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THE (MISSING) RELATION BETWEEN ACQUISITION ANNOUNCEMENT RETURNS AND VALUE CREATION

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ABSTRACT

Cumulative abnormal returns (CAR) computed around acquisition announcements are widely considered to be market-based assessments of expected value creation. We show, however, that announcement returns do not correlate with commonly used and new measures of ex-post outcomes. A simple characteristics-based model using standard information known at the announcement date can predict these outcomes reasonably well, yet CAR even fails to capture the predictions from this model. Evidence suggests that information about the standalone acquirer dominates CAR, making it virtually impossible to extract deal-related information. We conclude that CAR is an unreliable measure of expected value creation.

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Cumulative abnormal returns (CAR) computed around acquisition announcements are overwhelmingly favored by financial economists to measure acquisitions' net present value (NPV), that is, the expected value created by the acquisition for the acquirer. Accordingly, positive announcement returns are interpreted by researchers as indicative of positive-NPV transactions and vice versa. Over the last five decades, CAR has been used to measure value creation in more than 92% of articles in top finance journals studying value creation in acquisitions.[1] The deep conviction in CAR can also be seen in business teaching and legal cases (Brealey et al., 2006; Brav and Heaton, 2015).

The fact that CAR became the status quo empirical measure for value creation is surprising, given disagreement about the underlying theory and mixed empirical evidence. Campbell et al. (1997) argue that CAR captures value creation in a well-functioning market: "...given rationality in the marketplace, the effect of an event will be reflected immediately in asset prices." Early empirical research in finance supports this view. For instance, Healy et al. (1992) link announcement returns to operating cash flow improvements based on a sample of 42 large acquisitions, and Kaplan and Weisbach (1992) show that announcement returns were lower for 37 transactions divested at aloss than for 71 transactions divested at a gain years after the initial announcement.[2]

However, many subsequent studies present evidence that CAR has imperfections. Some researchers, for example, show that CAR-based inferences about deal NPV can be distorted by anticipation, leakage, low completion probability, feedback, and price pressure.[3] For ex-

[1]Our review of articles in the *Journal of Finance*, *Journal of Financial Economics*, and *Review of Financial Studies* from 1972 to 2021 reveals that of the 4.8% of articles that focus on topics related to mergers and acquisitions (M&As), 54.8% compute measures of acquisition value creation. Of these, 92.2%—a total of 202 articles—use CAR to measure value creation. We further detect no declining trend in its use.

[2]Later small-scale European market studies find mixed evidence (Schoenberg, 2006; Papadakis and Thanos, 2010). Studies such as Jensen and Ruback (1983) and Bradley et al. (1988) document that mergers often generate net positive wealth gains overall, and Moeller et al. (2005) demonstrate that aggregated CARs provide insights into acquisition waves, showing substantial wealth effects for shareholders in large-scale deals.

[3]See Asquith et al. (1983), Jensen and Ruback (1983), Schipper and Thompson (1983a), Malatesta and Thompson (1985), Jarrell and Poulsen (1987), Eckbo et al. (1990b), Meulbroek (1992), Schwert (1996), Betton and Eckbo (2000), Bhattacharya et al. (2000), Song and Walkling (2000), Fuller et al. (2002), Mitchell et al. (2004), Bhagat et al. (2005), King et al. (2005), Luo (2005), Betton et al. (2008), Viswanathan and Wei (2008), Cai et al. (2011), Cornett et al. (2011), Edmans et al. (2012), Offenberg and Officer (2012),

ample, the well-documented run-up in stock prices prior to merger announcements could indicate that prices often incorporate a portion of the expected deal value before the event window used for CAR calculation, leading to an underestimation of synergies and a muted response at announcement (Malatesta and Thompson, 1985; Schwert, 1996; Cai et al., 2011). In addition, acquisitions by serial acquirers and those acquired via competitive bidding contests can influence the distribution of CARs over time and across parties, further complicating interpretation as value is incrementally revealed.[4] CAR may also contain information unrelated to value creation (e.g., earnings-per-share (EPS) accretion) and omit information known at the time of the announcement.[5] Prior studies provide evidence that CAR may also contain information about the standalone acquirer, for example, information related to overvaluation, strategy, managerial skill, managerial risk-aversion, and investment opportunities.[6] In their textbook, Grinblatt and Titman (2002) summarize this literature as follows: "The stock returns of the bidder at the time of the announcement of the bid may tell us more about how the market is reassessing the bidder's business than it does about the value of the acquisition." Despite the evidence that CAR may not contain information relevant to determine the value created in acquisitions, CAR has remained the primary tool of financial economists to assess the value created in acquisitions.

Our study aims to assess whether the distortions above are sufficiently small for CAR to serve as a good proxy for value creation, or whether they overwhelm the underlying information about NPV. Our results show that such distortions are pervasive and economically substantial.

To assess CAR's validity as a measure of value creation in acquisitions, we employ a comprehensive sample of over 47,000 acquisition announcements over almost four decades

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Betton et al. (2014), Edmans et al. (2015), Wang (2018), Bennett and Dam (2019), and Irani (2020).

[4]See Asquith et al. (1983), Schipper and Thompson (1983a), Fuller et al. (2002), Moeller et al. (2005), Billett and Qian (2008), Aktas et al. (2013), and Macias et al. (2016) for discussion on CARs for repeat acquirers.

[5]See Mitchell and Stafford (2000), Powell and Stark (2005), Malmendier et al. (2018), Dasgupta et al. (2024), and Ellahie et al. (2025).

[6]See Jensen and Ruback (1983), Schipper and Thompson (1983a), Roll (1986), Travlos (1987), Fishman (1989), Berkovitch and Narayanan (1990), Eckbo et al. (1990a), Chang (1998), Fuller et al. (2002), Hietala et al. (2003), Shleifer and Vishny (2003), Rhodes-Kropf and Viswanathan (2004), Rhodes-Kropf et al. (2005), Bhagat et al. (2005), Ang et al. (2006), Dong et al. (2006), Jacobsen (2014), Ben-David et al. (2015), Pan et al. (2016), Eckbo et al. (2018), and Gokkaya et al. (2024).

(1980 to 2018). In the first part of the paper, we rely on several widely used measures of ex-post value creation, and we also introduce novel measures. We find no meaningful correlation between these measures and announcement returns. Instead, they are predictable at the time of the announcement using standard deal information known at the announcement. However, CAR also does not even correlate with this component, which indicates that CAR fails to reflect relevant information at the time of the announcement. In the second part of the paper, we argue that the endogeneity of acquisition announcements is particularly problematic for inferences based on the $CAR = NPV$ assumption. We show that even under the most favorable conditions, CAR includes both information about the NPV arising from the transaction *and* non-NPV information related to the event triggering deal announcement. We demonstrate that assuming that CAR contains information strictly about deal-related NPV yields a high prevalence of economically unrealistic inferences. A formal empirical analysis of the second moment of CAR reveals that the non-NPV component related to the acquirer likely dominates the deal information contained in CAR. We conclude that CAR is an unreliable measure of value creation in acquisitions.

In our first set of empirical tests, we examine whether CAR aligns with observable ex-post transaction- and firm-level outcome measures. Value creation is unobservable, so we employ empirical measures—both established and novel—based on diverse data sources to capture various facets of acquisition success. At the transaction level, we construct an acquisition failure dummy that is equal to one if the deal is associated with goodwill impairments, that is, accounting write-offs indicating that the target is no longer worth its original price.[7] At the acquirer level, we employ both short- and long-term abnormal return on assets (ROA), measures commonly used in the literature (e.g., Healy et al., 1992; Harford and Li, 2007). Importantly, despite being derived from different sources and capturing both the left tail of the distribution and the entire distribution, these ex-post measures are significantly positively correlated with each other. Following the literature, we also consider whether managers "listen to" CAR and include completion (versus withdrawal) as an additional outcome

[7]Unlike other commonly used measures of performance, the goodwill impairment data are linked to specific transactions rather than the acquirer. In Section III of the Internet Appendix, we verify that goodwill impairment is a robust signal of value destruction by relating it to several indirect symptoms of failure: poor stock and operating performance, distressed delisting, and management turnover. Our measure is similar in spirit to that of Mitchell and Lehn (1990), Kaplan and Weisbach (1992), and Berger and Ofek (1996), who construct a transaction failure measure based on transactions for which targets are divested at a loss. However, the sample size used in these studies is small since, to determine merger success, the sample is limited to acquirers that disposed of their target in later years.

variable (Asquith et al., 1983; Jennings and Mazzeo, 1991; Luo, 2005; Kau et al., 2008).

We document that announcement returns are largely uncorrelated with nonimpairment and short- and long-term abnormal ROA. We find no meaningful correlation in univariate and multivariate settings, either in- or out-of-sample, across multiple announcement return windows and estimation techniques. Similar to Luo (2005), we find that announcement returns are significantly positively related to deal completion. However, the economic magnitudes of these effects are very small.

We find that CAR continues to fail to correlate with outcomes in simple subsamples based on different time periods and on an extensive number of acquirer characteristics (e.g., serial versus first-time bidders), target characteristics (e.g., public versus private), and transaction characteristics (e.g., cash versus stock)—even in subsamples where we expect CAR to perform better. We conduct a brute-force data-mining effort, searching among complex subsample formations to find the "golden subset" in which CAR consistently correlates with outcomes. We are unable to identify a group of transactions for which CAR is a reliable predictor of outcomes. We therefore conclude that CAR's unreliability is not limited to specific subsets of data or specific periods. In other words, the lack of correlation between CAR and deal outcomes appears to be systematic.

Given that CAR does not correlate with ex-post outcomes, we next explore its negligible correlation with acquisition outcomes by relating CAR to another ex-ante measure, namely, a simple benchmark that we construct using standard deal and acquirer characteristics available at the time of the announcement. While outcomes can be predicted reasonably well using deal and acquirer characteristics (both in-sample and out-of-sample), CAR performs poorly relative to this simple benchmark. In out-of-sample tests, we examine the link between CAR and expected acquisition outcomes, that is, outcomes predicted by characteristics known at the time of the announcement. Our results show that CAR does not correlate with this measure of expected outcomes, indicating that announcement returns do not reflect all relevant information publicly available at the time of the announcement.

We corroborate our inference of a wide disparity between the predictive ability of CAR and that of a characteristics-based model by linking predicted acquisition outcomes to long-term returns. We sort acquirers into deciles of predicted outcomes using CAR or the characteristics model (i.e., characteristics that predict more favorable acquisition outcomes, such as high abnormal ROA, for acquirers in top deciles) and compute returns for each decile. The characteristics model generates a large return spread: the performance spread in the five-year characteristics-adjusted cumulative buy-and-hold monthly returns (DGTW-adjusted BHAR used by Daniel et al., 1997) between the top and bottom three deciles ranges from 7.9% to 10.7%. In contrast, the return spread between the top and bottom three deciles as determined by CAR ranges from 0.8% to 2.8%. The link between acquisition outcomes predicted by characteristics and long-term returns further validates our value-creation proxies, providing additional evidence that announcement returns do not reflect all relevant information at the time of the announcement.

We next consider whether the lack of a material relation between acquisition outcomes and announcement returns can be explained by anticipation (the announcement is not a surprise), truncation (the component of CAR related to deal completion uncertainty), selection (completed deals are not a random sample of announced deals), or feedback (managers take action, such as canceling the deal or working harder, in response to negative or positive CAR). We shed light on the magnitude of these effects using three tests. First, we identify a sample of deals that are "explicitly" or "potentially" anticipated and find that these deals are not driving the lack of correlation between outcomes (e.g., nonimpairment, abnormal ROA) and CAR. Second, since completion outcomes can be predicted reasonably well out-of-sample using deal and acquirer characteristics, we next document that the lack of correlation between CAR and outcomes persists even for a sample of deals with a high likelihood of completion. Third, we consider the benefits of "listening" to CAR. We find that withdrawing (versus completing) negative-CAR deals and completing (versus withdrawing) positive-CAR deals generate a long-term return loss of −5%. In contrast, "listening" to the benchmark characteristics model generates a long-term return spread of more than 20%. We conclude that these four effects, while present, are unlikely to be the primary driver of the lack of correlation between CAR and outcomes.

Following the extensive body of literature that examines the "types" of transactions that create or destroy value, we consider how inferences are altered by the lack of association between announcement returns and ex-post outcomes. Our four ex-post outcomes are associated with similar deal and acquirer characteristics. Strikingly, we find no association (in terms of sign and relative importance) between the characteristics for which CAR predicts failure or success and those associated with ex-post failure or success. Using the four most common characteristics used in the literature (form of payment, the target's public status, acquirer size, and relative transaction size), we form 16 "clusters" of transactions and find minimal overlap in the performance of clusters based on CAR versus our ex-post outcomes. For example, the cluster considered to create the most value according to CAR has the poorest ex-post outcomes among the 16 clusters, and the cluster considered to destroy the most value according to CAR is associated with above-median ex-post outcomes. These results indicate that inferences generated from CAR regarding deal quality are unreliable.

In the final section, we argue that CAR fails to capture NPV-related information because it reflects significant acquirer-related factors unrelated to the deal's NPV. Specifically, CAR embeds information about the economic conditions prompting the acquisition, likely stemming from the endogenous nature of acquisition decisions. Firms engage in acquisitions in response to internal or external triggers, for example, a failure of an internal R&D project or a strategic decision to enter a new market. Therefore, the acquisition announcement updates investors' beliefs about the value of the standalone acquirer, in addition to providing NPV information about the deal.

We conjecture that CAR reflects two distinct signals: the deal's true NPV and a separate acquirer-specific signal (X), with the latter likely dominating. To illustrate this, we examine the statistical properties of CAR. Economically, one would expect the range of value-creation outcomes to scale with transaction size. Consider, for example, an acquirer undertaking two vastly different transactions: one with a ratio of 1:1,000 relative to its market capitalization, and another at 1:10. Intuitively, the smaller transaction should yield a narrower range of value-creation outcomes. However, empirically, the distribution of CAR appears almost invariant to transaction size. Figure 1 highlights this finding: CAR distributions for small deals (e.g., 1:1,000) closely mirror those of considerably larger deals (e.g., 1:100 or 1:10). To illustrate the economic implausibility, consider a \$1,000bn acquirer making two acquisitions—one \$100bn and the other \$1bn. According to the CAR distribution shown in Figure 1, the implied range of value creation for the larger \$100bn acquisition spans $-$\$54bn (10^{th} percentile) to \$77bn (90^{th} percentile). Surprisingly, the $100\times$ smaller \$1bn deal exhibits nearly the same CAR distribution, $-$\$40bn and \$47bn, implying an economically unreasonable value-creation range. These results strongly suggest that CAR predominantly captures acquirer-specific signals rather than the actual deal NPV.

We further show that ignoring the acquirer-related information leads to several implausible conclusions. Specifically, the assumption that $CAR = NPV$ yields unreasonably large positive or negative implied NPVs and fails to account for significant changes in the acquirer's value when deals are announced and subsequently withdrawn. These findings suggest that traditional methods of controlling for acquirer characteristics are insufficient because the acquirer-related information embedded in CAR is often idiosyncratic and not captured by standard firm-level variables.

Our empirical analysis demonstrates that acquirer-related information (X) dominates what CAR captures. Specifically, we find that variations in CAR are influenced by the acquirer's size and standalone information about the firm more than by the size or specifics of the deal. This dominance renders CAR a poor proxy for the deal's NPV.

To conclude, across multiple methodologies and samples, we find that CAR is an unreliable measure of NPV. It appears to be swamped by information unrelated to the value created by the deal itself, and as a result, researchers cannot extract the true deal NPV from

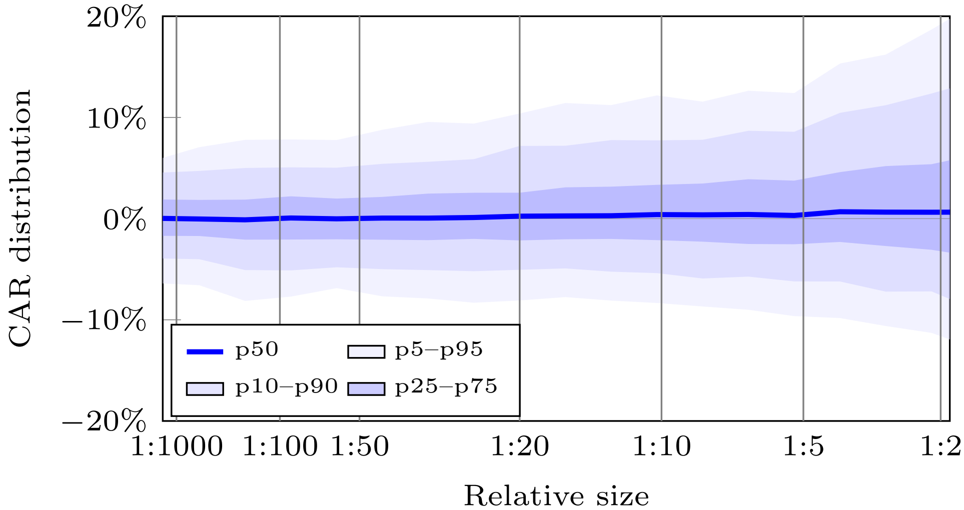

Figure 1. The distribution of CAR$[-1, 1]$ with respect to relative size. This figure shows the distribution of cumulative abnormal returns (CAR$[-1, 1]$) around acquisition announcements, plotted against the ratio of transaction value to the acquirer's market capitalization, for deals with ratios below 1:2. CAR distributions vary only modestly across different deal sizes, indicating that CAR is only very weakly related to relative transaction size.

CAR. Researchers should, therefore, reconsider economic inferences based on CAR. This insight underscores the need for alternative measures to assess acquisition outcomes more accurately.

The paper is organized as follows. In Section I, we describe the sample and outcome measures. In Section II, we test the ability of announcement returns to capture the acquisition outcome measures and in Section III, we discuss why CAR fails to reflect acquisition outcomes. Section IV concludes.

I. Sample and Outcome Measures

This section details the construction of our acquisition sample and the ex-ante and ex-post measures used to capture both transaction- and acquirer-level outcomes.

A. *Acquisition Sample Construction*

Our sample of mergers and acquisitions comes from the Thomson Reuters Securities Data Company (SDC) Domestic Merger and Acquisition database. The sample begins in 1980 and ends in 2018, which allows us to track acquisition outcomes over the five years following the transaction. We include transactions that satisfy the following criteria: (i) the merger or acquisition was announced on or after January 1, 1980, and was effective by

December 31, 2018, (ii) the acquirer is a U.S. company, (iii) the acquirer is a publicly traded firm, (iv) the deal is not classified as a leveraged buyout, spinoff, repurchase, self-tender, recapitalization, privatization, stake purchase, or acquisition of partial or remaining interest, (v) the percentage of shares acquired (or sought, for not-completed deals) is at least 50%, (vi) the percentage of shares held by the acquirer six months before the announcement is less than 50%, (vii) Compustat has accounting data on the bidder, and the Center for Research in Security Prices (CRSP) database has stock data for the month of the deal announcement, and (viii) the deal value is nonmissing in the SDC database. These requirements result in an initial sample of 47,543 deals, of which 42,354 are completed, 2,227 are withdrawn (the deal outcome is known in these cases), and 2,962 are neither completed nor withdrawn (e.g., the transaction may be pending or the outcome is unknown; we exclude these cases from the main analysis in Section II but include them in robustness tests and retain them in Section III). Internet Appendix Table IA.I provides a detailed summary of our sample construction and number of observations.[8]

B. Acquisition Performance Measures

For each transaction, we compute acquirer announcement returns. We estimate daily abnormal returns using a market model and a value-weighted index. The market model parameters, α_i and β_i, are estimated from 361 to 61 trading days before the deal announcement day, and r_{mt} is the CRSP value-weighted index. CARs are then computed by summing the daily abnormal returns over various event horizons. Following the literature (e.g., Betton et al., 2008), we estimate CARs over three days $[-1, 1]$ and 11 days $[-5, 5]$ surrounding each acquisition announcement. CAR may understate absolute value expectations if the probability of deal completion is uncertain. Thus, we also estimate "Deal CARs" over the entire acquisition process beginning two days before the deal announcement and ending two days following the deal completion $[\text{Announcement} - 2, \text{Close} + 2]$. The advantage of this longer window is that uncertainty regarding deal completion is resolved, but the disadvantage is that returns are measured over a long window and may include other acquirer-specific information. We therefore focus primarily on the short-term CAR measures.

We construct transaction- and firm-level proxies for acquisition outcomes to assess the core relation between acquisition announcement returns and value creation. Due to differences in data availability across outcome measures, the sample sizes vary for each measure. We provide further details on sample filters and the number of observations for the various outcome variables in Section I of the Internet Appendix.

[8]The Internet Appendix is available in the online version of the article on the *Journal of Finance* website.

B.1. Transaction-Level Ex-Post Measure: Goodwill Nonimpairment

Measuring the extent to which *specific* acquisitions create or destroy value for the acquiring firm is challenging since the target is typically merged into the acquiring entity. Normally, one cannot directly observe the target's ex-post performance or the synergies generated from the combined firms. We overcome this hurdle by focusing on goodwill impairment, as acquirers must write down the goodwill associated with a target when it declines in value. Following an accounting rule change in 2001 (SFAS-142), acquirers must evaluate goodwill balances more frequently and must provide more detail regarding transaction-level goodwill. We rely on this increased transparency in accounting rules for goodwill impairment to construct a new transaction-level measure of acquisition failure. Specifically, we construct a dummy indicating whether the goodwill associated with the transaction was materially impaired within five years of the deal's completion date.

We manually collect a sample of transactions with goodwill write-downs identified at the *transaction level*. These data offer a direct and quantifiable representation of ex-post value destruction in the acquiring firm for at least three reasons. First, goodwill, defined as the portion of the purchase price over the fair value of the target's identifiable net assets, reflects the going-concern value of the target, the value of expected synergies, and overpayment. The write-down of goodwill therefore reflects value destruction caused by the following factors: overvaluation of existing assets, overestimated synergies, or the inability to realize synergies due to firm, industry, or economy-wide shocks. Second, the quality of goodwill impairment data has improved in recent years. The Statement of Financial Accounting Standards (SFAS) 142, passed in 2001, was implemented with the intent that unsuccessful acquisitions would be reflected more precisely and quickly in firms' financial statements. After the completion of an acquisition, firms must conduct impairment tests following "material" events, and for many years in our sample, firms were required to conduct routine annual impairment tests to check for reductions in the value of goodwill.[9] The new standard also requires increased transparency for goodwill and impairment reporting at the reporting unit level rather than at the firm level, making it easier to link impairment to a specific triggering transaction. Third, prior research documents that goodwill impairment events are value-relevant (Henning and Stock, 1997; Bens et al., 2011; Chen et al., 2008; Gu and Lev, 2011; Li et al., 2011).

We provide evidence that goodwill impairment is a signal of value destruction by relating our impairment measure to several indirect symptoms of acquisition failure in Internet

[9] In September 2011, the Financial Accounting Standards Board (FASB) modified SFAS 142 such that formal valuations to produce comparisons of fair value and carrying value of a reporting unit are required only when certain qualitative indicators of impairment exist.

Appendix Section III. First, acquirers that impair goodwill are more likely to experience distressed delisting and poor operating and stock performance in the years following the acquisition relative to acquirers without impairment. Second, the market reaction to earnings announcements that contain goodwill impairment news is negative and large in magnitude, -2.8% on average.[10] Third, CEOs are more likely to be fired in the period surrounding goodwill impairments than following negative CARs surrounding the original acquisition announcements, indicating that the labor market regards impairment as an important signal for managerial discipline.[11]

One drawback of goodwill impairment as a measure of acquisition failure is the potential for subjectivity. Research documents managerial discretion in the write-down decision, mainly about the amount and timing of the impairment.[12] In this paper, we focus on substantial goodwill impairments, a setting in which strategic manipulation is less likely because extreme losses must be revealed at some point.[13] Further, we focus on a dummy for impairment, and thus, our results are less sensitive to the amount and timing of impairment.

Linking goodwill impairment to specific transactions is not straightforward because goodwill and impairment data reported on financial statements are based on *aggregate* firm-level data. As a result, we manually collect goodwill and impairment (if any) data. We identify all sample firms with firm-level goodwill impairments indicated in Compustat. For these "potentially" impaired transactions, we use the Notes to Consolidated Financial Statements in both the acquisition and impairment years to determine whether and how much of the impairment is due to the specific transaction in our sample. We focus on impairment within five years of the deal's effective date.[14]

[10]Note that impairment news is a strictly negative piece of news about an event that has already happened. The fact that the market reaction is negative, given this stale negative news, does not conflict with the main finding of the study that CAR is not associated with future changes in cash flows.

[11]Of course, there are settings in which impairment may not imply value destruction. For example, a target may be shut down once a target technology is exploited or a competitor is eliminated (e.g., Cunningham et al., 2021). Our results indicate that, in the vast majority of settings, goodwill impairment is associated with value destruction.

[12]See Elliott and Shaw (1988), Francis et al. (1996), Beatty and Weber (2006), Ramanna and Watts (2012), and Li and Sloan (2017).

[13]Our initial sample of potentially impaired deals requires firm-level impairment of at least 5% of acquirers' assets.

[14]To our knowledge, we are the first to construct a comprehensive data set that includes transaction-

For our analyses involving goodwill impairment, we impose additional filters on the 42,354 completed deals described in Internet Appendix Section I. First, we start our sample in 2003, when we can begin tracking goodwill impairment at the transaction level due to the implementation of SFAS 142 in 2002. Second, we require that the transaction value exceed \$10 million and that it be at least 5% of the acquirer's market capitalization at the end of the fiscal year before the deal is announced. This filter allows for a more precise measure of impairment: for very small deals (in both dollar and relative terms), it is difficult to determine the source of the impairment and, in many instances, the amount of goodwill originally produced from the transaction. These filters result in 8,367 deals. We next exclude deals that have missing or zero Compustat goodwill balances in both the year of and the year after the completion date, which yields 6,767 deals. Of these, we can reliably classify acquisition outcomes (transaction-level impairment or not) for 6,437 deals, of which 6,128 have the required announcement return and control variables.

Internet Appendix Table IA.III provides further details about the data collection procedure and shows that we successfully link impairment events to specific transactions. As reported in Internet Appendix Table IA.IV, goodwill impairments are relatively common: 14.8% of transactions in our sample experience an impairment event. The value lost from impairments is substantial: the average impairment constitutes 83% of transaction-level goodwill, 57% of the purchase price, and 11% of acquirer assets.

B.2. Firm-Level Ex-Post Measure: Abnormal Return on Assets

We follow existing studies that approximate the contribution of acquisitions to acquirers' cash flows by calculating their abnormal ROA (e.g., Healy et al., 1992; Chen et al., 2007; Fu et al., 2013). The idea is that the change in the acquirer's cash flows can be detected relative to both the acquirer's performance and the industry in the period preceding the acquisition.

We follow the procedure in Chen et al. (2007) and compute abnormal ROA over the years following the acquisition. To measure abnormal ROA, we regress the post-acquisition industry-adjusted n-year average ROA $(t+1, t+2, \ldots, t+n)$ on the corresponding pre-

specific goodwill balances and transaction-specific impairment outcomes in the post-SFAS 142 period. Hayn and Hughes (2006) also trace initial goodwill balances and subsequent impairments at the transaction level, but they exclude 55% of transactions due to insufficient information. Overall, they focus largely on the pre-SFAS 142 period, a time when the disclosure of initial goodwill and the source of the impairment was generally less comprehensive.

acquisition measure $(t-n,\ldots,t-2,t-1)$ and a constant,

$$\frac{1}{n}\sum_{t=1}^{n}[ROA_{i,t}-ROA_{Industry,t}]=\alpha+\beta\frac{1}{n}\sum_{t=-n}^{-1}[ROA_{i,t}-ROA_{Industry,t}]+\varepsilon_i, \tag{1}$$

where the residual ε_i measures the abnormal ROA.

For our "short-term abnormal ROA" measure, we define the post-acquisition (pre-acquisition) period as the three years after (before) the deal's effective date. We use three years as a plausible horizon because the median acquirer with goodwill impairment writes down by the third year following the acquisition. For our "long-term abnormal ROA" measure, we change the post-acquisition period to years four, five, and six after the deal's effective date. The longer horizon of up to six years is intended to capture synergy realization.

Industry definitions are based on the Fama-French 48 industries (Fama and French, 1997). As discussed in Chen et al. (2007), this model considers the possibility that pre-acquisition operating performance could predict post-acquisition operating performance. Because of data availability issues, we can compute short-term (long-term) abnormal ROA and the required control variables for 28,710 (22,577) transactions out of the 42,354 completed acquisitions. We provide further details on sample filters and the number of observations for the various outcome variables in Internet Appendix Table IA.II.

The acquirer-level abnormal ROA performance measure has both advantages and disadvantages relative to our transaction-level goodwill impairment dummy. The transaction-level deal success dummy (nonimpairment) is binary and captures extreme value loss (when the dummy is set to 0). In contrast, acquirer-level measures, like CAR or abnormal ROA, are continuous and may potentially capture nuanced outcomes. However, these measures can also be impacted by firm or market factors that are unrelated to the transaction.

B.3. Transaction-Level Ex-Post Measure: Deal Withdrawal

If announcement returns reflect expected value creation from the transaction, managers should utilize this signal to continue or cancel the acquisition. Following the literature, we construct a dummy variable for whether the deal was completed or withdrawn (e.g., Asquith et al., 1983; Jennings and Mazzeo, 1991; Luo, 2005; Kau et al., 2008).[15]

Earlier studies present mixed evidence about the correlation between CAR and withdrawal propensity. Jennings and Mazzeo (1991) find no such correlation, while Luo (2005)

[15] We include only completed and withdrawn deals in this analysis as the outcome is often uncertain for deals that neither completed nor formally withdrawn.

and Kau et al. (2008) find that deal withdrawal is more likely following negative CAR, particularly in settings where managerial "learning" is likely more important. We expand on these studies by including a large sample of 39,585 transactions (of completed and withdrawn deals with nonmissing control variables) in the period 1980 to 2018, and unlike previous studies (e.g., Luo, 2005), we include both public and private targets. We study the relation between CAR and withdrawal in-sample and out-of-sample and examine whether CAR fails to capture all information regarding withdrawal probability at announcement.[16]

The use of withdrawn/completed deals brings an additional benefit. We can now examine the dampening effects of "feedback"—managers withdraw the bid if CAR is too negative—on the core relation between CAR and outcomes. We discuss feedback in greater detail in Section II.

C. *Descriptive Statistics of Outcome Measures*

We report our sample summary statistics and correlations in Table I. We winsorize all continuous variables at the 1% level to reduce the effect of outliers. Panel A shows summary statistics for our acquisition performance measures. On average, 85% of transactions do not experience firm-level impairment. Of the subset of failed acquisitions, Internet Appendix Table IA.IV shows that 79% of impairments occur within the third year following the deal effective date, and the remaining 21% of impairments happen in the fourth and fifth year. The average acquirer has a small negative short-term abnormal ROA of −0.22% and long-term abnormal ROA of −0.21%. In our sample, 94% of transactions are completed and not withdrawn.[17] On average, announcement returns are positive in the immediate period surrounding the event, with three-day and 11-day CARs ranging from 0.83% to 0.88%. However, the cumulative return from the announcement to the deal-closing date (Deal CAR)

[16] Of course, some deal cancellation decisions are outside the control of the acquiring firm's management (e.g., transactions canceled due to regulatory pressure or a failed vote by target shareholders). Jacobsen (2014) shows that of withdrawn deals, 14% are canceled due to regulatory or judicial obstacles, and 13% are canceled due to target shareholders blocking or voting against the deal.

[17] This completion rate is slightly higher than the rates reported in Luo (2005), Kau et al. (2008), and Ellahie et al. (2025), who focus either on only public targets or on an earlier period. For example, in our sample, only 82% of public target deals are completed, whereas 97% of private target deals are completed. We find similar completion rates if we look at the sample period used in Luo (2005) and Kau et al. (2008), mostly in the 1990s.

Table I.

Descriptive Statistics of the Measures

This table reports descriptive statistics of the ex-ante and ex-post measures of acquisition quality. Panel A presents summary statistics, and Panel B shows correlations. All continuous variables are winsorized at the 1% level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A. Summary Statistics

	N	Mean	Std Dev	P10	P50	P90
Nonimpairment	6,128	0.852	0.355	0	1	1
Short-term abnormal ROA (ST abROA)	28,710	−0.002	0.082	−0.078	−0.006	0.091
Long-term abnormal ROA (LT abROA)	22,577	−0.002	0.091	−0.086	−0.007	0.097
Completion	39,585	0.944	0.230	1	1	1
$\text{CAR}[-1,1]$	28,710	0.009	0.066	−0.056	0.003	0.080
$\text{CAR}[-5,5]$	28,710	0.008	0.100	−0.099	0.002	0.122
$\text{DealCAR}[\text{A}-2,\text{C}+2]$	28,710	−0.011	0.199	−0.218	−0.001	0.183
DGTW-adjusted BHAR (Adj BHAR)	27,355	−0.096	1.225	−1.289	−0.293	1.214

Panel B. Correlations

	Non-impair	ST abROA	LT abROA	Comp-letion	CAR $[-1,1]$	CAR $[-5,5]$	DealCAR $[A-2,C+2]$	Adj BHAR
Nonimpairment	1							
ST abROA	0.133***	1						
LT abROA	0.127***	0.671***	1					
Completion	—	—	—	1				
$\text{CAR}[-1,1]$	−0.004	0.003	−0.010	0.015***	1			
$\text{CAR}[-5,5]$	0.002	−0.002	−0.005	0.015***	0.628***	1		
$\text{DealCAR}[\text{A}-2,\text{C}+2]$	0.047***	0.020***	0.009	—	0.354***	0.398***	1	
Adj BHAR	0.252***	0.221***	0.257***	0.018***	0.040***	0.070***	0.121***	1

is negative at −1.12%.[18]

We also measure characteristics-adjusted cumulative buy-and-hold monthly returns (DGTW-adjusted BHAR used by Daniel et al., 1997), in line with the literature on the long-term performance of acquirers (e.g., Mitchell and Stafford, 2000; Dong et al., 2006; Ben-David et al., 2015).[19] The average DGTW-adjusted BHAR over the 60 months beginning from the month before the deal's announcement is −9.6%. Since expected value creation/destruction from a particular deal is not observable, we use the multiple outcome measures above, which are derived from different sources (each with its strengths and weaknesses), and, importantly,

[18]Our three-day CAR estimate is similar to that of Betton et al. (2008), who document a mean three-day CAR of 0.73%.

[19]The DGTW adjustment procedure involves adjusting returns by the returns of benchmark portfolios based on characteristics. We form $5 \times 5 \times 5$ portfolios each month based on size, book-to-market, and 12-month past returns.

we find they are correlated. Table I, Panel B shows that the correlation coefficients across the four ex-post outcome variables range between 0.13 and 0.67. These correlations dramatically exceed correlations with CAR for each outcome variable. Correlation coefficients with three-day and 11-day CAR range from -0.01 to 0.02. Correlations between outcomes and Deal CAR, measured over a longer window, range from 0.01 to 0.05. We also find that our outcome variables are correlated with long-term returns: correlations between DGTW-adjusted BHAR and nonimpairment and the two abnormal ROA measures range from 0.22 and 0.26. In contrast, the correlations between DGTW-adjusted BHAR and three-day and 11-day CARs range from 0.04 to 0.07, and the correlation between DGTW-adjusted BHAR and CAR from announcement to close (Deal CAR) is 0.12. Indeed, the correlations between the three CAR definitions are also not strong in a relative sense: the correlation between three-day CAR and Deal CAR is only 0.35, whereas the correlation between short-term and long-term abnormal ROA is 0.67.

To summarize, across four ex-post acquisition outcome measures—transaction-level impairment, short-term and long-term abnormal ROA, and deal completion probability—we observe significant correlations but only very weak correlations between CAR and these acquisition outcome measures. We now turn to formal tests of these correlations.

II. Predicting Acquisition Outcomes

In this section, we test the ability of announcement returns to capture acquisition value creation by relating CAR to the observable ex-post measures described in Section I. We follow a multipronged approach to test our null hypothesis that CAR measures NPV, that is, the net present value of cash flows arising from the acquisition.

We first test the correlation between CAR and realized acquisition outcomes (nonimpairment, short- and long-term abnormal ROA, and completion). Because the measures capture realized rather than "expected" outcomes and because there is no clear guide for the level of correlation that establishes CAR as an adequate measure of NPV, we construct a simple benchmark measure using data also available at the time of the acquisition announcement, based on the standard set of deal and acquirer characteristics used in previous studies. We compare the forecasting ability of CAR and the benchmark characteristics model in-sample and out-of-sample, and across decades, industries, deal types, and acquirer characteristics.

In the second set of tests, we relate CAR to other "ex-ante" outcomes. Because our benchmark characteristics model correlates reasonably well with ex-post outcomes, we consider whether CAR captures other value-relevant information known at the time of the announcement by relating CAR to predicted outcomes based on the benchmark model. To further assess whether value-relevant information exists at the time of the announcement and to validate our acquisition value-creation proxies, we link predicted acquisition outcomes (by the benchmark characteristics model or by CAR) to long-term returns. We consider the return spread between acquirers expected to do well (e.g., the benchmark characteristics model or CAR predicts high ROA) and acquirers expected to do poorly.

In the third set of tests, we assess the impact of anticipation of deal announcement, uncertainty about deal completion, selection issues (due to nonrandom deal cancellation), and feedback effects (managers respond to CAR) on the core relation between CAR and outcomes.

Our final tests consider inferences generated by CAR relative to those generated by our benchmark measure on the "types" of deals associated with ex-post value creation. We consider whether the types of transactions (e.g., cash, private target) predicted to create or destroy value by CAR align with the types of transactions that do well ex-post and the types of transactions predicted to do well by our benchmark characteristics measure.

A. Visual Examination

We examine the unconditional relation between transaction- and acquirer-level outcomes and CAR. The implicit assumption behind using CAR to estimate value creation is that CAR is positively correlated with ex-post outcomes.

The results of the visual examination are presented in Figure 2. We sort CAR$[-1, 1]$ into 20 equally sized bins and present the related outcome statistics. In Panel A, we present the fraction of transactions without transaction-level impairment. The panel shows little correlation between the realized likelihood of nonimpairment and CAR: impairment outcomes vary little across CAR vigintiles.

Panels B and C show firm-level outcomes related to abnormal ROA. Panel B presents the relation between the average realized percentile of short-term abnormal ROA (percentiles within the sample) and CAR vigintiles, Panel C shows the relation between long-term abnormal ROA and CAR vigintiles, and Panel D presents the relation between the fraction of transactions completed (rather than withdrawn) and CAR. For the lowest CAR bins, completion rates are particularly low. However, for the remaining bins, there is little relation between completion rates and CAR, and even a reduction in completion rates for the very highest CAR bins.

Overall, Figure 2 shows no meaningful association between CAR and any of the four ex-post outcome measures.

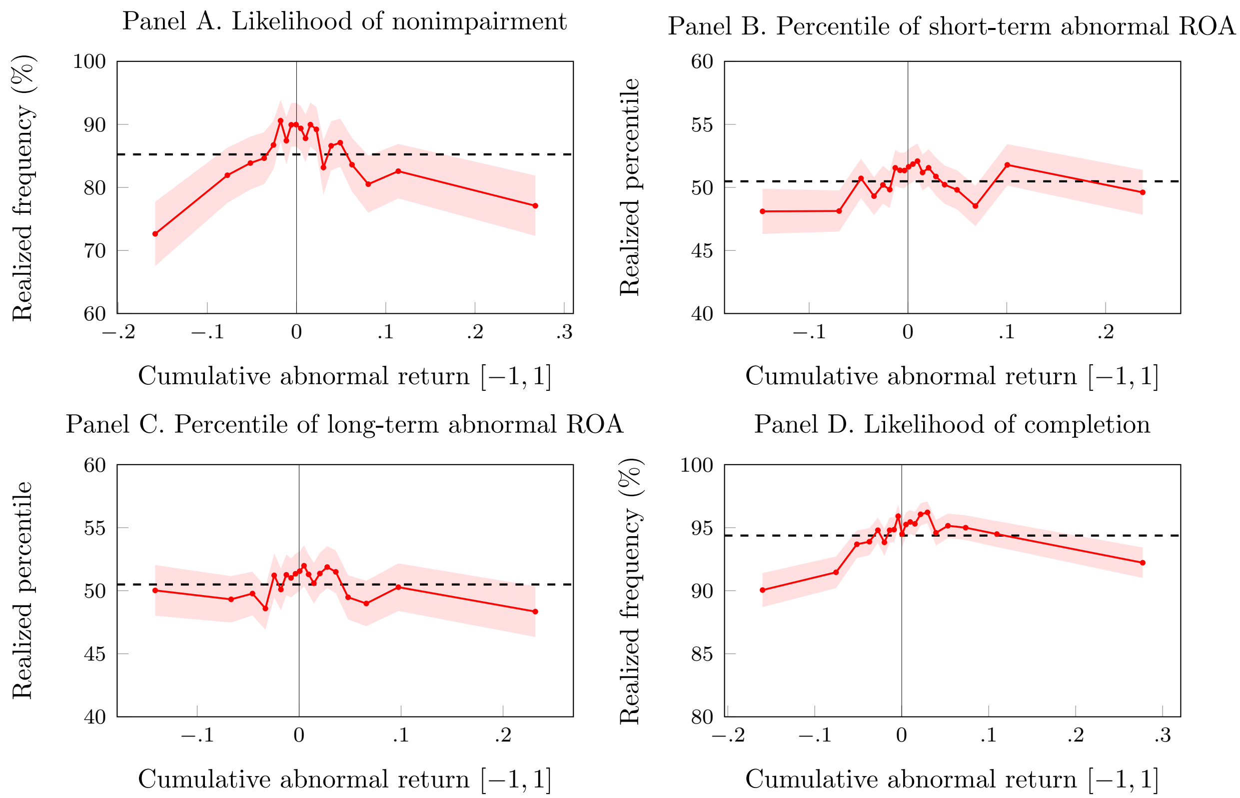

Figure 2. CAR$[-1,1]$ and ex-post outcomes.
Observations are sorted into 20 equally sized bins based on their CAR$[-1,1]$. Panel A plots the percentage of transactions without impairment for each acquirer's CAR$[-1,1]$ vigintile (solid red line). Panels B and C present the average realized percentile of short- and long-term abnormal ROA, respectively, and Panel D presents the realized frequency of completion for each vigintile of CAR. The light red shading indicates 95% confidence intervals. The horizontal black dashed line represents the unconditional realized frequency (Panels A and D) and unconditional realized percentile (Panels B and C) in our sample.

B. *In-Sample Tests: CAR versus Characteristics*

Next, we explore the correlation between the various outcome variables and CAR in a regression framework. Table II reports regressions with acquisition outcome measures as the dependent variables and acquirer CARs over multiple windows surrounding the deal announcement as the key independent variables of interest. Panel A reports results of ordinary least squares (OLS) regressions that model the probability of no goodwill impairment within five years of the deal's effective date. Panels B and C report results of OLS regressions with short- and long-term abnormal ROA as the dependent variable, respectively, and Panel D reports results of OLS regressions with the probability of completion as the dependent variable. Some regressions include the following acquirer and deal characteristics as controls: log market capitalization, leverage and free cash flow scaled by previous-year assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, and a

Table II.

Acquirer CAR and Acquisition Outcomes

This table presents regression results analyzing the relationship between acquisition outcomes and acquirer cumulative abnormal returns (CAR). The dependent variables include a nonimpairment dummy (Panel A), short-term abnormal ROA (Panel B), long-term abnormal ROA (Panel C), and a completion dummy (Panel D). Columns (1) to (3) use CAR as the sole independent variable. Column (4) adds firm characteristics, and column (5) incorporates characteristics as well as year and industry fixed effects. Column (6) includes characteristics only, and column (7) combines characteristics with year and industry fixed effects. The control variables include log market capitalization, leverage, and free cash flow scaled by lagged assets, Tobin's Q, prior-quarter market-adjusted stock returns, relative deal size, and dummy variables for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals. Firm-level characteristics are measured in the year prior to the deal announcement. Standard errors are shown in parentheses, and ***, **, and * denote statistical significance at the 1%, 5%, and 10% level, respectively.

CAR window:	$[-1,1]$	$[-5,5]$	$[\mathrm{A}-2,\mathrm{C}+2]$	$[-1,1]$		n.a.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Probability of Nonimpairment ($N = 6,128$, DV: Nonimpairment Dummy)							
CAR	−0.020	−0.008	0.089**	0.082	0.080	Controls	Controls
	(0.105)	(0.081)	(0.040)	(0.083)	(0.089)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	0.000	0.000	0.002	0.036	0.088	0.036	0.088
Panel B. Short-Term Abnormal ROA ($N = 28,710$, DV: Short-Term Abnormal ROA)							
CAR	0.004	−0.001	0.009**	0.019	0.019*	Controls	Controls
	(0.012)	(0.006)	(0.003)	(0.011)	(0.010)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	0.000	0.000	0.000	0.026	0.078	0.026	0.078
Panel C. Long-Term Abnormal ROA ($N = 22,577$, DV: Long-Term Abnormal ROA)							
CAR	−0.015	−0.004	0.004	0.001	−0.002	Controls	Controls
	(0.014)	(0.007)	(0.003)	(0.012)	(0.012)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	0.000	0.000	0.000	0.034	0.109	0.034	0.109
Panel D. Probability of Completion ($N = 39,585$, DV: Completion Dummy)							
CAR	0.048*	0.031*	—	0.050**	0.041**	Controls	Controls
	(0.023)	(0.014)		(0.019)	(0.016)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	0.000	0.000	—	0.147	0.153	0.147	0.153

set of dummy variables for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals.

Columns (1) to (3) of the first three panels of Table II show that the relationship between CAR and outcomes is weak. For nonimpairment and short-term and long-term abnormal ROA outcomes, three- and 11-day CARs are never statistically significant. CAR is statistically significant in two regressions when a longer CAR window is used. Still, the economic significance is low. For example, in column (3) of Panel A, for every one percentage point reduction in CAR, the probability of impairment increases by 0.09%. (Compared with the 14.80% unconditional probability of impairment for the sample, the point estimate represents less than a 1 basis point (bp) increase.)[20] Since the mean time to close a deal is 74 days, CAR measured over the period of announcement to completion includes, on average, more than 10 weeks for which other information (related to ROA or impairment likelihood) may be released.[21]

Column (4) of Table II adds characteristics known at the time of the announcement, and column (5) further saturates the model with year and industry fixed effects.[22] Again, in these two columns, across all six regressions in Panels A to C, CAR is statistically significant at the 10% level (and takes the correct sign) in only one regression. With the inclusion of controls, for short-term abnormal ROA, the adjusted R^2 increases from 0.00% (column (1)) to 2.6% when characteristics are added (column (4)) to 7.8% when characteristics and year and industry fixed effects are added (column (5)). In columns (6) and (7), we regress outcome variables on characteristics rather than CAR. For example, column (7) of Panels B and C shows that year and industry controls and deal and firm characteristics alone can explain 7.8% and 10.9% of the variation in short- and long-term abnormal ROA, respectively; adding CAR to the regression in column (5) does not improve the adjusted R^2, indicating that the explanatory power comes entirely from the controls and not from CAR.[23]

[20]In column (3) of Panel B, an increase in CAR from the 25$^{\text{th}}$ percentile to the 75$^{\text{th}}$ percentile leads to a 0.02-standard-deviation increase in short-term abnormal ROA.

[21]Part of the information about the expected value created by the acquisition may already be impounded in the price before the announcement due to leakage or anticipation of the acquisition (e.g., Schwert, 1996; Bhattacharya et al., 2000; Mitchell et al., 2004; Betton et al., 2008; Edmans et al., 2012; Offenberg and Officer, 2012; Wang, 2018; Bennett and Dam, 2019; Irani, 2020). In Internet Appendix Table IA.V, we follow Schipper and Thompson (1983b) and extend the CAR measurement period to begin 41 days before the announcement and end one day following the announcement. The results show that extending the window does not change our inference about CAR's lack of predictive ability.

[22]We consider the standard characteristics used in the M&A literature: log market capitalization, leverage, free cash flow scaled by previous-year assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, and dummies for stock-only consideration, mixed payment, diversifying acquisition, hostile deal, competing bidders, and public targets.

[23]In Internet Appendix Table IA.VI, we show that the results are robust to using two alternative definitions

Panel D of Table II reports results of regressions of deal completion on acquirer CAR. Similar to the results reported in Luo (2005) and Kau et al. (2008), we find that CAR correlates with completion outcomes: the coefficient takes the correct sign and is significant at the 10% level for the majority of specifications. The results indicate that some managers respond to signals generated by CAR. However, CAR has little economic significance: focusing on column (4), which has the largest point estimate in the panel, we find that for every one percentage point reduction in CAR, the probability of withdrawal increases by 0.05%. Compared with the 94.4% unconditional probability of completion for the sample, the point estimate represents less than a 1 bp increase.[24]

To summarize, our in-sample tests show that CAR is uninformative about acquisition outcomes. In most specifications, the relation between CAR and the acquisition outcome is not statistically different from zero. When CAR is statistically significant, the economic magnitude of CAR's explanatory power is weak. Further, CAR does not seem to provide additional information related to deal value creation over and above the information contained in deal and firm characteristics. In Section II.C.4, we formally assess whether deal and firm characteristics, also known at the time of the deal announcement date, dominate CAR as predictors.[25]

We also test whether CAR is better at predicting short-term or long-run outcomes, for example, impairment within the first year as opposed to within five years, and abnormal ROA the year following the completion date versus abnormal ROA five years following the completion date. We rerun the earlier regressions (as in Table II) but define the dependent variable as the outcome within a particular period relative to the deal's effective date (up to

of nonimpairment that classify transactions that lack information as not impaired (Panel A) or impaired (Panel B) and to using industry-adjusted ROA rather than abnormal ROA (Panel C).

[24]In Panel D of Internet Appendix Table IA.VI, we include deals that still may be pending or for which the outcome is unknown as the outcome variable. We find that the relation between CAR and completion is no longer statistically significant.

[25]In Table II, short- and long-term abnormal ROA are measured three and six years following the deal close, respectively. Due to the potential effects of firm attrition, in Internet Appendix Table IA.VII, we adjust the abnormal ROA computation and calculate abnormal ROA by taking all years with ROA data available up to six years after acquisition close. We then carry out weighted least squares (WLS) regressions of abnormal ROA on acquirer cumulative abnormal returns (CAR), where the number of years of available ROA data is used as the weight. The results presented in Internet Appendix Table IA.VII are similar to the main findings reported in Panels B and C of Table II.

five years) for nonimpairment and abnormal ROA, and relative to the announcement date for completion. In Internet Appendix Figure IA.1, we plot the coefficients on CAR for each year.

Panels A and B of Internet Appendix Figure IA.1 show that CAR performs better on some short-term outcomes. Specifically, CAR's coefficient is statistically significant when considering one-year nonimpairment and one- and two-year abnormal ROA. Yet the practical impact of the coefficients remains minimal: a one-standard-deviation shift in CAR (7.2% and 6.6% for Panels A and B, respectively) correlates with a minute increase in the short-term probability of nonimpairment of 0.02 standard deviations, and a similar increase in abnormal ROA of 0.03 standard deviations.

These results show that even though CAR performs better for short-term outcomes, it is still an ineffective predictor of value creation. In particular, CAR's meager economic significance and lack of explanatory power for short-term outcomes render it an uninformative indicator of value creation relative to the explanatory power of standard deal and acquirer characteristics available at the time of the announcement.

Thus far we have examined whether acquirer announcement returns can detect ex-post value creation. We now assess whether the combined returns of the target and acquirer, which reflect total expected synergy gains (as opposed to the division of synergy gains), can predict outcomes. We zoom in on the subsample of transactions with public targets (which represents 15% to 18% of the nonimpairment, abnormal ROA, and completion samples) and compute combined dollar gains by summing the product of acquirer CAR and acquirer market capitalization in the year prior to the deal announcement date and the product of target CAR and target market capitalization in the year prior to the deal announcement. We compute combined percentage returns by dividing combined dollar gains by the sum of the acquirer and target market capitalizations. The results for combined CAR$[-1, 1]$ are reported in Internet Appendix Table IA.VIII. The results are similar to those reported in Table II: the coefficient on combined CAR is not statistically significant when nonimpairment and short- and long-term abnormal ROA are the outcome variables while it is statistically significant when completion is the outcome variable, but again with low economic significance.

C. In-Sample Tests: By Subsample

Given that CAR has no material explanatory power over outcomes in the universe of acquisition announcements, we try to find the "golden subset," that is, a subsample in which CAR has a stronger correlation with acquisition outcomes.

C.1. Subsamples by Time Periods

Figure 3, Panels A to D show the coefficient and 95% confidence intervals for regressions of outcomes on CAR based on the specification in Table II, column (5), for each of the four decades in our sample. Panel A shows that the coefficient on CAR in regressions of nonimpairment on CAR is insignificant in the 2003 to 2010 and 2011 to 2018 periods. Panel B shows that when short-term abnormal ROA is the outcome variable, CAR is significant (and takes the correct sign) at the 5% level for the 1980 to 1990 period, but it is not statistically significant (and in some periods has the wrong sign) in the 1991 to 2000, 2001 to 2010, and 2011 to 2018 periods. Panel C shows that when long-term abnormal ROA is the outcome variable, CAR is not statistically significant (and in two periods has the wrong sign) for all four subperiods. Similarly, Panel D shows that CAR correlates with completion for only one of the four subperiods. This result contrasts with the statistically significant (at the 5% level) and positive (albeit economically weak) relation between CAR and completion reported in Table II.

C.2. Subsamples by Industries

We also split the sample by industries. Internet Appendix Figure IA.2 replicates Table II, column (5), by Fama-French 12-industry classifications and reports the coefficients on CAR and 95% confidence intervals. Across 48 regressions (four outcome variables $\times$ 12 industries), the coefficient on CAR takes the correct sign and is statistically significant at the 5% level for only four regressions. Although CAR correlates with some outcomes in a few select industries, importantly, for these select industries, CAR does not correlate with all outcomes. Like the time period results, the correlation between CAR and completion rates (Panel D) has the correct sign and is statistically significant for only one of the 12 industries. This result again indicates that the relationship between the withdrawal decision and CAR is economically weak.

C.3. Subsamples by Deal and Acquirer Characteristics

We further consider whether a particular set of deal or acquirer firm characteristics drives the lack of relation between outcomes and CAR. For example, existing literature discusses anticipation (e.g., serial acquirers), new information on acquiring firm valuation (e.g., stock or diversifying deals), difficulty in assessing value creation due to lack of information (e.g., private and high-tech targets, and small deals), or price pressure from merger arbitrageurs (e.g., public targets) as potential explanations for the lack of a relation.

In Internet Appendix Table IA.IX, we replicate Table II, column (5), for 29 subsamples

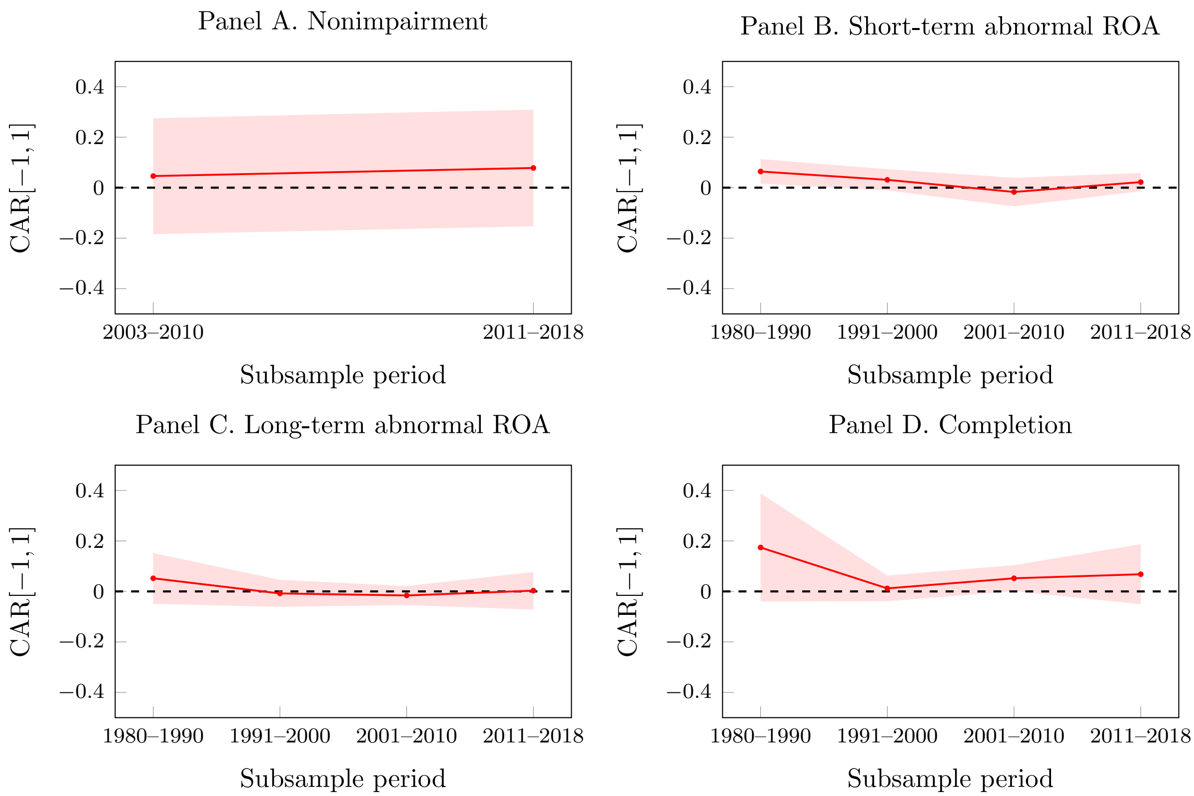

Figure 3. Acquirer CAR and acquisition outcomes: By decade.
This figure plots coefficients and 95% confidence intervals for regressions of outcomes on CAR based on the specification in Table II, column (5), for each of the four decades in our sample (except for nonimpairment, which we can determine for only two decades due to data limitations). Panels A, B, C, and D use nonimpairment, short-term abnormal ROA, long-term abnormal ROA, and completion, respectively, as the key dependent variables. The red dots represent the point estimates, and the light red shading represents 95% confidence intervals.

based on the deal and acquirer characteristics used in Table II, column (5), as well as dummy variables for serial acquirers and high-tech targets.[26] Across 116 regressions (four outcome variables $\times$ 29 subsamples), the coefficient on CAR takes the correct sign and is statistically significant at the 5% level for only 19 regressions. Of more importance is whether CAR's

[26]The deal characteristics we consider are the target's public status, form of payment (stock, cash, mix), diversifying, competitive, hostile, relative size, and high-tech. The acquirer characteristics we consider are serial acquirer, market capitalization, Tobin's Q, past returns, free cash flow, and leverage, defined using either a dummy variable or above/below median values. We include serial acquirers who made more than one deal in a five-year window to capture potential anticipation. We include a high-tech dummy as Luo (2005) finds that the relation between CAR and completion is related to high-tech industry classification. We obtain the high-tech dummy from SDC.

performance improves systematically in particular subsamples. We find that CAR achieves statistical significance for two of the four outcome variables in only five subsamples, and in no subsample does CAR achieve statistical significance for three or more outcome variables. Overall, the results indicate that, to the extent that characteristics correlate with potential explanations for CAR's lack of explanatory power, these particular subsamples do not drive the result.[27]

C.4. Brute Force Subsamples by Characteristic Combinations

Particular combinations of deal or acquirer firm characteristics may drive the lack of relation between outcomes and CAR. We therefore allow for interactions between characteristics. Following the same approach as in the previous subsection, we create the following 10 dummy variables based on the characteristics: log market capitalization, leverage, free cash flow scaled by lagged assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, cash payment, diversifying deals, serial deals, and public target deals. If the characteristic is continuous, we create the dummy variable by splitting the sample at the median. We then form subsamples based on all of the unique interactions between these variables and retain subsamples with at least 30 observations. We next split the sample into two time periods, and, for each subsample and time period, we regress outcomes on CAR$[-1, 1]$ and record the corresponding t-statistic. The results are reported in Internet Appendix Table IA.X. We report the number of transactions with a t-statistic greater than two, less than minus two, or with an absolute value less than two for both periods. Taking Panel A as an example, for nonimpairment, we run 22,298 regressions and find that only 5% of transactions $(1,091/22,298)$ have the correct sign and a t-statistic of at least two in the first period, and only 3% $(735/22,298)$ meet these conditions in the second period. Furthermore, only 0.26% $(59/22,298)$ have the correct sign and are statistically significant in *both* periods. We obtain similar results using the three other outcome variables.

To summarize, even with extensive data mining, we cannot locate a sample for which CAR

[27]Goodwill impairment tests are performed at the reporting unit level. When several targets operate under a single reporting unit, operating performance improvements by one target may obscure the poor operating performance of the failed acquisition, thereby stalling goodwill impairment. For acquisitions that are large relative to the acquirer's size, it is less likely that other businesses can hide value reductions in the target. In Internet Appendix Table IA.IX, the coefficient on CAR remains insignificant when we zoom in on the sample of acquisitions that are relatively large in size. Further, we focus on extreme impairments, because such large value destruction is difficult to mask.

consistently captures outcomes. We conclude that the lack of relation between outcomes and CAR is systematic and not driven by a particular time period (e.g., the financial crisis), industry, or combination of deal and acquirer characteristics.

D. *Out-of-Sample Tests: CAR versus Characteristics*

Next, we compare the ability of CAR versus characteristics-based models to predict deal and acquirer outcomes in out-of-sample settings. We conduct out-of-sample tests by analyzing the ability of characteristics and CAR to predict outcomes in a second period, which was not used to estimate the model's parameters.

Our analysis compares the performance of two "prediction models." First, we estimate a CAR-only OLS regression model that uses our transaction- and firm-level ex-post outcome measures as the dependent variable and CAR as the independent variable. Second, we estimate a characteristics-only OLS regression model that uses our transaction- and firm-level ex-post outcome measures as the dependent variable and the characteristics used in column (7) of Table II as independent variables. (Note that we do not include industry and year controls.) For regressions with nonimpairment as the outcome variable, we use the 2003 to 2010 period to estimate coefficients and predict the probability of transaction impairment within five years for the 2011 to 2018 period. For regressions with abnormal ROA and completion as outcome variables, we use the 1980 to 2000 period to estimate coefficients and predict outcomes for the 2001 to 2018 period.

We next compare the quality of the predictions made by CAR and the characteristics-based model out-of-sample. We present the results in Table III. Panel A shows that the predicted outcome by CAR is not correlated with the realizations of nonimpairment, short-term abnormal ROA, or long-term abnormal ROA (columns (1), (3), and (5), respectively). In contrast, the predicted outcome by the characteristics-based model is positive (the correct direction) and significant at the 1% level for all three outcomes (columns (2), (4), and (6)).

Similar to the results reported in Table II, CAR predicts completion outcomes better than nonimpairment and abnormal ROA outcomes. The coefficient on CAR in column (7) of Table III, Panel A has the correct sign and is significant at the 5% level. However, the result is economically weak: when the probability of completion predicted by CAR goes from the 25^{th} percentile to the 75^{th} percentile, the likelihood of completion increases by 0.3%. The coefficient on the prediction based on characteristics is statistically significant at the 1% level and economically significant: when the predicted probability of deal completion by characteristics goes from the 25^{th} percentile to the 75^{th} percentile, the likelihood of completion increases by 6.0%.

Table III.

Out-of-Sample: Predicted versus Realized Outcomes

We first estimate OLS regressions of deal outcome measures on CAR$[-1,1]$ only and characteristics only using the first half of transactions in each sample as a fit period to estimate coefficients. We then use the parameter estimates from this first half to predict outcomes in the second half of each sample. In Panel A, we assess the correlation between realized outcomes and predicted outcomes produced by the CAR-only model (columns (1), (3), (5), (7)) and the characteristics-only model (columns (2), (4), (6), (8)). In Panel B, we assess the correlation between the predicted outcome by the characteristics-only model and acquirer CAR. Standard errors are reported in parentheses below the coefficients. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A. Predicted versus Realized Outcomes								
Dependent variable:	Realized Outcome							
	Nonimpairment		ST abROA		LT abROA		Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted based on CAR	26.039		−0.855		6.573		1.090**	
	(30.177)		(1.031)		(4.985)		(0.404)	
Predicted based on characteristics		0.708***		1.338***		1.188***		0.995***
		(0.138)		(0.368)		(0.228)		(0.059)
Observations	2,862	2,862	14,358	14,358	10,713	10,713	18,014	18,014
Adjusted R^2	0.000	0.027	0.000	0.039	0.000	0.041	0.000	0.148

Panel B. Is CAR Correlated with the "Predictable" Component of Outcomes?								
Dependent variable:	Predicted Outcome by a Characteristics Model							
	Nonimpairment		ST abROA		LT abROA		Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CAR$[-1,1]$	−0.104***		−0.014***		−0.010***		−0.008	
	(0.005)		(0.001)		(0.003)		(0.017)	
CAR$[-5,5]$		−0.078***		−0.009***		−0.008***		−0.011
		(0.009)		(0.002)		(0.003)		(0.008)
Observations	2,862	2,862	14,358	14,358	10,713	10,713	18,014	18,014
Adjusted R^2	0.010	0.009	0.005	0.006	0.001	0.002	0.000	0.000

Our analysis so far identifies a set of characteristics that are useful in predicting acquisition outcomes out-of-sample. When acquisitions are announced, is the announcement CAR correlated with the out-of-sample characteristics-based prediction (which we already know is reliable)? We investigate this question in Panel B of Table III, which reports results for regressions of the predicted outcome by the characteristics-only model on acquirer CAR. Results show that the acquirer CAR in the later sample is not correlated with the *predictable* part of acquisition outcomes (columns (7) and (8)) or has the wrong sign (columns (1) to (6)).

Figure 4 depicts out-of-sample tests graphically, similar in spirit to the tests reported in

Table III. We estimate OLS outcome models on CAR or characteristics for the transaction-level failure measures (nonimpairment and completion). We then use the coefficients estimated in the first half of the sample to estimate the predicted probability *decile* in the second half of the sample. Next, we report the fraction of transactions with nonimpairment and deal completion rates for each predicted probability decile. Similarly, for the abnormal ROA outcome variables, we estimate OLS models of outcomes on CAR or characteristics. We again use the coefficients estimated in the first half of the sample to estimate the predicted outcome *decile* in the second half of the sample, and we report the realized outcome decile for each predicted outcome decile.

Focusing first on nonimpairment, if the model has predictive power, then the realized nonimpairment rate should increase monotonically as we move from decile 1 (low predicted probability) to decile 10 (high predicted probability). Alternatively, if the model lacks predictive power, the realized nonimpairment rate should be close to 89% (the unconditional nonimpairment rate in the second half of the sample) for all deciles. In Panel A, we see little evidence of significant predictive power for the CAR-only model. The realized nonimpairment rate is nonmonotonic as we move from decile 1 to 10. Moreover, realized nonimpairment rates are close to 89% for many deciles, and nonimpairment rates for the highest CAR deciles are reduced. In contrast, Panel B, the characteristics-only model, shows a stable positive upward trend, indicating that deciles with higher predicted nonimpairment are associated with a higher fraction of realized nonimpairment rates.

The results for the firm-level abnormal ROA outcome variables are generally similar. In Panels C and E—the CAR-only model—realized outcome deciles vary little from the unconditional average decile in the second half of the sample (as indicated by the dashed line) across predicted outcome deciles. In contrast, Panels D and F—the characteristics-only model—show an upward trend in realized outcome deciles as we move from low predicted deciles to high predicted deciles.

In terms of completion (Panels G and H), for the characteristics-only model, realized completion is 99.0% for the highest predicted completion decile and 81.6% for the lowest; for the CAR-only model, realized completion for the highest decile is 95.7% and for the lowest decile is 93.3%.

In sum, the out-of-sample tests support the conclusion from the earlier in-sample tests: CAR has only weak predictive power for acquisition outcomes, whereas acquisition outcomes can be predicted relatively well by characteristics known at the time of the announcement.

These results relate to Ellahie et al. (2025), who develop a measure of merger and acquisition quality (implied return-on-equity improvement; IRI) that quantifies the minimum improvement in the acquirer's return on equity (ROE) that the acquirer must generate over

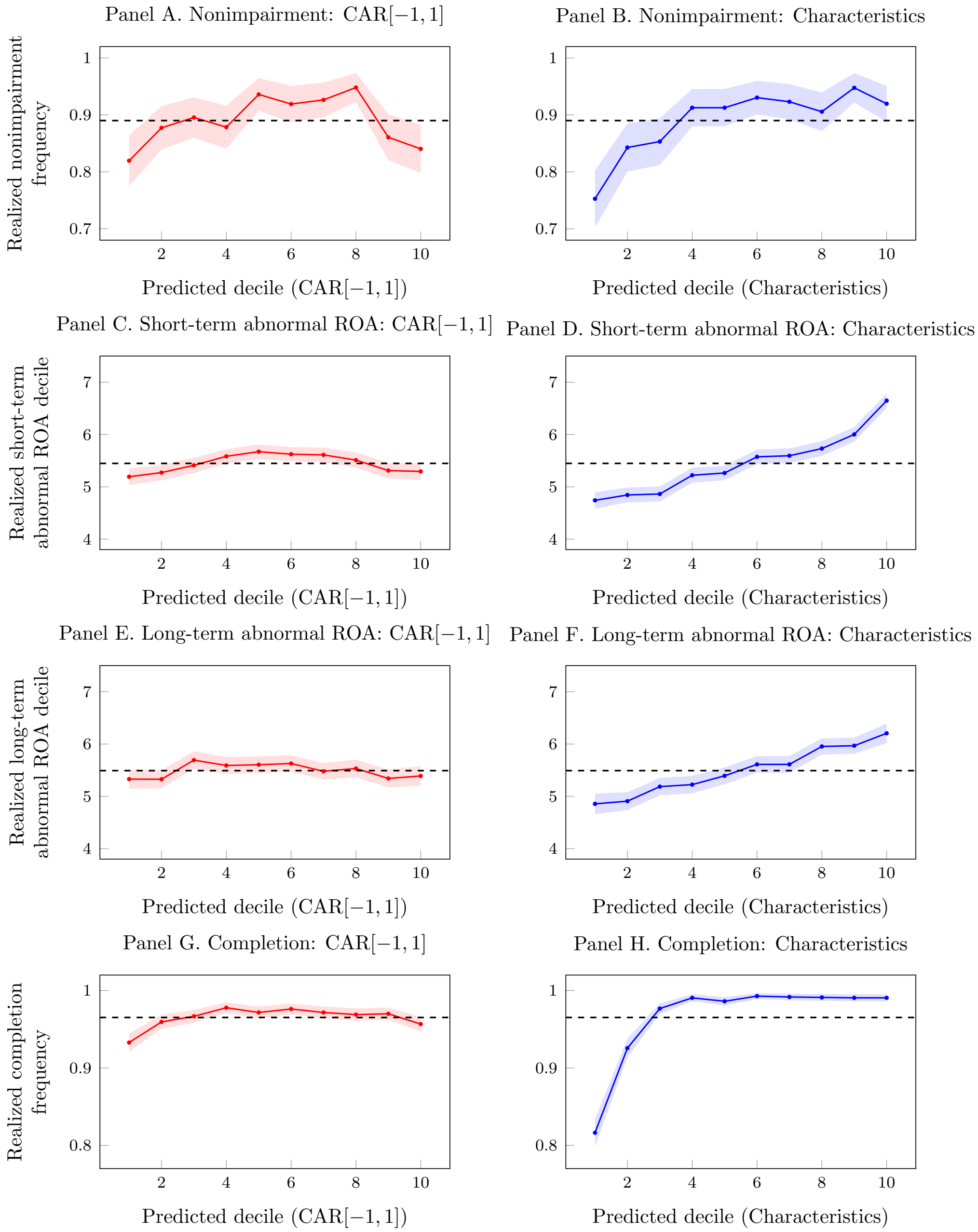

Figure 4. Out-of-sample: Predicted versus realized outcomes.
This figure depicts out-of-sample results. We use the first half of each sample to fit OLS regressions that use nonimpairment, short-term and long-term abnormal ROA, and deal completion as outcome variables. Panels A, C, E, and G include only acquirer CAR$[-1,1]$ as a dependent variable. Panels B, D, F, and H include only deal and firm characteristics as the independent variable. Using the estimates, we obtain predicted outcome deciles for the second half of each sample. For our transaction-level measures, for each predicted probability decile we report the fraction of transactions with realized success or completion. We report the realized outcome decile for our firm-level measures for each predicted decile. The dashed line indicates the unconditional realized frequency (for success and completion) and the unconditional realized outcome decile (for short-term and long-term abnormal ROA) for the second half of each sample. The shaded area represents the 95% confidence interval.

the investment horizon to justify the acquisition price.[28] Using a sample of public targets, they find that high IRI (constructed using transaction, accounting, and stock characteristics) predicts worse acquirer performance in the three years following the deal. Similar to our results, they relate their IRI measure to announcement returns and find that the information content of IRI is not fully incorporated into stock returns around the announcement. We use a large, comprehensive sample of acquisitions and show that a simple characteristics model outperforms CAR in predicting acquisition outcomes.

E. *Validation of Outcome Variables*

Our earlier tests of the hypothesis that acquirer announcement returns (CAR) capture value creation rely on the assumption that the ex-post value-creation measures that we employ effectively represent realized value creation. These measures (or their variations)—nonimpairment likelihood, abnormal ROA, and deal completion rates—are extensively used in the literature and are correlated with one another despite originating from different sources. However, these measures are subject to similar validity critiques as CAR: these proxies themselves may not fully capture the value generated by acquisitions.

Our analyses thus far show that CAR cannot predict our outcome variables, but a simple benchmark measure using characteristics known at the time of the announcement can reasonably predict our value-creation proxies. To further validate our ex-post value-creation measures, we relate these *predicted* outcomes (with a particular focus on outcomes predicted by characteristics) to long-term stock returns following the acquisition announcement. If characteristics (or CAR) correlate with outcome proxies and these ex-post measures sufficiently capture value creation, then predicted outcomes based on these measures should correlate with long-term returns, providing additional evidence of their validity.

For each announcement year in our sample, we estimate OLS regressions of the probability of nonimpairment, short-term and long-term abnormal ROA, and the likelihood of deal completion on the set of deal and acquirer characteristics (or on CAR).[29] Using each transaction's predicted outcome from these regressions, we sort the predicted values into

[28]Notably, the criterion of ROE improvement (synonymous with EPS accretion) does not necessarily align with positive-NPV deals (Ben-David and Chinco, 2024, 2025). Negative-NPV deals can result in ROE improvements, while positive-NPV deals may lead to ROE deterioration. Empirical evidence further indicates that CAR tends to be positively correlated with EPS accretion (Dasgupta et al., 2024).

[29]To be specific, we estimate Table II, columns (4) and (6), for CAR and deal characteristics regressions, respectively.

Table IV.

Long-Term Returns and Predicted Outcomes

This table reports 60-month equal-weighted DGTW-adjusted returns computed beginning at the month-end of the deal announcement date. In columns (1) to (4), we estimate yearly OLS regressions of the probability of nonimpairment, short-term and long-term abnormal ROA, and the probability of completion, respectively, on firm and deal characteristics. We then compute the imputed outcome for each year and sort predicted values into 10 outcome deciles. We report the equal-weighted 60-month DGTW-adjusted BHAR for acquirers in the bottom-three and top-three deciles and the p-value for the difference test between the top and bottom deciles. The characteristics include log market capitalization, leverage and free cash flow scaled by lagged assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, and a set of dummy variables for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals. We measure firm-level characteristics in the year before the deal announcement. Columns (5) to (8) are computed analogously, except we use CAR$[-1, 1]$ to predict outcomes.

Prediction model:	Characteristics-Only Model				CAR-Only Model			
Predicted variable:		Abnormal ROA				Abnormal ROA		
	Nonimpair.	ST	LT	Completion	Nonimpair.	ST	LT	Completion
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top 3 deciles	−2.7%	−5.1%	−3.8%	−5.0%	−6.9%	−10.9%	−8.9%	−10.9%
Bottom 3 deciles	−12.0%	−15.8%	−13.2%	−12.9%	−6.9%	−13.7%	−8.1%	−13.7%
Difference	9.2%	10.7%	9.4%	7.9%	0.1%	2.8%	−0.8%	2.8%
p-value	0.005	0.000	0.000	0.000	0.986	0.158	0.726	0.089

deciles based on the announcement year. We then compute DGTW-adjusted buy-and-hold abnormal returns (BHAR) over 60 months starting from the month before the announcement date for the top 30% (top three deciles) and bottom 30% (bottom three deciles) of acquirers based on the predicted outcomes.

Table IV, columns (1) to (4) summarize returns for predicted outcomes by characteristics for the top and bottom three deciles. Column (1) shows that deals in the top three deciles with the highest predicted nonimpairment likelihood earn average abnormal returns of −2.7% over five years. In comparison, deals in the bottom three deciles earn average returns of −12.0%. The difference of 9.2% is statistically significant. Similarly, average returns are 9.4% to 10.7% higher for deals in the top three deciles of predicted short-term and long-term abnormal ROA by characteristics relative to the bottom three deciles, with differences statistically significant at the 1% level (columns (2) and (3)).

We also focus on completion outcomes. We estimate the likelihood of deal completion based on characteristics (or CAR) for each announcement year to compute predicted outcomes, sort these into deciles, and retain only completed deals.[30] In column (4), the dif-

[30]Since this analysis is conditional on completion, the bottom three deciles represent deals that are

ference in DGTW-adjusted returns between the top and bottom three deciles is 7.9%, again statistically significant at the 1% level.

These results indicate that the return spread generated by characteristics is at least 7% across all four outcome variables. This analysis suggests that not only do characteristics correlate with our ex-post outcomes (e.g., nonimpairment, abnormal ROA, completion), but predicted ex-post outcomes based on characteristics also correlate with long-term returns around the announcement period. The significant relationship between predicted outcomes and long-term returns provides additional validation for our ex-post value-creation proxies.

For completeness, we conduct similar tests using CAR instead of characteristics in columns (5) to (8). We regress our four outcome measures on CAR for each announcement year and sort the predicted values into deciles. Consistent with our earlier findings—which show that CAR does not correlate with ex-post outcomes—we find that outcomes predicted by CAR also do not correlate with long-term returns. Sorting on CAR's predictions, unlike the characteristics model, does not generate a return spread. Given the strong performance of the characteristics-based model and the additional validation of our ex-post measures through their association with long-term returns, this result further confirms that CAR fails to capture information known at the time of the announcement.

In summary, while recognizing the potential limitations of our ex-post value-creation measures, we provide evidence in support of their validity through their significant correlation with long-term stock returns. Our analysis demonstrates that a benchmark model based on observable deal and acquirer characteristics effectively predicts value creation and that these predicted outcomes correlate with long-term returns. In contrast, CAR is unable to generate these correlations. Therefore, relying solely on CAR to assess expected acquisition value creation may be misleading. Our findings support using characteristics-based models as more reliable ex-ante measures of potential value creation in acquisitions.

F. Dampening Effects on CAR

The analyses thus far measure the performance of announcement returns by testing the correlation between ex-post outcomes (e.g., nonimpairment and abnormal ROA) and CAR for the sample of completed deals. These analyses implicitly assume that (i) the deal is a surprise, that is, there are no anticipation effects, (ii) the deal outcome is known, that is,

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completed despite characteristics suggesting lower value creation relative to other deals announced in the same year. We exclude canceled deals from this analysis due to the ambiguity in their expected returns; acquirers with canceled negative CAR transactions may generate positive returns following the transaction process.

there are no truncation effects from deal completion uncertainty, (iii) completed deals are a random sample of those announced, that is, there are no selection effects, and (d) ex-post outcomes are unaffected by management that heeds announcement returns, that is, there are no feedback effects. Anticipation, truncation, selection, and feedback would largely have a *dampening* effect on CAR, diminishing the observed relation between CAR and outcomes. In this section we assess the importance of each of these factors and whether they can explain CAR's failure to capture outcomes.

F.1. Anticipation

Leakage of acquisition intentions has been widely documented in the literature. For example, Betton et al. (2014) find substantial run-ups (averaging 7%) in acquirers' stock prices before the deal announcement. Bid anticipation has also been documented, particularly for serial acquirers and acquirers in industries undergoing significant consolidations.[31]

As a first pass to address the possibility that deal anticipation distorts our results, we extend CAR's measurement window from CAR$[-1, 1]$ to CAR$[-41, 1]$. The results in Internet Appendix Table IA.V repeat the main tests and show that extending CAR's measurement window does not alter the main results: CAR is not meaningfully correlated with merger outcomes. However, a "one-size-fits-all" approach to extending the window may be too coarse, as deals vary in the degree of anticipation: some announcements surprise investors, while others are old news. Deals also vary in the timing of anticipation news: some deals may be leaked in the weeks prior to the announcement, while others may be anticipated years prior to the announcement.

The ideal empirical setting would be to designate a custom measurement window date and size for each announcement that captures the period over which investors assess the deal. Because it is practically impossible to have deal-tailored windows, we resort to a second-best approach in which we remove transactions likely anticipated by investors and retain transactions likely to be surprises to investors.

To identify highly anticipated announcements, we draw on several data sources to construct four measures of anticipation. First, to identify deals with "explicit anticipation," we collect *M&A Rumors and Discussions* data from S&P Capital IQ and retain headlines mentioning both the acquirer and the target that appear up to three years before the deal announcement. In addition, we include deals flagged as rumors in SDC data. These sources

[31] See Asquith et al. (1983), Schipper and Thompson (1983a), Malatesta and Thompson (1985), Betton and Eckbo (2000), Song and Walkling (2000), Fuller et al. (2002), Betton et al. (2008), Cai et al. (2011), and Wang (2018).

together identify 1,047 announcements of completed and withdrawn deals.[32] Second, to identify "potential anticipation" deals, we use less stringent criteria and require that only the acquirer *or* the target be mentioned in the *M&A Rumors and Discussions* headlines, which results in 3,681 announcements of completed and withdrawn deals.[33] Third, we consider deals by repeat acquirers—those announcing at least one acquisition in the previous five years—as potentially anticipated, which leads to 28,266 announcements of completed and withdrawn deals.[34] Finally, we follow Cai et al. (2011) and identify initial industry bids as bids preceded by a long interval since the last industry bid, as such bids are less likely to be anticipated. We consider two intervals, classifying deals as "potentially anticipated" if they occur within one year or six months of another deal in the same four-digit SIC industry.[35]

In Internet Appendix Table IA.XI, we replicate Table II, column (5) for the subsamples that *exclude* the deals flagged as explicitly or potentially anticipated (e.g., deals for which NPV expectations may already be reflected in acquirers' valuations). Regardless of sample definition, CAR continues to underperform across all subsamples.

We also take a brute-force approach and examine returns leading up to the announcements as a measure of anticipation. In Internet Appendix Table IA.XII, Panel A, we sort deals into three terciles based on the run-up CAR$[-41, -2]$: tercile 1 includes announcements with the most negative returns, and tercile 3 includes those with significant run-ups. In Panel B, we replicate Table II, column (5) and focus on tercile 2, which lacks substantial run-ups and presumably reflects the least anticipation. The coefficients on CAR are not statistically significant in any of the regressions, suggesting that even when isolating moderate run-ups, we do not identify a consistently better-performing CAR.

F.2. Truncation, Selection, and Feedback

Announcement returns may not capture the full effect of market expectations of deal value if there exists uncertainty about deal completion ("truncation effect"). Further, if announcement returns reflect market expectations of deal value absent any managerial re-

[32]This sample represents 3% to 8% of the nonimpairment and abnormal ROA samples.

[33]This sample represents 10% to 21% of the nonimpairment and abnormal ROA samples.

[34]This sample represents 48% to 72% of the nonimpairment and abnormal ROA samples.

[35]The one-year-interval sample represents 92% to 95% of the nonimpairment and abnormal ROA samples, and the six-month-interval sample represents 84% to 90% of the nonimpairment and abnormal ROA samples. Cai et al. (2011) identify 82% of deals as anticipated, but they use a smaller sample that ends in 2009 and they exclude financial firms.

sponse, they need not correlate with ex-post outcomes that do reflect a managerial response. If managers learn from CAR and take corrective action, the correlation between CAR and ex-post outcomes will be affected. Such an effect could arise via a "selection effect" due to the elimination of withdrawn bids or via a "feedback effect" (Edmans et al., 2012, 2015) whereby managers respond to CAR and alter the outcome. For example, a manager who observes a negative CAR may cancel the transaction or allocate more resources to increase the chance of deal success. Conversely, following a positive CAR, a manager might decrease the resources allocated toward completing and integrating the combined entity, leading to higher chances of failure.

Truncation, selection, and feedback effects imply a correlation between CAR and withdrawal rates, which we document both in-sample and out-of-sample. These effects therefore help explain CAR's failure to capture outcomes. We caution, however, that the relation is economically weak with low explanatory and predictive power (Tables II and III), lacking consistent results across periods (Figure 3) and subsamples (Internet Appendix Table IA.IX).

Specifically, to assess the importance of truncation, we use out-of-sample tests that rely on the insight that the likelihood of canceling a deal is predictable using acquirer and deal characteristics (e.g., Luo, 2005; Betton et al., 2009; Wang, 2017). As Table III and Figure 4 show, characteristics predict deal completion reasonably well out-of-sample. Using the first half of the sample, we regress the completion dummy on characteristics. We then predict the cancellation probability for transactions in the second half of the sample. We sort transactions based on their completion probabilities into three terciles and repeat the Table II tests for both the lowest tercile (low withdrawal probability) and the highest tercile (high withdrawal probability). Internet Appendix Table IA.XIII shows that CAR does not perform better for the sample of transactions with a low cancellation probability than it does for the sample of transactions with a high cancellation probability: of the 21 regressions in Panels B, D, and F, the coefficient on CAR is statistically significant for only one. Thus, truncation effects are not likely to be the primary driver of the failure of CAR.

To assess the importance of the feedback effect and the related selection effect, although we cannot isolate the counterfactual (outcomes that do not reflect managerial action), we first note that both effects imply a flattening of the relation between CAR and deal outcomes for very negative CAR and an ambiguous relation for very positive CAR. We do not observe a flattening for very negative CAR in Figures 2 and 4. Internet Appendix Table IA.XIV replicates Table II, column (5), but removes CAR's extreme top and bottom deciles. The lack of correlation between CAR and outcomes persists for the remaining eight nonextreme deciles.

We also check whether the feedback provided by CAR is useful. To the extent that is the

Table V.

"Listening" to CAR

This table reports 60-month equal-weighted DGTW-adjusted portfolio returns computed starting from the month-end of the deal announcement date. In columns (1) and (2), we estimate OLS regressions of the withdrawal probability on deal characteristics using the early years of the sample, before 2000. We then compute the imputed outcome for years after 2001. Columns (3) and (4) are computed analogously, except we use CAR$[-1, 1]$ to predict outcomes. In Panels A and B, we limit the sample to completed and withdrawn deals, respectively, and we sort predicted values into outcome deciles. We report the equal-weighted 60-month DGTW-adjusted mean (median) BHAR for acquirers in the bottom three and top three deciles. Panel C reports differences between the different signals and the overall return if one consistently "listened" to CAR.

Prediction model:	60-Month DGTW-Adjusted BHAR			
	Characteristics		CAR	
	Mean	Median	Mean	Median
	(1)	(2)	(3)	(4)
Panel A. Completed Deals				
(a) Top 3 deciles of withdrawal prediction	−12%	−28%	−15%	−26%
(b) Bottom 3 deciles of withdrawal prediction	1%	−7%	−1%	−21%
Panel B. Withdrawn Deals				
(c) Top 3 deciles of withdrawal prediction	−9%	−25%	−35%	−50%
(d) Bottom 3 deciles of withdrawal prediction	−17%	−30%	−16%	−28%
Panel C. "Listening to CAR"				
(c)−(a) Listened to withdrawal signal (canceled) vs. not	3%	3%	−20%	−24%
(b)−(d) Listened to signal not to withdraw (completed) vs. not	18%	23%	15%	7%
((c)−(a)) + ((b)−(d)) Consistently listened	21%	26%	−5%	−17%

case, "listening" to CAR from a long-term return perspective should be beneficial. Following Table IV, we again report 60-month equal-weighted DGTW-adjusted returns in Table V. We estimate OLS regressions of the completion probability on CAR (or characteristics) using the early years of the sample, before 2000. We then compute the imputed outcome for years after 2001 and sort deals into deciles of the imputed outcome. In columns (1) and (2), we consider the returns to listening to characteristics that predict withdrawal (withdrawing versus completing transactions in the bottom three deciles) and the returns to listening to characteristics that predict completion (completing versus canceling transactions in the top three deciles). In columns (3) and (4), we consider the returns to listening to negative CAR (withdrawing versus completing transactions in the bottom three deciles) and the returns to listening to positive CAR (completing versus canceling transactions in the top three deciles).

The results in Table V indicate that listening to CAR results in losses. Panel C shows that listening to negative CAR signals or withdrawing versus completing deals in the bottom three deciles results in mean losses of 20% (column (3), row (c) minus row (a)) and median losses of 24% (column (4), row (c) minus row (a)), whereas listening to negative signals by characteristics (bottom three deciles of predicted completion) results in positive returns of 3% (again row (c) minus row (a)). CAR performs better for positive signals (deals in the top three deciles of completion probability by CAR), with completing versus withdrawing generating returns of 7% to 15%, but signals generated from characteristics produce higher returns of 18% to 23% (row (b) minus row (d)). The net effect of listening to CAR is −5% to −17%, while the net effect of listening to characteristics is 21% to 26%.

In sum, although selection and feedback effects are present, they are likely not the primary (or only) driver of the lack of correlation between CAR and ex-post outcomes.

G. Which Deals Create Value?

Another way to investigate the forecasting ability of CAR is to consider the quality of inferences regarding deal quality generated from announcement returns relative to ex-post measures. To do so, we consider the "types" of transactions (defined by deal, target, or acquirer characteristics) that CAR predicts will create or destroy the most value. We then relate these deal types to realized outcomes. For example, if CAR for the average public target is negative, one might infer that acquiring a typical public target destroys value. How reliable are these inferences? We address this question in multiple ways.

G.1. Univariate Tests: One Characteristic at a Time

We run 65 univariate regressions in which we regress five dependent variables—CAR, nonimpairment, short- and long-term abnormal ROA, and completion—against one of 13 independent deal and firm characteristics. We thus obtain 65 coefficients (13 independent variables × 5 outcomes). All acquirer characteristics are computed before the announcement. Leverage, free cash flows, assets, and Tobin's Q are computed the year before the announcement. Past returns are computed in the quarter and month before the announcement.[36]

We standardize the 65 coefficients and present them in Figure 5. The coefficients are sorted by characteristics that predict the lowest CAR (large acquirer, public target, large

[36]This test also helps address an errors-in-variables critique. Specifically, our main tests regress ex-post outcomes on announcement returns. Standard regression analysis assumes that regressors are observed without noise, but CAR could be noisy and hence lead to attenuated coefficients—an econometric issue often referred to as errors-in-variables in the literature. In this section, however, CAR is the dependent variable rather than an independent variable.

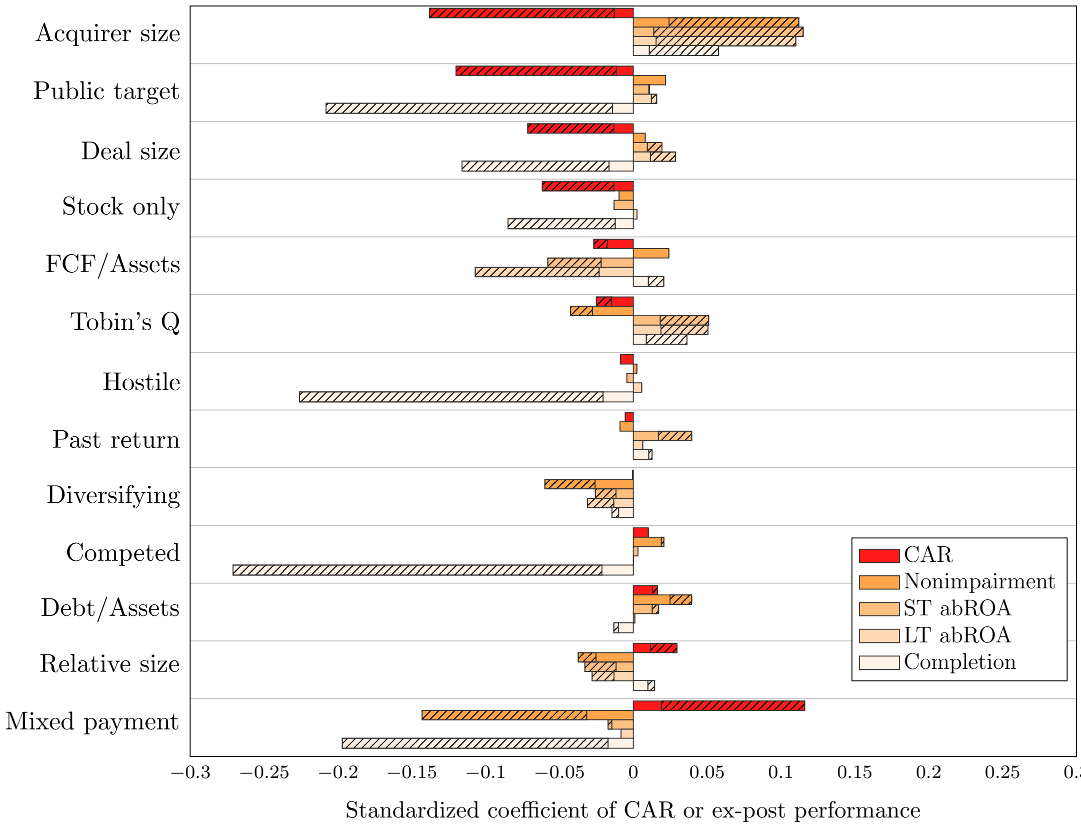

Figure 5. Correlation of CAR and outcomes with characteristics.
This bar chart shows the standardized coefficients for regressions in which the dependent variable is CAR, nonimpairment, short- and long-term abnormal ROA, or completion on the various deal and firm characteristics. Each characteristic enters each regression individually (univariate regressions). The red bars indicate the standardized coefficients from regressions in which CAR is the dependent variable, and the four lighter bars indicate regressions for which nonimpairment, short-term and long-term abnormal ROA, and completion are the dependent variables. The patterned portion of the bars indicates a coefficient larger than 1.96 standard errors of the standardized coefficient, that is, statistically significant at the 5% level or better. All acquirer characteristics are computed before the announcement. Leverage, free cash flows (FCF), assets, and Tobin's Q are computed the year before the announcement. Past returns are computed in the quarter before the announcement.

deal size, stock-only) to those that predict the highest CAR (large relative size, mixed payment, high leverage).[37] In general, the relations between CAR and the deal and firm characteristics that we document match those in earlier studies that explore this relation, although often in different periods and using different samples.

Two important inferences can be drawn from Figure 5. First, the coefficients on the four

[37] As discussed in Section I, the sample size varies across ex-post outcome measures due to data availability. We report the coefficients for regressions in which CAR is the dependent variable using the short-term abnormal ROA sample. The results are nearly identical when we use the samples associated with our three other outcome variables.

ex-post outcomes correlate despite originating from different sources. This result implies that characteristics associated with a high likelihood of success (e.g., large acquirer size) are also associated with high ex-post performance, as indicated by low impairment outcomes, high short-term and long-term abnormal ROA, and high completion rates. Similarly, characteristics associated with a low likelihood of success (e.g., diversifying deals and large relative size) are also associated with poor ex-post performance. This evidence provides further validation of our ex-post proxies for acquisition quality.

Second, and strikingly, Figure 5 shows no association in sign or relative importance between the characteristics for which CAR predicts failure or success and the characteristics associated with failure or success ex-post. For example, transactions with large acquirer size are associated with low CARs but are not associated with an increased rate of impairment, withdrawal, or low abnormal ROA, while transactions with large relative size are associated with high CARs but are not associated with higher nonimpairment or completion or abnormal ROA.

Overall, on a univariate basis, there is often a mismatch between the deal types and acquirers predicted to do well versus to destroy value by CAR and the ex-post realizations of these deal types. The results in this section show that the inferences about the quality of acquisition decisions generated by CAR are inconsistent with those generated from ex-post measures.

G.2. Combining CAR-Based Inferences into a Single Predictor

We further consider the combination of characteristics often used in the M&A literature. Earlier studies find that announcement returns are persistently associated with particular characteristics and thus conclude that deals with specific characteristics create value for acquirers, on average, while others destroy value.

We construct a single measure of CAR-predicted deal success based on characteristics. We first predict CAR by regressing acquirer CAR on characteristics. The in-sample predicted CAR summarizes the associations of CAR with all of the regressors. We then sort the predicted CAR into deciles such that the top (bottom) deciles contain transactions that have characteristics associated with high (low) CAR, implying that, on average, they should predict high- (low-) NPV transactions.

We use these predictive regressions to explore whether high-NPV transactions, according to CAR, are associated with better ex-post outcomes. In the four panels on the left-hand side of Figure 6, Panels A, C, E, and G, we present the ex-post outcomes for predicted CAR deciles. Panel A shows that the rate of no goodwill impairment does not vary among the first eight deciles and declines for the highest deciles of the combined CAR predictor.

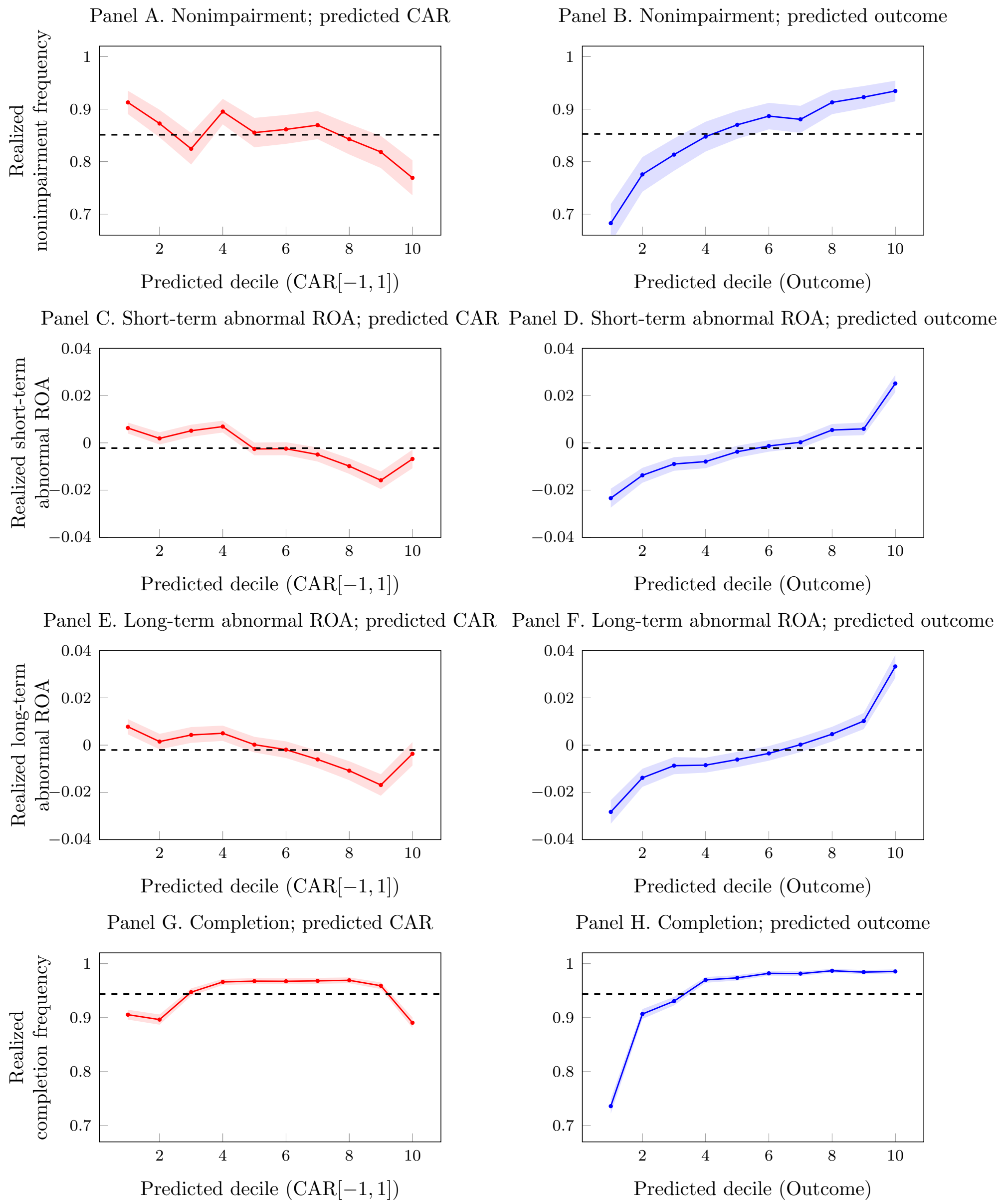

Figure 6. CAR-based predictors versus characteristics-based predictors.
We use the coefficients from a regression of CAR on characteristics to obtain an in-sample predicted CAR for the sample of completed transactions, that is, a summary of what CAR would be given the set of deal and acquirer characteristics. We then sort the predicted CAR into deciles. On the left-hand side of the figure, for each predicted CAR decile, we report (solid red line) realized nonimpairment frequency (Panel A), average realized short- and long-term abnormal ROA (Panels C and E, respectively), and realized completion frequency (Panel G). Red shading indicates 95% confidence intervals. Similarly, we use the coefficients from regressions of ex-post outcomes on characteristics to obtain in-sample predicted nonimpairment, short-term abnormal ROA, long-term abnormal ROA, and completion. We then sort predicted values into deciles, with results presented on the right-hand side of the figure. For each predicted decile, we report (solid blue line) realized nonimpairment frequency (Panel B), average realized short- and long-term abnormal ROA (Panels D and F, respectively), and realized completion frequency (Panel H). Blue shading indicates 95% confidence intervals.

In Panels C and E, the sign is wrong—realized abnormal ROA declines from the lowest to highest deciles of the combined CAR predictor. In Panel G, realized completion rates are lower for the lowest and highest deciles but do not vary across combined CAR predictor deciles 3 to 9.

We next test whether characteristics do a better job of predicting ex-post outcomes. We first use the coefficients from regressions of ex-post outcomes on characteristics to obtain in-sample predicted nonimpairment, short-term abnormal ROA, long-term abnormal ROA, and completion. We then sort predicted values into deciles. On the right-hand side of Figure 6, for each predicted decile, we report realized nonimpairment frequency (Panel B), average realized abnormal ROA (Panels D and F), and average realized completion rates (Panel H). All four panels show a clear positive slope, suggesting that characteristics are good predictors of ex-post outcomes.

In sum, the results indicate that CAR is not directly or indirectly associated with outcomes via characteristics. These results contrast with the moderate ability of characteristics to predict transaction- and firm-level acquisition outcomes.

H. Common Determinants of Acquisition Quality

Next, we zoom in on the most common determinants of acquisition quality discussed in the literature (and taught in the classroom): the form of payment, the target's status as public or private, acquirer size, and relative transaction size.[38]

We form 16 combinations of these characteristics (in their binary forms) and calculate average CAR and average ex-post outcomes for each combination. Table VI presents the results. The combinations are sorted by their average CARs.[39] To facilitate interpretation, statistics within each column are color-coded from red (signifying the worst performance) to green (signifying the best performance) for each measure.

Table VI shows no positive association between announcement returns and ex-post outcomes. If anything, the association is often negative. The transactions ranked as having the

[38]For studies that link announcement returns to these characteristics, see, for example, Travlos (1987), Eckbo et al. (1990a), Morck et al. (1990), Chang (1998), Andrade et al. (2001), Fuller et al. (2002), Moeller et al. (2004), Moeller et al. (2005), Faccio et al. (2006), Officer (2007), Bayazitova et al. (2009), Harford et al. (2012), Eckbo et al. (2018), and Hu et al. (2020).

[39]As discussed in Section I, sample size varies across ex-post outcome measures due to data availability. We report average CARs based on the short-term abnormal ROA sample. The results are nearly identical when we sort CAR using the samples associated with our three other outcome variables.

Table VI.

Acquisition Outcomes and CAR, Grouped by Characteristics

This table reports the average of the acquisition outcome variables and CAR for acquisitions grouped by the characteristics identified in extant literature as correlated with CAR. *Rank* is the average rank of the four outcome variables. To facilitate interpretation, statistics within each column are color-coded from red (signifying the worst performance) to green (signifying the best performance) for each measure.

Acquisition Characteristics					Ex-Post Outcomes				
Cash Only	Public Target	Large Acquirer	Large Relative Size	Avg CAR$[-1,1]$	Non-impairment	ST abROA	LT abROA	Comp-letion	Avg Rank
			Y	0.028	0.753	−0.008	−0.008	0.935	14
Y			Y	0.022	0.818	−0.007	−0.010	0.973	11
		Y	Y	0.018	0.834	−0.006	−0.005	0.950	7
Y	Y		Y	0.018	0.791	−0.015	−0.010	0.818	16
Y		Y	Y	0.014	0.856	0.002	0.002	0.975	5
Y				0.010	0.874	−0.008	−0.010	0.989	6
Y	Y			0.008	0.925	−0.002	−0.013	0.876	10
Y	Y	Y	Y	0.007	0.823	0.002	0.005	0.804	8
				0.004	0.821	−0.013	−0.012	0.981	12
Y		Y		0.002	0.913	0.012	0.011	0.986	1
		Y		0.002	0.919	0.004	0.004	0.979	3
Y	Y	Y		0.001	0.888	0.020	0.018	0.931	4
	Y	Y		−0.003	0.953	0.008	0.009	0.938	2
	Y			−0.004	0.949	−0.014	−0.024	0.892	12
	Y		Y	−0.011	0.849	−0.012	−0.011	0.789	15
	Y	Y	Y	−0.026	0.837	0.000	0.004	0.843	8

best performance according to CAR (2.8%) have the following acquisition characteristics: not limited to cash, private target, small acquirer, and large relative size. However, their ex-post outcomes are the worst: only 75% do not impair (versus a sample mean of 85.2%), and their average short-term and long-term abnormal ROA is −0.8% (versus a sample mean of −0.2%). In contrast, the bottom half of the characteristic combinations according to CAR (indicated by red shading) are often ranked in the top half of nonimpairment and ROA outcomes (as indicated by green shading). For completion, there does not appear to be a negative relation between CAR and nonwithdrawal rates, but high withdrawal rates (indicated by red shading) appear throughout the CAR distribution.

Notably, the types of deals indicated to be successful by our four ex-post measures are largely correlated (particularly for nonimpairment and ROA). Cash-only deals, deals with a public target, deals in which the acquirer is not large, and relatively large deals destroy value based on these outcomes. The results in Table VI suggest that the following deal types create value: those that are not cash-only, those with a private target, those in which the acquirer is large, and deals that are not relatively large in size.

Overall, these results echo our earlier findings that CAR is not a reliable indicator of acquisition quality and is beaten by a simple model of characteristics known at the time of announcement.

III. Why CAR Fails to Reflect Acquisition Outcomes

We have shown that CAR does not consistently capture acquisition outcomes, whether in-sample, out-of-sample, or across subsamples defined by time, industry, deal, or firm characteristics. This inconsistency suggests that CAR's limitations are systematic and not confined to specific deal types. Moreover, factors such as anticipation, truncation, feedback, selection biases, and measurement errors do not fully explain the lack of correlation, indicating that there could be fundamental reasons why CAR may not be a good proxy for deal NPV.

We propose that CAR blends NPV-related and -unrelated information. Over one-quarter of observed CAR values imply economically implausible deal valuations, suggesting that CAR incorporates nondeal factors. Moreover, for canceled deals, returns around the withdrawal announcement fail to offset earlier returns, indicating that CAR reflects extraneous signals rather than pure deal content.

We next explore what drives the economic values implied by CAR. Its magnitude correlates 6× more with acquirer size than with target features. Thus, the CARs of a serial acquirer appear to be drawn from a stable distribution regardless of the target's size. This implies that a firm like Cisco acquires targets of different sizes, but its CARs are drawn from almost the same distribution. This pattern suggests that most information embedded in CAR relates to the acquirer—private information, misvaluation, or changing risk—rather than deal outcomes.

A. \$CAR Must Contain Non-NPV Information

We propose that CAR contains at least two components: information about the deal's NPV and an additional component (which we call X) that represents value-relevant information about the standalone acquirer. To explore these two components of CAR, we introduce \$CAR, defined as the dollar value created (or destroyed) based on CAR:

$$\begin{aligned} \$\text{CAR} &= \text{CAR} \times \text{Acquirer market capitalization} \\ &= \underbrace{NPV}_{\text{Deal-related value}} + \underbrace{X}_{\text{Acquirer-related value}}. \end{aligned} \tag{2}$$

Previous research also recognizes that CAR can include value-relevant information beyond the specific deal's NPV. For example, investors might infer insights about the CEO's decision-making skills (Pan et al., 2016), the firm's strategic planning processes (Gokkaya et al., 2024), or misvaluation signals (Hietala et al., 2003; Shleifer and Vishny, 2003; Bhagat et al., 2005; Dong et al., 2006; Ben-David et al., 2015).

We argue that the CAR associated with an acquisition announcement *always* reflects more information than simply the deal's NPV: it also includes value-relevant information about the circumstances driving the acquisition. A firm's decision to acquire another firm is among its most significant actions and is not random—something triggers such a decision at a particular time. The trigger must surprise investors, as otherwise any NPV gains would already be embedded in the stock price.

Triggers can be internal—like the outcome of an R&D project or a strategic decision to enter a new market—or external, such as the emergence of a target or regulatory changes. These factors can alter the deal's outlook and prompt an announcement. Some triggers signal positive news, like a strategic move by a newly hired CEO, while others reveal challenges, such as the failure of an internal R&D project, that nonetheless lead to a deal. Exogenous triggers, where firms randomly decide to acquire targets without underlying reasons, rarely occur.

CAR must therefore reflect a combined signal of both the economic circumstances prompting the deal and its NPV. The acquisition announcement invariably conveys new information to investors about the acquirer's strategic direction, managerial insights, or previously unknown intentions—information we denote by X. For example, a firm may decide to enter a new market by acquiring a target with an existing presence, or a company lacking the capability to develop new technology might acquire a target that already possesses the given technology. Depending on investors' prior beliefs, the acquisition decision may signal either positive or negative news. As Grinblatt and Titman (2002) aptly state,

> The stock returns of the bidder at the time of the announcement of the bid *may* tell us more about how the market is reassessing the bidder's business than it does about the value of the acquisition. (p. 708)

Given that CAR combines two components (one deal-related and the other acquirer-related), whether one can extract NPV from the combined signal \$CAR is an empirical question. This task is complex because both components—NPV and X—are unobservable and may vary independently in magnitude and direction. Unless X is economically insignificant, which our empirical findings suggest is not the case, \$CAR cannot reliably estimate NPV.

B. $CAR = NPV$? High Prevalence of Implausible NPV Values

We begin by assessing whether CAR measures NPV, implying that $X \approx 0$, by assuming that CAR reflects NPV and evaluating the plausibility of the economic inference one would make by observing CAR.

We propose that the magnitude of deal-related value creation is likely proportional to the deal size, as NPVs should generally be bounded by the size of the deal, from both above and below. On the downside, an acquirer typically cannot lose more than the invested amount (the deal size). On the upside, unless target shareholders sell at a significant discount (greater than 50%), NPV is unlikely to exceed the deal size. We therefore assume $|NPV/DealSize| \leq 1$.

B.1. CAR Implies Unrealistic Fat-Tail Value Creation and Destruction

In 2022, Microsoft acquired Activision Blizzard for $68 billion. The CAR around the announcement was 2.42%, suggesting value creation of $55.3 billion. Two years earlier, in 2020, Microsoft purchased CyberX. With a CAR of 2.25%, this signaled an astonishing value creation of $33.5 billion. In this instance, taking CAR at face value would imply that investors believed that Microsoft's leadership has a golden touch, transforming its $0.16 billion investment in CyberX into $33.6 billion, a staggering 200× multiple. Furthermore, Microsoft's CAR implies that CyberX's investors sold their shares for mere pennies on the dollar. Both of these interpretations strain credibility.

The Microsoft example is not unique. We perform a similar analysis for all acquisitions made by Cisco Systems. Cisco is one of the largest U.S. technology firms, known for its decades-long acquisition strategy. Many strategy and finance case studies have been written about Cisco's acquisitions. Figure 7 analyzes 140 deals of different relative sizes (relative size is defined as deal size scaled by acquirer size) made by Cisco.[40] The dashed gray lines represent the reasonable bounds on NPV (i.e., $|NPV/DealSize| \leq 1$) as discussed above. The blue hollow dots represent deals that fall within these reasonable bounds, and the red solid dots indicate CAR values outside these reasonable bounds. Figure 7 shows that an astounding 91% of deals (128 out of the 140) are outside the plausible range.

With the extreme example of Cisco in mind, we turn back to our analysis and consider the full sample of 47,543 deal announcements for which CAR$[-1, 1]$ is available.[41] We study

[40] For presentation purposes, we drop nine deals with large relative sizes (> 0.03) or extreme returns (outside ±8%).

[41] Note that the base sample here contains all acquisition announcements regardless of the availability of

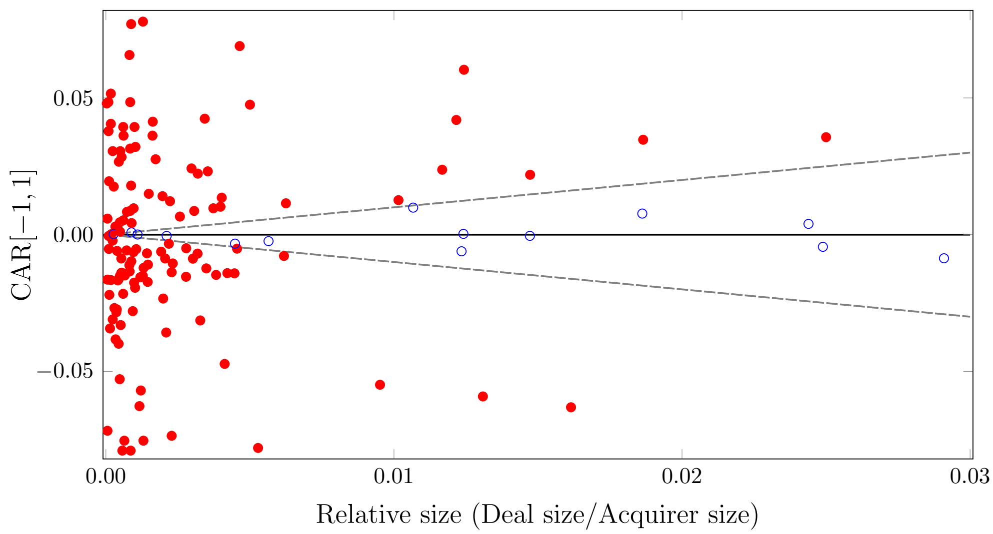

Figure 7. Cisco's acquisitions and CARs.
This figure plots CAR[−1, 1] and relative size (i.e., deal size divided by acquirer size) of Cisco's acquisitions within the range of relative size ≤ 3% and CAR between (±8%). The dashed gray lines represent the reasonable bounds on NPV (i.e., $|NPV/DealSize| \leq 1$). The blue hollow dots represent deals that fall within these reasonable bounds, and the red solid dots indicate CAR values outside these bounds.

the relationship between $CAR and deal size by plotting the frequency of transactions in which value is created or destroyed (based on CAR) relative to their deal sizes.

Figure 8 presents the transaction frequency as a function of $CAR and $deal size. For ease of presentation, we split the sample by CAR's sign. Panel A includes deals with positive CAR, whereas Panel B comprises deals with negative CAR. Darker colors represent a higher concentration of deals. The black lines represent $\$CAR = \$DealSize$. In Panel A, deals above the black line indicate that the $CAR-implied value created exceeds the amount paid. If $CAR does indeed measure NPV, then the conclusion from Panel A is that in about 27% of value-creating deals, target shareholders sold their firm at a discount deeper than 50%. Similarly, the conclusion from Panel B is that in about 27% of value-destroying deals, the value destroyed was much greater than the original amount invested by the acquirer.

B.2. CAR Does Not Reverse Upon Deal Withdrawal

Another empirical setting in which $\$CAR = NPV$ has a specific empirical prediction is the subset of deals that have been withdrawn. If CAR reflects only the NPV accrued to the acquirer at the initial announcement, withdrawing a deal should undo the initial effect, that is, $CAR_{i,Withdrawal} \approx -CAR_{i,Announcement}$.

post-announcement data or conditional deal completion, and thus it is not subject to concerns like truncation and survival bias.

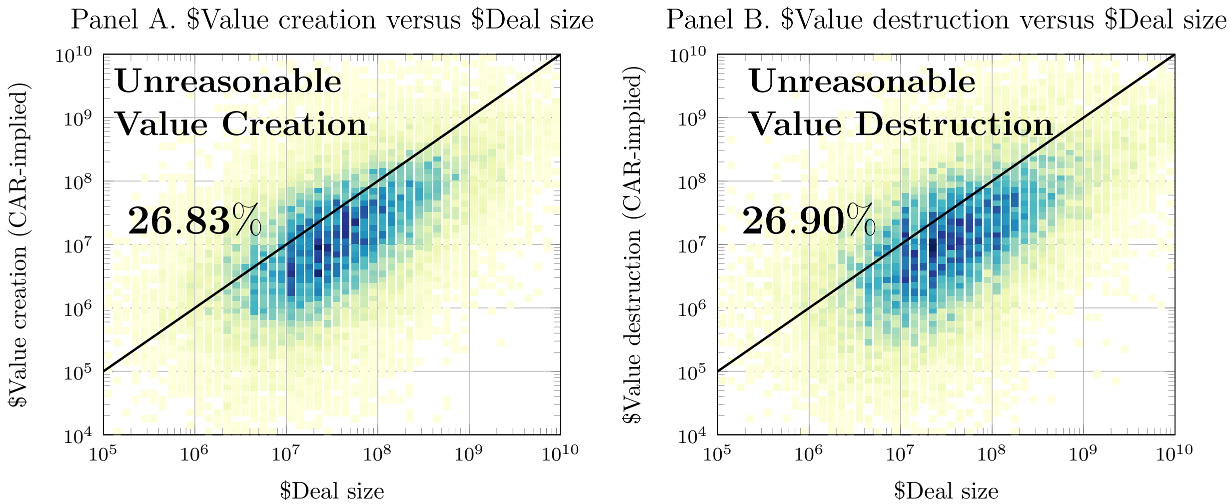

Figure 8. $CAR and $Deal size.
The panels plot the frequency of transactions, presented as a function of $CAR and $deal size. Panel A focuses on the subsample of positive $CAR deals, and Panel B on the subsample of negative $CAR deals. Darker colors represent a higher concentration of deals. The black lines represent $\$CAR = \$DealSize$.

We assess the distributional properties of the $2,000+$ withdrawn deals described in Section B.3.[42] Because, like announcement CAR, withdrawal CAR can be dampened by anticipation and related effects, we focus on the signs rather than magnitudes of CARs, specifically, on instances in which withdrawal CAR is the opposite sign of announcement CAR. In Figure 9, we present histograms of withdrawal $[-1, 1]$ returns for both negative announcement CAR deals (Panel A) and positive announcement CAR deals (Panel B). Suppose withdrawal returns reflect NPV revisions and not X. In that case, we would expect negative announcement CAR deals to be skewed toward positive withdrawal returns (i.e., the acquirer is canceling a value-destroying deal). Positive announcement CAR deals would similarly be skewed toward negative withdrawal returns (i.e., the acquirer is canceling a positive NPV project).

The distributions of both histograms are notably similar. Only 53% of negative announcement CAR deals are associated with *positive* withdrawal CARs. Similarly, only 55% of deals with positive announcement CARs are associated with *negative* withdrawal CARs. These results imply that for about 50% of deals in both subsamples, the deal withdrawal CAR does not appear to simply be a reversal of NPV expectations. This evidence implies

[42]In this analysis, we focus on 2,141 withdrawn deals. From the 2,227 withdrawn deals, we remove 50 deals without data on withdrawal announcement returns. We also remove 36 deals with the same announcement and withdrawal date. For each withdrawn deal, we compute a three-day CAR around the withdrawal date provided in SDC.

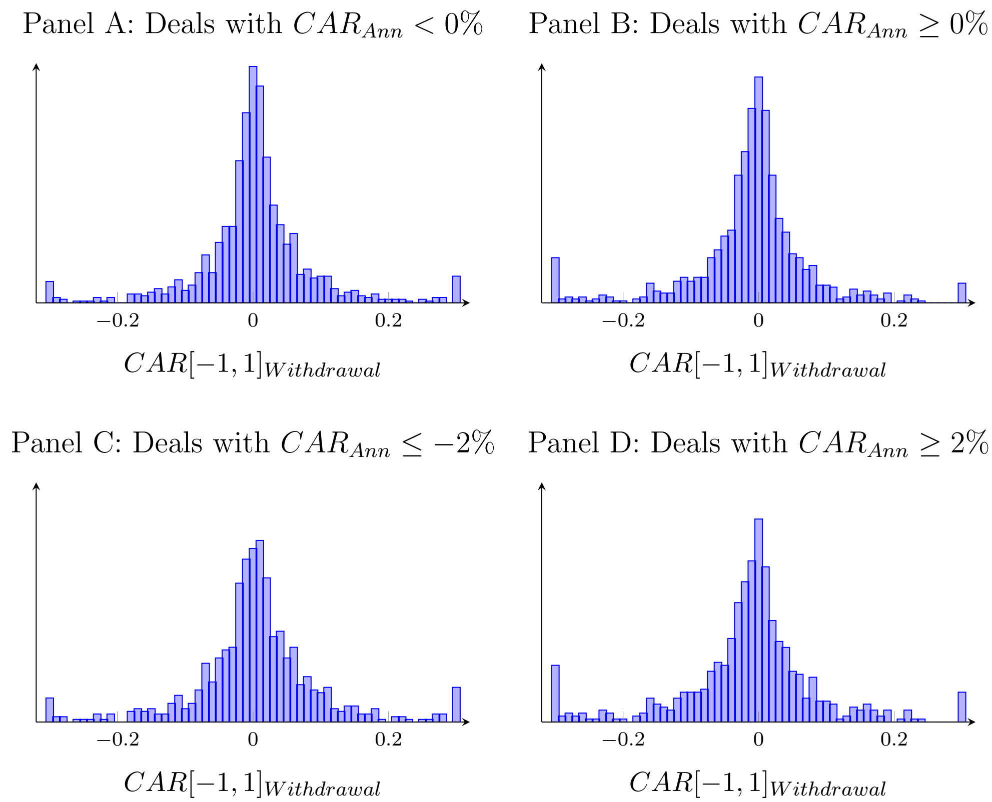

Figure 9. CAR around withdrawal announcements.
This figure plots histograms of CAR around withdrawal announcements. Panels A and B present histograms of CAR$[-1,1]_{Withdrawal}$ (withdrawal abnormal announcement returns) for negative and positive announcement CAR deals, respectively. Panels C and D present histograms of CAR$[-1,1]_{Withdrawal}$ for deals with $CAR_{Announcement} \leq -2\%$ and $CAR_{Announcement} \geq 2\%$, respectively.

that nondeal-related information has been incorporated into the acquirers' prices, suggesting that X is large.[43]

In Panels C and D of Figure 9, we repeat the analysis for transactions with extreme CAR: $CAR_{Announcement} \leq -2\%$ (Panel C) and $CAR_{Announcement} \geq 2\%$ (Panel D). These subsets include the transactions that should arguably have the largest NPV effects if CAR does indeed reflect deal NPV. Again, the distribution of withdrawal returns for negative (Panel C) and positive (Panel D) announcement CAR deals are relatively similar, implying no observable reversal of the acquisition announcement return. Indeed, both distributions are heavily centered near zero, indicating that X is contained in CAR and does not reverse when the deal cancellation is announced.[44]

[43]The withdrawal itself may generate additional standalone information related to financing availability, increased regulatory pressures, or management quality (Jacobsen, 2014).

[44]The results are robust to longer windows around the withdrawal announcement that capture anticipation

C. How Important Is X in \$CAR?

In the previous section, we provide evidence that the assumption that CAR reflects NPV may not hold for many deals, suggesting that X is likely sufficiently large to distort any inferences that could be made about NPV based on observations of CAR. To quantify the relative importance of X in CAR, we analyze the variation in CAR, specifically, the dollar value created or destroyed as indicated by CAR. Our central question is whether this variation likely reflects information about the deal itself or the acquirer.

While we cannot directly observe the decomposition of CAR into the deal's NPV and the acquirer's standalone value updates, we can assess their relative contributions using an identifying assumption: deal-related value contributions scale with deal size, and acquirer-related value contributions scale with acquirer size. Specifically, we assume that $|NPV| \propto$ DealSize and $|X| \propto$ AcqMarketCap.

For instance, the potential value creation or destruction from a deal is naturally constrained by the scale of the transaction itself, as the benefits or costs of synergies, integration, or overpayment typically relate to the deal's size, supporting the assumption that $|NPV| \propto$ DealSize. Similarly, information about the acquirer's management quality, strategic decisions, growth prospects, or governance applies to the entire standalone acquirer, supporting the assumption that $|X| \propto$ AcqMarketCap. In other words, our empirical procedure estimates the extent to which the distribution from which the observed $|\$CAR|$ has been drawn is related to the acquirer's market capitalization or the target's size.

The dominance of acquirer-related factors in explaining the variation in \$CAR (cumulative abnormal returns) is clearly illustrated in a visual representation. We log-transform (base 10) both the acquirer's market capitalization and deal size and then divide each dimension into bins with increments of 0.1. This creates a grid of bins based on the logged values, yielding a final sample of 1,236 bins, restricted to those with more than five transactions.

In Figure 10, Panel A displays the standard deviation of \$CAR for the transactions within each bin, with darker colors indicating higher variation. Not surprisingly, larger acquirers tend to make bigger deals. What stands out, however, is the color pattern: the color scale remains nearly *constant across deal sizes* (i.e., up and down within acquirer size) and *intensifies across acquirer sizes* (i.e., from left to right). This suggests that deal size has little association with the variation in \$CAR, while acquirer size has materially stronger association with \$CAR variation.

In Panel B of Figure 10, we focus on bins that contain deals with negative \$CAR values only—those typically interpreted as value-destroying acquisitions. To assess the magnitude

of the deal cancellation.

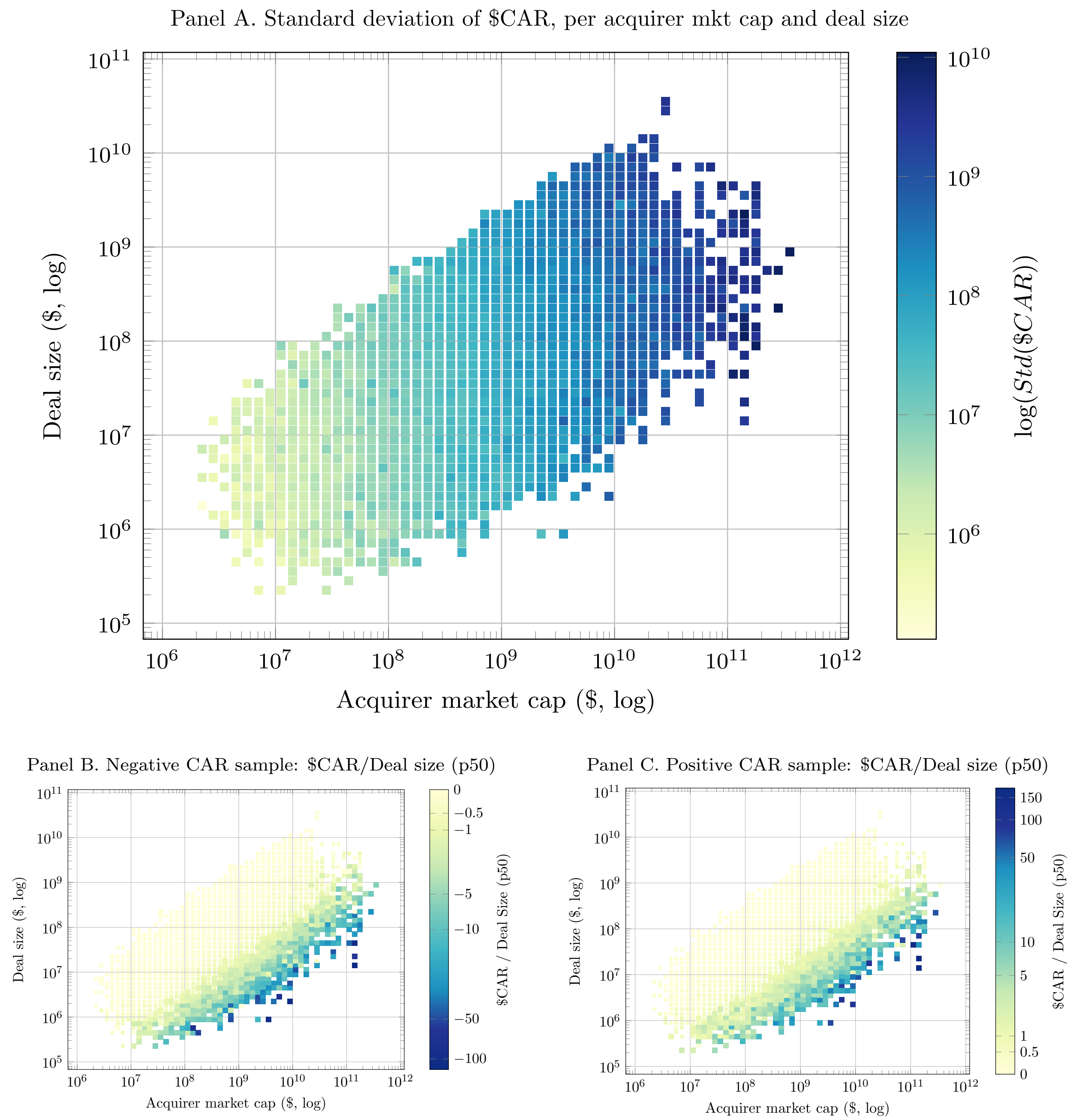

Figure 10. Std($CAR) and acquirer market capitalization and deal size.
This figure presents heatmaps illustrating the variation in $CAR based on acquirer market capitalization and deal size. We log-transform (base 10) both acquirer market capitalization and deal size, and then divide each dimension into bins with increments of 0.1. This creates a grid of bins based on the logged values, resulting in a final sample of 1,236 bins, where we restrict attention to those with more than five transactions. Panel A shows the standard deviation of $CAR within each bin, with darker colors indicating higher variation. Panel B is limited to deals with negative CAR, and Panel C is limited to deals with positive CAR. In Panels B and C, cell colors represent the median value of $CAR/Deal size on a $1 + \log(x)$ scale, and cells with median values $|x| > 1$ are represented by large markers.

of the value destruction, the color of each cell represents the \$CAR/Deal size (median) within each bin (on a $1+\log(x)$ scale). Median values below one are represented with large markers. This plot reveals a strong pattern. Based on the interpretation that CAR represents NPV, acquirers that engage in relatively small deals tend to experience devastating losses many times greater than the original deal size—often exceeding 10×, and at times 100×, the deal size. Conversely, deals of the same size appear to destroy less economic value when made by smaller acquirers. For example, consider a target valued at \$100 million, where the acquirer experiences negative CAR upon announcement. For a relatively small acquirer (say, with a market cap of \$1 billion), the median negative \$CAR implies that the value destroyed is less than the deal size itself. However, for an acquirer with a market cap of \$10 billion, the median value destroyed exceeds the deal size, and for a \$100 billion acquirer, the value destruction can exceed 40× the deal size. The magnitude of these losses is economically unreasonable.

Panel C of Figure 10 shows a similar analysis for value-creating deals. As expected, smaller acquirers generate reasonable economic value relative to the deal size. However, for larger acquirers, the median value created appears disproportionately high, sometimes reaching up to 100× the original investment. Again, these values are economically fantastic.

Our conjecture is that these extreme values of value destruction and creation, as implied by \$CAR, do not simply reflect the NPV of acquisitions but also capture other factors unrelated to the deals. This would help explain why \$CAR often shows economic values many multiples greater than target deal sizes, especially for large acquirers.

Given that CAR includes both deal- and acquirer-related information, we can estimate the relative importance of these two components using the regression specification

$$\log|\$CAR_i| = \alpha + \beta\log(DealSize_i) + \gamma\log(AcqMarketCap_i) + \varepsilon_i, \tag{3}$$

where the dependent variable is the logged absolute value of CAR$[-1,1]$ and the dependent variables are the logged deal size and the logged market capitalization of the acquirer. Since this specification does not rely on deal completion, we run it on the entire sample of acquisition announcements (nearly 40,000 announcements).

We present the results in Table VII, columns (1) to (3). The coefficient on logged acquirer market capitalization is about 6.5× larger than that of logged deal size.

We conduct additional tests assessing the factors contributing to \$CAR in columns (4) to (7) of Table VII. In columns (4) and (5), we use a bin-level sample, where each observation represents a bin defined by a specific deal-size segment and acquirer-market-cap segment. Because each bin includes at least five transactions, we can calculate the logged standard deviation of the \$CARs. We then regress this statistic on the bin's median logged deal size

Table VII.

Determinants of the Variability in CAR

This table reports standardized regression results of the variability in $CAR on deal size and the acquirer's market capitalization. Columns (1) to (3) present regressions using a sample based on individual acquisitions. The dependent variable is the logged absolute value of $CAR. $CAR is the change in the acquirer's market capitalization around the announcement event (window of $[-1, 1]$). In columns (4) to (7), observations are defined over bins of acquisition announcements, and the dependent variables are the logarithms of the standard deviation of acquisitions' $CAR within each bin. In columns (4) and (5), binning is based on the interaction of rounded logged deal size and rounded logged acquirer market capitalization (both rounded to the nearest 0.1). In columns (6) and (7), acquisitions are binned by acquirers. We require that bins have at least five acquisitions. All regressions include an intercept, which is not reported. We include the following deal characteristic controls (medians within each bin): leverage, free cash flow scaled by lagged assets, Tobin's Q, and previous-quarter market-adjusted stock returns, as well as dummy variables for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals. We measure firm-level characteristics in the year before deal announcement. All variables are standardized to have a standard deviation of one. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	log(abs($\$CAR$))			log(Std($\$CAR$))		log(Std($\$CAR$))	
Sample:	All			Bins Based on AcqMktCap & DealSize		Acquirer-Based Bins	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log(Deal size)	0.106***	0.120***	0.123***		0.109***		0.055***
	(0.010)	(0.009)	(0.007)		(0.007)		(0.012)
log(Acquirer market cap)	0.757***	0.742***	0.803***	0.982***	0.917***	0.822***	0.776***
	(0.015)	(0.012)	(0.021)	(0.006)	(0.009)	(0.017)	(0.027)
Acquirer FE	No	No	Yes	No	No	No	No
Deal characteristic controls	No	Yes	Yes	No	No	No	No
Observations	39,585	39,585	35,687	1,236	1,236	2,509	2,509
Adjusted R^2	0.684	0.694	0.717	0.973	0.981	0.740	0.743

and median logged acquirer market cap. The ratio of these coefficients reveals that acquirer market cap is about eight times more influential than deal size in explaining the variation in $CAR. Columns (6) and (7) use a sample based on a different binning definition: deals are placed in bins were announced by a specific acquirer. This specification is designed to answer the question: for the same acquirer, to what degree does $CAR vary with deal size? The results show that even grouping deals at the acquirer level, acquirers' median size explains 13× more variation in $CAR than their median deal size.

The overall conclusion from the results presented in Table VII is that the information contained in $CAR is influenced by acquirer-related information 6× to 13× more than deal-related information.

Thus far we show that the variation in $CAR depends largely on the size of the acquirer.

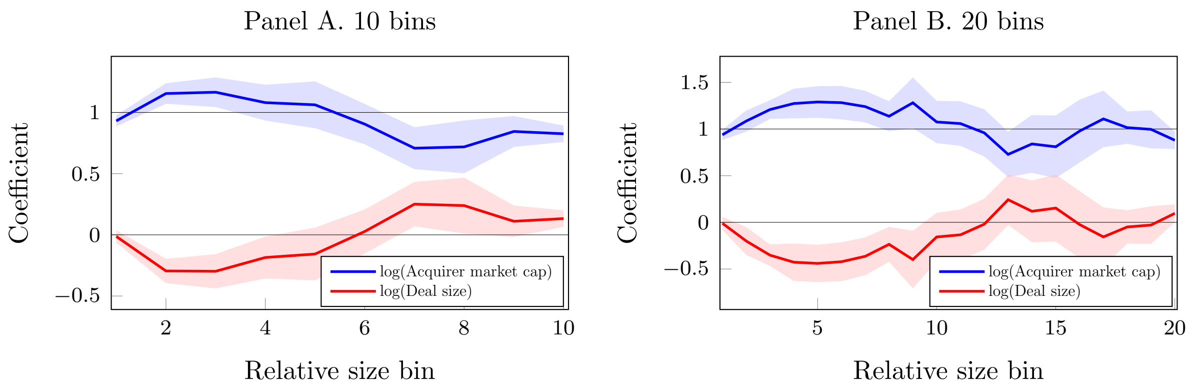

Figure 11. Variability in CAR, by relative size.
This figures plots the estimated coefficients from equation (3) across relative size deciles. Panels A and B show results for specifications with 10 and 20 relative size bins, respectively. Blue lines represent estimates for the acquirer-size component (γ_l), and red lines represent estimates for the deal-size component (β_k). The shaded areas reflect 95% confidence intervals. These plots illustrate how the importance of deal size relative to acquirer size evolves with the relative size of the deal.

This fact raises the possibility that \$CAR in deals of large relative size (i.e., deal size is large relative to the acquirer) may contain more information about the deal. To the extent this is the case, we may still be able to learn about NPV by studying deals of large relative size. To assess this possibility, we estimate the coefficients in equation (3) independently for different relative size deciles,

$$\begin{aligned}\log|\$CAR_i| \quad &= \quad \alpha + \sum_{k=1}^{10} \beta_k I(RelativeSize_i = k) \cdot \log(DealSize_i) \\ &\quad + \sum_{k=1}^{10} \gamma_k I(RelativeSize_i = k) \cdot \log(MarketCap_i) + \varepsilon_i.\end{aligned} \tag{4}$$

We plot the coefficients and their confidence intervals in Figure 11. The results show that for deals below the median relative size, the magnitude of \$CAR is determined exclusively by the acquirer's size and not by the size of the deal. As relative size increases, the relative importance of deal-related information increases and that of acquirer-related information decreases. However, the rise in the importance of deal-related information never reaches the critical point of dominating \$CAR. At its best (around a relative size of one, that is, "a merger of equals"), deal-related information explains about half of the information contained in \$CAR.

Overall, these findings imply that CAR often contains more information about the acquirer than the target. In other words, in terms of $\$CAR = NPV + X$, X plays a significantly more important role than NPV in CAR. As a result, without knowing what the market learns about the standalone acquirer, researchers cannot extract NPV from CAR.

D. Practical Implications

We argue that attempts to extract NPV from $\$CAR$ yield biased and unreliable estimates. As we discuss above, CAR comprises both the deal's NPV and an acquirer-specific component X_i that represents information unrelated to the deal's NPV, such as revealed private information or information about misvaluation. We can express this observation as

$$\$CAR_i = NPV_i + X_i. \tag{5}$$

Researchers may try to isolate NPV_i by regressing $\$CAR_i$ on observable acquirer characteristics Z_i (e.g., size or past performance),

$$\$CAR_i = \alpha + \beta Z_i + \epsilon_i, \tag{6}$$

and interpreting α as the average NPV, assuming ϵ_i is random noise. However, this approach is flawed because the error term ϵ_i includes both the residual of NPV_i after accounting for Z_i and the unobservable X_i:

$$\epsilon_i = (NPV_i - \alpha - \beta Z_i) + X_i. \tag{7}$$

Since X_i is unobservable, it cannot be fully captured by Z_i and remains in the error term. In addition, X_i may be correlated with NPV_i, as both reflect strategic information revealed at the acquisition announcement. This correlation introduces endogeneity, making the error term ϵ_i correlated with NPV_i and leading to omitted variable bias.

Therefore, without directly observing or accurately modeling X_i, we cannot reliably extract NPV_i from $\$CAR_i$. The intertwined effects of X_i and NPV_i within $\$CAR_i$ make interpreting α as the average NPV invalid. Thus, controlling for observables alone cannot disentangle NPV_i from $\$CAR_i$, rendering $\$CAR_i$ an unsuitable proxy for the true NPV of the deal.

IV. Conclusion

Whether cumulative abnormal returns (CARs) around acquisition announcements are a reliable measure of net present value (NPV) has important implications for corporate finance, the judicial system, and the economy. If CAR were a reliable barometer of the value created in executive decision-making, it should be harnessed to improve economic efficiency. For example, executives' incentive pay and promotion could be tied directly to the value created in specific deals they worked on; firm directors could use value destruction, as indicated by negative CAR, as a cause to dismiss the executive team; and the judgment of antitrust investigators could be questioned if legal actions by the Department of Justice's antitrust division are uncorrelated with the information conveyed in CAR (Gao et al., 2017).

Our tests reveal that CAR is not meaningfully correlated with ex-post outcomes. We use four measures of ex-post acquisition outcomes: two transaction-level measures—goodwill impairment and deal completion—and two acquirer-level measures of ex-post performance, short-term and long-term abnormal ROA. Despite capturing different aspects of acquisition performance, these measures are correlated.

We first document that CAR has no meaningful correlation with transaction-specific outcomes or measures of the acquirer's future performance, implying that CAR is a poor measure of value creation or destruction. We next show that, unlike CAR, a standard list of deal and acquirer characteristics known at the time of the announcement can predict acquisition outcomes reasonably well. We use this superior predictability to assess the relation between CAR and the predictable component (using these characteristics) of acquisition outcomes, but we find no relation. Thus, announcement returns fail to reflect all information available at acquisition announcement and are likely unable to capture expected acquisition outcomes sufficiently. We show that the poor performance of CAR cannot be fully explained by anticipation, truncation, selection, and feedback effects. Further, using CAR results in unreliable inferences regarding the types of transactions (i.e., stock versus cash deals, public versus private targets, or large versus small acquirers) that create or destroy value.

Why does CAR fail to capture NPV? Although we do not claim to definitively answer this question, we argue that CAR is likely dominated by non-NPV information. Specifically, since acquisition decisions are endogenous, their announcement must reveal information about their triggers. Our empirical investigation shows that the variability in CAR is often larger than one would expect if it only measures NPV. Furthermore, upon investigating the variability in CAR, we document that the dollar magnitude of CAR comoves with acquirer size to a greater extent than with deal size, implying that CAR likely contains materially more information about the acquirer than the target. As NPV is related primarily to deal characteristics, the trigger (often associated with the acquirer) likely dominates NPV in determining CAR. As a result, one cannot easily extract information about the value created in the announced transaction.

In sum, the results suggest that researchers should avoid using CAR to measure deal quality. CAR is a poor predictor because it is swamped by information related to the standalone acquirer (e.g., the trigger that prompted the announcement), with the information contained in CAR influenced by acquirer-related information 6× to 13× more than deal-related information. Researchers should instead consider using a vector of publicly available characteristics that do a better job of predicting acquisition outcomes (e.g., Ellahie et al., 2025). More research is warranted on this issue.

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Internet Appendix for "The (Missing) Relation Between Acquisition Announcement Returns and Value Creation"

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In this internet appendix, we first describe the sample construction in Section I, then provide additional tests for our main results in Section II, and validate impairment as a measure of value destruction in Section III.

I. Sample Construction

Our sample of mergers and acquisitions comes from the Thomson Reuters Securities Data Company (SDC) Domestic Merger and Acquisition database. Our sample begins in 1980 and ends in 2018, allowing us to track acquisition outcomes in the five years after the transaction. We include transactions that satisfy the following criteria: (i) the merger or acquisition was announced on or after January 1, 1980, and completed by December 31, 2018, (ii) the acquirer is a U.S. company, (iii) the acquirer is a publicly traded firm, (iv) the deal is not classified as a leveraged buyout, spinoff, repurchase, self-tender, recapitalization, privatization, stake purchase, or acquisition of partial or remaining interest, (v) the percentage of shares acquired (or sought for not completed deals) is at least 50%, (vi) the percentage of shares held by the acquirer six months before the announcement is less than 50%, (vii) Compustat has accounting data on the bidder, and the Center for Research in Security Prices (CRSP) database has stock data for the month of the deal announcement, and (viii) the deal value is nonmissing in the SDC database. These requirements result in an initial sample of 47,543 deals, of which 42,354 are completed, 2,227 are withdrawn (the deal outcome is known in these cases), and 2,962 are not completed but not withdrawn (e.g., the transaction may be pending, or the outcome is unknown; we exclude these deals from the analysis). Table IA.I below lists the steps and number of deals remaining after each filter.

For each transaction, we compute acquirer announcement returns. We estimate daily abnormal returns using a market model and a CRSP value-weighted index (r_{mt}). The market model parameters, α_i and β_i, are estimated from 361 to 61 trading days before the deal announcement day. CARs are then computed by summing the daily abnormal returns over

Table IA.I.

Sample Construction

This table reports the filters applied to the Thomson Reuters Securities Data Company (SDC) Domestic Merger and Acquisition database.

Step	Filter Description	# of Deals		
		Completed	Not Completed	Total
1	Date announced: 1/1/1980 to 12/31/2018			
2	Acquirer country: U.S.			349,687
3	Acquirer is public	117,566	48,504	166,070
4	Eliminate leveraged buyouts, spinoffs, repurchases, self-tenders, recapitalization, privatization, stake purchase, acquisitions of partial or remaining interest	103,015	23,443	126,458
5	Percent of shares acquired ("sought" for deals not completed) in the transaction: 50 to Hi	99,939	21,538	121,477
6	Percent of shares held by acquirer six months before the announcement: 0 to 49	99,881	21,527	121,408
7	Drop duplicate deals in terms of the announcement and effective date, acquirer and acquirer parent name, deal value, target and acquirer SIC code, and % of stock as method of payment	97,745	21,116	118,861
8	Require match to CRSP	81,086	13,850	94,936
9	Require match to Compustat	80,444	13,630	94,074
10	Require CAR$[-1, 1]$ measure to be nonmissing	78,406	13,256	91,662
11	Deal value is nonmissing	42,354	5,189	47,543

various event horizons. We estimate CARs over a three-day $[-1, 1]$ and an 11-day period $[-5, 5]$ surrounding each acquisition announcement, and over the entire acquisition process beginning two days before the announcement and ending two days following the deal completion $[\text{Announcement} - 2, \text{Close} + 2]$. CAR may understate absolute value expectations if the probability of deal completion is uncertain; using the long window that includes the deal completion date overcomes this issue as the probability of completion has moved toward one.

We construct transaction- and firm-level proxies for acquisition outcomes to assess the core relation between acquisition announcement returns and value creation. Due to differences in data availability across outcome measures, the sample sizes vary for each measure. In the next two subsections, we provide further detail on sample filters and the number of observations for the various outcome variables.

Table IA.II.

Abnormal ROA and Deal Completion Samples

This table reports the filters applied to obtain the short-term abnormal ROA (Panel A), long-term abnormal ROA (Panel B), and deal completion (Panel C) samples.

Filter Description	# of Deals
Panel A. Firm-Level Short-Term abROA Sample	
Short-term abROA to be nonmissing	31,266
Controls nonmissing	28,710
Panel B. Firm-Level Long-Term abROA Sample	
Long-term abROA to be nonmissing	24,497
Controls nonmissing	22,577
Panel C. Transaction-Level Deal Completion Sample	
Deal withdrawn to be nonmissing	44,825
Controls nonmissing	39,585

A. Abnormal ROA and Deal Completion Samples

Table IA.II presents the additional filters we use to obtain the short-term abnormal ROA (Panel A), long-term abnormal ROA (Panel B), and deal completion (Panel C) samples. We require the particular outcome measure to be nonmissing and all firm-level control variables to be nonmissing, including log market capitalization, leverage, and free cash flow scaled by lagged assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, and a set of dummy variables for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals.

B. Goodwill Impairment Data

In an acquisition, the acquirer exchanges consideration (cash, stock, or both) for the target's stock or assets. In most cases, the acquirer pays more than the value of the identifiable assets of the target. As such, on the acquirer's balance sheet, the value of the target is recorded as a combination of the value of the identifiable assets and goodwill. Goodwill is the account on the acquirer's balance sheet that captures the difference between the consideration paid in the acquisition and the value of the identifiable net assets:

$$Goodwill_i = Price_i - Value(Identifiable\ Assets)_i. \tag{IA1}$$

From an economic point of view, goodwill can include the value of (i) a standalone going-concern element, which reflects the higher value of a collection of assets over assets held independently, (ii) a synergy element, which reflects the value from combining the acquirer and target businesses, and (iii) any overpayment or overvaluation of the stock consideration (Johnson and Petrone, 1998; Henning et al., 2000).

Accounting rules require occasional downward adjustments to the goodwill account (goodwill write-downs or impairments). The impairment of goodwill can arise for the following reasons: overvaluation of existing target assets, overestimated synergies, or the inability to realize synergies due to firm, industry, or economy-wide shocks.

The Financial Accounting Standards Board (FASB) published a new financial accounting standard, SFAS 142, effective December 2001, intended to increase transparency and generate goodwill balances that better reflect the underlying economic value of the acquisition on an ongoing basis (Foster et al., 2003). SFAS 142 introduced four significant changes to the existing rules. First, goodwill assignment and impairment tests must be conducted at the "reporting unit" level (an operating segment or one component level below a segment), making it easier to identify the goodwill recorded and the source of future impairments at the transaction level. Second, acquirers can "write up" the target's assets to fair value at the time of the acquisition.[2] Third, goodwill is no longer amortized but is considered an asset that can stay on the firm's balance sheet indefinitely.[3] Fourth, firms must conduct impairment tests to determine whether the value of goodwill changes following "material" events, and for many years in our sample, annual impairment tests were conducted. If

[2]Identifiable intangible assets, such as patents and customer lists, are no longer included in goodwill balances.

[3]Before SFAS 142, acquisition goodwill was amortized over a maximum of 40 years.

the appraised value is less than the recorded value, then a goodwill "impairment" occurs. The amount of goodwill is reduced on the balance sheet, and an impairment expense is incurred on the income statement as a component of income from continuing operations. In September 2011, FASB modified SFAS 142 so that formal valuations to produce comparisons of fair value and carrying value of a reporting unit are only required when certain qualitative indicators of impairment exist; thus, impairment tests are no longer required to be conducted annually.[4]

Goodwill reflects the premium paid over the identifiable assets in nominal terms. Thus, the impairment of goodwill indicates that the remaining value of the target is lower than the nominal value paid a few years earlier at acquisition. A large goodwill impairment, therefore, likely captures value destruction. Due to the increased precision and timeliness of goodwill reporting required by SFAS 142, we can construct goodwill balances and impairment at the transaction level, yielding a direct and quantifiable representation of transaction-specific acquisition failure.

To construct the goodwill nonimpairment sample, we start with the 42,354 completed deals described in Table IA.I. To align with the SFAS 142 roll-out, we retain transactions announced between 2003 and 2018. We include additional filters not imposed on our samples that use ROA and completion data. Specifically, we require the transaction value to exceed

[4]Before the 2001 rule change, SFAS 121 prescribed only nonroutine impairment tests following certain triggering events that indicated that goodwill might no longer be recoverable. Under SFAS 142, the impairment amount must be determined using a fair value approach based on a two-step impairment test. In the first step, the reporting unit's fair value is compared to the book value. A second step is performed if the fair value is less than the book value. In the second step, the fair value of the unit's (nongoodwill) net assets is determined, and the fair value of goodwill is the difference between the unit's fair value and the fair value of the unit's identifiable net assets. The impairment amount is the excess of the book value of goodwill and the newly assessed fair-value estimate of goodwill. Firms often use a weighted combination of discounted cash flow, public comparable company multiples, and precedent acquisition transaction multiples valuation as techniques to determine fair value.

Table IA.III.

Sample Construction for Goodwill Impairments

The table shows the sample construction. Panel A includes transactions from SDC that were announced from January 2003 and completed by December 2018. Sample filters are described in the text. Panel B describes the classification of the "potentially impaired" transactions. For this sample, we read through the 10-K Notes and Factiva to identify the target(s) that triggered the impairment. Panel C shows the final sample composition.

Panel A. Sample Construction	
# Deals	6,767
# Transactions without acquiring firm-level impairment within 5 years of deal effective date	5,229
# Transactions "potentially impaired" with acquiring firm-level impairment within 5 years	1,538

Panel B. Classification of "Potentially Impaired" Transactions	
Deals classified in goodwill impairment sample	
Impairment linked directly to target and exact impairment amount can be identified	543
Impairment linked directly to target, other targets in firm or segment also linked	126
Target is in impaired segment, target goodwill > 20% of segment goodwill	277
Total (% of deals potentially impaired)	946 (62%)
Deals classified in no goodwill impairment sample	
Impairment is not in target's segment or 10-K specifies another target as a source of impairment	262
Total (% of deals potentially impaired)	262 (17%)
Deals excluded from sample: cannot classify as impaired or not impaired	
Target is in the impaired segment, but target goodwill is < 20% of segment goodwill	159
No information on goodwill created from acquisition	136
No information on the source of impairment	17
No goodwill created from acquisition	18
Total (% of deals potentially impaired)	330 (21%)

Panel C. Final Goodwill Impairment Sample Summary	
Impairment sample	946
Nonimpairment sample	5,491
Total	6,437
Controls nonmissing	6,128
Final impairment sample	906
Final nonimpairment sample	5,222

\$10 million and to be at least 5% of the acquirer's market capitalization at the end of the fiscal year before the deal was announced. These filters allow for a more precise measure of impairment. For very small deals (both in dollar and relative terms), it is difficult to determine the source of the impairment and, in many instances, the amount of goodwill originally produced from the transaction. These filters yield 8,367 transactions.

Next, we link sample firms to Compustat goodwill data and identify all acquirers with firm-level goodwill impairments. In this step, we exclude transactions with missing assets in the year of deal close and transactions with missing or zero goodwill in both the year of and the year after close. This yields 6,767 transactions.

To identify goodwill impairments in the data, we follow Bens et al. (2011). We initially screen for potential goodwill impairments by flagging instances in which the Compustat variable "Impairments of Goodwill Pretax" (item 368 or GDWLIP) is at least 5% of previous-year total acquirer assets in any year between the year of the acquisition close and five years following. This requirement ensures that the impairment event has detectable valuation effects. Of the 6,767 transactions in the sample, 1,538 deals are associated with a firm-level impairment within five years of the deal's effective date. This sample construction is summarized in Table IA.III.

The Compustat goodwill and nonimpairment data are based on aggregate firm-level data, so it is not directly possible to identify transaction-specific measures. To identify the amount of goodwill recorded for each transaction in our sample, we read through the Notes to Consolidated Financial Statements in the first 10-K filing after the deal's effective date. Following an acquisition, the notes include an "Acquisitions" or "Business Combinations" section that presents the preliminary allocations of the aggregate purchase price based on the assets and liabilities estimated at fair values to line items, such as net tangible assets, identifiable intangible assets, and goodwill. In this step, we also determine the recording unit for which the goodwill has been allocated.

For the years with indicated firm-level impairment, we use the Notes to Consolidated Financial Statements to determine whether and how much of the impairment is due to the specific transaction in our sample. We also read news articles and press releases in Factiva if more information is required.

In many instances, the source and the amount of the impairment assigned to each target are straightforward. In the most uncomplicated scenarios, the targets with goodwill impairment and the amount of target-level impairment are directly listed in the Notes section of the 10-K, or the firm writes off the entirety of its goodwill balance. In other cases, the Notes list the reporting unit(s) that suffered the loss. We search the 10-K, the Notes, and Factiva in the year of the goodwill allocation to determine the reporting unit(s) to which the target's goodwill is allocated. If target goodwill is 100% of the impaired reporting unit goodwill, the impairment attributable to the target is straightforward. For 543 transactions in the potentially impaired sample of 1,538, we can link the impairment directly to the target and determine the exact impairment amount.

In 126 other instances, the target is listed as impaired in the Notes, but the impairment amount is unknown due to other targets also triggering the impairment. If the impairment is at the reporting-unit level, we set target impairment equal to unit impairment $\times$ (target goodwill/unit goodwill). If the impairment is reported at the consolidated firm level, we set target impairment equal to total impairment $\times$ (target goodwill / total goodwill). Note that our variable of interest is the occurrence of an impairment, which will be unaffected by errors in the estimated impairment size.

For some transactions, we are uncertain as to the source and amount of the impairment. If the target is in the impaired segment and target goodwill is at least 20% of segment goodwill, we conclude that it is reasonably likely that the target has been impaired and include these 277 transactions in the impairment sample. We estimate the size of the impairment using the relative size of the target goodwill as described above. Therefore, of the 1,538 "potentially impaired" deals, we can classify $543 + 126 + 277 = 946$ as "impaired deals."

For 262 transactions flagged as potentially impaired, we determine that the impairment is not in the target's segment or that other targets have been listed as the source of the impairment. These transactions are included in the nonimpairment sample.

We cannot reasonably classify some transactions as impaired or not impaired, and thus, exclude them from the sample. We exclude 17 deals for which the 10-K provides no details on the source of the impairment and another 159 deals where the target is in the impaired segment but its related goodwill is less than 20% of segment goodwill. (We run robustness tests in Table IA.VI and show that our results are unaltered if these deals are included in the sample and classified as either impaired or not impaired.) We exclude 136 deals that lack information in the Notes on the amount of goodwill created from the particular acquisition and another 18 deals where goodwill was not created from the acquisition.

Table IA.III, Panel B, shows that we could successfully link impairment events to specific transactions. Of 1,538 transactions flagged as potentially impaired, we can credibly classify 62% as impaired and 17% as not impaired, and we cannot classify 21% of transactions. Moreover, for 71% ((543+126)/946) of the transactions classified as impaired, we know unambiguously the source of the impairment. To our knowledge, we are the first to construct a comprehensive data set that includes transaction-specific goodwill balances and transaction-specific impairment outcomes in the post-SFAS 142 period. Hayn and Hughes (2006) also trace initial goodwill balances and subsequent impairments at the transaction level, but they exclude 55% of transactions due to insufficient information. Overall, they focus largely on the pre-SFAS 142 period, when the disclosure of initial goodwill and the source of the impairment was generally less comprehensive.

Table IA.III, Panel C, shows that the sample (6,437) is further reduced when we require announcement returns to be nonmissing (6,435) and controls to be nonmissing (6,128). Thus, our final sample for goodwill impairment analyses is 6,128 transactions, of which 906 are classified as impaired, and 5,222 are classified as not impaired.

Table IA.IV shows summary statistics for this sample. We find that 14.8% of transactions are impaired by year 5 and that, conditional on impairment, 79% of the impairments occur by year 3. In Table IA.IV, Panel B, we report goodwill and impairment statistics for the sample of 906 deals associated with transaction-level impairment. The dollar values of goodwill impairments are large. On average, acquirers write down 83% of the original goodwill allocated to the deal, and the impairment size is about 11% of the acquirer's assets.

Table IA.IV.

Sample Statistics

This table provides summary statistics. Panel A shows sample statistics for the percentage of transactions with goodwill impairment within five years of the deal's effective date. Panel B shows statistics for the 906 transactions in the impairment sample.

Panel A. Transaction-Level Impairment Percentages		
	%	N
Year 0–1	5.5%	339
Year 2–3	6.1%	374
Year 4–5	3.1%	193
Impaired by year 5	14.8%	906
Not impaired by year 5	85.2%	5,222
Total completed deals	100.0%	6,128

Panel B. Transaction-Level Impairment Statistics		
	Mean	Std dev
$ Goodwill ($m)	422.3	1,252.8
Goodwill/Net purchase price	69%	73%
Goodwill/Total assets	14%	12%
Impairment $ loss ($m)	−242.1	643.7
Impairment/Goodwill	83%	35%
Impairment/Purchase price	57%	76%
Impairment/Total assets	11%	11%

II. Predicting Outcomes: Additional Tests

We consider whether our tests in Section II of the main article that indicate a lack of correlation between outcomes (e.g., nonimpairment, abnormal ROA, and completion) and CAR are robust to alternative definitions of CAR and outcomes and to alternative measurement periods for the outcome variables of interest, across industries, deal types, and firm characteristics.

Table IA.V below replicates Table II, columns (1), (4), and (5), but redefines CAR using a longer event window $[-41, 1]$ to capture potential leakage or deal anticipation (e.g., Ahern and Sosyura, 2014; Betton et al., 2014). The results in all four panels confirm the lack of relation between CAR and outcomes.

Panels A and B of Table IA.VI replicate Table II, Panel A, but we redefine the nonimpairment dummy in two alternative ways. For 159 deals in which the target is in the impaired segment but the goodwill associated with the target is less than 20% of segment goodwill, we cannot reasonably classify these deals as impaired or not impaired (see Table IA.I), so we exclude them in our main tests. In Panel A, we retain these deals and assume they did not result in impairment; in Panel B we retain these deals and assume they resulted in goodwill impairment. Table IA.VI, Panel C, replicates Table II, Panel B, but uses industry-adjusted ROA (measured as ROA minus the median Fama-French 12-industry ROA and averaged over the three years after deal completion) rather than short-term abnormal ROA. In Panel D, we include deals that still may be pending or for which the outcome is unknown as the outcome variable. The results in all four panels confirm the lack of relation between CAR and alternative definitions of ex-post outcomes.

Firm attrition might become an issue when we track firm performance several years after the deal (i.e., using ROA as the outcome measure in Table II, Panels B and C). To address this possibility, we adjust the ROA computation. We calculate abnormal ROA by taking all years with ROA data available up to six years after acquisition. Then, we run weighted least squares (WLS) regressions of abnormal ROA on acquirer CAR. In each regression, the

Table IA.V.

Acquirer CAR$[-41, 1]$ and Acquisition Outcomes

This table reports results from regressions of acquisition outcome measures on acquirer cumulative abnormal returns (CAR), measured over $[-41, 1]$. The dependent variable is a nonimpairment dummy (Panel A), short-term abnormal ROA (Panel B), long-term abnormal ROA (Panel C), or a completion dummy (Panel D). In column (1), CAR is the only independent variable. In addition to CAR, column (2) includes characteristics. Column (3) further includes year and industry fixed effects as independent variables. The characteristics used as controls include log market capitalization, leverage, free cash flow scaled by lagged assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, and a set of dummy variables for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals. We measure firm-level characteristics in the year before the deal announcement. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)
Panel A. Probability of Nonimpairment ($N = 6,128$, DV: Nonimpairment Dummy)			
CAR$[-41, 1]$	0.039	0.067	0.066
	(0.061)	(0.059)	(0.061)
Controls	—	Char	Year, Ind, Char
Adjusted R^2	0.000	0.036	0.089
Panel B. Short-Term Abnormal ROA ($N = 28,710$, DV: Short-Term Abnormal ROA)			
CAR$[-41, 1]$	0.001	0.005*	0.004
	(0.003)	(0.003)	(0.003)
Controls	—	Char	Year, Ind, Char
Adjusted R^2	0.000	0.026	0.078
Panel C. Long-Term Abnormal ROA ($N = 22,577$, DV: Long-Term Abnormal ROA)			
CAR$[-41, 1]$	0.004	0.006	0.004
	(0.004)	(0.005)	(0.004)
Controls	—	Char	Year, Ind, Char
Adjusted R^2	0.000	0.035	0.109
Panel D. Probability of Completion ($N = 39,585$, DV: Completion Dummy)			
CAR	−0.003	0.009	0.007
	(0.005)	(0.006)	(0.006)
Controls	—	Char	Year, Ind, Char
Adjusted R^2	0.000	0.056	0.111

number of years of available ROA data is used as the weight. The results, presented in Table IA.VII, are similar to the main findings.

We next define our ex-post outcomes at various periods following the deal completion date. We again replicate Table II, this time redefining the dependent variable each year

Table IA.VI.

Acquirer CAR and Acquisition Outcomes: Alternative Definitions

This table presents regression results examining the relationship between acquirer CAR and various acquisition performance measures: alternative nonimpairment dummy definition 1 (Panel A), which classifies deals that lack information as not impaired, and alternative nonimpairment dummy definition 2 (Panel B), which classifies deals that lack information as impaired, industry-adjusted ROA (Panel C), and a completion status dummy that includes unknown or pending deals (Panel D). Columns (1) to (3) use CAR as the sole independent variable. Column (4) incorporates additional characteristics, while column (5) adds year and industry fixed effects. Column (6) includes only characteristics, and column (7) combines characteristics with year and industry fixed effects. Control variables include log market capitalization, leverage, free cash flow scaled by lagged assets, Tobin's Q, prior-quarter market-adjusted stock returns, relative deal size, and dummy variables for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

CAR window:	$[-1,1]$	$[-5,5]$	$[\mathrm{A}-2,\mathrm{C}+2]$	$[-1,1]$		n.a.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Probability of Nonimpairment (Alt Definition 1, $N = 6,278$, DV: Nonimpairment Dummy)							
CAR	−0.002	0.001	0.085**	0.110	0.106	Controls	Controls
	(0.106)	(0.069)	(0.037)	(0.088)	(0.101)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	0.000	0.000	0.002	0.027	0.081	0.027	0.081
Panel B. Probability of Nonimpairment (Alt Definition 2, $N = 6,278$, DV: Nonimpairment Dummy)							
CAR	−0.023	0.010	0.088**	0.076	0.072	Controls	Controls
	(0.103)	(0.082)	(0.040)	(0.081)	(0.085)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	0.000	0.000	0.002	0.036	0.086	0.036	0.086
Panel C. Industry-Adjusted ROA ($N = 30,060$, DV: Industry-Adjusted ROA)							
CAR	−0.038	−0.009	0.015*	0.028	0.030	Controls	Controls
	(0.031)	(0.020)	(0.007)	(0.019)	(0.017)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	0.000	0.000	0.001	0.245	0.327	0.245	0.326
Panel D. Probability of Completion ($N = 41,951$, DV: Completion Dummy)							
CAR	−0.017	0.004	—	0.029	0.022	Controls	Controls
	(0.029)	(0.017)		(0.021)	(0.020)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	0.000	0.000	—	0.062	0.072	0.062	0.072

relative to the deal's effective date (up to five years) for nonimpairment and abnormal ROA, and relative to the announcement date for completion. In Figure IA.1, we plot the coefficients on CAR and 95% confidence intervals. These results show that even though CAR performs

Table IA.VII.

Acquirer CAR$[-1, 1]$ and Abnormal ROA: Weighted Least Squares Regressions

This table reports results from weighted least squares (WLS) regressions of abnormal ROA on acquirer cumulative abnormal returns (CAR). The dependent variable is abnormal ROA, calculated by taking all years with ROA data available up to six years after acquisition. In each regression, the number of years of available ROA data is used as the weight. In column (1), CAR is the only independent variable. In addition to CAR, column (2) includes characteristics. Column (3) further includes year and industry fixed effects as independent variables. The following characteristics are used as controls: log market capitalization, leverage and free cash flow scaled by lagged assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, and a set of dummy variables for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals. We measure firm-level characteristics in the year before the deal announcement. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Abnormal ROA up to Six Years ($N = 32,937$)		
	(1)	(2)	(3)
CAR$[-1, 1]$	−0.008	0.011	0.010
	(0.015)	(0.014)	(0.012)
Controls	—	Char	Year, Ind, Char
Adjusted R^2	0.000	0.033	0.072

better for short-term outcomes, it is still an ineffective predictor of value creation.

We next examine whether the combined returns of the target and acquirer, which reflect total expected synergy gains (as opposed to the division of synergy gains), can predict outcomes. We zoom in on the subsample of transactions with public targets (which represents 15 to 18% of the nonimpairment, abnormal ROA, and completion samples) and compute combined dollar gains by summing the product of acquirer CAR and acquirer market capitalization in the year prior to the deal announcement date and the product of target CAR and target market capitalization in the year prior to the deal announcement. We compute combined percentage returns by dividing combined dollar gains by the sum of acquirer and target market capitalization. The results for combined CAR$[-1, 1]$ are reported in Table IA.VIII. The results are similar to those reported in Table II: the coefficient on combined CAR is not statistically significant when nonimpairment and short- and long-term abnormal ROA are the outcome variables and statistically significant when completion is the outcome variable but with low explanatory power.

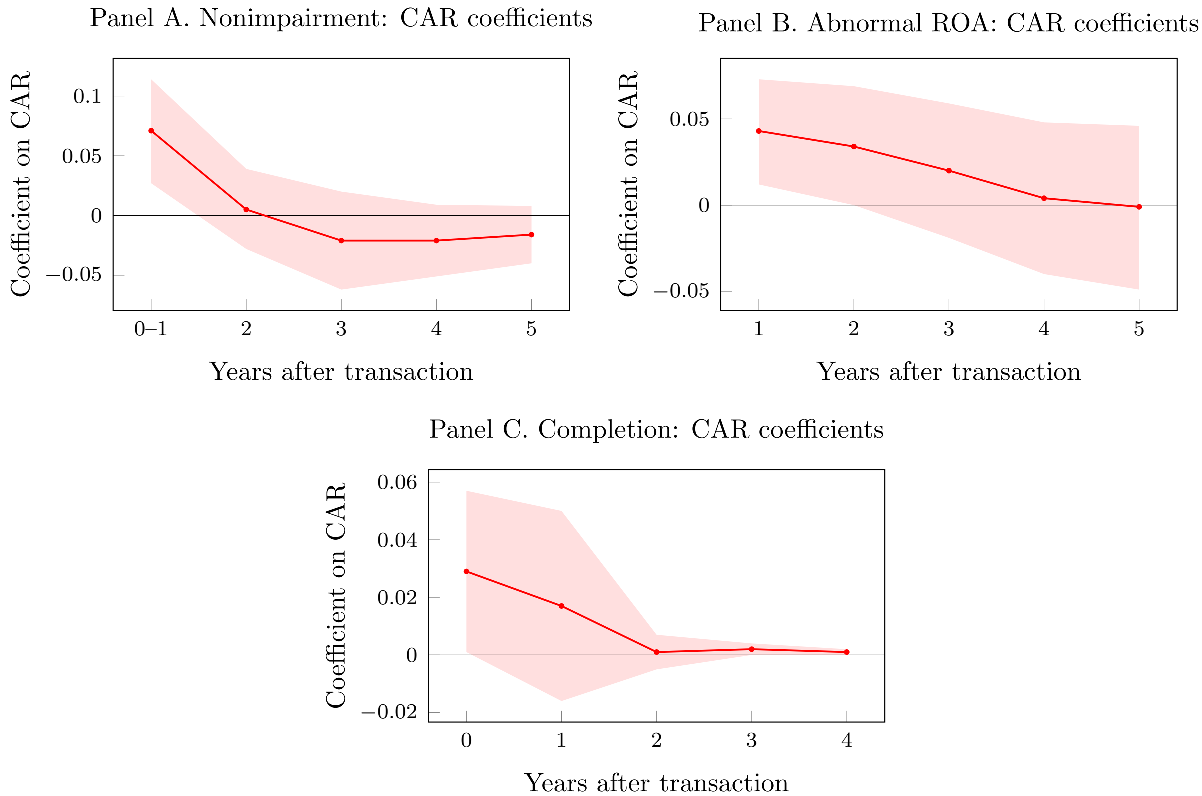

Figure IA.1. Predictive performance of CAR by year.
Panel A reports the coefficients of OLS regressions of the nonimpairment dummy on CAR$[-1, 1]$. Panels B and C are similar, except the dependent variable is abnormal ROA and a completion dummy, respectively. In Panel A, in the Year 1 regression, the dependent variable is the nonimpairment dummy. In the Year 2 regression, we exclude firms with impaired transactions within one year, and the dependent variable is the nonimpairment dummy in Year 2. The Year 3 regression excludes firms with impaired transactions in Years 1 or 2, and the dependent variable is the nonimpairment dummy in Year 3. Year 4 and Year 5 regressions are computed similarly. In Panel B, we measure abnormal ROA at the end of Years 1, 2, 3, 4, and 5 following the deal completion. In Panel C, we measure deal completion at the end of Years 0, 1, 2, 3, and 4 since the announcement. The shaded region indicates the 95% confidence interval.

We next replicate Table II for each Fama-French 12-industry classification. We report the results in Figure IA.2. Panels A to D show the coefficient and 95% confidence intervals for regressions of outcomes on CAR based on the specification in Table II, column (5), for each of the 12 industries. Panel A shows that the coefficient on CAR in regressions of nonimpairment on CAR is only significant at the 5% level (and the correct sign) for the "other" industry (industry 12). Panel B shows that when short-term abnormal ROA is the outcome variable, CAR is only significant (and the correct sign) at the 5% level for the "business equipment" (industry 1) and "nondurable" (industry 8) industries and is not statistically significant

Table IA.VIII.

Acquirer CAR and Acquisition Outcomes: Combined CAR

This table reports results from regressions of acquisition outcome measures on combined cumulative abnormal returns (Combined CAR). Combined CAR is the sum of the product of acquirer CAR and acquirer market capitalization in the year prior to the deal announcement and the product of target CAR and target market capitalization in the year prior to the deal announcement. The dependent variable is a nonimpairment dummy (Panel A), short-term abnormal ROA (Panel B), long-term abnormal ROA (Panel C), or a completion dummy (Panel D). In column (1), Combined CAR is the only independent variable. In addition to Combined CAR, column (2) includes year and industry fixed effects as well as characteristics as independent variables. Column (3) includes year and industry fixed effects, and characteristics as independent variables. The characteristics used in the controls include log market capitalization, leverage and free cash flow scaled by lagged assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, and a set of dummy variables for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals. We measure firm-level characteristics in the year before the deal announcement. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A. Probability of Nonimpairment ($N = 1,124$, DV: Nonimpairment Dummy)			
	(1)	(2)	(3)
Combined CAR$[-1,1]$	−0.024	0.220	Controls
	(0.218)	(0.144)	only
Controls	—	Year, Ind, Char	Year, Ind, Char
Adjusted R^2	−0.001	0.132	0.131
Panel B. Short-Term Abnormal ROA ($N = 4,174$, DV: Short-Term Abnormal ROA)			
Combined CAR$[-1,1]$	−0.028	0.019	Controls
	(0.017)	(0.028)	only
Controls	—	Year, Ind, Char	Year, Ind, Char
Adjusted R^2	0.001	0.180	0.180
Panel C. Long-Term Abnormal ROA ($N = 3,423$, DV: Long-Term Abnormal ROA)			
Combined CAR$[-1,1]$	−0.027	0.008	Controls
	(0.022)	(0.026)	only
Controls	—	Year, Ind, Char	Year, Ind, Char
Adjusted R^2	0.000	0.228	0.228
Panel D. Probability of Completion ($N = 6,093$, DV: Completion Dummy)			
Combined CAR	−0.192**	0.196***	Controls
	(0.081)	(0.062)	only
Controls	—	Year, Ind, Char	Year, Ind, Char
Adjusted R^2	0.001	0.173	0.173

for the remaining 10 industries. Panel C shows that when long-term abnormal ROA is the outcome variable, CAR is not statistically significant (and in one industry has the wrong sign) for all 12 industries. Similarly, Panel D shows that CAR correlates with completion for only

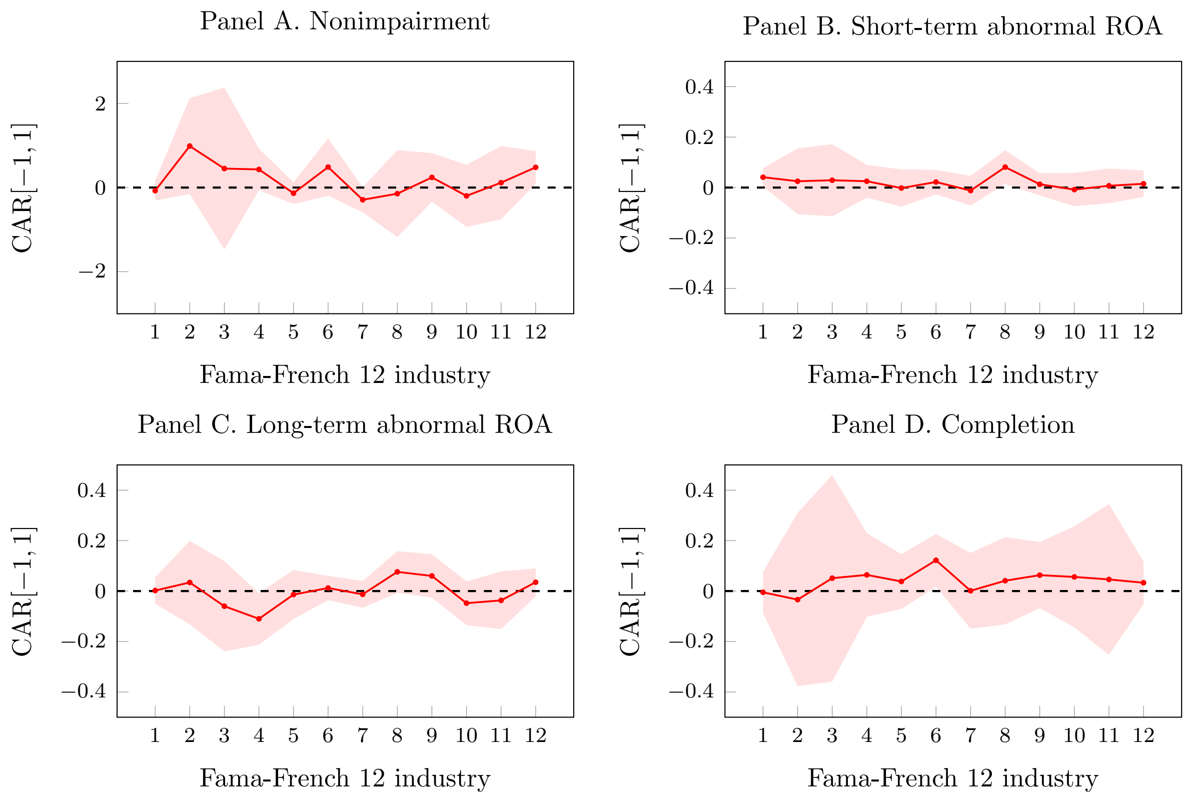

Figure IA.2. CAR and acquisition outcomes: Fama-French 12-industry classification.
This figure plots the coefficients and 95% confidence intervals for regressions of outcomes on CAR based on the specification in Table II, column (5), for each of the Fama-French 12 industries. Numbers 1 through 12 on the x-axis correspond to "business equipment," "chemicals and applied products," "consumer durable," "oil, gas, and coal extraction and products," "healthcare, medical equipment, and drugs," "manufacturing," "finance," "consumer nondurables," "wholesale, retail, and some services," "telephone and television transmission," "utilities," and "other," respectively. Panels A, B, C, and D use a nonimpairment dummy, short-term abnormal ROA, long-term abnormal ROA, and completion dummy, respectively, as the key independent variable. The red dots represent the point estimates, and the light red shading represents 95% confidence intervals.

one of the 12 industries (i.e., "manufacturing"). This result is in contrast to the statistically significant (at the 5% level) and positive (but economically weak) relation between CAR and completion reported in Table II. Although CAR correlates with outcomes in a few select industries, importantly, there is no overlap in these industries across outcome variables. The results provide additional evidence that the lack of correlation between CAR and outcomes is persistent across industries.

Table IA.IX replicates Table II across various deal types and acquirer characteristics. More specifically, we run OLS regressions of each outcome on CAR, year fixed effects, and industry fixed effects using 29 subsamples. We split the sample based on the following deal and firm characteristics: log market capitalization, leverage and free cash flow scaled by lagged assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, and a set of dummy variables for serial acquirer, stock-only, mixed-payment, cash-payment, diversifying, competed, hostile, high-tech, and public target deals. In Table IA.IX, we report the t-statistic of each regression in a given cell. The green shading identifies coefficients that are the correct sign and statistically significant at the 5% level or better. The results indicate that CAR's performance does not improve systematically in particular subsamples: in only five subsamples does CAR achieve statistical significance for two of the four outcome variables, and in no subsample does CAR achieve statistical significance for three or more outcome variables.

Table IA.IX examines deal and firm characteristics individually. We next allow for the interaction of characteristics. We create 10 dummy variables based on the following deal and firm characteristics: log market capitalization, leverage and free cash flow scaled by lagged assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, cash payment, diversifying deals, serial acquirer, and public target deals. If the characteristic is continuous, we create a dummy variable by splitting the sample at the median. We then form subsamples based on all the unique interactions of these variables and retain subsamples with at least 30 observations. We then split the sample into two time periods, and, for each subsample and time period, we regress outcomes on CAR$[-1, 1]$ and record the corresponding t-statistic. For both periods, we report the number of transactions with a t-statistic greater than or equal to two, between two and minus two, and less than or equal to minus two. In Table IA.X, Panel A, for nonimpairment, we run 22,298 regressions for each period and find that only 5% of transactions have the correct sign and a t-statistic of at least two in the first period; only 3% do so in the second period; and only 0.26% have the correct sign and statistical significance in *both* periods. Similarly, using short-term abnormal ROA, long-term abnormal ROA, and a completion dummy in Panels B to D, respectively, we find no more

Table IA.IX.

Acquirer CAR and Acquisition Outcomes: Subsamples

This table reports results from regressions of acquisition outcome measures on acquirer cumulative abnormal returns (CAR) using 29 different subsamples. The dependent variable is a nonimpairment dummy (column (1)), short-term abnormal ROA (column (2)), long-term abnormal ROA (column (3)), or a completion dummy (column (4)). Using subsamples, we run OLS regressions of each outcome on CAR, year fixed effects, and industry fixed effects. The subsamples are based on the following deal and firm characteristics: log market capitalization, leverage and free cash flow scaled by lagged assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, and a set of dummy variables for serial acquirer, stock-only, mixed-payment, cash-payment, diversifying, competed, hostile, high-tech, and public target deals. We measure firm-level characteristics in the year before the deal announcement. We split the sample at the median if the characteristic is continuous. Each cell represents a regression. We report the t-statistic and shade the cell green if the coefficient is statistically significant at or above the 5% level.

Dependent variable:	Nonimpairment	ST abROA	LT abROA	Completion
	(1)	(2)	(3)	(4)
Private target	−0.051	1.426	−0.718	−0.011
Public target	2.831	0.249	0.337	3.945
Stock deals	4.460	0.131	−0.956	1.121
Cash deals	0.823	1.026	−0.335	1.942
Mixed-payment deals	0.221	2.876	0.490	1.348
Diversifying deals	1.776	0.322	0.106	−0.185
Not diversifying deals	−0.177	3.567	−0.359	3.469
Competitive deals	0.489	3.727	1.159	1.274
Not competitive deals	1.011	1.628	−0.190	1.550
Serial acquirer	0.919	2.726	−0.457	1.531
Not serial acquirer	0.981	1.808	1.188	0.976
Hostile deals	—	1.684	−0.509	0.735
Not hostile deals	0.991	1.871	−0.211	2.491
Large acquirer	2.317	1.667	−0.107	3.402
Small acquirer	−0.677	1.133	−0.110	0.841
Large deal	1.260	3.314	1.107	4.955
Small deal	−0.308	0.148	−1.058	−0.091
Large Tobin's Q	−0.257	2.577	−0.606	0.582
Small Tobin's Q	1.957	0.500	0.252	2.377
High past return	0.593	2.078	-1.394	2.097
Low past return	1.265	1.154	0.938	0.849
High free cash flow	0.302	0.656	0.239	2.871
Low free cash flow	1.321	1.267	−0.502	0.525
High debt	1.277	2.784	0.505	3.232
Low debt	0.546	0.694	−0.526	0.711
High relative size	2.049	1.660	0.822	1.618
Low relative size	0.539	0.139	-2.755	1.616
High-tech	−0.754	1.041	0.605	1.511
Not high-tech	1.513	3.972	−0.474	2.181

than 10% of the regressions have the correct sign and a t-statistic of at least two in either period. Only 0.54%, 0.11%, and 1.45% of the regressions have the correct sign and statistical significance in *both* periods for short-term abnormal ROA, long-term abnormal ROA, and a completion dummy, respectively.

In Table IA.XI, we replicate Table II, column (5), for the subsamples that *exclude* the deals flagged as explicitly or potentially anticipated (e.g., deals where NPV expectations may already be reflected in returns). CAR continues to underperform across all subsamples.

We also take a brute-force approach by examining returns leading up to the announcements as a measure of anticipation. In Table IA.XII, Panel A, we sort deals into three terciles based on the run-up CAR$[-41, -2]$: tercile 1 contains announcements with the most negative returns, and tercile 3 comprises those with significant run-ups. In Panel B, we replicate Table II, column (5), and focus on tercile 2, which lacks substantial run-ups and presumably reflects the least anticipation. The coefficients on CAR are not statistically significant in any of the regressions, suggesting that even when isolating moderate run-ups, we do not identify a consistently better-performing CAR.

We next test whether truncation bias and feedback effects can explain the lack of correlation between CAR and ex-post outcomes. Specifically, we rely on the insight that the likelihood of canceling a deal is predictable using acquirer and deal characteristics. Indeed, Table III and Figure 4 show that characteristics predict deal completion reasonably well out-of-sample. To conduct the test, we regress the completion dummy on characteristics using the first half of the sample. We then predict the cancellation probability for transactions in the second half of the sample. We sort transactions based on their completion probabilities into three terciles, then repeat the Table II tests for both the lowest (low withdrawal probability) and highest tercile (high withdrawal probability). Table IA.XIII shows that CAR does not perform better for the sample of transactions with a low cancellation probability (which are less likely to face feedback effects): of the 21 regressions reported in Panels B, D, and F, the coefficient on CAR is statistically significant in only one regression.

Table IA.X.

Acquirer CAR and Acquisition Outcomes: Interactions

This table reports aggregated regressions of acquisition outcomes on acquirer CAR$[-1,1]$, allowing for interactions among characteristics. Panels A–D correspond to a nonimpairment dummy, short-term abnormal ROA, long-term abnormal ROA, and a completion dummy. We define 10 dummy variables for log market capitalization, leverage, free cash flow scaled by lagged assets, Tobin's Q, prior-quarter market-adjusted stock returns, relative deal size, and indicators for cash-payment, diversifying, serial, and public-target deals. Continuous characteristics are split at the median. All characteristics are measured in the year before the deal announcement. We form subsamples from all unique interactions of these variables and retain only those with at least 30 observations. The sample is split into two periods, and for each subsample–period cell we regress the outcome on CAR$[-1,1]$, recording the t-statistic. For each period, we report the number of transactions with t-statistics ≥ 2, not significant (n.s.), or ≤ -2. # indicates regressions with significant coefficients with the correct sign in both periods.

Panel A. Nonimpairment Dummy					
Total number of regressions: 49,902			First Period		
Drop $N \leq 30$ in both periods: 22,298			t-stat≥ 2	n.s.	t-stat≤ -2
			1,091	20,554	653
Second Period	≥ 2	735	59#	674	2
	n.s.	20,000	990	18,444	566
	≤ -2	1,563	42	1,436	85
Panel B. Short-Term Abnormal ROA					
Total number of regressions: 50,103			First Period		
Drop $N \leq 30$ in both periods: 16,354			t-stat≥ 2	n.s.	t-stat≤ -2
			1,777	13,932	645
Second Period	≥ 2	1,074	88#	920	66
	n.s.	14,153	1,584	12,019	550
	≤ -2	1,127	105	993	29
Panel C. Long-Term Abnormal ROA					
Total number of regressions: 49,614			First Period		
Drop $N \leq 30$ in both periods: 15,551			t-stat≥ 2	n.s.	t-stat≤ -2
			796	13,779	976
Second Period	≥ 2	686	17#	622	47
	n.s.	13,780	721	12,292	767
	≤ -2	1,085	58	865	162
Panel D. Completion Dummy					
Total number of regressions: 40,594			First Period		
Drop $N \leq 30$ in both periods: 16,394			t-stat≥ 2	n.s.	t-stat≤ -2
			1,186	14,438	770
Second Period	≥ 2	1,067	238#	818	11
	n.s.	14,601	927	13,008	666
	≤ -2	726	21	612	93

Table IA.XI.

Acquirer CAR$[-1, 1]$ and Anticipation Deals

This table reports results from regressions of acquisition outcome measures on acquirer CAR$[-1, 1]$. The dependent variable is a nonimpairment dummy (Panel A), short-term abnormal ROA (Panel B), long-term abnormal ROA (Panel C), or a completion dummy (Panel D). Across all columns, CAR is the key independent variable. We include characteristics and year and industry fixed effects as independent variables. The characteristics used as controls include log market capitalization, leverage and free cash flow scaled by lagged assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, and a set of dummy variables for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals. We measure firm-level characteristics in the year before the deal announcement. Column (1) excludes "explicit anticipation" deals from Capital IQ and SDC. Columns (2) to (5) exclude "potential anticipation" deals from Capital IQ, repeat acquirers, and repeat industry bids. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	Exclude Explicit Anticipation Deals	Exclude Potential Anticipation Deals			
				Repeat Industry Bids	
Sample:	Capital IQ + SDC	Capital IQ	Repeat Acquirers	1 Year	6 Months
	(1)	(2)	(3)	(4)	(5)
Panel A. Probability of Nonimpairment (DV: Nonimpairment Dummy)					
CAR$[-1, 1]$	0.043	0.061	0.094	0.516	0.361
	(0.100)	(0.106)	(0.138)	(0.371)	(0.235)
Controls	— All specifications include Year, Ind, and Char —				
Observations	5,666	4,813	3,185	480	962
Adjusted R^2	0.088	0.098	0.103	0.109	0.111
Panel B. Short-Term Abnormal ROA (DV: Short-Term Abnormal ROA)					
CAR$[-1, 1]$	0.017	0.016	0.012	0.029	0.022
	(0.010)	(0.012)	(0.016)	(0.037)	(0.036)
Controls	— All specifications include Year, Ind, and Char —				
Observations	27,885	25,650	8,203	1,282	2,838
Adjusted R^2	0.076	0.065	0.051	0.042	0.043
Panel C. Long-Term Abnormal ROA (DV: Long-Term Abnormal ROA)					
CAR$[-1, 1]$	−0.004	−0.003	−0.001	0.052	0.034
	(0.012)	(0.011)	(0.025)	(0.055)	(0.038)
Controls	— All specifications include Year, Ind, and Char —				
Observations	21,988	20,421	6,282	1,225	2,590
Adjusted R^2	0.106	0.094	0.069	0.051	0.052
Panel D. Probability of Completion (DV: Completion Dummy)					
CAR$[-1, 1]$	0.047**	0.036**	0.032	0.115	0.122*
	(0.017)	(0.015)	(0.033)	(0.107)	(0.062)
Controls	— All specifications include Year, Ind, and Char —				
Observations	38,538	35,904	11,319	1,224	2,902
Adjusted R^2	0.152	0.148	0.148	0.159	0.160

Table IA.XII.

Acquirer CAR and Acquisition Outcomes: Run-Up

This table reports results from regressions of acquisition outcome measures on cumulative abnormal returns (CAR). We sort deals into three terciles based on the run-up CAR$[-41,-2]$. Panel A reports the summary statistics of CAR$[-41,-2]$ for each tercile. Panel B reports the regression results. The dependent variable is a nonimpairment dummy (column (1)), short-term abnormal ROA (column (2)), long-term abnormal ROA (column (3)), or a completion dummy (column (4)). CAR is the key independent variable. We also include year and industry fixed effects, as well as characteristics as independent variables. The characteristics used in the controls include log market capitalization, leverage and free cash flow scaled by lagged assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, and a set of dummy variables for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals. We measure firm-level characteristics in the year before the deal announcement. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Nonimpairment	ST abROA	LT abROA	Completion
	(1)	(2)	(3)	(4)
	Panel A. Summary Statistics of CAR$[-41,-2]$			
Sample mean:				
Tercile 1	−0.149	−0.168	−0.164	−0.183
Tercile 2	−0.006	−0.006	−0.005	−0.007
Tercile 3	0.131	0.159	0.157	0.174
	Panel B. Outcomes and CAR for "Tercile 2"			
CAR$[-1,1]$	−0.031	0.025	0.023	0.017
	(0.118)	(0.015)	(0.034)	(0.047)
Controls	— All specifications include Year, Ind, and Char —			
Observations	2,047	9,579	7,550	13,193
Adjusted R^2	0.071	0.117	0.157	0.149

Given that extreme announcement returns could also point to feedback effects, in Internet Appendix Table IA.XIV, we replicate Table II, column (5), without extreme CAR, that is, we eliminate deals with CAR in the top and bottom 10% of the sample. The coefficient on CAR is not statistically significant for any of the outcomes (despite achieving significance for a few outcomes in Table II, column (5)).

Table IA.XIII.

Acquirer CAR and Withdrawal Prediction

This table reports regressions of acquisition outcomes on acquirer CAR for high (top tercile) and low (bottom tercile) withdrawal probability samples. Cancellation probabilities are predicted using a completion regression on characteristics from the first sample half and applied to transactions in the second half. Panels A to F use nonimpairment dummy, short-term abnormal ROA, and long-term abnormal ROA as dependent variables. Columns (1) to (3) use CAR as the sole independent variable. Columns (4) to (7) incrementally add characteristics, year, and industry fixed effects. Controls include log of market cap, leverage, free cash flow scaled by lagged assets, Tobin's Q, prior-quarter market-adjusted returns, relative deal size, and dummies for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals. Firm characteristics are measured pre-announcement. Standard errors are in parentheses. ***, **, and * denote 1%, 5%, and 10% significance levels, respectively.

CAR window:	$[-1,1]$	$[-5,5]$	$[A-2,C+2]$	$[-1,1]$		n.a.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Probability of Nonimpairment — High W/D Probability ($N = 954$, DV: Nonimpair.)							
CAR	−0.019	−0.036	0.011	−0.037	0.063	Controls	Controls
	(0.114)	(0.097)	(0.025)	(0.139)	(0.097)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	−0.001	−0.001	−0.001	0.009	0.033	0.010	0.034
Panel B. Probability of Nonimpairment — Low W/D Probability ($N = 954$, DV: Nonimpair.)							
CAR	0.014	0.118	0.113	0.243	0.244	Controls	Controls
	(0.137)	(0.107)	(0.092)	(0.140)	(0.144)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	−0.001	0.000	0.005	0.068	0.128	0.065	0.124
Panel C: ST Abnormal ROA — High W/D Probability ($N = 4,783$, DV: ST abROA)							
CAR	0.012	0.010	0.027**	0.034	0.022	Controls	Controls
	(0.023)	(0.012)	(0.010)	(0.023)	(0.026)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	0.000	0.000	0.002	0.047	0.228	0.047	0.228
Panel D. ST Abnormal ROA — Low W/D Probability ($N = 4,786$, DV: ST abROA)							
CAR	−0.024	−0.020	0.000	−0.011	−0.007	Controls	Controls
	(0.030)	(0.012)	(0.006)	(0.028)	(0.025)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	0.000	0.000	0.000	0.056	0.111	0.056	0.111
Panel E. LT Abnormal ROA — High W/D Probability ($N = 3,571$, DV: LT abROA)							
CAR	−0.045	−0.034	0.021	−0.015	−0.023	Controls	Controls
	(0.035)	(0.019)	(0.014)	(0.033)	(0.025)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	0.000	0.001	0.001	0.045	0.274	0.045	0.274
Panel F. LT Abnormal ROA — Low W/D Probability ($N = 3,571$, DV: LT abROA)							
CAR	−0.026	−0.022	0.007***	−0.014	−0.007	Controls	Controls
	(0.039)	(0.030)	(0.002)	(0.031)	(0.029)	only	only
Controls	—	—	—	Char	Year, Ind, Char	Char	Year, Ind, Char
Adjusted R^2	0.000	0.000	0.000	0.062	0.133	0.062	0.133

Table IA.XIV.

Acquirer CAR and Acquisition Outcomes: Trim Extreme Values

This table replicates Table II, column (5), but we eliminate deals with CAR in the top and bottom 10% of the sample. The dependent variable is a nonimpairment dummy (column (1)), short-term abnormal ROA (column (2)), long-term abnormal ROA (column (3)), or a completion dummy (column (4)). CAR$[-1, 1]$ is the independent variable. The regressions include year and industry fixed effects, and characteristics as independent variables. The following characteristics are used as controls: log market capitalization, leverage and free cash flow scaled by lagged assets, Tobin's Q, previous-quarter market-adjusted stock returns, relative deal size, and a set of dummy variables for stock-only, mixed-payment, diversifying, competed, hostile, and public target deals. We measure firm-level characteristics in the year before the deal announcement. Standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Dependent variable:	Nonimpairment	Short-Term abROA	Long-Term abROA	Completion
	(1)	(2)	(3)	(4)
CAR$[-1, 1]$	-0.077	0.003	0.001	0.039
	(0.189)	(0.014)	(0.025)	(0.027)
Controls	— All specifications include Year, Ind, and Char —			
Observations	4,893	22,976	18,063	31,668
Adjusted R^2	0.084	0.087	0.124	0.165

III. Validation of Impairment as a Measure of Value Destruction

In this section, we validate goodwill impairment events as a signal of value destruction. To do so, we examine (i) the market's reaction to the news that the goodwill of a past transaction has been impaired, (ii) distressed delistings following the impairment announcement, (iii) the operating and financial performance of the impaired acquirers after the deal announcement, and (iv) management turnover around the impairment announcement.

A. Market Response to Impairment News

We test whether investors perceive goodwill impairment as conveying negative news, that is, whether they recognize that value has been lost. Our test replicates prior research in the accounting literature documenting that goodwill impairment events are value-relevant.[5]

We use Compustat quarterly data to identify the first quarter in which each transaction in our impairment sample experienced a goodwill write-down and the earnings announcement date for that quarter. Unique earnings announcement dates for an acquirer are included in the sample only once if multiple transactions experience a goodwill impairment announcement for a particular acquirer on the same earnings announcement date. We create three control samples. First, for the nonimpairment sample, we generate pseudo-impairment dates on earnings announcements three years following the deal's effective date. (The mean time to impairment is about three years.) Our second control sample, "Matched Control Sample 1," comprises firms that announce earnings in the same quarter and have the same fiscal year-end and two-digit SIC code as the impaired firm. Our third control sample, "Matched Control Sample 2," comprises firms that announce earnings in the same quarter, have the same fiscal year-end and two-digit SIC code as the impaired firm, and are in the same market

[5] In tune with this literature, we interpret this result as a response to a revelation of *past* value destruction (e.g., Henning and Stock, 1997; Bens et al., 2011; Chen et al., 2008; Gu and Lev, 2011; Li et al., 2011).

Table IA.XV.

Market Reaction to Goodwill Impairment News

This table reports the mean cumulative abnormal returns (CAR) surrounding quarterly earnings announcement dates. For the impairment sample, we focus on the first earnings announcement when a goodwill impairment is announced for a particular transaction. Unique earnings announcement dates for an acquirer are included in the sample only once if multiple transactions experience a goodwill impairment announcement for a specific acquirer on the same earnings announcement date. For the nonimpairment sample, we generate "pseudo" impairment dates three years (the average time to impair) following the deal close date. We also create two matched samples of control firms that did not announce impairment news. "Control1" is a matched sample that includes firms that announce earnings in the same quarter and have the same fiscal year-end and two-digit SIC code as the impaired firm. "Control2" is a matched sample that includes firms that announce earnings in the same quarter and have the same fiscal year-end and two-digit SIC code and are in the same market capitalization tercile as the impaired firm. CARs are based on market-adjusted returns using the Center for Research in Security Prices (CRSP) value-weighted index. The event period is listed in brackets."Difference" refers to the differences between the Impairment and Control samples. Tests for differences are based on the t-test. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. "ns" denotes mean CARs or differences that are not statistically different from zero.

Sample:	Impairment	Nonimpair.	Control1	Control2	Difference ($t-$test)		
Window:	(1)	(2)	(3)	(4)	(1)−(2)	(1)−(3)	(1)−(4)
CAR$[-1,1]$	−2.8% ***	0.3% **	0.2% ***	0.2% ***	−3.1% ***	−3.0% ***	−3.0% ***
CAR$[0,1]$	−2.9% ***	0.1% ns	0.0% ns	0.0% ns	−3.0% ***	−2.9% ***	−2.9% ***
CAR[-5,5]	−3.3% ***	0.4% **	0.7% ***	0.8% ***	−3.7% ***	−4.0% ***	−4.1% ***
CAR$[-10,10]$	−3.7% ***	0.7% ***	1.5% ***	1.7% ***	−4.4% ***	−5.2% ***	−5.4% ***

capitalization tercile as the impaired firm. To avoid estimating market model parameters in pre- and post-acquisition periods, we compute market-adjusted returns using the Center for Research in Security Prices (CRSP) value-weighted index.

Table IA.XV shows the results over four event windows. For the impairment sample, cumulative abnormal returns are negative and statistically different from zero for all four event windows (mean CARs range from −2.8% to −3.7%). The market response to earnings announcements for the three control samples is small and positive for all four event windows (mean CARs range from 0.0% to 1.7%). Importantly, the market response to earnings announcements containing goodwill impairment is statistically lower than the three control samples for all event windows. Although earnings announcements contain other information besides goodwill impairment news, the results suggest that the market considers goodwill impairment events bad news.

Table IA.XVI.

Post-Deal Performance for Firms with Goodwill Impairment

Panel A reports univariate statistics on the number of acquirer firms that exit the public market within five years of the deal's effective date. Panel B reports median industry-adjusted accounting performance in the third year after the deal announcement. Tests for differences between samples are based on the Wilcoxon rank test. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A. Post-Deal Public Market Exits

Sample:	Impairment		Nonimpairment		Difference	
	#	%	#	%		
Merged/Went private	124	15%	1,098	23%	−9.0%	***
Delisted	77	9%	113	2%	6.6%	***
Bankrupt/Liquidated	23	3%	35	1%	1.9%	***

Panel B. Industry-Adjusted Accounting Performance During 3 Years After Deal

	Impairment Sample	Nonimpairment Sample	Difference	
Sales growth	−5.3%	0.7%	−6.0%	***
COGS/Sales	2.0%	−1.5%	3.0%	***
SG&A/Assets	−0.3%	−1.2%	0.9%	***
PP&E growth	−4.0%	1.5%	−5.5%	***
FCF/Assets	−3.1%	0.9%	−4.0%	***
ROA	−0.1%	1.1%	−1.2%	***
ROE	−7.1%	0.5%	−7.6%	***
Tobin's Q	−26.8%	−0.7%	−26.1%	***
Earnings/Price	−4.7%	0.8%	−5.5%	***

B. *Acquirer's Distressed Delisting*

Table IA.XVI, Panel A, shows univariate statistics on the number of acquirer firms that exit the public market within five years of the deal's effective date. Public market exit data are obtained using the CRSP delisting code. Acquirers are categorized as "Merged/Went private" for delisting codes 200 to 390 and 573. Acquirers are classified as "Delisted" for delisting codes between 500 and 600 (excluding 573 and 574) and "Bankrupt/Liquidated" for delisting codes 400 to 490 and 574. We retain only one observation when an acquirer in the impairment or nonimpairment sample announces multiple transactions in the same year.

Table IA.XVI, Panel A, shows that firms in the impairment sample are significantly more likely to be delisted and to go through a bankruptcy or liquidation process than firms in the nonimpairment sample. In contrast, firms in the nonimpairment sample are more likely to merge or go private. These findings imply that impairment is a good proxy for deal failure.

C. Acquirers' Long-Term Performance

We examine industry-adjusted accounting and stock performance for the three years after the deal announcement. We retain only one observation when an acquirer in the impairment or nonimpairment sample announces multiple transactions in the same year. We report the following median performance measures, adjusted by the median Fama-French 48-industry value: sales growth; cost of goods sold (COGS) scaled by sales; selling, general, and administrative expenses (SG&A) scaled by sales; property, plant, and equipment (PP&E) growth; free cash flow (FCF) scaled by assets; return on assets (ROA); return on equity (ROE); Tobin's Q; and the earnings-to-price ratio.

Table IA.XVI, Panel B, reports median industry-adjusted statistics and tests of statistical differences between the nonimpairment and impairment samples. We observe statistically superior performance for the nonimpairment sample relative to the impairment sample for the three years following the acquisition announcement for all nine performance measures.

Figure IA.3, Panels A to F, show the operating performance from one year before to three years following the acquisition. Across panels, we generally observe that industry-adjusted performance measures begin to materially diverge in the years following the deal announcement for the impairment sample (red lines) and the nonimpairment sample (blue lines), indicating that impairment firms encounter significant firm-level adverse shocks in the years following the acquisition. For many measures, the divergence begins in the year following the acquisition but widens further two years after.

Figure IA.4, Panels A to D, show the financial performance from two years before to three years after the acquisition. Note here that the gap between the blue and red lines increases not so much before but after the deal announcement. Figure IA.4, Panel D, shows that the returns to the realized impairment sample remain relatively flat at the announcement but decline dramatically thereafter. Returns to the realized nonimpairment sample continue their steady growth; consequently, the gap between the two subsamples widens.

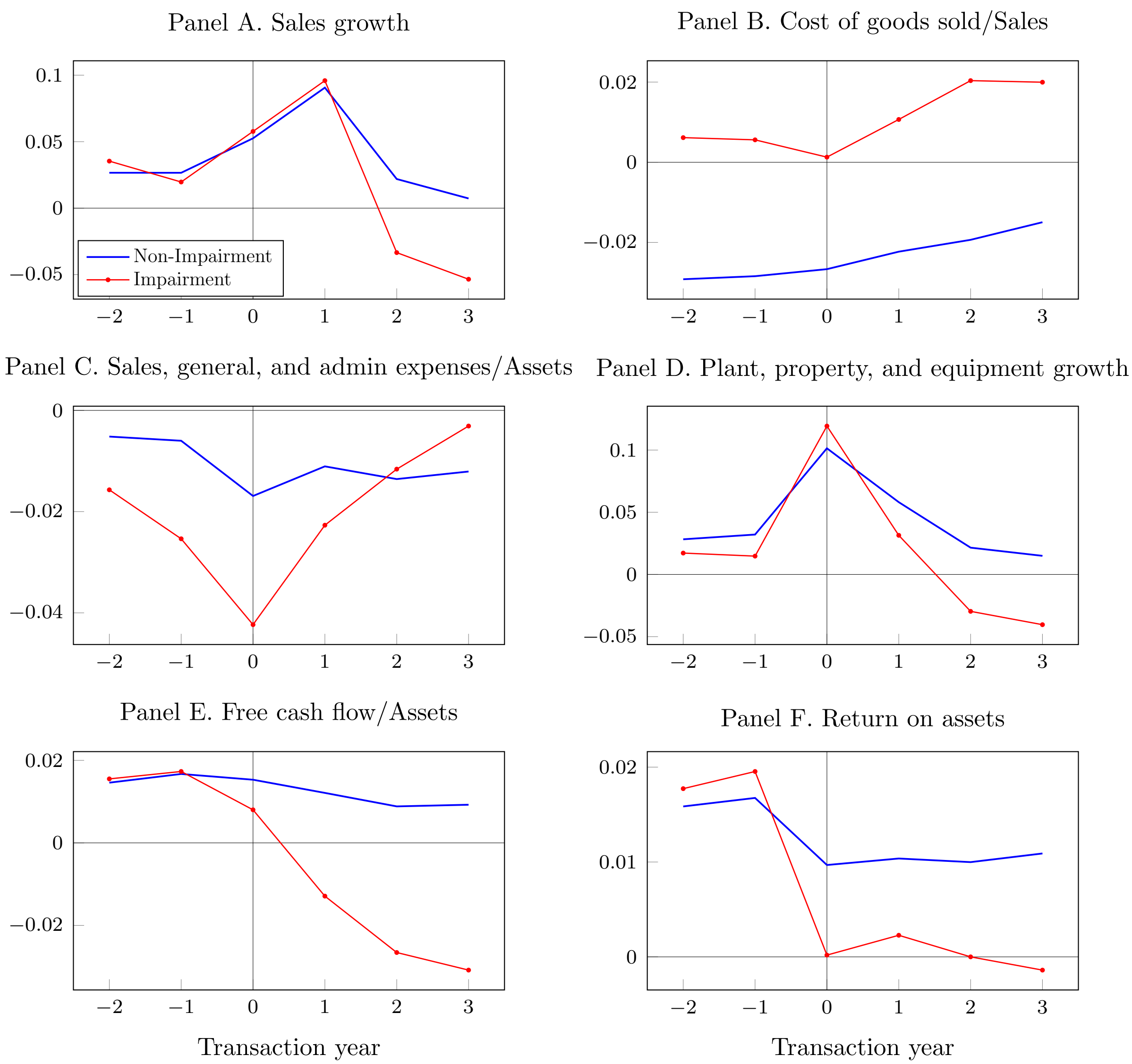

Figure IA.3. Operating performance and goodwill impairment.
This figure shows the industry-adjusted operating performance of acquirers that impaired goodwill (in red) relative to acquirers that did not impair goodwill (in blue). The period begins two years before the acquisition and ends three years after. Panel A shows sales growth. Panel B shows the cost of goods sold/sales. Panel C shows sales, general, and administrative expenses/assets. Panel D shows plant, property, and equipment growth. Panel E shows free cash flow/assets. Panel F shows the return on assets.

D. CEO Turnover Around Goodwill Impairment

We consider both the likelihood of CEO turnover following the deal and the timing of turnover for the impairment sample. In independent work, Cowan et al. (2023) perform a similar analysis and conclude that goodwill impairment is a good indicator of CEO underperformance.

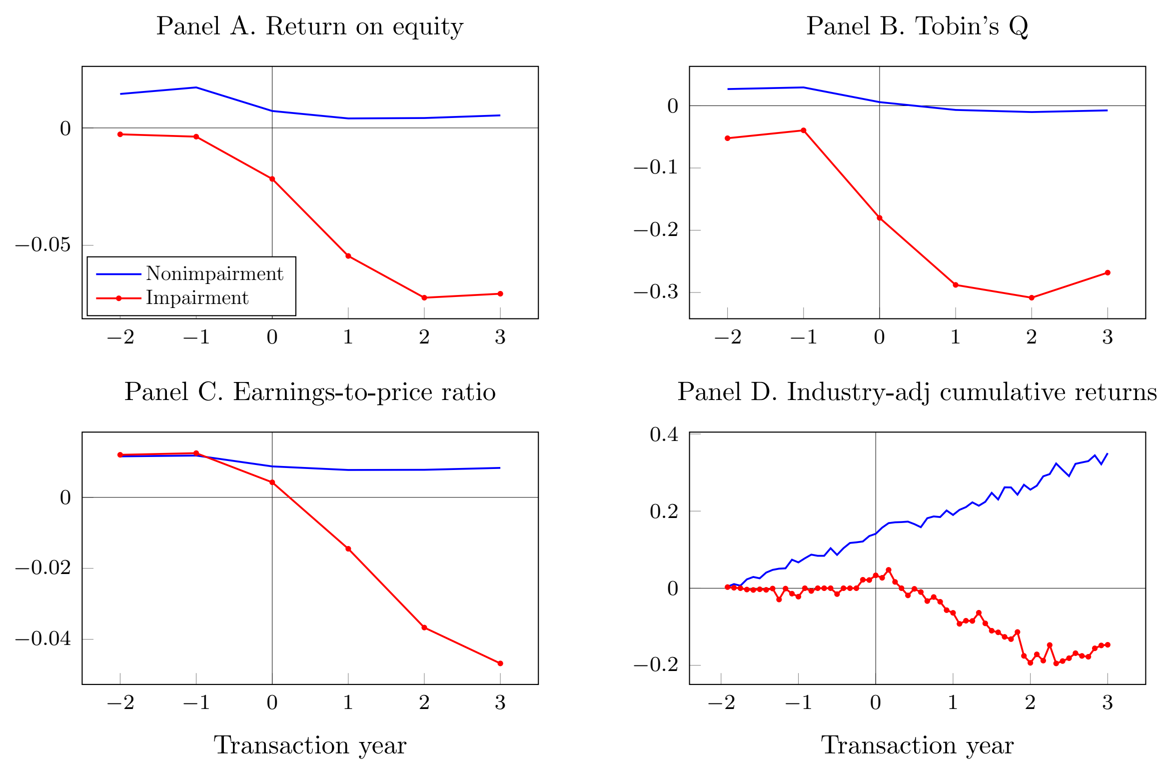

Figure IA.4. Financial Performance and goodwill impairment.
The figure shows the industry-adjusted financial performance of acquirers that impaired goodwill (in red) relative to acquirers that did not impair goodwill (in blue). The period begins two years before the acquisition and ends three years after. Panel A shows the return on equity. Panel B shows Tobin's Q. Panel C shows the earnings-to-price ratio. Panel D shows industry-adjusted buy-and-hold cumulative returns.

The tests in the previous subsection utilize the full sample of 906 goodwill impairments. In this section, because turnover events require manual hand-collected data, we report the results for a smaller subsample of 355 impairments that was utilized in a previous version of the paper. This subsample only includes impairments between 2003 and 2013 and uses more stringent filters than the current version of the paper (e.g., excludes acquisitions by financial firms).

We track turnover events between the deal announcement and four years after the first impairment event. This analysis is conducted at the CEO-impairment level. If a CEO is associated with multiple impairment events, we retain only the transaction with the largest impairment amount. We identify three types of forced CEO turnover: (i) internal turnover (fired by the board), (ii) takeover turnover, and (iii) bankruptcy turnover. Turnover events are identified using proxy statements, press releases, and news articles in Factiva. We follow Weisbach (1995), Parrino (1997), and Lehn and Zhao (2006) in identifying turnover events. If the CEO is reported as fired, forced from his or her position, or departed due to unspecified policy differences, then the CEO is classified as experiencing an internal turnover event. If the CEO is under the age of 65 and the reason for departure is unrelated to death, poor health, or the acceptance of another position, or if it is announced that the CEO is retiring and yet the announcement is not at least six months before succession, then the CEO is classified as experiencing an internal turnover event. For firms that are acquired, if we cannot find evidence that the CEO retained a role in the acquiring entity, then the CEO is classified as experiencing a takeover turnover event. For firms that enter bankruptcy, if we cannot find evidence that the CEO retained his or her job during the bankruptcy process, then the CEO is classified as experiencing a bankruptcy turnover event.

Table IA.XVII presents results for the full sample of transactions in the impairment sample. We find that 45% of CEOs experience a turnover event between the deal announcement and four years following the impairment, indicating that close to half of the impairment sample CEOs are disciplined by the labor market. To provide a relative comparison, Jenter and Lewellen (2021) show that, not conditional on acquisition activity, on average, 12% of CEOs turnover in a given year. For acquiring firms (that may or may not experience impairment), Lehn and Zhao (2006) find a 47% CEO turnover propensity within five years of the deal announcement date.

However, our main interest is the turnover timing, which allows us to assess whether the CEO's departure results from the market's assessment of value destruction at the deal announcement or results from the subsequent impairment event itself. If value destruction is anticipated, CEOs should be more likely to be fired immediately following the acquisition announcement rather than the impairment. We find that 13% of impaired firm CEOs are terminated in the year of or year following the deal effective year, whereas 41% are fired in the year of or year following the impairment year.

Table IA.XVII.

Post-Deal CEO Turnover for Firms with Goodwill Impairment

This table reports univariate statistics for CEO turnover for the sample of firms experiencing a goodwill impairment. We track CEO turnover events between the deal announcement and four years after the first impairment event.

Impairment sample	#	%
% Turnover between deal announcement year and impairment year + 4	142	45%
Firms subject to internal turnover	118	38%
Firms subject to takeovers	19	6%
Firms subject to bankruptcy	5	2%
% Turnover year of or year after deal effective year (% of total sample)	19	13%
% Turnover year of or year after impairment year (% of total sample)	58	41%

To summarize, the results in Table IA.XVII indicate that the majority of turnover events in the impairment sample do not result from anticipated value destruction at the deal announcement but rather because of deal failure that is signaled by goodwill impairment. Specifically, CEO turnover events are three times more likely to occur immediately following the impairment than the deal announcement. This finding implies that the labor market considers impairment a proxy for deal failure.

To conclude, the results in this section provide strong evidence that firms in the impairment sample experience all symptoms of deal failure—forced CEO turnover, delistings, bankruptcies, poor accounting, and poor stock performance—supporting our conclusion that goodwill impairment is a good proxy for deal failure.

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