Rumor Has It: Sensationalism in Financial Media

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Abstract

The media has an incentive to publish sensational news. We study how this incentive affects the accuracy of media coverage in the context of merger rumors. Using a novel dataset, we find that accuracy is predicted by a journalist's experience, specialized education, and industry expertise. Conversely, less accurate stories use ambiguous language and feature well-known firms with broad readership appeal. Investors do not fully account for the predictive power of these characteristics, leading to an initial target price overreaction and a subsequent reversal, consistent with limited attention. Overall, we provide novel evidence on the determinants of media accuracy and its effect on asset prices. (*JEL* G14, G34, L82)

The business press plays a key role in capital markets as a distributor of information (Tetlock, 2010; Engelberg and Parsons, 2011; Peress, 2013). This role is not passive, however, as business newspapers actively compete for readership. To win readers' attention, newspapers have an incentive to publish sensational stories, namely attention-grabbing, speculative news with broad readership appeal. Understanding this incentive is important. Media coverage that is skewed towards speculative stories, possibly at the expense of accuracy, could distort investors' beliefs and impact asset prices. While prior research shows that the incidence of media coverage influences financial markets, there is relatively little evidence on its accuracy.

In this paper, we study accuracy in the business press in the context of merger rumors. These stories attract a broad audience because mergers have a dramatic impact on a wide range of corporate stakeholders. For employees, customers, and rivals, mergers lead to layoffs, discontinued products, and increased competition, in addition to the 15–20% abnormal return realized by target investors. At the same time, merger rumors provide a convenient setting to study accuracy in the press because we can observe *ex post* whether a rumor comes true.

To illustrate the trade-off between readership appeal and accuracy, consider an article that appeared on the front page of the *Seattle Times* on September 2, 1993, entitled, "Could GE Buy Boeing? It's Speculation Now, But Not Entirely Far-Fetched." The article states,

> *A scenario by which fiercely independent Boeing succumbs to an opportunistic corporate raider has been quietly percolating in certain corners of Wall Street for the past year... GE's ambitious Chairman Jack Welch, 57, has been taking steps to position GE to make a major acquisition... Although he hasn't said so explicitly, Welch appears to covet Boeing.*

A letter to the editor that was published a few days later provides insight into how this article was received by readers. The letter states,

> *In my opinion, your paper chose to give this story front-page attention only for the purpose of selling newspapers. Unfortunately, judging by the fact that* The Times *newspaper box outside the gate where I work was empty when I left work (this is the first time I've noticed this occurrence), you succeeded. —J.J. Pruss, Bellevue*

This anecdote illustrates a number of interesting features of merger rumors. First, the article is designed to attract readers. Since Boeing is a major corporate presence in Seattle, a merger with GE would impact a large number of *Seattle Times* readers. Second, the article is written with provocative language that one might find in a paperback novel, such as 'opportunistic raider,' 'Boeing succumbs,' and 'Welch covets.' Finally, as the letter to the editor reveals, while not everyone was convinced by the article, the sensational reporting style was successful in selling newspapers. In the end, however, the rumor never materialized – GE never made a bid for Boeing.

We use merger rumors to investigate two main questions. First, which characteristics of media articles predict whether a rumor will come true? Second, do investors account for the characteristics that predict accuracy? While merger rumors allow us to address these questions in a relatively clean setting, we believe the answers can shed light on the accuracy of the business press in more general settings.

To answer these questions, we construct a novel database of merger rumors. We manually search Factiva to identify 'scoop' articles that first report a merger rumor, whether they appeared in the online or print edition of a newspaper. Our sampling procedure yields 501 unique merger rumors in 2000–2011. The aggregate book asset value of rumor targets in our sample is 31% of the aggregate book assets of over 2,000 public targets acquired in the same period. Consistent with an incentive to win readers' attention, newspapers are more likely to report rumors about newsworthy targets: large, public firms with recognizable brands and large advertising expenditures. For instance, 88% of targets in merger rumors are publicly traded, compared to 38% of targets in actual mergers. Also, 15% of rumor targets appear on league tables of the most valuable brands, compared to 1% for all merger targets.

Central to this paper is the definition of accuracy. One definition of accuracy of rumors is the literal definition. As long as a rumor is discussed in any setting, an article is literally accurate. A more relevant definition to newspaper readers is based on whether the merger materializes in the future, not whether someone is making idle speculation. Therefore, we define a rumor to be accurate if the rumor target receives an official takeover bid within one year. Using this definition, 33% of rumors in our sample are accurate. One concern with our definition is that a rumor that is true at the time of publication would be classified as inaccurate if the merger negotiations fail before a public announcement. To address this concern, we show that the determinants of rumor accuracy are unrelated to the likelihood of failed merger negotiations.

The accuracy of rumor articles has a significant impact on stock prices. Targets of accurate rumors earn an abnormal return of 6.7% on the rumor date, compared to 3.0% for targets of inaccurate rumors. This dichotomy implies that returns are informative about a rumor's accuracy. However, for the average firm, we find a significant reversal of −1.4% over the ten days following the publication of the rumor. This finding suggests that investors overestimate the accuracy of the average rumor.

To address our first question on the determinants of accuracy, we estimate logit regressions of the likelihood that a rumor comes true based on four sets of factors: the newsworthiness of the target, characteristics of journalists, details in the text of the article, and attributes of newspapers. Since some rumors likely circulate before they are published in a newspaper, in all of our tests we control for stale information using the run-up in the target's stock price before the rumor is published.

First, we find that rumors about newsworthy targets are significantly less likely to come true. This finding suggests that newspapers may be willing to publish rumors that are less accurate if they feature large, well-known firms with broad readership appeal.

Second, we find that characteristics of journalists significantly predict the accuracy of a rumor. Using a variety of independent sources, we hand-construct a comprehensive dataset on journalists' education, experience, and demographics. For example, we document that a third of the reporters in our sample majored in journalism in college and about half of the reporters are assigned to the New York bureau of national newspapers. We find that a journalist is more accurate if he is older, has an undergraduate degree in journalism, and specializes in the target's industry. These results are consistent with the intuitive explanation that a journalist with more experience and a relevant education is better able to assess a rumor's accuracy.

Third, details in the text of the article signal a rumor's accuracy. From the article text, we extract two types of information. First, we record several context-specific details, such as the alleged source of the rumor, the stage of the merger negotiations, and the disclosure of a takeover price. We find that accurate rumor articles are more likely to mention a specific takeover price, to discuss possible bidders, and to indicate that negotiations are in an advanced stage. Second, we measure the ambiguity of the article's text using the dictionary developed in Loughran and McDonald (2011). In particular, we find that an article's use of weak modal words, such as "maybe," "appears," and "conceivable," indicates that a rumor is less likely to come true.

Finally, we find that newspaper characteristics are less important for accuracy than journalist and article characteristics. While newspaper fixed effects help to explain accuracy, we cannot identify the specific characteristics that drive this result. In particular, a newspaper's age, circulation, form of ownership, and location are not significantly related to accuracy. Overall, in answer to our first question, we find that rumors are more likely to be accurate when the target firm is less newsworthy, when journalists are more experienced, and when the article text provides specific details and uses explicit language.

To address our second question on the impact of accuracy on stock prices, we develop an empirical model that identifies whether investors account for the factors that predict accuracy. In particular, in the logit tests described above, we control for the market's perception of the rumor's accuracy using the target's stock returns and the expected takeover premium. If the market's perception is correct, factors that determine media accuracy should have no explanatory power after controlling for the target's stock return. Factors that continue to have explanatory power must be overlooked by investors.

We find that stock prices do not fully reflect many of the factors that predict accuracy. After controlling for the market's response to the rumor, we still find that traits of newsworthy firms are significantly related to accuracy, as are characteristics of the article's text. Moreover, journalists' age, experience, and education remain significantly related to accuracy. Given the difficulty of observing many of these factors, it is plausible that the average newspaper reader is unaware of their importance, which contributes to a short-run mispricing in targets' stocks.

These results are consistent with the theory that limited attention leads investors to overlook valuable public information (Hirshleifer and Teoh, 2003; Hirshleifer, Lim, and Teoh, 2011) and supporting empirical evidence (Engelberg, 2008; Da, Gurun, and Warachka, 2013). While this previous work has focused on the behavior of investors, we show that limited attention has important implications for the media. Because readers' attention is limited, the media competes for their attention by publishing sensational news. These news stories skew the information environment and move asset prices.

Our results have several implications. First, we challenge the view that the business press is a passive conduit of financial information. Instead, we show that the media's incentive to attract readers is associated with more speculative reporting. This underscores the distinction of media articles from corporate disclosures, which are typically more informative for large, well-known firms. Second, our results show that while the media impacts asset prices, it also introduces noise through speculative articles. Finally, we uncover important cross-sectional variation in the media's accuracy. This variation implies that the relation between information and asset prices could vary based on who is relaying the information to investors.

The central contribution of this paper is to provide new evidence on the determinants of accuracy in the business press. Previous research shows that individual investors prefer stocks with attention-grabbing news (Barber and Odean, 2008; Da, Engelberg, and Gao, 2011). Our findings suggest that newspapers might sacrifice accuracy in order to appeal to individual investors. This provides one explanation why investors trade stocks based on narratives in newspaper articles, despite easy access to firms' press releases and analysts' reports (Engelberg and Parsons, 2011). Furthermore, because media speculation is difficult to disprove, our results help explain why media articles affect even the prices of large and widely-followed stocks (Tetlock, 2007). By identifying features of the text that predict accuracy, we also extend prior research on textual analysis in finance (Tetlock, Saar-Tsechansky, and Macskassy, 2008; Loughran and McDonald, 2011, 2014; Gurun and Butler, 2012). Finally, we provide new evidence on the role of journalists in the stock market. Dougal, Engelberg, Garcia, and Parsons (2012) show that the identity of the authors of a popular *Wall Street Journal* column helps to predict next-day market returns. We show that accuracy varies across journalists and identify specific characteristics that help to explain this variation.

1. Data and Summary Statistics

To collect merger rumors, we use a multi-step approach. First, using a wide filter, we identify target firms named in merger rumors. We focus on targets rather than bidders because targets experience larger stock price responses and changes in operations than do bidders. After we identify the rumor target, we search for the first article to report the rumor, or as we call it, the 'scoop' article.

More specifically, in the first step, we manually search the Factiva database using the following filters. First, we limit our sample dates to January 1, 2000 through December 31, 2011. Second, we search within Factiva's set of publications called "Major news and business publications: U.S." This set includes the 33 largest domestic newspapers. Within these bounds, we search for articles that include at least one of these words: "acquire," "acquisition," "merger," "deal," "takeover," "buyout," or "bid," and at least one of these words or phrases: "rumor," "rumour," "speculation," "said to be," or "talks." This search provides a noisy sample which we further refine by reading the articles to identify those that report merger rumors. For example, this first sample includes articles that discuss a merger and then an unrelated rumor, such as a rumor about a change in management. Once we identify a merger rumor, we extract the article text, the name of the target, the alleged bidders (if named), the media outlet, and the publication date.

Next, we search for the scoop article. To find the scoop, we first trace backward in time using the source of the rumor stated in the articles we have identified. When a rumor is re-reported, journalists typically cite a newspaper article that reported the story previously. In this second-pass search, we place no restriction on the newspaper's size or location. This means our sample includes foreign newspapers and small media outlets. In addition, our sample also includes online versions and blogs of print newspapers, such as Dealbook by the *New York Times*, which might publish rumors in advance of print versions. We follow the citation trail until we find an article that does not cite another media source. To verify that it is the scoop article, we search for all articles on the target firm starting one week before this potential scoop to find any previous articles on the rumor. In some cases, articles do not report a source. In these cases, we search backward in time for articles about the target firm until we find the earliest article that reports the rumor, using all sources in Factiva.

Using the scoop article date, we search for all articles that include the target's name in the following week to measure how widely a rumor is reported. From this sample, we read the articles to identify those that refer to the merger rumor. We identify separate rumors for the same target firm if a year has passed between rumors. Finally, we search through all merger bids announced between 2000 and 2012 in the SDC global merger database to identify any rumors that were followed by a formal public merger announcement.

The final sample includes 2,142 articles covering 501 rumors about 354 target firms. Targets include large, well-known firms, such as American Airlines, Alcoa, Sprint, and US Steel, as well as foreign firms, such as InterContinental Hotels Group, Roche Holding, and Samsung, and private firms, such as Calvin Klein, Skype, and Groupon.

Of the 501 rumors, 167 (33.3%) were followed by a public bid for the target within one year, whether the deal was completed or not. Though we cannot know for sure whether a rumor is false, we can state that the majority of rumors do not come true.

1.1. Time Series Statistics

Panel A of Table 1 presents the number of all articles, scoop articles, and public announcements by year from 2000 to 2011. There is an overall increasing pattern, with the year 2010 having the most articles and scoops (393 and 75), and the years 2004, 2009, and 2011 having the fewest. There is a positive but insignificant correlation between the number of scoop articles in a given year and the number of formal merger announcements in the SDC database (0.30, p-value=0.34). The correlation between the percent of rumors that emerged and the number of bids in SDC is weaker at 0.17 (p-value= 0.60). These correlations suggest that the prevalence of rumors is not closely tied to actual merger activity. To better reflect the time trend, Figure 1 presents a three-year rolling average of the number of rumor articles in the sample, normalized by the total number of articles in the *Wall Street Journal* or *New York Times* that include any of the following words: "merger," "acquisition," or "takeover." This figure shows an increasing time trend in rumor articles, controlling for the general volume of media articles about mergers.

In Panel B of Table 1, we find relatively uniform timing in articles across calendar months. In untabulated data, we find no seasonality in total circulation for a set of prominent newspapers, consistent with uniform coverage of merger rumors by month.[1] In untabulated statistics, we find that few articles appear on Saturday or Sunday. Wednesday and Thursday are slightly more common than other weekdays for rumor articles, but overall, there is not much meaningful variation by day of the week.

1.2. Newsworthiness Characteristics

To empirically identify the newsworthiness of firms named in merger rumors, we refer to commonly cited characteristics of newsworthiness in journalism studies: breadth, prominence, and proximity (Eadie, 2009). Breadth refers to the size of the audience that would be interested in a specific firm, prominence refers to how well-known is a firm, and proximity refers to how close is a firm.

[1]We use quarterly circulation data from the Audit Bureau of Circulations for the *Wall Street Journal* and the *New York Times* from 2005 to 2012.

We use a number of variables to measure newsworthiness. First, large public firms are more likely to interest readers because they employ more people, sell more products, and have more diverse stockholders. As a measure of firm size, we use log (book assets) from Compustat. As shown in Panel A of Table 2, nearly 90% of rumor targets are publicly-traded and the average firm has book assets worth $12 billion. Second, as evident in households' stock portfolios (Frieder and Subrahmanyam, 2005), firms with high brand recognition are more likely to interest a broad audience. To identify firms with recognizable brands, we use data from the marketing consultancy firms Interbrand and BrandZ, each of which publishes a list of the 100 most valuable brands in the world every year, starting in 2000 and 2006, respectively. Because these lists are so selective, we simply record a dummy variable for any target firm that appears on either list in any year from 2000 to 2011. Roughly 16% of rumor targets have valuable brands.

Additional measures of breadth and prominence are the ratio of a firm's advertising expenditures to total assets and the fraction of sales to households. Prior research shows that advertising expenditures significantly increase a firm's prominence to households and lead to greater ownership breadth, more trading, and intensified purchases by retail investors (Grullon, Kanatas, and Weston, 2004; Lou, 2014). The average firm in our sample has an advertising-to-assets ratio of 0.8%. To measure a firm's fraction of total sales to households, we use the fraction of sales by the target's industry that are purchased by households, according to the 1997 Input-Output tables of the Bureau of Economic Analysis. This measure identifies firms that sell more products directly to customers, compared to those that sell intermediate goods in the supply chain. About 38% of rumor targets' industry sales go to households. Our final measures of prominence are innovativeness (R&D/assets) and growth potential (Tobin's Q).

Proximity implies that firms located closer to readers are more newsworthy because readers are more likely to work for such firms, buy their products, and invest in their stocks (Huberman, 2001; Ivković and Weisbenner, 2005). To record proximity, we use two measures of distance. In the first tests, in which we compare rumored merger targets to actual targets, we record whether a firm is domestic or foreign. In the tests of rumor targets, where we have a newspaper article for each target firm, we calculate the great-circle distance in miles between the headquarters of the firm and the newspaper. This measure also helps to account for the media slant toward local firms documented in Gurun and Butler (2012). Foreign firms account for 25% of our sample and the average distance between a newspaper and the firm it covers is 387 miles.

1.3. Journalist Characteristics

To collect biographical data for the 382 journalists who authored or coauthored any scoop article in our sample, we access a wide range of sources. We provide a detailed description of our collection methods in the Internet Appendix and summarize the main data sources here.

First, we collect journalists' birth year and gender. An older journalist could be better at assessing a rumor's accuracy than a younger journalist due to experience or better connections. This relation could also be driven by selection, in which only the more accurate journalists remain employed. Gender differences between male and female journalists may arise if female journalists have different connections to business insiders than male journalists. We collect birth year and gender from the Lexis Nexis Public Records (LNPR) database. This database aggregates information on 450 million unique U.S. individuals (both alive and deceased) from sources such as drivers' licenses, property tax assessment records, and utility connection records.

We compute the average age across article coauthors and then use its logarithmic transformation in regression analysis. Panel B in Table 2 shows that the average age of journalist teams is 37 (log (age) = 3.6), and the 25th and 75th percentiles are 32 and 41 years old. For gender, we create a dummy variable equal to one if the article has any female coauthors, an outcome observed in 45% of rumors. In 17% of scoop articles, journalists are unnamed. Of the articles that report journalists' bylines, the average number of journalists per article is 1.5, with 62% of articles sole authored, 27% authored by two journalists, and 11% authored by more than two journalists.

Next, we collect data on journalists' education. We record the university attended by a journalist from biographical sketches on newspaper websites and the social networking site LinkedIn. To verify a journalist's degree, year of graduation, and academic specialization, we contact the registrars of the universities attended by journalists or, if necessary, the National Student Clearinghouse, a degree-verification service provider. To verify degrees of female journalists, we use their maiden names from the LNPR database if we are unable to verify the degree under the journalist's current family name.

We record two characteristics of a journalist's education: undergraduate major and the quality of the undergraduate institution. Reporters who received more relevant academic training, such as that in journalism or business, could be better equipped to assess a rumor's accuracy and the integrity of its sources. Also, journalists who attended higher-ranked universities may have access to a more valuable alumni network, which can serve as an important channel of information transfer (e.g., Cohen, Frazzini, and Malloy, 2008, 2010; Engelberg, Gao, and Parsons, 2012).

We record a dummy variable equal to one if an article coauthor has an undergraduate major in one of these six academic areas: Business & Economics, Journalism, English, Political Science, History, and Other.[2] Panel B in Table 2 shows that the most common undergraduate major is Journalism (33%), followed by English (31%), Political Science (19%), History (26%), Business & Economics (10%), and Other (10%). To measure the quality of a journalist's undergraduate training, we use the university's median verbal SAT score, expressed as a percentile. Since most journalists attended liberal arts programs, the verbal score is arguably the more relevant score of quality for journalists. Table 2 shows that the journalists in our sample attended selective undergraduate programs, with a mean (median) SAT score percentile of 83.7 (87.0).

Next, we collect journalists' primary and secondary areas of professional specialization from LinkedIn. We conjecture that a journalist with expertise in the industry of the rumor target may be better positioned to evaluate a rumor's accuracy than a journalist specializing in another industry. In some cases, a journalist's specialization is evident from his or her professional job title (e.g., 'Reporter, Automotive'), while in others, it is provided by the newspaper in the journalist's biographical sketch. We verify the reported specialization by reading samples of the journalists' articles. We then match the journalists' industry specializations to the Fama-French 17-industry classification and create a dummy variable equal to one if any of the coauthors has a primary or secondary expertise in the industry of the rumor target. In our sample, 55% of articles are written by teams with at least one journalist who is an expert in the target's industry.

Because a journalist's location may be important for access to information, we also record the geographic location of the journalist. Since many of the relevant information sources of merger rumors, such as investment bankers and stock traders, are concentrated in New York City, we create a dummy variable to identify New York-based journalists. We first identify a journalist's office location from his or her job title (e.g., 'Correspondent,

[2] Please see the appendix for the complete list of fields that are included in each of the six categories.

Atlanta Bureau') or from the newspaper's biographical sketch. Then we verify these data using journalists' residential addresses from the LNPR Database and match them to the location of newspaper bureaus. In 49.5% of articles, at least one of the article authors is stationed in New York.

Finally, we collect information on a journalist's awards, which may serve as a signal of superior skill. We consider the most prestigious journalist awards: the Pulitzer Prize, the Gerald Loeb Award, and the Society of American Business Editors and Writers (SABEW) Award. We collect information on award winners from the databases maintained by the award-bestowing organizations and record a dummy variable equal to one if any of the article's coauthors has been awarded or nominated for one of these awards. In our sample, 17.6% of articles are written by an award-winning journalist.

Panel A of Table 3 presents statistics on the 12 most prolific journalists in our sample, each with at least six scoop articles. The most prolific is Dennis Berman of the *Wall Street Journal* with 24 scoops, followed by Andrew Ross Sorkin of the *New York Times* with 19 scoops, and Nikhil Deogun and Robert Frank of the *Wall Street Journal*, each with 13 scoops. In general, more prolific journalists are more accurate than the average journalist. In particular, Berman's accuracy rate is 62.5% and Sorkin's is 42.1%, above the average journalist accuracy rate of 37.6%.

1.4. Article Characteristics

Using the text of the newspaper article, we record two types of information contained in the article: 1) the ambiguity of the language used in the article, and 2) details specific to merger rumors.

First, because we focus on rumor accuracy, we study the frequency of weak modal words – a measure of an author's confidence – based on the word list for financial texts from Loughran and McDonald (2011), as updated in August 2013. This list includes 26 words, including "apparently," "maybe," "perhaps," and "suggests." The complete word list appears in the appendix. We predict that rumors in articles that contain a greater fraction of weak modal words are less likely to come true. When calculating the frequency of weak modal words, we are careful to avoid spurious matches. For example, the weak modal word 'may' could refer to the calendar month of May, the retailer May Department Stores, or journalist's contact information such as "the author may be reached at..." The Internet Appendix explains how we address this issue.

Panel C of Table 2 shows that the mean frequency of weak modal words in merger rumors is 0.75%, noticeably higher than in annual reports (0.43%) or final IPO prospectuses (0.62%) documented in Loughran and McDonald (2011, 2013). As expected, the text of merger rumors is more speculative than that of financial disclosures.

Second, we collect details about the article that are specific to merger rumors. In particular, we collect the original source of the rumor cited in the article text. The vast majority (92%) are anonymous, with the rest made up of analysts, portfolio managers, bidder and target management, and others. We next collect the targets' comments in response to the rumor. In 46% of rumors, the target declines to comment on the rumor. In 38% of rumors, there is no mention that the newspaper attempted to contact the target for a comment. In 8% of cases, the article states that the target could not be reached. We also record the stage of the merger talks in seven categories based on the text of the article. Panel C of Table 2 shows that most rumors are in the 'Speculation' stage, accounting for 51% of the sample. The remainder is made up by 'Preliminary talks' (9%), 'In talks' (27%), 'Preparing a bid' (4%), 'Made offer' (5%), 'Evaluating bids' (2%), and 'For sale' (3%). We also record a number of additional variables that may signal the accuracy of a rumor. In particular, we record whether the article mentions the rumor in the headline (85%), reports the number and identity of alleged bidders (1.5 on average), and states an alleged takeover price (39%). We also count the number of rumor articles across all sources on the scoop date (1.7 on average).

1.5. Newspaper Characteristics

Finally, we collect additional information about the newspapers that publish the articles in our sample. We obtain circulation and founding year from company reports and Audit Bureau of Circulation statistics. The average founding year of newspapers in our sample is 1922. The oldest newspaper in our sample is the *Times of London*, founded in 1785. The average daily circulation is 908,909 copies, and the most widely-circulated newspaper is the *Wall Street Journal* with a circulation of 2,092,523 in 2011. We also identify the ultimate owner of each newspaper and record whether it is a family-run firm, which is the case for 74% of articles in the sample.

Panel B of Table 3 presents summary statistics of the number and accuracy of articles published by the newspapers in our sample. The *Wall Street Journal* is the most prolific publisher of rumor articles, with 158 scoops, followed by *Dow Jones News Service* (67 scoops) and the *New York Times* (38 scoops). The rumors published in the *Wall Street Journal* and *Dow Jones News Service* are also more accurate than the average rumor, with accuracy rates of about 39%, compared to 33% for the average rumor. In contrast, the *Los Angeles Times* and *NYT Blogs* have accuracy rates less than 20%.

1.6. Accuracy

It is important to define accuracy in the context of merger rumors. In the literal sense, as long as any person, anywhere, with any degree of knowledge suggests to someone else that a firm is ripe for a takeover, a merger rumor published in the press is accurate.

However, this is an extremely low bar for accuracy. It just implies that the journalist is not fabricating the rumor.

We define accuracy in what we believe is a more relevant way. In our setting, a rumor is accurate if it is followed by a public announcement of a proposed merger within one year, whether or not it results in a completed deal. This is the measure of ultimate interest to a newspaper's readers. The consequences of the merger, such as the premium paid to target shareholders, the change in control, and employee layoffs are what the average reader cares about, not just that someone is making idle speculation.

As in any definition, our measure of accuracy is subjective. For instance, we could define accurate rumors as those that are followed by an official announcement within a shorter time frame than one year. In our sample, 27.5% of rumors come true within six months and 15.8% come true within one month, compared to 33% using our 12-month window. We could also require accurate rumors to correctly name the true bidding firm, which occurs in 15% of our sample. If we require accurate rumors to come true within one month and also correctly name the bidding firm, 8.4% of rumors in our sample are accurate. These statistics show that correctly identifying bidders is more challenging than reporting timely rumors. The fact that many rumors do not name any bidders at all corroborates this point. We choose to use the more generous definition of accuracy based on the 12-month window without the requirement to correctly name a bidder.

We acknowledge that our definition of accuracy is not without limitations. An article could accurately report that two firms are in advanced merger negotiations, which then ultimately fail. This would be considered an inaccurate rumor using our definition. However, for our definition to be biased, the likelihood of deal failure would have to be systematically related to a characteristic of the merger negotiations or the firms involved that the journalist does not consider. Given that newspapers select stories to publish from a vast set of new information, it is reasonable that readers expect journalists to consider the likelihood of deal failure when they choose to publish a rumor. In section 4.1.1, we provide empirical evidence that the likelihood that public merger negotiations fail is unrelated to the characteristics of firms typically named in rumors.

2. Which Types of Rumors are Covered by the Business Press?

We first document the characteristics of target firms in merger rumors that attract newspaper coverage. We would ideally compare firms discussed in published rumors to firms discussed in unpublished rumors. Since it is difficult to observe unpublished rumors, we use actual mergers as a benchmark for comparison. As long as the firm characteristics we document are unrelated to the likelihood of a firm is discussed in a rumor, whether published or not, using actual mergers as a comparison group is unbiased. To help ensure that this is the case, we use three samples of actual mergers as comparisons: all mergers, mergers of large public targets, and mergers of US targets only. The first subsample includes all mergers in SDC from 2000 to 2011 with a deal value of at least $250 million. In the second subsample, we include publicly traded targets and set the minimum size threshold of actual merger targets such that their average value of log book assets is equivalent to that in the rumor sample. Finally, the third subsample includes only US merger targets worth at least $250 million.

Table 4 presents univariate t-tests between average target characteristics in our rumor sample, compared to the three different subsamples of actual mergers. In the rumor sample, 88% of targets are publicly traded, more than double the fraction found in the universe of SDC targets (38%) or in the sample of US targets (37%). We also find that the average value of book assets of rumored targets is significantly larger than that of actual merger targets. The difference between rumored targets and actual targets is even more stark for brand value. More than 15% of rumored targets have high brand values, compared to less than 1% for all mergers. Even in the size-matched sample, less than 3% of actual merger targets have high brand values. Rumored targets also spend significantly more on advertising than targets in any of the three samples of merger targets. Similarly, rumored targets sell 38% of their output to households, on average, significantly more than the 31% in all mergers and 34% in large mergers. Additionally, 75% of rumored targets are domestic firms, compared to 44% in the entire SDC sample.[3] We also find that rumored target firms spend more on R&D and have higher Tobin's Q values than comparable large public merger targets.

Internet Appendix Table 1 presents results from analogous multivariate regressions. Controlling for industry and year effects, we find results consistent with the univariate evidence, whether using logit models or OLS linear probability models. In addition, these effects are economically meaningful. For example, the odds of a rumor being published in the press if a target firm is public are 11.4 times as large as the odds if the firm is private. The odds of a rumor for a public firm with a valuable brand are six times as large as the odds for a public firm without a valuable brand, even after controlling for firm size, industry, and year effects.

These results provide consistent evidence that the financial press skews coverage towards more newsworthy firms. Rumors are more likely to be published for firms that appeal to a broader audience and have greater prominence, consistent with the theoretical models of media profit motives in Mullainathan and Shleifer (2005) and Gentzkow and Shapiro (2006).

[3]In this setting, we cannot compare actual distances between newspapers and firms because the firms in the actual merger samples do not have a newspaper associated with the merger. Since most of our rumor articles are published in US newspapers, the fraction of foreign firms proxies for distance.

3. How Do Merger Rumors Affect Stock Prices?

If investors can perfectly infer the likelihood that a rumor will come true, stock returns of targets on the date the rumor is published should reflect all information with no systematic over-reaction. Instead, if investors incorrectly believe that rumors are more accurate than they truly are, the average target of a rumor will experience a reversal following the publication of the rumor. To test these predictions, we calculate abnormal stock returns by subtracting the daily return on the value-weighted CRSP index from the daily return of the target's stock. Cumulative abnormal returns are the time-series sum of the abnormal returns.

In Figure 2, we plot the cumulative abnormal returns in event time from 20 trading days before the rumor to 20 trading days after. First, we find evidence of a run-up in stock prices for all rumored targets before the publication of the rumor. At the publication of the rumor, stock prices of target firms increase dramatically, though the increase is larger in rumors that will eventually come true compared to inaccurate rumors. Targets of accurate rumors continue to experience positive abnormal returns after the rumor has been published, while targets of inaccurate rumors experience persistent negative returns.

These results show that investors are adept at separating accurate from inaccurate rumors immediately, but not perfectly. The figure also reveals a substantial reversal in stock prices for the average target. This means that investors appear to systematically overestimate the accuracy of rumors.

Table 5 presents a numerical analysis of the stock returns in event time. On the date of the rumor publication (Day 0), the average target in a rumor experiences a 4.3% abnormal stock return.[4] Targets named in accurate rumors have abnormal returns of

[4]We use Day 0 returns throughout the paper rather than Day -1,0 to be conservative. This ensures that the responses reflect the rumor article, rather than the run-up.

6.9% on the rumor date, compared to 3.0% for inaccurate rumors, a highly statistically and economically significant difference. The average target experiences a substantial run-up of about 3% over the period twenty days before the rumor, with no significant difference in the run-up between accurate and inaccurate rumors. In contrast, in the 20 days following the publication of the rumor, targets in accurate rumors experience returns that are 492 basis points higher than targets in inaccurate rumors. This is driven by a significant reversal in the inaccurate rumors of −2.7%. Aggregating returns over the entire 41 day period, target firms of inaccurate rumors realize a complete reversal, where the total cumulative abnormal return is statistically indistinguishable from zero.

These results show that rumors in the press have large stock price effects. They also show that the market overreacts to the average merger rumor, suggesting that investors cannot perfectly distinguish the accuracy of merger rumors in the press. In particular, for the average rumor, there is a significant and large reversal of −1.4% over the ten days following the publication of the rumor.

4. What Predicts Accuracy in Merger Rumors?

We design a set of four tests to identify the factors that predict a rumor's accuracy and the factors that influence the stock price reaction to the rumor. In the first baseline test, we run a logit regression of the likelihood that a rumor comes true on the factors described above, controlling for year and industry fixed effects.

In the second test, we include the abnormal stock return of the target on the day the rumor is published (Day 0 return) as an explanatory variable in the logit test. This test identifies which explanatory factors are reflected in stock prices and which are not. If the day zero return reflects the likelihood that a rumor is true, then a variable that remains significantly related to the rumor's accuracy after controlling for the day zero return is not fully reflected in the market reaction to the rumor.

In the third test, we refine the target's day zero return as a control variable. In the spirit of Bhagat, Dong, Hirshleifer, and Noah (2005), if we ignore the time lag between the rumor and a public merger announcement, the day zero return has two components: the likelihood that the rumor will come true and the expected return of the target if the rumor does come true. Thus, the day zero return can be expressed as $r_0 = p \cdot r_a$, where p is the probability that the rumor comes true, and r_a is the return of the target on the day of the public announcement of the merger.[5] Rewriting this expression as $p = r_0/r_a$ isolates the component of the day zero return related to the accuracy of the rumor from the component related to the expected value of an accurate rumor.

To estimate p, we first estimate r_a. To do so, we run a linear regression of target announcement day returns on target size, industry, and year fixed effects in a sample of 2,555 official merger announcements of public targets over 2000 to 2011 from SDC. We use the coefficients from this model to fit estimates of $\hat{r}_a$ for each rumored target firm in our sample. The coefficient estimates are presented in Internet Appendix Table 2.[6] Using the estimate $\hat{r}_a$, we estimate $\hat{p}$.

Following this procedure, we replace the day zero return as an explanatory variable in the logit test with $\hat{p}$ in the third test. This logit test estimates the likelihood that a rumor comes true, controlling for the component of the day zero return related to the accuracy of the rumor. This means that variables that continue to predict accuracy in this test are not fully reflected in the stock market response to the rumor.

[5]The return r_a itself has two components: the probability that the deal completes and the value of the completed deal. For simplicity in our estimations and because of the noise inherent in estimating compound probabilities, we do not decompose the announcement return further.

[6]An alternative model that includes additional control variables provides little additional explanatory power. To avoid limitations from missing data, we use the parsimonious model in the paper.

Finally, in the fourth test, we run a regression of the day zero target abnormal returns on the same explanatory variables as in the logit tests. In addition, we control for $\hat{r}_a$, the estimated official announcement return. While the first three tests identify which factors predict accuracy and whether the market fully accounts for these factors, this fourth test identifies investors' beliefs about which factors influence accuracy, whether or not these factors actually predict accuracy.

In all of our tests, we also control for the staleness of the rumor. As mentioned above, newspapers are just one link in the diffusion of information from insiders to outsiders. Though our data collection process is designed to ensure that our sample correctly identifies the date and original source of the rumor among all media sources in Factiva, we do not claim that the rumors in our sample do not circulate in other venues first. As the theoretical models of Van Bommel (2003) and Brunnermeier (2005) argue, informed traders have an incentive to leak inside information in advance of official announcements. It is possible that journalists uncover rumors when investigating the causes of unexplained price runups. Thus, some rumors may be more stale than others when they are published in the press. Tetlock (2011) shows that stock returns respond less when media reports are more stale. If the staleness of the information varies across our sample firms, we could make incorrect inferences. For example, we could misinterpret a small stock price reaction to a variable that significantly explains accuracy as investor inattention, when in fact the price reaction is small because the information has already been incorporated in the stock price.

To control for staleness and information leakage, we use the cumulative abnormal returns of the target in the five trading days before the rumor is published. If the rumor has been widely circulated before the newspaper article is published, the pre-publication returns are expected to be higher. In unreported robustness tests, we obtain similar results if we use the cumulative abnormal returns over the twenty trading days before the article (to control for a longer pre-publication period) or the five trading days that end two days before the scoop article (to ensure we are not accidentally including a response to the rumor article itself). Our results are also unchanged if we use the reported stage of merger negotiations discussed in the article (speculation, early talks, advanced talks, etc.) to account for staleness, under the assumption that the amount of information leakage grows as negotiations advance.

4.1. Does Newsworthiness Predict Media Accuracy and Stock Returns?

In column 1 of Table 6, we find that the same factors that are associated with greater readership appeal are also associated with less accurate reporting. Rumors about large firms with valuable brands and greater advertising expenditures are significantly less likely to come true. These results are economically substantial. The odds ratio that a rumor comes true about a firm that does not have a valuable brand is 1.65 times as large as the odds ratio for a firm with a valuable brand. For a one standard deviation increase in target log(assets), the odds ratio that a rumor comes true decreases by 43%.

In column 2 of Table 6, we add the target's day zero returns. As expected, the day zero returns are positively related to accuracy. However, even after controlling for the day zero returns, the characteristics of newsworthy firms are still negatively and significantly related to rumor accuracy. In column 3, after including the estimated deal likelihood as a control variable, the results persist. These findings indicate that investors do not fully account for the incentives of newspapers to publish rumors about newsworthy firms.

In column 4 of Table 6, we find that firms with valuable brands, high Tobin's Q, and low R&D expenditures experience lower returns on the rumor day.[7] This indicates that

[7]Because the estimated announcement return is based on firm size, industry, and year fixed effects, we exclude these variables from the regression.

investors' perceptions of the rumor's accuracy are based on some characteristics, such as Tobin's Q and R&D, that are not significant predictors of accuracy.

4.1.1. Likelihood of Withdrawals. As mentioned previously, one concern with our measure of accuracy is that rumors about merger negotiations that do not advance to a public bid could be classified as inaccurate, even if there were actual merger talks happening. This could confound our tests if more newsworthy firms are also more likely to engage in negotiations that ultimately fail. A direct test of this alternative explanation would require a sample of all rumors, both published and unpublished, and their outcomes. Since we cannot observe such a sample, we use a similar setting where we can identify negotiation failures: withdrawals of public merger bids.

Using a large sample of bids from SDC, in Internet Appendix Table 3, we regress our variables of newsworthiness on a dummy variable equal to one if a bid is withdrawn. We find no significant positive relationships between the likelihood of withdrawal and any of our measures of newsworthiness. Instead, we find a negative and significant relationship between brand value and withdrawals. Interpreting these results in our setting implies that, if anything, rumored negotiations that involve more newsworthy firms are more likely to succeed than negotiations that involve less newsworthy firms.

4.2. Do Journalists Predict Rumor Accuracy and Stock Returns?

In Table 7, we run identical regressions as in Table 6, but use journalist characteristics as explanatory variables. Column 1 shows that older journalists are significantly more accurate than younger journalists. Second, articles written by reporters that studied journalism in college are significantly more accurate than articles written by journalists who studied other fields, though the quality of the college, as proxied by SAT scores, is unrelated to accuracy. Third, journalists that specialize in the target's industry are more accurate. Finally, journalists based in New York City are also more accurate.

Once we control for the target's day zero stock returns in column 2 or the estimated deal likelihood in column 3, we find no change in these results, except the effect of New York-based journalists becomes insignificant. In column 4, we find that a journalist's age, education, and expertise do not affect the day zero stock returns. However, rumors written by New York-based journalists have a significantly higher stock price reaction on the day the rumor is published.

These findings are intuitive. Older journalists with more relevant experience may be better able to filter out false rumors, or they may have culled more reliable information sources than younger journalists. An undergraduate degree in journalism may equip reporters with investigative skills useful for verifying suspicious claims. The insignificant effect of SAT scores may indicate that these basic principles of journalism are taught equally at high and low ranked colleges. In contrast, inexperienced or untrained journalists may be more naïve and more easily fooled by a false rumor. Location also matters, as New York-based journalists tend to be more accurate. This could occur because the best business journalists end up in New York, or because New York-based journalists have better connections to Wall Street insiders. The fact that investors do not fully account for most of the journalist characteristics is reasonable, given that this information is not prominently made available.

Though we have identified the biographical traits of journalists that we believe are the most important for predicting accuracy, other unobserved characteristics of journalists are likely to be related to accuracy as well. In Internet Appendix Table 4, we run journalist fixed effects regressions where the dependent variables are accuracy and day zero returns. We only include dummy variables for the most prolific journalists with at least four scoop articles. Consistent with the summary statistics in Table 3, journalists Berman, Sorkin, and Sidel have positive fixed effects on the likelihood that a rumor comes true. For instance, the odds a rumor comes true are roughly six times higher if the article is written by Berman, compared to all other journalists. These results hold after controlling for the day zero return and the estimated deal likelihood. These results indicate that some journalists are more accurate than others, but that stock prices do not fully reflect this variation.

It is not surprising that investors do not perfectly account for journalist fixed effects. Given the large number of journalists and limited attention of readers, the cost to a retail investor of accounting for a journalist's historical accuracy rate is likely prohibitive. The marginal effect for Andrew Ross Sorkin illustrates how limited attention is likely to drive these effects. Sorkin is a well-known author of the best-selling book "Too Big To Fail," which was made into a television-movie for HBO. He is also known as the founder of the *New York Times* news service on mergers called *Dealbook*, which uses the masthead, "DealBook with Founder Andrew Ross Sorkin." Without controlling for the day zero return, the magnitude of Sorkin's fixed effect is 1.64. However, once the day zero return is included, the fixed effect drops to 1.27, indicating that the stock returns account for Sorkin's accuracy, at least partially. Compare this to Dennis Berman, a prolific journalist with high accuracy rates, but not nearly as well-known as Sorkin. The magnitude of Berman's fixed effect is 1.79 without controlling for the day zero return. Once the day zero return is included, Berman's fixed effect remains virtually unchanged at 1.77. Rumors reported by Berman are more accurate than the average rumor, but stock prices do not reflect this additional accuracy.

This evidence is consistent with the theory that limited attention may lead investors to overlook valuable public information and cause distortions in stock prices (Hirshleifer and Teoh, 2003; Hirshleifer, Lim, and Teoh, 2011).[8] Our findings extend this literature by showing that investors do not fully account for the media's incentive to publish sensational stories, or the characteristics of journalists that predict accuracy.

4.3. Does the Article Text Predict Rumor Accuracy and Stock Returns?

In Table 8, we run identical regressions as before using article characteristics as explanatory variables. In the first column, consistent with our prediction, we find a strong negative relationship between the use of weak modal words and the accuracy of a rumor. We also find that when targets confirm a rumor, it is substantially more likely to be accurate, compared to when targets decline to comment. Next, an article that alleges that the firms are already engaged in merger talks is more likely to be accurate than an article that is purely speculative. We also find that an article that mentions a takeover price or lists more prospective bidders is also more likely to be accurate. While these article characteristics help predict a rumor's accuracy, investors do not appear to fully account for their predictive power. The effect of each of these article characteristics persists after controlling for the day zero stock return and the estimated deal likelihood.

Investors do respond to some article characteristics. For example, when a rumor is covered by more newspapers, the rumor is more likely to be accurate, and this accuracy is reflected in the stock price response. In contrast, targets' day zero returns are higher when the rumor comes from an anonymous source than when there is an identified source, yet anonymity of the rumor source is unrelated to the likelihood that the rumor is accurate.

[8]Empirical evidence in support of limited attention has been documented in the context of financial information (Tetlock, 2011; Da, Gurun, and Warachka, 2013), earnings announcements (Engelberg, 2008; DellaVigna and Pollet, 2009; Hirshleifer, Lim, and Teoh, 2009), economic shocks (Cohen and Frazzini, 2008), and investment choices (Barber, Odean, and Zheng, 2005).

In the Internet Appendix, we present estimates from identical regressions on article characteristics, in which we also include journalist fixed effects (Internet Appendix Table 5) and newspaper fixed effects (Internet Appendix Table 6). We find that virtually all of the results are unchanged. This means that even among the articles published by a particular newspaper or written by a particular journalist, the characteristics of the text predict the accuracy of a rumor.

These results indicate that the language in the rumor articles is informative of the rumor's accuracy but overlooked by investors. This is consistent with Tetlock, Saar-Tsechansky, and Macskassy (2008), who find that negative words in the financial press predict lower firm earnings, but stock prices reflect this information only after a delay.

4.4. Do Newspapers Predict Rumor Accuracy and Stock Returns?

Finally, we investigate the predictive power of the newspapers that publish merger rumors. In fixed effects regressions presented in Internet Appendix Table 7, we test whether accuracy varies significantly across newspapers. In general, newspapers display fewer statistically significant fixed effects than journalists. This suggests that the characteristics of journalists are better predictors of accuracy than the identity of a newspaper.

To further investigate the role of newspaper characteristics, in Internet Appendix Table 8, we test whether a newspaper's age, circulation, and ownership influence the accuracy of rumors. We find that these variables are unrelated to accuracy. However, newspaper characteristics do influence the stock returns on the day the rumor is published. A rumor that appears in an older newspaper with a larger circulation generates greater day zero stock returns than one appearing in a younger newspaper with a lower circulation. These results are consistent with the view that the media influences stock returns, even if it doesn't provide new or relevant information (Tetlock, 2007, 2011).

4.5. Summary of Predictive Power and Economic Magnitudes

In summary, though there is good reason to believe that newspapers and journalists have an incentive to be more accurate when the stakes are larger, we find no evidence that any of our measures of newsworthiness are positively related to accuracy. In contrast, we find that though newspapers disproportionately cover large firms with recognizable brands, they are substantially less accurate when they do so. At the same time, stock returns do not fully account for newspapers' incentives to publish newsworthy articles, journalists' characteristics, or details in the text of the article.

To better understand the economic consequences of price distortions following rumor articles, we calculate the returns of portfolios formed according to the likelihood of a rumor's accuracy. In particular, using the predicted probabilities from the regression results presented above, we classify rumors as "More Likely" if the fitted value is greater than the unconditional average accuracy rate, and "Less Likely" otherwise. We then use these classifications to form two calendar-time daily portfolios from 2000 to 2012; one for targets in rumors that are more likely to come true and one for targets in rumors that are less likely to come true. Firms enter a portfolio on the day the rumor was published and stay in the portfolio for up to one year. A firm's first return in the portfolio is on the first day after the date of the rumor's publication. The portfolios are equally-weighted and rebalanced daily. For days in which both portfolios include at least five stocks, we calculate the long-short portfolio returns from holding a long position in the More Likely portfolio and a short position in the Less Likely portfolio.

If investors perfectly account for the characteristics of rumors, the long-run returns of the long-short portfolio should be zero. Instead, we find large positive returns. In particular, when we use the regression results from newspaper and journalist fixed effects to classify rumors as More Likely or Less Likely, the return on the long-short portfolio is 76 basis points per month. Using the target's newsworthiness characteristics to predict accuracy yields a monthly return of 58 basis points. Finally, the information contained in the text of the article yields a monthly return of 36 basis points. Thus, the distortions in stock prices caused by ignoring information are economically meaningful.[9]

5. Do Rumors Affect Insiders?

While we have documented that merger rumors have substantial effects on stock prices and that investors do not fully account for all information, we would like to know if the publication of a merger rumor influences important decisions made by insiders. We examine two such settings: markup pricing in the takeover premium and insider trading.

5.1. Markup Pricing in Premiums

Schwert (1996) shows that takeover premiums include two components: the run-up in the target's stock price before the announcement of a merger and the markup from the announcement to the close of the merger. If the bidder believes the run-up simply reflects the anticipation of the upcoming merger bid, it would revise its takeover price down accordingly. Schwert finds the opposite: the run-up is an added cost to the bidder, and there is no trade-off between the run-up and the markup.

To test this hypothesis in the setting of rumors, we include the full sample of official merger bids in SDC over the period 2000–2011 for public targets and record a dummy variable equal to one if the deal was preceded by a merger rumor identified in our main

[9]It is important to note that we do not claim these results are predictive regressions. We also don't claim that this is an implementable trading strategy, since we have not accounted for transaction costs, and the portfolio sizes are small. Instead, these tests provide an in-sample measure of the economic magnitude of the distortions associated with rumor articles.

sample. Following Schwert (1996), we calculate the target's cumulative abnormal stock return over the period from 42 trading days before the public announcement of the merger until one day before the announcement. The second period is the period from the day of the public announcement to five days after the announcement.[10] The total premium is calculated as the target's cumulative abnormal returns from 42 days before the announcement to five days after it. We then regress the total premium on the rumor dummy variable, plus a host of factors that might influence target returns, including target size, industry fixed effects, and deal characteristics.

Table 9 presents the results from these regressions. Consistent with our prior findings, rumors increase target returns in the run-up period by about 8 percentage points, on average. This is true after accounting for variables that could affect the accuracy of the rumor, such as brand value and size, as well as deal characteristics, such as payment method and the use of takeover defenses. The second set of regressions shows that rumors have a strong and statistically significant negative effect on target returns at the announcement of about 8 percentage points. Thus the markup for rumored deals is substantially reduced. Finally, the third set of regressions shows that rumors have no significant effect on the total takeover premium. The marginal effect of the rumor variable is insignificant and economically minuscule.

These results show that rumors do not contribute to the premium paid in mergers. In contrast to uninformed outsiders who may have limited attention, insiders appear to correctly attribute the additional stock returns caused by the rumor and adjust takeover prices downward accordingly.

[10]Schwert (1996) extends this period for a longer duration, but the vast majority of the returns occur within the first few days of the announcement.

5.2. Insider Trading

The significant run-up and reversal in stock prices for inaccurate rumors provides an attractive trading opportunity for those who know the rumor is false. In particular, though the target executives cannot know for sure whether another firm will propose a takeover, if they know that the rumor is likely to be false, they have an incentive to sell their shares on the rumor news, in anticipation of the reversal. However, insiders' ability to act on any private information about the rumor is constrained by insider trading laws, since executives would be trading based on material non-public information.

To test whether insiders act on their knowledge, we collect insider trading data for target officers in our sample from the TFN Insider database following prior conventions.[11] We find no significant change in insider trading in the 40-day window surrounding the rumor date. We also find no statistical difference in trading between accurate and inaccurate rumors. Following the procedure in Cohen, Malloy, and Pomorski (2012) to identify routine and opportunistic trades, we still find no significant relation between insider trading and merger rumors.

6. Conclusion

In the context of merger rumors, we show that media coverage of rumors is biased towards newsworthy firms that appeal to a broad audience. At the same time, we find that newsworthiness is a strong predictor of inaccurate reporting. Rumors about more newsworthy firms are substantially less likely to come true, compared to rumors about

[11] We only include open market purchases or sales, delete observations marked as inaccurate or incomplete ('cleanse' field of S or A), and only include observations that record all of the following information: the number of shares traded, the date, and the price per share in the transaction.

less newsworthy firms. However, stock returns do not reflect the reduced accuracy related to newsworthiness.

We also provide new evidence that the biographical traits of journalists are strong predictors of accurate reporting. Older reporters who received degrees in journalism and specialize in the rumor target's industry are significantly more accurate. Consistent with limited attention of investors, stock prices do not fully reflect the predictive power of these traits. In addition, the specific language used in the text of a media article helps to predict whether the rumor is accurate. For example, a discussion of a specific takeover price, the disclosure of potential bidders, and the use of weak modal words that indicate uncertainty provide important signals of a rumor's accuracy. Nevertheless, investors do not appear to recognize their predictive power.

We believe our results have important implications for the role of the financial media in the stock market that extend beyond merger rumors. Prior research shows that the media performs an important function in financial markets by disseminating news and reducing information asymmetry (Tetlock, 2010; Peress, 2013). Generalizing beyond merger rumors, our results suggest that the media selectively provides more information about large, public firms with wide readership appeal, but this information is likely to be less accurate.

Appendix: Variable Definitions

Newsworthiness Variables

Target book assets	Total book assets, as reported in Compustat.
Public target	Dummy variable equal to one if the rumor target is publicly traded at the time of the rumor.
Valuable brand	Dummy variable equal to one if the target firm was listed in the top 100 most valuable brands by the Interbrand or Brandz data in any year from 2000 to 2011.
Advertising/Assets	Advertising expenses/Total book assets, as reported in Compustat.
Industry sales to households	The fraction of the target industry's sales that are purchased by households. Data are from the 1997 Bureau of Economic Analysis Detailed-level Input-Output tables.
Tobin's Q	(Total assets − common equity + market equity)/Total assets. Data from CRSP and Compustat.
R&D/Assets	R&D/Total book assets, as reported in Compustat.
Distance	Great circle distance in miles between the headquarters of the newspaper that published the scoop article and the target firm.
Foreign target	Dummy variable equal to one if the rumor target is headquartered outside of the US.

Journalist Variables

Age	The average age (in years) of all journalists listed as authors of a scoop article.
Undergraduate major	Dummy variable equal to one if an article is written by a journalist who graduated with a major in one of the following categories:
Business & Economics	Degrees in business, economics, finance, and management
Journalism	Degrees in broadcasting, communication, journalism, mass media, and media studies
English	Degrees in creative nonfiction, English, literature, literary studies, and screenwriting
Political Science	Degrees in government, international affairs, international relations, law, politics, political science, public policy, and public relations

History	Degrees in ancient history, American studies, art history, Asian history, Chinese history, classics, history, and modern history
Other	Degrees in animal science, anthropology, biology, biopsychology, criminal justice, East Asian languages, East Asian studies, electrical engineering, environmental biology, film, general studies, Germanic studies, human development, liberal arts, mathematics, philosophy, psychology, religion, Russian studies, sociology, teaching, urban affairs, veterinary medicine, and zoology.
College SAT percentile	The average verbal SAT percentile of the undergraduate institutions of all journalists listed as authors of a scoop article.
Expert in target industry	Dummy variable equal to one if any journalist who authored an article is an expert in the same industry as the primary industry of the rumor target, using Fama-French 17 industry codes.
New York-based	Dummy variable equal to one if at least one of the authors of an article is based in New York City.
Award winner	Dummy variable equal to one if at least one of the authors of an article has been nominated for or received the Pulitzer Prize in Journalism, the Gerald Loeb Award, or the Society of American Business Editors and Writers (SABEW) award.
Gender	Dummy variable equal to one if an article has at least one female coauthor.
Article Variables	
Weak modal words	The fraction of weak modal words in the text of an article. Weak modal words are defined in Loughran and McDonald (2011) and include the following words: *apparently, appeared, appearing, appears, conceivable, could, depend, depended, depending, depends, may, maybe, might, nearly, occasionally, perhaps, possible, possibly, seldom, seldomly, sometimes, somewhat, suggest, suggests, uncertain,* and *uncertainly*.
Anonymous source	Dummy variable equal to one if an article does not identify a specific source of the rumor.
Target comment	Categorical variable that records the target firm's response to the rumor, according to the text of the newspaper article: No comment, Has conversations from time to time, Confirmed rumor, Denied rumor, Couldn't be reached, or Wasn't asked.

Merger stage	Categorical variable that records the stage of the rumored talks, according to the text of the newspaper article: Speculation, Preliminary talks, In talks, Made offer, Preparing a bid, For sale, or Evaluating bids
Articles on scoop date (#)	The total number of articles reporting the rumor published on the same date as the scoop article.
Rumor in headline	Dummy variable equal to one if the rumor article refers to the rumor in the headline of the article.
Number of bidders mentioned	The number of firms mentioned in the text of the article as potential bidders.
Price mentioned	Dummy variable equal to one if a specific takeover price is mentioned in the text of the article.

Newspaper Variables

Family-run media company	Dummy variable equal to one if a newspaper is owned by a family-run firm.
Newspaper age	The age of the newspaper in years from its original founding date to the date of article publication.
Newspaper circulation	The total daily circulation of the newspaper, as recorded in the Audit Bureau of Circulation reports.

Other Control Variables

Day 0 return	The abnormal stock return of the target firm on the day the scoop article is published. Abnormal returns are calculated as the firm's return minus the CRSP value-weighted index return.
Estimated deal likelihood	Day 0 return/Estimated announcement return
Estimated announcement return	The fitted value of the expected announcement return of the target of an actual merger announcement. Fitted values are based on the coefficients in Internet Appendix Table 2.
Returns$_{(-5,-1)}$	The cumulative abnormal stock returns over the period from five days to one day before the scoop article is published. Abnormal returns are calculated as the firm's return minus the CRSP value-weighted index return. Cumulative returns are the sum over the five days of the abnormal returns.
Industry fixed effects	Dummy variables for the target firm's primary Fama-French 17 industry code.
Year fixed effects	Dummy variables for the year the scoop article is published.

Target market equity	The target stock price times the number of shares outstanding two days before the announcement of the merger.
Completed	Dummy variable equal to one if a merger bid is successfully completed, as reported in SDC.
Majority cash	Dummy variable equal to one if a merger bid uses cash as the majority form of payment, as reported in SDC.
Tender offer	Dummy variable equal to one if a merger bid is a tender offer, as reported in SDC.
Leveraged buyout	Dummy variable equal to one if a merger bid is classified as a leveraged buyout, as reported in SDC.
Cross-border	Dummy variable equal to one if a merger bid is a cross-border bid, as reported in SDC.
Target takeover defenses	Dummy variable equal to one if a target employed any defensive antitakeover provisions following an unsolicited merger bid, as reported in SDC.

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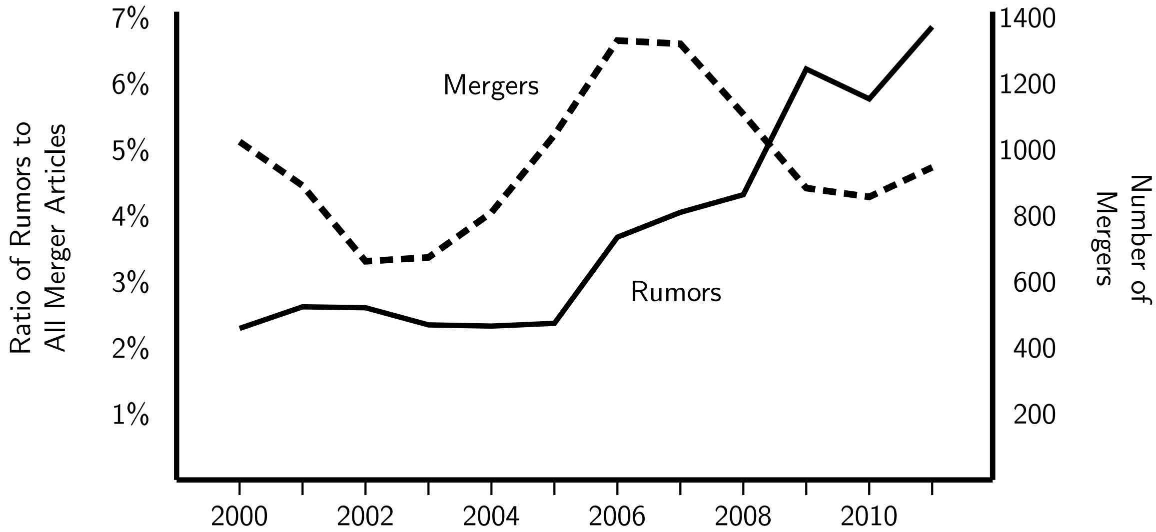

Figure 1
The Increasing Prevalence of Merger Rumors
This figure presents the time series of rumor articles and actual merger announcements. The solid line represents the ratio of merger rumors in our sample per year to the yearly total number of articles in the *Wall Street Journal* and the *New York Times* that contain any of the words, "merger," "acquisition," or "takeover." The dashed line represents the total number of global mergers in the SDC database where the deal value is greater than \$500 million. To illustrate trends, both time series are three-year averages, centered on the observation year.

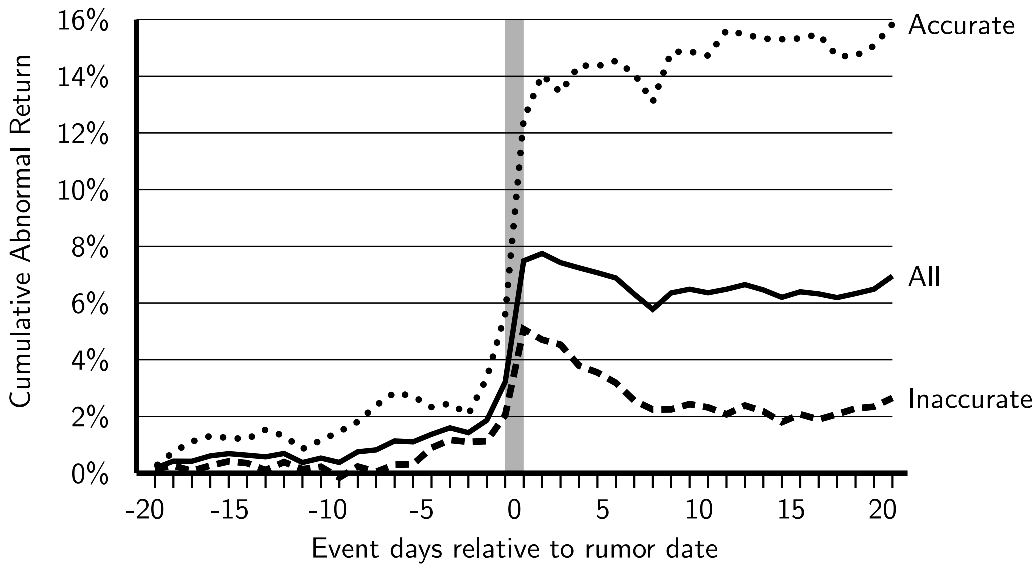

Figure 2
Abnormal Returns of Accurate and Inaccurate Merger Rumors
This figure presents the time series of cumulative abnormal returns of merger rumor targets for three time series relative to the date of the first publication of a merger rumor. There are 415 rumors with stock price data over 2000-2011. Cumulative abnormal returns are the cumulative sum of daily abnormal returns, calculated as the firm's daily return minus the value-weighted CRSP index. The solid line represents the average of all target firms. The dotted line represents the subsample of merger rumor targets where a public merger announcement was made within one year of the rumor date. The dashed line represents the subsample of merger rumor targets where no public merger announcement was made in the following year.

Table 1
Rumor Articles by Calendar Period
This table presents counts of rumor articles by year and month. 'All articles' includes all rumor articles in the sample. The scoop article is the first article that reports a rumor. 'Percent of scoops' is the fraction of total scoop articles in the sample that were published in a given year or month. 'Percent of bids in SDC' is the fraction of bids in the SDC database where the original public announcement is in a given calendar period. 'Rumors that came true' is the number of rumors with a publicly announced bid in the SDC database within one calendar year of the original scoop article. 'Percent that came true' is the number of rumors that came true divided by the number of scoop articles for a given time period.

	All articles	Scoop articles	Percent of scoops	Percent of SDC bids	Rumors that came true	Percent that came true
Panel A: Yearly						
2000	155	35	7.0	9.6	12	34.3
2001	123	40	8.0	6.6	19	47.5
2002	185	60	12.0	6.1	15	25.0
2003	130	37	7.4	6.6	9	24.3
2004	58	14	2.8	7.3	4	28.6
2005	184	37	7.4	8.6	18	48.6
2006	185	56	11.2	9.7	15	26.8
2007	279	70	14.0	11.2	25	35.7
2008	214	31	6.2	9.1	9	29.0
2009	131	25	5.0	7.4	7	28.0
2010	393	75	15.0	8.8	26	34.7
2011	105	21	4.2	9.0	8	38.1
Total	2142	501			167	33.3
Panel B: Monthly						
January	209	46	9.2	7.5	12	26.1
February	134	32	6.4	7.4	12	37.5
March	183	51	10.2	8.9	17	33.3
April	223	47	9.4	8.0	17	36.2
May	246	48	9.6	8.5	20	41.7
June	157	43	8.6	8.9	16	37.2
July	150	36	7.2	8.8	7	19.4
August	100	28	5.6	7.9	4	14.3
September	256	53	10.6	8.0	15	28.3
October	156	42	8.4	8.3	10	23.8
November	185	40	8.0	8.2	17	42.5
December	143	35	7.0	9.6	20	57.1

Table 2
Summary Statistics of Predictive Variables
This table presents summary statistics for the main variables used throughout the paper for 501 merger rumors over the period 2000–2011. Observations are at the scoop article level. The scoop article is the newspaper article that first reports the merger rumor. Journalist characteristics are aggregated from individual journalist characteristics to the scoop article level. Variable definitions are presented in the appendix.

			Percentile		
	Mean	Std. Dev.	25th	50th	75th
Panel A: Target newsworthiness					
Public target	0.884	0.320	1	1	1
Log(Target book assets)	9.394	1.990	8.056	9.472	10.563
Valuable brand	0.156	0.363	0	0	0
Advertising/Assets	0.008	0.024	0	0	0
Industry sales to households	0.384	0.279	0.102	0.371	0.591
Tobin's Q	1.731	1.471	0.961	1.199	2.007
R&D/Assets	0.015	0.043	0	0	0
Log(1+Distance)	5.960	2.849	5.101	7.111	8.149
Foreign target	0.251	0.434	0	0	1
Panel B: Journalist characteristics					
Log(Journalist age)	3.612	0.191	3.481	3.584	3.724
Undergraduate major					
Business & Economics	0.098	0.297	0	0	0
Journalism	0.328	0.470	0	0	1
English	0.311	0.463	0	0	1
Political Science	0.192	0.395	0	0	0
History	0.257	0.438	0	0	1
Other	0.098	0.297	0	0	0
College SAT Percentile	83.729	12.438	78	87	95
Expert in target industry	0.554	0.498	0	1	1
New York-based	0.495	0.501	0	0	1
Award winner	0.176	0.381	0	0	0
Gender	0.454	0.498	0	0	1
Panel C: Article characteristics					
Weak modal words (%)	0.745	0.542	0.395	0.677	0.975
Anonymous source	0.920	0.272	1	1	1
Target Comment					
Declined to comment	0.459	0.499	0	0	1

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Table 2 - *Continued*

	Mean	Std. Dev.	Percentile 25th	Percentile 50th	Percentile 75th
Has conversations	0.010	0.099	0	0	0
Confirmed rumor	0.032	0.176	0	0	0
Denied rumor	0.038	0.191	0	0	0
Couldn't be reached	0.084	0.277	0	0	0
Wasn't asked	0.377	0.485	0	0	1
Merger Stage					
Speculation	0.507	0.500	0	1	1
Preliminary talks	0.086	0.280	0	0	0
In talks	0.269	0.444	0	0	1
Made offer	0.050	0.218	0	0	0
Preparing bid	0.036	0.186	0	0	0
For sale	0.032	0.176	0	0	0
Evaluating bids	0.020	0.140	0	0	0
Rumor in headline	0.848	0.359	1	1	1
Number of bidders mentioned	1.500	1.517	1	1	2
Price mentioned	0.386	0.492	0	0	1
Articles on scoop date (#)	1.729	1.338	1	1	2
Panel D: Newspaper characteristics					
Family-run media company	0.735	0.442	0	1	1
Log(Newspaper age)	4.493	0.966	4.718	4.779	4.927
Log(Newspaper circulation)	13.720	1.071	12.902	14.277	14.554

Table 3
Accuracy Rates of Journalists and Media Sources
This table presents publishing activity for journalists and media sources in the sample from 2000-2011. The scoop article is the newspaper article that first reports the merger rumor. Accuracy rate is the fraction of scoop articles in which a formal takeover bid is made for the target firm within one year. The total number of articles and scoops for journalists exceeds the total number for media sources because some articles include multiple authors. Abbreviations in parentheses indicate the most recent newspaper that employed a journalist. *WSJ* indicates *Wall Street Journal*, *NYT* indicates *New York Times*, *DJNS* indicates *Dow Jones News Service*, and *NY Post* indicates *New York Post*.

Journalist/Media Source	All Articles	Percent of All Articles	Scoop Articles	Percent of Scoops	Accuracy Rate
Panel A: Journalists					
Dennis K. Berman (WSJ)	71	3.2	24	4.0	62.5
Andrew Ross Sorkin (NYT)	67	3.0	19	3.2	42.1
Nikhil Deogun (WSJ)	27	1.2	13	2.2	53.8
Robert Frank (WSJ)	20	0.9	13	2.2	23.1
Robin Sidel (WSJ)	23	1.0	9	1.5	55.6
Anupreeta Das (WSJ)	21	0.9	7	1.2	42.9
Michael J. de la Merced (NYT)	32	1.4	6	1.0	16.7
Jeffrey McCracken (Bloomberg)	19	0.9	6	1.0	50.0
Anita Raghavan (NYT)	19	0.9	6	1.0	16.7
Suzanne Kapner (NYT)	18	0.8	6	1.0	33.3
Sarah Ellison (DJNS)	16	0.7	6	1.0	33.3
Erica Copulsky (NY Post)	14	0.6	6	1.0	33.3
786 Others	1867	84.3	482	79.9	36.3
Total	2214	100.0	603	100.0	37.6
Panel B: Media Sources					
Wall Street Journal	448	20.9	158	31.5	38.6
Dow Jones News Service	625	29.2	67	13.4	38.8
New York Times	219	10.2	38	7.6	28.9
Reuters News	73	3.4	26	5.2	19.2
New York Post	95	4.4	24	4.8	37.5
Barron's	38	1.8	15	3.0	26.7
NYT Blogs	59	2.8	12	2.4	16.7
Bloomberg	16	0.7	10	2.0	80.0
Boston Globe	38	1.8	8	1.6	25.0
Financial Times	15	0.7	8	1.6	62.5
Los Angeles Times	7	0.3	6	1.2	16.7
94 Others	509	23.8	129	25.7	25.2
Total	2142	100.0	501	100.0	33.3

Table 4
Target Characteristics in Rumors Versus Actual Mergers
This table presents average characteristics of target firms in the rumor sample compared to targets in actual mergers. Targets in actual mergers are taken from SDC over the period 2000-2011 and exclude mergers that are in the rumor sample. The column denoted 'All Mergers' includes private, public, and subsidiary mergers of targets across the globe, where deals must be worth at least $250 million. Mergers in the column denoted 'Large Public Targets' include the subset of public targets where the minimum target book assets is set such that the average firm in the subsample has the same book assets as the average firm in the rumor sample. The column 'US Merger Targets' only includes targets in the US, but does not constrain size or public status of the target. The numbers in parentheses are p-values from t-tests of the average of each merger column with the rumor column average. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

	Rumors	All Mergers	Large Public Targets	US Merger Targets
Public target (%)	88.42	38.10***	100.00***	36.87***
		(< 0.001)	(< 0.001)	(< 0.001)
Log(Target book assets)	9.39	7.45***	9.39	6.99***
		(< 0.001)	(0.998)	(< 0.001)
Valuable brand (%)	15.57	0.48***	2.27***	0.44***
		(< 0.001)	(< 0.001)	(< 0.001)
Advertising/Assets (%)	0.83	0.14***	0.10***	0.38***
		(< 0.001)	(< 0.001)	(< 0.001)
Industry sales to households (%)	38.41	30.53***	34.42***	29.83***
		(< 0.001)	(0.003)	(< 0.001)
Tobin's Q	1.73	1.64	1.24***	1.85
		(0.299)	(< 0.001)	(0.189)
R&D/Assets (%)	1.55	0.61***	0.43***	1.26
		(< 0.001)	(< 0.001)	(0.152)
Foreign (%)	25.15	66.08***	76.99***	0.00***
		(< 0.001)	(< 0.001)	(< 0.001)

Table 5
Target Abnormal Event Returns and Reversals
This table reports average cumulative abnormal returns in percentages. Abnormal returns are raw returns minus the CRSP value-weighted index. Rumors that came true are those in which an official takeover announcement was made within one year of the first report of the rumor in the press. The numbers in parentheses are p-values. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

		Rumor Came True		
	All	Yes	No	Difference
Panel A: Rumor Publication Date				
Day 0	4.271***	6.865***	3.029***	3.835***
	(< 0.001)	(< 0.001)	(< 0.001)	(0.001)
Panel B: Run-up Period				
Days [−20, −1]	3.093***	5.267***	2.045**	3.222
	(< 0.001)	(0.003)	(0.041)	(0.112)
Days [−10, −1]	2.483***	3.850***	1.824**	2.026
	(< 0.001)	(0.005)	(0.029)	(0.205)
Days [−5, −1]	2.116***	2.903***	1.736***	1.167
	(< 0.001)	(0.005)	(0.003)	(0.320)
Panel C: Post-Rumor Period				
Days [+1, +5]	−0.850	1.313*	−1.885**	3.198***
	(0.177)	(0.090)	(0.027)	(0.005)
Days [+1, +10]	−1.395*	1.422	−2.743***	4.165***
	(0.062)	(0.107)	(0.007)	(0.002)
Days [+1, +20]	−0.849	2.480	−2.442**	4.922**
	(0.371)	(0.114)	(0.039)	(0.012)
Panel D: Complete Period				
Days [−20, +20]	6.550***	14.736***	2.633	12.103***
	(< 0.001)	(< 0.001)	(0.112)	(< 0.001)

Table 6
Rumor Accuracy and Stock Returns: Target Newsworthiness
This table examines the relationship between target newsworthiness and 1) the likelihood that a rumor comes true and 2) the target stock returns on the day the rumor is first published. Columns 1–3 present fixed effect logit regression coefficients in which the dependent variable is a dummy variable equal to one if a merger rumor came true within one year of the first rumor date. Column 4 presents OLS regression coefficients where the dependent variable is the rumor target's abnormal stock return on the date the first rumor article is published. Abnormal returns are calculated as the target returns minus the CRSP value-weighted index return. Variables are defined in the appendix. Industry fixed effects include Fama-French 17 industry codes. Standard errors are clustered at the industry level and p-values are reported in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
Day 0 Return		4.458^{***}		
		(< 0.001)		
Estimated deal likelihood			0.282^{***}	
			(< 0.001)	
Estimated announcement return				0.083
				(0.393)
Log(Target book assets)	-0.287^{***}	-0.264^{***}	-0.279^{***}	
	(0.004)	(0.005)	(0.007)	
Valuable brand	-0.500^{*}	-0.382^{*}	-0.461^{*}	-0.042^{***}
	(0.078)	(0.082)	(0.077)	(0.006)
Advertising/Assets (%)	-0.068^{**}	-0.073^{***}	-0.069^{**}	0.001
	(0.035)	(0.007)	(0.021)	(0.796)
Industry sales to households	0.323	0.377	0.432	0.002
	(0.315)	(0.351)	(0.196)	(0.901)
Tobin's Q	−0.045	−0.022	−0.030	-0.005^{**}
	(0.206)	(0.574)	(0.420)	(0.020)
R&D/Assets (%)	0.007	−0.005	0.004	0.003^{**}
	(0.847)	(0.860)	(0.910)	(0.022)
Log(1+Distance)	0.006	−0.013	−0.010	0.003^{**}
	(0.729)	(0.475)	(0.619)	(0.020)

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Table 6 - *Continued*

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
$\text{Returns}_{(-5,-1)}$	0.012	1.500	0.553	-0.279^{**}
	(0.990)	(0.271)	(0.548)	(0.034)
Industry fixed effects	Yes	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes	No
Observations	399	399	399	399
Pseudo/Adjusted R^2	0.103	0.138	0.126	0.112

Table 7
Rumor Accuracy and Stock Returns: Journalist Characteristics
This table examines the relationship between journalist characteristics and 1) the likelihood that a rumor comes true and 2) the target stock returns on the day the rumor is first published. Columns 1–3 present fixed effect logit regression coefficients in which the dependent variable is a dummy variable equal to one if a merger rumor came true within one year of the first rumor date. Column 4 presents OLS regression coefficients where the dependent variable is the rumor target's abnormal stock return on the date the first rumor article is published. Abnormal returns are calculated as the target returns minus the CRSP value-weighted index return. Variables are defined in the appendix. Industry fixed effects include Fama-French 17 industry codes. Standard errors are clustered at the industry level and *p*-values are reported in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
Day 0 Return		8.569***		
		(< 0.001)		
Estimated deal likelihood			0.496***	
			(0.006)	
Estimated announcement return				0.103*
				(0.085)
Log(Journalist age)	1.378**	1.247**	1.232**	0.038
	(0.027)	(0.015)	(0.039)	(0.370)
Undergraduate Degree				
Business & Economics	0.223	0.104	0.102	0.025
	(0.697)	(0.842)	(0.853)	(0.211)
Journalism	1.177***	1.214***	1.265***	0.007
	(0.003)	(0.002)	(< 0.001)	(0.638)
English	0.069	−0.051	0.074	0.019
	(0.851)	(0.901)	(0.844)	(0.338)
Political Science	0.272	0.105	0.281	0.019
	(0.506)	(0.798)	(0.488)	(0.128)
History	0.626	0.450	0.547	0.050
	(0.153)	(0.399)	(0.298)	(0.135)
Other	0.548	0.334	0.557	0.021

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Table 7 - *Continued*

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
	(0.280)	(0.442)	(0.217)	(0.408)
College SAT Percentile	0.016	0.013	0.013	0.000
	(0.261)	(0.292)	(0.352)	(0.655)
Expert in target industry	0.592*	0.627**	0.572*	0.003
	(0.073)	(0.047)	(0.086)	(0.847)
New York-based	0.440*	0.082	0.295	0.037***
	(0.052)	(0.785)	(0.333)	(0.004)
Award winner	0.209	0.591*	0.308	−0.057*
	(0.469)	(0.094)	(0.361)	(0.069)
Gender	−0.512	−0.425	−0.495	−0.012
	(0.143)	(0.193)	(0.158)	(0.231)
Returns$_{(-5,-1)}$	2.532***	5.007***	3.360***	−0.243***
	(0.001)	(< 0.001)	(< 0.001)	(0.001)
Log(Target book assets)	−0.340***	−0.290***	−0.334***	
	(< 0.001)	(< 0.001)	(< 0.001)	
Industry fixed effects	Yes	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes	No
Observations	296	296	296	296
Pseudo/Adjusted R^2	0.201	0.264	0.233	0.121

Table 8
Rumor Accuracy and Stock Returns: Article Characteristics
This table examines the relationship between article characteristics and and 1) the likelihood that a rumor comes true and 2) the target stock returns on the day the rumor is first published. Columns 1–3 present fixed effect logit regression coefficients in which the dependent variable is a dummy variable equal to one if a merger rumor came true within one year of the first rumor date. Column 4 presents OLS regression coefficients where the dependent variable is the rumor target's abnormal stock return on the date the first rumor article is published. Abnormal returns are calculated as the target returns minus the CRSP value-weighted index return. Variables are defined in the appendix. Industry fixed effects include Fama-French 17 industry codes. Standard errors are clustered at the industry level and p-values are reported in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
Day 0 Return		4.582***		
		(< 0.001)		
Estimated deal likelihood			0.273***	
			(0.001)	
Estimated announcement return				0.099
				(0.118)
Weak modal words (%)	−0.834***	−0.877***	−0.857***	0.005
	(0.001)	(0.003)	(0.001)	(0.444)
Anonymous source	0.367	0.306	0.366	0.019***
	(0.603)	(0.676)	(0.602)	(0.004)
Target response				
Has conversations	−0.193	−0.040	−0.041	−0.007
	(0.718)	(0.942)	(0.940)	(0.529)
Confirmed rumor	1.234***	0.975*	1.119**	0.076**
	(0.010)	(0.081)	(0.026)	(0.016)
Denied rumor	−0.999	−1.068	−0.984	0.007
	(0.271)	(0.246)	(0.250)	(0.655)
Couldn't be reached	0.446	0.259	0.403	0.048***
	(0.209)	(0.458)	(0.250)	(< 0.001)

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Table 8 - *Continued*

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
Wasn't asked	−0.101	−0.158	−0.055	0.014
	(0.828)	(0.731)	(0.903)	(0.165)
Merger stage				
Preliminary talks	0.728**	0.599*	0.620*	0.014
	(0.019)	(0.061)	(0.060)	(0.312)
In talks	1.319***	1.340***	1.319***	−0.003
	(0.001)	(< 0.001)	(0.001)	(0.785)
Made offer	0.611	0.604	0.684	−0.003
	(0.505)	(0.487)	(0.509)	(0.820)
Preparing bid	0.648	0.582	0.536	0.017
	(0.488)	(0.520)	(0.571)	(0.613)
For sale	0.361	0.459	0.590	−0.015
	(0.461)	(0.385)	(0.201)	(0.406)
Evaluating bids	0.965	1.057	0.959	−0.022
	(0.455)	(0.407)	(0.449)	(0.299)
Articles on scoop date (#)	0.164**	0.057	0.107	0.022**
	(0.043)	(0.437)	(0.176)	(0.031)
Rumor in headline	0.002	0.019	−0.051	0.000
	(0.998)	(0.978)	(0.940)	(0.992)
Number of bidders mentioned	0.098**	0.101**	0.101***	−0.001
	(0.011)	(0.036)	(0.010)	(0.763)
Price mentioned	0.667***	0.674***	0.634***	0.008
	(0.002)	(0.005)	(0.005)	(0.487)
$\text{Returns}_{(-5,-1)}$	0.013	1.229*	0.442	−0.266*
	(0.982)	(0.068)	(0.496)	(0.056)
Log(Target book assets)	−0.273***	−0.242**	−0.260**	
	(0.004)	(0.020)	(0.015)	
Industry fixed effects	Yes	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes	No
Observations	372	372	372	372
Pseudo/Adjusted R^2	0.224	0.244	0.237	0.202

Table 9
Runup and Total Premium
This table presents fixed effects OLS models of the cumulative abnormal stock returns of merger targets in the three windows: runup (event window $(-42,-1)$ relative to the public announcement date), announcement $(0,+5)$, and the combined period $(-42,+5)$. Observations include mergers for public US targets that were announced in 2000-2011. Variables are defined in the appendix. All regressions include target size (log of market equity). Firm-level controls include advertising/assets, industry sales to households, R&D/assets, Tobin's Q, and a dummy for a valuable brand. The numbers in parentheses are p-values from standard errors clustered at the Fama-French 17 industry level. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Dependent variable:	Target CAR (-42,-1)		Target CAR (0,+5)		Target CAR (-42,+5)	
Rumor	0.077***	0.076***	−0.078***	−0.079***	0.000	−0.008
	(< 0.001)	(< 0.001)	(<.001)	(< 0.001)	(0.994)	(0.503)
Completed		0.029**		0.040***		0.072***
		(0.050)		(< 0.001)		(< 0.001)
Majority cash		−0.001		−0.018		−0.017
		(0.806)		(0.248)		(0.253)
Tender offer		0.038***		0.096***		0.141***
		(< 0.001)		(<.001)		(< 0.001)
Leveraged buyout		−0.017**		0.015		−0.015
		(0.046)		(0.213)		(0.110)
Cross-border		0.014		0.002		0.012
		(0.126)		(0.892)		(0.495)
Target takeover defenses		−0.021		0.020		0.003
		(0.212)		(0.144)		(0.907)
Firm-level controls	No	Yes	No	Yes	No	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2431	2431	2431	2431	2431	2431
Adjusted R^2	0.035	0.053	0.063	0.110	0.062	0.113

Internet Appendix for

"Rumor Has It: Sensationalism in Financial Media"

1. Data Collection

This section of the Internet Appendix elaborates on the data collection process for journalist and article characteristics.

1.1. Journalist Characteristics

To obtain data for the 382 journalists who authored or coauthored any scoop article in our sample, we begin by collecting journalists' age and gender. To reliably establish a journalist's age and gender, we use the Lexis Nexis Public Records database, which aggregates information on 450 million unique U.S. individuals (both alive and deceased) available from various federal, state, and county records, such as drivers' licenses, property tax assessment records, marriage and divorce records, voter registration records, utility connection records, and many others. This information is combined into a comprehensive person report for each individual, which provides the year and month of birth, history of residential addresses, maiden names for women, and information on employment, among many other characteristics. To identify journalists in this database, we use their first, middle, and last name, as well as the approximate age based on the year of college graduation (discussed below) and then verify each match by ensuring that the person's employment record in the Lexis Nexis database matches that of the journalist.

Next, we collect data on journalists' education, following a two-step process. First, we read the journalists' biographical sketches from personal web pages and professional profiles on the social networking site LinkedIn. We supplement these sources with web searches, which often bring up helpful academic resources, such as university alumni publications which discuss journalists as alumni and provide their educational background.

In the second step, we contact the registrars of the universities attended by journalists to verify their degree, year of graduation, and academic specialization. While many registrars provide this information to us directly, some universities have outsourced the degree verification service to a third-party data repository, the National Student Clearinghouse (NSC). In these cases, we verify the degree by contacting the NSC. For a few observations, we are unable to verify the journalists' undergraduate majors because a small minority of schools, mostly foreign universities in the UK and Canada, require an additional consent form. To verify degrees of female journalists, we also obtain their maiden names from the Lexis Nexis Public Records database if the university registrar is unable to verify the degree under the journalist's current family name.

To measure the quality of a journalist's undergraduate training, we use the university's median SAT score, expressed as a percentile. Since most journalists attended liberal arts programs, we focus on the verbal score, the arguably more relevant score of quality for journalists. Since our journalists attended colleges at different times, we hand-collect three cross-sections of SAT scores from the College Handbook published by the College Entrance Examination Board for the following entry classes: 1979, 2004, and 2012. We find that while score levels have increased significantly with time, the relative ranking of colleges according to percentile scores (which account for time trends and changes in the applicant pool) has been stable. Therefore, to achieve the most complete data coverage, we focus on the 2012 scores. If the SAT score is unavailable from the College Board, we contact the university's admissions directly, thereby also obtaining this information for foreign universities that accept SAT scores as one of the entrance exams.

Next, we collect journalists' primary and secondary areas of professional specialization from LinkedIn. In some cases, a journalist's specialization is evident from his or her professional job title (e.g., 'Reporter, Automotive'), while in others, it is provided by the newspaper in the journalist's biographical sketch. We verify the reported specialization by reading samples of the journalist's articles.

We also record the geographic location of the journalist at the time of the publication of the rumor article. Since many newspapers have regional bureaus (e.g., the *Wall Street Journal* has 12 U.S. bureaus), the journalist is often stationed in a different city than the newspaper's headquarters. We collect these data in two steps. First, we extract a journalist's office location from his or her job title (e.g., 'Correspondent, Atlanta Bureau') or from the newspaper's biographical sketch. Second, we verify and complement these data by obtaining journalists' residential addresses from the Lexis Nexis Public Records Database and matching them to newspaper bureaus. The Lexis Nexis database provides residential addresses based on a person's utility connection records as well as real estate deed records, which include the starting and ending dates. The reliance on utility connection records allows us to trace a journalist's location regardless of whether he rents, owns, or relocates for a temporary job assignment.

1.2. Article Characteristics

We calculate the frequency of weak modal words in an article based on the dictionary for financial texts from Loughran and McDonald (2011), as updated in August 2013. The list of weak modal words includes the following words: *apparently, appeared, appearing, appears, conceivable, could, depend, depended, depending, depends, may, maybe, might, nearly, occasionally, perhaps, possible, possibly, seldom, seldomly, sometimes, somewhat, suggest, suggests, uncertain*, and *uncertainly*.

When calculating the frequency of weak modal words, we are careful to avoid spurious matches. The first spurious match pertains to the weak modal word 'may,' which often appears at the bottom of an article to indicate a journalist's contact information, such as "the author may be reached at…" To control for these matches, we separate the body and headline of the article text from the publication date, media source, and journalist contact information. Second, we manually search for spurious instances of weak modal words that occur when one of the weak modal words coincides with a proper noun, such as the month of May or May Department Stores. To control for this type of spurious matches, we remove capitalized weak modal words, unless they appear in the headline or at the beginning of a sentence. We then check the headlines manually to ensure there are no spurious matches.

Internet Appendix Table 1
The Likelihood of Rumors

This table presents fixed effects logit and linear probability models of the probability that a rumor article will be published about a potential merger. The dependent variable equals one if a rumor article was published about the target. Observations include targets discussed in 501 merger rumors published in newspapers as well as targets of actual merger bids announced in 2000-2011. The logit models are fixed effects logits with year and industry effects (using Fama-French 17 industry definitions). The OLS models are linear probability models. The numbers in parentheses are p-values from standard errors clustered at the industry level. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

	All Targets		Public Targets	
	Logit	OLS	Logit	OLS
Public target	2.436***	0.048***		
	(< 0.001)	(< 0.001)		
Valuable brand	3.050***	0.412***	1.786***	0.309***
	(< 0.001)	(< 0.001)	(0.002)	(0.002)
Industry sales to households	0.743*	0.017	−0.804**	−0.016**
	(0.086)	(0.224)	(0.011)	(0.037)
Foreign	−1.925***	−0.047***	−2.418***	−0.046***
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)
Log(Target book assets)			0.737***	0.017***
			(< 0.001)	(< 0.001)
Advertising/Assets (%)			0.073**	0.006**
			(0.014)	(0.017)
Tobin's Q			0.299***	0.010***
			(< 0.001)	(0.006)
R&D/Assets (%)			0.092***	0.003***
			(< 0.001)	(< 0.001)
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	18,325	18,325	4,523	4,523
Pseudo/Adjusted R^2	0.274	0.114	0.353	0.151

Internet Appendix Table 2
Determinants of Target Announcement Returns
This table presents fixed effects OLS models of the cumulative abnormal stock returns of merger targets in the three days centered on the first official announcement of the merger. Observations include mergers for public US targets that were announced in 2000-2011. Variables are defined in the appendix. The numbers in parentheses are p-values from standard errors clustered at the Fama-French 17 industry level. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *. Year 2000 and "Industry 17: Other" are omitted.

	Target $CAR_{(-1,+1)}$
Log(Target book assets)	-0.030^{***}
	(< 0.001)
Industry 1: Food	0.030^{***}
	(< 0.001)
Industry 2: Mining and Minerals	-0.057^{***}
	(< 0.001)
Industry 3: Oil and Petroleum Products	-0.007^{*}
	(0.067)
Industry 4: Textiles, Apparel, & Footware	-0.024^{***}
	(< 0.001)
Industry 5: Consumer Durables	-0.024^{***}
	(< 0.001)
Industry 6: Chemicals	0.077^{***}
	(< 0.001)
Industry 7: Drugs, Soap, Perfumes, Tobacco	0.049^{***}
	(< 0.001)
Industry 8: Construction	0.066^{***}
	(< 0.001)
Industry 9: Steel Works	0.004
	(0.105)
Industry 10: Fabricated Products	0.015^{*}
	(0.052)
Industry 11: Machinery and Business Equipment	0.035^{***}
	(< 0.001)
Industry 12: Automobiles	0.040^{***}
	(< 0.001)
Industry 13: Transportation	0.023^{***}
	(< 0.001)
Industry 14: Utilities	-0.011^{*}
	(0.051)

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Internet Appendix Table 2 - *Continued*

	Target $\text{CAR}_{(-1,+1)}$
Industry 15: Retail Stores	-0.005^{**}
	(0.013)
Industry 16: Finance	0.015^{***}
	(0.003)
Year 2001	-0.007
	(0.831)
Year 2002	-0.083^{***}
	(0.010)
Year 2003	-0.076^{***}
	(0.001)
Year 2004	-0.055^{***}
	(0.002)
Year 2005	-0.046^{***}
	(0.010)
Year 2006	-0.037^{*}
	(0.080)
Year 2007	-0.018
	(0.324)
Year 2008	-0.003
	(0.897)
Year 2009	-0.059
	(0.153)
Year 2010	0.018
	(0.416)
Year 2011	0.049^{**}
	(0.011)
Constant	0.400^{***}
	(< 0.001)
Observations	2555
Adjusted R^2	0.089

Internet Appendix Table 3
Likelihood of Withdrawal

This table presents fixed effects linear probability models estimated using OLS. The dependent variable is a dummy variable equal to one if a merger bid is withdrawn and zero otherwise. Observations include mergers announced in 2000-2011 for public US targets. Numbers in parentheses are p-values from standard errors clustered at the Fama-French 17 industry level. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

	Dependent variable: Bid Withdrawn			
	(1)	(2)	(3)	(4)
Rumor		−0.005		−0.005
		(0.708)		(0.659)
Log(Target assets)	−0.001	0.000	−0.002	−0.001
	(0.789)	(0.823)	(0.335)	(0.380)
Valuable brand			−0.036*	−0.036*
			(0.081)	(0.073)
Advertising/Assets			0.220	0.220
			(0.177)	(0.177)
Industry sales to households			−0.021	−0.021
			(0.132)	(0.130)
Tobin's Q			−0.009	−0.009
			(0.024)	(0.026)
R&D/Assets			−0.082	−0.083
			(0.373)	(0.374)
Majority cash			−0.022	−0.023
			(0.255)	(0.248)
Tender offer			−0.025	−0.025
			(0.142)	(0.137)
Leveraged buyout			0.076***	0.076***
			(< 0.001)	(< 0.001)
Cross-border			−0.006	−0.006
			(0.444)	(0.453)
Target takeover defenses			0.308***	0.307***
			(< 0.001)	(< 0.001)
Industry and year fixed effects	Yes	Yes	Yes	Yes
Observations	5831	5831	5831	5831
Adjusted R^2	0.008	0.008	0.031	0.031

Internet Appendix Table 4
Rumor Accuracy and Stock Returns: Journalist Fixed Effects
This table examines the relationship between journalist fixed effects and 1) the likelihood that a rumor comes true and 2) the target stock returns on the day the rumor is first published. Columns 1–3 present fixed effect logit regression coefficients in which the dependent variable is a dummy variable equal to one if a merger rumor came true within one year of the first rumor date. Column 4 presents OLS regression coefficients where the dependent variable is the rumor target's abnormal stock return on the date the first rumor article is published. Abnormal returns are calculated as the target returns minus the CRSP value-weighted index return. Variables are defined in the appendix. Industry fixed effects include Fama-French 17 industry codes. Fixed effects include all journalists with at least five scoops. For brevity, we report coefficients for the 12 most prolific journalists. Standard errors are clustered at the industry level and p-values are reported in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
Day 0 Return		4.301***		
		(< 0.001)		
Estimated deal likelihood			0.317***	
			(0.001)	
Estimated announcement return				0.135**
				(0.034)
Dennis K. Berman	1.791***	1.773***	1.769***	0.003
	(< 0.001)	(< 0.001)	(< 0.001)	(0.584)
Andrew Ross Sorkin	1.641***	1.266**	1.458**	0.093
	(0.005)	(0.014)	(0.013)	(0.185)
Nikhil Deogun	−0.488	−0.531	−0.563	−0.016
	(0.592)	(0.528)	(0.525)	(0.210)
Robert Frank	−14.700***	−14.540***	−14.600***	−0.050***
	(< 0.001)	(< 0.001)	(< 0.001)	(0.001)
Robin Sidel	2.113**	2.022***	2.340**	0.018
	(0.011)	(0.002)	(0.018)	(0.374)
Anupreeta Das	−0.083	−0.104	−0.227	0.030
	(0.708)	(0.651)	(0.284)	(0.265)

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Internet Appendix Table 4 - *Continued*

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
Michael J. De La Merced	-2.408^{*}	-1.948	-2.200^{*}	-0.116
	(0.058)	(0.147)	(0.091)	(0.199)
Jeffrey McCracken	0.408	0.664	0.690	-0.019
	(0.640)	(0.438)	(0.460)	(0.119)
Anita Raghavan	-0.893^{*}	-1.323^{***}	-1.213^{***}	0.054
	(0.066)	(0.001)	($<$ 0.001)	(0.327)
Suzanne Kapner	0.792	0.932	0.855	-0.019
	(0.230)	(0.247)	(0.221)	(0.426)
Sarah Ellison	0.049	-0.118	-0.155	0.035
	(0.972)	(0.938)	(0.916)	(0.238)
Erica Copulsky	0.776	0.836	0.805	-0.024
	(0.160)	(0.219)	(0.160)	(0.277)
$\text{Returns}_{(-5,-1)}$	0.197	1.411^{**}	0.717	-0.245^{*}
	(0.728)	(0.050)	(0.220)	(0.081)
Log(Target book assets)	-0.309^{***}	-0.280^{***}	-0.300^{***}	
	($<$ 0.001)	(0.006)	(0.003)	
Industry fixed effects	Yes	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes	No
Observations	406	406	406	406
Pseudo/Adjusted R^2	0.174	0.198	0.192	0.103

Internet Appendix Table 5
Rumor Accuracy and Stock Returns:
Article Characteristics and Journalist Fixed Effects

This table examines the relationship between article characteristics and 1) the likelihood that a rumor comes true and 2) the target stock returns on the day the rumor is first published, while controlling for journalist fixed effects. Columns 1–3 present fixed effect logit regression coefficients in which the dependent variable is a dummy variable equal to one if a merger rumor came true within one year of the first rumor date. Column 4 presents OLS regression coefficients where the dependent variable is the rumor target's abnormal stock return on the date the first rumor article is published. Abnormal returns are calculated as the target returns minus the CRSP value-weighted index return. Variables are defined in the appendix. Industry fixed effects include Fama-French 17 industry codes. Fixed effects include all journalists with at least five scoops. For brevity, we report coefficients for the 12 most prolific journalists. Standard errors are clustered at the industry level and p-values are reported in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
Day 0 Return		4.625^{***} (< 0.001)		
Estimated deal likelihood			0.302^{***} (0.001)	
Estimated announcement return				0.082 (0.238)
Weak modal words (%)	-0.755^{***} (0.008)	-0.809^{**} (0.011)	-0.784^{***} (0.009)	0.007 (0.245)
Anonymous source	0.351 (0.611)	0.274 (0.704)	0.340 (0.614)	0.018^{***} (0.002)
Target response				
Has conversations	0.109 (0.842)	0.245 (0.664)	0.259 (0.650)	−0.009 (0.383)
Confirmed rumor	1.346^{***} (< 0.001)	1.162^{***} (< 0.001)	1.107^{***} (< 0.001)	0.070^{***} (0.005)
Denied rumor	−0.978 (0.350)	−1.034 (0.337)	−0.950 (0.345)	0.001 (0.945)

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Internet Appendix Table 5 - *Continued*

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
Couldn't be reached	0.438	0.255	0.393	0.041***
	(0.255)	(0.499)	(0.306)	(< 0.001)
Wasn't asked	0.048	0.009	0.104	0.011
	(0.930)	(0.986)	(0.849)	(0.360)
Merger stage				
Preliminary talks	0.666	0.584	0.549	0.015
	(0.114)	(0.188)	(0.215)	(0.307)
In talks	1.519***	1.570***	1.543***	−0.007
	(< 0.001)	(< 0.001)	(< 0.001)	(0.603)
Made offer	0.278	0.281	0.369	−0.004
	(0.708)	(0.709)	(0.656)	(0.746)
Preparing bid	0.301	0.254	0.194	0.016
	(0.737)	(0.751)	(0.828)	(0.627)
For sale	0.038	0.160	0.321	−0.017
	(0.958)	(0.832)	(0.661)	(0.245)
Evaluating bids	1.428	1.645	1.414	−0.032*
	(0.273)	(0.218)	(0.310)	(0.060)
Articles on scoop date (#)	0.150	0.040	0.085	0.021**
	(0.107)	(0.665)	(0.378)	(0.029)
Rumor in headline	−0.094	−0.086	−0.163	0.000
	(0.881)	(0.894)	(0.798)	(0.990)
Number of bidders mentioned	0.130***	0.139***	0.138***	−0.001
	(< 0.001)	(< 0.001)	(< 0.001)	(0.712)
Price mentioned	0.720***	0.686**	0.658**	0.011
	(0.008)	(0.014)	(0.013)	(0.357)
Journalist Fixed Effects				
Dennis K. Berman	1.403***	1.373***	1.376***	0.002
	(< 0.001)	(< 0.001)	(< 0.001)	(0.814)
Andrew Ross Sorkin	0.212	−0.083	0.100	0.047
	(0.869)	(0.947)	(0.941)	(0.239)
Nikhil Deogun	−0.340	−0.398	−0.369	0.002

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Internet Appendix Table 5 - *Continued*

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
	(0.656)	(0.567)	(0.618)	(0.836)
Robert Frank	-17.740^{***}	-16.720^{***}	-16.920^{***}	-0.052^{**}
	(< 0.001)	(< 0.001)	(< 0.001)	(0.017)
Robin Sidel	1.940^{**}	1.801^{**}	2.265^{**}	0.002
	(0.027)	(0.016)	(0.027)	(0.934)
Anupreeta Das	-0.974^{***}	-0.691^{***}	-0.786^{***}	-0.034
	(< 0.001)	(0.007)	(0.005)	(0.223)
Michael J. De La Merced	-1.977	-1.677	-1.848	-0.050
	(0.154)	(0.218)	(0.210)	(0.403)
Jeffrey McCracken	-0.818	-0.577	-0.302	-0.030^{**}
	(0.502)	(0.642)	(0.818)	(0.044)
Anita Raghavan	-1.100	-1.491^{*}	-1.286^{*}	0.040
	(0.151)	(0.059)	(0.092)	(0.433)
Suzanne Kapner	0.545	0.694	0.548	-0.015
	(0.321)	(0.291)	(0.335)	(0.556)
Sarah Ellison	-0.714	-0.835	-0.829	0.035
	(0.442)	(0.395)	(0.405)	(0.145)
Erica Copulsky	0.363	0.300	0.112	-0.010
	(0.277)	(0.516)	(0.761)	(0.735)
$\text{Returns}_{(-5,-1)}$	-0.183	1.036	0.275	-0.257^{*}
	(0.834)	(0.212)	(0.737)	(0.065)
Log(Target book assets)	-0.289^{**}	-0.260^{**}	-0.280^{**}	
	(0.015)	(0.040)	(0.033)	
Industry fixed effects	Yes	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes	No
Observations	372	372	372	372
Pseudo/Adjusted R^2	0.288	0.306	0.301	0.197

Internet Appendix Table 6
Rumor Accuracy and Stock Returns:
Article Characteristics and Media Source Fixed Effects

This table examines the relationship between article characteristics and 1) the likelihood that a rumor comes true and 2) the target stock returns on the day the rumor is first published, while controlling for newspaper fixed effects. Columns 1–3 present fixed effect logit regression coefficients in which the dependent variable is a dummy variable equal to one if a merger rumor came true within one year of the first rumor date. Column 4 presents OLS regression coefficients where the dependent variable is the rumor target's abnormal stock return on the date the first rumor article is published. Abnormal returns are calculated as the target returns minus the CRSP value-weighted index return. Variables are defined in the appendix. Industry fixed effects include Fama-French 17 industry codes. Newspaper fixed effects include newspapers with at least five scoop articles. Standard errors are clustered at the industry level and p-values are reported in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
Day 0 Return		4.967***		
		(< 0.001)		
Estimated deal likelihood			0.313***	
			(0.007)	
Estimated announcement return				0.107
				(0.107)
Weak modal words (%)	−0.840***	−0.875**	−0.855***	0.005
	(0.005)	(0.011)	(0.004)	(0.412)
Anonymous source	0.482	0.442	0.524	0.017**
	(0.509)	(0.561)	(0.474)	(0.030)
Target response				
Has conversations	−0.257	−0.116	−0.049	−0.010
	(0.531)	(0.787)	(0.905)	(0.342)
Confirmed rumor	1.205	0.954	1.133	0.066**
	(0.127)	(0.258)	(0.134)	(0.024)
Denied rumor	−1.099	−1.084	−1.052	0.000
	(0.308)	(0.312)	(0.302)	(0.977)
Couldn't be reached	0.269	0.143	0.271	0.040***
	(0.422)	(0.673)	(0.404)	(< 0.001)
Wasn't asked	−0.234	−0.260	−0.169	0.013

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Internet Appendix Table 6 - *Continued*

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
	(0.660)	(0.630)	(0.760)	(0.306)
Merger stage				
Preliminary talks	0.763***	0.628**	0.640**	0.016
	(0.006)	(0.028)	(0.019)	(0.257)
In talks	1.323***	1.365***	1.335***	−0.009
	(0.001)	(0.001)	(0.001)	(0.565)
Made offer	0.916	0.916	0.996	−0.009
	(0.283)	(0.235)	(0.313)	(0.664)
Preparing bid	0.610	0.592	0.524	0.013
	(0.495)	(0.500)	(0.555)	(0.692)
For sale	0.092	0.273	0.394	−0.029
	(0.884)	(0.708)	(0.554)	(0.125)
Evaluating bids	0.912	1.043	0.922	−0.027*
	(0.488)	(0.449)	(0.484)	(0.082)
Articles on scoop date (#)	0.142*	0.026	0.077	0.021**
	(0.067)	(0.728)	(0.339)	(0.046)
Rumor in headline	0.079	0.077	0.003	0.004
	(0.917)	(0.921)	(0.997)	(0.758)
Number of bidders mentioned	0.115**	0.116**	0.118***	0.000
	(0.012)	(0.049)	(0.009)	(0.874)
Price mentioned	0.734***	0.747***	0.711***	0.010
	(< 0.001)	(< 0.001)	(< 0.001)	(0.418)
$\text{Returns}_{(-5,-1)}$	−0.236	0.989	0.256	−0.279*
	(0.720)	(0.207)	(0.741)	(0.058)
Log(Target book assets)	−0.271***	−0.243***	−0.249***	
	(< 0.001)	(0.002)	(0.002)	
Newspaper fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes	No
Observations	372	372	372	372
Pseudo/Adjusted R^2	0.255	0.277	0.271	0.190

Internet Appendix Table 7
Rumor Accuracy and Stock Returns: Newspaper Fixed Effects

This table examines the relationship between newspaper fixed effects and 1) the likelihood that a rumor comes true and 2) the target stock returns on the day the rumor is first published. Columns 1–3 present fixed effect logit regression coefficients in which the dependent variable is a dummy variable equal to one if a merger rumor came true within one year of the first rumor date. Column 4 presents OLS regression coefficients where the dependent variable is the rumor target's abnormal stock return on the date the first rumor article is published. Abnormal returns are calculated as the target returns minus the CRSP value-weighted index return. Variables are defined in the appendix. Industry fixed effects include Fama-French 17 industry codes. Newspaper fixed effects include all media sources with at least five scoop articles. We only report the coefficients for the most prolific US-based newspapers. Standard errors are clustered at the industry level and *p*-values are reported in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
Day 0 Return		5.053***		
		(< 0.001)		
Estimated deal likelihood			0.348***	
			(0.005)	
Estimated announcement return				0.153**
				(0.012)
Wall Street Journal	0.482	0.587	0.611	−0.008
	(0.402)	(0.299)	(0.279)	(0.601)
New York Times	0.427	0.285	0.385	0.027
	(0.378)	(0.525)	(0.417)	(0.134)
New York Post	0.621	0.675	0.720	−0.002
	(0.458)	(0.466)	(0.391)	(0.911)
Barron's	0.155	0.341	0.435	−0.021
	(0.823)	(0.630)	(0.484)	(0.211)
Bloomberg	2.711***	2.638**	2.708***	0.060**
	(0.009)	(0.012)	(0.006)	(0.037)
Boston Globe	−0.087	0.379	0.247	−0.045***
	(0.888)	(0.543)	(0.719)	(0.010)

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Internet Appendix Table 7 - *Continued*

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
Los Angeles Times	0.668	1.069	0.791	-0.042^{**}
	(0.651)	(0.454)	(0.556)	(0.015)
Denver Post	-0.195	0.100	-0.009	-0.058^{***}
	(0.881)	(0.937)	(0.994)	(0.003)
Pittsburgh Post-Gazette	1.157^{*}	1.161^{*}	1.070^{*}	0.004
	(0.066)	(0.057)	(0.063)	(0.896)
$\text{Returns}_{(-5,-1)}$	-0.139	1.429^{*}	0.456	-0.277^{**}
	(0.796)	(0.074)	(0.457)	(0.036)
Log(Target book assets)	-0.254^{***}	-0.225^{***}	-0.246^{***}	
	($<$ 0.001)	(0.002)	(0.002)	
Industry fixed effects	Yes	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes	No
Observations	406	406	406	406
Pseudo/Adjusted R^2	0.132	0.166	0.155	0.080

Internet Appendix Table 8
Rumor Accuracy and Stock Returns: Newspaper Characteristics
This table examines the relationship between newspaper characteristics and 1) the likelihood that a rumor comes true and 2) the target stock returns on the day the rumor is first published. Columns 1–3 present fixed effect logit regression coefficients in which the dependent variable is a dummy variable equal to one if a merger rumor came true within one year of the first rumor date. Column 4 presents OLS regression coefficients where the dependent variable is the rumor target's abnormal stock return on the date the first rumor article is published. Abnormal returns are calculated as the target returns minus the CRSP value-weighted index return. Variables are defined in the appendix. Industry fixed effects include Fama-French 17 industry codes. Standard errors are clustered at the industry level and p-values are reported in parentheses. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *.

Dependent variable:	Rumor Comes True			Return Day 0
	(1)	(2)	(3)	(4)
Day 0 Return		6.192***		
		(< 0.001)		
Estimated deal likelihood			0.378***	
			(0.002)	
Estimated announcement return				0.138***
				(0.005)
Family-run media company	0.240	0.269	0.284	−0.005
	(0.459)	(0.383)	(0.342)	(0.618)
Log(Newspaper age)	−0.097	−0.153	−0.107	0.009**
	(0.507)	(0.335)	(0.482)	(0.030)
Log(Newspaper Circulation)	−0.048	−0.088	−0.036	0.005*
	(0.685)	(0.501)	(0.752)	(0.092)
$\text{Returns}_{(-5,-1)}$	−0.190	1.120*	0.289	−0.194**
	(0.725)	(0.093)	(0.590)	(0.037)
Log(Target book assets)	−0.271***	−0.239***	−0.268***	
	(< 0.001)	(0.002)	(0.001)	
Industry fixed effects	Yes	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes	No
Observations	390	390	390	390
Pseudo/Adjusted R^2	0.101	0.147	0.128	0.050