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THE EFFECTS OF SCHOOL DESEGREGATION ON TEENAGE FERTILITY

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Abstract

The school desegregation efforts following the historic Supreme Court ruling in Brown v. Board of Education (1954) represent one of the most important social policy initiatives of the 20th century. Despite a large research literature on desegregation and educational outcomes, its effects on the lives of individuals are still not fully understood. In this paper we examine the effects of desegregation on the fertility of teenagers. Our findings suggest that desegregation increased the fertility of African American teens and is unrelated to the fertility of white teens.

JEL No. I24, J13, J15, J18

Key Words: Desegregation, Teenage Fertility

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I. Introduction

School desegregation efforts following the historic Supreme Court ruling in *Brown v. Board of Education* (347 US 483, 1954) represent one of the most important social policy initiatives of the 20th century. Changes in the distribution of students across schools were sweeping, particularly, but not exclusively in the South, and the profound effects of these efforts have been widely studied. Despite a large literature on desegregation, the effects of school desegregation efforts on individual students are not fully understood. In this paper we examine the effects of desegregation on the fertility of teenagers.

Teen fertility is an important component of the broader social and economic inequalities among racial and ethnic groups. Fertility rates are much higher among minority teens, and particularly African-Americans, than among white teens. As of 2010, teen fertility rates were more than twice as high among black teens as white teens. Such disparities are of public concern because teenage fertility has been associated with adverse outcomes such as premature birth, neonatal mortality, and high dropout rates among both teen fathers and mothers (Chen et al. 2007; Fergusson & Woodward 1999; Fletcher & Wolfe 2009, 2012; Fraser, Brockert, & Ward 1995), although there is considerable debate over the causal impact of teen fertility on these outcomes (Geronimus & Korenman 1992; Hoffman 1998; Hotz, McElroy, & Sanders 2005).

There are at least three reasons to believe that school desegregation efforts might have affected teen fertility rates. First, ending the isolation of black students in poorly resourced schools might have influenced perceptions of self-worth, social and economic opportunities, and attitudes about American society among African-American adolescents (Clark 1950; Weiner, Lutz, and Ludwig 2009). Second, in at least some areas, school desegregation efforts led to increased funding and improved resources for schools attended by African-American students (Clotfelter 2004; Reber 2011), and evidence suggests that school desegregation reduced high school dropout rates among African-Americans (Guryan, 2004; Reber 2010). Together these factors can influence both the real and perceived opportunity costs of teen parenthood and the development of the skills that help teens avoid unintended pregnancies. A number of scholars find that greater educational and employment opportunities are associated with decreased birth rates among black females, particularly teens (Colon, Geronimus, and Phipps 2006; Duncan and Hoffman 1990; Wolfe, Wilson, and Haveman 2001); evidence from the STAR, class-size reduction experiment indicates that increases in school resources may help reduce teen pregnancy rates (Schazenbach 2007); and Kirby (2002) provides evidence that increased school engagement and reduced dropout rates are associated with decreased teen pregnancy rates.

Third, it is well-documented that desegregation resulted in greater interracial contact, and more generally, changed the set of peers to which students are exposed in schools (Clotfelter 2004; Guryan 2004; Reber 2005). Contagion models of peer influence suggest that the prevalence of a behavior in the social settings can influence individual choices and behavior. Although existing evidence on the effect of peers and social norms on adolescent sexual activity and birth rates is inconclusive, some studies find that higher rates of sexual activity and teen pregnancy in one's high school or neighborhood can influence the sexual activity and fertility rates of individual teens (Crane, 1991; Fletcher, 2007).

The direction of the effects that we would expect the changes in peer environments, racial isolation, and school resources associated with desegregation to have on teen fertility rates is ambiguous. Most obviously the effects of desegregation on the peer environments and school resources of black students were different from the effects on the environments of white students, and thus, we might expect different effects of desegregation on the fertility rates of black and white teens.

Almost certainly, desegregation moved some black students into schools with lower prevalence of teen births, higher achievement levels, and greater resources. We would expect that if desegregation had any effect on these students, it would have been to reduce teen fertility rates. However, it is also possible that desegregation resulted in increased social and economic isolation among poor, black students, at least in some schools. If only some of the black students in a district were placed in racially integrated schools as a result of desegregation efforts, while others remained in racially isolated schools, and if those black students placed in racially integrated schools tended to be at relatively low risk for experiencing teenage motherhood, then desegregation could have increased the prevalence of teenage fertility in the schools of those blacks left behind in racially isolated schools. Furthermore, those high risk black adolescents left-behind in racially isolated schools might have developed more negative attitudes about their future opportunities and American society. In this case, desegregation might be expected to have increased teen fertility among some groups of African-Americans, and expectations about the net effect of desegregation on fertility rates among black teens would be ambiguous. This line of reasoning echoes arguments made by William Julius Wilson over two decades ago (Wilson 1987), that race-specific policies emanating from the civil rights movement may have served to increase the isolation of the most disadvantaged African-Americans from more advantaged African-Americans, thereby decreasing their economic opportunities and increasing their exposure to concentrations of dysfunctional behaviors.

We are aware of only one study that examines the effects of school desegregation on teenage fertility. Using data from U.S. Census Integrated Public Use Microdata Series (IPUMS) sample, Liu *et al*. (2012) compares changes in fertility rates of black and white teens that occurred between 1970 and 1980 in districts that implemented a school desegregation plan during the 1970s to changes in teen fertility rates over the same decade in districts that initiated school desegregation either in the 1960s or in the 1980s. Using this difference-in-differences strategy, they find that desegregation plans adopted in the 1970s are associated with decreases in teenage fertility among black females, and are not associated with any changes in fertility rates among white adolescents.

Our study differs from this previous study in several ways. Most importantly while this earlier study relies on sample estimates of fertility during years of the U.S. Census, we employ vital statistics data which provide annual population birth counts by county. These data allow us to construct more precise measures of fertility and to control for county-specific trends in fertility rates that predate the adoption of desegregation plans. These data also provide measures of fertility that are more proximate to the adoption of desegregation plans in time, and which, thereby, allow us to match the timing of the policy change to the outcome more accurately.

Using models that control for county-specific time trends, we find that the implementation of school desegregation court orders in a sample of large city school districts is associated with subsequent increases in fertility rates among African-American teens. Our preferred estimates indicate that, relative to preexisting trends, black teen fertility rates increased by 5.0 births per 1,000 15- to 19-year-olds in the first three years following the adoption of a desegregation court order and by 8.0 births per 1,000 teenagers in the fourth through sixth years following desegregation. These estimates represent increases between 3.6 to 5.7 percent of black, teenage fertility rates in 1970, which are smaller than the effects on teenage fertility that have been estimated for abortion legalization and similar in magnitude to estimated effects from the expansions in family planning services in the Medicaid program (Guldi 2008; Kearney and

Levine 2009; Levine et al. 1999). Consistent with Liu *et al*. (2012), we find no effect on the fertility of white teens.

Supplementary analysis suggest that the effects of desegregation that we observe are unlikely to be due primarily to changes in the composition of the black population that coincides with the adoption of desegregation. We also present evidence consistent with the hypothesis that desegregation may have increased the social and economic isolation of disadvantaged blacks in some schools, and thus, which supports the hypothesis that desegregation would have increased fertility among at least some groups of black teens.

The rest of the paper is organized as follows. Section II provides background on desegregation efforts and teen fertility rates, and previews our identification strategy. Section III describes the data used in our analysis. Section IV describes the analysis we used to identify the impacts of school desegregation, and Sections V through VII present in greater detail the results highlighted above. Section VIII investigates reasons for the differences between our finding and those of Liu et al. (2012) and a concluding section briefly summarizes our findings.

II. Background

The history of school desegregation is well-documented by, among others, Armor (1995), Cascio et al. (2010), Clotfelter (2004), and Guryan (2004). Significant school desegregation efforts did not follow the landmark *Brown v. Board of Education* decision immediately. The second Brown decision (*Brown II*; 349 U.S. 294, 1955) established that desegregation requirements would be determined case-by-case by federal district courts, and, as a result, the timing of the adoption of effective school desegregation plans varied across school districts.

Little significant desegregation occurred during the first decade following the original *Brown* decision. The later part of the 1960s and early 1970s, however, saw important pieces of legislation and a series of Supreme Court decisions that led to a rapid increase in desegregation efforts. Supreme Court rulings in *Green v. New Kent County* (1968), *Alexander v. Holmes* (1969), and *Swan v. Charlotte-Mecklenberg* (1971) paved the way for federal district court decisions requiring substantial desegregation efforts, and, as a result, the early 1970s saw dramatic decreases in school segregation, principally, but not exclusively, in the South. A 1973 Supreme Court ruling, *Keyes v. Denver School District No. 1*, made it easier to subject districts outside the South to judicial desegregation requirements, and court orders mandating desegregation plans continued to be handed down through the 1970s and into the 1980s.

Guryan (2004) argues that court ordered desegregation plans tended to generate large and immediate changes in school segregation. Figure 1 presents evidence on this point similar to that provided by Guryan. The figure tracks two common measures of segregation: the black-white dissimilarity index and the black-white exposure index, for a sample of 105 large school districts (discussed further below) during the years leading up to and following the districts' desegregation orders. The dissimilarity index ranges from zero to one, with higher values representing more segregation. A value of zero indicates that the racial composition in every school in the district matches the racial composition of the district as a whole, and a value of one indicates that all students attend schools exclusively with students of their own race. More generally, the dissimilarity index can be interpreted as the percent of black (or white) students who would have to change schools to achieve a racial composition in each school that matches the racial composition of the district as a whole. The exposure index can be interpreted as the proportion of students who are white in the typical black student's school, and thus, higher values represent greater exposure of black students to white students.

These indices were calculated for each district in the sample in each year, and Figure 1 plots the average of each measure over time, where time is measured relative to the date each district adopted a court ordered desegregation plan. The figure demonstrates that on average dissimilarity rates were high (0.66 to 0.70) and roughly constant in the years leading up to desegregation, saw a large drop in the initial year of implementation, an additional smaller drop in the next year, and then remained roughly constant at the lower level of about 0.40 for several years after adoption of court ordered desegregation. Similarly, the black-white exposure index was low (less than 0.30) and roughly constant in the years leading up to desegregation, saw a sharp increase in the initial year, followed by a smaller increase in the next year, and then remained roughly constant at the higher level of about 0.45 for the next several years.

Guryan (2004) also argues that variation in the timing of desegregation court orders across districts is unlikely to be systematically related to other determinants of adolescent outcomes. He claims that the private groups that initiated most school desegregation cases, most prominently the NAACP, followed a deliberate strategy of choosing districts where litigation could establish favorable legal precedents, rather than districts where desegregation was likely to have the largest impacts on African-Americans. Thus, court ordered desegregation plans represent arguably exogenous shocks that dramatically reduced desegregation levels.

To test Guryan's argument, we estimate a discrete-time event history model of the hazard rate of desegregation with a variety of covariates. In Table 1, we report the results from the model using the 125 school districts in the Welch and Light data file (see description below). We modeled the baseline hazard rate with a linear and quadratic term for time, but results were identical under a variety of specifications. In Model 1, we report results using data from the 1960 census, including covariates for the nonwhite female population of teens, the land area of the county, the total population of the county, the proportion of the population residing in urban areas, the median income in the county, the percentage of the population aged 25 or older that had a high school education, and the percentage of the civilian labor force unemployed as well as regional indicators. In Model 2, we also include the teenage birth rate for nonwhite females in 1960. The only factor in either model that is related to the timing of desegregation is the South regional dummy. We ran a joint test of the null hypothesis that all of the coefficient estimates other than the regional dummies are zero, and report the p-value from this test in the last row of the table. This test fails to reject the null hypothesis, which is consistent with Guryan's argument that the timing of desegregation was exogenous to the social circumstances in the counties. To account for differences in timing across regions, we estimate models that control for region-specific year effects, so that our estimates are identified by differences in the timing of desegregation across district within the same region.

During the period when the bulk of desegregation was taking place, fertility rates were generally falling for both white and black teens. Ventura and Freedman (2000) show that fertility rates for African Americans teens fell from 156.1 per 1,000 15- to 19- year-olds in 1960 to 112.8 per 1,000 teenagers in 1990. Among white teens, the rates dropped from 79.4 to 50.8 per 1,000 teens. The largest decreases in teen fertility occurred among African-Americans during the 1970s, which is also the time period of the most intensive school desegregation efforts. The purpose of the analyses in this paper is to determine whether school desegregation efforts contributed to the decline in teen birth rates, particularly among African-Americans, or whether those declines might have been even larger in absence of school desegregation.

III. Data

Our identification strategy exploits the arguably idiosyncratic variation in the timing of desegregation court orders between 1960 and 1988 to estimate changes in county-level, teen fertility rates that control for preexisting, county-specific trends as well as region-specific year effects. The analysis requires data that identifies when districts implemented court ordered desegregation plans as well as teenage fertility rates, defined here as the number of births to females aged 15 to 19 per 1,000. Information on the implementation of desegregation plans is drawn from a dataset compiled by Welch and Light (1987) for the U.S. Commission on Civil Rights. This dataset includes information on a sample of 125 districts that includes every district with more than 50,000 students that had between 20 and 90 percent minority enrollment in 1968 and a random sample of districts with more than 15,000 students that had between 10 and 90 percent minority enrollment in 1968. This sample is not representative of U.S. school districts, but does account for nearly half of all minority enrollment in the U.S. as of 1968 (Welch and Light 1987), and has been used in a number of desegregation studies (e.g., Guryan 2004; Liu *et al*. 2012; and Reber 2005)

Counts of births to teenage mothers are drawn from Vital Statistics records. For the 1968 to 1988 period, annual counts of births to 15-19 year old females by race for each county and for each Standard Metropolitan Statistical Area (SMSA) in the U.S. were obtained from Natality Data Files produced by the National Center of Health Statistics (NCHS).[1] Counts for 1968 through 1971 are based on information obtained from a 50 percent sample of all birth certificates. Beginning in 1972, counts are based on a census of all birth certificates. Birth counts by county are not generally available for years prior to 1968. Counts by age and race of

[1] See http://www.cdc.gov/nchs/data_access/Vitalstatsonline.htm. In all cases, 1960 definitions of SMSAs are used.

the mother for each SMSA for 1960 through 1967 and for cities with populations of 100,000 or more for 1965 through 1967, however, were obtained from Vital Statistics of the United States, Volume I, Natality published by NCHS.[2] Thus, for counties that are coterminous with either an SMSA or a large city (53 of the 105 counties in our sample), we were able to obtain birth counts prior to 1968. In other areas, birth counts prior to 1968 are only available at the SMSA level.

To convert birth counts to fertility rates we used age and race specific population estimates from the NCHS Compressed Mortality File.[3] These files cover the years 1968 to 1988. For earlier years, age and race specific population counts for each SMSA were obtained from the 1960 U.S. Census,[4] and simple, linear interpolations were used to impute population counts for the years 1961 through 1967. Relying on such simple linear interpolations makes the population estimates and thus, estimated fertility rates more error prone for the 1961 through 1967 period than for the post-1967 period. Birth counts by age and race are available for more narrow age groupings than 15- to 19-year-olds from the sources described above, but the population estimates just described are only available for 15- to 19-year-olds.

One complication for our study is that the Welch and Light dataset provides information on district desegregation efforts, but teen fertility rates are measured at the county or SMSA level. We used information from the 69-70 School District Geographic Reference File, Bureau of Census, 1970 (ICSPR 3515)[5] to match districts to counties and SMSAs. In the analyses that

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[2] See http://www.cdc.gov/nchs/products/vsus.htm. These counts are also based on a 50 percent sample of birth certificates and population estimates are obtained by doubling the sample counts.

[3] See http://nber.org/data/vital-statistics-compressed-mortality-data.html

[4] 1960: Eighteenth Decennial Census of the United States, 1960 Population, Volume I, Characteristics of the Population. (Washington, D. C. 1962) Haines, Michael R., and Inter-university Consortium for Political and Social Research. Historical, Demographic, Economic, and Social Data: The United States, 1790-2002 [Computer file]. ICPSR02896-v3. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2010-05-21. doi:10.3886/ICPSR02896.v3http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/2896?archive=ICPSR&q=2896

[5] See http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/3515/

follow, we treat the county or the SMSA as the unit of analysis and consider a county or SMSA as exposed to treatment if it contains a district that implemented a court ordered desegregation plan. It is possible that some teenagers in a particular county were not directly influenced by a desegregation plan implemented in only one of several districts within the county, which might impede our ability to detect the effects of desegregation plans. A few factors, however, bolster our ability to detect effects. For 44 of the 105 counties in our sample, the district that adopted a court ordered desegregation plan covers the entire county, and in most of the other counties, the district that adopted the desegregation plan was large relative to the rest of the county. In 19 of the remaining 61 counties, the desegregating district contained over 70 percent of the district and in 36 of the 61 counties, the desegregating district contained over 50 percent of the county population in 1970. In only 4 cases did the desegregating district contain less than twenty percent of the county population in 1970.

Analyses that examine county and SMSA level birth rates have an important advantage over district level analyses. School desegregation might be associated with changes in patterns of residential mobility within a county or SMSA that result in changes in the population composition of particular districts. If so, it would be difficult to determine whether any changes in birth rates that follow desegregation are due to effects on individual behavior and outcomes, or merely changes in district composition. Desegregation is less likely to be associated with changes in migration into and out of counties or SMSAs than with changes in district populations, and thus, changes in county or SMSA level birth rates that follow the adoption of desegregation plans are easier to interpret. We return to this issue below and consider more carefully whether the results of our analyses are likely to be due to compositional changes.

We matched the 125 districts in the Welch and Light dataset to 116 different counties. Four counties contained multiple districts from the Welch and Light dataset, which is why the number of counties is less than the number of districts. Of these 116 counties, 105 contained a district that had adopted a court ordered desegregation plan prior to 1988.[6] Table 2 presents teenage birth rates, measures of segregation and other variables drawn from the 1960 Census for the samples used in our analysis. The table shows first that teen birth rates are substantially higher in the sample counties exposed to court ordered desegregation plans than in the districts that never adopted court order desegregation, particularly among blacks. Birth rates among the sample counties exposed to desegregation are close to national rates--rates for black teens in the sample counties are slightly higher than the national rates in both 1970 and 1980, and rates for white teens in the sample counties are nearly identical to national rates. The counties exposed to desegregation also have higher levels of school segregation, as indicated by both the dissimilarity and exposure index, and have larger populations, on average, than the other sample counties.

Another challenge for the analyses presented here is using the information from the Welch and Light dataset to identify the desegregation starting dates for each district. Many districts adopted multiple desegregation plans at different points in time. In all but two cases, we use the adoption date for the first desegregation plans adopted by the district that Welch and Light (1987) characterize as a major plan. In nearly all cases, this is the plan that was followed by the most marked changes in segregation indices in the district. In two cases, Buffalo, NY and San Jose, CA, where another plan adopted by the district led to a substantially larger change in desegregation indices than the initial plan, we used the plan that was followed by greater

[6] The computation of dissimilarity and exposure indices presented in Figure 1, are based on these 105 districts.

reductions in segregation. For counties with more than one district that adopted a court ordered desegregation plan, we used the adoption date for the largest of the districts that desegregated.

Appendix A provides a list of all the districts in the Welch and Light data, the county and SMSA where the district is located and the desegregation implementation date as we coded it. Our identification of desegregation starting dates matches those used by Weiner, Lutz and Ludwig (2005) in a study of the effects of desegregation on crime. Figure 2 presents the distribution of desegregation implementation start dates in the study sample. We can see in this figure that the bulk of southern districts in the study sample initiated desegregation between 1969 and 1973, and a majority of district outside the South initiated desegregation in the mid to late 1970s. Nonetheless, there is substantial variation in the timing of desegregation within regions, and it is this variation that helps us to identify the effects of desegregation on teen fertility rates.

In addition to the data on teen birth rates and desegregation implementation, we draw on a number of additional data sources including the decennial Census, measures of access to abortion and oral conception used in other studies, and the High School & Beyond survey administered by the National Center of Education Statistics. These additional data are described below when we present the analyses that make use of them.

IV. Analytic Methods

In order to isolate the effect of desegregation on teenage fertility rates, we estimate regression models that control for county fixed effects, county-specific time trends, and region-specific year effects. Specifically, we estimate the following regression model:

$$Y_{irt} = \beta_1 (D1-3)_{irt} + \beta_2 D4_{irt} + \phi_{rt} + \gamma_i + \eta_i T + \varepsilon_{irt},$$

where *Y* is a race-specific teenage fertility rate for county *i*, in region *r*, and year *t*, and where regions include the South, Northeast, Midwest and West. $(D1\text{-}3)_{irt}$ is a variable that takes on the value one in counties that contain districts that implemented court-ordered desegregation plans in the first three years following the implementation start date and zero otherwise. By treating the year following the adoption of a desegregation plan as the first "post-treatment" year, we are assuming that any impact of desegregation on teen fertility would not be realized until a year after desegregation is initiated. $D4_{irt}$ takes on the value of one in counties that contain districts that implemented court-ordered desegregation four or more years after the implementation start data and zero otherwise. This specification of the treatment variable allows us to estimate short term as well as longer term impacts of desegregation. ϕ_{rt} is a region-by-year fixed effect, which controls for any shocks that have constant effects on teen fertility rates within a region. Together $\gamma_i + \eta_i T$ specifies the intercept and slope of a linear, county specific trend in teenage fertility, which controls for unobserved factors that have constant effects on the level of and changes in fertility rates over time. Model parameters are estimated using a least squares, fixed effects estimator, and Huber-White standard error estimates robust to clustering at the county level.

The parameters of interest, β_1 and β_2, are identified in this regression by comparing deviations in teen fertility rates from the preexisting trend in each county that has adopted a desegregation plan to deviations from preexisting, county-specific trends during the same calendar year in districts that have not yet desegregated. The assumption required to interpret the resulting estimates as the causal effect of desegregation is that, in the absence of the desegregation plan, trends in teenage fertility rates would have changed similarly in counties that desegregated as in other counties in the same region that had not yet desegregated. If the timing of desegregation is exogenous with respect to factors affecting changes in teenage fertility trends, then this identifying assumption is plausible. After presenting our primary results and robustness checks, we present analyses that examine more fully the plausibility of this assumption.

In our primary analysis, we restrict the study sample in two ways. First, we limit the sample to districts that adopted a court order desegregation plan prior to 1988. As shown in Table 2, the districts that did not adopt desegregation plans differ substantially from those that did at the beginning of the desegregation period. Thus, changes in teenage fertility trends in these districts do not provide a plausible approximation of what would have been observed in districts that did adopt desegregation plans in the absence of those plans. Second, we limit the sample to observations that are six or fewer years prior to or six or fewer years following the implementation of court order desegregation. Fertility rates that are far away from the desegregation adoption date in time may not be as relevant for predicting the counterfactual fertility rates that we would observed in the absence of desegregation, and thus, including those years in the sample might distort the estimates of fertility rate trends.

Several aspects of our primary analytic sample are worth noting. For the years 1960-67 we have county level birth counts only for those counties that are coterminous with the SMSA. The six-year pre-desegregation window reaches into the 60-67 period for 53 of the 105 counties in our sample, and for 29 of these counties fertility rates are not available for the years prior to 1968. For 18 of these 29 counties with missing county level birth rates, SMSA level birth rates are available. In our primary analysis, we use the SMSA level birth rates, where available, in place of any missing county level birth rates. Also, two districts that desegregated in 1961 only have one year prior to desegregation and one district that desegregated in 1986 only has 2 years post desegregation observed. We retain these districts in our primary analysis. In the end, we have 1199 usable observations on 105 counties, and there are a total 13 districts for which we observe less than the 6 observations prior to desegregation and one for which we observe less than six years following desegregation. Finally, we do not have any measures of teenage fertility, either at the county or SMSA level for blacks, prior to 1964, and thus, the estimated effects on black teenage fertility are based on a slightly smaller sample (1177 observations on 103 districts) than the estimates for white and nonwhite teens. After presenting the results of our primary analysis, we present robustness checks that examine how sensitive of our results are to these decisions about the analytic sample.

V. Primary Results and Robustness Checks

Table 3 presents our primary estimates of the effect of desegregation on teenage fertility rates. Teenage fertility rates are measured as the births to 15- to 19-year-old females per 1,000, and effect estimates are computed separately for non-white (inclusive of black), black, and white teens. Effects of desegregation on each race specific fertility rate are computed using regressions that control for region-by-year and county fixed-effects, without controls for county-specific time-trends (Model 1), as well as regressions that include county-specific trends (Model 2).

Consistent with Liu *et al*. (2012), we find that desegregation had no effect on the teen fertility of whites. In sharp contrast to Liu *et al*., however, our estimates indicate that desegregation increased teenage fertility rates among non-whites and particularly among blacks. The estimated effects from the models that control for county specific time-trends, which are slightly larger than the estimates from models that do not control for these trends, indicate that fertility rates among non-whites were 4.4 births per 1,000 females higher in the first three years after desegregation and 7.5 births per 1,000 females higher four to six years after desegregation than we would have expected in the absence of desegregation. For black teens the estimated effects are slightly larger. Relative to the black, teenage fertility rate in 1970, the estimates for black teens represents an increase of 3.6 percent one to three years after desegregation and 5.7 percent four to six years after desegregation. These estimated increases in the fertility rate, are at the low end of estimates of the reduction in teen fertility that resulted from the legalization of abortion—which range from 4 to 12 percent (Guldi 2008; Levine *et al*. 1999), and similar in magnitude to the 4 percent reduction in fertility found following expansions in the family planning services supported by the Medicaid program (Kearney and Levine 2009).

Table 4 presents the results of analyses designed to determine whether or not the results in Table 3 depend on particular decisions made in the construction of fertility rates, the analytic sample, and model specification and estimation. We present the results for models that include controls for county-specific trends, for non-white, black, and white teens, for six alternative samples. We also present estimates that control for other policy changes that might have influenced teen fertility as well as estimates that weight counties by the size of the 15- to 19-year-old female population. Regardless of the analytic choices made, the estimated effect of desegregation on the fertility of white teens is always close to zero and statistically insignificant. In the discussion that follows, we focus on the results for non-white and black teens.

The first row presents the results of alternative analyses that use all the years observed in the data, 1960-1988, rather than limiting the sample to observations within six years of the adoption of desegregation. Despite the additional observations, these effect estimates are in most cases less precise than the primary estimates presented in Table 3, which might reflect difficulties fitting a linear time trend over such a long period of time. The point estimates of the effects on non-white and black teenage fertility rates in this row are somewhat smaller (10 to 35 percent) than the primary effect estimates. However, each estimate is still positive and statistically significant, except the estimated effects on black teen fertility one to three years after desegregation, which is considerably less precise than the corresponding estimate in Table 3.

Row 2 presents the results of analyses that include in the analytic sample the 11 counties that did not adopt court order desegregation. Here we use observations from 1968-80 for these non-adopting counties, and code the treatment variables as zero in each of these years.[7] Estimates from these models are slightly larger, but otherwise match the primary effects estimates from Table 3 closely. Row 3 of Table 4 presents results from a sample that includes both the districts that did not desegregate and all years observed in our data. In this analysis, we avoid the need to choose which years to include for those counties that were not exposed to desegregation plans. The estimates from this sample are similar to those obtained in Row 1—somewhat smaller and less precise than those presented in Table 3 and only statistically significant four or more years after desegregation.

In Row 4, we drop from our primary analytic sample counties for which we do not observe fertility rates for each of the six years preceding and each of the six years following the adoption of desegregation. The estimated effects of desegregation using this sample are similar to those in Table 3. In Row 5 we drop all of those observations prior to 1967 for which we used SMSA birth rates to approximate county level birth rates. The estimated effects of desegregation on non-whites obtained using this sample are virtually the same as our primary effect estimates.

In Row 6, we dropped observations where SMSA birth rates were used in place of county birth rates and dropped counties that did not have six years of observations before and after the adoption of desegregation after excluding the observations that do not have county level measures. These edits resulted in dropping 30 counties from our original sample of 105, but are based on the most reliable measures of fertility rates available. The point estimates for non-

[7] The median adoption year for the districts in the study sample that did adopt desegregation plans is 1972. The 1968 to 1980 period comes the closest to matching the time period used for this median district, while also avoiding pre-1968 fertility measures, which are less reliable than measures from 1968 forward.

white and black teenage fertility obtained using this sample are positive, but somewhat smaller (23 to 30 percent) than our primary effect estimates. The effect estimates are also slightly less precise than those reported in Table 3, and while the effect estimates 4-6 years after desegregation are statistically distinguishable from zero, the estimates for 1-3 years after desegregation are only marginally significant. The differences between the estimates from this sample and the full sample of counties exposed to desegregation might simply reflect that the effects of desegregation were somewhat smaller in this sample of 75 counties than in the full sample of 105 counties. Alternatively the difference in results might reflect bias in our original analysis that results from using less reliable measures of teenage fertility during the pre-1968 period or from relying on a relatively small number of pre-desegregation years to estimate preexisting, county specific trends. In any case, the point estimates from this alternative sample are not statistically distinguishable from those obtained from our primary analytic sample, and are substantively similar to those estimates.

Around the time desegregation plans were being implemented in school districts throughout the country, several important policy changes occurred that affected the fertility of young women in the United States. Beginning in the late 1960s, several states legalized abortion, and the *Roe v. Wade* decision in 1973 made abortion available across the county (Levine, 2004). Many states, however, required some form of parental involvement when a minor requested an abortion, effectively creating a barrier to abortion for young women (Guldi 2008). Similarly, while married women had access to oral contraception beginning in 1960, several states did not allow young women access to oral contraception without parental consent until they reached the age of majority. Over the same period that desegregation was occurring, a series of state laws and federal court decision lifted these restrictions making both abortion and oral contraception more accessible to teenagers (Ananat and Hungerman 2012; Bailey 2006; Guldi 2008). Also, Murray (1984) argues that during the early 1970s, welfare benefits were expanding and more generous benefits were inducing single women to bear children out-of-wedlock. Moffitt (2003) reviews the empirical evidence on the fertility effects of AFDC and concludes, "welfare is likely to have some effect on family structure (p. 336)." If differences across counties in the timing of these policies changes are correlated with the timing of desegregation, then the increase in fertility among African American teens reported above might not be due solely to desegregation.

To determine if our desegregation estimates are biased by the potential correlation between desegregation and changes in abortion, contraception and welfare policies, we add several controls for policy changes to our primary regression model. These variables include an indicator variable equal to one during the years abortion was available to teenagers in the state without parental involvement, and zero in other years, and another indicator equal to one after teenagers in the state were allowed access to oral contraceptives, and zero in other years.[8] Because the information we have on teenage access to abortion and oral contraception ends in 1978, we assume that in the final ten years of our time series (1979-1988), abortion and oral contraception access was the same as reported in 1978. To further control for the effects of changes in access to abortion, we use Joyce, Tan, and Zhang's (2012) estimates of the distance from the centroid of each county each year to the nearest abortion provider.[9] Finally, to control

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[8] We are grateful to Melanie Guldi for sharing the variable codes for legal access to oral contraception and abortion for minors she used in Guldi (2008). In this work, Guldi finds that access to abortion and oral contraceptives reduced the fertility of white teenagers, but did not have a statistically distinguishable effect on nonwhites.

[9] We are thankful to Ted Joyce, Ruoding Tan, and Yuxiu Zhang for sharing their data on the distance to the nearest abortion provider from their 2012 NBER working paper. For the years 1970 to 1972, they assume the only abortion providers are in Buffalo, NY, New York City, San Francisco, CA, Los Angeles, CA, and Washington D.C., and they calculate the distance from the centroid of the county to the closest metropolitan area listed above. After 1973, they calculated the distance to the nearest provider based on Guttmacher's abortion provider surveys. Their data series also terminates in 1979.

for the potential effects of the AFDC program, we add the state maximum benefit level for a family of four, inflated to 1990 dollars using the CPI.[10]

Row 7 of Table 4 presents the estimated effects of desegregation from models that include these controls for other policy changes. The results are virtually identical to the effect estimates that do not control for these policy changes. In Row 8, we both include the controls for other policy changes and limit the sample to years prior to 1979, so that we avoid making assumptions about changes in abortion and contraceptive access. The results in Row 8 are similar to those in Row 7, although the estimate effects of desegregation four to six years after desegregation are somewhat smaller.

All of the analysis discussed so far uses unweighted counties as the unit of analysis. The resulting estimates can be interpreted as the average effect of court ordered desegregated plans for the counties in our sample. However, because these estimates weight small counties and large counties equally, the estimates do not necessarily tell us the effect of desegregation efforts on aggregate teen fertility rates. To examine this question the last row in Table 4 presents the results for estimates that weight each county by the size of its 15- to 19-year-old female population. These estimates are also positive, but are considerably smaller (40 to 55 percent smaller) than the unweighted effect estimates. These results indicate that the effect of desegregation on teen fertility tends to be greater in smaller counties, which is confirmed in unweighted estimates of models that include interactions between the treatment variables and the county's 15- to 19-year-old population (results not shown).

[10] We used Robert Moffitt's welfare data file available at http://www.econ2.jhu.edu/people/moffitt/datasets.html (accessed February 27, 2013) for these benefit measures. Moffitt's data does not include data from 1961-1963 and from 1965-1967. We assume the welfare benefits from 1961-1963 were the same as those in 1964, and that the benefits from 1965-1967 were the same as those in 1968.

In sum, our primary analyses indicate that the adoption of court order school desegregation had no effect on the fertility rates of white teens, but increased fertility rates among non-white and black teens. The magnitude of the estimated effects on non-white and black teens are both plausible and substantial when compared to estimated effects of other policies, and do not appear to depend on specific choices made about the measures of fertility or samples used in the analysis. The average effects estimated tend to be larger in small counties, which means the effect of desegregation on aggregate teen fertility rates are smaller than the estimated average effects.

VI. Placebo Tests

A causal interpretation of the estimates reported above relies on the assumption that, in the absence of school desegregation, deviations from teenage fertility trends in counties exposed to desegregation would have been similar to deviations from teenage fertility trends in counties in the same region that had not yet desegregated. One way to test this identifying assumption is to check that the regression models used above do not detect effects where we would not expect any. The results of two such "placebo tests" are presented in Table 5.

For the regressions presented in the top panel of Table 5, we use a sample consisting of observations from the nine years prior to desegregation in each of the districts that desegregated prior to 1988. Using this sample we define a pseudo treatment variable, $\tilde{D}_{irt}$, for each county as equal to one for the first, second and third year immediately preceding desegregation (before we would expect to see any effects of desegregation on teen births), and zero for all the other years. We then substitute this pseudo treatment variable for the actual treatment variables in our regression models. The estimated effects of our pseudo-treatment, or placebo, on non-white fertility rates are close to and statistically indistinguishable from zero. The failure to reject the null-hypotheses is not due to the fact the estimated coefficients in this analysis are less precise than in our actual analysis, but rather because the point estimates are very small and close to zero. Thus, the identification strategy used does not detect effects in this placebo test.

In the bottom panel of Table 5, we use the same models reported in Table 3, but we replace teen birth rates with birth rates for women aged 25-34. This is a group of women whose fertility should not have been affected by school desegregation orders, at least not due to the same mechanisms affecting the teens. We chose age 25-34 since our data source for population counts grouped women after their teen years in 10-year increments. For the first three years following desegregation, we obtain positive effect estimates, but these are less than one-third the size of the estimated effect on 15-19 year olds, and statistically indistinguishable from zero. For four to six years following desegregation the estimated effects on the fertility of 25-34 years are negative, but very close to and statistically indistinguishable from zero. Similarly we find no evidence of effects on 25-34 year old white female fertility rates. Together the results of these two sets of placebo tests provide strong support for our identification strategy.

VII. Exploring Potential Mechanisms

Given prior findings that desegregation had desirable effects on black students (Guryan 2004; Weiner, Lutz, and Ludwig 2009; Reber 2010) our findings are perhaps surprising. In this section we assess the plausibility of our findings by investigating the potential mechanisms through which desegregation might have increased teen birth rates among non-whites. First, we examine changes in population that accompanied desegregation, and assess the extent to which the estimated changes in birth rates may have been due to changes in the composition of counties that experienced desegregation rather than behavioral changes of individuals. Then, assuming the effects that we estimate are at least partially due to behavioral changes, we provide evidence consistent with the idea that some black students may have experienced increased isolation from more advantaged populations as the result of incomplete school desegregation.

Compositional Changes

Several studies have found that desegregation increased the migration of white families from the central cities of metropolitan areas, where most of the desegregation plans were implemented, to suburban districts where desegregated schools could be avoided (Reber 2005; Welch and Light 1987). There is also evidence from several communities that African Americans moved from the central city into suburban areas during this period (Clotfelter 2004). Childbearing rates are much higher among low socioeconomic status teens compared to high socioeconomic status teens. If high socioeconomic status teens left the city school districts for neighboring school districts, then we may simply be observing the higher rate of teenage childbearing among the low socioeconomic status African American teenagers who remained in the city schools. Similarly, although there is less research on this question, it is possible that desegregation efforts influenced migration across metropolitan areas. If so, then that could also account for the changes in fertility rates trends that we observe above.

Table 6 presents estimates of the effect of desegregation on county and metropolitan populations. In these analyses, we regress the log of the population of females aged 15- to 19-year-old (nonwhite, black, and white) in the county on our desegregation variables controlling for region-by-year fixed effects, county fixed effects, and county-specific trends, as in our analysis of the effects of desegregation on fertility rates. The models in the first column of each panel suggest that, relative to preexisiting trends, the population in counties with desegregating school districts declined following the adoption of desegregation plans. Consistent with the "white flight" literature, we find that following desegregation, the population of white teenagers declined 0.6 percent during the first three years after desegregation, an estimate that is statistically significant at the 0.05 level. While the declines for non-white and black populations are not statistically significant, the point-estimates are similar in magnitude to that observed for the white population. Although these results do not necessarily imply that the socioeconomic composition of white and nonwhite families changed as a result of desegregation, it does indicate that there was net migration out of counties in response to desegregation.

The second column of each panel in Table 6 presents estimates of the effect of desegregation on metropolitan populations. Migration out of metropolitan areas that experienced desegregation was lower than migration out of desegregating counties in the first three years after desegregation, but higher than migration out of desegregating counties four to six years after desegregation. None of the estimated effects of desegregation four to six years after it was initiated, however, are statistically significant, and why desegregation would have caused a net migration of nonwhites away from desegregating metropolitan areas is unclear.

One way to test whether the estimated effects of desegregation on teen fertility rates are driven by changes in population composition that may have resulted from migration within metropolitan areas is to look at the effect of desegregation on fertility rates at the SMSA level. Table 7 presents such estimates. In these models, SMSA level teenage fertility rates are regressed on an indicator of whether one of the districts in the SMSA had adopted desegregation controlling for region-by-year fixed effects, SMSA fixed effects, and SMSA specific trends. The results are consistent with the results from our county level analysis. Among non-white teenagers, we find a statistically significant increase of 3.84 births per 1,000 teenagers annually during the first three years following desegregation (compared to a 4.42 increase in our preferred estimates) and a 6.31 birth increase four to six years after desegregation (compared to a 7.46 in our preferred estimates). Both estimates are statistically significant at conventional levels.

Given that that we find similar results whether we use county level or SMSA-level analysis, it seems unlikely that our primary finding is due to compositional changes resulting from migration within metropolitan areas. It remains possible that patterns of migration across metropolitan areas resulted in changes in the composition of the nonwhite population. For instance, a net migration in of low-SES black families or a net migration out of high-SES black families could explain the results reported in Table 3. However, given the plausibly exogenous timing of desegregation court orders, and the fact that other studies show that desegregation was associated with improvements in educational attainment (Guryan 2004; Reber 2010), such an explanation is unlikely.

Selective Desegregation

One potential explanation for our findings pertains to the incomplete and nonrandom desegregation of white and black students. If administrators responding to desegregation court orders were more inclined to move high socioeconomic status African American students into integrated schools, leaving low socioeconomic status African American students in schools with high proportions of minority students, then that could have created conditions that increased fertility rates in schools attended by low socioeconomic status African-Americans. Alternatively, if school administrators had attempted to minimize the distances African-Americans had to travel to reach majority white schools, in many communities they would have integrated low socioeconomic status white students with high socioeconomic status African American teenagers (relative to the population of African-Americans). In his seminal book, *The Truly Disadvantaged*, Wilson (1987) argues that social problems became more prevalent in many cities during the 1970s and 1980s when middle-class African-Americans moved out of segregated neighborhoods leaving behind a population of low-income African-Americans. This concentrated poverty led to a number of social dislocations, including growth in teenage childbearing. If school desegregation also generated concentrated poverty within all-black schools, then teenage fertility might have increased as a result.

To the best of our knowledge, research that would inform this hypothesis is limited. Hawley et al. (1983) reports that most school districts did not explicitly include socioeconomic status criteria in their desegregation plans; however, Los Angeles adopted plans that had the effect of separating individuals across schools by socioeconomic status. They write that the school board believed that segregating by socioeconomic status would reduce white flight.

In the Northeastern school districts in our study sample, 29 percent of African-Americans remained in schools that were more than 90 percent nonwhite even after the adoption of desegregation plans. Corresponding figures for sample districts in the Midwest and the South were 22.2 and 20.6 percent, respectively. These figures represent substantial reductions in the percentage of African-Americans in racially isolated schools, but nevertheless, not all black students in these districts experienced desegregated schools, and substantial numbers remained in virtually all-black schools. If those students who remained in racially isolated schools were disproportionately low-income blacks at relatively high risk of teen pregnancy, that would provide a potential explanation for our findings.

To investigate this possibility, we use the sophomore cohort in the High School and Beyond (HSB) survey to determine if low socioeconomic status African-Americans disproportionately attended racially isolated schools following desegregation. The HSB was conducted on behalf of the National Center for Education Statistics in 1980 and included over

30,000 high school sophomores, approximately 13.3 percent of whom are African-American. The HSB Sophomore cohort is close to ideal for our purposes since it contains a group who likely attended school while desegregation was taking place. Sophomores in 1980 would have been in first grade in roughly 1971, sixth grade in 1976, and ninth grade in 1979. While we do not know which school district the students attended, some proportion of the sample would have attended desegregated school districts, at least during sixth and ninth grade, if not earlier.

In 1980, each respondent was asked “When you were in first, sixth, and ninth grades, about how many students in your class were Black?” Response categories were none, few, about half, most, and all. We collapsed the “none” and “few” options into the same category (“Few or less”) since these responses were relatively rare. The HSB also calculated a socioeconomic status measure for each sophomore based on the father’s occupation and education, the mother’s education, the family’s income, and the material possessions of the household. This measure was then converted into a percentile socioeconomic status rank for each survey respondent.

Table 8 shows the mean socioeconomic status percentile for the African-American subsample by the grade the respondent attended and the racial composition of the respondent’s school. The evidence suggests that the socioeconomic status of African-American students in racially isolated schools tended to be lower than the socioeconomic status for African-Americans in more racially mixed schools. For example, in first grade, the mean socioeconomic status percentile for African Americans students who attended schools that were mostly white was 41.3 compared to 32.0 for students in mostly black schools and 35.6 for those in all black schools. One finds similar results in sixth grade. By the ninth grade, there is a monotonic decline in the mean socioeconomic status as one moves from racially integrated to racially isolated schools.

While far from definitive, this evidence indicates that students in primarily black schools tended to have lower socioeconomic status than African-Americans in integrated schools.

VIII. Contrast with Prior Findings

Our finding that desegregation is associated with increases in fertility rates among black teens stand in sharp contrast to the results reported by Liu *et al*. (2012), which estimates that desegregation decreased teenage fertility rates by 14.5 percent. There are several differences between our analysis and that conducted by Liu *et al*. that might contribute to the differences in findings.

First, while our analysis uses counties as the unit of analysis, Liu *et al*. (2012) presents the results of individual level analyses, which effectively provide estimates weighted by the teenage population in the county. We have already seen in Table 4 that when we apply population weights to our analysis, the estimated effect of desegregation is less positive than those obtained from unweighted, county level analyses. Whether weighted or unweighted analyses are more appropriate depends on the question one would like to answer. Unweighted estimates provide the average effect of desegregation on county birth rates for our sample of counties. Weighted estimates provide the effect of desegregation on aggregate birth rates across the counties in our sample. Both parameters are interesting for policy purposes. In any case, given that the point estimates we obtain using our data and identification strategy remain positive and marginally significant when we weight by population, the choice of weights only plays a small role in explaining the differences between our findings and those of Liu, *et al*.

Second, the measure of teen fertility used in the two studies are different. We use an annual measure reflecting the proportion of teens residing in a county who bear a child during the calendar year, which is based on a census of birth records in each county and population estimates. The measure used by Liu, *et al*. reflects the proportion of teens who have had a live birth at any time, and is based on self-reports from a survey sample. Our measure has the advantages of drawing on census counts from official records rather than self-reports and survey samples. Also, since Liu *et al.*'s data does not allow them to determine where a woman resided when she gave birth--some of the individuals they count as teen mothers may have resided elsewhere when they gave birth. Our measure, however, has the disadvantage of relying on population estimates that may contain some error.

To test the extent to which the different measures of fertility account for the different results of the two analysis, we applied the estimation method used by Liu *et al*., using our measures of black teen fertility. Specifically, we obtained difference-in-differences estimates that compare the change in teen fertility rates between 1970 and 1980 among districts that desegregated during the 1970s to the change in teen fertility rates over the same period among districts that desegregated in the 1960s and 1980s. When we weight observations by the female population we obtain effect estimates of -2.429, i.e. we find that desegregation is associated with decreases in fertility rates among black teens. The estimated effect is smaller than that obtained by Liu *et al*., implying a decrease of 1.7 percent rather than 14.5 percent, and is not statistically distinguishable from zero. So, difference in measures of fertility might be playing some role in creating the differences in findings between our analysis and that of Liu et al. However, the fact that we obtain negative impact estimates using their analytic approach with our measures of fertility, suggests that differences in identification strategy may be playing a larger role than differences in measures in explaining the why the two analyses yield such disparate results.

Although they both exploit variation in the timing of desegregation across districts, there are three advantages that the identification strategy that we employ has relative to that used by

Liu *et al.* Our estimates control for preexisting trends in teen fertility rates and Liu *et al.*'s do not. *Liu et al.*'s estimates the effect of desegregation that took place during the 1970s, while our estimates are based on desegregation that took place in the 1960s and 1980s, as well as the 1970s. Finally, the Liu *et al*. estimates are based solely on measures of fertility obtained in 1970 and 1980, while we draw on measures from 1960 through 1988. In each of these ways, our analysis makes use of a fuller range of information the Liu *et al*.

Table 9 presents results that provide some indication of how much these differences in identification contribute to the differences in results. Focusing on the estimated effects of desegregation on black teens, the first row of Table 9 presents our primary effect estimates from Table 3. The second row presents the results obtained from our models when we weight observations by the population, results that also appear in Table 4. The third row presents the results from an analysis that both weights by population and drops controls for trends from our preferred model. Both adding weights and removing controls for trends results in estimated effects that are less positive than we obtained and the estimated effects of desegregation are no longer statistically distinguishable from zero. The fourth row of Table 9 not only weights for population and removes controls for trends, but also limits the sample to the years 1970 to 1980 and only counts counties that contain districts that desegregated during the 1970s as treatment group schools. This sample restriction results in estimates that are even closer to zero than those in row 3. Finally, the last row of Table 9 limits the estimation sample to observation from 1970 and 1980, and only counts districts that desegregated during the 1970s as treatment group schools. In this estimation, each of the advantages that our data allow us to achieve are removed, and like in Liu *et al*., the estimates of the effects of desegregation become negative.

Thus, it appears that although differences in the units of analysis and measures of fertility may play some role, the difference between our results and those of Liu *et al*., are due primarily to differences in the estimation strategies. Because our estimation draws on more information and exploits annual measures of fertility rates over a large number of years to control of preexisting trends, we believe that our estimates are more accurate than those of Liu *et al*.

IX. Conclusion

School desegregation was one of the most important policy changes in the history of the United States. Yet nearly 60 years after *Brown vs. the Board of Education* decision, we know very little about its impact outside of the field of education. In this paper, we examine how school desegregation affected the fertility of females aged 15 to 19. Using models that control for county fixed-effects and county-specific time trends, we find that the implementation of school desegregation court orders in a sample of large city school districts was associated with increases in fertility rates among black teens. Our preferred estimates indicate that, relative to preexisting trends, nonwhite teen fertility rates increased by 4.4 births per 1,000 teenagers aged 15 to 19 during the first three year following desegregation and 7.5 births per 1,000 teenagers in the fourth through sixth year following the adoption of a desegregation court order, increases of 3.1 and 5.3 percent of the black, teenage fertility rate in 1970, respectively. We find no evidence of an influence on the fertility of white teens.

Supplementary analysis suggest that the effects of desegregation that we observe are unlikely to be explained by changes in the composition of the black population that coincides with the adoption of desegregation. We also present evidence consistent with the hypothesis that desegregation may have increased the social and economic isolation of disadvantaged blacks in some schools, and thus, supports the hypothesis that desegregation could have increased fertility among at least some groups of black teens.

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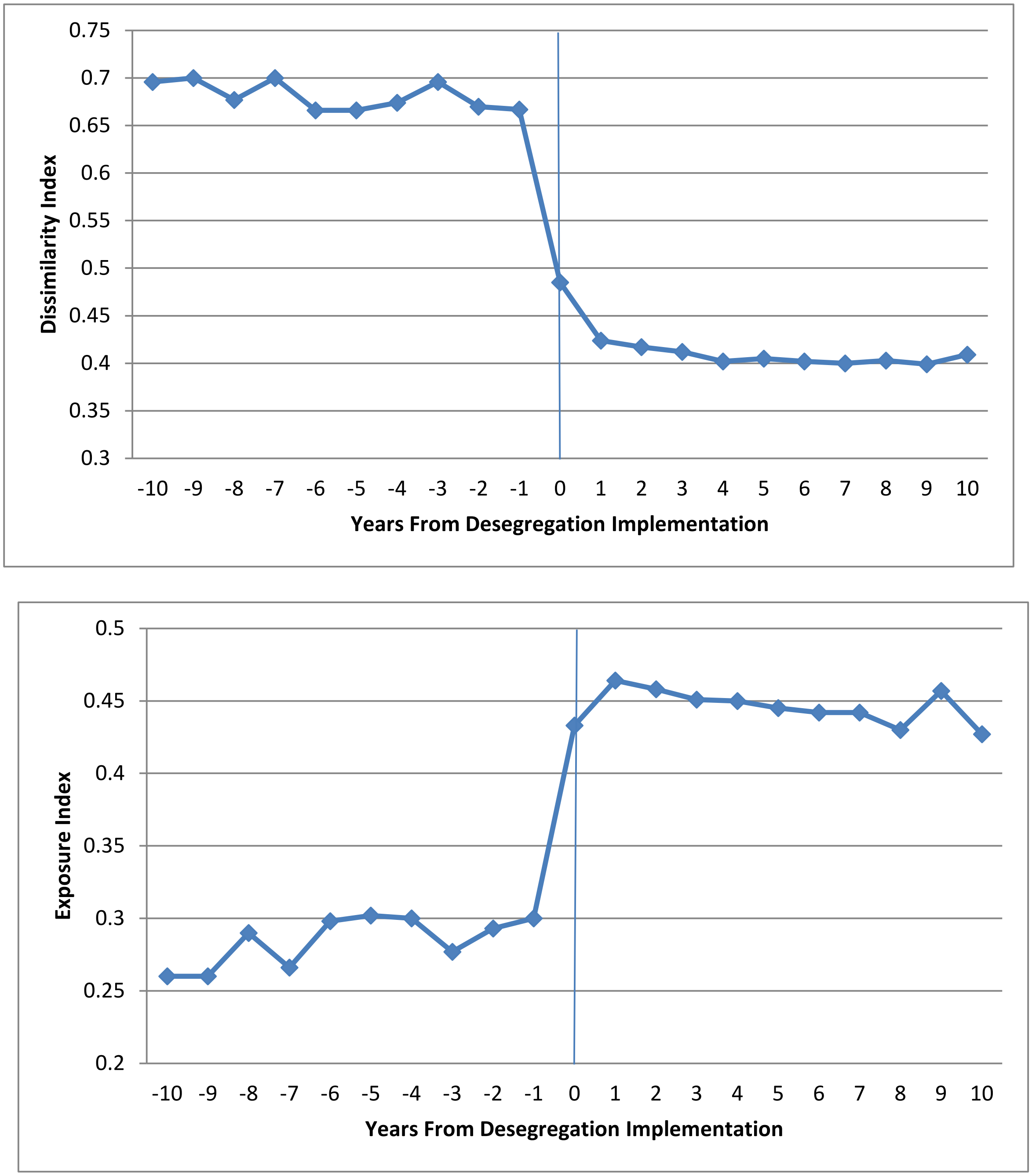

Figure 1: The Effect of Court Order Desegregation Plans on School Racial Segregation
Authors' calculations using data from Welch and Light (1987).

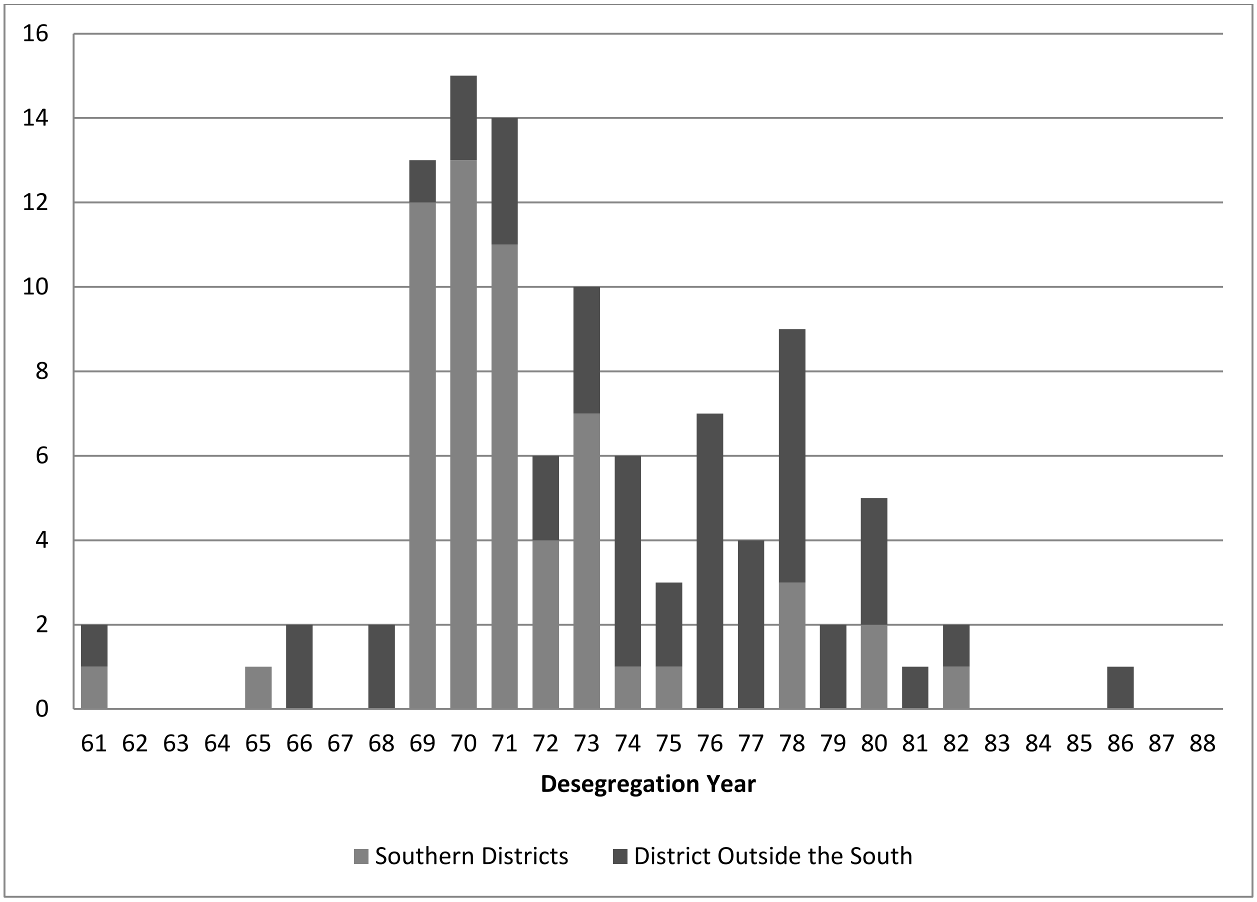

Figure 2: Distribution of the Start of Court-Order Desegregation Plans

Authors' calculations using data from Welch and Light (1987).

Table 1: Event History Model of Timing to Desegregation

	Model 1	Model 2
Nonwhite female population 15-19 (000s)	-0.030	-0.029
	(0.030)	(0.030)
Land area (000s)	-0.057	-0.068
	(0.057)	(0.062)
Total population (000s)	0.000	0.000
	(0.000)	(0.000)
% of population urban	-0.006	-0.008
	(0.016)	(0.017)
Median income	-0.082	-0.080
	(0.229)	(0.232)
% of population 25+ with high school education	0.027	0.028
	(0.021)	(0.021)
% of labor force unemployed	0.112	0.115
	(0.108)	(0.110)
Northeast	0.366	0.334
	(0.565)	(0.572)
Midwest	0.096	0.014
	(0.465)	(0.510)
South	1.305**	1.214**
	(0.507)	(0.532)
Nonwhite teenage birthrate 1960		0.002
		(0.004)
p-value for Chi-Sq. test; H_0: All coefficients = 0 (other than regions & baseline hazard)	0.6834	0.6542
N	1602	1602

Notes: ** $p < 0.05$; Standard errors clustered at county-level; baseline hazard measured with quadratic function; similar results with indicators for time periods.

Table 2: Sample Descriptives

	Sample Counties with Court Ordered Desegregation	Other Sample Counties
Black Teen Fertility Rate - 1968	146.0	125.6
White Teen Fertility Rate - 1968	56.4	48.5
Black Teen Fertility Rate - 1970	148.4	121.6
White Teen Fertility Rate - 1970	58.4	51.1
Black Teen Fertility Rate - 1980	101.9	77.1
White Teen Fertility Rate - 1980	45.4	42.2
Black-White dissimilarity Index - 1968	0.735	0.594
Black-white exposure Index - 1968	0.252	0.396
Age 15-19 White Female Population - 1960	16,866	29,230
Age 15-19 Non-White Female Population - 1960	2,940	4,316
Total population - 1960	571,930	450,260
Percent urban - 1960	83.4	81.0
Median income - 1970	9,654	9,845
Percent of adults aged 25-44 w H.S. diploma -1960	43.8	42.4
Percent Unemployed - 1960	5.2	5.6

Table 3: Effects of Desegregation Court Orders on Teen Fertility Rates

	Non-Whites		Blacks		Whites	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Desegregation Effect	3.789**	4.420**	4.454**	5.003**	-0.049	0.040
(1 to 3 Years after)	(1.686)	(1.704)	(1.911)	(1.895)	(0.777)	(0.768)
Desegregation Effect	5.727**	7.459**	6.914**	7.998**	-0.814	-0.064
(4 and more year after)	(2.826)	(2.596)	(2.891)	(2.716)	(1.372)	(1.225)
Controls for:						
Region-by-year effects	yes	yes	yes	yes	yes	yes
County fixed effects	yes	yes	yes	yes	yes	yes
County trends	no	yes	no	yes	no	yes
Number of Counties	105	105	103	103	105	105
Number of Observations	1199	1199	1177	1177	1199	1199

Teenage fertility rates are measured as births per 1,000 15-19 year old females. Desegregation effect estimated using sample consisting of six years prior to desegregation and 6 years following desegregation for all districts the desegregated before 1988. Robust standard errors in parentheses. **statistically significant at 0.05 level.

Table 4: Effects of Desegregation Court Orders on Teen Fertility Rates, Robustness Checks

	Non-Whites Effect of Desegregation		Blacks Effect of Desegregation		Whites Effect of Desegregation	
	1-3 Years After	4+ Years After	1-3 Years After	4+ Years After	1-3 Years After	4+ Years After
1. Include all years observed for each county	3.682*	4.814**	4.551	6.331*	0.495	-0.383
	(1.994)	(2.294)	(2.833)	(3.665)	(0.665)	(1.117)
Number of counties/observations	105/2941	105/2941	103/2527	103/2527	105/2941	105/2941
2. Include districts that did not desegregate[1]	4.688**	8.787**	4.995**	8.471**	0.282	0.926
	(1.764)	(2.787)	(1.989)	(3.016)	(0.764)	(1.227)
Number of counties/observations	116/1331	116/1331	116/1317	116/1317	116/1331	116/1331
3. Include all years & districts that did not desegregate	3.139	5.363**	3.940	6.759*	0.555	-0.282
	(2.066)	(2.426)	(2.843)	(3.863)	(0.667)	(1.111)
Number of counties/observations	116/3244	116/3244	116/2844	116/3844	116/3244	116/3244
4. Drop districts that do not have the six observations before and after desegregation	4.090**	6.823**	4.602**	7.198**	-0.552	-1.165
	(1.607)	(2.567)	(1.874)	(2.780)	(0.769)	(1.184)
Number of counties/observations	91/1092	91/1092	87/1044	87/1044	91/1092	91/1092
5. Drop observations where SMSA birth rates are used to approximate county level birth rates.	4.297**	7.317**	4.886*	7.939**	-0.020	-0.278
	(1.738)	(2.662)	(1.914)	(2.739)	(0.782)	(1.214)
Number of counties/observations	104/1151	104/1151	104/1147	104/1147	104/1151	104/1151
6. Drop observations where SMSA birth rates are used to approximate county birth rates & that do not have six pre- and post-desegregation observations.	3.399*	5.630**	3.793*	5.572*	-0.612	-0.986
	(1.796)	(2.880)	(2.084)	(3.138)	(0.875)	(1.314)
Number of counties/observations	75/900	75/900	72/864	72/864	75/900	75/900
7. Control for other policy changes[2]	4.205**	7.145**	4.885**	7.677*	-0.917	-0.070
	(1.620)	(2.567)	(1.824)	(2.715)	(0.772)	(1.220)
Number of counties/observations	105/2941	105/2941	103/2527	103/2527	105/2941	105/2941
8. Control for other policy changes[2] & limit sample to pre-1979 observations	4.099**	6.774**	4.182**	6.445**	0.633	1.074
	(1.782)	(2.941)	(1.960)	(3.062)	(0.707)	(1.274)
Number of counties/observations	104/1024	104/1024	102/1002	102/1002	104/1024	104/1024
9. Estimation weighted by 15-19 female population	2.641	3.604*	2.598	3.589*	0.106	0.576
	(1.930)	(2.153)	(1.901)	(2.118)	(0.517)	(0.708)
Number of counties/observations	105/2941	105/2941	103/2527	103/2527	105/2941	105/2941

Each estimate from a separate regression that includes region-by-year fixed effects, district fixed effects, and district trends. Figures in parentheses are standards errors robust to county level clustering. *statistically significant at the 0.10; ** statistically significant at 0.05.

1. Using observations from 1968-80 for the counties that did not desegregate.

2. Including controls for access to abortion, access to oral contraceptives, distance to nearest abortion provider, and maximum state AFDC grant.

Table 5: Placebo Tests

	Non-White Age 15-19 Females		Whites Age 15-19 Females	
	Model 1	Model 2	Model 1	Model 2
Placebo[1]	-0.063	0.511	0.879	0.354
	(2.355)	(2.827)	(0.985)	(0.790)
Number of Districts	103	103	103	103
Number of Observations	742	742	742	742
	Non-White Age 25-34 Females		**Whites Age 25-34 Females**	
	Model 1	Model 2	Model 1	Model 2
Desegregation Effect (1 to 3 Years after)	1.317 (1.540)	1.333 (1.490)	0.296 (0.762)	-0.210 (0.728)
Desegregation Effect (4 and more year after)	-0.147 (2.681)	-0.206 (2.046)	0.204 (1.302)	-0.061 (1.058)
Number of Counties	105	105	105	105
Number of Observations	1199	1199	1199	1199
Controls for:				
Region-by-year effects	yes	Yes	yes	Yes
County fixed effects	yes	yes	yes	yes
County trends	no	Yes	no	Yes

Figures in parentheses are robust standard errors. None of the estimates presented are statistically distinguishable from 0.

1. Estimated using sample consisting of nine years prior to desegregation. Placebo defined as being 1, 2, or 3 years before desegregation.

Table 6: Estimated Effect of Desegregation on County and SMSA Population, By Race

	Non-White		Black		White	
	Population of Females 15-19 (ln) (County)	Population of Females 15-19 (ln) (SMSA)	Population of Females 15-19 (ln) (County)	Population of Females 15-19 (ln) (SMSA)	Population of Females 15-19 (ln) (County)	Population of Females 15-19 (ln) (SMSA)
Desegregation Effect	-0.0053	-0.0041	-0.0060	-0.0048	-0.0058**	-0.0036*
(1 to 3 Years after)	(0.0040)	(0.0032)	(0.0041)	(0.0033)	(0.0025)	(0.0021)
Desegregation Effect	-0.0033	-0.0049	-0.0034	-0.0055	-0.0027	-0.0023
(4 and more year after)	(0.0047)	(0.0038)	(0.0048)	(0.0040)	(0.0035)	(0.0026)
Controls for:						
Region-by-year effects	yes	yes	yes	yes	yes	Yes
County/SMSA fixed effects	yes	yes	yes	yes	yes	yes
County/SMSA trends	yes	yes	yes	yes	yes	yes
Number of Counties	105		103		105	
Number of SMSAs		87		86		87
Number of Observations	1199	1102	1069	972	1199	1102

Estimated using sample consisting of six years prior to desegregation and 6 years following desegregation for all districts the desegregated before 1988. Robust standard errors in parentheses. **statistically significant at 0.05 level, * statistically significant at 0.10 level

Table 7: Estimated Effects of Desegregation on SMSA Teenage Fertility Rates

	NonWhite		Black		White	
	Primary Estimates	Birth Rates in SMSA	Primary Estimates	Birth Rates in SMSA	Primary Estimates	Birth Rates in SMSA
Desegregation Effect	4.420**	3.835**	5.003**	3.815**	0.040	-0.270
(1 to 3 Years after)	(1.704)	(1.492)	(1.895)	(1.745)	(0.768)	(0.655)
Desegregation Effect	7.459**	6.311**	7.998**	5.257**	-0.064	-0.374
(4 and more year after)	(2.596)	(2.420)	(2.716)	(2.464)	(1.225)	(1.007)
Controls for:						
Region-by-year effects	yes	yes	yes	yes	yes	Yes
County/SMSA fixed effects	yes	yes	yes	yes	yes	yes
County/SMSA trends	yes	yes	yes	yes	yes	Yes
Number of Counties	105		103		105	
Number of SMSAs		87		86		87
Number of Observations	1199	1102	1177	972	1199	1102

Estimated using sample consisting of six years prior to desegregation and 6 years following desegregation for all districts the desegregated before 1988. Robust standard errors in parentheses. *** statistically significant at the 0.01 level; **statistically significant at 0.05 level.

Table 8: Socioeconomic Status (Percentile) of Families by Proportion of School the Respondent Attended Reported Black, Black Sub-Sample

	Few or less	Half	Most	All
1st Grade	41.3	34.2	32	35.6
6th Grade	41.3	35.6	32.5	34.2
9th Grade	38.6	37.2	33.2	32.7

Source: High School and Beyond Data Set, 1980 Sophomore Sub-Sample. All results retrieved using DAS Online Extraction Tool

Table 9: Analysis of Differences from Previous Study

	Non-Whites Effect of Desegregation		Blacks Effect of Desegregation	
	1-3 Years After	4-6 Years After	1-3 Years After	4-6 Years After
1. Preferred Estimates (from Table 3)	4.420**	7.459**	5.003**	7.998**
	(1.704)	(2.596)	(1.895)	(2.716)
2. Estimation weighted by 15-19 year old population	2.641	3.604*	2.598	3.589*
	(1.930)	(2.153)	(1.901)	(2.118)
3. Remove control for trends	1.683	2.429	1.747	2.461
	(2.191)	(3.753)	(2.202)	(3.844)
4. Sample limited to 1970-1980, and only count counties that desegregated during 1970s as treatments	0.112	-0.560	0.088	-0.226
	(2.135)	(3.783)	(2.169)	(3.889)
5. Limit sample to 1970 & 1980, and only count counties that desegregated during 1970s as treatments	-3.957	-6.264	-2.957	-5.915
	(5.515)	(5.163)	(4.683)	(5.220)

Each estimate from a separation regression that includes region-by-year fixed effects and county fixed effects. Rows 1 and 2 also includes county specific trends. Estimates in rows 2 through 5 are weighted by population of females age 15-19 years old. Figures in parentheses are standard errors robust to county level clustering. *statistically significant at 0.10; ** statistically significant at 0.05.

APPENDIX A: SAMPLE OF DISTRICTS, COUNTIES, AND SMSAS USED IN THE STUDY

St.	County Name	1960 SMSA Name	School District	Desegregation Year
AL	Jefferson	Birmingham, AL	Birmingham	1970
AL	Jefferson	Birmingham, AL	Jefferson County*	1971
AL	Mobile[c]	Mobile, AL	Mobile	1971
AR	Pulaski	Little Rock-North Little Rock, AR	Little Rock	1971
AZ	Maricopa	Phoenix, AZ	Mesa	
AZ	Pima	Tucson, AZ	Tucson	1978
CA	Alameda	San Francisco-Oakland, CA	Fremont*	
CA	Alameda	San Francisco-Oakland, CA	Hayward*	
CA	Alameda	San Francisco-Oakland, CA	Oakland	1966
CA	Alameda	San Francisco-Oakland, CA	San Lorenzo*	
CA	Contra Costa	San Francisco-Oakland, CA	Richmond	1969
CA	Fresno	Fresno, CA	Fresno	1978
CA	Los Angeles	Los Angeles-Long Beach, CA	Compton*	
CA	Los Angeles	Los Angeles-Long Beach, CA	Long Beach*	1980
CA	Los Angeles	Los Angeles-Long Beach, CA	Los Angeles	1978
CA	Los Angeles	Los Angeles-Long Beach, CA	Norwalk-La Mirada*	
CA	Los Angeles	Los Angeles-Long Beach, CA	Pasadena*	1970
CA	Sacramento	Sacramento, CA	Sacramento	1976
CA	San Bernardino	San Bernardino-Riverside-Ontrario, CA	San Bernardino	1978
CA	San Diego	San Diego, CA	San Diego	1977
CA	San Francisco[c]	San Francisco-Oakland, CA	San Francisco	1971
CA	Santa Clara	San Jose, CA	San Jose	1986**
CA	Santa Clara	San Jose, CA	Santa Clara*	
CA	Solano	San Francisco-Oakland, CA	Vallejo	1975
CA	Stanislaus		Modesto	
CO	Denver[c]	Denver, CO	Denver	1974
CO	Pueblo	Pueblo, CO	Pueblo	
CT	Fairfield	Bridgeport-Stamford-Norwalk, CT	Stamford	1970
CT	Hartford	Hartford-New Britain-Bristol, CT	Hartford	1966
DE	New Castle	Wilmington County, DE-NJ	Wilmington County (Wilmington)	1978
FL	Brevard[c]		Brevard County (Melbourne)	1969
FL	Broward[c]	Fort Lauderdale-Hollywood, FL	Broward County (Fort Lauderdale)	1970
FL	Dade[c]	Miami, FL	Dade County (Miami)	1970
FL	Duval[c]	Jacksonville, FL	Duval County (Jacksonville)	1971
FL	Hillsborough[c]	Tampa-St. Petersburg, FL	Hillsborough County (Tampa)	1971
FL	Lee[c]		Lee County (Fort Meyers)	1969

FL	Orange[c]	Orlando, FL	Orange County (Orlando)	1972
FL	Palm Beach[c]	West Palm Beach, FL	Palm Beach County (West Palm Beach)	1970
FL	Pinellas[c]	Tampa-St. Petersburg, FL	Pinellas County (St. Petersburg)	1970
FL	Polk[c]		Polk County (Lakeland)	1969
FL	Volusia[c]		Volusia County (Daytona)	1969
GA	Dougherty[c]	Albany, GA	Dougherty County (Albany)	1980
GA	Fulton	Atlanta, GA	Atlanta	1973
GA	Muscogee[c]	Columbus, GA-AL	Muscogee County (Columbus)	1971
IL	Cook	Chicago, IL	Chicago	1982
IL	St. Clair	St. Louis, MO-IL	East St. Louis	
IL	Winnebago	Rockford, IL	Rockford	1973
IN	Allen	Fort Wayne, IN	Fort Wayne	1971
IN	Lake	Gary-Hammond-East Chicago, IN	Gary	
IN	Marion	Indianapolis, IN	Indianapolis	1973
IN	St. Joseph	South Bend, IN	South Bend	1981
KS	Sedgwick	Wichita, KS	Wichita	1971
KS	Wyandotte	Kansas City, MO-KS	Kansas City	1977
KY	Fayette[c]	Lexington, KY	Fayette County (Lexington)	1972
KY	Jefferson	Louisville, KY-IN	Jefferson County (Louisville)	1975
LA	Caddo[c]	Shreveport, LA	Caddo Parish (Shreveport)	1969
LA	Calcasieu[c]	Lake Charles, LA	Calcasieu Parish (Lake Charles)	1969
LA	East Baton Rouge[c]	Baton Rouge, LA	East Baton Rouge Parish	1970
LA	Jefferson[c]	New Orleans, LA	Jefferson Parish	1971
LA	Orleans[c]	New Orleans, LA	New Orleans Parish	1961
LA	Rapides[c]		Rapides Parish (Alexandria)	1969
LA	Terrebonne[c]		Terrebonne Parish	1969
MA	Bristol	Fall River-New Bedford, MA	New Bedford	1976
MA	Hampden	Springfield-Holyoke, MA	Springfield	1974
MA	Suffolk	Boston-Lowell-Lawrence, MA	Boston	1974
MD	Baltimore City[c]	Baltimore, MD	Baltimore	1974
MD	Harford[c]		Harford County	1965
MD	Prince George's[c]	Washington, DC-MD-VA	Prince George’s County	1973
MI	Ingham	Lansing, MI	Lansing	1972
MI	Kent	Grand Rapids, MI	Grand Rapids	1968
MI	Saginaw	Saginaw, MI	Saginaw	

MI	Wayne	Detroit, MI	Detroit	1975
MN	Hennepin	Minneapolis-St. Paul, MN	Minneapolis	1974
MO	Jackson	Kansas City, MO-KS	Kansas City	1977
MO	St. Louis City[c]	St. Louis, MO-IL	St. Louis	1980
NC	Cumberland[c]		Fayetteville/Cumberland County	1969
NC	Gaston[c]		Gaston County (Gastonia)	1970
NC	Mecklenburg[c]	Charlotte, NC	Mecklenburg County (Charlotte)	1970
NC	New Hanover[c]		New Hanover County (Wilmington)	1969
NE	Douglas	Omaha, NE-IA	Omaha	1976
NJ	Essex	Newark, NJ	Newark	1961
NJ	Hudson	Jersey City, NJ	Jersey City	1976
NM	Bernalillo	Albuquerque, NM	Albuquerque	
NM	Danna Ana		Las Cruces	
NY	Erie	Buffalo, NY	Buffalo	1976**
NY	Monroe	Rochester, NY	Rochester	1970
NY	New York[c]	New York, NY	New York	
NY	Westchester	New York, NY	Yonkers	1986***
NV	Clark[c]	Las Vegas, NV	Clark County (Las Vegas)	1972
OH	Cuyahoga	Cleveland, OH	Cleveland	1979
OH	Franklin	Columbus, OH	Columbus	1979
OH	Hamilton	Cincinnati, OH-KY	Cincinnati	1973
OH	Lorain	Lorain–Elyria, OH	Lorain	
OH	Lucas	Toledo, OH	Toledo	1980
OH	Montgomery	Dayton, OH	Dayton	1976
OH	Summit	Akron, OH	Akron	1977
OK	Comanche	Lawton, OK	Lawton	1973
OK	Oklahoma	Oklahoma City, OK	Oklahoma City	1972
OK	Tulsa	Tulsa, OK	Tulsa	1971
OR	Multnomah	Portland, OR-WA	Portland	1974
PA	Allegheny	Pittsburgh, PA	Pittsburgh	1980
PA	Philadelphia[c]	Philadelphia, PA-NJ	Philadelphia	1978
SC	Charleston[c]	Charleston, SC	Charleston	1970
SC	Greenville[c]	Greenville, SC	Greenville County	1970
SC	Richland	Columbia, SC	Richland County	1970
TN	Davidson[c]	Nashville, TN	Nashville	1971
TN	Shelby	Memphis, TN	Memphis	1973
TX	Bexar	San Antonio, TX	San Antonio	1969
TX	Dallas	Dallas, TX	Dallas	1971
TX	Ector[c]	Odessa, TX	Odessa	1982
TX	El Paso	El Paso, TX	El Paso	1978
TX	Harris	Houston, TX	Houston	1971
TX	Lubbock	Lubbock, TX	Lubbock	1978
TX	McLennan	Waco, TX	Waco	1973

TX	Potter	Amarillo, TX	Amarillo	1972
TX	Tarrant	Fort Worth, TX	Fort Worth	1973
TX	Travis	Austin, TX	Austin	1980
VA	Arlington[c]	Washington, DC-MD-VA	Arlington County	1971
VA	Norfolk City[c]	Norfolk-Portsmouth, VA	Norfolk	1970
VA	Pittsylvania[c]		Pittsylvania County	1969
VA	Roanoke City[c]	Roanoke, VA	Roanoke	1970
WA	King	Seattle, WA	Seattle	1978
WA	Pierce	Tacoma, WA	Tacoma	1968
WI	Milwaukee	Milwaukee, WI	Milwaukee	1976
WV	Raleigh[c]		Raleigh County (Beckley)	1973