Online Appendices: School Desegregation and Black Teacher Employment

Appendix A: Data

As discussed in Section 4 of the main paper, data on race-specific teacher employment counts and student desegregation for the 1968, 1970 and 1972 school years was drawn from surveys conducted by the US Office of Civil Rights (OCR) that were generously converted from the original binary files and made publicly available by Ben Denckla and Sarah Reber of UCLA. The OCR data was merged across these three school years using unique numerical identifiers created by OCR. These OCR district codes appear to be equivalent to the district codes currently used by the National Center for Educational Statistics (NCES), except that (1) NCES district codes lead with a state identifier defined with standard state FIPS codes, while OCR district codes lead with a state code of their own creation and (2) the OCR codes contain an additional leading zero between the state prefix and the district identifier.

The OCR data also contains a string variable giving the name of each school "system" (district). These names were used to match the districts included in the OCR surveys to data from 1967 contained in NCES (1967) and to data from state department of education reports from around 1964. Within the eight states included in the working sample used above, over 98% of the districts from the OCR data had an unambiguous match in the 1967 and 1964 data based on district name.

As noted, state department of education and superintendent annual reports were used to construct race-specific enrollment and teacher employment totals for the period prior to non-token desegregation, which in the studied districts began with the 1965 school year. Some complications arise because some states issued biannual rather than annual reports, while other states stopped reporting race-specific information in the years leading up to 1964, perhaps in anticipation of increased scrutiny of racial segregation in their systems. As a result, not all states have valid data for the 1964 school year specifically, and in these cases I use the latest available school year prior to 1964. The last available academic year for Alabama, Georgia and South Carolina was 1963, and the last available academic year for Mississippi and Tennessee was 1962. All other states had data available for the 1964 academic year.

Additionally, South Carolina only reported data by county for 1964, not by school district. Given this, I restrict the South Carolina sample to the 25 counties that contained only one school district, out of 95 total school districts in the state.

Descriptive statistics are shown in Table A1 and discussed in Section 4 of the main text.

Appendix B: Robustness

To preserve district observations that employed a positive number of black teachers in 1964 but zero black teachers in a subsequent sample year, the dependent variable in the main analysis was transformed as $ln(BlackTeachers_{dy}+1)$. This transformation preserved observations from 14 districts, and in Column 1 of Table A2 I report results that instead exclude these districts. The estimated effect of student desegregation on black teacher employment is virtually unchanged.

Because the analysis above measured black teacher employment in logs, a given reduction in the number of black teachers had a larger impact on the estimates within districts that had smaller initial levels of black teacher employment, since a particular level reduction in black teaching positions constitutes a larger percentage reduction in districts with smaller baseline black teacher employment. Two reasonable alternative dependent variables that do not have this feature are the share of each district's teacher labor force that is black and the level count of black teachers, and the results of models with these alternative dependent variables are shown in Columns 2 and 3 of Table A2, respectively.[1] The estimates indicate that fully implementing student desegregation reduced the share of the typical school district's teacher labor force that was black by 8.1 percentage points and reduced the number of black teachers in the typical district by 19.98 teachers. In 1964, the average school district in the sample had a black teacher employment share of 30.6% and employed 72 black teachers, so that the estimates reported in Columns 2 and 3 both translate to black teacher employment reductions of approximately 27%.

The fact that these percent reductions are somewhat lower than the baseline estimate from Column 1 of Table 1, and are very similar to the weighted estimates from Column 3 of Table 1, reflects the fact that relative black teacher disemployment effects were stronger in districts with smaller initial black teacher employment levels. Because of this, modeling choices that put less weight on districts employing relatively few black teachers, whether explicitly through applying weights or implicitly through the choice of dependent variable, lead to somewhat smaller (though still normatively large) treatment effect estimates. Which type of estimate is preferred will depend on the context and question of interest.

A methodological issue arises from the fact that the number of black teachers in a district is an overdispersed count variable: A district can employ no fewer than zero black teachers, while a relatively small number of districts employ very large numbers of black teachers, causing the variance of black teacher employment to well exceed its mean. Given these features of the dependent variable, a Negative Binomial specification may be more appropriate than using the log of the black teacher count as the dependent variable, and the results of such a specification are reported in Column 4 of Table A2. The coefficient on the student integration variable in this specification, which can be interpreted as a semi-elasticity, is -.335, and therefore leads to very similar conclusions as the baseline specification in Table 1.[2]

The sample used above consisted of 781 school districts from eight southern states for which the required data components could be constructed for the 1964, 1967, 1968, 1970 and 1972 school years. However, as noted in Section 4 above, it is also possible to assemble a data set for a larger sample of 1,123 school districts from all eleven states of the former Confederacy, but information on black teacher employment in this larger set of districts is only available beginning in the 1967 school year, when the student desegregation process was

[1] Note that the denominator of the ratio used as the dependent variable in the model from Column 2 implicitly controls for any changes in overall teacher employment levels, which is functionally similar to Column 2 of Table 1.

[2] The Negative Binomial model in Column 4 reports bootstrapped standard errors with 400 repetitions. The results are virtually identical if a Poisson model is used in place of a Negative Binomial model.

approximately half complete. Column 5 of Table A2 reports results using this expanded sample of districts observed over a shorter span of school years, and the estimated treatment effect falls to -.314 log points. To determine the extent to which this reduction is attributable to using an expanded set of school districts versus using a shorter span of school years, I have estimated models (not shown) that use the baseline sample of 781 school districts but exclude data from the 1964 school year. The estimated treatment effect in this sample falls to -.259 log points, which suggests that the reduced magnitude of the estimate in Column 5 of Table A2 is primarily due to the truncation of years, not the expanded set of districts, and also suggests that the most severe black teacher employment reductions likely occurred in the earlier stages of the student desegregation process.

Another robustness related issue is how to account for school district consolidations and splits. As noted, the main data set was constructed by matching school districts across years using school district names, but if a school district absorbed a neighboring district or split off from an existing district, while maintaining the same name, it is possible that the actual school district boundaries and composition changed over the course of the study period. It is also possible that district re-organizations were themselves a response to the imposition of desegregation, for instance by intentionally packing African American students into a newly created municipal district while creating a predominantly white district in the balance of the county.

One approach to accounting for district reorganizations is to restrict the sample to districts that did not undergo a merger or split during the study period. Cascio et al. (2013) use data from state school finance reports to identify districts undergoing a re-organization between 1961 and 1969, and their sample includes all of the states used in the analysis above except for Texas. In Column 6 of Table A2 I re-estimate the baseline specification with the set of districts used in Cascio et al. (2013), which excludes 52 districts believed to have undergone a reorganization during the study period (as well as all Texas districts). The estimated effect of student desegregation on black teacher employment is -.323 log points, very similar to the baseline findings.

Another method of accounting for district reorganizations is to estimate models with the data aggregated to the county level. This approach takes advantage of the fact that when district consolidations or splits did occur, they typically involved a municipal district joining or leaving the school district operated by the county in which the municipality was located. The 781 school districts in the current sample were located in 563 unique counties. Column 7 of Table A2 reports the results of estimating the main specification with these counties as the unit of analysis, and the estimated treatment effect is -.333 log points. While county aggregation may introduce or exacerbate measurement error, in general the effect of aggregating measures of school characteristics are ambiguous and case-specific (Hanushek et al. 1996; Carruthers & Wanamaker 2017), and for present purposes the most important point is that the large estimated effect of desegregation when using county level data make bias due to district consolidations or mergers an unlikely explanation for the main findings from Table 1.

The paper's main findings measured student desegregation as the fraction of African American students in each district attending a school where 5% or more of the enrolled students were white. While this measure is intuitive and has easily interpretable units, alternative desegregation measures are available, and these alternative measures may be especially useful in districts with very large or very small African American student shares, where the baseline desegregation measure could conflate racial integration with the racial composition of a school district.

The two most widely used measures of segregation are the exposure index and the dissimilarity index (Massey

& Denton 1988). The exposure index (formally defined as $\sum_s \frac{B_s}{B_d} \times \frac{W_s}{B_s+W_s}$, where B_s and B_d denote the number of black students in a given school and in a given district and similarly for W_s and W_d) calculates the probability that a randomly drawn schoolmate of a black student will be white. Notably, the minimum value for the exposure index is the overall share of the district's students who are black, which is a particularly useful feature for measuring desegregation in districts with very large or very small African American student shares. The dissimilarity index, defined as $\frac{1}{2}\sum_s \left|\frac{B_s}{B_d} - \frac{W_s}{W_d}\right|$, calculates the share of black (or white) students who would need to change schools in order to make the racial composition of each school match that of the district overall.

Results of estimating the baseline model while using these alternative segregation measures are reported in Columns 8 and 9 of Table A2. In both cases, the estimated effect of student segregation on black teacher employment remain substantively and statistically significant: A one unit increase in the exposure index is estimated to increase black teach employment by .650 log points, while a one unit increase in the dissimilarity index is estimated to increase black teacher employment by .437 log points. The average district in the current sample experienced a .60 point decline in the exposure index and a .75 point decline in the dissimilarity index between 1964 and 1972, so that the coefficients from Columns 8 and 9 suggest that the full post-CRA student desegregation process reduced black teacher employment by approximately 30-40%, similar to the baseline estimates above.

Several robustness checks for the Census-based results are reported in Table A3 and discussed in Section 6 of the main text.

Appendix References

Carruthers, C., & Wanamaker, M. (2017). Separate and unequal in the labor market: Human capital and the Jim Crow wage gap. *Journal of Labor Economics*, 35(3), 655-696.

Cascio , E., Gordon, N., & Reber, S. (2013). Local responses to federal grants: Evidence from the introduction of Title I in the South. *American Economic Journal: Economic Policy*, 5(3), 126-159.

Hanushek, E., Rivkin, S., & Taylor, L. (1996). Aggregation and the estimated effects of school resources. *Review of Economics and Statistics*, 78(4), 611-627.

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Table A1: Descriptive Statistics

	8-State Sample (1964-1972)				11-State Sample (1967-1972)			
	Mean	Standard Deviation	10th Percentile	90th Percentile	Mean	Standard Deviation	10th Percentile	90th Percentile
Total Teachers	301	587	45	579	320	652	40	618
Black Teachers	75	201	4	139	76	195	3	147
White Teachers	226	437	28	449	244	500	26	493
Total Students	7,006	14,056	923	13,037	7,606	21,940	807	14,252
Black Students	2,115	5,792	166	3,795	2,185	5,683	167	4,015
White Students	4,891	9,777	456	10,209	5,422	19,087	389	10,889
Alabama	0.119	-	-	-	0.087	-	-	-
Arkansas	-	-	-	-	0.095	-	-	-
Florida	-	-	-	-	0.053	-	-	-
Georgia	0.157	-	-	-	0.113	-	-	-
Louisiana	0.058	-	-	-	0.04	-	-	-
Mississippi	0.097	-	-	-	0.078	-	-	-
North Carolina	-	-	-	-	0.119	-	-	-
South Carolina	0.031	-	-	-	0.025	-	-	-
Tennessee	0.093	-	-	-	0.069	-	-	-
Texas	0.321	-	-	-	0.233	-	-	-
Virginia	0.123	-	-	-	0.086	-	-	-
District-Year Observations	3,905	3,905	3,905	3,905	4,492	4,492	4,492	4,492
Number of Unique Districts	781	781	781	781	1,123	1,123	1,123	1,123

Table A2: Additional Robustness

	(1) Excluding Districts with no Black Teachers	(2) Black Teacher Share	(3) Black Teacher Count	(4) Negative Binomial	(5) Eleven-State Sample	(6) Reorganizing Districts Excluded	(7) County Level Data	(8) Exposure Index	(9) Dissimilarity Index
Student Desegregation	-0.368***	-0.081***	-19.975***	-0.335***	-0.314***	-0.323***	-0.333***	0.650***	0.437***
	(0.026)	(0.009)	(3.196)	(0.024)	(0.038)	(0.029)	(0.031)	(0.053)	(0.033)
District-Year Observations	3,835	3,905	3,905	3,905	4,492	2,390	2,815	3,905	3,905
Number of Unique Districts	767	781	781	781	1,123	478	563	781	781

Notes: The model in Column 1 uses a sample that excludes 14 districts that employed at least one black teacher in 1964, but employed no black teachers in at least one observed year after 1964. The models in Columns 2 and 3 respectively use the fraction of the teachers in each district-year who were black and the level count of black teachers in each district-year as dependent variables. The model in Column 4 uses a Negative Binomial estimator rather than OLS. The model in Column 5 uses an expanded sample of school districts from all eleven states of the former Confederacy, but excludes data from the 1964 school year. The model in Column 6 uses the sample of districts from Cascio et al. (2013), which excludes districts that underwent a split or a merger between 1961 and 1969, as well as Texas districts. The model in Column 7 uses data collapsed to the county level, and the reported sample sizes refer to counties rather than school districts. The models in Columns 8 and 9 respectively use the Exposure Index and the Dissimilarity Index as the measure of student desegregation, while all other models use the share of black students attending desegregated schools. All models contain school district (or county) and year fixed-effects. Total teacher employment is measured in logs in all models except Column 3, where it is measure in levels. All models give each district equal weight. Standard errors, clustered at the school district (or county) level, are in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively.

Table A3: Robustness of Census Estimates

	(1) Southern Teacher	(2) Other Southern Professional	(3) Southern Non-Professional	(4) Teacher Outside of South	(5) Non-Teacher Outside of South	(6) Not Working
A: No Controls						
Black × Y1970	-0.027**	-0.006	0.007	0.022***	0.006	-0.002
	(0.011)	(0.008)	(0.011)	(0.006)	(0.012)	(0.009)
Observations	43,128	43,128	43,128	43,128	43,128	43,128
B: 1970-1980						
Black × Y1980	-0.039***	0.009	-0.047***	-0.001	0.029***	0.049***
	(0.007)	(0.006)	(0.008)	(0.005)	(0.009)	(0.007)
Observations	67,529	67,529	67,529	67,529	67,529	67,529

Notes: The models in Panel A are identical to those in Panel A of Table 4 but exclude all covariates. The models in Panel B are identical to those in Panel A of Table 4, but use data from the 1970 and 1980 Decennial Censuses rather than the 1960 and 1970 Censuses. Robust standard errors are in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% levels, respectively.