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Blacks, Whites, and Brown: Effects on the Earnings of Men and Their Sons

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Abstract

By combining difference estimators that capture racial and regional variation with intergenerational linkages in the Panel Study of Income Dynamics, we calculate the effects of *Brown v. Board of Education* on earnings across two generations of men. The longer a black man was exposed to post-*Brown* primary and secondary schools, the higher his earnings. *Brown* especially affected black men living in states that showed greater openness to later desegregation efforts. We speculate that these positive earnings effects reflect a hope for future change, both in schools and the workplace, which provided greater incentive to acquire human capital. We find weak evidence that *Brown* also increased earnings for a second generation of black men. This may indicate relaxed credit constraints due to larger earnings for the first generation. Although workplace-reform legislation such as the Civil Rights Act of 1964 certainly affected black earnings, particularly in the South, the evidence presented here suggests that *Brown* itself mattered as well.

Fifty years ago, the landmark cases of *Brown v. Board of Education* forged legal history.[1] The Supreme Court repudiated the "separate but equal" doctrine put forth in *Plessy v. Ferguson*[2] and required school districts to move "with all deliberate speed" to end racial discrimination. Because districts emphasized the "deliberate" at the expense of the "speed," *Brown* did very little to integrate schools. Economists therefore have viewed the cases as largely symbolic and focused instead upon the effect of school integration, which did not begin in earnest until a decade or more after *Brown*. We examine the impact of *Brown* itself. By using intergenerational linkages in the Panel Study of Income Dynamics (PSID), we estimate effects of the decision across two generations of black men.

We find that earnings were higher the longer a black man was exposed to post-*Brown* primary and secondary schools. We also discover that *Brown* particularly affected black men who lived in states that showed greater openness to desegregation efforts in the 1970s and 1980s. These findings are somewhat surprising given that *Brown* itself had virtually no impact on integration. We speculate that these positive earnings effects reflect a hope for future change, both in schools and the workplace, which provided greater incentive to acquire human capital. Although some of our estimators may echo later workplace legislation as well as *Brown*, our measures using cross-state variation suggest that *Brown* itself still mattered. We further find evidence, albeit weak, that *Brown* increased earnings for a second generation of black men – the sons of post-*Brown* fathers. This may indicate relaxed credit constraints due to the larger earnings for the first generation.

I. Related Research

Most existing economic studies have approached the question of school desegregation by estimating the impact of realized integration on black achievement. For instance, Crain and Strauss (1985) compare the occupations and educational attainment of blacks who did and did not choose to participate in an integration program in Hartford, Connecticut.[3] They find that students educated in an integrated environment exhibit higher educational attainment and a greater propensity to work in predominantly white-collar occupations. Similarly, Boozer et al. (1992) report lower educational attainment, diminished wages, and a higher proportion of minority coworkers among blacks who attended high schools with proportionally larger black populations. Hanushek et al. (2004) use panel data from Texas to show that a greater percentage of black schoolmates has a strong adverse effect on the achievements of black students, particularly those with above-average ability. Guryan (2004) also finds a positive relationship between realized integration and educational attainment, reporting that dropout rates among blacks fell faster in school districts implementing integration plans in the 1970s than in non-integrating districts. Interestingly, Guryan finds no evidence that the length of exposure to integrated schooling matters.

While important, these studies reflect the experiences of only a subset of the black population—those schooled in integrating districts. Due to racial segregation in housing, particularly in the South, many blacks lived in districts so racially homogeneous as to rule out meaningful integration. Even where district-wide integration was possible, effective desegregation plans simply did not appear before the late 1960s.[4] A more representative assessment of integration's effects might include the experience of the many blacks in districts that did not have desegregation programs. Ashenfelter et al. (2006) take an approach that allows them to investigate this possibility. Like others, they focus on a time period when actual integration was occurring. But they use variation in earnings and graduation rates across cohorts in the 1990 IPUMS to capture the impact of integration on the entire population. As integration filtered through Southern schools during the 1960s and 1970s, these authors track a larger increase in earnings and graduation rates in the South than in the North. Like Guryan (2004), Ashenfelter et al. do not find evidence that the length of exposure to integration had any additional effect.

The data used in all the studies above contain no information on the experiences of blacks educated before *Brown* and so cannot assess the impact of the decision itself. Moreover, the cross-sectional nature of these data prohibits any intergenerational analysis. Thus, previous research has been unable to test whether any gains won by black Americans in the mid-twentieth century extend beyond one generation. The present study explores these questions.

II. Economic Theory and *Brown*

First-Generation Effects

Brown may have raised future earnings of black men in the education system at the time of the decision via two possible channels: resources and expectations. Consider the first: *Brown* could have improved the quality of schooling for blacks by increasing educational access. But if "access" refers to "racial integration," *Brown* appears to have failed. The fraction of blacks educated in segregated schools remained largely unchanged for at least a decade after the decision (Orfield 1983).

A broader definition of "access" includes the resources available to black schools, including state funding for smaller class sizes, longer school years, and the like. But a closer examination of the data raises doubts about the connection between *Brown* and school funding. Lawsuits brought by the NAACP beginning in the 1930s resulted in substantial gains among black schools.[5] For instance, Boozer et al. (1992) report that between 1915 and 1954 the racial gap in the pupil-teacher ratio fell by nearly 20 percentage points to around 5 percent. Margo (1990) finds that reductions of racial disparities in pupil-teacher ratios are mirrored in a host of school-quality measures during the 1930s and 1940s.[6] Data on basic educational achievement suggest that these improvements had an effect: the racial literacy gap fell[7] and disparities in K-12 enrollment all but disappeared.[8] Following *Brown,* school quality for blacks continued to improve, but at a much slower rate. Although *Brown* may have enhanced some aspect of unobserved school quality, the data suggest little reason to doubt the conventional claim that the decision had minimal institutional effect.

Yet even if *Brown* had only symbolic value, it might still have led to greater educational achievement among blacks if they perceived it as signaling future changes.[9] As novelist Ralph Ellison exclaimed in a letter to a friend on 19 May 1954, “What a wonderful world of possibilities are unfolded for the children!”[10] This is the second channel through which the Court’s ruling could have operated: perhaps blacks correctly understood that *Brown* was the first major step in dismantling institutional barriers across the economy (including integration of colleges, the Civil Rights Act of 1964, the Voting Rights Act of 1965, the Elementary and Secondary Education Act of 1965, the Fair Housing Act of 1968, and so forth).[11] Referring to his belief that change would soon follow *Brown*, plaintiff’s lawyer Thurgood Marshall speculated it might take “up to five years” to eliminate school segregation completely and an additional five to eradicate segregation “in all forms.”[12] The panicked reaction of whites to

Brown, particularly in the South, likely reinforced the perception of blacks that something momentous had occurred.[13]

If *Brown* operated through expectations in this manner, black schoolchildren and their parents would have perceived a greater payoff to good schoolwork and to longer time spent in the classroom. Likewise, teachers and administrators in black schools might have modified the curriculum, as they could reasonably have expected that their charges would have access to a wider variety of occupations. Crain (1969) and Weiner (2007), for example, document the efforts of black parents in the latter half of the 1950s to upgrade the quality of the curriculum in black schools. By kick-starting adult literacy programs such as the Citizenship Schools (begun in 1958), *Brown* may have transformed the home environment even further as parents recognized the benefits of education first-hand.[14] Even if the funding and racial composition of schools remained unchanged after *Brown*, altered expectations about the value of education in the workplace could have led to greater investments in human capital by black children and thus higher earnings for them as adults.[15]

The court's landmark decision did not stand alone, of course. Not only did a series of actions lead up to the case, but the civil rights movement also pushed for a wide array of reforms soon after *Brown* was decided. This effort culminated with the Civil Rights Act of 1964 (CRA64). The effects of CRA64 – particularly Title VII on workplace discrimination – are the focus of much research based on repeated cross-sectional data. Certainly, these legislative efforts may have affected relative earnings for blacks, particularly in the South. (See for example Smith and Welch 1986, Butler et al. 1989, Heckman and Payner 1989, Donohue and Heckman 1991, Card and Krueger 1991, Chay and Honoré 1998, and Vigdor 2006.) One challenge for our work is to disentangle the *Brown* effect on earnings from the impact of later workplace-reform legislation.

Second-Generation Effects

The children of blacks educated after *Brown* may have reaped additional benefits as a result of their parents' advancements. Becker and Tomes (1986) present a model of intergenerational human capital acquisition emphasizing the possible role of credit constraints. These authors show that, in a world with perfectly functioning credit markets, the level of human capital purchased for a child is a function of the return to education alone. In particular, parental income does not matter. However, when access to credit is limited, parental income becomes important because families with higher incomes are able to self-finance a greater level of human capital investment.

That credit access constrained the educational choices of blacks around the time of *Brown* seems very likely.[16] Earnings in black families were low due to previous educational inequalities and present workplace discrimination. And credit was limited to the private sector, which often offered discriminatory rates. (The Guaranteed Student Loan Program was not created until 1965.) In this context it seems plausible that any gains in earnings resulting from *Brown* might have precipitated a second generation of effects by increasing blacks' ability to self-finance educational investments.

III. Data and Regression Methods

The PSID Sample

Using data drawn from several waves of the PSID, we select male heads of household such that some were educated before the *Brown* decision and others were educated after. Of particular interest to us is the experience of black men. Because blacks make up a relatively small percentage of the representative portion of the PSID sample (the Survey Research Center component), we also include individuals from the Survey of Economic Opportunity component.[17] The latter source over-samples poor households, a disproportionate fraction of which are black. To avoid the complications associated with female labor supply, we focus only on the outcomes of men.

Although the PSID lacks the large sample size of the IPUMS data used by Ashenfelter et al. (2006), it does contain repeated observations. This feature allows us to mitigate the effects of measurement error that result from transitory earnings. In the first-generation analysis, log earnings are observed each year from 1975 to 1984 and then averaged. To further reduce the importance of unrepresentative earnings observations we require annual earnings to exceed $200.[18] (All dollar figures are in year-2002 dollars.) The second-generation analysis studies an analogous measure of average log earnings based on the seven PSID waves available between 1994 and 2003. In both cases, we exclude men with incomplete earnings histories from the sample.[19] This restriction avoids variation in measurement error across observations that could result when earnings data are missing at different points in the business cycle or the individual's lifecycle.

We impose age restrictions, requiring men in the first-generation analysis to be at least 25 years old in 1975 and no older than 60 years old in 1984. These restrictions mitigate sampling bias based on education or retirement decisions. Similarly, in the second-generation analysis, the men are no younger than 25 years old in 1993. Because we match these men to their fathers in 1972, we require that they be no older than 18 years in 1972 (that is, 48 years old in 2002). The "fathers" in the PSID sample are actually male heads of household rather than biological fathers. To reduce the possibility of drawing a household with a grandfather rather than a father as head, we eliminate those households with heads whose age exceeds 78 years in 1972 (that is, those older than age 60 at the date of the oldest possible son's birth).

Accounting for Brown

In our primary analysis, two variables record the individual's (or, in the case of the second-generation analysis, the individual's father's) experience vis-à-vis *Brown.* The first variable simply codes whether the individual was age 17 or younger at the time of the 1954 *Brown* ruling, which means he was exposed to at least one year of potential post-*Brown* secondary-school experience.[20] This discrete treatment is motivated by the results of both Ashenfelter et al. (2006) and Guryan (2004), who find that the length of exposure to integration in the 1960s and 1970s does not appear to alter earnings and educational achievement. To test whether the impact of *Brown* increased with years of exposure, we create a second variable that measures the years of grades 1-12 education remaining for the individual after the date of the decision. Controls for age and age-squared capture life-cycle- earnings effects.

Of course, exposure to *Brown* is correlated with exposure to many subsequent civil rights developments. While controlling for every possible event and policy innovation would lead to perfect multicollinearity, the prominence of the Civil Rights Act of 1964 (CRA64) demands particular attention. Recall that we observe earnings for all men in common years – 1975 to

1984 – for birth cohorts very close in age (see Table 1 below). Yet we cannot rule out the possibility that workplace legislation affected some cohorts more than others. If labor-market conditions at the time of initial entrance into the labor force mark lifetime earnings for years to come, for example, our *Brown* variables may partly capture the impact of CRA64.

We address this concern in two ways for the first generation of men. (Sample sizes are too small to explore second-generation effects.) First, we construct CRA64-exposure measures analogous to those reflecting exposure to *Brown.* Including these variables in regressions allows us to estimate the effects of *Brown* conditional on exposure to CRA64.

Following Ashenfelter et al. (2006), in our second approach we divide the men into five birth cohorts of roughly equal sample size: 1924-30, 1931-36, 1937-1942, 1943-46, and 1947-1950. The first two cohorts graduated from high school in or before 1954 and so experienced no education under *Brown*. The high school graduation year of individuals in the final three cohorts is 1955 or later, with the final group graduating in 1965 or later (post-CRA64). If the civil rights movement gradually improved conditions over the entire time period, we would expect to see a steady improvement across cohorts with no discernible break in trend. By contrast, a positive effect of *Brown* should be visible in the third cohort and beyond, whereas the impact of CRA64 would be seen only in the final cohort.

Difference Estimators

We construct several difference estimators to measure the influence of *Brown*. The first is a simple difference (D) estimator that identifies the effect of *Brown* through earnings differences found between pre- and post-*Brown* blacks. A cohort bias may appear in this estimator, however. If labor-market conditions at the time of entrance have a long-term influence upon earnings, for instance, then D may pick up these cohort effects twenty years after the fact and attribute them to *Brown*. (See Smith and Welch 1977, 1984, and 1989.) Alternatively, this estimator may capture changes in the black age-earnings profile over time.

More complex estimators reduce these potential biases. In their study of integration effects, Ashenfelter et al. (2006) use the experience of non-Southern blacks as a control in a regional difference-in-difference (DD) estimator. They also suggest, although they do not formally craft, a difference-in-difference-in-difference (DDD) estimator combining racial variation with regional differences between Southern and non-Southern states. We operationalize these estimators using information on "the state in which the head grew up" as recorded in the 1972 wave of the PSID.[21] Assuming the reported state is the one in which relevant education took place, our estimates will not be affected by the migration of blacks, which is well-documented in Vigdor (2006).

We have reservations concerning estimates based on simple regional variation. First, these estimators rest on the assumption that *Brown* did not affect non-Southern blacks. Yet, although Northern schools did not follow *de jure* segregation, districts practicing *de facto* segregation may nevertheless have responded to the ruling. In fact, separating states into "South" and "non-South" is itself a challenge: *Brown* arose in Kansas, technically a "non-South" state. Moreover, if the effects of *Brown* worked through the anticipation of broader change in education and labor markets, then the ruling could matter to all blacks regardless of region. Finally, regional variation in economic industrial structure may bias these difference estimators. According to Bureau of Economic Analysis data, for instance, the South was far more dependent upon agricultural and government sectors, whereas the North was much more heavily influenced by manufacturing.[22] For these reasons, we also estimate the effect of *Brown* on all blacks regardless of region, using whites as a control group—a race DD estimator.

Although we can calculate all these estimators for the first-generation men, limitations of the PSID make it impossible to generate region DD and DDD estimators for the second generation because the sample includes too few non-Southern blacks. In the first-generation analysis, our data include eight pre- and thirteen post-*Brown* observations—enough to create region-based estimates, but too few to place great confidence in them.[23] In the second-generation analysis, three pre- and five post-*Brown* observations are available. Consequently, we report estimates for the region-based difference estimators only for the first-generation regressions.

The concerns and limitations associated with the region DD and DDD estimators led us to search for an alternative geographic variable. A more nuanced measure could potentially capture variations in social environment or political atmosphere across states, signaling the strength of segregation policy—either *de jure* or *de facto*. One possible measure is the Dissimilarity Index (DI). A continuous variable, the DI measures the degree to which school-specific race compositions deviate from district averages.[24] Comprehensive DI data only cover the late 1960s and early 1980s. This is obviously too late to capture "access" effects due to *Brown*. However, if the impact of *Brown* operated through expectations and if DI changes from 1968 to 1984 proxy for the extent of other civil rights reforms during that time, then reductions in the DI may parallel expectations for change following *Brown*. If people correctly anticipated the degree of progress in schools and labor markets that would occur in their state, their actions concerning human capital investments would reflect these expectations. In turn, incremental human capital investments would show up in increased earnings. We therefore construct additional region DD and DDD estimators, replacing the Southern dummy variable with the reduction in the DI. Appendix 1 provides detailed specifications of all the estimators.

Table 1 offers summary statistics by race. The upper panel summarizes data used in the first-generation regressions. The lower panel reports corresponding summaries for data used in the second-generation analysis. Even in the raw data a possible *Brown* effect appears. Note that, in both the first- and second-generation samples, the post-*Brown* whites earn less than their pre-decision counterparts. This is unsurprising given the age difference between the two groups. Among blacks, however, post-*Brown* earnings are actually higher despite a similar age gap.

IV. Estimates of the Effect of *Brown*

Our initial empirical results support the hypothesis that *Brown* increased earnings in the first generation. We also find weaker evidence of earnings gains in the second generation. Unlike Guryan (2005) and Ashenfelter et al. (2006), who uncover no evidence that length of exposure to school integration matters, we find that the earnings gain increases as post-*Brown* exposure lengthens.

Yet difference estimators based on racial and regional variation potentially conflate the *Brown* effect with the influence of later reforms. When we add controls for exposure to CRA64 to the regressions, difference estimators based on variation in the dissimilarity index continue to point to a significant impact of the Court's decision. When we probe these results more deeply using birth cohorts, the timing of earnings gains also indicates a positive role for *Brown*. These findings reinforce our suspicion that a *Brown* effect exists.

First-Generation Effects

Table 2 presents results of regressions with no control for CRA64. The table reports the estimated first-generation log-earnings effect of *Brown* under the four difference estimators D, race DD, region DD, and race-region DDD. The first row of coefficients shows the estimated increase assuming all post-*Brown* students were equally affected regardless of their length of exposure. None is significant.

One reason the discrete *Brown* variable may not significantly predict earnings is that it fails to differentiate those who experienced only one or two years of post-*Brown* education from those whose exposure was much greater. The second row of coefficients shows that, once length of exposure is accounted for, the data divulge a significant earnings increase associated with the Court's ruling. Both DD estimators and the DDD estimator show a statistically significant earnings increase of between 2.7 and 5.3 percent for each year exposed to post-*Brown* primary and secondary schooling.[25]

The relative magnitudes of the estimators are consistent with the discussion of potential biases in the previous section. The D estimate exceeds both DD estimators, indicating that D mistakenly attributes some increase in black earnings over time to *Brown*. In addition, both region-based estimators—the region DD and DDD—produce larger estimates than the race DD estimator, which suggests that the former estimators pick up regional economic convergence occurring during this time period (see, for example, Kim 1998) as well as a *Brown* effect.

Of course, significant difference estimators can result either from a positive effect on the treatment group (blacks in the race DD estimate and southern blacks in the region DD and DDD estimates) or a negative effect on the control. While constant black earnings in the face of falling white earnings might be consistent with a *Brown* effect, the interpretation is perhaps more plausible if we find a *positive* treatment effect. Both the DD and the DDD regressions do indeed estimate a positive impact on the treatment group following the ruling. Appendix 2 contains full regression results.

Guryan (2004) and Ashenfelter et al. (2006) find that the inclusion of discrete controls for integration eliminate any exposure effect: students who experienced even one year of integrated schooling reaped the same rewards as those whose entire school experience was integrated. By contrast, our earnings regressions including both discrete and exposure variables show positive exposure effects in both the regional and race DD models: those who experienced more post-*Brown* years are more positively affected than those who experienced fewer. Appendix 3 reports these outcomes.

Longer time spent in the post-*Brown* environment meant more exposure to post-*Brown* schools, but it also meant exposure to the civil-rights revolution generally and workplace legislation specifically. In a repeated cross-section analysis over the relevant time period, Vigdor (2006) presents evidence of a narrower black-white earnings gap in the South relative to the North for younger cohorts of working-age men. Isolating the *Brown* effect thus requires us to control for subsequent civil-rights reforms and to include a source of variation finer than the simple "North-South" dichotomy. The change in DI across states offers just such a source.

Table 3 includes controls for CRA64 and adds regressions replacing the simple regional dummies with cross-state variation in the DI change. As in Table 2, the first four columns report D, race DD, region, DD, and race-region DDD estimates. With controls for CRA64 included, little evidence for a *Brown* effect is apparent from these regressions. The DI DD and DI DDD estimates shown in the last two columns of Table 3, however, reveal positive and significant effects of *Brown*. That is, although exposure to CRA64 does *not* predict earnings gains for blacks educated in states that subsequently integrated, exposure to *Brown* does. By using the reduction in DI as a proxy for expectations, we find that gains in earnings were especially pronounced for men who lived in states that were ripe for educational reform. This reinforces our suspicion that *Brown* yielded tangible results via its effects upon people's hope for the future. Appendix 4 contains full regression results.

Birth-cohort analysis provides an alternative means of separating the effects of *Brown* from the impact of other civil rights changes that preceded and followed the ruling. The D and race-region estimates are not precise enough to show statistical significance. But, as Table 4 and Figure 1a disclose, many of them exhibit a common pattern: following a setback for the cohort born 1931-36, the 1937-42 cohort—the first group of men exposed to *Brown*—begins a turnaround that continues in the two following cohorts.[26] When we consider changes in the dissimilarity index (final two columns of Table 4 and Figure 1b), a sharper picture emerges. After no discernible change between the 1924-30 and 1931-37 cohorts, both the DI estimators show a significant, positive effect associated with the 1937-42 cohort. The fact that the subsequent cohorts appear to maintain, but not expand upon, this earnings growth suggests that it is *Brown* and not some other civil rights event that explains the DI results of Table 3. Appendix 5 contains full regression results.

In summary, our initial DD and DDD estimators show a positive impact of the *Brown* ruling—each year of post-*Brown* education increases earnings between 2.7 and 5.3 percent. The correlation between exposure to *Brown* and exposure to CRA64 leads to positive but insignificant *Brown* estimates when we include CRA64 controls. This multicollinearity problem can be partially addressed by examining the results across birth cohorts: D, DD, and DDD estimators all show gains to earnings that began with the *Brown* generation. An alternative approach to extricating the impact of *Brown* from effects associated with other civil rights innovations divides states based on their eventual responsiveness to *Brown's* mandates. The earnings of blacks educated in states that ultimately complied with the ruling increased more than those of blacks educated in other states. These results suggest that, where the Court's ruling was taken seriously, it produced positive effects on black earnings.

So how exactly might *Brown* have generated these effects, particularly by its influence on individual behavior? An expectation-driven increase in human capital investment could manifest itself in either more years of schooling, higher-quality investment for a given number of years, or a combination of the two. Table 5 suggests the last is the most compelling explanation. When we include years of education as an explanatory variable in earnings equations, the *Brown* coefficients are about 10 percent smaller but show slightly greater statistical significance. The reduction in point estimates is consistent with a positive covariance between years of education and exposure to *Brown*. The fact that *Brown* exposure remains a significant predictor even after education controls are included points to increased investment in quality as well. Post-*Brown* black men spent more time in school on average than their pre-*Brown* counterparts (see Table 1); they also received a relatively higher return to each year of schooling, despite the fact that most of them remained in segregated schools.

Second-Generation Effects

These first-generation effects raise the possibility that the children of post-*Brown* men may also have enjoyed earnings gains due to a relaxation of the credit constraint. Table 6 examines this possibility. Again, the top and bottom rows of coefficients present results based on the discrete and exposure *Brown* measures, respectively. Due to sample-size limitations, we do not report the region DD and DDD estimators. When we assume *Brown* affects all blacks equally independent of the length of exposure, the simple difference estimator D predicts an earnings gain of roughly 17 percent. Alternatively, D predicts a 3 percent gain per year of father's exposure. These D estimates are not as prone to the cohort bias that likely plagues the same estimator in the first generation. Although racial wage convergence was rapid in the 1960s, it was minimal in the 1980s when the second generation entered the labor force. Moreover, Table 1 documents very little difference in age between men whose fathers were educated pre- and post-*Brown.* Whereas neither the discrete- nor the exposure-based race DD estimate is statistically significant, their magnitudes are close to those of the simple difference estimators. Again, the black "treatment" group experienced a positive effect following *Brown*.

Taken at face value, the size of these second-generation effects is quite large—about 80 percent as large as that of the first-generation effects. At first it may seem that the second-generation effect would have to be smaller than the first-generation counterpart, but closer examination suggests this need not be the case. The education choice of fathers in the first generation may have been constrained by both discrimination and financial constraints. If *Brown* (partially) addressed the former, the effects may have been muted by the latter. Yet the sons of these *Brown* beneficiaries approached the lowered discriminatory hurdles with the benefit of their fathers' greater earnings. The combined effects of reduced discrimination and eased credit constraints might explain why the size of the estimated second-generation effects rivals that of the first-generation impact.

V. Conclusion

Half a century after *Brown v. Board of Education* marked the symbolic end of Jim Crow laws in education, the economic impacts of this watershed ruling remain unclear. While many scholars have studied civil rights reforms generally, we examine the specific effect of *Brown* on earnings.

Our research adds to the literature in several important ways. First, we use the heretofore unexploited Panel Study of Income Dynamics to analyze the effect of *Brown*. These data have the added virtue of multiple years of earnings for each individual. The intergenerational links in the data also allow us to investigate the impact of the case upon two generations. Finally, our work focuses on the effect of changes in law upon the black population as a whole rather than merely upon those blacks who experienced *de facto* integration.

Somewhat surprisingly, we uncover evidence that black men educated after *Brown* earned significantly more than those educated before the decision. The size of the earnings gain is substantial—those whose entire high-school career followed the ruling earned over $3,600 (in 2002 dollars) more than those who passed secondary-school-graduation age before *Brown*. What is more, men living in states that acted relatively quickly to desegregate schools in the 1970s and 1980s enjoyed relatively greater gains in earnings. If these education reforms act as proxies for broader civil rights gains in the labor market, this result suggests that blacks accurately forecasted the degree of change effected in their state. Although our race and region estimators potentially mix together *Brown* effects with earnings gains attributable to later reforms, our analyses using estimates that incorporate cross-state variation – both when we control for the Civil Rights Act of 1964 and when we divide our data into birth cohorts -- indicate *Brown* itself still mattered. We find weak evidence of gains in earnings for the sons of men who experienced post-*Brown* education. If real, these effects may be the result of relaxed credit constraints following the earnings gains of the first generation.

The emphasis on integration found within the existing literature suggests that many scholars have decided that *Brown's* value was rendered, in the words of Derrick Bell, "more symbolic than real" by school boards' resistance to integration.[27] While acknowledging that desegregation did not seriously begin until a decade or more after *Brown* (and remains incomplete to this day), we offer evidence that the symbolism of *Brown* may have raised black earnings by signaling that substantive change would arrive shortly.

The question we cannot answer, however, is how long a symbol can motivate change without subsequent events that fulfill expectations of a paradigm shift. As we see schools re-segregating (Orfield and Lee 2004, Clotfelter 2004, Clotfelter et al. 2006), this may be an important question indeed.[28]

Table 1: Means and Standard Deviations of Regression Variables

	Black		White	
	pre-*Brown*	post-*Brown*	pre-*Brown*	post-*Brown*
	First-generation sample			
Average log earnings	10.41	10.42	10.82	10.68
	(0.11)	(0.07)	(0.03)	(0.02)
Years *Brown* exposure	0.00	7.86	0.00	8.34
	(0.00)	(0.67)	(0.00)	(0.16)
Age	49.44	35.28	49.59	34.92
	(0.56)	(0.73)	(0.19)	(0.17)
Years education	9.72	12.23	12.70	13.58
	(0.58)	(0.30)	(0.16)	(0.12)
Fraction Southern	0.87	0.92	0.26	0.36
	(0.07)	(0.04)	(0.02)	(0.02)
Sample size	107	152	360	544
	Second-generation sample			
Average log earnings	10.30	10.37	10.79	10.66
	(0.07)	(0.08)	(0.04)	(0.06)
Years *Brown* exposure (of father)	0.00	4.87	0.00	4.61
	(0.00)	(0.77)	(0.00)	(0.29)
Age	37.70	34.46	37.11	33.03
	(0.67)	(0.80)	(0.27)	(0.30)
Sample size	51	31	222	98

Note: First-generation statistics are weighted using 1976 individual weights. Second-generation statistics are weighted using 1972 individual weights (for the head of household with whom the son is matched in 1972).

Table 2: First-Generation Effects of *Brown* on Log Earnings

	D	Race DD	Region DD	Region DDD
	Estimates based on discrete *Brown* measure			
Brown effect	0.225	0.134	0.103	0.307
	(0.231)	(0.133)	(0.231)	(0.191)
	Estimates based on exposure *Brown* measure			
Brown effect	0.056	0.027**	0.051*	0.053**
	(0.062)	(0.011)	(0.027)	(0.027)
Sample size	259	1163	259	1163

Note: Standard errors are in parentheses. Regressions are weighted using 1976 individual weights.

** *Significant at the 5% level*
* *Significant at the 10% level*

Table 3: First-Generation Effects of *Brown* on Log Earnings with Controls for CRA64

	D	Race DD	Region DD	Region DDD	DI DD	DI DDD
	Estimates based on discrete *Brown* and CRA64 measures					
Brown effect	0.226	-0.017	-0.018	0.162	1.279**	1.267*
	(0.243)	(0.154)	(0.218)	(0.205)	(0.636)	(0.707)
CRA64 effect	0.003	0.319**	0.547	0.393	-0.192	-0.698
	(0.317)	(0.138)	(0.382)	(0.354)	(0.528)	(0.601)
	Estimates based on exposure *Brown* and CRA64 measures					
Brown effect	0.096	0.018	0.000	0.011	0.174**	0.155*
	(0.106)	(0.019)	(0.035)	(0.037)	(0.081)	(0.093)
CRA64 effect	0.065	0.042	0.289**	0.225	-0.383	-0.515
	(0.163)	(0.068)	(0.136)	(0.145)	(0.311)	(0.326)
Sample size	259	1163	259	1163	259	1163

Note: Standard errors are in parentheses. Regressions are weighted using 1976 individual weights.

*** Significant at the 5% level*
** Significant at the 10% level*

Table 4: Effects of *Brown* on First-Generation Log Earnings Using Cohorts

	D	Race DD	Region DD	Region DDD	DI DD	DI DDD
	Change in log earnings relative to previous cohort					
Birth cohort 1931-36	-0.363*	-0.354*	-0.326	-0.356	-.000	0.224
	(0.196)	(0.204)	(0.232)	(0.263)	(0.957)	(0.997)
Birth cohort 1937-42	0.120	0.145	0.240	0.310	1.392*$^{+}$	1.206^{+}
	(0.181)	(0.190)	(0.212)	(0.245)	(0.829)	(0.878)
Birth cohort 1943-46	0.023	0.136	0.232	0.131	-0.467	-0.835
	(0.253)	(0.258)	(0.451)	(0.463)	(0.982)	(1.012)
Birth cohort 1947-50	0.130	0.201	0.353	0.283	0.198	0.081
	(0.238)	(0.243)	(0.539)	(0.545)	(0.951)	(0.981)
Sample size	259	1163	259	1163	259	1163

Note: Standard errors are in parentheses. Regressions are weighted using 1976 individual weights. The omitted birth cohort is 1924-1930.

**Significant at the 10% level*

+ Difference from 1924-1930 cohort significant at the 10% level

Table 5: First-Generation Effects of *Brown* on Log Earnings Conditional on Education

	D	Race DD	Region DD	Region DDD	DI DD	DI DDD
	Estimates based on discrete *Brown* and CRA64 measures					
Years education	0.102***	0.082***	0.102***	0.080***	0.098***	0.081***
	(0.021)	(0.006)	(0.021)	(0.006)	(0.020)	(0.006)
Brown effect	0.003	-0.155	-0.193	0.057	1.079**	1.069*
	(0.208)	(0.140)	(0.206)	(0.188)	(0.493)	(0.548)
CRA64 effect	0.020	0.320**	0.483*	0.350	-0.347	-0.641
	(0.186)	(0.130)	(0.266)	(0.256)	(0.464)	(0.529)
	Estimates based on exposure *Brown* and CRA64 measures					
Years education	0.101***	0.082***	0.100***	0.079***	0.097***	0.080***
	(0.022)	(0.006)	(0.023)	(0.006)	(0.021)	(0.0006)
Brown effect	0.011	0.004	0.001	0.021	0.156**	0.154**
	(0.082)	(0.015)	(0.028)	(0.029)	(0.063)	(0.077)
CRA64 effect	-0.004	0.060	0.164	0.116	-0.456*	-0.544**
	(0.111)	(0.055)	(0.117)	(0.117)	(0.243)	(0.276)
	Change in log earnings relative to previous cohort					
Years education	0.101***	0.081***	0.102***	0.078***	0.097***	0.079***
	(0.022)	(0.006)	(0.022)	(0.006)	(0.021)	(0.006)
Birth cohort 1931-36	-0.261	-0.255	-0.455**	-0.383	0.128	0.172
	(0.170)	(0.175)	(0.201)	(0.229)	(0.662)	(0.743)
Birth cohort 1937-42	-0.124++	-0.025	0.167	0.241	1.094++	1.006+
	(0.143)	(0.172)	(0.188)	(0.223)	(0.667)	(0.735)
Birth cohort 1943-46	-0.060+++	0.097	0.307	0.234	-0.509	-0.771
	(0.180)	(0.189)	(0.318)	(0.357)	(0.708)	(0.772)
Birth cohort 1947-50	0.095	0.219	0.230	0.183	0.135	0.171
	(0.135)	(0.153)	(0.342)	(0.388)	(0.652)	(0.721)
Sample size	259	1163	259	1163	259	1163

Note: Standard errors are in parentheses. Regressions are weighted using 1976 individual weights.
*** Significant at the 5% level*
** Significant at the 10% level*
+ Difference from 1924-1930 cohort significant at the 10% level
++ Difference from 1924-1930 cohort significant at the 5% level
+++ Difference from 1924-1930 cohort significant at the 1% level

Table 6: Second-Generation Effects of *Brown* on Log Earnings

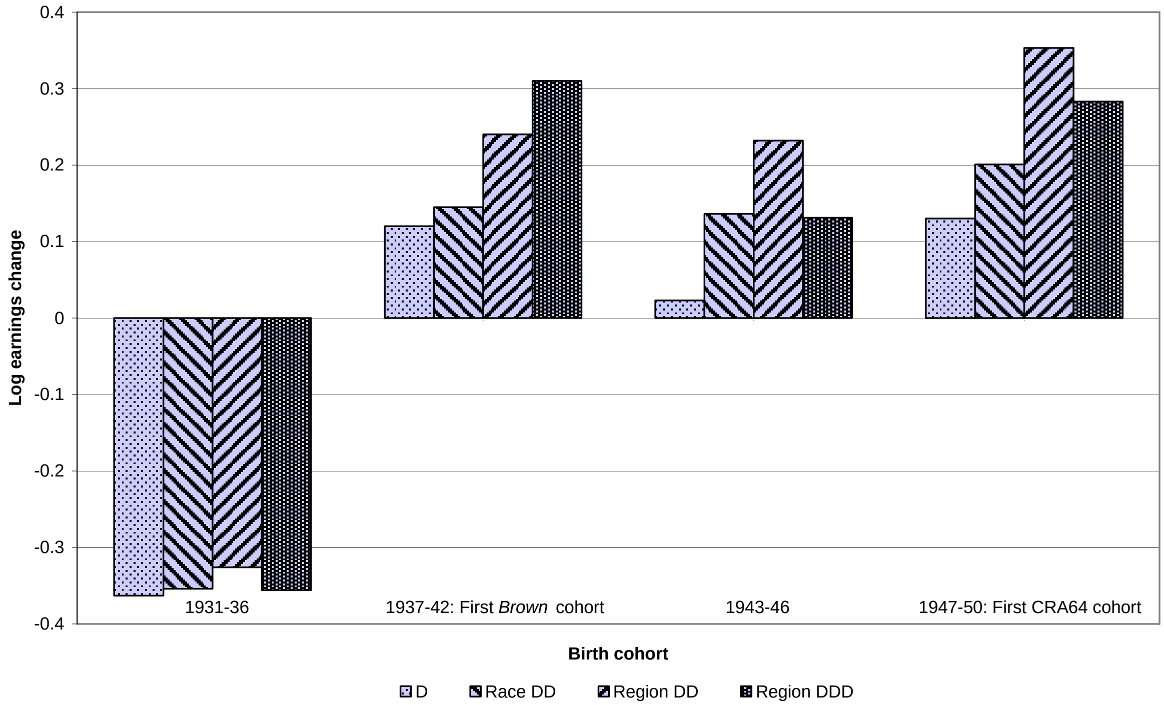
	D	Race DD	DI DD	DI DDD
	Estimates based on discrete *Brown* measure			
Discrete *Brown* effect	0.174*	0.183	0.050	-0.112
	(0.10)	(0.12)	(0.39)	(0.51)
	Estimates based on exposure *Brown* measure			
Exposure *Brown* effect	0.029	0.028	0.024	-0.036
	(0.02)	(0.02)	(0.08)	(0.10)
Sample size	82	402	82	402

Note: Standard errors are in parentheses. Regressions are weighted using 1972 individual weights (for the head of household with whom the son is matched in 1972).

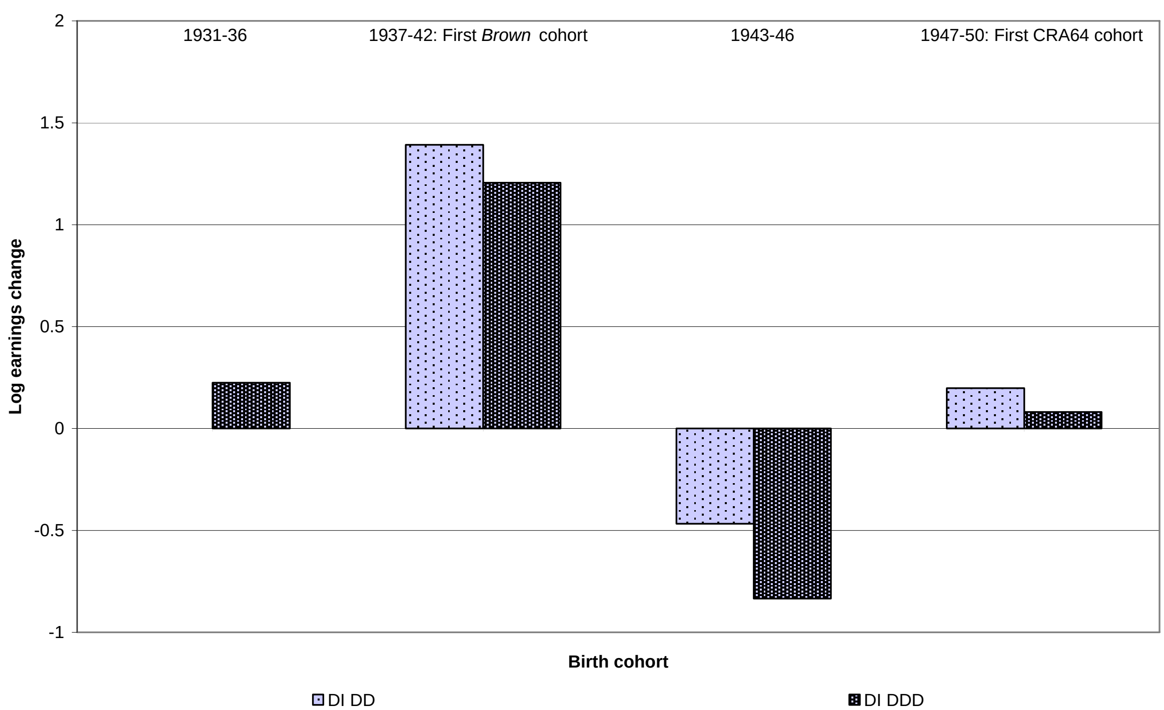
* *Significant at the 10% level*

Figure 1: Change in Log Earnings Relative to Previous Birth Cohort

a) Difference Estimators



b) DI Estimators



Appendix 1: Regression Models

Variable Definitions

y: log earnings, averaged over ten years (seven years in the second-generation case)
a: average age at earnings observation
A: [a a^2]
D: 0 if age >17 in 1954, 1 otherwise (in second-generation regressions, father's age in 1954 is used)
D64: 0 if age >17 in 1964, 1 otherwise
De: number of years of elementary and secondary schooling remaining after *Brown*, minimum=0, maximum=12 (in second-generation regressions, father's exposure is used)
De64: number of years of elementary and secondary schooling remaining after CRA64, minimum=0, maximum=12
C: [C_2 C_3 C_4 C_5] set of birth cohorts, where C_2=1 if born 1931-50 and 0 otherwise, C_3=1 if born 1937-50 and 0 otherwise, C_4=1 if born 1943-50 and 0 otherwise, C_5=1 if born 1947-50 and 0 otherwise; omitted cohort born 1924-30
R: 0 if white, 1 if black
S: 0 if grew up in non-Southern state, 1 otherwise
I: change in the dissimilarity index (DI) in the state of residence (in second-generation regressions, father's childhood state is used)

Regression Models

Brown effect(s) = γ
CRA64 effect = λ

Model 1: Simple Difference Estimator

$$y = \alpha + \boldsymbol{\beta_0}*\mathbf{A} + \gamma*D + \varepsilon$$

or

$$y = \alpha + \boldsymbol{\beta_0}*\mathbf{A} + \gamma*D + \lambda*D64 + \varepsilon$$

or

$$y = \alpha + \beta_0 C_2 + \gamma_1 C_3 + \gamma_2 C_4 + \lambda C_5 + \varepsilon \qquad \text{(A1)}$$

Model 2: Racial Difference-in-Difference Estimators

$$y = \alpha + \boldsymbol{\beta_0}*\mathbf{A} + \beta_1*R + \beta_2*D + \gamma*D*R + \varepsilon$$

or

$$y = \alpha + \boldsymbol{\beta_0}*\mathbf{A} + \beta_1*R + \beta_2*D + \beta_3*D64 + \gamma*D*R + \lambda*D64*R + \varepsilon$$

or

$$y = \alpha + \boldsymbol{\beta_0 * C} + \beta_1 * R + \beta_2 * C_2 * R + \gamma_1 * C_3 * R + \gamma_2 * C_4 * R + \lambda * C_5 * R + \varepsilon \quad \text{(A2)}$$

Model 3: Regional Difference-in-Difference Estimators (Z = S or I)

$$y = \alpha + \boldsymbol{\beta_0 * A} + \beta_1 * Z + \beta_2 * D + \gamma * D * Z + \varepsilon$$

or

$$y = \alpha + \boldsymbol{\beta_0 * A} + \beta_1 * Z + \beta_2 * D + \beta_3 * D64 + \gamma * D * Z + \lambda * D64 * Z + \varepsilon$$

or

$$y = \alpha + \boldsymbol{\beta_0 * C} + \beta_1 * Z + \beta_2 * C_2 * Z + \gamma_1 * C_3 * Z + \gamma_2 * C_4 * Z + \lambda * C_5 * Z + \varepsilon \quad \text{(A3)}$$

Model 4: Difference-in-Difference-in-Difference Estimators (Z = S or I)

$$y = \alpha + \boldsymbol{\beta_0 * A} + \beta_1 * R + \beta_2 * Z + \beta_3 * D + \beta_4 * R * Z + \beta_5 * D * R + \beta_6 * D * Z + \gamma * D * R * Z + \varepsilon$$

or

$$y = \alpha + \boldsymbol{\beta_0 * A} + \beta_1 * R + \beta_2 * Z + \beta_3 * D + \beta_4 * D64 + \beta_5 * R * Z + \beta_6 * D * R + \beta_7 * D * Z + \beta_8 * D64 * R + \beta_9 * D64 * Z + \gamma * D * R * Z + \lambda * D64 * R * Z + \varepsilon$$

or

$$y = \alpha + \boldsymbol{\beta_0 * C} + \beta_1 * R + \beta_2 * Z + \boldsymbol{\beta_3 * C} * R + \boldsymbol{\beta_4 * C} * Z + \beta_5 * R * Z + \beta_6 * C_2 * R * Z + \gamma_1 * C_3 * R * Z + \gamma_2 * C_4 * R * Z + \lambda * C_5 * R * Z + \varepsilon \quad \text{(A4)}$$

The effects of exposure to *Brown* and CRA64 are estimated by replacing the discrete measures D and D64 with the exposure measures De and De64 in the first two equations within each set of regressions.

Appendix 2: Full Regression Results for Regressions Estimating First-Generation Effects of *Brown* on Log Earnings

Independent variable	D	Racial DD	Region DD	Region DDD
		Estimates based on discrete *Brown* measure		
D	0.225	0.012	0.107	-0.004
	(0.231)	(0.081)	(0.294)	(0.085)
R		-0.399***		-0.191***
		(0.110)		(0.039)
DR		0.134		-0.182*
		(0.133)		(0.106)
Z			-0.151	-0.216***
			(0.168)	(0.064)
DZ			0.103	0.049
			(0.231)	(0.079)
RZ				-0.088
				(0.139)
DRZ				0.307
				(0.191)
age	-0.200**	0.063**	-0.190**	0.064**
	(0.082)	(0.027)	(0.090)	(0.027)
age^2	0.003**	-0.001*	0.002**	-0.001*
	(0.001)	(0.000)	(0.001)	(0.000)
constant	13.98***	9.23***	13.99***	9.321***
	(1.564)	(0.536)	(1.638)	(0.539)
N	259	1163	259	1163
R^2	0.0591	0.0449	0.0624	0.0704
		Estimates based on exposure *Brown* measure		
De	0.056	-0.020	0.013	-0.028
	(0.062)	(0.021)	(0.064)	(0.021)
R		-0.450***		-0.232***
		(0.093)		(0.046)
DeR		0.027**		-0.029
		(0.011)		(0.023)
Z			-0.259**	-0.240***
			(0.131)	(0.057)
DeZ			0.051*	0.010

			(0.027)	(0.007)
RZ				-0.090
				(0.124)
DeRZ				0.053**
				(0.027)
age	-0.013	0.009	0.012	-0.003
	(0.199)	(0.065)	(0.197)	(0.066)
age^2	0.001	-0.000	0.000	-0.000
	(0.002)	(0.001)	(0.002)	(0.001)
constant	9.692*	10.679***	9.384*	11.099***
	(4.950)	(1.634)	(4.862)	(1.666)
N	259	1163	259	1163
R^2	0.0553	0.0484	0.0716	0.0763

Note: Standard errors are in parentheses. Regressions are weighted using 1976 individual weights.

*** *Significant at the 1% level*
** *Significant at the 5% level*
* *Significant at the 10% level*

Appendix 3: Full Regression Results for Regressions Estimating First-Generation Effects of *Brown* on Log Earnings with Both Discrete and Exposure Measures

Independent variable	D	Racial DD	Region DD	Region DDD
D	0.196	0.062	0.361	0.104
	(0.234)	(0.085)	(0.255)	(0.093)
De	0.042	-0.026	-0.007	-0.036*
	(0.061)	(0.022)	(0.060)	(0.022)
R		-0.399***		-0.190***
		(0.110)		(0.039)
DR		-0.210		-0.190*
		(0.194)		(0.097)
DeR		0.043***		-0.013
		(0.016)		(0.027)
Z			-0.152	-0.218***
			(0.165)	(0.064)
DZ			-0.245	-0.109
			(0.249)	(0.138)
DeZ			0.067**	0.019
			(0.031)	(0.013)
RZ				-0.089
				(0.140)
DRZ				0.013
				(0.261)
DeRZ				0.050
				(0.034)
age	-0.069	-0.006	-0.019	-0.015
	(0.202)	(0.067)	(0.203)	(0.069)
age^2	0.001	0.000	0.001	0.000
	(0.002)	(0.001)	(0.002)	(0.001)
constant	10.718**	10.972***	9.741**	11.311***
	(4.915)	(1.663)	(4.918)	(1.702)
N	259	1163	259	1163
R^2	0.0622	0.0496	0.0778	0.0785

Note: Standard errors are in parentheses. Regressions are weighted using 1976 individual weights.

**** Significant at the 1% level*
*** Significant at the 5% level*
**Significant at the 10% level*

Appendix 4: Full Regression Results for Regressions Estimating First-Generation Effects of *Brown* on Log Earnings with Controls for CRA64

	D	Race DD	Region DD	Region DDD	DI DD	DI DDD
	Estimates based on discrete *Brown* and CRA64 measures					
D	0.226	0.040	0.203	0.030	-0.300	0.008
	(0.243)	(0.087)	(0.277)	(0.092)	(0.302)	(0.101)
D64	0.003	0.025	-0.533	-0.051	0.135	-0.062
	(0.317)	(0.079)	(0.490)	(0.081)	(0.394)	(0.101)
R		-0.399***		-0.190***		-0.033
		(0.110)		(0.039)		(0.216)
DR		-0.017		-0.172*		-0.555*
		(0.154)		(0.092)		(0.294)
D64R		0.319**		-0.134		0.547**
		(0.138)		(0.318)		(0.262)
Z			-0.152	-0.216***	-1.064**	-0.455***
			(0.168)	(0.064)	(0.491)	(0.152)
DZ			-0.018	-0.002	1.279**	0.238
			(0.218)	(0.089)	(0.636)	(0.208)
D64Z			0.547	0.131	-0.192	0.330
			(0.382)	(0.092)	(0.528)	(0.236)
RZ				-0.089		-0.746
				(0.140)		(0.556)
DRZ				0.162		1.267*
				(0.205)		(0.707)
D64RZ				0.393		-0.698
				(0.354)		(0.601)
age	-0.199	0.082**	-0.196	0.071*	-0.151	0.076*
	(0.143)	(0.041)	(0.150)	(0.041)	(0.136)	(0.041)
age^2	0.003	-0.001*	0.003	-0.001	0.002	-0.001
	(0.002)	(0.000)	(0.002)	(0.000)	(0.001)	(0.000)
constant	13.951***	8.774***	14.145***	9.136***	13.417***	8.982***
	(3.248)	(0.918)	(3.329)	(0.914)	(3.188)	(0.925)
N	259	1163	259	1163	259	1163
R^2	0.0591	0.0479	0.0693	0.0755	0.1122	0.0656
	Estimates based on exposure *Brown* and CRA64 measures					
De	0.096	0.019	0.116	0.007	0.093	0.028
	(0.106)	(0.039)	(0.112)	(0.039)	(0.098)	(0.040)

De64	0.065	0.056	-0.185	0.029	0.316	0.026
	(0.163)	(0.046)	(0.187)	(0.047)	(0.196)	(0.056)
R		-0.440***		-0.246***		-0.187
		(0.096)		(0.051)		(0.187)
DeR		0.018		0.005		-0.053
		(0.109)		(0.030)		(0.039
De64R		0.042		-0.195		0.250*
		(0.068)		(0.125)		(0.141)
Z			-0.239*	-0.229***	-0.935**	-0.435***
			(0.128)	(0.058)	(0.410)	(0.137)
DeZ			0.000	0.004	0.174**	0.027
			(0.035)	(0.011)	(0.081)	(0.026)
De64Z			0.289**	0.035	-0.383	0.126
			(0.136)	(0.039)	(0.311)	(0.104)
RZ				-0.073		-0.485
				(0.129)		(0.472
DeRZ				0.011		0.155*
				(0.037)		(0.093)
De64RZ				0.225		-0.515
				(0.145)		(0.326)
age	0.182	0.194	0.281	0.147	0.504	0.239
	(0.497)	(0.161)	(0.500)	(0.161)	(0.463)	(0.162
age^2	-0.001	-0.002	-0.002	-0.002	-0.005	-0.002
	(0.005)	(0.002)	(0.005)	(0.001)	(0.005)	(0.002)
constant	4.937	6.163	2.798	7.428*	-2.551	5.102
	(12.117)	(3.973)	(12.135)	(3.976)	(11.298)	(4.008)
N	259	1163	259	1163	259	1163
R^2	0.0566	0.0501	0.0823	0.0785	0.1104	0.0686

Note: Standard errors are in parentheses. Regressions are weighted using 1976 individual weights.

*** *Significant at the 1% level*
** *Significant at the 5% level*
* *Significant at the 10% level*

Appendix 5: Full Regression Results for Regressions Estimating Effects of *Brown* on First-Generation Log Earnings Using Cohorts

Independent variable	D	Racial DD	Region DD	Region DDD	DI DD	DI DDD
			Change in log earnings relative to previous cohort			
C_2	-0.363*	-0.009	-0.031	-0.007	-0.332	0.019
	(0.196)	(0.061)	(0.029)	(0.073)	(0.343)	(0.101)
C_3	0.120	-0.025	-0.130*	0.004	-0.431	-0.046
	(0.181)	(0.060)	(0.077)	(0.072)	(0.305)	(0.101)
C_4	0.023	-0.113**	-0.180	-0.131**	0.213	-0.197**
	(0.253)	(0.057)	(0.358)	(0.063)	(0.353)	(0.098)
C_5	0.130	-0.070	-0.211*	-0.105*	0.039	-0.102
	(0.238)	(0.054)	(0.476)	(0.062)	(0.315)	(0.097)
R		-0.233*		-0.193***		0.109
		(0.140)		(0.046)		(0.276)
C_2R		-0.354*		-0.024		-0.351
		(0.204)		(0.078)		(0.354)
C_3R		0.145		-0.134		-0.384
		(0.190)		(0.105)		(0.318)
C_4R		0.136		-0.049		0.410
		(0.258)		(0.360)		(0.363)
C_5R		0.201		-0.106		0.141
		(0.243)		(0.475)		(0.326)
Z			-0.122	-0.230***	-1.141*	-0.362*
			(0.175)	(0.086)	(0.649)	(0.195)
C_2Z			-0.326	0.030	-0.000	-0.225
			(0.232)	(0.127)	(0.957)	(0.313)
C_3Z			0.240	-0.069	1.392*	0.186
			(0.212)	(0.127)	(0.829)	(0.313)
C_4Z			0.232	0.101	-0.467	0.368
			(0.451)	(0.123)	(0.982)	(0.284)
C_5Z			0.353	0.070	0.198	0.117
			(0.539)	(0.112)	(0.951)	(0.279)
RZ				0.109		-0.779
				(0.193)		(0.671)
C_2RZ				-0.356		0.224
				(0.263)		(0.997)
C_3RZ				0.310		1.206
				(0.245)		(0.878)
C_4RZ				0.131		-0.835

				(0.463)		(1.012)
C_5RZ				0.283		0.081
				(0.545)		(0.981)
constant	10.586***	10.819***	10.682***	10.875***	11.014***	10.906***
	(0.136)	(0.039)	(0.002)	(0.046)	(0.271)	(0.064)
N	259	1163	259	1163	259	1163
R^2	0.0526	0.0490	0.0672	0.0775	0.1153	0.0682

Note: Standard errors are in parentheses. Regressions are weighted using 1976 individual weights.

**** Significant at the 1% level*
*** Significant at the 5% level*
**Significant at the 10% level*

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[1] 347 U.S. 483 (1954), 349 U.S. 294 (1955). We use the earlier date to classify our data, and we refer to *Brown* as singular throughout the paper, although both cases were certainly important.
[2] 163 U.S. 537 (1896).
[3] The program gave randomly selected blacks the right to be bused to a predominantly white school. However, the fact that students and their families could opt to remain in their neighborhood school re-introduces selection into the participation decision.
[4] Boozer et al. (1992) report that, even as late as 1989-90, more than 65 percent of blacks attended predominantly minority schools. In the Northeast, the fraction exceeded 75 percent.
[5] For reviews of these cases, see Clotfelter (2004), Heaney (1985), and Orfield (1983).
[6] Aside from Mississippi, per-pupil expenditures in 1950 on nonwhites in Southern states were between about 65 and 95 percent of expenditures on whites, up from 25 to 50 percent in 1935. The school year in days was almost exactly the same for whites and nonwhites by 1950 (except in Mississippi), and class sizes were only about 10 to 15 percent greater in nonwhite schools by 1950 (except in Mississippi and Louisiana) as compared to 30 to 40 percent greater in 1935. Teacher salaries in nonwhite schools were between 70 and 96 percent of salaries in white schools as of 1950 (except in Mississippi), compared to 30 to 60 percent in 1936.
[7] *Historical Statistics of the United States: Colonial Times to 1970* (1975) reports that, among those aged 14 to 24 years old as of 1953, the illiteracy rate for whites was 0.8 percent compared with 3.9 percent of nonwhites. In 1900 the illiteracy rates for whites and blacks of all ages stood at 6.2 and 44.5 percent respectively.
[8] As of 1953, 99.7 percent of white children aged 7 to 13 were enrolled in school compared with 97.3 percent of nonwhite children in the same age group. Similarly, children aged 14 to 17 show comparable enrollment rates across race (86.4 percent and 82.3 percent for whites and blacks respectively).
[9] For instance, the Freedom Riders regarded *Brown* a primary motivator, scheduling their journey to end in New Orleans on 17 May 1961, the seventh anniversary of the decision. Proctor (1966), Sitkoff (1993), Dittmer (1994), and Fairclough (2001), among others, document the energy and heightened aspirations in the black community following *Brown*.
[10] Cited in Callahan (1999).
[11] Because earlier efforts at improving conditions for blacks paved the way for *Brown*, anticipation of the case could mean that empirical estimates of its effect centered around the date of the decision will not fully capture the impact of *Brown*. On the other hand, if the ruling in *Brown* were completely unexpected, people might not have found it credible. Our data do not permit us to test the counterfactual. Similarly, our data do not allow us to separate the effects of *Brown* from those attributable to other events – Rosa Parks's refusal to give up her bus seat, library and lunch-counter sit-ins, boycotts, and the like – that occurred in the mid- to late-1950s.
[12] Reported on p. 16 of the *New York Times* on May 18, 1954.
[13] That nearly all Southern congressmen signed the 1956 "Southern Manifesto" denouncing *Brown* and pledging to resist it certainly shows that Southern whites considered the case a serious threat to the status quo. For discussion of white resistance to *Brown*, see Kluger (1975) and Garrow (1986). Lee (2002), p. 135, analyzes a sample of letters written to the President between 1948 and 1965 and documents the sharp increase in letters from Southern whites just after the *Brown* decision.
[14] For a good history of Citizenship Schools, see Levine (2004).
[15] Ashenfelter et al. (2006) suggest a similar role for expectations in explaining the increase in earnings found in black men educated just before significant integration in the late 1960s.
[16] While access to credit markets was undoubtedly limited among blacks, we cannot rule out the possibility that discrimination in the job market was so severe as to render the credit constraint moot.
[17] To account for non-random sampling, we use the individual weights for 1976, the first year of observation for the first generation (in which 1975 earnings are reported), and 1972 (the year sons are matched to fathers) in the first- and second-generation analyses respectively.
[18] One second-generation black and two second-generation whites are added to the sample by eliminating this selection rule. Not surprisingly, this change does not meaningfully affect the results.
[19] Incomplete earnings histories might indicate something about human capital. For instance, the less well-educated might have longer spells of unemployment or a greater incidence of jail time. Robustness checks indicate that inclusion of men with incomplete earnings histories does not meaningfully alter our results. See note 25.

[20] In all measures of *Brown* we use expected graduation year rather than information on whether the individual was still in school at that point in time. This is done for two reasons. First, so long as he was younger than high-school leaving age, the young man may have returned to school. More importantly, knowing that an individual was not in school in 1954 can mean that either he was born earlier than 1937 or that he acquired little schooling. To avoid an education bias introduced by the latter reason, we measure exposure based on expected graduation year. Also, in later regressions, we control for individual years of education.

[21] States coded as "Southern" include the Southern and border states that were segregated by law -- Alabama, Arkansas, Delaware, District of Columbia, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, Missouri, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia.

[22] In 1955, 9 percent of Southern earnings went to the farm sector, 18 percent to government, and 21 percent to manufacturing. The same figures for the North were 4 percent, 12 percent, and 33 percent. Employment data were not reported before 1969; as of that year, 7 percent of Southerners worked in agriculture and 19 percent in manufacturing. For Northerners, the figures were 5 percent and 24 percent. Bureau of Economic Analysis (2005), Historical Income Tables, 1929-57, and Income and Employment Tables by SIC Code, 1969.

[23] The number of observations increases to 9 and 26 respectively when the number of required earnings observations is reduced from 10 to seven.

[24] The dissimilarity index is defined as $\Sigma\, t_s\, |p_s - p|/2Tp(1-p)$, where s indicates a particular school, t_s is total enrollment in school s, p_s is the fraction of minority students in school s, p is the district average for p_s, and T is number of students in the school district. We obtained indices by district from Welch and Light (1987), then calculated state-wide indices using information about district size and composition. Welch and Light did not include districts and states – such as Mississippi -- for which integration was virtually impossible because districts were so racially homogeneous. The change in DI for these states therefore equaled 0. The average absolute change in DI for the entire sample was 0.278, with a standard deviation of 0.207. For most districts, the starting date was 1967 or 1968 and the ending date was 1984 or 1985.

[25] When we expand our sample to include respondents with incomplete earnings histories (five earnings observations), the significance of the exposure region DD and DDD estimates rise to the 2.5% and 1% level respectively while the exposure race DD estimate becomes insignificant.

[26] Because earnings tend to increase over the lifecycle up to a certain age, we might expect the D estimates to have negative coefficients that become larger in absolute magnitude for younger cohorts, absent other effects. (Recall that the omitted cohort is the oldest group.) This may help explain the negative coefficient on the second cohort. Because the DD and DDD estimators measure effects relative to a control group, however, this story does not explain the negative (albeit insignificant) coefficient on the second cohort in these regressions.

[27] Bell (2004), p. 19.

[28] Resegregation *per se* arguably is not the issue; school quality is what matters. Rivkin (2000) finds little evidence to support the belief that mandatory desegregation significantly affects future earnings for blacks. He suggests that a more promising policy for improving labor market outcomes for blacks is to raise the quality of the schools they currently attend.