The Specter of Slavery

Election Timing and the Campaign Agenda in the Party Press, 1860–1864

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Abstract

Was slavery's place in the Northern press of 1860 set by the election campaign or by the news? A keyword dictionary applied to six million newspaper articles traces slavery coverage around the staggered congressional elections of 1860–1864. In the 1860 cycle, coverage fell after the vote by about two-fifths of its normal level, net of trend, along with campaign and tariff coverage, while the rest of the non-campaign agenda held steady. The drop followed each state's own election date rather than the national calendar. It did not recur in 1862 and was smaller and not significant in 1864, when the campaigns turned on the war rather than on slavery's extension. Within cities, Democratic papers used the vocabulary of racial equality more often than their Republican rivals, most clearly in 1864. Slavery behaved as campaign material at the margin of a persistent debate that did not follow the vote.

Keywords: newspapers, agenda-setting, slavery, Civil War, congressional elections, media partisanship

JEL Codes: N41, L82, D72

1 Introduction

Slavery was the issue that broke the old party system (McPherson, 1988, p. 121), and in 1860 it was both the subject of the campaign and the subject of the news. This paper asks which of the two set slavery's place in the Northern press. If slavery coverage was campaign material, it should have left the page when the campaign ended, on each state's own election date. If it was news, it should have followed events, which reached every paper at once. The distinction matters for two literatures. For the political economy of the media, it is a test of whether an outlet's content follows the electoral calendar, which is hard to establish because elections are usually national and coincide with everything else in the news. For the history of the coming of the war, it asks how the issue that divided the country reached Northern readers in the year the country divided.

Most of the papers studied here were weeklies that took a party's side. The panel's 242 newspapers are spread across 167 places; only 35 are dailies, 200 are weeklies, and 7 came out two or three times a week. From their own pages, 171 can be assigned to the Republican or the Democratic side. Papers like these belonged to a press whose editors lived on politics: on subscriptions and advertising bought by political supporters, on printing contracts, and often on offices their party could give. Big advertising-financed dailies overwhelmed that model in the major eastern cities, but in many places outside them it survived (Pasley, 2003, pp. 4, 15) (Section 2). A paper that lived on its party had a reason to give the campaign room. It also had little room to give. The typical issue ran about 4 pages, and adding a page meant more paper, more type to set, and more time on the press. An election brought a surge of campaign matter: tickets, nominations, rally notices, returns. If the paper did not grow, that surge had to push something else out.

The three congressional cycles differ in what the campaign was about. In 1860 it turned on slavery's extension into the territories; in 1862 and 1864 it turned on the conduct of the war (Section 2). The war did not settle how central slavery would be to Northern politics. Maintaining the Union "always ranked first among war aims for most citizens in the United States" (Gallagher, 2011, p. 34), and part of the Democratic Party "stridently opposed emancipation" while remaining "committed to a war against the rebellion" (Gallagher, 2011, p. 5).

The design uses the staggered congressional calendar. States chose their members of Congress on different dates, 35 of them in the panel, so in any calendar month some papers were close to their own state's vote and others far from it. Each paper is compared with itself in months far from any congressional election, and calendar-month fixed effects absorb everything national, so the estimates come only from papers at different distances from their own election in the same month. Because each paper is its own benchmark, the design measures the margin, not the base: slavery held a share of the page in months far from any election, and the

question is how much the campaign added to that share and how much left with it. It is a question about the agenda more than about tone: which issues a party paper put before its readers, and when.

Presidential elections fall on one national day, so their timing cannot be told apart from the calendar month (Section 4.1), and the paper is about congressional elections throughout. Its first cycle, called the 1860 cycle here, is the election for the Thirty-Seventh Congress. Most states held it in the autumn of 1860, but Kansas voted in December 1859 and six states in the estimation sample voted in 1861, four of them after the war had begun (Section 3.6).

The paper makes one narrow causal claim: election dates were fixed by law in advance, so once each paper is compared with itself and the national calendar is absorbed, the date is the only thing that separates papers close to their vote from papers far from it. The exception would be some other shock that struck only the states voting on a given date, a possibility the limits below take up. The design therefore identifies the effect of a state's own election on what its papers printed. It does not identify why: editors could have supplied campaign content to help their party, or readers could have wanted more of it as their own vote approached, and both produce the same timing. Nor does it identify any effect of coverage on votes. The rest of the evidence is descriptive: what the campaign displaced, how the two parties' papers differed, and what the timing implies about whether slavery travelled as campaign material or as news.

The identified finding is a break at the vote. In the 1860 cycle, slavery coverage fell in the month after each state's congressional election, measured against its trend in the months before. Slavery lost 41 percent of its normal share of 3.9 percent of articles: about 1.3 fewer slavery articles in a weekly issue of some 82 items or, measured in words, 0.13 of a page in a 4-page issue. The break is significant on both the article and the word share, under an exact wild cluster bootstrap on the cycle's 11 election dates and under randomization inference over which state voted when (Section 5.1). Placebo breaks three or four months from the vote are half its size or smaller. Estimators that let the response differ by election date make the article-share break larger and leave the word-share break only suggestive (Section 6.5). Slavery fell with campaign coverage and with the other platform item, the tariff and land policy, while the rest of the non-campaign agenda did not move, a contrast specified before it was estimated (Section 5.3). It left the page when each state's campaign did. Coverage had also been higher in the three months before the vote, by 37 percent of its normal level. That rise is less secure than the break: against the far months before the vote alone, one cycle cannot tell it from a steady drift (Section 6.1).

Two limits apply to that finding. The first is geographic, and it is the more serious. The break is concentrated in the four states that voted on October 9, 1860: Pennsylvania, Ohio, Indiana, and Iowa. Without them it is a third as large on the article share and not significant, and in the eight states that voted on November 6 there is no break. The months after the October 9 vote are the onset of secession.

Calendar-month fixed effects absorb a reaction to secession common to every region, not one confined to some, so the break cannot be told apart from a regional reaction (Section 5.2). The second limit is that the pattern belongs to 1860. In 1862 and 1864 the pre-election estimates are close to zero and neither vote shows a significant break (Section 5.1). The weak 1864 response does not come from the dictionary missing more of that year's slavery discussion: a looser rule that finds most of it gives the same proportional break, still not significant (Section 6.7). Pooled across the three cycles the pre-election rise is significant, but it averages one cycle with a large response and two with none.

The other results are descriptive. Papers did not grow before elections: article counts, word counts, and pages per issue were flat in every cycle, so the campaign had to displace other matter (Section 5.4). Republican and Democratic papers both show the 1860 pattern, and the design cannot tell their timing apart (Section 5.6). The parties differed more clearly in how they wrote than in how much. In 17 cities with papers of both parties, Democratic papers used the vocabulary of racial status and equality (amalgamation, suffrage, and the period's slurs) more often than their Republican rivals, by 0.63 percentage points of articles, 60 percent of the Republican papers' own rate, a significant gap that is clearest in 1864 (Section 5.8).

In 1860, then, slavery coverage behaved as campaign material at the margin. It was less purely so than the tickets, conventions, and returns themselves, whose coverage fell by more than its whole normal level: slavery sat inside the campaign agenda without being confined to it (Section 5.3). Whether slavery coverage also followed the news is unclear. The dictionary does not single out the months of the Emancipation Proclamations, which may mean that coverage did not follow slavery news or only that the dictionary misses how papers wrote about emancipation.

The paper makes two contributions. The smaller is to measurement. I build monthly newspaper-level shares of slavery and campaign coverage from a transparent keyword dictionary, checked against blind labels on a random sample of the corpus (Section 3.2). The corpus is segmented into articles, which matters for a second reason. Keyword search in digitized newspaper archives, Chronicling America's included, works at the level of the page (Beach & Hanlon, 2023), and so does any measure of terms appearing together that is built on search results. Because the articles here keep their page identifiers, the error that page-level measurement introduces can be sized directly. A rule that counts every article on a page where two families of terms both appear flags 22.4 to 95.9 times as many articles as a rule that requires both in the same article, across 14 such pairs. For race terms alongside crime terms, the pair closest to the measure of Ottinger & Posch (2026), the factor is 61.8. Their keywords are narrower than these families, and they flag the problem themselves: in a hand check of 1,003 pages their search returned, the keywords were part of the same article on 213 (Ottinger & Posch, 2026). These figures compare two measures on one corpus; they are not a bound on the bias in any particular estimate.

The larger contribution is to the political economy of the media. That literature has shown that newspapers raise political participation (Gentzkow et al., 2011, p. 2980), that governments respond more to local crises where newspaper circulation is higher (Besley & Burgess, 2002, p. 1415), that a slanted outlet can move vote shares (DellaVigna & Kaplan, 2007, p. 1187), that reader demand explains far more of measured slant than ownership does (Gentzkow & Shapiro, 2010, p. 35), and that House members whom the local press covers less are less known to their voters and work less for their districts (Snyder & Strömberg, 2010, p. 355). Slant can work through what an outlet covers as well as through how it covers it, and empirical work classifies outlets by "the amount of coverage they give to different politically relevant topics" as well as by tone (Puglisi & Snyder, 2015, pp. 651–652). Theory treats coverage itself as a choice: advertising-financed media give more news to groups advertisers value (Strömberg, 2004, p. 265), and a firm catering to a partisan audience may suppress what that audience does not like hearing (Bernhardt et al., 2008, p. 1092). In the model of Mullainathan & Shleifer (2005), competing papers segment the market and slant toward extreme positions on divisive issues, a prediction the within-city framing comparison speaks to (Section 5.8). The closest antecedent to the question asked here is Puglisi (2011): during presidential campaigns the *New York Times* gave more emphasis to issues the Democratic party owned when the incumbent president was a Republican. Newspapers that endorsed Democrats also gave more coverage to high unemployment under Republican presidents than under Democratic ones, relative to papers that endorsed Republicans (Larcinese et al., 2011, p. 1178). A second line of work treats the news hole as fixed: other newsworthy events crowd out television coverage of foreign disasters, and with it U.S. relief (Eisensee & Strömberg, 2007, p. 693), and Israeli attacks were more likely when the next day's U.S. news would be dominated by important predictable events (Durante & Zhuravskaya, 2018). In both lines of work the timing is national: a presidential campaign or a news event reaches every outlet at once, so election timing cannot be told apart from everything else in the news. The staggered congressional calendar of the 1860s puts papers at different distances from their own election in the same month, which is what lets this paper separate the campaign from the news. The setting helps in other ways too: most papers printed their party's ticket in their own columns, their output is measurable, and in some towns papers of both parties competed for the same readers.

For the historical American press, economic work has asked whether political power shaped newspapers. Using changes in party control of state governments from 1869 to 1928, Gentzkow et al. (2015, p. 29) find no evidence "that the party in power affects the partisan composition of the press." That is a question about which papers existed and whom they served. This paper takes the papers that existed as given and asks whether what they printed followed the electoral calendar. The closest historical antecedent is Ottinger & Posch (2026), who find evidence consistent with Democratic elites in the 1890s South orchestrating a racist propaganda campaign in the press to sway white voters, and with Democrats benefiting at the next

election. Winfree (2022) finds that newspaper propaganda accusing Black voters of corruption weakened biracial coalitions in the same region, and that large changes in exposure were needed to move outcomes where voters were already routinely exposed. Both study a one-sided campaign with a measurable payoff; the papers here are observed on both sides of the contest. The racial appeal also differs. The within-city divergence is in the vocabulary of racial status and equality, the terms of the Northern argument over what emancipation would bring, not the crime-framed racial threat of the 1890s South.

Section 2 describes the setting. Section 3 builds the measures and Section 4 sets out the designs. Section 5 reports the results, Section 6 the robustness checks, and Section 7 concludes.

2 Institutional Background

The press these papers belonged to had grown quickly. Between 1840 and 1860 the number of American newspapers rose from 1,404 to 3,725, and annual circulation from 10.9 to 28.2 copies per person (Ratner & Teeter, 2003, p. 9). In the cities competition among papers was intense, and "catering to readers' tastes as well as to their prejudices and opinions made newspapers a vital force" (Ratner & Teeter, 2003, p. 8). Once the war began, newspapers often reached the home front before soldiers' letters did (Winfield & Painter, 2023, p. 887).

In the party press of the early republic and the Jacksonian era, an editor's main income came from subscriptions and advertisements bought by political supporters, from job-printing contracts, and often from appointive offices secured by party colleagues in government (Pasley, 2003, p. 15). Government printing contracts were the main source of that patronage (Baldasty, 1992, p. 20), and editors often became officeholders themselves (Pasley, 2003, p. 14). A second model appeared in the 1830s. The penny papers of New York, Philadelphia, and Baltimore sold for a penny instead of the six cents a partisan paper cost, and their financial support shifted from subscribers and patronage to advertisers (Baldasty, 1992, pp. 46, 48). The penny press was "distinctly an urban phenomenon" (Baldasty, 1992, p. 38), and where advertising revenue was higher, nineteenth-century papers were likelier to be independent of the parties (Petrova, 2011, p. 790). The party tie loosened most in the big dailies. After the 1840s politicians gave the press less financial support and sought other ways to reach voters (Baldasty, 1992, p. 37), and for the largest big-city dailies "reliance on printing contracts and other politically based support faded" (Ratner & Teeter, 2003, p. 20). Even those papers "frequently battled on behalf of particular parties and their candidates" (Ratner & Teeter, 2003, p. 20). In many places outside the major eastern cities, where large commercial dailies came to overwhelm the party press, "the system of newspaper-based politics remained fully operational until the early twentieth century" (Pasley, 2003, p. 4). Hazel Dicken-Garcia, quoted by Ratner & Teeter (2003, p. 21), describes most

newspaper content "up to the Civil War" as "intended to argue political points of view."

What these papers covered in 1860 was a sectional crisis. The Kansas–Nebraska Act of 1854 reopened the question of slavery in the western territories and gave rise to a Republican party pledged to keep slavery out of them. In the four-way presidential contest of 1860 Lincoln won 54 percent of the vote in the North while Bell and Breckinridge divided the South (McPherson, 1988, p. 232), and over the following months eleven slave states left the Union. News reached Northern papers over what one history calls "the North's more complete communication network," which "helped to maintain a more effective community of ideas during the war" (Ratner & Teeter, 2003, p. 17).[1] Abolitionist papers formed a separate and much smaller strand of the press; many of them, "begun with little financial support and a small circulation base, ceased publishing after only a few years" (Risley, 2008, p. xiv).

Three congressional cycles define the panel, each in a different political setting. The first, the election for the Thirty-Seventh Congress, did not take place on one day. Kansas voted in December 1859; Vermont and Maine in September 1860; Pennsylvania, Ohio, Indiana and Iowa on October 9; most other states alongside the presidential contest in November; and several states in 1861, after secession had begun. Four of the 1861 states in the sample voted after the war had begun (Section 3.6). Congressional candidates ran on the same party tickets as the presidential candidates: in the November states the two votes fell on one day, and in the October states the congressional vote came four weeks before the presidential one. The campaign they ran in turned on slavery's extension into the territories. Republicans disavowed any direct federal attack on slavery in the states where it already existed (Foner, 1970, p. 301), and in the lower North their speakers often presented the party as the true "White Man's Party," arguing that the "exclusion of slavery from the territories … meant exclusion of black competition with white settlers" (McPherson, 1988, p. 227). The panel's Democratic papers were themselves split between the Douglas and Breckinridge tickets, and the Breckinridge papers were not confined to the border states: 9 of the 12 in the 1860 cycle were in free states.

The 1862 midterms fell in the middle of a change in what the North was fighting for. Northerners had gone to war for the Union, and for many of them the Union stood against the Slave Power, whose "inordinate influence in the antebellum federal government" had blocked the "greater expansion of political and economic opportunity" (Gallagher, 2011, p. 56). Freeing the enslaved was not the object. By the summer of 1862 Lincoln had come to see emancipation as "a military necessity, absolutely essential to the preservation of the Union" (McPherson, 1988, p. 504), and on September 22, five days after Antietam, he issued the preliminary Emancipation Proclamation, which declared free the enslaved people of any state still in rebellion on January 1, 1863. He justified it as a war measure, in part to keep the support of Democrats who would back a war for

[1] California was a partial exception. Three papers, one in Sacramento and two in San Francisco, allied with the New York Associated Press and the telegraph companies "to secure a monopolistic control over the Eastern news" (Chandler, 1976, p. 459).

the Union but not one to free slaves (Gallagher, 2011, p. 51). Democrats made emancipation a campaign issue, with headlines asking, "Shall the Working Classes be Equalized with Negroes?" (McPherson, 1988, p. 507), and gained House seats, though the Republicans kept their majority. Over the rest of the war most loyal citizens came to accept emancipation "as a tool to punish slaveholders, weaken the Confederacy, and protect the Union from future internal strife" (Gallagher, 2011, p. 2). Because the Proclamation fell inside the pre-election window this paper measures, and closer still to the vote in the states that voted in October, a spike in coverage driven by it could be mistaken for election-timed emphasis. The 1862 cycle shows no pre-election rise at all (Section 6.3).

Hostility to emancipation remained a Democratic theme through 1863 and 1864. An Ohio editor called the Proclamation "monstrous, impudent, and heinous," because "it declares those 'equal' whom God created unequal" (McPherson, 1988, p. 595), and in Ohio Copperhead politicians used Negrophobia and the fear of miscegenation to build support (Jackson, 1980, p. 250). In 1864 the congressional vote again ran on the presidential tickets, and again its timing differed by state: Indiana, Ohio, Pennsylvania, and West Virginia voted for Congress in October, two to four weeks before the presidential vote, and most other states on the same November day. The Republicans ran as the National Union party, on a platform that called for a constitutional amendment abolishing slavery; the Democrats nominated George McClellan on a platform that called the war a failure, and that summer editorials in Democratic newspapers demanded "Stop the War!" (McPherson, 1988, p. 761). Atlanta fell to Sherman in early September, and Lincoln won reelection comfortably. In the panel the opposition ticket is labelled National Democratic (the McClellan ticket); "Copperhead" names a faction, not a ticket.

What makes the setting useful is that slavery's salience was not fixed. Political actors, editors included, could press it or let it recede: in 1860 the campaign turned on slavery's extension into the territories, and in 1862 and 1864 on the conduct of the war, with emancipation one of the issues fought over. Editors were participants in this, not conduits. They chose which events to cover, how prominently, and how to frame them. A report of a battle could be told as a war story or as a story about the abolitionists said to have caused the war; coverage of Congress could dwell on tariff schedules or on the status of the enslaved. What editors chose to emphasize, and whether they changed that emphasis as an election approached, is what this paper measures.

3 Data and Measurement

3.1 The corpus

The corpus is the American Stories dataset (Dell et al., 2023), which extracts article-level text from the Library of Congress's Chronicling America scans. Each article carries its newspaper title, date, page number, headline where present, and word count. Because the scans are split into articles rather than pages, every share in this paper counts articles, and Section 3.5 can measure how much a page-level count overstates an article-level one. After a minimum-length and optical-character-recognition quality filter, the corpus has 6,309,545 articles published between 1860 and 1865 in 406 titles. Every measure in the paper is computed on this one set of articles.

Coverage is uneven. Even the most extensive newspaper datasets hold only a selected sample of the historical press (Beach & Hanlon, 2023), and which titles survive and were digitized is not random. The estimates speak to the papers in the corpus; how far they generalize depends on how closely those papers resemble the press as a whole.

3.2 Measuring topical emphasis

I measure topical emphasis with a keyword dictionary. For each of 23 topics it lists the words and phrases the newspapers of the 1860s used for that subject, 4,581 entries in all. An article counts for a topic if it contains at least two different entries for that topic. No fitted model stands between the text and the measure, and a reader can inspect the vocabulary and judge it. Entries come in tiers. Strong entries name the subject outright: *slavery*, *fugitive slave law*, *abolitionist*, *emancipation proclamation*. Formula entries are the stock phrases of one kind of item, such as *ran away from the subscriber* in a runaway-slave advertisement. Medium entries belong to the topic but turn up elsewhere: *negro*, *contraband*, *institution*. All three tiers count toward the two entries. The two-entry rule is what lets the medium tier in: *contraband* alone proves little, but *contraband* beside *emancipation* proves a good deal. A fourth tier of common words that mean something only beside the others, such as *master* or *plantation*, is listed but not counted.

The slavery measure is the dictionary's slavery-and-abolition topic: 155 counted entries on the institution, the slave trade and fugitives, antislavery politics, and emancipation. The slavery share is the share of newspaper j's articles in month t that contain at least two different slavery entries,

$$s_{slav,jt} = \frac{1}{n_{jt}} \sum_{a \in \mathcal{A}_{jt}} \mathbf{1}[\text{article } a \text{ contains two different slavery entries}], \tag{1}$$

where $\mathcal{A}_{jt}$ is the set of n_{jt} articles paper j publishes in month t. The word share instead divides the words

in those articles by all the words the paper published that month. The measure is broad, and the breadth has a cost. An article that mentions slavery twice in passing counts the same as an editorial about it, and a Republican editorial on the territorial question in 1860 counts the same as a Democratic editorial attacking emancipation in 1864, though they are not the same political issue. "Slavery coverage" in this paper means the presence of slavery vocabulary, not a stance on slavery; Section 3.3 reports how often a hit is on topic.

Campaign coverage, $s_{pol,jt}$, is built the same way from the dictionary's elections topic: 158 counted entries on candidates, tickets, conventions, polling, and returns, from *ballot box* and *Chicago convention* to *Lincoln and Hamlin* and *election returns*. War reporting and the draft are topics of their own and do not count as campaign coverage. The same rule gives a share for each of the other topics: 9 other political issues, from the tariff and land policy to temperance, the draft, and the Copperhead movement, and 12 kinds of routine content such as commercial news, legal notices, crime reports, obituaries, and advertisements. Section 5.4 uses them to ask which other matter shrank as campaign coverage grew.

To separate what a paper discussed from how it framed it, the dictionary has a race-and-equality topic. It covers the vocabulary of racial status and of the period's racial politics: "Negro equality," "amalgamation," "Africanization," miscegenation, Black suffrage and citizenship, and the slurs of the period. These are the campaigns' own terms: Jackson (1980) documents Negrophobia, miscegenation, and the fear of an "Africanization" of the free states in the Copperhead campaigns of Ohio.

I built the dictionary in two passes. The first wrote each topic's vocabulary from period usage and from earlier keyword lists for this corpus. The second checked it against 5,000 articles drawn at random and assigned to topics by a language model that never saw the dictionary. I dropped or demoted entries whose hits were mostly off topic, and reviewed the words that most set each topic's articles apart from the rest, adding some of them. Optical-character-recognition errors are handled by listing the misreadings that actually occur. The 4,526 misreadings of longer entries that recur in the corpus count as the entry they garble, so that *abolitiouists* and *aholitionists* count as *abolitionists*; the online appendix describes how they were chosen.

3.3 Validation

Three checks bear on whether the dictionary tracks real content. The first scores it against the 5,000 articles drawn at random and labelled blind (Section 3.2). The slavery topic finds 50 percent of the articles labelled as about slavery or abolition, and 70 percent of the articles it flags carry that label. Because it misses some slavery articles and some of its hits are off topic, the analysis uses changes in each newspaper's coverage, not its level. The misses matter less than they might: if the dictionary catches a fixed share of slavery articles, they scale every estimate toward zero, and they could produce a break at the vote only if the share caught

changed with each state's own election date.

The second check is whether each topic peaks when a documented event should make it peak. I average each paper's share, after removing that paper's own mean, by calendar month, and rank the 72 months of 1860–65. The war topic ranks the month of Antietam 1st and the month of Gettysburg 2nd, and the draft topic ranks the month of the New York draft riots 4th, with its peak in August 1862. A placebo month in the spring of 1860 ranks 69th on the war topic, 62nd on the draft topic, and 22nd on the slavery topic. The slavery topic is the weakest of the three on this check: although it carries the emancipation vocabulary, it ranks January 1863, the month of the final Proclamation, 12th and September 1862, the month of the preliminary Proclamation, 34th, and its peak is January 1860. The third check is that the campaign topic moves on the electoral calendar as campaign dynamics predict. It rises significantly in the three months before a congressional vote (+0.026 pooled over cycles; +0.043 in 1860, 62 percent above its far-month level of 7.0 percent of articles) and falls across the 1860-cycle vote (−0.073).

3.4 Measuring output

Whether the campaign pushed other matter out turns on whether a newspaper grew. I measure output three ways at the newspaper-month level: the count of articles n_{jt}, total words, and the mean of the highest page number observed per issue. The first two measure published volume; the third is the closest available proxy for the physical size of the sheet. None of them is editorial space net of advertising, and page count was not a publisher's only margin: a paper under cost pressure could reduce page dimensions, set smaller type, narrow margins, cut advertising, or change issue frequency. Flat output therefore means that measured output did not grow, not that the paper could not have grown; Section 7 describes the newsprint-cost variation that would identify that constraint.

Advertising sits inside every topic share, because the corpus does not separate advertisements from editorial matter. Of the 5,000 articles drawn at random and labelled blind, 40 percent (95 percent interval 39 to 42) are advertisements or legal and standing notices. Every share in this paper therefore has advertising in its denominator; Section 6.8 sets out which results that exposes and which it does not.

3.5 Page-level and article-level co-occurrence

Keyword search in digitized newspaper archives, Chronicling America's included, applies across the full page (Beach & Hanlon, 2023), so a co-occurrence rule built on search results counts two terms as appearing together whenever they share a page, even when they sit in different stories. Because this corpus is split into articles and keeps page identifiers, both counts can be computed and the difference measured. I use

nine term families from an earlier, regular-expression version of the dictionary: race, slavery, abolition, crime, insurrection, equality, labor, influx, and citizenship, defined in the online appendix. An article counts for a pair such as `race × crime` if it contains at least one term from each family; the families yield 14 such pairs.

Over the full corpus, a rule that counts every article on a page where both families appear flags between 22.4 and 95.9 times as many articles as a rule that requires both in the same article (median about 59): 61.8 for `race × crime`, 50.9 for `slave × crime`, 25.3 for `slave × abolition`, and 95.9 for `race × influx`. The online appendix reports all 14. These figures compare two measures on one corpus. They are not a bound on the bias in any published estimate built from page-level co-occurrence, because the bias that measurement error induces in a regression coefficient depends on the structure of the error, not on the inflation factor alone. Ferrara et al. (2024) show how a second measure built from digitized newspapers can correct for non-classical measurement error in historical data.

3.6 Newspaper panel

The panel has one row per newspaper and calendar month. Each month is attributed to the nearest congressional election in the paper's state, provided it falls within twelve months of it, and event time k_{jt} is the calendar month minus the month of that election: negative before the vote, zero for the month containing election day, positive after. Months at least seven months from the attributed election, on either side, are the far months that serve as the reference in Section 4. The newspaper panel is 242 newspapers and 7,929 newspaper-months across 26 states. The pooled regressions use 7,911 of these months, from 224 papers, after newspapers observed only in a single fixed-effect cell drop out; the 1860-cycle regressions use 159 papers.

Table 1 describes the newspaper panel. The mean newspaper-month contains about 560 articles and 118,000 words, a mean pulled up by the dailies: the 200 weeklies publish about 4 issues and 327 articles a month, the 35 dailies about 25 issues and 2,165 articles. Slavery coverage appears in 4.0 percent of articles but 9.7 percent of words, because articles about slavery run longer than the average article; Section 5.5 re-estimates the main results in word shares for that reason. Campaign coverage is more common, in 6.7 percent of articles and 12.8 percent of words, and war coverage more common again, in 16.5 and 30.5 percent.

The sample is geographically uneven (Table 2). Ohio, Iowa, Pennsylvania, and Indiana, the four states that voted for Congress on October 9, 1860, supply 78 of the 174 newspapers in the 1860 cycle, and Section 5.2 returns to what that concentration does to the central result. Figure 1 maps the sample by place, one panel per congressional cycle, with each place's papers split by the ticket they endorsed.

Election dates are state-specific, and that variation is the source of identification once calendar time is absorbed. The panel contains 35 distinct congressional election dates. The 1860 cycle was the most staggered,

Table 1: Sample composition and newspaper-month characteristics

A. Sample (counts)		
Newspapers	242	
Newspaper-months	7,929	
States	26	
Distinct congressional election dates	35	
Cities	167	
Cities with competing partisan papers	17	
Election cycles	3	
B. Newspapers by affiliation (count; share of newspapers)		
Republican	113	46.7%
Democratic	54	22.3%
Switched affiliation	4	1.7%
Unclassified	71	29.3%
C. Founding (median year; percent of titles)		
Median founding year	1854	
Founded before 1860	78.8%	
D. Newspaper-month output (mean; standard deviation)		
Articles per month	562	711
Words per month	118,100	165,188
Words per article	212	50
Highest page number per issue	3.87	1.27
E. Dictionary shares, percent (mean; standard deviation)		
Slavery, article share	4.0	3.2
Slavery, word share	9.7	7.1
Campaign, article share	6.7	5.4
Campaign, word share	12.8	9.3
War, article share	16.5	10.5
War, word share	30.5	16.5

Note: The newspaper panel is newspaper-months from titles in the AmericanStories corpus, 1860–1865, attributed to a congressional election within twelve months and with at least one article. Each panel heading states what its two columns report; in Panel B the second column is each group's share of the 242 newspapers, and in Panels D–E the columns are newspaper-month means and standard deviations. Affiliation is hand-coded by the author from each paper's own front-page ticket notice and is election-specific. Constitutional Union (Bell) papers are not assigned to either party. "Switched affiliation" denotes titles carrying different party labels in different cycles; "Unclassified" titles contribute to the newspaper panel but not to partisan comparisons. Founding years are reconstructed from Library of Congress preceding-title chains. Dictionary shares are newspaper-month means, in percent, of the share of articles that contain at least two different entries of the topic in the keyword dictionary (slavery and abolition, elections, war); word shares are the words in those articles over all words published. Source: AmericanStories corpus (Chronicling America); author's calculations.

Table 2: Newspapers by state and election cycle

State	1860	1862	1864
Ohio†	34	33	31
Iowa†	18	15	17
Pennsylvania†	15	17	21
West Virginia	13	6	9
Vermont	12	12	12
California	11	9	5
Indiana†	11	13	11
Wisconsin	9	8	8
Maryland	8	9	10
Minnesota	7	3	3
Delaware	6	9	7
Tennessee	6	0	2
Michigan	5	5	6
New York	5	4	4
Kansas	3	5	5
Maine	3	4	4
Illinois	2	3	5
New Jersey	2	0	2
Connecticut	1	1	1
Kentucky	1	2	1
Massachusetts	1	1	1
Rhode Island	1	0	0
Nevada	0	1	1
Oregon	0	1	2
South Carolina	0	0	2
Virginia	0	0	2
Total	174	161	172

Note: Cells are counts of distinct newspapers in the panel contributing observations to each congressional election cycle; a title appearing in more than one cycle is counted in each. The 1860-cycle estimation sample is 159 papers after fixed-effect singletons are dropped. States are ordered by their 1860 count. † marks the four states that voted for Congress on October 9, 1860. Papers in occupied or border areas of seceded states appear where the corpus holds them and a congressional date is recorded. Source: AmericanStories corpus (Chronicling America); author's calculations.

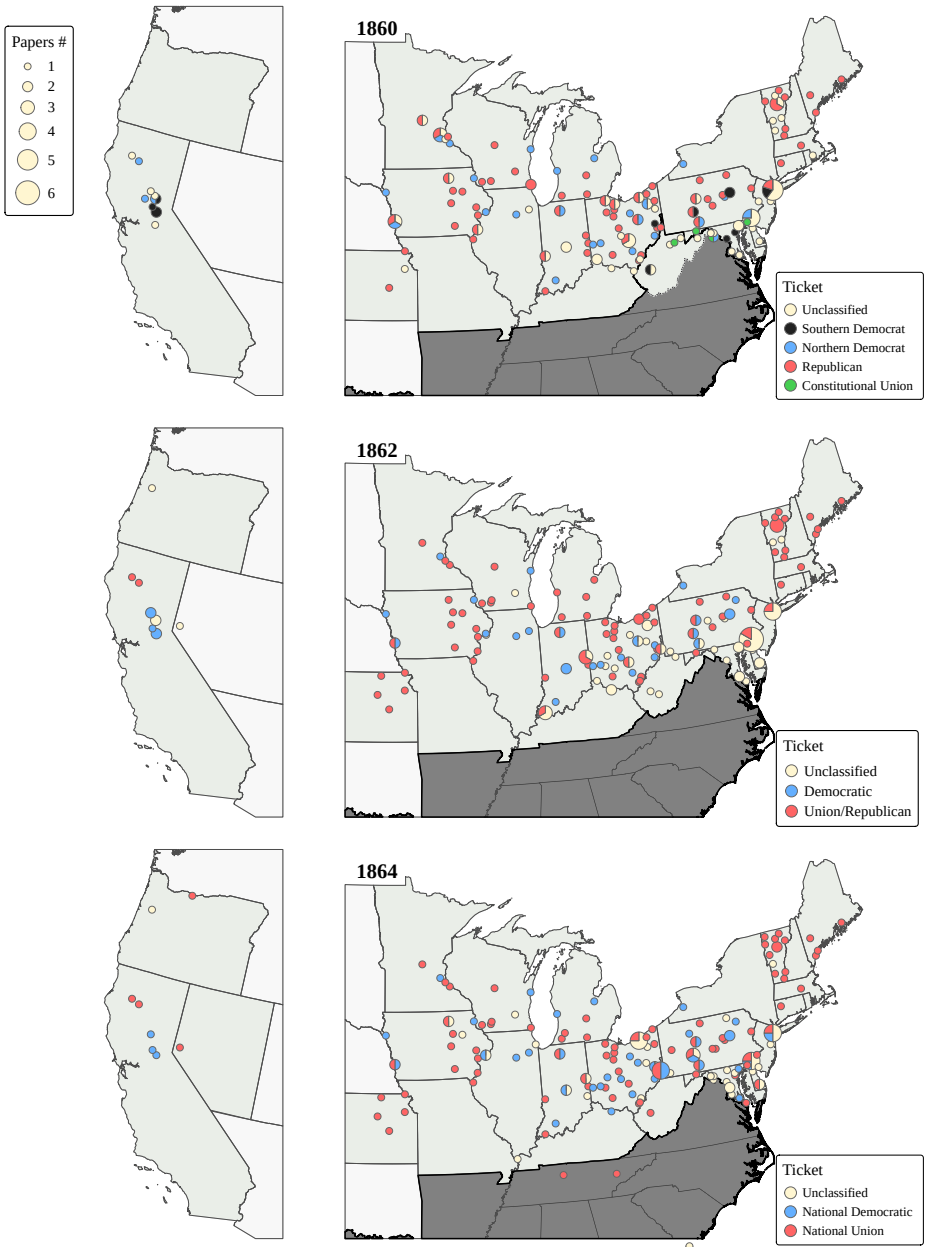


Figure 1: Newspaper panel by place and congressional cycle. Each dot is a place with at least one panel newspaper that cycle; dot size gives the number of papers (1 to 6) and the slices their ticket endorsements: in 1860 Republican, Northern Democrat (Douglas), Southern Democrat (Breckinridge), and Constitutional Union (Bell); in 1862 Union/Republican and Democratic; in 1864 National Union and National Democratic (McClellan). Unclassified papers carry no recorded ticket that cycle. The map draws 168, 161, and 172 papers in the three cycles. Tennessee's 6 papers in the 1860 cycle are not drawn, because the state's August 1861 congressional vote came after it had left the Union, but they remain in the estimation sample; the two papers from occupied South Carolina in 1864 lie south of the frame. Dark grey marks the states that joined the Confederacy, not the edge of the sample: the 1864 Tennessee papers and the papers from occupied Virginia appear on it. Territories are pale grey. West Virginia is a dotted outline inside Virginia in 1860 and its own state from 1862; Nevada appears from 1864. The Pacific states are drawn in a separate panel. Source: AmericanStories corpus (Chronicling America) metadata; 1860 and 1870 state boundaries from IPUMS NHGIS; author's calculations.

and several states held it in 1861. In the estimation sample, Kansas voted in December 1859; Vermont on September 4, 1860; Maine on September 10; Indiana, Iowa, Ohio and Pennsylvania on October 9; the November 6 states alongside the presidential contest; and then Connecticut (April 1, 1861), Rhode Island (April 3), Maryland (June 13), Tennessee (August 1), California (September 4), and West Virginia (October 24). These are 11 distinct dates held by 159 newspapers; Kentucky and New Hampshire also voted in 1861 but contribute no papers to the estimates. Section 6 reports the main estimates on the votes held in calendar 1860 alone.

3.7 Partisan affiliation

Each newspaper's partisan affiliation is hand-coded, election by election, from the paper's own pages rather than from a secondary source. Nineteenth-century party papers announced their position with a ticket notice, most often set at the top left of the front page in the weeks before an election and naming the party's nominees down the ticket, and that notice is the primary evidence for the 1860, 1862, and 1864 codes. Where no surviving issue carries a notice, the affiliation is inferred from sustained editorial content, such as endorsement language or attacks on the rival ticket, and flagged as presumptive. Papers are coded Republican (including the Union/Republican label of 1862 and the National Union ticket of 1864), Democratic (the Douglas and Breckinridge tickets in 1860 and the Democratic ticket after), or neither, and Constitutional Union (Bell) papers in 1860 are not assigned to either party. Papers with no party assignment contribute to the newspaper panel but not to partisan comparisons; Panel B of Table 1 gives the counts. Chronicling America's essays on each title supply founding and ownership context, not the affiliation codes.

Treating affiliation as predetermined requires founding dates, and the corpus metadata does not supply them. Its date fields record the span of each title's *digitized run*, clipped to the study window, so every title has a first-issue date inside 1860–65; a balance test on that field would measure when papers entered the digitized archive, not when they were founded. I therefore reconstruct founding years from the Library of Congress serials catalogue, following the preceding-title chains under which nineteenth-century papers were repeatedly renamed. The chain was needed for 45 percent of titles: the Wheeling *Daily Intelligencer* is catalogued as beginning in 1865, but its predecessor titles reach back to 1859. Founding years resolve for 464 of 485 titles, with a median of 1854, and Section 4.3 uses them to test whether affiliation is predetermined.

4 Empirical Strategy

Three designs carry the paper. The first is a within-newspaper event study of coverage around congressional elections, which locates the movement in slavery coverage in the 1860 cycle. The second splits the first by

party. The third compares competing papers in the same city and month, which separates framing from topic. Two more are reported in the online appendix: one asks what the campaign displaced, given that measured output did not grow, and one links county coverage to vote shares. Only the first design carries a causal claim, the effect of a state's own congressional election on its papers' coverage; the others describe what moved around that effect and how papers differed. Newspapers are indexed by j, months by t, and cities by m. Three symbols keep one meaning throughout: y_{jt} is an outcome, α_j and λ_t are the newspaper and calendar-month fixed effects, and G is a number of clusters. Other symbols are defined where they are used.

4.1 Design 1: within-newspaper event study

Event time k_{jt} is the calendar month minus the month of the congressional election to which paper j's month t is attributed (Section 3.6). The event study is

$$y_{jt} = \sum_{k=-6}^{6} \delta_k \cdot \mathbf{1}[k_{jt} = k] + \alpha_j + \lambda_t + \varepsilon_{jt}, \tag{2}$$

where y_{jt} is the slavery dictionary share $s_{slav,jt}$, its word-weighted counterpart, or another issue's dictionary share; α_j is a newspaper fixed effect absorbing each paper's stable coverage level; and λ_t is a calendar-month fixed effect. The omitted category is the far months, $|k_{jt}| \geq 7$, so each δ_k compares coverage k months from the vote with the same paper's coverage far from any congressional election, net of the calendar month.

The calendar-month fixed effect is what makes the design more than a description of the calendar. With it, the δ_k are identified only from newspapers in states voting on different dates being at different distances from their own election in the same calendar month. The staggered congressional calendar supplies that variation. A presidential contest does not: it falls on one national day, so within a presidential sample distance to the vote is a deterministic function of the calendar month. Estimating the grouped-window version of Equation 2 on presidential timing confirms it, since all 5 window terms are collinear with the fixed effects and none can be estimated. Every event-study result below uses congressional elections only. The identifying assumption is that, absent its own election, a paper's coverage would have moved with that of papers voting on other dates in the same calendar month: no shock specific to the states that voted on a given date coincided with their vote. A national shock cannot violate it; a regional one aligned with an election date can. In 1860 the leading candidate is the secession crisis, which broke in the months just after the October 9 states voted (Section 5.2).

The far months matter because without them event time is an exact function of the two sets of fixed effects: in the 1860 Republican sample restricted to months -9 through $+3$, the newspaper and calendar-month fixed effects explain all of the variation in k_{jt}. The level of coverage before the vote is defined only against some reference, and in the 1860 cycle every available reference is weak. The far months after the vote ($k_{jt} \geq 7$) fall

in 1861 or 1862, when the country was at war, for every state except Kansas, whose December 1859 vote puts them in the second half of 1860; a level estimated against them alone mostly compares the campaign with wartime newspapers. The far months before the vote ($k_{jt} \leq -7$) are a short stretch at the start of the corpus for the states that voted in the autumn of 1860, because the corpus begins in January 1860, and for the states that voted in 1861 they are the months of the presidential campaign and the secession winter. Against them alone the problem is deeper. Within one cycle event time moves one-for-one with the calendar, so when every comparison month lies before the vote, a rise just before the vote cannot be told apart from a steady drift in coverage across the whole period, and the estimate depends on how the months near the vote are grouped.

The headline estimand therefore does not depend on the level. It is the break in coverage across the vote:

$$\theta = (\delta_{+1} - \delta_{-1}) - \tfrac{2}{3}\,(\delta_{-1} - \delta_{-4}). \tag{3}$$

The first term is the change from the month before the election to the month after it. The second removes the pre-vote trend: the slope from four months before the vote to one month before, $(\delta_{-1} - \delta_{-4})/3$ per month, carried forward the two months from $k = -1$ to $k = +1$. The election month itself is left out, because it mixes days before and after the vote. Because θ is a difference of differences in event time, a shift in level or a linear trend that separates one choice of comparison months from another cancels out of it. The break is therefore much less sensitive to the reference than the level, though not immune: against the far pre-election months alone it is about 21 percent smaller (Section 5.1), because that specification drops the far post-election months and so rests on a smaller sample (1,867 against 2,477 newspaper-months, 145 against 159 papers), and because the gap between the two event-time paths is close to linear but not exactly so. I estimate θ directly as a single regression coefficient, report placebo breaks centered three and four months before and after the vote, and vary the window over which the pre-vote trend is measured.

With far months on both sides as the omitted category, Equation 2 is an event study with binned endpoints, which Schmidheiny & Siegloch (2023, p. 706) show is equivalent to a distributed-lag model; their generalization covers units that experience the event more than once, as papers do across the three cycles, provided each event has the same effect (Schmidheiny & Siegloch, 2023, p. 708). Within the 1860 cycle the coefficients pool 11 election-date cohorts. If cohorts respond differently, each pooled coefficient is a weighted average of their responses with weights that need not be the cohort shares, and it can pick up responses from other event times (Sun & Abraham, 2021).[2] Within a cycle each state votes once, so the estimators built for heterogeneous responses to a one-time event apply, and Section 6.5 reports the break separately for each

[2] I do not use the decomposition of Goodman-Bacon (2021). It applies to a single static two-way fixed-effects difference-in-differences coefficient, whose difficulty is that units already treated serve as controls for units treated later. The specification here estimates a separate coefficient for each month from the vote, so the relevant difficulty is the contamination across event times described above, which the cohort-by-cohort estimates address.

election date and under three of them (Borusyak et al., 2024; de Chaisemartin & D'Haultfœuille, 2026; Sun & Abraham, 2021).

A second break was specified before it was estimated. Each dictionary issue's 1860-cycle share is divided by its own mean in the far months, so that a break becomes a proportional change. The primary contrast is slavery's proportional break minus that of the rest of the non-campaign agenda, where the rest of the agenda is the sum of the other issue shares divided by the sum of their far-month means. Weighting by baseline in this way keeps small issues from dominating the comparison. Two secondary contrasts set slavery against campaign coverage and against the narrow platform item, the dictionary's tariff and land-policy topic, which covers the homestead bill. The break of a difference is the difference of the breaks, so the same estimator and the same inference apply.

For the level itself I use a grouped-window version of Equation 2 that collapses the months into windows of one to three and four to six months before the election, the election month, and one to three and four to six months after, against the far months on both sides. With that two-sided reference the point estimates barely depend on the grouping, but they do depend on the reference, so Section 5.1 reports the level against each reference in turn. I also estimate a Poisson count model in which the outcome is the raw number of slavery articles and $\log(\text{words}_{jt})$ enters as a control, which distinguishes a genuine increase in slavery content from a compositional shift within a paper of changing size.

4.2 Design 2: partisan decomposition

Design 1 is re-estimated separately for Democratic and Republican papers, and with an interaction of the pre-election window with Democratic affiliation that tests the difference directly, both within 1860 and pooled across cycles. Under a common-shock account both parties' papers respond to the same electoral calendar. Under an issue-ownership account, candidates emphasize the issues on which they are advantaged and their opponents are less well regarded (Petrocik, 1996), so the party that owns slavery presses it and the other gives it less room. The same specification is run on the race-and-equality topic, to ask whether the two sides differed in how much racial-status language they raised. The number of election dates limits what this design can say: within a cycle each party's papers draw on only a handful of congressional dates, several cycle-by-party cells rest on three to five, and Section 4.4 explains why such cells support no informative inference.

4.3 Design 3: within-city cross-paper comparison

Cities where the archive indexes newspapers of both affiliations allow a within-market comparison. Within the same city and month, a Democratic and a Republican paper face the same local events, the same local economy, and the same election date, and a city × month fixed effect absorbs all of them:

$$y_{jt} = \gamma \cdot \text{isDem}_{jt} + \mu_{m(j)t} + u_{jt} \tag{4}$$

where isDem_{jt} indicates that paper j endorsed the Democratic ticket in the cycle to which month t belongs, $m(j)$ is paper j's city, and $\mu_{m(j)t}$ is a city × month fixed effect absorbing every local shock common to the competing papers. Every within-city estimate in the paper uses it; for the pooled gaps, a version with city fixed effects only, which also compares papers across months, is reported as a check. I estimate $\hat{\gamma}$ with topical measures (the slavery dictionary share and its word-weighted counterpart) and with a framing measure (the race-and-equality topic share), because the question is whether they give different answers.

What the design does not hold fixed is the readership. Two papers in one city need not serve the same readers, and the media-economics literature gives good reason to expect they did not: in modern data, right-wing newspapers "circulate relatively more in zip codes with a higher proportion of Republicans, even within a narrowly defined geographic market," and left-wing papers show the opposite pattern (Gentzkow & Shapiro, 2010, p. 37). The estimand is therefore the difference in editorial content between partisan newspapers operating in the same local market, holding the local political and economic environment fixed. It is not a supply-side difference with reader demand held constant. That is the weaker reading, but it is also the more interesting one for Mullainathan & Shleifer (2005), whose prediction concerns exactly the case of outlets serving differently disposed segments of a common market.

The sample is 33 papers in 17 cities, observed over 1,352 newspaper-months. Cities are keyed on city and state, so that places sharing a name in two states, such as Marysville, California and Marysville, Kansas, are not pooled. With 17 city clusters, conventional cluster-robust inference is unreliable, so every within-city estimate carries a wild cluster bootstrap p-value on city clusters (Cameron et al., 2008).

The identifying assumption is that partisan affiliation is predetermined relative to the elections studied. Two facts bear on it. First, affiliation is nearly stable: 4 of the 33 papers carry different party labels in different cycles. For those 4, isDem_{jt} varies over time, so they contribute within-paper rather than cross-paper variation, and dropping them removes 4 of the 17 cities. Section 5.8 reports the estimates without them. Second, the affiliations are balanced on founding date. Using the reconstructed founding years of Section 3.7, Democratic papers in the dual-affiliation cities were founded in a mean year of 1848 and Republican papers in 1852, a difference indistinguishable from zero under both a t-test and a Wilcoxon test. Of the 27 papers in

these cities with a resolved founding year, 24 were founded before 1860 and 20 before 1856. Dropping the papers founded during the study period, whose affiliation could in principle respond to the sectional crisis, leaves the framing estimates essentially unchanged (Section 5.8).

4.4 Inference

Treatment timing is assigned by election date, not by newspaper: every paper in a state shares its election date exactly. Standard errors are therefore clustered two-way, on the newspaper and on the congressional election date. The date clustering matters: on the pooled dictionary estimate it raises the standard error by 33 percent over newspaper clustering alone, and by 59 percent within the 1860 cycle, without changing the point estimate. The sample contains 35 congressional election dates, 34 of which remain in the pooled regressions once fixed-effect singletons are dropped, but a single cycle has about a dozen; the 1860 cycle has 11.

With so few clusters within a cycle, single-cycle estimates carry an exact wild cluster bootstrap p-value, clustered on election date (Cameron et al., 2008). With G date clusters there are 2^G sign patterns, and the bootstrap enumerates all of them, so its p-values move in steps of $1/2^G$. Ties count as at least as extreme as the observed statistic, which makes the test conservative, and where the bootstrap t-statistic is numerically unstable no bootstrap p-value is reported. The smallest attainable p-value is $2/2^G$: 0.25 with three dates, 0.125 with four, 0.0625 with five. Some cells have only three to five dates: the 1860 Democratic papers (5), the 1862 and 1864 Democratic papers (3 and 4), and the contrast between the October and November 1860 voting cohorts (3). Those cells cannot support informative inference, so I report their point estimates and do not quote analytic p-values for them.

The bootstrap, like randomization inference below, yields a p-value and no interval. The confidence intervals drawn in the figures therefore come from the analytic two-way standard errors; with about a dozen date clusters they are approximate and can be too narrow, and where a drawn interval and a reported p-value disagree, the p-value is the test. In the text, an estimate is *significant* when its p-value on the stated test is below 0.05, *marginal* when it lies between 0.05 and 0.10, and *indistinguishable from zero* when it is 0.10 or more; the tables report the p-values themselves.

For the 1860 cycle I also use randomization inference over the one thing the design treats as random: which state voted on which date. Each of 1,000 permutations reassigns the 1860-cycle congressional dates across states, recomputes event time, and re-estimates the coefficient, holding the calendar and every newspaper's coverage fixed. A purely national shock cannot produce a coefficient in the tail of that distribution. A regional shock that coincided with one election date can, because it lines up with the true assignment of

dates to states and with almost none of the reshuffled ones. Randomization inference therefore rules out the first and not the second; dropping the October 9 states (Section 5.2) and estimating the break date by date (Section 6.5) examine the second without settling it.

5 Results

5.1 Slavery coverage on the electoral calendar

In the 1860 cycle, slavery coverage fell in the month after each state's congressional vote. The break in Equation 3, the drop from the month before the election to the month after net of the pre-vote trend, is −0.016 on the dictionary article share (SE 0.007) and −0.033 on the dictionary word share (SE 0.014). Both are significant under the exact wild cluster bootstrap on the cycle's 11 election dates and under randomization inference over the state-to-date map. Estimators that let the response differ by election date give a larger break on the article share, and a word-share break of about the same size that is no longer significant (Section 6.5). In the far months slavery coverage took 3.9 percent of a paper's articles and 9.5 percent of its words, so the article-share break is 41 percent of the far-month level: slavery lost about two-fifths of its normal share of articles across the vote. A typical weekly printed some 82 items an issue, so the break of 1.6 percentage points is about 1.3 fewer slavery articles in each issue. The word-share break of 3.3 points is 35 percent of the far-month level, or about 0.13 of a page in a 4-page issue. The break is concentrated in the four states that voted on October 9 and absent in the eight that voted on November 6 (Section 5.2, Section 6.5).

Figure 2 shows the monthly path for all 1860-cycle papers against the far months. On both measures coverage sits above the far months throughout the six months before the vote, is lowest four months out, rises to a peak two months before the vote, stays high through the election month, and falls in the month after. The break is the gap, at one month after the vote, between the estimate and the pre-vote trend carried forward.

The break sits at the vote. The same estimator centered three or four months before or after the election finds smaller breaks, none significant on either measure. The largest, four months after the vote on the article share (−0.008), is half the size of the break at the vote and marginal. The break at the vote changes little when the pre-vote trend is measured differently. Taking the trend over five months rather than three gives −0.014 on the article share, now marginal, and −0.030 on the word share, still significant. Taking the trend symmetrically from both sides of the vote gives −0.015 and −0.035, both significant. Within the six-issue family of Section 6.2, slavery's break stays significant after a Benjamini–Hochberg correction and is marginal after a Bonferroni correction.

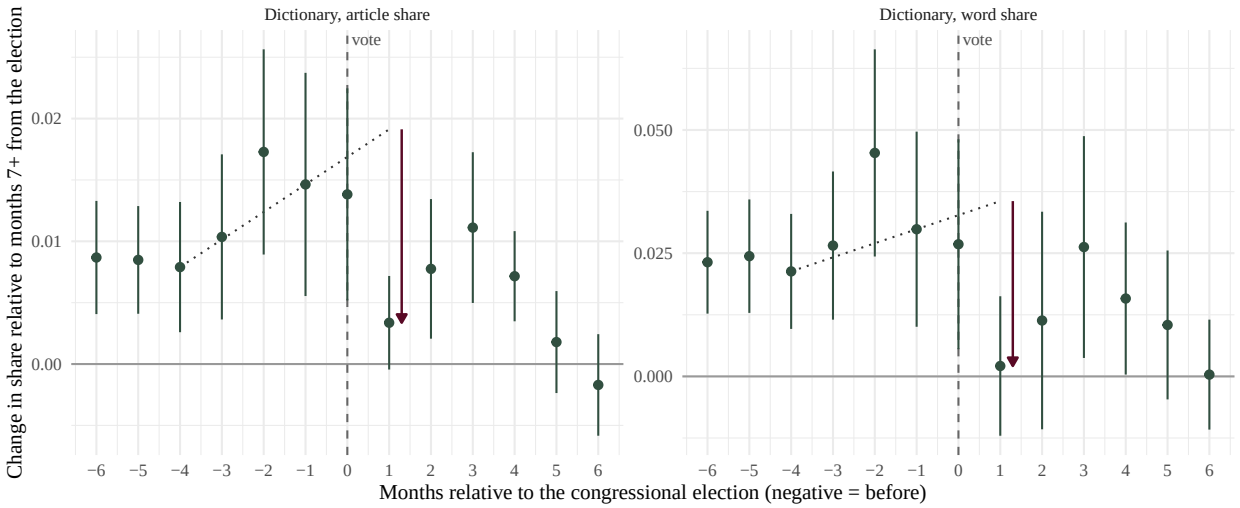


Figure 2: Slavery coverage around the 1860-cycle congressional elections. Each point is an event-study coefficient from Equation 2 for all 1860-cycle papers: the newspaper-month share of articles containing two different slavery entries of the dictionary, and of words in those articles, k months from the paper's congressional election, relative to the same paper's share in months at least seven months from it, with newspaper and calendar-month fixed effects. Panels: dictionary article share, dictionary word share. Bars are 95 percent confidence intervals from standard errors clustered two-way on newspaper and election date; with 11 election-date clusters they are approximate, and the break is tested in the text with the exact wild cluster bootstrap and randomization inference. The dashed line marks the election month. The dotted line extends the pre-vote slope, measured from four months to one month before the vote, to one month after it; the burgundy arrow at one month after the vote is the break of Equation 3, running from that extrapolation down to the estimate. Source: AmericanStories corpus (Chronicling America), 1860–1865; author's calculations.

The *level* of coverage before the vote is less secure than the break. Against the far months on both sides of the vote, the dictionary article share is +0.0142 higher in the one to three months before it and the word share +0.0336, both significant under the bootstrap. Each is a little over a third above its far-month level (37 and 36 percent): about 1.2 more slavery articles in a weekly issue, or 0.13 of a page. The article estimate barely moves when the months near the vote are grouped differently (+0.0139 to +0.0142), though under some groupings it is marginal rather than significant. A Poisson model of slavery-article counts with paper size controlled agrees on direction: the count of slavery articles rises 23 percent before the 1860 vote (incidence-rate ratio 1.23), significant on the analytic standard error, so the share result is not an artifact of share arithmetic.

The level depends on the reference, and the break much less. Against the far pre-election months alone the level estimates move a great deal, because with a one-sided reference a single cycle cannot separate a rise just before the vote from a steady drift (Section 4.1). The one-sided article estimate is +0.0082 with the grouped windows, +0.0054 when months four to six before the vote get their own indicators, and about zero with monthly indicators; none of the three is distinguishable from zero. Against the far post-election months alone, which are mostly wartime, the level is +0.0089, also indistinguishable from zero. The break against the far pre-election months alone is −0.012 on the article share (SE 0.007), about 21 percent smaller than with the two-sided reference (−0.016) and just under two standard errors from zero.[3]

Table 3 reports the pre-election window cycle by cycle against the two-sided reference. The rise is an 1860 phenomenon. In 1862 and in 1864 the article-share estimates are close to zero (−0.0001 and +0.0007, against +0.0142 in 1860), and so are the word-share estimates; none of the four is distinguishable from zero. Dropping 1860 removes the effect entirely. Pooling all three cycles leaves an article-share estimate of +0.0042, 11 percent of the pooled far-month level, and a word-share estimate of +0.0108, both significant. The pooled number is an average of one cycle with a large response and two with none.

The break across the vote tells the same story. Slavery coverage shows no break across the 1862 vote (article share +0.003, word share −0.003, both indistinguishable from zero). Across the 1864 vote the article share breaks by −0.005 and the word share by −0.014, against −0.016 and −0.033 in 1860. The 1864 breaks are about a third and two-fifths of the 1860 breaks. The article-share break is indistinguishable from zero on both tests; the word-share break is marginal under the bootstrap and indistinguishable from zero under randomization inference. Both cycles carry a caveat. In 1862 the placebo break four months before the vote is itself significant and the one three months before is marginal, so coverage in that cycle moved at other times too. In 1864 the states that voted for Congress in October held the presidential vote the following month, inside the post-vote window, so their break mixes the two contests.

[3] I did not run the bootstrap for the one-sided break, and did not estimate a break against the far post-election months alone.

Table 3: Pre-election slavery coverage by election cycle, congressional elections

Cycle	Article share	Word share	Date clusters
1860	+0.0142** (0.0037) [0.031]	+0.0336*** (0.0082) [0.004]	11
1862	−0.0001 (0.0035) [0.977]	+0.0020 (0.0079) [0.933]	11
1864	+0.0007 (0.0018) [0.727]	+0.0035 (0.0035) [0.423]	12
Dropping 1860	+0.0001 (0.0022) [0.956]	+0.0012 (0.0050) [0.815]	22
All cycles	+0.0042** (0.0020) [0.045]	+0.0108** (0.0043) [0.016]	34

Note: Each cell is the coefficient on the one-to-three-month pre-election window in a grouped-window regression of a newspaper-month coverage share on event-time windows, against the same newspaper's months at least seven months from its congressional election, with newspaper and calendar-month fixed effects. Standard errors clustered two-way on newspaper and election date in parentheses. Brackets: exact wild-cluster-bootstrap p-value (election-date clusters) for the single-cycle rows; analytic two-way p-value for the pooled rows. Stars use the bracketed p-value: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The article measure is the share of a paper's articles containing at least two different entries of the dictionary's slavery-and-abolition topic; the word measure is the share of the paper's words that sit in such articles. Source: AmericanStories corpus, 1860–1865; author's calculations.

The second caveat can be checked directly, by dropping every 1864-cycle state whose congressional vote fell in October. That rule removes Indiana, Ohio, Pennsylvania, and West Virginia, which all voted in October 1864. It also catches Virginia, but only because Virginia's 1864-cycle vote was held in October 1865; it has 2 papers. The sample keeps 20 states, 17 of which voted on November 8, 1864 or later, with 9 election dates and 87 papers. The rule was fixed before estimation. The article-share break is larger than in the full cycle but not significant on either test: −0.009. The word-share break is −0.026, about three-quarters of the 1860 break; it is not significant under the bootstrap and marginal under randomization inference. Among Republican papers (7 dates, 55 papers) the article break is −0.009, not significant on either test, and the word-share break is −0.029, marginal on both. Democratic papers have 2 election dates, too few for informative inference. With 9 dates, nearly all in November, the test is weak, and the caveat is not resolved: the 1864 evidence points the same way as 1860 on both measures but is significant on neither.

The concentration fits the setting described in Section 2, where only the first of the three cycles turned on slavery's extension into the territories and the later two on the conduct of the war. The first cycle is itself mixed: 6 of its 11 congressional votes fell in 1861, 4 of them after the war had begun, though most of its papers are in states that voted in the autumn of 1860. Coverage followed the issue the campaign was about.

5.2 Election timing and the secession crisis

The 1860 concentration raises an obvious objection. Autumn 1860 is not only the presidential campaign; it is the run-up to disunion. South Carolina seceded on December 20 and six more states followed by February. A movement in slavery coverage around November 1860 might simply be the sectional crisis, with the election incidental. Three pieces of evidence bear on this, and one caveat limits them.

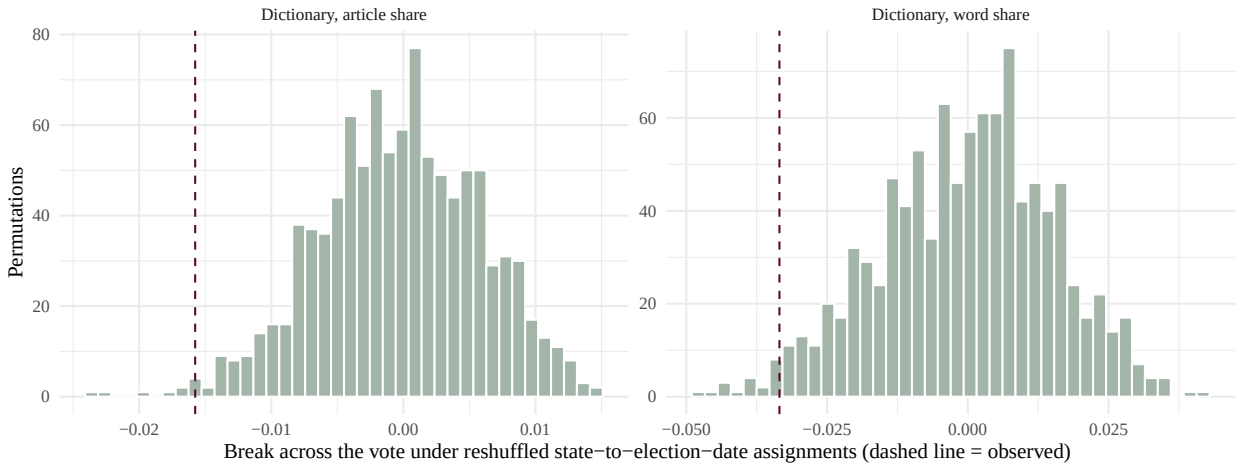


Figure 3: Randomization inference on the break across the vote, 1860 cycle. Histograms show the break of Equation 3 across 1,000 random reassignments of the 1860-cycle congressional election dates to states, holding the calendar and each paper's coverage fixed; the dashed line marks the observed break. Panels: dictionary article share, dictionary word share. Source: AmericanStories corpus (Chronicling America), 1860–1865; author's calculations.

The first is that coverage tracks each state's own election date, and the 1860-cycle dates ran from December 1859 to October 1861 (Section 3.6). Calendar-month fixed effects absorb everything national, so the estimates are identified only from papers at different distances from their *own* election in the same calendar month. Reassigning the 1860-cycle dates across states at random places the observed break in the tail of the resulting distribution, significantly so on both the article and the word share (Figure 3), and the pre-election level likewise. A purely national timeline cannot produce either.

The second is the direction of the post-vote movement. If the autumn rise were crisis-driven, slavery coverage should have kept rising through the winter of secession. It fell instead. Within three months of the vote, the dictionary share is 0.8 percentage points below its pre-election level, a significant fall: about half of the pre-vote rise was gone. For the states that voted in the autumn of 1860, that fall came in exactly the months when disunion moved from threat to fact. The third is that the timing is not seasonal. In odd years, away from congressional elections, the same papers that were Republican in 1860 show no autumn rise in slavery coverage. The dictionary share shows a significant *decline* (-0.006). September through November is not a high-slavery season for these papers.

The caveat is regional. The October 9 group is Pennsylvania, Ohio, Indiana, and Iowa, and the months after its vote, November 1860 through January 1861, are the onset of secession. Calendar-month fixed effects absorb any reaction to secession common to all papers, but not one that differed by region, and none of the evidence above addresses it: the permutation test reshuffles which state voted when, and the placebo breaks sit at other months. The direct check is to drop the October 9 states. Without them, on 85 papers and 10

election dates, the break is −0.005 on the article share (SE 0.006), or 0.5 percentage points, and −0.018 on the word share (SE 0.015). That is about a third of the full-cycle break on articles and half on words, and neither can be told from zero under the bootstrap or randomization inference. Dropping every October date, which also removes West Virginia's October 1861 vote, leaves −0.003 on articles and −0.011 on words, again indistinguishable from zero. Estimated date by date (Table 5), the break is largest in the October 9 states and absent in the eight states that voted on November 6, which hold 35 of the 85 papers left once October 9 is dropped. The smaller breaks in California and West Virginia, which voted in wartime in 1861, do not settle the question (Section 6.5). A regional reaction to secession in the October 9 states therefore cannot be ruled out: coverage followed each state's own calendar, but the evidence does not say whether the four October states followed it for electoral or for sectional reasons.

5.3 Campaign material or news?

A paper could carry slavery in two ways around an election. As campaign material, it would arrive and leave with the tickets, nominations and returns, timed to the paper's own vote. As news, it would follow events, and the vote would not matter. The drop across the vote was not specific to slavery; the 1860 campaign agenda shared it. Campaign coverage fell hardest, by −0.073 on the dictionary measure, 7.3 percentage points against a far-month level of 7.0 percent of articles. The other item of the Republican platform fell with it: the tariff and land policy, which covers the homestead bill, by −0.006. Both falls are significant. No other issue broke downward at the vote. Immigration, labor, temperance, finance, the draft, race and equality, and the Copperhead and states'-rights topic all show breaks indistinguishable from zero under the bootstrap. War coverage rose significantly across the vote (+0.015), but that estimate is hard to read. Calendar-month fixed effects absorb the national course of the war, not where it was fought, and the four cohorts that voted after Fort Sumter did so in wartime, three of them (Maryland, Tennessee and West Virginia) close to the fighting, while the 1860 cohorts voted in peace. The war estimate mixes the election response with when and where the war was fought. Slavery moved with the campaign agenda.

A comparison specified before it was estimated puts a number on this. Measured as a share of its own far-month level, slavery coverage fell 41 percent across the vote. The rest of the non-campaign agenda, weighted by how much space each issue normally took, was essentially unchanged (1 percent). The difference, −0.42 (SE 0.15), is large and significant under both the bootstrap and randomization inference. Slavery was election-timed far more than the general news mix, and *less* than the pure campaign items. Campaign coverage fell by more than its whole far-month level (104 percent), and so did the tariff and land-policy topic (115 percent). Slavery's proportional drop is smaller than campaign coverage's by 0.63 and smaller than the

platform topic's by 0.74, in units of each topic's own far-month level. The gap from campaign coverage is significant on both the bootstrap and randomization inference, and the gap from the platform topic only on the bootstrap. Slavery sat inside the 1860 campaign agenda without being the campaign itself.

Two features of the comparison group, both fixed in advance, cut against a clean reading. The rest of the agenda includes the race-and-equality topic, which is close to slavery and would, if anything, pull the two series together. And it is dominated in weight by war coverage, which rose across the vote, so the finding that the rest of the agenda held leans partly on a series whose timing is hard to read. Neither was changed after estimation, though the topic definitions date from after the comparison was fixed. The contrast is also estimated on the whole 1860 cycle, including the October 9 states.

The 1860 timing points to campaign material. Slavery coverage was timed to each state's own vote, which a national run of news cannot produce. News that reached some regions differently, the secession crisis after the October 9 votes above all, is not ruled out (Section 5.2). Slavery left the page together with campaign coverage and the other platform item, and no other issue broke downward at the vote. The evidence on the news side is ambiguous. The slavery topic does not single out the months of the Emancipation Proclamations, ranking them 12th and 34th of 72 (Section 3.3). That may say that slavery coverage did not follow slavery news, or only that the dictionary misses how papers wrote about emancipation, and the data cannot separate the two. In 1862 and 1864, when slavery was no longer the campaign's subject, its coverage shows no significant election timing (Section 5.1).

5.4 Flat output

Papers did not grow before elections. In 1860, article counts in the three months before the vote were 0.050 log points lower than in the far months (about 4.8 percent), word counts 0.011 lower (about 1.1 percent), and pages per issue flat at about 3.9; none of the three is distinguishable from zero. The same holds in 1862 and 1864, where article and word counts are slightly higher before the vote and again indistinguishable from zero. Whatever the campaign added had to come out of something. The online appendix asks what. Campaign coverage gained most before the 1860 vote. Among the other issues, slavery and the secession vocabulary of the Copperhead and states'-rights topic gained most, each above what a squeeze in proportion to each topic's size would predict. No category of routine content fell significantly, so no measured category carries the loss. Those estimates are pre-election levels and share the reference dependence above, and flat output fits a publisher who could not expand as well as one who chose not to.

5.5 Articles or column inches?

Most estimates so far are shares of *articles*, while any claim about room on the page is about *space*. Articles differ in length (212 words on average, with a standard deviation of 50), so a rise in the article share need not be a rise in column inches. The word share, the words in articles that contain a dictionary term over all words published, measures space directly.

The results hold in words and are larger. Before the 1860 vote the word share of slavery coverage rises +0.034 against +0.014 for the article share, both significant, and across the vote it breaks by −0.033 against −0.016. Pooled across cycles, the word share rises +0.0108 against +0.0042 for the article share.

The gap between the two has a simple source: articles that mention slavery are longer than average, taking 9.7 percent of words against 4.0 percent of articles across the panel (Section 3.6). Before the 1860 vote the average article of any kind also ran about 8 words longer than in the far months, 4 percent of a far-month mean of 218, a marginal difference. Because those articles run long, a shift of space toward slavery shows up more strongly in words than in articles. The article share is kept as the headline because it is the simpler unit and the more conservative of the two.

5.6 Both parties in 1860

Republican and Democratic papers both show the 1860 pattern in their point estimates, and neither party's estimates are significant on their own. Among Republican papers the pre-election dictionary share rises +0.012, on 7 election dates, and coverage breaks across the vote by −0.017; neither is distinguishable from zero. The Republican level, like the full-cycle level, depends on the reference months. Against the far pre-election months alone, where a single cycle cannot pin down the level (Section 5.1), the Republican pre-election rise disappears (−0.0004); against the far post-election months alone ($k \geq 7$) it is +0.011, also indistinguishable from zero. Among Democratic papers the rise is +0.010 and the break −0.007, on 5 election dates, too few for informative inference. Estimated directly, the Democratic-minus-Republican difference in 1860 is −0.002, indistinguishable from zero (Table 4). The 1860 party comparison is underpowered: it rests on 5 Democratic and 7 Republican election dates, and it cannot tell a common response from a moderately asymmetric one.

An issue-ownership account (Petrocik, 1996), in which the party that owned slavery pressed it and the other gave it less room, is a hypothesis this design cannot separate from a common response to the calendar. Both parties' papers moved with the 1860-cycle vote, and the difference between them is imprecise. Pooled across all three cycles, Democratic papers raised their slavery coverage before elections more than Republican papers did (+0.011 on the article share, +0.022 on the word share, both significant on analytic standard

Table 4: Pre-election slavery coverage by party, congressional elections

	Dictionary	Race and equality	Date clusters
1860 Republican papers	+0.0117 (0.0068) [0.203]	+0.0019 (0.0016) [0.312]	7
Democratic minus Republican, 1860	−0.0015 (0.0070) [0.871]	+0.0020 (0.0018) [0.406]	9 (5 Dem.)
Democratic minus Republican, all cycles	+0.0105*** (0.0033) [0.004]	+0.0036*** (0.0009) [0.000]	28 (12 Dem.)

Note: The first row is the coefficient on the one-to-three-month pre-election window for 1860 Republican papers, from the specification in the note to the by-cycle table. The other rows are the coefficient on the interaction of that window with a Democratic indicator, within 1860 and pooled across the three cycles. Standard errors clustered two-way on newspaper and election date in parentheses. Brackets: exact wild-cluster-bootstrap p-value (election-date clusters) for the 1860 rows; analytic two-way p-value for the pooled row. Stars use the bracketed p-value: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dictionary column is the share of articles containing at least two different entries of the dictionary's slavery-and-abolition topic. The race-and-equality column is the share of articles with at least two different entries of its race-and-equality topic. Democratic papers in 1860 alone rest on five election dates, too few for informative inference, and are reported in the text and the online appendix. Source: AmericanStories corpus, 1860–1865; author's calculations.

errors). The article-share gap of 1.1 points is 27 percent of the pooled far-month level. They also raised race-and-equality coverage more (+0.004, 27 percent of that topic's pooled far-month level of 1.3 percent), a pattern the within-city design below examines with a sharper comparison. The pooled difference comes from 1862 and 1864, where the Democratic cells rest on 3 or 4 election dates; within those cycles no difference in slavery coverage is distinguishable from zero under the bootstrap. The pooled tests are analytic, and they lean on cells too thin for informative inference; the online appendix reports all of them.

Did 1860 Republican papers reprioritize their agenda toward slavery, or add slavery on top of a larger political section? The evidence leans toward the second. Their campaign coverage rose significantly before the vote, by 3.8 percentage points (+0.038), about three times as much as their slavery coverage (1.2 points), and a within-paper index of slavery relative to campaign coverage, defined and estimated in the online appendix, is indistinguishable from zero; its point estimate is negative, as adding slavery on top of a larger campaign section would imply. In 1860 slavery rose with the campaign, not ahead of it.

5.7 Papers with a rival in town

Mullainathan & Shleifer (2005) predict that where reader beliefs diverge, as on politically divisive issues, competing newspapers segment the market and slant toward extreme positions. For the early twentieth century, newspapers used their political orientation to differentiate from competitors (Gentzkow et al., 2014, p. 3073). If a rival raised the stakes of slavery coverage, the 1860 break should be larger for papers with a competitor in the same town. This test was specified before it was estimated.[4]

The point estimate of the break is larger for papers with a rival, but the difference cannot be told from zero. On the dictionary article share the break is −0.0266 for the 36 papers with a rival and −0.0138 for the

[4]The specification named an earlier article-share measure as the primary outcome. The dictionary article share takes its place, with the word share as confirmation; the substitution was made after the original estimates and does not change the verdict. The definition of a rival was also corrected after the first estimates, to fix a coding error; the online appendix gives both.

123 without, 2.7 against 1.4 percentage points. The difference, −0.0127, is indistinguishable from zero under the wild cluster bootstrap, and the minimum detectable difference, 0.0159, is larger than the estimate. The test is uninformative about competition rather than evidence against it, and the word share gives the same answer. It also measures how much slavery coverage a paper ran and when, not its slant; the within-city framing gap of Section 5.8 is the comparison closer to the Mullainathan–Shleifer prediction. The online appendix gives the definition of a rival and the details of inference.

5.8 Framing and topic within the same city

The party comparisons of Section 5.6 compare different papers in different places. The within-city design holds the market constant instead. It compares two things: how much slavery coverage a paper ran, and how often it used the vocabulary of racial status and equality. I call the second the framing measure. It is a separate topic share over all articles, so it records which vocabulary a paper used, not how its slavery articles were written. As Puglisi & Snyder (2015) note, following McCombs, coverage can also "emphasize particular attributes of the topic, making these attributes more salient" (p. 654). The vocabulary share is the nearest the dictionary comes to measuring that.

Democratic papers ran somewhat more slavery coverage than their Republican neighbours. The Democratic-minus-Republican gap in the slavery dictionary share is +0.009, marginal under the wild cluster bootstrap: 0.9 percentage points, 22 percent of the Republican papers' mean share of 3.8 percent in these cities. Measured in words the gap is +0.019, also marginal. With city fixed effects only, which also compare the papers across months, the gaps are +0.006 on articles, marginal, and +0.014 on words, significant. The topical gap sits near the edge of what the design can detect. The minimum detectable effect at eighty percent power is 0.011, about 1.3 times the estimate. The power calculation uses the t distribution with one degree of freedom fewer than the number of cities that identify the gap: those with papers of both parties in the same month, 10 of the 17. An equivalence test that takes the framing gap as the bound cannot show the topical gap to be smaller than it. With so few cities the design can neither confirm nor rule out a topical gap of the size it estimates.

What differs clearly is framing. The gap in the race-and-equality share is +0.0063, significant under the bootstrap, and +0.0048 with city fixed effects only, also significant. Republican papers in these cities used that vocabulary in 1.1 percent of their articles, so the gap is 60 percent of their rate. In absolute terms it is small: about 0.5 more such articles in a weekly issue of some 82 items. It holds without the 4 papers that changed affiliation (+0.0063) and without the papers founded during the study period (+0.0064). Facing the same local conditions, Democratic editors wrote about race, in the vocabulary of equality, amalgamation, suffrage,

and the period's slurs, more often than their Republican competitors across the street. The race-and-equality topic is a noisy measure, so the gap is firmest as a gap in that vocabulary, not in substantive coverage of race.

The framing gap is significant under the bootstrap only in 1864. Within a single cycle the gap is identified only by cities with papers of both parties in that cycle: 7 of 17 in 1860, 4 of 14 in 1862, and 8 of 17 in 1864. In 1860 the framing gap is positive and marginal (+0.0037), and Republican papers ran *more* slavery coverage than their Democratic rivals in the same town (−0.013), also a marginal gap. The 1862 cell is too thin to test: no 1862 estimate, topical or framing, is distinguishable from zero under the bootstrap. In 1864 the framing gap is +0.0080, significant and 68 percent of the Republican papers' rate that cycle, and Democratic papers also ran significantly more slavery coverage (+0.017). That is the campaign in which Democratic politicians in Ohio had made Negrophobia and miscegenation central to their appeal (Jackson, 1980).

The comparison with Ottinger & Posch (2026) turns on what the racial appeal was about. Their 1890s Southern Democratic campaign ran on crime-framed racial threat, measured by race terms appearing alongside crime terms. The race-and-equality topic measures something else: the vocabulary of racial status and of the Northern argument over whether emancipation would bring Black social and political equality. The divergence here is in that vocabulary. Two papers serving one market differed only marginally in how much they covered slavery and clearly in how often they used the vocabulary of racial status and equality. A measure of slavery coverage alone would have found a less certain difference.

6 Robustness and Interpretation

6.1 Reference months, trends, and fixed effects

Against the two-sided reference, the 1860-cycle level holds up under most changes to the specification; the break's smaller dependence on the reference is reported in Section 5.1. Moving the reference to months at least ten from the vote leaves the pre-election estimate at +0.0139, and restricting to papers observed in every window gives +0.0134; both remain significant under the bootstrap. County-by-cycle fixed effects leave the estimate unchanged. Newspaper-specific linear trends shrink it to +0.0094, and it is then indistinguishable from zero on the analytic standard error. Pooled across all three cycles the level is fragile. Against the far pre-election months alone ($k \leq -7$) the pooled estimate is +0.0054, which is significant; against the far post-election months alone ($k \geq 7$) it is +0.0002, essentially zero. This is one reason the paper does not lead with a pooled number.

6.2 Multiple testing

Slavery is not the only issue that moved in the 1860 cycle, so its significance has to be judged within the family of issues tested. The family consists of era-persistent issues comparable across the whole panel: slavery, the tariff and land policy, finance, immigration and nativism, labor, and war. The family was fixed with seven members before any of the estimates reported here; the tariff and the homestead question are now one topic of the dictionary, so it has six. For the break across the vote, slavery and the tariff and land-policy topic both stay significant after the Benjamini–Hochberg false-discovery-rate correction, and both are marginal after the Bonferroni correction. For the pre-election level the correction bites harder. On the bootstrap no issue stays significant after either correction; slavery and the tariff and land-policy topic come closest, both marginal after the false-discovery-rate correction. The sharper test of issue specificity is the proportional contrast in Section 5.3, which compares slavery with the rest of the agenda directly rather than one issue at a time against zero. In the pre-specified comparison of papers with and without a rival in town (Section 5.7), no difference in the six-issue family or the campaign topic is even marginal after the false-discovery-rate correction.

6.3 The Emancipation Proclamation

The preliminary Proclamation of September 22, 1862 falls inside the 1862 pre-election window, and closer still to the vote in the states that voted in October rather than November, so a Proclamation-driven spike could inflate that cycle's coefficient. Estimated within the 1862 cycle, the pre-election estimate is −0.0001 on the dictionary article share and +0.002 on the word share, both indistinguishable from zero, against +0.0142 and +0.0336 before the 1860 vote: there is no 1862 rise for the Proclamation to explain.

6.4 Stress tests on the 1860 result

The central finding rests on one cycle, and four checks bear on it. The first asks whether a handful of heavy-coverage titles drive the pre-election level.[5] Dropping the five newspapers with the highest slavery coverage leaves the 1860 pre-election estimate at +0.0141, and winsorizing the outcome at the first and ninety-ninth percentiles gives +0.0141, against +0.0142 in the full sample; both remain significant.

The second asks whether the 1861 votes drive the break. In the estimation sample, 6 states held their 1860-cycle vote in 1861 and Kansas in December 1859 (Section 3.6). Restricted to the 4 election dates held in calendar 1860, on 124 papers, the break is −0.014 on the article share and −0.030 on the word share, close to the full-cycle estimates. With 4 dates the exact bootstrap cannot produce a p-value below 0.125, and both

[5] I did not run this check on the break.

estimates sit at that floor, so it cannot reach conventional levels here. Randomization inference over the 4 dates is the informative test, and under it neither break is distinguishable from zero. Adding Kansas, 5 dates and 127 papers, gives breaks of −0.019 and −0.043; under randomization inference the article break is significant and the word break marginal, while the bootstrap again sits at its floor of 0.0625. The point estimates do not depend on the 1861 votes, but 4 or 5 dates are too few to test them sharply.

The third is geographic, and it is the check the break fails: Section 5.2 reports that the break cannot be told from zero without the four October 9 states, or without every October date. The two-sided pre-election level is less exposed: without every October date it is +0.0113, four-fifths of the full-cycle level, and still significant. Among Republican papers alone, dropping the October states leaves 5 election dates, too few for informative inference.[6]

The fourth concerns inference. Single-cycle estimates rest on about a dozen election dates, so single-cycle p-values in the paper are exact wild-cluster-bootstrap p-values (Section 4.4), with three exceptions stated where they occur: the Poisson count model, the specification with newspaper-specific trends, and the within-city estimates, whose bootstrap is on city clusters. The estimates that survive the bootstrap are the 1860-cycle break on both the article and the word share, the two-sided level for all papers, and the within-city framing gap. Neither the Republican level nor the Republican break does. Democratic cells with three to five election dates are reported as point estimates only.

6.5 The break by election date and under heterogeneity-robust estimators

The event study pools the 11 election dates of the 1860 cycle into one set of event-time coefficients, and when the response differs across cohorts, as the October 9 check says it does, the pooled coefficients are weighted averages that can pick up responses at other event times (Sun & Abraham, 2021), as Section 4.1 sets out. The break can therefore be estimated separately for each election date, and re-estimated with estimators built for heterogeneous responses. Within a single cohort every newspaper shares one election date, so distance to the election is collinear with the calendar-month fixed effect and a cohort's own months cannot identify its break; the far months of the other cohorts supply the calendar-month effects instead.

Table 5 gives the break by election date. Each cell is the break of Equation 3 from a specification in which every cohort has its own event-time coefficients (Sun & Abraham, 2021), so a cohort's own papers fix their newspaper effects while the calendar-month effects come from papers in other states, and standard errors are clustered on newspaper because each cohort is one election date. The break is negative in 6 of the 8 cohorts that can be estimated, though two of the six carry little information: Maine's rests on three

[6]The 1860-only sample of the second check still contains the October 9 group, and the sample without the October 9 states still contains the six votes held in 1861. I did not estimate a sample that excludes both; it would keep only the September and November 1860 dates and Kansas, too few for informative inference.

Table 5: The break across the vote by election date, 1860 cycle

Election date	States	Papers	Articles	Words
Dec 01, 1859	Kansas	3	—	—
Sep 04, 1860	Vermont	12	−0.0173 (0.0111)	−0.0473 (0.0359)
Sep 10, 1860	Maine	3	−0.0899 (0.0349)	−0.2208 (0.1014)
Oct 09, 1860	Indiana, Iowa, Ohio, Pennsylvania	74	−0.0382 (0.0112)	−0.0455 (0.0353)
Nov 06, 1860	Delaware, Illinois, Massachusetts, Michigan, Minnesota, New Jersey, New York, Wisconsin	35	+0.0106 (0.0144)	+0.0284 (0.0435)
Apr 01, 1861	Connecticut	1	+0.0167 (0.0249)	+0.0289 (0.0608)
Apr 03, 1861	Rhode Island	1	—	—
Jun 13, 1861	Maryland	8	−0.0017 (0.0101)	−0.0045 (0.0276)
Aug 01, 1861	Tennessee	5	—	—
Sep 04, 1861	California	11	−0.0131 (0.0065)	−0.0418 (0.0206)
Oct 24, 1861	West Virginia	6	−0.0242 (0.0096)	−0.0469 (0.0215)

Note: Each cell is the break across the vote defined in the text, estimated from a two-way fixed-effects event study in which every election-date cohort has its own event-time coefficients (Sun and Abraham 2021), with the months at least seven months from the cohort's own vote as the reference. Standard errors clustered on newspaper in parentheses, since each row is a single election date. Papers are those the 1860-cycle regressions use; a paper with a single month in the cycle drops out with its own fixed effect. A dash marks a cohort observed in too few event-time months to form the break. The article measure is the share of a paper's articles containing at least two different entries of the slavery-and-abolition topic; the word measure is the share of its words in such articles. Source: AmericanStories corpus, 1860–1865; author's calculations.

papers, and Maryland's, −0.002, is close to zero. Among the cohorts of more than three papers it is largest in the October 9 states, −0.038 on the article share (SE 0.011) across 74 papers (Maine's −0.090 is larger but rests on too few papers to weigh), and it is absent in the eight states that voted on November 6, +0.011 (SE 0.014) across 35 papers. Vermont, which voted in September 1860, shows −0.017, and California and West Virginia, which voted in September and October 1861, show −0.013 and −0.024. Both 1861 votes came seven to nine months after the secession winter; their breaks are smaller than October 9's, on small cohorts whose post-vote months are wartime, so they do not settle the regional caveat.

Table 6 re-estimates the pooled break with three estimators built for heterogeneous responses, each with the randomization p-value of Section 4.4. The first estimates the break separately for each cohort (Sun & Abraham, 2021) and averages the cohort breaks over the cohorts observed in all three months that define it, each weighted by its observations in those months, so the average is a weighted mean of cohort breaks. The second fits the newspaper and calendar-month effects on the far months alone, imputes them into the months near the vote, averages the residuals by cohort and event time, and aggregates the cohort breaks in the same way, over the 5 of the 8 cohorts for which it can impute every month the break needs (Borusyak et al., 2024). The third is the dynamic estimator of de Chaisemartin & D'Haultfœuille (2026), with the vote itself as the switch: it compares the change from the month before the vote to the month after among papers that had just voted with the same change among papers in states that had not yet voted, so it uses no already-voted paper as a control. Its placebo four months before the vote, estimated the same way, supplies the pre-vote

Table 6: The 1860 break under heterogeneity-robust estimators

Estimator	Articles	Words
Two-way FE event study (paper)	−0.0158 [0.009]	−0.0335 [0.027]
Sun–Abraham, all 8 cohorts	−0.0239 [0.020]	−0.0324 [0.190]
Sun–Abraham, imputation's 5 cohorts	−0.0223 [0.024]	−0.0254 [0.314]
Imputation (fixed effects from far months), 5 cohorts	−0.0212 [0.026]	−0.0240 [0.340]
de Chaisemartin–D'Haultfœuille, vote as switch	−0.0242 [0.015]	−0.0300 [0.239]

Note: Each cell is the break across the vote in the 1860 congressional cycle, all papers, with its randomization-inference p-value in brackets. Every row is judged against the same reshuffles of the state-to-election-date map, which are also those behind the randomization p-values in the results section: all 1,000 for rows 1–4 and the first 200 for row 5, whose estimator is refitted on every reshuffle. Row 1 is the paper's two-way fixed-effects event study. Row 2 estimates cohort-specific event-time coefficients and averages the cohort breaks over the 8 cohorts observed in all three months that define the break, weighted by their observations in those months (Sun and Abraham 2021). Row 3 is the same average over the 5 cohorts that row 4 can estimate. Row 4 fits newspaper and calendar-month effects on the far months only, averages imputed residuals by cohort and event time, and aggregates the cohort breaks as in row 2 (Borusyak, Jaravel and Spiess 2024). Row 5 is the dynamic switching estimator of de Chaisemartin and D'Haultfœuille (2026) with the vote as the switch, against papers in states that had not yet voted: its effect in the month after the vote plus two-thirds of its placebo four months before, the same trend-adjusted break; the two terms rest on slightly different sets of switching papers, as the estimator defines them. Source: AmericanStories corpus, 1860–1865; author's calculations.

trend, so its break is the trend-adjusted break of Equation 3.[7]

On the article share the three estimators agree with the event study in sign and give larger estimates: −0.024 under the cohort aggregation, −0.021 under imputation, and −0.024 under the switching estimator, each significant under randomization inference. Most of the gap between the first two comes from the cohorts they cover: averaged over the 5 cohorts that imputation can use, with the cohort-specific estimator's own weights, its break is −0.022 on articles and −0.025 on words, close to imputation's. The event study's coefficients weight the cohorts implicitly and can pick up responses at other event times; the three estimators avoid both, and they put the break higher. On the word share the point estimates are close to the event study's, −0.032, −0.024, and −0.030, and none of the three is distinguishable from zero under randomization inference. A likely reason is that these estimators use fewer comparisons than the event study, so their null distributions under reshuffling are wider, and the word share is the noisier outcome. The word-share break is therefore suggestive rather than secure. Without the October 9 states the switching estimator's break is +0.002 on articles, close to zero as in the event study.[8]

[7] I also tried defining the switch as entering the six-month campaign window. That leaves no estimate after the vote, because by the month before each election almost no paper in the cycle is still more than six months from its own; the estimable effects stop at 2 months before the vote, on 103 to 21 switchers.

[8] The switching estimator's randomization p-values rest on the first 200 of the 1,000 reshuffles, because each reshuffle re-estimates the switching model; none of its fits failed.

6.6 Is the measured output an artifact of segmentation?

The output measures and every article share depend on how the optical-character-recognition pipeline divided each page into articles. If the segmenter chopped the same printed matter into more pieces as elections approached, article counts would rise while words stayed fixed, and mean article length would fall. It does not. Before elections, pooled across cycles, the average article is 1.4 words longer than in the far months, indistinguishable from zero, and before the 1860 vote it is about 8 words longer, 4 percent of its far-month length and a marginal difference.

The gradual version of the objection is testable through the elasticity of words with respect to articles, which under a splitting story should fall over time as the segmenter becomes more aggressive. By year it is 0.90, 0.96, 0.93, 0.93, 0.94, and 0.92 for 1860 through 1865, with no trend. Mean article length does decline within papers over the period, by about 1.6 percent a year, but the stable elasticity rules out progressive over-segmentation as its cause. Articles are also about 4 percent shorter in October and November than in the rest of the year. The pattern is not election-related: its interaction with election years is small and not significant (+0.015, SE 0.010), and calendar-month fixed effects absorb it in every specification.

6.7 The slavery measure's recall across the war

Against the blind labels, the slavery measure misses more of the slavery discussion after 1860 than in it. Missed articles pull an estimate toward zero, so the weak election timing in the later cycles could be the measure failing to see it. Counting an article on a single dictionary entry instead of two finds most of the labelled slavery articles in every year, at a cost in precision, and raises the far-month level of slavery coverage from 3.9 to 9.4 percent of articles.

Re-estimated on that measure, the 1860 break stays: −0.029 on the article share, 31 percent of the new far-month level against 41 percent at the paper's threshold, significant under randomization inference and marginal under the bootstrap. The 1862 null survives on a measure that misses little of the slavery discussion: neither the pre-election level nor the break is distinguishable from zero on either share.

In 1864 the break is larger in points, −0.014 on the article share against −0.005 at the paper's threshold and −0.027 on the word share against −0.014, but so is the level it is measured against. As a fraction of each measure's own far-month level in that cycle, the article-share break is 14 percent against 13 percent, and the word-share break 14 percent against 15 percent. At the looser threshold the article-share break is indistinguishable from zero under the bootstrap and marginal under randomization inference, and the word-share break is marginal under both. The 1864 pre-election level is indistinguishable from zero on the article share and marginal on the word share (+0.011). The looser threshold is also less precise in 1864 than

in 1860, so its false hits dilute the 1864 break somewhat more. The lower recall does not explain the weak 1864 response. A measure that finds most of that year's slavery articles finds the same proportional break as the paper's measure. Because the 1860 break is proportionally smaller at the looser threshold, the 1864 break is a third to two-fifths of 1860's at the paper's threshold and about half at the looser one, estimated somewhat more precisely but still not significant.

6.8 What the measures capture, and what they miss

The slavery share s_{slav} measures topical presence: how much of a paper is about slavery, not whether that coverage argues for or against the institution. The race-and-equality topic recovers some of the missing dimension. A Copperhead paper warning of amalgamation registers on it, and an antislavery paper indicting slaveholder brutality mostly does not, but it is a dictionary measure and cannot distinguish quotation from endorsement. A Republican paper reprinting a Democratic race-baiting editorial in order to denounce it will register on the race-and-equality topic. The within-city design is partly protected against this, since both papers face the same incentive to quote the other, but the concern is real and is a reason the paper does not interpret framing measures as measures of editorial belief.

Advertising is the larger measurement problem. About 40 percent of corpus items are advertisements or paid, legal, or standing notices (Section 3.4), and every topic share has them in its denominator. A shift in advertising volume at the vote would move every topic's share in the same proportion, so it cannot produce a 41 percent drop in slavery while the rest of the agenda, measured the same way, holds. The break and the proportional contrast are protected in that sense. The displacement results in the online appendix are not. The dictionary's advertisement topic fell before the 1860 vote, though not significantly, and it is too blunt to take advertisements out of the denominator. An article-level advertising classifier, which would take advertisements out of the denominator and test directly whether their volume moves around elections, is left to future work.

Finally, the identification separates election-timed coverage from each paper's own coverage away from elections; it does not separate editors leading reader attention from editors following it. Nothing here establishes that readers wanted less slavery coverage than they got, or more.

7 Conclusion

In many places outside the big eastern cities, the party press of the Civil War North was still a working part of party politics. This paper asks whether slavery's place in that press in 1860 was set by the election campaign or by the news. The staggered congressional calendar identifies one answer: slavery coverage was

timed to each state's own election. It fell in the month after each state's vote, by about two-fifths of its normal share on the dictionary measure, or about 1.3 fewer slavery articles in a typical weekly issue, while the rest of the non-campaign agenda did not move. In 1862 and 1864, when the campaigns no longer turned on slavery's extension into the territories, slavery coverage did not rise before the vote, and neither vote shows a significant break. The 1864 breaks point the same way as 1860's but are a third to two-fifths of its size and not significant; without the states that voted for Congress in October and for president a month later they are about a half to three-quarters of its size, still not significant (Section 5.1).

The media-economics literature has shown that newspaper presence raises participation and that slanted outlets can move votes (DellaVigna & Kaplan, 2007, p. 1187; Gentzkow et al., 2011, p. 2980), and that consumer demand explains more of modern slant than ownership does (Gentzkow & Shapiro, 2010, p. 35). This paper looks at the agenda: which issues a party paper carried, and when. Measured against its own normal level, slavery lost more across the vote than the rest of the issue agenda did and less than the campaign matter itself. The timing points to campaign material, though a regional reaction to secession is not ruled out.

Modern newspapers show partisan selection of issues (Larcinese et al., 2011, p. 1178; Puglisi, 2011). In 1860 the design cannot tell the two parties' timing apart, and that comparison rests on few election dates; partisanship shows most clearly in vocabulary. Mullainathan & Shleifer (2005) predict that on politically divisive issues newspapers "segment the market and slant toward extreme positions." Within cities the evidence fits that prediction on framing. Competing papers in one town clearly differed in how often they used the vocabulary of racial status and equality, with Democratic papers using it more, a gap significant within a single cycle only in 1864. Democratic papers also ran somewhat more slavery coverage than their neighbours over the whole period, but that gap is marginal. Nor can the data say whether papers with a rival in town broke harder at the 1860 vote: the pre-specified test is uninformative at this sample size. Read beside Ottinger & Posch (2026), the racial appeal of the 1860s looks different from the one they study in the 1890s South. Their measure pairs race with crime; the divergence here is in the vocabulary of racial status and equality.

Several limits bound these claims. The headline rests on one cycle, and within it mostly on the four states that voted on October 9, 1860; without them the break is a third as large on the article share and not significant, and a regional reaction to secession in those states cannot be ruled out. Estimators built for responses that differ by cohort confirm the article-share break and leave the word-share break suggestive (Section 6.5). Advertisements and notices sit in the denominator of every share. A shift in advertising moves every share alike, so it cannot produce a drop in slavery alone, but it does expose the displacement results in the online appendix. The party comparison rests on too few Democratic election dates for informative

inference. The county association between pre-election slavery coverage and Republican vote share, reported in the online appendix, is positive in presidential contests and absent in the small congressional sample, and it is an association and no more: in modern data, public economic evaluations typically lead newspaper coverage rather than follow it (Hopkins et al., 2017, p. 2), one reason not to read the county association as an effect of coverage. And the mechanism is a pattern rather than an identified cause: flat output is consistent with a publisher who could not expand and with one who judged expansion not worth its cost.

That last limit points to the natural extension. Output was flat in every cycle, so capacity cannot explain why 1860 differed; flat output says only that the campaign displaced something. An exogenous shifter of paper cost, such as wartime newsprint prices combined with each paper's distance from the mills, would turn the capacity constraint from something seen through its consequences into something identified. It would also test whether the cost of paper shaped what a partisan press could afford to say about slavery.

The design identifies that coverage answered to the election. It does not say whether editors supplied campaign content or readers asked for it, and it does not reach the vote itself; those are the next questions. The question usually asked of a partisan press is how slanted it was. The question asked here is when it spoke: whether the issue that divided the country reached Northern readers on the calendar of the campaign or of the news. A staggered electoral calendar and article-segmented digitized corpora now make that question answerable.

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