THE EFFECTS OF SCHOOL DESEGREGATION ON CRIME

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This paper studies the impact of school desegregation on youth violence, which may be influenced by many of the same mechanisms that are relevant for children's academic outcomes including improvements in school quality or changes in peer groups. Our research design exploits the fact that since the Supreme Court's 1954 *Brown* decision, the majority of the nation's largest school districts were subject to mandatory, court-ordered desegregation plans. The timing of when these plans went into effect is idiosyncratic and plausibly exogenous to other determinants of youth outcomes across jurisdictions. Our results suggest school desegregation reduces homicide victimization rates for both black and white youth. These results appear to be due in part to some combination of school racial integration for blacks, increased enrollment in alternative public schools and private schools for whites and, for both blacks and whites, increased spending on law enforcement in areas with court-ordered desegregation.

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I. INTRODUCTION

In 1954 the U.S. Supreme Court ruled in *Brown v. Board of Education of Topeka* (347 US 483) that racial segregation in the public schools "denies to Negro children the equal protection of the laws guaranteed by the Fourteenth Amendment," launching one of the most important social policy changes of the 20th century. The Supreme Court's ruling was based in part on the belief that a school's racial makeup affects the outcomes of minority children. Whether the court was correct in this belief remains of considerable policy interest given the possibility of some re-segregation of public schools in recent years,[1] as well as signs of declining support for school desegregation among African-Americans.[2]

The central challenge confronting researchers for the past 50 years is that children who attend more segregated schools may be systematically different from those who enroll in more racially mixed schools in ways that are both difficult to measure and relevant for children's outcomes. Only recently have researchers made real headway on this problem, with a handful of studies now providing scientifically credible evidence that reducing school racial composition improves schooling outcomes for African-American students, and perhaps whites as well (Hoxby, 2000,

[1] The fraction of black students attending majority non-white schools increased in the late 1980s and early 1990s (Clotfelter, 2004), although this seems to be driven in part by a general increase in American public schools in the share of students who are non-black and non-white (Logan, 2004, Clotfelter, Ladd and Vigdor, 2005). Other measures of school desegregation are relatively constant over this period.

[2] For example Jaynes and Williams (1989, p. 84) note that the fraction of blacks who report that they support school desegregation declined from 78 percent in 1964 to 55 percent in 1978. Recent years have also seen a split on desegregation between the national NAACP and local branches of the organization, a striking development given the NAACP's leading role in the original *Brown* challenge.

Hanushek, Rivkin and Kain, 2004) and that court-ordered school desegregation plans reduce African-American dropout rates (Guryan, 2004 and Lutz, 2005).

What is striking is that almost all of the existing research focuses on *academic* outcomes, even though the impacts of school desegregation on non-academic outcomes could either substantially enhance – or potentially offset – the social welfare gains from desegregation impacts on academic outcomes. In principle the same mechanisms through which school desegregation might improve test scores or schooling attainment could lead to beneficial impacts on non-academic outcomes as well. However it is also possible that school desegregation could increase inter-racial tensions, thereby contributing to some deterioration in other youth outcomes.

Particularly important among these other outcomes may be criminal behavior given the enormous costs that crime imposes on society – on the order of $2 trillion per year by one recent estimate[3] -- which can greatly alter the benefit-cost calculation for social programs. The Job Corps is one noteworthy example of the importance of considering impacts on crime (Burghardt et al., 2001, Schochet et al., 2003). Another example comes from the Perry Preschool early childhood intervention, for which Belfield et al. (2006) find that fully two-thirds of the benefits come from reductions in criminal behavior.

In this paper we present what we believe are the first estimates for the effects of school desegregation on crime that rely on a plausibly exogenous source of identifying

[3] Ludwig's (2006) estimate of $2 trillion per year for the social costs of crime update previous work by Anderson (1999), Cohen et al. (2004) and Cohen (2005) to reflect 2005 figures and incorporate the costs of economic as well as "street" crimes.

variation. The key to our research design is that a year after the landmark *Brown* ruling, the Supreme Court required that public schools be desegregated "with all deliberate speed" but did not indicate a more specific timetable. In practice school districts were quite slow to desegregate, and as a result most of the major school districts were eventually forced to desegregate by local court order. Guryan (2004) argues that within the set of districts subject to court order, the timing of when the orders are executed is plausibly random. This difference in the timing of desegregation orders among the districts that were ever subject to such orders serves as the identifying variation we use to estimate the effects of school desegregation on crime.

Specifically we use annual mortality data from the Vital Statistics (VS) over the period 1959-89 to relate trends across counties in homicide rates to black and white youth to the timing of court desegregation orders. One advantage of the VS data is that they are more reliable than other commonly-used sources of information about local crime rates, such as data on crimes reported to the police compiled by the FBI's Uniform Crime Reporting (UCR) system (see for example Maltz, 1999). A disadvantage is that the VS data only provide information about homicides and not other crimes, although murder does account for a disproportionately large share of the overall social costs of crime.[4] Another disadvantage is that the VS data identify the race and age only of the victim, not the offender, a point to which we return below.

[4] Ludwig (2006) estimates that the victimization costs from street crime in the U.S. in 2004 or 2005 equal around $694 billion. The costs for murder alone equal around $156.5 billion, derived by multiplying Cohen's (2005) figure of $9.7 million societal willingness to pay for each murder averted by the FBI's count of homicides in 2004, equal to 16,137.

We find that court-ordered school desegregation reduces homicide victimization rates to both black and white youth by around 20 and 35 percent, respectively. We also provide evidence on several of the potential mechanisms linking desegregation and crime. First, the decline in homicide rates for both groups seems to be due in part to an overall increase in police spending in counties following implementation of a court-order to desegregate the public schools. Second, we find some evidence to suggest that the decline in homicides for whites might be partially due to an increase in white enrollments at alternative, non-desegregated public schools and private schools. Finally, we provide suggestive evidence that the increase in racial integration produced by desegregation reduced black homicides. The next draft of this paper will also examine how school desegregation orders affect the proportion of homicides that involve offenders of the same versus different race as the victim, as a way to learn more about the underlying mechanisms behind the reduced-form policy effects that we estimate.

The remainder of the paper is organized as follows. The next section discusses previous literature on the effects of school racial composition and desegregation efforts. We then review the data used in our study, our analytical methods, and our findings. We conclude with some discussion of the limitations of our findings, next steps, and potential policy implications.

II. PREVIOUS RESEARCH

Perhaps the most obvious candidate mechanism through which school racial segregation might affect outcomes for minority children is by limiting their exposure to high-quality schools. In fact documenting such disparities was the main motivation for the massive data collection effort that led to the famous Coleman Report (1966). Section 402 of the Civil Rights Act of 1964 required that the U.S. Commissioner of Education "shall conduct a survey … concerning the lack of availability of equal educational opportunities for individuals by reason of race, religion, or national origin in public institutions at all levels in the United States."

The fact that James Coleman and his colleagues did not find the expected large differences in school inputs between schools serving disproportionately minority versus white student bodies does not by itself eliminate the possibility of school quality as an explanation for segregation effects on student outcomes. Previous research finds, for instance, that measurable teacher characteristics are weakly correlated with student learning but that nonetheless there are very important differences across teachers in their classroom effectiveness (as measured by "value added" in student test scores) and that more effective teachers tend to prefer teaching in schools that serve more affluent and white student bodies (Hanushek and Rivkin, 2006).

The Supreme Court itself assumed that school segregation harms the outcomes of minority children for reasons beyond access to quality schools, as suggested by the *Brown* ruling itself: "… segregation of children in public schools solely on the basis of race deprives children of the minority group of equal educational opportunities, even

though the physical facilities and other 'tangible' factors may be equal." Attending a racially segregated school could influence perceptions of self worth among minority children, a possibility suggested by Kenneth Clark's landmark "doll study" that was explicitly cited in the *Brown* decision.[5] Attending segregated schools could cause minority children to conclude that they have limited opportunities to succeed in American society. Or because race continues to be correlated with both social class and academic achievement, school segregation could influence youth achievement through the usual sorts of "peer effects" stories.[6]

The Coleman Report itself found that school racial composition seems to be weakly correlated with student achievement, and that having more affluent schoolmates was instead a much stronger predictor of individual student test scores. Even recent studies that also use cross-section variation like the Coleman Report typically research similar conclusions (Mayer, 1991, Rivkin, 2000, Rumberger and Palardy, 2005, Card and Rothstein, 2006).[7]

More recent research that uses different research designs typically finds stronger evidence that school racial composition affects the academic achievement of

[5] Clark (1947) found that the majority of a group of sixteen black children attributed positive characteristics to white dolls and negative characteristics to otherwise identical black dolls. He suggested this response was a manifestation of the implied inferiority of racial segregation imprinted on the African-American psyche.

[6] Attending schools with higher-achieving and pro-social peers could improve student outcomes by affecting their preferences (social stigma about achievement), information (returns to achievement), or constraints (availability of teacher time to teach rather than discipline disruptive students, or opportunities to form study groups with higher-achieving classmates) (Manski, 2000). Jencks and Mayer (1990) note, however, that exposure to higher-achieving peers could in principle reduce a given student's achievement as well if the competition for grades in such environments become more intensive and so some students reduce effort and give up, or alternatively have a negative psychological reaction to being surrounded by higher-achieving and more affluent peers.

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

African-American students and perhaps white children as well. Hoxby (2000) uses plausibly random variation across cohorts in student demographic composition in Texas and finds that a 10% increase in the share of one's classmates that are black reduces achievement test scores by blacks by around .1 standard deviations in reading and .06 standard deviations in math, with effects on whites about one-quarter as large. Hanushek, Rivkin and Kain (2004) also use Texas data and find that a 10% increase in a school's percent black would reduce test scores for blacks by around .025 standard deviations with no statistically significant effect on whites.

Christopher Jencks and colleagues (1972) note that in principle the effects of school desegregation policies need not be the same as what we would expect from naturally occurring variation in school racial composition if desegregation efforts exacerbate racial tensions. That is, there could be an important distinction between the effects of "desegregation" versus "integration" if policy efforts to create more racially diverse schools fail to change the frequency or nature of social interactions between students of different races and economic backgrounds.

In this sense, particularly important for policy purposes are Guryan's (2004) findings that court-ordered school desegregation plans reduce black dropout rates by 2-3 percentage points, with no detectable effect on whites. Guryan's analysis focuses just on those school districts that were subject to a court-ordered desegregation plan during his study period, and argues that among this set of districts the timing of when these orders went into effect is plausibly random. He shows that there are no systematic differences between districts with "early" versus "late" court orders in

socio-demographic trends in the period before these court orders went into effect. We rely on a similar identification strategy in our own analysis. However unlike with Guryan, who relies on decennial census data, we are able to more fully exploit differences in the specific year when these court desegregation orders go into effect by using annual data from the Vital Statistics (described below). In any case, Lutz (2005) finds qualitatively similar effects when he examines the impact of termination of many of these desegregation plans during the 1990s.

Almost all of the large body of research in this area has focused on understanding the effects of school racial composition or desegregation policies on academic outcomes, although a few studies have considered effects on earnings and wages. Vigdor (2006) shows that the black-white earnings gap was generally larger in the South than in the North for cohorts born before 1950, but displayed no association with region for cohorts born after that date when the South transitioned from having the nation's most segregated schools to having the most integrated schools. Boozer, Krueger and Wolkon (1992) also use variation across states over time in school segregation and find that attending schools with more racially mixed peers increases schooling attainment and wages. Studies that use cross-section variation in school racial composition on wages or earnings yield conflicting results (Grogger, 1996, Rivkin, 2000).

There are reasons to believe that if school desegregation improves educational attainment and earnings there may be effects on crime as well. One reason is available evidence suggesting that improvements in schooling attainment, work or wages reduce

crime (Grogger, 2000, Raphael and Winter-Ebmer, 2001, Lochner and Morretti, 2004). It is also possible that whatever psychological, peer or school quality mechanisms lead to school desegregation effects on academic outcomes and earnings may directly affect youth involvement with anti-social behavior. Kling, Ludwig and Katz (2005) find that moving low-income minority youth from high-poverty schools and neighborhoods into more economically and racially mixed social environments leads to a decline in violent criminal behavior, although males may become more involved with property offending. Ludwig and Kling (2007) find that neighborhood racial composition may be more important than economic integration or local crime rates (which are central to "contagion" models) in explaining these community effects on violent crime.

To the best of our knowledge only one previous study examines the effects of school desegregation on crime. Lafree and Arum (2006) use a research design that follows Card and Krueger (1992, 1996) and examines whether incarceration rates are higher in the 1970, 1980 and 1990 decennial censuses for people who were born in states with more racially segregated schools, holding adult state of residence constant. They find that blacks brought up in states where schools were relatively more segregated have higher incarceration rates as measured by the census, and that these associations increase in magnitude over time for more recent birth cohorts. However their identification comes from comparing people who move out of state between birth and adulthood. These mobility decisions could themselves be influenced by segregation in schools or other public institutions, there necessarily remains some

question about whether these estimates adequately capture the causal effects of school desegregation on crime.

III. DATA AND SAMPLE

Our main source of data for measuring crime comes from the Vital Statistics (VS) system of the United States. Administered by the National Center for Health Statistics (NCHS), a division of the Centers for Disease Control and Prevention, the VS provides a census of all death certificates. 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.[8] For earlier years we use

[8] 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.

micro-mortality records and aggregate up to the level of the county, cause-of-death and decedent category ourselves.

The key explanatory variable for our analysis is the date that school districts were subject to local court orders to desegregate. These dates come from a dataset compiled by Finis Welch and Audrey Light that was commissioned by the U.S. Commission on Civil Rights, and provides information on desegregation status for a sample of 125 school districts that desegregated between 1961 and 1986 (see Appendix Table A1 for a complete list of the districts and their date of desegregation). In addition to information on the year and type of desegregation plan that was implemented, the Welch and Light dataset contains variables on public school racial composition and racial integration level. While the sample represents less than 1 percent of U.S. school districts, the sample includes most of the largest districts in the country and so covers almost 20 percent of total U.S. high school enrollment and fully half of total minority enrollment as of 1968.

One complication for our study is that the Welch and Light dataset has the school district as the unit of analysis, while our Vital Statistics data are available at the county level. 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.[9]

In order to understand how the relative frequency of homicide to whites and blacks changes in response to court-ordered school desegregation we also require some information on annual county population by age and race. Population counts for 1960, 1970, 1980 and 1990 come from the decennial census. For the inter-censal years for the 1968-91 period the CMF provides population figures that are calculated by the Census Bureau that begin by linearly interpolating population counts from the decennial censuses, and adjusting for data on births and deaths in each county.[10] 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 over the 1960-68 period.

Measurement error for county population could in principle lead to systematic biases with our estimates if one consequence of court-ordered desegregation is to increase "white flight" to other counties. In this case because our analysis focuses on county-level homicide rates, or in count models on total homicides adjusting for

[9] 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.

[10] 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.

"exposure" (county population), mismeasured white flight during intercensal years would lead us to understate homicide rates for whites following desegregation. In practice this does not seem to be much of a concern. While there is strong evidence that desegregation caused white families to exit public school districts subject to desegregation plans (Reber 2005), this does necessarily mean that families exited their county of residence. Alternatives to moving out of the county include moving to a non-desegregated public school district located in the same county, or attending a private school. We provide evidence suggesting that "white flight" at least partly induced families to flee to these alternative schooling options, as opposed to exiting the county. Specifically, we find no effect of desegregation orders on the log of county population of whites 15-19 using only data from the decennial censuses conducted between 1960-1990 (and so quite reliably measured), and we also provide evidence suggesting desegregation increased private school attendance for whites.

In our analysis below we also examine various candidate mechanisms through which school desegregation orders may affect crime. One potential mediating mechanism is changes in county-level police expenditures, which represents one possible response by local public officials who may be concerned about social unrest in response to court-ordered school desegregation. A dramatic example of this response is provided by the 1974 desegregation of Boston's schools. Rioting by whites opposed to desegregation, as well as inter-racial fighting at desegregated schools, produced an aggressive police response – "a cop for every kid." (HGSE News, 2000) Data on police officers and spending by all law enforcement agencies within our Welch and

Light sample of counties are available from ICPSR for the years 1971-75, 1979, 1985, and 1988. The ICPSR data for 1985 do not match published figures from the *Sourcebook of Criminal Justice Statistics* so we exclude data for that year from our analysis.[11]

Another potential mediating mechanism is private school attendance and alternative public school attendance. Whites may respond to desegregation by fleeing the desegregated public school for either a private school or an alternative, non-desegregated, public school district within the same county. If these schools produce students with a lower crime propensity, the shift in attendance patterns produced by desegregation may lower homicide rates. In order to investigate this hypothesis, we examine how desegregation impacted the propensity for children to attend the desegregated school district. We also examine the impact on the propensity to attend private school. We examine this outcome using micro census data from 1970 and 1980. County identifiers are not available in the 1970 and 1980 census and we therefore use consolidated county groups as the unit of observation[12].

[11] Data for full-time employment and payroll for police protection services were extracted from the Inter-University Consortium for Political and Social Research study number 7636, "Expenditure and Employment Data for the Criminal Justice System: CJEE Longitudinal File, 1971-1979, 1985, 1988". The data were assembled from official state and local records; self-enumerated forms; Budget of the United States Government, Fiscal Year 1987; and Budget of the United States Government, Fiscal Year 1990. The original sample consisted of all 50 state governments and Puerto Rico, all county governments, all municipalities with populations of 10,000 or more, a probability sample of cities and townships with populations of less than 10,000 selected according to the relative size of annual expenditures, and special campus police of public universities and colleges.

[12] The consolidated county group is the smallest unit of observation available on the 1970 and 1980 censuses. It is a larger geographic area than the county. Individual 15, 16 and 17 year olds are matched from county groups to the school districts in the Welch and Light (1987) sample using mappings provided by Jon Guryan. See Guryan (2004) for more information on this procedure.

Of primary interest for our study are the effects of school desegregation on the criminal behavior of black and white youth. VS data on homicide victimizations to black and white youth 15-19 will be correlated with offending by youth in these categories given that the age and race of killers and victims are positively correlated. However this correlation in victim and offender demographics is something less than perfect (see for example Cook and Laub, 1998). If school desegregation affects the frequency with which youth perpetrate lethal violence but does not affect the age distribution of their victims, our analysis of VS data will simply be less precisely estimated than what would result from analysis of data on offenders.

In the next version of the paper we will also examine data from the FBI's Supplemental Homicide Reports (SHR), which provides some information about homicide offenders and victims. The SHR suffer from many of the same limitations of the general UCR data, which includes incomplete reporting by local police departments, and offender information is only available in cases where an arrest is made. In any case we plan to use the SHR data to examine effects of school desegregation on homicide offending rates for black and white youth, as well as how desegregation impacts patterns of homicide offending across race lines. These data in principle could also enable us to examine how homicide rates change during the school year versus the summer months when school is not in session.

IV. METHODS

Our basic empirical approach is to examine how homicide victimization rates to white or black youth change in response to court school desegregation orders. Our main outcome variable of interest y_{it} is some measure of homicides in county i in year *t*. Our key explanatory variables are a set of indicators $D_{p,it}$ equal to one if in calendar year *t*, district i had a desegregation plan implemented *p* years beforehand, and equal to 0 otherwise. In most models we use the year before desegregation plans are implemented as our reference group and define indicators for each of the five years before and after the desegregation orders go into effect, as well as indicators for more than six years beforehand and more than seven years afterwards. We also condition on a set of county and year fixed effects, γ_i and δ_t. Our baseline estimating equation is then given by (1)

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

The coefficients of interest, the β_p vector, are identified under the assumption that, in the absence of the desegregation plans, homicide rates would have trended similarly in districts which had desegregation plans implemented at different times. The vector of pre-desegregation plan coefficients provides a partial test of this assumption. If the timing of desegregation is uncorrelated with trends in the homicide rate, there should be no evidence of a trend in the β_p coefficients in the years prior to desegregation. The β_p coefficients in the years after desegregation measure the impact of desegregation on crime rates. The specification allows for effects of desegregation

on crime that are either immediate or gradually unfold over time. This strategy of comparing trends across jurisdictions before and after desegregation orders is arguably the best way available to assess the causal impact of desegregation (Guryan, 2004).

It is important that the entire β_p vector be identified from the same set of counties. Otherwise changes in the magnitudes of the coefficients for different times from desegregation implementation may reflect sample composition issues, not the response to desegregation. We therefore restrict our sample to counties which contribute to each of the first six points in the post desegregation vector and at least 4 of the last five years in the pre desegregation vector[13]. This restriction removes approximately 8 percent of the county-year observations from the sample. Estimates produced using the full sample are similar to those produced using the restricted sample.

In our main set of estimates we treat the individual counties as the observational unit for our "natural experiment" and estimate equation (1) without weighting by county population, to estimate the effect of school desegregation on the average county. However we also show that the results are qualitatively similar when we estimate the effects on the average juvenile instead by estimating equation (1) using county population as weights.

[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. We therefore require that each county contribute to the identification of 4 of the last 5 pre vector coefficients, instead of contributing to all 5.

We initially estimate equation (1) using OLS and calculate standard errors that are clustered at the county level to account for arbitrary forms of serial correlation (Bertrand et al., 2004). It is not certain, however, that this is the correct functional form. We see substantial differences across counties in the cross section in homicide rates, especially for black youth,[14] which at first glance would suggest that a log linear specification that estimates proportional effects from school desegregation orders may be preferable to a linear model that assumes constant absolute effects. However it is also the case that many counties record no homicides to youth in some years, especially for black youth. The log linear specification is problematic because observations equal to zero are undefined when the log transformation is taken.

In order to estimate a proportional response model using OLS, we employ the method proposed by Pakes and Griliches (1980). The homicide rate is transformed by replacing any zero values with ones. The log of this transformed variable is used as the dependent variable. A dummy variable, equal to one for all instances in which the true homicide rate equals zero, is included as an explanatory variable. While the method allows for estimation of a proportional response using a linear model, it is biased because the dummy variable is endogenous. For ease of exposition, this model will be referred to as the log linear dummy variable specification.

We also estimate a quasi-maximum likelihood (QML) count model. Wooldridge (1999) demonstrates that the QML model has good consistency properties

[14] For example in 1980 the homicide rates across counties in the Welch and Light sample for white youth 15-19 at the 25th, 50th and 75th percentiles of the distribution equaled 4.5, 8.5 and 14.6. For black youth the figures at these percentiles equal 0, 23.4, and 43.2.

relative to alternative count models. The absolute count of homicides to youth is used as the dependent variable and we control for county population as the "exposure."[15] The interpretation of the model's coefficients is the same as for a log linear specification using the homicide rate as the dependent variable. 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.

As an additional check to ensure that our estimates are not mistakenly attributing pre-existing county- or region-specific trends in youth homicides to school desegregation orders, we examine the sensitivity of our estimates to models that re-estimate (1) including region-year fixed effects and county-specific linear trends. As displayed on Figure 1, there is a clear regional pattern to the timing of desegregation. A majority of southern districts desegregated between the mid-1960s and the mid-1970s, while a majority of northern districts desegregated between the mid-1970s and the mid-1980s[16]. This regional pattern suggests that inclusion of region-year effects in the model may be of particular importance.

We also estimate a model which controls for trends in crime associated with county demographic characteristics measured at the start of the sample period

[15] Specifically, county population is entered into the model as an explanatory variable with a coefficient constrained to equal one.

[16] The regional correlation with the timing of desegregation is a product of the evolution of legal doctrine. Prior to 1973, court-ordered desegregation could only occur in school districts proved to have engaged in *de jure* segregation. The 1973 *Keyes* decision ruled that court-ordered desegregation could proceed in areas which had not practiced *du jure* segregation. As a result, desegregation became more viable in school districts outside of the south in which *de facto* segregation was present.

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

where X_i is the vector of time-invariant county characteristics measured as of the 1960 Census[17] and λ_t is the vector of time varying coefficients on these characteristics. This model will be referred to as the base demographic model because it conditions on demographic characteristics measured in the `base' period of the sample. The model controls, in an extremely flexible manner, for trends in crime associated with the characteristics. For instance, median family income is included in the X_i vector. The model controls for the possibility that low income communities may have increasing rates of crime over time. Time-variant demographic variables are not included in the model because they are likely endogenous to desegregation[18].

Finally, when we investigate the mechanisms behind our homicide results we often have outcome variables only for the years 1970 and 1980. For instance, this is the case when we explore the effect of desegregation on white private school attendance. In these cases, we use the difference-in-difference estimation strategy employed by Guryan (2004)

(3) $$y_{it} = \alpha + \theta Deseg70_i * 1980_t + 1980_t + \gamma_i + \varepsilon_{it}$$

where $Deseg70_i$ is an indicator equal to one if county i had a school district desegregate in the 1970s and 1980_t is an indicator equaling one if the year equals 1980.

[17] Ideally, these characteristics would be measured as of the first year of the sample, 1959. The lack of data measured as of 1959 dictates the use of the 1960 census data contained in the County and City Databook.

[18] We have experimented with including a time-varying measure of non-school desegregation race riots (such as the 1965 Watts Riot in Los Angeles). Inclusion of these variables has no effect on the results presented below.

School districts that desegregated in the 1970s form the treatment group, while districts that desegregated in the 1960s and 1980s form the control group. When we estimate using micro census data, the equation becomes

(4) $$y_{jit} = \alpha + \theta Deseg70_i * 1980_t + 1980_t + \gamma_i + \varepsilon_{jit}$$

where y_{jit} is the outcome variable for individual j, residing in county group i at time t.

V. RESULTS

Table 1 provides some general background on our analytic sample of counties in the Welch and Light dataset. As noted above these are unusually large counties, with a mean population of around 691,000 over our entire study period, of whom around 16 percent are African-Americans. Homicide victimization rates to white youth 15-19 in these counties increase dramatically from 1960 to 1980, from 2.6 to 9.8 per 100,000, while homicide victimization rates to black youth 15-19 start off much higher (15.8 per 100,000), more than double from 1960 to 1970, and then decline during the 1970s. This convergence in black and white youth homicides starting in 1970 continues at least through the mid-1980s (Cook and Laub, 1998, p. 44).

In what follows, we first present selected results for the effects of court-ordered school desegregation orders on homicide victimizations to black youth. These estimates are somewhat imprecisely estimated given the relatively small number of black homicides per county overall, but are generally consistent with fairly large proportional reductions in homicide as a result of school desegregation. We then show

that school desegregation also reduces homicide victimization rates to white youth, which are more precisely estimated than what we observe for blacks given the greater absolute numbers of homicide events to whites. We conclude by showing that increased private-school and alternative public school enrollments, police spending and racial integration seem to be part of the explanation for why school desegregation orders reduce homicides.

A. Black Homicide Victimizations

Figure 2 presents the coefficients for years before and after school desegregation orders go into effect from estimating equation (1) for homicide victimization rates to blacks 15-19. The reference point (year 0) is the year *before* these desegregation orders go into effect. Homicide victimization rates decline after school desegregation orders are put into place, but at the same time we also see some signs of pre-existing trends and more generally all of the coefficients are very imprecisely estimated (the dashed lines display the 95% confidence intervals). Estimation with the log-linear dummy variable model (Panel B) or the QML count model (Panel C) yields a qualitatively similar pattern of results.

Figure 2 tries to improve the precision of our estimates by expanding the age range of victims that we consider. Cook and Laub (1998) examine SHR data from the period 1985-95 and find that around three-quarters of killers age 13-17 were younger than their victims, and around two-thirds were three or more years younger than their victims. To look for evidence of changes in homicide *offending* by black school-age

youth, in Figure 2 we expand our focus to include black homicide victims between the ages of 15 and 24.

Panel A of Figure 2 shows that homicide victimization rates to blacks 15-24 are now relatively stable in the years prior to when local courts ordered public schools to desegregate, but seem to exhibit a break in trend thereafter. Five or six years after desegregation implementation, black youth homicide has declined by about 1 per 100,000 (around 2 percent of the overall mean homicide victimization rate to blacks 15-24 in our sample period, of 48.8; see Table 1). The final coefficient measures the effect of desegregation seven *or more* years after the start of desegregation. The value of this coefficient suggests that the effect of desegregation on crime is persistent.[19] Looking across the panels in Figure 3 we see the results are qualitatively similar when we condition on region-year fixed effects (panel B), region-year fixed effects and the base-year demographic characteristics[20] (panel C) or control for county-specific linear trends (panel D).

Figure 4 displays the models which estimate a proportional response. While there is some evidence of a downward trend in the pre-desegregation period in the standard QML count model (panel A), this trend disappears when either region-year effects are controlled for (panel B) or region-year and base characteristic-year effects

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

[20] The base year demographic characteristics, allowed to influence crime in a time-variant manner (see equation (2)) are: median household income, percent of population over the age of 25 with a high school degree, the percent of employment in manufacturing and the percent of the population which is non-white.

are controlled for (panel C). The implied impacts are much larger than what we estimate using OLS and assuming a constant absolute impact. Four or more years after implementation, school desegregation orders reduce black homicide rates by around 20 percent. The log linear dummy variable specification (panel D) produces similar results. The fact that the count model and log linear dummy variable model yield similar results is reassuring because a large share of our county-year observations has zero values for the homicide rate. The similarity of results from the log linear dummy specification with the count model, which is well suited to handling counties with no homicides, suggests our estimates are not sensitive to this issue.

The results in Figure 4 suggest that the effect of desegregation on crime increases over the first several years of desegregation. Desegregation plans were often phased in over two or more years (Welch and Light 1987). The gradual implementation of desegregation in many counties may have produced a gradual effect on criminal behavior. In addition, the effects of desegregation may accumulate over time. For instance, peer effects may operate in a cumulative fashion. One year of exposure to a given peer group may have little impact on the propensity for criminal behavior, but several years of exposure may have a significant effect.

The models discussed above are unweighted. Appendix Figure A1 displays results for models weighted by the black juvenile county population. The results are similar to those discussed above. Appendix Figure A2 presents results which use the full sample – i.e. the 8% of districts which do not meet the requirement of contributing

a sufficient number of points to the pre and post desegregation vectors are not dropped from the sample. Again, the results are similar to those above.

B. White Homicide Victimizations

Figure 5 shows that that school desegregation orders seem to reduce homicide victimization to whites as well as blacks. For example Panel A shows the results of estimating equation (1) using homicide victimization rates per 100,000 to white youth ages 15-19. We see almost no signs of any pre-existing trend in white youth homicides before implementation of court desegregation orders, and then a very clear downward trend thereafter. After 6 or 7 years white youth homicide rates have declined by around 3 or 4 per 100,000, a very large change given that the average rate over our entire study period is around 6 per 100,000 (Table 1). The other panels in Figure 5 show that these results are not sensitive to conditioning on region-year fixed effects (Panel B), region-year effects and the base demographic characteristics (Panel C), or county-specific linear trends.

The overall pattern of results is similar when we estimate the count model (displayed in Panel A of Figure 6). The proportionate impact is very large, equal to an approximately 35 percent decrease in white youth homicide seven years after the start of desegregation. The estimates are quite similar when region-year effects are included (Panel B), when region-year and base demographic characteristics are included (Panel C) and when the log linear dummy variable model is used (Panel D). The results for whites are also usually qualitatively similar when we focus on homicide victimizations

to whites ages 15-24, although somewhat less precisely estimated (results available upon request). Appendix Figure A3 presents results which are weighted by the white juvenile county population and Figure A4 presents results which use the full sample. Both sets of results are quite similar to those presented above.

C. Specification Tests

Are these results really due to school desegregation orders or to other factors that are changing coincident to these court orders? The fact that we do not see systematic differences between desegregating and other counties in the immediate years before these court orders go into effect provides some partial reassurance against a counter-explanation for our results that rests on some omitted variables story. We are also encouraged that our findings are not very sensitive to conditioning on either region-year effects, base demographic characteristics or county-specific linear trends. Another way to address this issue is to examine whether we see impacts of school desegregation orders on youth outcomes that should logically not be affected.

In Figures 7 and 8 we present such a falsification exercise. These are estimates of the "effect" of school desegregation orders on mortality rates to black and white youth from illnesses[21], which should not be affected by the same school quality, peer influence or other mechanisms hypothesized to drive desegregation impacts on

[21] Specifically we look at the effect of desegregation on mortality from the following eight illnesses: septicemia, neoplasms (cancer), respiratory (bronchitis, pneumonia, influenza, asthma, etc), circulatory (heart disease, hypertension, etc), anemias, meningitis, ill defined (ill defined conditions) and congenital (congenital abnormalities)

academic and non-academic behavioral outcomes. The mortality rate from illness in our sample for those aged 15 to 19 is 15.8 per 100,000, compared to a rate of 10.8 for homicides. Neither the OLS estimates in levels (Panel A of Figures 7 and 8), nor the QML count model estimates (Panel B, Figures 7 and 8) provide evidence that desegregation reduced the number of deaths from illness.

As discussed in section III, measurement error in the county population variable could in principle lead to systematic biases in our estimates. Specifically, if the imputed census population figures for non-Census years misses population loss due to "white flight", our estimates will be downwardly biased. In order to address this concern, Table 2, panel A, presents white homicide results from the OLS and QML count models with the sample restricted to decennial census years. Population data for these years are measured accurately. To account for the greatly reduced number of data points, we estimate two variants of equation (1). The first restricts the $D_{p,it}$ vector to a single zero-one indicator variable for being desegregated. The second restricts the $D_{p,it}$ vector to two indicator variables – one for being in the first 5 years of desegregation and another for being in the 6th or greater year of desegregation. Estimates from the second model are quite similar to those produced using the whole sample and suggest measurement error in the population variable is not responsible for the results presented above. Panel B presents estimates for blacks. The magnitude of the effect of desegregation on crime is quite similar to that produced using the entire sample, but the estimates are imprecise.

Another explanation for the results is that instead of inducing a lower crime propensity through mechanisms such as peer effects, desegregation induced migration across county boundaries that produced a county population with a lower propensity for crime. Such migration would likely be out-migration for whites, i.e. "white flight". For blacks, desegregation might spark in-migration, as black families seek to obtain the improved educational environment produced by desegregation plans. In order to explore this possibility, we estimate equation (1) with the log of the county population of 15 to 19 year olds. A version of the model is also estimated which includes a south census region-year effect and the vector of base demographic characteristics with time varying coefficients, similar to equation (2). The sample is again restricted to the decennial census years of 1960, 1970 and 1980 to avoid issues with measurement error. These estimates are presented on Table 3.

There is no evidence that desegregation induced migration across county boundaries for either whites or blacks. The point estimates are small and very imprecise. To facilitate comparison with estimates presented on subsequent tables, estimates from equation (3), the two period difference-in-difference specification, are also presented (see column (4)). These estimates are also small and imprecise.

Table 4 further explores the possibility of migration across county boundaries by examining how desegregation affects the demographic characteristics of 15, 16 and 17 year-olds residing in desegregated county groups. The estimates are produced by estimating the equation (4) on micro census data from 1970 and 1980. The characteristics examined are family income, an indicator for the mother having

attended college and an indicator for the father having attended college. As on Table 3, there is little evidence of migration. The estimates for blacks are all small and imprecise. The same is true for the white estimates, with one exception. There is some indication that desegregation lowered the family income of whites. The estimate which controls for both south region-year effects, as well as base county group and desegregated public school characteristics (implemented as in equation (2)), indicates that the family income of white teenagers decreased by approximately $750. The estimate, however, is only marginally significant at the 10% level and is small, equal to about 2 ½ percent of mean income in 1970. There is no indication that desegregation impacted the education level of whites.

D. Mechanisms

If desegregation does not spark migration across county boundaries, why do we see a reduction in homicide victimization rates to whites in our data? For that matter, what drives the decline in homicide victimizations to blacks? We explore a number of possible mechanisms through which desegregation may affect the homicide rate of both black and whites including public school racial integration, private school attendance, alternative, non-desegregated public school attendance and police spending.

Racial Integration of Public Schools

The sorting of students within a district is measured using the dissimilarity index, defined as

$$D_t = \frac{1}{2} * \sum_{i=1}^{n} \left| \frac{b_{it}}{B_t} - \frac{w_{it}}{W_t} \right|, \quad (6)$$

where b_{it} and w_{it} refer to the number of black and white students, respectively, at school *i* at time *t* and B_t and W_t refer to the total number of black and white students, respectively, in the school district. The dissimilarity index ranges from 0 to 1, with 1 denoting complete segregation. It is interpretable as the percent of black students who would need to be reassigned to a different school for perfect integration to be achieved given the districts overall racial composition. An increase in segregation is reflected by an increase in the dissimilarity index.

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

$$E_t = \frac{1}{B_t} \sum_{i=1}^{n} b_{it} * \frac{w_{it}}{t_{it}}, \quad (7)$$

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

In order to assess the extent to which desegregation increased racial integration in the desegregated school district, we estimate equation (1), including region-year effects, with the dissimilarity and exposure index as the outcome variables. These estimates, presented on Figure 9, are very similar to those estimated in Reber (2004). Desegregation produces a sharp and persistent increase in racial integration, as measured by both indices. The increase in racial integration may have induced a reduction in homicide rates by changing the peer group students interacted with in the desegregated school district.

Private School Attendance and Alternative Public School Attendance

In order to explore the impact of desegregation on where white children attend school, we estimate equation (3) with the ratio of the number of students enrolled at the desegregated public school to the ratio of school age children in the county as the dependent variable. These estimates are presented on Table 5. Desegregation decreased the ratio by approximately .03 for whites, indicating that whites left the desegregated public school, but continued to reside within the same county. The estimated decrease is equal to 7% of the ratios sample mean in 1970. There is no evidence that the black ratio was impacted by desegregation.

Table 3 examined the effect of desegregation on the denominator of the ratio, county population, and found no impact. It is therefore likely that the change in the white ratio is accounted for by numerator, the enrollment at the desegregated public school. Panel B, which uses enrollment as the dependent variable, confirms this.

Desegregation significantly reduces white enrollment in public school districts (see Reber, 2005, for more detailed estimates and discussion of "white flight" from desegregated public school districts).

Where are these white children fleeing to if not to suburban counties? The answer must be either alternative public school districts located in the same county, or else to private schools. In order to assess the extent of flight to private school, Table 4 uses micro data from the decennial censuses for 1970 and 1980. We estimate equation (4) as a linear probability model, where y_{jit} is an indicator variable equaling one if youth *j*, in county group *i,* at time *t* is attending a private school. These estimates are presented on Table 6.

The results suggest that desegregation increases the propensity of whites to attend private school by around .02 percentage points – an increase of approximately 15 percent relative to the mean 1970 propensity. The result is significant at only the 10% level and the result is not robust to inclusion of the south region-year and base characteristics controls.

Table 7 provides firmer evidence that desegregation changed private school attendance patterns by examining the demographic characteristics of private school students (again using the 1970 and 1980 census data). The models which condition on south region – year and base characteristic – year interaction terms suggest that the demographic profile of white private school students changes in response to desegregation. After desegregation, the population has lower income and lower parental education.

Private schools may provide a school environment less likely to promote criminal behavior than public schools. Under this assumption, an increased rate of private school attendance is a potential mechanism through which desegregation reduces white homicides. Furthermore, individuals from lower income households have a higher propensity to engage in criminal behavior than those from more wealthy households and this relationship is particularly strong with regards to serious crimes (Bjerk 2006). The shift in private school attendance pattern appears to have been concentrated in the portion of the income distribution most likely to engage in serious crimes such as homicide, increasing the likelihood that private school attendance is one of the mechanisms which decreases white homicide.

Law Enforcement Expenditures

Table 8 provides evidence that local policymakers increase spending on police in response to the implementation of a court-order to desegregate the local public schools. These results are produced by estimating equation (1) using an unbalanced panel including the following years: 1971-75, 1979, and 1988. The dependent variable is either police employment per capita, or total police payroll per capita. There is no evidence that desegregation affects police employment. There is, however, evidence that police pay per capita increases substantially and that this increase is persistent, lasting beyond the first five years of desegregation. Relative to the mean per-capita police pay in the sample, the estimates suggest that pay increased by 7 to 16 percent as the result of desegregation. The fact that we observe a larger increase in police payroll

than in the number of police personnel could be due to increased use of officer overtime in desegregating jurisdictions. Alternative explanations include the possibility that local officials increase "hazard pay" to their officers in response to a belief that the policing environment has become more dangerous, or the possibility that police departments shift officers from desk to street duty and need to compensate them for the shift.

Connection between Mechanisms and Homicides

Finally, Table 9 provides evidence on the association between the mechanisms identified above and homicide rates. This is done by estimating homicide models with the data restricted to the decennial census years of 1960, 1970 and 1980 (like those presented on Table 2). The $D_{p,it}$ vector is restricted to two indicators, for the first five years of desegregation and six or more years. These indicators are then interacted with the change in the mediating mechanism variables occurring after desegregation. These changes are measured as the change from one year prior to desegregation to the fourth year of desegregation and hence are five-year changes. For example, in order to ascertain the association between homicides and changes in racial integration, the five-year change in the exposure index is interacted with the two points of the $D_{p,it}$ vector. While these estimates cannot be interpreted as identifying the causal link between the

mediating mechanism and homicides, they do provide suggestive evidence about the hypothesized links[22].

There is a robust relationship between homicide reduction and racial integration for blacks (panels A and B). Focusing on the estimates for six or more years of desegregation produced by the QML count model, both the standard model and the model conditioning on south region-year and base characteristic-year interaction terms indicates that the more racial integration a district achieved, the more the black homicide rate fell. This relationship holds for integration as measured by both the dissimilarity index and the exposure index. The relationship is much weaker for whites, with only the standard model using the exposure index providing evidence of a link between racial integration and homicides.

The contrasting results by race suggest that different mechanisms may have produced the decline in homicides for whites and blacks. Blacks may have experienced the decline due to factors directly associated with desegregation such as peer effects, increased school quality and incapacitation associated with lengthy bus rides in districts which used busing to achieve integration. Whites may have experienced the decline primarily due to secondary mechanisms such as increases in police spending and the shift to private schools and alternative public districts. Panel C of Table 9 explores the association between homicides and the exit of whites from the desegregated public school to private and alternative public schools by interacting the

[22] The changes in the various mechanism occurring after desegregation may be correlated with other changes occurring at the same time. For example, a district which was particularly successful at racial integration as measured by the change in the exposure index may also have experienced other, unobserved, changes such as increases in school funding or teacher quality.

change in the ratio of white enrollment at the desegregated district to the white school age population in the county with the indicators for desegregation. The interaction terms are general imprecise and provide little evidence of a relationship between exit from the desegregated district and homicides.

VI. CONCLUSIONS

Our estimates suggest that court-ordered school desegregation reduces homicide victimization rates by around 20 percent for black youth and up to 35 percent for white youth. These findings come from our QLM count model, which is arguably the preferred specification given the particular features of our data: substantial differences across counties in homicide levels in the cross section, suggesting we might prefer to estimate desegregation impacts in proportional rather than absolute terms, but with a substantial number of zero counts for white and especially black homicides.

The main potential challenge to our findings is the possibility that courts may implement school desegregation orders in response to trends in black or white youth outcomes. But our preferred count model specifications show relatively little evidence of pre-existing trends in homicides to black youth, particularly after we condition on region-year fixed effects (Figure 4), and absolutely no evidence of pre-existing trends for white youth (Figure 6). In addition we find no detectable impacts of court-ordered school desegregation orders on youth mortality rates from causes that should not be affected by desegregation, namely, illnesses.

The primary limitation of the current draft's estimates is that we rely on Vital Statistics data that identifies only the race and age of the homicide victim, not the offender, while we are primarily interested in the effects of desegregation on the violent behavior of white and black youth. The next draft of the paper will seek to address this problem by drawing on data from the FBI's Supplemental Homicide Reports, which provide information on homicide offenders at least in those cases where an arrest is made by law enforcement officials.

Our findings are important because in principle court-ordered school desegregation could have led to deleterious impacts on youth anti-social or criminal behavior, as anyone who followed Boston's experiences with public school desegregation in the 1970s might reasonably have conjectured. But our results suggest that instead, school desegregation at least on average generates beneficial and quite sizable improvements in youth crime.

One implication is that previous studies that focus on academic outcomes, or in some cases adult earnings, will understate the social welfare gains from school desegregation. Our preferred count model estimates imply something on the order of 10 fewer homicides per 100,000 blacks and 2 fewer homicides per 100,000 whites.[23] Cohen and colleagues (2004) estimate that the social costs per homicide equal around $9.7 million in current dollars. Our estimates thus imply social benefits of nearly $1,000 per black student and nearly $200 per white student. As one benchmark for

[23] Figure 4 suggests desegregation orders reduce homicide victimization rates to blacks 15-24 by around 20 percent, while Table 1 shows a mean homicide victimization rate to this group over our entire sample period of around 48.8. Figure 6 suggests desegregation orders reduce homicide rates by around 35 percent to whites 15-19 and Table 1 shows an average homicide victimization rate of 6.2.

assessing the magnitude of these benefits, Hanushek's figures (2003, Table 1) suggest average per-pupil public school spending over our study period in the U.S. overall was probably something on the order of $4,500 (in constant 2000 dollars).

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

The findings reported here could also potentially have implications for understanding black and white homicide trends more generally. Youth account for a disproportionately large share of homicide offenders (15 percent of all homicide arrests in 1997 were made to people under 18 years of age; see *Sourcebook of Criminal Justice Statistics, 1998*, p. 338). School desegregation could also have larger impacts

on aggregate homicide trends if people who attend school after court-ordered desegregation experience changes in their lifetime offending behavior, a possibility that is not readily captured in the present draft's focus on victims 15-19 or 15-24 but one that will be addressed in the next iteration of this paper.

In any case it is intriguing to note that overall black homicide arrest rates to people of all ages increased continuously and dramatically from 1953 to 1968, from around 5 to 35 per 100,000, but then abruptly stopped this upward trend in the late 1960s (Jaynes and Williams, 1989, pp. 458-9) – just as many of our desegregation orders begin to go into effect. This decline can be seen on Figure 10 which displays historical homicide rates and homicide arrest rates by race.

Our results also provide a candidate explanation for why homicide rates declined so dramatically in the U.S. for most of the 1990s but then this progress halted at the end of the decade, despite the fact that there were few changes in the factors hypothesized to drive the decline in crime – increased imprisonment and police spending, ebbing of the crack cocaine epidemic, and abortion legalization (Levitt, 2004).[24] Our findings suggest one countervailing force that may have occurred over this period is the growing number of large public school districts that had their court desegregation orders dismissed over the decade (Clotfelter, Ladd and Vigdor, 2005, Lutz, 2005).

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

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Figure 1
Desegregation Implementation Dates

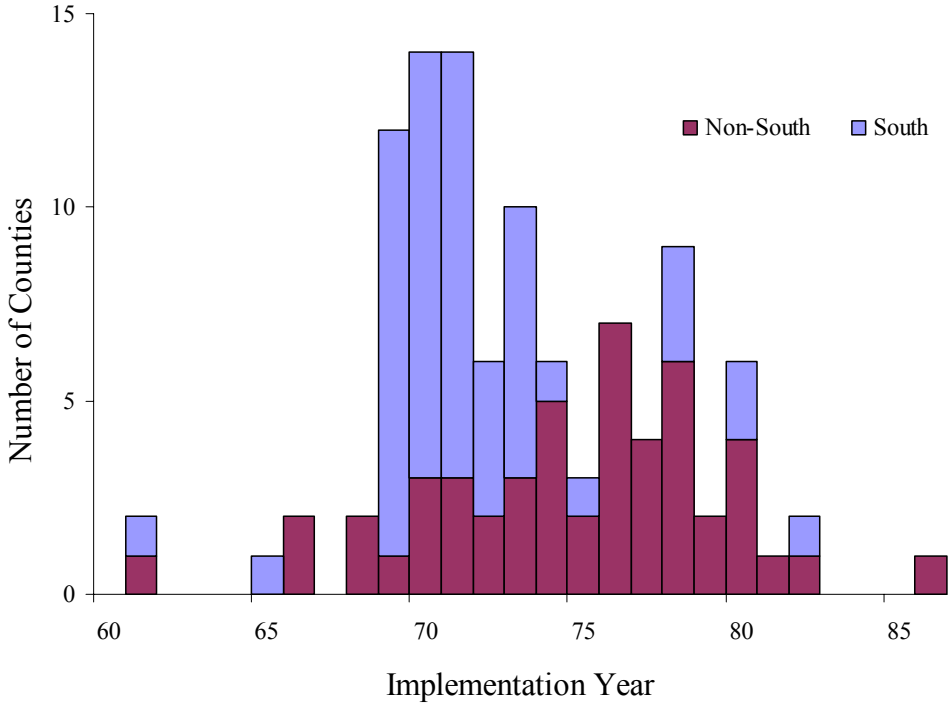
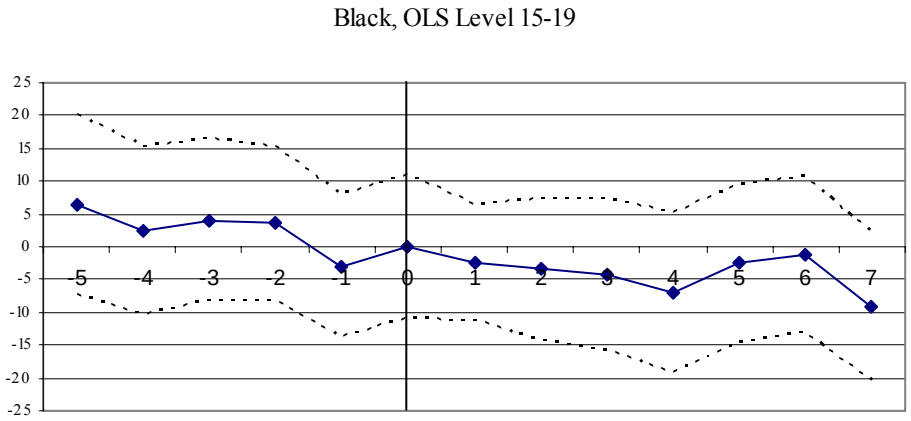


Figure 2: School Desegregation & Homicide Victimizations, Black Youth, 15-19
Panel A:



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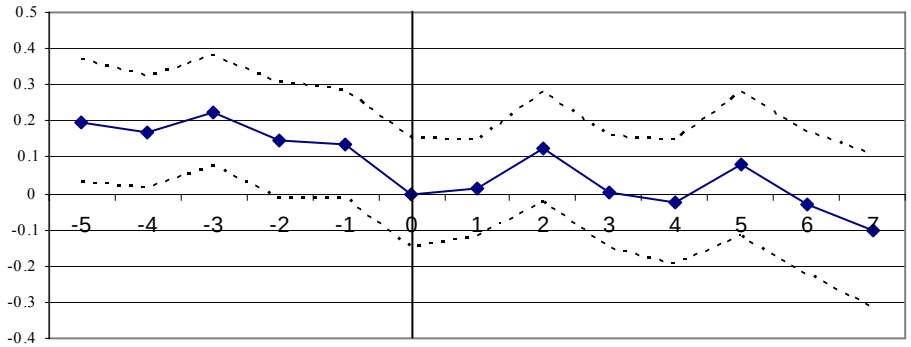


Panel C:

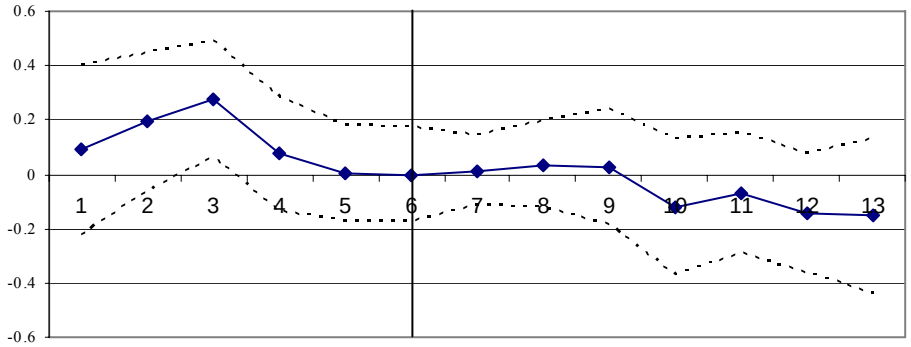
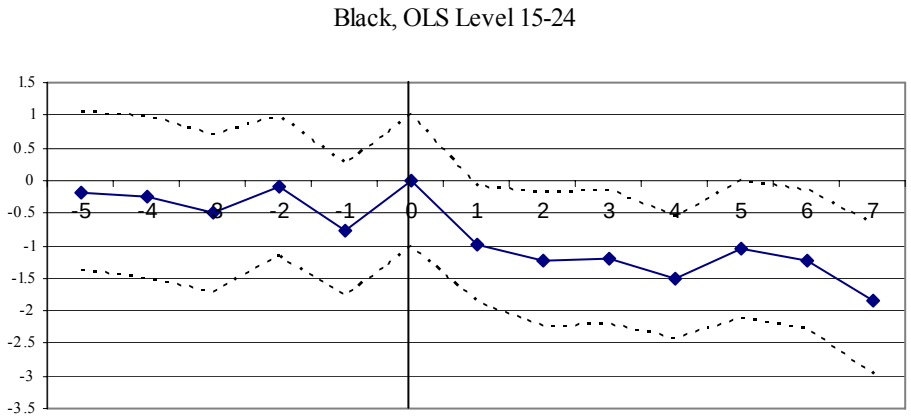


Figure 3: School Desegregation & Homicide Victimizations, Black Youth, 15-24
Panel A:



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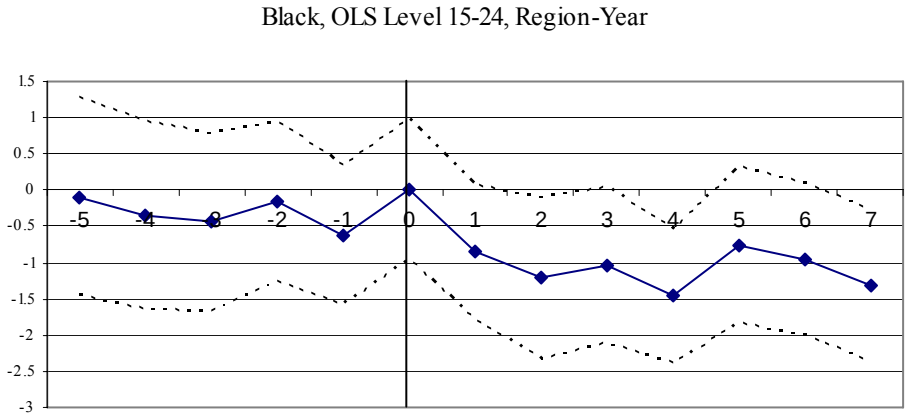

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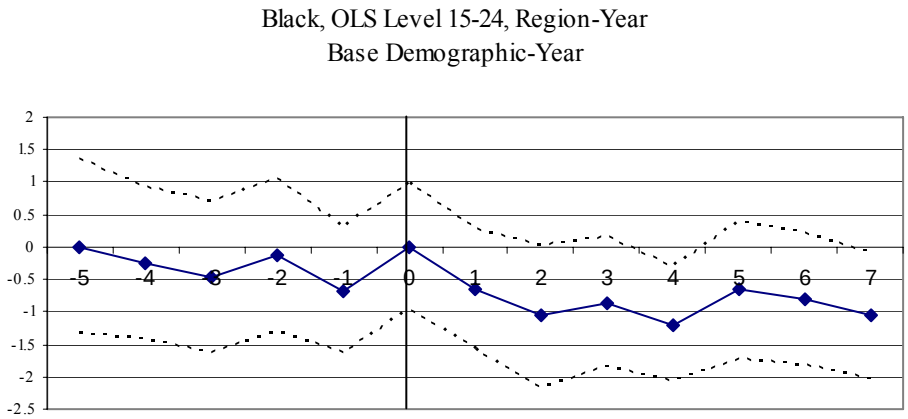


Figure 3 (continued): School Desegregation & Homicide Victimizations, Black Youth, 15-24
Panel D:

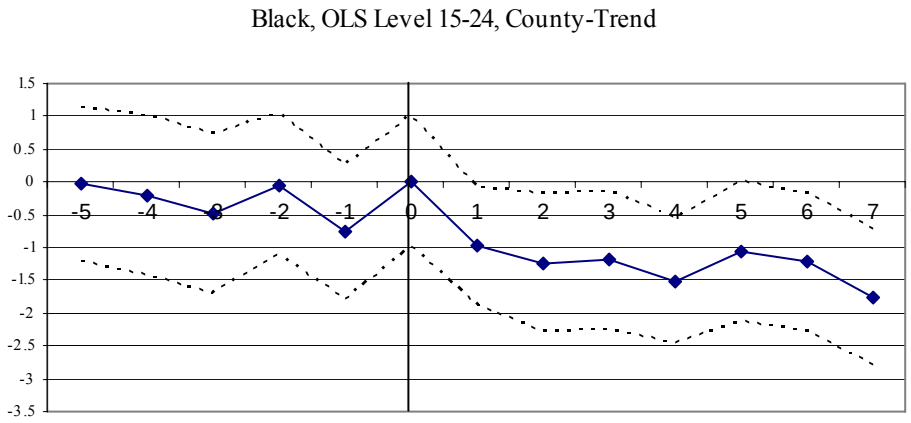
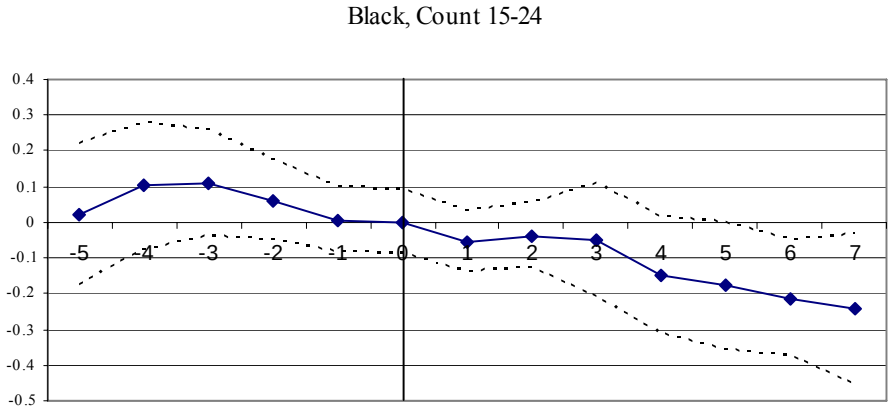


Figure 4: School Desegregation & Homicide Victimizations, Black Youth 15-24, Proportional Response Models

Panel A:



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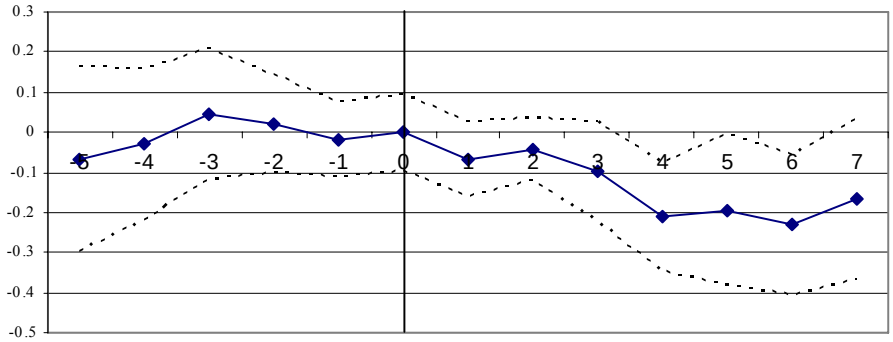


Panel C:

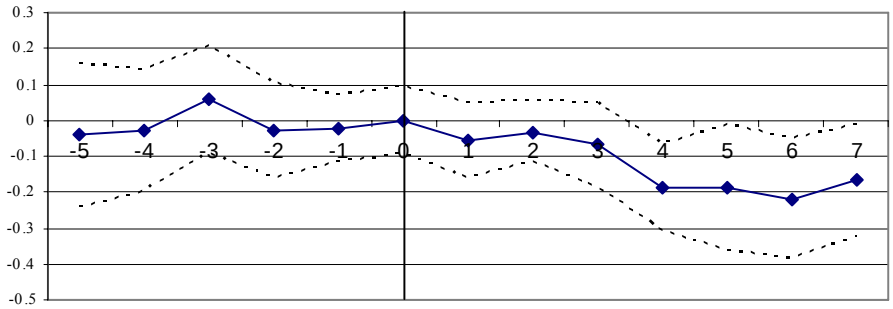


Figure 4 (continued): School Desegregation & Homicide Victimizations, Black Youth 15-24, Proportional Response Models
Panel D:

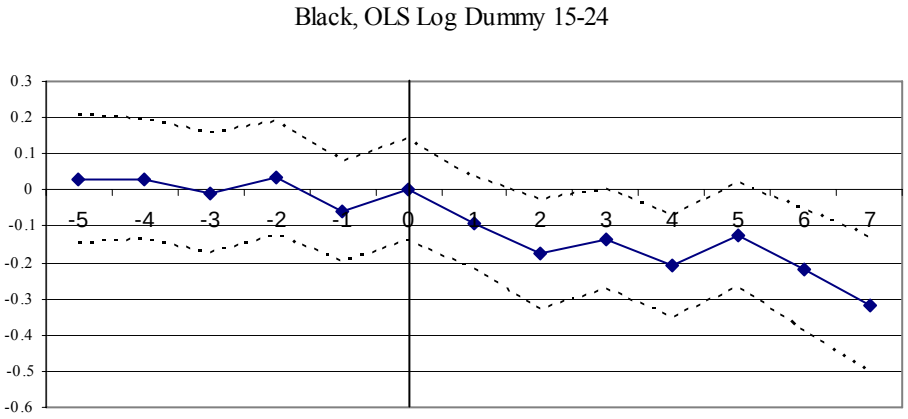
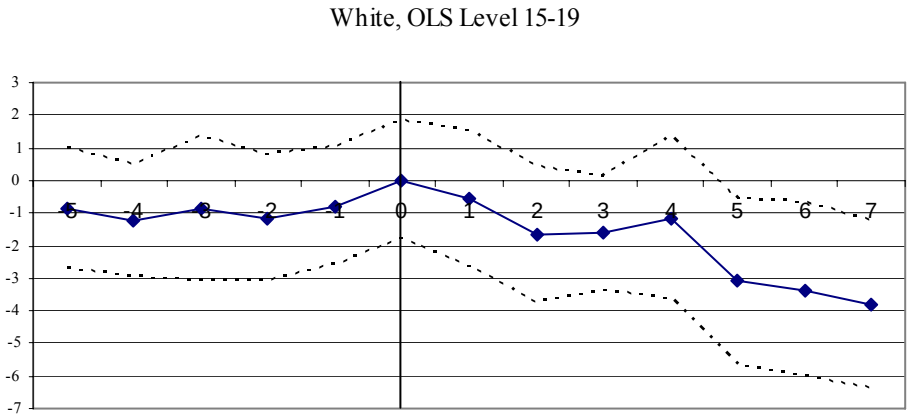
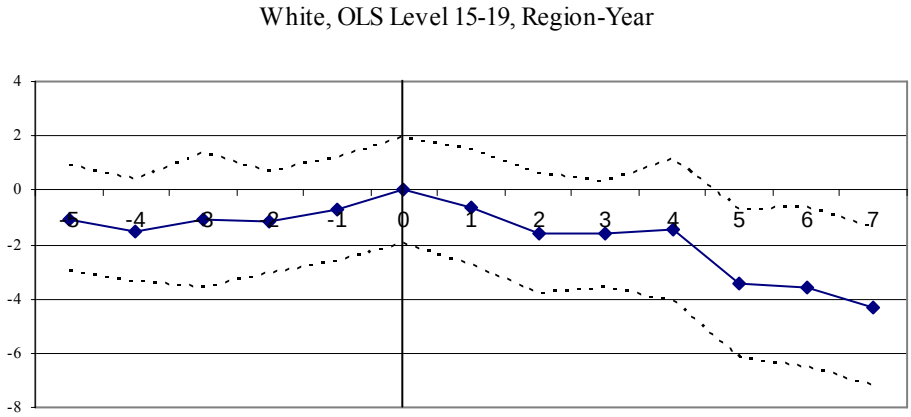


Figure 5: Homicide Victimization, White Youth 15-19
Panel A:



Panel B:



Panel C:

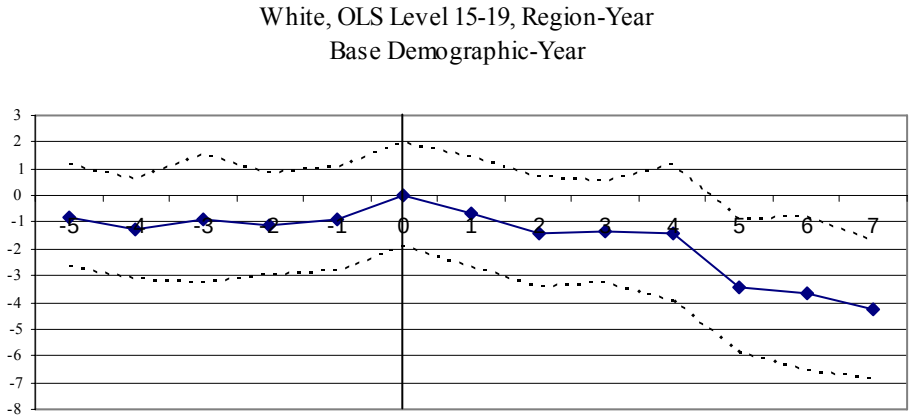


Figure 5 (continued): Homicide Victimization, White Youth 15-19
Panel D:

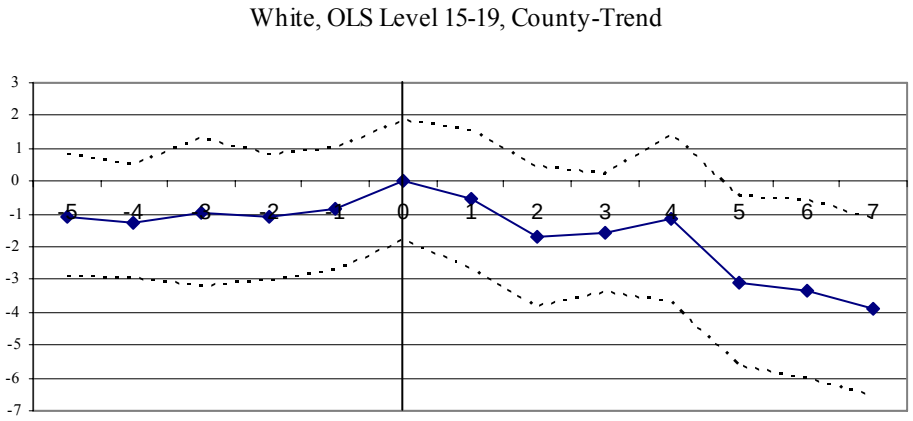
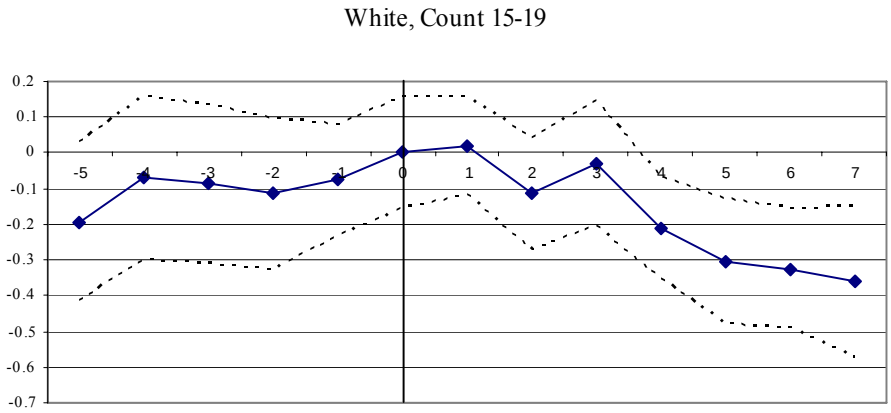
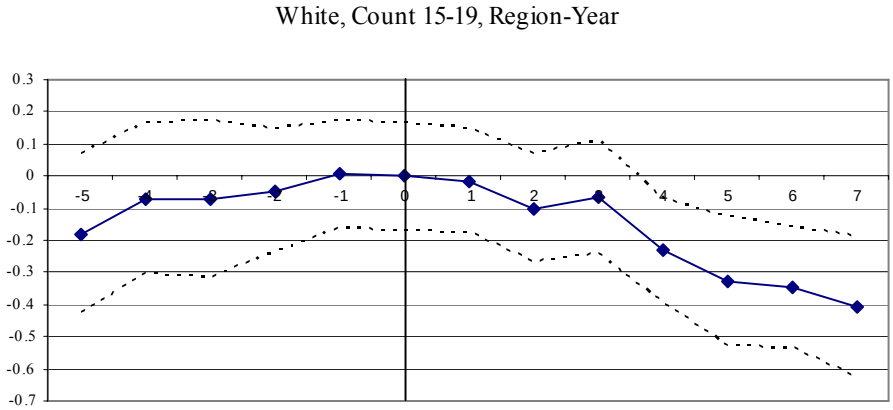


Figure 6: School Desegregation & Homicide Victimizations, White Youth 15-19, Proportional Response Models

Panel A:



Panel B:



Panel C:

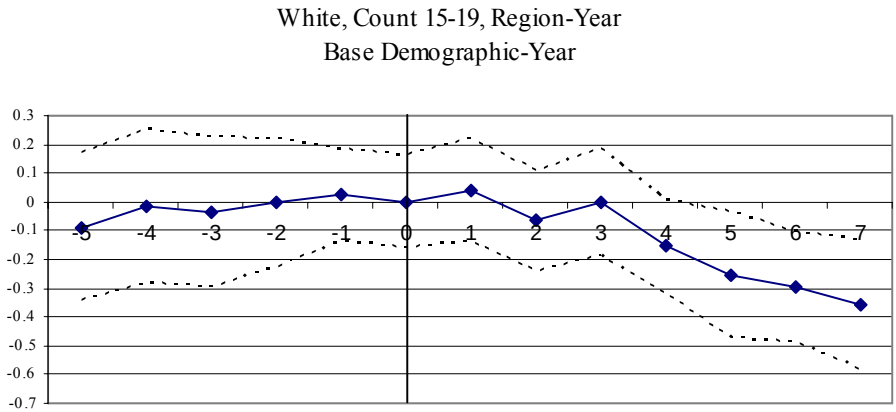


Figure 6 (continued): School Desegregation & Homicide Victimizations, White Youth 15-19, Proportional Response Models
Panel D:

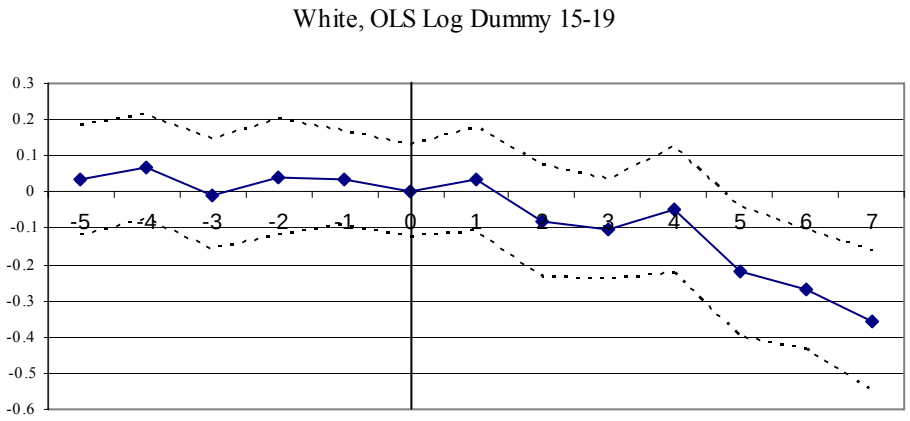
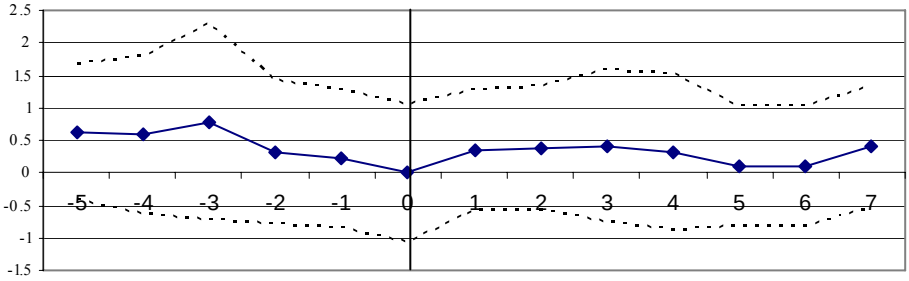


Figure 7: Falsification Check – Desegregation Effects on Mortality from Illness, Black Youth 15-24

Panel A:

Black, OLS Level 15-24
(Falsification Check: Top Ten Causes of Death, excluding Homicide, Suicide and Accidents)



Panel B:

Black, Count 15-24
(Falsification Check: Top Ten Causes of Death, excluding Homicide, Suicide and Accidents)

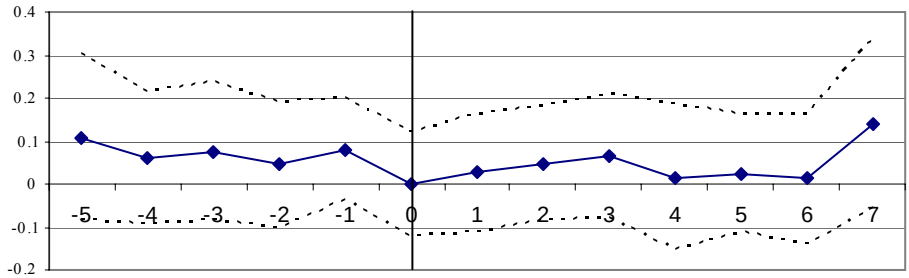
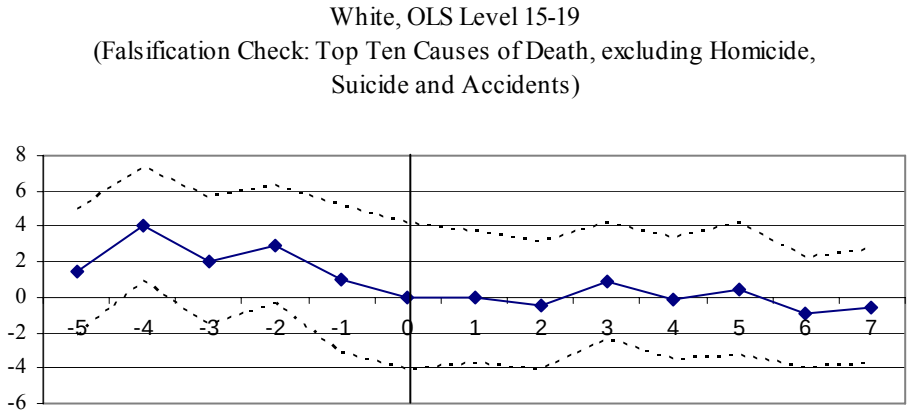


Figure 8: Falsification Check – Desegregation Effects on Mortality from Illness, White Youth 15-19

Panel A:



Panel B:

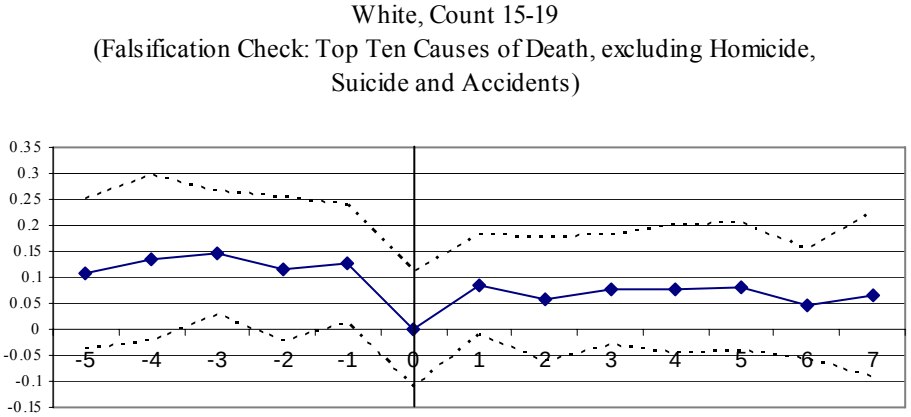
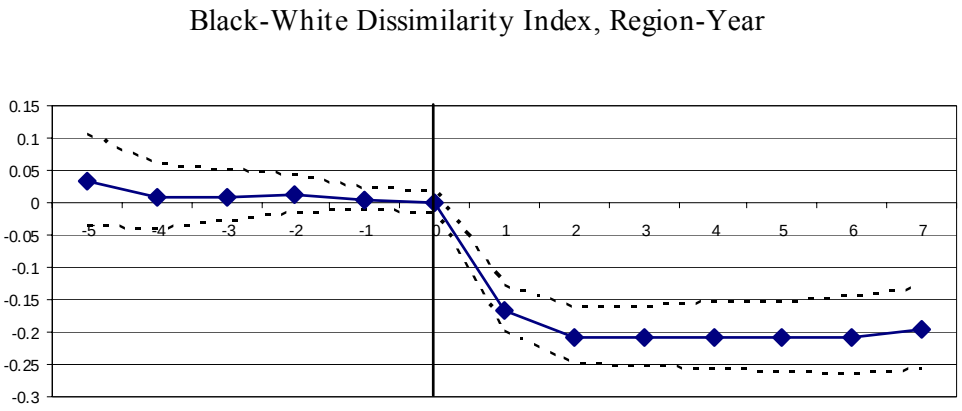


Figure 9: Segregation Indices
Panel A:



Panel B:

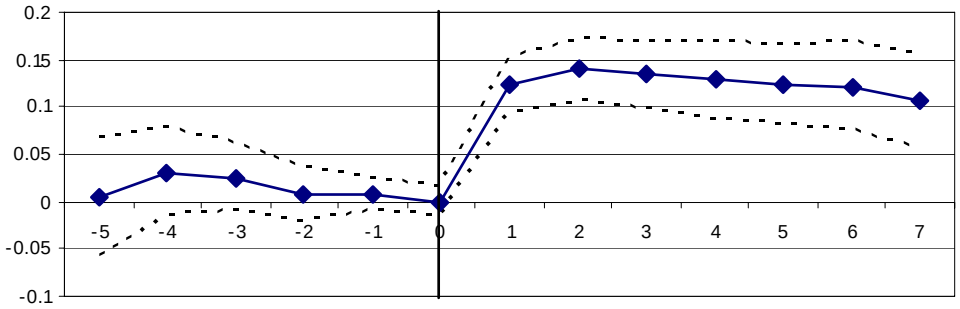
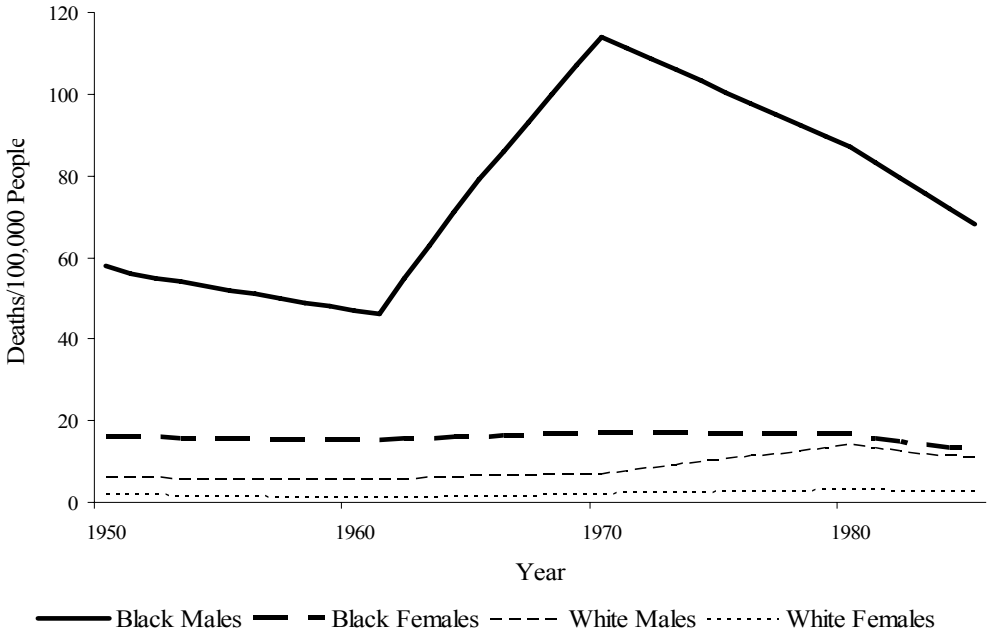


Figure 10
Historical Homicide Rates
Panel A: Homicide rates for people aged 15-24



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

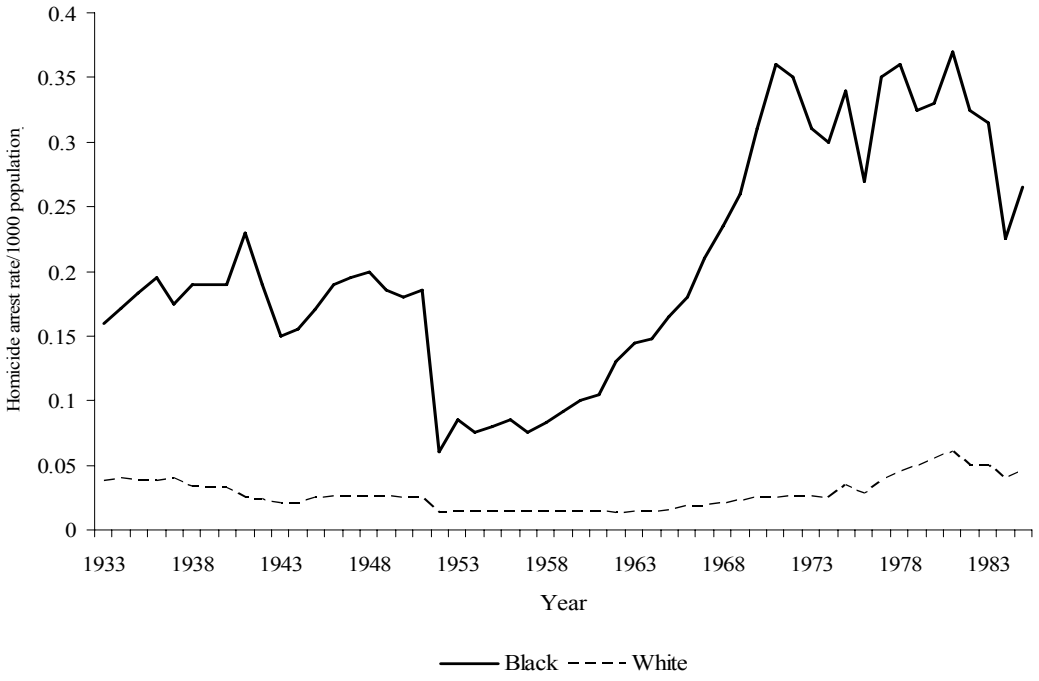


Table 1
Descriptive Statistics

	Full Sample	1960	1970	1980
	A. County Population Means			
Total	691485	583743	668187	712624
Total white	560712	501490	554817	567351
Total black	114860	82252	104675	126010
White 15-19	44576	34182	49107	49118
Black 15-19	11031	5607	10594	13687
White 15-24	91834	64878	95922	104411
Black 15-24	21129	11105	19064	26538
	B. Homicide rates per 100,000			
Total	11.0	6.0	11.1	14.2
Total white	6.4	3.2	6.1	9.3
Total black	38.2	25.8	44.5	42.4
White 15-19	6.2	2.6	5.0	9.8
Black 15-19	32.3	15.8	36.6	27.1
White 15-24	8.1	3.6	6.0	12.8
Black 15-24	48.8	29.5	57.4	49.0

Note. The cells display county means.

Table 2
Homicide Victimization, Sample Restricted to Decennial Census

	OLS Level		Count	
	(1)	(2)	(3)	(4)
		A. White		
Post Desegregation	0.441		0.127	
	(1.368)		(0.234)	
Post Desegregation Years 1 - 5		0.351		0.077
		(1.315)		(0.158)
Post Desegregation Years 6+		-3.679**		-.457**
		(1.864)		(0.202)
Number of observations	299	299	291	291
		B. Black		
Post Desegregation	-0.825		0.018	
	(0.753)		(0.133)	
Post Desegregation Years 1 - 5		-0.847		0.052
		(0.761)		(0.162)
Post Desegregation Years 6+		-1.81		-0.172
		(1.137)		(0.229)
Number of observations	299	299	288	288

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

Table 3
Effect of Desegregation Plan on County Population

	(1)	(2)	(3)	(4)
	A. Log of White Children in County			
Post Desegregation	-0.009	0.0004		
	(0.024)	(0.022)		
Post Desegregation Years 1 - 5			-0.009	
			(0.025)	
Post Desegregation Years +6			0.072	
			(0.061)	
70s Desegregator *1980				-0.035
				(0.052)
	B. Log of Black Children in County			
Post Desegregation	-0.013	0.033		
	(0.029)	(0.030)		
Post Desegregation Years 1 - 5			-0.013	
			(0.030)	
Post Desegregation Years +6			0.033	
			(0.064)	
70s Desegregator *1980				0.030
				(0.042)
Number of Observations	303	303	303	202
South Region *Year Effect		X		
1960 County characteristics *Year Effect		X		
Sample Restricted to 1970 and 1980				X

Note. Standard errors clustered by county in parentheses. The dependent variable for each of the panels is given in the panel title. The unit of observation is county-year. In columns (1) - (3), the estimation sample includes the years 1960, 1970, and 1980. In columns (4) and (5) the sample is restricted to 1970 and 1980.

Table 4
Effect of Desegregation Plan on Demographic Characteristics of Desegregated County Groups

	Family Income		Mother Attended College		Father Attended College	
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Whites			
70s Desegregator *1980	-197	-746*	-0.008	-0.005	-0.007	-0.007
	(643)	(404)	(0.007)	(0.007)	(0.001)	(0.008)
Number of Observations	193,028	193,028	195,113	195,113	195,113	195,113
			A. Blacks			
70s Desegregator *1980	410	-227	-0.005	-0.006	0.010	0.016
	(753)	(514)	(0.009)	(0.011)	(0.009)	(0.010)
Number of Observations	49,226	49,226	49,963	49,963	49,963	49,963
1970 School and County Group Characteristics *1980		X		X		X
South Region * 1980		X		X		X

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

Table 5
Effect of Desegregation Plan on Percent of Children Attending the Desegregated School District

	White		Black	
	(1)	(2)	(4)	(5)
	A. Ratio of Enrollment at Desegregated School to Children in the Country			
70s Desegregator *1980	-0.032**	-0.028**	-0.014	-0.017
	(0.011)	(0.010)	(0.016)	(0.012)
	B. Log of Enrollment at Desegregated School			
70s Desegregator *1980	-0.122*	-0.124**	0.043	0.024
	(0.086)	(0.051)	(0.039)	(0.034)
Number of Observations	202	202	202	202
South Region *Year Effect		X		X
1970 School characteristics *Year Effect		X		X
1960 County characteristics *Year Effect		X		X

Note. Standard errors clustered by county in parentheses. The dependent variable for each of the panels is given in the panel title. The unit of observation is county-year. The estimation sample includes the years 1970 and1980. The school characteristics, interacted with year effects in column (3), include total enrollment, percent of enrolled students who are black, black-white dissimilarity index and the black-white exposure index . The 1960 county characteristics are median income, percent of population which is non-white, percent of population age 25 or greater with a high school degree and the percent of employment in manufacturing.

Table 6
Effect of Desegregation Plan on Private School Attendance

	(1)	(2)
	A. White	
70s Desegregator *1980	0.018*	0.006
	(0.011)	(0.011)
Number of Observations	177,783	177,783
	B. Black	
70s Desegregator *1980	-0.003	0.011
	(0.006)	(0.007)
Number of Observations	45,252	45,252
Time-varying Demographic Characteristics		
1970 School and County Group Characteristics *1980		X
South Region * 1980		X

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

Table 7
Effect of Desegregation Plan on Demographic Characteristics of White Private School Students

	Family Income		Mother Attended College		Father Attended College	
	(1)	(2)	(3)	(4)	(5)	(6)
70s Desegregator *1980	-747	1647**	-0.020	-.026**	-0.192	-0.035**
	(1152)	(704)	(0.016)	(0.012)	(0.019)	(0.017)
Number of Observations	38,800	38,800	39,880	39,880	39,880	39,880
1970 School and County Group		X		X		X
South Region * 1980		X		X		X

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

Table 8
Effect of Desegregation Plan on Police Resources

	(1)	(2)	(3)	(4)
	A. Ratio of Police Employment to Population			
Post Desegregation	-0.030	0.041		
	(0.042)	(0.044)		
Post Desegregation Years 1 - 5			-0.030	0.040
			(0.043)	(0.045)
Post Desegregation Years +6			-0.033	0.032
			(0.058)	(0.063)
	B. Ratio of Police Pay to Population			
Post Desegregation	0.420**	0.273**		
	(0.130)	(0.115)		
Post Desegregation Years 1 - 5			0.426**	0.279**
			(0.133)	(0.117)
Post Desegregation Years +6			0.476**	0.360**
			(0.170)	(0.160)
Number of Observations	702	702	702	702
South Region * Year Effect		X		X
1960 County characteristics * Year		X		X

Note. Standard errors clustered by county in parentheses. The dependent variable for each of the two panels is given in the panel title. The unit of observation is county-year. The sample includes the following years for which data is avaliable: 1971, 1972, 1973, 1974, 1975, 1979 and 1988. The ratio of police employment to population has been multiplied by 1000.

Table 9
Homicide Victimization, Interactions, Sample Restricted to Decennial Census

	Black				White			
	OLS Level		Count		OLS Level		Count	
	(1)	(2)	(3)	(4)	5	6	(7)	(8)
				A. Exposure Index				
Post Deseg. Years 1 - 5	-1.34	-1.37	0.21	0.18	0.54	-0.57	0.082	0.19
	(0.91)	(0.85)	(0.20)	(0.17)	(1.77)	(1.73)	(0.18)	(0.18)
Post Deseg. Years 6+	-2.03*	-1.19	-0.09	-0.02	-3.14*	-4.11**	-0.39**	-0.42*
	(1.09)	(1.03)	(0.15)	(0.13)	(1.83)	(1.89)	(0.20)	(0.20)
Post Deseg. Years 1 - 5 *	1.8	3.51	-1.57**	-0.95	-1.96	4	-0.59	-0.72
Δ Exposure Index	(2.08)	(2.46)	(0.77)	(0.75)	(6.06)	(6.48)	(0.78)	(0.94)
Post Deseg. Years 6+ *	-0.56	1.16	-0.82**	-0.72*	-4.27	-2.91	-1.09**	-0.81
Δ Exposure Index	(1.54)	(1.63)	(0.34)	(0.42)	(3.67)	(5.42)	(0.43)	(0.51)
Number of observations	303	303	288	288	303	303	291	291
				B. Dissimilarity Index				
Post Deseg. Years 1 - 5	-1.01	-0.95	0.16	0.18	0.8	0.71	0.15	0.22
	(0.77)	(0.72)	(0.17)	(0.15)	(1.22)	(1.14)	(0.18)	(0.16)
Post Deseg. Years 6+	-1.28	-0.64	-0.02	0.50**	-3.72**	-3.63*	-0.39*	-0.39*
	(1.17)	(1.08)	(0.18)	(0.13)	(1.90)	(1.88)	(0.22)	(0.20)
Post Deseg. Years 1 - 5 *	-0.97	-2.16	0.54	0.4	1.99	0.53	0.42	0.42
Δ Dissimilarity Index	(1.18)	(1.41)	(0.36)	(0.39)	(2.97)	(3.27)	(0.57)	(0.59)
Post Deseg. Years 6+ *	2.3*	0.72	0.73**	0.70**	-0.37	0.78	0.29	0.31
Δ Dissimilarity Index	(1.40)	(1.57)	(0.30)	(0.32)	(1.81)	(2.65)	(0.30)	(0.31)
South Region *Year Effect		X		X		X		X
1970 County Char. * Year Effect		X		X		X		X
Number of observations	303	303	288	288	303	303	291	291

Table 9 (cont.)
Homicide Victimization, Interactions, Sample Restricted to Decennial Census

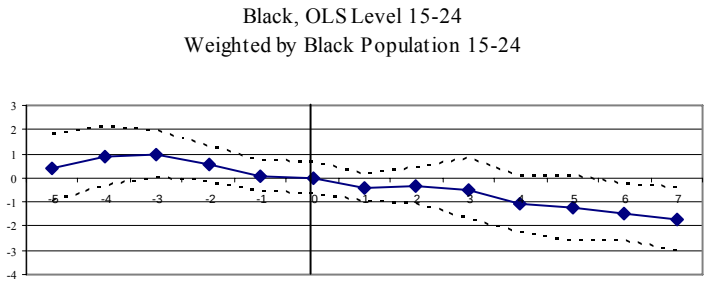
	Black				White			
	OLS Level		Count		OLS Level		Count	
	(1)	(2)	(3)	(4)	5	6	(7)	(8)
	C. Ratio of Whites in Desegreagted School to Whites in County							
Post Deseg. Years 1 - 5	-0.92	-0.67	0.03	0.06	0.28	0.52	0.23	0.11
	(0.76)	(0.74)	(0.15)	(0.13)	(1.29)	(1.24)	(0.17)	(0.16)
Post Deseg. Years 6+	-1.75	-0.68	-0.16	-0.07	-4.28**	-4.02**	-0.57**	-0.53**
	(1.20)	(1.08)	(0.18)	(0.14)	(1.86)	(1.86)	(0.22)	(0.22)
Post Deseg. Years 1 - 5 *	-5.13	-7.08*	-0.79	-1.15	4.56	5.38	-0.61	-0.79
Δ Ratio School-County	(3.51)	(3.80)	(1.25)	(1.12)	(7.03)	(7.10)	(0.88)	(0.83)
Post Deseg. Years 6+ *	0.54	0.03	0.13	-0.02	-4.35	-1.35	-0.78*	-0.55
Δ Ratio School-County	(1.60)	(1.73)	(0.41)	(0.41)	(3.27)	(4.49)	(0.42)	(0.36)
South Region *Year Effect		X		X		X		X
1970 County Char. * Year Effect		X		X		X		X
Number of observations	303	303	288	288	303	303	291	291

Note. Standard errors clustered by county in parentheses. The unit of observation is county-year. The sample is restricted to 1960, 1970 and 1980. The dependent variable for each panel is given in the panel heading. Δ refers to the change in the variable from one year prior to the implementation of desegregation to the fourth year after desegregation implementation.

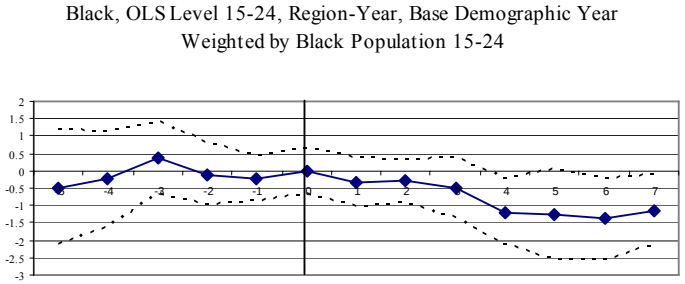
APPENDIX

Figure A1: Black Results Weighted by Black Population

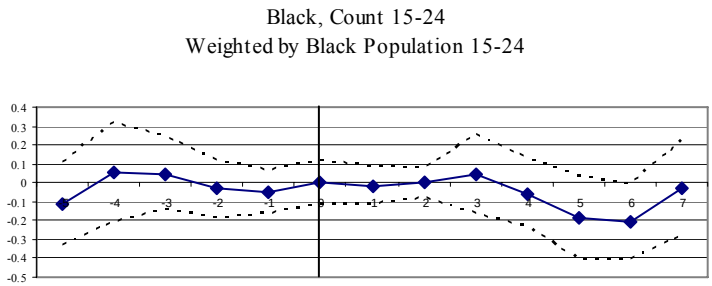
Panel A:



Panel B:



Panel C:



Panel D:

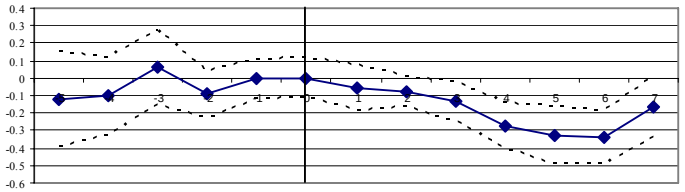
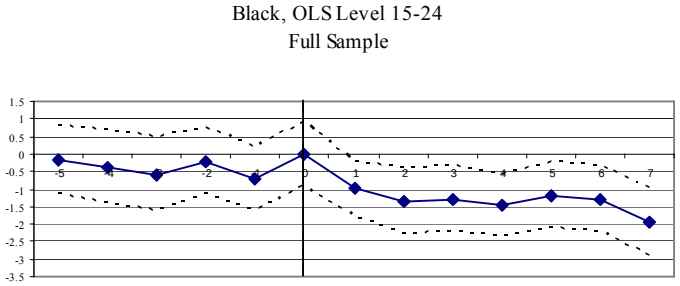
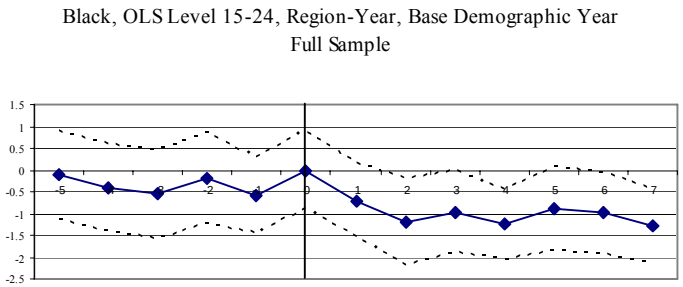


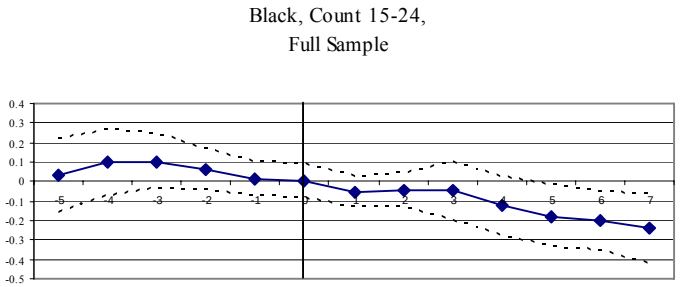
Figure A2: Black Results Using Full Sample
Panel A:



Panel B:



Panel C:



Panel D:

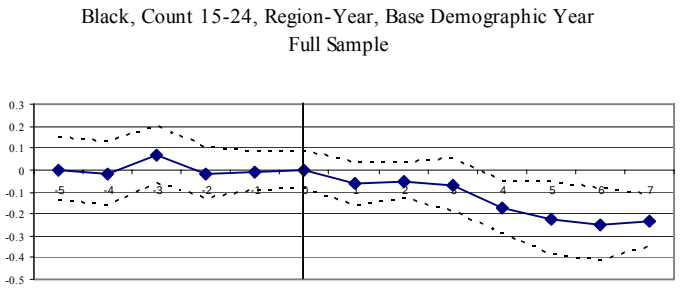
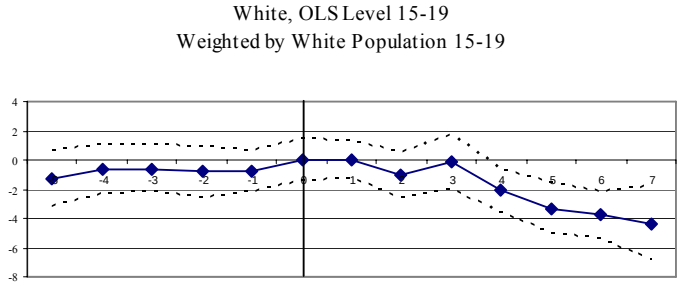
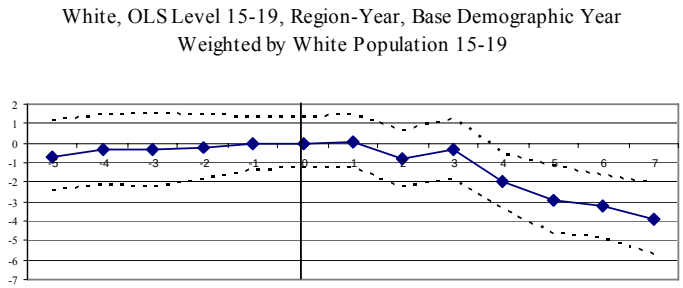


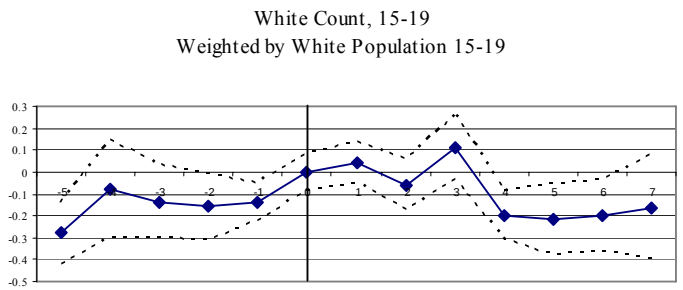
Figure A3: White Results Weighted by White Population
Panel A:



Panel B:



Panel C:



Panel D:

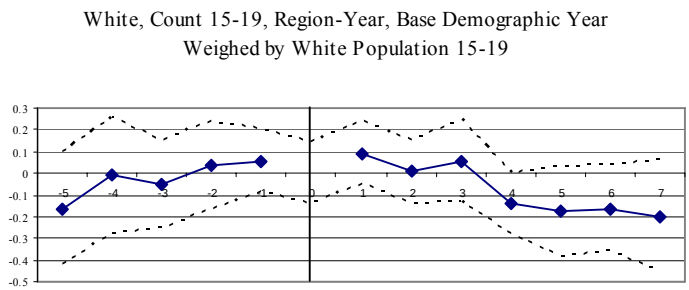
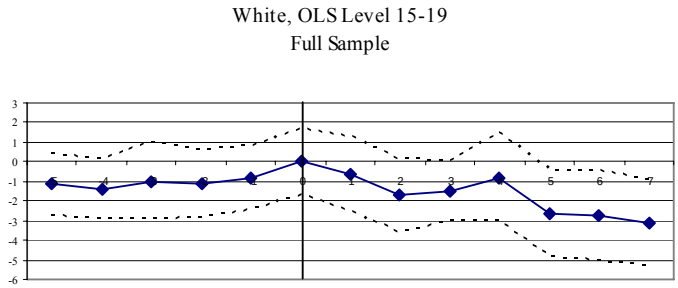
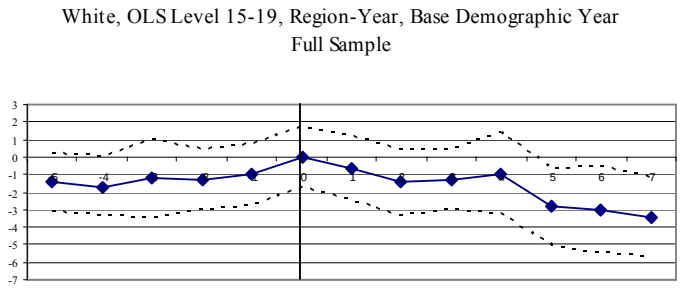


Figure A4: White Results Using Full Sample

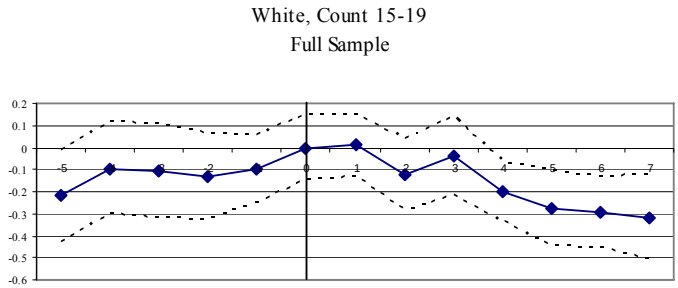
Panel A:



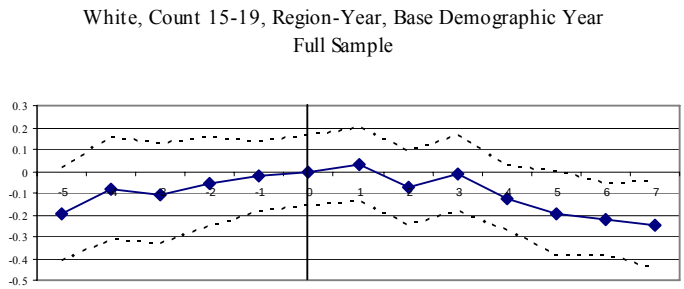
Panel B:



Panel C:



Panel D:



Appendix Table 1
Counties and School Districts in Sample and Year of Desegregation

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

E. Baton Rouge	East Baton Rouge Parish	LA	1970
Jefferson	Jefferson Parish	LA	1971
Orleans	New Orleans Parish	LA	1961
Rapides	Rapides Parish (Alexandria)	LA	1969
Terrebonne	Terrebonne Parish	LA	1969
Bristol	New Bedford	MA	1976
Hampden	Springfield	MA	1974
Suffolk	Boston	MA	1974
Baltimore City	Baltimore	MD	1974
Harford	Harford County	MD	1965
Prince George's	Prince Georges County	MD	1973
Ingham	Lansing	MI	1972
Kent	Grand Rapids	MI	1968
Wayne	Detroit	MI	1975
Hennepin	Minneapolis	MN	1974
Jackson	Kansas City	MO	1977
St. Louis City	St. Louis	MO	1980
Cumberland	Fayetteville/Cumberland County	NC	1969
Gaston	Gaston County (Gastonia)	NC	1970
Mecklenburg	Mecklenburg County (Charlotte)	NC	1970
New Hanover	New Hanover County (Wilmington)	NC	1969
Douglas	Omaha	NE	1976
Essex	Newark	NJ	1961
Hudson	Jersey City	NJ	1976
Clark	Clark County (Las Vegas)	NV	1972
Erie	Buffalo	NY	1976
Monroe	Rochester	NY	1970
Cuyahoga	Cleveland	OH	1979
Franklin	Columbus	OH	1979
Hamilton	Cincinnati	OH	1973
Lucas	Toledo	OH	1980
Montgomery	Dayton	OH	1976
Summit	Akron	OH	1977
Comanche	Lawton	OK	1973
Oklahoma	Oklahoma City	OK	1972
Tulsa	Tulsa	OK	1971
Multnomah	Portland	OR	1974
Allegheny	Pittsburgh	PA	1980
Philadelphia	Philadelphia	PA	1978
Charleston	Charleston	SC	1970
Greenville	Greenville County	SC	1970
Richland	Richland County	SC	1970
Davidson	Nashville	TN	1971
Shelby	Memphis	TN	1973
Bexar	San Antonio	TX	1969
Dallas	Dallas	TX	1971
Ector	Odessa	TX	1982
El Paso	El Paso	TX	1978
Harris	Houston	TX	1971

Lubbock	Lubbock	TX	1978
McLennan	Waco	TX	1973
Potter	Amarillo	TX	1972
Tarrant	Fort Worth	TX	1973
Travis	Austin	TX	1980
King	Seattle	WA	1978
Pierce	Tacoma	WA	1968
Milwaukee	Milwaukee	WI	1976
Raleigh	Raleigh County (Beckley)	WV	1973