Slavery, Politics, and Causality[*]

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1 Introduction

How has slavery shaped the politics of the United States over the last century and a half since emancipation? Our book, *Deep Roots* (Acharya, Blackwell and Sen, 2018), tackled this question using a combination of quantitative, historical, and theoretical tools. Building on our earlier article published in *The Journal of Politics* (Acharya, Blackwell and Sen, 2016), our book shows a clear, persistent correlation between the proportion of enslaved people in a Southern county in 1860 and the political attitudes of whites living in those counties in the 20th and early 21st centuries, especially on issues related to race. The book employs various identification strategies and falsification tests to establish these relationships as plausibly causal. It presents evidence that these patterns cannot easily be explained by theories of racial threat or by antebellum attitudes on race, but, rather, that the political economy of the post-Civil War period generated incentives for whites of all social strata to adopt strongly anti-Black views, which have been passed down in local communities over time.

Recently, Bateman and Schickler (2023, hereafter B&S) have made a valuable contribution on the relationship between slavery and political outcomes in the antebellum South. B&S use their core empirical results to question the causal claims presented in *Deep Roots*. They argue that the existence of correlations between slavery and specific antebellum political patterns implies a pre-existing causal relationship between slavery and antebellum political outcomes. They argue that this refutes the identifying assumptions in *Deep Roots* and undermines the book's substantive arguments regarding the political importance of the post-emancipation period.

While B&S's results show that antebellum political outcomes varied with slavery prior to 1860, these relationships do not speak to the validity of the arguments in *Deep Roots*. As B&S point out,

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> The identification strategy for [Acharya, Blackwell & Sen's] causal argument hinges on the assumption that the local prevalence of slavery in the South was *conditionally* uncorrelated with support for the institution, adherence to white supremacy, or other racial attitudes in the pre-Civil War era. (B&S, p. 97, emphasis added).

However, almost all of the relationships that B&S present are *unconditional* bivariate relationships. None of their tests condition on the basic set of controls (geographic, social, or demographic controls) that guide our identifying assumptions and theory and are essential for making causal claims in the antebellum period. When we correct this omission and incorporate basic geographic and/or demographic controls, most of B&Ss's descriptive relationships vanish. This suggests little to no evidence of a *conditional* slavery-politics relationship in the antebellum period, undermining B&S's claim that their descriptive results are informative of the causal argument of *Deep Roots*. Their empirical evidence tests assumptions of a design that *Deep Roots* did not use.

There are two broad pieces of quantitative evidence that B&S analyzes, both of which suffer from these shortcomings. First, in the Secession Crisis of 1850 from Georgia, Mississippi, and South Carolina, B&S find slavery becomes a significant predictor of voting in these elections once past county partisanship is added as an interactive term. In fact, including basic geographic and social controls—the same ones used in our book in our instrumental variables and selection-on-the-observables designs, respectively—yield statistically and substantively insignificant results. Second, we see a similar pattern with roll call votes in North Carolina and Tennessee in the early 1830s, finding that B&S's results disappear when we either condition on basic covariates, account for clustering of delegates using clustered standard errors, or use an instrumental variable strategy to assess the causal effect of slavery. In Virginia roll call votes, we find that significant effects of slavery hold for only two of the six votes analyzed by B&S. As a robustness check on the main *Deep Roots* results, we drop Virginia (and thus also West Virginia)—the one state where a robust, perhaps causal relationship between slavery and antebellum politics exists—and show that our main results hold.

However, a collection of null results cannot prove the casual assumptions of *Deep Roots* are correct. Those assumptions, such as no unmeasured confounders or the exclusion restriction, are fundamentally untestable. What if B&S are right that past levels of slavery are confounders for slavery in 1860, and the imperfect proxies of politics in both B&S and *Deep Roots* capture this confounding poorly? To investigate this, we replicate the main findings of *Deep Roots* conditioning on various measures of slavery before 1860 and find the results consistent with the original results, in spite of high degrees of multicollinearity between these variables. Thus, even if earlier slavery were a confounder for slavery in 1860, this confounding does not appear to drive the main results of *Deep Roots*.

Ultimately, B&S's bivariate results are not relevant to our identification assumptions or whether these patterns explain our persistence findings. Their findings are noteworthy historical facts, but

they are not tests of the causal design in *Deep Roots*.

We proceed as follows. In Section 2 we clarify why conditioning on covariates is so crucial to testing causal assumptions. Section 3 presents the analysis of the secession votes of the early 1850s, while Section 4 focuses on the roll call votes of the early 1830s. In Section 5, we present the replication of the *Deep Roots* results conditioning on earlier slavery. We conclude in Section 6.

2   How to Test Causal Assumptions

As discussed above, one advantage of the modern approach to causal inference is the transparency of the assumptions required to justify interpreting a relationship as causal. The explicit worry that B&S discuss is that slavery at points earlier than 1860 may have affected antebellum politics in ways that have persisted until today. We clarify their critique in the context of the 1850 secession votes using a directed acyclic graph (DAG) in Figure 1. The figure shows the confounding between slavery in 1860 and 1860 covariates, which formed the basis of our selection-on-observables strategy in *Deep Roots*. B&S are interested in how slavery in 1850 might affect politics in 1850 (the blue path labeled (a)), which in turn might affect politics today (path (b), which we have dashed because these arrows are not explored by us or B&S). B&S see the existence of path (a) as a crucial test of our assumptions. If paths (a) and (b) both exist, there would be unmeasured confounding biasing our estimates of the effect of 1860 slavery on modern attitudes.

How should we test for this crucial path (a)? B&S focus on a descriptive approach that conditions on minimal covariates. However, as Figure 1 shows, these types of descriptive results might themselves be biased in establishing a causal relationship because they exclude key confounders. Thus, to estimate the effect of 1850 slavery on politics in 1850 and to assess if the assumptions of *Deep Roots* are invalid, we have to investigate the relationship between 1850 slavery and politics *conditional* on plau-
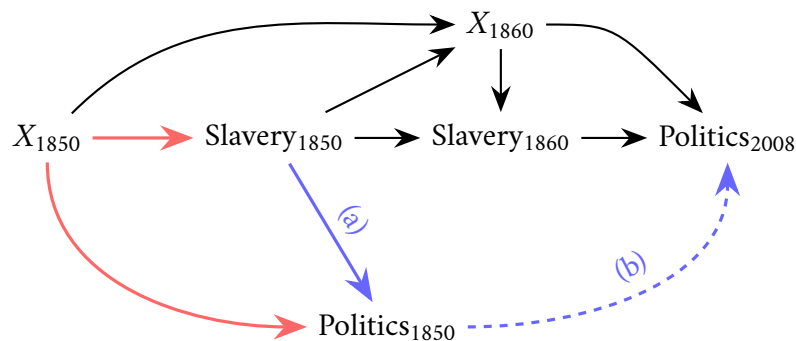

Figure 1: Directed acyclic graph showing the causal relationships under selection on observables.
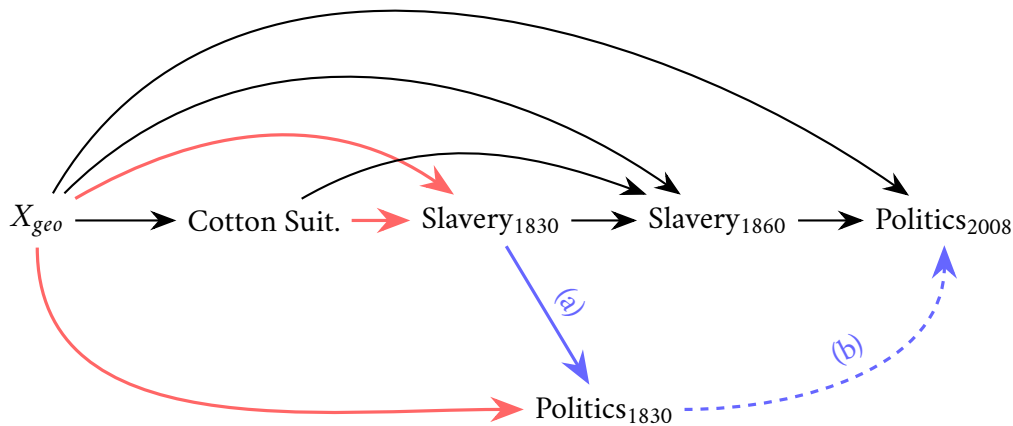
Figure 2: Directed acyclic graph showing the causal relationships under instrumental variables.

sible pretreatment covariates. In our empirical tests below, we extend the B&S descriptive findings in precisely this way and find little evidence for this crucial (a) path.

The situation changes little when we consider our instrumental variable (IV) strategy instead of selection on observables. In the design of *Deep Roots*, we instrumented slavery with cotton suitability conditional on geographic controls that might confound the relationship between cotton suitability and politics today. B&S worry that cotton suitability could have impacted slavery earlier, such as in 1830, which might have affected politics in 1830, which in turn may have had a persistent effect on politics today. This worry can be found in the two blue paths in Figure 2 and would represent a violation of the exclusion restriction of cotton with respect to slavery in 1860: there is a causal pathway from the instrument (cotton suitability) to the outcome (politics in 21st century) not through the treatment (slavery in 1860). B&S again focus on estimating the crucial (a) path in this DAG, from slavery in 1830 to politics in 1830. If such a path could be established, we would rightly worry about an exclusion restriction violation, which would call into question our IV estimates.

Unfortunately, B&S focus on descriptive relationships to estimate the (a) path, not conditioning on any of the geographic controls we included in *Deep Roots*. Thus, their estimates are potentially subject to omitted variable bias for the effect of cotton suitability and, ultimately, slavery in 1830. If this pathway disappears when we condition on these geographic controls, then this pathway cannot affect the results we document in *Deep Roots*. Below, this is largely what we find.

Any attempt to undermine the validity or plausibility of causal assumptions must engage with the design that is actually being critiqued. The descriptive facts that B&S find are important, but their

relevance to the causal argument of *Deep Roots* is minimal.

3   Evidence from the Secession Crisis of 1850

The first empirical evidence that B&S provides for the antebellum slavery-politics connection extends analyses in *Deep Roots*. For presentational purposes, *Deep Roots* presented bivariate relationships between the density of slavery and the county-level electoral outcomes of the 1851 gubernatorial races in Georgia and Mississippi. B&S take those basic relationships and add an interaction with the average vote for the Whig party in the previous five years and find that slavery now is a significant predictor. We can see this contrast in results in the first two columns of Tables 1 and 2.

Upon closer inspection, these results are highly sensitive to the inclusion of the identical controls *Deep Roots* used in the main analyses. We consider two sets of basic controls from the *Deep Roots* analyses. First, we use geographic controls for the proportion enslaved in 1850, which include the latitude and longitude of a county (and their squared terms), log of the county area, a measure of the ruggedness of the county, and a measure of water access in 1850. These are also the basic controls we used when estimating our effects based on our instrumental variables approach. Second, we include an expanded set of covariates ("all covariates") that adds the basic social and demographic controls that *Deep Roots* used in the selection-on-observables strategies.[1]

For B&S's argument about our assumptions to be persuasive, we would expect the slavery prevalence variable to continue to predict the 1850/1851 election outcome even conditional on these attributes, which are causally prior to the slave measure and the election outcome. However, as the third and fourth columns of Tables 1 and 2 show, the slavery prevalence variable loses almost all predictive power for the 1850/1851 election outcomes once conditioning on these variables. Moreover, we also conducted F-tests on the inclusion of "prop enslaved" in these models in both the interaction and the lower-order term. Across the 16 specifications in Tables 1 and 2, we find one test significant at the 0.05 level and two at the 0.1 level, which is consistent with any slavery-politics relationships here being entirely due to random chance.

As B&S point out, the identification strategy of *Deep Roots* relies on conditional uncorrelatedness (B&S p. 97). Once we include those conditioning variables, the relationships that drive skepticism about *Deep Roots*' designs disappear. We note that these null findings are not driven by issues of statistical power. While the standard errors of our estimates increase slightly when including covariates, the point estimates are also moving fairly sharply toward zero. This indicates that our controls are

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[1]These include the proportion of free Blacks in 1850, the log of the total 1850 population, the presence of rail transport in 1850, the log of the farm value per capita in 1850, and the log of the total acres improved in 1850. In *Deep Roots*, we also included a measure of land inequality and the proportion of small farms, but the inputs to these variables are not present in the 1850 census.

Table 1: Relationship between slavery and electoral outcomes in Georgia, 1850-51

	DR	B&S	B&S + Geo Covs	B&S + All Covs
GA Convention, 1850				
Prop Enslaved	-0.194	-1.411***	-0.866+	-0.470
	(0.132)	(0.371)	(0.484)	(0.562)
Prop Enslaved x Prop Whigs		1.959**	1.133	1.100
		(0.734)	(0.854)	(0.877)
Num.Obs.	91	87	87	87
R2	0.024	0.221	0.350	0.386
GA Governor, 1851				
Prop Enslaved	-0.094	-0.865***	-0.125	0.057
	(0.076)	(0.176)	(0.202)	(0.226)
Prop Enslaved x Prop Whigs		0.993**	-0.070	-0.081
		(0.347)	(0.363)	(0.355)
Num.Obs.	93	90	90	90
R2	0.016	0.461	0.633	0.683
GA Congress, 1850				
Prop Enslaved	-0.102	-0.945***	-0.143	0.042
	(0.080)	(0.192)	(0.212)	(0.237)
Prop Enslaved x Prop Whigs		1.130**	0.023	0.004
		(0.380)	(0.381)	(0.373)
Num.Obs.	93	90	90	90
R2	0.018	0.434	0.644	0.693
GA Swing, 1849-1851				
Prop Enslaved	-0.491***	-0.856***	-0.197	0.018
	(0.065)	(0.178)	(0.217)	(0.229)
Prop Enslaved x Prop Whigs		1.115**	0.123	0.112
		(0.353)	(0.389)	(0.360)
Num.Obs.	91	90	90	90
R2	0.387	0.524	0.639	0.722

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2: Relationship between slavery and electoral outcomes in Mississippi, 1850-51

	DR	B&S	B&S + Geo Covs	B&S + All Covs
MS Convention, 1850				
Prop Enslaved	0.336**	-0.857+	-0.656	-0.470
	(0.111)	(0.445)	(0.459)	(0.477)
Prop Enslaved x Prop Whigs		1.800+	1.430	1.894
		(0.960)	(1.150)	(1.130)
Num.Obs.	57	57	57	57
R2	0.144	0.418	0.551	0.649
MS Governor, 1851				
Prop Enslaved	0.232**	-0.448+	-0.174	-0.332
	(0.080)	(0.256)	(0.241)	(0.279)
Prop Enslaved x Prop Whigs		0.721	-0.050	0.378
		(0.553)	(0.597)	(0.650)
Num.Obs.	52	52	52	52
R2	0.145	0.625	0.769	0.793
MS Congress, 1850				
Prop Enslaved	0.291***	-0.366	-0.087	-0.094
	(0.079)	(0.283)	(0.258)	(0.297)
Prop Enslaved x Prop Whigs		0.678	-0.021	0.231
		(0.611)	(0.631)	(0.686)
Num.Obs.	58	58	58	58
R2	0.194	0.568	0.735	0.746
MS Swing, 1849-1851				
Prop Enslaved	-0.123*	-0.666*	-0.417	-0.487
	(0.056)	(0.259)	(0.260)	(0.305)
Prop Enslaved x Prop Whigs		1.271*	0.458	0.796
		(0.557)	(0.644)	(0.710)
Num.Obs.	52	52	52	52
R2	0.087	0.180	0.423	0.470

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 3: Relationship between slavery and a vote for Southern Rights conference delegates in South Carolina in 1850

	B&S	B&S + Geo Covs	B&S + All Covs
Prop Enslaved	−0.803***	−0.675*	−0.231
	(0.206)	(0.290)	(0.518)
Num.Obs.	25	25	25
R2	0.398	0.676	0.743

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

possibly accounting for confounding in these slavery-politics relationships—exactly why we leverage them in the main analyses of *Deep Roots*.

We find a similar pattern in the results for B&S's South Carolina convention delegate elections results. As B&S point out, there was comparatively little Whig voting in South Carolina in this period, so they investigate the bivariate relationship between the proportion enslaved and the vote for delegates who supported South Carolina seceding from the Union alone after other Southern states had rejected secession. Table 3 shows, however, that this striking bivariate relationship is also not robust to the inclusion of covariates.

4   Roll Call Votes in NC and TN

We now turn toward the second set of results presented in B&S: a series of roll-call votes about slavery and the free Black population in the early 1830s in North Carolina, Tennessee, and Virginia. These data connect the votes of delegates to the state legislatures with the density of slavery in the counties of their districts. In the supplemental materials of B&S, they use instrumental variables to estimate the causal effect of slavery in 1830 on these votes.

We find two flaws in these analyses. First, as with the evidence from the 1850s, B&S do not include the basic geographic controls on which we based our instrumental variable strategy. Including these controls is vital: the instrument of cotton suitability has a good deal of spatial dependence, so it is crucial to include these pretreatment confounders in any analysis based on this IV. (We focus on IV analyses here instead of the OLS-based selection-on-observables because the 1830 census has very few economic or demographic controls available.)

Second, B&S analyze these votes at the delegate level, but many counties have multiple delegates, leading to a lack of independence and standard errors that are biased toward zero. (In other words, they might double count certain counties and makes results appear statistically significant when they

are not.) We correct this by appropriately clustering standard errors at the county level (or district in Virginia).

In Table 4, we present the results for the votes in NC and TN, corresponding to Figure 6 in B&S. Including basic controls makes one of the conditional relationships insignificant. The instrumental variable results show that none of these relationships is statistically significant.[2] The bivariate relationships identified by B&S would pose a threat to the causal identification of *Deep Roots* if they were consistent with the same variation used in our instrumental variable analysis. But they are not. While there is interesting variation across NC or TN in these 1830s votes, this variation does not speak to the validity of *Deep Roots'* instrumental variable strategy.

In Table 5, we present the results for the Virginia votes, where there was a debate over slavery and the status of free Blacks in 1830-1832. Here we see a similar pattern as in TN and NC but with more robust relationships. In Virginia, two votes appear to be related to slavery in 1830.[3]

While these results could be a possible threat to *Deep Roots'* identification strategy, this interpretation has three weaknesses. First, as a substantive matter, many of these votes are, at best, weak proxies for the kinds of progressive views on race that would be necessary for a falsification test. Specifically, supportive views of abolition ("aye" votes) did not necessarily reflect more racially progressive views: many (perhaps most) "aye" delegates wanted to end slavery and then forcibly remove any Black people from the state of Virginia and deport them to Africa (see Wolf, 2006; Root, 2010, for a summary and collection of the debates surrounding these votes).[4] In addition, many pro-abolition forces in the western part of the state were resentful of how the enslaved were counted in apportionment decisions since it led the white planter elite to have an outsized role in state politics. Only a small minority of petitions on the issue, mainly from Quakers, spoke to the issue of the rights or treatment of the enslaved.[5] Taken together, the "aye" votes do not appear to clearly correspond to

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[2]The relatively large but noisily estimated effect of slavery on the free Black suffrage in NC is driven by a weak first-stage relationship between cotton suitability and slavery in that state after conditioning on our geographic controls. This implies that any slavery-politics relationships in North Carolina in the 1830s are unlikely to be causing bias in our main IV analyses since the instrument does not impact the treatment in that state. This does not invalidate the IV analyses. Instead, it clarifies that IV analyses focus on units that "comply" with the treatment.

[3]For these analyses, we also include an indicator for whether the county corresponds to West Virginia after the two states split in 1863. This accounts for the distinct ruggedness of the western Virginian counties that made the land considerably less suitable for cotton.

[4]The specific emancipation proposal under debate was made by Thomas Jefferson Randolph of Albemarle County (51.6% enslaved) and "sought not to guarantee freedom for African Americans so much as to rid Virginia of slaves" (Wolf, 2006, p. 213). In reality, legislators never directly voted on this measure but instead voted on the question of whether a committee report should be amended to say that "it is expedient for the present to make any legislative enactments for the abolition of slavery," where the original used "inexpedient" in place of "expedient" (Wolf, 2006, p. 212). The vote analyzed by B&S was the vote on this amendment.

[5]The historian Eva Sheppard Wolf states that the "Virginians calling for emancipation in 1831-32, with the exception

Table 4: Estimated effects of slavery on roll call votes in North Carolina and Tennessee in the early 1830s.

	B&S	+ Covariates	Reduced Form	First Stage	IV
Vote Against Free Black Suffrage, 1834 (TN)					
Slavery, 1830	1.356*	0.665			0.210
	(0.562)	(0.927)			(1.572)
Cotton Suitability			0.169	0.804***	
			(1.357)	(0.211)	
Num.Obs.	55	55	55	55	55
R2	0.101	0.215	0.140	0.605	0.149
Vote Against Free Black Suffrage, 1834 (NC)					
Slavery, 1830	1.435***	1.546**			8.694
	(0.322)	(0.500)			(9.537)
Cotton Suitability			-0.888	-0.102	
			(0.602)	(0.147)	
Num.Obs.	121	121	121	121	121
R2	0.156	0.332	0.266	0.550	-1.607
Vote Against Free Black Suffrage, 1834 (NC)					
Slavery, 1830	1.435***	1.546**			8.694
	(0.322)	(0.500)			(9.537)
Cotton Suitability			-0.888	-0.102	
			(0.602)	(0.147)	
Num.Obs.	121	121	121	121	121
R2	0.156	0.332	0.266	0.550	-1.607
Vote to Block Emancipation, 1834 (TN)					
Slavery, 1830	2.264***	1.012			0.740
	(0.424)	(0.936)			(1.634)
Cotton Suitability			0.529	0.715***	
			(1.281)	(0.204)	
Num.Obs.	57	57	57	57	57
R2	0.272	0.400	0.358	0.589	0.382

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Estimated effects of slavery on roll call votes in Virginia in the early 1830s

	Bivariate	+ Covariates	Bivariate IV	IV + Geo Covs
Restriction 1				
Slavery, 1830	1.683***	1.225***	1.542***	1.099
	(0.133)	(0.318)	(0.275)	(0.750)
Num.Obs.	114	102	108	102
R2	0.447	0.499	0.455	0.480
Restriction 2				
Slavery, 1830	1.683***	1.416**	1.774***	2.341*
	(0.133)	(0.432)	(0.264)	(0.978)
Num.Obs.	114	99	106	99
R2	0.447	0.455	0.436	0.419
Abolition				
Slavery, 1830	-1.957***	-1.966***	-2.144***	-2.441***
	(0.108)	(0.268)	(0.271)	(0.628)
Num.Obs.	126	110	119	110
R2	0.619	0.708	0.641	0.695
Consenual Removal				
Slavery, 1830	-1.010***	-0.380	-0.407	0.684
	(0.193)	(0.464)	(0.363)	(0.909)
Num.Obs.	120	105	113	105
R2	0.162	0.255	0.109	0.148
Removal Bill				
Slavery, 1830	0.890***	0.968+	1.069*	0.288
	(0.227)	(0.511)	(0.408)	(0.996)
Num.Obs.	114	99	108	99
R2	0.137	0.301	0.123	0.247
Police Bill				
Slavery, 1830	1.588***	2.143***	0.967*	-0.558
	(0.158)	(0.399)	(0.423)	(1.364)
Num.Obs.	115	101	110	101
R2	0.427	0.478	0.369	0.215

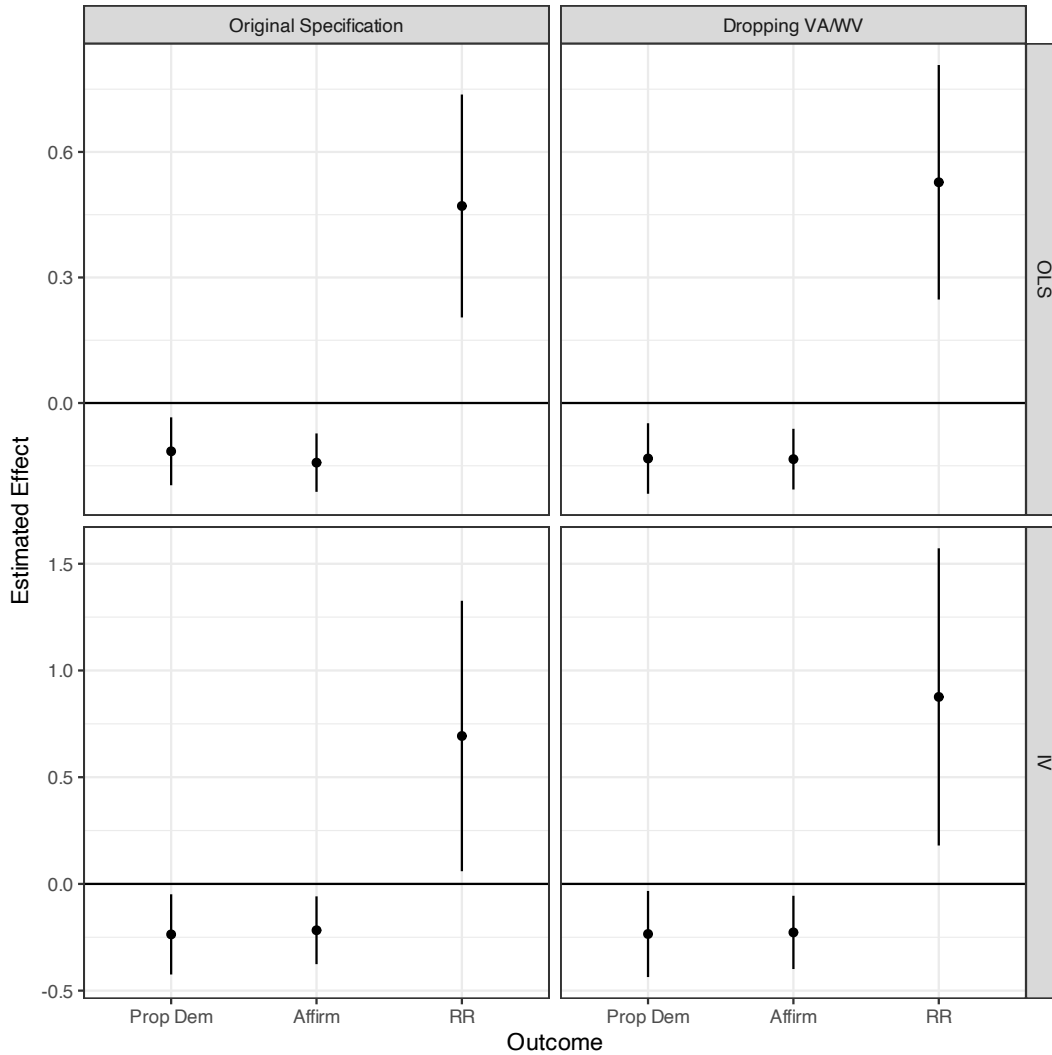+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 3: Original ABS results (left), results dropping VA and WV (right).

more progressive views on race or condemnation or rejection of slavery, as would be necessary for a good proxy outcome.

Second, as an empirical matter, if these debates were materially relevant to our main persistence results, then dropping Virginia (and West Virginia) from our analyses should impact the estimates of persistent effects. In Figure 3, we show that this is not the case. Comparing the first column with our original OLS and IV results with the second column, which has the same specification but drops

of the Quakers, acted out of self-interest more than because of religious or philosophical ideals" (Wolf, 2006, pp. 201-2). Further, she argues that the opposition was due more to the economic divisions in the state and the growing number of Virginians "who saw African Americans more as a threat than a necessity" (Wolf, 2006, p. 202). Most of the delegates that spoke in favor of emancipation would "more accurately be described as antislave than antislavery" (Wolf, 2006, p. 224).

VA and WV, we see that the results are almost identical. In addition, there are theoretical reasons to exclude Virginia from our analyses in the first place. It is a state where many of the former bastions of slavery are now populated by members of the federal government and the industries that support it, many of whom hail from faraway states. In this sense, Virginia is a state where large-scale population changes have likely undermined the effects of intergenerational socialization central to *Deep Roots'* theory. However, regardless of excluding or including Virginia, its inclusion does not affect *Deep Roots'* main results and cannot be strong evidence against the identification strategy.

5   Conditioning on Earlier Slavery

Taken together, there is little quantitative evidence of a robust relationship between slavery and political outcomes before 1860, at least when using these imperfect proxies for political attitudes. But what if the arguments of B&S were true, but simply poorly measured by this particular set of votes? In that case, earlier slavery would be an important confounder on which *Deep Roots* should have conditioned its analyses. In this section, we replicate those main analyses including pre-1860 measures of slavery as controls to account for that confounding.

Unfortunately, directly using proportion enslaved in 1830, as B&S do in an appendix analysis, suffers from severe bias due to missing values of slavery for states that had not joined the US or counties that had not yet formed. We focus on two alternative measures instead. First, we calculate the "earliest proportion enslaved," which is the earliest measurement of slavery density available in censuses of 1830–1850.[6] Second, we calculate the "average earlier proportion enslaved," which is the average proportion enslaved across the 1830, 1840, and 1850 US censuses. Our replications of *Deep Roots* adds these variables separately as controls in both the OLS and IV main analyses. Finally, we also calculate an entirely different treatment variable, which is the difference between slavery in 1860 and the "earlier proportion enslaved" variable. We present an alternative specification that uses this variable in place of the proportion enslaved in 1860 as the treatment variable while also conditioning on the "earlier proportion enslaved" variable.

Figure 4 shows the results of this replication. The first column shows the original results of *Deep Roots*, while the second column shows the results after conditioning on the earliest proportion enslaved, and they are extremely similar to the original, with slightly larger effect sizes for the racial resentment outcome. The third column shows the results when conditioning on the average of proportion enslaved between 1830 and 1850, and here we see a marked increase in uncertainty due to

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[6]Several counties in Alabama and Georgia were formed from Cherokee tribal lands after the 1830 US census and so those counties had missing slavery data in both *Deep Roots* and B&S. To maximize the available sample of 1830s counties, we use Cherokee census records to determine the density of slaveholding in these counties around 1830 (McLoughlin and Conser, 1977).
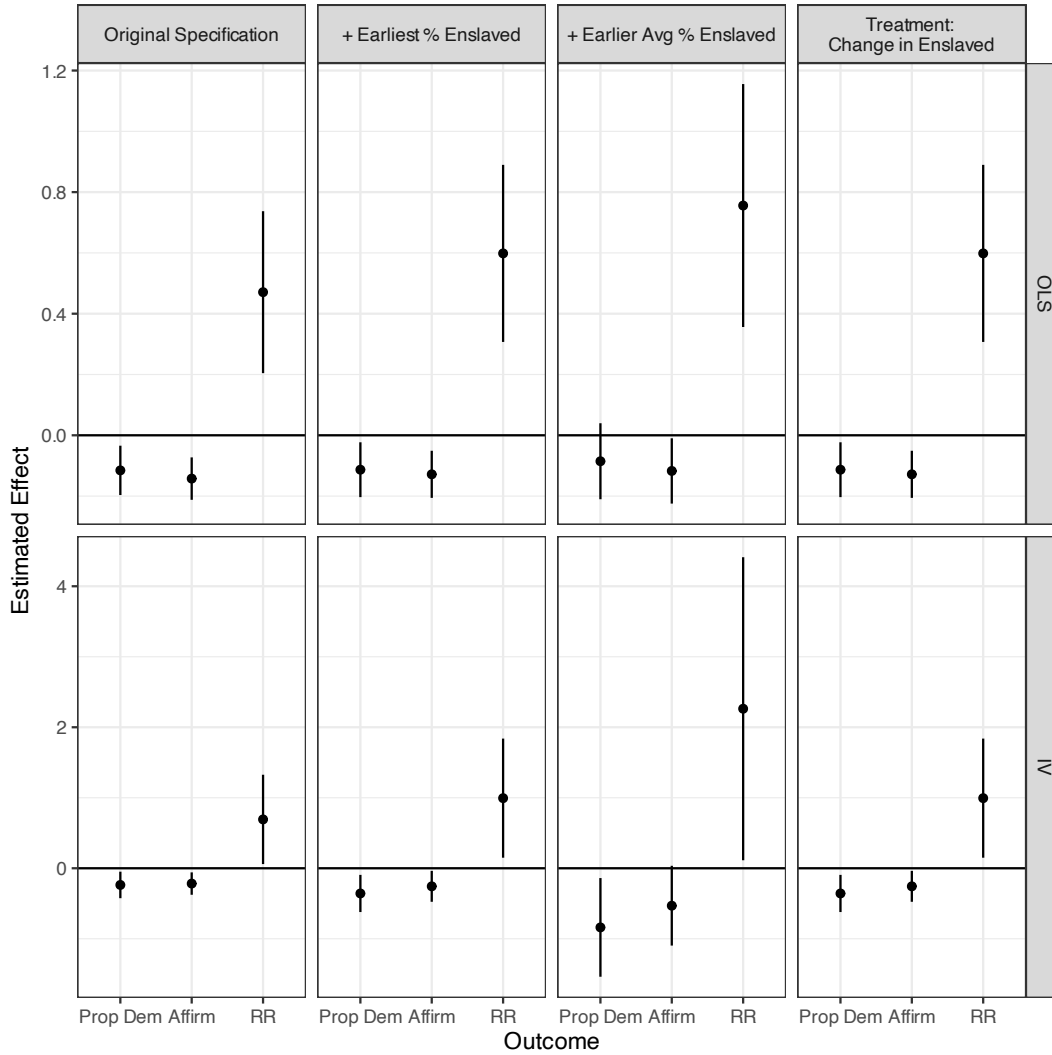
Figure 4: Original *Deep Roots* results (left), additionally controlling for earliest proportion enslaved (middle left), controlling for average of proportion enslaved between 1830 and 1850 (middle right), and results using the change in proportion enslaved between 1860 and the earliest proportion enslaved as the treatment (right).

the collinearity between this variable and slavery in 1860. However, the estimated effects are all consistent with the original *Deep Roots* findings. Finally, using the change in proportion enslaved also gives similar findings.

In sum, there is little evidence that slavery before 1860 is an important confounder for the slavery in 1860. This bolsters the original arguments of *Deep Roots* that the period during and after emancipation were a critical juncture for the effect of slavery on American politics.[7]

6   Conclusion

B&S pay careful attention to how historical data speak to essential questions, bringing important substantive points to evaluate causal assumptions. They produce a series of descriptive findings, which are interesting in their own right. As B&S acknowledge, understanding how slavery has impacted the development of US politics is vital.

However, the analyses of B&S do not undermine the arguments in *Deep Roots*. To undermine the assumptions behind the *Deep Roots* analysis, B&S must show that antebellum slavery would have to causally impact antebellum political outcomes. They do not show this; they instead show unconditional descriptive relationships. Once we include the appropriate controls drawn from the *Deep Roots* main selection-on-the-observables or IV analyses, most of these results become insignificant. In the one case where an effect remains (Virginia), we have reason to think these are not good substantive tests. Additionally, without Virginia, the main findings of *Deep Roots* hold. Thus, we conclude that B&S present fascinating descriptive findings that fail to speak to the assumptions behind the *Deep Roots* analyses.

B&S are more generally skeptical toward the use of slavery in 1860 as a measure of slavery's prevalence, given the long history of slavery in many states and, presumably, the complexity of accounting for confounders. (B&S refer to this as an "unfolding process.") The fact that earlier slavery was bound up with politics is only relevant to our story if there is evidence that it invalidates our empirical design, which there is not. But, more broadly, treatments across different contexts—including not just historical ones but also randomized medical treatments, economic interventions, and long-running political forces—are often complex and last long periods. Medical randomized trials, for example, are superficially quite simple, but the actual biochemical processes that take place within cells—for example, when a vaccine is administered—are highly complex and can unfold slowly over

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[7]B&S conducted similar analyses in their supplemental materials but found very different conclusions. Why is this? B&S only focus on slavery as measured in 1830, but as discussed above, this variable cannot be interpolated onto modern county boundaries for 15%-20% of the original sample of *Deep Roots*, depending on the OLS or IV designs. Our measures of pre-1860 slavery cover more counties (roughly 96% of the original sample) and take into account more of the pre-1860 period than those of B&S.

years, impacted by unobserved epigenetic factors. Observational causal studies, too, are frequently highly complicated and evolve over time. For example, observational studies established the cancer-promoting effects of smoking over half a century ago; however, it took several decades for scholars to establish the pathways of the effects and how variations in smoking duration and behavior can impact outcomes (which include types of cancer besides lung cancer). We agree that many historical forces were complex as well. But a high degree of complexity should not stop scholars from studying their impact using appropriate tools, data, and assumptions.

Skepticism of assumptions is a hallmark of good science. We should always interrogate these assumptions with the best data available. We are grateful for B&S's engagement with our work and the topic of slavery's impact. Their work reflects skepticism about the causal design in *Deep Roots*. But their results fail to prove that skepticism right.

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