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In Too Deep: The Effect of Sunk Costs on Corporate Investment

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ABSTRACT

Sunk costs are unrecoverable costs that should not affect decision making. I provide evidence that firms systematically fail to ignore sunk costs and that this leads to significant investment distortions. In fixed-exchange-ratio stock mergers, aggregate market fluctuations after parties enter into a binding merger agreement induce plausibly exogenous variation in the final acquisition cost. These quasi-random cost shocks strongly predict firms' commitment to an acquired business following deal completion, with an interquartile cost increase reducing subsequent divestiture rates by 8% to 9%. Consistent with an intrapersonal sunk cost channel, distortions are concentrated in firm-years in which the acquiring CEO is still in office.

VIRTUALLY EVERY INVESTMENT A FIRM makes entails sunk costs that the firm has incurred and cannot recover. Basic economic theory establishes that managers should disregard these costs when making subsequent decisions, as they are, by definition, sunk. Instead, the old adage "throwing good money after bad" encapsulates the intuition that people frequently act in striking

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contrast to this principle and are more likely to stay committed to ventures in which they have invested substantial resources.

Empirical evidence that convincingly demonstrates the existence of this *sunk cost effect* is, however, sparse, and little is known about the extent to which it affects firm decision making specifically. This is despite warnings by behavioral researchers that sunk costs influence "decisions large and small" (Kahneman (2011)) and agreement among standard corporate finance textbooks that sunk costs likely play a major role in the corporate realm. For example, Berk and DeMarzo (2017) caution that basing decisions on sunk costs constitutes a "common mistake" and can result in "financial disaster," while Brealey, Myers, and Allen (2017) urge readers to "forget sunk costs."[1]

The lack of comprehensive field evidence on the sunk cost effect is due to a fundamental identification challenge: ruling out screening effects inherent in purchase decisions (Roy (1951), Ashraf, Berry, and Shapiro (2010)). By way of example, imagine that a good is sold at different prices across stores, and that these prices are even randomly assigned. A person who buys the good at a higher price not only incurs higher sunk costs, but also has a greater willingness to pay on average, and thus a greater general propensity to use the product. As a result, any (potentially unobserved) variable affecting a person's purchase decision at a given price could explain subsequent behavior.

In this paper, I devise a test to assess the effects of sunk costs on firm decision making that overcomes this identification challenge. Focusing on one high-stakes type of firm investment, namely, mergers and acquisitions (M&As), I isolate plausibly exogenous variation in acquisition costs that unfolds *after* transacting parties sign a definitive merger agreement. I then investigate whether these quasi-random cost shocks affect divestiture rates of acquired businesses.

To obtain variation in post-agreement acquisition costs, I exploit specific contract features of stock acquisitions. In fixed exchange ratio stock mergers, the final transaction price in dollars is unknown when parties sign a merger agreement that fixes all transaction terms. Since these acquisitions stipulate a fixed number of acquirer shares to be exchanged in the transaction, changes in the acquirer's stock price between merger agreement and completion translate directly into changes in the final acquisition cost. To account for the endogeneity of the acquirer's stock price movements, I focus on acquisition cost variation induced by aggregate stock market fluctuations. Differential cost shocks do not lead to any mechanical dissimilarity in operational characteristics (e.g., cash holdings) between acquirers. My analysis identifies sunk cost effects from differences in divestiture patterns of acquisitions that are undertaken in the same year, but are exposed to different post-agreement market fluctuations.

[1] Figure IA.1 in the Internet Appendix displays the key paragraphs in Kahneman (2011), Berk and DeMarzo (2017), and Brealey, Myers, and Allen (2017). The Internet Appendix is available in the online version of the article on *The Journal of Finance* website.

An identifying assumption is that acquirers are attentive to post-agreement changes in acquisition costs.[2]

This setting requires information on both divestitures of previously acquired businesses and the precise exchange ratio terms of each acquisition. To this end, I perform a systematic search of divestitures using newspaper articles and news wires from Nexis (formerly LexisNexis) for a large sample of U.S. stock acquisitions by public acquirers since 1980. I then hand-collect the exact acquisition terms for all identified divested acquisitions as well as a matched sample of nondivested acquisitions from Securities and Exchange Commission (SEC) filings, analyst conference call transcripts, and news articles. The matching procedure is based on standard firm and deal characteristics (see Section II.D for details). As an alternative to using a fixed exchange ratio (hereafter, *Fixed Shares*), transacting parties can structure a stock acquisition using a floating exchange ratio (hereafter, *Fixed Dollar*), which fixes a merger consideration in dollars and adjusts the number of shares based on the acquirer's share price at deal completion. Standard databases do not provide information on the type of exchange ratio used (see, for example, Ahern and Sosyura (2014)). I identify the precise deal terms for 89% of the acquisitions in my sample. The rate increases to 93% for acquisitions since 1994, when firms began filing reports through the SEC's Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system. These rates are large both in and of themselves and in comparison with existing studies (see Section II.B for details).

The resulting data set, which consists of divested and nondivested deals, includes large and salient transactions. The median acquisition cost, for example, is $99 million. This sample comprises *Fixed Shares* mergers only, as changes in post-agreement acquisition costs are unique to this deal structure. This feature preempts any concerns about omitted variables that might simultaneously affect selection into deal structure type and divestiture rates. The market-induced variation in acquisition costs in my sample is economically meaningful, with the interquartile range (IQR) of the market return between merger agreement and completion equal to 8.5 percentage points (pp).

The key finding of this paper is that there is a strong link between exogenous variation in acquisition costs and subsequent divestment decisions, consistent with the sunk cost hypothesis. I estimate an 8% to 9% reduction in divestiture rates of acquired businesses associated with an interquartile increase in quasi-random acquisition costs. This effect is economically significant yet plausible. For example, the effect size roughly corresponds to that of moving from the 50^{th} to the 65^{th} percentile in post-merger annual stock performance.

This result is robust to various specifications, including a Cox (1972) proportional hazards model, stratified hazard models, a logit model that accounts for

[2] This assumption appears well justified. For example, the media frequently report on stock price–induced transaction value changes, which suggests that these changes should be particularly salient to managers. See, for example, the following *New York Times* article discussing a transaction price decrease in Facebook's (FB) acquisition of Instagram as a result of a drop in FB's stock price: https://dealbook.nytimes.com/2012/08/20/how-instagram-could-have-cut-a-better-deal.

the passage of time (Efron (1988), Jenter and Kanaan (2015)), and a two-stage control function estimation method (Wooldridge (2015)). In addition, this result is unlikely to be explained by differential selection into deal completion versus withdrawal after an official merger agreement has been reached. Increases in post-agreement acquisition costs could make acquirers with low perceived or true synergy potential, but not those with high synergy potential, more likely to withdraw. This would also predict lower divestiture rates after increases in acquisition costs, even in the absence of sunk cost effects. However, stock deal withdrawals after market increases are highly infrequent, which suggests that this concern is of limited empirical importance. The results also remain unchanged when I successively remove observations with the highest estimated ex ante withdrawal probabilities from the sample.

Three additional findings extend the main result and further support the sunk cost–based explanation. First, a remaining concern is that market movements might affect other aspects related to the acquirer or acquired business that are themselves relevant at the time of the divestment decision. To address this concern, I implement placebo tests involving hypothetical changes in acquisition costs. These tests are motivated by the idea that potential alternative channels should also be present for market fluctuations that did not shift actual acquisition costs. One placebo test uses post-deal completion market fluctuations to construct hypothetical cost changes (Bernstein (2015)). A separate placebo test leverages an additional sample of *Fixed Dollar* acquisitions, for which I use market fluctuations from the actual period between merger agreement and completion to construct hypothetical cost changes. The placebo tests find no evidence that hypothetical cost variation predicts divestiture rates. The results on actual cost changes also remain unchanged when I include time-varying controls that might be affected by the final acquisition price, such as various performance measures and leverage. Both sets of findings corroborate the sunk cost interpretation.

Second, I find that the link between acquisition cost shocks and divestiture rates is amplified in financially unconstrained firms and dampened in constrained firms. These patterns are consistent with constraints counteracting increased commitment to costly acquisitions as a result of sunk cost effects.

Third, the documented effect is concentrated in firm-years in which the chief executive officer (CEO) who led the acquisition is still at the helm and is 30% to 50% lower after this CEO steps down. This result suggests an intrapersonal sunk cost mechanism and relates to survey evidence by Graham, Harvey, and Puri (2015), who find that CEOs make M&A-related decisions "in relative isolation." The finding of a CEO-specific sunk cost effect further elevates hurdles for alternative explanations based on firm, industry, or market characteristics.

Why are managers influenced by sunk costs? Given the identification from post-merger agreement cost changes which addresses screening effects, the results are inconsistent with high sunk costs simply reflecting positive ex ante CEO information or beliefs about targets. As I discuss in detail in Section V.C, the results are also at odds with a "learning by doing" channel, with a standard CEO entrenchment mechanism, and with sunk costs affecting firms'

investment budgets. My findings are most consistent with sunk costs generating psychological frictions in managerial decision making. In general, this can happen for at least two, nonmutually exclusive reasons: CEOs themselves are subject to the behavioral forces that underlie sunk cost effects, and CEOs respond to the sunk cost–based thinking of other parties (e.g., board members) due to career concerns. In additional mechanism tests, I find no evidence that the results are driven by younger CEOs with longer careers ahead of them. Instead, they are more pronounced for CEOs who are likely to be less sophisticated, as indicated by cross-sectional sorts on firm size (Gabaix and Landier (2008)), and validated with additional data on CEO education. These findings point to managerial psychology as an important determinant of the documented sunk cost effects in my setting.

One caveat to the results in this paper is that it is difficult to precisely quantify the efficiency costs of sunk cost–induced divestment distortions, due to a lack of detailed data on divestiture transaction prices and segment cash flows. That said, various factors suggest important real costs for firms. First, a simple conceptual framework formalizes the intuition that "sunk cost managers" ignore negative signals about costly acquisitions and deviate from the optimal net present value (NPV) divestment rule. Second, the divestment distortions are pronounced in diversifying acquisitions, a plausible proxy for inferior deal quality (e.g., Malmendier and Tate (2008)). Third, contemporaneous related work concludes that many divestitures of acquisitions are "corrections of failure" (Cronqvist and Pély (2024)), a pattern that is supported in my sample and suggests that, on average, delaying divestiture of a costly acquisition entails efficiency costs. Finally, a counterfactual exercise, which estimates an earlier divestiture date for acquirers had they experienced no acquisition cost increase, shows that firms underperform between the counterfactual and actual divestiture announcement dates, possibly due to delayed divestment. This interpretation is strengthened by the fact that performance deterioration is largely confined to observations for which the divested business constitutes a substantial part of the firm. Further research is needed to fully quantify the efficiency effects of sunk costs for firms. As an important step in this direction, this paper provides the first cleanly identified evidence that sunk costs matter in corporate finance.

This paper makes three main contributions to the literature. First, I add to the literature on behavioral corporate finance. In providing empirical evidence of sunk cost effects, my paper advances this field by considering a frequently discussed phenomenon that can have far-reaching consequences for firm outcomes. My findings specifically add to the literature on nonstandard managerial preferences, with sunk costs triggering disutility upon divestment, or a sunk cost effect rooted in prospect theory (Kahneman and Tversky (1979)) as in Thaler (1980). The majority of existing work focuses instead on belief-based biases (e.g., overconfidence and optimism, as in Malmendier and Tate (2005, 2008), Landier and Thesmar (2008), and Gervais, Heaton, and Odean (2011), or competition neglect, as in Greenwood and Hanson (2014)) and more recently on heuristics (e.g., the weighted average cost of capital

(WACC) fallacy (Krüger, Landier, and Thesmar (2015)), representativeness and extrapolation (Greenwood and Hanson (2014)), gut feel (Graham, Harvey, and Puri (2015)), and the availability heuristic (Dessaint and Matray (2017))). With regard to preference-based biases, Shue's (2013) findings on peer effects in managerial decision-making are consistent with "keeping up with the Joneses" preferences. Other work studies the influence of prospect theory in initial public offerings (IPOs) and CEO compensation (Loughran and Ritter (2002), Dittmann, Maug, and Spalt (2010)). In addition to advancing the literature on nonstandard managerial preferences, I contribute to the study of investment distortions induced by nonstandard decision-makers more broadly (e.g., Malmendier and Tate (2005), Greenwood and Hanson (2014), and Krüger, Landier, and Thesmar (2015)).

Second, I add to the corporate finance literature on acquisitions and divestitures. My paper documents significant distortions in firms' divestment decisions, and links these distortions to differences in sunk costs that firms experience during the acquisition process. This focus on deviations from basic economic principles differs from prior research, which mostly examines neoclassical theories and the role of social ties in explaining divestitures of previously acquired businesses, with the latter encompassing both information and agency channels. Previously identified factors include whether an acquisition is industry-diversifying (Porter (1987), Kaplan and Weisbach (1992), Maksimovic, Phillips, and Prabhala (2011)), the degree of human capital transferability (Tate and Yang (2024)), acquirer-target social ties (Ishii and Xuan (2014)), as well as industry shocks and cultural mismatch (Cronqvist and Pély (2024)).[3] Weisbach (1995) documents a higher propensity to divest an acquired business after the CEO who led the acquisition is replaced (see also Hayward and Shimizu (2006) and Pan, Wang, and Weisbach (2016)). This finding of a greater general commitment to a business by acquiring CEOs could, however, be due to a variety of factors, including differences in beliefs or information between the incumbent and new CEO, or the CEO change reflecting the board's attempt to effect a change in corporate strategy. By contrast, I document variation in CEOs' commitment to an acquired business due to differential exposure to sunk costs. This allows me to attribute behavior to a specific channel, namely, a sunk cost effect.

Third, I contribute to the behavioral economics literature on sunk cost effects. I study sunk costs in a cleanly identified and high-stakes setting. Few prior papers find suitable field settings to isolate sunk cost effects, and these settings tend to focus on individuals making personal consumption choices rather than on professional decision-makers. In motivating their sunk

[3] Another literature studies divestitures independent of whether a divested segment was previously added through an acquisition. Again, the focus of prior work has been on neoclassical and social factors, including performance decline (Shleifer and Vishny (1992)), productivity gains from asset reallocations (Maksimovic and Phillips (2001)), reputation concerns (Boot (1992), Grenadier, Malenko, and Strebulaev (2014)), segment industry liquidity (Schlingemann, Stulz, and Walkling (2002)), and segment-headquarters proximity and social interactions (Landier, Nair, and Wulf (2007)).

cost field experiment involving water purifiers in Zambia, Ashraf, Berry, and Shapiro (2010) note that evidence on sunk costs has been "confined largely to hypothetical choices and a single, small-scale field experiment [involving theater subscriptions (Arkes and Blumer (1985))]." Two additional papers provide evidence for sunk costs affecting the auction behavior of consumers (Augenblick (2015)) and car usage among Singaporean drivers (Ho, Png, and Reza (2017)). Other related work, while focusing on high-stakes contexts, is consistent with various explanations and underlying mechanisms. Staw and Hoang (1995) and Camerer and Weber (1999) document escalation of commitment by teams in the National Basketball Association (NBA) to high-ranking draft picks. While consistent with a sunk cost interpretation, the alternative hypothesis of optimistic ex ante beliefs about player quality coupled with gradual learning is difficult to rule out (Eyster (2002)). Belief-based mechanisms are also difficult to dispel in Jin and Scherbina's (2011) finding that mutual fund managers are less reluctant to close losing positions that they inherit from others. By cleanly documenting that sunk costs matter in an economically important setting involving the most sophisticated decision-makers, namely, CEOs, who typically have decades of professional experience (Dittmar and Duchin (2015), Schoar and Zuo (2017)), my paper clarifies that the inclination to account for sunk costs is deeply rooted and not easily corrected through education or professional experience.

The rest of the paper proceeds as follows. Section I introduces a simple conceptual framework of managerial decision making in the presence of sunk cost effects and discusses microfoundations. Section II describes the data and presents summary statistics. Section III discusses the empirical strategy. Sections IV and V present the main results, documenting the effects of sunk costs on firms' divestment behavior and discussing channels and implications. Sections VI concludes.

I. Conceptual Framework

A. *Reduced-Form Framework*

Setup. The framework, summarized in Figure 1, features three periods. At $t = 0$, the manager of a firm can buy an asset at cost $\overline{C} = C + \Delta C$, where C is known to the manager upon making the investment decision, whereas ΔC is a mean-zero random variable determined at some unmodeled time between $t = 0$ and $t = 1$. The manager has sufficient budget to make the investment. At $t = 1$, the manager can decide to keep the asset or divest it to an unrelated firm at some price P (the "market price"), independent of ΔC and specified below.[4] If she keeps the asset, it generates, or requires the firm to pay, an interim cash flow X at $t = 1$, where X is also a mean-zero random variable independent of

[4] Consequently, the cost shock ΔC generates nonfundamental fluctuations in acquisition prices, that is, variation in *unrecoverable* (sunk) costs. The empirical analysis presents extensive tests consistent with post-merger-agreement market fluctuations inducing nonfundamental variation in acquisition prices (see, for example, Section III.C for details).

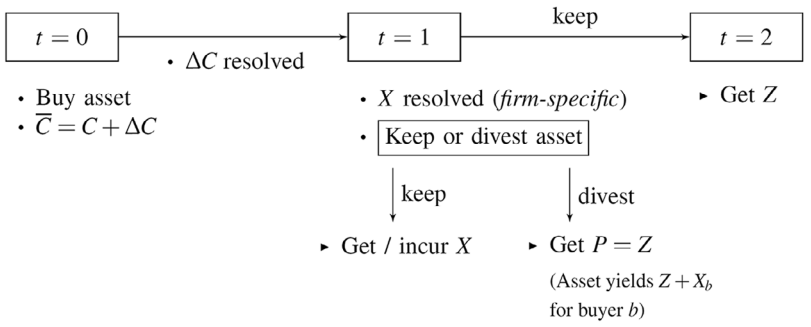

Figure 1. Framework timeline. This figure summarizes the conceptual framework presented in Section I.A.

ΔC. Note that X is determined at $t = 1$ prior to the manager's decision to keep or sell the asset. At $t = 2$, the asset delivers a certain cash flow of Z, known at $t = 0$, to its owner. The discount factor for all cash flows is one.

Empirically, the interim cash flow X can be thought of as *synergies*, which can be positive ($X > 0$) or negative ($X < 0$). Synergies are firm-specific. If the firm owning the asset at $t = 1$ sells it to another firm after learning its realized synergy level X, the asset payoff for the new buyer (b) will involve a new synergy draw (X_b). The assumption of uncorrelated synergies is common in the literature (Betton, Eckbo, and Thorburn (2008)) but could be relaxed. Intuitively, a motive to divest ensues as long as asset synergies are imperfectly correlated across firms.[5]

Sunk Cost Manager. The manager deciding whether to buy the asset at $t = 0$ is risk-neutral and has standard beliefs but potentially has nonstandard preferences. In this case, sunk costs affect her utility. Specifically, the manager incurs a disutility cost from divesting the asset that is increasing in the overall cost $\overline{C}$ required to buy the asset. (This reduced-form modeling of sunk costs is similar to Augenblick (2015). Section I.B and Internet Appendix Section II discuss more psychology-driven modeling approaches.) Conditional on buying the asset at $t = 0$, the manager at $t = 1$ solves

$$\max_{d_1 \in \{0,1\}} (1 - d_1)(X + Z) + d_1 \left(P - \underbrace{\kappa \overline{C}}_{\substack{\text{sunk cost} \\ \text{disutility}}} \right),$$

where $d_1 = 1$ indicates divestment and $d_1 = 0$ indicates continuation, P is the asset's market price, and $\kappa = 0$ captures a standard manager whereas $\kappa > 0$ captures a manager affected by sunk costs.

[5] Positive firm-specific synergies might stem from economies of scale, market power, product complementarities, or a combination of talent. Negative firm-specific synergies might stem from inefficient deployment of managerial resources, high integration and operating costs, or a misfit of company cultures.

Market Price. Managers at other firms are also risk-neutral and have standard beliefs. The asset's expected value to other firms at $t = 1$ is Z since, as discussed, synergies are independent across firms and unknown prior to owning the asset. For simplicity, I assume no transaction costs and a competitive bidder market at $t = 1$, in which case the divestiture price is simply $P = Z$.[6]

Implications. The framework delivers straightforward results regarding the divestment distortions of the firm that buys the asset at $t = 0$ and whose manager accounts for sunk costs.

RESULT 1 (Standard Manager): *For a standard manager ($\kappa = 0$), the probability of divesting the asset at $t = 1$ (conditional on asset ownership at $t = 0$) is unrelated to the realized cost shock ΔC.*

A standard manager divests the asset if and only if $X + Z < P$, when the market price exceeds the true value of the asset to the firm. With $P = Z$, this depends only on the synergy level X.

RESULT 2 (Sunk Cost Manager): *For a sunk cost manager ($\kappa > 0$), the probability of divesting the asset at $t = 1$ (conditional on asset ownership at $t = 0$) is decreasing in the realized cost shock ΔC.*

A sunk cost manager, by contrast, divests the asset if and only if $(X + Z) < P - \kappa(C + \Delta C)$, and with $P = Z$, if and only if $X < -\kappa(C + \Delta C)$. Clearly, for a given known cost component C, a higher cost shock ΔC makes it less likely that the divestment condition will be met.

Contrasting Results 1 and 2 yields a natural and testable prediction for sunk cost effects: post-investment-decision cost shocks are associated with managers' subsequent propensity to divest. Testing this prediction in the data is the key contribution of this paper. The framework also highlights two points. First, finding an empirical relation between past cost shocks and propensity to divest is not easy to reconcile with optimal decision making. Second, sunk cost managers deviate from the NPV-optimal divestment rule, implying real implications of sunk cost effects for firms in general.[7]

B. Microfoundations of Sunk Cost Effects

Perhaps surprisingly considering the presumed importance of sunk cost effects, there is no consensus view on the precise mental processes that lead people to take sunk costs into account.

[6] These assumptions are not important for delivering the key sunk cost predictions, and I relax them in Internet Appendix Section II when embedding sunk costs in a prospect theory framework (Kahneman and Tversky (1979), Thaler (1980)). Also, with only three periods and thus one divestment period, it is not necessary to make assumptions on whether other managers are subject to sunk cost effects, and if so, whether they are naïve or sophisticated about sunk cost effects.

[7] In the framework, the NPV of the asset at $t = 1$ is $X + Z - P$, and with $P = Z$, it is captured entirely by the realized synergy level X. The sunk cost manager divests when $X < -\kappa(C + \Delta C)$, implying continuation under a negative NPV (as long as the NPV is not too negative).

One leading microfoundation of path dependence through sunk cost effects is prospect theory (Kahneman and Tversky (1979), Thaler (1980)). In Internet Appendix Section II, I derive the key sunk cost prediction regarding the dependency of the divestiture probability on the realized post-investment cost shock in a prospect theory framework in which I explicitly add managerial reference dependence and diminishing sensitivity to losses and gains to the framework of Section I.A. Intuitively, a prospect theory manager codes a higher cost shock as a larger loss. Diminishing sensitivity to losses can induce the manager to keep the asset, even when facing additional costs from negative synergies, *because of* a high initial cost shock—the further the manager is in the loss domain, the lower the additional disutility from additional costs. Internet Appendix Figure IA.2 depicts this argument.[8]

While prospect theory is an intuitive psychological underpinning of sunk cost effects, other work emphasizes additional potential microfoundations, in particular the concept of cognitive dissonance (Festinger (1957)).[9] For example, Staw, Barsade, and Koput (1997) argue that people frequently persist in a course of action, and commit additional resources, to seek consistency and avoid dissonance that would be experienced upon taking a conflicting action—such as, in the present context, divesting a previously acquired segment. Through this lens, the sunk cost disutility cost in the reduced-form framework may also be interpreted as a dissonance cost upon divestiture, with the magnitude of dissonance increasing in the cost sunk into the asset.

Overall, there are several plausible, potentially complementary theories in psychology and economics to microfound the key prediction in Section I.A on the relation between sunk acquisition costs and subsequent propensity to divest.[10] In addition, it can be useful to consider how sunk cost effects relate to another empirical phenomenon, namely, the disposition effect (i.e., investors' tendency to sell winning stocks and hold losing stocks; see Shefrin and Statman (1985)). The disposition effect and sunk cost effects could be related to the extent that they arise from the same underlying psychological mechanism.[11] At the same time, there are differences. Related to my setting, the majority of divestitures happen at a loss relative to the acquisition price (see Section V.D),

[8] In behavioral economics, prospect theory and reference dependence have been applied to a broad range of additional settings, including labor supply, insurance, housing markets, and lottery choice (see DellaVigna (2009) for a comprehensive survey).

[9] Cognitive dissonance theory has been recognized as the "most important development" and "perhaps the most famous idea" in social psychology (Aronson (1997), Barberis (2009)). In contemporaneous work, Eyster, Li, and Ridout (2022) develop a tractable economic model of ex post rationalization that formalizes the concept of cognitive dissonance.

[10] Martens and Orzen (2021), who provide novel laboratory evidence on sunk cost effects, conclude that their findings are "best explained by a combination of prospect theory … [and] self-justification [due to cognitive dissonance]."

[11] Chang, Solomon, and Westerfield (2016) propose cognitive dissonance as a potential mechanism underlying the disposition effect. Barberis and Xiong (2009, 2012) show that prospect theory frequently predicts the *opposite* of the disposition effect and develop realization utility as an alternative preference-based explanation (see also Ingersoll and Jin (2013)). Ben-David and Hirshleifer (2012) instead emphasize belief revisions as a possible explanation for the disposition effect.

suggesting that firms do not tend to divest "winners."[12] Compared to investors' stock investments, it would seem less likely that firms acquire a business with the *intention* to fully re-sell it in the future. Furthermore, sunk cost effects can apply broadly beyond investment–resale contexts. In Section V.F, I provide a more speculative discussion of how sunk cost effects might be at play in a variety of further settings in corporate finance, including other sunk cost–intensive types of firm investment such as research and development (R&D) as well as financial intermediation.

II. Data

This paper features two key data elements. First, I identify divestitures of previously acquired businesses for a comprehensive set of stock acquisitions. Second, I collect detailed data on acquisition terms, which are central to my identification strategy. I describe the key data steps in this section and provide additional detail in Section III of the Internet Appendix.

A. Divestitures of Previously Acquired Businesses

I start from a standard data set on stock acquisitions from the Securities Data Company (SDC) Platinum M&A database. Applying standard data filters (Fuller, Netter, and Stegemoller (2002), Moeller, Schlingemann, and Stulz (2004), Betton, Eckbo, and Thorburn (2008), Netter, Stegemoller, and Wintoki (2011)), the sample comprises several thousand domestic acquisitions by U.S. public acquirers between 1980 and 2016. I then identify divestitures from two sources, as detailed below.

Divestitures from SDC. I extract all transactions involving U.S. entities that SDC flags as a Divestiture, Spinoff, or Leveraged Buyout. These transactions comprise *any* asset sales, independent of whether the seller grew the business parts organically or previously acquired them. I then link the acquisition and divestiture data sets using SDC's six-digit Committee on Uniform Securities Identification Procedures (CUSIP) identifier. One advantage of this approach is that it is immune to name changes of the acquirer or the acquired business.

Divestitures from News Search. One limitation of the SDC approach is that CUSIPs can change over time, implying that the matching procedure above might fail to identify some divestitures. To obtain a comprehensive divestiture sample, I perform a systematic divestiture news search in Nexis, similar to Cronqvist and Pély (2024), for acquisitions not identified as a "divestiture candidate" through SDC. Even in the presence of name changes, newspaper articles and news wires often reference former firm or segment names, which allows one to accurately track acquisitions over time.

[12] While the disposition effect compares the proportion of gains and losses that investors realize, seminal papers on the disposition effect find that conditional on a stock sale, the majority of sales involve gains rather than losses (Shefrin and Statman (1985), Odean (1998), Grinblatt and Keloharju (2001)).

Verifying Divestitures. To verify the correctness of each divestiture, I rely on additional newspaper articles as well as SEC filings, such as firms' annual, quarterly, and current reports (10-K, 10-Q, and 8-K, respectively) including exhibits (in particular, Exhibit 21, Subsidiaries of the registrant). After eliminating incorrect divestitures, partial divestitures, and divestitures by a new owner (i.e., after the original acquirer has itself been acquired), the initial combined SDC-Nexis sample consists of 543 correctly identified full divestitures. I exclude partial divestitures (following Kaplan and Weisbach (1992)) to focus on cases in which a firm *truly decommits* to a previously acquired business, an essential requirement to identify the effects of sunk costs on decision making. I disregard divestitures after the acquirer has itself been taken over (in contrast to Kaplan and Weisbach (1992) and Cronqvist and Pély (2024)) to focus on cases in which the divesting firm is the same firm that experienced the original acquisition cost change. I also exclude divestitures in which the initial acquisition involves an option-to-acquire agreement or resulted in a lawsuit about the purchase price, as these features interfere with my identification strategy requiring no remaining procedural and contractual uncertainty. Similarly, I disregard divestitures that are management buyouts (MBOs), as these deals involve management acting on both sides of the transaction. Internet Appendix Table IA.II provides a step-by-step overview of the divestiture sample construction.

B. Collection of Acquisition Terms

In a next step, I hand-collect the exact merger terms of the initial acquisition, that is, the deal in which the divesting firm originally acquired the subsequently divested business. I am frequently able to find the actual merger agreement between parties, if firms attach it as Exhibit 2 (Plan of acquisition) to an SEC filing, such as an 8-K, 10-Q, 10-K, or S-4 (Registration of securities issued in business combination transactions) filing. Alternatively, I retrieve deal terms from the main body of SEC filings, as well as analyst conference call transcripts, news articles, and news wires. Section III.B of the Internet Appendix displays several examples of merger agreements from my sample.

I find the precise deal terms for 89% of acquisitions in my sample. This fraction is large both in its own right and compared to prior work, although comparisons are difficult (see Internet Appendix Section III.B for details). Since my identification hinges on exposure to market fluctuations between merger agreement and completion, I narrow the sample to acquisitions with a *transaction period* (Ahern and Sosyura (2014)), which I define as the period from two days after the final merger agreement to the merger completion date, of at least 10 days. In the few instances in which the dates in SEC filings differ from those in SDC, I rely on the dates from the official SEC documents. Most commonly, I make adjustments when SDC bases the announcement date on a letter of intent to merge, a legally *nonbinding* document that only stipulates a preliminary agreement to merge.

C. Additional Data and Final Divestiture Sample

I supplement the data set with standard data from the Center for Research in Security Prices (CRSP) and Compustat. Since my empirical approach features an event-time analysis (time between acquisition and divestiture), I construct both deal- and deal-year-level control variables. The Appendix contains definitions of all variables that I use in this study. Dropping observations with incomplete data on control variables yields a final sample of 370 divested acquisitions. Of these, 279 acquisitions, or 75%, are *Fixed Shares* deals; the remaining 25% are *Fixed Dollar* deals. These relative frequencies are nearly identical to those in prior papers (Mitchell, Pulvino, and Stafford (2004), Ahern and Sosyura (2014)).

D. Matched Sample of Nondivested Acquisitions

In a final step, I extend the sample to include *Fixed Shares* acquisitions that are not subsequently divested. This allows me to capture the possibility that sunk costs might induce managers to *continually* not divest an acquired business.[13] In a nutshell, I construct the broadened sample by matching each divested *Fixed Shares* acquisition to a similar nondivested acquisition based on standard firm and deal characteristics. While the main analysis is based on this broadened sample, Section IV.D replicates the findings for the within-divestiture sample, that is, omitting the matching step. To further bolster the results, I show robustness to a sample of divested and nondivested acquisitions constructed without matching, as discussed in Section IV.B and in more detail in Internet Appendix Section V.B.

This outcome-based matching approach, or *case control sampling*, differs from the more standard approach of collecting a random sample of acquisitions and determining their divestiture status over time. Having its origin in the fields of statistics and epidemiology, the case control design is frequently used to study *rare* outcomes.[14] In such contexts, a major advantage of this design is that it often has higher power than standard sampling and thus requires smaller sample sizes (Schlesselman (1982)). Intuitively, standard sampling tends to require large samples when studying rare outcomes since much of the sample will remain free of the outcome. Given the infrequency of full divestitures in the data and the time-intensive nature of merger terms collection from (predominantly) appendices to SEC filings, case control sampling is the natural choice in my setting.

Beyond power considerations, case control sampling is attractive since both logit and hazard models—the two models relevant to this paper (see

[13] This does not require that the acquirer continues to hold a business up to the present. A firm that repeatedly fails to divest a nonperforming business might, for example, become a takeover target. The empirical analysis treats such cases as nondivested acquisitions censored at when the acquirer is taken over (see Section III.E for details).

[14] For example, the case control design is commonly used to examine whether patients with a rare disease have had a differential exposure to a given factor of interest compared to similar subjects that are free of the disease.

Section III.E)—can be directly applied to case-control–based samples. In particular, the logit and hazard parameters of interest on the market-induced acquisition cost change are unaffected, and their interpretation is identical to that in standard sampling (Mantel (1973), Prentice and Breslow (1978), Schlesselman (1982)). Loosely speaking, with odds or hazard ratios, differential sampling fractions from case control sampling will affect the constant term but *not* the slope parameter of interest once the log transformation is applied. As a result, the discussion and interpretation of the empirical strategy (see Section III) remains unaffected as well. Section V of the Internet Appendix presents the full argument establishing the equivalence of case control and standard sampling in terms of the logit and hazard parameters of interest.

To implement the case control matching procedure, I proceed in three steps (see Section III.C of the Internet Appendix for full details). First, I focus the set of potential matches on "divestable" acquisitions—those that are industry-diversifying and involve out-of-state target firms—to ensure that matched acquisitions have a similar ex ante propensity to be divested. Matched subjects being similarly *susceptible* to the outcome of interest (i.e., a divestiture in my setting) is an important requirement in case control designs (Grimes and Schulz (2005)).[15] Second, using this set of nondivested deals, I perform propensity score matching to find the acquisition that is most similar to a given divested *Fixed Shares* acquisition. I match on standard firm and deal characteristics as detailed in the Internet Appendix. As is crucial for case control sampling, I do *not* match on the key variable of interest, that is, the (endogenous or market-induced) cost change of the initial acquisition. Third, I verify whether each matched acquisition used a *Fixed Shares* structure and, if not, I take the next-closest match from the previous step until I end up with a *Fixed Shares* match. This procedure results in matched acquisitions that are similar in terms of deal and firm characteristics used as matching and control variables (see the balance table in Internet Appendix Table IA.III).

The resulting main sample consists of 4,461 firm-year observations (years since acquisition) that correspond to the 279 divested *Fixed Shares* acquisitions from Section II.C and the matched, nondivested acquisitions.

E. Summary Statistics

Figure 2 shows the frequency distributions of acquisitions and divestitures over time for this main sample. Many acquisitions were undertaken in the late 1980s and, especially, the mid-to-late 1990s (Panel A). My sample therefore appears representative of stock mergers in general, as these were the periods that witnessed a surge in stock merger activity (e.g., Betton, Eckbo, and Thorburn (2008)). Among the divested deals, there is considerable variation

[15] Previous literature has documented a significantly higher divestiture propensity among industry-diversifying acquisitions and out-of-state firm segments (Kaplan and Weisbach (1992), Landier, Nair, and Wulf (2007)). Internet Appendix Table IA.VIII confirms that both of these characteristics are also strong divestiture predictors in my general M&A sample.

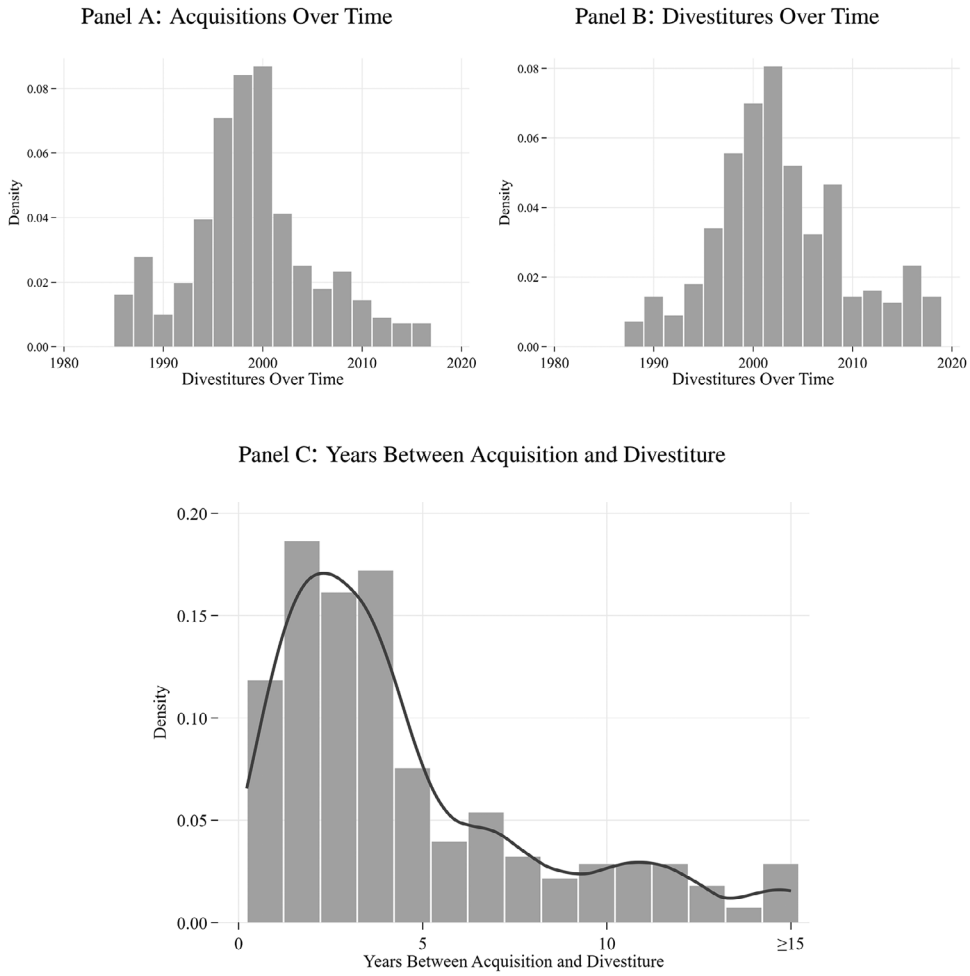

Figure 2. Acquisitions and divestitures over time. This figure shows the frequency distributions of acquisitions and divestitures in my sample over time. Panel A shows acquisition frequencies. Panel B shows divestiture frequencies. Panel C shows the distribution of the time span between acquisition and divestiture in years.

as to when the divestiture occurs (Panel B). While divestiture activity is more pronounced during economic downturns, many divestitures also occur during other periods, such as the mid-2000s. The average (median) acquisition is divested after 4.70 (3.37) years, and almost 90% of divestitures occur within 10 years of the acquisition (Panel C).

Table I presents summary statistics for the main sample. Panel A shows deal-level variables. Both the average and median acquisition in my sample experiences a negative stock market reaction at deal announcement (three-day cumulative announcement return (CAR) of −0.30% and −0.68%, respectively). An unfavorable announcement reaction on average is typical for stock mergers

Table I
Summary Statistics

This table reports summary statistics for the main sample, which consists of divested and nondivested fixed exchange ratio (*Fixed Shares*) stock acquisitions. Panel A reports summary statistics on deal-level characteristics used as control variables, as well as statistics on the acquisition and divestiture timelines. Panel B reports summary statistics on time-varying control variables. Panel C reports summary statistics on the key variables pertaining to acquirer and market returns during the period between merger agreement and completion, as well as statistics on the resulting acquisition cost changes. The Appendix provides variable definitions.

Panel A: Deal-Level Variables ($N = 558$)

	Mean	Median	*SD*	*P*25	*P*75
CAR (%)	−0.30	−0.68	10.80	−5.82	4.35
CAR < 0	0.54	1	0.50	0	1
Deal Value ($ millions)	1, 058.60	99.43	3, 611.97	26.56	522.71
Deal Value (ln)	4.85	4.60	2.10	3.30	6.26
Acquirer Size ($ millions)	5, 577.30	626.40	20, 576.70	139.27	2, 867.48
Acquirer Size (ln)	6.43	6.44	2.18	4.94	7.96
Public Target	0.50	1	0.50	0	1
Beta	1.16	1.14	0.35	0.97	1.34
All-Stock Deal	0.56	1	0.50	0	1
Transaction Period (Days)	105	90	79.07	50	133
Years Until Divestiture	4.70	3.37	4.32	1.88	6.13

Panel B: Deal-Year-Level Variables ($N = 4, 461$)

	Mean	Median	*SD*	*P*25	*P*75
12-Month Return	1.18	1.06	0.81	0.76	1.38
Industry Distress	0.36	0	0.48	0	1

Panel C: Acquisition Cost Change Variables ($N = 558$)

	Mean	Median	*SD*	*P*25	*P*75
ΔR^{Acq} (%)	3.81	4.29	30.52	−9.97	19.95
ΔC^{Acq} (% of Market Cap)	1.99	0.29	8.27	−0.97	2.60
ΔR (%)	0.59	1.07	9.08	−3.16	5.40
ΔC (% of Market Cap)	0.55	0.08	3.19	−0.33	0.95

(Betton, Eckbo, and Thorburn (2008)), and, particularly for *Fixed Shares* mergers (Mitchell, Pulvino, and Stafford (2004)). The median acquisition had a transaction value of $99 million, and thus my setting involves decisions that are of substantial economic importance. Half of the deals involve public targets and 56% are pure stock deals. The average length between merger agreement and completion—the key period for the construction of market-induced acquisition cost changes—is 105 days, similar to the average lengths reported in Giglio and Shue (2014), Ahern and Sosyura (2014), and Hackbarth and Morellec (2008). Panel B shows deal-year variables. The median acquirer's

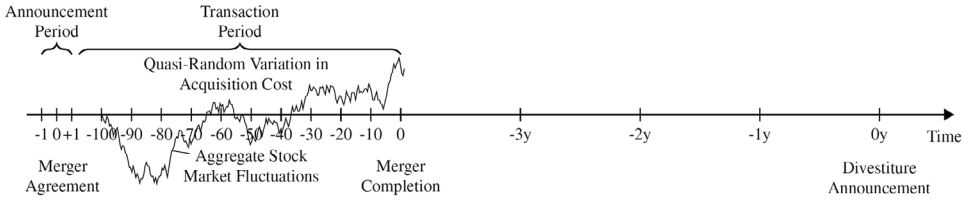

Figure 3. Event timeline. This figure summarizes the event timeline. Numbers refer to days or years relative to one of three main dates: merger announcement/agreement, merger completion, and divestiture decision. The lengths of the respective periods shown below correspond roughly to the average observation in my sample (see Table I).

12-month return between acquisition and divestiture is +6%, and about one in three years is classified as a year in which the industry of the acquired business is in distress. I defer the discussion of the variables pertaining to acquisition cost changes (Panel C) to Section III.B.

Overall, my sample is representative of stock mergers more generally as gauged by, for example, market reaction at announcement, transaction period length, and merger frequency over time.

III. Empirical Strategy

The key component of my identification strategy is that in *Fixed Shares* acquisitions, aggregate market fluctuations between parties entering into a binding merger agreement and completion of the acquisition trigger plausibly exogenous changes in acquisition costs. The empirical analysis relates these acquisition cost shocks to firms' propensity to subsequently abandon the acquired business through divestiture. In other words, I study the link between quasi-random variation in the intensity of sunk costs and firms' intensity of commitment to an acquisition. Figure 3 summarizes the event timeline.

A. Fixed Shares Acquisitions

In general, transacting parties can structure a stock acquisition using a *Fixed Shares* or *Fixed Dollar* structure. In a *Fixed Shares* merger, parties stipulate a fixed number of acquirer shares to be exchanged in the merger agreement. In a *Fixed Dollar* acquisition, parties specify a variable exchange ratio, such that the merger consideration in dollars remains fixed. In my sample, deals across the two structures are indistinguishable along many observable characteristics, including deal value, diversifying and public target deal status, and length of time between deal agreement and completion (Internet Appendix Table IA.IX). These patterns are consistent with Ahern and Sosyura (2014), who exploit the similarity across the two stock deal structures for identification. An attractive feature of my identification is that it entirely circumvents any concerns about selection into *Fixed Shares* versus *Fixed Dollar* deal structures. The empirical analysis focuses on *Fixed Shares* acquisitions, and

uses differential exposure to market movements within this set of acquirers. In addition, the *Fixed Dollar* acquisitions constitute a suitable placebo group, especially in light of the observed similarity of deals across the two deal types. For *Fixed Dollar* deals, I can construct hypothetical acquisition cost changes also based on aggregate market movements (see Section III.C for details).

Acquisition agreements can specify various closing conditions, implying that parties may or may not know the closing date with certainty when signing the final agreement (Mitchell, Pulvino, and Stafford (2004)). If aggregate market fluctuations are plausibly exogenous, it is irrelevant for identification whether there is uncertainty about the length of exposure to these quasi-random fluctuations. Furthermore, since *Fixed Shares* acquisitions fix the number of shares to be exchanged at deal agreement, the period of interest for identification (i.e., the period inducing post-agreement acquisition price variation) is always the period between final agreement and completion. By contrast, deal specifics can vary in *Fixed Dollar* deals that calculate floating exchange ratios. This can involve either the price at completion or the average price over a predetermined period, such as the 10- or 30-day period prior to closing or the entire period between agreement and closing. Such heterogeneity in periods of interest across deals and share price averaging does not exist in *Fixed Shares* acquisitions.

B. Empirical Design

Change in Acquisition Cost. I compute the endogenous change in acquisition cost, ΔC_i^{Acq}, induced by post-agreement fluctuations in the acquirer's stock price in *Fixed Shares* mergers as

$$\underbrace{\Delta C_i^{Acq}}_{\text{Acq. Cost Change}} = \underbrace{\Delta R_i^{Acq}}_{\text{Cumulative Return}} \times \%stock_i \times \underbrace{\frac{Deal\ Value_i}{Market\ Cap_i^{Acq}}}_{\text{Relative Deal Value}}, \tag{1}$$

where $\%stock_i \in (0, 1]$ denotes the fraction of the merger consideration that acquirer i pays in stock, relative deal value is the deal value when the parties enter the merger agreement relative to the acquirer's market capitalization as of trading 21 days prior to deal announcement, and the cumulative return is defined as the cumulative daily return to the acquirer, $R_{i,t}^{Acq}$, during the transaction period,

$$\Delta R_i^{Acq} = \sum_{t=\tau_1+2}^{\tau_2} R_{i,t}^{Acq}, \tag{2}$$

where τ_1 is the merger agreement date and τ_2 is the merger completion date. Scaling by the acquirer's market capitalization in equation (1) implies that I analyze sunk costs relative to the acquirer's size, that is, *proportional* sunk costs. Intuitively, a \$10 million change in acquisition costs presumably looms larger in a firm with a market capitalization of \$100 million compared to a firm with a market capitalization of \$1 billion.

To isolate plausibly exogenous variation, I replace the acquirer's daily stock return in equation (2) with the daily market return, accounting for the acquirer industry's market beta (estimated as in Krüger, Landier, and Thesmar (2015)). This approach is reminiscent of event studies estimating a firm's counterfactual return. To account for the fact that the market return is positive on average, I subtract the expected daily market return (calculated as the average yearly return to the CRSP value–weighted index since the beginning of my sample period in 1980, which equals 12%, divided by 365) in this modified equation. Disregarding the average market appreciation would lead to a mechanical correlation of the market return variable with the length between merger agreement and completion, which in turn is correlated with observable deal characteristics. In sum, I modify equation (2) to

$$\Delta R_i = \sum_{t=\tau_1+2}^{\tau_2} \hat{\beta}_{i,\tau_1}\left(R_t^{Mkt} - E_{\tau_1}\left[R_t^{Mkt}\right]\right), \tag{2'}$$

where ΔR_i is purged of any endogeneity as it is purely determined by unexpected aggregate market movements. I then compute the market-driven change in acquisition cost as

$$\Delta C_i = \Delta R_i \times \%stock_i \times \frac{Deal\ Value_i}{Market\ Cap_i^{Acq}}. \tag{1'}$$

Equation (1′) differs from equation (1) because it uses the market-induced cumulative return instead of the endogenous cumulative return to the acquirer, and hence isolates plausibly exogenous variation in acquisition cost.

Summary Statistics. Panel C of Table I provides summary statistics on the variables pertaining to acquisition cost changes. The average return to the acquirer during the transaction period is 3.81%. The corresponding average market return, after accounting for expected returns, is 0.59%. The variation in aggregate stock market fluctuations across deals is economically meaningful, with the IQR of the market return being about 8.5 pp. These returns also induce economically relevant variation in acquisition costs, with the IQR of the market-induced cost change being slightly larger than 1 pp relative to the acquirer's market capitalization.

Estimating Equation. To test for sunk cost effects, I next relate the market-induced change in acquisition cost calculated in equation (1′) to the rate of subsequent divestiture:

$$\Pr\left(Divestiture_{i,t}\right) = \alpha + \kappa\, \Delta C_i + \boldsymbol{\delta}' \boldsymbol{X}_{i,t} + \nu_{j(Acq)} + \nu_{j(Tar)} + \mu_{t_0} + \varepsilon_{i,t}, \tag{3}$$

where i denotes an acquisition, t is the time that has elapsed since the acquisition in years, t_0 denotes the acquisition (calendar) year, $Divestiture_{i,t}$ is an indicator variable that equals zero in all years prior to the divestiture and one in the year of divestiture, and ΔC_i is the main variable of interest. If the identifying assumptions hold (see Section III.C), and under the null hypothesis that sunk costs do not affect firm decision making, κ should not be

statistically different from zero. The vector of control variables, $\boldsymbol{X}_{i,t}$ comprises time-invariant and time-varying controls, $\nu_{j(Acq)}$ and $\nu_{j(Tar)}$ are acquirer and target industry fixed effects, and μ_{t_0} are acquisition year fixed effects. By including the latter, I identify sunk cost effects from differences in divestitures rates of deals that were undertaken in the same year but exposed to different market returns between merger agreement and closing.

C. Identifying Assumptions

For κ in equation (3) to identify the effect of sunk costs on divestiture decisions, (i) market fluctuations need to strongly affect acquirers' returns during the transaction period and need to be "as good as randomly assigned" conditional on covariates, and (ii) market fluctuations should not predict any other aspect related to the acquirer or acquired business that is itself relevant at the time of the decision to divest (Angrist and Pischke (2008), Wooldridge (2010), Pischke (2017)).

Assumption (i). Using the main sample, Panel A of Table II regresses cumulative firm returns during the transaction period on cumulative market returns, net of expected returns. Column (1) regresses firm returns only on market returns. Columns (2) to (4) add controls and industry and acquisition year fixed effects. The slope coefficient is highly significant across all columns and the Kleibergen and Paap (2006) F-statistic is above 70, confirming that aggregate market movements "partially affect" (Wooldridge (2010)) acquirer returns once other covariates are netted out.

Panel B of Table II regresses cumulative market returns, net of expected returns, on an array of observable deal and firm characteristics, including announcement return, deal value at merger agreement, acquirer size, acquirer market beta, and indicators for whether the deal is a diversifying or geographically diversifying deal, involves a public target, or is an all-stock deal. I find no evidence that market fluctuations experienced by acquirers between deal agreement and completion are predictable, when considering covariates individually or jointly. For example, the F-statistic for the joint significance of all variables is 0.56 (p-value of 0.81). These results are consistent with exogenous market movements inducing quasi-random acquisition cost variation between merger agreement and completion.

Assumption (ii). There are at least two main concerns regarding the assumption that market fluctuations do not affect any divestiture-relevant aspect at the time of the divestiture decision other than having affected sunk acquisition costs. First, market movements around acquisitions might influence business conditions for acquirers more generally, which in turn could affect future divestiture decisions. Second, acquisition price changes from market movements might affect the trajectories of acquirers and acquired segments, for example, by affecting synergies, investment constraints, and profitability, which in turn might affect divestiture rates. Related to these concerns, the firm dynamics literature has shown that startups founded in boom periods (when market

Table II

Market Fluctuations between Merger Agreement and Completion

This table reports results of the tests of the identifying assumptions that market fluctuations affect firm returns and that market fluctuations are "as good as randomly assigned" in the period between merger agreement and completion (the transaction period). In Panel A, the dependent variable is ΔR^{Acq}, the cumulative daily return to the acquirer during the transaction period (see equation (2)), expressed in %, where ΔR is the cumulative market return minus the cumulative expected market return during the transaction period (see equation (2′)), also in %. When control variables are included, all variables listed in Panel B are added to the model. In Panel B, the dependent variable is ΔR. The Appendix provides variable definitions. In both panels, all columns are estimated using ordinary least squares (OLS). t-statistics are shown in parentheses. Standard errors are clustered by quarter of the acquisition. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Market Fluctuations Affect Firms Returns

	(1)	(2)	(3)	(4)
ΔR	1.479***	1.527***	1.539***	1.413***
	(8.72)	(9.68)	(9.67)	(8.65)
Controls	No	Yes	Yes	Yes
Industry FE	No	No	Yes	Yes
Acquisition Year FE	No	No	No	Yes
N	558	558	558	558
Adjusted R^2	0.19	0.20	0.24	0.24
F-Statistic	76.07	93.78	93.59	74.78

Panel B: Market Fluctuations "as Good as Randomly Assigned"

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CAR < 0	0.092							
	(0.12)							
Deal Value (ln)		0.033						
		(0.15)						
Aquirer Size (ln)			−0.114					
			(−0.55)					
Diversifying Deal				−0.043				
				(−0.06)				
Geo-Diversifying Deal					−0.282			
					(−0.24)			
Public Target						−0.043		
						(−0.04)		
Beta							−1.402	
							(−0.78)	
All-Stock Deal								1.531
								(1.48)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acquisition Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	558	558	558	558	558	558	558	558

p-value (test for joint significance): 0.81.

movements likely increase acquisition prices) have higher growth potential (e.g., Sedláček and Sterk (2017)).

With respect to the concern of broader effects of market movements, my setting allows me to implement two separate placebo tests, discussed in detail in Sections IV.C and IV.E. In brief, the placebo tests rest on the idea that other channels through which the market movements might affect divestitures should also be detectable during periods other than between merger agreement and completion, and for acquisitions not structured as a *Fixed Shares* deal. However, the placebo tests find no evidence that hypothetical cost changes (calculated from market fluctuations following deal completion or for the *Fixed Dollar* acquisitions of Section II.B from market fluctuations between merger agreement and completion) predict divestiture rates.

Several additional aspects and tests help further alleviate the above identification concerns. First, all analyses include acquisition year fixed effects, that is, identification comes from within-year market fluctuations, to help account for business cycle effects. Second, differential market-induced final acquisition prices do not induce mechanical differences in operational characteristics such as cash holdings between *Fixed Shares* acquirers. Third, my findings are robust to (time-varying) measures of firm performance and constraints (Section IV.B), and are driven by firm-years in which the acquiring CEO is still in office (Section V.B). The finding of a CEO-specific effect elevates the hurdles for alternative explanations based on firm, industry, or market characteristics.

After establishing the main results and teasing out additional mechanisms that are consistent with the sunk cost interpretation, in Section V.E, I revisit and extend the discussion of identification concerns and alternative explanations.

D. Collar Clauses and Acquisition Withdrawals

Collars. About 10% of *Fixed Shares* acquisitions in my final sample involve so-called collars as part of the deal terms. In *Fixed Shares* deals, collars define caps and floors for the acquirer's stock price outside of which the merger terms may change according to a formula specified in the merger agreement. I address collars in three ways. First, my identification uses the exogenous *component* of acquisition cost changes stemming from market movements, rather than endogenous changes induced by the acquirer's stock price movements on which collars are based. For the roughly 10% of deals with collars, I modify the acquirer return calculation in equation (2) by limiting it to the maximum or minimum return that still results in an acquisition cost change. The results in Panel A of Table II are in fact those that obtain after accounting for collars, that is, the specifications regress *collar-adjusted* firm returns on market returns. A precise interpretation is thus that market movements strongly predict acquirer stock price movements after adjusting for collar bounds.

Second, my results remain unchanged (and in fact become slightly stronger) when I restrict the sample to the roughly 90% of "pure" *Fixed Shares* acquisitions with no collars (see Section IV.B).

Third, in Section IV.C of the Internet Appendix, I implement an alternative two-stage estimation approach that directly includes the endogenous cost change, taking into account collar caps and floors, as the main variable of interest. This approach, discussed in more detail at the end of Section IV.B, relies on the control function method (Wooldridge (2015)) to control for the endogeneity in the system. Section V.E provides further discussion of collars and potential other hedging strategies.

Acquisition Withdrawals. Discussion of the empirical strategy thus far assumes that once agreed upon, acquisitions are completed. One potential issue is that acquisition deals can be withdrawn, even after a merger agreement is reached. Empirically, only a small fraction of about 10% of stock deals are withdrawn, and many withdrawals happen for exogenous reasons such as regulatory or judicial obstacles. In fact, previous work exploits the frequent exogenous failure of mergers for identification (e.g., Savor and Lu (2009), Jacobsen (2014), and Malmendier, Opp, and Saidi (2016)).

Nonetheless, there remains a possibility of strategic withdrawals and endogenous selection into deal completion. As the regression results in Panel B of Table II reveal, acquirers' experienced market return between deal agreement and completion is not predicted by a wide array of deal and firm variables, allaying concerns based on selection on observables. With respect to selection on unobservables, the remaining concern is that post-agreement acquisition cost increases could make acquirers with low (perceived or true) synergy potential, but not those with high synergy potential, more likely to withdraw. Such differential sorting into withdrawal would similarly predict reduced divestiture rates after acquisition cost increases, even in the absence of sunk cost effects.

Several factors help alleviate this specific concern. Internet Appendix Figure IA.5 reveals a strongly nonmonotonic influence of post-agreement market fluctuations on stock acquisition withdrawal probabilities. Withdrawals after market increases are highly infrequent, suggesting that the concern of differential selection into deal withdrawal in response to acquisition price increases is of limited empirical importance.[16] Relatedly, unilateral deal cancellations are not costless. Beyond any forgone synergy gains, merger contracts can include sizable termination fees. The median and average breakup fee to be paid by the canceling party to the counterparty (often in cash) is 3% to 4% of deal value in the data (Officer (2003), Bates and Lemmon (2003)). To further address selection concerns, I conduct additional robustness checks in which I successively remove observations with the highest estimated withdrawal probabilities—for which the selection concerns are arguably most relevant—from the sample. As I discuss in Sections IV.B and IV.D, the estimated effect

[16] Two contemporaneous papers examine the relation between post-agreement market fluctuations and merger withdrawals in further detail (Fos and Yang (2020), Heath and Mitchell (2020)). While some of the sample criteria and results differ across the two papers, both are in line with the result in Figure IA.5 of the Internet Appendix that, particularly after market increases, only a small fraction of deals to be paid with stock are withdrawn.

of acquisition cost changes on divestiture rates remains stable as I gradually narrow the sample.

E. Estimation Method

In the main analyses, I estimate equation (3) using the semiparametric Cox (1972) proportional hazards model, commonly used for survival and duration analysis (time-to-event analysis).[17] The hazard model treats nondivested acquisitions as censored. The censoring date corresponds to the day before I begin the divestiture news search (December 15, 2018), or, if the acquirer is itself taken over at some point, to the acquisition date. The Cox (1972) model assumes the form

$$h(t|\boldsymbol{X}_i) = h_0(t)\exp\left(\boldsymbol{\delta}'\boldsymbol{X}_i\right), \tag{4}$$

where t denotes survival time and $h(t)$ is the hazard function that is determined by a set of covariates $\boldsymbol{X}_i$ and $h_0(t)$, the baseline hazard. The hazard function reflects the risk of failure at time t conditional on survival until t. The model is semiparametric as it makes no assumption on the functional form of the baseline hazard. It accommodates time-varying covariates when reshaping the data into "sub-spells" over which the covariates $\boldsymbol{X}_i$ are time-invariant. I reshape the data into one-year-long sub-spells, that is, time indicates years elapsed since acquisition (see equation (3)).

The standard Cox (1972) model assumes proportional hazards, that is, that the ratio of the hazards of any two observations is constant over time. A useful feature is that, for each covariate in the model, one can check whether this assumption might be violated using so-called Schoenfeld residuals (Schoenfeld (1982)), and if so, augment the model by including an interaction of that variable with a function of time. All of my analyses account for the possibility of time-dependent effects. I provide additional details in the results section, and an in-depth description of Schoenfeld residuals and how to use them to test for proportional hazards in Section VI of the Internet Appendix.

IV. Sunk Costs and Firm Decision Making

A. Main Result

The first set of results investigates the effect of quasi-random variation in acquisition costs on divestiture rates. Table III establishes the main result, implementing the estimating equation (equation (3)) using the Cox (1972) proportional hazards model. The dependent variable is an indicator variable that

[17] See the discussion in Jenter and Kanaan (2015) in the context of CEO turnover. In my context, *survival* corresponds to an acquisition that has not been divested (yet), *failure* corresponds to a divestiture, and *duration* at time t corresponds to the time interval between the acquisition date and t. While the hazard model is arguably the most suitable choice in my setting, Section IV.B shows robustness to using a logit specification instead (Efron (1988), Jenter and Kanaan (2015)).

Table III

Quasi-Random Sunk Acquisition Costs and Subsequent Divestiture Rates

This table reports estimates of the effect of quasi-random variation in acquisition costs on subsequent divestiture rates. The dependent variable is an indicator variable that equals one in the year in which an acquired business is divested and zero otherwise. The main variable of interest, ΔC, is the change in acquisition cost between merger agreement and completion induced by market fluctuations, as a percentage of the acquirer's pre-acquisition market capitalization (see equation (1′)). The Appendix provides variable definitions. All columns are estimated using the Cox (1972) proportional hazards model and show regression coefficients, not hazard ratios. Columns (3) and (4) allow covariates with a p-value below 0.15 in the Schoenfeld (1982) test for proportional hazards (please refer to Sections III.E and IV.A as well as Section VI of the Internet Appendix for additional details) to vary linearly with time. Column (5) allows these covariates to vary with log-time. All models include acquirer and target industry fixed effects as well as acquisition year fixed effects. z-statistics are shown in parentheses. Standard errors are clustered by quarter of the acquisition. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	(1)	(2)	(3)	(4)	(5)
ΔC	−0.065***	−0.068***	−0.075***	−0.077***	−0.074***
	(−2.77)	(−2.89)	(−3.05)	(−3.18)	(−3.14)
CAR < 0	−0.001	−0.015	0.083	0.068	0.140
	(−0.01)	(−0.08)	(0.37)	(0.31)	(0.58)
Deal Value (ln)	−0.003	−0.019	−0.074	−0.085	−0.019
	(−0.04)	(−0.30)	(−0.82)	(−1.00)	(−0.21)
Aquirer Size (ln)	−0.058	−0.045	−0.085	−0.099	−0.147*
	(−0.83)	(−0.70)	(−1.03)	(−1.23)	(−1.81)
Public Target	−0.208	−0.143	−0.333*	−0.266	−0.607***
	(−1.21)	(−0.81)	(−1.65)	(−1.35)	(−2.78)
Beta	0.240	0.153	0.625**	0.416	0.587*
	(1.00)	(0.64)	(2.01)	(1.38)	(1.71)
All-Stock Deal	0.216	0.193	0.291	0.257	0.282
	(1.26)	(1.15)	(1.13)	(1.05)	(1.05)
12-Month Return		−0.550***		−0.559***	−0.550***
		(−3.73)		(−3.70)	(−3.69)
Industry Distress		0.396***		0.465**	0.392*
		(2.63)		(2.25)	(1.72)
Time Interactions	No	No	Linear	Linear	Log
Industry FE	Yes	Yes	Yes	Yes	Yes
Acquisition Year FE	Yes	Yes	Yes	Yes	Yes
N	558	558	558	558	558
N (Firm-Years)	4,461	4,461	4,461	4,461	4,461

equals one in the year in which an acquired business is divested and zero otherwise. All columns include acquirer and target industry fixed effects as well as acquisition year fixed effects. Additionally, all columns show Cox (1972) regression coefficients, not hazard ratios. Thus, a coefficient of zero means that a given covariate is not found to affect the rate of divestiture. I cluster standard errors by time (quarter of the acquisition), since treatment is assigned based on market fluctuations between merger agreement and completion, with the

typical transaction taking about three months to complete (see Table I and Abadie et al. (2017)).[18]

Column (1) includes the main variable of interest, the market-induced change in acquisition costs, ΔC, as well as the characteristics used in the propensity score matching of Section II.D and further deal- and firm-level controls that might plausibly affect divestiture rates. The coefficient on acquisition cost variation is negative and strongly significant (at the 1% level). This result reveals that an increase in quasi-random acquisition costs reduces the rate of subsequent divestitures, consistent with managers taking sunk costs into account in their divestment decisions. The coefficient estimate of −0.065 implies that an interquartile market-induced cost increase relative to the acquirer's market capitalization (1.28 pp, see Table I) is estimated to reduce divestiture rates by 8%.

Column (2) adds time-varying controls to the specification, in particular the acquirer's stock return over the previous 12 months and an indicator that identifies years in which the industry of the acquired business is in financial distress. While the added controls are strongly significant (discussed below), the cost change coefficient remains almost unchanged. It increases slightly in magnitude (−0.068) and remains significant at the 1% level.

The economic magnitude of these distortions associated with quasi-random acquisition costs is meaningful yet plausible when compared to the effect sizes associated with control variables. For example, a 10 pp decrease in the 12-month return is estimated to increase divestiture rates by 5%, which is slightly less in magnitude than the estimated interquartile sunk cost effect. Periods in which the industry of the acquired business is in financial distress are, by contrast, associated with a larger effect size, increasing divestiture rates by close to 50%. The insignificant coefficients on the negative announcement return indicator, deal value at merger agreement, acquirer size, and public target status reflect the fact that these variables were used as matching variables and thus are similar for divested and nondivested acquisitions. The acquirer's market beta and the all-stock indicator do not significantly predict divestiture rates either.

Columns (3) to (5) take into account the results from the Schoenfeld (1982) tests for proportional hazards (see Internet Appendix Section VI for a detailed discussion of the Schoenfeld test results). Internet Appendix Table IA.XII reports how the Schoenfeld residuals for each of the covariates in column (2) of Table III depend on (linear) time. (Internet Appendix Table IA.XIII restricts the sample to acquisitions that are subsequently divested. I discuss this approach in Section IV.D.) In both tables, the correlation of the acquisition cost

[18] Abadie et al. (2017) argue that clustering choices should be guided by whether "clusters of units, rather than units, are assigned to a treatment" (see also Kline (2016)). Consistent with this, clustering by time is common in M&A settings (e.g., Baker, Pan, and Wurgler (2012), Fich, Lantushenko, and Sialm (2024)). That said, in Internet Appendix Table IA.VI, I show robustness to other clustering choices, in particular clustering by industry given that industry conditions at least partially contribute to divestiture patterns.

change residuals with time is clearly insignificant. Thus, there is no indication that the proportional hazards assumption might be violated for the main variable of interest. The lack of time dependence is also confirmed visually by the nearly perfectly flat line through the Schoenfeld residuals when plotted against time (Internet Appendix Figure IA.11). For some control variables, the Schoenfeld tests suggest that their effect on divestiture rates might be time-dependent. In the remaining columns of Table III, I therefore allow for time-dependent effects. To be conservative, I use a p-value cutoff of 0.15 to determine which variables to interact with time.

Columns (3) and (4) reestimate columns (1) and (2), respectively, allowing for linear time interactions. Column (5) reestimates column (2) as well, allowing for interactions with log-time.[19] Across columns, the coefficient on market-induced acquisition cost changes is very similar to that in specifications without time-dependent effects. If anything, the coefficient of interest increases slightly in magnitude and becomes more significant. For example, in column (4), an interquartile increase in market-induced acquisition cost is estimated to reduce divestiture rates by 9.4%.

In sum, the results of Table III document economically and statistically significant distortions in firms' divestment decisions triggered by quasi-random acquisition costs, as predicted if managers take sunk costs into account in their decision making.

B. Robustness Tests

I perform a series of robustness checks that I briefly summarize here and detail in Internet Appendix Section IV.A. My results are robust to various sample restrictions, including removing collar deals, restricting to deals paid primarily with stock, and restricting to deals with longer transaction periods. The results are also robust to alternative specifications that control for the length of the transaction period, add year fixed effects, construct the market-induced acquisition cost change without beta adjustment, and use a logit instead of the hazard model. Related to the identification concerns from Section III.C, the results are unchanged when controlling for the acquirer industry's market-to-book ratio at acquisition as a proxy for general industry conditions, and when including as additional time-varying controls the acquirer's leverage, an indicator variable for lowest quintile stock price performance, and measures of financial constraints. Finally, the results are robust to estimating various stratified Cox (1972) models.

Moving beyond the robustness tests in Internet Appendix Section IV.A, Panel A of Internet Appendix Figure IA.4 reestimates the acquisition cost change coefficient, ΔC, when gradually removing observations with the highest probabilities of acquisition withdrawal, for which the concern of differential selection into deal completion or withdrawal is arguably most

[19] The time interaction coefficients are omitted for brevity. Internet Appendix Table IA.X reproduces Table III with the time interaction coefficients displayed.

relevant (see Section III.D). (I defer the discussion of Panel B to Section IV.D.) To estimate deal-by-deal withdrawal probabilities, I augment the final M&A sample detailed in Internet Appendix Table IA.I with a similarly constructed sample of withdrawn acquisitions (applying the SDC "Status: Withdrawn" filter). I then estimate an ordinary least squares (OLS) regression of an indicator variable for the acquisition being withdrawn on an array of deal and firm characteristics, listed in the figure notes, and obtain the estimated withdrawal probability as the predicted value from this regression. The hazard coefficient remains stable as I gradually move the cutoff for remaining included in the estimation from the top (full sample) to the 80th percentile of the deal withdrawal probability distribution, complementing the arguments in Section III.D that endogenous selection into deal failure is unlikely to drive the results.

Furthermore, Internet Appendix Section IV.C presents an alternative two-stage estimation approach in lieu of the one-stage approach in Table III. In this approach, I directly include the endogenous acquisition cost change, that is, the actual cost change the acquirer faces due to its stock price movements, in the estimation. I implement this approach using the residual inclusion (control function) method (see Wooldridge (2015)). In the first stage, I regress the endogenous cost change on the market-induced change as well as control variables. In the second stage, I estimate the hazard model based on the endogenous change and include the residual from the first stage to control for the endogeneity in the system. The two-stage estimation results (Internet Appendix Table IA.VII) corroborate those in the main article. The coefficient on acquisition cost changes remains negative and strongly significant, and the implied economic magnitudes are similar to those in Table III.

Finally, Section V.B of the Internet Appendix shows robustness to an alternative sample that also consists of both divested and nondivested acquisitions but is constructed via standard sampling, that is, without case control matching. Leaving the details of the alternative sample construction to the Internet Appendix, this approach estimates sunk cost distortions of very similar economic magnitudes, although the case control approach helps with the statistical power of the analysis (see the discussion in Section II.D). After the placebo test results for the main sample in the next section, Section IV.D presents further evidence on sunk costs effects that does not use the case-control-matched sample, but instead uses the sample of divested acquisitions only.

C. Placebo Tests

As outlined in Section III.C, one remaining concern for the sunk cost interpretation is that market fluctuations might affect firms' divestiture-related decision making through other channels beyond their effect on sunk costs. To investigate this possibility, I construct hypothetical acquisition cost changes for the deals in my main sample from Table III, using aggregate stock market fluctuations immediately following deal completion (see Bernstein (2015) for a similar approach in an IPO setting). If market movements matter through

Table IV
Placebo Tests

This table reports placebo test results for the main sample involving hypothetical acquisition cost changes calculated from post-completion market fluctuations. The dependent variable is an indicator variable that equals one in the year in which an acquired business is divested and zero otherwise. ΔC^{Hyp} is the hypothetical change in acquisition cost induced by post-completion market fluctuations, as a percentage of the acquirer's pre-acquisition merger capitalization. Panel A uses market fluctuations in the three-month window immediately following deal completion. Panel B uses market fluctuations from varying window lengths, corresponding to the deal-specific length of the period between merger agreement and completion. The order of inclusion of control variables, time interactions, and fixed effects is identical to that in Table III. Please refer to Table III and Section IV.C for additional details. The Appendix provides variable definitions. z-statistics are shown in parentheses. Standard errors are clustered by quarter of the acquisition. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Three-Month Post-Completion Window

	(1)	(2)	(3)	(4)	(5)
ΔC^{Hyp}	0.010	0.011	0.009	0.009	0.011
	(0.33)	(0.38)	(0.30)	(0.30)	(0.40)
Controls	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls	No	Yes	No	Yes	Yes
Time Interactions	No	No	Linear	Linear	Log
Industry FE	Yes	Yes	Yes	Yes	Yes
Acquisition Year FE	Yes	Yes	Yes	Yes	Yes
N	558	558	558	558	558
N (Firm-Years)	4,461	4,461	4,461	4,461	4,461

Panel B: Deal-Specific Post-Completion Window

	(1)	(2)	(3)	(4)	(5)
ΔC^{Hyp}	0.026	0.028	0.026	0.026	0.029
	(0.89)	(0.92)	(0.90)	(0.88)	(0.97)
Controls	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls	No	Yes	No	Yes	Yes
Time Interactions	No	No	Linear	Linear	Log
Industry FE	Yes	Yes	Yes	Yes	Yes
Acquisition Year FE	Yes	Yes	Yes	Yes	Yes
N	558	558	558	558	558
N (Firm-Years)	4,461	4,461	4,461	4,461	4,461

other channels, divestitures should be similarly affected by market movements between merger agreement and completion and those immediately following completion. In constructing the placebo cost changes, I apply the exact same steps and formulas described in Section III.B except that I use post-completion market fluctuations.

Table IV presents the results, with the inclusion of controls, fixed effects, and time interactions being identical to that in Table III. Panel A calculates

Table V
Within-Divestiture Sample

This table reports estimates of the effect of quasi-random variation in acquisition costs on subsequent divestiture rates for the subsample of divested acquisitions. The dependent variable is an indicator variable that equals one in the year in which an acquired business is divested and zero otherwise. ΔC is the change in acquisition cost between merger agreement and completion induced by market fluctuations, as a percentage of the acquirer's pre-acquisition market capitalization (see equation (1′)). The order of inclusion of control variables, time interactions, and fixed effects is identical to that in Table III. Please refer to Table III and Section IV.D for additional details. The Appendix provides variable definitions. z-statistics are shown in parentheses. Standard errors are clustered by quarter of the acquisition. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	(1)	(2)	(3)	(4)	(5)
ΔC	−0.070**	−0.068**	−0.067**	−0.066**	−0.065**
	(−2.39)	(−2.50)	(−2.29)	(−2.39)	(−2.32)
Controls	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls	No	Yes	No	Yes	Yes
Time Interactions	No	No	Linear	Linear	Log
Industry FE	Yes	Yes	Yes	Yes	Yes
Acquisition Year FE	Yes	Yes	Yes	Yes	Yes
N	279	279	279	279	279
N (Firm-Years)	1,581	1,581	1,581	1,581	1,581

hypothetical cost changes using market fluctuations in the three-month window immediately following acquisition completion (the median acquisition in my sample takes three months to complete; see Table I). Across all five columns, the hypothetical cost change coefficients are close to zero and insignificant. They range between 0.009 and 0.011, that is, they also switch sign relative to the coefficients on actual acquisition cost changes in Table III. Panel B instead calculates hypothetical cost changes using market fluctuations from deal-specific window lengths, corresponding to the length of each deal's transaction period (the period between merger agreement and completion). Doing so, I continue to find no evidence that the hypothetical acquisition cost changes significantly predict divestiture rates.

These placebo test results are in line with the hypothesis that market fluctuations affect divestiture rates only through their effect on sunk costs, and thus corroborate the hypothesis that the documented divestment distortions are induced by managerial sunk cost effects.

D. Within-Divestiture Sample

If sunk acquisition costs shift managers' inclination to make a subsequent divestiture, this effect should also generate differential divestiture patterns among the acquisitions that are subsequently divested. Table V revisits the main results presented in Table III while conditioning on divested acquisitions, that is, omitting the case control sampling step from Section II.D. The

structure of the table is identical to that of Table III.[20] Consistent with the reasoning above, the effect of sunk acquisition costs on subsequent divestiture rates is also strongly detectable in the sample of divested acquisitions. All columns again document economically and statistically significant distortions in divestiture rates induced by quasi-random acquisition costs.

The implied economic magnitudes of the within-divestiture sunk cost effects are similar but slightly smaller than those estimated for the main sample comprising divested and nondivested acquisitions. The specifications with the full set of controls and time interactions in columns (4) and (5) imply a reduction in divestiture rates of 8.0% to 8.1% for an interquartile increase in acquisition cost, compared to 9.0% to 9.4% in the corresponding specifications in Table III for the main sample.

I again examine the robustness of the results to successively dropping observations with the highest estimated acquisition withdrawal probabilities, mirroring the robustness check for the main sample discussed in Section IV.B. As shown in Panel B of Internet Appendix Figure IA.4, the hazard coefficient estimates remain stable as I narrow the within-divestiture sample, which further supports the conclusion that differential selection into deal withdrawal is unlikely to drive the results.

Overall, the within-divestiture sample results corroborate the evidence on corporate sunk cost effects from the previous sections.

E. Within-Divestiture Sample Placebo Tests

I also implement placebo tests on the within-divestiture sample. I first replicate the placebo tests from Section IV.C based on post-completion market fluctuations, with the results shown in Internet Appendix Table IA.V in the interest of space. As was the case in Table IV, the hypothetical cost change coefficient is slightly positive and insignificant across all specifications, for both the fixed three-month post-completion window (Panel A) and the deal-specific post-completion windows (Panel B). Thus, consistent with the previous tests, there is no evidence that divestiture rates are predictable by the placebo cost changes.

In a separate and final placebo test, I construct hypothetical acquisition cost changes for the divested *Fixed Dollar* acquisitions from Section II.B. The reasoning behind this placebo test is similar to before. If market movements affect divestitures through other channels, these should also be present in *Fixed Dollar* deals, particularly given their similarity to *Fixed Shares* deals along many observables (see Internet Appendix Table IA.IX and Ahern and Sosyura (2014)). I construct the *Fixed Dollar* placebo cost changes using the market fluctuations between the actual dates of merger agreement and completion,

[20] Table V also adds controls for an acquisition being diversifying in terms of industry or location. The coefficient on quasi-random acquisition costs is very similar with and without these added controls. The two variables are not included in Table III as they are used in the matching procedure to identify the set of divestable acquisitions (Section II.D).

Table VI
Within-Divestiture Sample Placebo Tests

This table reports placebo test results for the within-divestiture sample involving hypothetical acquisition cost changes from market fluctuations between merger agreement and completion for divested *Fixed Dollar* acquisitions. The dependent variable is an indicator variable that equals one in the year in which an acquired business is divested and zero otherwise. ΔC is the actual change in acquisition cost for divested *Fixed Shares* acquisitions between merger agreement and completion induced by market fluctuations, as a percentage of the acquirer's pre-acquisition market capitalization (see equation (1′)). ΔC^{Hyp} is the hypothetical market-induced change for divested *Fixed Dollar* acquisitions. The inclusion of control variables, time interactions, and fixed effects in columns (1) and (3) is identical to that in column (4) of Table V. Columns (2) and (4) correspond to column (5) of Table V. Columns (3) and (4) are estimated on the no-collar subsample. Please refer to Table V and Section IV.E for additional details. The Appendix provides variable definitions. z-statistics are shown in parentheses. Standard errors are clustered by quarter of the acquisition. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	(1)	(2)	(3)	(4)
ΔC × Fixed Shares	−0.057**	−0.055**	−0.082***	−0.083***
	(−2.02)	(−2.00)	(−2.89)	(−2.89)
ΔC^{Hyp} × Fixed Dollar	0.057	0.058	0.039	0.058
	(0.36)	(0.37)	(0.09)	(0.14)
Fixed Dollar	−0.299**	−0.300***	−0.459***	−0.453***
	(−2.53)	(−2.68)	(−2.79)	(−2.79)
Controls	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes
Time Interactions	Linear	Log	Linear	Log
Industry FE	Yes	Yes	Yes	Yes
Acquisition Year FE	Yes	Yes	Yes	Yes
N	370	370	311	311
N (Firm-Years)	2,128	2,128	1,740	1,740

that is, the cost changes that would have ensued had these acquisitions been structured as a *Fixed Shares* deal. I implement this placebo test on the joint sample of all divested acquisitions, that is, the sample of divested *Fixed Shares* acquisitions from Table V augmented by the divested *Fixed Dollar* acquisitions, the latter comprising the placebo group observations.[21] While the placebo test leverages variation in market fluctuations within the subset of *Fixed Dollar* deals, Internet Appendix Figure IA.6 shows that the distribution of acquisitions over time is very similar across *Fixed Shares* and *Fixed Dollar* deals. This implies that across the two deal structures, there are continually deals that experienced similar aggregate market fluctuations, adding to the evidence that deals across the two structures are similar along many observable dimensions.

Table VI presents the results. Columns (1) and (2) correspond to columns (4) and (5) of Table V. The coefficient on the hypothetical acquisition cost

[21] I implement the *Fixed Dollar* based placebo test only in the within-divestiture setting since only a minority of acquisitions are structured as *Fixed Dollar* deals, which makes it exceedingly difficult to find a similar nondivested *Fixed Dollar* acquisition for each divested *Fixed Dollar* deal based on the matching approach in Section II.D.

variation for *Fixed Dollar* deals is insignificant and, as in the first placebo test, the point estimate has the opposite sign compared to the coefficient capturing truly experienced acquisition cost changes. Columns (3) and (4) again restrict the sample to "pure" deals without collar clauses. Similar to *Fixed Shares* deals, *Fixed Dollar* deals can also contain collars, stipulating that the dollar consideration of the merger remains fixed only within a prespecified stock price range. I note that any such collars will again apply to the endogenous acquisition cost change rather than the exogenous market-induced component. Regardless, the "pure" deal results deliver the same conclusions. The point estimate on hypothetical changes for the placebo group deals remains insignificant and of opposite sign. If anything, in the linear time interaction specification (column (3)), it is even closer to zero.[22]

In conclusion, these within-divestiture sample placebo tests further ameliorate concerns regarding other channels through which market fluctuations might affect divestiture decision making.

V. Mechanisms and Implications

A. *Sunk Costs and Financial Constraints*

With regard to mechanisms, I first explore how the documented distortions interact with acquirers' financial constraints. Intuitively, the existence of constraints might help limit distortive firm investment behavior induced by sunk cost effects. In line with this, in the conceptual framework from Section I.A, sunk cost distortions will be reduced if the firm has insufficient funds at time $t = 1$ to bear a given realized negative interim cash flow X, and as a result has to sell the asset.

To measure firms' financial constraints, I use the Whited and Wu (2006, WW) index, and similar to prior work (e.g., Baker, Stein, and Wurgler (2003)), use different ways to classify firms in the sample as constrained versus unconstrained. Specifically, I use both a sample-wide and an annual cutoff to identify constrained firms, and following Fahlenbrach, Rageth, and Stulz (2021), I define firms as constrained if their WW index is in the top quartile of the relevant distribution.

Panel A of Table VII presents how the sunk cost distortions vary in the main sample by whether the acquirer is classified as financially constrained, implementing the specifications with linear and log time interactions from columns (4) and (5) of Table III. Consistent with the reasoning above, the distortions are more pronounced for unconstrained acquirers, both economically and statistically, and irrespective of the sorting scheme (sample-wide in columns (1) and (2), and annual in columns (3) and (4)). Averaging across columns, an interquartile market-induced increase in acquisition costs is estimated to reduce

[22] Table VI also shows that truly experienced acquisition cost changes within the set of *Fixed Shares* deals continue to strongly affect divestiture rates when augmenting the sample with the *Fixed Dollar* deals. Additionally, this joint analysis reveals that *Fixed Dollar* deals are associated with lower divestiture rates on average.

Table VII
Sunk Costs and Financial Constraints

This table reports estimates of the effect of quasi-random variation in acquisition costs on subsequent divestiture rates by whether the acquiring firm is financially constrained or unconstrained. The dependent variable is an indicator variable that equals one in the year in which an acquired business is divested and zero otherwise. ΔC is the change in acquisition cost between merger agreement and completion induced by market fluctuations, as a percentage of the acquirer's pre-acquisition market capitalization (see equation (1′)). Firm-years are classified as constrained or unconstrained based on the Whited and Wu (2006) index and a sample-wide or annual cutoff as indicated in the table. *Constrained* is an indicator variable that equals one for observations in the top quartile of the relevant distribution. *Unconstrained* is the complement of *Constrained*. The inclusion of control variables, time interactions, and fixed effects in Panel A (Panel B) is identical to that in columns (4) or (5) of Table III (Table V). The Appendix provides variable definitions. z-statistics are shown in parentheses. Standard errors are clustered by quarter of the acquisition. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Panel A: Main Sample

	(1)	(2)	(3)	(4)
ΔC × Constrained	−0.047	−0.049	−0.044	−0.046
	(−1.45)	(−1.52)	(−1.27)	(−1.37)
ΔC × Unconstrained	−0.113***	−0.108***	−0.111***	−0.106***
	(−3.42)	(−3.38)	(−3.98)	(−3.94)
Constrained	0.522**	0.531**	0.432*	0.434**
	(2.18)	(2.27)	(1.94)	(1.96)
Constraint Cutoff	Sample-Wide	Sample-Wide	Annual	Annual
Time Interactions	Linear	Log	Linear	Log
N	548	548	548	548
N (Firm-Years)	4,242	4,242	4,242	4,242

Panel B: Within-Divestiture Sample

	(1)	(2)	(3)	(4)
ΔC × Constrained	−0.042	−0.042	−0.043	−0.043
	(−1.07)	(−1.14)	(−1.01)	(−1.07)
ΔC × Unconstrained	−0.083**	−0.077**	−0.078**	−0.073**
	(−2.24)	(−2.16)	(−2.22)	(−2.12)
Constrained	0.618**	0.638**	0.379	0.389
	(2.18)	(2.24)	(1.50)	(1.50)
Constraint Cutoff	Sample-Wide	Sample-Wide	Annual	Annual
Time Interactions	Linear	Log	Linear	Log
N	271	271	271	271
N (Firm-Years)	1,513	1,513	1,513	1,513

divestiture rates by 13% for unconstrained firms, compared to 6% for constrained firms. Panel B repeats the exercise for the within-divestiture sample and finds very similar results. Here, the estimated divestment distortions are approximately twice as large when acquirers are unconstrained as well.

Overall, the evidence in Table VII adds to the broader theme in the literature that constraints on managerial actions, such as financial, legal, and organizational constraints, can curb distortive decision making by managers (see Banerjee, Humphery-Jenner, and Nanda (2015), Guenzel and Malmendier (2020), and Camerer and Malmendier (2007)).

B. Firm versus CEO-Specific Effect

Another natural question is whether the documented divestment distortions can be linked to specific decision-makers inside the firm. In M&A, the obvious decision-maker to focus on is the acquirer's CEO. Survey evidence by Graham, Harvey, and Puri (2015) finds that CEOs consider themselves as the dominant decision-maker in M&A decisions, and as making them "in relative isolation."

To explore this question, I collect information on CEO changes over time for all *Fixed Shares* acquisitions in my sample that are subsequently divested. Specifically, I collect information on who the CEO was at the acquirer's firm at the time of the acquisition, and when this CEO stepped down. I am able to retrieve these data from Execucomp for about 50% of firms in my sample. For the remaining firms, I hand-collect the data from SEC filings and newspaper articles. For 43% of firms, the CEO making the acquisition and divestiture decision is the same. For the remaining 57% of firms, there is a CEO change during this period. I then analyze whether the association between quasi-random acquisition cost and divestiture rates weakens after a CEO change at the acquiring firm, that is, after the manager who personally experienced the acquisition cost change while at the helm leaves the CEO position. In rare cases, the attribution of experienced cost changes to a specific CEO is ambiguous in my sample. I remove these observations from the analysis below to provide for a cleaner test. My results are nearly identical when keeping all observations in the sample.[23]

This test differs from research that examines CEO "styles" (e.g., Bertrand and Schoar (2003), Dittmar and Duchin (2015), Schoar and Zuo (2017)) and from the analysis in Weisbach (1995). My focus is not on whether, on average, firms' divestment policies change after (possibly exogenous) CEO changes. Instead, the test separates the effect of quasi-random acquisition costs on divestiture rates based on whether the decision-maker at the helm personally experienced this change or not.

Table VIII presents the results. Columns (1) and (2) include controls, fixed effects, and time interactions as in column (4) of Table V. Column (3) includes log-time interactions. First, in column (1), I reestablish the main effect of quasi-random acquisition costs on divestiture rates (documented for the divested acquisitions in Table V) after disregarding 13 ambiguous CEO transitions as

[23] Occasionally, the CEO changes between acquisition agreement and completion, or the target CEO becomes the CEO of the combined firm. I disregard these observations, as well as a few observations in which the acquirer's CEO remains affiliated with the divested business after the divestiture, as in these cases incentives and the "psychological affiliation" around the divestiture decision might be unclear.

Table VIII
Firm versus CEO-Specific Effect

This table reports results of the test for a firm-level versus CEO-level channel for the association between quasi-random variation in acquisition costs and subsequent divestiture rates. The dependent variable is an indicator variable that equals one in the year in which an acquired business is divested and zero otherwise. ΔC is the change in acquisition cost between merger agreement and completion induced by market fluctuations, as a percentage of the acquirer's pre-acquisition market capitalization (see equation (1′)). *Acquiring CEO* is an indicator that equals one in firm-years in which the CEO who made the acquisition is still in office. *New CEO* is the complement of *Acquiring CEO*. The inclusion of control variables, time interactions, and fixed effects in columns (1) and (2) is identical to that in column (4) of Table V. Column (3) corresponds to column (5) of Table V. Please refer to Table V and Section V.B for additional details. The Appendix provides variable definitions. z-statistics are shown in parentheses. Standard errors are clustered by quarter of the acquisition. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	(1)	(2)	(3)
ΔC	−0.073**		
	(−2.49)		
ΔC × Acquiring CEO		−0.105**	−0.102**
		(−2.51)	(−2.33)
ΔC × New CEO		−0.060*	−0.056
		(−1.65)	(−1.55)
New CEO		0.575***	0.589***
		(4.41)	(4.65)
Controls	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes
Time Interactions	Linear	Linear	Log
Industry FE	Yes	Yes	Yes
Acquisition Year FE	Yes	Yes	Yes
N	266	266	266
N (Firm-Years)	1,555	1,555	1,555

discussed above. With this modification, the coefficient of interest remains unchanged. Next, in columns (2) and (3), I separate the main effect based on whether the CEO responsible for the acquisition is still at the helm (*Acquiring CEO*) or not (*New CEO*). Consistent with the predictions of an intrapersonal sunk cost channel, the acquisition cost effect is driven by the *Acquiring CEO* regime. For example, column (2) implies that before (after) a CEO transition, an interquartile increase in market-induced acquisition cost relative to the acquirer's size is associated with a 13% (7%) reduction in divestiture rates. Further, the acquisition cost coefficient pertaining to the *Acquiring CEO* regime is strongly significant, while that pertaining to the *New CEO* regime is barely significant in column (2) and insignificant in column (3).

In sum, this analysis corroborates the existence of a CEO-specific sunk cost channel. This finding is also in line with a recent active literature documenting how managers' personal experiences, including those in the professional domain, affect their decision making (e.g., Malmendier, Tate, and Yan (2011), Dittmar and Duchin (2015), Bernile, Bhagwat, and Rau (2017), Schoar and Zuo (2017)). In addition, the CEO-specific results are not easily predicted by

potential alternative channels for the relation between quasi-random acquisition costs and divestitures based on firm or market characteristics.

C. CEO Mechanisms

This section delves further into mechanisms to tease out why firms and CEOs take sunk costs into account in their decision making.

CEO Information, Beliefs, and Learning. Some existing work (e.g., Camerer and Weber (1999), McAfee, Mialon, and Mialon (2010)) argues that sunk costs could in fact be relevant for optimal decision making, if they reflect underlying CEO information, beliefs, or updating. Investment levels, and thus the amount of sunk costs, are generally correlated with (objective or subjective) information or beliefs, which could explain an association between sunk costs and subsequent decisions. My setting directly addresses this point by focusing on cost variation *after* the parties sign a binding merger agreement, thus overcoming the problem of selection effects inherent in purchase decisions. Alternatively, sunk costs may be related to optimal decision making if higher expenditures lead to a higher probability of project success. While such mechanisms are plausibly important in contexts in which managers "learn by doing," firms in my context do not learn from the acquisition cost shocks as they are triggered by plausibly exogenous market fluctuations. This conclusion is reinforced by the placebo tests based on hypothetical market-induced cost changes.

Investment Budgets. Additionally, firms may find it optimal to stick with a given investment rather than change the course of action if they have a fixed investment budget. The acquisition cost shocks in my setting do not, however, imply any mechanical differences in operational characteristics of acquirers including in cash holdings. This, in combination with the placebo tests and the CEO-specific distortions, is not easy to reconcile with investment budget considerations.

CEO Entrenchment. Another mechanism one may propose is that entrenched incumbent managers become resistant to making divestitures and instead seek to engage in empire-building. While such a channel could help explain general differences in divestiture rates between incumbent and newly appointed CEOs, it does not predict the key finding of differential divestiture rates triggered by differential exposure to plausibly exogenous sunk acquisition cost changes.

Behavioral Frictions. Given the evidence and discussion thus far, I argue that the residual relation between market-induced acquisition cost changes and divestiture rates is most consistent with sunk costs generating behavioral frictions that affect managerial decision making.

Here it is important to note that, in general, there are a number of plausible, nonmutually exclusive channels for managerial sunk cost effects. On the one hand, sunk cost effects can arise if CEOs themselves use mental processes that lead to sunk cost thinking (e.g., exhibit prospect theory preferences or cognitive dissonance; see Section I.B). On the other hand, sunk cost effects can arise from interactions between managers and biased third parties such

Table IX
CEO Mechanisms

This table reports estimates of the effect of quasi-random variation in acquisition costs on subsequent divestiture rates by various characteristics of the acquiring CEO. The dependent variable is an indicator variable that equals one in the year in which an acquired business is divested and zero otherwise. ΔC is the change in acquisition cost between merger agreement and completion induced by market fluctuations, as a percentage of the acquirer's pre-acquisition market capitalization (see equation (1′)). Columns (1) and (2) interact the effect of quasi-random variation in acquisition costs on subsequent divestiture rates during the acquiring CEO's tenure with proxies for managerial career concerns. *High Career Incentives* is an indicator variable for acquiring CEOs in the bottom quartile based on age at acquisition (column (1)), or based on the median age between age at acquisition and CEO departure or divestiture (column (2)). *Low Career Incentives* is the complement of *High Career Incentives*. Columns (3) and (4) interact the effect of quasi-random variation in acquisition costs on subsequent divestiture rates during the acquiring CEO's tenure based on firms' market capitalization at acquisition. Please refer to Section V.C for additional details. The inclusion of control variables, time interactions, and fixed effects in all columns is identical to that in column (4) of Table V. The Appendix provides variable definitions. *z*-statistics are shown in parentheses. Standard errors are clustered by quarter of the acquisition. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	(1)	(2)	(3)	(4)
ΔC × Acquiring CEO				
× High Career Incentives	−0.060	−0.077		
	(−0.87)	(−1.07)		
× Low Career Incentives	−0.117**	−0.113**		
	(−2.19)	(−2.11)		
× Market Cap ≤ *P*50			−0.111**	
			(−2.33)	
× Market Cap > *P*50			−0.081	
			(−1.35)	
× Market Cap ≤ *P*25				−0.137
				(−1.55)
× *P*25 < Market Cap ≤ *P*75				−0.099**
				(−2.21)
× Market Cap > *P*75				−0.067
				(−0.81)
Controls	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes
Time Interactions	Linear	Linear	Linear	Linear
Industry FE	Yes	Yes	Yes	Yes
Acquisition Year FE	Yes	Yes	Yes	Yes
N	265	265	266	266
N (Firm-Years)	1,549	1,549	1,555	1,555

as board members. If the parties evaluating managers mistakenly take sunk costs into account in their assessment of managers, CEOs with career concerns might respond by taking sunk costs into account as well.

While, as stated, these different psychological sunk cost channels are not mutually exclusive, and their relative importance might vary across settings, it is interesting to further explore them in the present M&A setting. Table IX

presents the corresponding results, building on the findings in Table VIII that the divestment distortions are generally driven by the acquiring CEOs, and separating the acquiring-CEO effect based on various CEO subgroups. I first explore whether the effect varies with acquiring CEOs' age. All else equal, CEOs' career-related incentives to respond to biases of others should be more pronounced in young CEOs who are at the beginning of their C-suite career (Fama (1980), Holmström (1982), Gibbons and Murphy (1992)). In column (1) of Table IX, I define CEOs in the youngest age quartile based on CEOs' age at acquisition as those with high career incentives, and the remaining CEOs as those with, relatively speaking, low career incentives. In column (2), I repeat the split based on acquiring CEOs' median age between age at acquisition and CEO departure or divestiture. In both columns, the estimated sunk cost distortions are not amplified in young CEOs, which does not lend support to managerial career concerns being the primary driver of the investment distortions in my data.

If the documented sunk cost effects and resulting divestment distortions instead arise primarily due to CEO biases, it is plausible that the magnitude of the distortions varies with the degree of CEO sophistication (Krüger, Landier, and Thesmar (2015), Dessaint and Matray (2017)). The equilibrium model of Gabaix and Landier (2008) in the cross section predicts that better CEOs manage larger firms. Motivated by this prediction, in the remaining columns of Table IX I split acquiring CEOs based on their firm's pre-acquisition market capitalization. To account for the fact that the Gabaix and Landier (2008) matching model is concerned with cross-sectional differences in firm size (and CEO talent), I sort firms by size conditional on same acquisition year, thus identifying small versus large acquirers in my sample in the cross section in a given year. I perform simple sorts of firms into smaller versus larger firms splitting at the median market capitalization, or I further sort firms into bottom size-quartile firms, medium-sized firms, and top size-quartile firms.

Before turning to the results, I validate the sorting approach by examining correlations with other plausible proxies for managerial sophistication. To this end, I collect information on acquiring CEOs' pay at the time of the acquisition (from Execucomp) and educational background (from BoardEx, Bloomberg, Capital IQ, and Who's Who in Finance and Industry, among other sources). Reassuringly, there is a strong correlation between the size-based and other sophistication proxies. For example, the correlation between the acquiring CEO running a larger firm (based on the sorting at the median) and log total pay is 0.27 (p-value < 0.005), and the correlation between larger-firm acquiring CEO and the CEO having attended a top-ranked university is 0.258 (p-value < 0.001).[24]

[24] I use the historical *U.S. News & World Report* university rankings, published (bi)annually since 1983. I use rankings from the 1980s and 1990s to ensure rankings accurately reflect educational conditions when the sample CEOs attended university. I define a top university as a top-five institution in at least one of the included rankings. The results are not sensitive to this definition as there is a strong congruence in rankings over time and across ranking publishers.

The results in columns (3) and (4) of Table IX are consistent with sunk cost effects being amplified in firms with less sophisticated CEOs and dampened in firms with more sophisticated CEOs. While some caution with respect to statistical power is due given that these subgroup effects are estimated on relatively small subsamples of CEOs, both columns estimate a consistent and monotonically decreasing relation between investment distortions and firm size as the proxy for CEO sophistication. In column (3), the estimates imply a decrease in divestiture rates, as a result of an interquartile sunk cost increase, of 9.8% for more sophisticated CEOs of larger acquirers, compared to 13.2% for less sophisticated CEOs of smaller acquirers. Column (4) implies a decrease of 8.2% for the most sophisticated CEOs of top-size quartile acquirers, increasing to 16.1% for the least sophisticated CEOs of bottom-size quartile acquirers, with the effect for CEOs of medium-sized acquirers falling in between at 11.9%.

Taken together, the results suggest that a mechanism related to managerial biases, pronounced in firms that are likely managed by less sophisticated CEOs, is an important underlying channel for the documented sunk cost effects in my setting.

D. Efficiency Costs

Discussion of Efficiency Costs. The conceptual framework of Section I.A (as well as the framework embedded in prospect theory in the Internet Appendix) formalizes that sunk cost managers hold on to costly acquisitions beyond the point at which the NPV turns negative. The fact that sunk cost–induced distortions entail deviations from the NPV-optimal decision rule clarifies the general efficiency cost implications of sunk cost effects.

Directly estimating the NPV paths of acquired businesses over time is difficult due to several data constraints. I lack detailed data for acquired segments on cash flows over time and expected future cash flows, and for about one-third of divestitures I lack data on the divestiture transaction price. Consequently, I cannot conclusively quantify the efficiency costs of sunk cost decision making in my setting. That said, additional aspects and tests suggest potentially significant real costs due to sunk cost distortions. First, Table X shows that the sunk cost–induced distortions are pronounced in diversifying acquisitions. Diversifying deals are often regarded as a proxy for inferior deal quality (e.g., Malmendier and Tate (2008); see also Seru (2014), who finds that diversifying mergers, but not same-industry mergers, reduce R&D productivity).

Additionally, in recent related work, Cronqvist and Pély (2024) shed light on the efficiency costs of divested acquisitions, extending the evidence in Kaplan and Weisbach (1992). They conclude that "up to 77% of [divestitures] could be seen as 'corrections of failure'," sold for a lower price (deflated by the S&P500) than the pre-M&A target equity value. Similarly, the majority of divestitures in my sample with divestiture price data occur at a lower price relative to the initial acquisition price, and many of them at a "discount" of 20% and more. It seems natural that accelerating "corrections of failure" would limit

Table X
Diversifying versus Same-Industry Acquisitions

This table reports estimates of the effect of quasi-random variation in acquisition costs on subsequent divestiture rates by whether the acquisition is diversifying or a same-industry deal. The dependent variable is an indicator variable that equals one in the year in which an acquired business is divested and zero otherwise. ΔC is the change in acquisition cost between merger agreement and completion induced by market fluctuations, as a percentage of the acquirer's pre-acquisition market capitalization (see equation (1′)). The order of inclusion of control variables, time interactions, and fixed effects is identical to that in Table III. Please refer to Table III and Section V.D for additional details. The Appendix provides variable definitions. z-statistics are shown in parentheses. Standard errors are clustered by quarter of the acquisition. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	(1)	(2)	(3)	(4)	(5)
ΔC × Diversifying Acq.	−0.068***	−0.070***	−0.078***	−0.079***	−0.076***
	(−2.89)	(−2.96)	(−3.12)	(−3.20)	(−3.14)
ΔC × Same-Industry Acq.	−0.011	−0.019	−0.021	−0.029	−0.035
	(−0.05)	(−0.11)	(−0.10)	(−0.17)	(−0.21)
Controls	Yes	Yes	Yes	Yes	Yes
Time-Varying Controls	No	Yes	No	Yes	Yes
Time Interactions	No	No	Linear	Linear	Log
Industry FE	Yes	Yes	Yes	Yes	Yes
Acquisition Year FE	Yes	Yes	Yes	Yes	Yes
N	558	558	558	558	558
N (Firm-Years)	4,461	4,461	4,461	4,461	4,461

the economic costs of divestitures, at least on average. This, in turn, suggests important real costs for firms that hold on to costly acquisitions due to sunk cost effects.

Finally, I construct for each divested acquisition that became exogenously more costly ($\Delta C > 0$) a counterfactual divestiture announcement date had the acquirer faced no acquisition cost shock ($\Delta C^{CF} = 0$). I then examine firms' stock market performance between these two dates.[25] To estimate counterfactual dates, I use the hazard model from column (1) of Table V and estimate the expected time until divestiture under $\Delta C > 0$ and $\Delta C^{CF} = 0$, holding fixed the deal's other characteristics. The counterfactual divestiture announcement date is calculated by subtracting the difference in the two expected survival times from the true divestiture announcement date. The average and median lengths of time between the counterfactual and actual divestiture announcement dates, referred to as the *sunk cost period*, are 87 and 38 days, respectively (see Internet Appendix Figure IA.7). Panel A of Figure 4 shows economically meaningful negative industry-adjusted performance for the average firm during the sunk cost period. The average buy-and-hold abnormal return is −3.9%.

There are obvious and valid caveats to interpreting this finding as conclusive evidence that firms delay divestiture of businesses with a negative NPV

[25] Of the 279 divested *Fixed Shares* acquisitions, 162 faced a market-induced increase in acquisition cost, and complete daily stock return data are available for 153 deals. Counterfactual results are based on these observations.

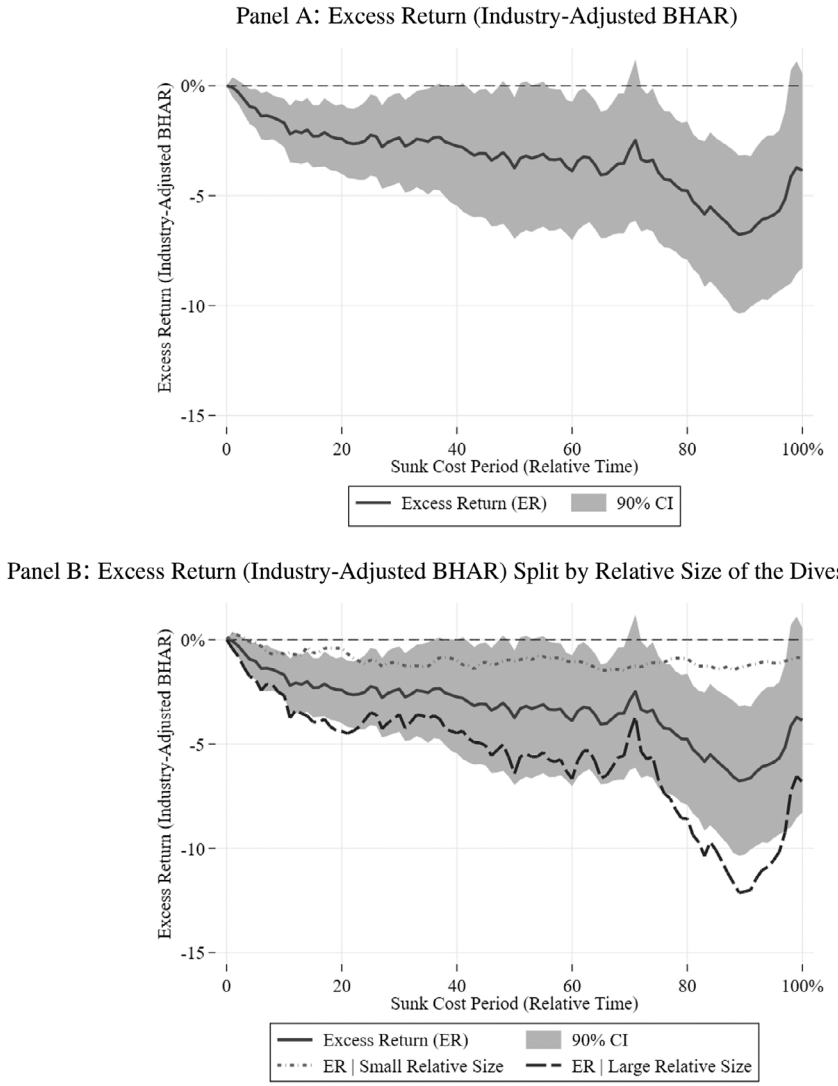

Figure 4. Efficiency costs. This figure shows plots of average excess returns (industry-adjusted buy-and-hold returns) between an estimated and the actual divestiture announcement date (the sunk cost period). Panel A plots the average excess return across all acquirers that faced a positive acquisition cost shock ($\Delta C > 0$). Panel B adds a split based on below-median (light blue line) and above-median (dark blue line) relative size of the acquired business. *Relative Size* is the transaction price of the original acquisition divided by the value of the combined firm. The estimated divestiture announcement date is calculated assuming a scenario in which the acquirer faced no cost shock, holding fixed all other characteristics. The figures normalize the sunk cost period to one and plot relative time (between 0% and 100%) passed between the estimated and actual divestiture announcement dates. Please refer to Section V.D for additional details.

and that this results in a decline in overall firm value—in particular, reverse causality concerns and the fact that a divestiture is a negotiated outcome that, unlike assumed here, cannot be unilaterally advanced. To partially address the first issue, Panel B of Figure 4 shows that the performance deterioration is driven by observations for which the to-be-divested business constitutes a significant part of the entire firm, whereas there is little underperformance of firms that divest relatively small segments. Overall, this simplified counterfactual analysis should be interpreted as providing suggestive evidence of efficiency costs, at least on average, associated with sunk cost–induced delays in divestiture negotiations and decisions.

Further Discussion. If the documented sunk cost effects are indeed due to firm and managerial mistakes (Section V.C), we would also expect there to be heterogeneity in stock market reactions around divestitures depending on the severity of the mistake.[26] In line with this conjecture, Internet Appendix Figure IA.8 shows that the counterfactual analysis (keeping in mind its limitations discussed above) reveals particularly negative industry-adjusted performance between the counterfactual and actual divestiture announcement dates for acquirers who faced large acquisition cost increases, that is, for which sunk cost thinking presumably leads to the biggest mistakes on average. Furthermore, the market appears to react more positively to the eventual announcement of divestitures for this subset of large-cost-increase acquirers, consistent with these firms correcting larger mistakes (Internet Appendix Figure IA.9).

E. Discussion of Alternative Explanations

This section recaps and extends the discussion of potential confounds and caveats in attributing the link between market-induced acquisition cost changes and divestiture rates to sunk cost effects.

Other Effects of Market Fluctuations. As detailed in Section III.C, one concern is that aggregate market movements between merger agreement and completion might affect divestiture decision making not solely by affecting sunk acquisition costs. Three findings help alleviate this concern. First, acquisition cost changes do not mechanically lead to operational and cash holdings differences between stock acquirers. Second, the placebo tests do not find any evidence that market fluctuations affect divestiture rates unless they affect actual acquisition costs. Third, alternative explanations based on market fluctuations affecting firm or industry conditions are not easily reconcilable with the results in Section V.B that the divestment distortions appear to be driven by the acquiring CEO.

Other Effects of Actual Acquisition Price Changes. A related key concern from Section III.C is that differences in the actual acquisition prices, even if induced by market movements, have other effects on acquirers and acquired segments

[26] Previous work that has examined stock market reactions to inform the analysis of managerial behavior in the context of M&A includes, for example, Malmendier and Tate (2008) and Krüger, Landier, and Thesmar (2015).

that in turn matter at the time of the divestiture decision. This might, for example, happen by acquisition prices affecting acquirer constraints, investment, and profitability (see the evidence on procyclical variation in startup growth in Sedláček and Sterk (2017)). It is important to note that the placebo tests cannot ameliorate this concern, since by design they do not involve actual acquisition cost changes. The inclusion of acquisition year fixed effects helps account for business cycle effects. It is also reassuring that the results remain unchanged when including various (time-varying) controls as proxies for acquirer trajectories, such as industry market-to-book ratio, acquirer leverage, performance, and measures of financial constraints (Internet Appendix Table IA.IV). The various tests on mechanisms further help with the concern of other effects of actual acquisition price changes. For one, it is again not obvious how such potential other effects would generate a CEO-specific effect. Additionally, if the results were driven by higher acquisition prices enhancing the productivity or profitability of acquired segments, this would not easily predict the distortions to be amplified in diversifying acquisitions (Table X), which Seru (2014) finds to substantially reduce the R&D productivity of acquired targets.

Hedging of Market Exposure and Acquisition Withdrawals. Another potential issue is that acquirers can use collars and other strategies to hedge exposure to post-agreement market fluctuations, dampening the link between market movements and acquisition cost changes. Collars are used in a minority of stock deals only (about 10% in my sample; see also Officer (2006)). My findings hold for the noncollar sample and the control function approach that uses the endogenous acquisition cost changes accounting for collars (Internet Appendix Section IV.C). Beyond collars, acquirers may also attempt to influence their stock price directly, for example, through media management. While prior work documents strategic media activity around mergers (Ahern and Sosyura (2014)), Table II confirms that above and beyond any such strategies, and after also accounting for collar bounds, market fluctuations do have a strong effect on acquirer stock price movements.

A related concern is that acquirers could be differentially inclined to withdraw from a signed acquisition agreement after post-agreement market increases depending on the perceived or true synergy potential. However, selection into deal completion or cancellation is unlikely to drive the results for several reasons. Withdrawals are often caused by exogenous factors such as regulatory disapproval, withdrawals after market increases are highly infrequent, and the estimated sunk cost effect on divestiture rates remains stable as I gradually eliminate observations with the highest estimated acquisition withdrawal probabilities (Internet Appendix Figures IA.4 and IA.5).

Salient Acquisition Cost Changes. One necessary ingredient for the possibility of sunk cost effects in my setting is that *Fixed Shares* acquirers are attentive to changes in post-agreement acquisition costs. In favor of this assumption, both the endogenous and market-induced changes in acquisition costs are economically meaningful, even after accounting for bounds in cost changes through collars (see Table I). Stock price–induced changes in deal

value are also frequently discussed by the business media (see footnote). Additional supporting evidence comes from actual merger contracts. Typically, "potential changes in stock price" is listed as a main risk factor in the official merger agreement (see, for example, Example 1 in Section III.B of the Internet Appendix). Merger specifics including exchange terms and associated risks are also frequently a central topic of discussion in managements' conference calls with analysts (see, for example, Example 4 in Section III.B of the Internet Appendix).

Divestiture Timing. It could also be the case that firms attempt to time divestitures or seek additional buyers before committing to a sale. While such considerations may plausibly play into firms' divestiture decision making and divestiture negotiations, it is not obvious why such timing motives would be correlated—other than through sunk cost effects—with differential exposure to post-agreement aggregate market movements in the initial acquisition, particularly given the inclusion of acquisition year fixed effects in all analyses and given that the results remain unchanged when including year fixed effects (Internet Appendix Table IA.IV).

Final Acquisition Cost as Benchmark. A possibility related to divestiture timing and negotiations is that the final acquisition cost might serve as a benchmark divestiture price for to-be-divested businesses. This could affect firms' ability to reach a divestiture agreement with a buyer in a way that is correlated with changes in sunk acquisition costs. Such benchmarking in negotiations would predict bunching of realized divestiture prices at the final price of the initial acquisition. As Figure IA.10 of the Internet Appendix shows, however, there is no evidence of systematic and marked bunching in my sample, and most divestitures happen at much different prices.

F. Implications and Generalizability

While the main goal of this paper is to devise a test for sunk cost effects in the well-identified setting of M&A and divestitures, the sunk cost mechanisms documented here are plausibly at play in many other firm investment contexts.

Anecdotal evidence can be one indicator of the broader applicability of the findings of this paper. Within the M&A context, practitioners warn that sunk cost thinking can also play an important distortive role in the initial decision of whether to pursue an acquisition bid.[27] Beyond M&A, Guler (2007) provides various insightful examples of sunk cost thinking in the venture capital (VC) industry, by way of semistructured interviews with VC managers. For example, one interviewee stated that "the more money that goes in, we are less likely to walk away," consistent with the sunk cost mechanism and findings proposed in

[27] See, for example, the following advice from https://www.successfulacquisitions.com/mergers-and-acquisitions/are-sunk-costs-driving-your-acquisition: "You've spent a lot of time, energy and money… it can be tempting to cite the amount of resources that have already been invested as reasons to follow through with the deal, but this would be a mistake."

this paper.[28] Yet another type of firm investment where sunk cost effects might be particularly prevalent is R&D, where investments are often large and unrecoverable (Stiglitz (1987)). A specific well-known example of sunk cost effects in this context is the Concorde aircraft project, which is why the tendency of basing decisions on sunk costs is also dubbed the *Concorde effect*.[29]

Of course, the above discussion on how sunk costs could permeate and distort different types of firm investment is more speculative. In Section VII of the Internet Appendix, I provide further suggestive evidence consistent with a sunk cost mechanism in the contexts of VC and R&D investment. Correlationally, VC firms with prior investments in a given startup have a high commitment to the startup in subsequent funding rounds, as indicated by the fraction of repeat versus new VC investors, particularly with respect to unsuccessful startups that ultimately fail. These patterns are suggestive of excessive commitment of VC firms to startups once an initial (sunk) investment is made. With respect to R&D investment, I provide evidence that stylized R&D patterns in the cross section are consistent with the seminal experimental evidence from the lab by Staw (1976), in which subjects (over)invested in underperforming R&D projects they had previously committed resources to. Additionally, Guenzel and Liu (2024) find evidence consistent with sunk cost effects in R&D in the context of drug development, where biotechnology firms are significantly less likely to abandon drug development projects after experiencing unexpected delays in clinical trials. This finding highlights the broad interpretation of sunk costs as sunk resources, encompassing money, effort, and time.

A challenge for future research is to cleanly identify sunk cost effects in other settings. The empirical design choices in this paper may be a useful guide for future work. First, my approach underscores the importance of the specific decision-maker within the firm responsible for a prior investment. In addition, it isolates exogenous variation in the intensity of prior sunk investments (as opposed to a binary measure comparing firms with and without prior investments) and analyzes the intensity of commitment by employing duration analysis (as opposed to a binary outcome). One promising avenue could be to try to isolate exogenous variation within such intensity-based approaches to test for sunk cost effects in other investment contexts. More broadly, exploiting heterogeneity in the intensity of managerial experiences or characteristics could also enrich the study of other managerial biases beyond sunk cost effects, and allow for more refined and nuanced predictions in other settings.

[28] Along the same lines, another VC manager interviewed by Guler (2007) responded that "We see [follow-on investments] primarily as a way to protect our initial investment."

[29] To justify their initial investments, the French and British governments continued to spend billions of dollars on the development of the Concorde aircraft, even after it was clear that it would not be economically viable (see, for example, https://forbes.com/sites/jimblasingame/2011/09/15/beware-of-the-concorde-fallacy and Internet Appendix Figure IA.1, Panel B).

VI. Conclusion

This paper shows that quasi-random changes in acquisition costs significantly predict subsequent divestiture rates of acquired businesses. These cost changes are induced by aggregate market fluctuations in fixed exchange ratio stock mergers and, importantly, occur after parties reach a binding merger agreement. As an acquisition becomes exogenously more expensive, firms' propensity to divest decreases substantially. Placebo cost changes using market fluctuations that did not shift actual acquisition costs do not predict divestitures. These findings are difficult to reconcile with optimal firm decision making and are instead most consistent with managerial sunk cost effects.

Various mechanism tests strengthen this sunk cost interpretation. The distortions are more pronounced in financially unconstrained firms, consistent with constraints curbing distortive investment behavior induced by sunk cost effects. Additionally, the distortions appear to be driven by the CEO who made the initial acquisition, pointing to a CEO-specific sunk cost channel, and are amplified in firms that are likely run by less sophisticated CEOs. CEO-specific effects elevate the bar for alternative explanations related to industry or firm characteristics.

These results point to several avenues for future work, especially considering that sunk costs are prevalent in nearly all investment decisions across organizational hierarchy levels. In the M&A-divestiture context, future work should aim to obtain detailed segment data on cash flows and post-acquisition investment. Such granular data would make it possible to improve on the efficiency cost tests of Section V.D, and to fully map out the NPV costs of sunk costs in this setting.

Another promising direction is to quantify the micro and macro impacts of sunk cost effects in other important contexts, such as financial intermediation or R&D investment and innovation (see Guenzel and Liu (2024) and the discussion in Section V.F). As discussed, the obvious empirical complication is to separate sunk cost effects from self-selection effects inherent in investment decisions. In this respect, the empirical methodology of this paper—exploiting variation in the intensity of sunk investments—can serve as a useful guide for finding new identification opportunities in other settings.

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Appendix: Variable Definitions

Variable	Definition
	Panel A: Acquisition-Related Variables
%stock	Fraction of the acquisition paid with stock

(*continue*)

Variable	Definition
Acquirer Size	Acquirer's market capitalization 21 trading days prior the acquisition announcement
All-Stock Deal	Indicator variable that equals one if the acquisition was paid with 100% stock
CAR	Three-day cumulative announcement return around the merger announcement date; following Krüger, Landier, and Thesmar (2015), the calculation uses the CRSP value-weighted return (including distributions) as the benchmark return in the calculation (mean-market model)
CAR < 0	Indicator variable that equals one if *CAR* is negative and zero otherwise
Deal Value	Price of the acquisition at merger agreement
ΔC	Change in acquisition cost during the transaction period induced by aggregate stock market fluctuations; see equation (1′) for details
ΔC^{Acq}	Change in acquisition cost during the transaction period induced by changes in the acquirer's stock price; see equation (1) for details
ΔC^{Hyp}	Hypothetical change in acquisition cost, when using post-completion market fluctuations, or for *Fixed Dollar* acquisitions
ΔR	Cumulative market return net of the expected market return during the transaction period; see equation (2′) for details
ΔR^{Acq}	Cumulative return to the acquirer during the transaction period; see equation (2) for details
Diversifying Deal	Indicator variable that equals one if the acquirer and target operated in different industries (based on Fama and French (1997) 49 industries) and zero otherwise
Fixed Dollar	Indicator variable that equals one if an acquisition is structured using a floating exchange ratio and zero otherwise
Fixed Shares	Indicator variable that equals one if an acquisition is structured using a fixed exchange ratio and zero otherwise
Geo-Diversifying Deal	Indicator variables that equals one if the acquirer's and target's headquarters are located in different states and zero otherwise
Public Target	Indicator variable that equals one if the target is a publicly listed firm and zero otherwise
Same-Industry Deal	Complement of *Diversifying Deal*
Transaction Period	Period between two days after the date of the merger agreement and the merger completion date (day of and first day after merger agreement excluded since used in the construction of the three-day abnormal announcement return)
Panel B: Firm-Related, CEO-Related, and Time-Varying Variables	
12-Month Return	Acquirer's previous-year stock return, calculated from monthly stock data
Acquiring CEO	Indicator variable that equals one in firm-years in which the CEO who made the acquisition is still in office and zero otherwise
Beta	Acquirer industry's sensitivity with the market estimated using 60-month rolling regressions as in Krüger, Landier, and Thesmar (2015) (based on Fama and French (1997) 49 industries and nonwinsorized)
Constrained	Indicator variable that equals one in firm-years in the top quartile of the WW-index distribution (sample-wide or annual cutoff) and zero otherwise

(*continue*)

Variable	Definition
High Career Incentives	Indicator variable that equals one for CEOs in the bottom age quartile (at acquisition or median age between acquisition and CEO departure/divestiture)
Industry Distress	Indicator variable that equals one in years in which the industry of the acquired business is in financial distress (median firm's forward-looking two-year stock return below 30% (Opler and Titman (1994), Babina (2020)) or backward-looking industry performance across all industries in the bottom quintile (Dinc, Erel, and Liao (2017))) and zero otherwise
Industry Market-to-Book	Acquirer industry's median market-to-book ratio (based on Fama and French (1997) 49 industries)
Leverage	Acquirer's long-term debt over assets
Low Career Incentives	Complement of *High Career Incentives*
Market Cap	See *Acquirer Size*
New CEO	Complement of *Acquiring CEO*
Stock Return Volatility	Volatility in the acquirer's daily stock return 80- to 200-trading-day window prior to acquisition announcement date
Unconstrained	Complement of *Constrained*
WW-Index	Whited and Wu (WW) index constructed as in Whited and Wu (2006)
	Panel C: Divestiture-Related Variables
Divestiture Price	Price at which acquired business is divested
Excess Return	Industry-adjusted (based on Fama and French (1997) 49 industries) buy-and-hold return during the sunk cost period
Relative Divestiture Price	*Divestiture Price* divided by the price of the original acquisition at merger agreement
Relative Size	Price of the original acquisition at merger agreement divided by the value of the combined firm, that is, the acquirer's market capitalization 21 trading days prior to the acquisition announcement plus the value of the acquired business as measured by the price at merger agreement
Sunk Cost Period	Period between counterfactual and actual divestiture announcement date; see Section V.D for details on the construction of counterfactual dates

Unless otherwise noted, non-logged continuous variables included in regressions are winsorized at the 1st and 99th percentiles, or for ΔR and ΔR^{Acq}, when they deviate from the median by more than ten times the interquartile range.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.
Replication Code.