Friends in Media

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Abstract

We test the independence of news content from journalists' social networks. We find that business news reported by connected journalists—such as those sharing a working relationship or common schooling institutions with the respective company management—are associated with markedly more favorable coverage. Connected articles significantly increase short-term stock returns but also distort longer-term capital allocation, suggesting real effects of journalist connections in the economy. We make causal inferences about the connection effects by exploring exogenous journalist turnovers and an ownership change of the *Wall Street Journal*.

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Keywords: media slant, social networks, financial journalism, real effects

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"What the blurbs did not mention was that each man was praising the work of a sometime boss. [...] [The journalists] have since written positively about Lord Black in their columns, though without mentioning their business dealings."

Jacques Steinberg and Geraldine Fabrikant
New York Times, December 22, 2003

The importance of independent media has spurred extensive research into both the extent and sources of media bias. Previous studies indicate that companies wield influence over editorial content with corporate money, such as advertising expenditures (Reuter and Zitzewitz, 2006; Gurun and Butler, 2012). Official press management represents, however, just a fraction of the influence a company exerts on media. Considerable influence is often built behind the scenes, for example, via personal social networks. Indeed, the *New York Times*' (*NYTimes*) newsroom policy cautions that "staff members [...] must be sensitive that personal relationships with news sources can erode into favoritism."[1] A 2003 *NYTimes* article bluntly described how personal connections between two reporters and Hollinger Incorporated led to positive slant on Hollinger's owner, Conrad Black (see the opening quote).[2] Despite the economic importance of business press in the economy, there is little systematic evidence on whether journalists' social networks affect the independence of media content.[3]

In this paper, we attempt to explore whether firm–journalist connections lead to media bias. Moreover, we study the real effects of the bias on (1) asset prices and (2) corporate investment outcomes. Previous research provides two predictions on how a journalist's firm connections could affect media slant. On the one hand, if business media compete using accuracy for rational economic agents, a well-connected journalist could yield more credible reporting by catalyzing the information flow. As Kirkpatrick and Fabrikant (2003) note, "Any company has to sell the credibility of its product, but a media company has nothing else to sell." On the other hand, reporters connected to a firm might have incentives to inject their personal

[1] Similarly, *Reuters*' handbook of journalism states that "while it is appropriate to entertain sources, including outside working hours, regularly spending substantial leisure time with them may raise [...] a perception of bias."

[2] See "Friendship and Business Blur In the World of a Media Baron," the *New York Times*.

[3] Two related papers are Gurun (2020) and Ru et al. (2020), who study board members' media expertise. As we review below, the economic channels through which a company's media directors influence coverage are distinct from that of a business reporter's social networks.

opinions and slant the story in favor of the firm because they rely on company management for information (Dyck and Zingales, 2003). Moreover, there is evidence of a positive bias in social networks due to "homophily," a term that refers to an affinity for similar others (McPherson, Smith-Lovin and Cook, 2001; Jackson, 2014).

We assess these predictions by assembling a unique set of financial articles published in the *Wall Street Journal* (*WSJ*) and the *NYTimes*.[4] We focus on one type of business news: mergers and acquisitions (M&As). M&As are a good laboratory in which to test for slant in business press for three reasons. First, the assessment of synergies in M&A deals is subject to individual perspectives. Gentzkow and Shapiro (2006) predict more media bias in events in which outcomes (e.g., investment synergies) are difficult to verify. Second, M&A bidding process comprises a series of observable stages. The availability of detailed data enables us to examine both the short-term effects of bias, such as stock market reactions, and real economic consequences, such as bid prices, competition, and consummation. Finally, acquisitions are the most important form of corporate investment, eliciting substantial public attention. Hence, any effects due to distortions in media stories are of first-order importance in the economy.

Our first set of analysis focuses on media slant in connected stories. We examine connections by university ties and working relationships. We obtain from major professional network websites the college names of the acquirers' CEOs and of the authors of the news articles. These allow us to observe whether, for instance, John Riccitiello, Chief Executive of Electronic Arts, attended the same college attended by the *WSJ* reporter, Nick Wingfield, who wrote about the Electronic Arts' acquisition of Take-Two in 2008. Additionally, we capture the potential working relationships between a firm and a journalist by examining if a specific reporter wrote multiple exclusive stories about that firm during the 12 past months. This approach follows the logic in Solomon (2012) and is based on the idea that journalists who frequently cover a firm are more likely to build a personal relationship with its employees.

We find a negative correlation between a journalist's connections and the use of negative

[4] The *WSJ* and the *NYTimes* are the largest and third-largest print newspapers in the United States. The average weekday circulation was around 2 million for both newspapers in 2013, according to the Alliance for Audited Media. The second-largest newspaper, *USA Today*, is a middle-market newspaper and therefore not included in our study (as in Reuter and Zitzewitz, 2006). The *WSJ* is extremely well-established among investment professionals. Prior studies (e.g., Roll, 1988; Pound and Zeckhauser, 1990; Mitchell and Mulherin, 1994; Tetlock, 2007; Tetlock, Saar-Tsechansky and Macskassy, 2008; Dougal et al., 2012) reveal that *WSJ* coverage has a significant impact on the stock market.

words (as defined by Loughran and McDonald (2011, 2016)). This result obtains in analyses in which we pool articles describing the *same* event and control for deal fixed effects and observable journalist characteristics. Here, deal fixed effects allow us to effectively compare slant on the same underlying event without the concern of reverse causation, namely, that connections are determined by deal synergies. For example, suppose we find that Dow Chemical's bid for DuPont received a more positive coverage in the *WSJ* than in the *NYTimes*. After controlling for deal fixed effects, the difference in slant is solely identified over Dow Chemical's journalist tie in the *WSJ* (but not in the *NYTimes*). This result gives us a strong indication that social networks influence slant in news coverage.

Next, we exploit two settings to propose a plausibly causal interpretation. In the first setting, we instrument the likelihood of connected coverage using turnover among a firm's media connections. The rationale is as follows. To return to the example of Electronic Arts, suppose the firm has access to one connected reporter. Shortly before Electronic Arts announces its bid, that reporter leaves the *WSJ*; therefore, the likelihood of this bid being covered by a friendly reporter becomes virtually zero. Importantly, journalist turnover is likely due to events exogenous to M&As or the firms they write about (Solomon, 2012). The two-stage least-squares (2SLS) regression results continue to support a connection bias. Moreover, they reject an alternative explanation that bias is driven by readers' demands.

The second experiment we explore is an exogenous change of the *WSJ*'s owner. In 2007, the *WSJ* was taken over by News Corp. The takeover represents an ideal laboratory in which to examine how news presentation changes for firms connected to the new media owner, Rupert Murdoch (CEO of News Corp). Prior to the takeover, the *WSJ* actually questioned whether the newspaper could retain its journalistic independence under the new owner:[5]

> 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 [...].

Important to our analysis, the ownership change is independent of journalists' writing styles but has a direct impact on the connections between firms and reporters (think of second-degree connections through the media owner). We collect firm connections to Murdoch, such as busi-

[5] See the *WSJ*, "Murdoch's Surprise Bid: $5 Billion for Dow Jones," by Berman and Ellison, May 2, 2007.

ness ties and shared board seats, and find that connected firms are associated with significantly more favorable coverage following Murdoch's takeover of the *WSJ* (but not before).

Having established the link between slant and connections, our next set of tests examines the real effects on capital allocation. For connection bias to affect investment outcomes, at least some set of market participants (e.g., investors, managers) must react to it. We first examine stock market reactions. We find a positive relation between journalist connections and abnormal stock returns upon news publication. This effect is mostly observed for the articles in the *WSJ*, probably because the *WSJ* is the leading business newspaper in the United States. While the correlation here is subject to the same concerns of endogeneity, we find the same result of stock returns by instrumenting the connection with journalist turnover. This, again, leads us to conclude that the most plausible explanation is the causal one, namely, that journalist ties drive more positive stock responses. Similar results are obtained using Murdoch's acquisition of the *WSJ* as a shock to journalistic independence.

Exploring the mechanism underlying the impacts on stock returns, we find three channels that are consistent with theoretical predictions, namely, arbitrage opportunities, stock liquidity, and investor attention (DeLong et al., 1990; Campbell, Grossman and Wang, 1993; Barber and Odean, 2008). Moreover, we show that short-term positive reactions to optimistic news stories are reversed in the long run: After 40 trading days, the documented higher returns to connected bidders eventually converge to levels similar to those of non-connected firms. The evidence of price correction clearly rejects the alternative explanation of an information advantage possessed by connected journalists. Under this alternative hypothesis, a connected journalist writes more accurately, predicting a permanent pricing of information.

Despite the price correction, the media bias has real implications for the bidding process. We show that connected media stories distort the public takeover bids because bid competition increases significantly following the connected publications. The initial (connected) bidder is also more likely to revise their offer price upward after the bid announcement. Finally, we find that some initial bidders withdraw from the auction after connected media coverage. This evidence is in line with the predictions from the models of salience and inattention (Chetty, Looney and Kroft, 2009; DellaVigna, 2009), in which non-fundamental information distorts people's behavior. Our contribution is to show that this type of heuristic thinking also manifests in the competitive M&A bidding and affects efficient capital allocation.

Our last set of tests assesses the external validity of our results. We extend the analysis to a different context: coverage of financial fraud. Like M&A bids, fraud is an important event, in which a friendly reporter could help the charged firms deflect the public blame. The results of this validation confirm the connection bias and its impact on the market response.

Our paper contributes to the literature on financial media in two ways. First, we show that business reporters' personal networks are important for the variation in media slant. Closely related is the work on official media spin using advertising expenditures and investor relations services (Reuter and Zitzewitz, 2006; Gurun and Butler, 2012; Solomon, 2012; Ahern and Sosyura, 2014). DellaVigna and Hermle (2017) examine ownership-related bias in movie reviews but do not find such a conflict of interest. Our findings of strong individual-level bias are surprising given its prevalence in the most reputable newspapers (i.e., *WSJ* and *NYTimes*). Dougal et al. (2012) recognize the individual fixed effects of business reporters. In this regard, our research also sheds some light on the potential channels of explaining the so-called journalist fixed effects.

Second, we expand the study on the real economic impacts of media. Most related work examines the impacts on stock returns.[6] We go beyond the study of the stock market to investigate the impacts on capital allocation and draw causal inferences. In this regard, our findings complement the studies documenting the real effects of media on corporate governance (Dyck and Zingales, 2002; Dyck, Volchkova and Zingales, 2008; Liu and McConnell, 2013).

Finally, our paper adds to the literature of social networks in economics.[7] More recent work has examined the effects of companies' media directors (Di Giuli and Laux, 2020; Gurun, 2020; Ru et al., 2020). In an M&A setting, Hossain and Javakhadze (2020) study the role of media directors in deal initiation and negotiation. The distinction between journalists and directors is important here: Unlike journalists, directors are corporate insiders who are directly involved in the firm's decision making. Therefore, directors' influence operates through a different channel from that for a business reporter, which we focus on.

[6] The following papers examine how media affect stock returns: Huberman and Regev (2001), Tetlock (2007, 2011), Tetlock, Saar-Tsechansky and Macskassy (2008), Fang and Peress (2009), Engelberg and Parsons (2011), Dougal et al. (2012), Peress (2014), Solomon, Soltes and Sosyura (2014), and Kaniel and Parham (2017). See Tetlock (2015) for a synthesis and Goldman, Martel and Schneemeier (2020) for a theory of financial media.

[7] See Goyal (2007) for an overview.

Overall, this study provides evidence that a new form of social networking—that between firms and reporters—has an economically meaningful impact on equity prices and capital allocation. Hence, the results have implications for both firms and financial media concerning their behavior in news production. The incentives of journalists and newsmakers may prove a crucial issue in related debates concerning financial market and media regulation.

I. Background and Hypotheses

The central question in this study is whether journalists' social networks lead to slant. In the US, the *WSJ* and the *NYTimes* are clearly the leading national newspapers by weekday circulation. The *WSJ* is traditionally a business newspaper and well-established among finance professionals.[8] The *NYTimes* targets general interest mass-market audience. While *USA Today* is also a popular newspaper, it mainly caters to a middle-market audience and lacks in-depth coverage of business news. Therefore, we do not include *USA Today* in our study.

In news production, editors are primarily responsible for selecting which stories to cover, while journalists write and develop assigned stories (see the Occupational Outlook Handbook by *U.S. Department of Labor*, 2000, p. 244). These different roles indicate that journalists may have little discretion over news selection. Given an assignment, journalists are usually chosen based on their "beats," or areas of coverage. While some reporters are general assignment reporters, the others are specialized in a specific area—usually an industry—in business news reporting. Although the process of journalist assignment is unobservable, we provide some descriptive statistics in Section II; moreover, we will study the selection bias of news coverage.

Gentzkow and Shapiro (2010) show that political newspapers have an incentive to cater to the demands exerted by readers' ideologies. However, this incentive is not clear in business news, because the latter is usually assumed to appeal to rational investors who demand accurate information about the underlying story. Therefore, if a well-connected journalist has an advantage in gathering informative corporate insight, news coverage of the connected firm should exhibit less slant.

On the other hand, there is good reason to expect more slanted stories where there are

[8] According to the statistics from the *WSJ*, the newspaper reaches 95% of all US institutional investors (`https://classifieds.wsj.com/advertise/legal-notices/`).

journalist–firm social ties. First, a business reporter relies on company management for information. This creates a *quid pro quo* incentive, as proposed by Dyck and Zingales (2003). A survey by Call et al. (2021) supports that private communications with companies constitute a major source of information for financial reporters. Intriguingly, their survey reports that journalists often face backlash from the firm in response to unfavorable reporting. Although a *quid pro quo* may also apply to an independent reporter, its benefit is far less clear than that flowing from the favoritism of a journalist in an existing relationship with the firm.

Second, due to homophily, in which journalists and executives share a common background, connected journalists have preferences and beliefs similar to those of corporate executives. This could lead the journalist and the CEO to both think a corporate decision is value-creating. As Uzzi (1996) notes, such a social tie "disposes one to interpret favorably another's intentions and actions." Furthermore, homophily may give the journalists insufficient motivation to consider the disadvantages of the corporate decision because they derive personal utility from the collaboration (Gompers, Mukharlyamov and Xuan, 2016).

Finally, a journalist in the firm's networks is more likely to have developed friendships with the firm's employees, because contact between people in the same social network occurs more frequently (McPherson, Smith-Lovin and Cook, 2001). These personal relationships could affect news content, as suggested by the anecdotal evidence from the opening quote.

II. Sample and Data

A. Sample of M&A articles

We focus on one type of business news, namely, M&As. We choose to focus on a homogeneous event type in our main analysis because news tone is predominantly determined by the underlying story. Lumping various events, say scandals and acquisitions, all together makes it difficult to control for the underlying event characteristics and could lead to biased estimates if event characteristics correlate with a journalist's connectedness. To assess external validity of our tests, we extend the analysis to a sample of financial fraud news in Section V.

We first collect all the M&A bids from 1997 to 2016 consisting of US public firms. These data come from ThomsonOne Securities Data Company (SDC) Platinum. We focus on the

US domestic firms to rule out potential media bias related to foreign countries (Golez and Karapandza, 2021). The sample period begins in 1997 because the main measure of the journalist connection requires a manual search on the *WSJ* engine, which starts in 1997. We follow the sample selection methods most often used in the M&A literature (see Masulis, Wang and Xie, 2007; Erel, Liao and Weisbach, 2012) and ensure that (1) the transaction value exceeds US$10 million, (2) the target is not undergoing bankruptcy proceedings, and (3) 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 newspapers. We search the media coverage using *WSJ / NYTimes*' websites and Factiva, a media research tool owned by Dow Jones. We require the news article to be the first report on the transaction following the official deal announcement. This requirement ensures that we are not selecting stories about M&A rumors. We also restrict attention to articles with authors' information. This leads us to drop newswire articles, which typically do not mention reporters' names.[9]

After matching the covered firms with Compustat (for financial data) and CRSP (for stock data), we are left with 1,131 articles from the *WSJ* and 993 articles from the *NYTimes*, corresponding to 47% and 42% coverage rates, respectively. However, the *NYTimes* frequently relies on newswire services and thus does not disclose authors' information.[10] After excluding the anonymous stories, we obtain a sample of 485 *NYTimes* articles. Figure 1 shows the number of M&A bids across years. The number of bids is lower after the dotcom-bubble crash in 2001 and again during the financial crisis in 2008, a pattern consistent with the argument by Shleifer and Vishny (2003) that market valuation drives merger activity. Overall, the *WSJ* provides more solid coverage of transactions than the *NYTimes*: The covered deals in the *WSJ* (*NYTimes*) collectively account for over US$6.9 trillion (4.2 trillion) in transaction value and have an average deal size of US$5.2 billion (8.8 billion). This compares to the average SDC deal size of US$2.6 billion. The relatively large *WSJ / NYTimes* transaction value is perhaps unsurprising, as big transactions attract greater media attention. Most of these M&A stories (over 95%) appear within two days following the official bid announcement by the company.

[9] A newswire distributes fast and (usually) more standardized news to media organizations. Compared to the articles published by newspaper journalists, the newswire articles are typically shorter and less analytical.

[10] For example, Bloomberg News, Bridge News, Dow Jones, Reuters, and The Associated Press account for 27% of the *NYTimes* articles over our sample period. Moreover, since 2001 the *NYTimes* has aggregated much of its M&A news in a column under the name of *DealBook*, also anonymous.

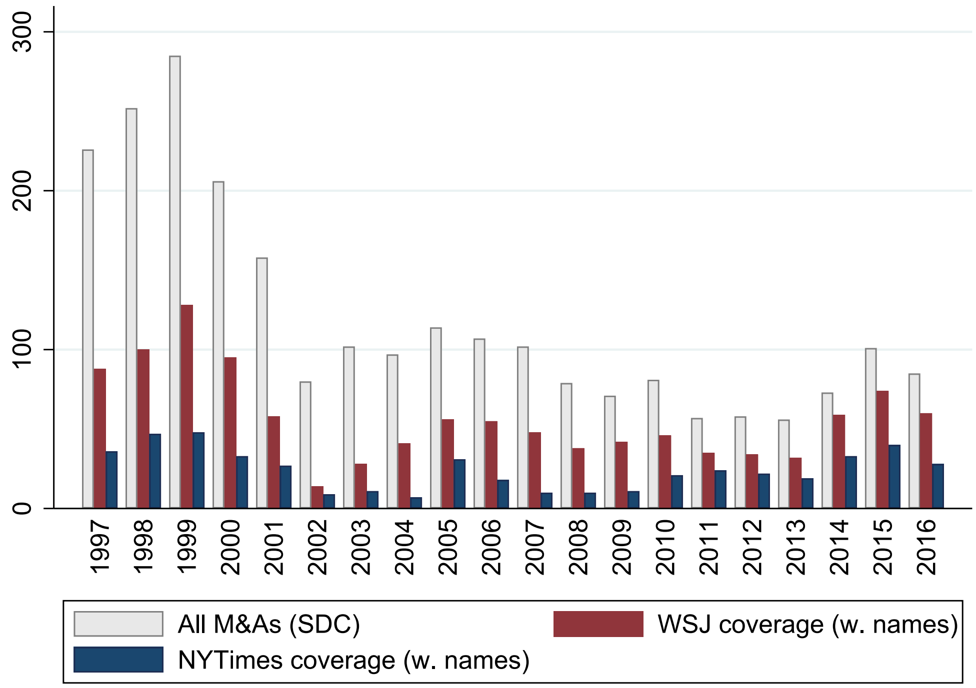

Figure 1: Sample of M&A Articles by Year

Notes: The figure shows the number of public M&A bids in the United States from SDC database (first gray bar), the number of M&A articles in the *WSJ* (middle red bar), and the number of M&A articles in the *NYTimes* (third blue bar). M&A articles without journalist information (e.g., newswire articles) are excluded from the graph. Section II.A. describes the sample selection criteria.

B. Journalist connections

Measuring journalists' social networks is complicated by the fact that individual connections are not directly observed. Therefore, we follow conventions in the economics literature for proxying such networks. For practical reasons, we focus on two types of connections: working relationships and educational ties. Admittedly, this practice omits other potential social ties, such as common board affiliations and second-degree connections. To the extent that these omitted networks matter for media slant, our focus on the work and educational ties provides a conservative estimate of connection effects.

We identify working relationships following the logic in Solomon (2012). Specifically, we check whether a reporter has written multiple exclusive stories about the same company in the 12 months prior to the merger. We examine past articles about any firm-specific story but we exclude articles on M&A rumors. For example, Betsy McKay, the author who covered PepsiCo's acquisition of Quaker Oats in December 2000, wrote more than 10 stories about PepsiCo in 2000, featuring several interviews with the company's top executives. It is likely that Betsy has established a working relationship with the company. Our measure of the working relationship, *CONNECT_WORK*, takes a value of one if any author of the M&A story has covered the bidding firm at least twice in the previous year.

We construct the educational networks by searching for the universities that the reporter and CEO attended. We use a variety of sources. The newspaper websites provide personnel biographical information of the authors. We supplement this information with data gathered from LinkedIn, a professional networking service. We also gather the CEOs' universities from the companies' proxy filings on the Securities and Exchange Commission (SEC)'s EDGAR database and Bloomberg. Finally, if we are unable to determine a reporter or a CEO's university with our primary sources, we perform general web searches to collect additional information. Our measure of educational ties, *CONNECT_UNIVERSITY*, equals one if any author of the article attended the same school as that attended by the bidder's CEO.[11]

While a social tie from the same alma mater is arguably exogenous to the concurrent economic fundamentals underlying a news event, the connection from a working relationship is not. Because journalists are often allocated to an event based on their "beats," we may worry that *CONNECT_WORK* partly captures expertise. However, we note that the effect of better knowledge about a specific firm is consistent with our hypothesis that connections can lead to more credible reporting. To explicitly control for a journalist's experience, we construct two variables, *Tenure* and *Industry expert*, to capture the general influence of expertise on media slant. In addition, we control for a journalist's gender and location. Table 1 summarizes these journalist characteristics. We see that about 48% of the articles are authored by a reporter specialized in the firm's industry. However, only 24% to 26% of the articles are written by a journalist with a working relationship. This evidence is consistent with the remark in Li (2015) that financial reporters rarely cover the same firm more than once in a year, suggesting that

[11] The databases do not provide records of graduation years, restricting our ability to observe the connections within a cohort. We verify that the most common institutions that provide such connections are typically elite universities with strong ties among alumni, including Harvard University, Princeton University, Stanford University, and UC Berkeley. Importantly, prior work has shown that general alumni networks can have significant impacts on the financial markets (e.g., Cohen, Frazzini and Malloy, 2008, 2010).

Table 1: Summary Statistics

	WSJ			***NYTimes***		
	Mean	S.D.	Median	Mean	S.D.	Median
Number of articles by journalists	1,131			485		
Journalist / news characteristics:						
CONNECT_WORK	0.261	0.439	0	0.241	0.428	0
CONNECT_UNIVERSITY	0.023	0.150	0	0.029	0.168	0
Negative slant (%)	1.396	0.868	1.245	1.380	0.799	1.268
Local journalist	0.227	0.419	0	0.190	0.392	0
Female	0.441	0.497	0	0.274	0.447	0
Tenure (months)	67	59	51	96	96	67
Industry expert	0.477	0.500	0	0.474	0.500	0
Deal event characteristics:						
Relative deal size	0.538	0.707	0.293	0.565	0.721	0.344
Absolute deal size ($billion)	5.231	12.711	1.544	8.788	16.716	3.229
Hostile	0.030	0.171	0	0.043	0.204	0
Unsolicited	0.047	0.211	0	0.052	0.221	0
Cross-industry	0.324	0.468	0	0.328	0.470	0
Financing (% cash)	47.251	43.851	45.000	46.614	43.656	40.921
Toehold (%)	0.348	3.314	0	0.384	3.518	0
Firm characteristics:						
Firm size (log)	8.592	1.761	8.617	9.169	1.619	9.268
Tobin's Q	2.764	2.949	1.868	2.854	3.059	1.910
Firm leverage	0.149	0.134	0.116	0.151	0.129	0.125
Firm cash	0.157	0.182	0.081	0.149	0.181	0.072
Firm profitability	0.043	0.119	0.052	0.044	0.127	0.054
Institutional ownership	0.621	0.256	0.667	0.606	0.249	0.643
# Analysts	15	10	14	17	10	17
CEO age	54	7	54	55	8	55
CEO duality	0.656	0.475	1	0.685	0.465	1
Classified board	0.447	0.497	0	0.386	0.487	0

Notes: This table reports the summary statistics for journalist, deal, and firm characteristics. *CONNECT* is a dummy variable that indicates the article is written by a connected reporter. *Negative slant* is the proportion (%) of negative words in an article. *Local journalist* (dummy) indicates that the journalist is based in the same city as the firm headquarter. *Female* (dummy) indicates female journalists. *Tenure* is the months of work experience. *Industry expert* (dummy) indicates journalists reporting in the firm's industry. *Relative deal size* is deal value scaled by the bidder's market value 4 days before the announcement. *Hostile* and *Unsolicited* are dummy variables for hostile and unsolicited bids. *Cross-industry* (dummy) flags bids for target firms unrelated to the bidder's (Fama-French 48) industry. *Financing* is the percentage of the transaction paid in cash. *Toehold* is the stake owned by the bidder prior to the bid. *Firm size* is the logarithm of book value of total assets. *Tobin's Q* is market value of assets over book value of assets. *Leverage* is book value of debts over market value of assets. *Cash* is cash holdings scaled by total assets. *Profitability* is the net income scaled by total assets. *Institutional ownership* is the stake of the bidder firm owned by financial institutions. *# Analysts* is the number of analysts following the bidder prior to the bid. *CEO duality* and *Classified board* are dummy variables that flag a CEO who is also the chairman of the board and a staggered board, respectively.

repeated coverage captures a connection beyond general expertise. In online Appendix Table A.1, we see that a working relationship is positively associated with a local journalist and an industry expert. Finally, we note that *CONNECT_UNIVERSITY* is largely uncorrelated with other journalist personal traits or event characteristics.

C. *Media slant*

We measure media slant by focusing on negative sentiment in a news story. Our choice is consistent with Gurun and Butler (2012) and is motivated by previous studies that suggest negative information has a stronger impact than positive information (e.g., Rozin and Royzman, 2001; Tetlock, 2007; Tetlock, Saar-Tsechansky and Macskassy, 2008). We use the negative-word categorization of Loughran and McDonald's Financial Dictionary to count the number of negative words[12] and calculate its fraction out of the total article word count:

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

The average negativity expressed in an M&A article equals 1.4% (see Table 1). This compares favorably to the 1.7% negative slant reported by Gurun and Butler (2012). To provide a sense of the slant, online Appendix 1 offers several examples of news articles (excerpts).

D. *Deal and firm characteristics*

We construct several firm and deal characteristics as control variables. These characteristics are motivated by the M&A literature and have been widely used to capture merger qualities. For example, we collect the deal's relative size, hostile attitudes, unsolicitations, cross-industry bids, payment methods, and toeholds from SDC. From Compustat, we gather information about firm size, Tobin's Q, leverage, cash holdings, and profitability. Institutional ownership data come from Thomson/Refinitiv. Analyst coverage comes from the Institutional Brokers' Estimate System. Additionally, we obtain CEO/board-related variables—including CEO age,

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

duality, and classified board—from the firms' proxy filings on EDGAR database.[13] Table 1 provides the definitions and summary statistics of all variables.

E. Endogeneity of media coverage

Before delving into the empirical analysis, we discuss the concern that a sample selection bias exists. Specifically, it would be worrisome if news coverage is correlated with reporters' networks with companies. Here, we investigate whether this is the case.

In online Appendix Table A.2, we use a probit estimator to predict media coverage using the full list of M&A bids. We find that journalist connections do not predict news coverage. Instead, coverage is primarily driven by firm and deal size, in which large transactions are more likely to be reported. This evidence confirms that reporters do not control which stories are eventually published.[14] We also find that more sensational news, such as hostile and unsolicited bids, is more likely to attract media attention. Given that our sample is biased towards larger firms and assuming that these firms are more likely to have unobserved connections (such as those with the media owner), the estimate of the connection effect would be underestimated in an ordinary least squares (OLS) regression. We propose an instrumental variable approach to address this concern in the next section.

III. Do Journalist Connections Cause Slant?

A. Empirical strategy and results

To investigate the impact of journalist connection, our general approach is to ask whether connections predict negative slant. Our baseline regressions compare slant *within* the same event to ensure that slant is not driven by an omitted variable related to the event (e.g., investment synergies). To do so, we pool all the articles covering the same M&A bid from

[13] To reduce the impact of possibly spurious outliers, we winsorize the continuous variables at the top and bottom 0.5 percentiles.

[14] In the regression, the dependent variable equals one if a deal is reported (and thus included in our media sample), and zero otherwise. *"Journalist connection"* is a dummy variable if a firm has been covered by a same journalist multiple times in the past year or has a reporter with a schooling tie with the CEO. Note that our connection measure does not automatically predict coverage of an M&A story as this measure is constructed over the coverage of *any* type of past event.

different media outlets and estimate the following OLS regression:

$$Negative\ slant_{ij} = \alpha + \beta \cdot CONNECT_{ij} + \gamma \cdot \mathbf{Z}_{ij} + Deal_i + Media_j + \epsilon_{ij} \tag{2}$$

where $Negative\ slant_{ij}$ is defined in (1). Subscript i indexes a specific bid, and j indicates the media outlet (e.g., the *WSJ* or *NYTimes*).

The variable of interest, $CONNECT$, corresponds to either measure of the reporter connections described in Section II. To test whether connections are related to slant, we estimate whether β is statistically different from zero. Equation (2) includes deal fixed effects ($Deal_i$), which allow us to benchmark slant across media while holding the information content of the event constant. Because deal fixed effects absorb all deal-level estimates, we control only for journalist characteristics, $\mathbf{Z}_{ij}$. $\mathbf{Z}$ includes a dummy variable indicating whether the journalist is in the same city as the firm's headquarter, journalist gender, tenure, and industry expertise. We use media fixed effects ($Media_j$) to control for the general writing style of the media outlet. Standard errors are clustered by event.

Table 2 reports the results. Looking across the columns, the coefficients on $CONNECT$ are all negative and highly statistically significant. The negative point estimates suggest substantially more favorable coverage in connected publications: For the same event, an article authored by a journalist with a working relationship contains 19.3% fewer negative words relative to an average article (based on column 1).[15] In the articles with a schooling tie, the use of negative words is 42.5% lower (based on column 2).

Call et al. (2021) indicate that financial journalists often use company-issued disclosures while developing their stories. Therefore, as another way of comparing slant, we benchmark the text in the newspapers to that in the press release issued by the bidding companies. Press releases are typically positive. The degree to which the media simply repeat the talking points of the firm, rather than conduct a critical analysis of the bid, reflects media coverage that is skewered in favor of the company. In columns 3 and 4 of Table 2, we pool articles from the *WSJ*, *NYTimes*, and press releases. The results continue to show a connection bias in favor of the firm. Here, the economic effects are that connections via a working relationship (schooling

[15] The calculation is as follows. We divide the coefficient estimate of $CONNECT\_WORK$ in column 1 (-0.270) by the mean of slant (1.40) and obtain 19.3%.

Table 2: Journalist Connections and Media Slant

	WSJ + NYTimes		*WSJ + NYTimes + Press Release*	
	1	2	3	4
CONNECT_WORK	-0.270***		-0.174***	
	(0.065)		(0.046)	
CONNECT_UNIVERSITY		-0.595**		-0.484***
		(0.230)		(0.133)
Local journalist	0.071	0.058	0.063	0.045
	(0.107)	(0.105)	(0.062)	(0.062)
Female	-0.071	-0.053	-0.116***	-0.111**
	(0.060)	(0.060)	(0.044)	(0.044)
Tenure	-0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Industry expert	-0.098	-0.117*	-0.035	-0.054
	(0.061)	(0.061)	(0.043)	(0.043)
Constant	1.587***	1.529***	1.294***	1.408***
	(0.048)	(0.046)	(0.053)	(0.068)
Deal FE	YES	YES	YES	YES
Media outlet FE	YES	YES	YES	YES
Observations	970	970	2,342	2,342
R^2	0.683	0.681	0.667	0.666

Notes: The table shows the effect of journalist connections on media slant. The dependent variable is *Negative slant*, measured as the fraction of negative words in the text of a news article. Columns 1 and 2 use a sample of articles that cover the same event from the *WSJ* and *NYTimes*. Columns 3 and 4 pool articles from the *WSJ*, *NYTimes*, and Press Releases. Definitions of control variables appear in Table 1. Media outlet and deal fixed effects are included. Standard errors are clustered by event (deal) and reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

tie) are associated with 12.4% (34.6%) less negativity.

To get a sense of magnitudes, we can compare these results to the well-documented advertising bias (Gurun and Butler, 2012): Gurun and Butler show that spending $100,000 per month for advertising expenditures on a national newspaper is associated with an 18% increase in slant. Thus, the impacts of social networks are of a similar, if not larger, magnitude to the effect of advertising bias per $100,000 in a month.

With respect to the control variables, we find that, on average, female journalists use fewer negative words. As expected, expertise, as captured by *Tenure* and *Industry expert*, is not systematically associated with slant, conditional on information content of the event.

Cross-sectional difference in slant: We also examine the cross-sectional difference in slant for each newspaper separately. Specifically, for all articles published in a given newspaper, we regress the negative slant on the journalist connection, controlling for journalist, deal, and firm characteristics.[16] The advantage of a cross-sectional analysis is that (1) we are able to directly visualize the extent of bias within each newspaper, and (2) we are not restricted to the events covered by both newspapers. Figure 2 shows that a connection bias exists in both the *WSJ* and *NYTimes*. For the *WSJ*, the connections via a working relationship (schooling tie) are associated with a 21.9% (27.4%) decrease in negative slant (relative to the other *WSJ* articles). For the *NYTimes*, the use of negative words is 16.2% lower in the work-related connections; the estimate on the university tie is statistically insignificant.

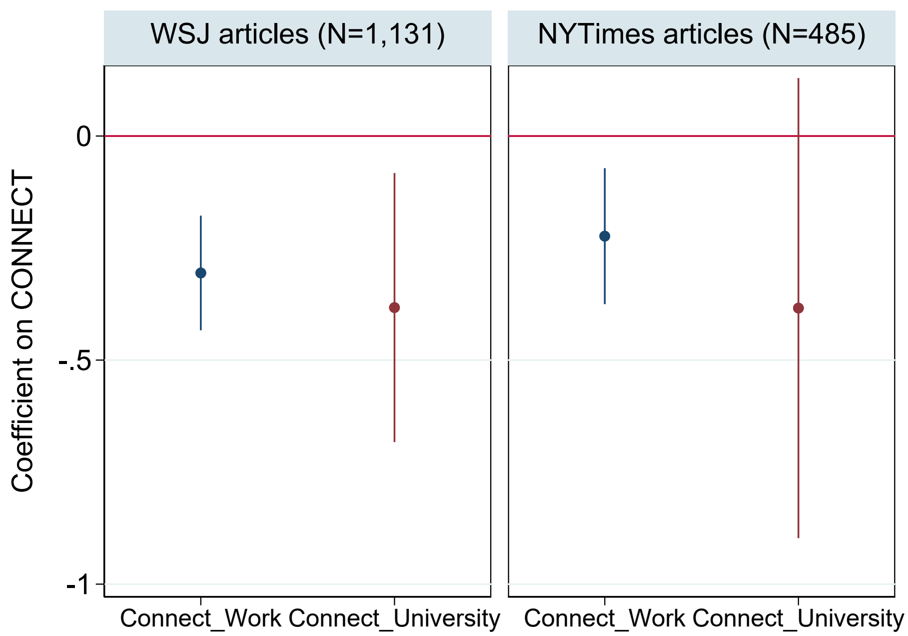

Figure 2: Cross-sectional Difference in Slant

Notes: The graph shows the coefficient estimates on *CONNECT* on negative slant within *WSJ* and *NYTimes* subsamples. The regression we run is $Negative\ slant_{it} = \alpha + \beta \cdot CONNECT_{it} + \gamma \cdot Controls_{it} + Fixed\ effects + \epsilon_{it}$, where the full model and results are reported in online Appendix Table A.3. The plotted model includes year, industry, and location fixed effects. The graph shows the 95% confidence intervals around the estimates.

[16] Online Appendix Table A.3 gives the detail of the regression specification and reports the full results. In Table A.4 of the online Appendix, we run a robustness test using Heckman (1979)'s model to correct for a potential sample selection bias. The results remain robust.

B. Instrumenting journalist connections

Despite the granular deal fixed effects in Table 2, we are left with two problems that challenge a causal interpretation of the connection effects. The first one is that journalists' assignment is not random. Some unobserved, journalist-level variables could be biasing the OLS estimates. For example, one could think of a firm's second-degree connections with reporters (e.g., a connection through an editor). However, such omitted social ties generally imply an *underestimated* effect of connections. Second, the bias may be driven by the readers' demands (Gentzkow and Shapiro, 2010; DellaVigna and Hermle, 2017). Under this hypothesis, media cater to their readers' passion about a specific set of firms and give them a positive slant. Although there is little reason to believe that such demands are systematically related to personal connections, it is not feasible to rule out this explanation with our tests in Table 2.

To address these concerns, we propose an instrumental variable (IV) that is correlated with the probability of a connected coverage but has no independent effect on slant. We contend that journalists' turnover satisfies both the relevance condition and the exclusion restriction. The rationale is as follows. The departure of a friendly reporter will decrease the likelihood of the retrospective firm being covered by a connected journalist in the future; however, turnover is likely driven by events in the reporters' personal lives and is thus exogenous to the firms they write about (Solomon, 2012). One may worry that, even in the presence of a turnover, editors might assign other connected reporters as a replacement. To alleviate this concern, we focus solely on cases where a firm is connected to *one* reporter prior to the turnover; hence, losing this one friendly reporter entails a complete loss of connections. Specifically, we examine the turnovers in the six months prior to the news. If a firm's sole journalist connection left the newspaper during this period, we set the indicator variable *turnover* equal to one. Note that most companies have only one connection (conditional on being connected): For example, the average number of connection at the *WSJ* is 1.7 and the median is one.[17]

In constructing the sample of turnovers, we let the following considerations guide us. First, with a firm's connectedness being endogenous, it does not make much sense to compare firms

[17] Online Appendix 2 provides a detailed description of how the IV, *turnover*, is constructed. We observe no cases wherein a firm has multiple connections and all of its connections turn over. We exclude cases wherein only one of a firm's several connections turn over to avoid potential replacements. However, the IV results remain similar if we include these cases.

that have zero connections with those with connections. Therefore, we concentrate on firms that have at least one journalist connection so that these firms are more similar. Second, because turnovers generate exogenous variation of connections in a time-series, the exercise is possible only for the firms that have conducted more than one transaction (see online Appendix 2 for a description). These restrictions lead us to lose a substantial number of observations. Therefore we perform this analysis only with the connectedness measure of working relationships.[18] Following the new sampling criteria, we obtain 318 and 287 articles from the *WSJ* and *NYTimes*, respectively.

A crucial assumption in the IV method is that our IV is uncorrelated with the error term. While testing for the exclusion restriction is inheritably infeasible (Wooldridge, 2002, p.86), we verify and confirm that journalist turnovers are not related to (1) the performance of the firms to which they are connected,[19] and (2) the average slant in the journalists' past publications.[20] In a further probe, we find that most reporters left the newspaper to found a company or to take a progressive new role as an editor in another media outlet. This evidence suggests that turnovers are mainly caused by personal career motives.

Table 3 reports the IV results for each newspaper separately. The first-stage results (columns 1 and 2) show that a connected journalist's turnover significantly reduces the probability that a future report is written by a reporter in the firms' networks. The F-statistic on the instrument is above the critical values proposed by Stock-Yogo, suggesting that the estimation is efficient.[21]

In columns 3 and 4, we report the estimate of the second-stage IV regression. The effect of connections retains statistical significance at the 1 percent level for both newspapers, pointing to the same direction of slant as that documented in Table 2. The Sargan χ^2 test cannot reject the joint null that the instrument is valid. The magnitude of the slant becomes somewhat larger than the OLS estimate in the similar cross-sectional analysis (see online Appendix Table

[18] Note that the variation of university ties is low in our sample, rendering the IV exercise infeasible. Moreover, working relationships are arguably more endogenous than connections formed from schooling institutions.

[19] See Table A.5 of the online Appendix for a regression analysis.

[20] Specifically, for the *WSJ*, the average slant of turnover and non-turnover reporters is 1.54 and 1.39, respectively. The t-statistic for the difference in slant is 0.63. For the *NYTimes*, the average slant of turnover and non-turnover reporters is 1.41 and 1.38, respectively. The t-statistic for the difference is 0.12.

[21] We use the Kleibergen-Paap Wald test for weak identification because the standard errors are not independent and identically distributed. The F-statistic (54.01) rejects the null hypothesis of weak instrument.

Table 3: 2-Stage Least Squares Regressions

	First stage: Connection		Second stage: Negative slant		Reduced form: Negative slant	
	WSJ	***NYTimes***	***WSJ***	***NYTimes***	***WSJ***	***NYTimes***
	1	2	3	4	5	6
Turnover	-0.592***	-0.640***			0.389*	0.892**
	(0.081)	(0.212)			(0.203)	(0.368)
CONNECT_WORK			-0.656***	-1.393***		
			(0.175)	(0.348)		
Controls	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Observations	318	287	318	287	318	287
R^2	0.260	0.327	0.846	0.745	0.341	0.324

Notes: Two-stage least squares (2SLS) regressions where *CONNECT_WORK* is instrumented with connected journalists' turnovers. The sample includes firms that are covered at least twice and who have at least one journalist connection. In the first stage (columns 1 and 2), *CONNECT_WORK* is instrumented by the connected journalist turnover. In the second stage (columns 3 and 4), *Negative slant* is regressed on the instrumented *CONNECT_WORK*. Columns 5 and 6 report the reduced-form regressions. Control variables include *local journalist*, *female*, *tenure*, *industry expert*, *deal relative size*, *toehold*, *hostile*, *unsolicited*, *cross-industry*, *financing (% cash)*, *firm size*, *Tobin's Q*, *leverage*, *cash*, *profitability*, *CEO age*, *CEO duality*, and *classified board*. Standard errors are double-clustered by industry and by year, and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

A.3), likely for two reasons. First, the smaller effect in the OLS models is in line with our concern that the OLS estimate is underestimated because the measure of connectedness does not capture unobserved social ties. This biased estimate may be particularly prominent in the IV subsamples, where firms are on average larger and therefore more likely to have other omitted media connections.

Second, the average treatment effect is local (Jiang, 2017). A local treatment effect makes it difficult to generalize the finding to a large population. We fully acknowledge that the local effect in this smaller sample is the cost of achieving a higher level of internal validity. To the extent that our purpose is to detect a connection bias, rather than quantify the slant, this exercise still offers valuable insight into the causal plausibility of our underlying hypothesis.

In the last two columns of Table 3, we follow the recommendation by Angrist and Krueger (2001) to examine a "reduced-form" relationship between the instrument and the dependent variable. The results are consistent with those in the two-stage IV model. These findings suggest that, insofar as the IV is plausible, connections cause media slant.

C. Ownership change of the WSJ

The analysis has so far focused on the impacts of social networks at the individual level. In this section, we study the influence of networks formed at the media level, namely, connections with the media owner. Examining media-level connections allows us to better pinpoint one mechanism of bias, namely, a *quid pro quo* incentive. This is because a bias at the individual level can arise due to both homophily and a *quid pro quo*, but a media-level connection is primarily driven by a *quid pro quo*. Specifically, we explore a change-of-ownership event that exogenously establishes a (second-degree) firm connection with reporters.

On May 2, 2007, Rupert Murdoch's News Corp made an unsolicited bid for Dow Jones and the *WSJ*. The takeover ended the newspaper's 105-year ownership by the Bancroft family and triggered a fierce debate over the potentially adverse effect on the journalistic independence. To see why this is a meaningful shock, consider the following *WSJ* article published on May 2, 2007. The article claimed "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."[22]

We adopt a difference-in-difference strategy to investigate whether business news becomes more favorable for companies connected to Murdoch after the *WSJ* takeover. To do so, we estimate the following regression:

$$\textit{News negativity}_{it} = \alpha + \beta \cdot Murdoch_i \times Post_t + \gamma . Murdoch_i + \eta \cdot \mathbf{Z}_{it} + \theta_t + \psi_j + \epsilon_{it} \qquad (3)$$

where *Murdoch* indicates companies connected to Murdoch/News Corp, and *Post* is a dummy variable that equals one for media publications after 2007. We control for industry (ψ_j) and year fixed effects (θ_t) and journalist, firm, and deal characteristics as those used in Table 3.

[22] Other evidence includes the resignation of Dow Jones director, Leslie Hill, in opposition to this deal and the appointment of Robert Thomson, described as Murdoch's "best friend," as the managing editor of the *WSJ* and editor-in-chief of Dow Jones Newswires (Auletta, K. 2011, "Murdoch's Best Friend," *The New Yorker*). Academic research shows that Murdoch's *WSJ* produces more political bias (Wagner and Collins, 2014; Archer and Clinton, 2018). Szeidl and Szucs (2021) provide additional evidence on the ownership effects on (political) media favor exchange. See Besley and Prat (2006) for a model of ownership and media capture.

Table 4: Media Slant Around *WSJ*'s Ownership Change

	All repeat acquirers		Excuding *WSJ*'s direct competitors	
	1	2	3	4
Murdoch × Post	-0.993***	-1.055***	-1.086***	-1.094***
	(0.236)	(0.260)	(0.195)	(0.256)
Murdoch	0.621***	0.642***	0.708***	0.688***
	(0.181)	(0.152)	(0.122)	(0.150)
Controls	NO	YES	NO	YES
Year FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Observations	392	392	385	385
R^2	0.177	0.288	0.178	0.289

Notes: The table reports the results of difference-in-difference analysis. The dependent variable is *Negative slant*. Variable *Murdoch* is a dummy variable that equals 1 if a firm is connected to News Corp or Rupert Murdoch. Variable *Post* takes a value of 1 for years from 2008 onward. Columns 3 and 4 exclude transactions in the Entertainment and Publishing (Printing) sectors. Control variables include *local journalist*, *female*, *tenure*, *industry expert*, *deal relative size*, *toehold*, *hostile*, *unsolicited*, *cross-industry*, *financing (% cash)*, *firm size*, *Tobin's Q*, *leverage*, *cash*, *profitability*, *CEO age*, *CEO duality*, and *classified board*. Standard errors appear in parentheses and are double-clustered by year and by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

To identify connections with Murdoch, we manually search for potential (1) business relations, such as asset purchases or supply chain links, between the two companies, and (2) common directors sitting on both Boards. The data sources are bidder's 10K and proxy filings and BoardEx. Following this exercise, we find that approximately 9% of bidders are connected to Murdoch. Online Appendix Table A.6 gives a list of such connections in our sample.

The coefficient of interest is β, which captures the average treatment effect. A negative β is consistent with media bias, given the identifying assumption that the change in ownership is uncorrelated with any unobserved M&A deal, firm, and journalist characteristics.

Our sample is limited to the firms that conducted more than one bid both before and after 2007. Despite the smaller sample, the results are striking: As the first two columns of Table 4 show, after the *WSJ* ownership change, coverage for firms connected to Murdoch contains significantly fewer negative words than does the coverage of independent firms. An equally interesting observation is that, on average, the *WSJ* coverage of these connected firms is actually more negative, as indicated by the stand-alone positive coefficient on *Murdoch*.

However, this relation is completely reversed after 2007.

Columns 3 and 4 of Table 4 assess the robustness of the findings by removing acquisitions within the Entertainment and Publishing sectors (the sectors in which the *WSJ* operates). Looking at events unrelated to these industries addresses the concern that the *WSJ* control change has a direct impact on its competitors' acquisition synergies.

We interpret these results causally given that Murdoch's takeover is reasonably exogenous to the writing styles of journalists or to the expected deal synergies of unrelated firms. This exercise also complements other studies that relate media ownership to the kind of news bias typically observed in political coverage (e.g., Gilens and Hertzman, 2000).

IV. Real Effects of Journalist Networks

Does the influence of journalist networks matter for financial markets and capital allocation? Inferences from the literature on political news suggest that media exposure affects viewers' voting decisions (DellaVigna and Kaplan, 2007; Enikolopov, Petrova and Zhuravskaya, 2011; Durante, Pinotti and Tesei, 2019). However, in the classic asset-pricing and investment models, investors and managers are often unbiased. Even if a set of investors responds to media bias in the short term, it would be surprising to observe any long-term impacts of journalist connections on the investment outcome (Tetlock, 2007). In this section we ask whether journalist networks create distortions in the real economy.

A. Journalist connections and stock market returns

A.1. Cross-sectional variation in returns

We assess the impact on stock returns by estimating the following regression:

$$CAR_{it} = \alpha + \beta \cdot CONNECT_{it} + \gamma \cdot \mathbf{Z}_{it} + \theta_t + \psi_j + \epsilon_{it} \tag{4}$$

where the dependent variable measures the bidder stock's cumulative abnormal return (CAR) from day 0 until day 1, where day 0 is the publication date. For example, if a firm announced a merger on Monday and the *WSJ* covered the deal on Tuesday, the dependent variable captures the overall abnormal returns from Tuesday until Wednesday. The CAR is the residual from the market model (Brown and Warner, 1985). Following the M&A literature, the model's parameters are estimated over a 200-day window ending 31 days before the deal announcement. The market portfolio is proxied by the CRSP index. The main explanatory variable of interest is *CONNECT*. **Z** includes control variables about deal and firm characteristics that are commonly used in the M&A research (e.g., Malmendier and Tate, 2008; Cai and Sevilir, 2012). θ_t and ψ_j are year and Fama-French 48-industry fixed effects, respectively. We cluster standard errors by industry and by year.

Panel A of Table 5 reports the results. For the *WSJ*, the coefficient on working relationship is positive and statistically significant at the 5 percent level. The effect is also economically significant. Based on column 1, connected articles are associated with 1.6 percent higher abnormal returns. This magnitude is comparable to that in other studies that examine connection effects on M&A returns. For example, Cai and Sevilir (2012) show that acquirer–target board connections improve bidders' CAR by 2 percentage points. Consistent with the message in Engelberg, McLean and Pontiff (2018), the magnitude of stock reactions is larger on important event days, such as M&As, than on other days.[23] With respect to university ties, evidence of impact is weaker. The coefficient on connection in column 2 is positive but statistically indistinguishable from zero. This might partly be explained by the low variation of university ties in data, which renders the estimate less informative. Column 3 explores the robustness of the result in column 1 by instrumenting connected coverage with journalist turnover. Here, we employ the turnover subsample as described in Section III.B. We see that the IV model yields results qualitatively similar to those reported in column 1.

In contrast to the results for the *WSJ*, the influence of connections in the *NYTimes* is statistically insignificant in our OLS regressions (columns 4 and 5). However, in the subsample where we could perform the IV analysis, we find these connections are associated with highly significant and positive market reactions after instrumenting connections with turnover (column 6). We believe the most likely explanation to these findings is that an endogenous

[23] The full model including control variables appears in Table A.7 of the online Appendix. The estimates of the control variables are in line with existing M&A studies. For instance, as in Masulis, Wang and Xie (2007), larger firms are associated with worse market response. As Bhagat et al. (2005) show, hostile bidders earn lower returns.

Table 5: Journalist Connections and Stock Returns

Panel A. Cross-sectional stock returns

	***WSJ* CAR**			***NYTimes* CAR**		
	OLS	OLS	2SLS	OLS	OLS	2SLS
	1	2	3	4	5	6
CONNECT_WORK	0.016**		0.034*	0.005		0.055***
	(0.008)		(0.020)	(0.009)		(0.017)
CONNECT_UNIVERSITY		0.001			0.010	
		(0.021)			(0.010)	
Controls	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Observations	1,131	1,131	318	485	485	287
R^2	0.126	0.120	0.292	0.181	0.181	0.114

Panel B. Returns around *WSJ*'s ownership change

	All repeated acquirers		Excluding *WSJ*'s direct competitors	
	1	2	3	4
Murdoch × Post	0.022*	0.032*	0.028**	0.035**
	(0.011)	(0.016)	(0.010)	(0.016)
Murdoch	-0.015	-0.025	-0.016	-0.025
	(0.009)	(0.015)	(0.010)	(0.016)
Controls	NO	YES	NO	YES
Year FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Observations	392	392	385	385
R^2	0.206	273	0.197	0.264

Notes: This table shows the effect of journalist connections on stocks' cumulative abnormal returns (CAR) from day 0 until day 1, where day 0 is the news publication date. In Panel A, columns 1 to 3 examine the impact of *WSJ* connections; columns 4 to 6 examine the *NYTimes* connections. Columns 3 and 6 report the second stage 2SLS results where *CONNECT_WORK* is instrumented with connected journalists' turnover. Panel B uses difference-in-difference regressions as described in Table 4. Control variables include *relative deal size*, *toehold*, *hostile*, *unsolicited*, *cross-industry*, *financing (all cash)*, *financing (all equity)*, *firm size*, *Tobin's Q*, *leverage*, *cash*, *profitability*, *CEO age*, *CEO duality*, and *classified board*. 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.

matching between firms and journalists, coupled with unobserved social connections, has biased the OLS estimate of a connection effect to zero (Engelberg, Gao and Parsons, 2012). However, again, we caution against extrapolating the effect size from the IV model to a general population because the average treatment effect is likely local to the subsample.

In the online Appendix (Table A.8), we show that these results are robust to using alternative windows to calculate abnormal returns and to restricting the sample to the articles published on the same day as the official deal announcement day.

How should we think about the influence of the *WSJ* versus that of the *NYTimes*? Note that the estimates in Table 5 are obtained using cross-sectional returns upon the publication date; in other words, for the same event, we can observe two returns if the publication dates differ across the newspapers. To directly compare the influence of the *WSJ* and the *NYTimes*, we run unreported OLS regressions using a sample of 339 transactions for which both newspapers' articles are published during the same day. Here, we include the connections in both newspapers in the same regression. We find that only the connections from the *WSJ* obtain statistically significant estimates (see online Appendix Table A.9). This finding is consistent with the notion that the *WSJ* has a greater impact among financial market investors.

Returns around the *WSJ* ownership change: In Panel B of Table 5, we explore the market reactions using the *WSJ* ownership change as a quasi-experiment. The regression specification is similar to that in equation (3) with the dependent variable being CAR.

The first two columns include all firms that make acquisitions around the ownership change. We see that firms with connections to Murdoch are associated with significantly higher news returns following Murdoch's takeover of the *WSJ*. The same relation remains when we remove articles on *WSJ*'s direct competitors in the Entertainment and Publishing sectors (see columns 3 and 4). Overall, these results suggest that investors fail to capture a firm's social ties with business reporters and react to the biased media content.

A.2. Mechanisms

Theoretical models propose several channels through which non-fundamental information in media could affect equity pricing. First, the models of noise traders (e.g., DeLong et al., 1990) postulate that noise traders, facing downward-sloping demand for risky assets, sell shares to rational arbitrageurs when there is a negative belief shock, pressuring prices downwards. Second, theories of liquidity traders (Campbell, Grossman and Wang, 1993) make similar predictions, with the media sentiment proxying for changes in risk aversion. Finally, investors' limited attention could lead them to incorporate only information from one source but not rational expectations from other sources (Barber and Odean, 2008; Fedyk, 2019).

We test these mechanisms in this section.[24] First, we consider different levels of rational arbitrageurs. To characterize the level of shares held by rational traders, we 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 (Kumar, 2009), firms with more analyst coverage (Zhang, 2006), or firms owned by more institutional investors (Barber and Odean, 2013). We interact these proxies with our connection measure, *CONNECT_WORK*. Table 6 presents the results. We see that all the estimates on the interaction coefficient are statistically significant. The first column suggests that firms covered by more analysts are less subject to connection effects. To illustrate the magnitude of the estimate, adding one additional analyst reduces the connection effect by approximately 10 basis points. In the same vein, columns 2 and 3 show that firm size and institutional ownership mitigate the distorted stock reaction to connected coverage.

Second, we test the channel of stock liquidity. To proxy for liquidity, we use Amihud (2002)'s illiquidity measure, which gauges the impact of trading volume on a stock's absolute return. Because this measure essentially captures the illiquidity of a stock, we find a positive estimate of the interaction coefficient, significant at the 1 percent level (see column 4).

Third, we examine limited attention. For each article, we classify whether it appears on the front page of the *WSJ* or somewhere else inside the newspaper. As Fedyk (2019) argues, front-page news articles are accompanied by larger price changes if investors are subject to limited attention. Indeed, column 5 shows that front-page articles are associated with significantly greater reactions to connected news, confirming the channel of investors' attention.[25]

Overall, these results are consistent with behavioral biases of equity investors and support the non-informational channels through which connected journalists influence the market.

[24] Following the evidence reported in Table 5, we only report the results of the *WSJ* working relationship.
[25] Importantly, we verify that the negative tone in the front-page and non-front-page articles is indistinguishable. The results are reported in online Appendix Table A.10.

Table 6: Mechanisms

	Arbitrage opportunities			Liquidity	Salience
Proxy:	# Analyst	Firm size	Institution ownership	Amihud iliquidity	Front-page
	1	2	3	4	5
CONNECT_WORK × *Proxy*	-0.001**	-0.011**	-0.064**	0.134***	0.031**
	(0.000)	(0.004)	(0.026)	(0.011)	(0.011)
Proxy	0.000	0.002	0.008	-0.006	-0.003
	(0.000)	(0.002)	(0.012)	(0.004)	(0.005)
CONNECT_WORK	0.036**	0.119***	0.054**	0.013**	-0.006
	(0.014)	(0.041)	(0.020)	(0.006)	(0.010)
Controls	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES
Observations	1,131	1,131	1,131	1,131	1,131
R^2	0.131	0.136	0.134	0.151	0.134

Notes: This table reports the results on interaction effects between *WSJ* journalist connection and proxies for the arbitrage opportunity, liquidity, and salience. The dependent variable is bidders' cumulative abnormal returns (CAR) from the article publication day (day 0) until the following day. Control variables include *relative deal size*, *toehold*, *hostile*, *unsolicited*, *cross-industry*, *financing (all cash)*, *financing (all equity)*, *firm size*, *Tobin's Q*, *leverage*, *cash*, *profitability*, *CEO age*, *CEO duality*, and *classified board*. 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.

A.3. Long-run stock price correction

Intuitively, if the higher short-run return to connected news is driven by bias rather than information, we should expect a stock price correction in the long run (Tetlock, 2007). Figure 3 confirms this intuition. Figure 3 shows the univariate comparison of daily CAR around media publications. It offers several insights. First, there is no statistically significant difference in returns between connected and unconnected firms on each single day before the publication day t. Second, consistent with the findings presented in Table 5, returns to connected articles are significantly higher immediately upon the news release. Finally, and more interestingly, the higher abnormal returns to connected firms gradually disappear in the long term.

We use a multivariate-regression framework to formalize this pattern. To ease the comparison of the results between tables, we first replicate the regressions of the short-term returns in the first column of Table 7. Moving right to column 2, we first observe that returns to firms with connections become significantly more negative over the [2,40] window. This find-

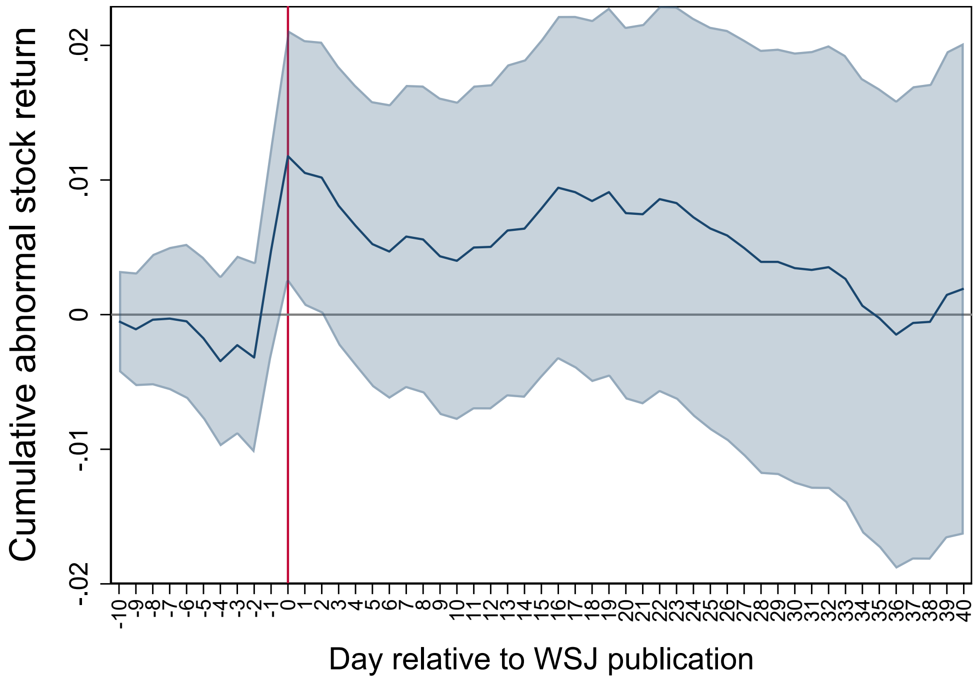

Figure 3: Difference in Cumulative Abnormal Return by Day

Notes: The figure shows the mean difference in CAR (with the 95% confidence intervals) from $t-10$ till $t+40$ ($t = 0$ is the *WSJ* publication day). The difference is calculated as the connected firms' CAR minus the unconnected firms' CAR.

ing supports the long-run price correction conjecture.[26] The post-announcement price reversal completely cancels out the initially more favorable responses to connected firms. Indeed, when the short- and long-run returns are combined, column 3 reveals that the difference in returns between connected and independent firms is indistinguishable from zero. Moreover, column 4 shows that returns calculated from one day before the deal announcement until the deal completion date are also similar between connected and non-connected firms. This evidence indicates that synergies are similar in connected and unconnected bids.

A potential concern with the price reversal results is that they might be driven by information released to the market in the long-term window. For example, investors might react to the news of bid withdrawals and other confounding events. Therefore, we run robustness tests in the online Appendix (see Table A.11). Specifically, we remove withdrawn deals and firms

[26] The horizon of the price correction appears comforting compared to the well-documented underreaction to Friday announcements (DellaVigna and Pollet, 2009), which lasts approximately 75 days, and to the reversal caused by contrast effects (Hartzmark and Shue, 2017), which dissipates after 50 days.

Table 7: Long-run Returns

	CAR[0,1]	CAR[2,40]	CAR[0,40]	CAR[-1,complete]
	1	2	3	4
CONNECT_WORK	0.016**	-0.025**	-0.008	0.029
	(0.008)	(0.011)	(0.011)	(0.031)
Controls	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Observations	1,131	1,131	1,131	852
R^2	0.126	0.088	0.084	0.096

Notes: This table shows the relation between *CONNECT_WORK* and long-run return reactions. Columns 1 replicates the results of bidder's CAR over [0,1]. The dependent variable in columns 2 is bidders' CAR over [2,40]. The dependent variable in columns 3 is bidders' CAR over [0,40]. The dependent variable in columns 4 is CAR from the day before the deal announcement until the deal completion date (for the subsample of completed deals). Control variables include *relative deal size*, *toehold*, *hostile*, *unsolicited*, *cross-industry*, *financing (all cash)*, *financing (all equity)*, *firm size*, *Tobin's Q*, *leverage*, *cash*, *profitability*, *CEO age*, *CEO duality*, and *classified board*. 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.

with potentially confounding events, such as earnings releases, during the [2,40] period. The regressions continue to document a short-run overreaction and subsequent price correction for the connected firms.

B. Real Effects on the Bidding Process

In this section, we ask whether journalist connections also distort the bidding process. In most bidding models, agents are unconstrained in their information processing abilities. However, a growing literature has shown that people are often guided by simple heuristics even in competitive auctions (Lacetera, Pope and Sydnor, 2012). Related to these observations, models of inattention postulate that salience distorts people's reaction to information (Chetty, Looney and Kroft, 2009; DellaVigna, 2009). We study whether heuristic thinking induced by salient media stories affects M&A bids. Given the high stakes of takeovers, any distortion to the bidding process could generate effects of first-order importance affecting the economy.[27]

[27] In this section, we focus on the impacts of the *WSJ* following the evidence in Table 5 that *WSJ* connections are most significantly correlated with stock market returns. Because the *WSJ* is more influential than the *NYTimes* among finance professionals, it is more likely to have impacts on the bidding process. We repeat all the tests in this section for the *NYTimes* and, in most cases, find insignificant results.

First, we study the public competition. We examine bid competition following the *WSJ* publication. If managers react disproportionately to a connected glowing article, we would expect connected M&A stories to attract more competing bids. We show that this is the case. In column 1 of Table 8, we use a probit model to predict public bid competition. The dependent variable is an indicator for competing bids received by the target firm over [0,40] following the news publication. Our regression specification is otherwise similar to that in equation (4). The result suggests that connected articles are associated with significantly more future bids. In column 2, we explore the causal effects of connection by instrumenting connected coverage with journalist turnovers. In this subsample analysis, we obtain qualitatively similar results with the IV estimation. The marginal effect of connection is to increase the probability of a bid competition by approximately 18 percent.

Given higher competition in connected bids, the challenged bidder may either revise the bid price or exit the auction. Columns 3 through 6 of Table 8 test these hypotheses. In columns 3 and 4, we examine whether the connected bidder is more likely to raise the bid price. The results show that these bidders are significantly more likely to revise their offers upward after the media publication. The economic effect is large in our IV results (column 4): At 28 percent, it more than doubles the predicted probability of a bid revision (at sample means) of 11 percent. Such bid price revisions may be unjustified by the investment synergies.

Finally, we examine deal consummation. With the probit model in column 5, we do not find evidence that a connected bid is more likely to be withdrawn. The insignificant impact on consummation could be partially explained by the fact that connected bidders pay a higher bid price. However, when we instrument connection with journalist turnovers, we find that connected bids are significantly less likely to be completed in this IV subsample (column 6).

Discussion: We draw an important distinction between our results and previous studies on media directors. A key difference is that our findings are not driven by information channels. For example, Hossain and Javakhadze (2020) note that companies whose directors are connected to media are more likely to initiate takeover bids and negotiate lower takeover premium. Unlike journalists, media directors are directly involved in the merger initiation and negotiation. In our online Appendix (Table A.12), we report additional tests that examine other deal outcomes, such as initial deal premium during merger negotiations. We do not find

Table 8: Real Effects on Bidding Process

	Competing bids over [0,40]		Bid price upward revision		Bidding success	
	Probit	2SLS	Probit	2SLS	Probit	2SLS
	1	2	3	4	5	6
CONNECT_WORK	0.332**	0.180*	0.263*	0.276***	-0.033	-0.071*
	(0.163)	(0.094)	(0.147)	(0.106)	(0.112)	(0.038)
Controls	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Observations	1,131	318	1,131	318	1,131	318
(Pseudo) R^2	0.276	0.297	0.274	0.256	0.449	0.960

Notes: This table tests whether journalist connections are associated with real effects in M&A bids. Columns 1 and 2 examine bid competition. The dependent variable equals one if a competing bid is received in the [0,40] window after the news publication, and zero otherwise. The dependent variable in columns 3 and 4 equals one if an upward price revision is received by the target, and zero otherwise. Columns 5 and 6 examine deal consummation. The dependent variable equals one if the deal is completed, and zero otherwise. In odd-numbered columns, probit models are used. In even-numbered columns, 2SLS regressions are used in which journalist connection is instrumented with connected journalist turnovers. Control variables include *relative deal size*, *toehold*, *hostile*, *unsolicited*, *cross-industry*, *financing (all cash)*, *financing (all equity)*, *firm size*, *Tobin's Q*, *leverage*, *cash*, *profitability*, *CEO age*, *CEO duality*, and *classified board*. 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.

that journalist connections are associated with these other outcomes. The difference in our findings reflects distinct economic channels through which these effects operate.

Finally, our findings of higher bidding contests and offer prices do not necessarily imply that company managers are hurt by their journalist connections. In fact, an extensive literature in finance suggests that managers benefit from short-run favorable stock market responses, which their friends in media bring. For example, Lehn and Zhao (2006) find that bidding CEOs are less likely to be fired when the initial deal announcement returns are higher. Gong, Louis and Sun (2008) show that better short-term returns reduce the litigation risks faced by bidding managers. On the other hand, our results suggest detrimental effects to capital allocation and shareholders' interests.

V. Robustness Tests and Extensions

A. *Robustness tests*

In this section we perform a number of robustness tests. We first evaluate positive slant, defined as the fraction of positive words from Loughran and McDonald (2011)'s Financial Word List. We use our preferred baseline specification in equation (2) and include all articles from the *WSJ* and *NYTimes*. Table 9 presents the results. Columns 1 and 2 show that, unlike negative slant, the difference in positive slant is statistically indistinguishable from zero between connected and independent media stories. These findings indicate that media spin is achieved mainly through the exclusion of critical views in a journalist's subjective reporting rather than the inclusion of positive comments.

How about "net slant," namely, the fraction of negative words minus positive words? In columns 3 and 4, we reproduce our baseline estimate with the alternative sentiment measure, in which we remove the percentage of positive words from the negative ones. In addition, we measure negative media slant using the term frequency–inverse document frequency (TF-IDF) method. TF-IDF weights each word by its importance in the documents and has been advocated in the natural language processing literature.[28] Overall, the results are in line with our baseline findings, supporting a connection bias.

In online Appendix Table A13, we report additional robustness checks. These checks include alternative fixed effects (e.g., journalist fixed effects) and statistical specifications. All of these tests confirm the robustness of our findings.

B. *External validity: Evidence from financial fraud*

Our main tests are based on a sample of M&A news. Although we believe corporate takeovers provide a relatively clean context to test for connection bias, a natural question is whether our findings extend to other events. We certainly acknowledge that bias is more likely in big events about which there can be a lot of discretion. Precisely because these events are impactful, the opinion in the media could matter. A similar setting is financial fraud: Firms and their

[28] Loughran and McDonald (2011) and Garcia, Hu and Rohrer (2020) show that tf-idf approach improves the predictive power relative to the word-count approach. In online Appendix 3, we provide a detailed description.

Table 9: Robustness Tests

	Positive slant		"Net" slant		TF-IDF method: negative slant	
	1	2	3	4	5	6
CONNECT_WORK	-0.037		-0.233**		-1.395***	
	(0.055)		(0.094)		(0.530)	
CONNECT_UNIVERSITY		-0.018		-0.577**		-2.653**
		(0.180)		(0.234)		(1.049)
Controls as in Table 2	YES	YES	YES	YES	YES	YES
Deal FE	YES	YES	YES	YES	YES	YES
Media outlet FE	YES	YES	YES	YES	YES	YES
Observations	970	970	970	970	970	970
R^2	0.613	0.612	0.665	0.665	0.658	0.656

Notes: Robustness tests for the results in Table 2. Columns 1–2 assess positive slant, defined as the fraction of positive words in an article. Columns 3–4 examine "net" slant, defined as the proportion of (negative − positive) words. In columns 5 and 6, term frequency–inverse document frequency (TF-IDF) method is used to calculate negative slant. Control variables include *local journalist*, *female*, *tenure*, and *industry expert*. Standard errors are clustered by event and reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

executives charged with fraud are faced with mounting public scrutiny and news sentiment could either amplify or attenuate public responses to such crises. We examine media coverage of financial fraud in this section. Our purpose is to assess external validity by applying our analysis in another important context where we expect journalist connections matter.

We follow the literature on accounting fraud to focus on the SEC's investigations of alleged violations of the securities laws (e.g., Files, 2012; Kogan, Moskowitz and Niessner, 2020). We identify fraudulent firms by searching SEC's Accounting and Auditing Enforcement Releases (AAER) during the years 1997–2016. For media coverage, we use Factiva to collect publications in the *WSJ* and the *NYTimes*. The final sample consists of 233 and 158 articles in the *WSJ* and the *NYTimes*, respectively. Unsurprisingly, these articles are much more negative than the M&A stories: For both newspapers, the mean value of negative slant is 4.3%. The two-day abnormal returns to these events are significantly negative, at -4% on average.

Table 10 reports the extended analysis. First, we examine connection bias in media slant by employing our baseline regression in equation (2). We pool fraud news from both newspapers and use firm fixed effects to control for the fraud event. Columns 1 and 2 show that the estimated coefficients on *CONNECT* are statistically significant at the 1 percent level. The

Table 10: Articles on Financial Fraud

	Negative slant: *WSJ+NYTimes*		*WSJ* CAR		*NYTimes* CAR	
	1	2	3	4	5	6
CONNECT_WORK	-0.836***		0.038*		0.068**	
	(0.223)		(0.021)		(0.027)	
CONNECT_UNIVERSITY		-1.413***		0.273*		0.094
		(0.415)		(0.143)		(0.063)
Fixed effects	Event, media	Event, media	Year, industry	Year, industry	Year, industry	Year, industry
Controls	YES	YES	YES	YES	YES	YES
Observations	391	391	201	201	134	134
R^2	0.428	0.436	0.160	0.202	0.060	0.057

Notes: This table probes the external validity, using a sample of news articles on corporate financial fraud. Fraud events are collected from SEC's Accounting and Auditing Enforcement Releases from 1997 to 2016. News articles reporting these frauds are from the *WSJ* and the *NYTimes*. Columns 1 and 2 examine the connection effect on media slant. The dependent variable is *Negative slant*. Columns 3 through 6 examine market reactions to news articles. The dependent variable is CAR[0,1], where day 0 is the news publication date. Control variables include *local journalist*, *gender*, *tenure*, *number of publications*, *industry expert*, *article length*, *firm size*, *Tobin's Q*, *leverage*, *cash*, and *profitability*. Fixed effects are indicated at the end of each panel. Standard errors are reported in parentheses and are clustered by year and by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

economic magnitude is also significant. Relative to sample means, the articles authored by a journalist with a working relationship contain 19.4% fewer negative words (column 1). For university ties, the magnitude is 32.9% (column 2). In our online Appendix, we run regressions for cross-sectional difference in slant for each newspaper independently. Consistently, we detect a connection bias in both newspapers (see Table A.14).

From columns 3 through 6 of Table 10, we study the impact on the stock returns. The dependent variable is CAR[0,1] for a set of firms with the stock data available on the news publication date.[29] The evidence indicates more positive returns to the firms covered by connected reporters. Though these estimates are obtained with a relatively small number of observations, given the strong and consistent evidence from our main analysis, it is tempting to formulate similar causal explanations for the impact of journalist connections here.

[29] We lose some observations in this test because, in several cases, trading is halted for the firms upon the news of SEC investigation. For example, on May 15, 2002, the Nasdaq Stock Market halted trading of Adelphia's shares as the stock exchange requested additional information from the firm upon SEC's investigation.

VI. Conclusions

What influences a journalist's slant? This paper investigates journalist's social networks with companies. Specifically, we ask two questions: (1) Do journalists' connections bias their news tone, and (2) if they do, do the connections have an impact on equity pricing and capital allocation? We find that the answer to both questions is "yes." Using a sample of takeover news from the *WSJ* and the *NYTimes*, we find such connections are associated with significantly less use of negative words in news articles, and the upbeat optimism of friendly reporters is related to markedly better stock reactions. Following the media publication, connected transactions experience higher bid competition and receive greater final bid premium. These findings suggest that the connection bias is economically meaningful. To validate our main conclusion, we extend the analysis to the media coverage of financial fraud and document consistent evidence.

Despite the strong evidence of connection bias and its impacts, it is important to note that we do not claim journalists are always consciously biasing their coverage. As the theories suggest, a bias could be conscious if it is motivated by a *quid pro quo*. On the other hand, subconscious bias could exist because of homophily in the social network—after all, the ethical costs of a conscious bias are so high that, for many, they could defeat the motivation of being a reporter in the first place. Although we document the existence of a *quid pro quo* bias with the quasi-experiment of the *WSJ* ownership change, the extent to which any bias is conscious remains an interesting question for future work.

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For Online Publication

Online Appendix

Friends in Media

Guosong Xu

1. Examples of news coverage about M&A deals

Example 1. NGC Corp's acquisition of Destec Energy

— The *WSJ* analysis (extracted from "*NGC Corp. Agrees to Acquire Destec Energy for $1.27 Billion*" by Carlos Tejada):

"Whether natural-gas and electricity combinations make money has yet to be proved. Some observers wonder whether it makes sense to own generating assets, [...]. "Will it work? We won't know for the next couple of years," said M. Carol Coale [...]

Also unclear is whether Destec's sales contracts, which cover two-thirds of the power it produces, will be worth in the future what they're worth today. As electricity prices drop, the company might have to renegotiate those contracts at less favorable terms, analysts said."

— The *NYTimes* analysis (extracted from "*Growing Natural-Gas Seller To Expand Electric Business*" by Agis Salpukas):

"Larry Crowley, an energy-industry analyst at Jefferies & Company, said the acquisition of Destec was "a very positive move," giving NGC a source of low-cost power and a base from which to build up its marketing of electricity."

Example 2. Cisco's acquisition of Scientific-Atlanta

— The *WSJ* analysis (extracted from "*Cisco's Bid*" by Mark Gongloff):

"With today's deal, Cisco is taking a big plunge, and its profit margins could suffer as a result. Digesting a company the size of Scientific-Atlanta will take time and not a little effort. In the meantime, investors already worried about Cisco's anemic growth rate may find little reason to jump back into the stock, which has fallen some 11% in the past year."

— The *NYTimes* analysis (extracted from "*Cisco Set to Enter Cable Field*" by Matt Richtel and Ken Belson):

"The news highlights a desire by equipment makers to take advantage of the growing convergence of Internet technology, telecommunications and entertainment. With the deal, Cisco will, for the first time, be able to sell digital television equipment that provides high-definition programming [...]"

Example 3. PepsiCo's acquisition of Quaker Oats

— The *WSJ* analysis (extracted from "*PepsiCo Develops Appetite For Quaker's Snack Foods*" by Betsy McKay and Jonathan Eig):

"There is no question that PepsiCo Inc. wanted Quaker Oats Co. for its powerhouse Gatorade sports drink. But Pepsi is also looking forward to munching on Quaker's snack foods. Quaker's food business [...] is highly profitable, and some of its products can enhance Pepsi's goal of becoming the leading seller of convenience foods and beverages, PepsiCo executives say."

— The *NYTimes* analysis (extracted from "*PepsiCo Sets a New Course With Deal for Quaker Oats*" by Greg Winter):

"But the scale of the acquisition [...] also introduces a measure of uncertainty at a time when Pepsi's new leadership is just taking over.

"There is a certain amount of transaction and execution risk in any deal," said Skip Carpenter, [...] "Now you have some more managerial risk than you did before, too.""

Example 4. Pfizer's acquisition of Wyeth

— The *WSJ* analysis (extracted from "*Pfizer Deal to Buy Wyeth Leaves Doubts*" by Jonathan Rockoff and Shirley Wang):

"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. [...] some of them expressed doubts about how the newly created behemoth [...] would discover enough new products to generate growth. "Moving that needle is going to be extraordinarily difficult," said Timothy Anderson, a health-care analyst [...]"

— The *NYTimes* analysis (extracted from "*Pfizer Agrees to Pay $68 Billion for Rival Drug Maker Wyeth*" by Andrew Ross Sorkin and Duff Wilson):

"Pfizer appears to be taking advantage of the bad market for credit to buy Wyeth at a lower price than it might fetch if competing bids were to emerge, [...] "They have a unique opportunity now because not everybody can get that capital," said Barbara Ryan, an analyst at Deutsche Bank."

2. Constructing journalist turnovers

We construct the instrumental variable (IV), *turnover*, in the following way. First, for each M&A bid by firm i published at time t, we record $CONNECT\_WORK$ to journalist j as described in Section II. We then extract the information about when j joined and left the media from the newspaper website and Linkedin. Next, we count the number of connections (N) for firm i when the next bid is covered at $t+1$. If a connected journalist left the newspaper in the six-month window prior to $t+1$, we set the turnover rate of firm i at $t+1$ as $1/N$. Similarly, if n connections turn over, the turnover rate of firm i at $t+1$ is n/N. As a result of this exercise, we can construct the turnover sample only for the bidders that appear more than once in our media sample. We also keep the firms that have at least one journalist connection (for fairer comparisons). Finally, to avoid the replacement of connected journalist, we examine only the cases if the turnover rate equals 1 or 0 (meaning that *all* journalist connections are lost or no turnover). The IV, *turnover*, equals 1 if the turnover rate is one, and 0 otherwise. Hence, we exclude the turnover cases in which only part of the firm's journalist connections left the newspaper.

3. TF-IDF sentiment analysis

Tf-idf stands for *term frequency–inverse document frequency* and is a statistical approach in information retrieval that measures how relevant a word is to a document in a corpus. A tf-idf score is the multiplication of two metrics: term frequency, which measures the number of times a given word appears in an article, and inverse document frequency, which assesses how frequent (or rare) the word appears in the collection of documents.

In our applications, we measure term frequency using the augmented frequency to prevent a bias towards longer documents. Denote the raw count of word t in document d as f_{td}, then

$$tf(t,d) = \frac{f_{td}}{\max\{f_{t'd} : t' \in d\}}$$

The inverse document frequency for t is defined as:

$$idf(t) = log \frac{N}{count(d \in D : t \in d)}$$

in which N is the total number of documents in the corpus D.

Therefore, $tfidf(t,d) = tf(t,d) \times idf(t)$.

We then sum the tf-idf scores of negative words in an article and take its natural logarithm as our measure of negativity.

Table A1. Correlations between Journalist Connections and Event Characteristics

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1) CONNECT_WORK	1.000										
(2) CONNECT_UNIVERSITY	0.043	1.000									
(3) Local journalist	0.130*	0.029	1.000								
(4) Female	0.044	0.054	0.007	1.000							
(5) Tenure	0.057	-0.027	-0.003	-0.086*	1.000						
(6) Industry expert	0.146*	-0.017	-0.003	0.078*	0.064*	1.000					
(7) Relative deal size	-0.179*	0.010	-0.065*	-0.007	-0.021	0.002	1.000				
(8) Hostile bid	-0.034	0.008	-0.046	-0.031	-0.007	0.029	0.086*	1.000			
(9) Unsolicited bid	-0.017	-0.006	0.000	-0.003	0.004	0.039	0.123*	-0.028	1.000		
(10) Firm size	0.395*	0.013	0.087*	0.060*	0.057	0.135*	-0.222*	0.020	-0.013	1.000	
(11) Firm profitability	0.037	-0.001	0.005	-0.030	0.007	0.032	-0.100*	0.016	0.014	0.352*	1.000

Notes: The table reports pairwise correlations between journalist characteristics and event (deal) characteristics. The sample is based on the *WSJ*. Symbol * indicates statistical significance at the 5% or higher.

Table A2. Determinants of Media Coverage

	WSJ coverage		NYTimes coverage	
	Probit coef.	*Marginal effect*	*Probit coef.*	*Marginal effect*
Journalist connection	-0.169	[-0.067]	0.087	[0.014]
	(0.104)		(0.110)	
Firm size	0.594***	[0.234]	0.567***	[0.091]
	(0.053)		(0.046)	
Deal relative size	0.779***	[0.307]	0.575***	[0.092]
	(0.105)		(0.095)	
Hostile bid	0.728**	[0.287]	0.822***	[0.132]
	(0.337)		(0.179)	
Unsolicited bid	0.405***	[0.160]	0.428***	[0.069]
	(0.054)		(0.145)	
Cross-industry	-0.205***	[-0.081]	-0.126	[-0.020]
	(0.057)		(0.085)	
All cash deal	-0.569***	[-0.225]	-0.410***	[-0.066]
	(0.131)		(0.147)	
All stock deal	0.014	[0.006]	0.144	[0.023]
	(0.070)		(0.161)	
#Analysts	0.003	[0.001]	0.004	[0.001]
	(0.007)		(0.005)	
Institutional ownership	0.517***	[0.204]	0.409**	[0.066]
	(0.141)		(0.190)	
Tobin's Q	0.109***	[0.043]	0.084***	[0.013]
	(0.026)		(0.027)	
Leverage	-1.302***	[-0.514]	-1.587***	[-0.255]
	(0.351)		(0.345)	
Cash holdings	0.398	[0.157]	0.484	[0.078]
	(0.292)		(0.406)	
Profitability	-0.658***	[-0.260]	-1.336***	[-0.214]
	(0.254)		(0.266)	
Past return	41.616***	[16.422]	63.352***	[10.163]
	(16.012)		(16.174)	
Industry FE	YES		YES	
Year FE	YES		YES	
HQ state FE	YES		YES	
Observations	2,387		2,387	
Pseudo R^2	0.381		0.371	

Notes: Probit regression that predicts whether a deal is covered by the *WSJ* or by the *NYTimes*. The dependent variable is a dummy variable that indicates *WSJ/NYTimes* coverage. *Journalist connection* is a indicator variable that equals one if a firm has a journalist connection (i.e., by a working relationship or a schooling tie), and zero otherwise. Definitions for the other variables are listed in Table 1. Marginal effects at the sample means are reported in even-numbered columns. A constant term is estimated but not reported. Standard errors are reported in parentheses and are double-clustered by industry and by year. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table A3. Cross-sectional Test of Media Slant

	WSJ				*NYTimes*			
	1	2	3	4	5	6	7	8
CONNECT_WORK	-0.306***	-0.247**			-0.223***	-0.249**		
	(0.061)	(0.099)			(0.072)	(0.098)		
CONNECT_UNIVERSITY			-0.383**	-0.336*			-0.384	-0.577**
			(0.143)	(0.176)			(0.245)	(0.234)
Local journalist	-0.031	-0.068	-0.046	-0.080	0.021	-0.049	-0.054	-0.148
	(0.087)	(0.145)	(0.083)	(0.137)	(0.153)	(0.187)	(0.150)	(0.179)
Female	-0.037	0.100	-0.042	0.111	-0.157	0.064	-0.131	0.098
	(0.067)	(0.109)	(0.066)	(0.110)	(0.115)	(0.192)	(0.116)	(0.185)
Tenure	0.000	0.000	-0.000	-0.000	-0.001*	-0.001	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.002)	(0.000)	(0.002)
Industry expert	-0.060	-0.200**	-0.092	-0.226***	-0.227**	-0.123	-0.219**	-0.101
	(0.071)	(0.074)	(0.071)	(0.072)	(0.091)	(0.125)	(0.093)	(0.126)
Relative deal size	0.088**	0.139**	0.085*	0.140**	-0.021	-0.005	-0.026	-0.007
	(0.041)	(0.055)	(0.042)	(0.056)	(0.063)	(0.075)	(0.061)	(0.084)
Toehold	0.012*	0.008	0.012*	0.006	-0.005	-0.014*	-0.006	-0.015
	(0.006)	(0.010)	(0.006)	(0.010)	(0.006)	(0.008)	(0.006)	(0.009)
Hostile	0.947***	0.967***	0.975***	0.989***	0.714***	0.769***	0.709***	0.731***
	(0.152)	(0.133)	(0.152)	(0.142)	(0.149)	(0.261)	(0.130)	(0.227)
Unsolicited	0.648***	0.587***	0.653***	0.600***	0.458***	0.542***	0.530***	0.605***
	(0.095)	(0.111)	(0.101)	(0.118)	(0.116)	(0.152)	(0.120)	(0.156)
Cross-industry	0.087*	0.060	0.091**	0.074	0.095	0.054	0.081	0.057
	(0.043)	(0.076)	(0.043)	(0.076)	(0.099)	(0.119)	(0.100)	(0.115)
Financing (cash payment)	-0.000	0.000	-0.000	0.000	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Firm size	0.074**	0.079*	0.036	0.052	0.093	0.103*	0.068	0.075
	(0.026)	(0.046)	(0.027)	(0.039)	(0.061)	(0.055)	(0.058)	(0.054)
Tobin's Q	0.000	-0.004	-0.003	-0.006	-0.022**	-0.007	-0.027***	-0.014
	(0.012)	(0.013)	(0.012)	(0.014)	(0.010)	(0.013)	(0.009)	(0.012)
Firm leverage	0.156	-0.148	0.280	-0.021	-0.376	0.076	-0.335	0.086
	(0.258)	(0.306)	(0.253)	(0.304)	(0.362)	(0.309)	(0.363)	(0.331)
Firm cash	0.164	0.380	0.153	0.393	0.252	0.303	0.170	0.194
	(0.266)	(0.358)	(0.296)	(0.358)	(0.313)	(0.245)	(0.321)	(0.233)

Continued on next page

Firm profitability	-0.654**	-0.850*	-0.527*	-0.762*	-0.285	-0.207	-0.351	-0.363
	(0.298)	(0.433)	(0.268)	(0.412)	(0.375)	(0.539)	(0.346)	(0.376)
CEO age	0.007	0.009	0.007	0.009	-0.002	-0.002	-0.003	-0.002
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
CEO duality	-0.093	-0.212***	-0.097	-0.222***	-0.125	-0.040	-0.123	-0.036
	(0.065)	(0.073)	(0.069)	(0.073)	(0.122)	(0.139)	(0.124)	(0.140)
Classified board	-0.123*	-0.120*	-0.128**	-0.125**	0.040	0.014	0.041	0.007
	(0.060)	(0.068)	(0.055)	(0.058)	(0.063)	(0.101)	(0.063)	(0.092)
Constant	0.484	0.422	0.757*	0.558*	1.049	0.694	1.285**	0.915*
	(0.381)	(0.318)	(0.371)	(0.279)	(0.608)	(0.510)	(0.587)	(0.492)
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES
Location FE	YES	YES	YES	YES	YES	YES	YES	YES
Journalist FE	NO	YES	NO	YES	NO	YES	NO	YES
Observations	1,131	1,131	1,131	1,131	485	485	485	485
R^2	0.216	0.421	0.205	0.416	0.352	0.431	0.348	0.430

Notes: The table shows the effect of journalist connections on negative slant. We run the following regression:

$$Negative\ slant_{it} = \alpha + \beta \cdot CONNECT_{it} + \gamma \cdot Controls_{it} + Fixed\ effects + \epsilon_{it}$$

where *Negative slant* is the fraction of negative words in the text of a news article. i indicates a transaction, and t indicates event year. *Fixed effects* include news year, industry (Fama-French 48), and (firm and journalist) location fixed effects. In even-numbered columns, we also include journalist fixed effects. Columns 1 to 4 examine the articles in the *WSJ*. Columns 5 to 8 examine the *NYTimes*. Standard errors are double-clustered by year and by industry, and are reported in parentheses. Definitions of control variables are in Table 1. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table A4. Cross-sectional Test of Media Slant (Heckman's Model)

	WSJ				***NYTimes***			
	1	2	3	4	5	6	7	8
CONNECT_WORK	-0.322***	-0.274**			-0.226***	-0.218**		
	(0.075)	(0.115)			(0.071)	(0.093)		
CONNECT_UNIVERSITY			-0.373**	-0.303			-0.403	-0.558**
			(0.152)	(0.188)			(0.277)	(0.240)
Heckman's lambda	0.138	0.154	0.072	0.075	0.102	0.160	0.166	0.266
	(0.112)	(0.163)	(0.103)	(0.144)	(0.129)	(0.185)	(0.153)	(0.197)
Controls as in Table A.2	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES
Location FE	YES	YES	YES	YES	YES	YES	YES	YES
Journalist FE	NO	YES	NO	YES	NO	YES	NO	YES
Observations	1,131	1,131	1,131	1,131	485	485	485	485
R^2	0.219	0.423	0.207	0.417	0.360	0.434	0.356	0.436

Notes: This table shows the robustness tests for the results in Table A3. We use the Heckman (1979) two-stage procedure to account for possible sample selection bias. In the first stage, media coverage is predicted by the following factors: journalist connections, firm size, deal relative size, hostile attitudes, unsolicited bid, cross-industry bid, all-cash financing, all-stock financing, # analyst coverage, Tobin's Q, leverage, cash holdings, profitability, past year stock returns, Fama-French 49 industries, HQ state, and year dummies. Heckman's lambda (or the inverse Mills ratio) is calculated based on the first stage and included in the second stage. In the second stage, the dependent variable is *Negative slant*. The same set of control variables and fixed effects as in Table A.3 are included. Definitions for the variables are listed in Table 1. Standard errors are reported in parentheses and are double-clustered by news year and by industry. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table A5. Are Journalist Turnovers Related to Firm Performance?

	WSJ	NYTimes
	1	2
Firm size	0.007	-0.000
	(0.007)	(0.001)
Institution ownership	0.001	-0.017
	(0.004)	(0.018)
# Analysts	-0.000	0.000
	(0.000)	(0.000)
Tobin's Q	-0.000	0.001
	(0.001)	(0.001)
Leverage	-0.040	0.014
	(0.039)	(0.021)
Cash holding	0.016	0.009
	(0.015)	(0.011)
Profitability	-0.049	-0.054
	(0.046)	(0.058)
NYC headquarter	0.009	0.007
	(0.008)	(0.008)
California headquarter	0.004	0.003
	(0.005)	(0.005)
Past year stock return	0.593	-1.049
	(0.632)	(1.148)
CEO age	-0.001	-0.000
	(0.001)	(0.000)
CEO duality	0.002	0.007
	(0.004)	(0.007)
Classified board	-0.001	-0.002
	(0.002)	(0.003)
Year FE	YES	YES
Industry FE	YES	YES
Observations	318	299
R^2	0.100	0.313

Notes: This table examines whether journalist turnovers are related to various firm characteristics. The sample is the same as that in Table 3. The dependent variable is a dummy variable that equals one if a firm's connection turns over before news coverage, and zero otherwise. We use an OLS model with year and industry fixed effects. A constant term is estimated but not reported. Standard errors are reported in parentheses and are double-clustered by year and by industry. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table A6. Connections to News Corp or Rupert Murdoch

Nature of relationship	**Percent**	**Example**
Common directors/executives	60%	Oracle
Business relations (e.g., asset purchase reported in 10K)	31%	Electronic Arts
Rupert Murdoch is an executive	9%	21st Century Fox

Notes: This table lists the nature of relationship between a bidder firm and News Corp (or Rupert Murdoch) in our media sample.

Table A7. Journalist Connections and Stock Returns—Full Model with Control Variables

	WSJ CAR			*NYTimes* CAR		
	OLS	OLS	2SLS	OLS	OLS	2SLS
	1	2	3	4	5	6
CONNECT_WORK	0.016**		0.034*	0.005		0.055***
	(0.008)		(0.020)	(0.009)		(0.017)
CONNECT_UNIVERSITY		0.001			0.010	
		(0.021)			(0.010)	
Relative deal size	-0.007	-0.007	0.004	-0.011**	-0.011**	-0.003
	(0.005)	(0.005)	(0.009)	(0.005)	(0.005)	(0.010)
Toehold	-0.000	-0.000	-0.001	-0.000	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Hostile	-0.026**	-0.027**	-0.026	-0.024*	-0.024*	-0.030**
	(0.010)	(0.010)	(0.017)	(0.012)	(0.013)	(0.013)
Unsolicited	0.003	0.003	-0.041**	-0.009	-0.010	-0.005
	(0.010)	(0.010)	(0.018)	(0.014)	(0.015)	(0.015)
Cross-industry	-0.005	-0.005	0.013	0.003	0.003	0.018**
	(0.005)	(0.004)	(0.010)	(0.006)	(0.007)	(0.009)
Financing: All cash	0.010	0.009	0.010	0.017*	0.017*	0.022**
	(0.007)	(0.008)	(0.014)	(0.009)	(0.009)	(0.009)
Financing: All equity	-0.003	-0.004	0.003	0.009	0.009	0.016
	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)	(0.011)
Firm size	-0.000	0.002	-0.003	-0.006***	-0.006**	-0.013***
	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.004)
Tobin's Q	0.000	0.000	0.002	0.003***	0.003***	-0.000
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)
Firm leverage	0.008	0.000	-0.022	0.048	0.048	0.026
	(0.023)	(0.023)	(0.061)	(0.050)	(0.050)	(0.109)
Firm cash	-0.034	-0.032	-0.008	-0.077**	-0.075**	-0.005
	(0.022)	(0.023)	(0.031)	(0.030)	(0.030)	(0.031)
Firm profitability	-0.024	-0.030	-0.120***	-0.082*	-0.080*	-0.162
	(0.037)	(0.040)	(0.039)	(0.042)	(0.042)	(0.120)
CEO age	-0.000	-0.000	0.000	0.001	0.001	0.002**
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
CEO duality	-0.004	-0.004	0.002	-0.017	-0.017*	-0.011

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	(0.005)	(0.005)	(0.004)	(0.010)	(0.010)	(0.009)
Classified board	-0.003	-0.003	-0.032***	-0.006	-0.006	-0.003
	(0.004)	(0.004)	(0.003)	(0.007)	(0.007)	(0.008)
Constant	0.014	-0.001	-0.002	0.015	0.010	0.085
	(0.033)	(0.034)	(0.049)	(0.040)	(0.040)	(0.076)
Time FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Observations	1,131	1,131	318	485	485	287
R^2	0.126	0.120	0.292	0.181	0.181	0.114

Notes: Full model of regressions of bidders' cumulative abnormal returns (CAR) as reported in Table 5. Columns 1, 2, 4, and 5 employ OLS regressions; columns 3 and 6 utilize 2SLS regressions, where *CONNECT_WORK* is instrumented with connected journalists' turnover (the first stage results appear in Table 3). In all regressions, year and industry dummies are included. Definitions for the variables are listed in Table 1. Standard errors, which are reported in parentheses, are double-clustered by news year and by industry. Symbols ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table A8: Robustness Tests for Journalist Connections and Stock Returns

	Alternative windows		Publications on M&A announcement day	
	CAR[-1,1]	CAR[*PR*-1, *WSJ*+1]	CAR[0,1]	CAR[-1,1]
	1	2	3	4
CONNECT_WORK	0.031**	0.029**	0.019*	0.023**
	(0.012)	(0.011)	(0.011)	(0.011)
Controls	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Observations	1,131	1,131	449	449
R^2	0.194	0.172	0.271	0.292

Notes: The table reports the robustness tests for Table 5. We use alternative short-term windows to calculate CAR. Column 1 shows the window [-1,+1] around news publication; column 2 shows the window from one day before the merger announcement until one day after *WSJ* publication. Columns 3 and 4 use the subsample of news articles published on the same day as the official M&A announcement day. Control variables include *relative deal size*, *toehold*, *hostile*, *unsolicited*, *cross-industry*, *financing (all cash)*, *financing (all equity)*, *firm size*, *Tobin's Q*, *leverage*, *cash*, *profitability*, *CEO age*, *CEO duality*, and *classified board*. Definitions for the other variables are listed in Table 1. Standard errors appear in parentheses and are clustered by year and by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table A9: Stock Reactions to Journalist Connections for Same-day Publications

	1	2
CONNECT_WORK at *WSJ*	0.018*	0.016
	(0.010)	(0.013)
CONNECT_UNIVERSITY at *WSJ*	0.057**	0.079**
	(0.025)	(0.034)
CONNECT_WORK at *NYTimes*	0.008	0.009
	(0.009)	(0.010)
CONNECT_UNIVERSITY at *NYTimes*	-0.009	-0.004
	(0.013)	(0.013)
Controls	YES	YES
Year FE	NO	YES
Industry FE	NO	YES
Observations	339	339
R^2	0.143	0.249

Notes: Regressions of bidders' cumulative abnormal returns (CAR) as reported in Table 5. The sample includes *WSJ* and *NYTimes* news publications on the same day. Control variables include *relative deal size*, *toehold*, *hostile*, *unsolicited*, *cross-industry*, *financing (all cash)*, *financing (all equity)*, *firm size*, *Tobin's Q*, *leverage*, *cash*, *profitability*, *CEO age*, *CEO duality*, and *classified board*. Definitions for the variables are listed in Table 1. Standard errors appear in parentheses and are clustered by year and by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table A10: Negative Slant in Front-page News

	1	2
Front page	0.019	-0.010
	(0.070)	(0.070)
CONNECT_WORK		-0.280***
		(0.060)
CONNECT_UNIVERSITY		-0.312*
		(0.158)
Controls	NO	YES
Year FE	YES	YES
Industry FE	YES	YES
Observations	1,131	1,131
R^2	0.077	0.179

Notes: The table compares the negative slant in front-page articles to that in non-front-page articles. The dependent variable is *Negative slant*. Column 2 controls for *local journalist*, *female*, *tenure*, *industry expert*, *relative deal size*, *toehold*, *hostile*, *unsolicited*, *cross-industry*, *financing (all cash)*, *financing (all equity)*, *firm size*, *Tobin's Q*, *leverage*, *cash*, *profitability*, *CEO age*, *CEO duality*, and *classified board.* Definitions for the variables are listed in Table 1. Standard errors are reported in parentheses and are clustered by year and by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table A11: Robustness Tests of Long-run Price Corrections

Sample that excludes withdrawn bids and confounding events over [2,40]				
	CAR[0,1]	**CAR[2,40]**	**CAR[0,40]**	**CAR[-1,complete]**
	1	2	3	4
CONNECT_WORK	0.017*	-0.033***	-0.011	0.038
	(0.008)	(0.009)	(0.013)	(0.040)
Controls	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Observations	670	670	670	552
R^2	0.161	0.137	0.135	0.130

Notes: Robustness tests of the results in Table 7. Starting from the *WSJ* sample as described in Table 7, we exclude withdrawn bids and firms with confounding news events over the window [2,40], where 0 is the news publication date. Confounding news is mainly earnings releases identified with the Institutional Brokers' Estimate System. The dependent variable in column 1 is bidder's CAR over [0,1]. The dependent variable in column 2 is bidders' CAR over [2,40]. The dependent variable in column 3 is bidders' CAR over [0,40]. The dependent variable in column 4 is CAR from the day before the deal announcement until the deal completion date. Controls include *relative deal size*, *toehold*, *hostile*, *unsolicited*, *cross-industry*, *financing (all cash)*, *financing (all equity)*, *firm size*, *Tobin's Q*, *leverage*, *cash*, *profitability*, *CEO age*, *CEO duality*, and *classified board*. Definitions for the variables are listed in Table 1. Standard errors appear in parentheses and are clustered by year and by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table A12: Additional Tests of the Real Effects

	Initial premium		Return premium		Target CAR[0,1]	
	1	2	3	4	5	6
CONNECT_WORK	0.001		0.026		-0.021	
	(0.046)		(0.027)		(0.021)	
CONNECT_UNIVERSITY		-0.106		-0.021		0.011
		(0.074)		(0.088)		(0.067)
Controls	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Observations	952	952	1,090	1,090	1,103	1,103
R^2	0.168	0.169	0.203	0.203	0.217	0.216

Notes: This table examines whether *WSJ* journalist connections are related to various M&A bidding processes. Columns 1 and 2 examine initial deal premium, defined as negotiated offer price over target's market capitalization 4 weeks prior to the announcement (data source: SDC). Columns 3 and 4 examine return premium, defined as target CAR over [-42,5]. Columns 5 to 6 examine targets' CAR over [0,1], where day 0 is the *WSJ* article publication date. Control variables include *relative deal size*, *toehold*, *hostile*, *unsolicited*, *cross-industry*, *financing (all cash)*, *financing (all equity)*, *firm size*, *Tobin's Q*, *leverage*, *cash*, *profitability*, *CEO age*, *CEO duality*, and *classified board*. Definitions for the variables are listed in Table 1. Standard errors appear in parentheses and are clustered by year and by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table A13: Additional Robustness Tests of Media Slant (Table 2)

	Journalist FE		Alternative model		Winsorize slant	
	1	2	3	4	5	6
CONNECT_WORK	-0.146***		-0.170***	-0.144**	-0.168***	
	(0.056)		(0.046)	(0.056)	(0.045)	
CONNECT_UNIVERSITY		-0.486***	-0.472***	-0.480***		-0.434***
		(0.131)	(0.138)	(0.132)		(0.113)
Controls as in Table 2	YES	YES	YES	YES	YES	YES
Deal FE	YES	YES	YES	YES	YES	YES
Media outlet FE	YES	YES	YES	YES	YES	YES
Journalist FE	YES	YES	NO	YES	NO	NO
Observations	2,342	2,342	2,342	2,342	2,342	2,342
R^2	0.736	0.736	0.669	0.738	0.663	0.663

Notes: The table reports additional robustness tests of the results in Table 2. We use the same regression as specified in Table 2. In columns 1 and 2, we add journalist fixed effects. In columns 3 and 4, we include both *CONNECT_WORK* and *CONNECT_UNIVERSITY* in the same model specification. In columns 5 and 6, the dependent variable, *Negative slant*, is winsorized at 1 percentiles at both tails. We include the following control variables: *local journalist*, *female*, *tenure*, and *industry expert*. Definitions for the variables are listed in Table 1. Standard errors appear in parentheses and are clustered by deal event. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table A14: Cross-sectional Test of Media Slant using Fraud News

	WSJ		***NYTimes***	
	1	2	3	4
CONNECT_WORK	-0.759*		-0.919**	
	(0.370)		(0.375)	
CONNECT_UNIVERSITY		-1.087		-2.432***
		(0.634)		(0.701)
Controls	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Observations	233	233	158	158
R^2	0.340	0.341	0.415	0.467

Notes: The table reports the cross-sectional difference in slant using a sample of financial fraud news. We use the same regression specified in Table A2 with the following control variables: *local journalist*, *gender*, *tenure*, *number of publications*, *industry expert*, *firm size*, *Tobin's Q*, *article length*, *leverage*, *cash*, and *profitability.* Definitions for the variables are listed in Table 1. Standard errors appear in parentheses and are clustered by year and by industry. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.