Web Appendix

I. School District Reorganization

School districts both consolidate and split apart during our sample period. We use the state records cited in Section II of the Data Appendix to establish a history of these reorganizations. For each year, we construct a crosswalk between the district and the largest unit to which the district is party between 1961 and 1969 (*agg_id*). We then merge this crosswalk to each data set and collapse key variables to the *agg_id*-year level. For example, if districts A and B merge in 1966 to form district C, we will observe A and B jointly as one observation prior to 1966, geographically identical to and with the same *agg_id* as district C. And if district X splits into districts Y and Z in 1964, we will observe Y and Z jointly as one observation beginning in 1964, geographically identical to and with the same *agg_id* as X.

If all districts in an *agg_id* are observed, we set the value of an indicator (e.g., for student and teacher desegregation) for the *agg_id* equal to one if any constituent district has the indicator set to one, zero otherwise. If not all constituent districts are observed, we code the indicator for the *agg_id* as one if any observed district has the indicator equal to one, missing otherwise. Numerical variables (e.g., the percent of blacks in desegregated schools, enrollment, pre-program percent black in enrollment, pre-program child poverty rate) are coded as missing if not all constituent districts are observed. Where all districts are observed, we sum up all components of the variables (e.g., number of blacks attending desegregated schools, total enrollment), and calculate values for the *agg_id* accordingly.

Web Appendix Table I summarizes how our sample is constructed from these aggregated data. There are 1,476 aggregated districts in the raw data, for 1,293 of which we have an estimate of percent black in enrollment in the early 1960s. The vast majority of the 183 districts lost from this sample restriction would have been dropped had their racial composition been directly observed: 146 (80 percent) of these districts filed an "assurance of compliance," or Form 441, with DHEW in 1966, signifying that they were uniracial districts. Of the 1,293 with data on racial composition, 1,088 were between 3 and 97 percent black, on average, in the early 1960s, and of these, 1,020 were not under court order in 1964. We drop 25 districts because they lack data on other key explanatory variables and another 79 districts because data on student desegregation in 1966 are not directly available or cannot be imputed. (See Data Appendix for discussion of imputation.) The resulting sample size is 916.

Forty-nine of the 916 aggregated districts in our estimation sample (5.35 percent) were involved in a consolidation or split at some point over 1961 to 1969. Our findings are robust to omitting these observations (available on request).

II. Robustness of the Estimates

A. Definition of the Dependent Variable

Web Appendix Figure I plots TSLS coefficients on Title I funding per pupil from the same specification shown in Table IV, but where the dependent variables are indicators for whether a district fell into narrower, two-percentage point bins across the entire distribution of student desegregation. The dashed lines represent the 95 percent confidence intervals on these estimates. The figure shows that the financial incentive affected behavior only in the lower tail of the distribution of desegregation.

B. Alternative Outcomes

If the size of the Title I grant at risk affected the likelihood that a district would do enough to comply with CRA, we would expect districts with larger grants to have been less likely to have had their federal funding deferred or terminated. The first column of Web Appendix Table II shows the effects of Title I funds on this outcome in the specification with the full set of controls. The TSLS coefficient of interest is negative, as expected, but not statistically significant. The imprecision of this finding is unsurprising considering the previously discussed inability of DHEW to defer or terminate funds precisely according to its own policy guidance. Nevertheless, school boards had little information about the likely strength of enforcement *ex ante*, so we are reassured to see that there is no statistically significant positive relationship.

C. Relationship between the Potential Grant and Prior Outcomes

In Table IV, Panel B, we showed that potential Title I grants were uncorrelated with pre-program student desegregation on the intensive margin, helping to rule out that the size of grants was correlated with unobserved tastes for segregation. However, in 1964, most of the variation in desegregation activity was on the extensive margin. In column (2) of Web Appendix Table II, we present TSLS estimates from a version of Equation (1') where the dependent variable is an indicator for whether a district had *any* black students in school with white students in 1964. The specification omits the indicator for student desegregation in 1964 as a control, but is otherwise identical to that presented in Table IV. The estimate is not statistically significant. Moreover, the coefficient is negative, implying that if anything, districts with larger per-pupil Title I grants were less inclined to have desegregated prior to the program's introduction. This suggests that we may be underestimating the effects of financial incentives.[1] Following the same logic, we also explore whether conditional federal funding had an "effect" on the likelihood that a district was under court order in 1964. The results of this exercise are shown in column (3) of Web Appendix Table II; the underlying specification is the same as that presented in column (2), and the sample is expanded to include districts under court order in 1964. There is no significant relationship, suggesting that the Title I funding formula did not target districts that were more likely to desegregate on their own.

D. Heterogeneity in the Effects of Conditional Funding

The strength of opposition to desegregation—and therefore the "price" required to meet desegregation targets—most likely varied across districts. Most notably, districts with stronger support for Strom Thurmond or a higher share of black enrollment might be expected to have responded less to the same incentive. Unfortunately, when we estimate the model separately for "high" (above median) and "low" (below median) Thurmond vote

[1] This substantive finding holds for desegregation in each of 1961 through 1963 as well, suggesting that our estimates are not biased from reversion to the mean.

share (or black share) subsamples, our results (not shown) become sufficiently imprecise as to rule out even sizeable differences in coefficient estimates across subsamples.

III. Comparing our Estimates to Existing Estimates of Willingness to Pay for Segregation

We estimate that it took on average $1,195 per pupil (in 2007 dollars) to move a district beyond token desegregation: that is, to change the probability that a district is in the zero to two percent of blacks in all-black schools bin from zero to one. As discussed in footnote 28, this estimate implies that house prices in a school district with "just enough" (two to six percent of blacks in desegregated schools) desegregation should have been about 1.6 percent lower compared to a district with token desegregation. To arrive at this figure, we relied on additional data from the 1960 Census public use sample. In the states in our sample, there were 0.811 public school children per household, implying a cost of $969 per household (0.811*$1195). The median price of an owner-occupied dwelling in the South in 1960 was $61,292 (again in 2007 dollars). On average, therefore, the "price" of moving a district beyond tokenism was about 1.6 percent of the median home value (($969/$61,292)*100). Absent the grant, if districts were forced to desegregate as much as they did, home prices would have fallen by that amount. If districts (correctly) anticipated imperfect enforcement of the DHEW guidelines, then our estimates will be lower bounds of willingness to pay for segregation.

Web Appendix Table I
Definition of 1966 Estimation Sample

	Number of Districts					
State:	Full Sample	With Early 1960s % Black in Enrollment Observed	Early 1960s % Black in Enrollment 3% to 97%	Not Under Court Order in 1964	With Other Explanatory Variables Observed	With Outcomes Observed
Alabama	113	113	100	92	92	83
Arkansas	387	205	148	146	145	121
Florida	67	67	67	61	61	56
Georgia	192	192	170	166	161	146
Louisiana	66	66	66	63	60	59
Mississippi	134	134	121	117	111	99
North Carolina	152	151	135	129	128	126
South Carolina	93	93	93	88	88	86
Tennessee	146	146	81	70	67	67
Virginia	126	126	107	88	82	73
Total	1,476	1,293	1,088	1,020	995	916

Notes: See Data Appendix for description of variables and sources. Each column requires all variables in columns to its left. "Outcomes" refers to data on the percent of blacks in desegregated schools. We impute the percent of blacks in desegregated schools for a small fraction of the 916 districts in our 1966 estimation sample; see Data Appendix.

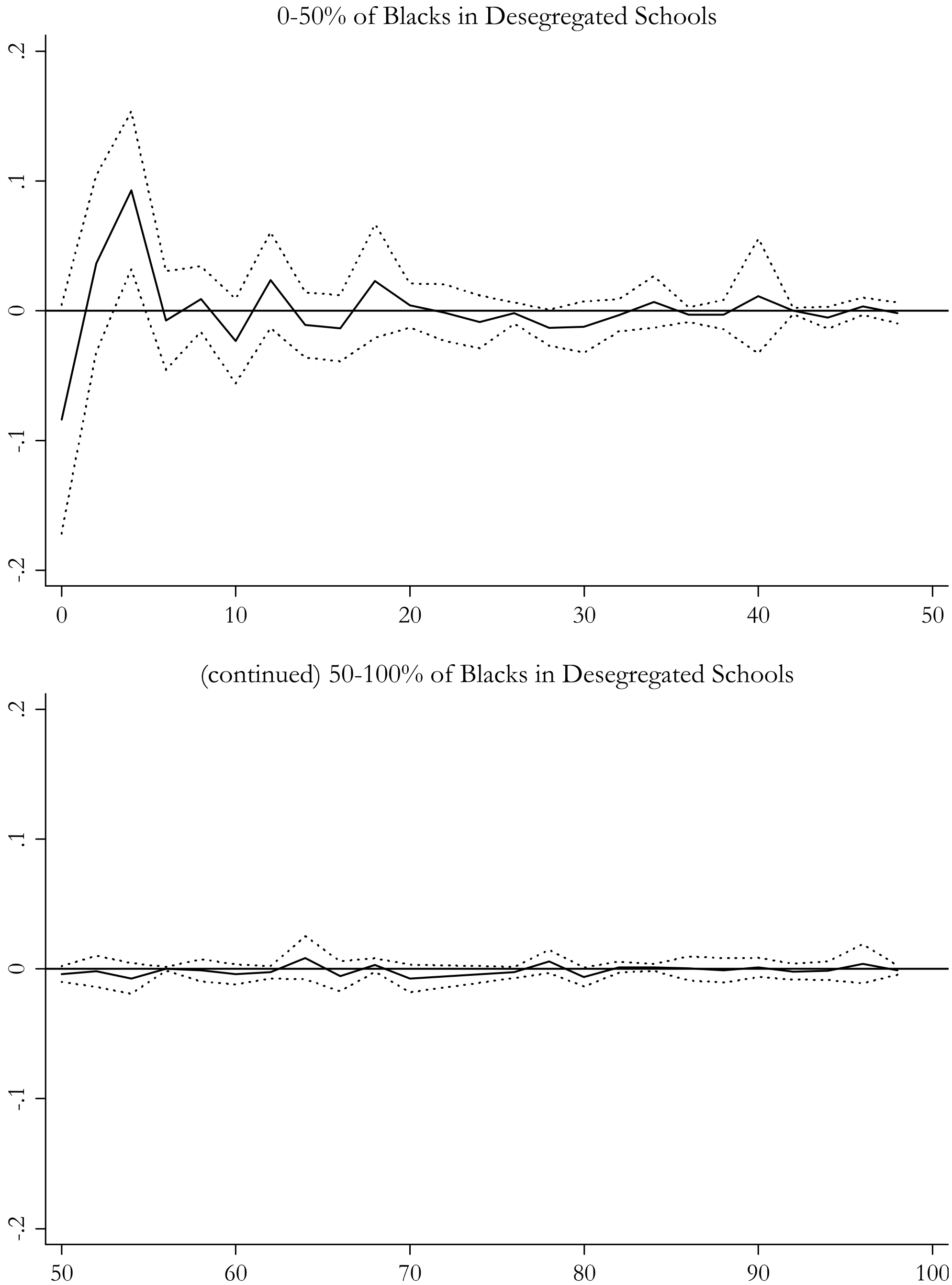
Web Appendix Table II
The Effect of Potential Title I Funding in 1966 on Other Outcomes

	=1 if Funds Deferred or Terminated, 1966	= 1 if Any Student Desegregation, 1964	=1 if Under Court Order, 1964
	(1)	(2)	(3)
Mean of Dependent Variable	0.204	0.176	0.0576
		A. Two-Stage Least Squares	
Title I Per Pupil , 1966 (in 100s of $2007)	-0.0360	-0.0362	-0.00463
	(0.0392)	(0.0253)	(0.0191)
First Stage Partial *F*-Stat for Excluded Instrument	400.1	400.9	402.2
RMSE	0.356	0.290	0.217
		B. Ordinary Least Squares	
Title I Per Pupil, 1966 (in 100s of $2007)	0.00264	-0.0243	0.00830
	(0.0297)	(0.0181)	(0.0146)
RMSE	0.355	0.290	0.217
R-Squared	0.260	0.448	0.174
Controls:			
State Fixed Effects	X	X	X
Early 1960s Child Poverty %:			
Dummies for 20 Quantiles			
Restricted Quantile Effects†	X	X	X
Early 1960s Black Enrollment % (Decile Dummies)	X	X	X
1948 Thurmond Vote % (Quintile Dummies)	X	X	X
Ln Early 1960s Enrollment	X	X	X
Early 1960s Exp. per Pupil (Quintile Dummies)	X	X	X
1960 County Characteristics‡	X	X	X
Number of Districts	916	916	972

Notes: Each column in each panel gives results from a different regression. The unit of observation is a school district; see text and Data Appendix for descriptions of the sample. In addition to the controls listed, the model in column (1) includes as an explanatory variable an indicator for whether the district had any student desegregation in 1964. Standard errors, in parentheses, are clustered on county. *** p<0.01, ** p<0.05, * p<0.1

† Dummies for nine deciles and the top two of the twenty quantiles.

‡ % with high school degree, % employed in agriculture, median family income ($2007), indicator for urban.



Web Appendix Figure I.
TSLS Estimates of the Impact of Potential Title I Funding on Each Level of Student Desegregation, 1966

Notes: The solid line connects TSLS coefficients on the 1966 per-pupil Title I grant (in 100s of $2007) from regressions where the dependent variables are a series of dummies for the corresponding two percentage-point range of the percent of blacks in desegregated schools in 1966. The regressions also include state fixed effects, restricted quantile effects for the early 1960s child poverty rate (see text), and the complete set of district and county-level controls described in the notes to Table III. The instrument for Title I funding per pupil is simulated Title I funding per-pupil (also in 100s of $2007); see text. The dotted lines represent the 95 percent confidence intervals for these estimates; standard errors are clustered on county.