DO NEWSPAPERS MATTER? EVIDENCE FROM THE CLOSURE OF THE CINCINNATI POST

Sam Schulhofer-Wohl
Miguel Garrido

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## **ABSTRACT**

The Cincinnati Post published its last edition on New Year's Eve 2007, leaving the Cincinnati Enquirer as the only daily newspaper in the market. The next year, fewer candidates ran for municipal office in the suburbs most reliant on the Post, incumbents became more likely to win re-election, and voter turnout and campaign spending fell. We exploit a difference-in-differences strategy and the fact that the Post's closing date was fixed 30 years in advance to rule out some non-causal explanations for these results. We show that local politics changed even though the Enquirer increased its coverage of the Post's former strongholds. Although our findings are statistically imprecise, they demonstrate that newspapers — even underdogs such as the Post, which had a circulation of just 27,000 when it closed — can have a substantial and measurable impact on public life.

Sam Schulhofer-Wohl
Princeton University
363 Wallace Hall
Princeton, NJ 08544
and NBER
sschulho@princeton.edu

Miguel Garrido
Princeton University
001 Fisher Hall
Princeton, NJ 08544
mgarrido@princeton.edu

*"Give light and the people will find their own way."*

– motto of the E.W. Scripps Co., owner of *The Cincinnati Post*

# 1   Introduction

A century ago, 689 cities in the United States had competing daily newspapers; at the start of this year, only about 15 did, and within months, three of those had already become one-paper towns.[1] Many monopoly newspapers are also struggling financially. The decline in competition and in the newspaper industry as a whole has prompted concern that the nation is losing a crucial source of information about public affairs. In the words of one observer, "More of American life will occur in shadows. We won't know what we won't know."[2]

This paper offers a case study of the consequences of closing a newspaper. *The Cincinnati Post* published its last edition Dec. 31, 2007, leaving the *Cincinnati Enquirer* as the only daily newspaper covering Cincinnati and its suburbs in southern Ohio and northern Kentucky. The closing was particularly important in the northern Kentucky suburbs, where the *Post* historically dominated circulation and, as we document, provided more than 80 percent of the combined local news coverage in the two papers. We use a difference-in-differences strategy to show that the *Post*'s closing made municipal politics in the Kentucky suburbs less competitive along several dimensions: Fewer people voted in elections for city council, city commission and school board; fewer candidates sought those seats; the remaining candidates spent less money on their campaigns; and, for councils and commissions, incumbents' chances

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[1]The 1909-1910 figure is from Busterna and Picard (1993). Today's precise count depends on the definition of competing newspapers. Cities with major competing, separately owned dailies include Boston; Charleston, W.Va.; Chicago; Detroit; Fort Wayne, Ind.; Honolulu; Los Angeles; New York; Salt Lake City; Trenton, N.J.; York, Pa.; and Washington. Denver dropped from the list on Feb. 27, when the *Rocky Mountain News* closed; Seattle on March 17, when the *Post-Intelligencer* cut 87 percent of its staff and ceased print publication, converting to a Web site with substantially different content; and Tucson, Ariz., on May 16, when the *Citizen* closed.

[2]Tom Rosenstiel, director of the Pew Research Center's Project for Excellence in Journalism, quoted in Starr (2009).

of retaining office improved. These changes happened even though the *Enquirer* increased its coverage of the *Post*'s former strongholds. Our analysis does not include any communities in Ohio, which has not held regular municipal elections since the *Post* closed. We emphasize that because the Kentucky sample is small, our results are subject to substantial statistical uncertainty. In addition, because the *Post* closed less than two years ago, we can calculate only short-run effects. We are publishing these results now because of intense public interest in the state of the newspaper industry. We plan to separately analyze outcomes in Ohio, and long-run outcomes in Kentucky, after future elections.[3]

Our results shed light on two important public policy concerns. First, our findings suggest that even a small newspaper – the *Post* sold about 27,000 copies daily in 2007, compared with 200,000 for the *Enquirer* – can make local politics more vibrant. Although competing publications or other media such as TV, radio and Web sites may take up some slack when a newspaper closes, none of these appears so far to have fully filled the *Post*'s role in municipal politics in northern Kentucky. Our findings confirm the fears of community leaders such as Boone County Judge-Executive Gary Moore, who said on learning of the *Post*'s impending closure: "I'm very concerned about Northern Kentucky news getting to our constituents. The *Post* has done a wonderful job through the years of being the daily informant of what's going on in the community to our residents" (Duke, 2007). To the extent that our findings apply beyond Cincinnati, they also suggest that local politics will become less competitive after closures of the much larger second newspapers in cities such as Denver (where the *Rocky Mountain News* shut down Feb. 27) and Seattle (where the *Post-Intelligencer* ceased printing March 17) or of monopoly papers in places such as Ann Arbor, Mich. (where *The Ann Arbor News* closed July 23).

Second, the *Post*, an afternoon newspaper whose weekday circulation fell nearly 90 per-

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[3]Adding data from future elections will not increase the precision of the estimates presented here unless we assume long-run and short-run impacts of a newspaper's closing are identical – an assumption that seems implausible.

cent in its last 30 years,[4] survived as long as it did thanks to an implicit government subsidy for newspaper competition. Under the Newspaper Preservation Act (1970), competing newspapers that are in "economic distress" can obtain an exemption from antitrust laws and form a joint operating agreement (JOA) that charges monopoly prices for subscriptions and advertising, as long as the papers retain independent newsrooms. The *Post* and the *Enquirer* formed a JOA in 1977. In passing the act, Congress determined that the value for democracy of preserving independent editorial voices outweighed the potential deadweight losses from monopoly pricing.[5] As then-Rep. Spark Matsunaga, Democrat of Hawaii and a sponsor of the act, put it in House debate: "Let us make no mistake about it, we are here being forced into making a choice between preserving a free press as opposed to keeping the sanctity of the antitrust laws. In a democratic society such as ours the choice is obvious – the free press must be preserved" (Matsunaga, 1970). Despite the explicit congressional rationale for the Newspaper Preservation Act, and even though 27 JOAs have existed over the years, ours is the first analysis we know of to measure the political impact of preserving competition through a JOA.[6]

The JOA between the *Post* and the *Enquirer* is central to our empirical strategy. Figure 1 lays out the timeline of events. Like most JOAs, the Cincinnati agreement specified a terminal date, in this case Dec. 31, 2007. Unusually, though, the *Post* survived exactly until this date chosen 30 years in advance, and no longer. (Of the 21 other JOAs that have ended so far, 17 ended early when the owners decided that publishing two newspapers was unprofitable and closed one paper or merged the two papers together. The other four lapsed

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[4]According to the *Editor & Publisher International Yearbook*, the *Post*'s Ohio and Kentucky editions had total Monday-to-Friday circulation of 246,323 in 1977. The decline was linear with time. The *Enquirer*'s weekday circulation in 1977 was 190,407.

[5]Newspapers in a JOA also combine their printing and delivery operations to exploit economies of scale, but publishers can obtain these savings without an antitrust exemption so long as they continue to compete in advertising and subscription sales.

[6]An existing literature investigates the effect of JOAs on newspaper content and profits (see, e.g., Busterna and Picard, 1993).
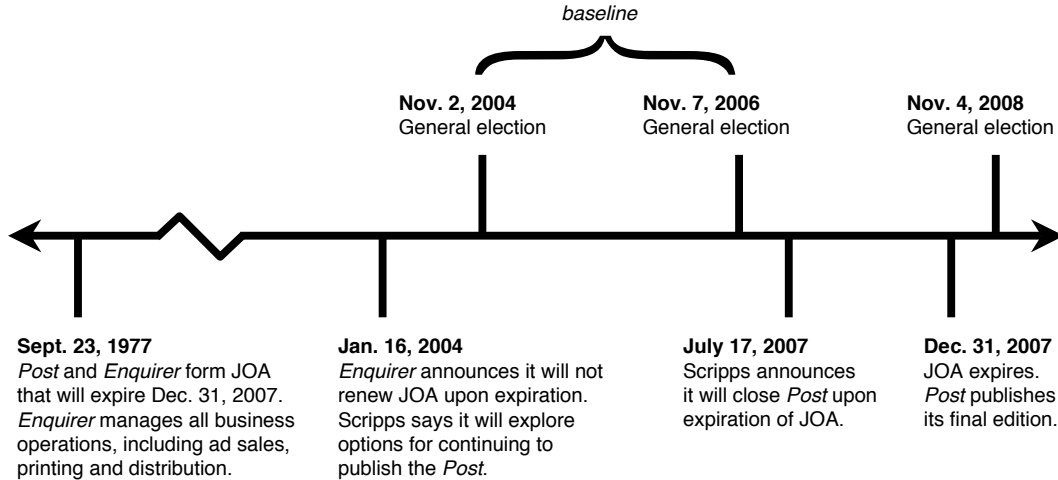
Figure 1: Key events for the empirical strategy.

or were dissolved with both newspapers continuing to publish.) The *Enquirer*'s owner, Gannett Co., announced in January 2004 that it would not renew the agreement at the terminal date, suggesting that Gannett thought publishing two newspapers no longer maximized joint surplus. (If Gannett had merely thought that profits from going it alone would exceed its share of JOA profits, it could have tried to renegotiate the agreement.) But the E.W. Scripps Co., owner of the *Post*, apparently preferred to keep publishing: Instead of agreeing with its partner to close the *Post* before the end of the JOA, as has been typical in other cities,[7] Scripps said it would explore "whatever options it may have to continue publishing newspapers in the Cincinnati market in some form" (E.W. Scripps, 2004). These options proved unattractive because, with the *Enquirer* managing the JOA's business operations, Scripps would have had to buy printing presses and hire advertising and circulation salespeople to keep the *Post* open. Still, Scripps took more than three years to announce that it would close the paper (E.W. Scripps, 2007).

The *Post*'s central role in Scripps' history may have motivated the company's reluctance.

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[7]Full disclosure: The first author worked from 1998 to 1999 at the *Birmingham (Ala.) Post-Herald*, a Scripps newspaper that participated in a JOA and later closed.

Company namesake Edward Willis Scripps made his reputation in the 1880s when he bought the *Post* and built it into what was, at the time, Ohio's largest daily (Baldasty, 1999; Stevens, 1969). The family-controlled media chain's headquarters remain in Cincinnati, and many corporate executives once worked in the *Post* newsroom. But even if Scripps' decision has an explanation other than historical sentiment, it seems unlikely that Scripps chose the *Post*'s exact closing date near the actual time of the closing.[8] Rather, the JOA partners picked a date 30 years in advance, and the *Post* closed on that date because it was the default outcome if the partners never changed the agreement.

Because the *Post*'s closing date was fixed so far in advance, changes in local politics after early 2004 – when Gannett announced it would not renew the JOA – cannot have caused the paper to close exactly when it did. Third factors such as short-run municipality-level economic fluctuations that might affect both local politics and the paper's viability are also less likely to be responsible for the timing of the closing, though Scripps perhaps would have kept the paper open if the towns it covered had experienced a sudden economic boom. Therefore, we can more plausibly attribute changes in political outcomes after the *Post* closed to the paper's closing instead of some other source. To help rule out the possibility that the political changes occurred for other reasons and only randomly coincided with the *Post*'s closure, we employ a difference-in-differences strategy, comparing changes in political outcomes before and after the closure in suburbs that received relatively more or less coverage from the paper. Suburbs that received less coverage serve as controls representing the likely change between 2004 and 2008 if the *Post* had never existed. We also account for the possibility that political outcomes and *Post* coverage both responded to the 2004 announcement on the paper's future by instrumenting for post-2004 coverage with 2003 coverage.

Many previous researchers have also studied newspapers' political impact. Most closely related are studies measuring the effect of newspapers' existence. Trounstine (2009) finds

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[8]The company has not publicly explained the timing of its decision to close the *Post*.

that incumbent advantage in city council elections is lower in cities with their own daily or weekly papers. Gentzkow et al. (2009) analyze historical data from 1869 to 2004 and show that the presence of a local daily newspaper raised presidential election turnout. Oberholzer-Gee and Waldfogel (2005) show that blacks are more likely to vote in areas that have black-targeted weekly papers. George and Waldfogel (2006, 2008) show that competition from *The New York Times* reduces local papers' circulation among highly educated readers and makes those readers less likely to vote. Adserà et al. (2003) show that states and countries with higher newspaper circulation per capita have less corruption. Mondak (1995) finds that voters in Pittsburgh saw themselves as less informed about local elections during a newspaper strike. More broadly, Gentzkow (2006) studies the effect of television on voter turnout, while Erikson (1976), Gerber et al. (2009), Kahn and Kenney (2002), Knight and Chiang (2008), Snyder and Strömberg (2008) and others measure the effect of newspapers' content on voters' and politicians' preferences and behavior, taking newspapers' existence as given.

Our contribution to the literature is twofold. First, nearly all previous studies – like our study – use observational data,[9] but even the most sophisticated observational studies run the risk that correlations between newspapers and political outcomes do not reflect a causal impact of newspapers: Unobserved and uncontrolled-for third factors may influence both newspapers and politics, or causality may run from politics to newspapers' content and availability rather than vice versa. In our case, the *Post*'s predetermined closing date reduces such concerns, though at the cost of limiting the analysis to a small number of municipalities in just one state. Second, the market for news media is changing rapidly with the rise of the Internet. Our data, more recent than those in other papers, show that newspapers' impact

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[9]The main experimental study is Gerber et al., which uses a randomized controlled trial to measure the effect of receiving a newspaper subscription. However, the impact of receiving a newspaper can differ from the impact of the newspaper's existence if readers tell non-readers about stories or if politicians behave differently when a newspaper might write about them.

persists in the present-day market environment.

The paper proceeds as follows. Section 2 summarizes our data, section 3 lays out our empirical strategy and results, and section 4 concludes.

# 2  Data

Our analysis covers all 48 incorporated municipalities in seven Kentucky counties: Boone, Campbell and Kenton, which formed the core of the *Post*'s Kentucky circulation area, as well as Bracken, Gallatin, Grant and Pendleton, which border the core counties.[10] We have data on the number of stories about each municipality in both the *Post* and the *Enquirer* in each year from 2003 to 2007, as well as the number of stories in the *Enquirer* in 2008; the results of every school board, city council and city commission election from 2004 to 2008;[11] candidates' campaign spending in each election; and demographics from the 2000 census.

## 2.1  Newspaper coverage

We obtain the 2003 to 2007 story counts by searching the widely used NewsLibrary database.[12] We include the county name as well as the municipality name in the searches to avoid counting irrelevant stories that would otherwise appear for municipalities with generic names such as Union. To see whether certain kinds of stories disproportionately include the county name, we picked two municipalities at random and performed searches with and without their respective county names. We found no salient differences in the content of stories with and without county names. We also found that the searches with county names returned very few irrelevant stories.

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[10]Our sample excludes the former city of Latonia Lakes, Kenton County, which was dissolved in 2006.

[11]We exclude mayoral elections because only three municipalities in our sample have held mayoral elections since the *Post* closed.

[12]http://www.newslibrary.com

The *Post* published both an Ohio edition and a Kentucky edition, with some stories appearing in both editions. In general, the Kentucky edition provides a more accurate measure of coverage in Kentucky. However, every Kentucky-edition story appears in our searches for the city of Covington because the *Post*'s Kentucky reporters were based there. Therefore, for all municipalities except Covington, we count stories in the Kentucky edition. For Covington, we count stories in the Ohio edition and multiply by that year's average ratio in other municipalities of Kentucky-edition stories to Ohio-edition stories. (This ratio ranges from 6 to 10.)

We use the story counts to construct an index of the *Post*'s importance in covering each municipality: the fraction of stories about that municipality that appeared in the *Post*. This index is a useful measure of the *Post*'s role because, all else equal, communities where the *Post*'s share of coverage was higher lost more coverage when the *Post* closed. (We cannot base our analysis on circulation data because independent dealers delivered the *Post* and the paper had no centralized list of subscribers' addresses. We also cannot use the Audit Bureau of Circulations' zip-code-level data because some towns in our sample share zip codes. Regardless, because broadcasters and bloggers often quote newspaper stories, the number of stories a paper publishes may matter more than the number of subscribers: One subscriber with a well-read blog or popular broadcast can multiply a story's impact many times. Such repetition may help explain how the *Post* could have a meaningful political impact despite its low circulation.) The *Post*'s share of coverage is highly serially correlated: R-squareds in regressions of 2004 through 2007 indexes on the 2003 index range from 0.71 to 0.83. Thus the index measures relatively permanent differences in the *Post*'s importance across municipalities.

We also counted stories in the *Enquirer* in 2008 to investigate whether it changed its product positioning after the *Post*'s exit: If the *Enquirer* added coverage in an effort to capture the *Post*'s former readers, it could have offset the loss of the *Post*'s coverage. We

could not obtain the 2008 story counts from NewsLibrary because the *Enquirer* removed all its stories from NewsLibrary in early 2009 and switched to a competing archive service, ProQuest Archiver.[13] ProQuest indexes *Enquirer* stories from 2006 to the present. Story counts in ProQuest are not directly comparable to those in NewsLibrary because ProQuest includes much material that does not appear in NewsLibrary and does not represent actual coverage by the newspaper, such as death announcements submitted by funeral homes and event listings. We excluded the death announcements from our ProQuest searches but could not find an efficient way to exclude other material. (ProQuest's charges for viewing full articles prohibited us from examining each story to determine whether to exclude it.) When we regress the ProQuest story count for each community in 2006 on the NewsLibrary count for that year, we obtain a statistically insignificant intercept of 14, a highly significant slope of 5.6 and an R-squared of 0.82, suggesting that ProQuest story counts are a multiple of NewsLibrary counts plus random error. We therefore think comparisons of ProQuest counts in 2006 and 2008 are useful for examining changes in the *Enquirer*'s coverage. We prefer not to use the *ProQuest* counts for our main analysis both because irrelevant results inflate the *ProQuest* counts and because *ProQuest* does not index pre-2006 *Enquirer* stories or any *Post* stories.

## 2.2   Political outcomes

News coverage potentially influences election outcomes in many ways. By revealing incumbents' misdeeds or making it easier for challengers to get their message out, a newspaper may reduce incumbent advantage (Trounstine, 2009). Alternatively, if newspaper stories increase incumbents' name recognition (Snyder and Strömberg, 2008), then a newspaper could increase incumbent advantage. Newspaper stories could also raise interest in politics or make citizens feel more connected to their communities, thus inspiring more people to vote

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[13]http://pqasb.pqarchiver.com/enquirer/search.html

or run for office. In addition, the closure of a newspaper could change campaign spending. Candidates may spend less if reduced competition lowers the need to advertise, or spend more if they must use paid ads to substitute for the free newspaper coverage they received previously.[14]

To measure these aspects of political engagement and competition, we obtained election records for 2004 through 2008 from county election supervisors for every municipality in the counties of interest. Kentucky holds all regular municipal and school board general elections in November of even-numbered years, simultaneously with elections for president, Congress and state legislature (but not for state executive offices, which are elected in odd-numbered years). The records include the date of the election; the election type (general or primary, although only two municipalities in our sample hold primaries); the name of the municipal body (for instance, City of Bromley or Covington Independent School District); the title of the elected office (for instance, city council member); the candidates' names and party affiliations (however, virtually all candidates are nonpartisan); the number of votes each candidate received; the identities of the winners; and the number of votes each voter could cast (some elections allow voters to cast multiple votes, corresponding to multiple seats). We also obtained records of each candidate's campaign spending from the Kentucky Registry of Election Finance.

Election records do not identify incumbents, so we determine whether a candidate is an incumbent by checking whether that candidate won the previous election for the same office. We verified a random sample of the results by contacting officeholders and found that our method was accurate. Because we do not have election data before 2004, we can identify incumbents only in 2006 and 2008. When studying incumbent advantage, we therefore

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[14]Many other outcomes are also potentially of interest but would be difficult to study in the context of Kentucky municipal elections. For example, we cannot study party affiliation, because nearly all candidates are nonpartisan. We also cannot study victory margins, because most races involve multiple candidates competing for multiple seats, with voters allowed to cast multiple votes, so that strategic voting is possible and vote totals do not necessarily reflect the strength of voters' preferences.

examine only city council and city commission elections, which are for two-year terms.

We use the election dataset to construct several measures of political engagement and competition. Our first measure is an estimate of voter turnout. Ideally, we would measure turnout in elections with only local races on the ballot, but we cannot do so because regular municipal elections in Kentucky are always simultaneous with national elections. Instead, we estimate the number of people who voted in each municipality's local races in the 2004 and 2008 general elections. This procedure helps us to measure citizens' engagement with local elections because people who vote for national but not for local offices will not affect our estimate of local voters. (People who vote for local offices but would not have come to the polls without a national election can still influence our results, however.) The 2004 and 2008 elections should be more comparable than the 2006 and 2008 elections because 2004 and 2008 were presidential election years, while 2006 was not. In addition, school board elections are for four-year terms, so comparing elections four years apart guarantees that we are comparing races for the same office. Because election records do not show the actual number of voters in a given race, we construct our estimate in two steps. For each local race on the ballot, we estimate the number of voters casting ballots in that race as the larger of the most votes received by any candidate or the ratio of total votes cast to the number of votes allowed per voter in that race. We then estimate the number of voters in all local races by the maximum across races of the number of voters in each race. There is no municipal-level data on voting-age population after 2000 for the small suburbs in our sample, so we use the number of voters as a proxy for turnout rates.

Our second measure is the ratio of candidates for local office to seats up for election. For each municipality and year, we count the people whose names appeared on a primary or general election ballot. We divide this number by the number of seats up for election. As with the number of voters, we construct this variable for 2004 and 2008.

Our third measure is the fraction of seats in a municipality that incumbents win in a

Table 1: Summary statistics: baseline characteristics ($N = 48$ municipalities).

| Variable | mean | s.d. | min | max |
|---|---|---|---|---|
| Demographics (2000 census) | | | | |
|    Voting-age population | 3,959 | 5,728 | 55 | 32,151 |
|    Voting-age percent black | 1.2 | 1.7 | 0.0 | 8.8 |
|    Voting-age percent ages 18-34 | 31.9 | 5.7 | 21.1 | 50.0 |
| *Enquirer* articles (NewsLibrary) | | | | |
|    2003 | 37.1 | 38.7 | 1 | 154 |
|    2004 | 36.2 | 40.9 | 0 | 163 |
|    2006 | 24.4 | 28.2 | 0 | 128 |
| *Post* articles (NewsLibrary) | | | | |
|    2003 | 173.3 | 208.0 | 7 | 1,310 |
|    2004 | 200.2 | 220.3 | 14 | 1,361 |
|    2006 | 160.6 | 198.1 | 8 | 1,270 |
| *Post* share | | | | |
|    2003 | 0.82 | 0.11 | 0.33 | 0.97 |
|    2004 | 0.86 | 0.09 | 0.55 | 1.00 |
|    2006 | 0.87 | 0.11 | 0.47 | 1.00 |

given year. We measure incumbent advantage by the fraction of seats won by incumbents rather than by the difference in probabilities of winning for non-incumbents and incumbents conditional on running because unpopular incumbents might not seek re-election. In that case, incumbents who appeared on the ballot would have a high probability of winning even if voters were, in effect, throwing out many other incumbents.

Our fourth measure is campaign spending per candidate: the total campaign spending recorded in the election finance registry for all school board, council and commission candidates in a municipality in a given year, divided by the number of candidates. We construct this variable for 2004 and 2008. We deflate the data to constant dollars using the Consumer Price Index.

## 2.3 Describing the data

Table 1 summarizes the baseline data on demographics and news coverage. The municipalities range in size from California, voting-age population 55 in 2000, to Covington, voting-age population 32,151. The *Post* dominated coverage of the Kentucky suburbs, publishing 82 to 87 percent of total stories in each year. But the *Post*'s importance varied substantially across municipalities, with the two papers splitting coverage of some places roughly equally and the *Post* publishing 100 percent of stories about other places.

In table 2, the descriptive statistics on incumbent advantage, the ratio of candidates to seats and campaign spending begin to tell our story about the *Post*'s impact: On average, incumbents were more likely to win and the ratio of candidates to seats was lower after the *Post* closed than before. Candidates also spent less money. Our task in the next section is to demonstrate that other factors that may have changed around the time of the *Post*'s closure did not cause the differences in incumbent advantage, ratio of candidates to seats, and campaign spending. The data on voters go the other way: More people voted after the *Post* closed than before. However, the 2008 presidential election had unusually high turnout. We show below that, controlling for the overall change in turnout, the number of voters fell in municipalities where the *Post* had dominated coverage. The data on news coverage show that the *Enquirer* published fewer total stories about the Kentucky municipalities after the *Post* closed than before. We find below, though, that the *Enquirer* wrote more in 2008 about towns formerly dominated by the *Post*.

# 3 Empirical strategy and results

Our basic model for the effect of *Post* coverage on an outcome $y_{it}$ in municipality $i$ in year $t$ is:

$$y_{it} = \alpha_i + \tilde{\theta}_{0,t} + \mathbf{x}'_i \tilde{\boldsymbol{\theta}}_{2,t} + \theta_1 postshare_{it} + \epsilon_{it}, \tag{1}$$

Table 2: Summary statistics: outcomes ($N = 48$ municipalities).

| Variable | mean | s.d. | min | max |
|---|---|---|---|---|
| *City council, city commission races* | | | | |
| Seats up for election | | | | |
| 2006 | 5.54 | 1.47 | 3 | 12 |
| 2008 | 5.46 | 1.52 | 2 | 12 |
| Seats won by incumbent | | | | |
| 2006 | 3.50 | 1.44 | 0 | 8 |
| 2008 | 3.98 | 1.41 | 2 | 8 |
| Fraction of seats won by incumbent | | | | |
| 2006 | 0.63 | 0.21 | 0.00 | 1.00 |
| 2008 | 0.73 | 0.18 | 0.33 | 1.00 |
| change | 0.10 | 0.25 | -0.33 | 1.00 |
| *School board, city council, city commission races* | | | | |
| Candidates | | | | |
| 2004 | 8.67 | 4.06 | 1 | 18 |
| 2008 | 8.44 | 3.86 | 2 | 19 |
| Seats up for election | | | | |
| 2004 | 6.02 | 2.24 | 1 | 14 |
| 2008 | 6.06 | 2.04 | 2 | 14 |
| Ratio of candidates to seats | | | | |
| 2004 | 1.41 | 0.39 | 1 | 2.83 |
| 2008 | 1.36 | 0.38 | 1 | 2.71 |
| change | -0.05 | 0.38 | -0.83 | 1 |
| Estimated number of voters | | | | |
| 2004 | 1,610 | 1,995 | 20 | 9,273 |
| 2008 | 1,700 | 2,106 | 29 | 9,203 |
| log change | 0.09 | 0.34 | -0.32 | 1.98 |
| Total campaign spending (in 2004 dollars) | | | | |
| 2004 | 5,062 | 26,324 | 0 | 180,919 |
| 2008 | 3,738 | 16,364 | 0 | 109,176 |
| change | -1325 | 10,847 | -71,743 | 11,715 |
| Spending per candidate (in 2004 dollars) | | | | |
| 2004 | 303 | 1475 | 0 | 10,051 |
| 2008 | 241 | 900 | 0 | 5,746 |
| change | -62 | 689 | -4,305 | 1,464 |
| *News coverage* | | | | |
| *Enquirer* articles (ProQuest) | | | | |
| 2006 | 151.3 | 175.0 | 3 | 826 |
| 2008 | 148.5 | 158.9 | 0 | 758 |
| change | -2.8 | 41.5 | -96 | 77 |
| % change | 20.8 | 67.4 | -100 | 300 |

14

where *postshare* is the *Post*'s share of all stories about the municipality, $\mathbf{x}_i$ represents demographic characteristics of the municipality and $\epsilon_{it}$ is all factors other than *postshare* that affect the outcome. The coefficient $\theta_1$ represents the effect of an increase in the *Post*'s coverage share on the outcome. If we interpret *postshare* as a proxy for the importance of the *Post* to a particular community, a positive value of $\theta_1$ implies that outcome $y$ was higher in communities where the *Post* played a larger role. Closing the *Post* sends *postshare* to zero, so $\theta_1$ is also the effect of closing the *Post* in a community that got all its coverage from the *Post*. We use the *Post*'s share instead of the total number of stories in the *Post* because larger municipalities may tend to have more stories in both newspapers. Taking the ratio of stories in the two papers controls for the overall level of coverage while limiting the number of regressors, which is desirable because we have only 48 observations.

The municipality fixed effects $\alpha_i$ in our model account for time-invariant unobservable differences across municipalities, such as permanent differences in voters' interest in politics. The year fixed effects $\tilde{\theta}_{0,t}$ account for unobservable differences across years that affect all municipalities equally, such as any national political events that have the same influence on all voters. We must also control for differences in time trends that affect different municipalities in different ways. For example, some municipalities might have demographic characteristics that led to larger changes in turnout between 2004 and 2008, and we would not want to confound these differences with the effect of the *Post*'s closure. The interaction of year effects with demographics $\mathbf{x}_i'\tilde{\boldsymbol{\theta}}_{2,t}$ accounts for observable differences in trends across municipalities. Related studies such as Oberholzer-Gee and Waldfogel (2005) and George and Waldfogel (2008) also include jurisdiction and year effects and interactions of year effects with demographics. With only two years of data, we cannot control for unobservable differences in trends using methods such as those in Gentzkow et al. (2009). We discuss below how unobservable differences in trends might influence our results.

We can estimate equation (1) by taking first differences. If we subtract equation (1)

for 2004 from the same equation for 2008, and observe that $postshare_{i,2008}$ is zero for every municipality, then we obtain

$$y_{i,2008} - y_{i,2004} = \theta_0 - \theta_1 postshare_{i,2004} + \mathbf{x}_i' \boldsymbol{\theta}_2 + (\epsilon_{i,2008} - \epsilon_{i,2004}), \tag{2}$$

where we have defined $\theta_0 = \tilde{\theta}_{0,2008} - \tilde{\theta}_{0,2004}$ and $\boldsymbol{\theta}_2 = \tilde{\boldsymbol{\theta}}_{2,2008} - \tilde{\boldsymbol{\theta}}_{2,2004}$. Equation (2) represents a difference-in-differences strategy: It compares changes in outcomes across years in municipalities with different levels of *Post* coverage.

The error term in (2) is $(\epsilon_{i,2008} - \epsilon_{i,2004})$. Hence ordinary least squares estimates of $\theta_1$ based on (2) will be biased if changes in other factors $(\epsilon_{i,2008} - \epsilon_{i,2004})$ are correlated with the initial level of *Post* coverage. The fact that the *Post*'s closing date was set 30 years in advance helps rule out many sources of correlation. For example, if Scripps had chosen the closing date based on economic trends around 2007 in the communities where the *Post* was strong, and if local economic trends were correlated with local political trends, then the error term would be correlated with the *Post*'s share in 2004. Because Scripps appears not to have set the closing date based on contemporaneous economic trends, we think this type of correlation is unlikely to be a problem.

Other sources of correlation may remain, however. Although differencing removes any differences between 2004 and 2008 that affected all municipalities equally, it cannot remove differences between the two years that affected some municipalities more than others. Our leading concern is that Barack Obama's historic presidential candidacy in 2008 may have increased turnout among young or black voters. Kentucky and national exit polls show that blacks made up a larger fraction of voters in 2008 than in 2004. National polls also show a slight increase in turnout among the young, though Kentucky polls show no such difference. (See National Election Pool, 2004 and 2008. The polls show no other significant differences between the 2004 and 2008 electorates in Kentucky.) Exit polls do not break

16

down data by municipality, but if the statewide and national differences carried through to the places we study, and if *Post* coverage varied with the age structure or racial composition of a community,[15] then *postshare* could be correlated with the error term in (2) even if *Post* coverage had no causal effect on turnout. We account for this possible correlation by including as regressors $\mathbf{x}_i$ in (2) the fraction of voting-age people who are black and the fraction who are ages 18 to 34 in the 2000 census.[16] While Obama's candidacy is an important difference between 2004 and 2008, other differences between the years may remain and may have interacted with demographics we do not observe. The possibility of such interactions is an important caveat to all difference-in-difference studies, including ours.

Another concern is that any unobserved factor $\epsilon_{2004}$ that affected politics in 2004 could also have prompted the newspapers to change their coverage, again producing a correlation between the error term and the *Post*'s coverage share in 2004. If political behavior and *Post* or *Enquirer* content both changed after 2004 in anticipation of the JOA's end, we could also find a spurious correlation. To guard against these possibilities, in some specifications we instrument for the *Post*'s 2004 share with its 2003 share, which will be uncorrelated with $\epsilon_{2004}$ if the errors are serially uncorrelated. Instrumenting for the *Post*'s share can also reduce attenuation bias in the estimate of $\theta_1$ if the *Post*'s share in any given year is a noisy measurement of the paper's true long-run importance in a community. The measurement error in $postshare_{it}$ appears to be classical: $\text{corr}(postshare_{it}, postshare_{is})$ does not depend on $t - s$ for $t \neq s$, consistent with a model where $postshare_{it} = trueshare_i + u_{it}$ with i.i.d. $u_{it}$.

Finally, ordinary least squares and instrumental variables models may be misspecified because each of our dependent variables has only a limited range. For example, no matter how low the local level of interest in politics, someone always runs for office, so the ratio of

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[15]We find marginally statistically significant evidence that the *Post*'s share was higher in communities where a larger share of the voting-age population is black or ages 18 to 34.

[16]We measure the fraction who are ages 18 to 34 as of 2000, rather than the fraction who will reach ages 18 to 34 by 2008, because the number of teenagers in 2000 will be a poor predictor of the number of young adults in 2008 if different communities are particularly attractive to people of different ages.
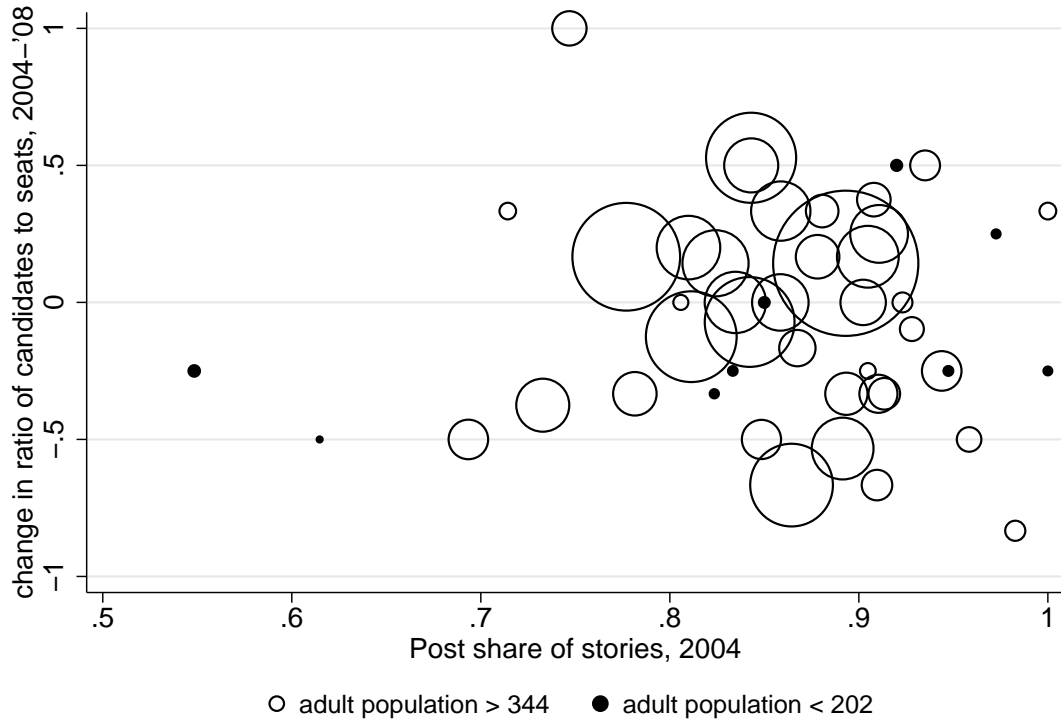
Figure 2: *Post* story share and changes in the number of candidates. Each observation is a municipality. The area of the circle is proportional to the municipality's voting-age population in 2000.

candidates to seats is left-censored at one; we account for the censoring in some specifications by estimating (2) with the identically censored least squares (ICLS) panel data estimator of Honoré (1992). Campaign spending is likewise left-censored at zero. Related, no matter how big or small incumbents' advantage is, the probability of an incumbent victory cannot exceed one or fall below zero, so we employ the two-sided identically censored least squares (ICLS2) estimator of Alan et al. (2008) in some specifications. Last, the number of voters must be a positive integer; because the zero bound on the number of voters never binds, OLS is unbiased, but a count model would be more efficient, and we estimate a Poisson conditional fixed effects model in some specifications. Unfortunately, the limited-dependent-variable models do not let us use an instrument for *Post* coverage.

Figure 2 illustrates the regression in (2), for the outcome of ratio of candidates to seats.

The general pattern is that municipalities with a higher share of *Post* stories experienced a larger drop in the competitiveness of elections, suggesting that the *Post*'s closure reduced competitiveness. However, a few municipalities with voting-age populations of about 200 or fewer are outliers, and if one counts all municipalities equally, these outliers could suggest the opposite pattern – no change or a small increase in competitiveness after the *Post* closed. Similar patterns appear when we graph the other outcomes. We conjecture that voters in the smallest communities can easily learn about candidates, who are also their neighbors, without the aid of newspaper reporters; thus newspapers likely matter more outside the smallest communities. Policymakers may also be more concerned about a newspaper's impact in places where more people live. We therefore prefer to weight municipalities by voting-age population in our estimates. Because some readers may disagree, though, we also report unweighted estimates as well as estimates that exclude the smallest municipalities.

Table 3 presents the results.[17] On all four measures of political engagement and competition, we find indications that the *Post*'s closure made elections less competitive:

**Incumbent advantage:** In the weighted estimates using all methods, municipalities where the *Post* was more important experienced a greater increase in incumbent advantage after the *Post* closed. However, if we do not weight by voting-age population, the relationship has a smaller magnitude and the opposite sign. The estimated effect is highly statistically significant in the weighted IV specifications. The point estimate in these specifications is implausibly large – changing the *Post*'s share from one to zero would raise incumbent advantage by more than 100 percentage points. (Recall that the sign of $\theta_1$ is the opposite of the sign of the regression coefficient.) Notice, though, that the range of *postshare* is substantially smaller than zero to one; our estimates will not reflect nonlinearities in the effect of *postshare* outside the observed

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[17]To be conservative, the table reports the larger of the heteroskedasticity-robust standard error or the non-robust standard error for each coefficient. Given the small sample size, we do not attempt to account for spatial correlation.

Table 3: Effect of the *Post* on political engagement and competition.

|  | UNWEIGHTED | | | | | | WEIGHTED | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **A. Change in probability that winner is incumbent** | | | | | | | | | | | | |
| *Post* share | -0.52 | -0.53 | -0.30 | -0.30 | -0.68 | -0.71 | 0.82 | 0.64 | 1.90 | 1.65 | 0.91 | 0.75 |
| of stories | (0.37) | (0.38) | (0.37) | (0.38) | (0.58) | (0.57) | (0.62) | (0.63) | (0.77) | (0.76) | (0.69) | (0.77) |
| R-squared | 0.05 | 0.07 | 0.04 | 0.06 | - | - | 0.05 | 0.09 | - | 0.02 | - | - |
| method | OLS | OLS | IV | IV | ICLS2 | ICLS2 | OLS | OLS | IV | IV | ICLS2 | ICLS2 |
| **B. Change in ln(voters in general election)** | | | | | | | | | | | | |
| *Post* share | -0.31 | -0.28 | -0.14 | -0.07 | -0.45 | -0.34 | -0.36 | -0.20 | -0.51 | -0.31 | -0.78 | -0.31 |
| of stories | (0.55) | (0.56) | (0.65) | (0.64) | (0.44) | (0.43) | (0.35) | (0.36) | (0.43) | (0.42) | (0.22) | (0.23) |
| R-squared | 0.01 | 0.02 | 0.00 | 0.02 | - | - | 0.02 | 0.07 | 0.02 | 0.07 | - | - |
| method | OLS | OLS | IV | IV | Poiss. | Poiss. | OLS | OLS | IV | IV | Poiss. | Poiss. |
| **C. Change in ratio of candidates to seats** | | | | | | | | | | | | |
| *Post* share | 0.07 | 0.07 | 0.29 | 0.22 | 0.08 | 0.06 | -0.24 | -0.69 | -1.02 | -1.23 | -0.32 | -1.39 |
| of stories | (0.65) | (0.67) | (0.72) | (0.72) | (0.75) | (0.73) | (0.96) | (1.08) | (1.36) | (1.12) | (1.31) | (1.46) |
| R-squared | 0.00 | 0.00 | 0.00 | 0.00 | - | - | 0.00 | 0.11 | 0.02 | 0.10 | - | - |
| method | OLS | OLS | IV | IV | ICLS | ICLS | OLS | OLS | IV | IV | ICLS | ICLS |
| **D. Change in campaign spending per candidate ($)** | | | | | | | | | | | | |
| *Post* share | -713 | -525 | -1362 | -478 | -29987 | -14766 | -10225 | -3670 | -19239 | -4906 | -36127 | -16670 |
| of stories | (1106) | (928) | (1312) | (1063) | (10536) | (11626) | (8471) | (3354) | (13476) | (3054) | (3797) | (5777) |
| R-squared | 0.01 | 0.34 | 0.00 | 0.34 | - | - | 0.11 | 0.74 | 0.02 | 0.74 | - | - |
| method | OLS | OLS | IV | IV | ICLS | ICLS | OLS | OLS | IV | IV | ICLS | ICLS |
| N | 48 | 48 | 48 | 48 | 48 | 48 | 48 | 48 | 48 | 48 | 48 | 48 |
| controls | no | yes | no | yes | no | yes | no | yes | no | yes | no | yes |

Standard errors (larger of heteroskedasticity-robust or non-robust) in parentheses. Panel A compares 2008 with 2006; panels B, C and D compare 2008 with 2004. *Post* share of stories is measured in the base year of the comparison, with the 2003 share used as an instrument for IV estimates. Weights are population age 18 and older in 2000 census. Controls are percentage of the population age 18 and older who are black and who are ages 18 to 34.

20

range. According to the weighted IV estimates, a one-standard-deviation increase in the *Post*'s share reduces incumbent advantage by 16 to 19 percentage points, still large but not impossible.

**Number of voters:** In all specifications, our point estimates show that relatively fewer people went to the polls after the *Post* closed in places where the *Post* was more important. A one-standard-deviation increase in the *Post*'s share is predicted to draw 1 to 8 percent more voters to the polls. The results are highly statistically significant in one of the weighted Poisson specifications but not statistically significant otherwise. Because our dependent variable is the natural logarithm of the number of voters, we can interpret our results as describing the effect on turnout rates: Changes in voting-age population, which is unobserved but is the denominator of the turnout rate, will enter the error term of (2), and our estimates will be unbiased if the population growth rate is uncorrelated with the *Post*'s coverage share.[18]

**Number of candidates:** The weighted estimates show that relatively fewer people ran for office after the *Post* closed in places where the *Post* was more important. According to the IV estimates, a one-standard-deviation increase in the *Post*'s share raised the ratio of candidates to seats by about 0.1. A few of the coefficients are marginally statistically significant against a one-sided alternative. As with incumbent advantage, the relationship has the opposite sign and smaller magnitude in the unweighted estimates.

**Campaign spending:** In all specifications, our estimates show that candidates spent relatively less money on their campaigns after the *Post* closed in places where the newspaper was more important. The result suggests that the lower need for spending in less-competitive elections outweighed the potential need to buy campaign ads to re-

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[18]We cannot check this assumption about population growth because the Census Bureau has no population counts after 2000 for communities as small as those we study.

place newspaper coverage. According to the weighted IV estimates with controls, a one-standard-deviation increase in the *Post*'s share raised campaign spending by about $500 per candidate. The estimated effect is surprisingly large relative to average spending, but because average spending includes many candidates who spent no money, the estimated effect is more plausible compared with the average spending of candidates who reported positive spending. The estimates generally are not statistically significant. In regressions not reported here, we also found a consistently negative effect of the *Post*'s closure on spending when the dependent variable was total spending or the natural logarithm of spending.

Controlling for race and age structure proves not to affect the qualitative results, though the magnitudes of the coefficients change in some cases. In regressions not reported here, we obtained similar results when we controlled only for race and when we controlled for adult population in addition to race and age structure. The small sample size makes the dangers of specification searching particularly high, so we deliberately did not experiment with other controls.

Table 4 investigates the effect of weights on our results by recalculating the estimates after excluding the nine municipalities with voting-age populations of 201 or fewer. (The next-smallest municipality has 345 voting-age residents.) The weighted and unweighted versions of each specification almost always have the same sign in the restricted sample, confirming that the weights' main effect is to reduce the influence of a few small suburbs. This finding supports our conjecture that newspapers simply have a different impact in very small communities: Where the candidates and voters are neighbors, voters can get the news without a newspaper.

In table 5, we use equation (2) to study how the *Post*'s closure affected the *Enquirer*'s coverage. Our dependent variable is the percent change in *Enquirer* stories about a munici-

Table 4: Results on political outcomes excluding nine smallest municipalities.

| | UNWEIGHTED | | | | | | WEIGHTED | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **A. Change in probability that winner is incumbent** | | | | | | | | | | | | |
| Post share of stories | -0.44 | -0.42 | 0.32 | 0.35 | -0.48 | -0.46 | 0.94 | 0.77 | 2.20 | 1.98 | 1.01 | 0.86 |
| | (0.85) | (0.85) | (0.71) | (0.73) | (0.84) | (0.86) | (0.67) | (0.69) | (0.83) | (0.86) | (0.71) | (0.85) |
| R-squared | 0.02 | 0.04 | - | - | - | - | 0.06 | 0.10 | - | 0.01 | - | - |
| method | OLS | OLS | IV | IV | ICLS2 | ICLS2 | OLS | OLS | IV | IV | ICLS2 | ICLS2 |
| **B. Change in ln(voters in general election)** | | | | | | | | | | | | |
| Post share of stories | -0.85 | -0.86 | -1.03 | -0.95 | -0.49 | -0.38 | -0.40 | -0.24 | -0.58 | -0.39 | -0.78 | -0.31 |
| | (0.82) | (0.86) | (0.94) | (0.94) | (0.46) | (0.46) | (0.38) | (0.40) | (0.47) | (0.47) | (0.22) | (0.24) |
| R-squared | 0.03 | 0.05 | 0.03 | 0.05 | - | - | 0.03 | 0.08 | 0.02 | 0.07 | - | - |
| method | OLS | OLS | IV | IV | Poiss. | Poiss. | OLS | OLS | IV | IV | Poiss. | Poiss. |
| **C. Change in ratio of candidates to seats** | | | | | | | | | | | | |
| Post share of stories | -0.98 | -1.02 | -1.54 | -1.53 | -1.37 | -1.61 | -0.30 | -0.79 | -1.23 | -1.50 | -0.39 | -1.48 |
| | (1.17) | (1.24) | (1.54) | (1.52) | (1.50) | (1.68) | (1.04) | (1.16) | (1.51) | (1.26) | (1.44) | (1.59) |
| R-squared | 0.03 | 0.03 | 0.02 | 0.03 | - | - | 0.00 | 0.11 | 0.02 | 0.10 | - | - |
| method | OLS | OLS | IV | IV | ICLS | ICLS | OLS | OLS | IV | IV | ICLS | ICLS |
| **D. Change in campaign spending per candidate ($)** | | | | | | | | | | | | |
| Post share of stories | -1393 | -1456 | -3259 | -2370 | -29987 | -14766 | -10759 | -3910 | -20732 | -5494 | -36127 | -16670 |
| | (1769) | (1467) | (2362) | (1723) | (10536) | (11626) | (8930) | (3600) | (14395) | (3428) | (4198) | (6387) |
| R-squared | 0.02 | 0.37 | - | 0.37 | - | - | 0.12 | 0.74 | 0.02 | 0.74 | - | - |
| method | OLS | OLS | IV | IV | ICLS | ICLS | OLS | OLS | IV | IV | ICLS | ICLS |
| N | 39 | 39 | 39 | 39 | 39 | 39 | 39 | 39 | 39 | 39 | 39 | 39 |
| controls | no | yes | no | yes | no | yes | no | yes | no | yes | no | yes |

Standard errors (larger of heteroskedasticity-robust or non-robust) in parentheses. Panel A compares 2008 with 2006; panels B, C and D compare 2008 with 2004. *Post share of stories* is measured in the base year of the comparison, with the 2003 share used as an instrument for IV estimates. Weights are population age 18 and older in IV estimates. Controls are percentage of the population age 18 and older who are black and who are ages 18 to 34.

23

Table 5: Effect of the *Post*'s closure on *Enquirer* coverage.

| | UNWEIGHTED | | | | WEIGHTED | | | |
|---|---|---|---|---|---|---|---|---|
| | *Percent change in Enquirer stories, 2006-2008* | | | | | | | |
| *Post* share | 249 | 236 | 251 | 234 | 204 | 243 | 171 | 240 |
| of stories | (97) | (103) | (100) | (103) | (92) | (93) | (117) | (110) |
| R-squared | 0.16 | 0.19 | 0.16 | 0.19 | 0.13 | 0.18 | 0.13 | 0.18 |
| method | OLS | OLS | IV | IV | OLS | OLS | IV | IV |
| N | 48 | 48 | 48 | 48 | 48 | 48 | 48 | 48 |
| controls | no | yes | no | yes | no | yes | no | yes |

Standard errors (larger of heteroskedasticity-robust or non-robust) in parentheses. *Post* share of stories is measured in 2006, with the 2003 share used as an instrument for IV estimates. Weights are population age 18 and older in 2000 census. Controls are percentage of the population age 18 and older who are black and who are ages 18 to 34.

pality from 2006 to 2008.[19] We take 2006 rather than 2007 as the baseline year for measuring *Enquirer* coverage because the *Enquirer* could have already begun to change its coverage in 2007 in anticipation of the *Post*'s exit. We would have preferred an earlier baseline but, as discussed in section 2, the ProQuest data on the *Enquirer* begin in 2006.

The estimates show that *Enquirer* coverage rose relatively more in municipalities where the *Post* initially provided a larger share of coverage. The finding survives when we instrument for the *Post*'s share, indicating that the result is not merely a consequence of reversion to the mean. Quantitatively, a one-standard-deviation increase in the *Post*'s initial share led to an increase in *Enquirer* stories of 17 percent to 25 percent after the *Post* closed. All of the coefficients are statistically significant at least at the 10 percent level, and most at the 5 percent level or better. The table reports estimates using the full sample. Estimates excluding the nine smallest municipalities and using the absolute rather than percent change in stories are similar.

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[19]We calculate the percent change rather than taking the logarithm of stories in each year because one municipality had zero *Enquirer* stories in 2008.

The *Enquirer*'s change in product positioning makes our results on political outcomes especially striking: Local politics changed when the *Post* closed even though its erstwhile competitor tried to replace the *Post*'s coverage. In addition, if the *Enquirer*'s added stories about towns with a high *Post* share came at the expense of stories about towns with a low *Post* share, then our estimates of the *Post*'s political impact are lower bounds on the true impact: If the *Post*'s closure caused the *Enquirer* to reduce coverage of the "control" municipalities where the *Post* was initially unimportant, then the differences between municipalities with high and low *Post* shares would be attenuated.[20] Of course, the quantity of stories is an imprecise measure of how much information a newspaper provides. The quality of the *Enquirer*'s stories – for example, their accuracy, timeliness and readability – may differ from the quality of the *Post*'s stories, either in all years or simply in 2008, if the *Post*'s exit changed the *Enquirer*'s incentives for producing high-quality stories. The story count data do not measure such effects, if any. The story counts also do not measure any changes in coverage from other media.

# 4    Conclusion

The logo of the E.W. Scripps Co., printed on the front page of all its newspapers, is a lighthouse. This paper describes what happened when one of Scripps' lights went out. *The Cincinnati Post* was a relatively small newspaper, with circulation of only 27,000 when it closed. Nonetheless, its absence appears to have made local elections less competitive along several dimensions: incumbent advantage, voter turnout, campaign spending and the number of candidates for office. We caution that although our preferred point estimates tell a compelling story, the results are statistically imprecise and sometimes sensitive to the

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[20]Since the *Enquirer*'s total stories about the Kentucky municipalities fell slightly from 2006 to 2008, the increased coverage of towns with a high *Post* share tells us that coverage fell in towns with a low *Post* share. However, since we do not know what would have happened to *Enquirer* coverage if the *Post* had not closed, this finding does not prove that the *Post*'s closure caused the *Enquirer* to reduce coverage of those towns.

treatment of very small municipalities. Further, our results cover only the Kentucky suburbs, because Ohio has not held regular municipal elections since the *Post* closed, and represent only the short-run consequences of the paper's closing. Future research could investigate whether political engagement and competition return to their pre-closure level in the long run.

Several other well-known newspapers have closed since the *Post* – the largest being Scripps' *Rocky Mountain News*, circulation 210,000, just this February – and more are in danger. Observers are energetically debating whether these closings matter: Do newspapers play a valuable, irreplaceable role in American democracy, or can new media fill the gap left when a paper closes? Starr (2009) argues that the newspaper industry's decline "raises practical questions for anyone concerned about the future of American democracy." On the other hand, after the *Rocky* closed, U.S. Rep. Jared Polis, Democrat of Colorado, said the paper's demise was "mostly for the better" (Crummy, 2009). Whether our results support Starr's view or Polis' depends on how one values competitive elections. But if voter turnout, a broad choice of candidates and accountability for incumbents are important to democracy, we side with those who lament newspapers' decline.

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