Online Appendix for "The Political Legacy of American Slavery"

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A Supplemental Information: Data details

Areal interpolation. We use areal weighting to interpolate data from the 1860 U.S. Census onto modern county boundaries (Saporito et al., 2007). While population-weighted interpolation generally produces better estimates of the underlying rates of various sub-populations, we use areal weighting because we are interested in interpolating both proportions (enslaved, free, etc) and levels (total population, improve farming acreage, etc), and areal interpolation allows us to use a consistent set of interpolation weights across these disparate measures. O'Connell (2012) and Reece and O'Connell (2015) use a population-weighting approach to estimate proportion slave in modern county boundaries and our measure correlates with this measure at $r = 0.986$. For historical county boundaries, we relied on the Newberry Atlas of Historical County Boundaries (Siczewicz, 2011).

The areal weighting scheme works as follows. For a given census year, we create a $n_s \times n_t$ matrix A, where n_s is the number of source (1860) counties and n_t is the number of target (2000) counties. A is a row-normalized matrix, where each entry a_{ij} is the proportion of the area of source county i that is contained in target county j. We follow O'Connell (2012) and set $a_{ij} = 1$ and $a_{ij'} = 0$ if more than 95% of an 1860 county is contained in a single 2000 county. Let y_t be the vector of target values that we are trying to estimate and y_s be the observed source vector of values. Then, we construct areal weighted estimated by $y_t = A'y_s$. Essentially, this distributed the population in each 1860 county is distributed to 2000 counties based on how much of the 1860 county is contained in the 2000 county. Areal weighting assumes that distributions are evenly spread throughout the source counties, which may be false, and many methods exist to incorporate additional information such as roads to correct for these issues. Unfortunately, there is little information about the distribution of individuals or farms within counties in the antebellum period that could be reliably used across the entire country.

Antebellum transportation. We rely on the work of Atack (2013) and the resulting GIS shapefiles to identify navigable waterways (canals and steamboat-navigable rivers) and railroads as they existed in the 19th century (Atack, 2015*a*,*b*,*c*). To measure the availability of water-based transportation in modern counties, we detected if a canal or steamboat-navigable river crossed the boundaries of the county or if that county was on the coast of an ocean, the Gulf of Mexico, or one of the Great Lakes. A county meeting any of these criteria was measured as a 1, indicating it had water-based transportation available to it in 1860. A county was counted as having rail-based transportation if a railroad crossed through its territory.

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Table A.1: Summary statistics for the county-level variables.

Statistic	N	Mean	St. Dev.	Min	Max
Outcomes					
Prop. Democrat	1,329	0.314	0.256	0.000	1.000
Support for Affirmative Action	1,329	0.222	0.227	0.000	1.000
Racial Resentment	1,168	4.116	0.701	1.000	5.000
Lynchings per 100,000 1920 Residents, 1882-1930	1,296	8.542	17.428	0.000	178.134
Geographic Variables					
County Area, 2000	1,326	0.151	0.090	0.001	1.497
Ruggedness	1,244	43.377	48.308	2.106	334.972
Latitude, 2000	1,326	34.528	3.068	24.850	40.521
Longitude, 2000	1,326	−87.297	6.675	−106.235	−75.685
1860 Variables					
Prop. Slave, 1860	1,242	0.284	0.214	0.000	0.924
Gini Coefficient for Land Holdings, 1860	1,222	0.481	0.084	0.000	0.789
Prop. Small Farms (< 50 Acres), 1860	1,226	0.466	0.215	0.019	1.000
Total Population, 1860	1,257	8,695.350	9,394.037	0.000	174,491.000
Farm Value per Capita, 1860	1,226	36.473	25.781	4.809	226.300
Prop. Free Black, 1860	1,242	0.010	0.022	0.000	0.246
Rail Access, 1860	1,326	0.245	0.430	0	1
Water Acces, 1860	1,326	0.407	0.492	0	1
Other Variables					
Cotton Suitability	1,261	0.449	0.169	0.002	0.921
Prop. Black, 2000	1,327	0.158	0.171	0.000	0.846
Tractor Growth, 1930-1940	1,295	0.020	0.032	−0.030	0.252

Note: Restricted to counties in the sample states (see text).

Table A.2: Summary statistics for individual-level data.

Statistic	N	Mean	St. Dev.	Min	Max
CCES, 2006, 2008, 2009, 2010, 2011					
Democratic Identification	40,154	0.341	0.474	0	1
Support for Affirmative Action	40,087	0.231	0.421	0	1
Racial Resentment	16,890	4.002	1.137	1.000	5.000
Age	40,154	52.083	15.380	18	96
Religious Importance	40,104	0.725	0.446	0	1
Gender (Female = 1, Male = 0)	40,154	0.524	0.499	0	1
Household Income (Bracket Midpoint)	35,600	64,571.420	43,993.970	5,000	425,000
ANES, 1984-1998					
Black Therm. Score	2,831	62.548	21.622	0	100
White Therm. Score	1,819	74.916	18.780	0	100
White-Black Therm. Difference	1,784	12.854	22.787	−50	100

Note: Restricted to self-identified whites living in the sample states (see text).

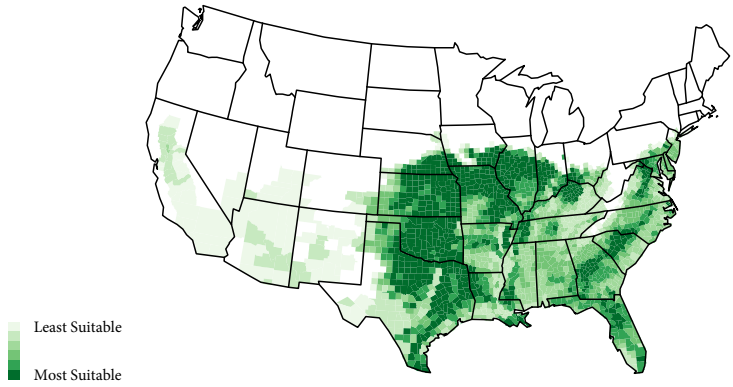

Figure A.1: Cotton suitability as evaluated by the U.N. Food and Agriculture Organization (FAO).

B Supplemental Information: Additional Analyses

B.1 Individual-Level Analyses

See Tables A.3 and A.4.

B.2 Falsification Test of the Instrumental Variable Exclusion Restriction

Figure A.1 presents the suitability of growing cotton in various areas of the country; this map shows that several non-slave areas of the country were suitable for cotton, including parts of the midwest (IL, IN, IA, and NE) and southwest (CA, NM, AZ, and OK).

We present the results of this falsification test in Table A.5. Columns (1), (3), and (5) present the reduced-form relationship between cotton suitability and the three outcome measures in the South, showing that the estimated effects are significant. On the other hand, columns (2), (4), and (6) show that there is no consistent relationship between cotton suitability and political attitudes outside the South. The relationship is only significant for affirmative action, but in this case the result in the opposite direction: higher cotton suitability leads to higher levels of support. If anything, such a positive relationship would bias our results in the conservative direction. As an additional test, we applied the same falsification test to a more historically complete source of data: presidential election returns (which we discuss in additional detail in Section). Drawing on county-level returns (Clubb, Flanigan and Zingale, 2006), we estimated the reduced-form relationships between cotton suitability and the percentage voting for the Democratic presidential candidate for both the South and the non-South in each presidential election from 1872 until 1972, separately. Figure A.2 plots the coefficient and 95% confidence intervals for each of these reduced-form models and shows that there is a strong reduced-form relationship over time in the South (explored further below), but a fairly precisely estimated non-effect in the non-South for the time period between the end of the Civil War and the Voting Rights Act of 1965. Thus, both historically and today, there is little evidence that cotton suitability has any effect on attitudes in the absence of the institution of slavery, making the exclusion restriction appear reasonable in this case and lending credibility to our causal estimates.

B.3 Matching Adjacent Counties

Although our results in Table 1 control for a number of historical and geographic covariates, it remains possible that our results are driven by differences between slaveholding and non-slaveholding areas not fully captured by these covariates. For instance, it could be that the "upland" regions of northern Alabama and Georgia differed systematically from the Black Belt (as suggested by Kousser, 2010). To test the robustness of our results to

Table A.3: Effect of slavery on individual white partisanship, views on affirmative action, and racial resentment.

	Prop. Democrat			Affirm. Action			Racial Resentment		
	Logit			Logit			OLS		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Prop. Slave, 1860	−0.627**	−0.465*	−0.526*	−0.838**	−0.677**	−0.818**	0.454**	0.356*	0.426**
	(0.240)	(0.232)	(0.237)	(0.217)	(0.223)	(0.227)	(0.161)	(0.154)	(0.156)
Log(County Area)	−0.083	−0.082	−0.067	−0.198**	−0.203**	−0.184*	0.183**	0.163**	0.160*
	(0.072)	(0.070)	(0.072)	(0.071)	(0.072)	(0.075)	(0.068)	(0.060)	(0.063)
Ruggedness	0.001†	0.002*	0.002†	−0.00001	−0.0002	−0.0001	−0.0001	0.00004	−0.0001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Land Inequality, 1860	0.076	−0.050	−0.141	−0.100	−0.304	−0.199	0.422	0.363	0.367
	(0.402)	(0.432)	(0.438)	(0.360)	(0.380)	(0.429)	(0.267)	(0.286)	(0.304)
Prop. Small Farm, 1860	0.144	0.196	0.311	0.230	0.300	0.242	−0.203	−0.196	−0.125
	(0.251)	(0.257)	(0.280)	(0.248)	(0.256)	(0.280)	(0.206)	(0.195)	(0.204)
Log(Total Pop., 1860)	−0.034	−0.017	−0.034	0.009	0.039	0.010	−0.080*	−0.074†	−0.070†
	(0.053)	(0.054)	(0.053)	(0.048)	(0.049)	(0.052)	(0.040)	(0.041)	(0.042)
Log(Farm Value per acre, 1860)	0.168**	0.137*	0.091	0.084	0.057	0.065	−0.034	0.010	0.005
	(0.061)	(0.064)	(0.065)	(0.059)	(0.060)	(0.064)	(0.043)	(0.043)	(0.045)
Prop. Free Black, 1860	0.127*	0.104†	0.124†	0.127*	0.109†	0.127*	−0.071	−0.055	−0.069
	(0.062)	(0.061)	(0.065)	(0.057)	(0.059)	(0.063)	(0.050)	(0.048)	(0.052)
Log(Improved Acres, 1860)	3.364*	3.101†	3.105†	1.725	0.925	0.514	−2.509†	−1.434	−1.524
	(1.694)	(1.721)	(1.718)	(1.776)	(2.008)	(2.104)	(1.329)	(1.280)	(1.319)
Rail Access, 1860	0.175**	0.140*	0.153*	0.039	0.009	0.007	−0.048	−0.018	−0.009
	(0.064)	(0.063)	(0.063)	(0.063)	(0.065)	(0.067)	(0.043)	(0.041)	(0.041)
Water Access, 1860	0.002	−0.004	−0.0001	0.029	0.047	0.058	−0.013	−0.035	−0.060
	(0.055)	(0.058)	(0.061)	(0.060)	(0.061)	(0.064)	(0.045)	(0.045)	(0.047)
Educ: HS Grad		−0.063	−0.074		−0.337**	−0.319**		0.235*	0.284**
		(0.102)	(0.109)		(0.095)	(0.102)		(0.093)	(0.098)
Educ: Some College		0.096	0.095		−0.263**	−0.242*		0.138	0.168†
		(0.107)	(0.115)		(0.101)	(0.106)		(0.092)	(0.096)
Educ: 2-year degree		−0.008	−0.021		−0.363**	−0.329**		0.301**	0.360**
		(0.124)	(0.134)		(0.118)	(0.126)		(0.102)	(0.107)
Educ: 4-year degree		0.198†	0.173		−0.204*	−0.188†		−0.157	−0.108
		(0.105)	(0.112)		(0.104)	(0.111)		(0.098)	(0.103)
Educ: Postgrad		0.725**	0.700**		0.392**	0.410**		−0.496**	−0.455**
		(0.126)	(0.134)		(0.123)	(0.129)		(0.098)	(0.102)
Family Inc: $20,000-50,000		−0.151*	−0.135†		−0.455**	−0.463**		0.133**	0.109*
		(0.070)	(0.074)		(0.068)	(0.072)		(0.045)	(0.048)
Family Inc: $50,000-100,000		−0.418**	−0.404**		−0.709**	−0.713**		0.160**	0.130*
		(0.069)	(0.072)		(0.067)	(0.070)		(0.047)	(0.051)
Family Inc: $100,000-150,000		−0.577**	−0.534**		−0.805**	−0.804**		0.179**	0.139*
		(0.085)	(0.087)		(0.087)	(0.091)		(0.059)	(0.063)
Family Inc: $150,000+		−0.501**	−0.445**		−0.706**	−0.679**		0.222**	0.173*
		(0.101)	(0.104)		(0.091)	(0.096)		(0.075)	(0.080)
Religion Import.		−0.943**	−0.965**		−0.481**	−0.495**		0.393**	0.394**
		(0.048)	(0.049)		(0.051)	(0.053)		(0.041)	(0.042)
Female		0.426**	0.419**		0.388**	0.371**		−0.077**	−0.073*
		(0.037)	(0.038)		(0.043)	(0.044)		(0.029)	(0.031)
Age		0.001	0.002		−0.010**	−0.010**		0.004**	0.005**
		(0.001)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)
Prop. Black in Zip, 2000			0.145			0.323†			−0.039
			(0.189)			(0.178)			(0.128)
Log(Median Zip Code Inc, 2010)			−0.247**			−0.116			0.083
			(0.076)			(0.084)			(0.056)
White Unemp. Rate, 2010-2014			−5.414**			−1.834			1.434
			(1.491)			(1.558)			(0.906)
Log(White-Black Median Inc Ratio)			0.110			0.093			0.007
			(0.119)			(0.108)			(0.078)
Constant	10.073	1.695	10.761	20.893	19.717	26.519†	−16.802	−11.792	−11.578
	(14.195)	(14.108)	(14.674)	(13.459)	(13.675)	(14.657)	(11.955)	(11.627)	(12.365)
State-Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Latitude/Longitude	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	33,670	30,796	28,569	33,611	30,737	28,515	13,876	12,651	11,721

Note: †p < .1; *p < .05; **p < .01. Cluster-robust standard errors in parentheses, clustered at the county level. All models include state-year fixed effects, latitude/longitude and their squared terms. "Religion Import" is 1 if a respondent says that religion is important to their lives. Education omitted category is less than a high school degree. Omitted category for family income is less than $25,000. Zip-code level data and white-black median incomes come from the American Community Survey.

Table A.4: Effect heterogeneity of slavery by individual and contextual variables.

	Prop. Democrat		Affirm. Action		Racial Resentment	
	Logit		Logit		OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Prop. Slave × Educ: HS Grad	−0.925†		−0.442		−0.337	
	(0.529)		(0.512)		(0.404)	
Prop. Slave × Educ: Some College	−0.507		−0.285		−0.450	
	(0.566)		(0.554)		(0.411)	
Prop. Slave × Educ: 2-year degree	−0.238		−0.253		−0.413	
	(0.645)		(0.700)		(0.443)	
Prop. Slave × Educ: 4-year degree	−0.253		−0.589		−0.451	
	(0.577)		(0.608)		(0.415)	
Prop. Slave × Educ: Postgrad	−0.514		−0.216		−0.113	
	(0.650)		(0.652)		(0.450)	
Prop. Slave × Family Inc: $20,000-50,000	−0.330		−0.420		0.272	
	(0.345)		(0.361)		(0.224)	
Prop. Slave × Family Inc: $50,000-100,000	−0.013		0.131		0.067	
	(0.352)		(0.348)		(0.235)	
Prop. Slave × Family Inc: $100,000-150,000	−0.001		−0.214		−0.019	
	(0.457)		(0.462)		(0.316)	
Prop. Slave × Family Inc: $150,000+	−0.716		−0.628		0.408	
	(0.467)		(0.460)		(0.328)	
Prop. Slave × Religion Import.	−0.084		−0.640*		0.302	
	(0.227)		(0.264)		(0.197)	
Prop. Slave × Female	0.139		0.062		0.137	
	(0.214)		(0.237)		(0.141)	
Prop. Slave × Age	0.007		0.002		0.002	
	(0.007)		(0.008)		(0.005)	
Prop. Slave × Prop. Black in Zip, 2000		−3.597**		−1.410		1.133†
		(0.957)		(0.893)		(0.614)
Prop. Slave × Log(Median Zip Code Inc, 2010)		−0.522		0.374		0.180
		(0.409)		(0.428)		(0.285)
Prop. Slave × White Unemp. Rate, 2010-2014		−8.091		−8.227		−3.246
		(6.651)		(6.829)		(4.026)
Prop. Slave × Log(White-Black Median Inc Ratio)		−0.060		0.762		0.185
		(0.541)		(0.608)		(0.314)
State-Year Fixed Effects	✓	✓	✓	✓	✓	✓
Lower-Order Terms	✓	✓	✓	✓	✓	✓
Individual-Level Covariates	✓	✓	✓	✓	✓	✓
Contextual Covariates		✓		✓		✓
N	30,796	28,569	30,737	28,515	12,651	11,721

Note: †p < .1; *p < .05; **p < .01. Cluster-robust standard errors in parentheses, clustered at the county level. All models include state-year fixed effects, latitude/longitude and their squared terms. "Religion Import" is 1 if a respondent says that religion is important to their lives. Education omitted category is less than a high school degree. Omitted category for family income is less than $25,000. Zip-code level data comes from the American Community Survey.

Table A.5: Reduced form relationships between cotton suitability and white attitudes in South and Non-South.

	Prop. Democrat		Affirm. Action		Racial Resentment	
	(1)	(2)	(3)	(4)	(5)	(6)
	South	Non-South	South	Non-South	South	Non-South
FAO Cotton Suitability	−0.096*	0.010	−0.096**	0.068*	0.303*	−0.142
	(0.040)	(0.033)	(0.034)	(0.031)	(0.135)	(0.105)
State Fixed Effects	✓	✓	✓	✓	✓	✓
Geographic Controls	✓	✓	✓	✓	✓	✓
N	1,180	476	1,180	476	1,038	416
R^2	0.167	0.297	0.083	0.119	0.096	0.252

Note: $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$. Included in the Non-South are the following states with some positive cotton suitability: AZ, CA, IA, IL, IN, KS, NE, NJ, NM, NY, OH, PA, UT.

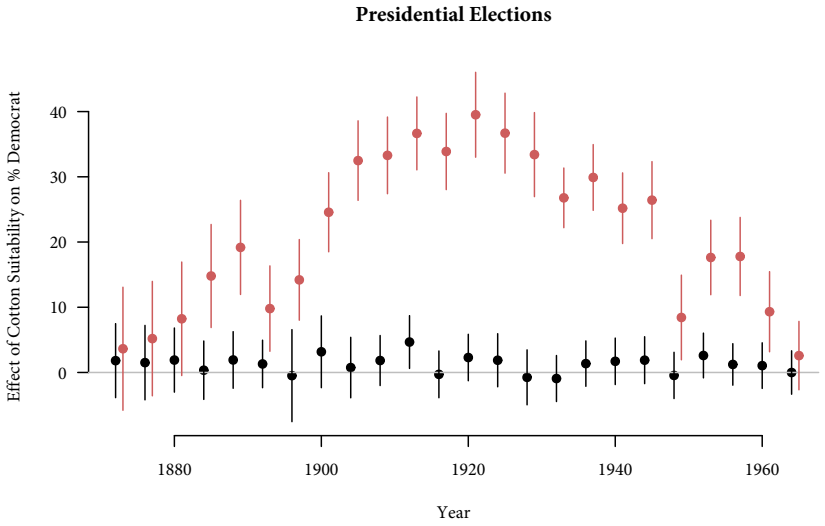

Figure A.2: Reduced-form coefficients and 95% confidence intervals for the effect of cotton suitability on the county Democratic vote-share in presidential elections in the South (red) and the non-South (black).

such confounders, we restrict our sample to the set of neighboring counties that border a county in which proportion slaves differs by more than 20 percentage points. This enables us to compare the effects of slavery across counties that are geographically and perhaps also politically, economically, and culturally similar (as Banerjee and Iyer, 2005, do with Indian districts). It also drops certain former high slave counties that are in regions where all of the neighbors are also high slave areas—for example, the Mississippi Delta. Columns (1), (3), and (5) of Table A.6 show that the results for all three of our CCES outcomes are robust to restricting our analysis to only these neighboring counties, even though this removes more than half of the counties in our original sample. Thus, even within fairly geographically concentrated areas, there are strong, statistically significant differences between counties with higher and lower past concentrations of slaves.

B.4 Counterfactual Comparisons to the North

If the effects that we estimate in Table 1 are genuinely attributable to the local prevalence of slavery, then we should see no difference in our outcomes between areas of the South that were largely non-slaveholding and

Table A.6: Neighbor matching within South and between South and Non-South.

	Prop. Democrat		Affirm. Action		Racial Resentment	
	(1)	(2)	(3)	(4)	(5)	(6)
Prop. Slave, 1860	−0.202**		−0.149**		0.450*	
	(0.068)		(0.056)		(0.228)	
Slave State		0.015		0.017		−0.068
		(0.030)		(0.029)		(0.103)
State Fixed Effects	✓		✓		✓	
1860 Covariates	✓	✓	✓	✓	✓	✓
50% Threshold Match	✓		✓		✓	
North-South Match		✓		✓		✓
N	444	384	444	384	393	354
R^2	0.248	0.118	0.156	0.077	0.195	0.106

Note: †p < .1; *p < .05; **p < .01. Columns (1), (3) and (5) show results of regressions with state fixed effects and 1860 covariates for those counties that border a county in which proportion slave is more than 20 percentage points different. Columns (2), (4) and (6) show difference between slave-state counties with few slaves (< 5% of 1860 population) and non-Southern counties, matched on geography, farm value per capita, and total population. Coefficients are from a regressions on the matched data, that include a dummy variable for "slave state" as well as the 1860 covariates. All models are WLS with county sample sizes as weights.

areas in other parts of the country that also did not have slaves, such as counties in the North. In addition, if no such differences exist, then that would provide evidence against the alternative theory that it is the institutional *legality* of slaveholding, rather than the *local prevalence* of slavery, that is driving our results. Making these comparisons with the North also enables us to address what we consider to be the appropriate counterfactual, which is what contemporary political attitudes in the South would have been had slavery been as non-prevalent in the South as it was in the North.

We therefore examine differences between Southern counties with very few slaves in 1860 and non-Southern counties with no slaves in 1860. To do this, we restrict the data to counties in slave states where fewer than 5% of the county population was enslaved, and then match these counties to similar counties in non-slaves states on geography (latitude/longitude), county size, farm value per capita, mixed-race population, and total county population.[1] We regress each of our three CCES outcome variables on the 1860 covariates as well as on a dummy variable for the county being in a slave state. Columns (2), (4) and (6) of Table A.6 show these results and confirm that there exists no difference between the Southern counties and the non-Southern counties beyond the effect of local slave prevalence. This provides evidence that the local prevalence of slavery, rather than state laws permitting the ownership of slaves, drives our results.

B.5 Rural versus Urban Counties

One possible explanation for our findings not covered in is that large slaveholding counties tend to be more rural today than counties that have smaller slave proportions, maybe because they had plantations and other large farms. Our results might therefore reflect the simple fact that rural counties tend to be more conservative than urban counties.

To examine this possibility, we remove from our dataset the top ten percentile of all Southern counties in terms of 1860 population density. Thus, we remove ten percent of counties that have been historically the most urban. Removing these counties hardly changes the estimated effects of slavery, as indicated in columns (1),

[1] We use coarsened exact matching (CEM) on these variables, employing the default cut-points (Iacus, King and Porro, 2012; Stefano Iacus and Gary King and Giuseppe Porro, 2009). To avoid biasing our results, we drop Maryland and Missouri from the Northern sample since both had non-trivial slavery in 1860. Replicating this analysis with propensity score matching or genetic matching does not substantively change the results (available upon request).

Table A.7: The effects of slavery after eliminating large urban centers and controlling for Civil War destruction.

	Prop. Democrat		Affirm. Action		Racial Resentment	
	(1)	(2)	(3)	(4)	(5)	(6)
Prop. Slave, 1860	−0.097*	−0.124**	−0.131**	−0.173**	0.346*	0.487**
	(0.047)	(0.044)	(0.041)	(0.038)	(0.154)	(0.144)
Civil War Destruction		0.001		−0.006		0.025
		(0.008)		(0.006)		(0.027)
State Fixed Effects	✓	✓	✓	✓	✓	✓
1860 Covariates	✓	✓	✓	✓	✓	✓
Dense 1860 Counties Dropped	✓		✓		✓	
N	1,030	1,011	1,030	1,011	909	909
R^2	0.181	0.198	0.082	0.098	0.089	0.137

Note: $^{\dagger}p < .1$;$^{*}p < .05$; $^{**}p < .01$. All models are WLS with within-county sample size as weights. Dense counties are those defined to be in the top ten percentile of population density in 1860.

(3) and (5) of Table A.7. Our results are therefore unlikely to be attributable to the sparse populations of former slaveholding counties.[2]

B.6 Civil War Destruction

Another possible explanation for our findings not covered in Section is that slaveholding counties were more adversely affected by the Civil War (1861–1865). The damage to infrastructure and the loss of life resulting from the War was extensive and affected the South's agricultural areas disproportionately (Goldin and Lewis, 1975).[3] This could affect our analysis in two ways. First, in light of the federal government's role in the war, whites in war-torn slave counties perhaps became more resentful of the federal government, which in turn they express through resentment toward blacks. Second, it may be that the Civil War disrupted the social fabric of these communities, aggravating racial strife in the process (Collier et al., 2013).

In either case, we would expect the effects of slavery to diminish once we control for Civil War destruction. Columns (2), (4) and (6) of Table A.7 therefore control for U.S. Census measurements of the percentage drop in the average value of farms in the county between 1860 and 1870, which is a proxy for Civil War destruction.[4] As Table A.7 indicates, slavery's effects on our three outcome measures are hardly affected by the inclusion of this variable.[5] Furthermore, in results not presented here, we find that even when we include an interaction term between proportion slave and Civil War destruction, the interaction is not significant.

[2] An alternative strategy is to restrict our attention to counties that are rural today as opposed to rural counties in 1860. This approach, however, potentially suffers from post-treatment bias since the prevalence of slavery in 1860 could affect population density today. Mindful of this possibility, we include such an analysis in the Appendix, noting that the results are consistent with those of Table A.7. Similar results are obtained when controlling for modern-day county population as opposed to 1860 county population.

[3] The correlation between proportion slave in 1860 and our measure of the Civil War's impact (described below) is positive at 0.23.

[4] We assume that ignorability is satisfied here for both slavery and Civil War destruction with the same set of covariates, which would make the effect on the slave variable the controlled direct effect. We believe that this is a more plausible assumption than that made with respect to contemporary black population. The reason is because conditioning on 1860s covariates is likely to result in an accurate estimate of which counties suffered more destruction during the Civil War, but would probably not be sufficient to separately identify the effect of black concentrations in the 20th century.

[5] Since the nature of land value changed dramatically before and after the Civil War due to the emancipation of slaves (Wright, 1986), we use an alternative measure of destruction based on the loss in livestock value in Table A.13. Results using this measure are almost identical to those presented here.

Table A.8: The effect of slavery and antebellum attitudes.

	Prop. Democrat		Affirm. Action		Racial Resentment	
	(1)	(2)	(3)	(4)	(5)	(6)
Prop. Slave, 1860	−0.226**	−0.118**	−0.260**	−0.179**	0.828**	0.499**
	(0.063)	(0.046)	(0.058)	(0.040)	(0.213)	(0.152)
Log Relative Slave Mortality, 1860	0.050**		0.009		−0.132*	
	(0.015)		(0.014)		(0.051)	
Avg. Residents per Slave Dwelling		−0.012**		−0.006*		0.036**
		(0.003)		(0.003)		(0.010)
State Fixed Effects	✓	✓	✓	✓	✓	✓
1860 Covariates	✓	✓	✓	✓	✓	✓
N	495	885	495	885	447	795
R^2	0.251	0.232	0.146	0.106	0.258	0.145

Note: $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$. All models are WLS with within-county sample size as weights. Log Relative Slave Mortality is the natural log of the ratio of the slave mortality rate and the white morality rate, both measured in 1859. Average Residents per Slave Dwelling were calculated by diving the number of slaves on a farm, divided by the number of slave dwellings, and then average across farms in the same county.

C Supplemental Information: Antebellum Measures

Can antebellum attitudes of whites completely explain our results? We addressed this question in the main text by examining presidential vote shares. Here, we provide some additional evidence to shed light on this question.

First, we attempt to proxy the antebellum attitudes of whites toward slaves by measuring the relative mortality of slaves to whites, as measured by the 1860 Census.[6] To account for between county variation in mortality, we measure the relative mortality of slaves to free whites in a county using the natural log of the ratio of the slave mortality rate to the white mortality rate. The mortality rate here refers to the number of deaths divided by the size of the subpopulation in 1860. Negative racial attitudes could have led white planters, farmers, or overseers to increase the morality of slaves, either directly through violence or indirectly through overwork, undernourishment, and poor medical care. Thus, relative slave mortality may, in part, reflect how whites viewed and valued black lives in the antebellum period. If the effect of slavery disappears when controlling for relative slave mortality in our baseline model, we might conclude that antebellum attitudes are driving our findings. These results, reported in in columns (1), (3), and (5) of Table A.8, are largely consistent with our baseline models, albeit with slight differences due to the availability of the mortality data. While these models represent an imperfect test, they do suggest that antebellum attitudes cannot explain the entirety of our baseline results.

Second, we explore the local treatment of slaves as a proxy for attitudes about race. Comprehensive data on racial views are not available in the antebellum period, so we instead look for measures that might be consequences of such attitudes. In particular we use samples from the slave schedules of the 1860 U.S. Census to calculate the average occupant size of slave quarters on farms in a county (Menard et al., 2004). Across the South, the average slave quarters housed around five individuals, though this number varied considerably across counties. Variation in the occupancy of such quarters may come from both variation in the size of slave families and also the propensity of farm owners to place multiple families in the same dwelling. Attitudes about race might affect both of these sources. In fact, there is some evidence that planters engaged in so-called "slave breeding," which entailed various ways of promoting and forcing high fertility rates among enslaved women (Sutch, 1975), though the extent of this practice is contested (Fogel and Engerman, 1995). Also, planters with

[6] We coded the mortality information from the original manuscripts of the Mortality Schedules of the 1860 U.S. Census, which asked census takers to record all deaths that occurred in the year 1859. Data was also collected on the enslaved status, occupation, and age of the deceased. Manuscripts were only available for Alabama, Georgia, Kentucky, Louisiana, South Carolina, Tennessee, Texas, and Virginia.

more extreme negative racial attitudes might provide less housing for their slaves, which would be measured as a higher occupancy in the average slave dwelling. Obviously there are other factors that affect this measure, but it may pick up some degree of planter cruelty or racial animus that is not captured by the density of slavery alone.

We add the the average number of occupants of slave quarters to our baseline specification in columns (2), (4), and (6) in Table A.8. Here we see that both the economic institution of slavery, as measured by proportion slave, and the relative treatment of slaves, as measured by the dwelling size, have independent and significant effects on the attitudes of whites today. Despite the potential significant effect of slave dwelling size on the outcome variables, however, we still see a strong effect of proportion slave on attitudes as well, indicating that the localized prevalence of slavery continues to matter once we account for aspects of how slaves were treated.

D Supplemental Information: Additional Evidence on Geographic Sorting

We also present some suggestive evidence against geographic sorting using data on between-county migration from 1995 to 2000 from the 2000 U.S. Census (Bureau, 2001). These data help us investigate the extent to which *contemporary*, as opposed to historical migration, explains our findings (see Dell, 2010, for a similar analysis). In order for geographic sorting to explain our results, two conditions must hold. The first is that there must be migration from low-slave areas to high-slave areas (or vice-versa); otherwise, there is no meaningful sorting of any kind. To test this condition, we use county-to-county migration data to calculate dyads of where people move to and from, measuring the absolute difference in the proportion 1860 slave between the departing and receiving county; this enables us to assess how much migration exists between low-slave and high-slave areas. Appendix Figure A.3 shows the relationship between these flows and the difference in proportion slave, and it demonstrates that, as the slavery differential grows, the migrations between counties drops significantly. Thus, the vast majority of contemporary migration is within low-slave areas or within high-slave areas, not between.

The second condition for sorting to explain our findings is that racially conservative whites are moving into high-slave areas, racially liberal whites are moving out of high-slave areas, or some combination. Even if there is very little migration between low- and high-slave counties (as shown in Appendix Figure A.3), the distribution of political beliefs among these migrants could be so highly skewed so as to produce our results. (For example, perhaps all of the out-migrants from high-slave counties are racially liberal and all of the in-migrants to high-slave counties are racially conservative.) With regard to the first possibility, this seems unlikely to be the primary mechanism as it relies on racially hostile whites moving to areas with extremely large proportions of African Americans. For example, Farley et al. (1994), show that anti-black attitudes are correlated with stronger preferences for geographic segregation. More plausible is the exodus of racially liberal whites from former slave counties. To check this, again using contemporary data, we examine the relationship between the proportion slave in 1860 and out-migration in 1995-2000 census records. We find that proportion slave actually has a *negative* effect on contemporary out-migration (Appendix Table A.9). Thus, we have no evidence drawn from contemporary data for any of the necessary conditions behind a geographic sorting explanation.

E Supplemental Information: Additional Tables and Figures

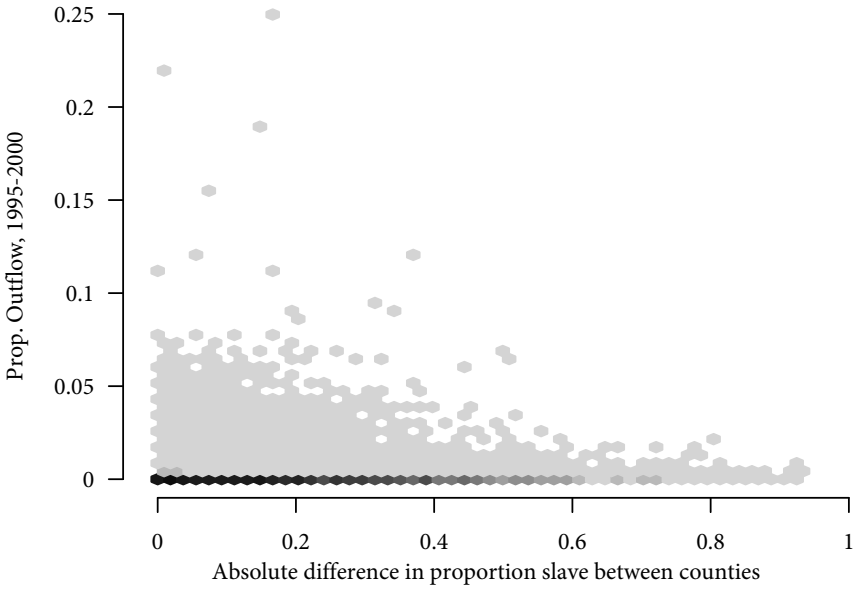

Figure A.3: Relationship between county-to-county migration and the similarity of those counties on proportion slave. The darker the hexagon, the more county-dyads in that bin. The x-axis represents the absolute difference between the counties in term of proportion slave in 1860 and the y-axis represents the migration (1995–2000) from the sending county to the receiving county as a proportion of the sending county's 2000 population.

Table A.9: Effect of slavery present-day migration.

	In-migration	Out-migration
	(1)	(2)
Prop. Slave, 1860	-0.058^{**}	-0.059^{**}
	(0.015)	(0.015)
State Fixed Effects	✓	✓
1860 Covariates	✓	✓
N	1,207	1,207
R^2	0.183	0.184

Note: $^{\dagger}p < .1; ^{*}p < .05; ^{**}p < .01$. Out-migration is defined as the total number of out-migrants from a county from 1995 to 2000 divided by the total population in 2000. In-migration is similarly defined.

Table A.10: Feeling thermometer score effect disaggregated by race.

	Black Therm. Scores		White Therm. Scores	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Prop. Slave, 1860	-18.678^{*}	-19.261^{**}	10.452	$17.226^{\dagger}$
	(7.710)	(5.043)	(6.847)	(9.494)
Clustered SEs	✓	✓	✓	✓
State-Year Fixed Effects	✓	✓	✓	✓
Geographic Controls	✓	✓	✓	✓
1860 Covariates	✓		✓	
N	2,407	2,006	1,517	1,277
R^2	0.112	0.069	0.085	0.069

$^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$. All analyses are at the individual level with standard errors clustered at the county level. Data from the ANES 1984-1998.

Table A.11: County-level regression models for the effect of slavery on difference between ANES feeling thermometer scores for whites and blacks.

	White Thermometer - Black Thermometer (-100 to 100)			
	OLS			IV
	(1)	(2)	(3)	(4)
Prop. Slave, 1860	27.215^{**}	19.850^{**}	$28.856^{\dagger}$	51.748^{*}
	(7.379)	(6.735)	(14.358)	(19.305)
Clustered SEs	✓	✓	✓	✓
State-Year Fixed Effects	✓	✓	✓	✓
Geographic Controls	✓	✓	✓	✓
1860 Covariates	✓		✓	
N	56	56	54	51
R^2	0.201	0.715	0.793	0.675

$^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$. All analyses are at the county level, weighted by within-county sample sizes adjusted for ANES survey weights. Data from the ANES 1984-1998.

Table A.12: First stage estimates from the sequential g-estimation model of Table 3.

	Prop Democrat	Affirm. Action	Racial Resentment
	(1)	(2)	(3)
Prop. Slave, 1860	$-0.092^{\dagger}$	-0.080^{*}	$0.289^{\dagger}$
	(0.048)	(0.041)	(0.156)
Prop. Black 2000	0.028	-0.112^{*}	-0.072
	(0.056)	(0.048)	(0.179)
Log Population, 2000	0.017^{**}	0.023^{**}	-0.071^{**}
	(0.006)	(0.005)	(0.021)
Percent High School Graduates, 1990	0.003^{**}	0.002^{*}	$-0.006^{\dagger}$
	(0.001)	(0.001)	(0.003)
Unemployment, 1999	0.004	-0.003	0.023^{*}
	(0.003)	(0.003)	(0.011)
Median Income, 2000	-0.139^{**}	-0.208^{**}	0.460^{**}
	(0.042)	(0.036)	(0.136)
White-Black Income Ratio, 2000	0.016	0.013	-0.019
	(0.012)	(0.010)	(0.039)
State Fixed Effects	✓	✓	✓
1860 Covariates	✓	✓	✓
N	1,093	1,093	976
R^2	0.241	0.148	0.186

Note: $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$. Note that conditional on the past, proportion black today lacks explanatory power. While the estimates of proportion slave are insignificant in these models, their estimates possess large amounts of post-treatment bias due to the contemporary variables. Each model includes weights for the within-county sample size.

Table A.13: Alternative measure of civil war destruction based on livestock value.

	Prop. Democrat	Affirm. Action	Racial Resentment
	(1)	(2)	(3)
Prop. Slave, 1860	-0.123^{**}	-0.171^{**}	0.483^{**}
	(0.044)	(0.038)	(0.144)
Livestock Value Loss, 1860-1870	0.0003	0.0004	-0.002
	(0.001)	(0.001)	(0.004)
State Fixed Effects	✓	✓	✓
1860 Covariates	✓	✓	✓
N	1,010	1,010	908
R^2	0.198	0.097	0.136

Note: $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$. Livestock value loss is the decrease in the value of livestock in a county between 1860 and 1870 as a proportion of 1860 livestock value.

Table A.14: Effect of slavery on measures of inequality and income.

	log White-Black Wage Gap, 1940		log White-Black Med. Income Gap, 2014		log White Median Income, 2014	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Prop. Slave, 1860	0.578**	1.316**	0.391**	1.047**	0.113*	0.820**
	(0.183)	(0.466)	(0.094)	(0.210)	(0.052)	(0.123)
State Fixed Effects	✓	✓	✓	✓	✓	✓
Geographic Controls	✓	✓	✓	✓	✓	✓
1860 Covariates	✓		✓		✓	
N	877	866	994	1,020	1,152	1,120
R^2	0.188	0.151	0.147	0.052	0.449	0.252

Note: $^{\dagger}p < .1$; $^{**}p < .05$; $^{***}p < .01$. Outcomes from the 1940 U.S. Census and the 2009-2014 American Community Survey.

Table A.15: Interaction of slavery with income on individual white attitudes.

	Prop Democrat	Affirm. Action	Racial Resentment
	logistic	logistic	OLS
	(1)	(2)	(3)
Prop. Slave, 1860	−0.483	−2.103†	0.317
	(1.131)	(1.232)	(0.844)
Income	−0.188**	−0.270**	−0.048*
	(0.035)	(0.038)	(0.024)
Prop. Slave × Income	0.0003	0.145	0.002
	(0.105)	(0.115)	(0.077)
State Fixed Effects	✓	✓	✓
1860 Covariates	✓	✓	✓
Clustered SEs	✓	✓	✓
N	29,944	29,888	12,213
R^2			0.030
AIC	37,739.610	32,067.970	

Note: $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$.

Table A.16: How the effect of slavery varies by declines in the black population in the 20th century.

	Prop. Democrat		Affirm. Action		Racial Resentment	
	(1)	(2)	(3)	(4)	(5)	(6)
Prop. Slave, 1860	−0.097*	−0.072	−0.112**	−0.119**	0.345*	0.162
	(0.049)	(0.054)	(0.042)	(0.046)	(0.162)	(0.176)
Prop Black Decline, 1940-1920	0.064		0.045		−1.012	
	(0.269)		(0.230)		(0.854)	
Prop Black Decline, 1970-1920		−0.118		−0.158†		0.402
		(0.110)		(0.094)		(0.336)
Prop Slave × Black Decline, 1940-1920	−0.466		−0.486		2.934	
	(0.599)		(0.513)		(1.903)	
Prop Slave × Black Decline, 1970-1920		−0.053		0.120		0.644
		(0.224)		(0.192)		(0.694)
State Fixed Effects	✓	✓	✓	✓	✓	✓
1860 Covariates	✓	✓	✓	✓	✓	✓
N	1,152	1,151	1,152	1,151	1,027	1,026
R^2	0.204	0.205	0.103	0.104	0.145	0.151

Note: †p < .1; *p < .05; **p < .01.

Table A.17: Effect of slavery on intermediate outcomes.

	Black-White Tenancy Share Gap, 1925		Black-White Ownwer Share Gap, 1925	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Prop. Slave, 1860	0.102*	0.894**	−0.029	−0.700**
	(0.048)	(0.153)	(0.048)	(0.144)
State Fixed Effects	✓	✓	✓	✓
Geographic Controls	✓	✓	✓	✓
1860 Covariates	✓		✓	
N	1,029	1,005	1,029	1,005
R^2	0.372	0.046	0.325	0.105

Note: †p < .1; *p < .05; **p < .01. Black-White Tenancy Share Gap is the difference between the proportion of all black farms under tenancy agreements minus the proportion of all white farms under tenancy agreements. Black-White Owner Share Gap is the difference between the proportion of all black farms owned by the (black) operator minus the proportion of all white farms owned by the (white) operator.

Table A.18: Association between tractor growth and racial disparities.

	Lynchings per 100,000 residents	Log Black-White Wage Ratio, 1940
	(1)	(2)
Tractor Growth, 1930-1940	−3.069	−1.021
	(16.532)	(0.818)
Prop. Slave, 1860	13.500**	−0.602**
	(4.061)	(0.185)
State-Year Fixed Effects	✓	✓
1860 Covariates	✓	✓
N	1,145	871
R^2	0.385	0.190

Note: $^{\dagger}p < .1$; $^{*}p < .05$; $^{**}p < .01$.

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