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Do Financial Advisors Matter for M&A Pre-Announcement Returns?

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Do Financial Advisors Matter for M&A Pre-Announcement Returns?*

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Abstract

This study documents economically meaningful and persistent financial advisor fixed effects in target firms' abnormal stock returns shortly prior to takeover announcements. Additional difference-in-differences analyses suggest that advisors are associated with lower pre-bid stock returns after their senior staff were defendants in SEC insider trading enforcement actions. Returns are higher for advisors with more previously advised deals and those located in NYC. The evidence helps explain the prevalent phenomenon of pre-bid stock returns. It contributes to the inconclusive literature on banks' exploitation of private information gained via advisory services, which is limited to disclosed, traceable activities indicative of information leakage.

JEL classification: G14, G15, G21, G34, K42
Keywords: Financial Advisors, Mergers and Acquisitions, Information Leakage, Target Runups

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1 Introduction

Significant runups in target firms' stock prices as well as abnormal trading activity in option markets shortly prior to takeover announcements are prevalent in the market for corporate control (see, e.g., Keown and Pinkerton, 1981; Jarrell and Poulsen, 1989; Acharya and Johnson, 2010; Augustin et al., 2019). This phenomenon indicates the prevalence of information leakage and potential exploitation of private information by parties involved early in the takeover process (e.g., Meulbroek, 1992). In their role as financial advisors, banks are typically one of those prematurely informed parties. Accordingly, the literature examines whether banks protect or exploit their clients' material non-public information obtained through M&A advisory service. While some studies provide evidence suggesting that banks exploit information (e.g., Bodnaruk et al., 2009; Jegadeesh and Tang, 2011; Lowry et al., 2019), other studies either find only weak evidence (Kedia and Zhou, 2014) or even conclude that banks do not use information in traceable manners (Griffin et al., 2012).[1] By providing evidence that banks are systematically associated with stock price runups prior to takeover announcements, our study contributes to the limited literature on pre-bid stock price movements as well as to the inconclusive literature on the question of whether banks protect the information they obtain through M&A services.

The existing work typically studies whether financial advisors use inside information by analyzing the revealed set of transactions by other bank divisions or clients, which are subject to mandatory disclosure. By design, these studies are unable to capture information leakage and the related trading activities based on concealed communication of inside information via *non-traceable* channels. Based on legal documents of enforced cases, Ahern (2017) provides evidence on the importance of social relationships and personal communication for illegal insider trading. He documents that inside information flows through private networks, taking on average three links until the tip reaches the investor. This evidence demonstrates substantial efforts to disguise the tipping of inside information, making it more difficult to

[1] Fich et al. (2020) observe that hedge funds increase and mutual funds decrease their holdings in future targets, while they do not link this pattern to relationships with M&A advisors.

detect illegal trading based on traceable behavior, in particular behavior that is subject to mandatory disclosure. Given that banks are strictly regulated (e.g., under the 1934 Securities Exchange Act prescribing so-called Chinese Walls) and given that financial advisors are arguably among the suspects whenever information on their advised deals leaks, bankers can be expected to rather distribute inside information via non-traceable channels. Against this background, our study takes a different approach, which allows to address the question of whether financial advisors are generally associated with information leakage, without relying on a measure of traceable behavior.

We address this question by conducting a fixed effects test, as established by Bertrand and Schoar (2003). Analogously to Edmans and Bao (2011), who regress bidders' takeover announcement returns on the identities of financial advisors to study whether advisors matter for bidders' stock returns, we regress pre-bid stock returns of takeover targets on advisor identities. As our main measure of short-term runups, we use the cumulative abnormal return in the 10 trading days preceding a takeover announcement, i.e., *CAR (-10,-1)*. This event window follows the literature (e.g., Acharya and Johnson, 2010) and captures the period after which deal negotiations are typically finished but before the deal is officially announced (see Ahern and Sosyura, 2014). Accordingly, during this period, financial advisors can predict deal outcomes with high precision.[2]

It is not clear ex ante whether financial advisors will exploit or rather protect their clients' inside information. On the one hand, there are several mutually non-exclusive reasons why financial advisors can be expected to extract rents from their client's information. First, bank employees can be tempted to privately exploit information about a pending takeover bid. In this regard, we identify 40 cases of takeover-related insider trading enforcement actions by the Securities and Exchange Commission (SEC) over our sample period, which involve 225 employees. Second, banks can have incentives to use non-public information obtained through

[2]In a recent complementary study of 545 acquisitions, Heitzman and Klasa (2021) find empirical support for the notion that private information created by nonpublic negotiation activities triggers abnormal returns in the target stock price. They document a mean distance of the negotiation activity to the first public disclosure is 20 days. As a consequence, the $(-10, -1)$ window we consider in our main analysis mostly comprises days after completed negotiations.

their M&A advisory service to promote other business lines, such as proprietary trading or brokerage services, or to share this information with important clients (e.g., Jegadeesh and Tang, 2011; Lowry et al., 2019). Furthermore, banks can have incentives to leak information about pending takeovers for strategic reasons acting in the interest of the clients they advise on these transactions. For example, buy-side advisors can leak information to increase the target's stock price once a binding purchase price or stock exchange ratio has been negotiated. On the other hand, theoretical and empirical studies argue that banks have strong incentives to protect their reputation, for example because more reputable M&A advisors can charge higher fees (see Golubov et al., 2012, and the literature therein), suggesting that banks have strong incentives to protect their client's private information.[3]

According to the aforementioned reasoning, it is an open empirical question whether financial advisors exploit or protect their clients' inside information. Furthermore, whether a financial advisor is associated with a relatively high or low level of information leakage will depend on various (often unobserved) factors, such as a bank's culture and incentive structures; or bankers' networks. As banks differ in terms of these (and other) factors, they can also be expected to differ in terms of the level of information leakage their advised deals are associated with, supporting the use of the fixed effects test as an appropriate method.

In this study, we provide evidence for a statistically and economically significant association between financial advisors and information leakage in the U.S. M&A market based on 5,316 transactions involving public targets.[4] Specifically, we find significant fixed effects for bidders' financial advisors in the pre-announcement returns of takeover targets. Our regressions account for a variety of takeover characteristics (e.g., deal volume, number of advisors, offer premium, pre-bid takeover rumors, and toeholds) as well as acquiror, industry and time fixed effects and time-varying acquiror and target characteristics. Advisor fixed effects re-

[3]However, several papers cast doubt on the performance of the reputation mechanism in the market for M&A financial advisors (see, e.g., Golubov et al., 2012; Rau, 2000). Against this background, non-traceable information leakage might be less likely to cause considerable reputational damage given that the absence of solid external evidence impedes litigation and thus accountability.

[4]In untabulated regressions, we also find significant fixed effects for target firms' financial advisors. However, we focus on bidding firms' advisors to alleviate concerns of endogenous target firm-advisor matching and strategic leakage.

main statistically significant when we additionally control for average target runups at the acquiror level to account for acquiror-specific leakage. These patterns are also confirmed when considering abnormal trading volume prior to takeover announcements.

In terms of economic significance, we find the interquartile range of advisor fixed effects to be 3.9% for *CAR(-10,-1)*, compared with the full-sample average of 4.8%. We find that the differences in target stock price runups across financial advisors are persistent over time and can be predicted based on past runups. More specifically, we sort advisors into quintiles based on the target runups of their advised takeovers over the past 2 or 3 years. Advisors in the top quintile are associated with significantly higher target runups over the next 1, 2 and 3 years when compared to advisors in the bottom quintile. The persistence of advisor-driven leakage may inform firms in their future selection of financial advisors as well as regulators attempting to detect potential insider trading.

One concern with the fixed effects approach is the reliability of the F-test, which can be more likely to reject the null hypothesis when many individual effects are tested simultaneously. To address this concern, we follow Fee et al. (2013) and conduct a placebo test to learn about the distribution of the F-value when the null hypothesis is true by design. To this end, we randomly allocate financial advisors to takeovers, while mimicking the actual distribution of the number of deals per advisor. We repeat this procedure 10,000 times. The actual F-values are more than two standard deviations away from the mean F-values derived from placebo data. This result suggests that the F-test is reliable in our context as it performs well in not rejecting the null hypothesis when we do not expect any impact of financial advisors by construction.

Another concern is that financial advisor fixed effects might capture an unobserved factor that is correlated with both the advisor choice and runups. Although the mere predictive power of our results has practical implications, we perform a second test to strengthen the link towards a more causal interpretation. To this end, we use M&A-related insider trading enforcement actions by the SEC against employees of financial advisors as shocks that arguably reduce advisor-specific propensities to leak or exploit inside information. The evidence in Meulbroek (1992) suggests that investigations are typically triggered by reasons unrelated to screening by regulators, i.e., external tips or whistleblowing, are the most important source of investigations. Similarly, Kacperczyk and Pagnotta (2019) argue that the decision to litigate depends on the availability of external evidence. They show that the SEC does not appear to choose cases based on profitability and that the patterns that originate from the whistleblower reward program and trade monitoring are comparable to those brought forward by alternative sources, which reject the notion of systematic selection in the SEC enforcement cases. Hence, the existing evidence supports an arguably quasi-exogenous interpretation of the enforcement events. Insider trading enforcement actions are likely to constitute shocks to financial advisors given their low probability of occurrence in conjunction with the high opacity in terms of SEC enforcement preferences. We conduct a difference-in-differences analysis around these enforcement actions that exploit variation in treatment intensity depending on whether senior or junior staff were defendants in the respective enforcement actions. Our results suggest target runups decrease by approximately one third of a standard devision after their senior staff were defendants in SEC enforcement actions, while we find no significant reduction in target runups after advisors' junior staff were defendants. This evidence is consistent with both lower (if any) reputational damage to the affected advisor and a smaller deterrence effect on other employees in case only junior staff is held responsible for M&A-related insider trading. Overall, the difference-in-differences approach supports the results from our advisor fixed effects tests as it further suggests that financial advisors matter for short-term runups in the stock prices of takeover targets.

To provide a better understanding of the association between the identity of the financial advisor and M&A information leakage, we investigate how advisor characteristics relate to target stock price runups. We find only a marginally significant positive relation between target stock price runups and whether a financial advisor is an investment bank (as opposed to a boutique or universal bank), or whether an advisor is publicly traded. However, we find that those advisors headquartered in New York City, the main U.S. financial center, and those with a higher number of past advised deals are associated with significantly higher target runups. The former finding indicates that inside information may be more likely to leak and spread if financial advisors are located in a large financial center where investment bankers and investors are well-connected and where bankers can more easily establish and maintain social ties (e.g., via club memberships or living next door to other bankers or investors). The latter result suggests that better connected and successful advisors, who rank among the top of the league tables, are more likely to leak inside information, indicating that reputational concerns may not significantly incentivize financial advisors to protect their clients' inside information, i.e., the gains from exploiting some clients may outweigh the potential costs.

There are two potential alternative interpretations of our results that warrant discussion. Betton et al. (2014) argue that target stock price runups prior to takeover announcements reflect rational deal expectations caused by publicly available signals, such as rumors that indicate potential takeover synergies. One interpretation of our findings is that financial advisors constitute a source of takeover signals as they, either unwittingly or deliberately, leak deal information. A less favorable interpretation is that our measure of target runups captures rational deal anticipation unrelated to leakage and that advisor fixed effects reflect varying abilities or preferences of advisors to advise high-synergy deals that can be anticipated. Supporting the first interpretation, we find a negative relation between target stock price runups and the SEC enforcement budget as a measure of regulatory enforcement intensity and insider trading deterrence, consistent with Del Guercio et al. (2017). Furthermore, we find that measures of synergy gains have no explanatory power for our measures of information leakage, which indicates that rational deal anticipation in terms of expected synergies does not explain our findings. Another potential issue is that significant advisor fixed effects for target stock price runups might reflect that some advisors systematically acquire equity stakes in target firms prior to takeover announcements, which may move targets' stock prices. Bodnaruk et al. (2009) find that in about a quarter of the M&A transactions in their sample bidders' financial advisors acquire a median (mean) stake, which corresponds to 0.16% (0.63%) of the target's market capitalization. If advisors acquire equity stakes shortly prior to takeover announcements, this pattern might provide an explanation for our results. We examine advisors' equity stakes in takeover targets (and acquirers) and find that the presence of such stakes does not explain target stock price runups prior to takeover announcements.

This study contributes to two strands of the literature. First, suggesting a different methodological approach than extant studies, we provide evidence that financial advisors are significantly associated with information leakage prior to takeover announcements. Thereby, we advance the inconclusive literature concerned with the question of whether banks exploit or rather protect their inside information obtained through their M&A advisory service. In contrast to extant work, which relies on traceable measures of information leakage, particularly disclosed trading activities (e.g., Bodnaruk et al., 2009; Griffin et al., 2012; Lowry et al., 2019), our approach allows to consider leakage in both traceable and non-traceable manners. Our findings indicate that banks may exploit their clients' inside information or, at least, that they do not sufficiently protect it. We also provide novel evidence regarding which banks are associated with more or less leakage. More generally, our study contributes to the literature on conflicts of interest in financial institutions (see Mehran and Stulz, 2007, for a review).

Second, we also contribute to the literature explaining the phenomenon of target stock price runups shortly prior to takeover announcements. While this phenomenon has been prevalent for decades, our understanding of what may cause it is still limited. Our study documents a significant and economically meaningful association between target stock price runups and financial advisors while accounting for the so far identified determinants of these runups. Jarrell and Poulsen (1989) find that pre-bid target runups are positively related to takeover rumors and negatively related to large acquiror toeholds, while Acharya and Johnson (2010) document a positive effect of the number of involved insiders. Further, Schwert (1996) documents that there is little substitution between pre-bid runups and announcement returns, suggesting that runups present an added cost to the bidder. In this regard, we provide evidence for persistent advisor fixed effects, which suggests that clients and regulators may use the average stock price runups of banks' previously advised M&A transactions as a proxy for expected information leakage when they select financial advisors or try to detect violations of insider trading laws.

The remainder of this paper is organized as follows. Section 2 presents the sample and variables. Section 3 provides the results of our fixed effects tests and related robustness tests. Section 4 presents a difference-in-differences analysis around SEC enforcement actions, while Section 5 studies potential heterogeneity across financial advisors. Section 6 provides a discussion concerning potential threats to the interpretation of our study, while Section 7 concludes.

2 Data and descriptive statistics

2.1 Takeover sample

Our initial sample consists of all takeovers announced between January 1, 1990 and December 31, 2014 available in the SDC Thomson Reuters database. Since we are primarily interested in the trading behavior around the announcement, we include both completed and failed transactions, as the future outcome of the proposed transactions is not known with certainty at the date of the announcement. We apply the following filter to the initial sample: we only consider transactions with a deal value of at least USD 1 million and where the target firm is based in the U.S. Further, we require that the acquiring firm seeks a majority stake (i.e., at least 50%) and owns less than 10% at the takeover announcement. We exclude transactions for which the target has no available stock returns in the Center for Research in Security Prices (CRSP) database, or for which there is no target accounting data available in Compustat. Moreover, we restrict the sample to observations with a positive offer premium. The rationale behind this choice is the following: a negative premium is unlikely to have large positive value implications for the target stock and individuals with access to private information about the announcement will not have a large opportunity to trade on the foreknowledge of an event with little expected value implications. We retrieve deal characteristics from SDC Thomson Reuters, except for rumor information, which is obtained from Capital IQ. Accounting data is from Compustat. Our final sample consists of 5,316 transactions.

SDC provides detailed information about financial advisors of the bidding firm. As advisor names are labeled inconsistently, we manually check all advisor entries in SDC and define dummy variables taking subsidiaries of each advisor and M&As in the financial industry into account. For each takeover in our sample, the financial advisor mentioned first is treated as the lead advisor. In our main analyses, we focus on the lead advisor of the acquiring firm. In total, there are 179 distinct lead financial advisors in our sample. Figure 1 depicts the distribution of deals across advisors sorted by decreasing number of transactions in our final sample on the left axis. On the right axis, the graph shows the cumulative percentage of transactions. Deals are concentrated with just few advisors: the top 25 advisors in terms of number of transactions alone account for approximately 75% of deals in our sample.

Table 1 presents summary statistics for the deal characteristics and the characteristics of target and acquiring firms. The median transaction value is USD 246 million, while the mean value is much larger at USD 1,258 million, which suggests that transaction values are skewed to the right. We note that in 34% of transactions, no lead advisor was indicated by SDC. We refer to these transactions as inhouse-advised deals. The median transaction with external advisors involves one financial advisor. The dummy variable *Rumor* is set to 1 if the target firm was mentioned in the Capital IQ M&A rumor database in the 60 calendar days prior to the first official takeover announcement, and to 0 otherwise.[5] According to this definition, only 3.5% of transactions were preceded by rumors. The dummy variable *Toehold* is set to 1 if the acquiring firm holds an equity stake in the target firm before the acquisition announcement, which is the case in 3.3% of cases, and to 0 otherwise. 14.1% of transactions involve a financial sponsor. Further, we construct the dummy variable *Public acquiror*, which is set to 1 if the acquiror is a publicly listed firm as indicated by SDC, and to 0 otherwise. In 72.2% of transactions, the acquiring firm is public. In 14.6% of cases, the acquiring firm is based outside of the U.S., as indicated by the dummy variable *Cross-border*. 2.6% of transactions are indicated as hostile. The average offer premium is 47.8% relative to

[5] The separation between private information and rumors is not distinct. We argue that if private information is disseminated over certain communication channels which is when databases such as Capital IQ capture rumors, the private information on the impending transactions has been partially turned into publicly available information since it is available to a broader community.

the stock price four weeks before deal announcement.

The average target size as indicated by the book value of total assets is USD 1,551 million, while the average size of the acquiror is much larger with a mean value of total assets of USD 18,677 million. Both acquirors and targets have a mean leverage of about 20%. While acquirors are more profitable, in terms of return on assets, and valued higher by the stock market than target firms, the latter have higher research and development expenditures and hold more cash.

2.2 Measures of abnormal stock behavior

To measure abnormal stock behavior, we rely on measures of abnormal stock returns and abnormal stock trading activity. These measures are known to be monotonically related to insider trading, which cannot be observed directly, as this would require knowledge of the entire information set and transactions of all traders in a specific stock as pointed out by Acharya and Johnson (2010). In this regard, Meulbroek (1992) investigates how actual illegal insider trades affect stock behavior based on known illegal insider trades that were subject to SEC enforcement actions. She finds that insider trades move prices substantially. Recently, Heitzman and Klasa (2021) document significant abnormal returns during material nonpublic merger negotiations, which take place before the official public announcement. These studies support the use of abnormal stock returns as a measure of suspicious trading activities.

To estimate abnormal stock returns, we use the Fama-French three-factor model, using data from Kenneth French's website, with the CRSP value-weighted index as the proxy for the market portfolio. The estimation window comprises 180 trading days ending 40 trading days prior to the announcement date of the M&A transaction. If the takeover is announced on a non-trading day, the event date is defined as the next trading day. We aggregate daily abnormal returns over a period of 10 days leading to the public announcement, i.e., *CAR(-10,-1)*. Cumulative abnormal returns are scaled by the standard deviation of residuals in the estimation window, denoted *SCAR(-10,-1)*, to account for cross-sectional differences in stock return volatility across target firms. We use *SCAR(-10,-1)* as our primary measure of target firms' abnormal stock returns prior to takeover announcements, referred to as target runups.

Figure 2 depicts the targets' average abnormal stock returns in the 30 days prior to the takeover announcement. Abnormal returns fluctuate around zero and start increasing approximately 15 days prior to the takeover announcement. Abnormal returns increase until the day of the announcement, with considerably strong increases in the 5 to 10 days before information about the takeover becomes public. The stock return behavior is identical to that illustrated in Keown and Pinkerton (1981).

To investigate the robustness of our results, we use several alternative measures of target stock price runups. First, we use the basic cumulative abnormal return over ten trading days prior to the M&A announcement date, denoted *CAR(-10,-1).* Second, we use the two variables proposed by Acharya and Johnson (2010): the maximum of the standardized abnormal returns (*MAX(-10,-1)*) and the sum of the positive standardized abnormal returns (*SUM(-10,-1)*) in the ten-trading-day period prior to the M&A announcement date. Third, in line with Jarrell and Poulsen (1989); Ackerman et al. (2008) we use the variable *FRACTION(-10,-1)* defined as *CAR(-10,-1)* divided by *CAR(-10,1)*, a measure that includes the market response to the announcement and thereby scales the runup with the entire information content of the announcement. The variable is truncated to take on values between 0 and 1. Finally, we use variations of the aforementioned variables based on different event windows, more specifically, the five-day period and the nine-day period ending two trading days prior to the deal announcement (e.g., *SCAR(-5,-1)* and *SCAR(-10,-2)*).

To provide additional evidence consistent with abnormal stock returns, we also use measures of abnormal stock trading volume. We estimate the expected trading volume based on a regression of the natural logarithm of daily volume on the natural logarithm of market volume and day of the week dummies. Analogous to the abnormal return measures, we use the cumulative abnormal volume (CAV) and the standardized version (SCAV), which scales the abnormal trading volume by the standard deviation of the residual volume in the estimation window.

We acknowledge the concern that strategic liquidity-based selection may confound measures of leakage as pointed out by Akey et al. (2022): Investors in possession of private information may time their transactions during periods of high liquidity to reduce the price impact of their trades (e.g., Glosten and Milgrom, 1985; Kyle, 1986). Ahern (2020) argues that the insiders' ability to time depends on the short-datedness of the private information. Given the relative short-lived information about impeding takeover announcements and the specificity of the information to a particular stock, we conclude that the possibility for selection is limited. Kacperczyk and Pagnotta (2019) find empirical support for the notion of strategic timing of transactions on days with high uninformed volume. However, they also document that informed trading gives rise to economically meaningful abnormal returns, supporting the use of return- and volume-based measures.

Table 1 provides summary statistics for our measures of abnormal stock behavior. The average cumulative abnormal return in the ten days leading up to the takeover announcement is 4.8%. The average value is much larger than the median value of 1.9%, indicating that the distribution of target runups is skewed to the right. The measure *FRACTION(-10,-1)* indicates that on average 30% of the total price impact of a takeover announcement is already incorporated into target stock prices in the ten days before the first public announcement. Scaling the cumulative abnormal value with the standard variation of residuals in the estimation period yields an average *SCAR(-10,-1)* of 0.35. The mean maximum standardized abnormal return in the 10 days leading up to the announcement is 2.2%, while the mean sum of positive standardized returns in this event period is 4.8%. Overall, these numbers suggest that pre-bid target runups are economically meaningful.

2.3 Baseline regression

We regress our aforementioned measures of abnormal stock behavior on the set of control variables described in Section 2.1. We control for variables frequently used in the M&A literature (see, e.g., Golubov et al., 2012; Moeller et al., 2004, 2007). More specifically, we estimate the following regression:

$$Runup_{i,t,j} = \beta_0 + \mu_t + targetindustry_i + \beta_1 \cdot dealchars_{i,t,j} + \beta_2 \cdot targetchars_{i,t} + acquiror_j + \varepsilon_{i,j,t} \quad (1)$$

, where i denotes the target dimension, t the time dimension, and j the acquiror dimension, *Runup* denotes a measure of abnormal stock behavior, μ_t are year fixed effects, *dealchars* denotes a vector of deal characteristics, *targetchars* denotes a vector of target characteristics.

Table 2 shows results of the above regression, which uses *SCAR(-10,-1)* as the dependent variable. In column 1, *SCAR(-10,-1)* is regressed on deal characteristics and year and industry fixed effects, while we add target characteristics in column 2 and acquiror fixed effects in column 3. The negative coefficients on the dummy variable *Hostile* suggests that the runup is smaller for hostile versus friendly transactions. However, this effect is statistically significant at only the 10% level. The runup is positively related to the size of the transaction, as indicated by the coefficient on *Log transaction volume*. Further, consistent with Guo and Zhang (2021), we find that cross-border transactions are associated with significantly larger runups.

In line with Jarrell and Poulsen (1989), we also find that runups are significantly larger if transactions are preceded by takeover rumors. Controlling for rumors is critical in our exercise, since we are interested in the variation of nonpublic takeover-specific information. Finally, we observe that the offer premium is positively associated with the runup. These significant relations are robust to the inclusion of target characteristics and acquiror fixed effects as additional control variables in columns 2 and 3, respectively. We note that the coefficient estimates across the three models shown in columns 1 to 3 do not change much, indicating that runups do not seem to be largely driven by unobserved heterogeneity at the acquiror level.

3 Financial advisor fixed effects

3.1 Test of joint significance

We estimate the regression from Equation 1 above and add a dummy variable for each financial advisor. We conduct an F-test for the joint significance of financial advisor dummies, which tests the null hypothesis that all financial advisor dummies are jointly equal to zero with the following test statistic:

$$F_0 = \frac{(SSR_r - SSUR_{ur})/q}{SSR_{ur}/(n-(k+1))} \tag{2}$$

, where $SSR_{(u)r}$ is the sum of squared residuals of the (un)restricted model, n is the number of observations, k is number of variables in the unrestricted model, q is number of restrictions (i.e., number of coefficients being jointly tested). If the null hypothesis cannot be rejected, including financial advisor dummies does not help explain variation in target stock price runups.[6]

Panel A of Table 3 presents the F-test statistic and the corresponding p-value in parentheses for various sets of control variables. We test for the joint significance of financial advisor fixed effects for advisors that are lead advisors for at least five transactions in our sample.

Row 1 shows the results from a regression of target runups on financial advisor dummies. The null hypothesis of all financial advisor dummies jointly being equal to zero is rejected. The null hypothesis continues to be rejected when we subsequently add industry and time fixed effects (row 2), deal characteristics (row 3) or target characteristics (row 4) to the regression model. Figure 3 shows the estimated individual advisor coefficients based on *CAR(-10,-1)* as an outcome variable, sorted by the number of deals advised in the sample in decreasing order. This figure indicates considerable variation in runups across advisors, a necessity for the F-test to yield significant results.

[6] We note that failing to reject the null hypothesis does not have a unique explanation: either advisors do not matter for leakage, or they causally matter for leakage, but do so to the same extent such that they are perfect substitutes in terms of their incremental contribution to leakage.

A concern with the above approach is that the matching between bidders and their financial advisors is likely to be non-random. In particular, advisor choice might be driven by unobservable factors at the acquiror level, which influence both the choice of the financial advisor and the level of target runups. To address this concern, we add acquiror fixed effects to control for unobservable time-invariant heterogeneity at the acquiror level. By controlling for acquiror fixed effects, i.e., keeping the acquiring firm constant, we test whether there is an incremental effect stemming from acquirors switching their financial advisors. The test statistic still rejects the null hypothesis if we control for acquiror fixed effects, as shown in row 5 of Panel A. The results are consistent with the notion that the identity of the financial advisor seems to matter for target runups. However, this interpretation hinges upon the assumption that there is no unobservable, time-*varying* factor, which affects both advisor choice and target runups - an assumption that we investigate below.

In row 6, we show results where we additionally control for time-varying acquiror characteristics. We include the natural logarithm of the book value of total assets, leverage, research and development expenditures, Tobin's Q, return on assets and cash holdings as additional control variables. Adding these controls reduces the number of observations to 2,196, which equals the number of takeovers involving an acquiror that is stock-listed and has available accounting data in Compustat.

Next, we explore the robustness of these results with respect to alternative measures of abnormal stock behavior (described in Section 2.2). We repeat the regression specification used in row 5 of Panel A using other measures of abnormal stock returns and abnormal trading volume. Panel B of Table 3 shows the results of this robustness test. We confirm that the F-test rejects the null hypothesis of the advisor dummy coefficients being jointly equal to zero in all cases, except for the measures *SUM(-5,-1)* and *FRACTION(-5,-1)*. Put differently, 13 out of 15 regressions with alternative measures support our finding that financial advisors matter for the abnormal stock behavior of target firms prior to takeover announcements. Among these measures are *SCAV(-10,-1)* and *SCAV(-5,-1)*, which capture abnormal trading volume of target firms' stock instead of abnormal returns.

As mentioned above, our fixed effects analysis implicitly assumes the absence of any unobservable time-varying confounders that directly affect target runups and advisor choice. To further address this issue beyond controlling for observable time-varying acquiror controls, we repeat the analysis presented in Panel A of Table 3 and additionally control for a time-varying measure of target runups in acquirors' past takeovers. Specifically, we construct a measure of past target runups based on an acquiror's last four transactions. This approach reduces our sample size to 2,913 observations as we focus on acquirors with several past transactions. Controlling for the average target runup over an acquiror's last deals takes into account that the extent to which acquirors are aware of and responsible for leakage of inside information and runups changes over time. The results of this robustness test are shown in Panel C of Table 3. They suggest that financial advisor fixed effects still matter for pre-bid target runups even after controlling for past runups at the acquiror level.

3.2 Placebo test

A statistical concern with the fixed effects approach is the reliability of the F-test. One may be concerned that we are more likely to reject the null hypothesis, and falsely conclude that financial advisors matter for target pre-bid stock runups, if we test many individual effects at the same time. To address this concern we follow Golubov et al. (2015) and Fee et al. (2013) and conduct a placebo test to learn about the distribution of the F-value in a situation where the null hypothesis is true by design. To this end, we randomly allocate financial advisors to takeovers, while mimicking the actual distribution of the number of transactions advised by each financial advisor. We repeat this procedure 10,000 times for regression models with different sets of control variables as shown in rows 1 to 5 of Table 4. For the different regression models, the table presents the distribution of the F-value for the placebo data generated from the 10,000 random draws. The mean F-value is close to 1 throughout the regression models. In all cases, the actual F-values are more than 2 standard deviations away from the mean F-values resulting from the use of placebo data. This test supports the use of the F-test in the context of our study, since the F-value performs well in not rejecting the null hypothesis when we do not expect any impact by construction.

3.3 Economic significance

Panels A and B of Table 5 show the distribution of the size of the coefficient estimates on financial advisor fixed effects for different sets of control variables. Depending on the set of controls, the interquartile range varies between 0.30% and 0.37% for *SCAR(-10,-1)*, which has a full sample mean of 0.5%. Using *CAR(-10,-1)* as an outcome variable, the interquartile range varies between 3.3% and 3.9%, compared with a full-sample mean of 4.8%. These numbers suggest that our results are economically meaningful.

Given that the differences in the pre-bid target runups are considerable, we further test whether these differences are predictable from past runups. If so, firms in the market for corporate control as well as regulators can use historical data to learn about expected target runups, which constitutes another aspect of economic significance given the practical implications of this strategy. To analyze the persistence of financial advisor fixed effects effects, we follow Edmans and Bao (2011) and sort financial advisors into quintiles based on *SCAR(-10,-1)* over the last n years where $n \in (1, 2, 3)$. We then calculate the average value of *SCAR(-10,-1)* over the future k years with $k \in (1, 2, 3)$ for each quintile. If financial advisor specific effects are persistent over time, we expect that the difference between the top quintile and the bottom quintile is positive, i.e., that financial advisors with low (high) runups in the past also exhibit low (high) runups in the future.

The results of this test are shown in Panel C of Table 5. All differences between the top and bottom quintiles, except for one difference when quintiles are measured over one year and future runups over three years, are positive. When quintiles are calculated over one year only, the differences fail to be statistically significantly different from zero. However, for larger time periods over which past average target runups are calculated, we observe that the differences in future target runups are statistically significant at least at the 10% level. Hence, differences across financial advisors persistent over time and are predictable based on past target runups. This finding has important implications as it suggests that our results are actionable by firms in their selection of future financial advisors and by regulators that attempt to detect potential insider trading in the market for corporate control.

4 Difference-in-differences analysis around SEC insider trading enforcement action

The empirical challenge to investigate whether financial advisors matter for pre-bid target stock price runups stems from the potential concern that advisor fixed effects might be driven by an unobserved factor, which is correlated with both the advisor choice and target runups. To strengthen the link towards a more causal interpretation, we use a difference-in-differences approach to study the implications of a shock to advisor-specific propensities to leak or exploit nonpublic information obtained through advising on takeovers. In particular, we use takeover-related insider trading enforcement actions by the SEC against employees of financial advisors as shocks, which reduce the likelihood that affected advisors will misuse inside information. The identifying assumption of this approach is that the shock is exogenous to unobservable characteristics that might affect both advisor choice and target runups (or leakage) directly.

SEC enforcement actions against insider trading can be assumed to constitute shocks to financial advisors because of their low probability of occurence and the high opacity in terms of SEC enforcement preferences. Enforcement is rare and difficult to predict as insider trading is difficult to detect and to prove in court. Overall, only approximately 50 cases of insider trading episodes are enforced per annum, while one suspects the true number of insider trading cases to be much higher (see, e.g., Augustin et al., 2019). Furthermore, there is substantial variation in terms of the timing of the enforcement, since there might be a considerable delay between the actual violation of insider trading laws and enforcement actions.

The evidence in Meulbroek (1992) suggests that investigations are typically triggered by reasons unrelated to screening by regulators, i.e., external tips or whistleblowing are the most frequent source of investigations. Similarly, Kacperczyk and Pagnotta (2019) argue that the decision to litigate depends on the availability of external evidence. They show that the SEC does not appear to choose cases based on profitability and that the pricing and volume patterns around informed trading that originate from the whistleblower reward program and trade monitoring are comparable to those brought forward by alternative sources, which reject the notion of systematic selection in the SEC enforcement cases. This evidence supports an arguably quasi-exogenous interpretation of the enforcement events.

We hand-collect SEC enforcement cases on insider trading that involve employees of financial advisors who exploit private information on pending takeovers.[7] Table A5 in the Appendix provides an overview of the identified SEC enforcement cases. The table lists the date of the release of the enforcement event, the date of violation[8], the sum of insider trading profits realized, the number of defendants, and the number of stocks on which the defendants traded.

We distinguish between enforcement cases that involve senior employees and those that involve junior employees. The former include managing directors, executive directors, or chairmen and CEOs of the financial advisors. The latter include analysts and interns, as well as support staff, such as administrative assistants, word processors, or employees of the compliance department.[9] The reason for this distinction is that we expect considerable differences with respect to visibility and attention, reputational damage, and the effect of deterring other employees depending on whether an advisor's senior or junior employee is involved in an insider trading case. We thus attempt to exploit variation in treatment intensity for cleaner identification.

We merge the enforcement events with our main sample. In total, there are 10 enforcement

[7]The information is collected from the litigation releases which are available from the enforcement section of the Securities and Exchange Comission website.

[8]If the insider trading case involved several insider trading violations, this date refers to the first transaction of the episode.

[9]Examples of these insider trading episodes include a summer intern of the M&A advisor department of JPMorgan who made approximately USD 360,000 in insider trading profits, or two desktop publishers working for an investment bank who exploited private information of approximately 23 transactions and made profits amounting to approximately USD 8 millions.

cases involving senior employees and 30 enforcement cases involving junior employees of financial advisors that advise on at least five takeovers in our sample. Advisors that have been the target of an SEC enforcement action advise on 2,062 takeovers in our sample. Advisors targeted by enforcement actions that involve senior employees advise on 873 takeovers in our sample, of which 538 occur before the enforcement episodes were disclosed and 335 afterwards. The enforcement events are staggered over time. The first event in our sample is from September 1997, while the most recent event is from December 2012.

We estimate the following difference-in-difference regression with multiple event dates:

$$\begin{aligned} Runup_{i,t,j,k} = \beta_0 + \mu_t + targetindustry_i + \beta_1 post_{t,k} + \beta_2 dealchars_{i,t,j,k} \\ + \beta_3 targetchars_{i,t} + acquiror_j + advisor_k + \varepsilon_{i,j,t,k} \end{aligned} \tag{3}$$

where *post* is set to 1 for transactions in the year the enforcement action for the affected financial advisor, and to 0 otherwise. X is a vector of transaction-level controls: deal characteristics, target characteristics, acquiror characteristics. t denotes the time dimension, k denotes the advisor dimension, i denotes the deal and target dimension and j that of the acquiror. μ_t denotes year fixed effects. The main coefficient of interest is β_1, which captures the difference in target stock price runups between affected financial advisors in the year after the enforcement event and during other times, taking potential differences between affected and non-affected advisors and common time trends into account.

Panel A of Table 6 reports the results from this regression. The regression coefficient on the variable *Post* in column 1 is negative but statistically significant only at the 10% level. There seems to be a slight decrease in runups after an enforcement event. However, in column 2 we distinguish between enforcement events targeting senior and junior employees. The coefficient on *Senior post*, i.e., enforcement actions involving advisors' senior staff, is -0.365 and statistically significant at the 1% level. In terms of economic significance, this decrease in target runups for affected financial advisors is substantial, as it accounts for approximately one third of one standard deviation of *SCAR(-10,-1)*. Regarding cases involving junior staff, the corresponding coefficient on *Junior post* is negative but small (-0.064) and statistically insignificant. The above results remain qualitatively unchanged when we drop acquiror fixed effects and deal controls in columns 3 and 4.

In column 5 we control for an indicator variable *Violation period*, which is set to 1 for the time between the start of the violation of insider trading laws and the enforcement date of the respective episode, and to 0 otherwise. As shown in Table A5 in the Appendix, the time between violation and enforcement can vary substantially, between four months and more than three years. We control for the time between the start of the insider trading violation and the enforcement date to test whether the reduction in target runups in the post-enforcement period can be attributed solely to the decrease relative to this violation period. The coefficient on *Senior post* amounts to -0.313, which is only slightly smaller than the estimate in column 2. The coefficient on *Violation period* is positive and statistically significant. This finding suggests that runups are larger in the violation periods, of which a certain part might in fact stem from the insider trading and tipping of the defendant. However, the fact that the coefficient on *Post senior* is still negative and both economically and statistically significant suggests that the drop in runups cannot be mechanically attributed to the defendant. Runups decrease also relative to the prior level, not just relative to the level within the violation period.

Figure 4 illustrates our results. It plots the coefficient on the interaction term between *Senior post* and an event time dummy variable indicating the timing around the enforcement event at t_0. The figure also shows the 95% confidence interval of these coefficients. Before t_0, the coefficients are close to zero and become negative after the enforcement action.

Panel B of Table 6 reports results from tests of the underlying parallel trends assumption. In these tests, we use leads of the *Senior post* dummy variable. In column 1 of Panel B, we assume that the enforcement event has happened three years earlier, i.e., we create a placebo *Senior post* dummy, denoted *Placebo post (minus 3 yrs)*, which equals 1 although actually no enforcement event has taken place yet. In column 2 and 3, we use placebo dummies, which assume that the enforcement took place four and five years earlier, respectively. In all three columns, the coefficients on the placebo dummies are statistically insignificant. In columns 4 to 6, we exclude all observations in the post-enforcement period to test directly whether pre-bid target runups in takeovers advised by financial advisors with an enforcement event and those without any enforcement event involving senior staff have been evolving on parallel paths before the enforcement. Again, the regression coefficients fail to be statistically significant. We conclude that the wedge in target runups between financial advisors with and without enforcement events coincides with the timing of the enforcement event.

Next, Panel C of Table 6 shows results of regressions where we estimate the same regression model as in column 2 of Panel A using alternative measures of target runups. For 7 out of 8 measures of runups, the coefficient on the dummy *Senior post* is negative and statistically significant. This test shows that the results are not very sensitive to our choice of the runup measure.

In a further robustness test, presented in Panel D of Table 6, we change the counterfactual underlying our difference-in-differences analysis. While advisors without any enforcement action over the sample period served as counterfactual outcomes in the previous analyses, we now select matched advisors as counterfactuals. This approach addresses the concern that the pre-treatment characteristics between treatment and control groups could be unbalanced. This condition might pose a potential threat, if the characteristics are associated with the dynamics of the outcome variable *SCAR(-10,-1)*. Hence, we match treatment subjects and control subjects based on pre-treatment characteristics to make these two groups more balanced. Matching of treatment and control advisors is based on two sets of pre-treatment characteristics: Either, the matching is based on *SCAR(-10,-1)* and all target and deal characteristics in our baseline regression shown in Table 2, or it is based on *SCAR(-10,-1)* only. We estimate a logistic regression where the dependent variable is a treatment indicator and the independent variables are the set of characteristics on which we match. Based on these outcomes, we predict the propensity scores. We construct the control groups by nearest neighbor matching, using the three nearest neighbors in our baseline test. For robustness, we also use nearest one and five neighbors. The coefficient on the post enforcement dummy is negative and statistically significant throughout all regressions shown in columns 1 to 5.

Depending on the set of matching variables and the number of matched advisors, the coefficient on the post enforcement dummy ranges between -0.326 and -0.524, conforming our earlier results.

In sum, runups decrease significantly, both in statistical and economic terms, after takeover-related insider trading enforcement actions by the SEC used as shocks to advisor-specific propensities to exploit or leak nonpublic information about pending takeovers. This evidence supports the idea that financial advisors matter for pre-bid target stock price runups. It also suggests that at least part of these runups may be attributed to financial advisors exploiting or leaking inside information about takeovers.

5 Observable advisor characteristics

The findings from Section 4 indicate that the identities of financial advisors matter for target stock price runups prior to takeover announcements. To complement our findings, this section investigates whether target runups are related to observable advisor characteristics, which are likely to explain differences in advisors' incentives or abilities to either limit or permit and promote leakage (or exploitation) of nonpublic information obtained from M&A advisory service. The results are shown in Table 7.

First, we look at the relation between advisor type and runups. We differentiate between universal banks, which provide both wholesale and retail banking services, boutiques, which primarily provide M&A advisory services, and investment banks, which offer a broad variety of wholesale services in addition to M&A advisory (e.g., underwriting, lending or brokerage services). One might expect differences in the extent to which these distinct types are exposed to conflicts of interests between the M&A advisory business and other services offered by the same institution. Boutiques are highly focused on the M&A advisory business, which is why we would expect them to have relatively little incentives (and opportunities) to leak or exploit nonpublic information. In contrast, investment banks might have relatively strong incentives and many opportunities to use this information. The results in column 1 suggest that runups are virtually unrelated to advisor type. The regression coefficients on the dummies investment bank, boutique and universal bank are all positive, only the coefficient on investment bank is marginally significant (relative to the reference group, comprising 'other' M&A advisors, such as accounting or management consulting firms). The differences between investment banks, boutiques and universal banks, respectively, also fail to be statistically significant.

Second, we test whether runups are related to advisor reputation. A priori, reputation might imply ambiguous empirical predictions. On the one hand, reputable advisors might put more effort into avoiding the misuse and leakage of nonpublic information, in order to protect their valuable reputation. This notion assumes that leakage is a negative component of deal quality and that deal quality affects reputation. On the other hand, reputable advisors, which typically have a large network of investors, can have incentives to milk their M&A-specific reputation and use information to their own or other clients' benefits. This notion is more likely in a situation where reputation is not strongly associated with deal quality, or where takeover-related information is leaked in the client's, i.e., acquiror's interest. The empirical literature on the relation between deal quality and advisor reputation is mixed: For example, Edmans and Bao (2011) show that acquiror returns are not strongly related to advisor reputation, while Golubov et al. (2012) provide evidence that deal quality is greater for top-tier advisors advising public transactions. We use two measures of advisor reputation: the number of past advised deals and an indicator variable that is set to 1 if an advisor is among the top 25 financial advisors in terms of the number of advised deals over the sample period.[10]

In columns 2 and 3, we regress target stock price runups on the two aforementioned variables. The regression coefficients on both variables are positive and statistically significant at least at the 5% level, indicating that more reputable advisors are associated with significantly higher pre-bid target runups. Specifically, the results in column 3 suggest that target runups tend to be greater by approximately 10% of one standard deviation for deals advised by the top 25 advisors as compared to the reference group of deals advised by lower-rank

[10]The results are quantitatively similar if we rank advisors by deal volume instead of deal count.

advisors. The result in column 4 suggests that potentially higher level of leakage associated with reputable advisors does not reflect that these advisors tend to be listed on a stock exchange and, thus, exhibit more pressure to maximize short-term profits. The regression coefficient on the indicator variable *Listed advisor* is positive and marginally significant at the 10% level. Further, in unreported regressions, we find that the positive relation between advisor reputation and target runups is only found for equity-financed deals. This result is consistent with financial advisors acting on behalf of their M&A clients, which might benefit from short-term runups once the stock exchange ratio has been fixed.

Lastly, we test whether the location of financial advisors' headquarters is related to runups. The result in column 5 suggests that runups are more than 10% of one standard deviation higher when advisors are headquartered in New York City, the financial capital of the U.S. This finding is consistent with the notion that the high concentration of investors and other financial professionals in New York City is conducive to the leakage of nonpublic takeover-specific information.

In sum, while the evidence we present suggests that runups differ across observable advisor characteristics, our findings do not rule out that idiosyncratic characteristics also shape advisor-specific target runup levels.[11]

6 Discussion

In the following, we discuss potential issues with respect to the results of our study and their interpretation. One potential caveat is the presence of rational deal expectation as an alternative interpretation for our findings. Among others, Betton et al. (2014) argue that stock price runups of target firms prior to takeover announcements are the outcome of rational deal anticipation, which is caused by signals such as rumors. These signals inform investors about potential takeover synergies resulting in stock price runups, which reflect the

[11] The paper by Golubov et al. (2015) on acquiror firm heterogeneity in deal quality arrives at similar conclusions. The authors do not find that deal quality varies systematically with certain acquiror characteristics and conclude that idiosyncratic differences seem to be the driver.

discounted value of the expected synergies. Against this background, one interpretation of our results is that financial advisors constitute a source of takeover signals because they leak deal information, either unwittingly or deliberately. An alternative interpretation, however, is that our measure of target runups captures rational deal anticipation unrelated to leakage and that the significant advisor fixed effects we document reflect advisors' varying abilities or preferences to advise deals with high potential synergies that can be anticipated.

We apply a simple test to address the concern that our primary measure of target runups, SCAR(-10,-1), captures rational deal expectation rather than leakage of inside information by financial advisors. Specifically, we examine the relation between SCAR(-10,-1) and the SEC enforcement budget. Del Guercio et al. (2017) use the enforcement budget as a measure of regulatory enforcement intensity and insider trading deterrence. While we expect a negative relation between SCAR(-10,-1) and the SEC enforcement budget if the former measures leakage, we do not expect to find any such relation if SCAR(-10,-1) instead measures rational deal anticipation. As can be seen from Figure 5, we find a negative relation between our measure of target stock price runups and the enforcement budget, consistent with Del Guercio et al. (2017).[12] We thus conclude that SCAR(-10,-1) is likely to measure leakage and unlikely to measure rational deal anticipation.

To further explore this alternative interpretation, we investigate how well takeover targets can be predicted based on observable characteristics. To this end, we run a logit regression analysis using variables, which have been shown to affect firms' takeover likelihood. The sample consists of all Compustat firms in the period from 1990 to 2014. We regress the indicator variable *takeover*, which equals 1 if a firm is the target of a takeover in a given year (0 otherwise), on a set of firm characteristics and additionally include controls for takeover rumors, the number of deals in a firm's industry, and year fixed effects. The regression results are shown in Table A2 in the Appendix. Based on the regression results, we estimate the likelihood that a firm receives a takeover offer. The mean (median) value of the takeover probability for actual targets within a year is 5.0% (3.6%). This implies a probability of

[12]Using international data, Ackerman et al. (2008) document an inverse relationship between the introduction and enforcement of insider trading laws and pre-bid stock price runups in corporate takeovers.

approximately 20 basis points to predict a takeover in a 10 day interval within a given year. This back of the envelope calculation strongly suggests that predicting takeovers based on observable firm characteristics is difficult. Given the large stock market reaction of target firms to takeover offers, the result also points out how valuable foreknowledge of a pending takeover can be to investors and, thus, how tempting insider trading can be.

As argued by Betton et al. (2014), runups can reflect expected synergies rather than information leakage. If that is the case, the results should be interpreted in terms of systematic differences in synergies rather than differences in leakage.[13] We directly test this alternative interpretation by calculating synergy gains as the sum of the target and acquiror CAR, and the value-weighted sum of these CARs respectively, and regressing our runup measure on these synergy measures in Table A3 in the Appendix. The coefficient estimates are indistinguishable from zero. These findings suggest that the differences in runups between financial advisors cannot be explained by differences in expected synergy gains.

Another issue is the acquisition of equity stakes in target firms by financial advisors. Significant advisor fixed effects for target stock price runups might reflect that some advisors systematically acquire equity stakes in targets prior to takeover bids, which might move targets' stock prices. In this regard, Bodnaruk et al. (2009) document that in about a quarter of the transactions in their sample of M&A transactions between 1984 and 2003 bidders' financial advisors acquire a median (mean) stake corresponding to 0.16% (0.63%) of the target's market capitalization. The authors find that this investment strategy is highly profitable. If some advisors frequently acquire equity stakes shortly prior to takeover announcements, this policy might provide an explanation for our results. Table A4 in the Appendix address this alternative explanation by regressing runups on dummy variables that are set to 1 if the acquiror advisor (Panel A) or the target advisor (Panel B) holds a stake in the target firm in the nth month prior to the takeover announcement, where $n \in \{1, ..., 7\}$. We do not find any significant relation between these dummy variables and our runup measure,

[13] Betton et al. (2014) consider longer-term runups between minus 42 to minus 2 days prior to the takeover announcement, while we consider short-term runups between minus 10 and minus 1 or minus 2 days to the takeover announcement.

rejecting that differences in the extent to which advisors hold stakes in the target firms can explain the results.

7 Conclusion

Although runups in the stock prices of target firms shortly prior to takeover announcements are a prevalent empirical phenomenon consistent with insider trading, little is known about their determinants. In this study, we investigate an intuitive but unaddressed question: given their central role in the takeover process, do financial advisors matter for pre-bid target runups?

Based on a sample of over 5,000 takeover announcements, we provide evidence that financial advisors matter for target stock price runups. We find significant fixed effects for bidders' advisors in the pre-announcement returns of U.S. takeover targets. Differences across advisors are economically meaningful, and persistent over time. A difference-in-differences analysis around takeover-related insider trading enforcement actions by the SEC, used as shocks to individual advisors' propensities to use nonpublic deal information, supports our finding. Regarding observable advisor characteristics, we find that the number of past advised deals and advisors' location explain differences in target runups across banks.

The results of our study inform firms in the market for corporate control in their future selection of financial advisors as well as regulators attempting to detect potential insider trading. The findings contribute to the literature by adding to our understanding of the determinants of pre-bid target runups and by providing evidence to the as yet contradictory literature concerned with the question of whether financial institutions exploit or leak nonpublic material information obtained through their M&A advisory service.

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Figure 1: Distribution of number of deals over advisors

This figure depicts the number of advised transactions and the cumulative percentage for each financial advisor in the sample in descending rank from left to right.

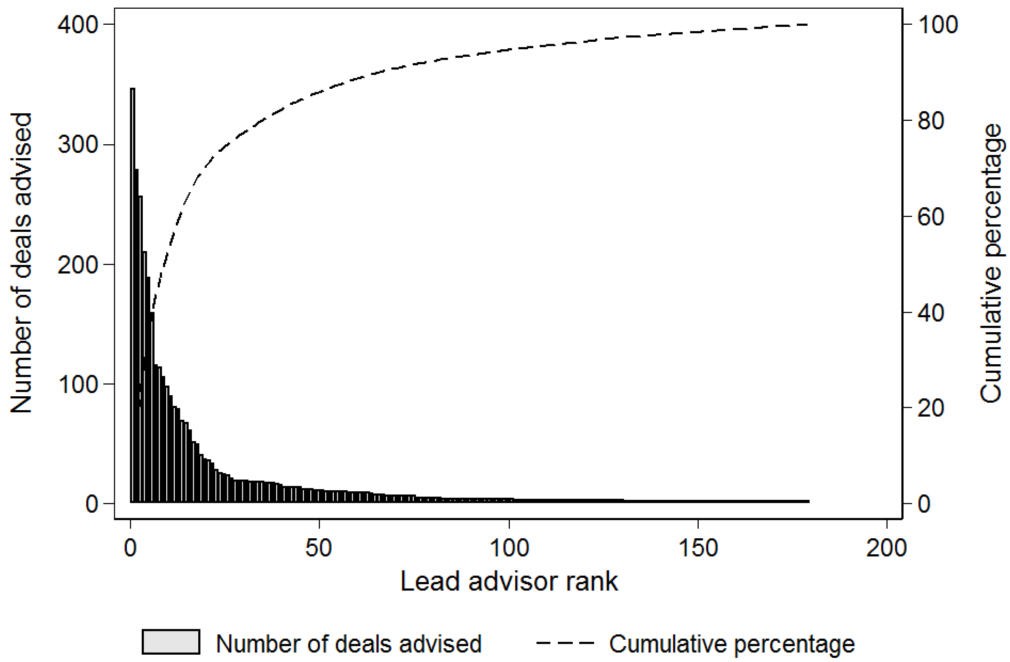

Figure 2: Abnormal stock returns around takeover announcements

This figure depicts the average daily abnormal returns prior to a takeover announcement in the target's stock. Variable definitions are provided in Table A1.

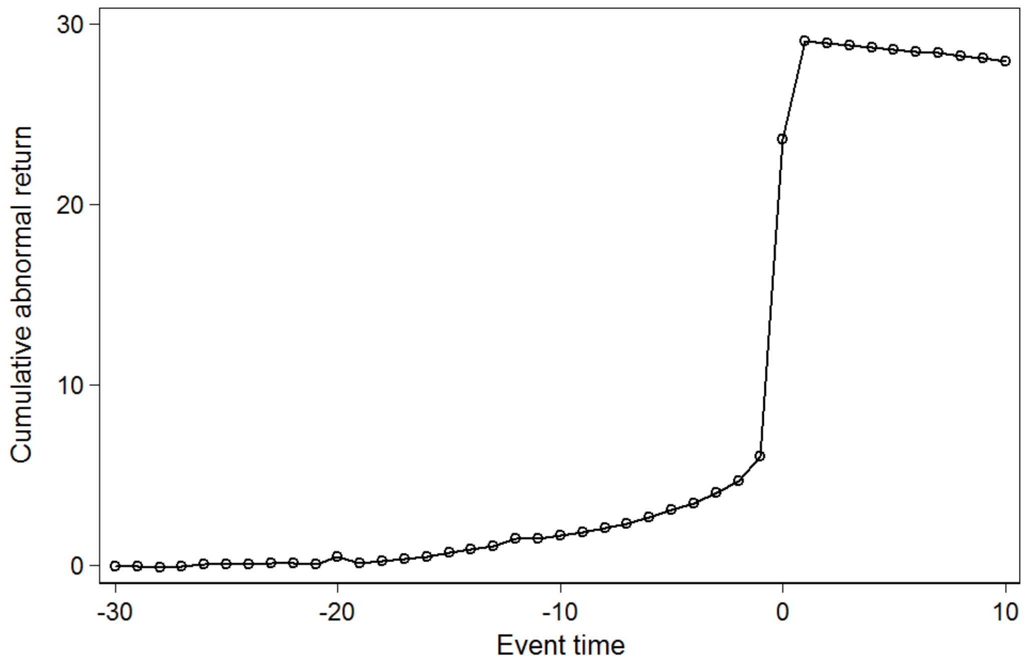

Figure 3: Financial advisor fixed effects

This figure depicts the coefficients on the 50 financial advisors with the highest number of transactions in the main sample. The coefficients are obtained from a regression of *CAR(-10,-1)* on financial advisor dummy variables and the standard control variables used in Table 2. Variable definitions are provided in Table A1.

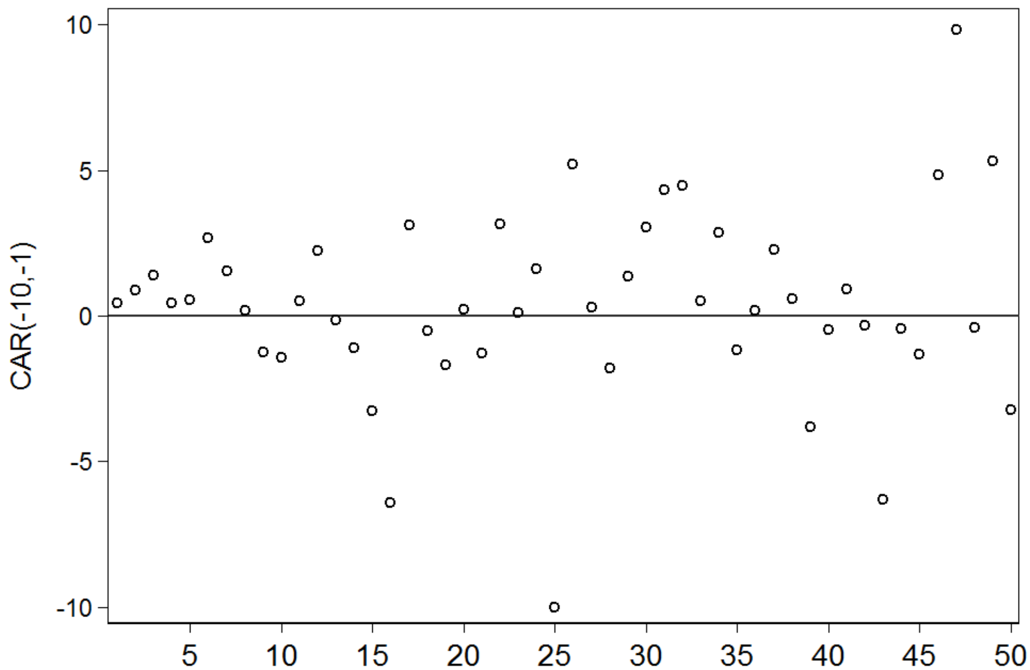

Figure 4: Runups around SEC insider trading enforcement actions

This figure depicts the estimated coefficients on the dummy variable *Senior post* over different time periods around the enforcement event based on a regression of *SCAR(-10,-1)* on financial advisor dummies, time and industry fixed effects, target and deal characteristics. $t2$ is equal to 1 from the enforcement year up to 2 years after the enforcement, and equal to 0 otherwise, $t4$ is equal to 1 for 3 and 4 years after the enforcement, $t6$ for 5 and 6 years after the enforcement, and so forth. Standard errors are clustered by acquiror and by year. Variable definitions are provided in Table A1.

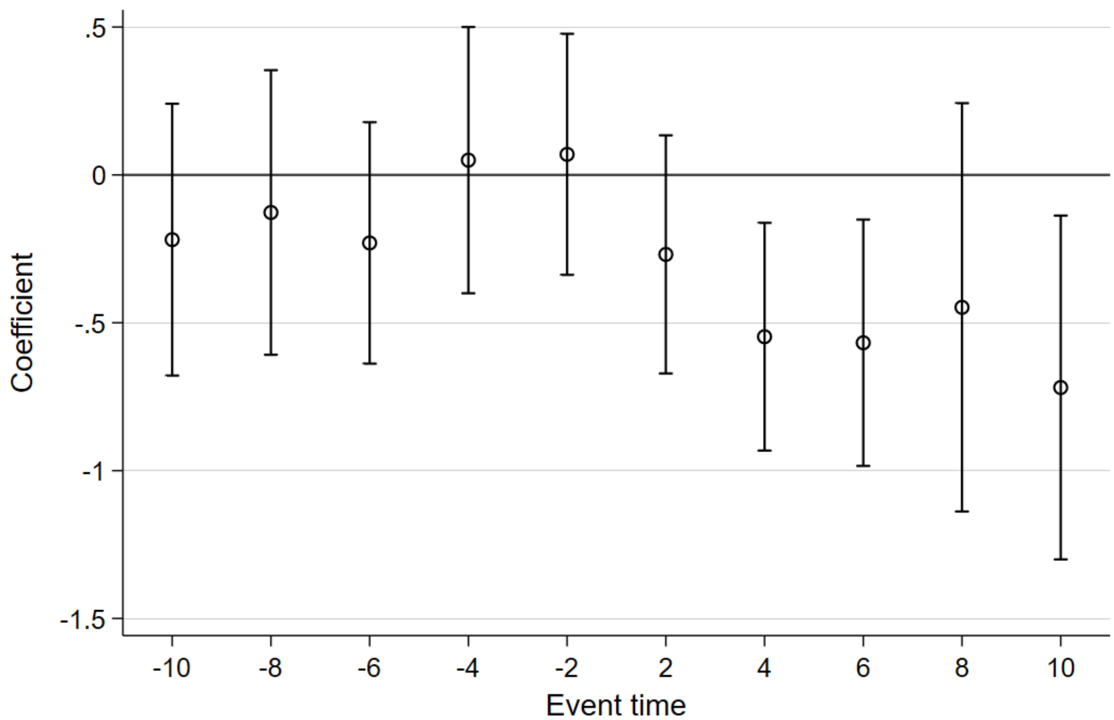

Figure 5: Runups and SEC enforcement budget

This figure plots the mean value of *SCAR(-10,-1)* and the SEC enforcement budget in USD million over 50 equally-sized bins. The variables are residualized on acquiror fixed effects, industry dummies, deal and target control variables. Variable definitions are provided in Table A1.

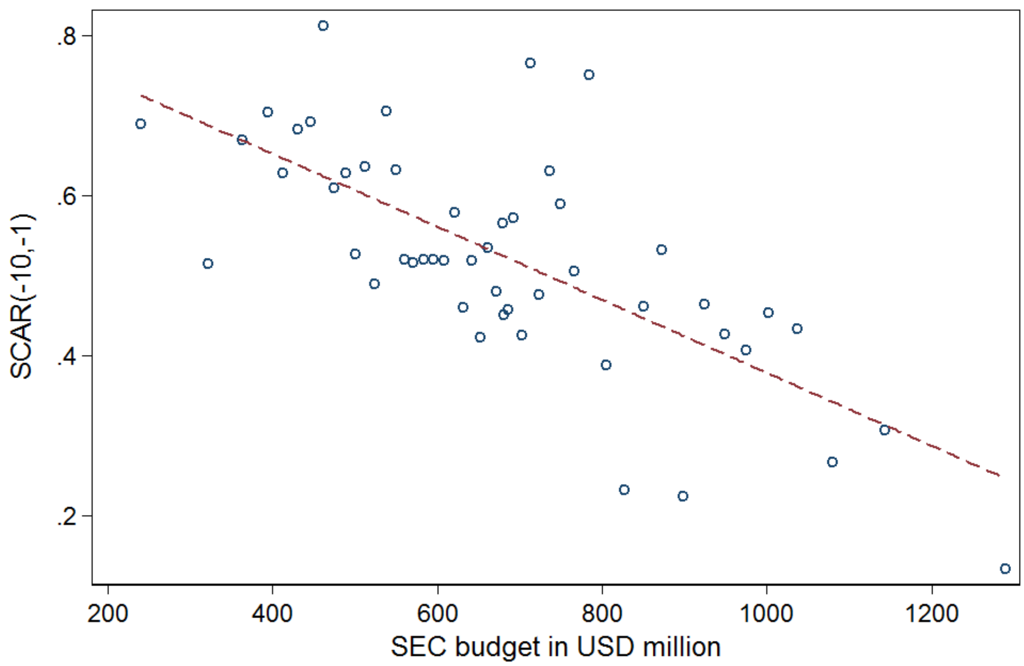

Table 1: Summary statistics

This table shows the mean and median values and the standard deviation of the deal, target and acquiror characteristics and of the runup measures. The number of observations decreases to 2,196 for acquiror characteristics, because not all acquirors in our sample are covered by the Compustat database. Variable definitions are provided in Table A1.

	(1) Obs	(2) Median	(3) Mean	(4) S.D.
Deal characteristics				
Transaction volume in USD million	5,316	246	1,265	3,053
Nr. of advisors	5,316	1.000	1.25	0.65
Cross-border (d)	5,316	0.000	0.146	0.35
Diversifying (d)	5,316	0.000	0.413	0.49
Hostile (d)	5,316	0.000	0.026	0.16
Tender offer (d)	5,316	0.000	0.231	0.42
Poison pill (d)	5,316	0.000	0.007	0.08
Cash pay (d)	5,316	1.000	0.501	0.50
Toehold (d)	5,316	0.000	0.033	0.18
Financial sponsor (d)	5,316	0.000	0.141	0.35
Public acquiror (d)	5,316	1.000	0.722	0.45
Offer premium	5,316	37.670	47.780	39.25
Rumor (d)	5,316	0.000	0.035	0.18
Inhouse (d)	5,316	0.000	0.338	0.47
Target characteristics				
Total assets in USD million	5,316	255	1,551	5,024
Leverage	5,316	0.152	0.212	0.22
R&D	5,316	0.000	0.086	0.28
Tobin's Q	5,316	1.267	1.689	1.20
RoA	5,316	0.088	0.066	0.17
Cash holdings	5,316	0.080	0.176	0.21

Table 1: Summary statistics (continued)

	(1) Obs	(2) Median	(3) Mean	(4) S.D.
Acquiror characteristics				
Total assets in USD million	2,196	2,635	18,677	54,345
Leverage	2,196	0.175	0.202	0.17
R&D	2,196	0.000	0.050	0.10
Tobin's Q	2,196	1.642	2.222	1.78
RoA	2,196	0.127	0.121	0.11
Cash holdings	2,196	0.076	0.159	0.19
Measures of stock behavior				
CAR(-10,-1)	5,316	2.854	4.815	16.91
CAR(-5,-1)	5,316	1.699	3.403	12.55
CAR(-10,-2)	5,316	1.849	3.361	15.29
SCAR(-10,-1)	5,316	0.317	0.514	1.20
SCAR(-5,-1)	5,316	0.255	0.518	1.34
SCAR(-10,-2)	5,316	0.230	0.385	1.10
MAX(-5,-1)	5,316	1.195	1.676	1.71
MAX(-10,-1)	5,316	1.612	2.157	1.88
SUM(-5,-1)	5,316	1.918	2.707	2.66
SUM(-10,-1)	5,316	3.947	4.820	3.55
FRACTION(-10,-1)	5,316	0.156	0.299	0.34
FRACTION(-5,-1)	5,316	0.095	0.251	0.33
SCAV(-10,-1)	5,165	0.513	0.509	2.34
SCAV(-5,-1)	5,165	0.525	0.547	1.89

Table 2: Regression of runups on control variables

This table shows the results of a linear regression of *SCAR(-10,-1)* on control variables. The table reports point estimates and standard errors clustered by acquiror and by year in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level, respectively. Variable definitions are provided in Table A1.

Dep. var.: SCAR(-10,-1)	(1) b/t	(2) b/t	(3) b/t
Hostile (d)	-0.234	-0.239*	-0.239*
	(-1.71)	(-1.74)	(-1.74)
Log transaction volume	0.044***	0.046***	0.046***
	(4.22)	(4.35)	(4.37)
Nr. of advisors	-0.008	-0.009	-0.009
	(-0.24)	(-0.26)	(-0.26)
Cross-border (d)	0.141***	0.139***	0.140***
	(3.03)	(2.92)	(2.86)
Cash pay (d)	0.014	0.010	0.010
	(0.32)	(0.23)	(0.23)
Toehold (d)	0.066	0.063	0.064
	(0.94)	(0.91)	(0.93)
Financial sponsor (d)	-0.035	-0.038	-0.038
	(-0.79)	(-0.86)	(-0.86)
Rumor (d)	0.465***	0.461***	0.460***
	(4.24)	(4.16)	(4.20)
Public acquiror (d)	0.069*	0.072*	0.072*
	(1.75)	(1.80)	(1.79)
Inhouse (d)	-0.059	-0.061	-0.061
	(-1.36)	(-1.40)	(-1.39)
Offer premium	0.006***	0.006***	0.006***
	(18.17)	(19.93)	(20.00)
Poison pill (d)	-0.164	-0.167	-0.165
	(-0.75)	(-0.76)	(-0.75)
Tender offer (d)	0.072	0.071	0.070
	(1.56)	(1.51)	(1.50)
Year FE	yes	yes	yes
Industry FE	yes	yes	yes
Target controls	no	yes	yes
Acquiror FE	no	no	yes
Adjusted R2	0.071	0.071	0.071
Obs	5,316	5,316	5316

Table 3: Advisor fixed effects

This table reports the test of joint significance of financial advisor fixed effects in the linear regression model of *SCAR(-10,-1)* on different sets of control variables as indicated in column 1. Column 2 reports the F-statistics for the joint significance of advisor fixed effects for advisors with at least 5 transactions in the sample and the corresponding p-values and the degress of freedom in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level. Panel A shows the results for *SCAR(-10,-1)*. Panel B shows the results for alternative measures of runups using the regression model indicated in row 5 in Panel A. Panel C adds the average acquiror-specific runups calculated based on the past four transactions as a control variable. This reduces the number of observations to 2,913. Variable definitions are provided in Table A1.

Abnormal call option volume - constant mean model

	(1) Controls	(2) Advisor FE F-test	(3) Obs	(4) Adj. R-sqr (%)	(5) R-squared (%)
(1)	None	1.73 (0.0003, 67)	1,539	3.09	7.31
(2)	Add industry and time FE	1.65 (0.0009, 67)	1,539	3.18	8.53
(3)	Add deal chars	1.58 (0.0023, 67)	1,539	2.95	12.22
(4)	Add target chars	1.38 (0.0259, 67)	1,539	6.38	16.18
(5)	Add acquiror FE	1.38 (0.0245, 67)	1,539	6.53	16.62
(6)	Add acquiror chars	1.22 (0.1235, 65)	1,539	-7.78	33.49

Panel B: Alternative measures

	(1) Controls	(2) Advisor FE F-test	(3) Obs	(4) Adj R-sqr (%)	(5) R-squared (%)
(1)	SCAR(-5,-1)	1.72 (0.0001, 75)	5,316	7.82	25.08
(2)	SCAR(-10,-2)	1.49 (0.0044, 75)	5,316	6.86	24.29
(3)	SCAR(-10,-3)	1.3 (0.0413, 75)	5,316	6.83	24.27
(4)	CAR(-10,-1)	1.43 (0.0096, 75)	5,316	8.25	25.42
(5)	CAR(-5,-1)	1.27 (0.0577, 75)	5,316	6.95	24.37
(6)	CAR(-10,-2)	1.28 (0.0519, 75)	5,316	6.75	24.20
(7)	MAX(-5,-1)	1.34 (0.0277, 75)	5,316	9.16	26.16
(8)	MAX(-10,-1)	1.32 (0.0352, 75)	5,316	8.89	25.95
(9)	SUM(-5,-1)	1.19 (0.1300, 75)	5,316	10.11	26.93
(10)	SUM(-10,-1)	1.44 (0.0078, 75)	5,316	11.13	27.76
(11)	FRACTION (-10,-1)	1.72 (0.0001, 75)	5,316	8.81	25.88
(12)	FRACTION (-5,-1)	1.15 (0.1800, 75)	5,316	7.93	25.16
(13)	FRACTION (-10,-2)	1.86 (0.0000, 75)	5,316	7.91	25.11
(14)	SCAV(-10,-1)	1.39(0.0163,75)	5,165	9.65	26.95
(15)	SCAV(-5,-1)	1.60(0.0009,75)	5,165	10.21	27.40

Panel C: Controlling for time-varying acquiror-specific runups

	(1) Controls	(2) Advisor FE F-test	(3) Obs	(4) Adj. R-sqr (%)	(5) R-squared (%)
(1)	None	6.96 (0.0000, 75)	2,913	1.23	3.81
(2)	Add industry and time FE	2.78 (0.0000, 75)	2,913	2.36	7.93
(3)	Add deal chars	5.25 (0.0000, 75)	2,913	7.67	13.38
(4)	Add target chars	4.78 (0.0000, 75)	2,913	7.84	13.72
(5)	Add acquiror FE	5.42 (0.0000, 75)	2,913	8.64	17.51
(6)	Add acquiror chars	5.87 (0.0000, 71)	1,044	13.67	33.62

Table 4: Placebo test with randomly assigned advisors

This table reports the distribution of the F-statistics of the joint significance of financial advisor fixed effects in the linear regression model of *SCAR(-10,-1)* on different sets of control variables as indicated in column 1. Financial advisors are randomly assigned to transactions while the number of transactions is maintained for each advisor. The summary statistics are based on the F-values of 10,000 randomly assigned financial advisors for each model specification, i.e., each row. Variable definitions are provided in Table A1.

	(1)	(2)	(3)	(4)	(5)	(6)	
				Placebo			Actual
	Controls	Mean	Median	5p	95p	St. dev.	
(1)	None	1.000	0.991	0.743	1.289	0.167	2.44
(2)	Add industry and time FE	1.001	0.990	0.744	1.291	0.167	1.79
(3)	Add deal chars	1.001	0.990	0.745	1.293	0.167	1.89
(4)	Add target chars	1.001	0.989	0.744	1.294	0.168	1.84
(5)	Add acquiror FE	1.003	0.996	0.743	1.299	0.170	1.74

Table 5: Economic magnitude

This table reports the economic significance of financial advisor effects. Panel A and B report statistics of the distribution of advisor fixed effects for the regressions reported in Panel A of Table 3 with *SCAR(-10,-1)* and *CAR(-10,-1)* as dependent variables. Panel C reports the univariate differences in mean values of *SCAR(-10,-1)* over the next 1, 2, or 3 years for the bottom and top quintile groups according to the mean value of *SCAR(-10,-1)* in the past 1, 2, or 3 years. t-statistics are indicated in parentheses.*, ** and *** denote statistical significance at the 10%, 5% or 1% level, respectively. Variable definitions are provided in Table A1.

Panel A: Economic magnitude (SCAR)

	(1) Control	(2) St. dev.	(3) 25th	(4) 75th	(5) Interquartile range
(1)	None	0.305	0.124	0.489	0.365
(2)	Add industry and time FE	0.295	0.163	0.466	0.303
(3)	Add deal chars	0.313	0.448	0.748	0.300
(4)	Add target chars	0.311	0.438	0.772	0.334
(5)	Add acquiror FE	0.326	0.576	0.911	0.336

Panel B: Economic magnitude (CAR)

	(1) Control	(2) St. dev.	(3) 25th	(4) 75th	(5) Interquartile range
(1)	None	4.428	-1.713	1.735	3.448
(2)	Add industry and time FE	4.124	-3.743	-0.136	3.337
(3)	Add deal chars	4.365	0.848	4.633	3.785
(4)	Add target chars	4.407	1.097	4.504	3.407
(5)	Add acquiror FE	5.026	2.180	6.039	3.859

Panel C: Persistence

	Future runups measured over		
Quintiles measured over	1yr	2yrs	3yrs
1 year runups			
Q1	0.51	0.52	0.60
Q5	0.63	0.59	0.58
Q5-Q1	0.12	0.07	-0.02
	(1.03)	(-0.83)	(0.22)
2 year runups			
Q1	0.45	0.51	5.53
Q5	0.65	0.64	0.63
Q5-Q1	0.19**	0.13**	0.10*
	(2.15)	(1.98)	(1.81)
3 year runups			
Q1	0.43	0.48	0.52
Q5	0.59	0.64	0.65
Q5-Q1	0.16*	0.16***	0.13***
	(1.80)	(2.49)	(2.51)

Table 6: Difference-in-differences analysis around SEC insider trading enforcement actions

This table shows the results of a linear regression of a measure of abnormal stock behavior on the dummy variables *Post* and additional control variables. Panel A shows the regression results of Post on financial advisor dummies, year fixed effects, the dummy variable *Post*, which is set to 1 for transactions that occur after an enforcement event. *Senior post* and *Junior post* refer to enforcement events involving senior or junior bank employee, respectively. Panel B regresses *SCAR(-10,-1)* on placebo dummy variables, where it is assumed that the enforcement event has occured 3, 4, or 5 years earlier. In columns 4 to 6, we exclude observations of the treatment group after the enforcement. Panel C shows the regression of column 3 in Panel A with alternative measures of runups. In Panel D the control sample of financial advisors is selected based on propensity score matching using either *SCAR(-10,-1)*, deal and target control variables, or only *SCAR(-10,-1)*. The control advisors are chosen based on nearest-neighbor matching. The tables report point estimates and standard errors clustered by acquiror and by year in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level, respectively. Variable definitions are provided in Table A1.

Panel A: Difference-in-differences regression

Dep. var.: SCAR(-10,-1)	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se
Post (d)	-0.109* (0.059)				
Post senior involvement (d)		-0.365*** (0.078)	-0.336*** (0.092)	-0.313*** (0.095)	-0.313*** (0.079)
Post junior involved (d)		-0.064 (0.059)	-0.095 (0.090)	-0.107 (0.092)	-0.064 (0.059)
Violation period (d)					0.293** (0.136)
Year FE	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes
Target controls	yes	yes	yes	yes	yes
Advisor FE	yes	yes	yes	yes	yes
Deal controls	yes	yes	yes	no	yes
Acquiror FE	yes	yes	no	no	yes
Adjusted R2	0.090	0.092	0.071	0.020	0.093
Obs	5,316	5,316	5,316	5,316	5,316

Panel B: Analysis of pre-event trends

Dep. var.: SCAR(-10,-1)	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se	(6) b/se
Sample	All	All	All	post = 0	post = 0	post = 0
Placebo post (minus 3 yrs)	-0.093			0.199		
	(0.084)			(0.131)		
Placebo post (minus 4 yrs)		-0.061			0.185	
		(0.089)			(0.137)	
Placebo post (minus 5 yrs)			-0.129			0.035
			(0.089)			(0.133)
Year FE	yes	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes
Target controls	yes	yes	yes	yes	yes	yes
Advisor FE	yes	yes	yes	yes	yes	yes
Deal controls	yes	yes	yes	yes	yes	yes
Acquiror FE	yes	yes	yes	yes	yes	yes
Adjusted R2	0.089	0.089	0.089	0.092	0.092	0.091
Obs	5,316	5,316	5,316	4,930	4,930	4,930

Panel C: Other measures

Dep. var.:	(1) SCAR(-10,-2) b/se	(2) SCAR(-5,-1) b/se	(3) FRAC(-10,1) b/se	(4) FRAC(-5,1) b/se	(5) SUM10 b/se	(6) SUM5 b/se	(7) MAX10 b/se	(8) MAX5 b/se
Senior post (d)	-0.246***	-0.625***	-0.031	-0.052	-0.928***	-1.096***	-0.530***	-0.675***
	(0.082)	(0.091)	(0.027)	(0.036)	(0.320)	(0.176)	(0.185)	(0.120)
Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes	yes	yes	yes
Target controls	yes	yes	yes	yes	yes	yes	yes	yes
Advisor FE	yes	yes	yes	yes	yes	yes	yes	yes
Deal controls	yes	yes	yes	yes	yes	yes	yes	yes
Acquiror FE	yes	yes	yes	yes	yes	yes	yes	yes
Adjusted R2	0.067	0.076	0.086	0.081	0.110	0.099	0.087	0.090
Obs	5,316	5,316	5,316	5,316	5,316	5,316	5,316	5,316

Panel D: Matched control sample

Dep. var.: SCAR(-10,-1)	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se
Senior post × treatment	-0.326***	-0.448***	-0.453***	-0.524***	-0.476***
	(0.098)	(0.132)	(0.161)	(0.141)	(0.146)
Post	0.065	0.166*	0.178*	0.174	0.259***
	(0.101)	(0.096)	(0.093)	(0.116)	(0.083)
Number of matched advisors	3	3	3	1	5
Matched controls	All	All	SCAR	All	All
Year FE	yes	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes	yes
Target controls	yes	yes	yes	yes	yes
Advisor FE	yes	yes	yes	yes	yes
Deal controls	yes	yes	yes	yes	yes
Acquiror FE	no	yes	yes	yes	yes
Adjusted R2	0.127	0.268	0.233	0.140	0.351
Obs	2,583	2,428	2,082	1,536	4,053

Table 7: Observable advisor characteristics

This table shows the results of a linear regression of *SCAR(-10,-1)* on control variables. The table reports point estimates and standard errors clustered by acquiror and by year in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level, respectively. Variable definitions are provided in Table A1.

Dep. var.: SCAR(-10,-1)	(1) b/t	(2) b/t	(3) b/t	(4) b/t	(5) b/t
Investment bank (d)	0.086*				
	(0.04)				
Boutique (d)	0.094				
	(0.08)				
Universal bank (d)	0.050				
	(0.07)				
Log of past advised deals		0.041**			
		(0.02)			
Top 25 (d)			0.114***		
			(0.04)		
Listed advisor (d)				0.071*	
				(0.04)	
NYC-based (d)					0.140***
					(0.04)
					(0.05)
Industry FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Deal chars	Yes	Yes	Yes	Yes	Yes
Target chars	Yes	Yes	Yes	Yes	Yes
Obs	5,316	5,316	5,316	5,316	5,316
R-squared	0.252	0.253	0.252	0.252	0.253
Adj. R-squared	0.094	0.096	0.096	0.095	0.097

Appendix

This Appendix includes tables supplementing the main paper. Table A1 provides variable definitions. Table A2 presents the results of a logit regression of a takeover indicator. Table A3 regresses runups on measures of deal synergies, while Table A4 investigates the relation between advisor toeholds and runups. Table A5 lists the takeover-related SEC enforcement actions used in the difference-in-difference analysis.

Table A1: Definition of variables

This table provides definitions of the variables used in this paper.

Variable	Definition
Measures of leakage	
CAR(-a,-b)	Cumulative abnormal return in the time period between a to b days prior to the takeover announcement.
SCAR(-a,-b)	CAR(-a,-b) scaled by the standard deviation of residual returns in the estimation window.
CAV(-a,-b)	Cumulative abnormal volume in the time period between a to b days prior to the takeover announcement.
SCAV(-a,-b)	CAV(-a,-b) scaled by the standard deviation of residual volume in the estimation window.
MAX(-a,-b)	The maximum value of the daily standardized returns in the time period between a and b days prior to the announcement.
SUM(-a,-b)	The sum of positive daily standardized returns in the time period between a and b days prior to the announcement.
FRACTION(-a,b)/(-a,1)	Cumulative abnormal returns in the time period between a to b days prior to the takeover announcement divided by cumulative abnormal returns between a days prior to 1 day after the announcement. This variable is winsorized at 0 and 1.
Deal and advisor characteristics	
Transaction volume	Transaction volume in USD million.
Rumor	Dummy variable which is set to 1 if the target firm is mentioned in the Capital IQ rumor database in the 60 days preceding the corporate takeover announcement, 0 otherwise.
Inhouse	Dummy variable set to 1 if no financial advisor was indicated in the SDC database, 0 otherwise.
Nr. of advisors	Number of financial advisors of the acquiring firm involved in the transaction.
Poison pill	Dummy variable set to 1 if the target firm adopts a poison pill according to the SDC database, and 0 otherwise.

Table A1: Definition of variables (continued)

Variable	Definition
Deal and advisor characteristics (continued)	
Public acquiror	Dummy variable set to 1 if the acquiring firm is publicly listed.
Hostile	Dummy variable set to 1 if the transaction is labeled as hostile in the SDC database, 0 otherwise.
Toehold	Dummy variable set to 1 if the acquiring firm has disclosed a stake in the target firm prior to the takeover announcement, 0 otherwise.
Offer premium	Offer price to target stock price four weeks prior to the announcement of the transaction.
Tender offer	Dummy variable set to 1 if the transaction is labeled as tender offer in SDC, 0 otherwise.
Cash pay	Dummy variable set to 1 if the acquiring firm pays by cash only
Cross-border	Dummy variable set to 1 if the acquiring firm is located outside the United States, 0 otherwise.
Diversifying	Dummy variable set to 1 if the 2 digit SIC code of the target firm differs from the 2 SIC code of the acquiring firm, 0 otherwise.
Financial sponsor	Dummy variable set to 1 if the deal involves a financial sponsor, 0 otherwise.
Top 25	Dummy variable set to 1 if the advisor firm is among the top 25 advisors in terms of number of deals within the sample in the past, and to 0 otherwise.
Past advised transactions	Number of transactions in the sample that the given financial advisor has been involved as a lead advisor for the acquiring firm.
Boutique	Dummy variable set to 1 if the financial advisor primarily offers M&A consulting services, 0 otherwise.
Investment bank	Dummy variable set to 1 if the financial advisor offers additional investment banking services to firms such as underwriting, trading, or financing.
Universal bank	Dummy variable set to 1 if the financial advisor also offers commercial banking, or retail banking services.
Listed advisor	Dummy variable set to 1 if the financial advisor is publicly listed, 0 otherwise.
NYC-based	Dummy variable set to 1 if the financial advisor is headquarterd in New York City, 0 otherwise.

Table A1: Definition of variables (continued)

Variable	Definition
Firm characteristics	
Total assets	Book value of total assets.
Firm age	Time since the firm first appeared in the Compustat database in years.
RoA	Operating income before extraordinary items scaled by the book value of total assets.
Leverage	Long-term debt and debt in current liabilities scaled by total assets.
R&D	Research and development expenditures scaled by total assets.
SG&A	Selling, general and administrative expenses scaled by total assets.
Tobin's Q	Book value of total assets minus book value of equity plus market value of equity (share price multiplied with the number of shares outstanding) divided by the book value of total assets.
Dividend	Dummy variable set to 1 if the firm is paying a dividend to shareholders, 0 otherwise.
Cash holdings	Cash and cash equivalents scaled by the book value of total assets.
Enforcement analysis variables	
Post	Dummy variable set to 1 after an SEC enforcement event.
Senior (junior) post	Dummy variable set to 1 after an SEC enforcement event involving a senior (junior) employee targeting the respective advisor, 0 otherwise.
Violation period	Dummy variable set to 1 in the period between the start of the violation period of a given SEC enforcement episode and the disclosure date of the enforcement event for transactions that are advised by the affected financial advisor.

Table A2: Predicting takeovers

This table shows the results of a logit regression of a takeover indicator which is set to 1 if a firm experiences a takeover in a given year. The table reports point estimates and heteroskedasticity-robust standard errors in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level, respectively. Variable definitions are provided in Table A1.

Dep. var.: Takeover indicator (d)	(1) b/se
Log total assets	0.020
	(0.01)
Leverage	0.271***
	(0.05)
R&D expenditures	0.011
	(0.04)
SG&A	-0.196**
	(0.09)
Tangibility	-0.542***
	(0.16)
Cash holdings	-0.252
	(0.19)
RoA	0.961***
	(0.19)
Tobin's Q	-0.173***
	(0.04)
Dividend (d)	-0.827***
	(0.08)
Firm age	0.737***
	(0.04)
Log (deals+1)$_{t-1}$	-0.584
	(0.45)
Log (industry deals+1)$_{t-1}$	-0.047
	(0.05)
Rumor (d)	0.931***
	(0.11)
Time FE	Yes
Obs	78,526
Pseudo R-squared	0.112

Table A3: Stock price runups and synergies

This table shows the results of a linear regression of runups on the sum of target and acquiror CAR (columns 1 and 3) and the weighted sum of target and acquiror CAR (columns 2 and 4). The weighted sum is calculated using the relative market capitalization of the target and the acquiror as weights. The table reports point estimates and standard errors clustered by acquiror and year in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level, respectively. Variable definitions are provided in Table A1.

	(1)	(2)	(3)	(4)
Dep. var.:	SCAR(-10,-1)	SCAR(-10,-1)	SCAR(-10,-2)	SCAR(-10,-2)
Sum of target and acquiror CAR	0.001		0.001	
	(0.02)		(0.02)	
Weighted sum of target and acquiror CAR		0.048		0.027
		(0.05)		(0.04)
Obs	2,684	2,076	2,684	2,076
R-squared	0.318	0.305	0.288	0.281
Adj. R-squared	0.110	0.080	0.071	0.048

Table A4: Stock price runups and advisor toeholds

This table shows the results of a linear regression of *SCAR(-10,-1)* on a dummy variable indicating whether the acquiror advisor (Panel A) or the target advisor (Panel B) holds a stake in the target firm in the respective month before the announcement of the takeover. The table reports point estimates and standard errors clustered by acquiror and by year in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level, respectively. Variable definitions are provided in Table A1.

Panel A Panel A: Acquiror advisor stakes

Dep. var.: SCAR(-10,-1)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Acq advisor stake minus 7 (d)								0.076
								(0.09)
Acq advisor stake minus 6 (d)							0.072	
							(0.09)	
Acq advisor stake minus 5 (d)						0.052		
						(0.09)		
Acq advisor stake minus 4 (d)					0.175			
					(0.14)			
Acq advisor stake minus 3 (d)				0.109				
				(0.11)				
Acq advisor stake minus 2 (d)			0.031					
			(0.10)					
Acq advisor stake minus 1 (d)		0.022						
		(0.09)						
Acq advisor stake minus 0 (d)	0.029							
	(0.09)							
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Deal chars	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target chars	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	5,309	5,309	5,309	5,309	5,309	5,309	5,309	5,309
R-squared	0.252	0.252	0.252	0.252	0.252	0.252	0.252	0.252
Adj. R-squared	0.095	0.095	0.095	0.096	0.096	0.095	0.096	0.096

Table A4: Stock price runups and advisor toeholds (continued)

Panel B Panel B: Target advisor stakes

Dep. var.: SCAR(-10,-1)	(1) b/se	(2) b/se	(3) b/se	(4) b/se	(5) b/se	(6) b/se	(7) b/se	(8) b/se
Target advisor stake minus 7 (d)								0.071
								(0.07)
Target advisor stake minus 6 (d)							0.076	
							(0.07)	
Target advisor stake minus 5 (d)						0.069		
						(0.07)		
Target advisor stake minus 4 (d)					-0.094			
					(0.07)			
Target advisor stake minus 3 (d)				-0.049				
				(0.06)				
Target advisor stake minus 2 (d)			-0.003					
			(0.06)					
Target advisor stake minus 1 (d)		0.029						
		(0.06)						
Target advisor stake minus 0 (d)	0.041							
	(0.06)							
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Deal chars	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Target chars	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	5,309	5,309	5,309	5,309	5,309	5,309	5,309	5,309
R-squared	0.252	0.252	0.252	0.252	0.252	0.252	0.252	0.252
Adj. R-squared	0.096	0.095	0.095	0.096	0.096	0.096	0.096	0.096

Table A5: Takeover-related insider trading SEC enforcement actions

This table summarizes takeover-related insider trading enforcement actions by the SEC which involve financial advisors in our sample.

Enforcement date	Violation date	Profits (USDk)	Nr of stocks	Nr. of individuals
With senior involvement				
21-Dec-99	1-Apr-98	88	6	3
29-Jul-04	23-Dec-02	79	1	1
1-Mar-07	25-Aug-06	15,000	24	14
10-May-07	16-Jan-07	601	3	2
27-Sep-07	20-Feb-04	68	3	2
5-Feb-09	18-Dec-06	11,600	3	7
5-Feb-09	18-Dec-06	11,600	3	7
30-Apr-09	25-May-07	6,305	2	6
16-Dec-09	23-Feb-07	4910	5	5
25-Mar-10	1-Sep-08	1,088	11	5
Without senior involvement				
15-Sep-97	29-Aug-97	364	4	1
1-Apr-98	15-Jan-98	1,800	4	8
15-May-98	17-Sep-97	145	4	2
15-May-98	17-Sep-97	145	4	2
17-Jun-98	21-Jul-95	60	2	1
15-Jul-98	28-Mar-90	2,098	5	4
6-Aug-98	1-Oct-97	1,500	1	2
11-Aug-98	5-May-97	416	1	4
3-Dec-98	17-Sep-97	1,550	13	14
3-Dec-98	17-Sep-97	1,550	13	14
4-Aug-99	1-Jul-99	54	9	2
17-Mar-00	1-Jan-00	8,000	23	19
17-Mar-00	1-Jan-00	8,000	23	19
13-Oct-00	6-Sep-00	48	2	3
31-Jan-01	15-Jul-98	4	1	1
18-Oct-01	30-Jul-98	964	1	3
14-Jan-02	30-Jul-98	56	1	2
7-Mar-02	15-Sep-97	157	3	3
4-Jun-03	28-Dec-01	46	1	2
14-Dec-04	21-Nov-03	439	3	2
28-Apr-05	24-Mar-03	901	4	6
28-Apr-05	24-Mar-03	901	4	6
25-Apr-06	19-Jun-01	1,336	1	4
1-Mar-07	25-Aug-06	15,000	24	14
1-Mar-07	25-Aug-06	15,000	24	14
10-Jun-09	13-Jun-05	124	8	3
15-Oct-09	1-Dec-05	350	1	5
21-Sep-12	2-Sep-10	1,680	1	4
28-Sep-12	16-Jun-10	600	2	2
6-Dec-12	30-Jul-12	11,000	7	10

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