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# THE EFFECTS OF SCHOOL DESEGREGATION ON CRIME

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## ABSTRACT

One of the most striking features of crime in America is its disproportionate concentration in disadvantaged, racially segregated communities, which has long raised concern that segregation itself may contribute to criminal behavior. Yet little is known about whether government efforts to reduce segregation can reduce crime. We address this question by studying the most important large-scale policy to reduce segregation in American life - court-ordered school desegregation. Our research design exploits variation across large urban school districts in the timing of when they were subject to local Federal court orders to desegregate. We find that for black youth, homicide victimization declines by around 25 percent when court orders are implemented; homicide arrests decline significantly as well. We also find evidence for spillover effects on other age and race groups, consistent with data indicating a sizable amount of offending across groups and with the fact that offending by different groups is also linked through the police budget constraint. Economic models for a "market for offenses" suggest the influence of this second mechanism should attenuate over time as victims respond to a shift in the supply of offenses by reducing investments in crime prevention. Consistent with this theory, we find police spending declines several years after court desegregation orders are enacted. The only detectable life-course-persistent effects are found among birth cohorts that attended desegregated schools.

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## I. INTRODUCTION

One of the most striking features of crime in America is its disproportionate concentration in disadvantaged, racially segregated communities. For example, in 2008 the homicide rate in Hyde Park, the economically and racially mixed neighborhood that is home to the University of Chicago, was 3 per 100,000. In directly adjacent Washington Park, where over half of residents are poor and 98 percent are African-American, the homicide rate was nearly 20 times as high (57 per 100,000).[1] Nationwide, homicide is by far the leading cause of death for African-Americans ages 15-24, responsible for more deaths in 2006 than the nine other leading causes combined. Because homicide is concentrated among young people, nearly as many years of potential life are lost among blacks from homicide as from the nation's leading killer, heart disease.[2] The costs of crime to society as a whole are also enormous, equal to perhaps 10 percent of GDP in developed nations (Anderson, 1999; Entorf and Spenger, 2002; Ludwig, 2006).

These patterns have generated long-standing concern that segregation itself might cause crime. For example, in its explanation of the riots of 1967, the so-called "Kerner Commission" cited the destructive role of "the black ghettos where segregation and poverty converge on the young to destroy opportunity and enforce failure. Crime, drug addiction, dependency on welfare, and bitterness and resentment against society in general and white society in particular are the result."[3] While economists have long been interested in the causes of racial segregation (Schelling, 1969; Becker and Murphy, 2000; Card, Mas and Rothstein, 2008), recent years have seen a growing literature about the possible effects on behavior from segregation and non-market social interactions more generally (Cook and Goss, 1996; Glaeser, Sacerdote and Scheinkman,

[1] See www.cchsd.org/cahealthprof.html and https://portal.chicagopolice.org/portal/page/portal/ClearPath
[2] These calculations are for years of potential life lost before age 65. http://www.cdc.gov/injury/wisqars/index.html
[3] http://www.eisenhowerfoundation.org/docs/kerner.pdf

1996; Cutler and Glaeser, 1997; Becker and Murphy, 2000; Gaviria and Raphael, 2001). The mechanisms that produce segregation can also contribute to increased variation across areas in the quality of local public goods such as schools or police (Becker and Murphy, 2000), which could also influence the costs and benefits of crime.

Previous studies provide evidence that criminal behavior is affected by social context, but these studies are not informative about whether large-scale policy efforts to re-sort people across social settings can reduce the overall volume of crime in society. A large non-experimental literature seems to find the strongest evidence for "neighborhood effects" in the area of criminal behavior (Sampson et al., 2002). While these studies are susceptible to concerns about selection bias arising from endogenous sorting of people across settings, studies using stronger research designs yield similar findings. Kling, Ludwig and Katz (2005) find that winning a randomized housing-voucher lottery that enables families to move into less-distressed areas reduces violent-crime arrests for youth by 38 percent, although property-crime arrests increase among males. Deming (2009) finds winning a public school-choice lottery in Charlotte reduces the chance of felony arrest for high-risk youth by 46 percent. Cullen, Jacob and Levitt (2006) find that winning a lottery to attend a higher-achieving school in Chicago reduces arrests by nearly 60 percent.

But lottery studies of moderately-sized housing or school choice programs can only identify partial equilibrium effects, in part because they only consider the effects of moving on the movers. If peer influences on behavior are constant and linear in peer characteristics, or if the effects on criminal behavior observed in the lottery studies are caused by moving to higher-quality schools and if school quality is in fixed supply, then re-sorting people across social settings could simply redistribute crime, not change the overall crime rate. The intended effects of large-scale desegregation efforts could also be offset by private responses, which could range from enrolling children in private school to residential relocation to outright violence. Evidence about general equilibrium effects will necessarily require research designs that do not have the benefit of randomized variation from wait-list lotteries for government programs.

Our paper seeks to empirically estimate the system-level impacts on crime from one of the largest-scale government efforts to reduce segregation in American life – court-ordered school desegregation. Government efforts to desegregate K-12 schools began with the U.S. Supreme Court's 1954 decision in *Brown v. Board of Education* (347 US 483),[4] which former Solicitor General Walter Dellinger called "the most important legal, political, social and moral event in twentieth-century American domestic history" (Williams, 1998, p. 400). Our study exploits the fact that most large school districts were slow to desegregate after *Brown*, and so were forced to desegregate by local Federal courts in response to lawsuits by the NAACP. Idiosyncratic differences across districts in the timing of court desegregation orders is our source of identifying variation, which is plausibly orthogonal to other determinants of youth outcomes given that the NAACP seems to have filed cases strategically when and where they were most likely to win, rather than to maximize short-term social benefits. Guryan (2004) and Reber (2005) show that these court orders reduce levels of school segregation, despite movement of

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

whites to suburbs and private schools. Guryan (2004) finds that these court orders reduce black dropout rates by around 25 percent, with no detectable effects on the schooling of whites.[5]

Our main finding is that court-ordered school desegregation is associated with very large declines in homicide victimization rates for school-age blacks, around 25 percent, with equally large declines in homicide arrests. We focus on homicide because this is widely regarded as the most reliably measured crime, and accounts for a disproportionate share of the total costs of crime. These crime impacts on school-age blacks are presumably due at least in part to their improved schooling outcomes; we find effects that are as large during the summer months as during the school year, and these effects persist into middle age. Our estimates can explain about one-quarter of the dramatic (but not widely-noted) convergence over time in black and white offending rates: From 1969 to 1999, the ratio of violent crime arrests to blacks vs. whites under 18 declined from 12:1 to 4:1, with most of the convergence occurring during the 1970s (Cook and Laub, 1998, 2002) – when most of the court orders we study were imposed.

Also of key interest are the spillover effects induced by court school-desegregation orders. Social interactions occur disproportionately, but not exclusively, among people of the same "type" (defined by age and race). Below we present some tabulations from FBI crime data documenting that a substantial amount of violent offending occurs across race and age groups. Criminal behavior of different groups is also connected through the law enforcement budget

[5] Guryan (2004) finds effects of these orders on schooling outcomes of blacks both in and outside the south, and Reber (2010b) finds a similar effect in Louisiana. Lutz (2005) finds the termination of court-ordered desegregation reduces black educational attainment, but only outside of the south. These studies are not necessarily in conflict in part because the phase-out of desegregation studied by Lutz occurred in a very different environment from the one in which these orders were implemented. Residential segregation has decreased significantly (Glaeser and Vigdor 2003), funding is more equalized across school districts (Card and Payne 1998; Murray et al., 1998; Hoxby 2001) and attitudes toward race have changed dramatically (Schuman et al., 1985; Quillian 1996). Desegregation may have caused permanent changes that outlive the end of court involvement. Finally, southern policy makers may take compensatory actions to help mitigate any negative impact from terminating a desegregation plan (Lutz, 2005).

constraint. A shock that reduces the number of offenses by any one group frees up the amount of police resources available to investigate crime by members of any group. These two mechanisms help explain why we see short-run declines in victimization among whites and black adults, as well as offending reductions by black adults.

Economic models of the "market for offenses," which emphasize that crime is a function of victim as well as offender behavior, predict the second mechanism described above (freed-up police resources) may be partly transitory because potential victims should eventually respond to beneficial changes to the supply of offenses by reducing protective behaviors (Ehrlich, 1981; Cook, 1986; Philipson and Posner, 1996). Consistent with this theory, we find that several years after court desegregation orders are enacted, the public "consumes" part of the benefits from reduced youth crime as curtailed spending on police. Over the longer term, the only detectable life-course-persistent effects are found among birth cohorts that actually attended desegregated schools. One general implication is that studies that do not account for compensating changes in avoidance behavior will understate the welfare effects of crime-prevention programs.[6]

With any quasi-experimental research design, including our difference-in-differences approach, there is always some uncertainty about whether the causal relationship of primary interest has been isolated successfully. Several supplemental findings point suggestively in the direction of our having isolated a real behavioral response to court-ordered school desegregation. First, our results are robust to conditioning on census region-year fixed effects, county-specific linear trends, and interactions of year effects with baseline county socio-demographic characteristics. Second, we find no systematic evidence of pre-existing homicide trends for black

[6] This is an important point in environmental economics as well; see Neidell (2009) and Moretti and Neidell (2010).

youth, black adults, white youth, or white adults in the years *before* these court desegregation orders are enacted. Third, we show our results are not due to compositional changes in the counties that we are studying (migration), or to measurement error in the county population counts that are the denominators of our homicide rates. Fourth, we find that black homicides declined the most in those districts that experienced the largest declines in school segregation levels. Fifth, we find no relationship between the timing of these court orders and the level or change in political composition of local federal courts. Sixth, the results do not seem to be due to increased policing, since spending on police declines over time; evidence that this is not a spurious finding comes from the fact that we do not see declines in spending on education or fire protection. Finally, we carry out a number of falsification tests and show these court orders have no "effect" on youth or adult mortality rates from causes that should not be affected by court orders (e.g. death from illness), or on other black teen and adult outcomes that should not be directly affected by changes in criminality among black youth, namely black employment rates.

The evidence taken together suggests that an omitted-variables counter-explanation for our findings would have to meet a very stringent set of criteria. It would have to be a shock that was almost exactly contemporaneous with desegregation (given the lack of evidence for pre-trends) yet was geographically constrained to the specific school districts being placed under order (given the robustness to region-year effects), that did not impact other important measures of social well being or behavior, that did not involve migration, and that was largest in places where school desegregation was most effective at achieving racial integration.

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

## II. BACKGROUND

Shortly after *Brown*, the Supreme Court declared school districts should desegregate "with all deliberate speed" (*Brown II;* 349 U.S. 294, 1955). What this meant in practice was not specified, and details were left to the lower Federal courts. Few districts saw much desegregation for many years. Smaller districts, particularly in the South, began to desegregate in the 1960s after the Federal government threatened to withhold Title I funds (Cascio et al., 2010). Large districts were slower to desegregate in a meaningful manner, although by 1966 virtually all school districts had engaged in at least token desegregation (Cascio et al., 2008). Most of the nation's largest districts had to be ordered to desegregate as a result of individual cases filed in local Federal court, since *Brown* directly bound only five districts (Klarman, 2004).

Our key identifying assumption is that among the set of large school districts ever subject to court desegregation orders, the timing of when these orders went into effect is unrelated to trends in other determinants of youth outcomes. This assumption seems plausible given that a large share of desegregation lawsuits were filed by the NAACP, which, given resource constraints, was selective in deciding when and where to file. The NAACP used a strategy starting well before *Brown* of filing lawsuits to establish a series of favorable legal precedents, rather than maximize short-term welfare gains. For example *Brown* targeted Kansas because the black-white school resource gap was more modest there than in other places, which was intended to focus the Supreme Court squarely on the question of segregation itself (Appendix A). Guryan

(2004) provides a model showing this is optimal in a legal system that assigns great importance to precedent.

Following the *Brown* and *Brown II* decisions, many large school districts enacted "freedom of choice" plans that ostensibly gave minority students the option to attend different schools, but in practice did not achieve much desegregation. These placement plans were prohibited by the Supreme Court in 1968 in *Green vs. New Kent County, Virginia* (391 U.S. 430), which in turn led to a surge of litigation activity in the lower Federal courts. Our focus is mostly on these major local Federal court decisions following *Green*, which, as we demonstrate below, actually helped desegregate schools. Finally, federal courts seem to have varied considerably in how they handled these desegregation cases (Klarman, 2007).

While this history suggests that the timing of local Federal court desegregation orders is plausibly orthogonal to trends in local social conditions, Southern districts do seem to have been disproportionately likely to be subject to court orders earlier in the period (see Figure 1). This regional patterning is itself the product of the evolution of legal doctrine,[7] and suggests the importance for our analysis of adequately controlling for region-specific trends in crime.

## III. CONCEPTUAL FRAMEWORK

Court school-desegregation orders could in principle generate nearly immediate changes in crime for purely mechanical reasons, by incapacitating youth on long bus rides. School desegregation could also potentially have abrupt effects on criminal behavior by blacks of all ages by changing perceptions of self worth (e.g., Clark, 1950), by reducing prejudice, as

[7] Prior to 1973, court-ordered desegregation could only occur in districts proved to have engaged in *de jure* segregation. The 1973 *Keyes v. Denver School District* decision (413 U.S. 189) ruled court-ordered desegregation could proceed in areas that had *de facto* segregation resulting from past state action, which made desegregation lawsuits more viable outside of the South.

suggested by the "contact hypothesis" in social psychology (Allport, 1954, Pettigrew, 1998; Pettigrew and Tropp, 2006), and by changing fundamental attitudes about American society, as suggested by the quote above from the Kerner Commission.

But several of the most important candidate mechanisms through which court orders might affect criminal behavior would be expected to have cumulative effects over time, as minority youth have increased exposure to more pro-social and developmentally productive peer groups and to higher-quality schools. These changes may directly influence youth behavior, as suggested by a large literature on contagion models or "peer effects,"[8] and may also reduce criminal behavior through improved schooling outcomes that increase both the opportunity costs of crime (Becker, 1968) and the cognitive, socio-emotional, and behavioral skills that enable youth to stay out of trouble (Oreopoulous and Salvanes, 2009).

The schooling mechanism even by itself might be quite large. Guryan (2004) finds desegregation orders reduce black dropout rates by around 25 percent, which the estimates from Lochner and Moretti (2004) suggest should in turn reduce black homicide arrests by at least 20 percent,[9] consistent with the idea that criminal offending is concentrated in the left tail of the behavioral distribution (Wolfgang et al., 1972; Tracy et al., 1990). Lochner and Moretti's (2004) study seems to suggest that the main effect of schooling is to act on the extensive margin of

[8] Epidemic models emphasize the tendency of "like to beget like" through peer interactions with higher local crime rates serving to reduce the actual or perceived probability of arrest as well as the stigma of criminal behavior (Sah 1991; Cook and Goss 1996; Glaeser, Sacerdote, and Scheinkman 1996).

[9] Lochner and Moretti (2004) suggest that a 10 percentage point increase in high school graduation rates would reduce overall violent crime arrest rates for blacks by 25 percent [see their footnote 36]. Their Table 11 shows that for blacks and whites pooled together, the estimated effect of dropout rates on murder specifically is about 2.66 times as large as the effect on the overall violent crime rate. If the ratio of effects on murders versus all violent crimes is the same for blacks and whites, then a 10 percentage point increase in graduation rates would reduce murder arrests for blacks by two-thirds (0.10 * 2.66 = 0.67). Guryan estimates a 3 percentage point decline in black dropout rates following desegregation, which suggests we should expect a decline in black murder offending by (3/10)*(2/3) = 20 percent.

criminal involvement (offending rate) rather than on the intensive (severity) margin, since they do not find any substitution from more to less serious crimes.

A key concern with court-ordered school desegregation was the possibility that whites would react in ways that create negative spillovers and undo some of the intended benefits of the court orders. For example, surveys conducted shortly after *Brown* found that 15-25 percent of Southern whites endorsed the use of violence if necessary to preserve racially segregated schooling (Klarman, 2007, p. 192), and many cities experienced riots while trying to desegregate the schools (Rodgers and Bullock, 1972; Greenberg, 2004; Williams, 1998).

Less obvious at the time these orders were being enacted was the possibility of beneficial rather than harmful spillover effects. One reason declines in criminal behavior by minority youth could benefit other groups is because a sizable share of homicide offending occurs across race and age lines. For example, Table 1 shows that for black homicide offenders ages 15-24, over half of victims were 25 and over, and nearly one out of five victims was white (see Appendix B for details; see also Cook and Laub, 1998). Because previous research in criminology suggests victims often contribute to the initiation or escalation of violent events (Wolfgang, 1958, 1967),[10] desegregation orders could potentially reduce offending as well as victimization rates of other groups by making black youth less "victimogenic." Table 1 shows that among black offenders ages 35+, roughly 15 percent of victims were under age 25.

The criminal behavior of different groups is also linked through the police budget constraint. If police resources are fixed in the short term, any shock that reduces the supply of

[10] Far and away the most common motivation to commit homicide is an altercation (FBI, 2007; Chicago PD, 2008). The characteristics of homicide offenders and victims are quite similar, with the vast majority of both groups having a prior arrest record (Chicago PD, 2008; Schreck et al., 2008).

offenses among any sub-group has the effect of increasing the police resources available to investigate or deter crimes committed by any group , which leads to more deterrence, less offending, yet more deterrence, still less offending, and so on. This type of feedback process is the reverse of what Kleiman (1993) calls "enforcement swamping." This could be an important mechanism since blacks under 25 account for one-fourth of homicide arrests (Table 1), and the elasticity of serious crimes like murder with respect to police resources has been estimated to be as large as -1.0 (Levitt, 2002, Evans and Owens, 2007).

The economic theory of a "market for offenses" predicts police resources should not be static in perpetuity, and that the magnitude of the reverse-enforcement-swamping mechanism should attenuate over time as potential victims respond to changes in the supply of offenses by reducing crime-avoidance behaviors (Ehrlich, 1981; Cook, 1986). This idea can be illustrated with a simple model adapted from Philipson and Posner (1996). Let the share of youth engaged in crime, $S(K,P)$, be a function of their human capital, $K$, and the public's effort to protect itself against crime, $P$, so that $dS/dK \leq 0$ and $dS/dP \leq 0$. Imagine $P$ is the share of households protected against criminal attack because there is a policeman nearby, so increased spending on police increases $P$. Spending on police is a function of crime, $P(S)$, with $dP/dS \geq 0$. The equilibrium crime rate, C*, occurs where the amount of police spending that people will support as a result of the level of local youth crime is just equal to that level of youth crime, so that:

(1) $$C^* = (1 - P^*)S^*$$

Now consider the effects of a policy that increases the human capital of youth, K:

(2) $$dC/dK = (1 - P)dS/dK - (dP/dS)dS/dK$$

The first term in equation (2) will be negative (less crime), although the second term will be positive since the decline in crime leads to a decline in protective activities. Philipson and Posner note that the change in avoidance behavior may occur with some delay, which they argue is one explanation for why crime trends are cyclical over time.

## IV. DATA

Our study focuses on the set of large school districts subject to court orders that were included in a dataset compiled by Welch and Light (1987) for the U.S. Commission on Civil Rights; the districts and the year of their court desegregation order are listed in Appendix Table A1. These data cover all districts that in 1968 were 20-90 percent minority with enrollments of 50,000+, and a random sample of districts that were 10-90 percent minority with enrollments of between 15,000-50,000. This sample is not representative of all districts in the U.S., but is still of great interest given it accounts for such a large share of minority students – and crime – in the U.S.[11] We seek to identify the effect of court ordered desegregation in these districts.

Our main data sources are homicide victimizations measured by the Vital Statistics (VS) and homicide offending measured by the FBI's Supplemental Homicide Reports (SHR), both aggregated to the county level. We have also examined other types of crimes using data from the FBI's Uniform Crime Reports. But these data have a great deal of measurement error, particularly at the county level (Maltz, 1999), and so our results for these other crimes are imprecisely estimated and ultimately not very informative (see the discussion in Appendix B).

One complication of working with county-level crime data is that the Welch and Light dataset is at the level of the school district. For 37 percent of the districts in our sample, the

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

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

The VS provides a census of all deaths and enables us to measure homicide victimization rates by county and year to separate age-race groups over the period from 1959 through 1988. We can use the SHR to capture information on homicide victims and, when police have made an arrest, offenders. During our study period the ratio of homicide arrests to homicide events is around 0.77. The main limitation of the SHR for our purposes is that the dataset starts only in 1976, which limits our ability to measure short-term effects of court orders since a large share of orders were enacted by then (Figure 1). County population comes from the Census Bureau and VS interpolations for inter-censal years. We show below that our results are not an artifact of measurement error in population counts during inter-censal years.[12]

Table 2 shows our analytic sample consists of large counties, with a mean population of around 677,000 over our study period. Around 17 percent of county residents are African-Americans. Homicide victimization rates to white youth 15-19 increase dramatically from 1960

[12] The potential concern would be that if whites are moving out of a county after a court desegregation order is enacted during the inter-censal period, then if the interpolation between census years overstates the number of whites living in the county in a given year, the estimated white homicide rate would be too low and our estimates would overstate the effects of desegregation orders in reducing white homicides. A similar problem could in principle occur for blacks, although the reverse bias may be more likely if blacks are moving into rather than out of desegregating school districts. In any case in practice this does not seem to be a problem, as described below.

to 1980, from 2.3 to 9.7 per 100,000, while victimization rates to blacks 15-19 start off much higher (20.3 per 100,000), almost double from 1960 to 1970, and then decline in the 1970s.

## V. METHODS

Our basic empirical approach is to examine how homicide victimization rates for whites or blacks in county *i* in year *t*, $y_{it}$, change in response to court school desegregation orders. Our key explanatory variables are indicators $D_{p,it}$ equal to one if in calendar year *t*, district $i$ had a desegregation plan implemented *p* years beforehand, 0 otherwise. In most models we use the year before the plans are implemented as our reference point. We define indicators for the period 6 or more years before the orders go into effect, for each of the five years individually before orders are enacted, for each of the six years individually after orders are enacted, and the period 7 or more years after the orders are implemented, although we also estimate more parsimonious specifications. We condition on county and census region-year fixed effects, $\gamma_i$ and $\psi_{t,r}$, the latter being particularly important given Figure 1 shows some regional pattern to the timing of court orders in our sample of counties. Our main estimating equation is given by (3).

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

The coefficients of interest, the $\beta_p$ 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 specification also allows for effects that are either immediate or gradually unfold over time, which is important because it might take several years for a given age cell to have been fully "treated" following a court order and the behavioral effects of desegregation could increase with time spent in desegregated school.

It is important that the entire $\beta_p$ vector be identified from the same set of counties, to avoid confusing the time path of how areas respond to desegregation with changes in the composition of counties in our analytic sample. Therefore in generating our "event-study" figures below we restrict our sample to counties that contribute to each of the first six points in the post-desegregation vector and at least four of the last five years in the pre-desegregation vector.[13] This removes around 8 percent of the county-year observations from the sample. Estimates from the full sample turn out to be quite similar.

In our main set of estimates, we treat the individual counties as the observational unit and estimate equation (3) without weighting by county population, to estimate the effect of school desegregation on the average county. However the results are similar when we estimate the effects on the average juvenile instead, by using each county's juvenile population as weights.

We initially estimate equation (3) using OLS in levels, and calculate standard errors clustered at the county level to account for serial correlation (Bertrand et al., 2004). This might not be the right functional form, though, since there is substantial cross-sectional variation in homicide rates, particularly for black youth (Figure 2). Estimating a proportional effects model is complicated by the fact that many counties record no youth homicides in some years.

We first re-estimate (3) using the "log linear dummy model" from Pakes and Griliches (1980). The homicide rate is transformed by replacing zeros with ones, and then logged. A

[13] 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.

dummy variable is included as an explanatory variable that is equal to one in all cases where the true homicide rate is zero. This allows us to estimate proportional responses using a linear model, but is biased because the dummy variable is endogenous.

We also estimate a fixed-effect Poisson count model using a quasi-maximum likelihood (QML) approach (Wooldridge, 1999; see our Appendix C for details). This estimator maximizes the same log-likelihood function as the standard fixed-effect Poisson model, but rather than assuming mean-variance equality, relies on a robust standard error calculation instead. The model is fully robust to distributional misspecification. We use total homicide counts for the relevant age-race group as the dependent variable, and control for the county population in that age-race group as the exposure variable.[14] (The computer code is available upon request.)

We also experiment with re-estimating different versions of equation (3), including a model that conditions on county-specific linear trends, and what we call a "base demographic model" that non-parametrically allows different "types" of counties to have different crime trends over time by interacting year fixed effects with a series of county characteristics measured in the 1960 Census (median household income, percent of population over age of 25 with a high school degree, the percent of employment in manufacturing, and percent non-white). Time-variant demographic variables are not included in the model because they may be endogenous to desegregation. We have experimented with including a time-varying measure of non-school desegregation race riots (such as the 1965 Watts Riot), which has no effect on our results.

## VI. RESULTS

[14] The results are quite similar whether we use the offender or the victim population as the exposure variable.

In this section we first document that court-ordered school desegregation does indeed successfully re-sort minority children across schools and increase their exposure to white children, replicating the results from Guryan (2004) and Reber (2005). We then present our results suggesting that these court orders reduce homicide victimization rates to black youth by around 25 percent, and for black adults and white youth, before turning to our results for homicide offending, a discussion of mechanisms, and then a variety of specification checks.

**A. Impacts on School Segregation**

The top panel of Figure 3 shows that following court desegregation orders, there is a sharp drop in the dissimilarity index, which ranges from 0 to 1 and is the percent of black students who would need to be reassigned to a different school for perfect integration to be achieved given the district's overall racial composition. The figure plots the regression coefficients on our indicator variables for years before and after desegregation orders go into effect (the year before is the reference period), using OLS to estimate equation (3) conditioning on county and region-year fixed effects. We see little evidence of pre-existing trends in our counties in the years prior to the court orders, followed by a large drop in the dissimilarity index. Within two years the impact is 0.2, a large share of the 1968 mean of 0.71 in our sample. Panel B of Figure 3 shows there is also an increase in the exposure index (percent of white students in the average black student's school) of 0.15 within two years, relative to the 1968 mean value in our sample of .28. The results in panels A and B are quite similar to those in Reber (2005).

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

1978), which suggests that average school quality in these counties could have increased (as does our finding below that desegregation was associated with an increase in education spending).

**B. Homicide Victimization**

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

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

As noted above, school-age offenders often kill older people, and so in the other panels of Table 3 we expand our focus to older victims as well. Compared to the results for black victims 15-19, the estimated effects are of about the same size in proportional terms for victims ages 15-24 and 25-34, and are slightly smaller for victims ages 35-44. In what follows we typically show results for both the 15-19 and 15-24 year old groups. In addition to accounting for teen offenses against young adults, focusing on 15-24 year olds brings more data to bear and by 6 years out, most 15-24 year olds would have been spent time in school after desegregation was enacted.

Note that all of our estimates in Table 3 condition on region-year fixed effects. Table 3 shows that our results are also robust to conditioning on county-specific linear trends, and to conditioning on interactions of baseline county characteristics and year fixed effects, which is a non-parametric way to allow different types of counties to follow different homicide trends. The point estimates barely change with these additional controls included in the model.

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

The key identifying assumption behind our study is that the timing of when these desegregation orders go into effect is unrelated to trends in other determinants of youth homicide. To explore this issue, we estimate the time path of homicide victimization rates using equation (3), which includes a full set of indicators for the years before and after these court orders go into effect. These specifications require estimating a large set of parameters and are quite demanding of the data, so in our event-history graphs the 95 percent confidence intervals (represented by the dashed lines in the figures) can often be quite sizable.

With that caveat, Figure 4 presents results for black victims ages 15-24 and 25-34. There is very little evidence of any pre-existing trend in homicide rates before desegregation orders go into effect for both age groups. Then when the court desegregation orders are implemented, we see a break in trend. The estimates for 15-19 year olds – which focus on a smaller age cohort and hence use much thinner data – are uniformly imprecise and therefore not so informative (Appendix Figure A1).

Figure 5 shows that there is no evidence of pre-existing trends in white homicide victimization rates either. Compared to the results for blacks, there appears to be more of a delay in when white victimization rates decline following implementation of the desegregation orders. The gradual impact of desegregation orders on white and to some extent black homicide victimizations might reflect the fact that the share of prime-age offenders exposed to school desegregation, as well as the average duration of this exposure, increase over time.[15]

**C. Homicide Offending**

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

Given those qualifications, Table 5 provides evidence for a decline in homicide offending by high-school aged blacks (15-19) after court desegregation orders go into effect. In order to see the results of truncating the panel, in column 1 we replicate our main victimization results using

[15] The results for black age 35-44 victims and for white age 15-19 and 35-44 victims are shown in Appendix Figure A1 and A2, respectively. The results for victims age 35-44 tend to be imprecisely estimated.

the VS data from just 1976 forward. We note that the victimization impacts for the subset of counties that desegregated in 1977 or later are larger in absolute value compared to the results from our full sample, which suggests that the effects on offending in our 1977+ sub-sample presented in Table 5 could also be larger than what we would see in our full sample.

For the sub-sample of counties for which we can estimate short-term offending effects,[16] our QML count model implies large declines in homicide arrests to black youth, equal to 30 percent during the first 5 years after the court orders and 52 percent 6+ years out. The OLS results, though, are imprecise. There is some suggestive evidence that offending by whites changed as a result of these orders as well, with a 22 percent decline in homicide arrests to whites ages 15-19 in the first five years after the court orders are enacted.

It is important to keep in mind that the SHR data underlying Table 5 are quite thin, so the magnitudes of the estimates should be interpreted cautiously. Nevertheless, the fact that the estimates point in the direction of potentially large effects seems consistent with other evidence that criminal behavior is very sensitive to environmental influences, including the massive time-series variation that we observe in overall U.S. crime rates. For example between 1984 and 1992 the homicide arrest rate to blacks 14-24 nationwide fully tripled*,* and then dropped by about one-half over the next seven years (Levitt, 2004, p. 180). The magnitudes of our results also fit quite comfortably alongside the partial equilibrium results from the housing and school-choice lottery studies discussed above (Kling, Ludwig, and Katz, 2005; Cullen, Jacob and Levitt, 2006; Deming, 2009). Ludwig and Kling (2007) find that racial composition may be the neighborhood

[16] The estimates are identified by variation in the date of desegregation within the group of post-1976 desegregators. However, the full sample is used in estimation – the pre-1977 desegregators serve as a control group.

attribute that is most strongly associated with violent criminal behavior in the MTO housing mobility experiment.

Homicide offending seems to have declined among black *adults* as well as youth in the years right after these school desegregation court orders are implemented, as shown in Table 5 (columns (5) – (8)). Our QML count model suggests that the magnitude of the effect in absolute value (in proportional terms) is largest for youth and declines steadily with age. For example 6+ years after the court orders are enacted, the estimated effect on homicide arrests is equal to 52 percent for black youth 15-19 years of age, 35 percent if we expand the age group to include older blacks (15-24), 22 percent among blacks 25-34, and 11 percent among blacks 35-44.

Table 6 shows there are large declines in offending rates *across* age groups among blacks following court desegregation orders, consistent with what we would expect based on the amount of cross-group offending documented in Table 1. The magnitudes of these estimates should be interpreted even more cautiously than those in Table 5, since we are now dividing our data up into even more detailed offender-victim cells. We focus on 10-year age groupings (15-24, 25-34, 35-44) to help address the thinness of the data, and lump together data on white victims of all ages for the same reason. The results shown in Table 6 suggest that the rate at which black offenders 15-24 killed older adults as well as other 15-24 year olds declined following court desegregation orders. The data provide at least suggestive evidence that the rate at which adults offended against younger people also declined following desegregation orders, with an estimated effect for the years 1-5 after the court desegregation orders are enacted equal to 43 percent for black 35-44 year old offenders.

We cannot reject the null hypothesis that the entire decline in victimizations to (or offending by) black adults 35-44 comes from fewer homicides that involve blacks 15-24 years old as offenders (or victims, respectively). For the decline in victimizations to (or offending by) black adults 25-34, we are able to reject the null hypothesis that all of this change comes from fewer homicides that also involve blacks 15-24 on the other side of the homicide offending or victimization boundary. But the results of our statistical tests are consistent with the possibility that a large share of the homicide reduction among black 25-34 year olds comes from homicides involving black 15-24 year olds. To reach these conclusions, we use the QML estimates and test the cross-equation hypothesis that the entire change in the black adult victimization or offending count is due to changes in homicides that involve blacks 15-24 years old (see Appendix D and Appendix Tables A2 and A3 for details). While the statistical power of our tests is somewhat limited, the findings are at least consistent with the idea that desegregation orders reduced victimization and offending among older blacks in large part by making younger blacks less criminogenic and victimogenic.

We also look at the degree to which desegregation orders may have changed offending rates across race groups in the short term (column (4)). While none of these point estimates are statistically significant, the standard errors are so large we cannot rule out either a zero effect or a very large effect.

Table 7 provides evidence that the behavioral impact of court school-desegregation orders on homicide offending may persist into adulthood for both blacks and whites. We focus on results from the QML count model; OLS results are uniformly imprecise. Homicide arrest rates for black 35-44 year olds whose counties desegregated 25 or more years ago (and so would have been of school age when their county's court desegregation orders were enacted) are substantially lower than those for black 35-44 year olds whose county-year observations fall within 20 years or less of enactment of a court order (the reference group of adults who were already out of school when the court orders are enacted). The effect appears to be particularly strong for homicides with black offenders and white victims.

These results also seem to support a causal interpretation by showing that there is a fairly sharp difference in the effect on cohorts that were born close together in time but differ in whether they actually attended desegregated schools. Assuming that individuals finish high school when they are 17 years of age, the 35-44 year old age cohort in our sample would have been exposed to an average of only 1 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 4 ½ years of school desegregation 25 to 29 years after the start of desegregation.[17] Yet the coefficient for desegregation 25-29 years ago is more than twice as large as the coefficient on desegregation 20-24 years ago and these coefficients can be statistically distinguished from each other in all three cases (columns (2), (4) and (6)).

It is not clear whether the short-term impacts of court desegregation orders on blacks who were already adults at the time the orders are enacted (Tables 5 and 6) fade out over time because any spillover effects are differenced away in this analysis (since outcomes for all birth cohorts are measured at some point after the desegregation orders are enacted). At the very least, we can say the only life-course-persistent changes in criminal behavior detectable by our data are for those birth cohorts of school age when the court school desegregation orders are enacted.

[17] The 35-44 year old age cohort in the omitted category, less than 20 years after the start of desegregation, would have been exposed to an average of 0.005 years of school desegregation.

Moreover the sharp difference in estimated long-run effects across cohorts helps rule out confounding influences from changing county demographics or social policies, which we would not expect to have such different influences on cohorts born just a few years apart.

**D. Robustness and Falsification Tests**

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

We also obtain similar results when we weight by the relevant age-race population count in each county, rather than calculate un-weighted estimates (Appendix Table A4). The pre/post vector approach (Figures 4 and 5) produces similar results when the full sample of county-year observations is used – that is, when we include the 8% of districts that do not meet the main sample requirement of contributing a sufficient number of points pre- and post-desegregation. (The results from the truncated model with points for 1-5 and 6+ years after the court orders, displayed in all the tables, always use the full sample.) Our SHR offending results are similar when we construct our rates using county-level counts of people living in jurisdictions that report crime data to the FBI's UCR system, rather than the Census-based county population estimates.

As another check on omitted variables concerns, we find that there is no systematic relationship between the politics of the local federal judges in each district and the timing of when court school-desegregation orders are enacted. We first estimate a cross-section regression and find that the baseline political composition of each federal judicial district is unrelated to the average year when court school-desegregation orders go into effect for the school districts in our sample located within each judicial district. We also find that changes in the political composition of these judicial districts over time are unrelated to the likelihood that a school district is subject to a desegregation order (results available on request).[18]

Perhaps the main threat to inference with our study, aside from omitted variables, is the possibility of cross-county population migration in response to school desegregation orders. One way this could affect our results is through measurement error in our county population variable.[19] Yet in Appendix Table A5 we show that our homicide victimization results are qualitatively similar (though less precisely estimated) when we restrict ourselves to the year before, year of, and year after each of the decennial censuses from 1960 through 1990, years in which we expect measurement error in county population characteristics to be less pronounced compared to years that fall further away from the decennial census. (Estimates that use just the individual decennial census years are qualitatively similar, but much less precisely estimated).

A different concern is that population migration could lead us to confound behavioral responses by county residents with compositional changes in the county population over time.

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

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

Yet Appendix E presents a detailed set of results showing there is no evidence of any effect of these court desegregation orders on the log of the county population of 15 to 24 year old whites, or log population of 15 to 24 year old blacks, or various county socio-demographic characteristics. We also show in Appendix E that our results are qualitatively similar when we use either MSAs as the unit of observation, or use data for larger geographic areas still – bordering county groups. These units of observation are less likely to be influenced by migration than are the smaller units of observation (usually counties) used elsewhere in the paper.

A final way to address the possibility of bias from population migration and other forms of omitted variable bias is to examine whether school desegregation orders have an "effect" on outcomes that should logically not be affected. Table 8 presents the results from such a falsification exercise. We estimate the effect of school desegregation orders on mortality rates from major illnesses,[20] which should not be affected by the school or peer quality or community attitudinal changes that we hypothesize drive our estimated effect of court school-desegregation orders on homicide. Whether we use our OLS levels, QML count or log dummy model, the estimated "effects" of desegregation orders on mortality from illness are much smaller in magnitude than what we see for homicide victimization rates and are never statistically significant for blacks or whites in any of our age groups (15-24, 25-34, or 35-44).

Table 9 shows that the estimated "effects" of court-ordered school desegregation on different measures of employment for minorities are always small and not close to statistically significant. Put differently, the only estimated effect of court-ordered school desegregation on

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

adults in our data is the one outcome (crime) that we expect to be linked to changes in criminal behavior by youth.

**E. Empirical Evidence on Mechanisms**

It is possible that the estimated changes in violence from school desegregation orders is simply the mechanical result of incapacitating youth on long bus rides during the high-crime hours after school, or, relatedly, simply the result of having black youth spend more time in the communities around their new schools where policing quality may be higher. We test this hypothesis by using SHR data on month-of-offense to examine effects on homicides over the summer months versus during the academic year.[21] We find the estimated effects are about as large for homicides over the summer as during the school year (Table 10).

Consistent with the predictions of a "market for crime," we find that the decline in the supply of offenses by youth leads to a decline in police spending, but with some lag. Table 11 shows that during the first five years after the court orders are enacted there is no detectable change in police spending, which implies that the decline in offending by black youth that we document above increases the ratio of police resources to offenses in our counties. By six years after the court desegregation orders are enacted, police spending declines by around \$9 per capita, equal to around 10 percent of the sample mean. This decline in police spending is not simply a reflection of some general negative shock to local budgets, since we find that education spending seems to have if anything *increased,* by around 6 percent, which in at least some districts may have been due to the requirements of the federal judges enacting the court orders.[22]

[21] The Vital Statistics (VS) data is available only on an annual basis. The VS data therefore cannot be used for this type of analysis that compares homicides during the academic year versus summer months.
[22] Reber (2010a) also finds that court-ordered desegregation produced an increase in school spending.

Spending on fire protection services does not seem to change much at all after these court orders. It is possible that some jurisdictions may have desegregated their police departments at the same time they desegregated their schools, but is unlikely to explain our findings since McCrary (2007) finds little impact on crime from changes in the racial composition of the local police.

We can provide some indirect evidence on what behavioral mechanisms might matter most by interacting changes in our measures of school segregation and other measures with our indicators for implementation of court orders (the results are presented in Appendix Tables A10-A11 and discussed in detail in Appendix F). Homicide victimization rates decline the most for blacks in districts where exposure of blacks to whites in the public schools increases the most. The fact that we observe the largest impacts on black homicide in places with the largest "treatment dose" from court orders provides some additional support for the credibility of our research design. But we note that these findings are at best suggestive, since those counties that experience particularly large changes in school segregation may also experience particularly large changes in other mechanisms that are not captured by our data.[23] Finally, in exploratory analyses we find some suggestive evidence that the relationship between changes in school segregation and criminal involvement could be non-linear, but these estimates are imprecise.[24]

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[23] Of particularly concern, the initial white share of the population and the extent of white flight are important determinants of the change in black exposure to whites at the time of desegregation. If trends in homicides are correlated with either the initial white population share or the extent of white flight, the change in exposure results cannot be interpreted in a causal sense. However, including an interaction of the desegregation treatment variables with both the initial white population share and the percent change in the white population in the years immediately following desegregation in the specification displayed in column (1) of Tables A8 and A9 has essentially no effect on the exposure index interaction terms (unreported).

[24] To generate these estimates we divide the sample into two bins, depending on whether the district's change in the exposure index is above or below the median for our study sample, and allow the slope of the linear relationship between changes in the exposure index and changes in homicide to differ for the two groups. The point estimates for the interactions of these variables with our indicators for years 1-5 and 6+ after the court desegregation orders are enacted tentatively point in the direction of a larger drop in homicides per unit of increase in the exposure index in

Finally, there is another potential mechanism that would be relevant only for whites – migration out of the desegregated school district. While there is no evidence of "white flight" out of the counties, there is evidence that whites must be moving from school districts subject to desegregation orders to other public school districts or private schools *within the same county* that are not subject to court-ordered school desegregation. We find the ratio of white enrollment in districts subject to court orders to the total number of white school-age children in the county declines by between 4 and 6 percentage points after these court orders go into effect (Appendix Table A12) – around a 15 percent decrease relative to the sample average of 0.39 (see also Reber 2005; and Baum-Snow and Lutz 2010).[25] If these new districts or private schools are less criminogenic than the districts subject to desegregation orders, this could provide another mechanism driving our result. One suggestive data point against this hypothesized mechanism is that white homicide victimizations do not appear to decline more in desegregating districts with the largest change (i.e. decline) in the percent of white children in the county enrolled in the desegregated school district (Appendix F and Appendix Table A11).

## VII. CONCLUSIONS

Our findings suggest the general potential for crime control from social policies that affect the level of racial and economic segregation in America. Previous research using housing

those counties with the largest declines in the exposure index, although the standard errors are sufficiently large so that we cannot rule out the null hypothesis that the effects across the two groups of counties are the same.

[25] This approach implicitly assumes that the school districts within the same county, but not in the Welch and Light (1987) sample, were not under a court-ordered desegregation plan. Outside of the South, the assumption is likely reasonable. With-in the South, some of these neighboring districts may have been under court-ordered desegregation plans. However, these neighboring districts were on average much smaller than the large districts which are the focus of this study and also had a smaller fraction of black students. These characteristics would have made them substantially less likely to have had a court-ordered desegregation plan (Cascio et. al. 2008; note that virtually all Southern districts which did not have court-ordered desegregation engaged in some form of voluntary desegregation). Even if the neighboring districts were under court-ordered plans, the lower fraction of black students would have made them attractive alternatives for families wishing to reduce the extent of contact with black students (Baum-Snow and Lutz 2010).

voucher or public school choice lotteries founds that violent crime rates decline substantially among minority youth who move into less disadvantaged neighborhood or school environments (Kling, Ludwig and Katz, 2005; Cullen, Jacob and Levitt, 2006; Deming, 2009). A key concern with studies of movers is that they cannot tell us whether policies to re-sort people across social settings leads to less crime overall, or simply redistributes crime. Our results from "system-level" data suggest that re-sorting policies may lead to overall declines in violent behavior.

Our estimates suggest that the large-scale policy efforts to desegregate schools beginning in the 1960s for the largest districts in the U.S. reduced homicide involvement for black youth, with declines in victimization rates of around 25 percent and offending rates that might be as large as 30-50 percent. The size of these estimates is fairly similar to the partial equilibrium effects found in previous housing or school lottery studies (Kling, Ludwig and Katz, 2005; Cullen, Jacob and Levitt, 2006; Deming, 2009). Because criminal offending is concentrated in the left tail of the behavioral distribution, desegregation orders would need to change behavior by just a small share of high-risk youth to generate large proportional changes in crime.

Unpacking the mechanisms through which court school-desegregation orders affect criminal behavior among black youth is somewhat challenging with our data, since these desegregation orders are changing the schools as well as peer environments of youth, and, as the Kerner Commission hypothesized, perhaps overall attitudes about American society as well. Moreover the set of candidate mediating measures that can be well measured for our sample of counties during our study period is somewhat limited. But changes in schooling attainment must presumably be at least part of the story. The estimated declines in homicide arrests to black youth are about as large in the summer as during the school year, and persist into middle age.

Combining Guryan's (2004) estimates of a 25 percent decline in black dropout rates with Lochner and Moretti's (2004) estimates for the schooling-crime relationship predicts a decline in black youth homicide arrests of around 20 percent, a sizable share of our overall effect on arrests.

We also find evidence of positive spillovers on other groups as well from court school-desegregation orders. Specifically, we find that court school-desegregation orders generate declines (at least in the short-term) in homicide victimization and offending rates among blacks who were already adults at the time the court orders are enacted. That there would be some decline in victimizations among black adults is not surprising, since Table 1 above shows that there is a substantial amount of offending by school-age youth against adults. But offending behavior by black adults could also be affected if desegregation orders make youth less likely to contribute to the initiation or escalation of violent events. In fact our data do not allow us to rule out the idea that most of the short-term decline in homicide offending by black adults is due to a decline in homicides against black youth.

Another reason offending rates by black adults may decline after court desegregation orders are enacted is because declines in youth offending free up police resources for addressing crimes committed by all groups. Table 1 shows that blacks 15-24 account for around one-fifth of all homicide offenders in our sample. Our estimates suggest offending rates by this group decline by as much as 30-50 percent. If the allocation of police resources is proportional to homicide offending behavior, and if the decline in homicide offending by black youth reflects declines in other criminal behavior by this group that we just cannot detect in the UCR because of data limitations (see Appendix B), then the decline in homicide offending by black youth induced by court desegregation orders may free up 6 to 10 percent of total police resources in the short term in these counties. Previous research in economics suggests that the elasticity of homicide with respect to police resources may be on the order of about -1.0 (Levitt, 1997, 2002; Evans and Owens, 2007). Police resources that are freed up by declines in youth offending may thus account for a 6-10 percent decline in homicide offending by black adults, a sizable share of the effects for these age groups shown in Table 5 (for example, the estimated 20 percent decline in homicide arrests to blacks 25-34 or 35-44 in the first five years after a desegregation order).

The economic model for a "market for offenses" suggests that victims will respond to beneficial changes in the supply of offenses by reducing their preventive behaviors. Consistent with this prediction, we find evidence that the public reduces police spending by around 10 percent by 6+ years after court school-desegregation orders are enacted.

Our results may help explain around one-quarter of the convergence in black-white homicide rates over the 1970s. Figure 6 shows that for black youth, homicide victimizations peaked in the late 1960s and then started to decline sharply (see also Cook and Laub, 1998, 2002) – just as the set of large urban districts we study, which account for a large share of all minority crime in the US, began to implement school desegregation orders.[26]

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

[26] The large counties in our sample account for nearly half of black homicides in the US as a whole in 1968 and over one-third of white homicides. Our estimates imply that over our study period desegregation orders in our counties lowered the *nationwide* homicide rate to blacks 15-24 by 13 percent and lowered the rate to whites 15-24 by 7 percent, and might account for around one-quarter of the convergence in black-white homicide rates over the 1970s.

desegregation treatment effects (unreported). We also find little systematic evidence that the specific design features of the desegregation plan influence the size of the effects on crime.[27]

Another reason that the effects of school desegregation could change over time is if a key mechanism underlying our results was the mixing of students from different socio-economic backgrounds. If that were true, then we might expect the effects of desegregation orders to decline over time, since the difference in black vs. white poverty rates has declined.[28] Yet we find no evidence that our estimated impacts vary according to the black-white difference in median family income in each county during our study period (unreported).

A complete benefit-cost analysis of policy efforts to de-segregate schools or other social settings is beyond the scope of this paper, and would need to consider costs that come for example from residential mobility and changes in urban land use patterns. Nevertheless our paper suggests that at least the gross benefits from desegregating schools due to reductions in violent crime may be quite large from a social welfare perspective. For example combining our estimates for the decline in homicide victimizations among black and white youth with Cohen et al.'s (2004) estimate of nearly $10 million in social costs per homicide imply benefits of nearly $1,000 per black student and nearly $200 per white student from reductions in homicide, which

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[27] Welch and Light (1987) provide a useful typology of the types of plans that were implemented, which include several different types of "voluntary" plans such as magnet programs that provide students some choice over where they attend school and are similar to those plans used most commonly today. "Involuntary" plans include rezoning of school catchment boundaries and pairing-clustering plans that integrate groups of schools by grade, and are thought to involve the greatest amount of busing among the different plan types. Welch and Light (1987, p. 27) explain: "Pairing and clustering involves reassigning students between a pair or group of schools, usually via grade restructuring, … [that] may have either contiguous or noncontiguous attendance zones. For example, a (predominantly) white school and a (predominantly) black school, both offering grades K-6, could be paired by converting one into a lower elementary school (grades 1-3) and the other into an upper elementary school (grades 4-6)." When we re-estimate our main specifications including interactions between time since desegregation order and plan type, we do not find any evidence for heterogeneity in treatment effects by plan type.

[28] The poverty rate for blacks and whites was 41.8 vs. 13.3 percent in 1965; 29.3 vs. 11.2 percent in 1995, and 24.9 vs. 10.6 percent in 2005.

are both large relative to the average per-pupil spending level of schools in our sample of around \$2,750. Our findings that victims compensate for reductions in youth criminality by scaling back crime-prevention behaviors suggest these figures will understate the gross benefits from court-ordered school desegregation. It is possible that some of our most cost-effective crime policies might not have anything at all to do with the criminal justice system.

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**Figure 1**
Desegregation Implementation Dates

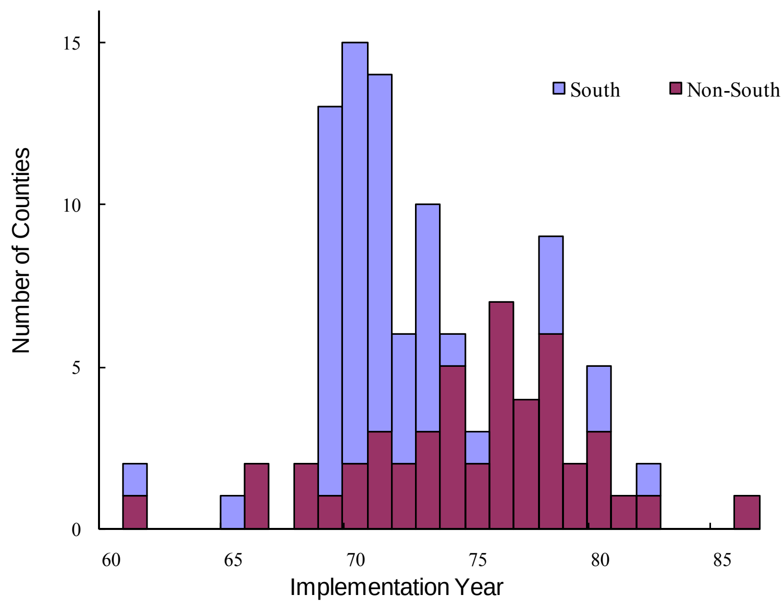
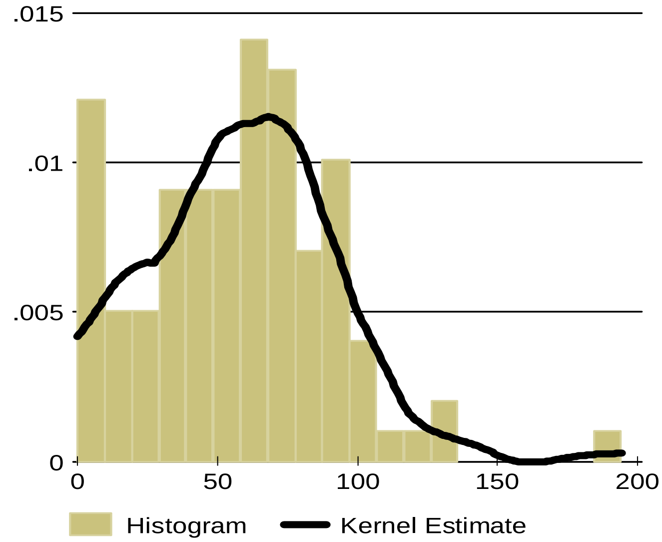
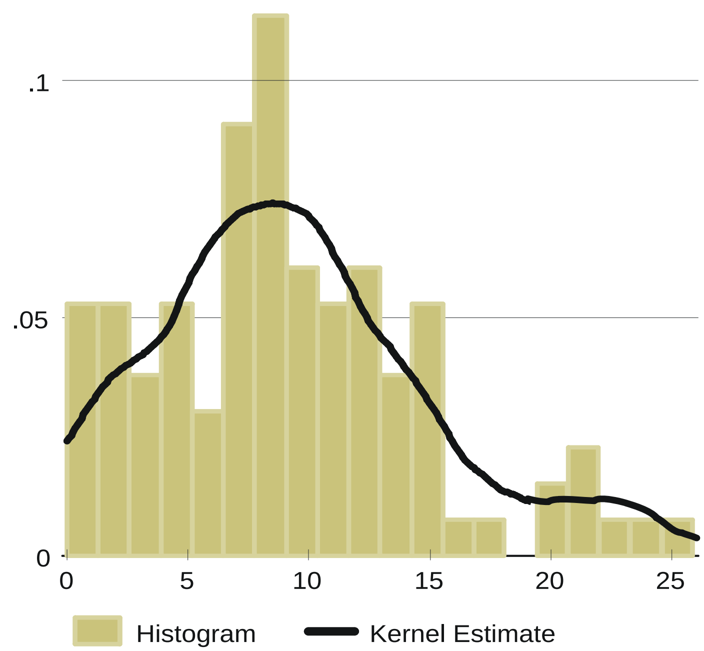


**Figure 2**

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



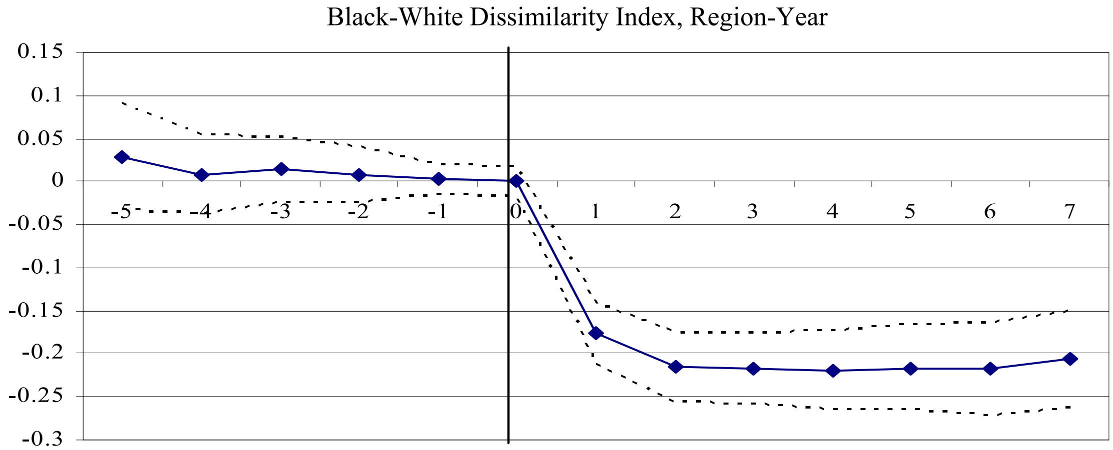

B. Distribution of 1975 White Age 15 – 24 Homicide Rates per 100,000



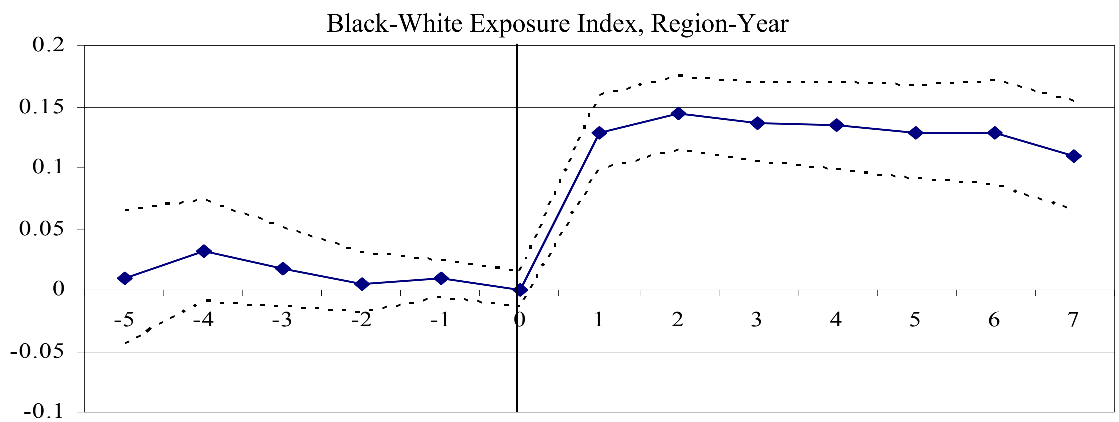

Note. The figures displays histogram and kernel density estimates of the 1975 age 15 – 24 homicide rate per 100,000. The kernel density estimate uses a Epanechnikov function and a bandwidth of 1.2. The sample is restricted to the counties in the Welch and Light (1987) sample with a major desegregation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

**Figure 3: Effects of Court-ordered Desegregation on Segregation and Number of Schools**

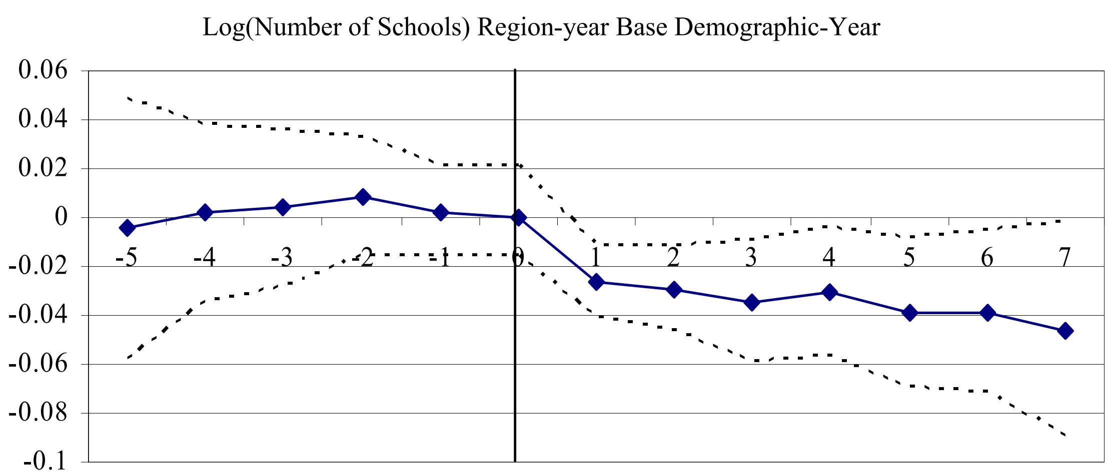
Panel A:




Panel B:



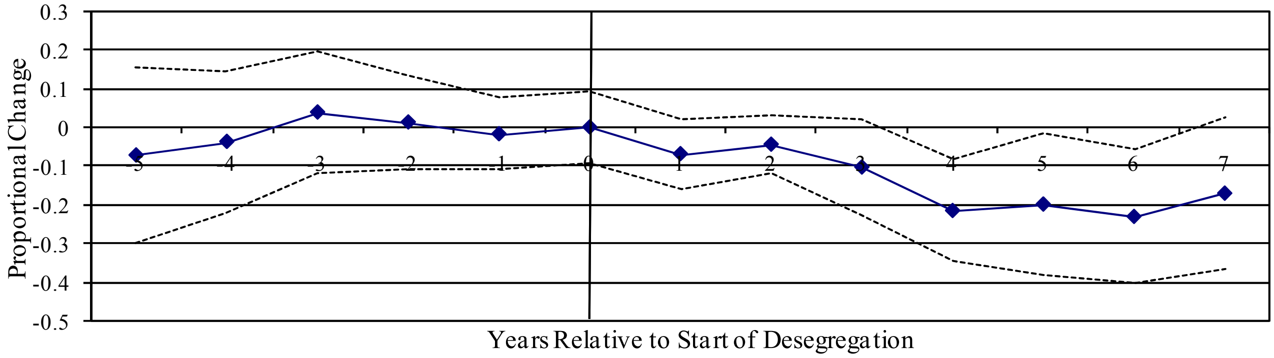

Panel C:



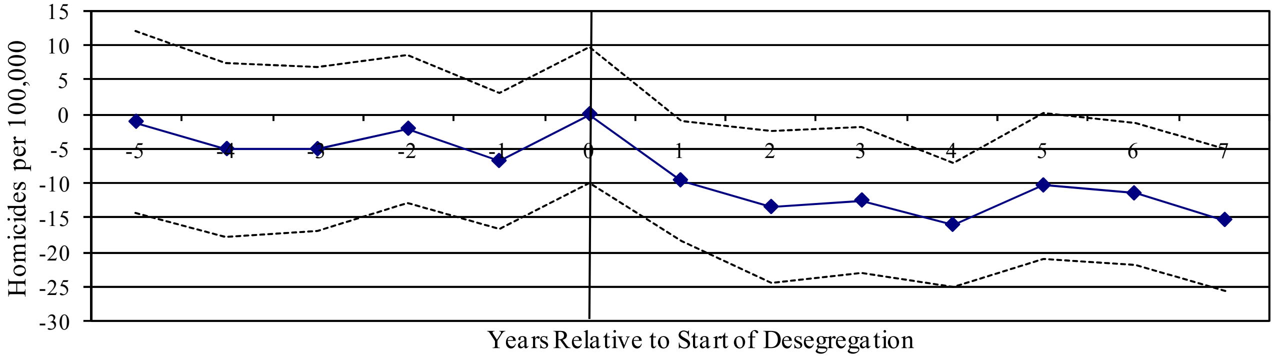

Note. The solid points display coefficient estimates and the dashed lines display the 95% confidence intervals around these estimates. The vertical axis displays the magnitude of the coefficient estimate. The horizontal axis displays years relative to the implementation of desegregation. Year "0" is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

**Figure 4: School Desegregation & Black Homicide Victimizations**

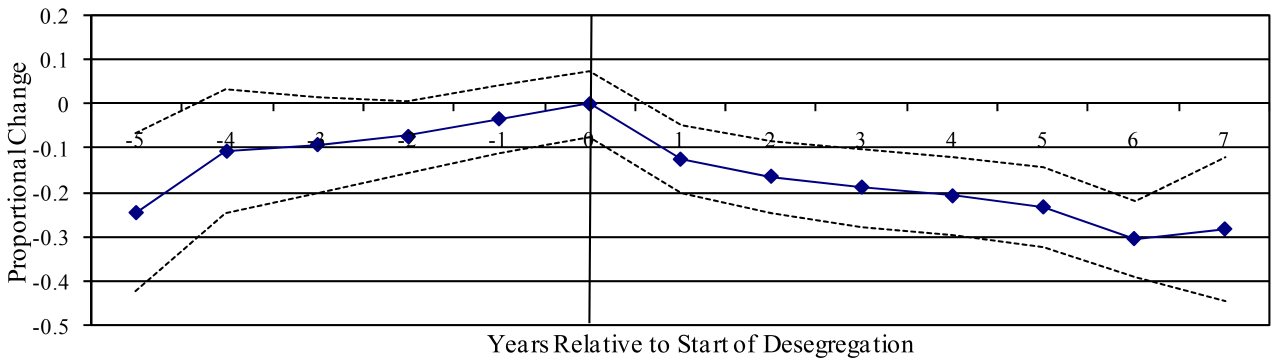
Panel A: Age Cohort 15-24 QML Count



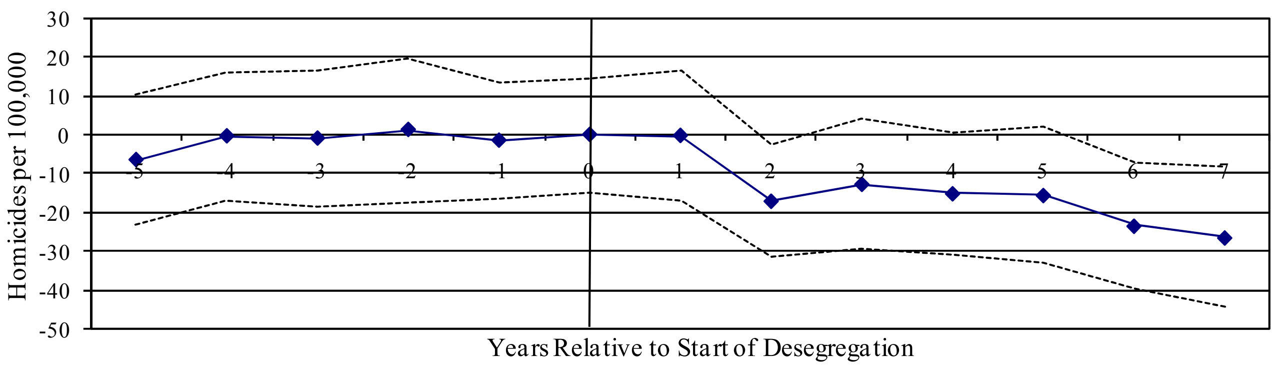

Panel B: Age Cohort 15-24 OLS Level




Panel C: Age Cohort 25-34 OLS Level



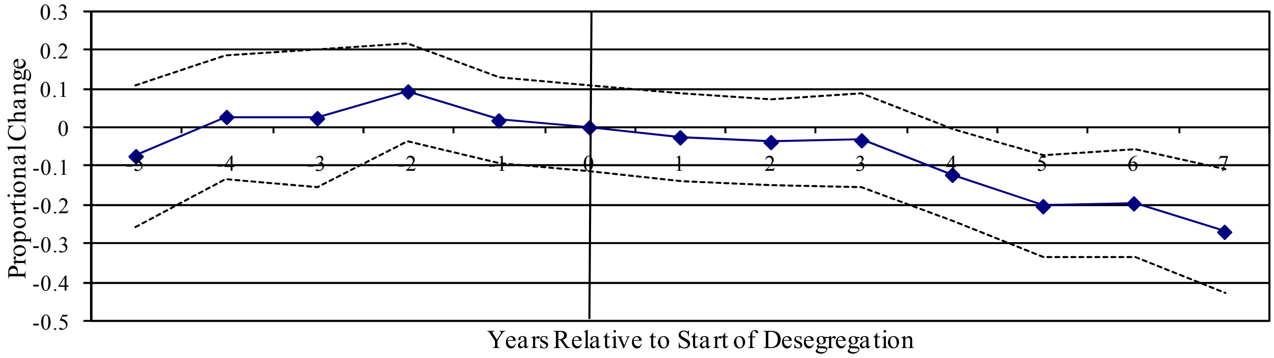

Panel D: Age Cohort 25-34 OLS Level



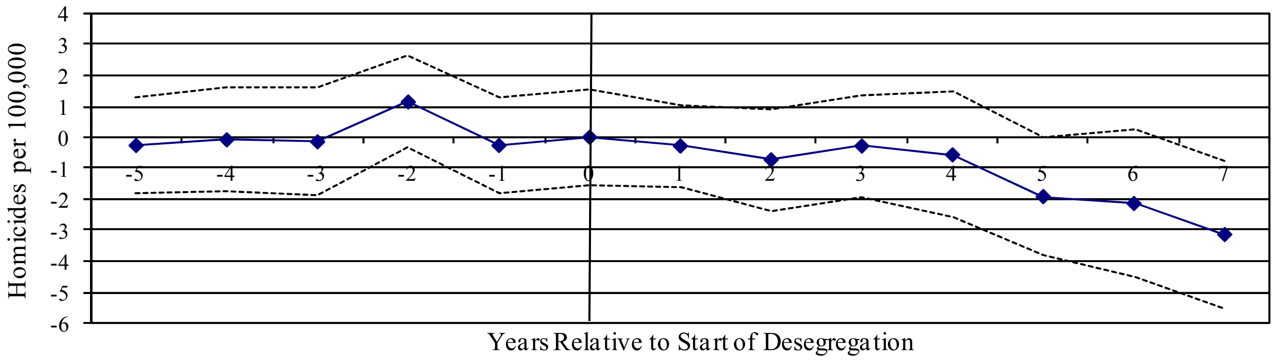

Note. The solid points display coefficient estimates and the dashed lines display the 95% confidence intervals around these estimates. Year "0" is the year immediately prior to the start of desegregation.

## Figure 5: School Desegregation & White Homicide Victimizations

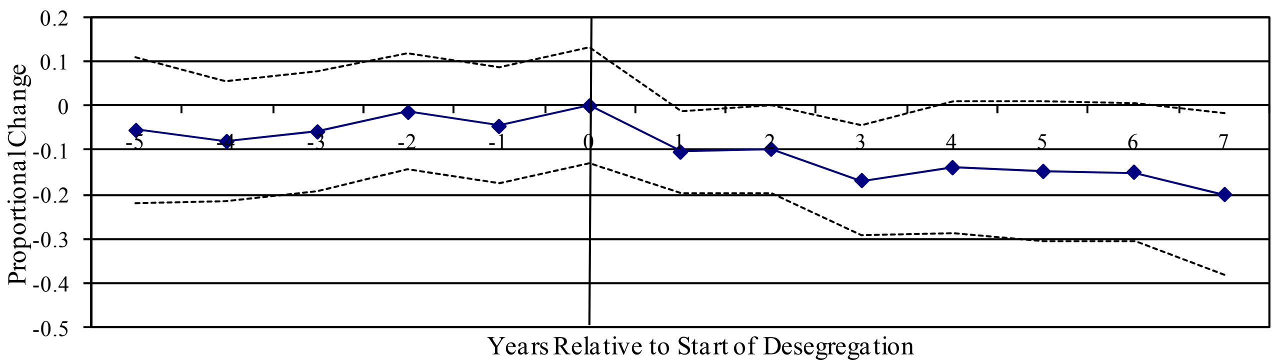
Panel A: Age Cohort 15-24 QML Count



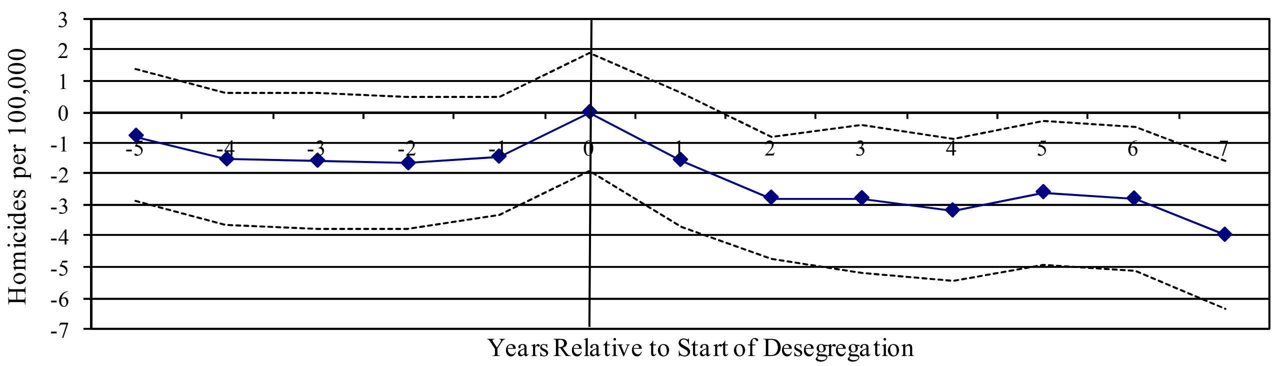

Panel B: Age Cohort 15-24 OLS Level




Panel C: Age Cohort 25-34 QML Count

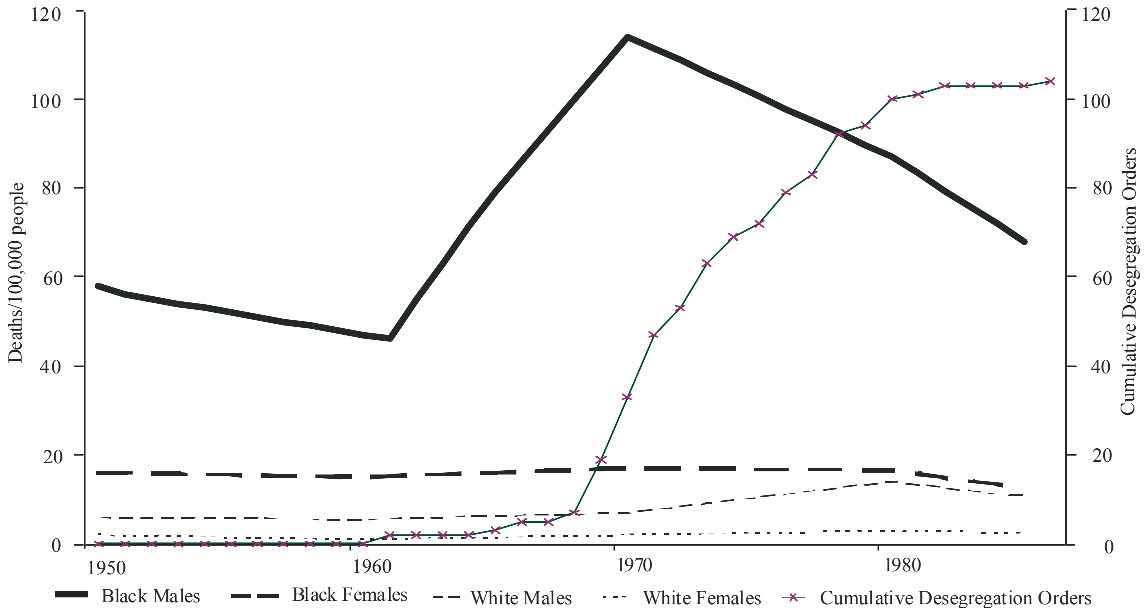



Panel D: Age Cohort 25-34 OLS Level




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

## Figure 6: Historical Homicide Rates for Individuals Aged 15-24




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

Table 1
Homicide Offending

| Offender | Victim | | | | | | |
|---|---|---|---|---|---|---|---|
| | Black 15-24 | Black 25-34 | Black 35+ | White 15-24 | White 25-34 | White 35+ | Total |
| Black 15-24 | 8461 | 5212 | 4098 | 1183 | 986 | 2207 | 22147 |
| | (.38) | (.24) | (.19) | (.05) | (.04) | (.10) | (1.00) |
| | {.58} | {.31} | {.23} | {.09} | {.08} | {.12} | {.24} |
| Black 25-34 | 3653 | 7158 | 4935 | 488 | 723 | 1091 | 18048 |
| | (.20) | (.40) | (.27) | (.03) | (.04) | (.06) | (1.00) |
| | {.25} | {.42} | {.28} | {.04} | {.06} | {.06} | {.19} |
| Black 35+ | 1728 | 3759 | 7727 | 184 | 297 | 608 | 14303 |
| | (.12) | (.26) | (.54) | (.01) | (.02) | (.04) | (1.00) |
| | {.12} | {.22} | {.44} | {.01} | {.02} | {.03} | {.15} |
| White 15-24 | 455 | 326 | 236 | 6469 | 3590 | 3872 | 14948 |
| | (.03) | (.02) | (.02) | (.43) | (.24) | (.26) | (1.00) |
| | {.03} | {.02} | {.01} | {.51} | {.28} | {.21} | {.16} |
| White 25-34 | 214 | 333 | 273 | 2889 | 4781 | 3844 | 12334 |
| | (.02) | (.03) | (.02) | (.23) | (.39) | (.31) | (1.00) |
| | {.01} | {.02} | {.02} | {.23} | {.37} | {.21} | {.13} |
| White 35+ | 161 | 232 | 248 | 1406 | 2677 | 6428 | 11152 |
| | (.01) | (.02) | (.02) | (.13) | (.24) | (.58) | (1.00) |
| | {.01} | {.01} | {.01} | {.11} | {.21} | {.36} | {.12} |
| Total | 14672 | 17020 | 17517 | 12619 | 13054 | 18050 | 92932 |
| | (.16) | (.18) | (.19) | (.14) | (.14) | (.19) | (1.00) |
| | {1.00} | {1.00} | {1.00} | {1.00} | {1.00} | {1.00} | {1.00} |

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

Table 2
Descriptive Statistics

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

Note. The cells display county means. The data is restricted to counties with a school district identified in the Welch and Light (1987) study as having had a "major" court-ordered desegregation plan. The "Full Sample" column contains data from 1959 - 1988.

Table 3
Black Homicide Victimization

| | Proportional Response | | | Levels | | |
|---|---|---|---|---|---|---|
| | QML Count | | OLS Log Dummy | OLS | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | | | A. Age 15 - 19 | | | |
| Post Desegregation Years 1 - 5 | -0.17 | -0.16 | -0.08 | -5.89 | -5.05 | -5.14 |
| | (0.07) | (0.07) | (0.05) | (2.86) | (2.84) | (3.01) |
| Post Desegregation Years 6+ | -0.27 | -0.28 | -0.15 | -6.52 | -5.71 | -6.26 |
| | (0.09) | (0.09) | (0.07) | (3.93) | (3.87) | (4.00) |
| | | | B. Age 15 - 24 | | | |
| Post Desegregation Years 1 - 5 | -0.14 | -0.11 | -0.13 | -8.91 | -7.45 | -8.59 |
| | (0.04) | (0.04) | (0.05) | (2.76) | (2.58) | (2.85) |
| Post Desegregation Years 6+ | -0.23 | -0.21 | -0.19 | -10.55 | -9.32 | -11.27 |
| | (0.06) | (0.06) | (0.08) | (3.81) | (3.59) | (3.69) |
| | | | C. Age 25-34 | | | |
| Post Desegregation Years 1 - 5 | -0.15 | -0.11 | -0.09 | -10.90 | -9.54 | -9.68 |
| | (0.04) | (0.03) | (0.05) | (4.90) | (4.88) | (5.21) |
| Post Desegregation Years 6+ | -0.29 | -0.21 | -0.18 | -23.61 | -21.68 | -21.54 |
| | (0.05) | (0.04) | (0.07) | (6.30) | (6.36) | (6.97) |
| | | | D. Age 35-44 | | | |
| Post Desegregation Years 1 - 5 | -0.12 | -0.12 | -0.10 | -12.28 | -12.37 | -10.04 |
| | (0.05) | (0.05) | (0.05) | (4.82) | (4.86) | (4.86) |
| Post Desegregation Years 6+ | -0.16 | -0.15 | -0.16 | -20.47 | -20.52 | -15.74 |
| | (0.10) | (0.08) | (0.07) | (9.10) | (7.92) | (8.26) |
| Number of observations | | 3039 | 3039 | 3039 | 3039 | 3039 |
| Region * Year Effects | X | X | X | X | X | X |
| 1960 County Charact. * Year | | X | | | X | |
| County-Specific Linear Trends | | | | | | X |

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

Table 4
White Homicide Victimization

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

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

Table 5
Supplemental Homicide Report Data: Homicide Offenders

| | Age 15 - 19 | | Age 15 - 24 | | Age 25-35 | | Age 35 - 44 | |
|---|---|---|---|---|---|---|---|---|
| | VS: Victim | SHR: Offender | VS: Victim | SHR: Offender | VS: Victim | SHR: Offender | VS: Victim | SHR: Offender |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | | A. Black QML Count | | | | |
| Post Desegregation Years 1 - 5 | -0.27 | -0.30 | -0.15 | -0.23 | -0.16 | -0.20 | -0.10 | -0.19 |
| | (0.16) | (0.14) | (0.12) | (0.12) | (0.06) | (0.11) | (0.07) | (0.10) |
| Post Desegregation Years 6+ | -0.43 | -0.52 | -0.26 | -0.35 | -0.25 | -0.22 | -0.09 | -0.11 |
| | (0.20) | (0.18) | (0.15) | (0.15) | (0.08) | (0.12) | (0.09) | (0.10) |
| | | | | B. Black OLS | | | | |
| Post Desegregation Years 1 - 5 | -0.74 | -5.53 | -7.05 | -1.41 | -19.09 | -4.80 | -11.29 | -4.37 |
| | (4.91) | (6.25) | (3.69) | (5.67) | (11.66) | (6.21) | (6.11) | (5.55) |
| Post Desegregation Years 6+ | -3.34 | -11.39 | -9.75 | -6.50 | -24.55 | -4.83 | -13.50 | -3.19 |
| | (5.62) | (7.90) | (4.64) | (6.28) | (13.66) | (7.97) | (9.67) | (6.17) |
| | | | | C. White QML Count | | | | |
| Post Desegregation Years 1 - 5 | -0.15 | -0.22 | -0.12 | -0.17 | -0.03 | -0.11 | 0.01 | -0.06 |
| | (0.07) | (0.11) | (0.05) | (0.10) | (0.07) | (0.08) | (0.07) | (0.11) |
| Post Desegregation Years 6+ | -0.28 | -0.12 | -0.22 | -0.15 | -0.02 | -0.11 | -0.05 | 0.02 |
| | (0.11) | (0.15) | (0.08) | (0.10) | (0.09) | (0.08) | (0.08) | (0.11) |
| | | | | D. White OLS | | | | |
| Post Desegregation Years 1 - 5 | -2.98 | -0.17 | -2.02 | 0.34 | -0.62 | -0.04 | 0.95 | 0.98 |
| | (1.22) | (1.18) | (0.70) | (1.03) | (0.77) | (0.84) | (1.61) | (1.37) |
| Post Desegregation Years 6+ | -4.80 | 1.47 | -3.82 | 0.82 | -1.04 | -0.62 | 0.28 | 1.37 |
| | (1.60) | (1.90) | (1.08) | (1.39) | (1.15) | (1.02) | (1.70) | (1.30) |
| Number of Obs. | | 1349 | 1363 | 1349 | 1363 | 1349 | 1363 | 1349 |
| Region * Year | X | X | X | X | X | X | X | X |

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The sample runs from 1976 through 1988 and includes the districts listed on Table A1 (the same set of districts in the sample used on Tables 1-4). The dependent variable is the homicide count in panels A and C and the homicide rate per 100,000 in panels B and D.

Table 6
Across-Age & Across-Race Homicide Offending

| | Offender | Victim: Black 15-24 (1) | Victim: Black 25-34 (2) | Victim: Black 35-44 (3) | Victim: White (4) |
|---|---|---|---|---|---|
| | | QML Count Model | | | |
| Post Desegregation Years 1 - 5 | Black 15-24 | -0.46 | -0.21 | -0.23 | -0.07 |
| | | (.13) | (.21) | (.12) | (.12) |
| Post Desegregation Years 6+ | | -0.65 | -0.18 | -0.32 | -0.12 |
| | | (.17) | (.25) | (.20) | (.15) |
| Post Desegregation Years 1 - 5 | Black 25-34 | -0.20 | -0.33 | -0.25 | 0.14 |
| | | (.14) | (.11) | (.13) | (.16) |
| Post Desegregation Years 6+ | | -0.17 | -0.33 | -0.13 | 0.09 |
| | | (.17) | (.12) | (.15) | (.19) |
| Post Desegregation Years 1 - 5 | Black 35-44 | -0.43 | -0.02 | -0.27 | -0.02 |
| | | (.15) | (.17) | (.14) | (.18) |
| Post Desegregation Years 6+ | | -0.28 | 0.06 | -0.22 | -0.07 |
| | | (.21) | (.19) | (.15) | (.22) |
| Number of observations | | 1336 | 1336 | 1209 | 1323 |
| Region * Year Effects | | X | X | X | X |

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The sample runs from 1976 through 1988 and includes the districts listed on Table A1 (the same set of districts in the sample used on Tables 1-4). The estimates are produced using the QML count model. The dependent variable is the count of homicides by the black age-group identified in the "Offender" column against the group identified in the "Victim" columns. The exposure variable is set equal to the population count of the offender group. The number of observations refers to the black 15-24 offender row.

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

| | Proportional Response | | | | | |
|---|---|---|---|---|---|---|
| | QML Count | | | | | |
| | A. Black Age 35 - 44 Offending | | B. Black Age 35 - 44 Offending Against Whites | | C. White Age 35 - 44 Offending | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post Desegregation Years 25+ | -0.12 | | -0.26 | | -0.18 | |
| | (0.06) | | (0.12) | | (0.08) | |
| Post Desegregation Years 20 - 24 | | -0.08 | | -0.13 | | -0.01 |
| (Average of 1 year of desegregated schooling) | | (0.05) | | (0.11) | | (0.05) |
| Post Desegregation Years 25 - 29 | | -0.19 | | -0.40 | | -0.21 |
| (Average of 4 1/2 years of desegregated schooling) | | (0.08) | | (0.16) | | (0.09) |
| Post Desegregation Years 30+ | | -0.21 | | -0.33 | | -0.09 |
| (Average of 9 years of desegregated schooling) | | (0.12) | | (0.22) | | (0.13) |
| Number of observations | 2760 | 2760 | 2669 | 2669 | 2760 | 2760 |
| Region * Year Effects | X | X | X | X | X | X |

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The sample runs from 1976 through 1988 and includes the districts listed on Table A1 (the same set of districts in the sample used on Tables 1-4). The dependent variable is the homicide count.

Table 8
Falsification Test, Death From Illness

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

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

Table 9
Effect of Desegregation Plan on Nonwhite and Black Employment

| | 1960 - 1990 | | 1970 - 1990 | | | |
|---|---|---|---|---|---|---|
| | Nonwhite Unemployment Rate * 100 | | Black Unemployment Rate * 100 | | Black Labor Force Participation Rate * 100 | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post Desegregation Years 1 - 5 | 0.07 | 0.15 | 0.51 | 0.75 | 0.17 | 0.30 |
| | (0.46) | (0.40) | (0.48) | (0.43) | (1.01) | (0.97) |
| Post Desegregation Years 6+ | -0.06 | -0.15 | 0.52 | 0.65 | -0.37 | -0.14 |
| | (0.87) | (0.77) | (1.02) | (0.89) | (1.37) | (1.27) |
| Number of Observations | 418 | 418 | 315 | 315 | 315 | 315 |
| Region *Year Effect | X | X | X | X | X | X |
| 1960 County characteristics *Year Effect | | X | | X | | X |

Note. Standard errors clustered by county in parentheses. The dependent variable is given in the column headings. The unit of observation is the county-year. The sample contains 1960, 1970, 1980 and 1990 in columns (1)-(2) and 1970, 1980 and 1990 in columns (3)-(6). The results for the 1960-1990 period focus on "non-white" because that is the only minority classification available for the 1960 data point. The 1960-1990 data is bade on those aged 14 or more and the 1970-1990 data is based on those aged 16 or more.

Table 10
Supplemental Homicide Report Data: Homicide Offenders

| | Proportional Response: QML Count | |
|---|---|---|
| | School Year | Summer |
| | (1) | (2) |
| | Black 15 - 19 | |
| Post Desegregation Years 1 - 5 | -0.35 | -0.22 |
| | (0.16) | (0.12) |
| Post Desegregation Years 6+ | -0.54 | -0.54 |
| | (0.20) | (0.18) |
| Number of observations | 1347 | 1347 |
| Region * Year Effects | X | X |

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The sample runs from 1976 through 1988 and includes the districts listed on Table A1 (the same set of districts in the sample used on Tables 1-4). The dependent variable is the count of homicides.

Table 11
Effect of Desegregation Plan on Local Public Good Provision

| | (1) | (2) | (3) |
|---|---|---|---|
| | A. Ratio of Police Expenditures to Population (sample mean: $92) | | |
| Post Desegregation Years 1 - 5 | -1.5 | -0.9 | -1.5 |
| | (2.1) | (2.1) | (2.2) |
| Post Desegregation Years +6 | -8.7 | -8.6 | -8.8 |
| | (4.0) | (4.0) | (4.2) |
| | B. Ratio of Education Expenditures to Population Age 5 - 19 (sample mean: $2733) | | |
| Post Desegregation Years 1 - 5 | 161.7 | 162.2 | 186.6 |
| | (83.9) | (78.9) | (79.7) |
| Post Desegregation Years +6 | 141.5 | 147.6 | 197.8 |
| | (87.1) | (86.0) | (85.8) |
| | C. Ratio of Fire Department Expenditures to Population (sample mean: $41) | | |
| Post Desegregation Years 1 - 5 | -0.8 | -0.6 | -1.3 |
| | (1.5) | (1.6) | (1.5) |
| Post Desegregation Years +6 | -2.3 | -2.4 | -2.8 |
| | (2.2) | (2.4) | (2.5) |
| Number of Observations | 734 | 734 | 734 |
| Region * Year Effect | 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 variables given in the panel titles are County Area tabulations from the Census Bureau's *Census of Governments* and are measured in 1990 dollars. The sample includes the following years: 1962, 1967, 1972, 1977, 1982,1987 and 1992.

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 County (Columbus) | GA | 1971 |
| Cook | Chicago | IL | 1982 |
| Winnebago | | 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 |

Note. The sample is restricted to counties with a school district identified in the Welch and Light (1987) study as having had a "major" court-ordered desegregation plan.

Appendix Table A2: Offending Reduction for Different Age Groups Explained by Reduction in Offending Against Black 15-24 year-olds

| | | Number of Black Offenders | | | | | |
|---|---|---|---|---|---|---|---|
| | | 15-24 | | 25-34 | | 35-44 | |
| | | Total 15-24 Offending | Total 15-24 Offending Against Black 15-24 | Total 25-34 Offending | Total 25-34 Offending Against Black 15-24 | Total 35-44 Offending | Total 35-44 Offending Against Black 15-24 |
| years 1 - 5 | upper 95% | 0.1 | -0.9 | 0.2 | 0.2 | 0.0 | -0.1 |
| | point estimate | -3.2 | -2.1 | -2.6 | -0.5 | -1.2 | -0.3 |
| | lower 95% | -6.5 | -3.3 | -5.4 | -1.1 | -2.3 | -0.5 |
| | p-value | 0.34 | | 0.06 | | 0.10 | |
| years 6+ | upper 95% | -0.8 | -1.4 | 0.2 | 0.4 | 0.5 | 0.1 |
| | point estimate | -4.9 | -3.0 | -2.9 | -0.4 | -0.7 | -0.2 |
| | lower 95% | -9.0 | -4.6 | -5.9 | -1.2 | -1.9 | -0.5 |
| | p-value | 0.22 | | 0.03 | | 0.37 | |

The "Total Offending" columns display the point estimate and confidence interval for the total decline in homicide offending for the group given in the column header (number of homicides; estimates calculated from SHR columns of Table 5 and relevant sample means). The "Total Offending Against Black 15-24" columns display the point estimate and confidence interval for the decline in homicide offending for the group given in the column header against black 15-24 year-olds (number of homicides; estimates calculated from the Black 15-24 *column* of Table 6 and the relevant sample means). The "p-value" row gives the p-value for the test that the "Total Offending" estimate and the "Total Offending Against Black 15-24" estimate are equal.

Appendix Table A3: Victimization Reductions for Different Age Groups Explained by Reduction in Offending by Black 15-24 year-olds

| | | Number of Black Victims | | | | | |
|---|---|---|---|---|---|---|---|
| | | 15-24 | | 25-34 | | 35-44 | |
| | | Total Victims 15-24 | Total Victims 15-24 with a Black 15-24 Offender | Total Victims 25-34 | Total Victims 25-34 with a Black 15-24 Offender | Total Victims 35-44 | Total Victims 35-44 with a Black 15-24 Offender |
| years 1 - 5 | upper 95% | 0.9 | -0.9 | -0.7 | 0.6 | 0.3 | 0.0 |
| | point estimate | -1.8 | -2.1 | -2.5 | -0.6 | -0.8 | -0.2 |
| | lower 95% | -4.5 | -3.3 | -4.3 | -1.9 | -1.9 | -0.5 |
| | p-value | 0.73 | | 0.01 | | 0.28 | |
| years 6+ | upper 95% | 0.5 | -1.4 | -1.4 | 1.0 | 0.6 | 0.1 |
| | point estimate | -3.0 | -3.0 | -3.9 | -0.6 | -0.7 | -0.3 |
| | lower 95% | -6.6 | -4.6 | -6.3 | -2.1 | -2.0 | -0.7 |
| | p-value | 1.00 | | 0.00 | | 0.52 | |

The "Total Victims" columns display the point estimate and confidence interval for the total decline in homicide victimization for the victim group given in the column header (number of homicides; estimates calculated from the VS columns of Table 5, which limit the sample to 1976-1988, and the relevant sample means). The "Total Victims with a Black 15-24 Offender" columns display the point estimate and confidence interval for the decline in victimization for the victim group given in the column header where the offender was a black 15-24 year-old (number of homicides; estimates calculated from the Black 15- 24 *row* of Table 6 and the relevant sample means). The "p-value" row gives the p-value for the test that the "Total Victims" estimate and the "Total Victims with a Black 15-24 Offender" estimate are equal.

Appendix Table A4
Black and White Homicide Victimization, Weighted by Population

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

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

Appendix Table A5
Homicide Victimization, Sample Restricted to Decennial Census

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

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

Appendix Table A6
Effect of Desegregation Plan on County Population

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

Note. Standard errors clustered by county in parentheses. The dependent variable is given in the column heading. The unit of observation is county-year. The estimation sample includes the years 1960, 1970, 1980, and 1990.

Appendix Table A7
Effect of Desegregation Plan on Demographic Characteristics of County

| | Log(Median Family Income) | | Percent Age 25+ w/ High School Degree* | | Percent Age 25+ w/ College Degree | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | | | A. Non-Whites | | | |
| Post Desegregation Years 1 - 5 | -0.011 | -0.012 | -0.016 | -0.007 | -0.005 | -0.003 |
| | (0.017) | (0.018) | (0.009) | (0.009) | (0.005) | (0.004) |
| Post Desegregation Years 6+ | -0.015 | -0.011 | 0.010 | 0.017 | -0.007 | -0.006 |
| | (0.028) | (0.029) | (0.012) | (0.014) | (0.007) | (0.007) |
| | | | B. Whites | | | |
| Post Desegregation Years 1 - 5 | 0.001 | 0.001 | 0.006 | 0.006 | 0.005 | 0.005 |
| | (0.009) | (0.009) | (0.004) | (0.005) | (0.005) | (0.004) |
| Post Desegregation Years 6+ | -0.017 | -0.011 | 0.009 | 0.009 | 0.004 | 0.004 |
| | (0.016) | (0.017) | (0.006) | (0.006) | (0.007) | (0.006) |
| Number of Observations | 420 | 420 | 419 | 419 | 420 | 420 |
| Region *Year Effect | X | X | X | X | X | X |
| 1960 County characteristics *Year Effect | | X | | X | | X |

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

Appendix Table A8
Homicide Victimization: MSA Sample

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

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

Appendix Table A9
Homicide Victimization: Bordering County Sample

| | Proportional Response: QML Count | | | Levels: OLS | | |
|---|---|---|---|---|---|---|
| | Bordering County Sample Estimate | Adjusted Bordering County Estimate | Actual County Sample Estimate (Tables 3 & 4) | Bordering County Sample Estimate | Adjusted Bordering County Estimate | Actual County Sample Estimate (Tables 3 & 4) |
| | $\beta_{BCG}$ | $\frac{\beta_{BCG}}{\delta}$ | $\beta$ | $\beta_{BCG}$ | $\frac{\beta_{BCG}}{\delta}$ | $\beta$ |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | | | A. Black Age 15 - 24 | | | |
| Post Desegregation Years 1 - 5 | -0.05 | -0.09 | -0.14 | -4.53 | -8.19 | -8.91 |
| | (0.04) | | | (2.27) | | |
| Post Desegregation Years 6+ | -0.11 | -0.21 | -0.23 | -5.64 | -10.21 | -10.55 |
| | (0.05) | | | (3.30) | | |
| | | | B. White Age 15 - 24 | | | |
| Post Desegregation Years 1 - 5 | 0.01 | 0.01 | -0.05 | -0.02 | -0.04 | -0.49 |
| | (0.04) | | | (0.31) | | |
| Post Desegregation Years 6+ | -0.07 | -0.12 | -0.18 | -0.69 | -1.25 | -2.2 |
| | (0.06) | | | (0.56) | | |
| Number of observations | 3040 | 3040 | 3040 | 3040 | 3040 | 3040 |
| Region * Year Effects | X | X | X | X | X | X |

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

Appendix Table A10

Black Homicide Age 15 - 24 Victimization Interactions

| | QML Count | | | OLS Level | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post Deseg. Years 1 - 5 | -0.07 | -0.04 | -0.07 | -3.42 | -2.28 | -3.01 |
| | (0.05) | (0.05) | (0.06) | (3.13) | (3.61) | (3.54) |
| Post Deseg. Years 6+ | -0.13 | -0.08 | -0.11 | -4.07 | -3.54 | -3.76 |
| | (0.08) | (0.10) | (0.12) | (4.18) | (4.43) | (4.39) |
| Post Deseg. Years 1 - 5 * | -0.54 | | -0.53 | -28.02 | | -24.01 |
| Δ Exposure Index | (0.20) | | (0.35) | (15.52) | | (19.47) |
| Post Deseg. Years 6+ * | -0.88 | | -0.71 | -27.29 | | -23.65 |
| Δ Exposure Index | (0.29) | | (0.50) | (14.61) | | (19.31) |
| Post Deseg. Years 1 - 5 * | | 0.29 | 0.00 | | 19.54 | 3.82 |
| Δ Dissimilarity Index | | (0.11) | (0.22) | | (11.47) | (13.14) |
| Post Deseg. Years 6+ * | | 0.56 | 0.14 | | 18.95 | 3.50 |
| Δ Dissimilarity Index | | (0.22) | (0.43) | | (10.54) | (12.37) |
| Region * Year Effects | X | X | X | X | X | X |
| Number of observations | 2693 | 2693 | 2693 | 2693 | 2693 | 2693 |

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the count of homicides in columns (1) - (3) and the homicide rate per 100,000 in columns (4)-(6). Δ refers to the change in the variable from one year prior to the implementation of desegregation to the fourth year after desegregation implementation.

Appendix Table A11
White Homicide Age 15 - 24 Victimization Interactions

| | QML Count | | | OLS Level | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post Deseg. Years 1 - 5 | -0.05 | -0.05 | -0.01 | -0.63 | -0.51 | -0.39 |
| | (0.06) | (0.06) | (0.07) | (0.54) | (0.62) | (0.54) |
| Post Deseg. Years 6+ | -0.09 | -0.05 | -0.12 | -1.76 | -1.30 | -2.17 |
| | (0.08) | (0.09) | (0.08) | (0.96) | (1.07) | (0.79) |
| Post Deseg. Years 1 - 5 * | 0.20 | | | 1.57 | | |
| Δ Exposure Index | (0.31) | | | (2.45) | | |
| Post Deseg. Years 6+ * | -0.26 | | | -2.24 | | |
| Δ Exposure Index | (0.36) | | | (3.28) | | |
| Post Deseg. Years 1 - 5 * | | -0.09 | | | -0.52 | |
| Δ Dissimilarity Index | | (0.18) | | | (1.85) | |
| Post Deseg. Years 6+ * | | 0.31 | | | 2.89 | |
| Δ Dissimilarity Index | | (0.25) | | | (2.45) | |
| Post Deseg. Years 1 - 5 * | | | 0.16 | | | -0.75 |
| Δ % white in deseg school | | | (0.73) | | | (3.85) |
| Post Deseg. Years 6+ * | | | -0.24 | | | -2.73 |
| Δ % white in deseg school | | | (0.77) | | | (4.30) |
| Region * Year Effects | X | X | X | X | X | X |
| Number of observations | 2694 | 2694 | 2694 | 2694 | 2694 | 2694 |

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the count of homicides in columns (1) - (3) and the homicide rate per 100,000 in columns (4)-(6). Δ refers to the change in the variable from one year prior to the implementation of desegregation to the fourth year after desegregation implementation.

Appendix Table A12

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

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

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The dependent variable is the ratio of enrollment in the desegregated school district(s) identified in Welch and Light (1987) to the population of children age 5 to 19 in the county. The sample includes 1970, 1980 and 1990.

## APPENDIX A: A BRIEF HISTORY OF SCHOOL DESEGREGATION DECISIONS

The NAACP's initial legal strategy was to attack the principle of "separate but equal" established by *Plessy v. Ferguson* (1896) by challenging discrimination in graduate and professional schools (see NAACP, 2004). The primary motivation for focusing first on post-graduate education, rather than K-12 schooling, was the perceived increased probability of winning – even if the number of students affected by desegregating post-graduate schools would be orders of magnitude smaller.[1] This strategy led to several key victories, which laid the groundwork for the *Brown* challenge.

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

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

One of the first relevant Supreme Court decisions was *McLaurin v. Oklahoma* (1950), in which the court ruled that the University of Oklahoma's decision to force a 68 year old African-American law student to sit apart from other students, separated by a rope, and eat lunch at a different time from whites,

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

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

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

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

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

Over time, the process generating local Federal lawsuits to desegregate schools seems to have become increasingly decentralized and idiosyncratic. As described by Jack Greenberg, director of the NAACP's Legal Defense and Educational Fund from 1961 to 1984: "Ours was not a regimented or even somewhat controlled operation as to sequence and, indeed, other matters. Local groups, usually although not always NAACP, and local lawyers just filed cases … To the extent to which we had influence it was because during early days the number of civil rights lawyers in the south was limited (black lawyers only took such cases and there weren't many black lawyers during early days) and there were more or less close personal relationships. … Also cases needed funding and we exercised some control when groups came to us for money, if not expertise, but cases cropped up on their own, particularly in the North where civil rights lawyers were more abundant during early years."[2] See also Greenberg (1994) and Klarman (2004).

Most recently in June 2007, the U.S. Supreme Court issued two 5-4 decisions striking down school desegregation plans in Seattle and Louisville. Justice Kennedy's controlling opinion leaves open the possibility for more narrowly-targeted desegregation policies such as strategic site selection for new schools or re-drawing school attendance zones. Race-conscious policies are subject to "strict scrutiny" by

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

the courts, which requires that they be “narrowly tailored” but also that there be a “strong basis in evidence” that the relevant policy serves a “compelling government interest.”

The Civil Rights Project has a useful summary of how the courts have interpreted these terms of art in previous cases. The courts generally find that policies to remedy the effects of past discrimination, or “remedial interests,” meet the test for a compelling government interest, but have been more divided over “non-remedial” interests such as promotion of educational diversity (the focus by Justice Powell in *Regents of the University of California v. Bakke*) or reducing racial isolation, and have rejected the use of race-conscious policies to remedy general societal discrimination or to provide role models for racial minorities. The “narrow tailoring” test examines the “fit” between the policy and the objective, where courts often strike race-conscious policies that achieve ends where race-neutral policies would also be an option.[3] As the Civil Rights Project notes, “[school] choice plans that consider multiple factors could be upheld with appropriate educational justification. … Permissible options may [also] include race-conscious efforts that do not single out any one student on the basis of his or her race such as siting schools in areas that would naturally draw students from a mixture of racial / ethnic backgrounds or magnet schools that have special programs that draw students from different backgrounds.” It is also important to note that the Louisville and Seattle decisions do not affect districts that are under court order to desegregate, only those that initiated desegregation efforts on their own.[4]

[3] www.civilrightsproject.ucla.edu/policy/legal_docs/cover.pdf.
[4] www.civilrightsproject.ucla.edu/policy/court/voltint_joint_full_statement.php

## APPENDIX B: DATA

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

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

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

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

Vigdor (2006), Lutz (2005), Orfield and Eaton (1996) and references therein). However, for the runs in which we only have decennial census data, we include 1990 in order to increase sample sizes.

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

One potential complicating factor in the SHR analysis is how to code homicide counts that link together offender and victim age-race characteristics. Take the hypothetical example of a robbery attempt resulting in homicide, with two black victims age 15 and 17 murdered by three white offenders age 18, 19, and 30. *Separately* coding the offender and victim age-race counts in the SHR is relatively straightforward: since there are two white offenders age 15-24 in this homicide event, the white offender age 15-24 count is coded as two. All separate SHR offender and victim age-race group counts in this paper are constructed using this methodology (e.g. as used on Table 5). Less obvious is the best approach to code for counts that *link together* offender and victim age-race characteristics (e.g. as used on Table 6). For these variables we have identified two possible coding methods. First, we can code these groups at the individual level. This method treats each homicide victim uniquely, meaning in the example above this method would identify two white offenders age 15-24 for the first victim and two white offenders age 15-24 for the second victim, added together for a total of four assigned to the white offender 15-24/black victim 15-24 group count. Second, we can code these groups at the homicide event level. Under this variation, if there are *one or more* offenders or victims from a particular age-race group in a given homicide event, that group count would be coded as one. In the above example, the white offender 15-24/black victim 15-24 group count would be assigned as one because at least one offender and one victim in the homicide event fit these criteria. One concern with this methodology is in the instance of a homicide event with either multiple offenders or multiple victims of the same age-race group, these homicide counts would be undercounted. In this paper we use the "individual" level coding methodology for the estimation of offender/victim linked-group results. Given the importance of offender characteristics to our study we are inclined to error on the side of inclusion. These estimates were re-calculated using the "homicide event" level methodology and results remained largely unchanged.

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

To construct homicide victimization and offending rates we also require some data on annual county population counts by age and race. For our VS analysis, population data for 1960, 1970, 1980 and

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

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

The propensity of police agencies to report, or report accurately, also varies across areas and over time; see for instance Maltz (1999) for a detailed discussion, with a focus on how measurement error with the UCR is particularly severe at the unit of observation for our study – the county. UCR data are noisy particularly at the county level because of inconsistent reporting practices by local police agencies that are not well documented in the UCR (Maltz, 1999). Police may also classify events into different crime categories differently over time. For example police practices for determining what counts as an aggravated versus simple assault seem to have changed sharply over time, as evidenced in part by the fact that UCR data show a substantial increase over our study period in aggravated assault rates, while victim reports to the National Crime Victimization Survey (NCVS) show flat trends (Blumstein, 2000). The other limitation of the UCR is that to identify offenses committed by population sub-groups we must rely on arrest data, and the fraction of offenses (aside from homicide) that result in arrest is quite low. Even the "clearance rate" for homicide itself is surprisingly low. Given these UCR data problems, it is not surprising that most of our results from analyzing the UCR are very imprecisely estimated.[5]

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

The data on government spending are obtained from the *Census of Government* (COG) for the years 1962, 1967, 1972, 1977, 1982, 1987, and 1992. We use the version of the COG contained in the County Area Finances Since 1957 Historical Database – a longitudinally consistent version of the COG produced by the Census Bureau. The COG data capture all direct expenditures of sub-state level governments, such as municipality and county governments, within the county. We examine school, police and fire spending.We do not examine other types of social program spending because so much of that is accounted for by higher levels of government not captured by our COG data.

The demographic data on counties are obtained from the 1960, 1970, 1980 and 1990 decennial censuses. We use versions of the census data summarized at the geographic level of the county. The 1960 data were obtained from hardcopy versions of Census of Population: 1960, Vol. 1, *Characteristics of the Population*. The 1970, 1980 and 1990 data were obtained in electronic format from the National Historic

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

Geographic Information System (NHGIS) maintained by the Minnesota Population Center, University of Minnesota.

## APPENDIX C: ADDITIONAL ESTIMATION DETAILS

### *I. QML Count Model*

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

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

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

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

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

By imposing the constraint that $\psi$=1, the $pop_{it}$ variable controls for "exposure". The parameters of interest, $\beta_p$, 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.[6] 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.

[6] The $\beta_p$ coefficients can also be interpreted as semi-elasticities in the linear log dummy variable model.

## APPENDIX D: DISCUSSION OF CROSS-AGE OFFENDING RESULTS

Our main results suggest that court-ordered school desegregation reduces homicide victimizations and offending among blacks who are already adults at the time the court orders are enacted, at least in the short term. As discussed in the text, we hypothesize that one mechanism for these changes among black adults comes from changes in the rate at which young blacks (15-19 or 15-24) commit homicides against black adults, and in the rate at which adults commit homicides against younger people. Appendix Tables A2 and A3 present the results of cross-equation hypothesis testing which determines how much of the change in the homicide victimization and offending of black adults can be explained by changes in homicide events that involve youth and adults on different sides of the offender / victim divide.

To illustrate our approach consider the middle panel of Appendix Table A2. The first column labeled "Total 25-34 Offending" presents the mean change in the annual homicide offender count for blacks age 25-34 implied by the QML estimates in Panel A, column (6) of Table 5 and the relevant population and homicide rate means. We use homicide counts, as opposed to the homicide rates used elsewhere in the paper, for these comparisons to avoid complications that arise from using county populations for different age groups as the denominators in the homicide rates. The estimates show that after a desegregation order is enacted, homicides by 25-34 year old blacks decline by -2.6 per county (with a 95 percent confidence interval from 0.2 to -5.4 deaths per county), while 6+ years after such a court order homicides to blacks 25-34 years old decline by -2.9 per county (with a confidence interval that ranges from 0.2 to -5.9). The estimates in the next column show that after 1-5 years of desegregation, homicides by 25-34 year old blacks against black age 15-24 victims decline by -0.5 (confidence interval 0.2 to -1.1) and for 6+ years equal to -0.4 (confidence interval 0.4 to -1.2). We use the seemingly unrelated estimation methodology discussed in Weesie (1999) to test the cross-equation null hypothesis that the decline in the total number of black 25-34 year old homicide offenders (first column) is the same as the decline in the number of black 25-34 year old homicide offenders with black age 15-24 victims (second column).

For the middle panel of Table A2, we can reject the null hypothesis for 1-5 years of desegregation, but only at the 10% significance level (p=0.06). We can just barely reject the hypothesis for 6+ years after desegregation at the 5% significance level (p=0.03). The fact that our test statistics hover right at the threshold for rejecting the null hypothesis is at least compatible with the idea that a large fraction, but not all, of the decline in offending by blacks age 25-34 was driven by declines in offending against blacks age 15-24. Turing to the right panel, we are unable to reject the null hypothesis for black age 35-44 offenders: Our data are consistent with the possibility that the *entire* decline in black age 35-44 offending was driven by a reduction in offending against black youth.

Table A3 considers victimization. In the right panel, we are unable to reject the hypothesis that the decline in homicide victimization to blacks age 35-44 was equal to the decline in homicide offending by blacks age 15-24 against black 35-44 age victims. We are able, though, in the middle panel to reject the hypothesis for black 25-34 age victims. Overall, we view these results as failing to rule out the possibility that a sizeable share of the change in black adult homicide offending and victimization was driven by a reduction in homicide events involving black youth.

## APPENDIX E: RESULTS OF TESTS FOR ENDOGENOUS MIGRATION

One potential concern with our results is that population migration could lead us to confound behavioral responses by county residents with compositional changes in the county population over time. To explore this issue, in Panel A of Appendix Table A6 we estimate the parsimonious version of equation (3) using as the dependent variables the log of the county population of 15 to 24 year old whites or 15 to 24 year old blacks. The sample is restricted to the decennial Census years of 1960, 1970, 1980 and 1990 to avoid issues with measurement error. There is no evidence that desegregation induced migration across county boundaries for either whites or blacks. At first brush these results might seem inconsistent with those in Baum-Snow and Lutz (2010), henceforth BSL, who find evidence of black migration into desegregated central city schools, but only outside of the south. Panel B therefore allows the desegregation effect to vary by region, and shows that there is no evidence of cross county migration in or outside of the South. Our results are easily reconciled with those of BSL by noting that BSL find in-migration into desegregated school *districts* – as opposed to the *counties* used in this paper. This migration was likely intra-county because non-southern school districts tend to be smaller than the counties in which they are located. This hypothesis is supported by the results in Panel B. County-wide school districts would perhaps have been more likely to have experienced cross county migration as the result of desegregation. However, estimates which allow the desegregation effect to vary by the presence of a county-wide school district provide no evidence of migration (unreported). The same thought process applies for whites as well: Although there is strong evidence that whites exited desegregated school districts (e.g. Reber 2005), our evidence suggests that they did not leave the county, but instead moved to nearby alternative public districts or went to private schools. Presumably much of the in-migration by blacks into urban school districts in BSL must be coming from inner suburbs within the same counties. (Boustan (2009) finds that in areas close to school district boundaries, desegregation caused both whites and blacks to migrate). These results also provide further assurance against the possible concern that measurement error in the denominator of the homicide rate is responsible for our results.

We can also check whether our findings are driven by compositional changes in county population by using decennial Census data from 1960, 1970, 1980 and 1990 to estimate the impact of desegregation on county demographic characteristics (Appendix Table A7). For blacks, the point estimates for median family income and the probability that an adult had finished high school or college are small, statistically insignificant, and generally negative, suggesting that if anything the county black population is becoming more, not less, crime prone (see Jacob and Ludwig, 2009).[7] While there is some evidence that the percent of whites finishing high school increased, the estimate is only marginally significant and is small in magnitude, suggesting around a 1 percentage point increase.

As another check on the possibility that our findings are driven by cross county migration, we re-calculate our estimates using MSA-year as the unit of observation (Appendix Table A8).[8] If our results were simply due to population migration across nearby county lines in response to desegregation orders, we would not expect any impact on homicide when the analysis is conducted at the level of the MSA. But the MSA-level estimates are quite similar to our main findings, suggesting endogenous migration does not explain our results.

In principle people could be migrating out of the MSA entirely, but when we replicate our results using larger geographic areas still (bordering county groups), our results, shown in Appendix Table A9, again do not seem to support an endogenous migration story. Unlike the MSAs, where a substantial majority of the population lives within a desegregated county, within the "bordering county groups," a substantial fraction of the population resides in non-desegregated counties. Specifically, 55 percent of blacks age 15 to 24 reside in desegregated counties and the remainder reside in counties which border a desegregated county. For whites age 15 to 19, the comparable figure is 44 percent. If our main findings represent a true causal relationship, then the bordering county group treatment effect, $\hat{\beta}_{BCG}$, divided by the

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

average percent of the population residing in desegregated counties (as opposed to bordering counties), $\delta$, should equal the standard, county-based treatment effect, $\hat{\beta}$: $\frac{\hat{\beta}_{BCG}}{\delta} = \hat{\beta}$ (this equality is derived below). We therefore expect the adjusted bordering county group estimate, $\frac{\hat{\beta}_{BCG}}{\delta}$, to range between $\hat{\beta}$ and 0, with $\hat{\beta}$ in the case of no endogenous migration and 0 in the case where our results solely reflect endogenous migration. The bordering county group estimates, $\hat{\beta}_{BCG}$, are presented in columns (1) and (4) of Table A9, the adjusted estimates, $\frac{\hat{\beta}_{BCG}}{\delta}$, in columns (2) and (5) and, for comparison, the standard county-based estimates, $\hat{\beta}$, in columns (3) and (6). The adjusted bordering county group estimates are similar to the standard estimates, particularly for the black results, suggesting endogenous migration does not explain our results.

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

**County DD estimator**

$i = 0$ : never desegregated

$i = 1$ : county desegregated at time $t = 1$, segregated at time $t = 0$

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

**Bordering County Group DD Estimator assuming no migration**

The treatment group can be seen as being composed of two sub-groups – the desegregated counties (same as above; $i$=1) and the counties not subject to court-ordered desegregation, but located in the same bordering county group as a desegregated county ($i$=2).

$i = 2$ : not desegregated

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

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

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

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

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

then

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

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

## APPENDIX F: ADDITIONAL RESULTS ON MECHANISMS

We can provide some indirect evidence on what behavioral mechanisms might matter most by interacting changes in our measures of school segregation and other measures with our indicators for implementation of court orders.[9] We note that these findings are at best suggestive, since those counties that experience particularly large changes in any one of our candidate mediators may also experience large changes in other potential mediating mechanisms not captured by our data. The fact that there is no evidence of pre-existing trends in homicides before the court orders are enacted means unmeasured mediators are probably not biasing our outcome estimates, but our ability to determine the specific mediators that are driving our observed homicide impacts is somewhat limited.

With these caveats in mind, Appendix Table A10 shows that homicide victimization rates declined the most for blacks in districts where exposure of blacks to whites in the public schools increased the most. These results come from estimating our preferred QML model (OLS results are usually qualitatively similar but less precise). When we include interactions of our "treatment" indicators (years post court desegregation order) with changes in the exposure and dissimilarity indices at the same time (column (3)), the former seems to be driving the result.[10] The fact that we observe the largest impacts on black homicide in places with the largest "treatment dose" from court orders provides additional support for the credibility of our research design. For whites (Appendix Tables A11), these interaction estimates are quite imprecise.

Finally, there is another potential mechanism that would be relevant only for whites – migration out of the desegregated school district. While there is no evidence of "white flight" out of the counties, there is evidence that whites move from school districts subject to desegregation orders to other districts *within the same county* that are not subject to court-ordered school desegregation. Appendix Table A12

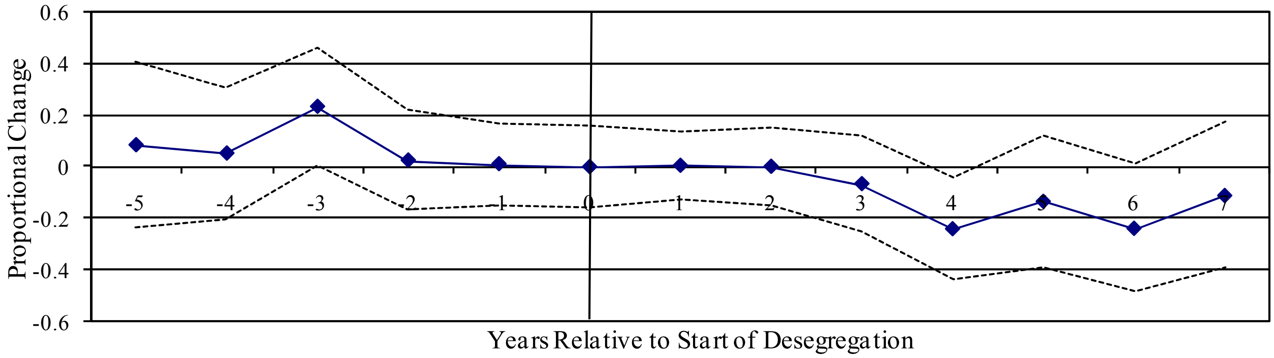
[9] The changes in the segregation indices are defined as the changes from one year prior to desegregation to four years after desegregation.

[10] 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.

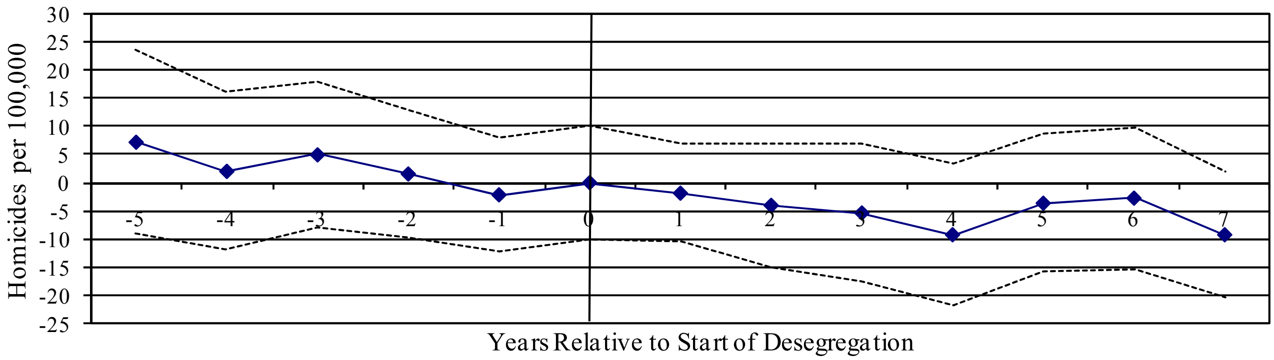
shows that the ratio of white enrollment in districts subject to court orders to the total number of white school-age children in the county declines by between 4 and 6 percentage points after five or more years of desegregation – around a 15 percent decrease relative to the sample average of 0.39 (see also Reber 2005, and Baum-Snow and Lutz 2010). These results, together with our finding of no decline in the overall number of school-age white children in our counties, imply that some white families must be moving to other public school districts (and, according to BSL, private schools for whites outside of the South) within the same county to avoid court-ordered desegregation. If these new districts or private schools are less criminogenic than the districts subject to desegregation orders, this could provide another mechanism driving our result. One suggestive data point against this hypothesized mechanism comes from Appendix Table A11, columns (3) and (6), which shows that the impact of desegregation orders on white homicide victimizations do not appear to be larger in desegregating districts with the largest change (i.e. decline) in the percent of white children in the county enrolled in the desegregated school district (i.e. the measure explored on Appendix Table A12).

**Figure A1 School Desegregation & Black Homicide Victimizations**

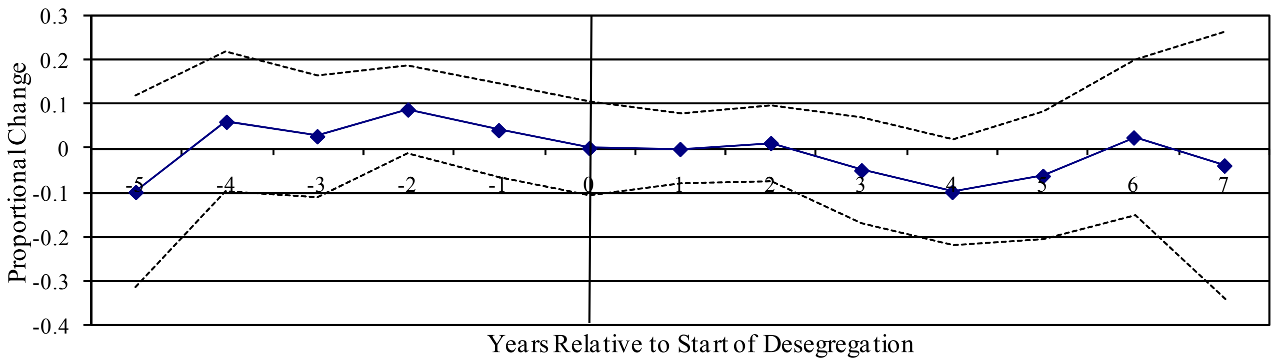
Panel A: Age Cohort 15-19 QML Count



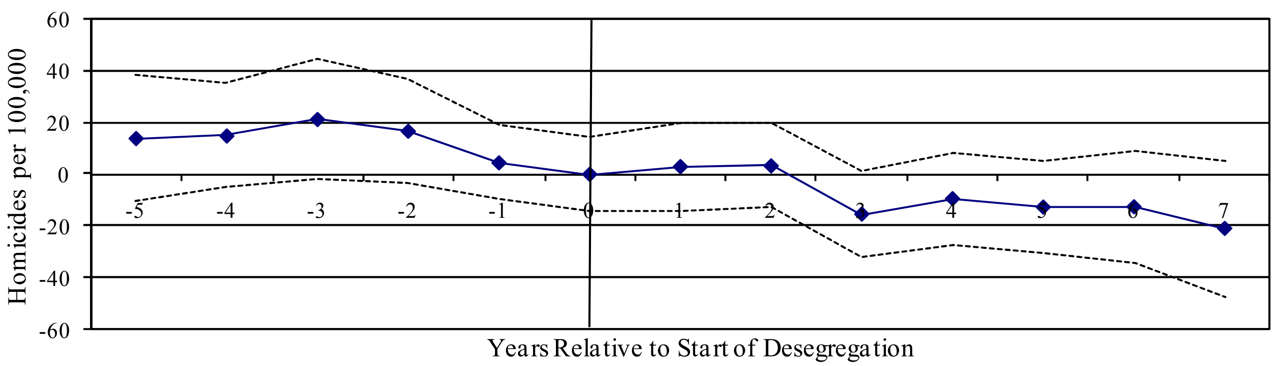

Panel B: Age Cohort 15-19 OLS Level




Panel C: Age Cohort 35-44 QML Count



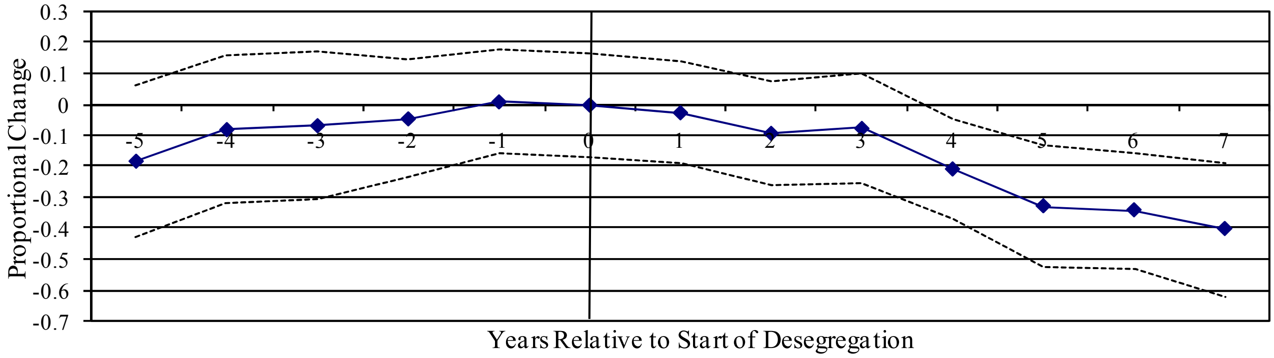

Panel D: Age Cohort 35-44 OLS Level



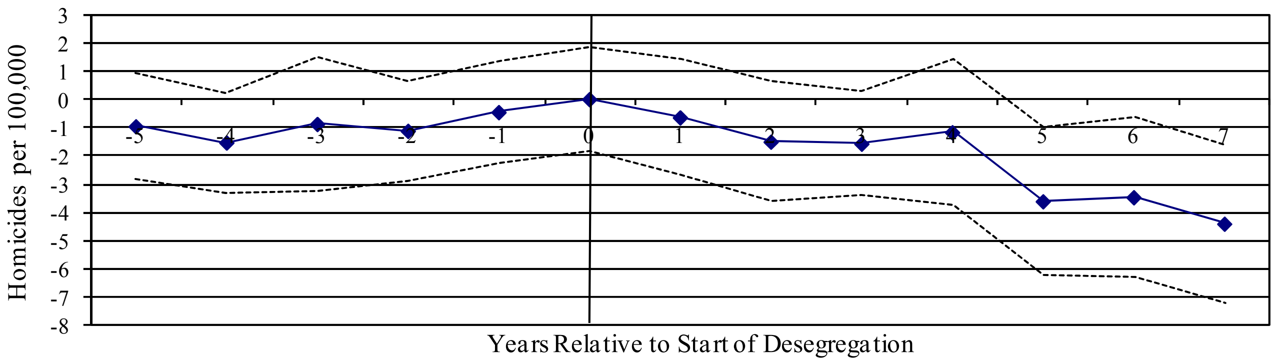

The solid points display coefficient estimates and the dashed lines display the 95% confidence intervals around these estimates. Year “0” is the year immediately prior to the start of desegregation.

**Figure A2 School Desegregation & White Homicide Victimizations**

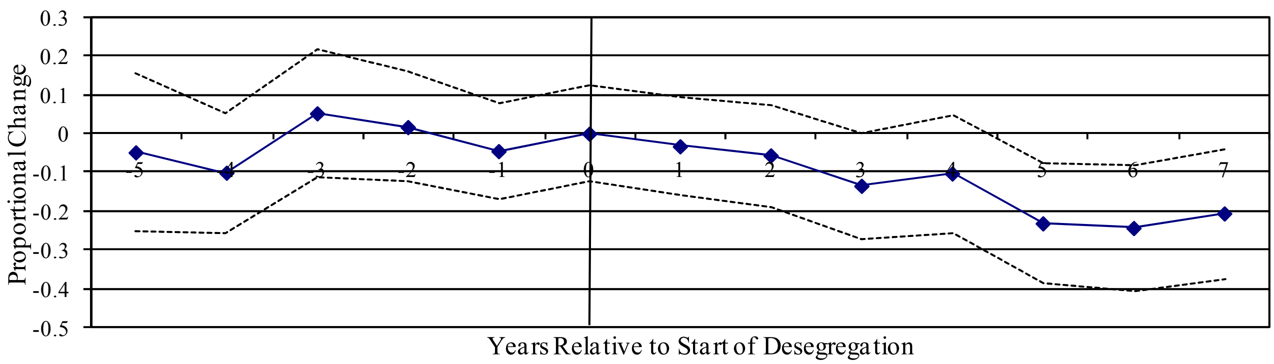
Panel A: Age Cohort 15-19 QML Count



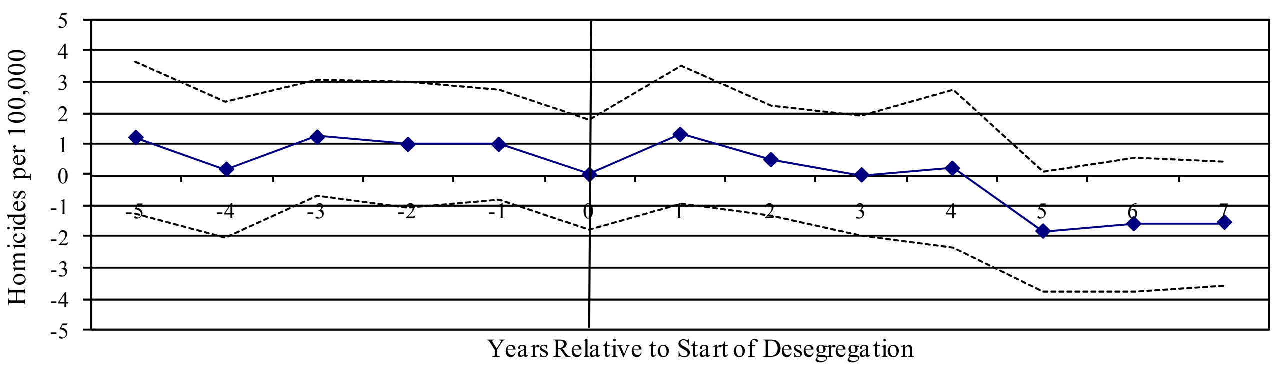

Panel B: Age Cohort 15-19 OLS Level




Panel C: Age Cohort 35-44 QML Count




Panel D: Age Cohort 35-44 OLS Level




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