral response by county residents.

In order to explore this possibility, in Table 9 we estimate equation (2) with the log of the county population of 15 to 19 year olds, separately for blacks and whites, with the sample 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 fairly imprecise. As with our homicide findings, the results for population migration do not appear to be sensitive to including the vector of base demographic characteristics with time varying coefficients.

As another check on whether our findings are driven by compositional changes in county population, we use data from Census micro-data from 1970 and 1980 and estimate a difference-in-difference model that compares changes over this decade in the characteristics of 16-17 year olds in county-groups that included a district with a desegregation order imposed between 1970-80 versus in other county-groups where district court orders went into effect either before 1970 or after 1980. The key explanatory variable of interest in this case is an interaction between an indicator for data from the 1980 census with an indicator for having a desegregation order imposed in 1970.[31] We see no evidence of any statistically significant changes in the average family income of white or black youth, or that the probability that the child's mother or father had attended college (Table 10). If anything many of these point estimates are in the direction of the county population becoming somewhat more criminogenic, such as negative estimates for income (see Jacob and Ludwig, 2007).

While there is no evidence of "white flight" out of the counties, Table 11 presents some evidence that whites do move from school districts subject to desegregation orders to other districts within the same county that are not subject to court-ordered school desegregation. Panel A shows that the ratio of white enrollment in districts subject to desegregation orders to the total number of white school-age children in the county declines by between 4 and 6 percent after these court orders go into effect (the sample is again restricted to census years). Panel B provides another

[31] The estimates produced using the micro census data use the county group as the unit of observation. The county group is a larger geographic area than the county. See Guryan (2004) for a more detailed discussion of both the county groups and the difference-in-difference specification.

way of seeing this – the log of enrollment in the desegregated district declines by 15-20 percent. In other results not shown here we find no evidence of an increase in white private school enrollment in these counties. Reber (2002) also finds that desegregation orders reduce the number of white children attending a school district.

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 across districts within the county to escape from court desegregation orders. If the districts not subject to desegregation orders are somehow less criminogenic than districts where these orders go into effect this could provide another reason for why we see a decline in homicide offending and victimization for whites, although we have no way to directly test this hypothesis. There is no evidence that desegregation impacts the number of black students attending a school district.

We can provide some at least indirect evidence on what other mechanisms might matter most for how desegregation orders affect homicides for whites and blacks by interacting changes in our measures of school segregation and school spending with our indicators for implementation of court orders. The changes in the segregation indices and spending measures are defined as the changes from one year prior to desegregation to four years after desegregation and hence are five-year changes. Table 12 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. That is, in columns (1) and (6), which display the OLS level and QML count specifications, respectively, we see large and statistically significant interaction terms between the

indicator for 1-5 and 6+ years after desegregation orders are imposed with the change in the school districts exposure index. The other columns in Table 13 show there are no statistically significant interactions between our "treatment" indicators (years post desegregation order) and either the dissimilarity index or school spending.[32]

F. Long-Run Response

Table 14 presents estimates for long-term homicide offending from the SHR. The outcome of interest is homicide offenses by people ages 35 to 44, while the key explanatory variable is an indicator equal to one if a desegregation order went into effect 25+ years before the calendar-year in which the SHR homicide offending data are measured. Put differently, our explanatory variable captures whether a desegregation order was in place when people ages 35-44 would have been age 19 or younger. Columns (2) and (3) show that our QML count model provides evidence for a decline in homicide offending of around 16-18 percent for whites (Panel A) and 13 or 14 percent for blacks (Panel B).

Table 15 shows the results of a falsification check that comes from estimating a model with an indicator for having a court desegregation order in effect 20-24 years before the SHR county-year observation, as well as for having an order 25 -30 years before and 30 plus years before. For people ages 35-44, a desegregation order imposed 20-24 years ago would have occurred when they were, on average, too old to have

[32] 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.

been "exposed" to court-ordered desegregation.[33] The coefficients for post desegregation years 20-24 are in the QML count models much smaller in absolute value than what we see for years 25 – 29 and 30 plus.

Table 15 also demonstrates that those districts which had the largest increase in black-white exposure at the time of desegregation saw the largest long-run decrease in adult homicide offending (the interaction term in column (2) is marginally significant at the 10% level and the main effect and interaction terms are jointly significant at the 5% level). This provides evidence of a link between the efficacy of desegregation in promoting inter-racial contact and the long-run decrease in adult homicide offending. In contrast to the white youth results, there is no evidence of a connection between the long-term decrease in white homicides and the change in school spending.

VII. CONCLUSIONS

Our estimates suggest that court-ordered school desegregation reduces homicide victimization and offending rates for blacks by around 25 and 40 percent, respectively. We also find evidence of a reduction in homicide victimization rates to whites of around 25 percent and some signs of a decline in white homicide offending as well. By way of comparison, Guryan (2004) finds that desegregation orders reduce black dropout rates by 2 or 3 percentage points, which equal 16-25 percent of the control mean in his sample. More generally it is useful to keep in mind that criminal behavior (like school dropout, for that matter) are behaviors that are concentrated

[33] Assuming that individuals finish high school when they are 17 years of age, the 35-44 year old age cohort would have been exposed to an average of only 1 ½ year of school desegregation 20 to 24 years after the start of desegregation. In contrast, the 35-44 year old age cohort would have been exposed to an average of 5 ½ years of school desegregation 25 to 29 years after the start of desegregation.

among the left tail of the behavioral distribution. Studies in criminology consistently find that around 6 percent of each birth cohort is responsible for around 60 percent of all crime committed by that birth cohort (see for example Wolfgang, Figlio, and Sellin, 1972; Tracy, Wolfgang and Figlio, 1990). School desegregation orders need change the behavior of just a small share of all high-risk youth to generate large proportional changes in overall criminal activity.

The main threat to our results is the possibility that courts impose school desegregation orders in response to trends in black or white youth outcomes. But our specifications show relatively little evidence of pre-existing trends in homicides to black or white youth. In addition we find no detectable impacts of court-ordered school desegregation orders on youth mortality rates from causes that should not be affected by desegregation, namely, illnesses. And in our analysis of long-term homicide offending behavior in the SHR we do not find any evidence of behavioral response among birth cohorts who were too old to have been affected.

One implication is that previous studies focused on academic outcomes or adult earnings will understate the social welfare gains from school desegregation. Our preferred count model estimates imply something on the order of 10 fewer homicides per 100,000 blacks and 2 fewer homicides per 100,000 whites (Tables 3 and 4, respectively). Cohen and colleagues (2004) estimate that the social costs per homicide equal around $9.7 million in current dollars. Our estimates thus imply social benefits of nearly $1,000 per black student and nearly $200 per white student. As one benchmark for assessing the magnitude of these benefits, Hanushek's figures (2003,

Table 1) suggest average per-pupil public school spending over our study period in the U.S. overall was probably something on the order of $4,500 (in constant 2000 dollars).

Our findings may also have implications for understanding larger trends in black and white youth homicide rates over time. Consider, for example, the decline in the ratio of black to white homicide arrest rates for people under 18 that occurred from the late 1960s through mid-1990s (Cook and Laub, 1998, p. 44), the causes of which have remained poorly understood. As noted above, the sample of school districts that we study here accounted for fully half of all minority-student public school enrollments in the U.S. as a whole in 1968. A very large share of the court school-desegregation orders that we examine were implemented in the window between 1968 and 1973. While our estimated effects of school desegregation orders on homicides are larger in proportional terms for whites than blacks, given the substantially higher homicide rate for blacks the impact is larger in absolute terms for blacks than whites. Our results thus suggest that part of the long-term convergence in homicide offending rates by black and white youth may be due to court-ordered school desegregation.

Our findings could also potentially have implications for understanding black and white homicide trends more generally. Youth account for a disproportionately large share of homicide offenders (15 percent of all homicide arrests in 1997 were made to people under 18 years of age; see *Sourcebook of Criminal Justice Statistics, 1998*, p. 338). School desegregation could also have larger impacts on aggregate homicide trends if people who attend school after court-ordered desegregation

experience changes in their lifetime offending behavior, a possibility that receives some support from our analysis of long-term homicide offending rates in the SHR.

It is intriguing to note that overall black homicide arrest rates to people of all ages increased continuously and dramatically from 1953 to 1968, from around 5 to 35 per 100,000, as seen in Figure 9. But this increase in black homicide arrest rates stopped abruptly in the late 1960s (Jaynes and Williams, 1989, pp. 458-9) – just as many of our desegregation orders begin to go into effect.

Our results also provide a candidate explanation for why homicide rates declined so dramatically in the U.S. for most of the 1990s but then this progress halted at the end of the decade, despite the fact that there were few changes in the fundamental factors that seem to have driven the decline in crime – increased imprisonment and police spending, ebbing of the crack cocaine epidemic, and abortion legalization (Levitt, 2004).[34] Our findings suggest one countervailing force that may have occurred over this period is the growing number of large public school districts that had their court desegregation orders dismissed over the decade (Clotfelter, Ladd and Vigdor, 2005, Lutz, 2005).

[34] The FBI's Uniform Crime Reports for 2005 show that the homicide rate declined from 1991 to 1999 from 9.8 to 5.7 per 100,000, but has held relatively steady since then, and was equal to 5.6 in 2005, the latest year for which data are available. See http://www.fbi.gov/ucr/05cius/data/table_01.html

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DATA APPENDIX

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.[35]

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

[35] The primary source of information about other types of crime 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. 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. The other major source of crime information is the National Crime Victimization Survey (NCVS), although the NCVS is only intended to provide estimates that are nationally representative and in any case geographic identifiers are not made available for NCVS data.

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 Supreme Court decisions. 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 constructing SHR homicide offending rates the VS county population data are not entirely appropriate, because not all local police agencies report data to the FBI and so the appropriate denominator would be the population living in jurisdictions within the counties covered by agencies that submit SHR data. For the next version of this paper we will re-estimate our SHR offending results using population data taken from the UCR, which provides county population totals for just agencies that report data to the FBI for the overall UCR system. The UCR county population count should be close but not identical to the ideal SHR population count since not all homicides that show up in the UCR are included in the SHR (in recent years this is about 90%).

Figure 1
Desegregation Implementation Dates

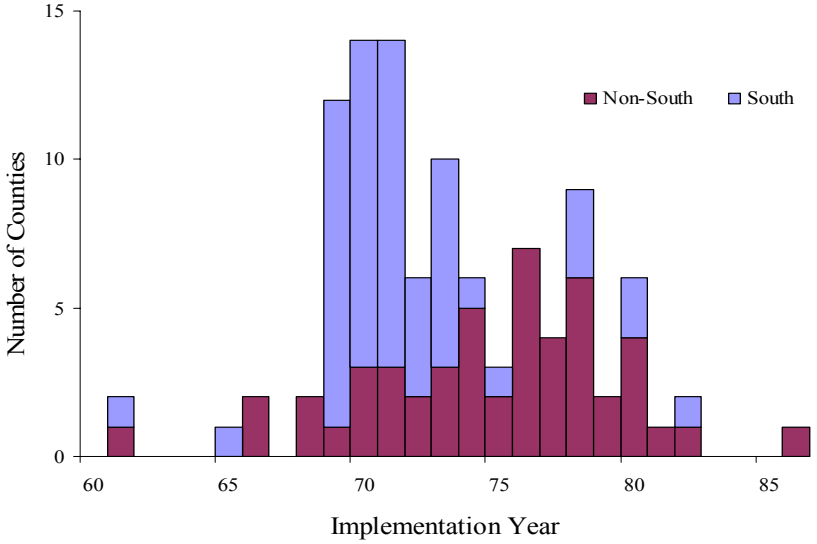
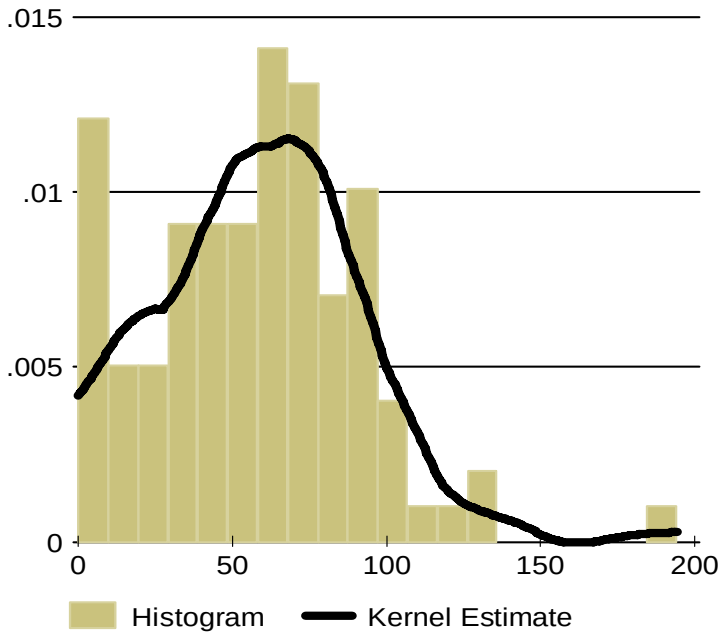


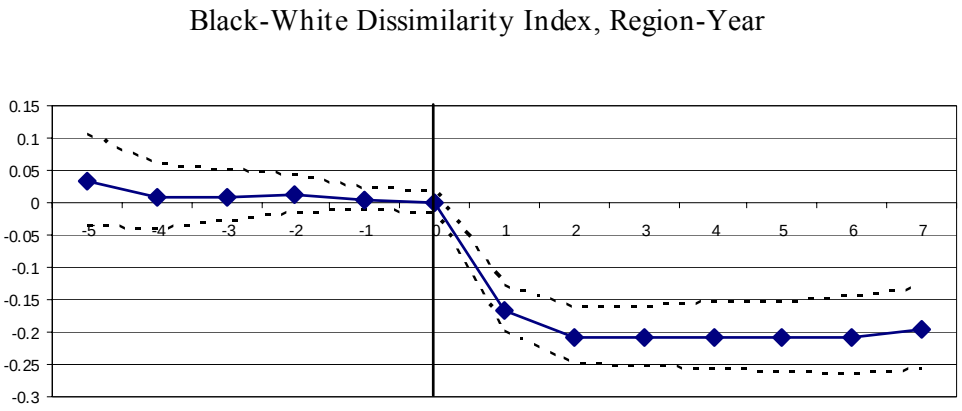
Figure 2
Distribution of 1975 Black Age 15 – 24 Homicide Rates per 100,000



Note. The figure displays histogram and kernel density estimates of the 1975 black 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 3: Segregation Indices

Panel A:



Panel B:

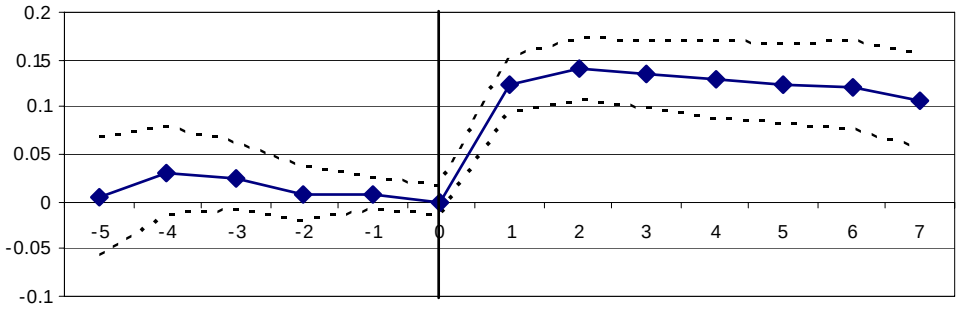
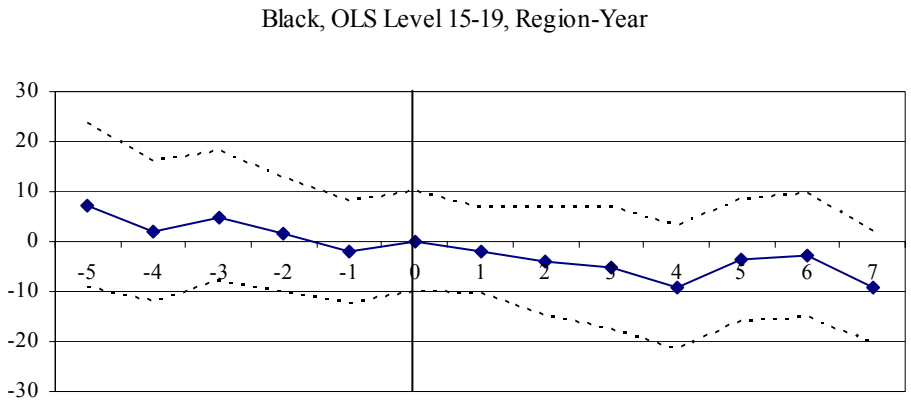


Figure 4: School Desegregation & Homicide Victimizations, Black Youth, 15-19
Panel A:



Panel B:

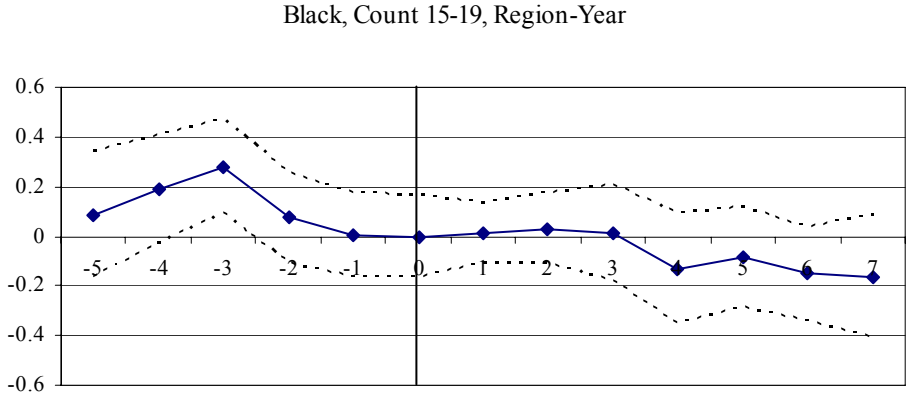
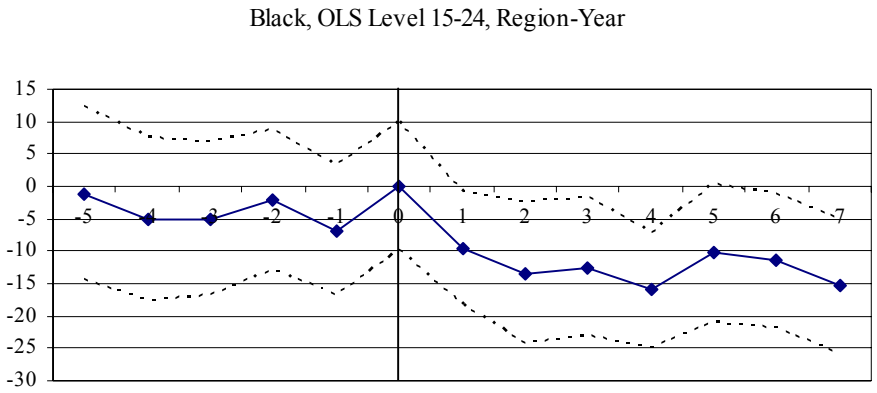
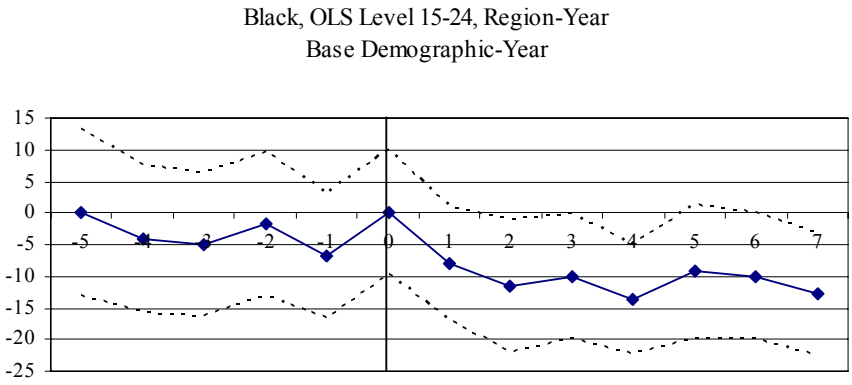


Figure 5: School Desegregation & Homicide Victimizations, Black Youth, 15-24, OLS Levels Models

Panel A:



Panel B:



Panel C:

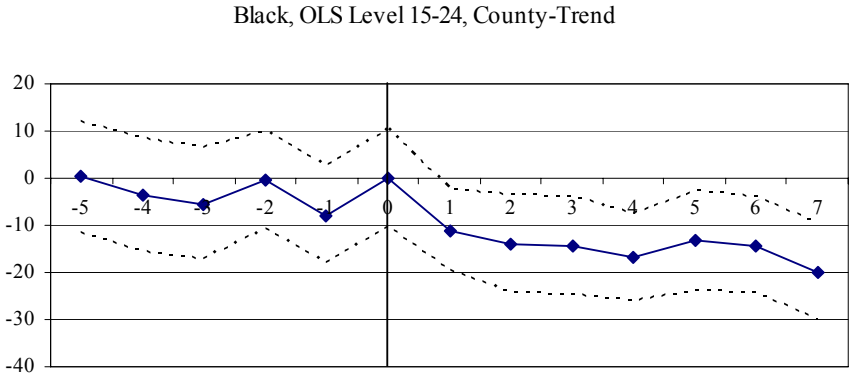
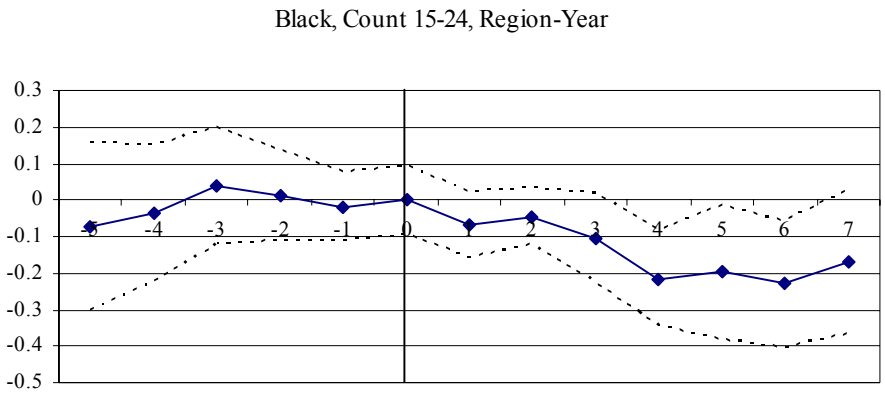


Figure 6: School Desegregation & Homicide Victimizations, Black Youth 15-24, Proportional Response Models

Panel A:



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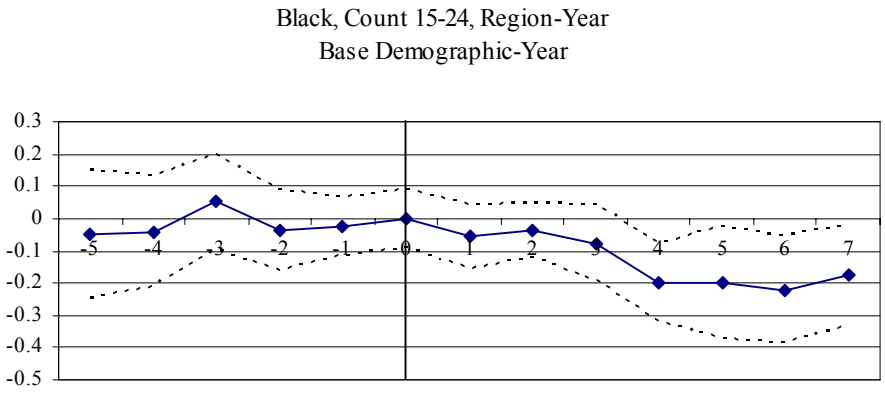

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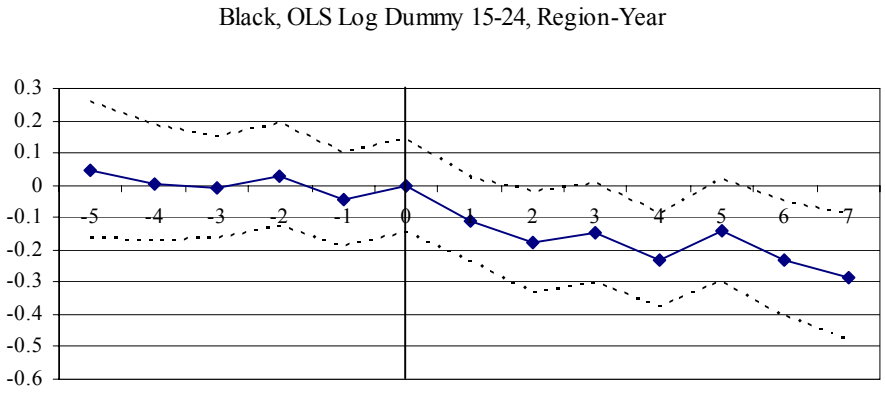
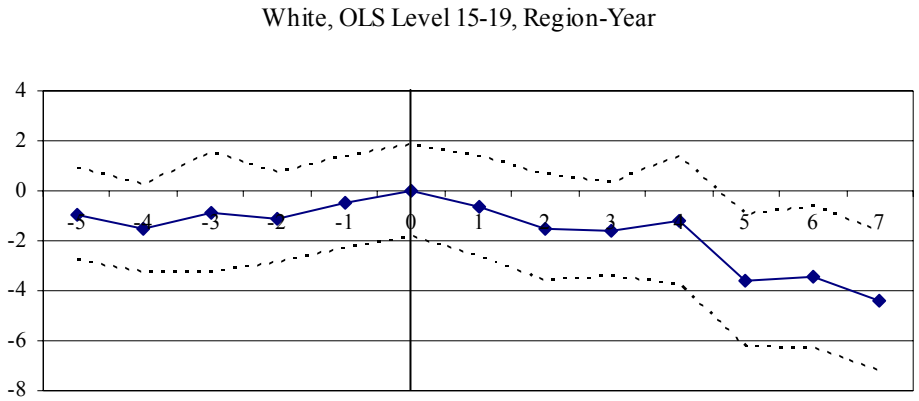
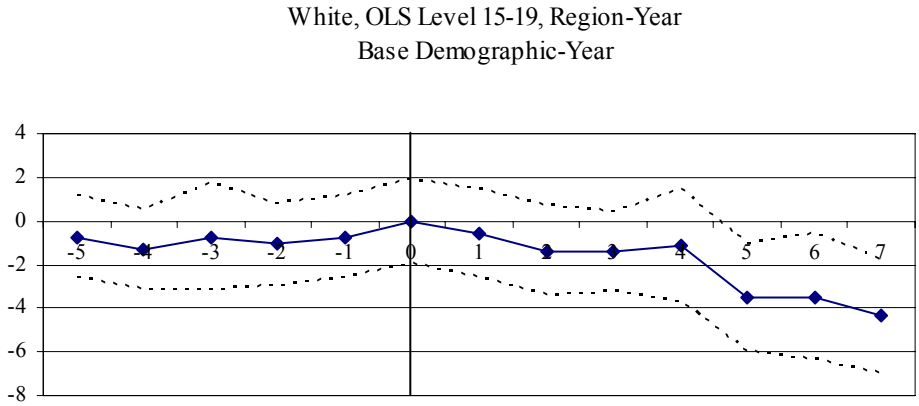


Figure 7: Homicide Victimization, White Youth 15-19, OLS Levels
Panel A:



Panel B:



Panel C:

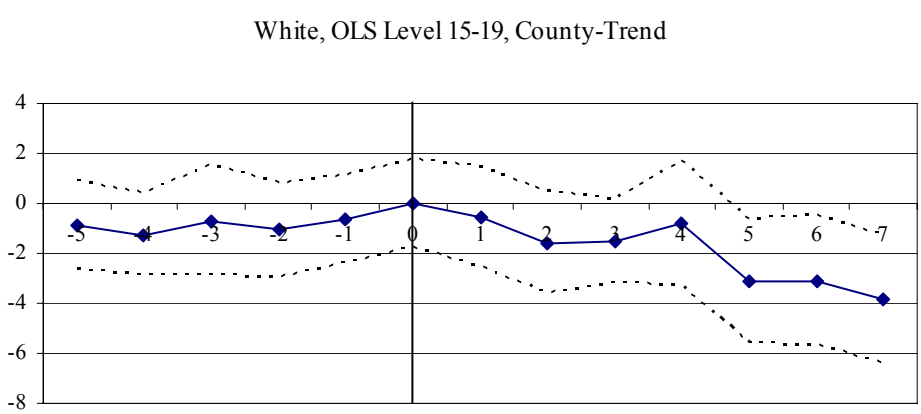
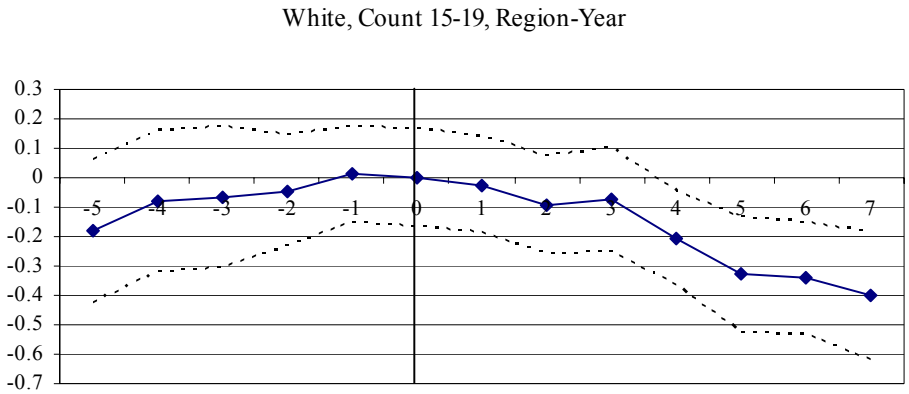
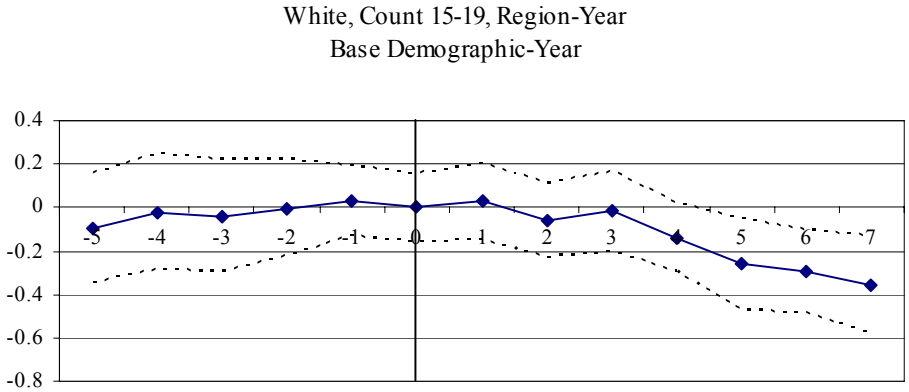


Figure 8: School Desegregation & Homicide Victimizations, White Youth 15-19, Proportional Response Models

Panel A:



Panel B:



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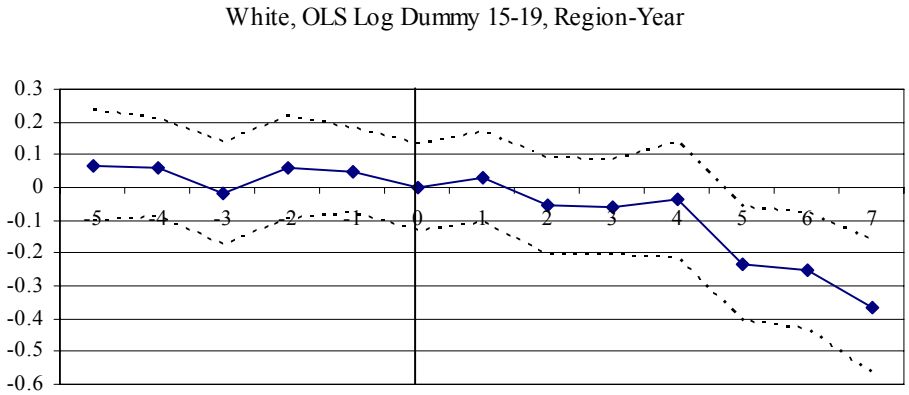
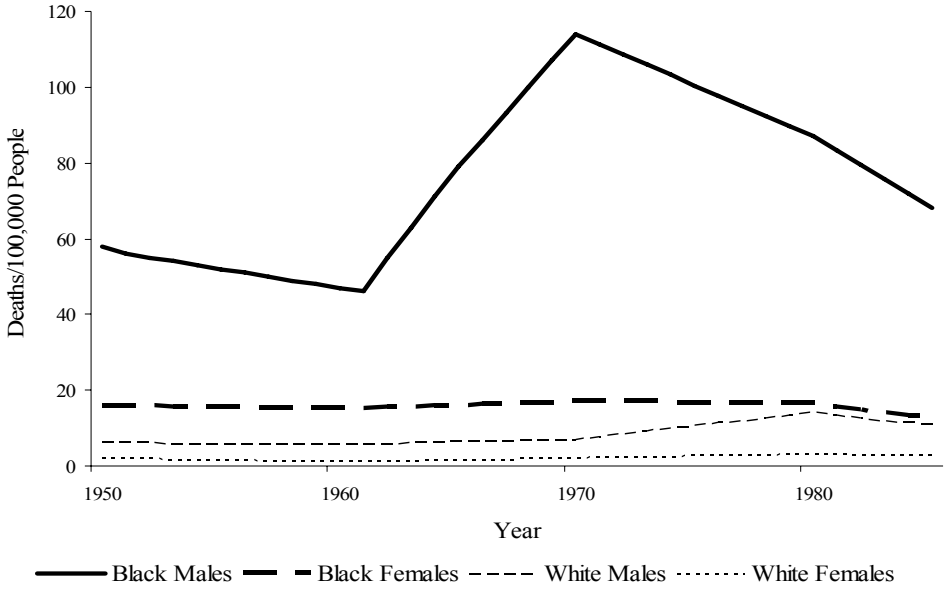
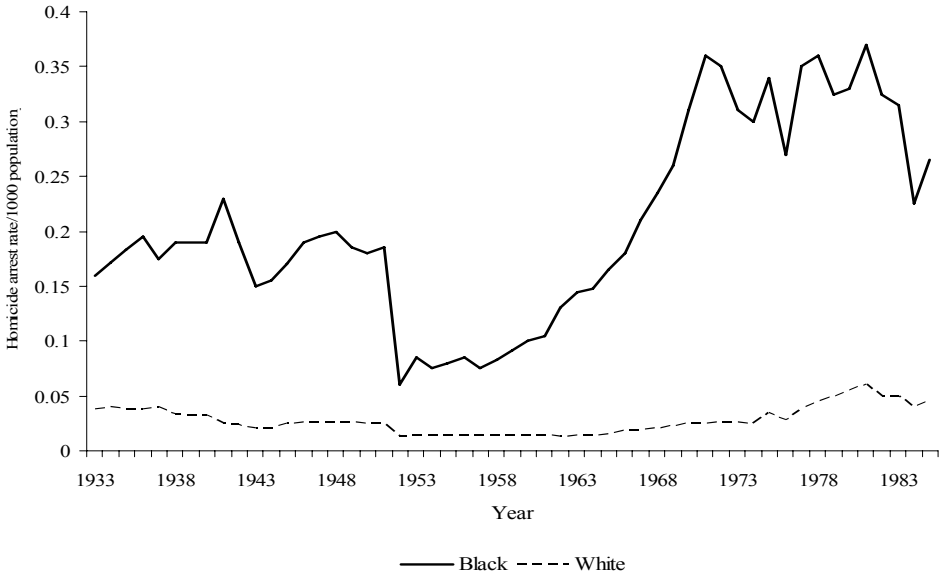


Figure 9
Historical Homicide Rates

Panel A: Homicide rates for people aged 15-24

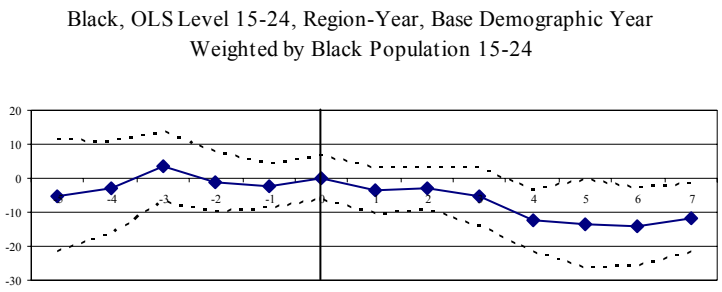


Panel B: Homicide arrest rates, by race, 1933-1985



APPENDIX

Figure A1: Black Results Weighted by Black Population
Panel A:



Panel B:

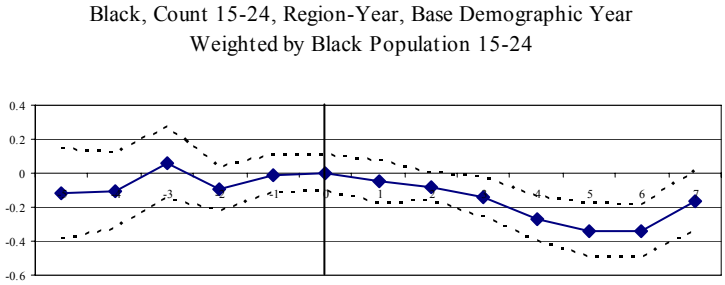
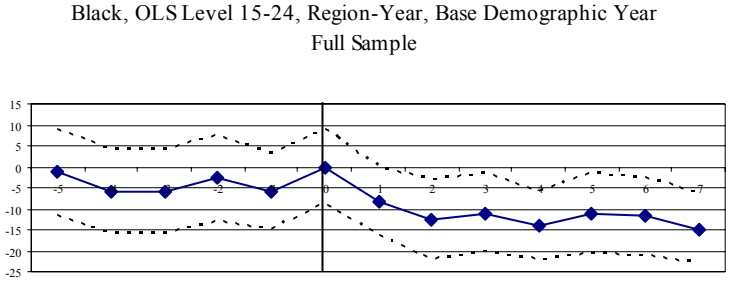


Figure A2: Black Results Using Full Sample
Panel A:



Panel B:

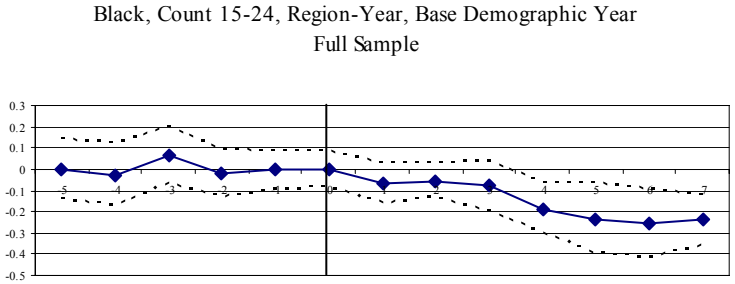
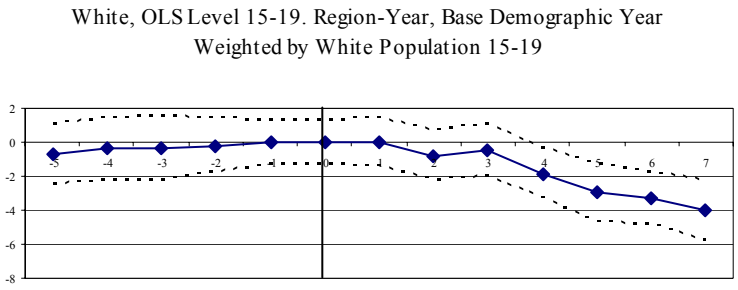


Figure A3: White Results Weighted by White Population
Panel A:



Panel B:

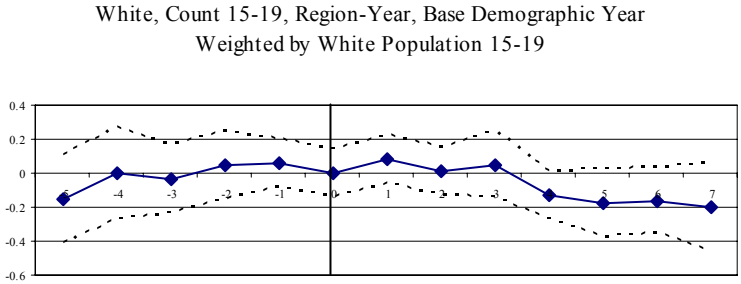
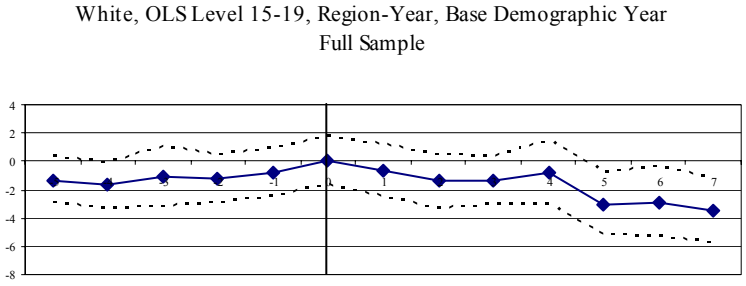


Figure A4: White Results Using Full Sample
Panel A:



Panel B:

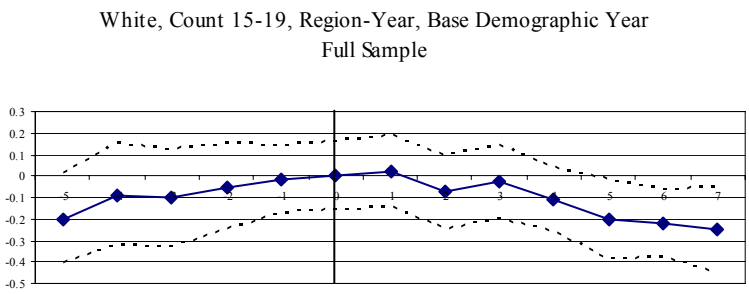


Table 1
Descriptive Statistics

	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
B. Homicide rates per 100,000				
Total	10.8	6.6	11.3	14.0
Total white	6.3	3.3	6.1	9.1
Total black	37.8	31.1	44.7	41.2
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

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
Effect of Desegregation Plan on Local Public Good Provision

	(1)	(2)	(3)	(4)
	A. Ratio of Education Expenditures to Population age 5 - 19			
Post Desegregation Years 1 - 5	0.17	0.16		
	(0.09)	(0.08)		
Post Desegregation Years +6	0.16	0.16		
	(0.09)	(0.09)		
Post Desegregation			0.18	0.16
			(0.09)	(0.08)
	B. Ratio of Police Expenditures to Population			
Post Desegregation Years 1 - 5	0.19	0.17		
	(0.28)	(0.28)		
Post Desegregation Years +6	-0.23	-0.29		
	(0.44)	(0.42)		
Post Desegregation			0.25	0.24
			(0.27)	(0.27)
Number of Observations	419	419	419	419
Region * Year Effect	X	X	X	X
1960 County characteristics * Year		X		X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent 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 thousands of 1990 dollars. The sample includes the following years: 1972, 1977, 1982 and 1987.

Table 3
Black Homicide Victimization

	Levels			Proportional Response		
	OLS			QML Count		OLS Log Dummy
	(1)	(2)	(3)	(4)	(5)	(7)
			A. Black Age 15 - 19			
Post Desegregation Years 1 - 5	-5.89	-5.05	-5.14	-0.17	-0.16	-0.08
	(2.86)	(2.84)	(3.01)	(0.07)	(0.07)	(0.05)
Post Desegregation Years 6+	-6.52	-5.71	-6.26	-0.27	-0.28	-0.15
	(3.93)	(3.87)	(4.00)	(0.09)	(0.09)	(0.07)
			B. Black Age 15 - 24			
Post Desegregation Years 1 - 5	-8.91	-7.45	-8.59	-0.14	-0.11	-0.13
	(2.76)	(2.58)	(2.85)	(0.04)	(0.04)	(0.05)
Post Desegregation Years 6+	-10.55	-9.32	-11.27	-0.23	-0.21	-0.19
	(3.81)	(3.58)	(3.69)	(0.06)	(0.06)	(0.08)
Number of observations	3039	3039	3039	3039	3039	3039
Region * Year Effects	X	X	X	X	X	X
1960 County characteristics * 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
White Homicide Victimization

	Levels			Proportional Response		
	OLS			QML Count		OLS Log Dummy
	(1)	(2)	(3)	(4)	(5)	(6)
			A. White Age 15 - 19			
Post Desegregation Years 1 - 5	-0.48	-0.38	-0.49	-0.05	-0.01	-0.07
	(0.50)	(0.51)	(0.53)	(0.06)	(0.05)	(0.05)
Post Desegregation Years 6+	-2.22	-2.24	-2.23	-0.23	-0.20	-0.24
	(0.82)	(0.80)	(0.87)	(0.09)	(0.08)	(0.07)
			B. White Age 15 - 24			
Post Desegregation Years 1 - 5	-0.49	-0.52	-0.43	-0.05	-0.02	-0.07
	(0.41)	(0.42)	(0.40)	(0.04)	(0.04)	(0.05)
Post Desegregation Years 6+	-2.20	-2.22	-1.97	-0.18	-0.15	-0.24
	(0.72)	(0.66)	(0.68)	(0.06)	(0.06)	(0.07)
Number of observations	3040	3040	3040	3040	3040	3040
Region * Year Effects	X	X	X	X	X	X
1960 County characteristics * 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 5
Falsification Test, Death From Illness

	Level	Proportional response	
	OLS Level	QML Count	OLS Log
	(1)	(3)	(3)
		A. Black 15 - 24	
Post Desegregation Years 1 - 5	-0.32	-0.04	-0.01
	(1.71)	(0.04)	(0.03)
Post Desegregation Years 6+	2.49	0.04	0.04
	(2.87)	(0.05)	(0.05)
Number of observations	3039	3039	3039
		B. White 15 - 19	
Post Desegregation Years 1 - 5	-0.70	-0.06	-0.06
	(0.57)	(0.04)	(0.03)
Post Desegregation Years 6+	-0.51	-0.03	-0.02
	(0.85)	(0.06)	(0.05)
Number of observations	3040	3040	3040
Region * Year Effects	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 and 1980. The dependent variable is the rate of death from illness per 100,000 in columns (1) and (2) and the count of death from illness in columns (3) and (4).

Table 6
Homicide Victimization, Sample Restricted to Decennial Census

	Level	Proportional response			
	OLS	OLS Log	QML Count		
	(1)	(2)	(3)	(4)	(5)
			A. White 15 - 19		
Post Desegregation Years 1 - 5	0.632	-0.031	0.122	0.079	
	(1.173)	(0.095)	(0.156)	(0.152)	
Post Desegregation Years 2 - 5					-0.067
					(0.170)
Post Desegregation Years 6+	-3.362	-0.282	-0.006	-0.377	-0.171
	(1.668)	(0.141)	(0.144)	(0.187)	(0.140)
Number of observations	420	420	404	297	404
			B. White 15 - 24		
Post Desegregation Years 1 - 5	0.730	-0.063	0.017	0.008	
	(1.219)	(0.126)	(0.118)	(0.109)	
Post Desegregation Years 2 - 5					-0.138
					(0.116)
Post Desegregation Years 6+	-2.486	-0.351	-0.120	-0.401	-0.244
	(1.747)	(0.155)	(0.115)	(0.134)	(0.097)
Number of observations	420	420	416	312	416
			C. Black 15 - 19		
Post Desegregation Years 1 - 5	-17.569	-0.175	0.025	-0.002	
	(8.634)	(0.106)	(0.150)	(0.173)	
Post Desegregation Years 2 - 5					-0.106
					(0.181)
Post Desegregation Years 6+	-25.112	-0.300	-0.385	-0.193	-0.452
	(11.568)	(0.158)	(0.203)	(0.249)	(0.217)
Number of observations	420	420	412	264	412
			D. Black 15 - 24		
Post Desegregation Years 1 - 5	-15.929	-0.250	0.103	0.060	
	(9.481)	(0.126)	(0.100)	(0.109)	
Post Desegregation Years 2 - 5					0.001
					(0.124)
Post Desegregation Years 6+	-20.225	-0.175	-0.128	-0.039	-0.196
	(12.528)	(0.163)	(0.145)	(0.123)	(0.158)
Number of observations	420	420	412	300	412
Region * Year Effects	X	X	X	X	X
Sample Restricted to 1960-1980				X	

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The sample is restricted to 1960, 1970 and 1980. The dependent variable is the homicide rate in columns (1) and (2) and the homicide count in columns (3) and (4).

Table 7
Supplemental Homicide Report Data: Black Age 15-24 Homicide Offenders

	Vital Statistics	Supplemental Homicide Report	
	Victim	Offenders	Offenders Against Whites
	(1)	(2)	(3)
		A. OLS	
Post Desegregation Years 1 - 5	-7.05	-3.09	-1.24
	(3.69)	(6.43)	(2.04)
Post Desegregation Years 6+	-9.75	-8.63	-4.40
	(4.64)	(6.93)	(2.29)
Number of observations	1363	1333	1333
		B. QML Count	
Post Desegregation Years 1 - 5	-0.15	-0.30	-0.03
	(0.12)	(0.14)	(0.10)
Post Desegregation Years 6+	-0.26	-0.43	-0.17
	(0.15)	(0.17)	(0.11)
Number of observations	1363	1326	1326
Region * Year Effects	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.

Table 8
Supplemental Homicide Report Data: White Age 15-19 Homicide Offenders

	Vital Statistics	Supplemental Homicide Report	
	Victim	Offenders	Offenders Against Blacks
	(1)	(2)	(3)
		A. OLS	
Post Desegregation Years 1 - 5	-2.98	-0.46	0.34
	(1.22)	(1.33)	(0.46)
Post Desegregation Years 6+	-4.80	0.70	0.29
	(1.60)	(20.81)	(0.62)
Number of observations	1363	1333	1333
		B. QML Count	
Post Desegregation Years 1 - 5	-0.15	-0.19	0.04
	(0.07)	(0.11)	(0.16)
Post Desegregation Years 6+	-0.27	-0.11	-0.10
	(0.11)	(0.15)	(0.23)
Number of observations	1363	1324	1213
Region * Year	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.

Table 9
Effect of Desegregation Plan on County Population

	(1)	(2)	(3)	
	A. Log of White Children in County			
Post Desegregation Years 1 - 5	-0.023	-0.024		
	(0.031)	(0.026)		
Post Desegregation Years +6	0.000	-0.008		
	(0.047)	(0.044)		
Post Desegregation			-0.022	-0.023
			(0.031)	(0.026)
	B. Log of Black Children in County			
Post Desegregation Years 1 - 5	0.053	0.035		
	(0.033)	(0.031)		
Post Desegregation Years +6	0.074	0.051		
	(0.050)	(0.046)		
Post Desegregation			0.054	0.036
			(0.033)	(0.031)
Number of Observations	420	420	420	420
Region *Year Effect	X	X	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. The estimation sample includes the years 1960 - 1990.

Table 10
Effect of Desegregation Plan on Demographic Characteristics of Desegregated County Groups

	Family Income		Mother Attended College		Father Attended College	
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Whites			
70s Desegregator *1980	-395	-742	-0.007	-0.006	-0.007	-0.016
	(515)	(433)	(0.007)	(0.008)	(0.010)	(0.008)
Number of Observations	193,028	193,028	195,113	195,113	195,113	195,113
			B. Blacks			
70s Desegregator *1980	-407	-647	-0.006	-0.018	0.008	0.010
	(513)	(542)	(0.009)	(0.010)	(0.010)	(0.013)
Number of Observations	49,226	49,226	49,963	49,963	49,963	49,963
Region * 1980	X	X	X	X	X	X
1970 School and County Group Characteristics *1980		X		X		X

Note. Standard errors clustered by county-group in parentheses. The dependent variable is given in the column headings. The unit of observation is individual-year. The sample is restricted to individuals between the ages of 15 and 17 who reside in county-groups in the Welch and Light (1987) sample. The estimation sample includes the years 1970 and 1980. 1970 school characteristics include total enrollment, percent of enrolled students who are black, black-white dissimilarity index and the black-white exposure index. 1970 county group characteristics include percent in poverty, percent jobs in manufacturing, percent population black, and median family income.

Table 11
Effect of Desegregation Plan on Percent of Children Attending the Desegregated School District

	White		Black	
	(1)	(2)	(3)	(4)
	A. Ratio of Enrollment at Desegregated School to Children in the Country			
Post Desegregation Years 1 - 5	-0.054	-0.032	-0.005	0.000
	(0.012)	(0.012)	(0.015)	(0.013)
Post Desegregation Years 6+	-0.064	-0.039	0.011	0.014
	(0.015)	(0.016)	(0.019)	(0.019)
	B. Log of Enrollment at Desegregated School			
Post Desegregation Years 1 - 5	-0.196	-0.147	0.029	0.029
	(0.081)	(0.057)	(0.040)	(0.041)
Post Desegregation Years 6+	-0.179	-0.196	0.081	0.060
	(0.111)	(0.082)	(0.050)	(0.051)
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. The estimation sample includes the years 1970 and1980. The school characteristics, interacted with year effects in column (3), include total enrollment, percent of enrolled students who are black, black-white dissimilarity index and the black-white exposure index . The 1960 county characteristics are median income, percent of population which is non-white, percent of population age 25 or greater with a high school degree and the percent of employment in manufacturing.

Table 12
Black Homicide Victimization Interactions

	OLS Levels					QML Count				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post Deseg. Years 1 - 5	-6.72	-8.61	-6.52	-3.59	-0.38	-0.12	-0.10	-0.08	-0.09	-0.08
	(2.99)	(2.62)	(2.97)	(4.18)	(5.72)	(0.05)	(0.05)	(0.05)	(0.06)	(0.07)
Post Deseg. Years 6+	-6.69	-9.88	-6.12	-5.80	-5.33	-0.19	-0.18	-0.14	-0.18	-0.20
	(3.85)	(3.82)	(4.01)	(6.34)	(6.78)	(0.06)	(0.08)	(0.08)	(0.08)	(0.09)
Post Deseg. Years 1 - 5 *	-20.40		-20.44			-0.47		-0.48		
Δ Exposure Index	(9.48)		(9.41)			(0.18)		(0.18)		
Post Deseg. Years 6+ * Δ	-26.22		-26.12			-0.69		-0.66		
Exposure Index	(8.01)		(7.94)			(0.20)		(0.19)		
Post Deseg. Years 1 - 5 *		1.14	0.67				0.12	0.12		
Δ Dissimilarity Index		(6.65)	(6.01)				(0.11)	(0.11)		
Post Deseg. Years 6+ * Δ		3.33	2.81				0.24	0.22		
Dissimilarity Index		(5.51)	(4.90)				(0.16)	(0.15)		
Post Deseg. Years 1 - 5 *					-9.01			-0.04		-0.03
Δ Ed. Expend. Per Pupil					(7.40)			(0.06)		(0.11)
Post Deseg. Years 6 + * Δ					0.44			-0.20		0.11
Ed. Expend. Per Pupil					(5.57)			(0.10)		(0.14)
Region * Year Effects	X	X	X	X	X	X	X	X	X	X
Desegregated after 1972				X	X				X	X
Number of observations	3039	3039	3039	1433	1433	3039	3039	3039	1433	1433

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. In columns (1) - (4) the dependent variable is the black homicide rate per 100,000. In columns (5) - (8) 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.

Table 13
White Homicide Victimization Interactions

	OLS Level				QML Count			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Deseg. Years 1 - 5	-0.53	-0.45	-1.81	-1.29	-0.06	-0.06	-0.12	-0.05
	(0.50)	(0.51)	(1.00)	(1.07)	(0.05)	(0.07)	(0.07)	(0.07)
Post Deseg. Years 6+	-2.27	-2.14	-4.72	-4.03	-0.23	-0.21	-0.31	-0.17
	(0.81)	(0.83)	(1.72)	(1.68)	(0.09)	(0.10)	(0.15)	(0.16)
Post Deseg. Years 1 - 5 *	0.42				0.20			
Δ Exposure Index	(1.32)				(0.34)			
Post Deseg. Years 6+ *	0.21				-0.04			
Δ Exposure Index	(1.41)				(0.31)			
Post Deseg. Years 1 - 5 *		0.17				-0.04		
Δ % white in deseg school		(1.12)				(0.06)		
Post Deseg. Years 6+ *		1.12				-0.20		
Δ % white in deseg school		(1.33)				(0.10)		
Post Deseg. Years 1 - 5 *				-1.61				-0.30
Δ Ed. Expend. Per Pupil				(1.41)				(0.14)
Post Deseg. Years 6 + *				-1.85				-0.44
Δ Ed. Expend. Per Pupil				(1.04)				(0.13)
Region * Year Effects	X	X	X	X	X	X	X	X
Desegregated after 1972			X	X			X	X
Number of observations	3040	3040	1433	1433	3040	3040	1433	1433

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. In columns (1) - (4) the dependent variable is the white homicide rate per 100,000. In columns (5) - (8) 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.

Table 14
School Desegregation and Long Run Homicide Offending - Age 35 - 44

	Levels	Proportional Response		
	OLS	QML Count		OLS Log Dummy
	(1)	(2)	(3)	(4)
		A. Black		
Post Desegregation Years 25+	0.55	-0.14	-0.13	-0.21
	(3.47)	(0.07)	(0.06)	(0.10)
Number of Observations	2659	2643	2643	2659
		B. White		
Post Desegregation Years 25+	-0.65	-0.16	-0.18	-0.03
	(0.54)	(0.08)	(0.09)	(0.04)
Number of observations	2659	2659	2659	2659
Region * Year Effects	X	X	X	X
1960 County characteristics * Year			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) and (3) is the count of homicide offenders. In column (4), the dependent variable is the log of the homicide rate. The sample runs from 1976 - 2002, the years for which the SHR data is avaliable.

Table 15
School Desegregation and Long Run Homicide Offending - Age 35 - 44: Exstensions

	Proportional Response: QML Count			
	Black		White	
	(1)	(2)	(3)	(4)
Post Desegregation Years 20 - 24	-0.03		0.01	
	(0.04)		(0.05)	
Post Desegregation Years 25 - 29	-0.17		-0.16	
	(0.08)		(0.09)	
Post Desegregation Years 30+	-0.18		-0.10	
	(0.13)		(0.13)	
Post Desegregation Years 25+		-0.10		-0.18
		(0.07)		(0.16)
Post Desegregation Years 25+ *		-0.44		
Δ Exposure Index		(0.27)		
Post Desegregation Years 25+ *				0.01
Δ Ed. Expend. Per Pupil				(0.18)
Number of Observations	2659	2643	2659	1267
Region * Year Effects	X	X	X	X

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the count of homicide offenders. The sample runs from 1976 - 2002, the years for which the SHR data is avaliable.

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