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 8-9%. Consistent with an intrapersonal sunk cost channel, distortions are concentrated in firm-years in which the acquiring CEO is still in office.

Keywords: Firm Investment, Mergers and Acquisitions, Sunk Costs, Divestment, CEOs, Managerial Biases

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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 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 to nothing 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 even leading traditional Corporate Finance textbooks concur 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 the reader 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. I focus on one high-stakes type of firm investment: mergers and acquisitions (M&A). Specifically, 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 post-agreement cost variation, 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 the 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

[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).

between merger agreement and completion directly translate 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 create 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 undertaken in the same year but exposed to different post-agreement market fluctuations. An identifying assumption is that acquirers are attentive to post-agreement changes in acquisition cost.[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. Then, I hand-collect the exact acquisition terms for all identified divested acquisitions as well as a matched sample of non-divested acquisitions from 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). Aside from 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 the 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 exchange type (cf. Ahern and Sosyura 2014). I find the precise deal terms for 89% of acquisitions in my sample. The rate increases to 93% for acquisitions since 1994, when firms began filing reports through SEC's Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system. These rates are large both on their own and in comparison with existing studies (see Section II.B for details).

The resulting dataset, comprised of divested and non-divested deals, includes large and salient transactions. The median acquisition cost, for example, is $99 million. This sample solely consists of *Fixed Shares* mergers since post-agreement acquisition cost changes are unique to this deal structure. This preempts any concerns about omitted variables that might simultaneously affect selection into deal structure type and divestiture rates. The market-induced acquisition cost variation in my sample is economically meaningful, with the interquartile range of the market return between

[2] This assumption appears well justified. For example, the media frequently reports on stock price-induced transaction value changes, indicating that these changes should also be particularly salient to managers. See, for example, this 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 (dealbook.nytimes.com/2012/08/20/how-instagram-could-have-cut-a-better-deal).

merger agreement and completion equaling 8.5 percentage points.

The key finding of this paper is that there is a strong link between exogenous acquisition cost variation and subsequent divestment decisions, consistent with the sunk cost hypothesis. I estimate an 8-9% reduction in divestiture rates of acquired businesses associated with an interquartile increase in quasi-random acquisition cost. 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 accounting for the passage of time (Efron 1988; Jenter and Kanaan 2015), and a two-stage control-function estimation method (Wooldridge 2015). Further, this result is unlikely to be explained by differential selection into deal completion versus withdrawal after an official merger agreement has been reached. 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. This would also predict lower divestiture rates after acquisition cost increases, even in the absence of sunk cost effects. However, stock deal withdrawals after market increases are highly infrequent, which suggests this concern is of limited importance empirically. My results also remain unchanged when I successively remove observations with the highest estimated ex ante withdrawal probabilities from the sample.

Three additional findings support and extend the main result and 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 decision to divest. To address this, I implement placebo tests involving hypothetical acquisition cost changes. These tests rest on 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 (cf. 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. Additionally, the results on actual cost changes 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 effect is concentrated in firm-years in which the CEO who led the acquisition is still at the helm and is reduced by 30-50% after this CEO steps down. This result relates to survey evidence by Graham, Harvey, and Puri (2015) which finds that CEOs make M&A-related decisions "in relative isolation," and suggests an intrapersonal sunk cost mechanism. The finding of a CEO-specific sunk cost effect also 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, my 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, they are also at odds with a "learning by doing" channel, 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, non-mutually exclusive reasons: CEOs themselves being subject to behavioral forces that underlie sunk cost effects, and CEOs responding to sunk-cost thinking by 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 amplified in CEOs who are likely less sophisticated, as gauged by cross-sectional sorts by firm size (cf. Gabaix and Landier 2008), validated with additional data on CEO education. These findings point to a mechanism related to managerial psychology as an important driver 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 the sunk cost induced divestment distortions, due to a lack of detailed data on divestiture transaction prices and segment cash flows. That said, various aspects 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 NPV-optimal 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 2020), a pattern that is supported in my sample and suggests that on average, delaying divestiture of a costly acquisition should entail efficiency costs. Finally, a counterfactual exercise, which estimates an earlier divestiture date for acquirers had they experienced no acquisition cost increase, yields that firms underperform between the counterfactual and actual divestiture announcement date, 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 studying 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, instead, focuses on belief-based biases (e.g. overconfidence and optimism, as for example in Malmendier and Tate 2005, 2008, Landier and Thesmar 2008, Gervais, Heaton, and Odean 2011; and competition neglect, Greenwood and Hanson 2014) and, more recently, also heuristics (e.g. the 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 has, for example, studied the influence of prospect theory in initial public offerings and CEO compensation (Loughran and Ritter 2002, Dittmann, Maug, and Spalt 2010). Across classes of biases, I add to the literature on investment distortions generated by nonstandard decision-makers (e.g. Malmendier and Tate 2005, Greenwood and Hanson 2014, and Krüger, Landier, and Thesmar 2015).

Second, I contribute to the corporate finance literature on mergers and 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 has mostly examined neoclassical theories and the influence of social ties to explain divestiture patterns of acquisitions, 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 2016), acquirer–target social ties (Ishii and Xuan 2014), as well as industry shocks and cultural mismatch (Cronqvist and Pély 2020).[3] Weisbach (1995) documents a higher propensity by firms 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 higher *general* commitment to a business by acquiring CEOs could, however, be due to a variety of reasons, including differences in beliefs or information between 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 triggered by 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. Only few other papers have found 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. Ashraf, Berry, and Shapiro (2010), in motivating their sunk cost field experiment involving water purifiers in Zambia, highlight 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 more recent papers provide evidence for sunk costs affecting 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 which they inherit from others. By cleanly documenting that sunk costs matter in an economically important setting involving the most sophisticated decision-makers—CEOs typically have decades of professional experience (Dittmar

[3] There is also a literature studying divestitures independent of whether a divested segment was previously added through an acquisition. Also here, 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).

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.

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 below, features three periods. At $t = 0$, the manager of a firm can buy an asset at cost $\overline{C} = C + \Delta C$. 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 further below.[4] If she keeps the asset, it generates, or requires the firm to pay, an interim cash flow X at $t = 1$. X is also a mean-zero random variable, independent of ΔC. 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 1.

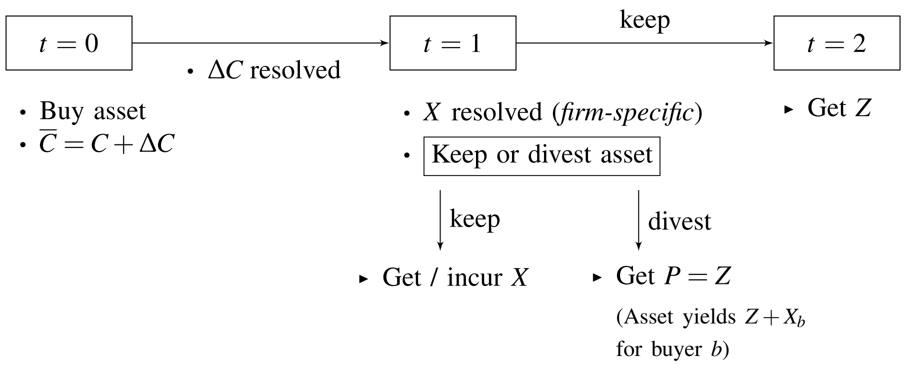
Empirically, the interim cash flow X can be thought of as *synergies* which can be positive ($X > 0$) or negative ($X < 0$). Furthermore, synergies are *firm-specific*. If the firm owning the asset at $t = 1$ sells it to another firm after learning its realized synergy level X, the asset payoff for the new buyer (b) will involve a new synergy draw (X_b). The assumption of uncorrelated synergies is common in the literature (cf. Betton et al. 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

[4] Consequently, the cost shock ΔC generates non-fundamental 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 non-fundamental acquisition price variation (see, e.g., Section III.C for details).

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

Framework Timeline



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

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

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

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 by 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

[6] These assumptions are not important for delivering the key sunk cost predictions, and I will 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.

the true value of the asset to the firm. With $P = Z$, this only depends 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 the cost shock ΔC makes it less likely that the divestment condition is met.

Contrasting Results 1 and 2 yields a natural and testable prediction for sunk cost effects: that 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 clarifies two points: First, finding an empirical relation between past cost shocks and propensity to divest is not easily reconcilable 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.

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 visualizes this argument.[8]

While prospect theory is an intuitive psychological underpinning of sunk cost effects, other

[7] In the framework, the NPV of the asset at $t = 1$ is $X + Z - P$, and with $P = Z$, it is entirely captured 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).

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

work has emphasized additional potential microfoundations, in particular the concept of cognitive dissonance (Festinger 1957).[9] For example, Staw et al. (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] Additionally, it can also be useful to consider how sunk cost effects relate to another empirical phenomenon: the disposition effect (i.e. investors' tendency to sell winning stocks and hold losing stocks; Shefrin and Statman 1985). Indeed, 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), suggesting that firms do not tend to divest "winners."[12] Relatedly, 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 will 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 R&D as well as financial intermediation.

[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 et al. (2021) 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 et al. (2016) propose cognitive dissonance as a potential mechanism underlying the disposition effect. Barberis and Xiong (2009, 2012) establish 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.

[12] While the disposition effect compares the proportion of gains and losses investors realize, seminal disposition effect papers 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).

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 dataset on stock acquisitions from the Securities Data Company (SDC) Platinum M&A database. Applying standard data filters (Fuller et al. 2002; Moeller et al. 2004; Betton et al. 2008; Netter et al. 2011), the sample comprises several thousand domestic acquisitions by U.S. public acquirers between 1980 and 2016. I then identify divestitures from two sources:

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 datasets using SDC's 6-digit 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 (2020), 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, allowing me to accurately track acquisitions through time.

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 pinpoint 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 2020) 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 divest*ing* firm originally acquired the subsequently divested business. Frequently, I am 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 on its own and compared to prior work though relative comparisons are difficult (cf. 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*—defined as the period from two days after the final merger agreement until the merger completion date (term from Ahern and Sosyura 2014)—of at least ten days. Infrequently, the dates in SEC filings differ from those in SDC, in which case 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 *non-binding* document that only stipulates a preliminary agreement to merge.

C. Additional Data and Final Divestiture Sample

I supplement the dataset 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. Appendix A contains definitions of all variables 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 (Ahern and Sosyura 2014, Mitchell et al. 2004).

D. Matched Sample of Non-Divested Acquisitions

In a final step, I extend the sample to include *Fixed Shares* acquisitions that are not subsequently divested. This allows me to capture 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 non-divested 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 also show robustness to a sample of divested and non-divested acquisitions constructed without matching, discussed in Section IV.B and in 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 oftentimes has higher power than standard sampling, thus requiring 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 the 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 (cf. 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.

[13] This does not require that the acquirer holds on to a business up to the present. A firm that repeatedly fails to divest a non-performing business might, for example, plausibly become a takeover target. The empirical analysis treats such cases as non-divested 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.

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 non-divested 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 appendix. As is crucial for case control sampling, I do *not* match on the key variable of interest, the experienced (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 *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 Appendix-Table IA.III).

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

E. Summary Statistics

Figure 1 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 1a). Thus, my sample appears representative of stock mergers in general, as these were the periods that witnessed a surge in stock merger activity (e.g., Betton et al. 2008). Among the divested deals, there is considerable variation as to when the divestiture occurs (Panel 1b). 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 ten years of the acquisition (Panel 1c).

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 (3-day CAR of −0.30% and −0.68%, respectively). An unfavorable

[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 et al. 2007). Internet Appendix Table IA.VIII confirms that both of these characteristics are also strong divestiture predictors in my general M&A sample.

announcement reaction on average is typical for stock mergers (Betton et al. 2008) and, in particular, for *Fixed Shares* mergers (Mitchell et al. 2004). The median acquisition had a transaction value of $99 million, 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—that is, 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 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 frequencies over time.

III. Empirical Strategy

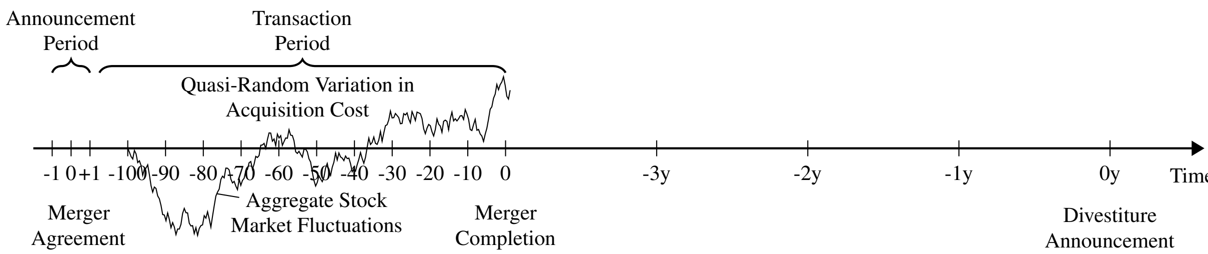
The key component of my identification strategy is that in *Fixed Shares* acquisitions, aggregate market fluctuations between when parties enter into the binding merger agreement and when the acquisition is completed trigger plausibly exogenous changes in acquisition cost. The empirical analysis relates 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. The figure below 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 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*

Event Timeline

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



versus *Fixed Dollar* deal structures. The empirical analysis is centered 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 an array of closing conditions, implying that parties may or may not know the closing date with certainty when signing the final agreement (Mitchell et al. 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 ten- or thirty-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:

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

$\%stock_i \in (0,1]$ denotes the fraction of the merger consideration that the 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 et al. 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:

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

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:

$$\Delta C_i = \Delta R_i \times \%stock_i \times \frac{\text{Deal Value}_i}{\text{Market Cap}_i^{Acq}} \tag{1'}$$

Equation (1') differs from Equation (1) exactly because it uses the market-induced cumulative return instead of the endogenous cumulative return to the acquirer, hence isolating 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 interquartile range (IQR) of the market return being about 8.5 percentage points (pp). These returns also induce economically relevant variation in acquisition cost, 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 then relate the market-induced change in acquisition cost calculated in Equation (1') to the rate of subsequent divestiture:

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

where i refers to an acquisition, t is the time passed since the acquisition in years, and t_0 denotes the acquisition (calendar) year. $\text{Divestiture}_{i,t}$ is an indicator variable that equals zero in all years prior to the divestiture and one in the year of divestiture. 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. $\boldsymbol{X}_{i,t}$ is a vector of control variables that 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; Pischke 2017; Wooldridge 2010).

Assumption (i). Panel A of Table II regresses, using the main sample, cumulative firm returns during the transaction period on cumulative market returns, net of expected returns. Column (1) regresses firm returns on solely 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, neither when considering covariates individually nor 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 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. The placebo tests find, however, no evidence that hypothetical cost changes, calculated from market fluctuations following deal completion or for the *Fixed Dollar* acquisitions from Section II.B from market fluctuations between merger agreement and completion, predict divestiture rates.

Several additional aspects and tests help to further alleviate the above identification concerns. First, all analyses include acquisition year fixed effects, that is, identification comes from within-year market fluctuations, to at least partially 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 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, Section V.E will 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 obtained 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. Thus far, the discussion of the empirical strategy has assumed 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 has exploited the frequent exogenous failure of mergers for identification (e.g., Savor and Lu 2009, Jacobsen 2014, and Malmendier et al. 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 aspects 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 (oftentimes in cash) is 3–4% 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. Leaving the details to Sections IV.B and IV.D, the estimated effect 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 semi-parametric Cox (1972) proportional hazards model, commonly used for survival and duration analysis (time-to-event analysis).[17] The hazard model treats non-divested acquisitions as censored. The censoring date corresponds to the

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[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 2021). While some of the sample criteria and results differ across the two papers, both are in line with the results 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.

[17] Cf. 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 refers 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 (cf. Efron 1988; Jenter and Kanaan 2015).

day before I begin the divestiture news search (Dec. 15, 2018), or, if the acquirer is itself taken over at some point, to the acquisition date. The Cox (1972) model assumes the following form:

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

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 semi-parametric 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 passed 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 one can check, for each covariate in the model, 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 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 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; Abadie, Athey, Imbens, and Wooldridge 2017).[18]

[18] Abadie, Athey, Imbens, and Wooldridge (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 et al. 2012, Fich et al. 2019). That said, in Internet Appendix Table IA.VI, I

Column (1) includes the main variable of interest, the market-induced acquisition cost change, 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 1%). This reveals that an increase in quasi-random acquisition cost 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 twelve 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 percentage point decrease in the twelve 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 are thus similar for divested and non-divested 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 VI for a detailed discussion of the Schoenfeld test results). Internet Appendix Table IA.XI reports how the Schoenfeld residuals for each of the covariates in Column (2) of Table III depend on (linear) time. (Internet Appendix Table IA.XII 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 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

show robustness to other clustering choices, in particular clustering by industry considering that industry conditions at least partially contribute to divestiture patterns.

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) re-estimate Columns (1) and (2), respectively, allowing for linear time interactions. Column (5) re-estimates Column (2) as well, allowing for interactions with log-time. The time interaction coefficients are omitted for brevity. Across columns, the coefficient on market-induced acquisition cost changes is very similar compared to the specifications without time-dependent effects. If anything, the coefficient of interest slightly increases 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 which 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. They 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 re-estimates 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 relevant (cf. 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 SDC's 'Status: Withdrawn' filter). I then estimate an 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 (cf. 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 paper. 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 is also comprised of both divested and non-divested acquisitions but is constructed via standard sampling, that is, without case control matching. Leaving the details of the alternative sample construction to the appendix, this approach estimates sunk cost distortions of very similar economic magnitudes, though the case control approach helps with the statistical power of the analysis (cf. the discussion in Section II.D). After the placebo test results for the main sample in the next section, Section IV.D will present further evidence on sunk costs effects that does not use the case-control-matched sample, but uses instead 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 (cf. Bernstein 2015 for a similar approach in an

IPO setting). If market movements matter through 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 (omitted for brevity), fixed effects, and time interactions being identical to that in Table III. Panel A calculates hypothetical cost changes using market fluctuations in the three-month window immediately following the 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, with controls omitted for brevity.[19] 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 non-divested

[19] 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).

acquisitions. The specifications with the full set of controls and time interactions in Columns (4) and (5) imply a reduction in divestiture rates of 8.0–8.1% for an interquartile increase in acquisition cost, compared to 9.0–9.4% 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, both for 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, in particular given their similarity to *Fixed Shares* deals along many observables (Internet Appendix Table IA.IX, 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, that is, these are 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.[20] While the placebo test leverages variation in market fluctuations within

[20] 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 non-divested *Fixed Dollar* acquisition for each divested *Fixed Dollar* deal based on the matching approach in Section II.D.

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 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 pre-specified 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.[21]

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 et al. 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

[21] 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.

identify constrained firms, and following Fahlenbrach et al. (2021), 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 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 (cf. Guenzel and Malmendier 2020, Banerjee et al. 2015, 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 et al. (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. For about 50% of firms in my sample, I am able to retrieve this data from Execucomp. For the remaining firms, I hand-collect it 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.[22]

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 (omitted for brevity), fixed effects, and time interactions as in Column (4) of Table V. Column (3) includes log-time interactions. First, in Column (1), I re-establish the main effect of quasi-random acquisition costs on divestiture rates (documented for the divested acquisitions in Table V) after disregarding thirteen ambiguous CEO transitions as discussed above. With this modification, the coefficient of interest remains unchanged. Then, 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 et al. 2011, Dittmar and Duchin 2015, Schoar and Zuo 2017, and Bernile et al. 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.

[22] 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.

CEO Information, Beliefs, and Learning. Some existing work (e.g. Camerer and Weber 1999, McAfee et al. 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, however, not 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 easily reconcilable 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, non-mutually 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; cf. Section I.B). On the other hand, sunk cost effects can also arise from interactions between managers and biased third parties such 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 also 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 (cf. Krüger et al. 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, 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 year of acquisition, thus identifying small versus large acquirers in my sample in the cross-section in a given year. I either perform simple sorts of firms into smaller versus larger firms splitting at the median market capitalization, or sort firms further 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).[23]

The results in Columns (3) and (4) of Table IX are consistent with sunk cost effects being amplified in firms with less sophisticated, 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%.

Altogether, 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) yields that sunk cost managers hold on to costly acquisitions beyond the point where 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 in the data 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 a third of divestitures 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

[23] 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 across time and ranking publishers.

(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 (2020) 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 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.[24] 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 date, referred to as the *sunk cost period*, are 87 and 38 days, respectively (see Internet Appendix Figure IA.7). Panel 2a of Figure 2 shows an 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 as conclusive evidence that firms delay divestiture of businesses with a negative NPV and that this materializes in a decline in overall firm value—in particular, reverse causality concerns and the fact that a divestiture is a negotiated outcome and unlike assumed here, cannot be unilaterally advanced, all else equal. To partially address the first issue, Panel 2b of Figure 2 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

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

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 (cf. Section V.C), we would also expect there to be heterogeneity in stock market reactions around divestitures depending on the severity of the mistake.[25] In line with this conjecture, Internet Appendix Figure IA.8 shows that the counterfactual analysis (keeping in mind its limitations discussed above) reveals a particularly negative industry-adjusted performance between counterfactual and actual divestiture announcement date for acquirers who faced large acquisition cost increases, that is, for which sunk cost thinking will presumably lead 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 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 (cf. 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

[25] 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 et al. (2015).

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 (cf. 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 non-collar 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 has documented 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.

Relatedly, one 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. Selection into deal completion or cancellation is, however, unlikely to drive the results for several reasons. Withdrawals are oftentimes 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 post-agreement acquisition cost changes. In favor of this, both the endogenous and market-induced acquisition cost changes are economically meaningful, even after accounting for bounds in cost changes through collars (cf. Table I). Stock price-induced deal value changes are also frequently discussed by the business media (cf. footnote 2). Further 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, e.g., 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, e.g., Example 4 in Section III.B of the Internet Appendix).

Divestiture Timing. It could also be that firms might 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. This is in particular given the inclusion of acquisition year fixed effects in all analyses, and given that the results remain unchanged when including, in addition, 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, there is, however, 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 for 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 very initial decision of whether to pursue an acquisition bid, for managers who base this decision at least partially on the resources already invested in, for example, the target search and deal negotiations.[26] Beyond M&A, Guler (2007) provides various insightful examples of sunk cost thinking in the venture capital (VC) industry, by way of semi-structured interviews with VC managers. For example, one interviewee stated that “the more money that goes

[26] See, for example, the following advice from successfulacquisitions.net/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.”

in, we are less likely to walk away," consistent with the sunk cost mechanism and findings proposed in this paper.[27] Yet another type of firm investment where sunk cost effects might be particularly prevalent is R&D. R&D investments are oftentimes large, and most expenditures on R&D are unrecoverable, that is, constitute sunk costs (Stiglitz 1987). A specific, well-known example of sunk cost effects in the context of R&D investment is the Concorde aircraft project, which is why the tendency of basing decisions on sunk costs is also dubbed the *Concorde effect*.[28]

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 some 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 gauged by the fraction of repeat vs. new VC investors, and in particular so 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.

The empirical challenge for future research remains to tightly identify sunk cost effects in other settings beyond the M&A–divestiture context. Some of the empirical design choices of this paper may be a useful guide for future work. For one, my empirical approach takes seriously the specific decision-maker in the firm who is responsible for a prior investment. Additionally, 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.

[27] 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."

[28] 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, e.g., forbes.com/sites/jimblasingame/2011/09/15/beware-of-the-concorde-fallacy and also Internet Appendix Figure IA.1b).

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, unfold after parties reach a binding merger agreement. As an acquisition becomes exogenously more expensive, firms' propensity to divest substantially decreases. 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. Instead, they are most consistent with managers systematically factoring sunk costs into their decision-making.

Various further mechanism tests strengthen the 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 open up 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 will make it possible to improve on the efficiency cost tests from 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 impact of sunk cost effects in other important contexts, such as R&D investment and innovation, or financial intermediation (cf. 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 a useful guide for finding new identification opportunities in other settings.

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Table I. Summary Statistics

This table reports summary statistics for the main sample, comprised of divested and non-divested 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. Appendix A provides variable definitions.

Panel A: Deal-Level Variables ($N = 558$)					
	Mean	Median	SD	$P25$	$P75$
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	$P25$	$P75$
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	$P25$	$P75$
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

Table II. Market Fluctuations Between Merger Agreement and Completion

This table reports the 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 %. 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. Appendix A 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-squared	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

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. ΔC, the main variable of interest, 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')). Appendix A 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 linearly vary with time. Column (5) allows these covariates to vary with log-time. Time interaction coefficients are omitted in the interest of brevity. 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

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. Appendix A 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

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 sub-sample 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. Appendix A 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

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 sub-sample. Please refer to Table V and Section IV.E for additional details. Appendix A 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)
$\Delta C \times$ Fixed Shares	-0.057^{**}	-0.055^{**}	-0.082^{***}	-0.083^{***}
	(-2.02)	(-2.00)	(-2.89)	(-2.89)
$\Delta C^{Hyp} \times$ 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

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) (*WW*) 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). Table notes indicating the inclusion of control variables and fixed effects are omitted in the interest of brevity. Appendix A 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

Table VIII. Firm Versus CEO-Specific Effect

This table reports the 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. Appendix A 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

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. In the interest of brevity, all columns report the interaction terms of interest only. Appendix A 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 $\leq P50$			−0.111**	
			(−2.33)	
× Market Cap $> P50$			−0.081	
			(−1.35)	
× Market Cap $\leq P25$				−0.137
				(−1.55)
× $P25 <$ Market Cap $\leq P75$				−0.099**
				(−2.21)
× Market Cap $> P75$				−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

Table X. Diversifying Versus Same-Industry Acquisitions

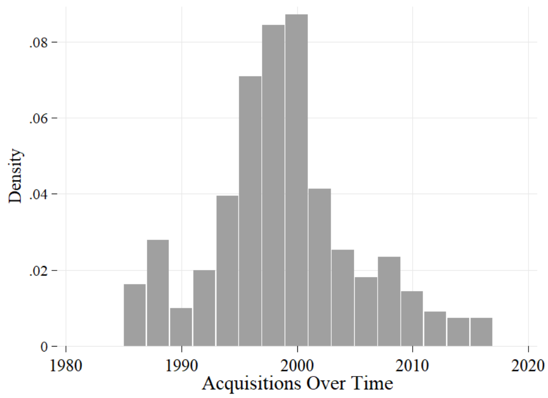
This table reports estimates of the effect of quasi-random variation in acquisition costs on subsequent divestiture rates by whether the acquisition is diversifying or a same-industry deal. The dependent variable is an indicator variable that equals one in the year in which an acquired business is divested 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. Appendix A 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

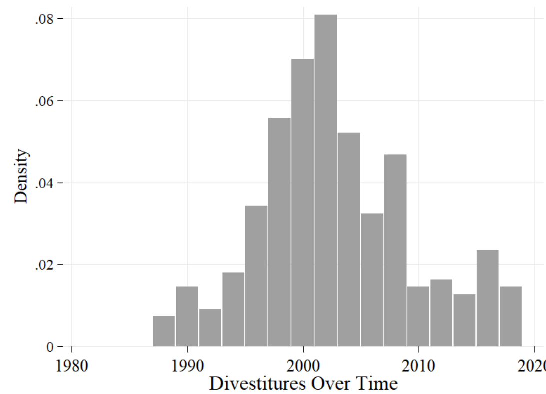
Figure 1. Acquisitions and Divestitures Over Time

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.

(a) Acquisitions Over Time



(b) Divestitures Over Time



(c) Years Between Acquisition and Divestiture

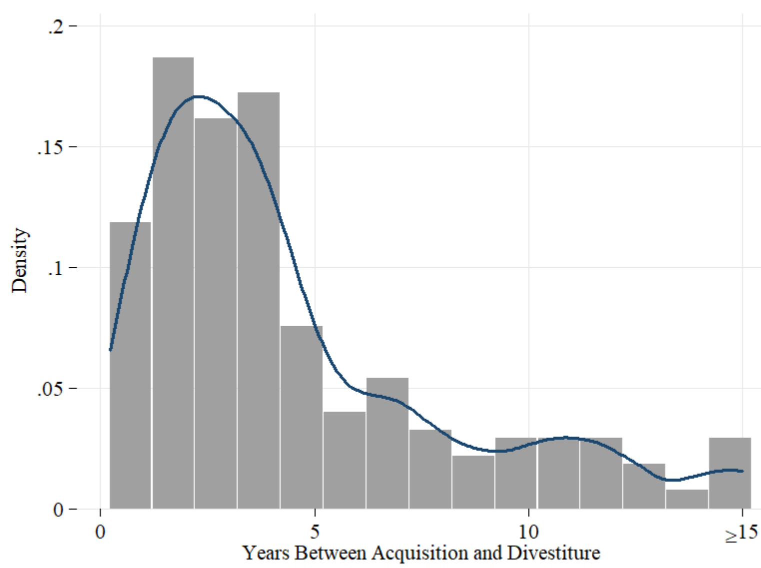
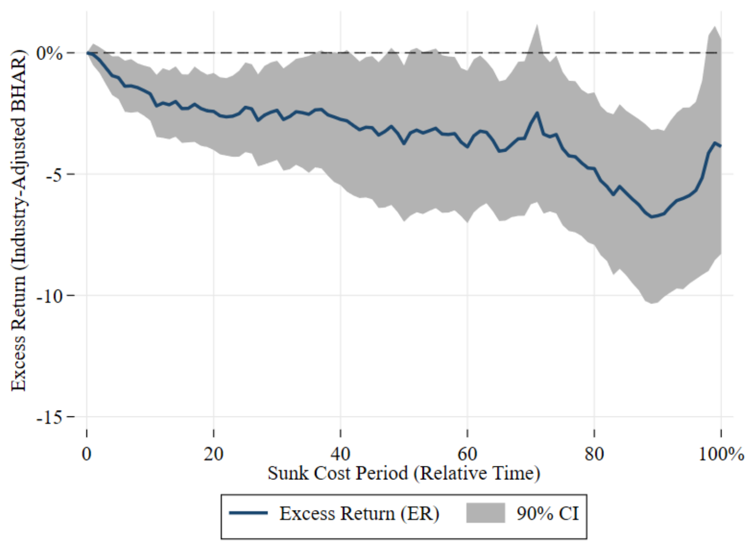


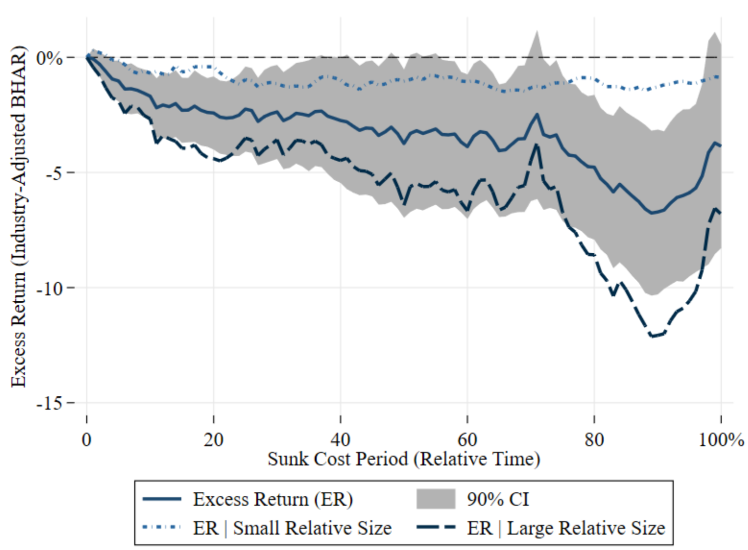
Figure 2. Efficiency Costs

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 acquirer's pre-acquisition market capitalization plus the value of the acquired business as measured by the transaction price). 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 1 and plot relative time (between 0% and 100%) passed between the estimated and actual divestiture announcement date. Please refer to Section V.D for additional details.

(a) Excess Return (Industry-Adjusted BHAR)



(b) Excess Return (Industry-Adjusted BHAR) Split by Relative Size of the Divested Business



Appendix A Variable Definitions

Variable	Definition
	Panel A: Acquisition-Related Variables
%stock	Fraction of the acquisition paid with stock
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 et al. (2015), the calculation uses the CRSP value-weighted return (including distributions) as the benchmark return in the calculation of CARs (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 et al. (2015) (based on Fama and French (1997) 49 industries)
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
High Career Incentives	Indicator variable that equals one for acquiring CEOs in the bottom age quartile (age at acquisition or median age between age at acquisition and CEO departure or divestiture)
Industry Distress	Indicator variable that equals one in each year subsequent to the acquisition 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 et al. (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 divestiture announcement dates

Internet Appendix for

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

Marius Guenzel

Contents

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I. Motivating Examples of Sunk Cost Effects Discussed in Leading Books

Figure IA.1. Sunk Costs in Prominent Books and Corporate Finance Textbooks

(a) Thinking, Fast and Slow, by Kahneman (2011)

A rational decision maker is interested only in the future consequences of current investments. Justifying earlier mistakes is not among the Econ's concerns. The decision to invest additional resources in a losing account, when better investments are available, is known as the *sunk-cost fallacy*, a costly mistake that is observed in decisions large and small. Driving into the blizzard because one paid for tickets is a sunk-cost error.

Imagine a company that has already spent \$50 million on a project. The project is now behind schedule and the forecasts of its ultimate returns are less favorable than at the initial planning stage. An additional investment of \$60 million is required to give the project a chance. An alternative proposal is to invest the same amount in a new project that currently looks likely to bring higher returns. What will the company do? All too often a company afflicted by sunk costs drives into the blizzard, throwing good money after bad ...

(b) Corporate Finance, by Berk and DeMarzo (2017)

COMMON MISTAKE **The Sunk Cost Fallacy**

Sunk cost fallacy is a term used to describe the tendency of people to be influenced by sunk costs and to "throw good money after bad." That is, people sometimes continue to invest in a project that has a negative NPV because they have already invested a large amount in the project and feel that by not continuing it, the prior investment will be wasted. The sunk cost fallacy is also sometimes called the "Concorde effect," a term that refers to the British and French governments' decision to continue funding the joint development of the Concorde aircraft even after it was clear that sales of the plane would fall far short of what was necessary to justify the cost of continuing its development. Although the project was viewed by the British government as a commercial and financial disaster, the political implications of halting the project—and thereby publicly admitting that all past expenses on the project would result in nothing—ultimately prevented either government from abandoning the project.

It is important to note that sunk costs need not always be in the past. Any cash flows, even future ones, that will not be affected by the decision at hand are effectively sunk, and should not be included in our incremental forecast. For example, if Cisco believes it will lose some sales on its other products whether or not it launches HomeNet, these lost sales are a sunk cost that should not be included as part of the cannibalization adjustments in Table 8.2.

(c) Principles of Corporate Finance, by Brealey, Myers, and Allen (2017)

Forget Sunk Costs Sunk costs are like spilled milk: They are past and irreversible outflows. Because sunk costs are bygones, they cannot be affected by the decision to accept or reject the project, and so they should be ignored.

Take the case of the James Webb Space Telescope. It was originally supposed to launch in 2011 and cost \$1.6 billion. But the project became progressively more expensive and further behind schedule. Latest estimates put the cost at \$8.8 billion and a launch date of 2018. In 2011, when Congress debated whether to cancel the program, supporters of the project argued that it would be foolish to abandon a project on which so much had already been spent. Others countered that it would be even more foolish to continue with a project that had proved so costly. Both groups were guilty of the *sunk-cost fallacy;* the money that had already been spent by NASA was irrecoverable and, therefore, irrelevant to the decision to terminate the project.

II. Sunk Cost Framework Embedded in Prospect Theory

A. *Prospect-Theory-Based Framework*

This section presents an extension of the reduced-form sunk cost framework, microfounding sunk cost effects with prospect theory (Kahneman and Tversky 1979) and building on Thaler (1980).

Setup. The setup is the same as in Section I.A, except for the following additions to flexibly incorporate reference dependence:

(A1) The manager buying the asset at $t = 0$ evaluates outcomes in terms of gains and losses relative to a reference point R, and exhibits diminishing sensitivity to gains and losses. I will refer to the prospect theory value function for gains as $v_G(\cdot)$ and to that for losses as $v_L(\cdot)$. With diminishing sensitivity, $v_G(\cdot)$ is concave, whereas $v_L(\cdot)$ is convex.[IA.1] The manager evaluates both costs and payoffs associated with asset ownership relative to R, specified further in (A4).

(A2) To allow for a flexible, non-zero reference point on the cost side, the known cost component of buying the asset, C, contains a "fair" component (C_F) and a "premium" (C_P), such that $C = C_F + C_P$.

(A3) I now introduce a wedge between the asset's payoff at $t = 2$, Z, and the (net) divestiture price at $t = 1$. Specifically, even though the asset pays Z at $t = 2$, the manager can sell it for $P_{net} = Z - \Delta P$ only, where $\Delta P > 0$. Such a wedge can arise due to, e.g., transaction costs or a less than fully competitive bidder market. Additional behavioral explanations, such as managerial overconfidence about the asset's payoff, are also conceivable.

(A4) The manager evaluates costs relative to the fair cost component C_F. She evaluates payoffs relative to the outside option of divesting the asset at $P_{net} = Z - \Delta P$. That is, R is two-dimensional and given by $R = (C_F, P_{net})$.

Relation to Sunk Cost Modeling in Thaler (1980). (A1)–(A4) imply a setup as in Thaler (1980). In his classic sunk cost example:

> A family pays $40 for tickets to a basketball game to be played 60 miles from their home. On the day of the game there is a snowstorm. They decide to go anyway, but note in passing that had the tickets been given to them [for free], they would have stayed home.

Restating Thaler's (1980) argument, attending the game yields pleasure of g, evaluated against the outside option of staying home (i.e., pleasure of 0). Buying the tickets (and driving through the snowstorm) yields a cost of $40(+c)$ on game day, evaluated against the counterfactual of free tickets (i.e., cost of 0). In other words, the implicitly assumed reference point is $R = (0, 0)$. Diminishing

[IA.1] With the functional forms $v_G(x) = x^{\alpha}$ and $v_L(x) = -\lambda(-x)^{\alpha}$ (where λ is the prospect theory loss aversion parameter), the average estimate of α—i.e., the average degree of diminishing sensitivity—across studies is approximately 0.7 (Booij et al. 2010).

sensitivity to losses due to the convexity of $v_L(\cdot)$ induces the family to attend the game *because of* the money spent on the tickets. To see this, suppose that the family would have been indifferent between going to the game during the snowstorm and staying home if the tickets had been free, i.e., $v_G(g) = -v_L(-c)$. Then

$$v_G(g) + v_L(-(c+40)) > v_G(g) + v_L(-c) + v_L(-40) = v_L(-40). \tag{IA.1}$$

It is easy to extend this example to incorporate non-zero reference points for pleasure and costs as in (A4). For example:

> A family buys *expensive* tickets to a basketball game to be played 60 miles from their home. On the day of the game there is a snowstorm. They decide to go anyway, but note in passing that had the tickets been *cheap*, they would have stayed home *and watched the game on television*.

The counterfactuals of "watch game from home" and "cheap tickets" (rather than "stay home" and "free tickets") continue to deliver a sunk cost effect in the same way as before.[IA.2] Moreover, these counterfactuals map more directly to the M&A–divestiture setting, in which the alternative to keeping the asset is to divest it (rather than to abandon it), and where a zero-cost counterfactual may seem too restrictive. This echoes Lewis, Rees-Jones, Simonsohn, and Simmons (2019), who summarize that "there are many reasons to believe that entire prices are *not* encoded as losses and that reference prices are *not* \$0" and that instead "a price paid is coded as a gain or a loss relative to a *fair* or an *expected* price."

Implications. Like the reduced-form framework, the prospect-theory-based framework with additions (A1)–(A4) delivers the same key result that a sunk cost manager's propensity to keep the asset is a function of the cost shock ΔC.

On the payoff side, keeping the asset results in a subsequent payoff of Z relative to the reference point of $P_{net} = Z - \Delta P$ (i.e., a gain of ΔP). On the cost side, C_P introduces additional costs relative to the reference point C_F (i.e., a loss) and crucially, ΔC introduces variation in losses relative to the reference point. Additionally, a negative synergy draw at $t = 1$ ($X < 0$), requiring an additional outlay, intensifies the perceived losses relative to C_F. Taken together, the manager will divest if and only if

$$v_G(\Delta P) + v_L(-(C_P + \Delta C) + X) < v_L(-(C_P + \Delta C)).\text{[IA.3]} \tag{IA.2}$$

As in the reduced-form framework, all else equal, a higher cost shock ΔC makes it less likely that the divestment condition is met, here due to the diminishing sensitivity to losses (convexity

[IA.2] Denote the new reference point by $R = (c_{cheap}, g_{home})$. Then $v_G(g - g_{home}) = -v_L(-(c - c_{cheap}))$ implies that $v_G(g - g_{home}) + v_L(-(c + 40 - c_{cheap})) > v_L(-(40 - c_{cheap}))$.

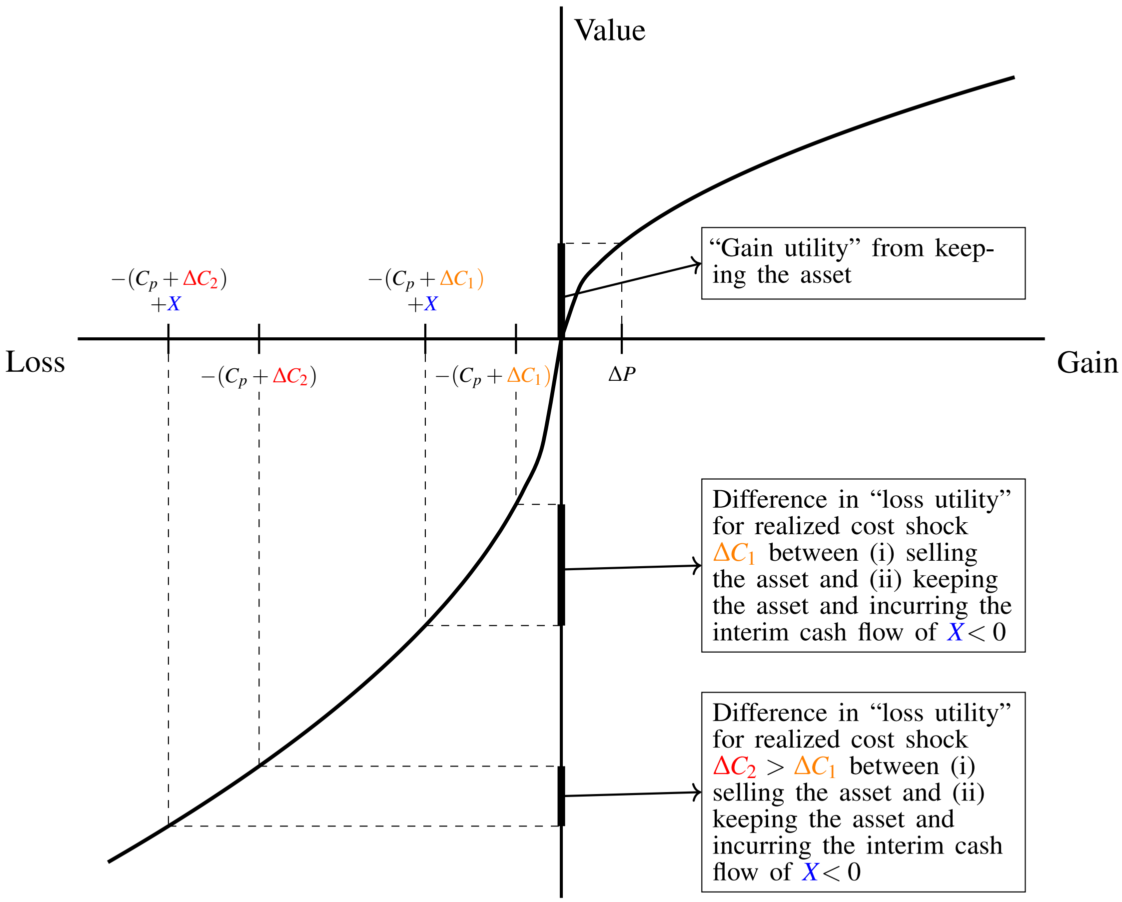
[IA.3] For a positive interim cash flow, $X > 0$, it is not obvious whether a decision-maker would mentally code this a reduction in the loss ($v_L(-(C_P + \Delta C) + X)$, as in Equation (IA.2)), an increase in the gain ($v_G(\Delta P + X)$), or as a separate gain ($v_G(\Delta P) + v_G(X)$). However, irrespective of the precise mental coding, the value from keeping the asset will be greater than the value from divesting and thus the decision-maker will not divest when $X > 0$.

of $v_L(\cdot)$). Figure IA.2 provides a graphical illustration of this result, and the following theorem establishes it formally.

Theorem 1. *For a sunk cost manager according to (A1)–(A4) above, the probability of divesting the asset at* $t = 1$ *(conditional on asset ownership at* $t = 0$*) is decreasing in the realized cost shock* ΔC.

Proof. See Internet Appendix Section II.B. □

Figure IA.2. Sunk Cost Effects and Prospect Theory



B. Proofs

Proof of Theorem 1. From Equation (IA.2), the manager divests the asset if and only if

$$\begin{aligned} & v_G(\Delta P) + v_L(-(C_P + \Delta C) + X) < v_L(-(C_P + \Delta C)) \\ \Longleftrightarrow\ & v_G(\Delta P) < v_L(-(C_P + \Delta C)) - v_L(-(C_P + \Delta C) + X). \end{aligned}$$

Define $F(\Delta C, X) = v_L(-(C_P + \Delta C)) - v_L(-(C_P + \Delta C) + X)$, and let $\tilde{V} = v_G(\Delta P)$. Further, define T as the threshold such that $F(\Delta C, T) = \tilde{V}$. When $X > 0$, we have $F(\Delta C, X) < 0 < \tilde{V}$, and the manager always chooses continuation. Thus, we will focus on the case when $X < 0$. For the manager to choose divestment when $X < 0$, it must be that $T < 0$.

The divestment condition of Equation (IA.2) can be rewritten as

$$F(\Delta C, X) > \tilde{V}. \tag{IA.3}$$

Since $F(\Delta C, X)$ is decreasing in X, for a given ΔC, condition (IA.2) is equivalent to

$$X < T, \tag{IA.4}$$

and the probability of divestiture is $\Pr(\text{divest}) = \Pr(X < T)$.

Since $F(\Delta C, T) = \tilde{V}$, by the *Implicit Function Theorem*, we have

$$\begin{aligned} \frac{\mathrm{d}T}{\mathrm{d}\Delta C} &= -\frac{-v_L^{'}(-(C_P + \Delta C)) + v_L^{'}(-(C_P + \Delta C) + X)}{-v_L^{'}(-(C_P + \Delta C) + X)} \\ &= 1 - \frac{v_L^{'}(-(C_P + \Delta C))}{v_L^{'}(-(C_P + \Delta C) + X)}. \end{aligned}$$

Since v_L is convex and $-(C_P + \Delta C) > -(C_P + \Delta C) + X$, we have $v_L^{'}(-(C_P + \Delta C)) > v_L^{'}(-(C_P + \Delta C) + X)$. Hence,

$$\frac{\mathrm{d}T}{\mathrm{d}\Delta C} < 0;$$

that is to say, the threshold T is decreasing in ΔC. Consequently, the probability of divestiture, $\Pr(\text{divest}) = \Pr(X < T)$, is decreasing in ΔC. □

III. Data Appendix

A. *Additional Detail on Divestitures of Previously Acquired Businesses (Section II.A)*

M&A Sample Construction. In a first step, I download all transactions by U.S. acquirers between 1980[IA.4] and 2016. Since my identification strategy (see Section III) exploits stock price fluctuations between deal announcement and completion, I then restrict the sample to acquisitions that the acquirer pays for at least partially with its stock. I require that the deal status be Completed and the target type be Public, Private, or Subsidiary, eliminating transactions that include government-owned entities and joint ventures (Netter et al. 2011). In addition, I restrict to Disclosed Dollar Value and Undisclosed Dollar Value deals, eliminating repurchases, self tenders, and stake purchases, and to deals in which the acquirer owned less than 50 percent of shares in the target six month prior to the transaction announcement, and acquired at least 50 percent of shares of the target (Fuller et al. 2002). Then, I remove duplicate observations and those in which the acquirer's and target's CUSIP identifiers coincide, and restrict the sample to public acquirers that are included in CRSP and are traded on the NYSE, NYSE American (AMEX), or NASDAQ stock exchange.[IA.5] I also require that the acquirer's and target's SIC codes be available from CRSP or SDC, and drop deals in which either party's Fama and French (1997) industry affiliation, based on 49 industry portfolios, is Other (Jenter and Kanaan 2015).

In a next step, I require that the deal value be no smaller than $1 million and the deal value relative to the acquirer's total assets be at least 1% (Fuller et al. 2002; Moeller et al. 2004). These filters, in conjunction with the minimum shares acquired threshold of 50 percent above, ensure that the acquisition constitutes a significant event from the perspective of the acquirer. I further limit the sample to deals for which the three-day cumulative abnormal return (CAR) to the acquirer is available and deals in which the acquirer is still included in CRSP at the time of deal completion. Finally, also for reasons of identification, I require that the gap between merger agreement and completion date be at least two days.[IA.6]

Taken together, these filters result in a final M&A sample of 7,862 acquisitions. Internet Appendix Table IA.I provides a step-by-step overview of the M&A sample construction.

Identifying Divestitures Through SDC. As described in the main paper, I merge SDC's transactions tagged as divestiture-related to the acquisitions included in the *final M&A sample* described above. For the merge, I require that (i) the target CUSIPs in the acquisition and divestiture deals match, (ii) the acquirer CUSIP or the acquirer's parent CUSIP in the acquisition deal matches

[IA.4] I follow Betton et al. (2008) in choosing 1980 as the starting year for the analysis. SDC only contains 66 observations prior to 1980.

[IA.5] To link SDC and CRSP, I reduce 8-digit CUSIPs in CRSP to 6-digit CUSIPs. When there are multiple observations with the same resulting 6-digit CUSIP, I retain the observation with the lowest seventh digit (Malmendier et al. 2016).

[IA.6] In all my analyses, I elevate this threshold to ten days (see Section II.B for details). I use a less stringent threshold at this point since I occasionally manually adjust the merger agreement or completion date, if SDC misreports the merger announcement or completion date (which is rare, see Fuller et al. 2002). In pilot searches, I find that date adjustments are more frequent when there is at least some gap between announcement and completion date reported in SDC, explaining the initial threshold choice of two days.

Table IA.I. M&A Sample Construction

	Sample Size
Announced acquisitions financed at least partially with stock, 1980-2016	21,796
Observations remaining after restricting to	
Status: Completed	18,328
U.S. Target	16,500
Target type: Public, Private, or Subsidiary	16,387
Deal type: Disclosed deal or Undisclosed Deal	16,074
Percentage of shares held 6 months prior to announcement: 0 to 49	15,848
Percentage of shares acquired in transaction: 50 to 100	15,734
Unique entries (no duplicates)	15,720
Acquirer CUSIP different from target CUSIP	15,715
Public acquirer, included in CRSP, and traded on NYSE, NYSE American (AMEX), or NASDAQ	11,890
Acquirer and target SIC codes available and Fama and French (1997) industry codes (based on 49 industry portfolios) different from "Other"	11,538
Deal value no smaller than $1 million	11,182
Deal value relative to acquirer's total assets no smaller than 1%	9,931
Acquirer still in CRSP at time of deal completion	9,824
3-day cumulative announcement return available	9,800
Difference between deal announcement and completion at least two days	7,862
Final M&A Sample	**7,862**
of which acquirer is non-financial firm (SIC code < 6000 or ≥ 7000)	5,893

the parent CUSIP in the divestiture deal, and (iii) the acquirer CUSIP and the acquirer's parent CUSIP in the acquisition deal differ from the acquirer CUSIP and the acquirer's parent CUSIP in the divestiture deal.

An example that illustrates how the CUSIP-based merge can be useful in the presence of name changes is the case of IVX Bioscience Inc. and Johnson Products Company. SDC correctly identifies this divestiture, even though IVX Bioscience Inc. was known as IVAX Corp. at the time when it acquired Johnson Products.

Through the SDC-based approach, I identify, after initial data checks and ruling out obvious wrong matches (e.g. if the alleged divestiture is said to have occurred before the acquisition), 298 matches ("divestiture candidates") for which I verify the accuracy of each divestiture in more detail.

Identifying Divestitures Through Nexis. I perform the news search using Nexis Uni and systemize it by establishing the following search phrase structure: Acquirer Name (shortened version) AND Target Name (shortened version) AND (sell OR divest OR spin off OR buyout). The AND and OR operators ensure that search results contain both the acquirer and target name and at least one of the four divestiture-related words. Nexis automatically returns articles that feature the past tense of the provided verbs (including the irregular past tense "sold," for example). For each acquisition not identified as a "divestiture candidate" through SDC, I first spend about five minutes on Nexis searching for sources that indicate a potential divestiture.[IA.7] The list of acquisitions I go through in this step comprises several thousand deals. To allay selection concerns, I only consider sources from December 15th, 2018 or earlier, the last day before I begin the news search. (Relatedly, the censoring date corresponds to the day before I begin the divestiture news search.) I then combine all identified potential divestitures with the "divestiture candidates" from the SDC-based approach and use additional sources to verify the correctness of each divestiture.

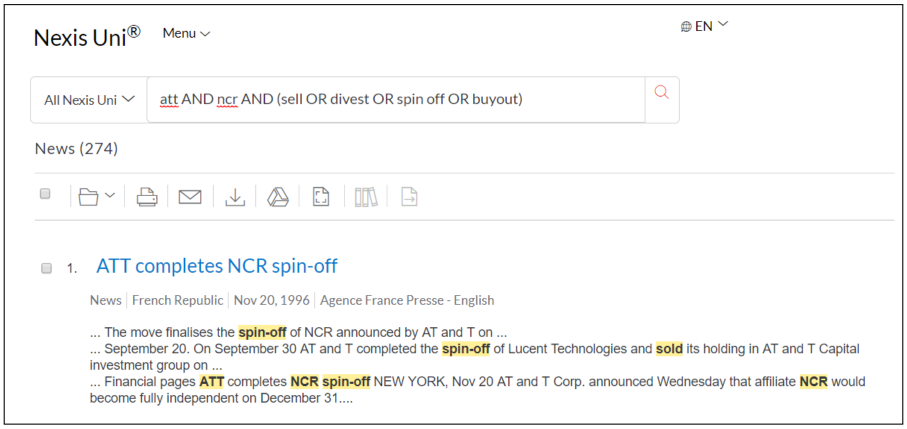
A prominent example of a divestiture undetected in the SDC-based approach is AT&T's acquisition and subsequent spinoff of NCR (Lys and Vincent 1995).[IA.8] To gauge the effectiveness of the Nexis divestiture search algorithm, I test it using the divestitures identified through SDC as well as the AT&T–NCR deal. I conclude that the algorithm performs as desired. For example, the very first article, when sorted by relevance, that Nexis returns for the AT&T-NCR search is titled "ATT completes completes NCR spin-off" (see Internet Appendix Figure IA.3).

The news search performs well even in the presence of name changes. Newspaper articles and news wires often reference former firm or business unit names, allowing me to accurately track acquisitions through time. For example, using again the IVX–Johnson divestiture as an illustrative example, The Atlanta Journal Contitution, reporting on the divestiture, added that added "Johnson ... was sold to Ivax Corp., now known as IVX, in 1993."

[IA.7] I exclude acquisitions in which the acquirer is a financial firm from the news search. This leaves deals in the sample in which a non-financial firm expands into the financial sector. I restrict the search to non-bank acquirers since bank names are oftentimes too similar (e.g. United Bank vs. United Community Bank), making name-based searches difficult. Additionally, excluding financial firms is common (e.g., Bernstein 2015, Weber 2018).

[IA.8] In fact, both AT&T's and NCR's CUSIPs in the acquisition and divestiture transaction differ in SDC. AT&T is included under CUSIPs 030177 and 001957. NCR is included under CUSIPs 628862 and 62886E.

Figure IA.3. Nexis Search Results for AT&T-NCR



Verifying Divestitures. The IVX–Johnson divestiture also illustrates the usefulness of SEC filings such as 10-Ks as well as Exhibit 21 (Subsidiaries of the registrant) in order to verify the correctness of a divestiture. IVX Bioscience's 10-K for fiscal year 1998 says "Effective July 14, 1998, IVAX sold Johnson Products Co. ... to Carson Products Company, a wholly-owned subsidiary of Carson, Inc., for approximately $84.7 million."[IA.9] In line with this, Johnson Products is still listed as a subsidiary in Exhibit 21 of IVX's 10-K from 1997 but no longer in that from 1998. Instead, it appears on the 1998 Carson Inc.'s subsidiaries list filed with their 1998 10-K.[IA.10]

As explained in the main text, I do not include partial divestitures in my sample to identify true de-commitments by acquirers to previously acquired businesses. An example of an excluded partial divestiture is that of Air Wisconsin (Air Wis) by United Airlines (UAL). While UAL sold Air Wis' fleet, it did not sell the landing slots acquired in the Air Wis deal, and the Wisconsin State Journal concluded that "UAL bought Air Wis in 1992 only to [retain] the valuable Air Wis landing slots at O'Hare."

Internet Appendix Table IA.II provides a step-by-step overview of the final divestiture sample construction from the initial sample of full divestitures.

[IA.9] Cf. sec.gov/Archives/edgar/data/772197/0000950144-99-003700.txt.

[IA.10] Cf. sec.gov/Archives/edgar/data/772197/0000950170-98-000591.txt and sec.gov/Archives/edgar/data/1019808/0001019808-99-000002.txt, respectively.

Table IA.II. Divestiture Sample Construction

This table presents an overview of the divestiture sample construction. See Sections II.A, II.B, and II.C for additional details. Transaction period refers to the period between two days after the merger agreement until the merger completion. *Fixed Shares* deals are acquisitions in which the transacting parties stipulate a fixed exchange ratio, i.e. a fixed number of acquirer shares to be exchanged in the acquisition.

	SDC	Nexis	Combined
Full divestitures	226	317	543
Observations remaining after removing			
Confounding events or otherwise unsuitable for identification (e.g., option to acquire, lawsuit about deal value, or MBO)	189	276	465
Imprecise or insufficient information about acquisition terms	172	244	416
Transaction period < 10 days	164	233	397
Incomplete data on control variables	160	210	370
Final Sample of Acquisitions Subsequently Divested	160	210	**370**
of which acquisition is a *Fixed Shares* deal	109	169	**279**

B. Additional Detail on Collection of Acquisition Terms (Section II.B)

Below are several examples of *Fixed Shares* and *Fixed Dollar* acquisitions from my sample. Note that all source links below need to be added to a valid Nexis URL "stub," which can vary depending on Nexis log-in options. Examples of "stubs" are: `https://advance.lexis.com/document` (on-campus) and `https://advance-lexis-com.libproxy.berkeley.edu/document/` (off-campus using VPN).

Example 1: Acquisition of Intirion by Mac-Gray (*Fixed Shares* deal)

Source: POS AM (post-effective amendment) filing

Link: `?pdmfid=%1516831&crid=db24f68d-b6fa-4058-baac-0c0a92996cee&pddocfullpath=2Fshared%2Fdocument%2Fcompany-financial%2Furn%3AcontentItem%3A4NPC-9FP0-TXDS-G2BS-00000-00&pddocid=urn%3AcontentItem%3A4NPC-9FP0-TXDS-G2BS-00000-00&pdcontentcomponentid=300324&pdteaserkey=sr0&pditab=allpods&ecomp=5ynk&earg=sr0&prid=29720526-77c1-4540-a629-08d3f6fa43b4`

> Agreement and Plan of Merger, dated as of December 22, 1997 ... RISK FACTORS RELATED TO THE MERGER Fixed Exchange Ratio Despite Potential Changes in Stock Price. The consideration being paid by Mac-Gray to acquire Intirion ... is fixed and will not be adjusted in the event of any increase or decrease in the price of Mac-Gray Common Stock ... the Closing Date will occur on the third business day following the satisfaction or waiver of the conditions to closing set forth in the Merger Agreement.

Example 2: Acquisition of Amrion by Whole Foods (*Fixed Shares* deal)

Source: Exhibit 2 to 10-Q filing

Link: `?pdmfid=1516831&crid=4ce8e681-f533-4af9-8fca-2b759c11f89c&pddocfullpath=%2Fshared%2Fdocument%2Fcompany-financial%2Furn%3AcontentItem%3A4NPS-MM00-TXDS-G315-00000-00&pddocid=urn%3AcontentItem%3A4NPS-MM00-TXDS-G315-00000-00&pdcontentcomponentid=300324&pdteaserkey=sr2&pditab=allpods&ecomp=1fyk&earg=sr2&prid=d2724e6b-d5c2-490e-8ab2-2fa8f3a23d87`

> This Agreement and Plan of Merger (the "Agreement" is made as of the 9th day of June, 1997, among Whole Foods Market, Inc., a Texas corporation ("WFM") ; Nutrient Acquisition Corp., a Colorado corporation (the "Merger Subsidiary"), which is wholly owned by WFM; ... and Amrion, Inc., a Colorado corporation ("Amrion") ... ARTICLE 2 ... 2.1. Conversion of Shares ... (a) Each share of common stock, $.0011 par value per share, of Amrion ("Amrion Common Stock") ... shall at the Effective Date, by virtue of the Merger and without any action on the part of the holder thereof, be converted into and represent the right to receive .87 shares of Common Stock, $.01 par value, of WFM (the "WFM Common Stock").

Example 3: Acquisition of Control Resources by P-COM (*Fixed Dollar* deal)

Source: Ex. 7(c)(2) to 8-K filing

Link: `?pdmfid=1516831&crid=09f1c3ca-d2c5-4495-a122-6c58f3f4bb88&pddocfullpath=%2Fshared%2Fdocument%2Fcompany-financial%2Furn%3AcontentItem%3A4NPY-YJR0-TXDS-G2CS-00000-00&pddocid=urn%3AcontentItem%3A4NPY-YJR0-TXDS-G2CS-00000-00&pdcontentcomponentid=300324&pdteaserkey=sr0&pditab=allpods&ecomp=1fyk&earg=sr0&prid=88e663c7-bfd3-44c6-9303-00f974634c58`

> THIS AGREEMENT AND PLAN OF REORGANIZATION, is dated as of April 14, 1997 ... The number of shares of P-Com Common Stock constituting the Aggregate Merger Consideration shall be equal to the number obtained by dividing (A) the amount of Twenty-Two Million Dollars ($22,000,000) by (B) the average closing sales price of the P-Com Common Stock ... for the thirty (30) consecutive trading days ending three (3) trading days prior to the Effective Time of the Merger.

Example 4: Acquisition of ResortQuest International by Gaylord Entertainment (*Fixed Shares* deal)

Source: Fair Disclosure Wire

Link: `?pdmfid=1516831&crid=1da340b0-4b42-4255-83b3-ad082acf7bfd&pddocfullpath=%2Fshared%2Fdocument%2Fnews%2Furn%3AcontentItem%3A497F-XV80-01GN-6541-00000-00&pddocid=urn%3AcontentItem%3A497F-XV80-01GN-6541-00000-00&pdcontentcomponentid=254610&pdteaserkey=sr0&pditab=allpods&ecomp=cy3k&earg=sr0&prid=d16ba6c0-0960-4186-ba47-05ab5b765e01`

> DAVID KLOEPPEL, CHIEF FINANCIAL OFFICER ... The transaction is structured ... as a stock for stock transaction ... in which each share of ResortQuest is exchanged for 0.275 of a Gaylord Entertainment share. This is a fixed exchange ratio with no caps or floors.

Example 5: Acquisition of HSB Group by American International Group (*Fixed Dollar* deal)

Source: The New York Times

Link: `?pdmfid=1516831&crid=f58defff-aa27-4d7e-a64c-a35627168ea4&pddocfullpath=%2Fshared%2Fdocument%2Fnews%2Furn%3AcontentItem%3A410S-5Y10-00MH-F1MP-00000-00&pddocid=urn%3AcontentItem%3A410S-5Y10-00MH-F1MP-00000-00&pdcontentcomponentid=6742&pdteaserkey=sr1&pditab=allpods&ecomp=1fyk&earg=sr1&prid=787d8f78-1311-47ba-b521-8592ea24299b`

> American International Group Inc., one of the world's largest insurers, agreed yesterday to acquire HSB Group Inc., parent of the venerable Hartford Steam Boiler Inspection and Insurance Company, for about $1.2 billion in stock. The deal will bolster A.I.G.'s range of products by adding several specialty insurance lines. Under the deal, A.I.G. will exchange $41 in stock for each HSB share.

As mentioned in the main paper, I am able to find the precise deal terms for 89% of deals in my divestiture sample. While this fraction is large both on its own and when compared to prior work, I note that relative comparisons are difficult. To my knowledge, Ahern and Sosyura (2014) is the only other paper that hand-collects merger deal terms and discusses sample attrition. Starting from a sample of 1,000 acquisitions, their final sample contains 507 deals. While they focus on larger and more recent deals for which deal specifics are more easily available, they require more deal information, including the date at which merger discussions began and availability of Factiva intelligent indexing codes.

C. Additional Detail on Matched Sample of Non-Divested Acquisitions (Section II.D)

Step 1: "Divestable" Acquisitions. As mentioned in the main text, an important requirement in case control designs is that control subjects are similarly *susceptible* to the outcome of interest (Grimes and Schulz 2005). To identify "divestable" acquisitions, I rely on the previous literature, which has documented a higher divestiture propensity among industry-diversifying acquisitions and out-of-state firm segments (Kaplan and Weisbach 1992, Landier et al. 2007). Both of these characteristics are also strong divestiture predictors in my general M&A sample. The odds of being divested are 115% higher for diversifying compared to same-industry acquisitions, and 34% higher for geo-diversifying compared to in-state acquisitions (Internet Appendix Table IA.VIII).

Step 2: Matching. Using the resulting set of non-divested acquisitions as the potential matches, I perform propensity score matching to find the acquisition that is most similar to a given divested *Fixed Shares* acquisition. The list of matching variables includes the target's industry, the deal value at merger agreement, acquirer size, public target status, and three-day cumulative announcement return (CAR), and thus uses all variables Internet Appendix Table IA.VIII identifies as divestiture predictors in the general M&A sample. In case control designs, it is crucial that the sampling of controls (i.e. non-divested acquisitions in my setting) occurs independent of the variable of interest (Grimes and Schulz 2005). Thus, as explained in the main text, I do *not* match on the experienced (endogenous or market-induced) cost change of the initial acquisition as the key variable I relate to the rate of divestiture in the empirical analysis.

Step 3: Collection of Acquisition Terms. For each matched acquisition, I again check whether this acquisition used a *Fixed Shares* structure. If so, I keep the observation in the sample. If not, I take the next-closest match from the previous step and repeat the deal term check, until I find a *Fixed Shares* match. For 66% of observations, the most similar matched acquisition used a *Fixed Shares* structure. For 95% of observations, the most similar, second-most similar, or third-most similar matched acquisition used a *Fixed Shares* structure.

Table IA.III. Balance Table

This table reports summary statistics separated by whether or not an acquisition is subsequently divested.

	Divested		Non-Divested		*p*-Value for Differences	
	Mean	Median	Mean	Median	*t*-test	Wilcoxon test
CAR (%)	−0.63	−0.88	0.04	−0.49	0.33	0.21
CAR < 0	0.54	1	0.54	1	1.00	1.00
Deal Value (ln)	4.84	4.69	4.85	4.53	0.89	0.74
Aquirer Size (ln)	6.53	6.60	6.33	6.20	0.19	0.36
Public Target	0.48	0	0.52	1	0.19	0.19
Beta	1.15	1.15	1.16	1.14	0.58	0.54
All-Stock Deal	0.59	1	0.53	1	0.11	0.11
Transaction Period	106	91	104	90	0.76	0.79

IV. Supplementary Results

A. Robustness of Main Result to Sample Restrictions and Alternative Specifications (Section IV.B)

This section summarizes several robustness tests to bolster the findings from Section IV.A, expanding on the discussion in Section IV.B. Unless otherwise specified, all robustness tests use the hazard model specification in Column (4) of Table III, allowing for linear time interactions of controls.

Panel A of Table IA.IV shows robustness to various sample restrictions. First, my results are robust to restricting to "pure" deals without collar clauses, retaining roughly 90% of the sample. Removing collar deals leads to a *larger* estimated effect of quasi-random acquisition cost changes on divestiture rates. Next, the results are also unchanged when restricting to acquisitions that use stock as the primary payment method, i.e. deals in which the share exchange should be particularly salient to the acquirer's management. The final column verifies that my results hold when excluding deals in which the period between merger agreement and completion is less than twenty days, i.e. when focusing on deals with a prolonged exposure to market fluctuations.

Panel B shows robustness to alternative specifications. The first column shows that my results are almost identical when adding a control for the length of the transaction period, i.e. the period during which acquisition cost changes unfold. The results are also robust to adding calendar year fixed effects (in addition to acquisition year fixed effects) to the specification. Next, I modify the construction of the main variable of interest, calculating the market-induced cost change without taking into account acquirers' sensitivity to market movements (i.e. setting $\beta = 1$ for all deals in Equation (1')). The results remains strongly significant with this simplification. In the final column, I use a logit instead of the hazard model, inspired by Efron (1988). In contrast to the hazard model, the logit model does not directly account for the passage of time, i.e. that divestiture frequencies will generally vary with time passed since the acquisition. Therefore, following Jenter and Kanaan (2015), I augment the specification with an explicit time control (years since acquisition). The coefficient of interest is very similar to that obtained when using the hazard models, and is also significant at 1%.

Panels C and D explore further robustness to the concern that changes in acquisition prices, even if induced by plausibly exogenous market movements, might affect the subsequent trajectory of acquired firms—for instance, by affecting investment constraints, synergies, and profitability—, which could itself matter for the decision to divest. In Panel C, the results are unchanged, both statistically and in terms of economic magnitude, when including as an additional control variable the acquirer industry's market-to-book ratio at acquisition (in addition to acquisition year fixed effects), to further account for potential business cycle effects (Sedláček and Sterk 2017). The results are similarly unchanged when including the acquirer's leverage as an additional time-varying control, and an indicator variable for lowest-quintile return performance (instead of the continuous 12-month return control in the main tables). In Panel D, the results are also unaffected with measures of post-acquisition financial constraints as additional controls, based on the Whited and Wu (2006) index, when adding an indicator variable for constrained firms as those in the top quartile of the index distribution (using a sample-wide or annual cutoff), or adding the continuous index.

Panel E estimates stratified Cox (1972) models, which admit different baseline hazards for observations with different values of the stratum variable. This constitutes a useful alternative way to control for covariates that potentially do not satisfy the proportional hazards assumption, in particular if their time dependence might take a complicated functional form (Kleinbaum 1998). I estimate stratified Cox (1972) models for all four categorical variables with a p-value of less than 0.15 in the Schoenfeld tests of Internet Appendix Tables IA.XI and IA.XII. Across all four models, the coefficient estimates and significance levels on the acquisition cost change variable remain unchanged.

Table IA.IV. Robustness Tests

This table reports robustness test results for the effect of quasi-random variation in acquisition costs on subsequent divestiture rates. Panel A presents results for various restricted samples. Panels B and C present alternative specifications. Panel D presents results for specifications with measures of financial constraints as additional control variables, based on the Whited and Wu (2006) (*WW*) index and using a sample-wide or annual constraint cutoff or the continuous WW index as indicated in the table. Panel E presents stratified Cox (1972) hazard models, admitting different baseline hazards for observations with different levels of the stratification variable. Across panels, all columns re-estimate the Cox (1972) hazard model in Column (4) of Table III, modified as indicated by the column headers, except for the final column in Panel B, which re-estimates Column (2) of Table III using a logit model (Efron 1988; Jenter and Kanaan 2015). Appendix A provides variable definitions. TP is short for Transaction Period. Please refer to Table III and Section IV.B for additional details. Table notes indicating the inclusion of control variables and fixed effects in all columns are omitted in the interest of brevity. 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: Sample Restrictions

	Excl. Collars	Majority-Stock	Transaction Period $\geq$ 20 Days
ΔC	-0.088^{***}	-0.077^{***}	-0.075^{***}
	(-3.48)	(-2.74)	(-3.08)
Time Interactions	Linear	Linear	Linear
N	503	442	536
N (Firm-Years)	4,018	3,566	4,348

Panel B: Alternative Specifications (I/II)

	Incl. TP Control	Incl. Year FE	$\Delta C^{\beta=1}$	Logit
ΔC	-0.077^{***}	-0.068^{***}	-0.092^{***}	-0.071^{***}
	(-3.17)	(-2.79)	(-3.92)	(-3.19)
Time Interactions	Linear	Linear	Linear	No
N	558	558	558	558
N (Firm-Years)	4,461	4,461	4,461	4,461

Table IA.IV. *Continued*

Panel C: Alternative Specifications (II/II)			
	Incl. Industry Mkt-to-Book Control	Incl. Leverage Control	Incl. Lowest-Quintile Perf. Control
ΔC	-0.077^{***}	-0.072^{***}	-0.079^{***}
	(-3.18)	(-2.72)	(-3.14)
Time Interactions	Linear	Linear	Linear
N	558	548	558
N (Firm-Years)	4,461	4,384	4,461

Panel D: Financial Constraints Controls			
	WW-Sample-Wide	WW-Annual	WW-Continuous
ΔC	-0.077^{***}	-0.080^{***}	-0.079^{***}
	(-2.84)	(-3.05)	(-2.98)
Time Interactions	Linear	Linear	Linear
N	548	548	548
N (Firm-Years)	4,242	4,242	4,242

Panel E: Stratified Cox (1972) Models				
	CAR	Public Target	All-Stock	Ind. Distress
ΔC	-0.078^{***}	-0.076^{***}	-0.075^{***}	-0.076^{***}
	(-3.24)	(-3.23)	(-3.16)	(-3.08)
Time Interactions	Linear	Linear	Linear	Linear
N	558	558	558	558
N (Firm-Years)	4,461	4,461	4,461	4,461

B. Additional Robustness

Table IA.V. Within-Divestiture Sample Placebo Tests—Post-Completion Market Fluctuations

This table reports placebo test results for the within-divestiture sample involving hypothetical acquisition cost changes from post-completion market fluctuations for divested *Fixed Shares* 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^{Hyp} is the hypothetical change in acquisition cost induced by post-completion market fluctuations in divested *Fixed Shares* acquisitions, 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. Appendix A 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}	0.030	0.024	0.030	0.024	0.024
	(0.96)	(0.83)	(0.96)	(0.83)	(0.91)
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

Panel B: Deal-Specific Post-Completion Window

	(1)	(2)	(3)	(4)	(5)
ΔC^{Hyp}	0.018	0.011	0.018	0.011	0.011
	(0.62)	(0.37)	(0.62)	(0.37)	(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	279	279	279	279	279
N (Firm-Years)	1,581	1,581	1,581	1,581	1,581

Table IA.VI. Alternative Clustering Schemes

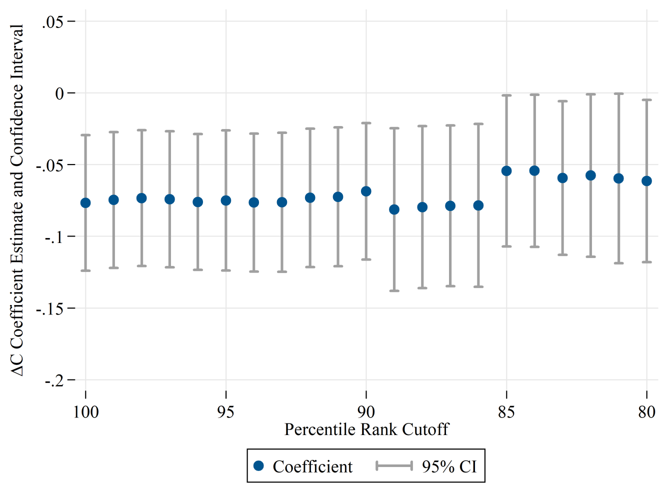
This table reproduces Table III, reporting estimates of the effect of quasi-random variation in acquisition costs on subsequent divestiture rates for the main sample, and reports z-statistics based on standard errors clustered at different levels. The standard error schemes include clustering by quarter of the acquisition (as in Table III) and clustering at various industry levels (four-digit SIC codes, three-digit SIC codes, and Fama and French (1997) 49 industries). Asterisks denoting significance are omitted.

	(1)	(2)	(3)	(4)	(5)
ΔC	−0.065	−0.068	−0.075	−0.077	−0.074
(Quarter)	(−2.77)	(−2.89)	(−3.05)	(−3.18)	(−3.14)
(SIC-4)	(−2.70)	(−2.91)	(−2.99)	(−3.20)	(−3.18)
(SIC-3)	(−2.59)	(−2.80)	(−2.92)	(−3.13)	(−3.09)
(Fama-French-49)	(−2.41)	(−2.49)	(−2.72)	(−2.81)	(−2.82)
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

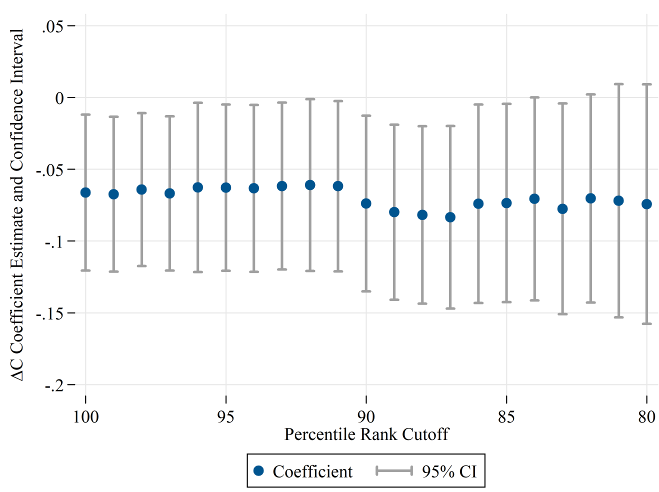
Figure IA.4. Removing Observations With High Acquisition Withdrawal Probabilities

This figure shows the evolution of the hazard regression coefficient on ΔC when successively removing observations with the highest estimated acquisition withdrawal probabilities. Panel A narrows the main sample and re-estimates the Cox (1972) hazard model in Column (4) of Table III. Panel B narrows the within-divestiture sample and re-estimates the Cox (1972) hazard model in Column (4) of Table V. *Percentile Rank Cutoff* indicates the cutoff percentile of the acquisition withdrawal probability distribution for remaining included in the estimation. To estimate withdrawal probabilities, I augment the final M&A sample detailed in Internet Appendix Table IA.I with a similarly constructed sample of withdrawn acquisitions obtained through SDC (applying the 'Status: Withdrawn' filter). I then estimate an OLS regression of an indicator variable for the acquisition being withdrawn on the $CAR < 0$ indicator, deal value (ln), acquirer size (ln), diversifying and geo-diversifying deal indicators, public target indicator, beta, all-stock indicator, Fama and French (1997) 49-industries acquirer and target fixed effects, and acquisition announcement month fixed effects ($N = 8,705$). The estimated withdrawal probability is the predicted value from this regression.

(a) Main Sample



(b) Within-Divestiture Sample



C. Two-Stage Control Function Approach

This section discusses the approach and results of the alternative, two-step estimation method, implemented using the residual inclusion method (control function method). I implement this approach for the main sample of divested acquisitions and similar non-divested acquisitions, i.e. the sample on which the main result in Table III is based.

General Approach. In the first stage, I regress the endogenous acquisition cost change, ΔC^{Acq}, on the plausibly exogenous, market-induced change, ΔC, as well as fixed effects and controls as included in the main model presented in Table III (and as included in the second stage of the approach implemented here).

$$\Delta C^{Acq}_{i,t} = a + b\,\Delta C_{i,t} + \boldsymbol{c}'\boldsymbol{X}_{i,t} + \nu_{j(Acq)} + \nu_{j(Tar)} + \mu_{t_0} + u_{i,t} \qquad \text{(First Stage)}$$

I estimate a coefficient of $\hat{b} = 0.648$, which is strongly significant (t-stat= 3.53, F-stat= 12.47; regression table omitted for brevity). The estimated coefficient is very similar to that when running the above First Stage regression on the larger general M&A sample; here, I obtain $\hat{b} = 0.650$ (t-stat= 10.21, F-stat= 104.15).

In the second stage, I again estimate a hazard model, now using the endogenous acquisition cost change as the main explanatory variable, together with the residual from the First Stage regression to control for the endogeneity in the system. This approach corresponds to the standard control function method appropriate when the second stage is a nonlinear model (cf. Wooldridge 2015).

$$\Pr\left(\text{Divestiture}_{i,t}\right) = \alpha + \kappa\,\Delta C^{Acq}_{i,t} + \boldsymbol{\delta}'\boldsymbol{X}_{i,t} + \delta_2 \hat{u}_{i,t} + \nu_{j(Acq)} + \nu_{j(Tar)} + \mu_{t_0} + \varepsilon_{i,t} \quad \text{(Second Stage)}$$

Hypothesis Testing. Since the two-step approach outlined above entails a generated regressor ($\hat{u}_{i,t}$), statistical inference based on the Second Stage standard errors is invalid. Therefore, I use bootstrap based inference, bootstrapping the outlined two-step approach using the block bootstrap method (one block refers to one acquisition year-quarter) and using 500 iterations. I then follow the procedure suggested by Kline (2016) for hypothesis testing. He considers tests based on the test statistic $T(\kappa) = \frac{\hat{\kappa}-\kappa}{\hat{\sigma}}$ that reject when $|T(\kappa_0)| > c$ to test the null hypothesis $H_0 : \kappa = \kappa_0$ against the alternative hypothesis $H_a : \kappa \neq \kappa_0$ at level α. Thus, we need to find c such that $\Pr(|T(\kappa_0)| > c) = \alpha$. The method advocated by Kline (2016) proceeds as follows:

- in each bootstrap sample b, compute $T^{(b)}(\kappa) = \frac{\hat{\kappa}^{(b)}-\kappa}{\hat{\sigma}^{(b)}}$
- use the $1-\alpha$ quantile of $|T^{(b)}(\hat{\kappa})|$ as the bootstrap estimate of c (note that the bootstrap test statistics are computed at $\hat{\kappa}$, i.e. at the full sample coefficient estimate).

Two-Stage Estimation Results. Table IA.VII presents the second-stage results. The results corroborate those presented in the main paper. The coefficient of interest, the coefficient on ΔC^{Acq}, remains negative and strongly statistically significant. Moreover, it implies a similar economic magnitude of the effect of sunk costs on divestiture rates compared to that estimated in the main tables.

Table IA.VII. Two-Stage Control Function Results

This table reports the results of the Second Stage of the control function estimation approach. The dependent variable is an indicator variable that equals one in the year in which an acquired business is divested and zero otherwise. ΔC^{Acq} is the endogenous change in acquisition cost between merger agreement and completion induced by the acquirer's stock price fluctuations, as a percentage of the acquirer's pre-acquisition market capitalization (see Equation (1)). *Residual* is the residual from the First Stage of the control function estimation approach. The inclusion of control variables, time interactions, and fixed effects in Column (1) is identical to that in Column (2) of Table III. Column (2) corresponds to Column (4) of Table III. Appendix A provides variable definitions. z-statistics (based on uncorrected standard errors clustered by acquisition year-quarter) are shown in parentheses. Critical values (for $\alpha = 0.05$) are calculated using the approach advocated by Kline (2016) and as described on page 23, and are shown in brackets next to the z-statistics. A coefficient is significant at the five percent level based on the method by Kline (2016) if the absolute value of the z-statistic exceeds the critical value next to it. Asterisks denoting significance are omitted.

	(1)		(2)	
ΔC^{Acq}	−0.099		−0.114	
	(−2.80)	[2.50]	(−3.09)	[2.73]
CAR < 0	0.171		0.298	
	(0.85)	[2.26]	(1.22)	[1.82]
Deal Value (ln)	0.028		−0.044	
	(0.43)	[2.50]	(−0.53)	[2.66]
Aquirer Size (ln)	−0.109		−0.156	
	(−1.59)	[2.17]	(−1.91)	[2.12]
Public Target	−0.145		−0.296	
	(−0.82)	[2.24]	(−1.45)	[2.12]
Beta	−0.008		0.192	
	(−0.03)	[1.94]	(0.62)	[1.89]
All-Stock Deal	0.108		0.144	
	(0.64)	[2.07]	(0.58)	[1.74]
12-Month Return	−0.515		−0.521	
	(−3.41)	[1.75]	(−3.37)	[1.85]
Industry Distress	0.395		0.475	
	(2.73)	[1.87]	(2.37)	[1.95]
Residual	0.130		0.145	
	(3.61)	[2.34]	(3.85)	[2.54]
Time Interactions	No		Linear	
Industry FE	Yes		Yes	
Acquisition Year FE	Yes		Yes	
N	558		558	
N (Firm-Years)	4,461		4,461	

D. Additional Tables and Figures

Table IA.VIII. Divestiture Predictors

This table reports results of a logit regression to identify deal and firm characteristics in acquisitions that are predictive of subsequent divestiture. The sample is based on the general M&A sample (see Section III.A of the Internet Appendix), disregarding partial divestitures and divestitures after an acquirer has itself been acquired, and restricting to observations with a transaction period of at least 10 days. The dependent variable is an indicator variable that equals one if an acquisition is divested and zero otherwise. Appendix A provides variable definitions. All columns report log-odds ratios. The regression includes 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)
CAR < 0	0.203*
	(1.73)
Deal Value (ln)	0.106**
	(2.15)
Aquirer Size (ln)	0.059
	(1.23)
Diversifying Deal	0.765***
	(6.08)
Geo-Diversifying Deal	0.296**
	(2.24)
Public Target	0.352***
	(2.65)
Beta	0.142
	(0.59)
All-Stock Deal	0.050
	(0.43)
Industry FE	Yes
Acquisition Year FE	Yes
N	6,458
Pseudo *R*-squared	0.14

Table IA.IX. Fixed Shares vs. Fixed Dollar Deals

This table reports summary statistics for the final divestiture sample from Section II.C separated by whether an acquisition uses a *Fixed Shares* or *Fixed Dollar* structure.

	Fixed Shares		Fixed Dollar		*p*-Value for Differences	
	Mean	Median	Mean	Median	*t*-test	Wilcoxon test
Stock Return Volatility	3.84	3.36	3.33	2.91	0.07	0.03
CAR (%)	−0.63	−0.88	0.49	−0.01	0.38	0.10
CAR < 0	0.54	1	0.51	1	0.52	0.51
Deal Value (ln)	4.84	4.69	4.48	4.53	0.16	0.22
Aquirer Size (ln)	6.53	6.60	7.01	7.35	0.09	0.06
Diversifying Deal	0.63	1	0.67	1	0.46	0.46
Geo-Diversifying Deal	0.77	1	0.75	1	0.60	0.60
Public Target	0.48	0	0.42	0	0.27	0.27
Beta	1.15	1.15	1.12	1.12	0.48	0.50
All-Stock Deal	0.59	1	0.43	0	0.01	0.01
Transaction Period	106	91	100	83	0.56	0.22
Years Until Divestiture	4.70	3.37	5.01	4.00	0.54	0.07

Figure IA.5. Market Fluctuations and Acquisition Withdrawals

This figure shows a binned scatterplot of the fraction of withdrawn acquisitions, sorting observations into equal-sized group based on the market return between merger agreement and completion or withdrawal (ΔR, see Equation (2')). The sample is the final M&A sample detailed in Internet Appendix Table IA.I augmented with a similarly constructed sample of withdrawn acquisitions obtained through SDC (applying the 'Status: Withdrawn' filter), for which all variables listed in Internet Appendix Figure IA.4 are available ($N = 8,705$).

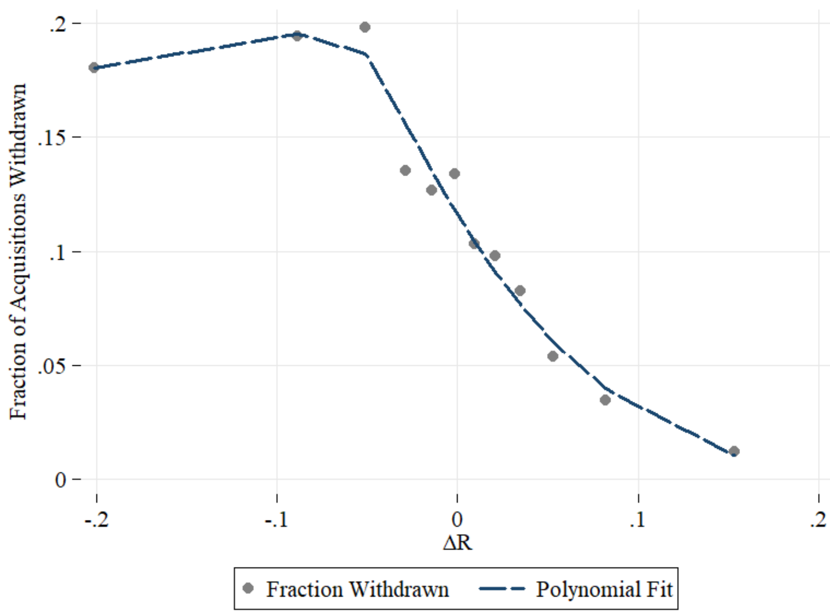


Figure IA.6. Fixed Shares vs. Fixed Dollar Deals: Acquisitions Over Time

This figure shows frequency distributions of acquisitions over time, comparing divested *Fixed Shares* and *Fixed Dollar* acquisitions.

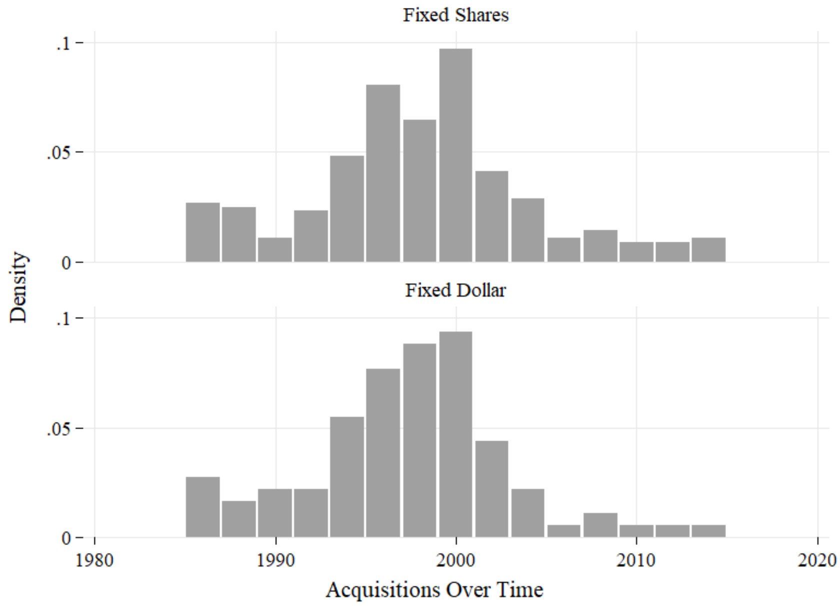


Figure IA.7. Sunk Cost Period

This figure shows the distribution of the length between estimated and actual divestiture announcement date (the sunk cost period). Please refer to Section V.D for additional details on the construction of the sunk cost period.

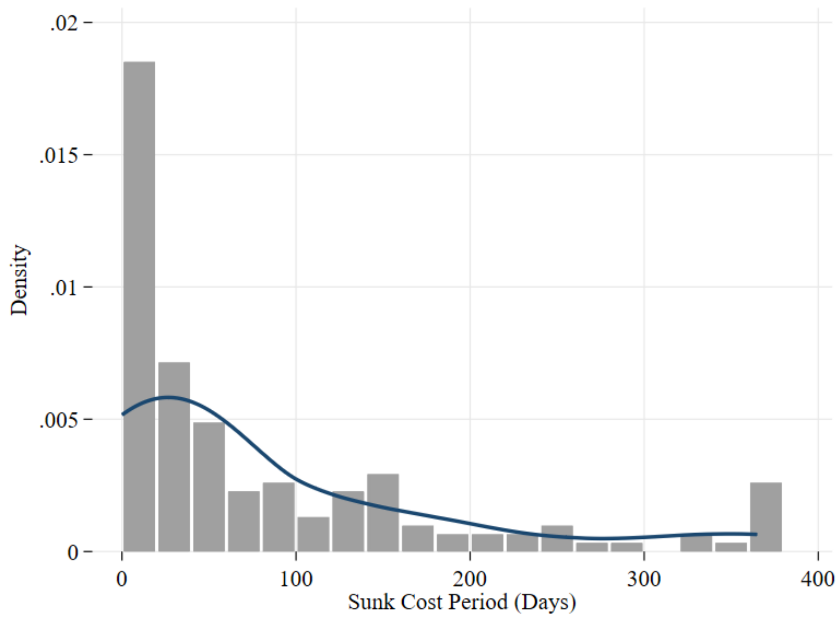


Figure IA.8. Excess Returns by Magnitude of Acquisition Cost Changes

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), split by the magnitude of the market-induced acquisition cost change ΔC. The estimated divestiture announcement date is calculated assuming a scenario in which the acquirer faced no cost shock instead of a cost increase, holding fixed all other characteristics. The figure normalizes the sunk cost period to 1 and plots relative time (between 0% and 100%) passed between the estimated and actual divestiture announcement date. Please refer to Section V.D for additional details.

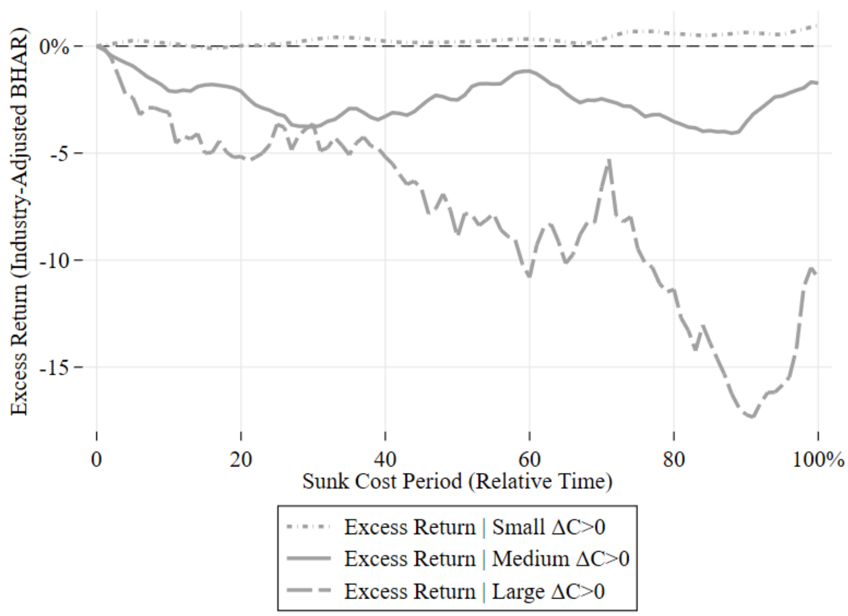


Figure IA.9. Divestiture Announcement Returns

This figure shows average divestiture announcement returns separated by whether the acquirer faced an acquisition cost decrease ($\Delta C < 0$) in the initial acquisition, or a small, medium, or large cost increase ($\Delta C > 0$) sorting observations into cost increase terciles. Panel (a) shows average cumulative abnormal returns (CARs) during the $[-1, +1]$ window around the divestiture announcement date. Panel (b) shows average CARs during the $[-5, +5]$ window around the divestiture announcement date.

(a) CAR$[-1, +1]$ **(b)** CAR$[-5, +5]$

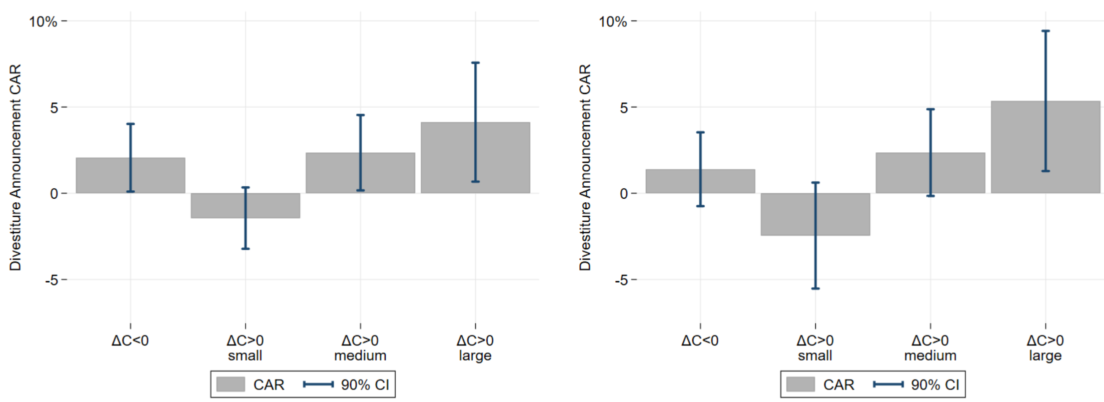
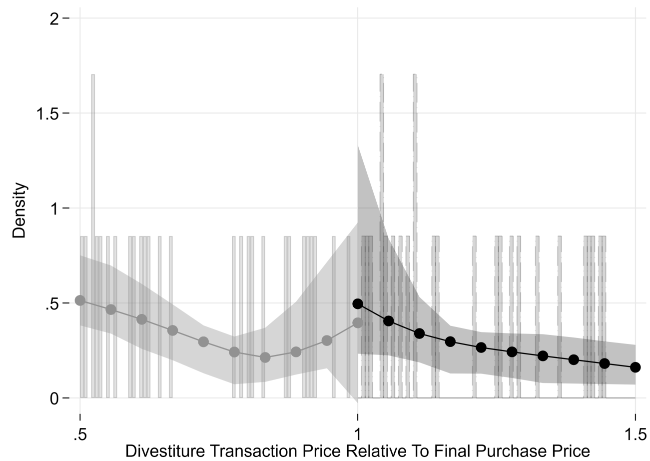


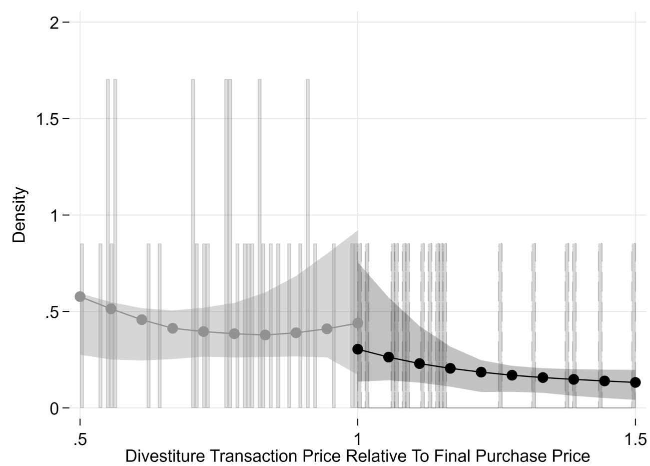
Figure IA.10. Divestiture Transaction Price Relative To Final Purchase Price

This figure shows the distribution of divestiture transaction prices relative to the final price of the initial acquisition. Panel A compares raw transaction prices. Panel B compares the divestiture price relative to the acquisition price adjusted by plus five percent per annum based on the time between merger completion and divestiture announcement. Both panels also show local quadratic density approximations (Cattaneo et al. 2018, 2020) to the left and right of the 100% cutoff (i.e. a divestiture occurring at the same price as the initial acquisition, in Panel B accounting for the time value of money). Both panels plot observations with divestiture prices within 50% of the final acquisition price.[IA.11]

(a) Raw Transaction Prices



(b) Transaction Prices Adjusted for Time Value of Money



[IA.11] The results on the effect of sunk acquisition cost changes on divestiture rates remain entirely unchanged, economically and statistically and in both the main sample (Table III) and the within-divestiture sample (Table V), when excluding the four observations near the cutoff in Panel A or Panel B of this figure.

V. Case Control Sampling

A. Econometric Discussion

This appendix presents the rationale for the equivalence of case control and standard sampling in terms of the parameter estimates of interest for the econometric models relevant to this paper.[IA.12] To convey the key arguments in the most straightforward way possible, I focus the discussion on the logit model, i.e. abstracting from the duration aspect of the hazard model. That said, as established in Prentice and Breslow (1978) and reiterated in Schlesselman (1982), the proportional hazards model "is also applicable to the analysis of case-control studies" (Schlesselman 1982, p. 230). (Also, recall the robustness check in Panel B of Table IA.IV based on a logit model, replicating the hazard-based results both qualitatively and quantitatively speaking.)

Applied to my setting, case control sampling involves assembling a sample of divested deals ("cases") and non-divested deals ("controls") with sampling fractions π_1 and π_2, respectively. I first present the baseline equivalence argument and then discuss straightforward extensions relevant to my setting.

Baseline Argument. The baseline argument involves drawing a random sample of cases and controls. For ease of notation, let $y \equiv Divestiture \in \{0,1\}$ and $x_C \equiv \Delta C$. Suppose that the probability of divestiture depends on a set of variables $x = (x_C, x_1, ..., x_p)$ according to the logistical model:

$$p_x = \Pr(y=1|x) = 1/\left(1+\exp\left(-\left(\beta_0+\beta_C x_C+\beta_1 x_1+...\beta_p x_p\right)\right)\right). \quad \text{(IA.5)}$$

Expressed in log odds, we have

$$\ln p_x/q_x = \beta_0+\beta_C x_C+\beta_1 x_1+...+\beta_p x_p \quad \text{(IA.6)}$$

where $q_x = 1-p_x = \Pr(y=0|x)$.

For a given observation, there are four potential outcomes:

(i) the observation is divested and is in the sample, which occurs with probability $\pi_1 p_x$
(ii) the observation is divested and is not in the sample, which occurs with probability $(1-\pi_1)p_x$
(iii) the observation is not divested and is in the sample, which occurs with probability $\pi_2 q_x$
(iv) the observation is not divested and is not in the sample, which occurs with probability $(1-\pi_2)q_x$.

Thus, the probability of divestiture in the case control sample is

$$p'_x = \pi_1 p_x/\left(\pi_1 p_x+\pi_2 q_x\right) \quad \text{(IA.7)}$$

and the odds of divestiture in this sample is

$$p'_x/q'_x = \pi_1 p_x/\pi_2 q_x \quad \text{(IA.8)}$$

where $q'_x = 1-p'_x$.

Using Equation (IA.8) in combination with Equation (IA.6), it follows that the log odds of

IA.12 The discussion is based on Schlesselman (1982, p. 235–236).

divestiture in the case control sample is given by

$$\begin{aligned} \ln p'_x/q'_x &= \ln \pi_1 p_x/\pi_2 q_x \\ &= \ln \pi_1/\pi_2 + \ln p_x/q_x \\ &= \beta'_0 + \beta_C x_C + \beta_1 x_1 + ... + \beta_p x_p \end{aligned} \quad \text{(IA.9)}$$

where $\beta'_0 = \ln \pi_1/\pi_2 + \beta_0$. The last equality in Equation (IA.9) shows that with case control sampling, the logistic parameters $(\beta_C, \beta_1, ..., \beta_p)$, and thus in particular the parameter of interest β_C, are unaffected and their interpretation is the same as with standard sampling.

Extensions. First, as discussed in Section II.A and Appendix-Section III.A, I assemble a comprehensive sample of divested acquisitions (cases). As a result, the sampling fraction of cases in my sample is $\pi_1 = 1$. Second, as discussed in Section II.D and Appendix-Section III.C, rather than drawing a random sample of non-divested deals, I take matched non-divested deals that are similar to divested deals in terms of a set of standard firm and deal observables. Sampling controls using a matching approach is common in case control designs and helps to ensure that controls are "similar in all important respects to cases" (Schlesselman 1982; Grimes and Schulz 2005, p. 1432). As discussed in the above-mentioned sections, I do *not* use the experienced acquisition cost change to identify similar deals. Consequently, the sampling fraction of controls π_2 will generally depend on an arbitrary subset of $(x_1, ..., x_p)$ from Equation (IA.5), either because the firm and deal characteristics are directly part of $(x_1, ..., x_p)$ or because they are correlated with (some of) these variables. Without loss of generality, suppose π_2 depends on $(x_l, ..., x_p) \subseteq (x_1, ..., x_p)$ with $l \geq 1$, i.e. $\pi_2 = \pi_2(x_l, ..., x_p)$. Importantly, the key continued assumption is that post-agreement market fluctuations are "as good as randomly assigned" (cf. Section III.C), such that controls are sampled randomly with respect to market-induced acquisition cost changes and π_2 does not depend on x_C. (Recall that in favor of the assumption of as-good-as-random assignment, Panel B of Table II shows that market fluctuations are not predicted by a large array of firm and deal characteristics, including all characteristics used to identify similar divested and non-divested deals.)

With these two modifications to the sampling of cases and controls, the probability of divestiture in my case-control-sample becomes

$$p''_x = p_x/\left(p_x + \pi_2(x_l, ..., x_p) q_x\right) \quad \text{(IA.10)}$$

and the odds of divestiture is

$$p''_x/q''_x = p_x/\pi_2(x_l, ..., x_p) q_x \quad \text{(IA.11)}$$

where $q''_x = 1 - p''_x$.

Combining Equation (IA.11) with Equation (IA.6), we then have that

$$\begin{aligned} \ln p''_x/q''_x &= \ln p_x/\pi_2(x_l, ..., x_p) q_x \\ &= -\ln \pi_2(x_l, ..., x_p) + \ln p_x/q_x \\ &= -\ln \pi_2(x_l, ..., x_p) + \beta_0 + \beta_C x_C + \beta_1 x_1 + ... + \beta_p x_p \end{aligned} \quad \text{(IA.12)}$$

Thus, as in the baseline case, the parameter of interest, the coefficient β_C on x_C, as well as any other coefficients on variables x_j on which π_2 does not depend, remain unaffected and their interpretation is the same as with standard sampling.

B. Empirical Robustness to Standard Sampling

This section presents robustness of the effect of quasi-random variation in acquisition costs on subsequent divestiture rates to standard sampling, corroborating the results based on case control sampling (Table III) and those based on the sub-sample of divested acquisitions only (Table V). I present robustness to two different samples that are assembled based on standard sampling. Both samples are comprised of both divested and non-divested acquisitions and do *not* involve matching of divested to non-divested deals.

Sample Construction. To construct the *main standard sample*, I start from the universe of "divestable" acquisitions, i.e. those that industry-diversifying and involve out-of-state target firms (cf. Section II.D), that are at least partially financed with stock and have a transaction period (the period between two days after the date of the merger agreement and the merger completion date) of at least ten days. To construct the *broad standard sample*, I apply the same deal criteria but keep same-state acquisitions (Appendix-Table IA.VIII shows that an acquisition being industry-diversifying is the most significant predictor of subsequent divestiture).

Since the collection of precise exchange terms for each deal in these significantly larger samples is infeasible ($N > 2,000$ and $N > 3,000$ acquisitions, respectively), I exploit the fact that the acquirer's stock price volatility prior to deal agreement is a key determinant of the choice between a *Fixed Shares* and *Fixed Dollar* deal structure (cf. Appendix-Table IA.IX and Ahern and Sosyura 2014). In Appendix-Table IA.IX, the odds of a *Fixed Shares* deal increase by a factor of 1.6 comparing acquisitions with above- and below-median stock price volatility. Specifically, I restrict to acquisitions with above-median stock price volatility as a proxy for deals with a *Fixed Shares* exchange structure. Doing so, any mis-inclusion of *Fixed Dollar* deals will likely push the coefficient of interest, ΔC, towards zero, given that the association between hypothetical acquisition cost changes and subsequent divestiture rates in *Fixed Dollar* deals is insignificant in Panel C of Table VI.[IA.13] To reduce the likelihood of mis-including *Fixed Dollar* acquisitions in the estimation, I drop all deals for which I know from the collection of deal terms in Section II.B that they used a *Fixed Dollar* structure. Finally, for a few divested acquisitions, the stock price volatility measure based on the preferred definition, the 80-to-200 trading day window prior to acquisition announcement date as described in Appendix A, is not available because of missing data in CRSP, in which case I calculate the measure based on alternative 120 trading day windows closer to the announcement date. After these steps, the resulting main and broad standard samples are comprised of $6,903$ and $10,741$ firm-year observations from 773 and $1,175$ acquisitions, respectively.

Estimation. For both of these standard samples, I then re-estimate the main empirical specification, relating post-agreement acquisition cost changes to subsequent divestiture rates while controlling for firm and deal characteristics, acquisition year fixed effects, and industry fixed effects (Equation (3)), and using the Cox (1972) proportional hazards model. As in the main analysis,

[IA.13] To recap, in Panel C of Table VI, the coefficient on hypothetical acquisition cost changes in *Fixed Dollar* deals is insignificantly positive, as opposed to the significantly negative coefficient on truly experienced cost changes in *Fixed Shares* deals.

I again allow for interactions of variables with linear or log event time (years since acquisition). In Appendix-Table IA.X, I also allow for time interactions with ΔC as the Schoenfeld residuals are up to 2.5 times as strongly correlated with event time compared to the case control sample (Appendix-Table IA.XI), in addition to interactions with all other variables previously interacted with event time.

Results. Appendix-Table IA.X presents the results, both for the main standard sample in Columns (1) and (2) and the broad standard sample in Columns (3) and (4). Across all columns, the results are very similar to those in the main paper. Upon deal completion, the propensity to make a divestiture is negatively related to the market-induced acquisition cost change acquirers experienced between merger agreement and completion. The economic magnitudes are very similar to before. An interquartile increase in post-agreement acquisition costs (based on Table I) is estimated to reduce divestiture rates by between 7.2% and 8.9% upon deal completion, compared to estimated effect sizes between 8.0% and 9.4% in Table III. As to be expected given the discussion on the usefulness of case control sampling in terms of power with rare outcomes (Section II.D), the statistical significance in Appendix-Table IA.X is slightly weaker (the ratio of divested to non-divested acquisitions in Appendix-Table IA.X is 1:10 and 1:11, respectively, compared to 1:1 in the case control sample). Still, the coefficient of interest remains around the threshold of being significant at 10% in Columns (2) and (3) (p-values of 0.083 and 0.102, respectively) and remains significant at 5% in Column (4).

Overall, the standard sampling results based on two different regularly constructed samples corroborate the findings in the main part of the paper on the relation between market-induced acquisition cost changes and divestiture rates of acquired businesses.

Table IA.X. Standard Sampling Results

This table reports estimates of the effect of quasi-random variation in acquisition costs on subsequent divestiture rates for an alternative sample of acquisitions, comprised of both divested and non-divested acquisitions and assembled using standard sampling as opposed to case control sampling as detailed in Section V.B of this Internet Appendix. 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')). Appendix A provides variable definitions. All columns are estimated using the Cox (1972) proportional hazards model and show regression coefficients, not hazard ratios. Control variables, time interactions, and fixed effects are included as indicated in the table notes. Please refer to Section V.B of this Internet Appendix for additional details. z-statistics are shown in parentheses. Standard errors are clustered by quarter of the acquisition. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Main Standard Sample		Broad Standard Sample	
	(1)	(2)	(3)	(4)
ΔC	−0.070	−0.073*	−0.058	−0.065**
	(−1.17)	(−1.73)	(−1.64)	(−1.98)
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	773	773	1,175	1,175
N (Firm-Years)	6,903	6,903	10,741	10,741

VI. Testing for Proportional Hazards in the Cox (1972) Model

This appendix contains a description of how to test for proportional hazards in the Cox (1972) model using Schoenfeld (1982) residuals and provides the results of the proportional hazards tests.[IA.14]

Construction of Schoenfeld Residuals. The Cox (1972) model assumes that the effect of covariates on the hazard rate is constant across time.[IA.15] Schoenfeld residuals can be used to assess, for any given covariate included in the hazard model, whether this assumption of proportionality might be violated. Loosely speaking, Schoenfeld residuals are derived at each failure time from differences in covariate values of observations that fail and those that still remain at risk; the proportional hazards assumption implies that these residuals are uncorrelated with event time (i.e., time since acquisition in my setting).

Formally, the Schoenfeld residual $r_{i,s,k}$ for covariate k and observation i that fails at time t_s is the covariate value $x_{i,k}$ of that observation minus a weighted average of the covariate values across all observations that remain at risk at t_s, where the weights are proportional to each observation's likelihood of failure at time t_s. The covariate-specific Schoenfeld residual $r_{s,k}$ corresponding to failure time t_s is then the sum of all residuals $r_{i,s,k}$ of observations that fail at time t_s.

Proportional Hazards Tests Based on Schoenfeld Residuals. Plotting the $r_{s,k}$ values[IA.16] across failure times against a chosen function of time reveals how the coefficient associated with covariate k varies with time. If the smoothed curve through the plotted points is flat, this indicates that the proportionality assumption for covariate k is likely satisfied.

Formally, one can test the proportional hazards assumption based on the slope of the linear regression through the scaled Schoenfeld residuals plotted against time. For covariate k, the slope of the regression line through the is $\hat{\theta}_k = \dfrac{\sum_{s=1}^{D}(t_s-\bar{t})\left(r_{s,k}^{scaled}-\bar{r}_k^{scaled}\right)}{\sum_{s=1}^{D}(t_s-\bar{t})^2} = \dfrac{\sum_{s=1}^{D}(t_s-\bar{t})\,r_{s,k}^{scaled}}{\sum_{s=1}^{D}(t_s-\bar{t})^2}$ where, following the notation above, s indexes ordered failure times t_s, $s \in \{1,...,D\}$, and $r_{s,k}^{scaled}$ denotes the sum scaled Schoenfeld residuals for covariate k across all observations that fail at time t_s. $\bar{t}$ and $\bar{r}$ denote the means of t_s and r_s, respectively. The second equality holds since, by definition, $\sum_{s=1}^{D} r_{s,k} = 0$. The test statistic for the proportional hazards assumption with respect to the kth covariate is $T_k(\hat{\theta}) = \dfrac{\hat{\theta}_k^2}{\text{Var}\left(\hat{\theta}_k\right)}$, which is asymptotically $\chi^2(1)$-distributed under the null hypothesis of proportional hazards. ρ_k is the Pearson correlation coefficient between the scaled Schoenfeld residuals for covariate k and time.

Schoenfeld Results. As summarized in the main text, the results in Internet Appendix Tables

[IA.14] Some of the discussion of Schoenfeld residuals is based on material by Dan Dillen, available at ics.uci.edu/dgillen/STAT255/Handouts/lecture10.pdf.

[IA.15] Dividing the hazard function of Equation (4) for two observations i and i' by one another, one obtains $\frac{h(t|\mathbf{X}_i)}{h(t|\mathbf{X}_{i'})} = \frac{\exp(\delta'\mathbf{X}_i)}{\exp\left(\delta'\mathbf{X}_{i'}\right)}$, which is independent of time.

[IA.16] To be precise, one uses a scaled version of these values, weighted by the inverse of the covariance matrix of $\hat{\beta}$.

IA.XI and IA.XII show that there is no indication that the proportional hazards assumption might be violated for the main variable of interest. This conclusion is corroborated in further robustness tests in which I perform the Schoenfeld test examining the correlation with log-time instead of linear time. In the test using the main sample, the *p*-value for the correlation of market-induced cost change with log-time remains basically unchanged (p=0.32), and in the test using the divested sample it further increases (p=0.98).

The control variables included in Table III that have a *p*-value of 0.15 or less in Internet Appendix Table IA.XI or IA.XII, and are thus allowed to depend on time in the hazard regressions with time interactions, are: the indicator for whether the market reaction to the deal was negative, the deal value at agreement, the acquirer's size and beta, and the indicators for target public status, all-stock deal, and industry distress of the acquired business.

Table IA.XI. Testing for Proportional Hazards (Main Sample)

This table reports the results of the formal test for proportional hazards based on scaled Schoenfeld residuals for the main sample. The specification used for the test corresponds to Column (2) of Table III. The definitions of ρ and T are provided on page IA.38. Appendix A provides variable definitions.

	ρ	T	p-Value
ΔC	−0.040	0.84	0.36
CAR < 0	−0.026	0.74	0.39
Deal Value (ln)	0.053	2.28	0.13
Aquirer Size (ln)	0.020	0.35	0.55
Public Target	0.035	0.91	0.34
All-Stock Deal	−0.029	0.47	0.49
Beta	0.061	2.62	0.11
12-Month Return	0.021	0.54	0.46
Industry Distress	0.122	12.93	0.00

As described above, another useful visual Schoenfeld test is to plot the Schoenfeld residuals against a function of time. Internet Appendix Figure IA.11 does this, using linear time, for the main variable of interest, the market-induced acquisition cost change, and for the $CAR < 0$ indicator, the variable with largest time dependence (p-value of < 0.01) in Internet Appendix Table IA.XII. For the cost change variable in Panel IA.11a, the smoothed line through the Schoenfeld residuals over time is almost perfectly flat. This visual check confirms the *lack of* time dependence of the main variable of interest. For the $CAR < 0$ indicator in Panel IA.11b, instead, the smoothed line fluctuates over time, supporting the inclusion of time interactions for this variable.

Table IA.XII. Testing for Proportional Hazards (Within-Divestiture Sample)

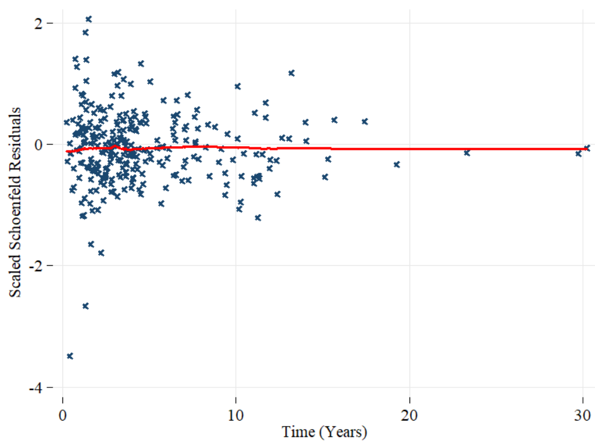
This table reports the results of the formal test for proportional hazards based on scaled Schoenfeld residuals for the within-divestiture sample. The specification used for the test corresponds to Column (2) of Table V. The definitions of ρ and T are provided on page IA.38. Appendix A provides variable definitions.

	ρ	T	p-Value
ΔC	−0.019	0.18	0.67
CAR < 0	−0.122	8.49	0.00
Deal Value (ln)	0.062	2.33	0.13
Aquirer Size (ln)	−0.065	2.51	0.11
Diversifying Deal	−0.037	0.77	0.38
Geo-Diversifying Deal	−0.066	3.12	0.08
Public Target	0.111	6.33	0.01
All-Stock Deal	0.110	6.49	0.01
Beta	−0.039	1.28	0.26
12-Month Return	0.003	0.01	0.94
Industry Distress	0.006	0.02	0.88

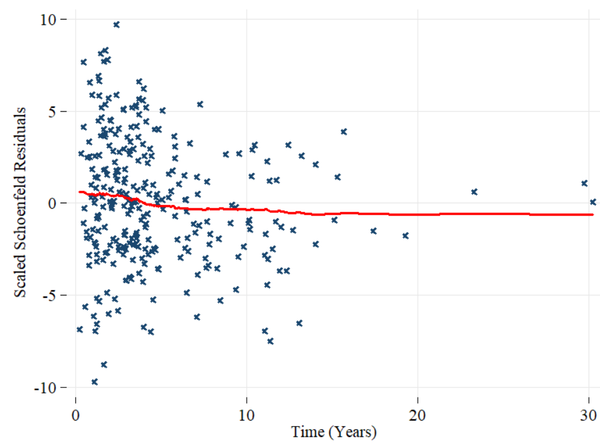
Figure IA.11. Schoenfeld Residuals Against Time

This figure shows plots of scaled Schoenfeld residuals against time (linear time in years). Panel (a) plots the residuals for ΔC, 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')). Panel (b) plots the residuals for the indicator variable identifying acquisitions with a negative stock market reaction at deal announcement. Please refer to page 38 for additional details on the construction of Schoenfeld residuals.

(a) ΔC



(b) CAR < 0



VII. Suggestive Evidence on Sunk Cost Effects in Other Investment Contexts

The main goal of this paper is to provide evidence for managerial sunk cost effects using the M&A–divestiture context as a useful setting for identification. As acquirers *exogenously* invest more sunk resources in the acquisition of a target firm, they increase their post-acquisition commitment to this business, manifested in a lower propensity to divest it.

In this section, I explore the generalizability of this sunk cost mechanism. Specifically, I explore the contexts of venture capital and R&D investment, following the discussion in Section V.F. In these contexts, I provide suggestive evidence that is consistent with the existence of a corresponding sunk cost mechanism—a positive relation between prior (i.e., sunk) resources that have been invested and subsequent commitment. I note this additional evidence from VC and R&D investment should be interpreted with caution, as it is centered on correlations in the data, rather than establishing causal evidence from a quasi-experiment.

A. Venture Capital

Sunk Cost Mechanism. As discussed in Section V.F, anecdotal evidence suggests the existence of sunk cost effects in the investment behavior of VC firms (recall, for example, one of the VC managers interviewed by Guler (2007) responding that "We see [follow-on investments] primarily as a way to protect our initial investment").

A simple application of the idea of increased commitment after initial sunk investments in the VC context would be that VC firms with prior investments in a startup increase their commitment to this startup, beyond the point where providing additional funding becomes a negative NPV project and other, previously non-invested VC firms refrain from providing funding. Then, correlationally, one would expect to see that for startups that have multiple funding rounds and ultimately fail, the fraction of VC investors that are repeat investors is larger on average.

Data and Approach. I use funding-round level data on U.S. venture capital investments from VentureXpert.[IA.17] Similar to Guler (2007), I focus on startups founded between 1989 and ten years prior to the end of the sample period in 2021, to ensure a sufficiently long time window to trace startup investment histories and final outcomes for all included startups.[IA.18] In terms of variables in the analysis below, the *fraction repeat VC investors* in a given funding round is the fraction of VC investors that were part of previous funding rounds of a given startup. Consequently, the analysis below focuses on funding rounds after the first round of funding, in order to be able to construct the repeat investor variable. *Round funding amount* is the total funding amount across all participating

[IA.17] Kaplan and Lerner (2016) note that VentureXpert provides more accurate information at the funding round level than alternative VC databases.

[IA.18] The quantitative analysis Guler (2007) finds evidence that the amount of prior funding in a startup is negatively related to a VC firm's propensity to terminate its investment in a startup. These findings are also consistent with sunk cost effects. That said, one caveat (beyond these results also being correlational in nature) is that the analysis in Guler (2007) examines prior funding in dollar units (rather than logged funding) which is highly skewed.

VC firms in a given round. A *failed venture* is defined as a startup whose exit status is *defunct* or *bankruptcy*.

Suggestive Evidence. Table IA.XIII presents suggestive evidence on sunk cost effects in VC firm investments. Columns (1) and (2) present analyses at the funding-round level. The baseline fraction of repeat VC investors in the data is 76%. Controlling for total VC funding in the investment round, funding round fixed effects, investment year fixed effects, as well as startup industry fixed effects in Column (2), the fraction of repeat investors increases by 3.4–3.8 percentage points, or about 5% relative to the baseline, for startups that ultimately are bankrupt or defunct. Columns (3) and (4) aggregate across funding rounds and are at the portfolio firm level.[IA.19] The correlational patterns are very similar. The average fraction of repeat VC investors across funding rounds is 72%. Controlling for observables, this fraction is estimated to increase by 3.5% relative to the baseline for startups that eventually fail.

Table IA.XIII. VC Repeat Investments

This table reports regressions in which the fraction of repeat (i.e., continuing) VC investors in a funding round is regressed on an indicator variable for whether a startup venture ultimately fails, the total funding amount across all participating VC firms in a given round, as well as fixed effects. Please refer to Internet Appendix Section VII.A for additional details. t-statistics are shown in parentheses. Standard errors are clustered by industry (of the portfolio firm). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Fraction Repeat VC Investors: Funding-Round Level		Fraction Repeat VC Investors: Portfolio-Firm Level	
	(1)	(2)	(3)	(4)
Failed Venture	0.038***	0.034***	0.028***	0.025***
	(6.02)	(4.78)	(3.96)	(3.23)
Round Funding Amount (ln)	−0.060***	−0.061***	−0.067***	−0.068***
	(−29.51)	(−30.28)	(−25.01)	(−25.39)
Funding Round FE	Yes	Yes	Yes	Yes
Industry FE	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
N	39,948	39,884	11,995	11,821

Summary. Overall, these correlations in the data are consistent with a sunk cost mechanism in which VC firms that have sunk prior resources in a given startup have a high propensity to provide subsequent funding relative to other, previously non-invested VC investors, and in particular so with respect to unsuccessful startups that ultimately fail.

[IA.19] In Columns (3) and (4), *fraction repeat VC investors* and *round funding amount* are averages across rounds, funding round fixed effects are total number of funding round fixed effects, and year fixed effects are first-year-of-funding fixed effects.

B. R&D Investment

Sunk Cost Mechanism. In a seminal laboratory experiment by Staw (1976), subjects are asked to take on the role of a corporate executive and dynamically allocate R&D funds between two segments, in a tech-oriented firm with recent deteriorating performance across segments. First, subjects select one segment to commit R&D funds to. Then, subjects receive a performance signal and are asked to invest additional R&D funds. The experiment finds that subjects *increase* their R&D commitments to their initially chosen segment after receiving negative signals about segment performance. These experimental patterns are consistent with excessive commitment once a prior (sunk) investment made. Below, I provide an approximation of the experiment in the data, noting that all comparisons are again drawn from correlations.[IA.20]

Data and Approach. I use standard firm data from Compustat. I focus on the R&D investment (scaled by lagged assets) in year t of firms with negative operating profit in year $t-2$. (In Staw (1976), the hypothetical firm described to subjects began to post losses prior to subjects' investment decision.) I compare R&D investment patterns across firms, as opposed to across segments in Staw (1976). Specifically, I compare the R&D investment of (i) firms with *deteriorating performance* (more negative operating profit in year $t-1$), and (ii) firms with *improving performance* (positive operating profit in year $t-1$). (In Staw (1976), the negative performance signal is described as a "deepening decline," whereas a positive signal is described as the division having "returned to profitable levels.") To get at overinvestment in R&D, I restrict the firms in (i) to those that never reach profitability subsequently (and that are not acquired at a price close to their market value in year t).[IA.21] In other words, I compare the R&D investment patterns of declining and, with hindsight, failing firms to those with improving operating performance.

Suggestive Evidence. Table IA.XIV finds suggestive evidence consistent with a sunk cost mechanism by which firms overinvest in R&D after prior R&D investments have been made. All columns control for standard determinants of R&D investment (firm size, firm age, cash holdings, Q, industry, and year). Column (1) finds that correlationally, firms with deteriorating operating performance—i.e., those receiving worsening negative performance signals—invest more in R&D than firms with improving operating performance, consistent with the experiment in Staw (1976). Deteriorating performance is associated with a 3 percentage point increase in R&D expenditures, relative to a baseline R&D-to-assets ratio of 9.6%. Column (2) finds that the positive correlation between deteriorating performance and R&D investment appears pronounced in firms with high prior (i.e., sunk) R&D investments. Further, Columns (3) and (4) find that the the positive correlation between deteriorating performance and R&D investment appears more pronounced in R&D intensive

[IA.20] In ongoing work, Guenzel and Liu (2023) find evidence consistent with sunk cost effects in R&D in the context of drug development. Biotech firms are significantly less likely to abandon drug development projects after experiencing *unexpected delays* in clinical trials, identified as the difference between actual trial completion date and anticipated completion date at trial start. These findings on heightened commitment to a chosen R&D project induced by unexpected delays highlight the broad interpretation and application of sunk costs as sunk resources, encompassing money, effort, and time invested.

[IA.21] The correlational results discussed below are similar without this restriction.

industries than R&D non-intensive industries.[IA.22]

Table IA.XIV. R&D Investment

This table reports regressions in which the R&D investment (scaled by lagged assets) is regressed on an indicator variable for a firm's performance is deteriorating (more negative operating profit in year $t-1$ compared to $t-2$), an indicator variable for whether a firm has made high prior R&D investments (higher lagged R&D investment relative to the median firm in the industry), as well as control variables as specified in the text, industry fixed effects, and year fixed effects. In Columns (3) and (4), R&D intensive industries are those for which the 25th-percentile firm-year (lagged, i.e. determined ex ante) has positive R&D investment, and R&D non-intensive industries are those for which the 75th-percentile firm-year has no R&D investment. Please refer to Internet Appendix Section VII.B for additional details. t-statistics are shown in parentheses. Standard errors are clustered by industry. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

	R&D Investment: All Industries		R&D Investment: R&D Intensive Industries	R&D Investment: R&D Non-Intensive Industries
	(1)	(2)	(3)	(4)
Deteriorating Performance	0.029***	−0.016***	0.039**	0.004*
	(2.74)	(−6.76)	(2.50)	(1.88)
High Sunk R&D Investments		0.090***		
		(7.23)		
Deteriorating Performance × High Sunk R&D Investments		0.071***		
		(3.98)		
Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	4,296	4,296	2,181	1,254

Summary. In sum, the correlations in the data are consistent with a type of overinvestment and a sunk cost mechanism in R&D, where deteriorating and failing firms have heightened R&D expenditures relative to firms with improving performance, driven by firms with high prior (i.e., sunk) R&D investments and firms in R&D intensive industries.

[IA.22] I define R&D intensive industries as those for which the 25th-percentile firm-year (lagged, i.e. determined ex ante) has positive R&D investment, and R&D non-intensive industries as those for which the 75th-percentile firm-year has no R&D investment. The results are not sensitive to these specific percentile cutoffs to identify R&D intensive and non-intensive industries.

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