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# Extraordinary acquirers ☆

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ABSTRACT

Firm fixed effects alone explain as much of the variation in acquirer returns as all the firm- and deal-specific characteristics combined. An interquartile range of acquirer fixed effects is over 6%, comparable to the interquartile range of acquirer returns. Acquirer returns persist over time, but mainly at the top end of the distribution. Persistence continues under different chief executive officers (CEOs), and attributes of the broader management team do not explain the fixed effect. Firm-specific heterogeneity in acquirer returns suggests that some organizations are extraordinary acquirers irrespective of the leadership at the top and the deal structures they choose. Implications for the M&A research are discussed.



*"Finally, knowledge of the source of takeover gains still eludes us."* (Jensen and Ruback, 1983, p. 47).

## 1. Introduction

The opening quote above motivated Roll's (1986) seminal paper on managerial hubris in takeovers. Strikingly, 30 years after the first review of the early takeover literature by Jensen and Ruback (1983), their conclusion as to the elusive nature of takeover gains remains relevant. While the explosion in large sample studies of mergers and acquisitions (M&A) over the last three decades has identified a number of robust determinants of acquirer performance, the overall variation in the returns to acquisition activity remains largely unexplained. For example, a widely cited study by Moeller, Schlingemann, and Stulz (2004) examines over 12,000 M&A deals, and, employing an extensive list of determinants, is able to explain just over 5% of the variation in acquirer returns (as judged by the adjusted $R^2$ of their main regression models).[1] Similar,

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[1] We emphasize that this is not to detract from the contribution of the Moeller, Schlingemann, and Stulz (2004) study. In fact, the acquirer size effect they document has proven to be one of the most robust determinants of acquirer returns; we also confirm it in this paper.

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albeit smaller sample studies such as Masulis, Wang, and Xie (2007) and Harford, Humphery-Jenner, and Powell (2012) report comparably low explanatory powers. If an exhaustive list of factors in combination does not explain the widespread variation in acquirer returns in a systematic way, then what does?

At the same time—and in contrast to the commonly held belief that mergers fail to deliver value—anecdotal evidence points out some persistent acquisition successes. Cisco Systems, Berkshire Hathaway, IBM, General Electric, and Diageo are some notable examples of frequent acquirers that most observers consider to have been consistently successful in their acquisitions.[2] These examples suggest that there could be some *firm-specific* driver of acquisition success—ignored by prior studies—that can help explain the cross-sectional variation in takeover gains. In other words, if there are systematic differences in the firms' ability to create value through acquisitions (over and above the known determinants), then we should observe significant firm-specific components in acquirer returns.

In this paper we show that acquirer returns are, indeed, best explained by an unobserved, time-invariant, firm-specific factor. In line with prior research, we show that the explanatory power of a comprehensive regression specification employing most of the widely used firm- and deal-specific characteristics explains only 5.0–6.4% (adjusted $R^2$, 3.6–6.0%) of the variation in acquirer returns. However, augmenting the same regression models with acquirer fixed effects roughly doubles their explanatory power. These findings suggest that the source of acquirer gains is not deal- but rather firm-specific. That is, some firms are stellar acquirers irrespective of their time-varying attributes and the deal structures they choose. The economic magnitude of the fixed effect is staggering. An interquartile range in acquirer fixed effects is over 6%, comparable to the interquartile range of the underlying acquirer cumulative abnormal return (CAR) distribution. Moving from the 25th to the 75th percentile of the fixed effects distribution is associated with $184 million ($28 million) in incremental shareholder value creation for a mean-(median)-sized acquirer at deal announcement.

We further show that acquirer returns are persistent over time. Acquirers in the top performance quintile continue to make better acquisitions than acquirers from the bottom performance quintile at least up to five years down the road. We find that persistence in acquirer returns is concentrated mainly in the best performers. There is a strong positive dependence of future returns on past returns at the 80th percentile of the return distribution, but not at the mean. Importantly, persistence in acquirer returns is unexplained by the usual characteristics shaping these returns, as we continue to find persistence in the component of acquirer returns that is orthogonal to the known determinants.

Naturally, we are interested in the economic forces behind the fixed effects/persistence in acquirer returns. Several plausible candidates emerge. First, these effects could be attributable to particular CEOs, or some other slow-moving attributes of the broader top management team. However, we show that acquirer returns continue to be persistent even when the CEO changes. We probe further into the "managerial" explanation and attempt to explain the acquirer fixed effect using the attributes of the top management team found to be important by the management science literature. We find that the inclusion of these variables does not improve the explanatory power of the acquirer returns model, and does not detract from the economic or statistical significance of the fixed effects. Persistence could also be generated by sticking to the "right" advisors; however, we verify that the firm fixed effect is distinct from (and much larger than) the advisor fixed effect. Finally, we show that industry affiliation, which is subsumed by firm fixed effects, explains little of the variation in the estimated fixed effects, further reinforcing the idea that the sources of superior takeover performance are to be found within the firm and not its environment.

It appears that some firms are extraordinary acquirers irrespective of the leadership at the top and the particular deal structures they choose. We discuss several qualitative factors that could be responsible for the observed firm fixed effect and persistence in acquirer returns that are not readily amenable to testing in a large sample study. The first group of explanations revolves around organizational knowledge/skill in deal-making. Such knowledge can reside with in-house M&A/corporate development teams charged with screening deals, performing due diligence, and undertaking most of the analysis behind acquisition decisions. It can also stem from the particular processes followed by acquiring firms in terms of post-merger integration. The second possibility is that persistent acquisition gains reflect some unique, bidder-specific synergies, for example, stemming from the nature of the firm's assets or its position in the production process that are particularly well-suited for acquisitions. Finally, persistence could also be generated endogenously with prior successes breeding further success. For instance, it could be the case that making a good deal (even if by luck) positions the firm for better performance in subsequent deals, thereby creating path-dependence in acquisition success.

Our paper is related to several strands of literature. Most notably, it is related to the growing "fixed effects" literature, but the distinguishing feature of our work is that we focus on the firm-specific heterogeneity in the *quality* rather than the *quantity* of a particular corporate policy. Bertrand and Schoar (2003) pioneered this line of research by showing that manager fixed effects explain various corporate policies ranging from financing to payout.[3] Lemmon, Roberts, and Zender (2008) show that

[2] For example, "Cisco defies the odds with mergers that work," *The Wall Street Journal*, 1 March 2000; "With his magic touch, Buffett may be irreplaceable for Berkshire," *The New York Times*, 21 May 2013; "Listen. It's GE's secret for a successful marriage," *The Guardian*, 2 September 2007; "Diageo's new boss could use a drink," *The Wall Street Journal*, 7 May 2013.

[3] More recently, Fee, Hadlock, and Pierce (2013) question whether the manager-specific effects can be interpreted as causal, and also raise a

capital structures are to a large extent explained by time-invariant firm-specific attributes, but DeAngelo and Roll (2015) demonstrate that capital structures are much less stable over longer periods of time and that firm-decade interactions explain leverage much better than firm effects alone. Graham, Li, and Qiu (2012) find that manager-firm fixed effects explain a significant portion of the variation in managerial compensation. In the M&A setting, Bao and Edmans (2011) show that a significant *advisor* fixed effect in acquirer gains exists, and Kaplan and Schoar (2005) find private equity house returns to be persistent.

Our study is also related to the strand of the literature that attempts to establish whether "managers matter" in corporate decisions and performance. Chang, Dasgupta, and Hilary (2010) provide evidence consistent with managerial effects explaining corporate performance. Kaplan, Sensoy, and Strömberg (2009) study the evolution of firms from early-stage to initial public offerings (IPOs) and conclude that investors bet on the business (the horse) rather than the management (the jockey). In contrast, Gompers, Kovner, Lerner, and Scharfstein (2010) find persistence in the success of serial entrepreneurs across their ventures. In the M&A context, Custódio and Metzger (2013) show that acquiring firm CEO's expertise in the target industry leads to better performance in diversifying acquisitions.

Finally, our paper is related to the M&A literature attempting to understand the distribution of acquirer returns. Notable examples are Moeller, Schlingemann, and Stulz (2005) and Fich, Nguyen, and Officer (2013), who examine large M&A losses and gains, respectively. Both papers conclude that episodes of extreme value creation/destruction are one-off aberrations. We contribute to this literature by exploring the fixed effects/persistence in acquirer returns more broadly, and our results suggest that takeover gains are characterized by significant firm-level persistence. Our findings also imply that, despite a large literature on acquirer returns, we still appear to be missing a major part of the puzzle, and that the elusive driver of takeover gains is likely to be found within, rather than outside, the firm. We discuss additional implications of our results for this literature and make suggestions for further research.

The remainder of the paper is organized as follows. Section 2 presents our data and establishes a highly significant firm fixed effect in acquirer returns. Section 3 confirms the persistence of acquirer returns over time. We examine the sources behind the acquirer fixed effect in Section 4. Section 5 discusses the key results and their implications. Finally, we close in Section 6 with some concluding remarks and suggestions for further study.

## 2. Sample and preliminary results

### 2.1. Sample selection

The M&A data are sourced from the Thomson Financial SDC US M&A database over the period from January 1, 1990 to December 31, 2011. We follow Fuller, Netter, and Stegemoller (2002) and Masulis, Wang, and Xie (2007) and impose the following restrictions:

1. The bidder must be a US publicly listed company, and the target must be a US public, private, or subsidiary firm.
2. The acquisition must be completed.[4]
3. The acquirer must own less than 50% of the target stock before the acquisition and achieve 100% after.
4. The transaction must be at least 1% of the acquirer's market capitalization 11 days before the announcement and also exceed $1 million.
5. The bidder's stock price data for 300 trading days prior to the announcement are available from Center for Research in Security Prices (CRSP), and accounting data for the year-end immediately prior to the announcement are available from Compustat.
6. Multiple deals announced by the same firm on the same day are excluded.

These requirements result in a sample of 12,491 transactions involving 4,128 unique firms. We use the standard event study methodology to compute the cumulative abnormal returns (CARs) of the sample acquirers over the event window $(-2, +2)$ around the announcement date.[5] The CARs are measured as returns in excess of those predicted by the market model with a benchmark being the CRSP value-weighted index and parameters estimated over a period from 300 to 91 days prior to the announcement. In the empirical tests that follow we work with three samples. The first sample consists of all deals and firms identified above (full sample hereafter). The other two samples comprise only acquirers having conducted multiple acquisitions to enable us to test persistence in returns. Our first subsample definition follows Fuller, Netter, and Stegemoller (2002) and requires at least five deals to be completed by the same acquirer within a three-year period (frequent acquirers hereafter). This definition reduces our sample quite dramatically and leaves us with 2,611 deals made by 333 unique acquirers. As an intermediate sample, we also define "occasional acquirers" as those which complete at least two deals in any three-year period (occasional acquirers hereafter). Under this definition we obtain 9,373 deals conducted by 2,219 unique acquirers.

Samples similar to ours have been extensively used in previous studies, so we refrain from presenting elaborate descriptive statistics but verify that they are in line with prior studies such as Masulis, Wang, and Xie (2007), Golubov, Petmezas, and Travlos (2012), and Harford, Humphery-Jenner, and Powell (2012). However, one noteworthy observation emerges. Restricting the sample to acquirers having completed at least two acquisitions in a three-year period (occasional acquirers) reduces the sample by less than 25%, from 12,491 to 9,373 deals. That is,

(*footnote continued*)
methodological issue regarding the use of the standard *F*-tests in assessing the joint significance of the estimated fixed effects. We address this issue below.

[4] We obtained qualitatively similar results when including unsuccessful bids.

[5] We also used CARs over event windows $(-1, +1)$ and $(-5, +5)$, as well as market-adjusted returns instead of the market model. All results remain unchanged.

**Table 1**
Benchmark OLS regressions of acquirer CARs.

This table presents the results of OLS regressions of acquirer CARs on acquirer and deal characteristics for the full sample as well as for occasional and frequent acquirer subsamples. The full sample includes all domestic M&A transactions completed during the period 1990–2011 from the Thomson Financial SDC M&A Database. Occasional acquirers are defined as those having completed two or more deals over a three-year window. Frequent acquirers are defined as those having completed at least five deals over a three-year window, consistent with Fuller, Netter, and Stegemoller (2002). The dependent variable in all the specifications is the cumulative abnormal returns of the acquiring firm stock over the event window $(-2, +2)$ surrounding the announcement date. The return is based on the market model with the benchmark being the CRSP value-weighted index. The *t*-statistics in parentheses are adjusted for heteroskedasticity. Symbols ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. The variables are defined in the Appendix.

| | Full sample | Occasional acquirers | Frequent acquirers |
|---|---|---|---|
| Intercept | 0.0321*** | 0.0162 | 0.0312** |
| | (2.9414) | (1.3952) | (2.2909) |
| Ln (acquirer size) | −0.0045*** | −0.0027*** | −0.0037*** |
| | (−5.4960) | (−3.3855) | (−3.0030) |
| Tobin's *Q* | −0.0022*** | −0.0021** | 0.0000 |
| | (−2.9659) | (−2.5763) | (0.0333) |
| Run-up | −0.0129*** | −0.0104*** | −0.0024 |
| | (−4.5124) | (−3.1982) | (−0.4417) |
| Free cash flow | −0.0124 | −0.0098 | 0.0083 |
| | (−1.3311) | (−0.9139) | (0.4399) |
| Leverage | 0.0169** | 0.0032 | 0.0085 |
| | (2.5231) | (0.4352) | (0.6784) |
| Sigma | 0.3500** | 0.3946*** | 0.0395 |
| | (2.3062) | (2.5953) | (0.1909) |
| Relative size | 0.0024 | 0.0159*** | 0.0088*** |
| | (1.5487) | (4.4645) | (2.8047) |
| Relatedness | −0.0003 | 0.0009 | −0.0048 |
| | (−0.1604) | (0.4496) | (−1.2725) |
| Tender offer | 0.0020 | 0.0022 | 0.0064 |
| | (0.3923) | (0.3626) | (0.6003) |
| Hostile | 0.0071 | −0.0148 | −0.0228 |
| | (0.5915) | (−1.2586) | (−0.9214) |
| Public × All-cash | −0.0028 | −0.0081** | −0.0128* |
| | (−0.7554) | (−2.0786) | (−1.8694) |
| Public × Stock | −0.0324*** | −0.0370*** | −0.0283*** |
| | (−12.2678) | (−12.5716) | (−5.8496) |
| Private × All-cash | −0.0041 | −0.0026 | −0.0011 |
| | (−1.5197) | (−0.8693) | (−0.2217) |
| Private × Stock | −0.0007 | −0.0002 | −0.0011 |
| | (−0.2586) | (−0.0761) | (−0.2089) |
| Subsidiary × All-cash | 0.0069*** | 0.0055* | −0.0012 |
| | (2.6064) | (1.8546) | (−0.2149) |
| Year FE | Yes | Yes | Yes |
| *N* | 12,491 | 9,373 | 2,611 |
| $R^2$ (Adj. $R^2$) | 0.057 (0.055) | 0.064 (0.060) | 0.050 (0.036) |

almost all deals are done by acquirers doing several deals and there is virtually no such thing as a one-off acquirer in a typical M&A sample found in most papers. The implication of this is two-fold. First, this structure of the data lends them well to the fixed effects analysis that we undertake below. Second, it suggests that M&A studies employing long-run abnormal stock returns or operating performance improvements (typically measured over three years following the deal) stand little chance of attributing the results to a particular deal (or deal characteristic) unless they exclude all frequent acquirers, which leaves only a small and, most likely, selected and unrepresentative sample.

### 2.2. First results

The first part of our empirical analysis is to a large extent modeled along the work of Bao and Edmans (2011) who examine investment bank fixed effects in M&A returns. We begin with a cross-sectional regression of acquirer CARs for the three samples to serve as our benchmark specification. We employ an extensive list of explanatory variables found in most recent and influential acquirer returns studies. Specifically, we follow Masulis, Wang, and Xie (2007), Golubov, Petmezas, and Travlos (2012), and Harford, Humphery-Jenner, and Powell (2012) and control for acquirer size, Tobin's *Q*, stock price run-up, idiosyncratic stock return volatility (sigma), free cash flow, and leverage. We also include deal-specific controls, namely, relative size, industry relatedness of the target, tender offer and hostile dummies, and a set of interactions between target listing status and the method of payment. We report our first results in Table 1. All variables are defined in the Appendix.

Most of the estimated coefficients are of the expected signs and consistent with prior studies although not always statistically significant. The most significant variables across all the three regressions are acquirer size and the interaction term of public targets and stock payment, which are both negatively associated with acquirer CARs. Tobin's *Q* and stock price run-up are consistently negative but significant only in the full and the occasional acquirer samples. The interaction term between public target and all cash deals is negatively associated with CARs even though it is significant in the serial acquirer subsamples only. Further we find a positive effect of sigma and relative size across all the three models although not always significant.

Most importantly, the $R^2$ (adjusted $R^2$) of these regressions are very modest, but comparable to those in prior studies (e.g., Moeller, Schlingemann, and Stulz, 2004; Masulis, Wang, and Xie, 2007; Harford, Humphery-Jenner, and Powell, 2012). All of the variables combined explain only 5–6% of the variation in acquirer returns. In short, a comprehensive regression model fails to capture the variation in acquirer returns in any major way. We now compare these results to a simple fixed effects model reported in Table 2.

We first focus our discussion on Panel A which employs the full sample, and then comment on the results for the occasional and frequent acquirer subsamples. Similar to Bertrand and Schoar (2003), we report *F*-statistics for test of the joint significance of the different sets of acquirer fixed effects for each of the three samples. Strikingly, a simple model with an acquirer fixed effect reported in the first row produces an $R^2$ (adjusted $R^2$) of 46% (19.4%). The fixed effects are highly jointly significant as evidenced by the *F*-statistic. Moving from the first row to the fourth, we first add the year fixed effects, followed by deal characteristics, and then by time-varying firm-level control variables to the basic fixed effects model. We find that the inclusion of these additional variables contributes only

**Table 2**
Acquirer fixed effects.

This table reports the joint significance of acquirer fixed effects (FE) in the regression model of acquirer CARs for the full sample (Panel A) and for occasional and frequent acquirer subsamples (Panels B and C, respectively). The full sample includes all domestic M&A transactions completed during the period 1990–2011 from the Thomson Financial SDC M&A Database. Occasional acquirers are defined as those having completed two or more deals over a three-year window. Frequent acquirers are defined as those having completed at least five deals over a three-year window, consistent with Fuller, Netter, and Stegemoller (2002). Acquirer CARs are regressed on acquirer fixed effects and the control variables specified in rows 1–4. Deal characteristics include relative size, relatedness, tender and hostile indicators, and full set of target listing status/payment method interactions. Acquirer characteristics include the natural logarithm of acquirer size, Tobin's Q, free cash flow, leverage, run-up, and sigma. *F*-statistics for the joint significance of acquirer fixed effects are reported, along with their corresponding *p*-values and the number of firms (in parentheses). The $R^2$ and the adjusted $R^2$ of the models are also shown. Symbols ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. The variables are defined in the Appendix.

| Row | Controls | Acquirer FE *F*-test | *N* | $R^2$ | Adj. $R^2$ |
|---|---|---|---|---|---|
| *Panel A: Full sample* | | | | | |
| (1) | None | 1.728*** (0.000,4128) | 12,491 | 0.460 | 0.194 |
| (2) | Year FE | 1.729*** (0.000,4128) | 12,491 | 0.464 | 0.197 |
| (3) | Deal chars., year FE | 1.725*** (0.000,4128) | 12,491 | 0.478 | 0.217 |
| (4) | Acquirer and deal chars., year FE | 1.692*** (0.000,4128) | 12,491 | 0.487 | 0.231 |
| *Panel B: Occasional acquirers* | | | | | |
| (1) | None | 1.368*** (0.000,2219) | 9373 | 0.298 | 0.080 |
| (2) | Year FE | 1.363*** (0.000,2219) | 9373 | 0.302 | 0.083 |
| (3) | Deal chars., year FE | 1.272*** (0.000,2219) | 9373 | 0.319 | 0.104 |
| (4) | Acquirer and deal chars., year FE | 1.287*** (0.000,2219) | 9373 | 0.332 | 0.120 |
| *Panel C: Frequent acquirers* | | | | | |
| (1) | None | 1.211*** (0.009,333) | 2611 | 0.150 | 0.026 |
| (2) | Year FE | 1.233*** (0.005,333) | 2611 | 0.165 | 0.035 |
| (3) | Deal chars., year FE | 1.101 (0.116,333) | 2611 | 0.178 | 0.046 |
| (4) | Acquirer and deal chars., year FE | 1.261*** (0.002,333) | 2611 | 0.199 | 0.068 |

modestly to the explanatory power of the basic model. Specifically, the $R^2$ (adjusted $R^2$) increases by only 2.7% (3.7%) as we move from the first row to the fourth. Moreover, the acquirer fixed effects remain highly statistically significant: the *F*-tests in all cases are significant at the 1% level leading us to reject the null hypothesis of no significant joint effects.

It should be noted at this stage that there are some instances of a single observation per firm in the full sample, so the explanatory power as indicated by $R^2$ figures above is overstated. We therefore replicate the fixed effect tests for the occasional and frequent acquirer subsamples and report the results in Panels B and C. In these subsamples each acquirer is found strictly more than once and, hence, the individual firm fixed effect coefficients—which we examine later—should be more precisely estimated. In all cases we find that the firm fixed effects are highly statistically significant (at the 1% level) with the only exception of model (3) in the frequent acquirers subsample. The $R^2$ (adjusted $R^2$) of the acquirer fixed effect models in the first rows of Panel B and C are 29.8% (8.0%) and 15.0% (2.6%), respectively. Moving from the first to the fourth row by adding year fixed effects, deal and acquirer characteristics increases the $R^2$ (adjusted $R^2$) by 3.4% (4.0%) and 4.9% (4.2%), respectively. As expected, the explanatory power of these models is lower due to their lower saturation with firm dummies (more data points per firm). That is, in Panel A, the ratio of unique firms to total number of acquisitions is 0.33, going down to 0.24 in Panel B, and further declining to 0.13 in Panel C, as we require each firm to have made more and more acquisitions. In other words, there is less firm heterogeneity to be picked up by the fixed effects in the more limited samples. However, even the most conservative adjusted $R^2$ figures (i.e., Panel C) are almost as high as those in our benchmark regression in Table 1. Thus, fixed effects *alone* explain almost as much or even more (depending on the sample) of the variation in acquirer returns than many of the important variables identified by prior literature *combined*.

So far we have established the statistical significance of acquirer fixed effects and to reinforce the substance of these results, we evaluate the economic magnitude of the fixed effects. In Table 3 we report the interquartile ranges of the estimated fixed effects for the three samples and four specifications ranging from fixed effects only to year fixed effects and a full set of deal and acquirer characteristics in that order.

As reported in Panel A, the interquartile range of the returns for the full sample is between 6.12% and 7.83%. For comparison, the interquartile range of the underlying dependent variable, acquirer CAR, is 7.58%. Note also that the model in row 4 includes a full set of controls, so that the estimated acquirer fixed effects do not reflect deal-specific features and time-varying, firm-specific characteristics. Our results show that acquirers are either extraordinarily good or bad irrespective of the deal structures they choose. Moreover, there is a wide gap in returns between very good and very bad acquirers. With the most conservative and rounded-down estimate of the interquartile range of 6%, moving from the 25th percentile to the 75$^{th}$ percentile results in a shareholder value gain of about $184 million ($28 million) at deal announcement for a mean-(median)-sized acquirer, which is highly economically important.[6]

While the interquartile range of acquirer fixed effects in the full sample should be interpreted with caution as the

[6] Mean (median) acquirer market capitalization is $3,067 million ($463 million).

**Table 3**
Distribution of acquirer fixed effects.

Panel A describes the distribution of the estimated acquirer fixed effects. Panel A presents the standard deviation, the 25th and 75th percentiles, the interquartile range, skewness, mean, and median of the estimated fixed effects in the full sample. Panels B and C repeat the same statistics for the occasional acquirers and frequent acquirers samples, respectively. The full sample includes all domestic M&A transactions completed during the period 1990–2011 from the Thomson Financial SDC M&A Database. Occasional acquirers are defined as those having completed two or more deals over a three-year window. Frequent acquirers are defined as those having completed at least five deals over a three-year window, consistent with Fuller, Netter, and Stegemoller (2002). Symbols ***, **, and * indicate that the mean/median fixed effect is significantly different from zero at the 1%, 5%, and 10% level, respectively. The variables are defined in the Appendix.

*Panel A: Full sample*

| | Standard deviation | 25th | 75th | Interquartile range | Skewness | Mean | Median |
|---|---|---|---|---|---|---|---|
| (1) | 9.50% | −3.33% | 2.98% | 6.31% | 5.06 | 0.45%*** | −0.48%*** |
| (2) | 9.50% | −3.30% | 2.91% | 6.21% | 5.06 | 0.44%*** | −0.53%*** |
| (3) | 11.98% | −3.28% | 2.84% | 6.12% | −13.59 | 0.12% | −0.41%*** |
| (4) | 10.86% | −4.70% | 3.13% | 7.83% | −5.73 | −0.68%*** | −0.58%*** |

*Panel B: Occasional acquirers*

| | Standard deviation | 25th | 75th | Interquartile range | Skewness | Mean | Median |
|---|---|---|---|---|---|---|---|
| (1) | 6.27% | −2.61% | 2.57% | 5.17% | 2.34 | 0.32%** | −0.29% |
| (2) | 6.24% | −2.61% | 2.53% | 5.14% | 2.36 | 0.33%** | −0.28% |
| (3) | 6.02% | −2.54% | 2.44% | 4.98% | 2.28 | 0.18% | −0.26%* |
| (4) | 6.46% | −3.80% | 3.12% | 6.92% | 1.01 | −0.40%*** | −0.24%*** |

*Panel C: Frequent acquirers*

| | Standard deviation | 25th | 75th | Interquartile range | Skewness | Mean | Median |
|---|---|---|---|---|---|---|---|
| (1) | 3.77% | −1.96% | 1.76% | 3.73% | 1.62 | 0.20% | −0.36% |
| (2) | 3.80% | −2.06% | 1.83% | 3.89% | 1.56 | 0.19% | −0.35% |
| (3) | 3.48% | −1.92% | 1.77% | 3.68% | 1.08 | 0.08% | −0.32% |
| (4) | 5.33% | −3.60% | 2.94% | 6.53% | 0.11 | −0.23% | −0.08% |

latter includes some instances of a single observation per firm, these magnitudes are confirmed in the occasional and frequent acquirer subsamples, where this is not a concern (Panels B and C, respectively). Depending on the set of controls, the interquartile range is between 4.98% and 6.92% for the occasional acquirer subsample, and between 3.68% and 6.53% for the frequent acquirer subsample. When year fixed effects, deal-, and time-varying firm-specific characteristics are included in row 4, the difference between the 75th and 25th percentiles is 6.92% and 6.53% for the occasional acquirers and frequent acquirers, respectively.

We also note that the distribution of the estimated fixed effects is somewhat positively skewed (except for models (3) and (4) in the full sample), and all medians are negative, albeit not always statistically significant, suggesting that there are slightly more firms with negative firm-specific components in acquirer returns.

### 2.3. Robustness of the acquirer fixed effect

Recently, Fee, Hadlock, and Pierce (2013) criticize the use of standard *F*-test procedures in establishing the joint significance of the estimated fixed effects. Replicating the analysis of Bertrand and Schoar (2003), they show that after scrambling the data and randomly assigning CEOs to firms—thereby destroying any CEO effect in the data—the standard *F*-tests and the associated *p*-values are hardly affected. This indicates a strong fixed effect even when none is present in the data by construction, casting doubt on the validity of inferences based on standard *F*-tests in this context. We take these concerns seriously and perform a similar data scrambling exercise to establish robustness of our main result.

Specifically, we follow Fee, Hadlock, and Pierce (2013) and break the structure of the data by randomly allocating deals to firms. We perform such Monte Carlo permutations of the data one thousand times, each time re-estimating the fixed effects models in Table 2 and recording the *F*-test on the joint significance of the firm fixed effect. If the firm-specific effect in acquirer returns is genuine, we would expect it to disappear when the deals are randomly allocated to firms. In all cases we find that the median *F*-test from the one thousand random permutations of the data loses statistical significance and does not reject the null of no significant firm-specific effects in acquirer returns.[7] This is relieving, as no firm effect is present in the data following the permutations. It turns out that, in our context, the standard *F*-test performs well, identifying a significant fixed effect when it appears to be present and failing to identify one when there is none by construction. Our later tests of the persistence in acquirer returns further alleviate the concerns that the fixed effect is spurious.

### 2.4. Time-invariant versus time-varying heterogeneity in acquirer returns

While the early capital structure literature has established significant firm fixed effects/persistence in

[7] The results are reported in Table A.1 in the Internet Appendix.

**Table 4**
Persistence of acquirer returns.

This table presents univariate tests of persistence in acquirer returns for occasional and frequent acquirers. The full sample includes all domestic M&A transactions completed during the period 1990–2011 from the Thomson Financial SDC M&A Database. Occasional acquirers are defined as those having completed two or more deals over a three-year window. Frequent acquirers are defined as those having completed at least five deals over a three-year window, consistent with Fuller, Netter, and Stegemoller (2002). In Panel A serial acquirers are sorted into quintiles based on their average CARs (denoted RET) over the last three calendar years. Q1 and Q5 represent serial acquirers with the lowest and highest past RET, respectively. The average CARs to acquisitions made by all the acquirers in Q1 and Q5 over the next $k$ calendar years are then computed, where $k=(1, 2, 3, 4, 5)$ and denoted as future RET. Panel B repeats the analysis where residual CARs obtained from regressions estimated in Table 1 are used to sort acquirers into performance quintiles (past RETRES) and to measure subsequent performance (future RETRES). The $t$-statistics for the differences in means between Q5 and Q1 are reported in parentheses. Symbols ***,**, and * denote significance at the 1%, 5%, and 10% level, respectively. The variables are defined in the Appendix.

*Panel A: Persistence in CARs*

| | Future RET measured over | | | | |
|---|---|---|---|---|---|
| Quintiles measured over 3 yr RET | 1 year | 2 years | 3 years | 4 years | 5 years |
| *Occasional acquirers* | | | | | |
| Q1 | 0.41% | −0.17% | −0.17% | −0.15% | −0.15% |
| Q5 | 0.72% | 0.90% | 0.91% | 1.16% | 1.22% |
| Q5–Q1 | 0.31% | 1.07% | 1.08% | 1.31% | 1.37% |
| | (0.63) | (2.73***) | (3.09***) | (4.17***) | (4.52***) |
| *Frequent acquirers* | | | | | |
| Q1 | 0.40% | −0.01% | 0.04% | 0.00% | −0.02% |
| Q5 | 0.43% | 0.50% | 0.69% | 0.98% | 1.06% |
| Q5–Q1 | 0.03% | 0.51% | 0.65% | 0.98% | 1.07% |
| | (0.06) | (1.20) | (1.66*) | (3.16***) | (3.63***) |

*Panel B: Persistence in residual CARs*

| | Future RETRES measured over | | | | |
|---|---|---|---|---|---|
| Quintiles measured over 3yr RETRES | 1 year | 2 years | 3 years | 4 years | 5 years |
| *Occasional acquirers* | | | | | |
| Q1 | −0.39% | −1.00% | −0.69% | −0.73% | −0.80% |
| Q5 | 0.52% | 0.25% | 0.13% | 0.36% | 0.54% |
| Q5–Q1 | 0.91% | 1.25% | 0.82% | 1.09% | 1.34% |
| | (1.58) | (2.69***) | (1.95*) | (2.93***) | (3.71***) |
| *Frequent acquirers* | | | | | |
| Q1 | −0.14% | −0.59% | −0.59% | −0.46% | −0.44% |
| Q5 | −0.19% | −0.15% | −0.25% | −0.20% | −0.19% |
| Q5–Q1 | −0.04% | 0.44% | 0.34% | 0.26% | 0.26% |
| | (−0.06) | (0.69) | (0.56) | (0.43) | (0.43) |

leverage ratios, more recent work by DeAngelo and Roll (2015) suggests that capital structures are much less stable than previously thought. They show that the similarity between leverage cross-sections is short-lived, with current relative leverage positions being poor predictors of future relative leverage positions five to ten years later. They also demonstrate that interactions of time and firm effects do a better job at explaining leverage than do firm effects alone.

Given those results, a natural question that arises is whether the firm-specific heterogeneity in acquirer returns we document is really time-invariant. Unfortunately, a dedicated analysis of this issue is complicated by two nuances of the takeover data. First, we do not have a long panel of takeover data with precise announcement dates. Given the evidence in DeAngelo and Roll (2015), a shorter panel means that we could be overstating the stability of the firm fixed effect. Second, unlike yearly leverage observations, takeover announcements are not evenly spaced in time, and, in most cases, do not occur every year for a given firm; other firms, in contrast, make several acquisitions per year.

With these limitations in mind, we rerun our models with firm-period interactions, where periods are defined as three (divides the sample roughly in seven), 5.5 (divides the sample in four), and 11 years (divides the sample in two). Results reported in the Internet Appendix (Table A.2) reveal that the additional explanatory power of such models as captured by the adjusted $R^2$ is economically modest, and in many cases, the $F$-test for the differences between the simple firm fixed effect and firm-period fixed effects models fails to reject the null of no additional explanatory power. These results suggest that firm-specific heterogeneity in acquirer returns is more fixed than time-varying.

## 3. Persistence of acquirer returns

The presence of a strong acquirer fixed effect implies that acquirer returns are persistent over time. In this section we perform formal tests of persistence in acquirer returns. Persistence tests explicitly require multiple acquisitions by all acquirers over time and as a consequence we restrict this part of the analysis to the occasional and

frequent acquirer subsamples. Our methodology here is similar to Jegadeesh and Titman (1993) for stocks, Carhart (1997) for mutual funds, and Bao and Edmans (2011) for investment bank advisors. We sort serial acquirers into quintiles based on their average CAR over the last three-year period (RET) consistent with our definitions. For each quintile, we compute the average RET to the future acquisitions made by all acquirers within that quintile over the next $k$ calendar years, where $k=(1, 2, 3, 4, 5)$. We then test for the difference in means between the top (Q5) and the bottom (Q1) quintiles.

Table 4, Panel A reports persistence in raw CARs and the results are consistent across the two subsamples. The differences in average CARs between Q5 and Q1 are positive and statistically significant from $k=2$ to $k=5$ for occasional acquirers, and from $k=3$ to $k=5$ for frequent acquirers. The lack of significant results for $k=1$ can be attributed to the small number of deals that are conducted within one year.

At this stage, it is still possible that persistence in acquirer returns is not really firm-specific but simply driven by firms routinely choosing (consciously or unconsciously) the types of acquisitions that are known to have superior performance (for example, private firm acquisitions, cash payment in public firm takeovers, etc.). While this strategy does benefit acquirer's shareholders, we are interested whether there is an effect *beyond* persistence in deal types (i.e., a genuine firm effect). We therefore repeat the persistence tests using residual CARs (RETRES)—defined as the average residual CAR obtained from the regressions in Table 1—and report the results in Panel B. This test ensures that the persistence in raw acquirer returns is not driven by the persistence in firm- or deal-specific characteristics, because RETRES is orthogonal to them by construction. There is persistence in residual CARs in the occasional acquirer sample but not in the frequent acquirer sample. This is consistent with firm effects having relatively less explanatory power than the usual controls in this subsample (recall Table 2). We will revisit this in the regression analysis that follows.[8]

It is remarkable to observe that the returns to the lower quintiles in each of the significant results cells are negative whereas they are positive for the upper quintile. These results provide additional evidence that certain acquirers are indeed extraordinary as they persistently generate positive average returns from their acquisitions.[9]

Next we perform multivariate versions of the persistence tests. Multivariate regressions allow us to use all firms and not just those in the top and bottom quintiles and thus establish whether the persistence is characteristic of all acquirers or just those at the extremes. Panel A of Table 5 reports the results of a simple ordinary least squares (OLS) regression of future returns (measured over one, two, three, four and five years) on past returns (measured over three years consistent with our definitions). Interestingly, this specification reveals no significant association between future and past returns of the same acquirer, apart from the last two columns where the future return is measured over four and five years. This suggests that the bulk of the persistence in returns as shown in Table 4 is, indeed, concentrated at the extremes, and there is little-to-no dependence of future returns on past returns *on average*.

Motivated by the significant univariate differences between the best and the worst acquirers and the lack of a strong significant association between future and past returns *on average*, we perform quantile regression analysis to further explore the persistence phenomenon.[10] We model the 20th and the 80th percentiles of the future returns distribution as a function of past returns of the same acquirer and report the results in Panels B and C of Table 5. The 20th and 80th percentiles are approximately consistent with the quintile tests. [11]

The results reported in Panel B show that past return is unable to explain future returns in the quantile regressions estimated at the 20th percentile point. Past RET is consistently insignificant in all the estimated models. However, Panel C reveals a very different and fascinating picture. We find a strong positive association between the past and future returns at the 80th percentile point of the future returns distribution. The results are highly robust across both occasional and frequent acquirer samples and across the time horizons over which future returns are measured: all coefficients are significant at the 1% level, except for $k=3$ and $k=5$ in the frequent acquirer sample which are significant at the 5% level. The intercepts are positive and significant at the top (the 80th percentile) and negative and significant at the bottom (the 20th percentile) of the distribution, consistent with our setup. These results continue to hold when residual returns (RETRES) instead of the raw returns are used in the regression tests of persistence (reported in Table A.4 in the Internet Appendix).

## 4. What explains the fixed effect?

Having established a significant acquirer fixed effect and its flip side, persistence in acquirer returns, we turn

[8] The persistence tests above for both RET and RETRES are based on equally weighted average acquirer returns. Such persistence could be misleading if an acquirer is good at conducting relatively small deals, but destroys a lot of value when it comes to large acquisitions. To rule the possibility of relatively small deals driving the averages out, we rerun the persistence tests using transaction value-weighted returns, where the weights are the ratios of deal value to the sum of transaction values of that acquirer over the period in which the performance is measured. The results (reported in Table A.3 of the Internet Appendix) are largely consistent with those based on equally weighted returns.

[9] An interesting question is why the bad acquirers continue to make acquisitions. This could be due to governance failures (e.g., weak board oversight, managerial entrenchment) or "honest" mistakes such as overconfidence.

[10] Whereas an OLS regression estimates the conditional mean function, quantile regressions allow for the estimation of the conditional $n$th percentile of the distribution as a function of the explanatory variable(s). The coefficients in a quantile regression are interpreted as the effect of a one-unit change in the explanatory variable on the $n$th percentile of the dependent variable.

[11] We also experiment with alternative percentile points. The 25$^{th}$ and 75$^{th}$ percentiles produce equally strong results, but the 10th and 90th percentiles do not.

**Table 5**
Regression analysis of persistence in acquirer returns.

This table presents the results of OLS and quantile regressions of future returns on past returns for occasional and frequent acquirers. The full sample includes all domestic M&A transactions completed during the period 1990–2011 from the Thomson Financial SDC M&A Database. Occasional acquirers are defined as those having completed two or more deals over a three-year window. Frequent acquirers are defined as those having completed at least five deals over a three-year window, consistent with Fuller, Netter, and Stegemoller (2002). Panels A, B, and C estimate the conditional mean (OLS), 20th percentile, and 80th percentile of the future returns distribution, respectively. The dependent variable is RET measured as the average CAR to all the acquisitions made by an acquirer over the next $k$ calendar years, where $k$=(1, 2, 3, 4, 5). The explanatory variable 'Past RET' is the average CAR to all acquisitions over the last three calendar years. For the OLS regressions the $t$-statistics in parentheses are adjusted for clustering by acquirer. Symbols ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. The variables are defined in the Appendix.

*Panel A: OLS regressions*

| | Future RET measured over | | | | |
|---|---|---|---|---|---|
| | 1 year | 2 years | 3 years | 4 years | 5 years |
| *Occasional acquirers* | | | | | |
| Intercept | −0.0012 | −0.0005 | 0.0008 | 0.001 | 0.0014 |
| | (−0.6569) | (−0.3582) | −0.5865 | −0.7336 | −1.0446 |
| Past RET | 0.0306 | 0.0452 | 0.0406 | 0.0532** | 0.0536** |
| | (0.6638) | (1.2655) | (1.4037) | (2.4065) | (2.5407) |
| $N$ | 3,209 | 4,215 | 4,687 | 4,854 | 4,975 |
| $R^2$ (Adj. $R^2$) | 0.001 (0.001) | 0.002 (0.002) | 0.002 (0.002) | 0.004 (0.004) | 0.005 (0.004) |
| *Frequent acquirers* | | | | | |
| Intercept | −0.0021 | −0.0019 | −0.0011 | −0.0012 | −0.0007 |
| | (−0.8880) | (−0.9560) | (−0.5897) | (−0.6514) | (−0.3669) |
| Past RET | −0.0275 | 0.0023 | 0.0018 | 0.037 | 0.0454 |
| | (−0.2920) | (0.0285) | (0.0288) | (0.9621) | (1.2731) |
| $N$ | 2,056 | 2,371 | 2,491 | 2,542 | 2,585 |
| $R^2$ (Adj. $R^2$) | 0.001 (0.000) | 0.000 (0.000) | 0.000 (0.000) | 0.002 (0.002) | 0.004 (0.003) |

*Panel B: 20th percentile*

| | Future RET measured over | | | | |
|---|---|---|---|---|---|
| | 1 year | 2 years | 3 years | 4 years | 5 years |
| *Occasional acquirers* | | | | | |
| Intercept | −0.0350*** | −0.0326*** | −0.0304*** | −0.0285*** | −0.0275*** |
| | (−23.2226) | (−25.8673) | (−37.6295) | (−33.7731) | (−37.2091) |
| Past RET | 0.0131 | 0.0091 | 0.0048 | 0.0058 | 0.0146 |
| | (0.6156) | (0.5224) | (0.4565) | (0.4862) | (1.4017) |
| $N$ | 3,209 | 4,215 | 4,687 | 4,854 | 4,975 |
| Pseudo $R^2$ | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| *Frequent acquirers* | | | | | |
| Intercept | −0.0310*** | −0.0287*** | −0.0274*** | −0.0266*** | −0.0262*** |
| | (−19.3509) | (−28.6226) | (−29.7629) | (−37.3366) | (−47.3677) |
| Past RET | 0.0129 | −0.0034 | −0.0015 | 0.0094 | 0.0138 |
| | (0.4072) | (−0.1905) | (−0.1045) | (0.8421) | (1.6089) |
| $N$ | 2,056 | 2,371 | 2,491 | 2,542 | 2,585 |
| Pseudo $R^2$ | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

*Panel C: 80th percentile*

| | Future RET measured over | | | | |
|---|---|---|---|---|---|
| | 1 year | 2 years | 3 years | 4 years | 5years |
| *Occasional acquirers* | | | | | |
| Intercept | 0.0327*** | 0.0324*** | 0.0322*** | 0.0305*** | 0.0305*** |
| | (21.0476) | (23.0026) | (29.5242) | (33.0408) | (33.8355) |
| Past RET | 0.0862*** | 0.1027*** | 0.0903*** | 0.0863*** | 0.0862*** |
| | (3.6178) | (4.6792) | (5.6280) | (6.4125) | (6.5128) |
| $N$ | 3,209 | 4,215 | 4,687 | 4,854 | 4,975 |
| Pseudo $R^2$ | 0.005 | 0.008 | 0.010 | 0.011 | 0.010 |
| *Frequent acquirers* | | | | | |
| Intercept | 0.0239*** | 0.0238*** | 0.0247*** | 0.0234*** | 0.0227*** |
| | (14.9072) | (18.2690) | (18.8849) | (18.9128) | (17.4635) |
| Past RET | 0.1007*** | 0.0817*** | 0.0619** | 0.0698*** | 0.0681** |
| | (3.4704) | (3.1849) | (2.3641) | (2.7933) | (2.5550) |
| $N$ | 2,056 | 2,371 | 2,491 | 2,542 | 2,585 |
| Pseudo $R^2$ | 0.007 | 0.006 | 0.005 | 0.007 | 0.007 |

our attention to the potential drivers of these phenomena. In other words, we are interested in the economic forces behind the statistical concept of fixed effects/persistence in returns. In Section 4.1 we test several plausible explanations, namely: (i) the CEO effect, (ii) attributes of the broader top management team, and (iii) the advisor effect. In Section 5 we also examine whether the firm effect stands-in for an industry effect, i.e., whether extraordinary acquirers simply come from the "right" industries. Finally, in Section 4.2 we discuss the more qualitative factors such as organizational knowledge/skill in deal-making, bidder-specific synergies, and path-dependence arising from success breeding success.

### 4.1. Formal tests

Existing literature stresses the role of CEOs in firm performance viewing them as the sole executives in charge of core corporate development activities such as acquisitions (e.g., Roll, 1986; Hartzell, Ofek, and Yermack, 2004; Billett and Qian, 2008; Aktas, de Bodt, and Roll, 2009, 2011, 2013). Since CEO turnover events are rare for most firms, it is possible that the firm effect is, in fact, the CEO effect. To disentangle the firm effect from the CEO effect one needs to estimate a model with both firm and CEO fixed effects. Naturally, for the CEO fixed effect to be identified separately one needs to observe a given CEO conducting deals in at least two different firms. Thus, not only a sample of CEO moves is required, but these moves have to be *between acquiring firms in our sample*. We are able to identify only 57 deals conducted by CEOs who can be found in at least two different acquiring firms in our full sample, with 42 and 14 observations in the occasional and frequent acquirer samples, respectively. This data limitation precludes any meaningful analysis in this regard. This is despite our best efforts to supplement the standard CEO data from Compustat's Execucomp—the usual source of data on corporate executives—with that from BoardEx whose coverage is broader.[12]

However, for the purpose of the persistence tests it is sufficient to focus on any CEO turnover event, not necessarily moves between acquiring firms. We therefore limit the sample to (i) acquirers experiencing a CEO turnover, and (ii) having conducted at least one deal within each of the three-year periods surrounding the CEO change (given that data are sparse, we do not require multiple deals to be conducted in each period). We are able to identify 104 such firms, of which 39 conducted multiple deals both before and after the turnover. We proceed to measure the average CAR and average residual CAR to the acquisitions performed by the firm in the three-year pre-turnover period (past *RET* or *RETRES*) and the three-year post-turnover period (future *RET* or *RETRES*). We then repeat the persistence regressions of Table 5. If the observed persistence in acquirer returns is solely attributable to the CEOs, we would expect to find no persistence in returns for deals conducted by the same firm but different CEOs. Table 6 reports the results.

Looking at the conditional mean, 20th percentile, and 80th percentile of the future raw return distribution, we do not find a statistically significant association with past returns, although the coefficient for the 80th percentile is positive. Turning to the residual returns, we find a significant association between past and future returns at the 80th percentiles, as well as in the OLS regression. The results based on residual returns ensure that the persistence (or lack thereof) is not driven by likely differences in deal structures under different CEOs, and hence should be preferred in the context of CEO turnovers. Overall, despite the CEO change (and despite the very small sample size) we continue to find some evidence of acquirer returns being persistent, and again mainly at the top end of the acquirer returns distribution. This finding suggests that persistence in acquirer returns is unlikely to be entirely attributed to CEOs.

Alternatively, the firm fixed effect could be picking up other slow-moving attributes of the managerial team. If this is the case, these variables should be strong predictors of acquirer returns. In fact, a singular focus on the CEO as the sole driver of acquisition decisions could be too narrow as it overlooks the interdependence among key executives in organizational structures. For instance, practitioners often emphasize the importance of managerial *teams* in making M&A deals a success or a failure. We therefore probe further into the managerial explanation for the observed firm fixed effects in acquirer returns by borrowing from the management science literature and identifying a set of variables characterizing the managerial team as a whole that has been found to affect various corporate outcomes.

The data on the top management team are extracted from Compustat's Execucomp database.[13] We define the top management team as executives with a listed title above the vice-president level reported in Execucomp as they constitute executives at the senior-most level. This is consistent with Chemmanur and Paeglis (2005) and Hambrick, Cho, and Chen (1996), among others. We take into account the dynamics in top management team over time by measuring all the variables at the end of the most recent fiscal year prior to the announcement date. We use several variables to capture different dimensions of the top management team characteristics across firms.

Our first measure is team size. The numerical strength of the team reflects the managerial resources available to the firm for it brings diversity to corporate decision-making in areas such as opportunity seeking and negotiations. Compared with small-sized teams, for example,

[12] We would like to thank Cláudia Custódio and Daniel Metzger for kindly sharing their extended Execucomp-BoardEx CEO data set used in Custódio and Metzger (2013).

[13] Execucomp collects up to nine executives from each company's annual proxy statement (Securities and Exchange Commission (SEC) form DEF14A) for a given year, and hence, cannot capture all of the company's top managers but for the purpose of our analysis this information is sufficient. According to the SEC DEF14A filing rules, a company is required to fully disclose information about compensations received by its most senior executives and directors. The executive officers named in a proxy statement are the most influential executives in the corporate decision-making process and should wield the greatest impact on acquisition strategies.

**Table 6**
Persistence of acquirer returns in CEO turnover firms.

This table presents the results of OLS and quantile regressions of future returns on past returns for the subset of firms experiencing CEO turnover events and having made a deal within three years before and after the CEO change. The full sample includes all domestic M&A transactions completed during the period 1990–2011 from the Thomson Financial SDC M&A Database. The dependent variable "Future RET" ("Future RETRES") is the average CAR (residual CAR) to all the acquisitions made by the acquirer within three years following the CEO turnover. The explanatory variable "Past RET" ("Past RETRES") is the average CAR (residual CAR) to all acquisitions made by the acquirer within three years prior to CEO turnover. For the OLS regressions the *t*-statistics in parentheses are adjusted for clustering by acquirer. Symbols ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. The variables are defined in the Appendix.

| Dependent: Future RET | OLS | 20th | 80th |
|---|---|---|---|
| Intercept | −0.0066 | −0.0306*** | 0.0237*** |
| | (−1.4514) | (−3.7932) | (3.5583) |
| Past RET | 0.0636 | 0.0544 | 0.1522 |
| | (1.1331) | (0.3875) | (1.3126) |
| *N* | 104 | 104 | 104 |
| $R^2$ (Adj. $R^2$) [Pseudo $R^2$] | 0.006 (−0.003) | [0.008] | [0.017] |
| Dependent: Future RETRES | OLS | 20th | 80th |
| Intercept | −0.0054 | −0.0244*** | 0.0245*** |
| | (−1.1705) | (−3.7716) | (4.2817) |
| Past RETRES | 0.1060** | 0.0671 | 0.1934* |
| | (2.0533) | (0.6050) | (1.9712) |
| *N* | 104 | 104 | 104 |
| $R^2$ (Adj. $R^2$) [Pseudo $R^2$] | 0.018 (0.008) | [0.013] | [0.017] |

large top management teams are able to enjoy a broader range of perspectives on a greater number of items, critical judgments, and alternative solutions for conducting comprehensive search and analysis of strategic options (Haleblian and Finkelstein, 1993). Such increased resources and capabilities can result in high-quality acquisition decisions and superior performance. Large top teams are, however, prone to conflicts and cooperation problems that would otherwise be absent in small groups (Jehn, 1995). Nevertheless, the complex, non-routine nature of M&As makes it possible that the benefits of enhanced capabilities accruing to large-sized teams would outweigh the costs associated with coordination problems (Haleblian and Finkelstein, 1993).

Another widely used indicator of top management team capability is team tenure. Prior evidence suggests that top team tenure is associated with persistence in strategic direction (Finkelstein and Hambrick, 1990). Higher average tenure can indicate greater cohesion and shared experiences in strategic decision-making. Consistent with Chemmanur and Paeglis (2005), top team tenure is calculated as the average number of years top team members have worked in the acquiring firm.

Long tenure, however, may create increasing rigidity and complacency in a team's interaction process. It is therefore critical for long-tenured teams to possess certain degrees of heterogeneity to offer new information sources and introduce new perspectives into the decision making. While disagreements are more likely to be present in heterogeneous teams, resolving such disagreements encourages team members to think carefully about the appropriateness of the proposed strategic solution. This is likely to initiate extensive investigations necessary for uncovering errors and producing sound evaluation results and corporate decisions (Miller, Burke, and Glick, 1998). In support of this view, prior studies show a positive link between top team heterogeneity and firm performance, suggesting that cognitive diversity is a valuable resource to a firm. Hambrick, Cho, and Chen (1996), for example, find that top management teams with greater tenure heterogeneity enjoy higher growth rates in both market share and profits. We define heterogeneity in team tenure as the coefficient of variation in team tenure.

The average age of the top management team is used as an additional proxy for the general experience of the top team members having worked within and outside the acquiring firm.

Finally, we consider the effect of a powerful CEO who can potentially make important corporate decisions on a stand-alone basis, disregarding other top managers' views. This could diminish any efficiency gains from a team, as team members may feel reluctant to participate, share information, or report ideas that run counter to the CEO. Following Finkelstein (1992), Hambrick and D'Aveni (1992), and Hayward and Hambrick (1997), among others, CEO power is measured using the pay differential between the CEO and other top managers, *defined as* salary plus bonus in the most recent fiscal year prior to the announcement date scaled by the average salary plus bonus of the other top management team members.

Our strategy is as follows. We first augment the fixed effects regression model in row 4 of Table 2 with managerial characteristics and examine whether these variables are significant determinants of acquirer returns. Results are reported in Panel A of Table 7. Surprisingly, none of the managerial characteristics explain acquirer returns in the full sample. We find, however, that tenure heterogeneity and average age are statistically significant in the frequent acquirers subsample whereas average tenure is positive and significant in the occasional acquirers subsample. Even though the top management team characteristics play a role in the occasional and frequent acquirer

**Table 7**
Top management team characteristics, acquirer CARs, and acquirer fixed effects.

Estimates reported in Panel A are from regressions of acquirer CARs on acquirer top management team characteristics and other controls listed in Table 1 for the full sample as well as for occasional and frequent acquirer subsamples. The full sample includes all domestic M&A transactions completed during the period 1990–2011 from the Thomson Financial SDC M&A Database. Occasional acquirers are defined as those having completed two or more deals over a three-year window. Frequent acquirers are defined as those having completed at least five deals over a three-year window, consistent with Fuller, Netter, and Stegemoller (2002). Only the coefficients on the top management team variables are reported, with the *t*-statistics in parentheses adjusted for heteroskedasticity. Panel B reports the joint significance of acquirer fixed effects (FE) in the regression model of acquirer CARs on acquirer fixed effects, year fixed effects, the deal and acquirer characteristics, and with and without the top management team characteristics. *F*-statistics for the joint significance of acquirer fixed effects are reported, along with their corresponding *p*-values and number of firms (in parentheses). The $R^2$ and the adjusted $R^2$ of the models are also shown. Panel C reports the distribution of acquirer fixed effects for the full sample as well as for the subsamples of occasional and frequent acquirers before and after the top management team variables are added. Team size is measured as the number of the acquiring firm's officers with a listed title above the vice president level in the most recent fiscal year prior to the announcement date. Average tenure represents the average number of years for which the top management team members have worked in the acquiring firm prior to the announcement date. Tenure heterogeneity is the coefficient of variation of the top management team members' tenures. The average age of top management team members is measured at the end of the most recent fiscal year prior to the announcement date. CEO dominance is calculated as CEO's salary and bonus divided by the average salary and bonus of other team members for the most recent fiscal year prior to the announcement date. Symbols ***, **, and * denote significance at the 1%, 5%, and 10% level, respectively. The variables are defined in the Appendix.

*Panel A: OLS regression of CARs on managerial characteristics*

| | Full sample | Occasional | Frequent |
|---|---|---|---|
| Team size | 0.0014 | 0.0011 | −0.0013 |
| | (0.9637) | (0.6726) | (−0.3973) |
| Average tenure | 0.0003 | 0.0005** | −0.0001 |
| | (1.6092) | (2.0604) | (−0.2055) |
| Tenure heterogeneity | 0.0057 | 0.007 | 0.0120* |
| | (1.4492) | (1.5921) | (1.8015) |
| Average age | 0.0002 | 0.0002 | 0.0020*** |
| | (0.4707) | (0.5961) | (2.8067) |
| CEO dominance | 0.0000 | −0.0001 | 0.0024 |
| | (−0.0069) | (−0.0940) | (1.4861) |
| Firm characteristics | Yes | Yes | Yes |
| Deal characteristics | Yes | Yes | Yes |
| *N* | 2,188 | 1,799 | 576 |
| $R^2$ (Adj. $R^2$) | 0.085 (0.068) | 0.097 (0.077) | 0.122 (0.062) |

*Panel B: Top management team characteristics and acquirer FE*

| | | Without managerial characteristics | | | With managerial characteristics | | | |
|---|---|---|---|---|---|---|---|---|
| | | Acq. FE *F*-test | $R^2$ | Adj. $R^2$ | Acq. FE *F*-test | $R^2$ | Adj. $R^2$ | *N* |
| (1) | Full | 1.133** (0.024,760) | 0.432 | 0.110 | 1.129**(0.028,760) | 0.433 | 0.109 | 2,188 |
| (2) | Occasional | 1.145** (0.032,509) | 0.380 | 0.113 | 1.143**(0.034,509) | 0.383 | 0.114 | 1,799 |
| (3) | Frequent | 1.473***(0.005,96) | 0.313 | 0.118 | 1.324**(0.033,96) | 0.316 | 0.112 | 576 |

*Panel C: Distribution of acquirer FE with top management team characteristics*

| | | Without managerial characteristics | | | | With managerial characteristics | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | SD | 25th | 75th | Interquar. range | SD | 25th | 75th | Interquar. range |
| (1) | Full | 5.95% | −2.63% | 2.75% | 5.38% | 5.97% | −2.83% | 2.72% | 5.55% |
| (2) | Occasional | 5.27% | −2.10% | 2.83% | 4.93% | 5.32% | −2.39% | 2.83% | 5.22% |
| (3) | Frequent | 4.22% | −2.33% | 2.67% | 5.00% | 4.24% | −2.79% | 2.59% | 5.38% |

subsamples, the results are, however, inconsistent across different samples.

Given that some of the managerial characteristics are important in some of the regressions, we further examine their contribution to the explanatory power of the models, and whether they reduce the statistical and economic magnitude of the acquirer fixed effects. Results are reported in Panels B and C of Table 7. Due to different sample composition, the acquirer fixed effects here are not directly comparable to those estimated in Table 2. We therefore compute acquirer fixed effects for the three samples without managerial characteristics and then repeat the estimation process by including the top management team variables. We hold the number of observations constant to facilitate comparisons. Essentially, the $R^2$ (adjusted $R^2$) do not change when we include the managerial characteristics. Similarly, the interquartile ranges reported in Panel C for the three models do not remarkably change with the inclusion of these variables. In short, we find that the addition of managerial characteristics variables does not detract from the statistical and economic significance of the fixed effects, suggesting that these characteristics are not behind acquirer fixed effects documented in this paper.

Alternatively, since there is persistence in the performance of M&A advisors (Bao and Edmans, 2011), firm-level persistence could, in principle, be generated by

retaining the "right" investment banks. However, this is an unlikely explanation given that acquirers tend to switch advisors from deal to deal quite frequently. For instance, Bao and Edmans (2011) report that only 21.4% of deals in their sample are advised by the same investment bank as all prior deals of the same acquirer in a five-year period. In unreported results, we verify that the inclusion of investment bank fixed effects does not detract from the economic and statistical significance of acquirer fixed effects. We also note that the economic magnitude of the firm-specific effect, as measured by the interquartile range in the estimated acquirer fixed effects, is several times larger than that of the advisor-specific effects documented by Bao and Edmans (2011).

### 4.2. Potential qualitative factors

Having ruled out several plausible explanations that were amenable to testing in a large sample study, we now discuss several qualitative factors that could explain the firm-specific heterogeneity in acquirer gains. While suggestive in nature, we believe these could be fruitful avenues of further research. The first possibility is that there is firm-specific acquisition skill. This can be due to organizational knowledge and/or particular processes with respect to acquisitions. It is difficult to pin down the exact source of this expertise, but we do offer some tentative conclusions based on anecdotal evidence. One possibility is that such expertise resides in the internal M&A/corporate development teams, who are charged with target screening, due diligence, and analysis underlying the acquisition decisions ultimately made by the top management. It is well-known that most large firms, and particularly frequent acquirers, maintain such in-house M&A teams.[14] This explanation implies that the best performers choose the right targets and/or pay the right price for them. Alternatively, persistently superior performance could reflect the market's expectation of particularly skillful post-merger integration. Again, anecdotes and case studies confirm that many frequent acquirers have developed and routinely follow procedures in this respect (see, e.g., Ashkenas, DeMonaco, and Francis, 1998). This explanation implies that the best performers extract the most value out of the acquired assets.[15]

The above two explanations imply firm-specific skill in deal-making. Alternatively, our results could be interpreted as evidence of bidder-specific synergies. For instance, it could be the firm's position in the production process or some unique assets that enable it to integrate and leverage the various acquisitions it makes. The social networking firm Facebook Inc. (FB) may be a good example. FB controls the user base and is, quite literally, a platform for other applications. Their recent acquisitions of the photo sharing service Instagram and the messaging service WhatsApp can be thought of as capitalizing on this position. It seems that FB is in a position to get the most out of these applications because of its commercial and technological centrality in the social media business. Smit and Moraitis (2010) express similar thoughts regarding Vodafone's acquisition strategy in the early 2000s. Generalizing from these examples, it is the nature of the firm's assets/business model that could be particularly well-suited for acquisitions.

Finally, persistence might not necessarily be evidence of skill. Rather, prior success could be breeding further successes. For example, if a firm pulls off a good deal, even if by pure luck (e.g., makes a low-ball offer which is accepted), its perceived reputation as a good deal-maker could help the firm next time around. For instance, the funding may be provided at more advantageous terms, key employees are more likely to stay on board, the target might not resist as much, etc. For instance, Hart and Holmstrom (2010) analyze Cisco and argue that its approach to treating the acquired firms and their managers created a reputation of a "well-liked" acquirer which the target firms did not resist. Similar sentiment is expressed towards Warren Buffett's approach to dealing with the acquired firms' management, which, together with its financial muscle, made Berkshire Hathaway a "go-to" acquirer.[16] In addition, the right acquisition can itself create the platform for future successful acquisitions, which ties this path-dependence argument back with the bidder-specific synergies story above. The key insight, though, is that persistence could be endogenous to prior successes while the latter may or may not be due to skill.

## 5. Discussion

In this section we discuss our findings in light of the broader literature and comment on their implications for the M&A research. The results we document are consistent with acquirers possessing acquisition skill. Jaffe, Pedersen, and Voetmann (2013) analyze skill differences in acquisitions by regressing acquirer returns in a given deal on the return of the same acquirer in its previous deal. They show that such a positive dependence exists, but only when the two deals are conducted by the same CEO (though noting that the latter finding could be due to low power arising from very few CEO moves in their sample). Our approach is much more general in that our econometric methodology allows us to study acquirer fixed effects in *all* deals by the same acquirer—be they prior to or even *after* the deal in question. A finding of persistent acquirer returns is broadly consistent with the findings of Kaplan and Schoar (2005) who document persistence in the returns of private equity (buyout and venture capital) funds, whose business is acquiring public and private firms. There also appears to be persistence in the performance of serial entrepreneurs

[14] For example, "AT&T calls on 'Deal Team,'" *The Wall Street Journal*, 14 May 2014; "The secrets of successful acquisitions," *The Wall Street Journal,* 22 September 2008; "The M&A forge," *The Wall Street Journal*, 16 January 2007; "Learn as you churn," *The Economist*, 6 April 2006.

[15] Note, however, that our models control for general performance indicators such as Tobin's *Q* and cash flows/equity, suggesting that extraordinary acquirers are not just generally better performing firms. Similar arguments can be made about the timing of deals.

[16] For example, "Going on safari with Warren Buffett," *Wall Street Journal*, 1 March 2011; "In deal hunt, big-game Buffett settles for small prey," *Wall Street Journal*, 8 August 2013.

funded by venture capitalists (Gompers, Kovner, Lerner, and Scharfstein, 2010).

Our results also add to the results of Fuller, Netter, and Stegemoller (2002), Billett and Qian (2008), and Aktas, de Bodt, and Roll (2011) who study serial acquirers and show that, for a given acquirer, performance declines from deal to deal. While not contradicting those findings, in this paper we show that some acquirers persistently perform above or below average and thereby generate or destroy value by doing deals. To get a sense of the shape of the acquisition skill distribution, Fig. 1, Panel A presents the frequency chart of the estimated acquirer fixed effects for the occasional acquirer sample, and Panel B does the same for the frequent acquirer sample. The distribution is reasonably symmetric. If one interprets the estimated fixed effects as firm-specific acquisition skill, there is a great degree of variation in acquisition ability. While most of the mass is naturally around the mean, there are also many extreme performers.

To shed more light on the extreme performers and their attributes, we examine the identity of the highest and lowest fixed effect acquirers. Table A.5 in the Internet Appendix provides information on the top ten and bottom ten acquirers sorted by acquirer fixed effects for the occasional and frequent acquirer subsamples (we do not perform this for the full sample as most of the extreme performers have conducted only one deal, and their fixed effects are less precisely estimated). We report the identity of the acquirer, the estimated fixed effect, the average CAR (RET), the average residual CAR (RETRES), and the acquirer's industry affiliation. Naturally, negative (positive) fixed effects are associated with negative (positive) CARs, though the relation is not monotonous given that the estimated fixed effects are after controlling for firm- and deal-specific characteristics. For example, in the frequent acquirer sample, AT&T Corporation has the second largest fixed effect of 15.52% with an average return of 1.16%, and Cisco Systems Inc. has a fixed effect of 9.94% but an average CAR of −1.05%. This pattern persists in the occasional serial acquirer subsample. Interesting observations emerge when we consider the industry affiliation of the best and the worst acquirers. Specifically, using Fama-French 48 industry groupings, we do not find a great deal of overlap in the industry classifications of the top ten and bottom ten acquirers. This could suggest that industry characteristics, which are subsumed by the firm fixed effects, may have a role to play. To establish whether certain industries are systematically associated with high/low fixed effects acquirers, we regress the estimated fixed effects on a set of Fama-French 48 industry dummies. We find that the industry dummies are jointly significant. However, the $R^2$ (adjusted $R^2$) of these regressions are only 1.67% (0.53%), 3.70% (1.66%), and 13.38% (2.52%) for the full, occasional, and frequent acquirer samples respectively, meaning that only a small fraction of the variation in acquirer fixed effects can be explained by industry affiliation.[17] These results further underpin the idea that forces that are unique to an organization, and not its environment or the top management team, determine extraordinary acquisition performance.

Taken together, our results have important implications for the M&A literature. Specifically, our results imply that despite the wealth of studies on M&A returns, the literature appears to have missed that the drivers of the variation in acquisitions gains are to a large extent firm-specific. Did 30 years of empirical M&A research get it wrong? We do not think this is the case. Existing studies on the determinants of takeover gains are highly informative. It is just that the effects they document do not appear to be first-order ones. For instance, Moeller, Schlingemann, and Stulz (2004) size effect in acquirer returns is attributed to greater agency problems in large firms. While one should be able to detect this governance quality in the cross-section of acquirer returns (as one does), it does not seem that these effects should be dominating. Similarly, many of the deal characteristics do not appear to be the major drivers of acquirer returns. For example, Travlos' (1987) method of payment effect in acquirer returns is attributed to the adverse selection of issuing equity, and, as such, is a manifestation of acquirer stand-alone value re-setting. But normally, we should not expect this stand-alone value revelation to dwarf the value implications of the deal itself (particularly for frequent acquirers, who regularly reveal private information about their value through payment method choices). Again, this is not to say that the effects identified by prior literature are unimportant. In fact, the size effect and the method of payment effect we pointed out here are among the most robust determinants of acquirer returns in various studies. Moreover, in the results reported in the Internet Appendix (Table A.6), we find that the effects of various determinants of acquirer returns used in Table 1 continue to remain significant in a firm fixed effects specification, where the identification is coming from within-firm variation in those variables. This suggests that prior findings on these determinants of acquirer returns are not simply capturing time-invariant firm-specific heterogeneity. Nevertheless, our message is that we are missing a much bigger piece of the puzzle, and that it appears to be firm-specific. We hope our findings will inspire further research in this direction.

## 6. Conclusion

In this paper we show that a large proportion of the variation in acquirer returns can be explained by a firm-specific, time-invariant factor. In fact, the explanatory power of the acquirer fixed effects matches and, in some cases, even overshadows that of many of the major firm- and deal-specific characteristics *combined*. Economically, there is a wedge of over 6% between the 25th and 75th percentiles of acquirer fixed effects, which is equivalent to $184 million ($28 million) in incremental shareholder value creation for a mean-(median)-sized acquirer at deal announcement. We further show that acquirer returns are persistent over time. Extraordinary good acquirers continue to make good acquisitions, while bad acquirers continue to perform poorly. Persistence in acquirer returns

[17] This conclusion is further substantiated by the fact the inclusion of industry fixed effects in the baseline regressions of Table 1 has a very modest effect on the explanatory power of those models.

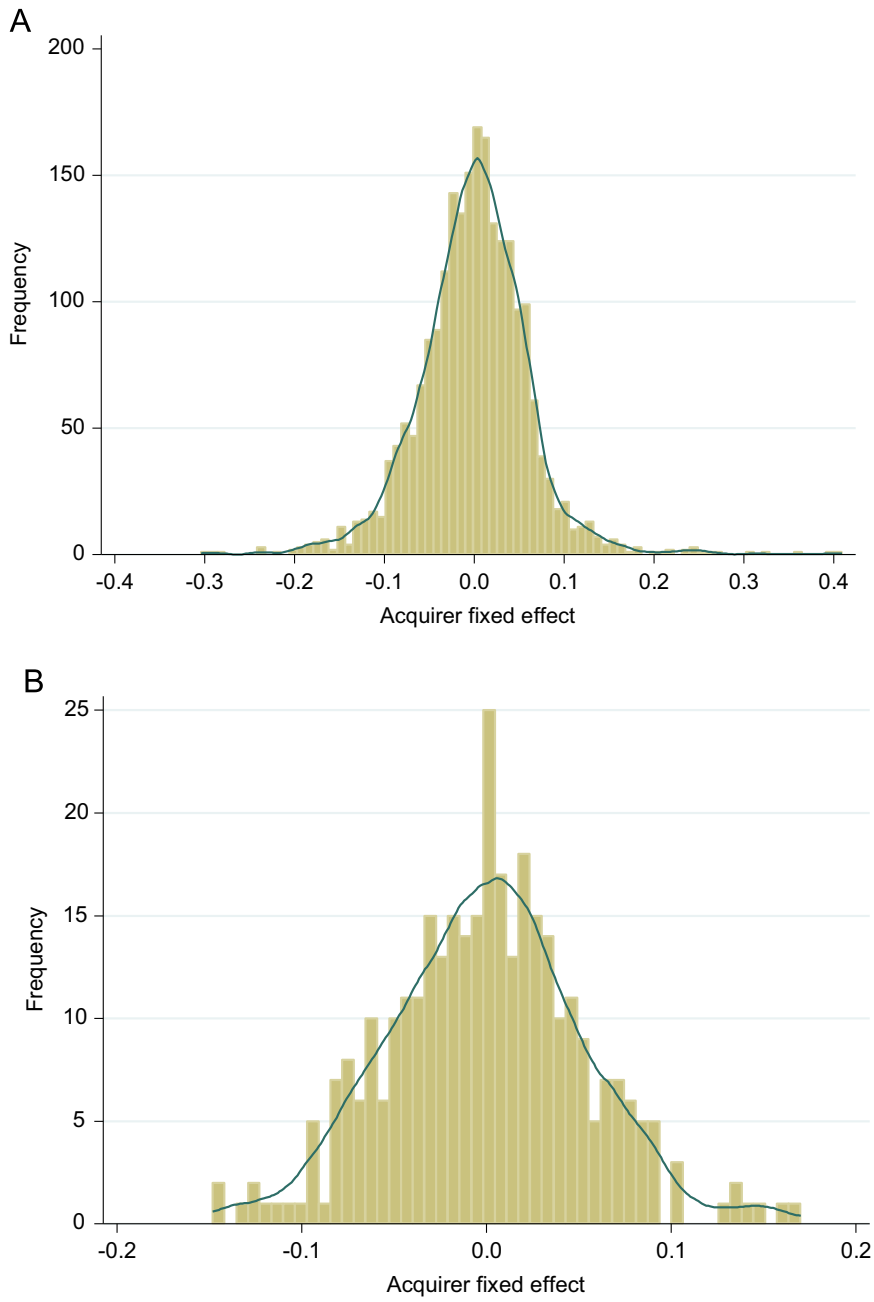


**Fig. 1.** Distribution of acquirer fixed effects. The figures depict the frequency distribution of the estimated acquirer fixed effect for the occasional acquirer (Panel A) and the frequent acquirer (Panel B) samples. The full sample includes all domestic M&A transactions completed during the period 1990–2011 from the Thomson Financial SDC M&A Database. Occasional acquirers are defined as those having completed two or more deals over a three-year window. Frequent acquirers are defined as those having completed at least five deals over a three-year window, consistent with Fuller, Netter, and Stegemoller (2002). The graphs are drawn using histograms and the kernel density estimation (curved line). Acquirer fixed effects are estimated using the regression model (4) of Table 2. Similar to Graham, Li, and Qiu (2012), the fixed effects are normalized so that the mean value is zero. This does not alter the shape of the distribution and its variance. In Panel A, an outlier (eMedSoft.com with a fixed effect of 76.07%) has been removed.

cannot be explained by firm and deal characteristics that shape these returns, as we continue to find persistence in the component of acquirer returns that is orthogonal to the known determinants.

We further examine the economic forces behind the statistical concept of acquirer fixed effects/persistence in acquirer returns. We investigate whether the fixed effects can be attributed to particular CEOs or the characteristics of the acquiring firm's broader management team. We find that acquirer returns continue to be persistent even under different CEOs. Further, various slow-moving attributes of the top management team found to be important in the management science literature do not explain the fixed effect away. Firm fixed effects are also independent of

investment bank advisor effects and are not capturing industry affiliation, reinforcing the idea that factors unique to a firm and not its environment drive the variation in acquirer returns.

Firm-specific heterogeneity in acquirer returns is consistent with several non-mutually exclusive explanations. First, it could reflect acquisition skill in the form of organizational knowledge or processes with respect to acquisitions. For instance, expertise residing in internal M&A/corporate development teams, or particular practices in terms of post-merger integration could be the elusive driver of acquisition performance. Second, our results can also reflect some bidder-specific synergies, for instance, derived from the nature of the firm's assets or its business model that are particularly well-suited for acquisitions. Finally, it is also possible that there is path-dependence in acquisition success, and persistence is generated endogenously whereby prior successes facilitate future acquisitions. We hope our findings inspire further investigation into these and other potential sources of persistent acquirer returns. A close-up examination of the best and worst acquirers that can be identified as part of our research design could serve as a potential starting point.

## Appendix. Variable definitions

| Variable | Definition |
|---|---|
| *Panel A: Return variables* | |
| CAR $(-2, +2)$ | Cumulative abnormal return of the acquiring firm stock over the event window $(-2, +2)$ surrounding the announcement date. The return is calculated using the market model with the benchmark being the CRSP value-weighted index. The model parameters are estimated over the $(-300, -91)$ period prior to the announcement. |
| Future RET | Average CAR $(-2, +2)$ to all the acquisitions made by an acquirer over the next $k$ calendar years, where $k=(1, 2, 3, 4, 5)$. |
| Past RET | Average CAR $(-2, +2)$ to all the acquisitions made by an acquirer over the last three calendar years. |
| RETRES | Average residual from an OLS regression of CAR specified in Table 1. |
| *Panel B: Acquirer characteristics* | |
| Acquirer size | The market value of the acquiring firm's equity 11 days before the announcement date in $US dollar million. The data are obtained from CRSP. |
| Tobin's $Q$ | Market value of the acquiring firm's assets divided by book value of its assets for the fiscal year prior to the acquisition. The market value of assets is equal to book value of assets plus market value of common stock minus book value of common stock minus balance sheet deferred taxes. The data are obtained from both CRSP and Compustat. |
| Leverage | The sum of the acquiring firm's long-term debt and short-term debt divided by the market value of its total assets measured at the end of the fiscal year prior to the acquisition. The data are obtained from both CRSP and Compustat. |
| Free cash flow | The acquiring firm's operating income before depreciation minus interest expense minus income tax plus changes in deferred taxes and investment tax credits minus dividends on both preferred and common share divided by its book value of total assets at the fiscal year-end before the announcement date from Compustat. |
| Sigma | Standard deviation of the market-adjusted daily returns of the acquirer's stock over a 200-day window $(-210, -11)$ from CRSP. |
| Run-up | Market-adjusted buy-and-hold return of the acquirer's stock over a 200-day window $(-210, -11)$ from CRSP. |
| *Panel C: Deal characteristics* | |
| Public | Indicator variable: one if the bid is for a public target and zero otherwise. |
| Private | Indicator variable: one if the bid is for a private target and zero otherwise. |
| Subsidiary | Indicator variable: one if the bid is for a subsidiary target and zero otherwise. |
| All cash | Indicator variable: one if the payment is pure cash and zero otherwise. |
| Stock | Indicator variable: one if the payment includes stock and zero otherwise. |
| Relative size | The deal value from Thomson Financial SDC divided by the market value of the bidding firm's equity 11 days prior to the announcement date from CRSP. |
| Relatedness | Indicator variable: one if the bidder and the target are operating in the same industries with a common two-digit Standard Industrial Classification (SIC) code and zero otherwise. Data from Thomson Financial SDC. |
| Hostile | Indicator variable: one if the deal is classified as 'hostile' by Thomson Financial SDC and zero otherwise. |
| Tender offer | Indicator variable: one if the deal is a tender offer and zero otherwise. Data from Thomson Financial SDC. |
| *Panel D: Management team characteristics* | |
| Team size | The size of the acquiring firm's top management team. It equals the number of officers with a listed title above the vice president level in the most recent fiscal year prior to the announcement date. Data from Execucomp. |
| CEO dominance | CEO salary and bonus divided by the average salary and bonus of other team members for the most recent fiscal year prior to the announcement date. Data from Execucomp. |
| Average tenure | The average number of years for which top team members have worked in the acquiring firm prior to the announcement date. Data from Execucomp. |
| Tenure heterogeneity | The coefficient of variation of the top team members' tenures. |
| Average age | The average age of the top team members measured at the end of the most recent fiscal year prior to the announcement date. Data from Execucomp. |

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