Friends at *WSJ*: Journalist Connection, News Tone, and Stock Returns

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Abstract

This paper studies the effect of the firm–journalist network on news tone and stock returns. Using a unique dataset on the firm's and the CEO's connections to the *Wall Street Journal* (*WSJ*) reporters, I find that such connections lead to markedly more favorable coverage of corporate M&A news and to better associated market reactions. The effect on the financial market is larger for the deals featured on the front page of the Journal. For identification, I instrument the connected coverage with the reporters' turnover and find similar results. Furthermore, using Rupert Murdoch's acquisition of the *WSJ* as an exogenous shock to journalistic independence, I show that firms previously connected to Mr. Murdoch received better coverage and more positive stock returns after the ownership change.

Keywords: News sentiment, Stock returns, Mergers and acquisitions, Financial journalism

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"I spend more time talking to journalists than perhaps any CEO in the country."

– Warren Buffett

Chairman and CEO of Berkshire Hathaway

1 Introduction

A number of recent studies reveal that the media have a causal influence on the financial market.[1] Key to this relation is an author's persuasion, views, or bias injected into the news article (Dougal et al., 2012). However, what fosters a journalist's view? Wisdom from Warren Buffett suggests that such views can at least be partially nurtured by firms themselves, as featured in the opening quote. In this paper, I study whether relationships between a firm and a journalist represent a mechanism for influencing the tone, or *"slant,"* of financial news and subsequently transmitting that sentiment into asset prices.

The expected effect of the journalists' connections is not obvious ex-ante. On the one hand, if financial media compete using accuracy for rational investors, a well-connected journalist could yield more accurate reporting by catalyzing the information flow. On the other hand, a business reporter beholden to a firm might inject his personal opinion and slant the story in favor of the firm, either intentionally or unconsciously, resulting in more biased coverage.

To assess the effect of the journalists' connections, I assemble a set of financial news articles published in the *Wall Street Journal* (*WSJ*) from 1997 through 2016. The choice of *WSJ* is natural because the *WSJ* is by far the largest print newspaper in the United States.[2] In addition, the *WSJ* is extremely well-established among finance and investment professionals. Given its strong reputation, the *WSJ* is less likely to slant toward a particular bias because finance professionals should prioritize information accuracy over individual beliefs. I focus on the most significant event, namely, the *WSJ* coverage of corporate takeovers because M&A transactions elicit substantial investor scrutiny and attention, but the assessment of the deal synergies is subject to an individual perspective.

[1] See, for example, Huberman and Regev (2001), Engelberg and Parsons (2011), Dougal et al. (2012), and Peress (2014).

[2] The average weekday circulation of the *WSJ* is above 2 million according to Alliance for Audited Media (AAM) as of March 31, 2013.

Next, I obtain from Bloomberg and major professional network websites the college names of the acquirers' CEOs and of the authors of the *WSJ* articles. This allows me to observe, for instance, whether John Riccitiello, the Chief Executive of Electronic Arts, attended the same college as the one attended by the *WSJ* reporter, Nick Wingfield, who wrote about the Electronic Arts' acquisition of Take-Two in February 2008. Additionally, using the *WSJ* search engines and Factiva, I also capture the potential working relationships between a firm and a journalist by examining if a specific reporter wrote at least two non-M&A related stories about that firm during the 12 months prior to an acquisition. This approach follows Solomon (2012), which reflects the idea that if a journalist frequently covers a firm, he is more likely to build a personal relationship with that firm's employees. Now the key question: do such relationships matter in terms of news tonality and investor reactions?

My main findings are twofold. First, I obtain robust evidence of a negative relation between a firm–journalist connection and negative sentiment in the news report. For example, all else equal, journalists who have previous working experiences with the acquirer use 24% fewer negative words, defined by Loughran and McDonald,[3] relative to an average article ($t = 4.03$ for the difference). Similarly, articles authored by a journalist who attended the same college as the acquirer's CEO contain markedly 13% fewer negative words.

Importantly, these results are not driven by the general writing styles of the journalists because the same effect and its associated magnitude remain in models in which I control for journalist fixed effects (Dougal et al., 2012). For example, when a bid is covered by a journalist who attended the same college as the one attended by the acquirer's CEO, the fraction of negative words in that article is found on average 23% lower than other M&A articles written by the *same* author ($t = 2.14$ for the difference). Moreover, when I alternatively evaluate news negativity using a machine learning classifier, or the *absolute* value of negative word count, the conclusion remains qualitatively unchanged.

Second, I find that this favorable coverage by connected reporters alters market reaction to the acquisition. All else being equal, stock returns to the acquirers whose deal is covered by a journalist with retrospective working relationships are 2% higher. More interestingly, the

[3] See Loughran and McDonald (2011, 2016, 2017), Bodnaruk et al. (2015), and the dedicated website: `http://sraf.nd.edu/`.

effects of the journalists' connections — either through working relationships or educational networks — are concentrated on the M&As featured on the front page of the *WSJ*, suggesting a true media-positioning effect. This evidence echoes recent work by Fedyk (2018), who shows that front-page positioning of news triggers the strongest market reactions.

Are these relations causal? If journalists are randomly assigned to an event, so that for the same corporate takeover, the authors who write about that story only vary in terms of their connections to the acquirer, then we could confidently claim a causal relation. This requirement, however, goes beyond the ability of researchers to observe the internal reporter rotation. Lacking random assignment, the documented effects are vulnerable to a number of unobservable factors that affect simultaneously the journalists' connections and the news tone or stock returns. For instance, the journalists' connections might simply characterize the firm's general connectedness, which might also facilitate better deal making.[4]

To provide a causal interpretation to my analysis, I adopt an instrument variable (IV) method. Specifically, I instrument the propensity of being covered by a connected journalist with the turnover rate of *all* connected journalists. The basic idea is as follows: Returning to the example, suppose Electronic Arts has access to 5 connected reporters. When Electronic Arts announced its bid in February 2008, one of Electronic Arts' connections left the *WSJ* in that month (turnover rate=0.2); therefore, the likelihood of this bid being covered by a connected reporter is negatively predicted by the turnover of the Electronic Arts' connections. Importantly, the turnover of journalists should be orthogonal to the quality of the M&A deal, which I assume to be true.[5] With the IV approach, I find that the connection effects on news tone almost double and that the effects on the stock returns almost quadruple. Because journalists' turnovers are exogenous to the deal synergy and its associated market reaction, this specification provides perhaps the strongest evidence for a causal relation.

While it might be clear that independence in journalism is affected by a reporter's personal relationships, such independence might also be shaped by factors related to the media itself. These factors may include, for example, the editor's view or even the preference of the media

[4] Prior studies show that CEO network matters for M&A transactions. However, El-Khatib et al. (2015) find that CEO network centrality is negatively correlated with acquirer and deal-level returns.

[5] Solomon (2012) makes a similar argument that "reporters leaving the sample is likely due to events exogenous to the companies they write about." (pg.616)

owner (think of Fox News). I, hence, extend my analysis to study an exogenous shock to the journalistic independence induced by an ownership change of the Journal. In 2007, the *WSJ* was taken over by News Corp. This takeover represents an ideal laboratory to examine how news presentation and related stock reactions are altered for firms that were previously connected to the new owner, Rupert Murdoch, CEO of News Corp. Indeed, the *WSJ* itself published an article prior to the takeover questioning whether the Journal could retain its editorial independence under the new owner:

> *"Mr. Murdoch has tended to put a strong personal imprint on papers he owns, [...] He is known for phoning editors and even reporters about individual stories. The Post's media and business sections sometimes delight in skewering rivals, and Mr. Murdoch's political preferences have been clear in the news pages [...]"*[6]

And this is precisely what I find in a difference-in-difference analysis. Using hand-collected firm connections to Mr. Murdoch for the subsample of acquirers who conducted M&A deals both before and after the 2007 *WSJ* ownership change,[7] I show that firms connected to Mr. Murdoch are better pictured in the *WSJ* articles post Murdoch's takeover. Strikingly, the use of negative words is reduced by 73% for the connected acquirers relative to non-connected ones ($t = 6.25$), and the stock returns to these acquirers increase by 2% ($t = 3.56$). Again, this specification represents additional causal evidence for the effects of journalistic independence, even though it captures a different type of connection other than the direct working or educational relationships.

While it might be intuitive to understand why connections lead to more favorable coverage of news, it may seem puzzling why investors nevertheless react to the biased reporting. Tetlock (2007) provides a preferred explanation for my findings. The author argues that media sentiment is transmitted to asset prices via noise and liquidity traders (DeLong et al., 1990; Campbell et al., 1993). These models also predict that the sentiment effect is attenuated by rational arbitrageurs and stock liquidity.

These predictions are confirmed by the data. On average, acquirers who receive greater analysts' coverage, who are larger in size, who exhibit a higher level of institutional ownership,

[6] See the Wall Street Journal, "Murdoch's Surprise Bid: $5 Billion for Dow Jones", May 2, 2007.

[7] Such connections include direct business relations with News Corp., common directors, and/or common political campaign seats.

and whose stocks are more liquid in general are less subject to the media-connection effects. Even so, these noise or liquidity measures do not completely eliminate the biased market reaction: Here, I show that the effects of the journalists' connections remain highly significant after controlling for the noise or liquidity factors.

I finally turn to ask the question of how long the distorted market reaction lasts. According to the sentiment theory, the short-term positive reaction to the optimistic news report will be reversed in the long run (Tetlock, 2007). I find that stock returns to the acquirers featured by connected reporters indeed converge toward the returns of those non-connected acquirers. This result is depicted in Figure 1: After 40 trading days, the documented higher returns to connected acquirers finally disappear. In this instance, the horizon of the post-announcement drift appears rather comforting compared to the well-documented underreaction to Friday announcements (DellaVigna and Pollet, 2009), which lasts approximately 75 days, or to the reversal caused by contrast effects (Hartzmark and Shue, 2017), which dissipates after 50 days.

The fact of long-run price convergence rejects an alternative explanation of information transmission. That is, journalists with connections craft articles with a higher level of accuracy or with richer information content. Additionally, I check that these journalists are not rotated to cover a specific type of takeover event: Neither are they more positioned to report rumored bids, nor are they assigned to deals with higher likelihoods of consummation. Importantly, I find no evidence that target stock reactions or offer price premiums are higher in the M&A stories written by connected journalists. Thus, any concerns over my measures capturing target or deal characteristics are eliminated.

By examining the relations between journalists' connections and news tone and stock returns, this paper contributes to two strands of burgeoning literature. Foremost, recent work reveals that firms are strategically marketing and spinning the news. For example, Solomon (2012) examines selective publicity through the use of investor relations (IR) firms; Gurun and Butler (2012) and Golez and Karapandza (2018) report that local media positively slant the news toward local firms; and Ahern and Sosyura (2014) uncover that bidders originate more positive news stories during stock mergers. All these studies consistently document a real effect on the financial market, which is also established by a series of papers such as Huberman and Regev (2001), Tetlock (2007, 2011), Tetlock et al. (2008), Fang and Peress (2009), Engelberg and Parsons (2011), Dougal et al. (2012), and Peress (2014).[8] In contrast to active media management (e.g., Solomon and Soltes, 2012) or even manipulative communications (e.g., Leuz et al., 2017), this paper suggests a hidden channel of influencing the media slant via firm–reporter networks. To the extent that my analysis does not preclude the other channels, such as connections through IR firms, the estimate documented in this paper provides a *lower* bound effect of the connections.

In this regard, this paper also contributes to a growing body of "network" literature. The idea of network mattering is certainly not new: In the context of M&As, Cai and Sevilir (2012) provide evidence that board connectedness creates value for acquirers; Schmidt (2015) shows mixed evidence of value brought by "friend" board members; El-Khatib et al. (2015) find a negative relation between deal returns and CEO network centrality. Exploring the performance of venture capitals, Gompers et al. (2016) find that social or ethnic ties attribute to poorer decision making. On a positive note, Cohen et al. (2008), and Engelberg et al. (2012) suggest that social networks facilitate information flow, leading to better investment or lending decisions. Finally, Shue (2013), and Fracassi (2016) both find that social networks affect the firm's financial or executive compensation decisions.

The rest of the paper is organized as follows. Section 2 describes the sample and summary statistics. Section 3 presents the empirical results on journalist connections and news tone. Section 4 further explores how the effects transmit to the financial market. Section 5 implements additional strategies to shed light on the causal relations. Section 6 provides additional evidence for the effect on the financial market. Finally, Section 7 concludes.

2 Data

2.1 Sample of M&A news articles

The analysis of this paper focuses on the *WSJ* reports on corporate takeovers. Prior studies (e.g., Roll, 1988; Mitchell and Mulherin, 1994; Tetlock, 2007; Tetlock et al., 2008; Dougal

[8] Tetlock (2015) provides an excellent synthesis on the role of media in finance.

et al., 2012) reveal that *WSJ* coverage has a significant impact on the financial market. I narrow down my search to the takeover news because mergers and acquisitions represent the most significant corporate event and, importantly because the interpretation of M&A synergies is highly subjective and clearly reflective of an author's personal view.[9] Accordingly, I gather the sample from the following sources: the SDC Platinum M&A Database for the M&A transactions and Factiva for related *WSJ* articles.

I first collect all the M&A deals during 1997–2016 consisting of US public bidders and targets. The sample period begins in 1997 because the main measure of the firm–journalist connection requires a manual search on the *WSJ* engine, which starts in January 1997. Following the selection methods most often used in the M&A literature, the sample excludes recapitalizations, self-tender offers, exchange offers, repurchases, partial equity stake purchases, acquisitions of remaining interests, privatizations, financial buyouts, as well as deals in which the target or the acquirer is a government agency.[10] I further ensure that (1) the transaction value exceeds US$1 million, (2) the acquirer owns at least 50% of the target equity after the deal completion, (3) the target is not undergoing bankruptcy proceedings, and (4) the parties are non-financial firms. These criteria yield a total number of 2,390 deals from SDC.

Obviously, not all takeovers are covered by the *WSJ*. I search the *WSJ* coverage using a Factiva algorithm that includes media sources from the *WSJ* U.S. print edition. To be included in the sample, I also require that the article is the first report on the transaction and appears within the two-day period after the official deal announcement.

After matching the *WSJ* sample firms with Compustat (for financial data), CRSP (for stock data), and after excluding articles whose authors' backgrounds (such as colleges) cannot be identified, the final sample consists of 1,131 M&A news articles. Table 1 reports the number of M&A transactions from SDC and valid *WSJ* sample across calendar years and months. Reassuringly, the *WSJ* provides a solid coverage of large transactions: Overall, 47% of all SDC deals are featured in the *WSJ*. The covered deals have an average transaction

[9] To be precise, the synergies captured by the bidder are stochastic and subject to individual interpretations. The return to the targets is primarily determined by the offer premium (Betton et al., 2008, Chapter 15, pg. 411). Therefore, this paper focuses on the bidder's returns.
[10] This selection procedure is similar to, e.g., Masulis et al. (2007) and Erel et al. (2012).

value of US$5.2 billion, compared to the average SDC value of US$2.6 billion. This is perhaps unsurprising because larger transactions attract greater media attention (e.g., Solomon and Soltes, 2012).

2.2 Journalists and measures of connections

Journalist names are collected directly from each *WSJ* article.[11] The journalists' personal relationships with firms, however, have to be inferred from additional sources. For practical reasons, in this paper, I examine two types of firm–journalist connections, namely, working relationships and educational ties. Admittedly, this practice omits other potential social ties, such as common board or club affiliations and second-degree connections through IR firms. To the extent that these additional networks matter, my focus on the work and educational relationships provides a conservative estimate of connection effects.

To identify direct working relationships between a reporter and a firm, I follow Solomon (2012) by checking whether a reporter has written at least two stories about the same acquirer in the 12 months prior to the M&A. Particularly, I read each pre-deal article to ensure these stories are unrelated to the M&A deal in question. The working relationship measure, *CONNECT_WORK*, takes a value of one if any author of the M&A story has covered the acquirer at least twice in the previous year.

I construct the educational network by searching for universities that a reporter and an acquirer CEO attended. The *WSJ* provides personnel biographical information on the authors' web page. I supplement this information with data crawled from Linkedin, a large professional networking service, and Muck Rack, a public relations firm that collects journalists' biographical information in its media database. I also gather the names of the acquirer CEO's universities from the companies' proxy filings on EDGAR and Bloomberg Executive Profile Database. The measure of the educational tie, *CONNECT_UNIVERSITY*, equals one if any author of the article attended the same school as the one attended by the acquirer CEO.

In the top panel of Table 2, I summarize the firm–journalist connections. The measures are calculated at the news article level: For example, out of the 1,131 stories, 26% are written by a

[11] See Internet Appendix A1 for a list of most frequent reporters in my sample.

reporter that has prior working relationships with the acquirer firm. Connections measured by graduate schools are far less common; the mean of *CONNECT_UNIVERSITY* equals 0.023, indicating that only 2.3% of the articles are written by an author who attended the same college as the CEO.

Ahern and Sosyura (2015) suggest that journalists' experience might affect the reporting accuracy. Along this line of findings, I collect control variables to capture a journalist's experience and individual characteristics. Using sources from the *WSJ*, Linkedin, and Muck Rack, I gather information on each journalist's gender, location (i.e., *WSJ* office), industry expertise, university degree, tenure (i.e., number of months she has worked for the *WSJ*), and total number of publications on the *WSJ*. The summary statistics, calculated at the article level and reported in Table 2, show that 44% of the articles have at least one female coauthor and that 23% have a reporter based in the same city as the acquirer's headquarter. On average, the coauthors have worked for 5.6 years at the *WSJ* and have written 344 stories. In addition, 78% of the coauthors hold a major degree in journalism or literature, and 48% have a professional specialization in the acquirer's industry. These statistics are largely consistent with those reported by Ahern and Sosyura (2015).

2.3 News tone and stock returns

The main dependent variable, *news negativity*, characterizes the negative sentiment in each news article. I focus on the negative sentiment because previous studies find that negative information has a stronger impact than positive information (e.g., Rozin and Royzman, 2001; Tetlock, 2007; Gurun and Butler, 2012). I use the negative word categorization of Loughran and McDonald 2016 Master Dictionary to count the number of negative words[12] and calculate its fraction out of the total article word count:

$$News\ negativity = (\#Negative\ Words / Total\ \#Words) \times 100 \tag{1}$$

[12] The Loughran and McDonald Dictionary (Loughran and McDonald, 2011; Bodnaruk et al., 2015; Loughran and McDonald, 2016) contains sentiment words in financial applications and has been widely applied in financial context analysis (e.g., Gurun and Butler, 2012; Solomon, 2012; Ahern and Sosyura, 2015). The most frequently occurring Loughran and McDonald (2011) negative words include, e.g., *loss(es), impairment, against, adverse(ly), failure, unable, doubtful,* etc.

The average negativity expressed in an M&A article equals 1.4% (See Table 2), largely in line with the 1.7% negative slant reported by Gurun and Butler (2012). To get a sense of the news tone, I illustrate in the first section of Internet Appendix two examples of both "positive" and "negative" excerpts from the *WSJ* articles.

To capture market reaction to an M&A announcement, I estimate the three-day cumulative abnormal return (CAR) centered on the deal announcement date, for each bidder. The CAR is the residual from the market model, whose parameters are estimated over a 200-day window ending 31 days before the announcement. This procedure requires 200 non-missing returns during the estimation window. The market portfolio is proxied by the value-weighted CRSP index. To reduce the influence of outliers, I winsorize the returns at 0.5% and 99.5% levels. Table 2 shows that the average bidder CAR is -1.9%. This estimate compares favorably to the -2.1% bidder CAR for a sample of 1,664 public deals analyzed by Cai and Sevilir (2012).

2.4 Additional control variables

A considerable part of my analysis concerns the bidder's M&A performance. As in prior M&A literature, I construct several bidder and deal related characteristics as control variables. For example, I collect deal relative size, attitudes (e.g., hostile bids), unsolicitations, diversifying bids, payment methods (e.g., cash versus stock), and completion rates from the SDC. From Compustat, I also gather acquirer characteristics, such as size, Tobin's Q, leverage, cash holdings, and profitability. Additionally, to capture the acquirers' corporate governance (e.g., Masulis et al., 2007), I obtain board-related variables, including CEO age, founder CEO status, duality, board size, and classified board, from the acquirers' proxy filings prior to the M&A announcement.

The summary statistics of these additional variables are reported in Table 2, and their definitions are outlined in the Appendix. In most important respects, the sample characteristics are similar to those analyzed in earlier studies: For instance, approximately 31% of the transactions are all-cash financed, and 3% are classified as hostile bids, which is in line with the 31% and 1% reported by Cai and Sevilir (2012); At 90%, the completion rate in my sample is comparable to that of 85% in Gaspar et al. (2005).

3 Journalist connections and news tone

I begin the empirical analysis by examining whether acquirers connected to a journalist receive more favorable coverage when their bids are featured on the newspaper. I present the empirical strategy and regression results in Section 3.1, and discuss the robustness of the results in Section 3.2.

3.1 Regressions

To get a sense of how the journalists' connections affect news tone, I estimate the following linear regression:

$$\textit{News negativity}_{it} = \beta_{it} \cdot CONNECT_{it} + \eta \cdot Controls + \theta_t + \psi_j + \epsilon_{it} \tag{2}$$

where $\textit{News negativity}_{it}$ is the news tone of an M&A article, as defined in (1). Subscript i indexes a specific transaction, and t indicates the time (i.e., year-month) of the deal announcement.

The variable of interests, $CONNECT$, corresponds to either measure of the firm–reporter connections described in Section 2.2. Across my specifications, acquirer industry fixed effects, indexed by j and classified based on Fama-French 38 industries, are controlled for. This is particularly important because Solomon and Soltes (2012) suggest that industry is a robust predictor of news coverage. In addition, I include time fixed effects (θ_t), defined as the year-month of the M&A announcement, to control for unobserved time trends such as general market sentiment (Baker and Wurgler, 2006). Standard errors are double-clustered by time and by industry.

One potential concern is the possibility that the connection measures might capture other firm characteristics because any social network is not randomly formed. To get a sense of how firm–journalist connections correlate with firm characteristics, I regress the connection measures on various firm and/or board variables. The results, reported in Table A2 of Internet Appendix, indicate that firm size, location, Tobin's Q, and performance are the most significant predictors of a journalist's connection. Unsurprisingly, larger firms are more likely to establish working relationships with a journalist. Firms located in New York City and in California are also more likely to have journalists' ties. High Q and low leverage are positively correlated with *CONNECT_WORK*. Crucially, the results do not show that journalists' connections are simply a proxy for "high sentiment" firms: Past stock returns and return skewness (a proxy for salience) are insignificantly correlated with connections; if anything, firms with poor past profitability are more likely to be covered by a connected reporter. These findings also speak to the importance of including firm/board characteristics in equation (2). With this caveat in mind, I proceed to the main analysis.

I begin with the specifications that examine working relationships. Controlling for journalist and deal characteristics, the regression results reported in the first column of Table 3 indicate a significant negative relation between *news negativity* and the journalists' connections (*CONNECT_WORK*). This negative relation is not mitigated when I include *WSJ* office and acquirer state fixed effects to control for newsworthiness related to geographies or home bias (as in column 2), and more firm/board related variables (as in column 3). The point estimate suggests a substantial more favorable coverage if the author has prior working experiences with the acquirer: For instance, according to column 3, an article authored by a connected reporter contains 24% fewer negative words relative to an average article.[13] Depending on the specification, the significance level of the connection effects is between 0.006% and 0.3%.

In the next set of specifications, I test the relation between the educational network and the news tone. The results are reported in columns 4 through 6. Similar to working connections, I find that educational ties significantly reduce the negative slant in the reporting of the news. According to column 6, the fraction of negative sentiment is 13% (= 0.1868/1.396) lower in stories written by a college alumnus compared to articles issued by independent journalists. Even though the college-connections are fairly rare in my sample, the point estimates across the specifications appear highly significant. Overall, the immediate message following these tests is that firm–reporter connections are an important determinant of news slant.

[13] The calculation is as follows: I divide the coefficient estimate of *CONNECT_WORK* (-0.3284) by the mean of news negativity (1.396), and obtain 23.5%.

On the other hand, I do not find evidence that the journalists' experiences affect news sentiment. For instance, the journalists' gender, college major, tenure, and industrial expertise all insignificantly contribute to the news tone. Particularly, geographical proximity captured by "same city" does not lead to either more positive or more pessimistic reporting, rejecting home bias of the journalists. There is weak evidence that reporters who have published more articles write more pessimistically. Deal characteristics, however, appear correlated with tonality: For example, hostile and unsolicited takeovers are significantly related to a higher proportion of negative words.

3.2 Robustness

To conclude the analysis in this section, I perform a number of robustness checks displayed in Table A3 of Internet Appendix. In Panel A, I assess whether the results are sensitive to some omitted variables. In the first two columns, I first repeat the regressions in columns 3 and 6 of Table 3 by additionally including journalist fixed effects.[14] Note that this is a powerful test, as it explores the variation of news tone within a journalist. This means that $CONNECT$ is identified solely from the differential negativity between a journalist's connections and non-connections. As a result, journalists who authored only once in my sample are dropped, leading to a smaller number of observations. Column 1 shows that $CONNECT\_WORK$ remains highly significant, even though the coefficient estimate decreases in magnitude compared to column 3 of Table 3. The interpretation to this result is that the news tone in articles on the acquirers with connections is significantly less negative than the news tone in the other articles authored by the *same* journalist. Therefore, any general journalists' writing styles, as suggested by Dougal et al. (2012), cannot drive my results. In column 2, when I focus on the university ties, I find the results go through, and the economic effect becomes markedly stronger than in the column 6 of Table 3.

Because Gurun and Butler (2012) suggest that firms might use advertising expenses to positively influence the media coverage, in columns 3 and 4, I additionally control for the bidder's advertising expenses. The estimates of connection effects remain essentially identical

[14] Specifically, I include the fixed effects of the first-author journalist to deal with articles with multiple coauthors. The connected journalists are always ordered as the first journalist to maximize the within-journalist variation.

to those reported in Table 3, and the associated significance level is largely unchanged. Unreported estimates show that bidder's advertising expenditure only insignificantly contributes to the news tone (-0.2666 and -0.6521 in column 3 and 4, respectively, with standard errors = 1.118 and 1.162). In models 5 and 6 where a vector of target characteristics, including size, Tobin's Q, leverage, cash holdings, profitability and pre-announcement stock runup, are additionally controlled for, the connection effects on the news tone remain significant.

In Panel B of Table A3, I replace the dependent variable with different ways to measure the news negativity. In regressions 1 and 2, I proxy for the negative sentiment using the logarithm of negative word count in a news article. This alleviates the concern that readers focus on the *absolute* number of negative words rather than its proportion. The results show that journalists' ties are associated with significantly less use of negative words even in their absolute value.

In addition, I assess the robustness of results using the probability of negative sentiment given by a machine learning (ML) classifier. The most prominent advantage of using the ML method is that each positive and negative word is weighted differently. For example, "great" might indicate a more favorable tone than the tone indicated by "good." Another potential benefit of using a classifier relates to the observable content metrics used to evaluate the article sentiment: While Loughran and McDonald's Dictionary accounts for the nuances in the financial language, these sources might still deviate from the particular language used in the financial news in the *WSJ*. Therefore, to train the classifier, I collect 11,669 *WSJ* articles on earnings news from 1990 to 2016 and manually label each article as either "positive" or "negative." I choose earnings news because stories on earnings usually exhibit a clear positive or negative tone. I then use the classifier to predict the sentiment of my M&A sample news. I outline the details of this process in Appendix 2.

In models 3 and 4 in Panel B, I run OLS regressions on the probability of negative articles.[15] We see that work connections are related to a 6% lower likelihood that an article is classified as negative by the ML classifier. Similarly, university connections are associated with a 16% smaller probability of being classified as negative. Both estimates are significant

[15] The probability is the prediction outcome given by the Naive Bayes Classifier. For example, a probability of 90% indicates that the classifier believes an article has 90% chances of being negative, based on what the classifier learnt from the training set.

at 10%.

Lastly, one may worry that the considered working relationship measure (*CONNECT_WORK*) may inadequately capture the journalist's connection, because a journalist that frequently covers the firm may not actually speak on the phone or in-person with the CEO. In Panel C, I use a strengthened *CONNECT_WORK* variable, where I require at least one quote from the CEO in the M&A article. Applying the same specifications as in Table 3, I find similar results when I focus on this refined measure of ongoing CEO-journalist personal relationship.

4 Journalists' connections and stock returns

To date, the results suggest a strong and robust relation between firm–journalist connections and news tone. Perhaps a natural and more interesting question that follows is whether this connection-induced sentiment has real effects on the financial market. As discussed in the introduction, a number of studies have established a link between news tone and market reactions.[16] Therefore, we should expect that returns to the acquirers featured by a connected journalist are higher than those covered by an independent journalist.

To assess the impact on the financial market, I estimate return regressions similarly specified as in (2) with the dependent variable of the bidder's CAR. The coefficient of *CONNECT* compares the differential market responses between connected and non-connected bidders.

In the first column of Table 4, I include only time and industry dummies. As expected, the estimate of *CONNECT_WORK* exhibits a positive and significant sign. The second column additionally controls for deal, acquirer and board characteristics that are commonly examined in the M&A literature. The magnitude of *CONNECT_WORK* becomes weaker but nevertheless remains highly significant ($t = 2.24$). The economic effect is such that covering by a connected journalist improves the abnormal returns by 2.1 percent. To put this magnitude of reaction into perspective, given an average acquirer's market capitalization of US$31 billion 4 days prior to the deal announcement, a 2.1% increase in return translates

[16] To replicate the correlation between news tone and stock returns, in Table A4 displayed in the Internet Appendix, I show that the negative tone in the *WSJ* articles is indeed significantly associated with lower bidder's CAR. These results, although non-causal, confirm the assumption that news tone matters for stock returns.

into a striking 651 million ($= 31000 \times 2.1\%$) dollar gains during the three-day announcement period.

Are these differential reactions truly driven by news sentiment? Plausibly, under the assumption of limited attention (e.g., Barber and Odean, 2008), investor reactions to the optimistic reporting are likely to be stronger for the news featured on the front page of the Journal. Fedyk (2018), in particular, shows that front page positioning of news induces higher trading volumes and larger price changes. Fortunately, Factiva provides the classification of news positions, which allows me to directly test this possibility. First, I classify the sample M&A articles into those appearing on the front page, including the well-known "*What's News*" Column of the *WSJ*, and those appearing somewhere else inside the newspaper. I then rerun the return regressions by including an interaction term between *CONNECT* and *Front page* to identify the incremental effect of front-page positioning.

In column 3, the estimate of the interaction term "$CONNECT\_WORK \times$ *Front page*" suggests that the connection effect is concentrated on the news prominently positioned on the front page of the newspaper. The associated magnitude of the effect also becomes slightly larger. Importantly, I verify in an untabulated analysis that a negative tone in the front-page and non-front-page articles is undistinguishable (t between -0.91 and 0.62 for the difference). This finding perhaps speaks convincingly to the media channel moderated by investors' attention and renders me confident that my empirical choices are not mechanically driving the results.

Moving to the right across Table 4, we first spot insignificant return differences between bidders covered by a same-college journalist and bidders without a same-college journalist tie. This is perhaps because the media slant by same-college journalists is in general weaker (as shown in Table 3) or because the proportion of college connections is small, which reduces the testing power (i.e., the standard errors are high, as seen from columns 4 and 5). However, when I examine the articles positioned on the front page, I again find a positive and significant estimate of "$CONNECT\_UNIVERISTY \times$ *Front page*" ($t = 2.15$). This effect, though driven by relatively few observations, is complementary to the evidence yielded from direct working relationships. Here, the main take-away is clear: Investors respond more positively to the acquisitions covered by connected journalists.

5 Are the relations causal?

An immediate challenge to a causal interpretation of the previous findings is that I do not observe whether the journalists' assignment is random. Without the luxury of random variation, a number of unobservable factors could potentially bias the estimate of connection effects. For example, the unobserved firm general social connectedness could simultaneously drive both the journalists' ties and the deal synergies, which in turn could affect news sentiment and stock returns.

To shed light on causality, I take an instrument variable (IV) approach. I describe the IV analysis in Section 5.1. Additionally, in Section 5.2, I explore an exogenous shock to the journalistic independence induced by an ownership change of the *WSJ* and analyze its effects on news tone, as well as on stock returns.

5.1 Instrumenting journalist connections

The basic idea of IV is that the considered "instrument" correlates with the propensity of connected coverage but has no independent effect on either news tone or stock returns. I contend that journalists' turnover satisfies both the relevance condition and the exclusion restriction. For instance, the departure of a friendly reporter from the *WSJ* will decrease the propensity of the retrospective firm being covered by a connected journalist; however, the turnover is mostly likely driven by the reporter's personal life events and thus is exogenous to the deal synergy or to stock returns.

To identify turnovers, I rely on the dates when a journalist joined and left the *WSJ*.[17] Unfortunately, the *actual* pool of connected (non-connected) reporters for a firm is unobservable. Therefore, I have to rely on the total number of connections in the sample that are accessible by a firm at a given point of time. To be precise, I count the number of an acquirer's connections on the date of the M&A news, including all old "friends" who wrote about the acquirer in the past and who are still working at the *WSJ*. Next, I examine the journalists' departures in the three-month window prior to the bid announcement. If one

[17] I use the official starting and leaving dates reported on the *WSJ* web page whenever possible. Otherwise, I achieve the turnover dates from Linkedin.

of the connections left the Journal in this window, I code the turnover rate by dividing one by the acquirer's total connections including the left reporter; otherwise, the turnover rate equals zero if there are no departures during this period.

Figure 2 illustrates how the turnover rate is calculated: Suppose *Bid.Inc* conducted three M&As in June 2010, December 2010, and May 2012. The first and the third bids are both covered by connected reporters, John and James, respectively. Suppose both John and James joined the Journal before the first bid occurred but that John left the newspaper in April 2012, right before the announcement of the third takeover. Because both connected reporters were accessible when the first bid was announced, the connected reporters' turnover rate for the measured period related to the first M&A news was zero, as was the turnover rate for the second M&A news. However, when the third bid occurred, there was only one connected reporter remaining, and the turnover rate became 0.5 (= 1/2).

This exercise is possible only for the acquirers who have conducted more than one transaction and who have at least one journalist connection (otherwise the denominator of the turnover rate equals zero). To obtain a meaningful variation, I focus solely on the connections measured by working relationships. Because of the imposed restriction, here, I obtain a substantially smaller sample size of 321 observations.

In the first column of Table 5, I show that the turnover rate of connected journalists is a significant predictor of whether the bid is covered by a connection. Controlling for the deal, firm and board characteristics, the analysis confirms the relevance condition which posits that departures of connections significantly reduce the propensity of a connected coverage.

From columns 2 through 5, I examine the connection effects on news tone and stock returns, using both OLS regressions and two-stage least squares (2SLS) regressions. The results are remarkable. For instance, when directly regressing news negativity on the connection indicator without an instrument (column 2), I find that articles with connections contain on average 27% fewer negative words than articles without connections, for this specific subsample.[18] This magnitude of the effect is comparable to the baseline regression in Table 3, despite a substantially smaller sample size. However, when instrumenting the connected

[18] The 27% lower negativity is derived from dividing 0.3623 (the coefficient estimate) by 1.3483 (mean negativity of the subsample).

coverage with journalists' turnover (column 3), I find the effects on the negative tone almost double: Those connected reports are 52% less pessimistic than non-connected articles, and the significance level remains at $< 1\%$.

The effects on the stock returns appear even more striking. The instrumented *CONNECT_WORK* (column 5) exhibits a roughly fourfold increase in magnitude compared with the OLS estimate (column 4), from 1.2 percent to 4.8 percent. This finding suggests that insofar as the IV is plausible, the OLS estimate might understate the connection effects on the financial market.[19]

Caution is required, however, when one attempts to infer the true economic magnitude. On the one hand, as suggested earlier, the OLS estimate of $CONNECT$ is likely downward biased because of omitted firm–journalist relationships not encoded in this interested variable (e.g., connections through an IR firm). On the other hand, Jiang (2017) proposes several explanations as to why an IV might lead to an inflated estimate, such as a weak instrument problem. While the excluded IV in my analysis has passed the test of weak identification in particular, potential issues, such as those stemming from a "local average treatment effect" (LATE) might still prevent generalizing the effect to the population. Overall, the immediate message from the IV analysis nevertheless supports the causal interpretation of the connection effects.

5.2 Additional evidence: Mr. Murdoch's takeover of the *WSJ*

To this point, the analysis has focused on direct working relationships and educational networks. However, journalistic independence could be affected by factors beyond the journalists' personal relationships. Among these factors, the political position of the press or the press owner could play a vital role in shaping how the news is written (e.g., DellaVigna and Kaplan, 2007). To broaden the scope of the probe, in this subsection, I analyze connections brought in by the owner of the newspaper. Specifically, I attempt to provide additional

[19] To address the concern that measurement errors in both variables, *Journalist turnover* and *CONNECT_WORK*, might be correlated so as to bias the second stage regression estimates (columns 3 and 5 in Table 5), I estimate reduced-form regressions where the dependent variable, *News negativity* and/or *CAR*, are regressed directly on the exogenous variable *Journalist turnover*. The results are consistent with Table 5 and are reported in Internet Appendix A5.

evidence by exploring an ownership change of the *WSJ*, which exogenously "shocks" the journalistic independence.

On May 2, 2007, News Corp., under the direction of Rupert Murdoch, made an unsolicited bid for Dow Jones, which owned the *WSJ.* The takeover officially occurred in August 2007, ending the Journal's 105-year ownership by the Bancroft family. There were valid concerns that the ownership transfer might adversely affect the journalists' independence at the *WSJ.* To requote the article published on May 2, 2007 by the Journal, *"Mr. Murdoch's bid promises to raise questions about whether the Journal would retain its editorial independence under his ownership,"* because Mr. Murdoch is "*known for phoning editors and even reporters about individual stories.*"

Assuming the owner effect on journalistic independence is true, we would expect that the news coverage becomes more favorable for the acquirers that are connected to the new owner after the *WSJ* takeover, relative to other unconnected acquirers. I test for this effect by running the following difference-in-difference regression:

$$News\ negativity\ or\ CAR_{it} = \beta_{it} \cdot Murdoch \times Post_{it} + \gamma . Murdoch + \eta \cdot Controls + \theta_t + \psi_j + \epsilon_{it} \quad (3)$$

where *Murdoch* indicates acquirers connected to Mr. Murdoch (or News Corp) prior to the *WSJ* bid, and *Post* is a dummy variable that equals one for deal announcement years after 2007.

To identify connections with Mr. Murdoch, I search in each acquirer's 10K and proxy filings to detect whether there exist (1) business relations, such as asset purchases or supply chain links, between the two companies, (2) common directors sitting on both Boards, and/or (3) other links such as political campaign seats. Following this exercise, I find that approximately 8% of acquirers are connected to Mr. Murdoch, in this subsample of analysis.

Because I include year dummies (θ_t) in (3), the estimate of *Post* is absorbed and thus not reported. I also include acquirer industry, state, and *WSJ* office fixed effects across specifications to account for time-invariant unobservable firm characteristics related to business sectors and geographies. In the presence of fixed effects, including additional controls may lead to biased estimates if the identifying construct (*Murdoch*×*Post* in this case) concurrently affects the control variables (Angrist and Pischke, 2009; Gormley and Matsa, 2014). Therefore, columns (1) and (4) in Table 6 suppress the control variables (i.e., deal, firm, and/or board characteristics). The coefficient of interest here is $Murdoch \times Post$, which captures the average treatment effect.

Requiring acquirers to conduct more than one bid both before and after 2007 substantially reduces the size of the sample. Hence, this analysis relies on only 392 observations. Despite the small sample size, the results are striking: As Table 6 shows, post the *WSJ* ownership change, news coverage for acquirers connected to Mr. Murdoch contains 57% to 62% fewer negative words than unconnected acquirers, depending on the specification (columns 1 through 3). On the other hand, market reactions to the takeovers by these connected acquirers become significantly more positive: Columns 4, 5, and 6 report that these firms achieve on average 2–3% higher returns than unconnected acquirers, upon their M&A public announcement.

To make causal inferences, it is crucial to ensure that the outcome is driven by journalistic independence induced by the press ownership change rather than by other firm attributes related to the *WSJ* takeover. To further refine the analysis, in Panel B of Table 6, I remove 7 acquisitions within the Entertainment and Printing/Publishing sectors based on Fama-French 48 industry codes. Looking at acquisitions unrelated to these industries addresses the concern that the *WSJ* control change has a direct impact on its competitors and their acquisition synergies. Panel B shows that the documented treatment effects are not driven by firms within the Entertainment and Printing/Publishing sectors: the effects on news tone and financial market remain similar in magnitude for firms outside these sectors.

Given that Mr. Murdoch's takeover of the Journal is reasonably orthogonal to the writing styles of journalists or the expected deal synergies of other market participants, I interpret the results from the difference-in-difference analysis causally. Although this exercise highlights a different type of firm–journalist connection, the spirit is similar to the work/university relationships examined earlier: Friends are valuable — when there is a connected reporter at the newspaper, the stories become rosier, and investors subsequently push the returns higher.

6 Additional evidence from the financial market

Next, consider explanations provided by Tetlock (2007) that summarize how media sentiment transmits into the stock market. Models of noisy traders, such as that in DeLong et al. (1990), posit that these noisy traders, facing downward-sloping demand for risky assets, sell shares to rational arbitrageurs when there is a negative belief shock, pressuring prices down; theories of liquidity traders, such as that of Campbell et al. (1993), make similar predictions, with the media sentiment proxying for a change in the levels of risk aversion.

Models in which noisy and liquidity traders transmit media sentiment into stock prices have two implications. First, these theories forecast that media sentiment has a lessened impact on the financial market if stocks are primarily held by rational arbitrageurs or are more liquid in general. Second, the theories also predict that stock prices reverse next period when a new belief shock kicks in.

I test these predictions in this section. In Section 6.1, I explore the heterogeneous responses to the connection effect. In Section 6.2, I focus on the long-run stock price drift.

6.1 Interaction effect

I first consider the heterogeneous responses induced by different levels of rational arbitrageurs and stock liquidities. To characterize the level of shares held by rational traders, I obtain the analyst coverage of a firm, total asset size, and the percentage of institutional ownership. Rational arbitrageurs are expected to account for a greater fraction of trades for larger firms, firms with more analysts' coverage, and/or firms owned by more institutional investors (e.g., Barber and Odean, 2013). To proxy for stock liquidity, I adopt Amihud (2002)'s illiquidity measure, which gauges the average impact of trading volume on a stock's absolute return.

I interact these proxies with the firm–journalist connection measure, $CONNECT\_WORK$. In Table 7, the first column suggests that firms covered by more analysts are less subject to the media effect due to more favorable connected news reports. To obtain a sense of the magnitude of the estimate, adding one additional analyst reduces the media effect by 0.2% on stock returns. This estimate is nevertheless only suggestive because the propensity of being connected might correlate with the analyst coverage.

Consistently, column 2 and 3 show that firm size and institutional ownership mitigate the distorted stock reaction induced by connected news coverage. Here, the economic effect is such that taking the example of institutional ownership, moving from the bottom quintile to the top quintile precipitate the decline of market reaction by almost 3%.

The last column examines the liquidity effect. Because the Amihud measure essentially captures the illiquidity of a stock, we find a significant and positive sign of the interaction coefficient, suggesting that higher illiquidity amplifies the sentiment effect induced by a connected coverage.

Do rational traders and stock liquidity eliminate the connection effects? The answer is no. As is clear from Table 7, the coefficient estimate of the constituent term, *CONNECT_WORK*, remains highly significant across all specifications. In other words, even liquid firms with rational investors continue to benefit from glowing reports by their newspaper connections.

6.2 Long-run stock price drift

My final set of tests builds on the sentiment theory that predicts a price reversal in the long run. In these tests, I focus on the long-term window from day two through day forty after the official merger announcement. Because stocks react more positively to the connected acquirers during the three-day announcement period, we should expect a more negative abnormal return to these connected firms over the long run.

To ease the comparison of the results between tables, I first replicate the regressions of the short-term returns (columns 1 to 2 of Table 4) in the first two columns of Table 8. Moving right to columns 3 and 4, we first observe that returns to firms with connections become significantly more negative than non-connected firms over the [2,40] window, supporting the long-run price correction. The magnitude of price correction is estimated at between 2.1% to 3.3%, depending on the specification. This post-announcement price drift, hence, almost completely cancels out the initial better reaction to connected acquirers upon deal announcements. Indeed, combining the short- and long-run returns together, in columns 5 and 6, we find insignificant difference in returns between connected and non-connected firms.

This return pattern is more intuitively illustrated by Figure 1. One can interpret Figure 1 as the univariate comparison of daily acquirer returns from day -5 till day 40. This figure makes it clear that acquirers connected to reporters with working relationships exhibit higher returns when the deal is initially announced. Over the time, returns to these connected acquirers drift toward those of the non-connected acquirers such that at the end of 40 trading days, the two groups of acquirers achieve approximately the same level of abnormal returns.

A potential concern with the price drift results in Panel A of Table 8 is that they might be due to updated information related to the bidder, target, and/or the transaction itself during the long-term window. For example, investors might react to the news of transaction withdrawals. To address this issue, I remove 119 cases where the deal is eventually withdrawn in Panel B. In addition, in Panel C, I eliminate further 342 observations with potentially confounding events during [2,40]. These confounding events are mainly driven by the acquirer and/or target earnings announcements, as identified by the I/B/E/S database. The regressions in Panel B and C continue to show a short-run overreaction and the subsequent price correction for the connected acquirer firms.

The result of price correction is important because it speaks against an alternative possibility that reports by connected journalists contain differential information from those non-connected reports. Under this alternative possibility, connected journalists might achieve a higher level of accuracy (and/or more information content) in their reporting. However, note that this alternative does not help make any clear predictions with regard to media slant because greater accuracy does not point to either more positive or more negative language sentiment. Therefore, the tests of news tone are not really helpful in discerning accuracy effects. The fact that we observe post-announcement price drift only for the connected group, on the other hand, clearly rejects the information hypothesis because if any, we should find a price drift for the non-connected group post announcement when additional information flows to the market.

In this regard, to be more reassured, one has to confirm that connected reporters are *not* rotated to write a specific type of acquisition. For example, are connected acquirers more likely to underpay the deal premium? Are connected reporters more likely to cover a rumored bid, or bids with higher probabilities of consummation? In Table A6, displayed in the Internet Appendix, I carry out tests that aim at examining how connected reporters are related to a specific type of bid or to the transaction outcome. Ideally, if there is no endogenous rotation to a deal type, we should expect no link between connections and deal outcomes.

And yet, this is what I find in Table A6. First, target returns, measured as CAR over [-1,1], exhibit an insignificant relation with both connection measures. Importantly, this result supports the construct of my connection measures as my measures explicitly point to relationships with the acquirer (rather than the target); so, the sentiment expressed in the news article is also more relevant for the acquirer's synergies. Moreover, deal premium, proxied by the offer price over the target price 4 weeks before the announcement, and return premium, constructed following Schwert (1996) as the target CAR over the period [-42,5], also appear unrelated with the journalists' connections. Therefore, the better stock reactions to connected acquirers cannot be driven by underpayment. Finally, I ask whether connected journalists are more likely to write about bids associated with higher consummation rates or associated with rumors. The results refute these possibilities. In other words, one does not observe differential deal characteristics or outcomes from the transactions covered by connected journalists. Hence, any concerns over the connection measures proxying for specific information content are eliminated.

7 Conclusion

This paper builds on a burgeoning field of studies that document the very existence of media effects on the financial market, especially those discovering predictable stock returns from journalists' writing styles (Dougal et al., 2012). However, what influences a journalist's tone in reporting? A potential answer to this question is the journalist's relationships with the firm. I focus on two types of relationships in this paper: direct working relationships and alumni networks with the CEO.

Specifically, I ask two questions: (1) do journalists' connections bias the news tone when a connected firm is covered, and (2) if so, does the news slant have an impact on the firm's stock returns? Using M&A news articles published in the *WSJ* from 1997 through 2016, I find that the answer to both questions is "yes." Such connections are associated with significantly less use of negative-sentiment words in the news article, and the upbeat optimism of friendly reporters is related to markedly better stock reactions, especially when the firms are featured on the front-page of the newspaper.

To credibly make a causal claim, I instrument the propensity of being covered by a connected journalist with the turnover rate of connected reporters. The two-stage regression results yield a consistent relation of connected coverage, news tone, and stock returns. Thus, the immediate takeaway is that favorable media slant and the associated better stock reactions seem to be *caused* by the firm–journalist connections.

While prior studies have investigated deliberate marketing and spinning of news (e.g., Solomon, 2012), my findings suggest an indirect channel of influencing media through social networks. However, caution is needed in interpreting these results: While my analysis points to a clear media slant, it does not indicate media manipulation. Indeed, biased reporting might be a product of unconscious psychological working, and various factors, such as institutional environment, could potentially affect journalistic independence. In this later regard, I show that an exogenous shock to the journalistic independence, induced by the News Corp.'s takeover of the *WSJ*, leads to more favorable coverage of firms that were previously connected to the new owner and subsequent better stock returns to these connected firms.

Overall, the evidence suggests that social networks have a greater impact on the firms' stock performance than one might have initially thought. Do firms take advantage of their media connections to achieve even manipulative communications (e.g., Leuz et al., 2017), such as delaying negative news publicity? I leave this question to future research.

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Appendix

1. Variable definition

Panel A: Journalist and news related variables	
News negativity	The fraction of negative words in the text of a news article. The negative words are defined in the Loughran and McDonald 2016 Master Dictionary. (Source: `http://sraf.nd.edu/`)
CONNECT_WORK	Indicator variable that identifies a direct working relation between the journalist and the firm. This variable equals one if at least one journalist has written two or more stories (unrelated to the considered merger) about the acquirer during the previous calendar year. (Source: *WSJ*)
CONNECT_UNIVERSITY	Indicator variable that equals one if the acquirer CEO and (at least one) journalist graduated from the same university. (Source: proxy filings, Linkedin, Bloomberg, *WSJ*, Muck Rack)
Same city	Indicator variable that equals one if the acquirer CEO and (at least one) journalist are located in the same metropolitan area. (Source: proxy filings, Linkedin, *WSJ*, Muck Rack)
Female	Indicator variable that equals one if the news article has at least one female journalist. (Source: Linkedin, *WSJ*)
Journalist tenure	The average number of months that a coauthor works at the *Wall Street Journal*. (Source: Linkedin, *WSJ*)
Number *WSJ* articles	The mean of total number of articles that a journalist writes for the *Wall Street Journal* until the date of the acquisition announcement. (Source: *WSJ*)
Industry expert	Indicator variable that equals one if any coauthor of the article has a primary focus on the industry of the acquirer firm. (Source: Linkedin, *WSJ*)
Journalism degree	Indicator variable that equals one if any journalist has graduated with a major in journalism or in literature. (Source: Linkedin)
Article length	Total number of words in the news article.
Front page	Dummy variable equals one if the news article appears on the front page (including "*What's News*" column) of the *Wall Street Journal*. (Source: Factiva)
Panel B: M&A deal related variables	
Bidder CAR [-1,+1]	Bidder's three-day cumulative abnormal return around announcement date calculated using the one-factor market model. The market model parameters are estimated over the (-230, -31) trading days prior to the announcement date with value-weighted CRSP market index. (Source: CRSP)
Relative deal size	Deal value reported by SDC scaled by the bidder's market value of equity four days prior to the announcement. (Source: SDC, CRSP)
Unsolicited	Dummy variable equals one if the transaction is classified as unsolicited in the SDC, zero otherwise. (Source: SDC)
Hostile	Dummy variable equals one if the transaction is classified as hostile in the SDC, zero otherwise. (Source: SDC)

Continued on next page

Toehold	Bidder's ownership in the target prior to the merger announcement. (Source: SDC)
All cash deal	Dummy variable equals one for purely cash-financed transactions, zero otherwise. (Source: SDC)
All stock deal	Dummy variable equals one for purely equity-financed transactions, zero otherwise. (Source: SDC)
Diversifying deal	Dummy variable equals one if the acquirer and the target are not in the same Fama-French 49 industry, zero otherwise. (Source: SDC)
Panel C: Bidder firm level variables	
Firm size	The logarithm of book value of total assets. (Source: Compustat)
Tobin's Q	Market value of assets (book value of assets minus book value of equity plus market value of equity) over book value of assets. (Source: CRSP, Compustat)
Firm leverage	Book value of debts over market value of total assets. (Source: CRSP, Compustat)
Firm cash	Cash or cash equivalent scaled by the book value of total assets. (Source: Compustat)
Firm profitability	Net income scaled by the book value of total assets. (Source: Compustat)
Analysts	Number of analysts who make quarterly earnings forecasts for a given bidding firm in the quarter prior to the M&A announcement. (Source: I/B/E/S)
Institutional ownership	Percent of shares owned by institutional investors. (Source: Thomson-Reuters Institutional Holdings Database)
Illiquidity	Illiquidity measure defined following Amihud (2002): $1/N \sum_{t\in[m-2,m]} \frac{\|R_{i,t}\|}{VOLD_{i,t}}$, where R is the daily stock return, $VOLD$ is the daily dollar trading volume, N is the number of days with available trading data in the three months up to the M&A announcement month (month m). (Source: CRSP)
Panel D: Bidder board characteristics	
CEO age	Age of the acquirer CEO as reported in the proxy filing in the year of M&A announcement. (Source: proxy filings)
CEO founder	Indicator variable that equals one if the acquirer CEO is the founder of the firm. (Source: proxy filings)
Dual	Indicator variable that equals one if the acquirer CEO is also the Chairman of the Board. (Source: proxy filings)
Board size	The total number of directors reported in the annual proxy filing prior to the M&A announcement. (Source: proxy filings)
Classified board	Indicator variable that equals one if the bidder's board is staggered. (Source: proxy filings)

2. Machine learning sentiment classification

Machine learning (ML) sentiment analysis takes the input of WSJ articles, trains the classifier with the set of input articles ("training set"), and makes predictions based on the features learned from the training set. The classifier gives different weights to the sentiment words and can learn the specific styles from the WSJ news.

Specifically, I use the Naive Bayes classifier[20] from the Natural Language Toolkit (NTLK) package. To assist a supervised learning, I downloaded 11,669 articles of earnings news (excluding financial industry) that appeared in the WSJ from 1990 to 2016. The choice of earnings news is natural because these stories usually contain a clear positive or negative tone such that the classification becomes more objective and less subject to my personal interpretation. The drawback, however, is that features learned from this set of earnings articles might not exactly fit into the M&A articles.

I manually classify every article as "positive" or "negative." When the tone of an article is ambiguous, I exclude this article from the training set. The following examples of news titles illustrate the sentiment in the training set:

- **Negative article:** Cal-Maine Sales Fall 54%; Weak Egg Export Demand Creates Oversupply; Sales fell from $546 million a year ago to $253.5 million;
- **Positive article:** Zara Owner Inditex Posts Higher Profit; Sales accelerate as Christmas approaches;
- **Ambiguous** (dropped from the training set): Avis Reports Lower Profits but an Improved Outlook.

Based on the learned features, the classifier predicts whether an M&A article in my sample is negative. Specifically, the classifier gives a probability to each article of being "negative." This probability is used subsequently as my alternative measure of news negativity.

[20] The Naive Bayes classifier applies Bayes' theorem with strong independence assumptions between features.

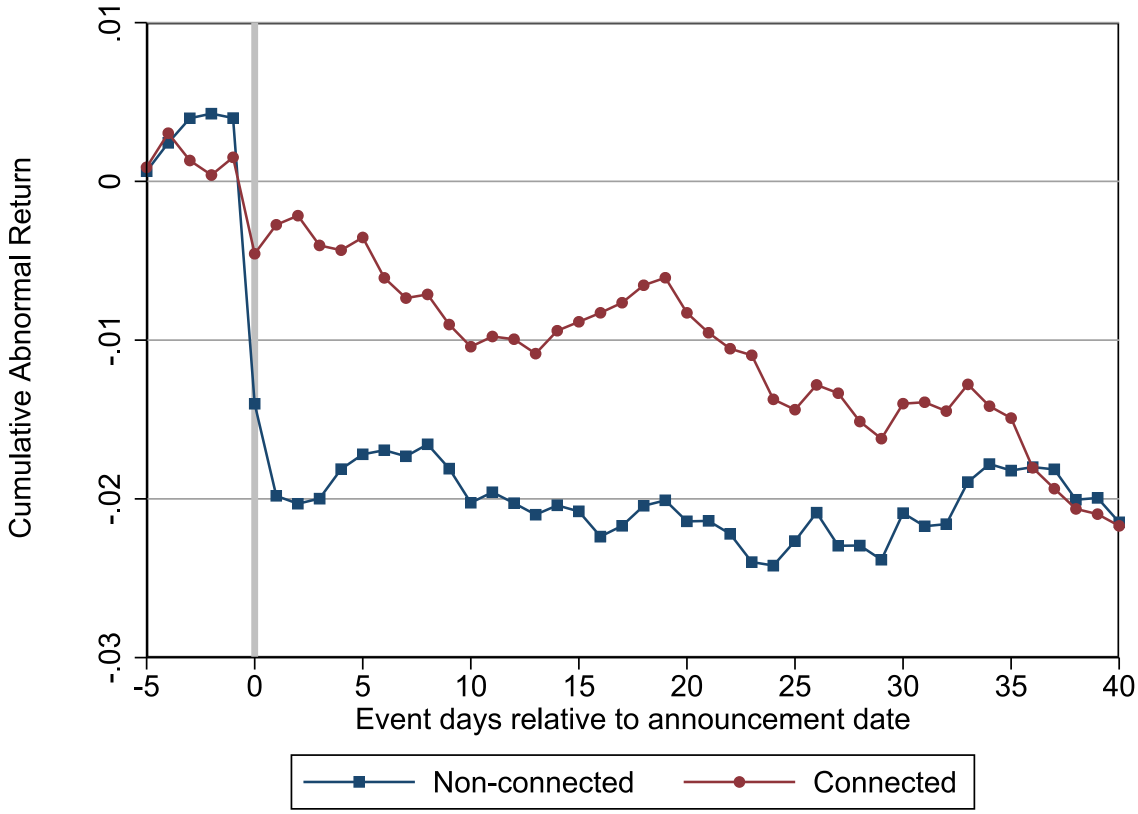

Figure 1: Bidder cumulative abnormal returns from day -5 through day 40 around the merger announcement.

The red line presents the bidders who are featured by a connected journalist ($CONNECT\_WORK$) on the *Wall Street Journal*, and the blue line presents the bidders who are covered by an independent journalist.

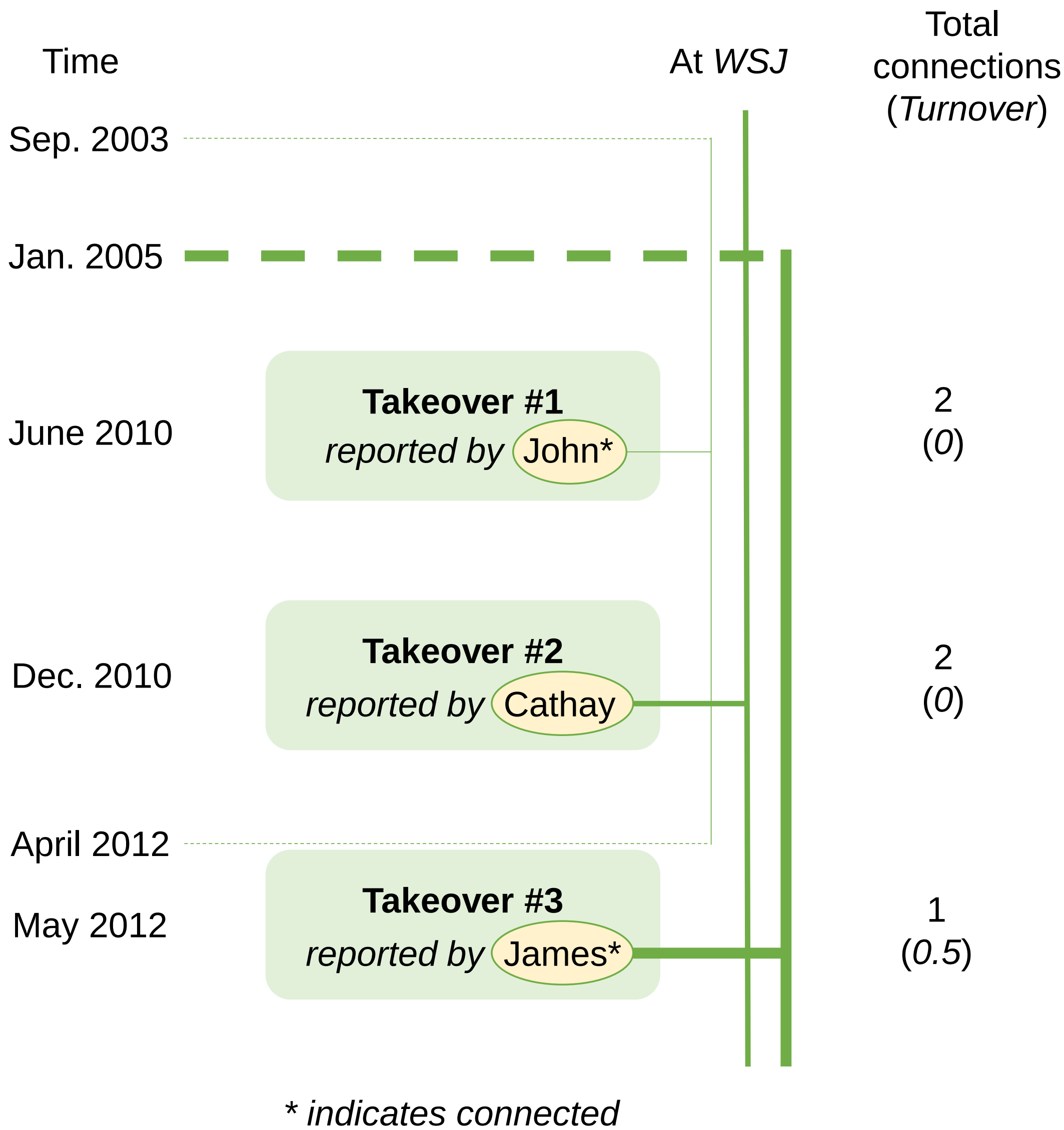

Figure 2: Coding connected-journalist turnover

Table 1: Sample M&A Articles by Calendar Time

This table lists the count of M&A news articles that appeared on the *Wall Street Journal* by year and by month. The column "SDC" presents the total number of M&A deals recorded by SDC which fulfill the sample selection criteria described in Section 2. The column "*WSJ* sample" presents the number of transactions covered by the *Wall Street Journal*. The column "Percent" presents the percentage of the "*WSJ* sample" out of the SDC sample.

	SDC	*WSJ* Sample	Percent *WSJ*/SDC
Panel A: Year			
1997	226	88	0.39
1998	252	100	0.40
1999	285	128	0.45
2000	206	95	0.46
2001	158	58	0.37
2002	80	14	0.18
2003	102	28	0.27
2004	97	41	0.42
2005	114	56	0.49
2006	107	55	0.51
2007	102	48	0.47
2008	79	38	0.48
2009	71	42	0.59
2010	81	46	0.57
2011	57	35	0.61
2012	58	34	0.59
2013	56	32	0.57
2014	73	59	0.81
2015	101	74	0.73
2016	85	60	0.71
Total	2,390	1,131	0.47
Panel B: Month			
January	162	64	0.40
February	190	92	0.48
March	187	88	0.47
April	200	94	0.47
May	233	113	0.48
June	234	115	0.49
July	205	107	0.52
August	199	93	0.47
September	157	73	0.46
October	228	98	0.43
November	199	97	0.49
December	196	97	0.49

Table 2: Summary Statistics

This table describes the summary statistics for the main variables used in the empirical tests. Definitions of these variables are in Appendix 1.

	N	mean	S.D.	p5	p25	p50	p75	p95
Journalist connections:								
CONNECT_WORK	1131	0.261	0.439	0	0	0	1	1
CONNECT_UNIVERSITY	1131	0.023	0.150	0	0	0	0	0
Deal characteristics:								
Bidder CAR[-1,1]	1131	-0.019	0.086	-0.174	-0.062	-0.010	0.025	0.111
Relative deal size	1131	0.538	0.707	0.008	0.082	0.293	0.753	1.835
Hostile	1131	0.030	0.171	0	0	0	0	0
Unsolicited	1131	0.047	0.211	0	0	0	0	0
Diversifying deal	1131	0.332	0.471	0	0	0	1	1
All cash deal	1131	0.316	0.465	0	0	0	1	1
All stock deal	1131	0.265	0.442	0	0	0	1	1
Completed	1131	0.895	0.307	0	1	1	1	1
Journalist and news variables:								
News negativity (%)	1131	1.396	0.868	0.247	0.820	1.245	1.852	2.947
Same city	1131	0.227	0.419	0	0	0	0	1
Female	1131	0.441	0.497	0	0	0	1	1
Journalist tenure (month)	1131	67	59	6	26	51	88	184
Number *WSJ* articles (log.)	1131	5.292	1.218	2.996	4.654	5.460	6.105	6.961
Industry expert	1131	0.477	0.500	0	0	0	1	1
Journalism degree	1131	0.781	0.414	0	1	1	1	1
Article length (wordcount)	1131	506	276	200	314	444	632	972
Firm characteristics:								
Firm size (log.)	1131	8.592	1.761	5.641	7.392	8.617	9.938	11.427
Tobin's Q	1131	2.764	2.949	1.079	1.408	1.868	2.802	7.604
Firm leverage	1131	0.149	0.134	0	0.043	0.116	0.222	0.420
Firm cash	1131	0.157	0.182	0.004	0.026	0.081	0.234	0.551
Firm profitability	1131	0.043	0.119	-0.109	0.022	0.052	0.094	0.173
Board characteristics:								
CEO age	1131	54	7.312	42	50	54	59	65
CEO founder	1131	0.131	0.337	0	0	0	0	1
Dual	1131	0.656	0.475	0	0	1	1	1
Board size (log.)	1131	2.280	0.278	1.792	2.079	2.303	2.485	2.708
Classified board	1131	0.447	0.497	0	0	0	1	1

Table 3: Journalist Connections and News Tone

OLS regressions of news negativity, using the sample of M&A news articles from *WSJ* as described in Table 1. The dependent variable, *News Negativity*, is measured as the fraction of negative words in the text of a news article. Columns 1 to 3 examine direct journalist-firm working relations; Columns 4 to 6 examine CEO-journalist educational network. Definitions for all variables are in Appendix 1. In all regressions, time (i.e. announcement year-month) and industry dummies are included. In columns 2 through 3, and 5 through 6, acquirer state and *WSJ* office dummies are additionally included. Standard errors, which are reported in parentheses, are double-clustered by deal year-month and by industry. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	News Negativity					
	1	2	3	4	5	6
CONNECT_WORK	-0.2077***	-0.2415***	-0.3284***			
	(0.064)	(0.081)	(0.081)			
CONNECT_UNIVERSITY				-0.1373***	-0.1946***	-0.1868**
				(0.037)	(0.070)	(0.085)
Female	-0.0455	-0.0191	-0.0196	-0.0521	-0.0317	-0.0345
	(0.042)	(0.049)	(0.052)	(0.045)	(0.056)	(0.063)
Jounalist tenure	-0.0002	-0.0004	-0.0004	-0.0001	-0.0003	-0.0003
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Number *WSJ* articles	0.0536*	0.0657*	0.0510	0.0388	0.0473	0.0342
	(0.030)	(0.035)	(0.039)	(0.031)	(0.037)	(0.040)
Industry expert	-0.0550	-0.0563	-0.0901	-0.0824	-0.0878	-0.1179
	(0.076)	(0.090)	(0.086)	(0.075)	(0.094)	(0.087)
Jounalism degree	0.0666	0.0721	0.0378	0.0635	0.0679	0.0414
	(0.066)	(0.061)	(0.063)	(0.064)	(0.059)	(0.059)
Same city	-0.0175	-0.0493	-0.0633	-0.0342	-0.0751	-0.0884
	(0.065)	(0.082)	(0.087)	(0.072)	(0.087)	(0.091)
Relative deal size	0.0406	0.0647	0.0647	0.0602	0.0846*	0.0640
	(0.045)	(0.044)	(0.050)	(0.045)	(0.046)	(0.051)
Toehold	0.0082	0.0102	0.0072	0.0065	0.0086	0.0059
	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.006)
Hostile	0.9803***	0.8866***	0.7430***	1.0115***	0.9240***	0.7929***
	(0.193)	(0.206)	(0.207)	(0.197)	(0.208)	(0.217)
Unsolicited	0.6305***	0.5851***	0.5732***	0.6363***	0.5949***	0.5801***
	(0.129)	(0.136)	(0.139)	(0.131)	(0.143)	(0.148)

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Diversifying deal	0.0264	0.0300	0.0334	0.0244	0.0312	0.0430
	(0.065)	(0.079)	(0.070)	(0.067)	(0.080)	(0.072)
All cash deal	0.1136*	0.1176*	0.1141	0.1122*	0.1186*	0.1383*
	(0.062)	(0.068)	(0.075)	(0.063)	(0.070)	(0.079)
All stock deal	0.0399	0.0203	0.0306	0.0577	0.0425	0.0538*
	(0.046)	(0.043)	(0.037)	(0.046)	(0.042)	(0.030)
Article length			0.0003***			0.0003***
			(0.000)			(0.000)
Firm size			0.0509			0.0095
			(0.035)			(0.036)
Tobin's Q			-0.0066			-0.0119
			(0.020)			(0.020)
Firm leverage			0.1912			0.2941
			(0.267)			(0.265)
Firm cash			0.2649			0.2545
			(0.376)			(0.415)
Firm profitability			-0.6646*			-0.5474
			(0.396)			(0.382)
CEO age			0.0053			0.0052
			(0.004)			(0.004)
CEO founder			-0.0602			-0.0550
			(0.075)			(0.070)
Dual			-0.1005			-0.1013
			(0.079)			(0.082)
Board size			-0.0406			-0.0445
			(0.169)			(0.168)
Classified board			-0.0595			-0.0638
			(0.073)			(0.072)
Constant	2.1575***	2.7731***	2.1415*	2.3282***	3.2382***	3.0483**
	(0.416)	(0.924)	(1.222)	(0.446)	(1.013)	(1.251)
Observations	1,131	1,131	1,131	1,131	1,131	1,131
R-squared	0.332	0.386	0.405	0.325	0.378	0.392
Time FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Acquirer State FE	NO	YES	YES	NO	YES	YES
WSJ Office FE	NO	YES	YES	NO	YES	YES

Table 4: Journalist Connections and Stock Returns

OLS regressions of bidders' cumulative abnormal returns (CAR), using the sample of M&A deals described in Table 1. Bidder CARs are calculated with a one-factor market model where the market returns are proxied by the value-weighted CRSP index. Market model parameters are estimated over a 200-day non-missing-value window ending 31 days before the announcement date. Columns 1 to 3 examine direct journalist-firm working relations; Columns 4 to 6 examine CEO-journalist educational network. Control variables are indicated in the last row and include: deal characteristics (D) (relative size, toehold, hostile, unsolicited, diversifying deal, all cash deal, all stock deal), firm characteristics (F) (firm size, Tobin's Q, leverage, cash, profitability), and board and CEO characteristics (B) (CEO age, CEO founder, dual, board size, classified board). Definitions for all variables are in Appendix 1. In all regressions, time (i.e. announcement year-month) and industry dummies are included. Standard errors, which are reported in parentheses, are double-clustered by deal year-month and by industry. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Bidder CAR[-1,1]					
	1	2	3	4	5	6
CONNECT_WORK	0.0248***	0.0210**	0.0001			
	(0.008)	(0.009)	(0.009)			
CONNECT_WORK×Front page			0.0298**			
			(0.015)			
CONNECT_UNIVERSITY				0.0020	0.0087	-0.0333
				(0.020)	(0.019)	(0.026)
CONNECT_UNIVERSITY×Front page						0.0797**
						(0.037)
Front page			-0.0046			0.0018
			(0.010)			(0.008)
Observations	1,131	1,131	1,131	1,131	1,131	1,131
R-squared	0.320	0.362	0.366	0.309	0.356	0.360
Time FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Controls	NO	D, F, B	D, F, B	NO	D, F, B	D, F, B

Table 5: 2-Stage Least Squares Regressions

Two-stage least squares (2SLS) regressions showing the relation between instrumented *CONNECT_WORK* and news negativity and bidder's CAR. The sample includes bidders who have conducted at least 2 M&A deals and who have at least one journalist connection in terms of working relationship. In the first stage (column 1), *CONNECT_WORK* is instrumented by the connected-journalist turnover in the quarter of M&A announcement. In the second stage, *news negativity* or *bidder CAR* is regressed on the instrumented *CONNECT_WORK*. Columns 2 and 3 examine the effect on news negativity, where the results of OLS regression without instrumenting are reported in column 2, and the second stage 2SLS results are shown in column 3. Columns 4 and 5 examine the effect on bidder's returns, where the results of OLS regression are reported in column 4, and the second stage 2SLS results are presented in column 5. Control variables include: deal variables (D) (relative size, toehold, hostile, unsolicited, diversifying deal, all cash deal, all stock deal), firm characteristics (F) (firm size, Tobin's Q, leverage, cash, profitability), board and CEO characteristics (B) (CEO age, CEO founder, dual, board size, classified board). Definitions for all variables are in Appendix 1. In all regressions, time (i.e. announcement year) and industry dummies are included. Standard errors, which are reported in parentheses, are double-clustered by deal year and by industry. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	CONNECT_WORK	**News Negativity**		**Bidder CAR[-1,1]**	
	1	2	3	4	5
	OLS	OLS	IV	OLS	IV
Journalist turnover	-1.0329***				
	(0.261)				
CONNECT_WORK		-0.3623***	-0.7026***	0.0123**	0.0480**
		(0.076)	(0.107)	(0.005)	(0.022)
Observations	321	321	321	321	321
R-squared	0.200	0.357	0.326	0.372	0.327
Time FE	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES
Controls	D, F, B	D, F, B	D, F, B	D, F, B	D, F, B

Table 6: Additional Evidence: WSJ's acquisition by News Corp

This table reports the results of difference-in-difference analysis, using a sample of repetitive acquirers who have conducted M&A deals before and after 2008. Variable *Murdoch* is a dummy variable that equals one if a bidder is connected to News Corp. or Mr. Rupert Murdoch. Variable *Post* takes value of one for years from 2008 onward. Panel A includes all deals. Panel B excludes transactions in the Entertainment and Publishing (Printing) sectors (based on Fama-French 48 industry codes). Control variables include: deal variables (D) (relative size, toehold, hostile, unsolicited, diversifying deal, all cash deal, all stock deal), firm characteristics (F) (firm size, Tobin's Q, leverage, cash, profitability), board and CEO characteristics (B) (CEO age, CEO founder, dual, board size, classified board). Definitions for all variables are in Appendix 1. In all regressions, time (i.e. announcement year), industry, acquirer state, and *WSJ* office dummies are included. Standard errors are reported in parentheses and are double-clustered by year and by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A. All available transactions

	News Negativity			**Bidder CAR[-1,1]**		
	1	2	3	4	5	6
Murdoch×Post	-0.8578***	-0.9228***	-0.9240***	0.0188*	0.0327**	0.0340**
	(0.228)	(0.242)	(0.186)	(0.011)	(0.014)	(0.014)
Murdoch	0.4998***	0.5008***	0.4477**	-0.0113	-0.0292*	-0.0318*
	(0.182)	(0.075)	(0.191)	(0.018)	(0.017)	(0.018)
Observations	392	392	392	392	392	392
R-squared	0.311	0.368	0.389	0.366	0.436	0.444
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Acquirer State FE	YES	YES	YES	YES	YES	YES
WSJ Office FE	YES	YES	YES	YES	YES	YES
Controls	NO	D, F	D, F, B	NO	D, F	D, F, B

Panel B. Excluding M&As in Entertainment and Publishing (Printing) sectors

	News Negativity			**Bidder CAR[-1,1]**		
	1	2	3	4	5	6
Murdoch×Post	-0.9128***	-0.9010***	-0.9332***	0.0239***	0.0315**	0.0336**
	(0.200)	(0.249)	(0.142)	(0.009)	(0.014)	(0.013)
Murdoch	0.5604***	0.4998***	0.4634**	-0.0124	-0.0247	-0.0282
	(0.130)	(0.095)	(0.182)	(0.019)	(0.018)	(0.018)
Observations	385	385	385	385	385	385
R-squared	0.312	0.368	0.389	0.327	0.401	0.411
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Acquirer State FE	YES	YES	YES	YES	YES	YES
WSJ Office FE	YES	YES	YES	YES	YES	YES
Controls	NO	D, F	D, F, B	NO	D, F	D, F, B

Table 7: Interaction Effects on Stock Returns

This table reports the results on interaction effects between journalist connection (*CONNECT_WORK*) and proxies for the arbitrage opportunity. The sample includes M&A deals covered by the *Wall Street Journal* described in Table 1. The dependent variable is bidders' cumulative abnormal returns (CAR). Proxies for the arbitrage opportunity include: analysts, firm size, institution ownership, and illiquidity using Amihud (2002)'s measure. Control variables include: deal variables (D) (relative size, toehold, hostile, unsolicited, diversifying deal, all cash deal, all stock deal), firm characteristics (F) (firm size, Tobin's Q, leverage, cash, profitability), board and CEO characteristics (B) (CEO age, CEO founder, dual, board size, classified board). Definitions for all variables are in Appendix 1. In all regressions, time (i.e. announcement year-month) and industry dummies are included. Standard errors are reported in parentheses and are double-clustered by deal year-month and by industry. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Bidder CAR[-1,1]			
	1	2	3	4
CONNECT_WORK×Analysts	-0.0016***			
	(0.001)			
CONNECT_WORK×Firm size		-0.0206***		
		(0.006)		
CONNECT_WORK×Instown.			-0.0555**	
			(0.026)	
CONNECT_WORK×Iliquidity				0.1421***
				(0.020)
CONNECT_WORK	0.0503***	0.2138***	0.0539***	0.0178**
	(0.016)	(0.056)	(0.018)	(0.008)
Analysts	0.0008*			
	(0.000)			
Firm size		0.0036*		
		(0.002)		
Instown.			0.0005	
			(0.000)	
Iliquidity				-0.0069
				(0.005)
Observations	1,131	1,131	1,131	1,131
R-squared	0.369	0.382	0.366	0.376
Time FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Controls	D, F, B	D, F, B	D, F, B	D, F, B

Table 8: Additional Evidence: Price Reversals

OLS regressions of bidders' cumulative abnormal returns (CAR). In Panel A, I use the full sample of acquisitions as described in Table 1. In Panel B, I exclude 119 withdrawn transactions. Panel C further excludes 342 observations in which the bidder and/or target firms have potentially confounding news over the [2,40] window. Columns 1 and 2 replicate the results in columns 1 and 2 of Table 4. The dependent variable in columns 3 and 4 is bidders' CAR over [2,40]. The dependent variable in columns 5 and 6 is bidders' CAR over [-1,40]. Control variables are included in the even-numbered columns: deal variables (D) (relative size, toehold, hostile, unsolicited, diversifying deal, all cash deal, all stock deal), firm characteristics (F) (firm size, Tobin's Q, leverage, cash, profitability), board and CEO characteristics (B) (CEO age, CEO founder, dual, board size, classified board). Definitions for all variables are in Appendix 1. In all regressions, time (i.e. announcement year-month) and industry dummies are included. Standard errors are reported in parentheses and are double-clustered by deal year-month and by industry. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A. Full sample

	CAR[-1,1]		**CAR[2,40]**		**CAR[-1,40]**	
	1	2	3	4	5	6
CONNECT_WORK	0.0248***	0.0210**	-0.0206*	-0.0326**	0.0043	-0.0117
	(0.008)	(0.009)	(0.011)	(0.013)	(0.014)	(0.017)
Observations	1,131	1,131	1,131	1,131	1,131	1,131
R-squared	0.320	0.362	0.240	0.259	0.242	0.267
Time FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Controls	NO	D, F, B	NO	D, F, B	NO	D, F, B

Panel B. Excluding withdrawn deals

	CAR[-1,1]		**CAR[2,40]**		**CAR[-1,40]**	
	1	2	3	4	5	6
CONNECT_WORK	0.0202**	0.0176*	-0.0308**	-0.0363***	-0.0110	-0.0191
	(0.008)	(0.009)	(0.012)	(0.013)	(0.015)	(0.016)
Observations	1,012	1,012	1,012	1,012	1,012	1,012
R-squared	0.341	0.380	0.251	0.261	0.252	0.270
Time FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Controls	NO	D, F, B	NO	D, F, B	NO	D, F, B

Panel C. Excluding withdrawn deals and observations with confounding news over $[2, 40]$

	CAR[-1,1]		**CAR[2,40]**		**CAR[-1,40]**	
	1	2	3	4	5	6
CONNECT_WORK	0.0222**	0.0257**	-0.0451**	-0.0504***	-0.0230	-0.0245
	(0.009)	(0.013)	(0.019)	(0.019)	(0.020)	(0.024)
Observations	670	670	670	670	670	670
R-squared	0.459	0.509	0.331	0.366	0.349	0.377
Time FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Controls	NO	D, F, B	NO	D, F, B	NO	D, F, B

Internet Appendix

1. Examples of positive versus negative news coverage:

The following excerpts illustrate two WSJ articles that carry an overall “positive” tone:

— FPL Group’s acquisition of Constellation Energy announced on December 18, 2005 (news titled: FPL, Constellation Reach Agreement On $11 Billion Deal):

“The deal enables Constellation to benefit from FPL’s stronger, A-grade credit rating and lower risk profile, which should help it continue to expand a power-trading business that increasingly competes against the A-rated operations of investment banks including Goldman Sachs Group Inc., Bear Stearns Cos. and Morgan Stanley.

FPL, in turn, will garner more unregulated assets able to capitalize on growing profit margins in lucrative markets including California, Texas and New England.”

— Brooks Automation’s acquisition of Helix Technology announced on July 11, 2005 (news titled: Brooks to Acquire Helix For $454 Million in Stock):

“Brooks Automation Inc.’s plans to acquire Helix Technology Corp. for about $454 million in stock are designed to bolster business from semiconductor makers by offering them a broader array of manufacturing tools from a single supplier, Edward Grady, Brooks’ president and chief executive, told Dow Jones in an interview.

“The whole premise of this is very strategic,” Mr. Grady said, adding that the combination of the two companies is “totally complementary.””

The next two excerpts, on the contrary, have an overall “negative” tone:

— Gannett’s acquisition of Central Newspapers announced on June 28, 2000 (news titled: Gannett Agrees To Buy Central Newspapers):

“Unlike Gannett’s other deals, which have been broadly welcomed by Wall Street, the proposed acquisition of Central poses some risks. Because of the deal’s accounting structure, it will reduce Gannett’s earnings per share by 7% in 2001, something Gannett executives like to avoid when crafting acquisitions because it upsets the company’s typically conservative investors. Not including the accounting charges, Gannett says the deal will add to its cash earnings this year.”

— Pfizer’s acquisition of Wyeth announced on January 26, 2009 (news titled: Pfizer Deal to Buy Wyeth Leaves Doubts):

“Pfizer Inc. hailed its planned $68 billion takeover of rival Wyeth as an ideal combination, but analysts say the deal will only partially solve some of the New York drug giant’s long-term problems.

The revenues generated by Wyeth’s most attractive products, such as pediatric vaccine Prevnar, won’t be sufficient to make up for the loss of $12.4 billion in annual revenues Pfizer faces when the patent on its anticholesterol drug Lipitor expires in 2011, analysts said.

And some of them expressed doubts about how the newly created behemoth, with a combined $71 billion in revenues, would discover enough new products to generate growth. “Moving that needle is going to be extraordinarily difficult,” said Timothy Anderson, a health-care analyst at Sanford Bernstein.”

Table A1: Top journalists

The list of *Wall Street Journal* reporters who appear most frequently in the sample.

Reporter name	Total # articles in sample
Don Clark	48
Steven Lipin	48
Dennis K. Berman	42
Nikhil Deogun	42
Tess Stynes	26
Dana Cimilluca	20
Dana Mattioli	20
Jonathan Rockoff	19
Rebecca Smith	18
Robin Sidel	17
Anupreeta Das	16
Chelsey Dulaney	16
Stephanie N. Mehta	14
Thomas M. Burton	14
Charles Forelle	13
Kathryn Kranhold	13
Josh Beckerman	12
Russell Gold	12
William M. Bulkeley	12
Alison Sider	11
Carlos Tejada	11
Dean Takahashi	11
Liz Hoffman	11
Rhonda L. Rundle	11

Table A2. Correlations of Journalist Connections and Firm Characteristics

OLS regressions of *CONNECTION* on firm and/or board characteristics. Definitions for all variables are listed in Appendix 1. A constant term is estimated but not reported. Standard errors are reported in parentheses and are clustered by industry. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	CONNECT_UNIVERSITY			CONNECT_WORK		
	1	2	3	4	5	6
Firm size	0.0004	0.0008	0.0016	0.1071***	0.1103***	0.0988***
	(0.004)	(0.004)	(0.005)	(0.016)	(0.016)	(0.017)
Institution ownership	0.0038	0.0044	0.0051	-0.1236*	-0.1198	-0.1167
	(0.025)	(0.024)	(0.025)	(0.072)	(0.073)	(0.070)
# Analysts	0.0001	0.0000	0.0000	0.0033	0.0030	0.0033
	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.003)
Tobin's Q	0.0010	0.0001	0.0001	0.0145***	0.0114**	0.0110**
	(0.001)	(0.001)	(0.001)	(0.005)	(0.005)	(0.004)
Firm leverage	0.0267	0.0282	0.0260	-0.4413***	-0.4161**	-0.3906**
	(0.030)	(0.026)	(0.024)	(0.147)	(0.153)	(0.151)
Firm cash	0.0312***	0.0292**	0.0204	-0.0221	-0.0419	-0.0090
	(0.011)	(0.012)	(0.013)	(0.111)	(0.105)	(0.106)
Firm profitability	-0.0130	-0.0037	-0.0019	-0.3720***	-0.2958**	-0.2925**
	(0.032)	(0.043)	(0.046)	(0.122)	(0.117)	(0.121)
New York	0.0346*	0.0333*	0.0343*	0.0944**	0.0859**	0.0841**
	(0.019)	(0.018)	(0.018)	(0.037)	(0.038)	(0.040)
California	0.0097	0.0085	0.0065	0.0868**	0.0797**	0.0852**
	(0.013)	(0.012)	(0.011)	(0.034)	(0.036)	(0.038)
Mean returns [-335, -30]		1.5506	1.5834		-1.2207	-1.1684
		(2.101)	(2.146)		(3.839)	(3.770)
Volatility of returns		0.3241	0.3067		1.9185**	1.9456**
		(0.392)	(0.384)		(0.813)	(0.835)
Skewness of returns		-0.0025	-0.0025		0.0066	0.0063
		(0.002)	(0.002)		(0.012)	(0.013)
CEO age			-0.0003			-0.0000
			(0.001)			(0.002)
CEO founder			0.0118			-0.0289
			(0.031)			(0.027)
Board size			-0.0101			0.1085**
			(0.017)			(0.048)
Classified board			-0.0074			-0.0065
			(0.006)			(0.014)
Observations	1,131	1,131	1,131	1,131	1,131	1,131
R-squared	0.009	0.011	0.012	0.234	0.239	0.242

Table A3. Robustness – Journalist Connections and News Tone

Panel A controls for additional journalist and firm characteristics. Columns 1 and 2 include journalist fixed effects. Columns 3 and 4 include bidder's advertising expenses*. Columns 5 and 6 include target characteristics (size, Q, leverage, cash, profitability and runup). In Panel B, I use alternative measures of *negativity*. Columns 1 and 2 measure negativity by log(#negative words). Columns 3 and 4 use the probability of a negative article given by the Naive Bayes Classifier (machine learning). In Panel C, I use a refined measure of *CONNECT_WORK*, where *CONNECT_WORK&CITE* flags journalists with working relationships and have cited the acquirer CEO in their reports. In all regressions, time (i.e. announcement year-month), industry, acquirer state, and/or *WSJ* office dummies are included. Control variables are indicated in the last row: journalist characteristics (J) (female, journalist tenure, number WSJ articles, industry expert, journalism degree, same city, article length), deal variables (D) (relative size, toehold, hostile, unsolicited, diversifying deal, all cash deal, all stock deal), bidder characteristics (F) (size, Tobin's Q, leverage, cash, profitability), and board/CEO characteristics (B) (CEO age, CEO founder, dual, board size, classified board). Definitions of all variables are in Appendix 1. Standard errors, which are in parentheses, are double-clustered by deal year-month and by industry. Symbols ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A. Controls for unobserved journalist style and additional firm characteristics

	Control for journalist FE		Control for advertising expenses*		Control for target characteristics	
	1	2	3	4	5	6
CONNECT_WORK	-0.2533**		-0.3272***		-0.3110***	
	(0.128)		(0.082)		(0.083)	
CONNECT_UNIVERSITY		-0.3194**		-0.1900**		-0.1703*
		(0.150)		(0.081)		(0.098)
Observations	979	979	1,131	1,131	1,115	1,115
R-squared	0.628	0.624	0.405	0.392	0.413	0.402
Time FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Acquirer state FE	YES	YES	YES	YES	YES	YES
WSJ Office FE	YES	YES	YES	YES	YES	YES
Journalist FE	YES	YES	NO	NO	NO	NO
Controls	J,D,F,B	J,D,F,B	J,D,F,B, Ad. exp.	J,D,F,B, Ad. exp.	J,D,F,B, Tgt. char.	J,D,F,B, Tgt. char.

Continued on next page

Panel B. Alternative measures of news negativity

	# Negative words		Machine learning: Prob(Negative)	
	1	2	3	4
CONNECT_WORK	-0.2584***		-0.0602*	
	(0.053)		(0.035)	
CONNECT_UNIVERSITY		-0.1824*		-0.1568*
		(0.099)		(0.094)
Observations	1,087	1,087	1,131	1,131
R-squared	0.668	0.659	0.385	0.386
Time FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Acquirer state FE	YES	YES	YES	YES
WSJ Office FE	YES	YES	YES	YES
Control	J, D, F, B	J, D, F, B	J, D, F, B	J, D, F, B

Panel C. Alternative measure of "CONNECT_WORK"

CONNECT_WORK&CITE	-0.2047***	-0.2418***	-0.3145***
	(0.065)	(0.079)	(0.081)
Observations	1,131	1,131	1,131
R-squared	0.331	0.385	0.403
Time FE	YES	YES	YES
Industry FE	YES	YES	YES
Acquirer state FE	NO	YES	YES
WSJ Office FE	NO	YES	YES
Control	J, D	J, D	J, D, F, B

* *Note: Advertising expenses are set to zero for firms with missing values from Compustat. This approach follows, e.g., Fich et al. (2016), though potential measurement errors may exist.*

Table A4: News Tone and Stock Returns

OLS regressions of bidders' cumulative abnormal returns (CAR) on news negativity, using the M&A sample described in Table 1. The following control variables are included in column 2: deal variables (D) (relative size, toehold, hostile, unsolicited, diversifying deal, all cash deal, all stock deal), firm characteristics (F) (firm size, Tobin's Q, leverage, cash, profitability), board and CEO characteristics (B) (CEO age, CEO founder, dual, board size, classified board). Definitions for all variables are in Appendix 1. In all regressions, time (i.e. announcement year) and industry dummies are included. Standard errors are reported in parentheses and are double-clustered by deal year and by industry. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Bidder CAR[-1,1]	
	1	2
News negativity	-0.0018***	-0.0032***
	(0.000)	(0.001)
Observations	1,131	1,131
R-squared	0.118	0.166
Time FE	YES	YES
Industry FE	YES	YES
Controls	NO	D, F, B

Table A5: Reduced-form Regressions of News Tone/Stock Returns on Journalist Turnover

This table reports the reduced-form OLS regressions of news negativity (columns 1–2) and bidder's CAR (columns 3–4) on the connected journalists' turnover, previously used as an instrument variable in Table 5. The sample and the definition of variables are the same as those in Table 5. In all regressions, time (i.e. announcement year) and industry dummies are included. Standard errors, which are reported in parentheses, are double-clustered by deal year and by industry. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	News Negativity		**Bidder CAR[-1,1]**	
	1	2	3	4
Journalist turnover	0.7733***	0.7258***	-0.0667***	-0.0495**
	(0.190)	(0.263)	(0.022)	(0.021)
Observations	321	321	321	321
R-Squared	0.164	0.326	0.172	0.369
Time FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Controls	NO	D, F, B	NO	D, F, B

Table A6: Journalist Connections and Deal Outcomes

This table examines whether journalist connections are related to various M&A deal outcomes, using the sample described in Table 1. Columns 1 to 2 examine targets' cumulative abnormal returns (CAR) over [-1,1]. Target CARs are similarly calculated as bidder CARs described in Table 4. Columns 3 and 4 examine 4-week premium, defined as offer price over target's market capitalization 4 weeks prior to the announcement. Columns 5 and 6 examine return premium, defined as target CAR over [-42,5]. Columns 7 and 8 examine the consummation of the deal, and columns 9 and 10 examine whether the deal is rumored. Regressions 1 through 6 are estimated with OLS, and 7 through 10 are estimated with logit models. In columns 7 to 10, marginal effects of the coefficient estimates are reported in the brackets. Control variables include: deal variables (D) (relative size, toehold, hostile, unsolicited, diversifying deal, all cash deal, all stock deal), firm characteristics (F) (firm size, Tobin's Q, leverage, cash, profitability), board and CEO characteristics (B) (CEO age, CEO founder, dual, board size, classified board), and target characteristics (T) (Target size, Tobin's Q, leverage, cash, profitability, target stock runup). Definitions for all variables are in Appendix 1. In all regressions, time (i.e. announcement year-month) and industry dummies are included. Standard errors are reported in parentheses and are double-clustered by deal year-month and by industry. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Target CAR[-1,1]		**4-week premium**		**Return premium: Target CAR[-42,5]**		**Consummation**		**Rumored**	
	1	2	3	4	5	6	7	8	9	10
	OLS	OLS	OLS	OLS	OLS	OLS	Logit	Logit	Logit	Logit
CONNECT_WORK	0.0251		1.1286		0.0207		-0.0076		0.8073	
	(0.017)		(5.386)		(0.017)		(0.540)		(0.533)	
							[-0.001]		[0.099]	
CONNECT_UNIVERSITY		0.0046		-13.6002		0.0189		-0.6179		0.8608
		(0.063)		(9.895)		(0.064)		(0.710)		(1.697)
								[-0.044]		[0.103]
Observations	1,115	1,115	1,129	1,129	1,115	1,115	517	517	426	426
R-squared	0.427	0.426	0.375	0.376	0.419	0.419	0.527	0.527	0.359	0.352
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	D, T	D, T	D, F, B	D, F, B	D, F, B	D, F, B	D, F, B	D, F, B	D, F, B	D, F, B