A Report on "Political Foundations of Racial Violence in the Post-Reconstruction South" by Testa and Williams (2025)

Reviewer 2

February 21, 2026

v1

Disclaimer

This report was generated by large language models, overseen by a human editor. It represents the honest opinion of The Catalogue of Errors Ltd, but its accuracy should be verified by a qualified expert. Comments can be made here. Any errors in the report will be corrected in future revisions.

> I am wiser than this person; for it is likely that neither of us knows anything fine and good, but he thinks he knows something when he does not know it, whereas I, just as I do not know, do not think I know, either. I seem, then, to be wiser than him in this small way, at least: that what I do not know, I do not think I know, either.
>
> Plato, *The Apology of Socrates*, 21d

> To err is human. All human knowledge is fallible and therefore uncertain. It follows that we must distinguish sharply between truth and certainty. That to err is human means not only that we must constantly struggle against error, but also that, even when we have taken the greatest care, we cannot be completely certain that we have not made a mistake.
>
> Karl Popper, 'Knowledge and the Shaping of Reality'

Overview

Citation: Testa, P. A., and Williams, J. (2025). Political Foundations of Racial Violence in the Post-Reconstruction South. *Quarterly Journal of Economics*. Vol. 141, No. 1, pp. 733–794. Unpublished paper.

URL: https://doi.org/10.1093/qje/qjaf045

Abstract Summary: This article examines how racial violence in the post-Reconstruction U.S. South was linked to the local performance of the anti-Black Democratic Party in presidential elections. It finds that Southern counties where Democrats narrowly lost between 1880 and 1900 were significantly more likely to experience Black lynchings in the subsequent four years.

Key Methodology: Regression discontinuity (RD) design based on close presidential vote shares, causal mediation analysis, and analysis of newspaper content.

Research Question: How was racial violence in the post-Reconstruction U.S. South tied to the local performance of the anti-Black Democratic Party in presidential elections?

Summary

Is It Credible?

Testa and Williams argue that racial violence in the post-Reconstruction U.S. South was strategically deployed by Democratic elites to suppress Black political power. Using a regression discontinuity design based on close presidential elections between 1880 and 1900, the authors claim that a narrow Democratic loss at the county level increased the probability of a Black lynching by roughly 10 percentage points over the subsequent four years (p. 735). They further argue that local elites operationalized this violence by amplifying anti-Black crime accusations in Democratic-aligned newspapers, ultimately helping to consolidate the "Solid South" by suppressing Black voter turnout and ensuring long-term Democratic electoral dominance.

The core empirical strategy relies on the assumption that counties where Democrats barely lost are fundamentally similar to those where they barely won. The authors provide standard statistical tests to support this, finding no evidence of sorting around the threshold. However, the historical reality of pervasive electoral fraud and voter intimidation in the post-Reconstruction South complicates this assumption. If Democratic elites selectively manipulated vote margins to secure narrow wins in highly contested areas, the "barely won" and "barely lost" counties might differ in unobservable ways that correlate with racial violence. While the authors argue that fraud was likely "wholesale" (e.g., ballot stuffing) rather than the "retail" manipulation of precise margins required to bias a regression discontinuity design, the tension between statistical smoothness and historical chaos remains. Furthermore, while the main effect is statistically robust, introducing county fixed effects reduces the point estimate by nearly half in some specifications (p. 760). It is important to note, however, that this reduction occurs in models using unique

county boundaries; when the authors control for spatial unobservables using county-pair fixed effects or restrict the sample to counties with fixed land areas, the estimates remain close to the baseline, suggesting the result is more stable than a cursory glance at the fixed-effects table implies.

The proposed mechanism—that elites used newspapers to foment racial hatred—faces a significant endogeneity challenge. Historical lynching databases are heavily reliant on newspaper reports. Consequently, the paper may be capturing an increase in the reporting of lynchings by aggrieved Democratic editors rather than an actual increase in violence. Alternatively, victorious Democratic elites might have successfully suppressed news of lynchings in their counties. Additionally, the media mechanism likely violates the assumption that treatment in one county does not affect outcomes in another, as newspaper circulation frequently crossed county boundaries. Finally, while the relative increase in anti-Black accusations following a Democratic loss is large (over 80%), the absolute increase is small—rising from roughly 0.20% to 0.37% of newspaper pages (p. 772). While this represents a doubling of specific virulent rhetoric, it raises questions about whether this shift in content volume was practically sufficient to mobilize lynch mobs, or if the qualitative nature of the shift mattered more.

The authors' broader claims about the long-term electoral reversal rely on causal mediation analysis, which requires strong assumptions about the absence of unobserved confounders between lynchings and later voting patterns. Notably, while the authors find a strong direct negative relationship between lynchings and subsequent voter turnout, the specific mediation pathway—that Democratic losses led to lynchings which *then* caused the electoral reversal via reduced turnout—is not statistically significant (p. xxxi, Online Appendix). The theoretical framework is also distinctly top-down, focusing on elite manipulation. While the authors discuss how elites respond to the "threat" of bottom-up mobilization, the analysis largely omits the strategic agency of Black communities or spontaneous white mob violence

4

that was not elite-directed. The generalizability of the theory is also constrained; in several border states excluded from the main analysis, the relationship between Democratic losses and lynchings is noisy or null, rather than positive (p. xii, Online Appendix). Moreover, the authors find that in the post-Jim Crow era, a Democratic loss was associated with a significant decrease in anti-Black newspaper accusations generally, and a null effect for Democratic papers specifically—a striking shift that complicates the primary narrative (p. 782).

Ultimately, the paper presents a provocative and rigorously modeled argument that links local political competition to racial terror. While the baseline statistical evidence is compelling, the inherent limitations of historical data, the potential for reporting bias, and the strong assumptions required for the mediation analysis suggest that the headline claims should be interpreted with a degree of caution.

The Bottom Line

Testa and Williams provide compelling quantitative evidence linking local Democratic electoral defeats to subsequent surges in Black lynchings in the post-Reconstruction South. However, the credibility of their proposed mechanism—that elites strategically used newspapers to incite this violence—is complicated by the fact that historical lynching records rely heavily on those same newspapers, introducing potential reporting bias. While the paper offers a powerful top-down framework for understanding racial terror as a tool of political suppression, the historical realities of electoral fraud and the strong assumptions required for the long-term mediation analysis suggest these headline claims should be viewed as suggestive rather than definitive.

Potential Issues

Endogeneity of the historical record and reporting bias: A central challenge to the paper's design is that the primary mechanism (newspaper reports) is also a primary source for the outcome measure (recorded lynchings). The authors acknowledge that historical lynching databases have "commonly been derived from or corroborated using historical newspaper data" (p. xv, Online Appendix). This creates a potential circularity where the paper may be measuring an increase in the *reporting* of lynchings rather than their *occurrence*. An alternative explanation for the findings is that a Democratic loss makes a Democratic-aligned newspaper more likely to report a violent death *as a lynching*, or to cover it more extensively, ensuring it enters the historical record. Similarly, it is plausible that in counties where Democrats secured a narrow victory, the entrenched Democratic elite and allied newspaper editors were more successful at suppressing news of a lynching than in a county where Democrats had just lost. This would lead to systematically fewer *recorded* lynchings on the "win" side of the threshold, creating the appearance of a discontinuous jump even if the true rate of violence was smooth. The authors address this by testing the density of the running variable for the subsample of lynching-positive counties, finding no evidence of sorting (p. xv, Online Appendix). They also point to the null effect on white lynchings as evidence against a general reporting bias (p. 754). While these are reasonable checks, they may not fully resolve the fundamental concern that the historical record itself is endogenous to the political conditions under study.

Plausibility of the no-sorting assumption in the context of electoral fraud: The validity of the regression discontinuity design depends on the assumption that political actors cannot precisely manipulate vote margins to sort around the zero-margin threshold. The historical context of the post-Reconstruction South, however, was characterized by widespread electoral fraud and voter intimidation, primarily by Democrats. It is plausible that such manipulation would be most intensely applied

6

in counties expected to be close, with Democrats exerting just enough effort to turn a narrow loss into a narrow win. If so, counties that end up as narrow Democratic wins would be systematically different from those that end up as narrow losses, violating the regression discontinuity assumption. The authors address this concern directly. They perform the standard McCrary density test and find no visual or statistical evidence of sorting (p-value of 0.4), and they conduct extensive balance tests on a wide range of relevant pretreatment covariates. Furthermore, they argue that historical fraud was likely "wholesale" (e.g., ballot stuffing) rather than "retail" (finely manipulating margins), which would not systematically bias a regression discontinuity design (p. 751, footnote 17). While these tests and arguments provide the standard empirical support for the design's validity, a tension between this statistical evidence and the historical reality of pervasive electoral fraud remains a debatable point.

Violation of the no-spillover assumption: The paper's regression discontinuity design treats counties as independent units, which requires the Stable Unit Treatment Value Assumption (SUTVA)—that the treatment status of one county does not affect outcomes in others. The paper's proposed mechanism, however, may structurally violate this assumption. Newspapers operate in media markets that cross county lines, meaning that anti-Black rhetoric triggered by a Democratic loss in a "treatment" county could be read in adjacent "control" counties where Democrats narrowly won. This spillover could contaminate the control group. If this contamination increased violence in control counties, it would cause the estimated effect of a Democratic loss to be attenuated, or biased toward zero, meaning the reported estimates may represent a conservative lower bound of the true effect. Conversely, if violence were displaced from treatment to control counties, the effect could be exaggerated. While the authors include spatial controls and cluster standard errors, these methods account for smooth spatial trends and correlations, not necessarily the sharp, network-based spillovers created by media markets (p. 751).

Strength of evidence from causal mediation analysis: The paper uses causal me-

diation analysis to argue that newspaper accusations were a channel to lynchings (p. xxviii, Online Appendix) and that lynchings were a channel through which early Democratic losses led to later Democratic victories (p. 784). However, this method requires a strong and unstated assumption of "sequential ignorability," meaning that no unobserved factors confound the relationship between the mediator and the final outcome, conditional on the initial treatment. This assumption is unlikely to hold; for example, the local strength of white supremacist organizations could independently increase the likelihood of both a lynching and long-term Democratic entrenchment. Furthermore, the evidence for one of the key mediating pathways is not statistically significant. While the authors find a strong direct negative effect of lynchings on voter turnout, the specific *indirect* effect of Democratic losses on turnout *through* lynchings is not statistically distinguishable from zero (p. xxxi, Online Appendix). In contrast, the analysis does find a statistically significant indirect effect for lynchings as a channel to subsequent Democratic electoral victories (p. 784). While the authors use cautious language, describing the evidence as "suggestive" (p. 783) and "plausible" (p. 786), the analysis may overstate the strength of the evidence for these specific causal pathways given the strong underlying assumptions.

Contradictory finding in the post-Jim Crow era: A key finding is that the effect of a Democratic loss on lynchings disappears after the implementation of Jim Crow disenfranchisement laws, suggesting violence was a strategic substitute for legal suppression. However, the analysis of the newspaper mechanism reveals a more complex pattern. In the post-Jim Crow era, a Democratic loss was associated with a large and statistically significant *decrease* in anti-Black crime accusations in newspapers generally (p. 782). For Democratic-affiliated newspapers specifically—the primary mechanism of the paper—the effect becomes null (p. 781). This suggests that once their power was consolidated, Democrats' reaction to a rare electoral loss was to stop inciting racial antagonism in the press, or at least not increase it. The authors acknowledge this "reversal of estimate sign" and offer plausible, though speculative,

explanations in a dedicated "Reverse Backlash" section (pp. 781–783). However, this striking result complicates the interpretation of the pre-Jim Crow findings and is not fully integrated into the paper's main conclusions.

Generalizability of the findings and exclusion of border states: The paper's focus on the 11 former Confederate states is historically justified by the unique political context of the "Solid South." However, this sample selection raises questions about the generalizability of the theory. The authors report in the Online Appendix that in several adjacent border states (Delaware, Kentucky, and West Virginia), the point estimates for the effect are negative, though not statistically significant, and the overall estimate for border states is "positive but small and noisy" (p. xii, Online Appendix). The paper's "power threat" theory would seem to apply in these states where Democratic power was less secure. The lack of a clear positive effect in these states suggests the political dynamics may be more complex than the main analysis implies. While the authors are transparent about these results in the appendix, their exclusion from the main analysis limits the external validity of the central claims.

Scope of the theoretical framework: The paper's conceptual framework is largely top-down, focusing on how "local (Democratic) elites have an incentive to foment a backlash" (p. 735). While the authors do acknowledge bottom-up dynamics—noting that election results serve as a signal for coordination and that elites respond to the threat of mobilization—the framework prioritizes elite manipulation. Furthermore, the framework largely omits the strategic responses of Black communities. While the authors' theory is predicated on Black political mobilization as the trigger for the white backlash (the "Black power threat"), Black political agency is not measured or modeled as an active component in the empirical analysis (p. 745). The analysis focuses on the actions of white elites, leaving the specific dynamics of Black political organizing and resistance unexamined in the data. While the evidence on newspapers supports the elite-driven channel, the overall framework may not fully capture the complex interplay of forces at work.

Stability of the estimated effect size: The paper claims its main finding is "robust" to the inclusion of county fixed effects (p. 759). While the effect on Black lynchings remains statistically significant ($p < 0.01$), the point estimate drops from 0.104 to 0.055 in the specification using unique county boundaries (p. 760). However, it is crucial to note that when the authors use county-pair fixed effects or restrict the sample to counties with fixed land areas, the estimates remain very close to the baseline (0.091 and 0.104, respectively). Thus, while the "unique boundary" fixed effects model suggests some sensitivity to unobservables, the broader array of spatial controls indicates the result is generally stable.

Statistical power in subgroup analyses: Several conclusions are drawn from comparisons between subgroups with very different and sometimes small sample sizes. For example, the analysis of non-Democratic newspapers is based on 206 observations, compared to over 1,900 for Democratic papers (p. 772), and the analysis of counties without a "white-only" elite is based on 138 observations, compared to over 1,200 for those with one (p. 779). The paper interprets the difference between a significant effect in the large group and a null or reversed-sign effect in the small group as evidence for its mechanism. However, it does not discuss the statistical power of these tests. With such small sample sizes in one arm of a comparison, a failure to find a statistically significant effect may be due to insufficient data rather than a true null effect, a limitation that warrants caution when interpreting these results.

Practical significance of the media effect: The paper finds that a close Democratic loss increased anti-Black crime accusations in newspapers by a relative margin of "28.9%–88.4%" (p. 771). While this relative change is large, the absolute change is small. The control group mean is approximately 0.20% of pages, meaning an 84% increase raises this share to about 0.37% of pages (p. 772). It is debatable whether this small absolute increase in coverage was practically significant enough to mobilize lynch mobs. However, given that lynchings themselves were rare events (the mean number of lynchings per county-election is 0.13), it is plausible that a small absolute

10

increase in media incitement could be sufficient to trigger a rare outcome (p. 755). The paper does not provide benchmarks to help interpret this magnitude.

Presentation issue: The note for Table II, Panel B is confusingly worded. It states that the sample is restricted to counties that were "electorally uncompetitive... within the median vote margin" (p. 756). The main text clarifies that the analysis correctly excludes counties with vote margins *within* that bandwidth from the previous election, meaning the sample consists of counties that were *not* competitive (p. 757). The table note appears to contain a clerical error in its phrasing.

Future Research

Disentangling reporting bias from actual violence: Future work could attempt to separate the occurrence of lynchings from their reporting by cross-referencing newspaper accounts with alternative historical records, such as death certificates, coroner reports, or private diaries, to test whether Democratic losses truly increased violence or merely the willingness of partisan editors to publicize it.

Modeling spatial spillovers in media markets: Because newspaper circulation areas rarely map perfectly onto county borders, future research should explicitly model spatial spillovers. Using historical circulation data to define media markets could help determine if anti-Black rhetoric in a "treated" county's newspaper influenced violence in neighboring "control" counties, thereby addressing potential violations of the stable unit treatment value assumption.

Incorporating bottom-up dynamics and Black agency: Subsequent studies could expand the theoretical framework beyond elite manipulation by incorporating measures of Black political organizing, economic independence, or armed resistance. Analyzing how these community-level factors interacted with electoral outcomes could clarify whether racial violence was primarily an elite-directed strategy or a broader, reactionary white mobilization against Black agency.