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 distortions in investment decisions. 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. Further tests reveal that excessive commitment triggered by sunk costs generates substantial real effects and erodes firm performance.

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

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1. Introduction

Corporate investment in the U.S. amounts to roughly $2 trillion annually.[1] 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 prominent Corporate Finance textbooks that sunk costs likely play a major role in the corporate realm. For example, Berk and DeMarzo (2017) caution that basing decisions on sunk costs constitutes a "common mistake" and can result in "financial disaster," while Brealey, Myers, and Allen (2017) urge the reader to "Forget Sunk Costs."[2]

The lack of comprehensive field evidence on the sunk cost effect is due to a fundamental conceptual 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 conceptual 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

[1] In 2018, investment in private nonresidential fixed assets (equipment, structures, and intellectual property products) totaled $1.96 trillion among nonfinancial corporations. Source: U.S. Bureau of Economic Analysis (BEA).

[2] Figure 1 displays the key paragraphs in Berk and DeMarzo (2017) and Brealey, Myers, and Allen (2017).

stipulate a fixed number of acquirer shares to be exchanged in the transaction, changes in the acquirer's stock price between merger agreement and completion 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 triggered 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 the effects of sunk costs from differences in divestiture patterns of acquisitions undertaken in the same year but that experienced different post-agreement market fluctuations. An identifying assumption is that acquirers are attentive to post-agreement changes in acquisition cost.[3]

This setting requires information on both divestitures of previously acquired businesses and the precise exchange ratio terms of each acquisition. To achieve 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 involves a propensity score matching approach based on standard firm and deal characteristics (see Section 3.4 for details). Aside from using a fixed exchange ratio (henceforth, *Fixed Shares*), transacting parties can structure a stock acquisition using a floating exchange ratio (henceforth, *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 3.2 for details).

The resulting dataset of 558 acquisitions, 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

[3] This assumption appears well justified. For example, even the media frequently reports on stock price-induced transaction value changes, indicating that these changes should also be particularly salient to managers. See, e.g., 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).

simultaneously affect selection into deal structure type and divestiture rates. Moreover, the acquisition cost variation in my sample is economically meaningful, with the interquartile range of the market return between 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 50th to the 65th percentile in post-merger annual stock performance. This result is robust to various specifications, including a Cox (1972) proportional hazards model, stratified hazard models, a logit model that controls for the passage of time (Efron 1988; Jenter and Kanaan 2015), and a two-stage control-function-type estimation method (Wooldridge 2015).

Three additional findings support and extend this main result. First, a remaining concern is that market movements might affect divestiture rates through channels other than their effect on acquisition cost. To address this, I implement two separate placebo tests involving hypothetical acquisition cost changes. Both tests rest on the idea that any such alternative channels should also be present for market fluctuations that did not shift actual acquisition costs. The first placebo test uses post-deal completion market fluctuations to construct hypothetical cost changes (cf. Bernstein 2015). The second 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. Neither placebo test finds evidence that hypothetical cost variation predicts divestiture rates, corroborating the sunk cost interpretation.

Second, I investigate the efficiency effects of these sunk cost-driven divestment distortions. In a simple conceptual framework, I formalize the intuition that "sunk cost managers" initially fail to respond to negative signals about costly acquisitions, such as high integration and operating costs or misfit of company cultures, and that this generates real effects. Empirically, I create a counterfactual scenario in which acquirers who experienced an increase in acquisition cost face no cost shock. In this "no-shock" scenario, firms decommit and divest earlier. I show that during the period between counterfactual and actual divestiture announcement date, the average firm earns an abnormal return of -3.8%. In principle, this could simply reflect the fact that firms on the verge of divesting perform worse. However, performance deterioration is largely confined to observations for which the divested business constitutes a substantial part of the firm. This strengthens the interpretation that the decline in value is directly attributable to the to-be-divested business.

Third, I test whether the sunk cost effect operates through a firm- or person-level channel. Graham, Harvey, and Puri (2015) present survey evidence that chief executive officers (CEOs) make M&A-related decisions "in relative isolation," implying that CEOs are likely the most influential decision-makers in my setting. I find that the link between acquisition cost shocks and divestiture rates 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 is consistent with an intrapersonal sunk cost mechanism.

Why are managers influenced by sunk costs? In principle, divestment distortions could stem from rational career concerns or a sunk cost effect.[4] In standard career concern models, a manager makes an investment in which the payoff or probability of success is correlated with her ability (see, e.g., Kanodia, Bushman, and Dickhaut 1989, Boot 1992, and Grenadier, Malenko, and Strebulaev 2014). Ability is the manager's private information and needs to be inferred by others from observed outcomes. Since abandonment signals poor skill, managers have an incentive to distort divestiture decisions. However, my findings do not support this explanation, as pre-acquisition cost shocks are empirically uncorrelated with both the quality of a given manager and that of an acquired business (see Section 4.3 for details). Instead, the evidence supports the hypothesis that sunk costs trigger in managers a psychological attachment to acquired firms, and that the higher the sunk costs, the more reluctant managers are to divest. Altogether, my paper provides the first evidence on the existence of a sunk cost effect in corporate finance and demonstrates its importance for firm outcomes.

This paper is related to several strands of literature. First, I add to the behavioral corporate finance literature on the effects of managerial biases on firm outcomes. My paper advances this field by studying a frequently discussed bias 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 extrapolation,

[4] In Section 6.3, I consider a variety of other potential explanations for why sunk costs might influence decision-making, including "learning by doing" and sunk costs affecting firms' investment budgets, and discuss why my findings do not support these explanations.

Greenwood and Hanson 2014) and, more recently, also heuristics (e.g. the WACC fallacy, Krüger, Landier, and Thesmar 2015; competition neglect, 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, 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 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).[5] Perhaps most related to the present paper is a study by Weisbach (1995), which documents a higher propensity by firms to divest an acquired business after the CEO who led the acquisition is replaced (see also 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. In important contrast to this study, 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 identify a rare setting that allows me to study the influence of sunk costs in the field. Ashraf, Berry, and Shapiro

[5] 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).

(2010), in motivating their field experiment on sunk cost effects involving a water purification solution in Zambia, highlight that evidence on sunk costs has been "confined largely to hypothetical choices and a single, small-scale field experiment." In this widely cited experiment, Arkes and Blumer (1985) randomize theater subscription discounts and document higher attendance rates among patrons receiving a smaller discount (and thus paying a higher overall price). Two 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). Two classic studies (Staw and Hoang 1995, 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, Eyster (2002) notes that the alternative hypothesis of high ex ante beliefs about player quality coupled with gradual learning is "hard to rule out." My paper substantially advances the field evidence on the importance of sunk costs for decision-making. Moreover, by documenting that sunk costs matter in a high-stakes context and among the most sophisticated decision-makers—CEOs typically have decades of professional experience (Dittmar and Duchin 2015, Schoar and Zuo 2017)—, I clarify that the inclination to account for sunk costs is a deeply rooted bias and cannot be easily unlearned through education.

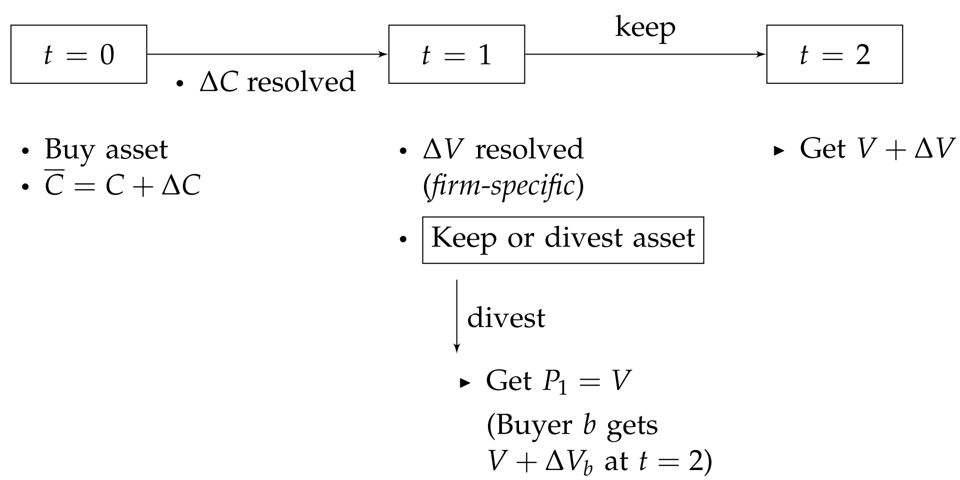
The rest of the paper proceeds as follows. Section 2 introduces a simple conceptual framework of managerial decision-making in the presence of sunk cost effects. Section 3 describes the data and presents summary statistics. Section 4 discusses the empirical strategy. Sections 5 and 6 present the main results, documenting the effects of sunk costs on firms' divestment behavior and providing evidence on real effects and channels. Sections 7 concludes.

2. Conceptual Framework

This section introduces a simple conceptual framework that pinpoints the consequences of sunk cost effects in the context of firms' divestment decisions. I first show how to parsimoniously model firm decision-making if managers take sunk costs into account and then trace out the real implications of such behavior for divestment patterns.

Setup. The framework, summarized below, features three periods: $t = 0, 1, 2$. The manager of a firm can buy an asset at cost $\overline{C} = C + \Delta C$ at $t = 0$. C is known to the manager upon making the investment decision, whereas ΔC is a mean-zero random variable, determined at some unmodeled intermediate time between $t = 0$ and $t = 1$. The manager has sufficient budget to make the investment. The asset delivers a cash flow of $V + \Delta V$ to its owner at $t = 2$. V is known at $t = 0$, whereas ΔV is also a mean-zero random variable determined at $t = 1$, i.e. after the investment is made. Also at $t = 1$, and after learning about ΔV, the manager can keep the asset or divest it to an unrelated firm at some price P_1 (the "market price"). There are multiple potential buyers for the asset, i.e. P_1 is determined competitively. The discount factor for all cash flows is 1.

Framework Timeline



Empirically, ΔV can be thought of as *synergies* that the asset creates for its owner. Synergies can be positive or negative, and are *firm-specific*. Thus, if the firm owning the asset at $t = 1$ sells the asset to some other firm after learning its realized synergy level ΔV, the asset payoff for the new buyer (b) will involve a new synergy draw (ΔV_b). Positive firm-specific synergies might stem from economies of scale, market power, product complementarities, or combination of talent, whereas negative firm-specific synergies might stem from an inefficient deployment of managerial resources, high integration and operating cost, or misfit of company cultures. The assumption of uncorrelated synergies is common in the literature (cf. Betton et al. 2008) but could be relaxed. Intuitively, absent informational or other frictions, a motive to divest ensues as long as asset synergies are imperfectly correlated across firms.

Managers and Market Prices. All managers make decisions that maximize the utility from their investments given their preferences and beliefs. The manager of the firm deciding whether to buy the asset at $t = 0$ is risk-neutral and has rational beliefs. That is, she correctly updates the value of the asset to the firm upon learning the realized synergy draw ΔV. I assume that $V > C$, so given risk neutrality, the manager will always buy the asset at $t = 0$. However, the manager has nonstandard preferences and historical costs affect her utility. Specifically, the manager incurs a disutility cost from divesting the asset, and this cost is increasing in the overall cost $\overline{C}$ required to buy the asset.[6] Conditional on buying the asset at $t = 0$, the manager's utility at time $t = 1$ is given by

$$\hat{V}_1^{\kappa} = (1 - d_1)\,(V + \Delta V) + d_1 \left(P_1 \quad - \underbrace{\kappa\,\overline{C}}_{\substack{\text{sunk cost}\\ \text{disutility}}} \right) \qquad \text{where}$$

$$d_1 = \underset{d_1 \in \{0,1\}}{\arg\max}\; (1 - d_1)\,(V + \Delta V) + d_1\left(P_1 - \kappa\,\overline{C}\right)$$

i.e. $d_1 = 1$ indicates divestment and $d_1 = 0$ indicates continuation. P_1 is the asset's market price, and $\kappa > 0$ captures the sunk cost effect. The maximization problem directly yields that the manager divests if and only if $P_1 - (V + \Delta V) > \kappa\,\overline{C}$.

Managers at other firms are also risk-neutral and have rational beliefs. The expected value of the asset to other firms at $t = 1$ is V since, as discussed above, realizations of the uncertain payoff component ΔV are independent across firms. As a result, the market price of the asset at $t = 1$ is given by $P_1 = V$.[7]

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

Result 1 (Divestment Distortions): *The sunk cost manager fails to divest the asset in certain states in which keeping the asset has a negative net present value.*

The net present value (NPV) of the asset at $t = 1$ is given by $V + \Delta V - P_1$, i.e. the true value of the asset to the firm minus the market price. Since $P_1 = V$, the NPV is entirely captured by the realized synergy level ΔV. The sunk cost manager keeps the asset if $(V + \Delta V) - P_1 \geq -\kappa(C + \Delta C)$, and since $P_1 = V$, if $\Delta V \geq -\kappa(C + \Delta C)$. This implies continuation under a negative NPV (as long as the NPV is not too negative).

A qualification of this result leads to the following directly testable prediction:

[6] See Thaler (1980) for a more psychology-driven modeling approach of sunk cost effects based on prospect theory (Kahneman and Tversky 1979).

[7] With only three periods and therefore one divestment period (at $t = 1$), it is not necessary to make any assumptions on whether other managers are subject to sunk cost effects, or whether the manager of the firm buying the asset at $t = 0$ is naïve or sophisticated about sunk cost effects.

Result 2 (Propensity to Divest): *Conditional on asset ownership at $t = 0$, the probability that the sunk cost manager divests the asset at $t = 1$ is decreasing in the realized cost shock ΔC.*

This result obtains since the divestment threshold is increasing ΔC. Consider two cost shocks ΔC_j, $j \in \{1, 2\}$, with $\Delta C_1 > \Delta C_2$. The manager divests at $t = 1$ if $P_1 - (V + \Delta V) > \kappa(C + \Delta C)$. For a given known cost component C, it follows that the difference in the divestment threshold between the two shock realizations is given by $\kappa(\Delta C_1 - \Delta C_2) > 0$ at any point in time. The testable prediction of Result 2 directly links to my empirical analysis below.

3. 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 Appendix B.

3.1. Divestitures of Previously Acquired Businesses

To identify divestitures of previously acquired businesses, I start from a standard data set on stock acquisitions, which I obtain 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. Using this sample, I then identify divestitures from two sources: SDC's divestiture database and a systematic news search through Nexis.

Divestitures from SDC. To identify divested acquisitions through SDC, I extract all transactions involving U.S.-based entities that SDC flags as a Divestiture, Spinoff, or Leveraged Buyout. These transactions comprise *any* asset sales, independent of whether the seller grew the business parts organically or previously acquired them. I then link the acquisition and divestiture data sets using SDC's 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-based approach is, however, that SDC's CUSIP identifiers can change over time, implying that the matching procedure above might fail to identify some divestitures. A prominent example of such an undetected divestiture is

AT&T's acquisition and subsequent spinoff of NCR (Lys and Vincent 1995). To obtain a comprehensive divestiture sample, I therefore perform a systematic news search of divestitures, similar to that in Cronqvist and Pély (2020), for all acquisitions not identified as a "divestiture candidate" through SDC.[8] I perform the searches using Nexis Uni and systemize them 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). One distinct advantage of this approach is that 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. I first spend about five minutes on each acquisition to identify acquisitions for which sources indicate a potential divestiture. I then combine the first-round potential divestitures with the "divestiture candidates" from the above SDC-based approach and analyze them in more detail.

Verifying Divestitures. To determine the correctness of a divestiture, I rely on additional newspaper articles as well as SEC filings, such as firms' annual, quarterly, or current reports (10-K, 10-Q, and 8-K, respectively). A further source that proves useful is Exhibit 21 (Subsidiaries of the registrant) that firms submit with their 10-K filings, among others. In particular, if a business is no longer included as a subsidiary of a firm, and instead appears on the subsidiaries list of a different firm, this is clear evidence of a divestiture.

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 and 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, which is an essential requirement to pinpoint the effects of sunk costs on decision-making.[9] 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 firm that makes the divestiture is the same firm that experienced the cost change in the original acquisition of the business. 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

[8] 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).

[9] An example of a partial divestiture not included 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."

disregard divestitures that are management buyouts (MBOs), as these deals involve management acting on both sides of the transaction. Appendix-Table B.2 provides a step-by-step overview of the final divestiture sample construction from the initial sample of full divestitures.

3.2. Collection of Acquisition Terms

In a next step, I hand-collect the exact merger terms of the initial acquisition, i.e. 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, deal terms can be discussed in the main body of an SEC filing (most frequently an 8-K or 10-K), or can be found in analyst conference call transcripts as well as news articles and wires (see Appendix B.2 for several examples of merger agreements from my sample).

I am able to find the precise deal terms for 89% of the acquisitions in my sample. This fraction is large both on its own and when compared to existing studies—though relative comparisons are difficult.[10] Since my identification hinges on exposure to aggregate market fluctuations between merger agreement and completion, I narrow the sample to only include acquisitions with a *transaction period*—defined as the period from two days after the date of the finalized merger agreement until the date of the merger completion (term adopted from Ahern and Sosyura 2014)—of at least ten days.[11] Given my identification strategy, it is also crucial that I identify all relevant dates correctly. Infrequently, the dates in SEC filings differ from those that SDC provides, in which case I rely on the relevant dates from the official SEC documents. Most commonly, I make adjustments when SDC bases the announcement date on a so-called letter of intent to merge, a legally *non-binding* document that only stipulates a preliminary agreement to merge.

[10] To the best of my knowledge, Ahern and Sosyura (2014) is the only other paper that systematically collects merger deal terms and discusses sample attrition. They start from a sample of 1,000 acquisitions and arrive at a final sample of 507 deals. Their approach is, however, not directly comparable to mine. In particular, while they focus on larger and more recent deals for which deal specifics are generally more easily available, they require more information on each deal, including the date at which merger discussions began and availability of Factiva intelligent indexing codes.

[11] I do not consider the day of and first day after the merger agreement since I use the returns during these days in the construction of the three-day abnormal acquisition announcement return.

3.3. *Additional Data and Final Divestiture Sample*

I supplement the data set with standard financial and firm information from the Center for Research in Security Prices (CRSP) and the Compustat North America (Compustat) database. Since my empirical approach features an event-time analysis (time between acquisition and divestiture in years), I construct both deal-level and deal-year-level variables that I use as controls in my analyses. Appendix A contains detailed definitions of all variables I use in this study. Dropping observations with incomplete data on control variables yields a final sample of 370 acquisitions that are subsequently divested. 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 previous papers. Ahern and Sosyura (2014), studying mergers between 2000 and 2008, report that 74% of deals in their sample used a *Fixed Shares* structure, the other 26% used a *Fixed Dollar* structure. Similarly, in Mitchell et al. (2004), who study mergers between 1994 and 2000, 78% of deals are *Fixed Shares* deals, whereas 22% are *Fixed Dollar* deals or involve more complicated terms.

3.4. *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 the fact that sunk costs might induce managers to *continually* not divest a previously acquired business, if the costs they have sunk into the business are sufficiently high.[12] Since it is infeasible to collect the merger terms for all non-divested acquisitions, covering several thousand deals, I construct the broadened sample by matching each divested *Fixed Shares* acquisition to a similar acquisition that is not subsequently divested.

To implement the matching procedure, I proceed in three steps. First, I focus the set of potential matches on acquisitions that are what I refer to as "divestable," to ensure that matched acquisitions have a similar ex ante propensity to be divested.[13] 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). I confirm in Appendix-Table C.1 that both of these characteristics are

[12] Note that 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 4.4 for details).

[13] This step uses intuition from case control designs in the medical literature, typically aimed at finding whether a certain factor contributes to a rare disease. Broadly speaking, these studies examine whether patients with the disease have had a differential exposure to the factor of interest compared to similar subjects that are free of the disease. A crucial requirement in such designs is that control subjects are also *susceptible* to the disease (Grimes and Schulz 2005).

also strong divestiture predictors in my general M&A sample. Second, using the resulting set of non-divested acquisitions as the potential matches, I perform standard propensity score matching to find the acquisition that is most similar to a given divested *Fixed Shares* acquisition. The list of variables I use for matching include the target's industry, the deal value at merger agreement, acquirer size, public target status, and three-day cumulative announcement return (CAR), and thus comprises all variables Appendix-Table C.1 identifies as divestiture predictors in the general M&A sample. Importantly, I do *not* match on the experienced (endogenous or market-induced) cost change of the initial acquisition, as this is the key variable I relate to the rate of divestiture in the empirical analysis. Third, for each matched acquisition, I again need to verify 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.

This procedure results in matched acquisitions that are similar along a wide array of deal and firm characteristics (see Section 3.5 for summary statistics). The resulting sample of divested acquisitions and similar non-divested acquisitions forms the basis for most of the subsequent analyses. It is comprised of 4,461 event-time observations (years since acquisition) from 558 acquisitions. In the following, I will frequently refer to this sample as the *main sample*.

3.5. Summary Statistics

Figure 2 shows the frequency distributions of acquisitions and divestitures over time for this main sample. Panel 2a shows that many acquisitions were undertaken in the late 1980s and, especially, the mid-to-late 1990s. Thus, my sample appears representative of stock mergers in general, as these were the periods that witnessed a surge in stock merger activity (see, e.g., Betton et al. 2008). Panel 2b shows that, among the deals that are subsequently divested, there is considerable variation as to when the divestiture occurs. While divestiture activity is more pronounced during economic downturns, many divestitures also occur during other periods, such as the mid-2000s. Panel 2c plots the time passed between acquisition and divestiture. The average (median) acquisition is divested after 4.70 (3.37) years, and almost 90% of divestitures occur within ten years of the acquisition.

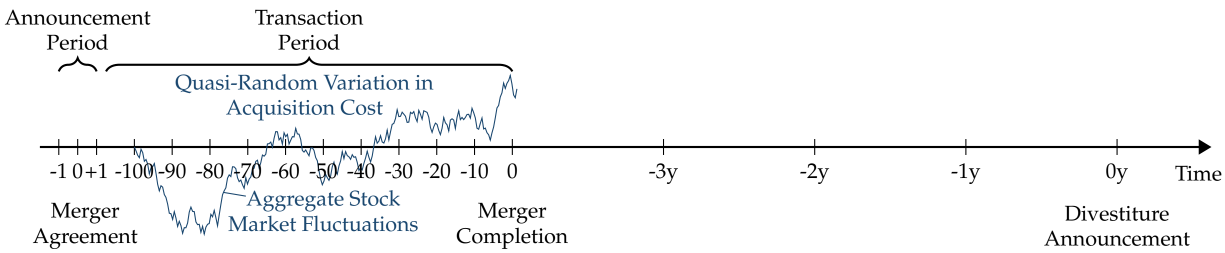
Table 1 presents summary statistics for the main sample. Panel A shows deal-level variables. Both the average and median acquisition in my sample experiences a negative stock market reaction at deal announcement (3-day CAR of −0.30% and −0.68%, respectively). An unfavorable announcement reaction on average 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 in my sample are acquisitions of public targets, and 56% of deals are pure stock deals. The average length between merger agreement and completion—i.e., 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 4.2. Panel D provides a balance table comparing the divested acquisitions to the matched non-divested acquisitions. Across all examined observables—including those not used for matching—there are no significant differences in the mean or distribution between the two sub-samples.

Overall, I conclude that my sample is representative of stock mergers more generally as gauged by typical patterns regarding, e.g., market reaction at announcement, transaction period length, and merger frequencies over time.

4. Empirical Strategy

The key component of my identification strategy is that in *Fixed Shares* acquisitions, aggregate market fluctuations in the period 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 quasi-random acquisition costs to firms' propensity to subsequently abandon the acquired business through divestiture. The figure below summarizes the event timeline. The 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 1).

Event Timeline



4.1. *Fixed Shares Acquisitions*

In general, transacting parties can structure a stock acquisition in one of two ways: using *Fixed Shares* or a *Fixed Dollar* structure. In a *Fixed Shares* merger, parties stipulate a fixed number of acquirer shares to be exchanged in the merger agreement. Alternatively, in a *Fixed Dollar* acquisition, parties specify a variable exchange ratio, such that the merger consideration in dollars remains fixed. Ahern and Sosyura (2014) show that deals across the two structures are indistinguishable along many observable characteristics, and exploit this similarity for identification. The only observed difference is that the acquirer's historical stock return volatility tends to be higher in *Fixed Shares* mergers.

An attractive feature of my identification is that it entirely circumvents any concerns about potential selection by transacting parties into *Fixed Shares* versus *Fixed Dollar* deal structures. The empirical analysis is centered on *Fixed Shares* acquisitions, and uses acquirers' differential exposure to market movements within this set of deals. In addition, the *Fixed Dollar* acquisitions constitute a nearly ideal 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 4.3 for details).

4.2. *Empirical Design*

I first discuss the calculation of the endogenous change in acquisition cost, ΔC_i^{Acq}, induced by post-agreement fluctuations in the acquirer's stock price in *Fixed Shares* mergers. I calculate this change 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, i.e. *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, taking into account the acquirer industry's co-movement with the market.[14] This approach is reminiscent of the methodology used in event studies, where one typically uses this same procedure to estimate a firm's counterfactual return. In addition, to account for the fact that the market return is positive on average, I subtract the expected daily market return in this modified equation.[15] Disregarding the average market appreciation would lead to a mechanical correlation of the market return variable with the length between merger agreement and completion. In this case, I would no longer isolate variation in acquisition cost that is plausibly exogenous to deal characteristics. In summary, 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. Using this market-induced component of the acquirer's stock price change, I compute the market-driven change in acquisition cost as:

[14] To estimate the acquirer industry's sensitivity with the market, I follow Krüger et al. (2015) and run 60-month rolling regressions of the returns to the value-weighted portfolio of firms in the acquirer's Fama and French (1997) industry, based on 49 industry portfolios, on the returns to the CRSP value-weighted index (including distributions).

[15] I calculate the expected daily market return as the average yearly return to the CRSP value-weighted index since 1980 (the beginning of my sample period), which equals 12%, divided by 365.

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

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.

To test for the effect of sunk costs on firm decisions, I then relate the market-induced change in acquisition cost calculated in Equation (1′) to the rate of subsequent divestiture. The main estimating equation takes the form:

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

where i refers to an acquisition, t is the time passed since the acquisition in years, and t_0 denotes the acquisition (calendar) year. 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 4.3), and under the null hypothesis that sunk costs do not affect firm decision-making, κ should not be statistically different from zero. $X_{i,t}$ 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.

Panel C of Table 1 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.

4.2.1. Collars

About 10% of *Fixed Shares* acquisitions in my final sample involve slightly more complicated deal terms, featuring so-called collars. In *Fixed Shares* deals, collars define bounds 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 strategy focuses on 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. Therefore, as long as aggregate market movements strongly predict acquirer stock price movements after accounting for collar caps and floors, the above empirical design remains valid. I show in Section 4.3 that this is the case. Second, since collar clauses are relatively infrequent in my final sample, I can take a more extreme route and exclude deals that specify collars from the analysis altogether. I verify in Section 5.2 that my results remain unchanged (and in fact become slightly stronger) when I restrict to "pure" *Fixed Shares* acquisitions. Finally, in Appendix E, I implement an alternative two-stage estimation approach that directly includes the endogenous cost change, taking into account caps and floors triggered by collars, as the main variable of interest. This approach, which I discuss in more detail at the end of Section 5.2, relies on the control function method (Wooldridge 2015) to control for the endogeneity in the system.

4.3. 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) aggregate market movements should not affect divestiture decisions through any channel other than their effect on firm returns and resulting acquisition cost changes (Angrist and Pischke 2008; Pischke 2017; Wooldridge 2010).

Market Fluctuations Affect Firm Returns and Are "as Good as Randomly Assigned". Panel A of Table 2 presents regressions, using the main sample, of the cumulative firm returns during the transaction period on the cumulative market returns, net of expected returns, during this period. For the roughly 10% of acquisitions involving collars, I modify the calculation of the cumulative return to the acquirer by limiting it to the maximum or minimum return that still results in an acquisition cost change. Column (1) regresses collar-adjusted firm returns on market returns alone. Columns (2) to (4) add controls as well as industry and acquisition year fixed effects. Unsurprisingly, the slope coefficient is highly significant across 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 2 shows regressions of the cumulative market returns, net of expected returns, on observable deal and firm characteristics. I consider an array of characteristics, including announcement return, deal value at merger agreement, acquirer size, the acquirer's 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 (Columns (1) to (8)) nor jointly (Column (9)). For example, in Column (9), the F-statistic for the joint significance of all variables is 0.56 (the associated p-value is 0.81).

Market Fluctuations Affect Divestitures Only Through Their Effect on Acquisition Cost. My setting allows me to implement two separate placebo tests that address concerns about the requirement that the only channel through which market fluctuations affect divestitures is through their effect on acquisition cost. The placebo tests rest on the idea that such potential alternative channels should also be detectable (i) during periods other than the period between merger agreement and completion, and (ii) for acquisitions not structured as a *Fixed Shares* deal.

In the first placebo test, inspired by Bernstein (2015), I construct hypothetical acquisition cost changes for the deals in my main sample using aggregate stock market fluctuations immediately following deal completion. If the final identifying assumption holds, the hypothetical market-induced cost changes should not affect divestiture decisions. In constructing the hypothetical cost changes, I apply the exact same steps and formulas described in Section 4.2, except that I use the hypothetical market fluctuations rather than those truly experienced by the acquirer.

In the second placebo test, I leverage the acquisitions from Section 3.2 that are divested but used a *Fixed Dollar* deal structure, i.e. the deal structure that fixes the merger consideration in dollars at the time of the merger agreement. For these deals, I again calculate hypothetical market-induced acquisition cost changes, now using the aggregate market fluctuations from the actual period between merger agreement and completion. Thus, I calculate the cost change an acquirer *would* have experienced had the acquisition been structured as a *Fixed Shares* instead of a *Fixed Dollar* acquisition. Applying the same logic as above, if the final identifying assumption holds, the hypothetical changes for *Fixed Dollar* acquisitions should not predict divestiture rates.

In both placebo tests, I find that hypothetical acquisition cost changes do not predict divestiture rates. In both tests, the coefficient on hypothetical changes is insignificantly different from zero and switches sign relative to the coefficient on truly experienced cost changes. I provide a more detailed discussion on the results of these tests in Sections 5.3 and 5.5, respectively.

4.4. *Estimation Method*

In the main analyses, I estimate Equation (3) using the semi-parametric Cox (1972) proportional hazards model. Hazard models are commonly used for survival data and duration analysis (time-to-event analysis). Consequently, they are the most natural choice in the context of divestitures of previously acquired businesses (see also the discussion in Jenter and Kanaan (2015) in the context of CEO turnover).[16] The hazard model treats acquisitions that are not subsequently divested as censored observations. The censoring date corresponds to the day before I begin the divestiture news search (December 15th, 2018). If the acquirer is itself taken over at some point, the censoring date is 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 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, i.e. time indicates years passed since acquisition (see Equation 3).

The standard Cox (1972) model assumes proportional hazards, i.e. that the ratio of the hazards of any two observations is constant over time.[17] 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 provide an in-depth description of Schoenfeld residuals and how to use them to test for proportional hazards in Appendix D.

While the hazard model is arguably the most suitable choice in my setting, my results do *not* hinge on this specific approach. In particular, I verify in additional tests (see Section 5.2) that my results are robust to using a logit specification instead (cf. Efron 1988; Jenter and Kanaan 2015).

[16] 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.

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

5. Results I: Sunk Costs and Firm Decision-Making

5.1. *Main Result*

The first set of results investigates the effect of quasi-random variation in acquisition costs on divestiture rates. Table 3 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. z-statistics are in parentheses, calculated based on standard errors clustered by acquisition year-quarter.

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 3.4 and further deal-level and firm-level controls that might plausibly affect divestiture rates. The coefficient on acquisition cost variation is negative and strongly statistically significant (at 1%), revealing that an increase in quasi-random acquisition cost reduces the rate of subsequent divestitures. This is precisely what one would expect if managers take sunk costs into account in their divestment decisions. The coefficient estimate of -0.065 implies that a 1 percentage point increase in market-induced acquisition cost relative to the acquirer's market capitalization is estimated to reduce subsequent divestiture rates by 6.3%. An interquartile cost increase (1.28 pp, see Table 1) is associated with an 8% reduction in divestiture rates.

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, as compared to these 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) modify the specification taking into account the results from the test for proportional hazards based on Schoenfeld residuals (Schoenfeld 1982). I briefly summarize the Schoenfeld test results here and provide more detail in Appendix D. Appendix-Table D.1 reports how each of the covariates in Column (2) of Table 3 depends on (linear) time. Appendix-Table D.2 restricts the sample to acquisitions that are subsequently divested. (I discuss this specification in Section 5.4.) In both tables, the correlation of the market-induced change in acquisition cost with time is clearly insignificant (p-values of 0.36 and 0.67, respectively). Thus, there is no indication that the proportional hazards assumption might be violated for the main variable of interest. The lack of time dependence is also confirmed visually by the nearly perfectly flat line through the Schoenfeld residuals when plotted against time (see Appendix-Figure D.1). For some of the control variables, however, the Schoenfeld tests suggests that the effect of the variable on divestiture rates might be time-dependent. In the remaining columns of Table 3, I therefore allow for time-dependent effects. To be conservative, I choose a p-value cutoff of 0.15 to determine the variables to be interacted with time.

Allowing for time interactions in the final three columns of Table 3 has no effect on the strongly negative association between quasi-random acquisition cost and divestiture rates. 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 3 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.

5.2. *Robustness Tests*

This section summarizes several additional robustness tests in order to buttress the findings from Section 5.1. Unless otherwise specified, all robustness tests use the hazard model specification in Column (4) of Table 3, allowing for linear time interactions of controls. Panel A of Table 4 shows robustness to various sample restrictions. First, I show that my results are robust to restricting to "pure" deals without collar clauses (corresponding to roughly 90% of sample). Removing collar deals leads to a *larger* estimated effect of quasi-random acquisition cost changes on divestiture rates. Next, my results are virtually unaffected when excluding the smallest 5% of acquirers from the sample. Thus, my findings do not stem from the smallest firms, for which divestment distortions might be less economically significant from an aggregate perspective. Additionally, Gabaix and Landier (2008) propose in their assortative matching model and calibration that more talented CEOs match with larger firms in equilibrium. In light of this, the firm-size based subsample test may also be recast as showing that the documented effects are not a function of CEO talent, consistent with the prediction in Berk and DeMarzo (2017) that failing to ignore sunk costs is a particularly common mistake, even among the most sophisticated decision-makers. Third, 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. Similarly, the coefficient of interest remains unchanged when 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%.

Panel C 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 Appendix-Tables D.1 and D.2. Across all four models, the coefficient estimates and significance levels on the acquisition cost change variable remain unchanged.

In addition to the robustness tests summarized in Table 4, Appendix E presents an alternative, two-stage estimation approach. In this approach, I directly include the endogenous acquisition change induced by movements in the acquirer's stock price in the estimation, together with the market-based cost change as the instrument. Since the hazard model is a nonlinear model, I implement this approach using the residual inclusion (control function) method (cf. Wooldridge 2015). In brief, 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. Since this approach involves a generated regressor, I use the block bootstrap method for statistical inference. Appendix-Table E.1 shows that the results of this two-stage estimation procedure corroborate those presented in the main paper. The coefficient on acquisition cost changes remains negative and strongly significant, and implies economic magnitudes of the effect of sunk costs on divestiture rates similar to those estimated in Tables 3 and 4.

5.3. *Placebo Test I: Post-Completion Market Fluctuations*

Table 5 presents the results of the first placebo test, using aggregate stock market fluctuations in the three-month period immediately following acquisition completion to construct hypothetical acquisition cost changes. In Panel A, I calculate 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 1). In Panel B, I use market flucutations from deal-specific window lengths, corresponding to the length of the deal's transaction period (the period between merger agreement and completion). In both panels, the inclusion of controls (omitted for brevity), fixed effects, and time interactions is identical to that in Table 3.

There is no evidence that the hypothetical acquisition cost changes significantly predict divestiture rates. Across all five columns, the hypothetical cost change coefficients are close to zero and clearly insignificant. They range between 0.009 and 0.029, i.e. they also switch sign relative to the coefficients on actual acquisition cost changes in Table 3. These results are in line with the hypothesis that the market returns affect divestiture rates only through their effect on truly experienced acquisition cost, and corroborate the hypothesis that the documented divestment distortions are induced by managers failing to ignore sunk costs in their decision-making.

5.4. *Within-Divestiture Sample*

If sunk acquisition costs shift managers proclivity to make a subsequent divestiture, this effect should also generate differential divestiture patterns among the acquisitions that are subsequently divested. To explore this, Table 6 revisits the main results presented in Table 3, conditioning on divested acquisitions. The structure of the table is again identical to that of Table 3, with controls omitted for brevity.[18] Consistent with the reasoning above, the effect of sunk acquisition costs on subsequent divestiture rates is also strongly detectable in the reduced sample of divested acquisitions. All five columns again document economically and statistically significant distortions in divestiture rates induced by quasi-random acquisition costs. In addition, the implied economic magnitudes are very similar to those estimated for the main sample. For example, the midpoint of the coefficient range, -0.0675, implies a reduction in divestiture rates of 8.3% for an interquartile increase in acquisition cost relative to the acquirer's size, which is very similar to the magnitudes estimated in Table 3 for the full sample.

5.5. *Placebo Test II: Fixed Dollar Acquisitions*

Table 7 presents the results of the second placebo test, leveraging the acquisitions from Section 3.2 identified as being subsequently divested but using a *Fixed Dollar* deal structure. I implement this placebo test on the joint sample of all acquisitions identified as subsequently divested, i.e. on the sample of divested *Fixed Shares* acquisitions from Table 6 augmented by the divested *Fixed Dollar* acquisitions, the latter comprising the placebo group observations. While the placebo test leverages quasi-random variation in market fluctuations within the subset of *Fixed Dollar* deals,

[18] Table 6 also adds control variables for whether an acquisition is diversifying in terms of industry or location. The coefficient on quasi-random acquisition costs is very similar with and without these additional controls. The two variables are not included as controls in Table 3 as they are used in the matching procedure to identify the set of divestable acquisitions (see Section 3.4).

Appendix-Figure C.1 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 in Ahern and Sosyura (2014) that deals across the two structures are similar along many observable dimensions.

Columns (1) and (2) of Table 7 correspond to the specifications in Columns (4) and (5) of Table 6, i.e. the specifications with the full set of controls and (linear or log) time interactions (control variables are again omitted for brevity). 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. In conclusion, the second placebo test also finds that hypothetical acquisition cost variation does not predict divestiture rates, and thus further corroborates the sunk cost interpretation of the results.[19]

6. Results II: Real Effects and Channels

6.1. Real Effects

This section investigates the real effects implications of the documented distortions in firms' divestment decisions. The median divestiture in my sample occurs at a "discount" of 50%, i.e. at half of the transaction value of the original acquisition.[20] While this is suggestive of firms ignoring bad signals about the quality of acquired businesses and "pulling the plug" too late, the conceptual framework of Section 2 lays out that sub-optimal divestiture behavior is determined

[19] Table 7 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.

[20] The information on divestiture prices is available for 63% of observations in my sample. The remaining divestitures occur at undisclosed prices.

by the potential sale price relative to the expected internal value. Thus, ideally, one would like to observe the market price as well as the internal value of the to-be-divested business at each point in time. This would require not only information on realized cash flows but also all possible future cash flows, including the probability distribution of these cash flows.

The conceptual framework also provides useful guidance on how to address the real effects question in spite of these data challenges. Specifically, Result 1 implies that managers who incurred higher sunk costs relative to expected cost will be especially prone to hold on to acquired businesses beyond the point at which their NPV becomes negative. To study this, I construct for each divested acquisition that became exogenously more expensive ($\Delta C > 0$) a counterfactual divestiture announcement date had the acquirer faced no acquisition cost shock ($\Delta C^{CF} = 0$).[21] To estimate counterfactual dates, I use the hazard model from Column (1) of Table 6 and estimate the expected survival time (i.e. the expected time until divestiture) under $\Delta C > 0$ and $\Delta C^{CF} = 0$, holding fixed the observation's other characteristics (i.e. the control variables). The counterfactual divestiture announcement date is then defined as the date obtained by subtracting the difference in the two expected survival times from the true divestiture announcement date. I refer to the period between the counterfactual and actual divestiture announcement date as the *sunk cost period*. Appendix-Figure C.2 provides the distribution of the sunk cost period lengths across observations. The average (median) estimated sunk cost period is 87 (38) days.

I then examine firms' stock market performance during the sunk cost period. Panel 3a of Figure 3 provides graphical evidence showing an economically substantial negative industry-adjusted performance for the average firm during this period. Since the length of the sunk cost period differs across observations, the figure normalizes the sunk cost period to 1 and plots relative time (between 0% and 100%) between the counterfactual and actual divestiture announcement date. The average buy-and-hold abnormal return is −3.8%. This is consistent with the idea that firms fail to abandon businesses with a negative NPV and that this materializes in a decline in overall firm value. To corroborate the idea that the downward trajectory in firm performance is directly attributable to the subsequently divested business, Panel 3b of Figure 3 splits observations by how large the to-be-divested business is relative to the overall firm. Specifically, I sort observations into two groups based on the size of the original acquisition divided by the size of the combined firm (the median relative size is 17%). Indeed, the effect is driven by observations for

[21] 162 out of the 279 divested *Fixed Shares* acquisitions faced a market-induced increase in acquisition cost ($\Delta C > 0$). Stock return data is available for 153 of these observations. All results in this section are based on these observations.

which the to-be-divested business constitutes a significant part of the entire firm. Table 8 presents the table version of Figure 3, providing all numbers for the cumulative industry-adjusted returns over the sunk cost period.

The evidence in Figure 3 and Table 8 suggests economically sizable magnitudes not only in returns but also in dollars, as illustrated by the following back-of-the-envelope calculation. To be conservative, assume that only the negative excess return beyond that estimated on the small relative size sub-sample is driven by the value evolution of the to-be-divested business. This still implies a typical dollar loss of around \$10 million associated with delayed divestment.[22] These magnitudes are in the same ballpark as the average real costs of \$16 million that Krüger et al. (2015) associate with the "WACC fallacy" (the mistake of discounting at the firm-wide weighted average cost of capital (WACC) rather than at project-specific rates) in the context of M&A.

It is important to note that sunk costs plausibly distort firm decision-making in a wide range of investment decisions beyond M&A, magnifying the potential real costs for firms. Other decision contexts in which sunk costs could easily have first-order economic effects include but are not limited to: new product development, failed product continuation, and projects plagued by cost overruns.[23] Taken together, this section's results and complementary discussion support the conclusion that taking sunk costs into account entails economically significant efficiency costs for firms.

6.2. *Firm Versus CEO-Specific Effect*

A natural question is whether the documented divestment distortions can be linked to specific decision-makers within the firm, i.e. whether the relation between sunk costs and divestment decisions operates through a firm or individual-specific channel. In the context of M&A (and divestitures, i.e. M&A "reversals"), the obvious decision-maker to focus on is the firm's CEO. Survey evidence by Graham et al. (2015) finds that CEOs consider themselves as being the dominant decision-maker in these decisions, and indicate that they make M&A decisions "in relative

[22] The average excess return estimated on the large relative size sub-sample beyond that on the small relative size sub-sample is -5.92%. Given a median relative size of 33% for the former group, the implied typical value evolution of the to-be-divested business over the sunk cost period is $-5.92\%/33\% = -17.9\%$. Given a median divestiture price of \$45 million, the implied value change in dollars is then $\$45\text{mn} - \$45\text{mn}/(1-0.179) = -\$10\text{mn}$.

[23] The potential adverse effects of sunk costs in decision contexts other than M&A is well exemplified by the Concorde aircraft project. Even after it was clear the Concorde would not be economically viable, the French and British governments continued to spend billions of dollars on its development. The Concorde never became a commercial success and was finally retired in 2003. Because the Concorde example is so widely known, the mistake of basing decisions on sunk costs is also dubbed the *Concorde fallacy* (see, e.g., this *Forbes* article: forbes.com/sites/jimblasingame/2011/09/15/beware-of-the-concorde-fallacy/).

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, i.e. 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 but note that my results are nearly identical when keeping all observations in the sample.[24]

I emphasize that this test is different from research that examines CEO "styles" (e.g. Bertrand and Schoar 2003, Dittmar and Duchin 2015, Schoar and Zuo 2017) and is also different 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 9 presents the results. Columns (1) and (2) include controls (omitted for brevity), fixed effects, and time interactions as in Column (4) of Table 6. 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 6) after disregarding thirteen ambiguous CEO transitions as discussed above. With this modification, the coefficient of interest remains unchanged. If anything, both the effect size and significance become slightly stronger. 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 (*Same CEO*) or not (*New CEO*). Consistent with the predictions of an intrapersonal sunk cost channel, the acquisition cost effect is driven by the *Same CEO* regime.

[24] 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 "psychological affiliation" around the divestiture decision might be unclear.

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 *Same CEO* regime is strongly significant (z-statistic of -2.51 and -2.33, respectively), 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 elevate the hurdles for alternative explanations of my findings based on firm or market characteristics. Any such explanations would not easily predict CEO-specific effects on the relation between quasi-random acquisition costs and divestitures.

6.3. Discussion

What drives the (person-specific) link between sunk costs and firm behavior? The evidence is consistent with a sunk cost effect, i.e. that abandoning a project or investment triggers disutility in managers, and that this disutility is increasing in the amount of sunk costs experienced. As a consequence, and as laid out in the conceptual framework of Section 2, managers' reluctance to divest will be an increasing function of sunk costs, in line with the empirical evidence presented in the previous sections.

It is important, however, to also take into consideration other potential explanations for the observed relation between sunk costs and firm behavior, as discussed by, e.g., Camerer and Weber (1999) and McAfee et al. (2010). A first such explanation is that more positive information or beliefs about a product imply both a higher willingness to pay and a greater propensity to use a product. However, my setting, exploiting cost variation *after* a firm's purchase commitment, carefully eliminates potential screening channels and thus, rules out information- or belief-based explanations for the observed relation between acquisition cost and firms' degree of commitment to acquisitions. For the same reason, other prominent behavioral explanations, such as overconfidence about target quality, cannot explain my findings.

Second, "learning by doing" could imply that the incurrence of greater expenditures makes it more attractive to continue an endeavor. If previous investments positively affect the probability of project success, incurring high sunk costs can make it optimal to remain committed to a course of action and even to invest additional resources. While potentially a valid argument in many contexts, firms in my context do *not* learn from the acquisition cost shocks between merger agreement and completion, as these shocks stem from plausibly exogenous market fluctuations.

Third, given the option nature of investments, a large negative initial return on a project can imply better news about the project's overall value than a small positive return, as the large negative return signals high variance in payoffs and thus, a high option value. In such cases, holding on to non-performing investments may be optimal. However, this argument centers around performance rather than initial investments made. Further, and most importantly, the acquisition cost shocks I analyze are *not* informative of future cash flows of acquired businesses, as these shocks are induced by quasi-random market movements.

Fourth, career or reputation concerns could trigger managers to distort divestiture decisions. As described in the Introduction, in these models a manager typically makes an investment in which the payoff is informative about her (unobserved) ability. Managers have an incentive to delay divestiture decisions in order to signal higher ability, as investment abandonment signals poor quality. However, as documented in Section 4.3, the acquisition cost shocks in my setting are not predicted by the market's reaction to the acquisition or the acquirer's size. Thus, there is no evidence that these shocks might be correlated with either target or managerial quality.[25]

Finally, financial constraints might lead firms to stick with a given investment rather than change the course of action. For example, if a firm has a fixed investment budget, the incentives to abandon an investment with a relatively low return per unit of invested capital and to invest the remaining resources into projects with a higher return, can be decreasing in the costs sunk into the low-return investment. However, the acquisition cost shocks in my setting do not imply any mechanical differences in operational characteristics of acquirers such as cash holdings, as they stem from market-induced differences in the value of shares exchanged. This, in combination with the placebo tests finding no link between hypothetical acquisition cost changes and divestiture rates, does not support explanations revolving around financial constraints.

Given these considerations as well as the additional evidence of a CEO-specific channel in Section 6.2, my findings support the sunk cost effect explanation, i.e. that CEOs' commitment to investments is positively affected by the amount of unrecoverable costs they have sunk into these

[25] One possible explanation consistent with my findings is that parties evaluating managers (e.g. members of the board of directors) take sunk costs into account in their assessment of managers. In this case, managers might respond by making decisions accordingly, i.e. by taking sunk costs into account as well. It is not clear, however, whether such a response would be optimal from the manager's perspective. Also, and most importantly, this possibility does not affect the key conclusion that sunk costs systematically affect corporate decision-making.

investments.

7. Conclusion

This paper argues that managers systematically fail to ignore sunk costs in their decision-making. I isolate plausibly exogenous variation in the transaction price in stock acquisitions, unfolding after parties reach a binding merger agreement. Post-agreement acquisition cost shocks significantly predict subsequent divestiture rates of acquired businesses. Firms' propensity to divest substantially decreases when the acquisition becomes exogenously more expensive. Further results strengthen a CEO-specific interpretation, i.e. the existence of a CEO-specific sunk cost channel, and suggest that the documented divestment distortions entail sizable efficiency costs.

These findings have implications far beyond the specific context of acquisitions and divestitures. Be it developing a product, building a new plant, or even hiring talent—nearly every firm investment generates sunk costs. Sunk costs exist at all hierarchy levels and across all types of firms, generating ample opportunity for detrimental effects on firm outcomes.

Considering that the leading finance textbooks used in many MBA curricula prominently discuss the potential perils of sunk cost effects, why do managers still fail to ignore sunk costs and why do corporate governance mechanisms not prevent costly managerial mistakes? In Guenzel and Malmendier (2020), we discuss a number of important contextual factors that likely impede managerial learning and debiasing. For example, top-level managers tend to experience more successes than failures on average. They might over-infer from these successes (self-attribution bias, cf. Miller and Ross 1975) and erroneously deduce that they are not susceptible to the mistakes of the average person. In addition, one of the most significant contributions of the field of *behavioral corporate finance* has been to demonstrate that certain biases are deeply rooted and affect even the most sophisticated decision-makers. From a governance perspective, it is generally difficult to assess the causal impact of CEO behavior (Jenter and Kanaan 2015), let alone whether a specific bias distorts CEO decision-making. That said, boards could aim to find new governance responses, tailored to address the most common biases of top-level managers. How exactly such governance structures might look is understudied but a promising avenue for future research.

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Tables

Table 1. 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. Panel D reports summary statistics separated by whether or not an acquisition is subsequently divested. 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 1. *Continued*

Panel D: Balance Table						
	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

Table 2. 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 acquisition year-quarter. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

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
Observations	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)	(9)
CAR < 0	0.092								0.003
	(0.12)								(0.00)
Deal Value (ln)		0.033							0.550
		(0.15)							(1.45)
Aquirer Size (ln)			−0.114						−0.527
			(−0.55)						(−1.63)
Diversifying Deal				−0.043					−0.160
				(−0.06)					(−0.19)
Geo-Diversifying Deal					−0.282				−0.201
					(−0.24)				(−0.18)
Public Target						−0.043			−0.499
						(−0.04)			(−0.43)
Beta							−1.402		−1.485
							(−0.78)		(−0.79)
All-Stock Deal								1.531	1.857*
								(1.48)	(1.66)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Acquisition Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	558	558	558	558	558	558	558	558	558
F-Statistic (Joint Sig.)	–	–	–	–	–	–	–	–	0.56

Table 3. 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 4.4 and 5.1 as well as Appendix D 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 acquisition year-quarter. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

	(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
Number of Deals	558	558	558	558	558
Observations	4,461	4,461	4,461	4,461	4,461

Table 4. 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. Panel B presents alternative specifications. Panel C 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 3, modified as indicated by the column headers, except for the final column in Panel B, which re-estimates Column (2) of Table 3 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 3 and Section 5.2 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 acquisition year-quarter. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Panel A: Sample Restrictions

	Excl. Collars	Excl. Small-Caps	Majority-Stock	TP $\geq$ 20 Days
ΔC	-0.088^{***}	-0.074^{***}	-0.077^{***}	-0.075^{***}
	(-3.48)	(-2.75)	(-2.74)	(-3.08)
Time Interactions	Linear	Linear	Linear	Linear
Number of Deals	503	530	442	536
Observations	4,018	4,320	3,566	4,348

Panel B: Alternative Specifications

	Incl. TP Control	Incl. Year FE	$\Delta C^{\beta=1}$	Logit
ΔC	-0.077^{***}	-0.073^{***}	-0.092^{***}	-0.071^{***}
	(-3.17)	(-2.91)	(-3.92)	(-3.19)
Time Interactions	Linear	Linear	Linear	No
Number of Deals	558	558	558	558
Observations	4,461	4,461	4,461	4,461

Panel C: 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
Number of Deals	558	558	558	558
Observations	4,461	4,461	4,461	4,461

Table 5. Placebo Test I: Post-Completion Market Fluctuations

This table reports the results of the first placebo test involving hypothetical acquisition cost changes. 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 3. Please refer to Table 3 and Section 5.3 for additional details. Appendix A provides variable definitions. z-statistics are shown in parentheses. Standard errors are clustered by acquisition year-quarter. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

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
Number of Deals	558	558	558	558	558
Observations	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
Number of Deals	558	558	558	558	558
Observations	4,461	4,461	4,461	4,461	4,461

Table 6. 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 3. Please refer to Table 3 and Section 5.4 for additional details. Appendix A provides variable definitions. z-statistics are shown in parentheses. Standard errors are clustered by acquisition year-quarter. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	(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
Number of Deals	279	279	279	279	279
Observations	1,581	1,581	1,581	1,581	1,581

Table 7. Placebo Test II: Fixed Dollar Acquisitions

This table reports the results of the second placebo test involving hypothetical acquisition cost changes. 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 for *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 corresponding hypothetical market-induced change for *Fixed Dollar* acquisitions. *Fixed Shares* and *Fixed Dollar* are indicator variables. The inclusion of control variables, time interactions, and fixed effects in Columns (1) and (3) is identical to that in Column (4) of Table 6. Columns (2) and (4) correspond to Column (5) of Table 6. Please refer to Table 6 and Section 5.5 for additional details. Columns (3) and (4) are estimated on the no-collar sub-sample. Appendix A provides variable definitions. z-statistics are shown in parentheses. Standard errors are clustered by acquisition year-quarter. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

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

Table 8. Real Effects

This table reports evidence on the real effects associated with divestment distortions. *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). *Divestiture Price* is the price at which the acquired business is subsequently divested. *Relative Divestiture Price* is the divestiture price divided by the transaction price of the original acquisition. *Excess Return* is the average industry-adjusted buy-and-hold return (BHAR), across acquirers that faced a positive acquisition cost shock ($\Delta C > 0$), between an estimated and the actual divestiture announcement date. The estimated divestiture announcement date is calculated assuming a scenario in which the acquirer faced no cost shock, holding fixed all other characteristics. Please refer to Section 6.1 for additional details.

		Relative Size of Divested Business:	
	All	Large	Small
Median Relative Size	17%	33%	7%
Median Divestiture Price	$45 million	$65 million	$35 million
Median Relative Divestiture Price	50%	63%	45%
Excess Return (BHAR)	−3.80%	−6.78%	−0.86%

Table 9. 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′)). *Same CEO* is an indicator that equals one in firm-years in which the CEO who made the acquisition is still in office and zero otherwise. *New CEO* is the complement of *Same 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 6. Column (3) corresponds to Column (5) of Table 6. Please refer to Table 6 and Section 6.2 for additional details. Appendix A provides variable definitions. z-statistics are shown in parentheses. Standard errors are clustered by acquisition year-quarter. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

	(1)	(2)	(3)
ΔC	-0.073^{**}		
	(-2.49)		
ΔC × Same 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
Number of Deals	266	266	266
Observations	1,555	1,555	1,555

Figures

Figure 1. Sunk Costs in Corporate Finance Textbooks

(a) 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.

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

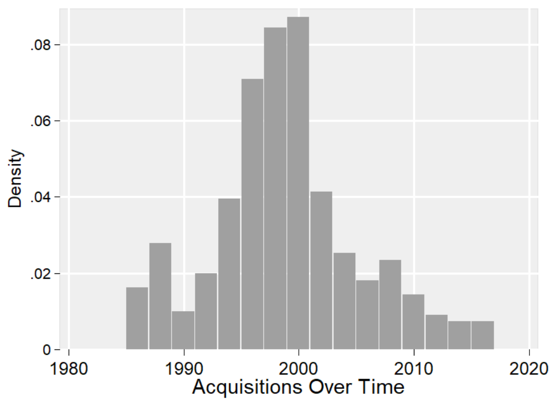
Forget Sunk Costs Sunk costs are like spilled milk: They are past and irreversible outflows. Because sunk costs are bygones, they cannot be affected by the decision to accept or reject the project, and so they should be ignored.

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.

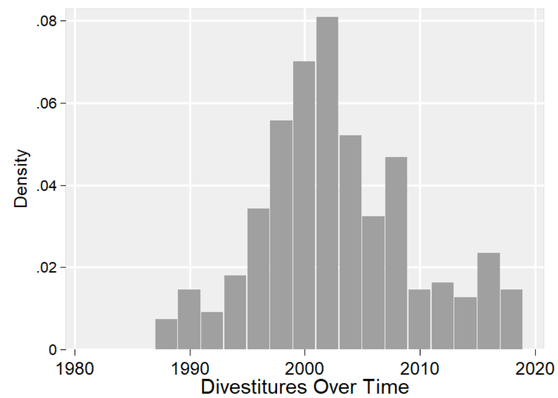
Figure 2. Acquisitions and Divestitures Over Time

This figure shows the frequency distributions of acquisitions and divestitures in my sample over time. Panel (a) shows acquisition frequencies. Panel (b) shows divestiture frequencies. Panel (c) shows the distribution of the time span between acquisition and divestiture in years.

(a) Acquisitions Over Time



(b) Divestitures Over Time



(c) Years Between Acquisition and Divestiture

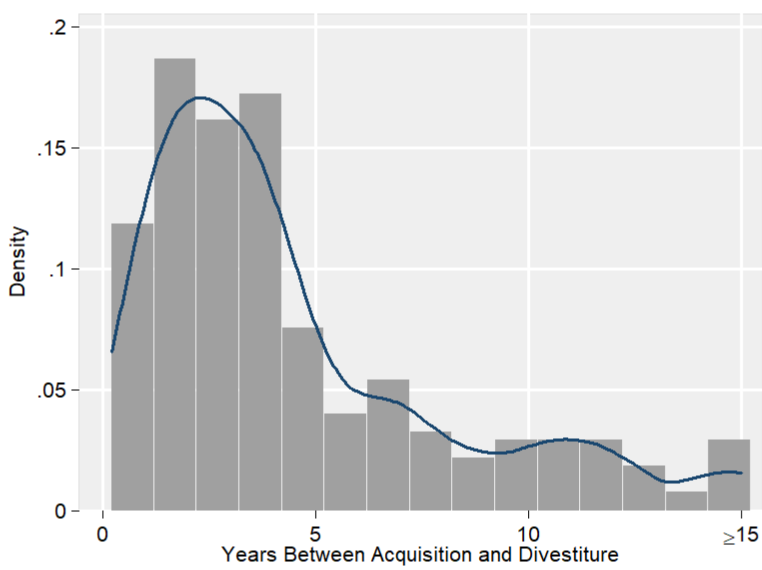
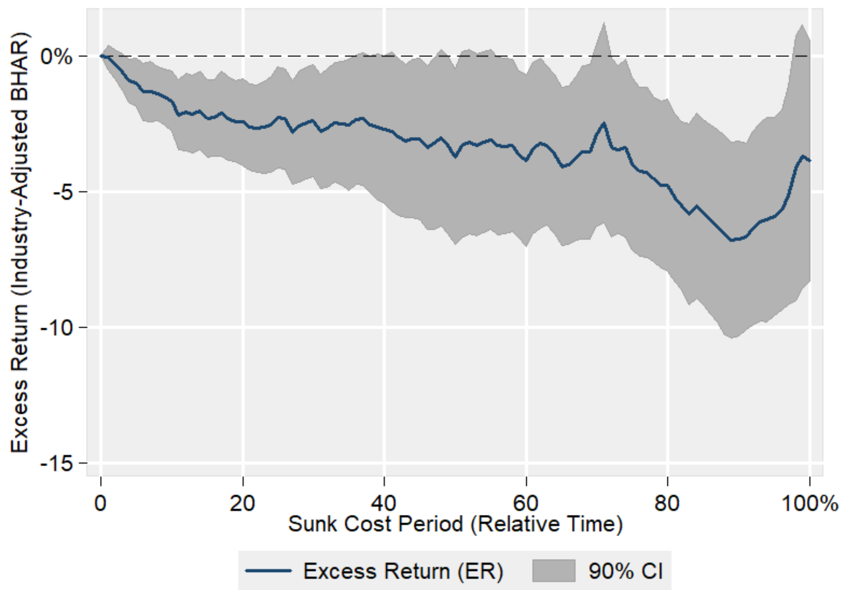


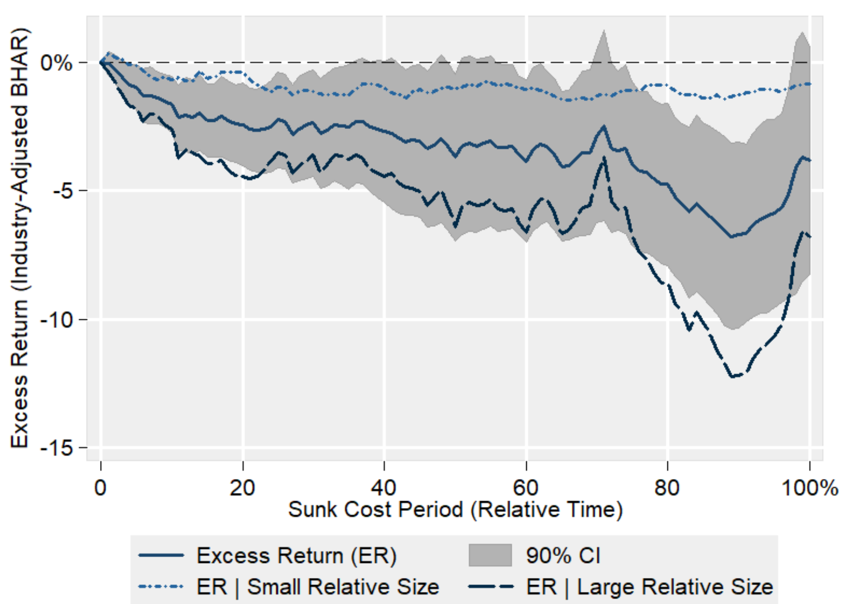
Figure 3. Real Effects

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 dotted line) and above-median (dark blue dashed 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. Since the length of the sunk cost period differs across observations, 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 6.1 for additional details.

(a) Excess Return (Industry-Adjusted BHAR)



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



Appendix

Section A provides variable definitions. Section B is a data appendix. Section B.1 provides supplementary information on the construction of the main dataset involving divestitures of previous acquistions. Section B.2 provides examples of acquisition terms collected from SEC filings, news wires, and newspaper articles. Section C contains additional tables and figures. Section D contains further details on the Cox (1972) proportional hazards model, including how to test for proportional hazards. Section E presents the results of the alternative two-stage estimation approach.

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 the Fama and French (1997) definition using 49 industry portfolios, 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
Transaction Period	Period between two days after the date of the merger agreement and the merger completion date

Variable	Definition
Panel B: Firm-Related and Time-Varying Variables	
12-Month Return	Acquirer's stock return over the previous year, calculated from monthly stock data
Beta	Acquirer industry's sensitivity with the market; following Krüger et al. (2015), estimated using 60-month rolling regressions of the returns to the value-weighted portfolio of firms in the acquirer's Fama and French (1997) industry, based on 49 industry portfolios, on the returns to the CRSP value-weighted index (including distributions); for each acquisition, the estimation window includes the returns in the 60-month window ending in the month prior to the merger agreement date
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; the distress definition combines a forward-looking measure (median firm's two-year stock return below 30%; Opler and Titman 1994, Babina 2019) and a backward-looking measure (recent industry performance across all Fama and French (1997) 49 industries in the bottom quintile; Dinc et al. 2017)
Market Cap	See *Acquirer Size*
New CEO	Indicator variable that equals one in firm-years in which the CEO who made the acquisition is no longer in office and zero otherwise
Same CEO	Indicator variable that equals one in firm-years in which the CEO who made the acquisition is still in office and zero otherwise
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, i.e. 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 6.1 for details on the construction of counterfactual divestiture announcement dates

Appendix B Data Appendix

B.1 *Additional Detail on Divestitures of Previously Acquired Businesses (Section 3.1)*

M&A Sample Construction. In a first step, I download all transactions by U.S. acquirers between 1980[B1] and 2016. Since my identification strategy (see Section 4) 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.[B2] 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.[B3]

Taken together, these filters result in a final M&A sample of 7,862 acquisitions. Appendix-Table B.1 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 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.

[B1] I follow Betton et al. (2008) in choosing 1980 as the starting year for the analysis. SDC only contains 66 observations prior to 1980.

[B2] 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).

[B3] In all my analyses, I elevate this threshold to ten days (see Section 3.2 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.

Appendix-Table B.1. M&A Sample Construction

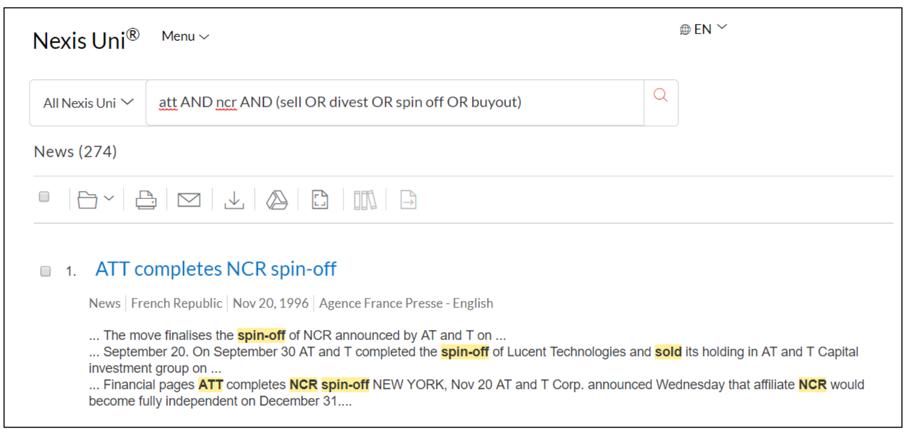
	Sample Size
Announced acquisitions financed at least partially with stock, 1980-2016	21,796
Observations remaining after restricting to	
Status: Completed	18,328
U.S. Target	16,500
Target type: Public, Private, or Subsidiary	16,387
Deal type: Disclosed deal or Undisclosed Deal	16,074
Percentage of shares 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 $\geq$ 7000)	5,893

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 examine the accuracy of each divestiture in more detail.

Identifying Divestitures Through Nexis. As specified in the main text, I perform the news search by connecting acquirer and target names with divestiture-related words (sell, divest, spin off, buyout) through Nexis' AND and OR operators. 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).

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 which, as explained in the paper, is not detected by in the SDC-based approach.[B4] 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 Appendix-Figure B.1).

Appendix-Figure B.1. Nexis Search Results for AT&T-NCR



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."

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."[B5] 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.[B6]

[B4] 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.

[B5] Cf. sec.gov/Archives/edgar/data/772197/0000950144-99-003700.txt.

[B6] Cf. sec.gov/Archives/edgar/data/772197/0000950170-98-000591.txt and sec.gov/Archives/edgar/data/1019808/0001019808-99-000002.txt, respectively.

Appendix-Table B.2 provides a step-by-step overview of the final divestiture sample construction from the initial sample of full divestitures.

Appendix-Table B.2. Divestiture Sample Construction

This table presents an overview of the divestiture sample construction. See Sections 3.1, 3.2, and 3.3 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.2 Additional Detail on Collection of Acquisition Terms (Section 3.2)

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.

Appendix C Additional Tables and Figures

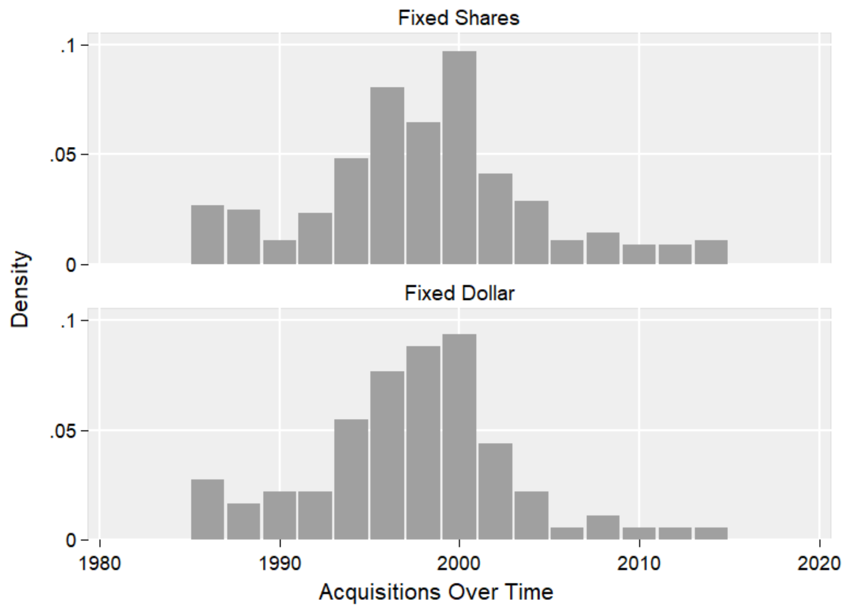
Appendix-Table C.1. Divestiture Predictors

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 Appendix B.1), 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. 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 acquisition year-quarter. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

	(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
Observations	6,458
Pseudo R-squared	0.14

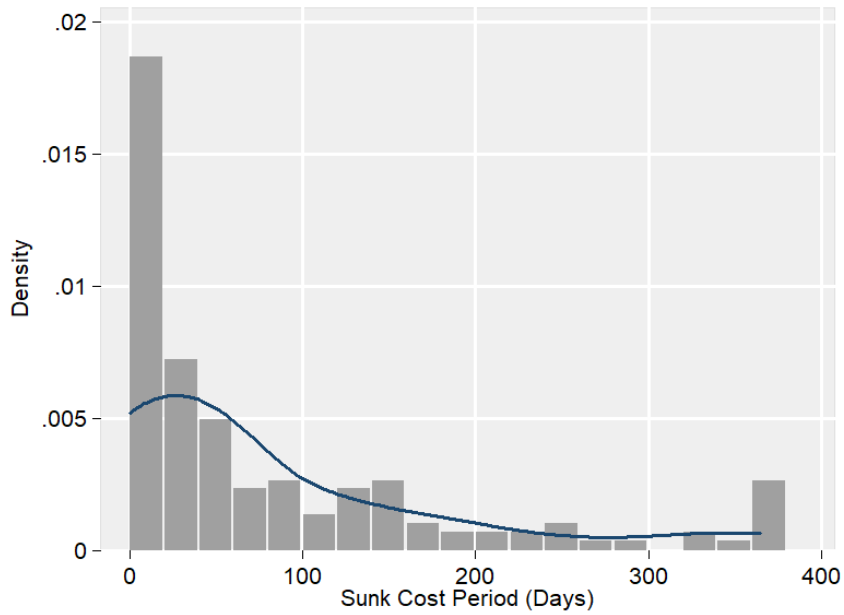
Appendix-Figure C.1. Fixed Shares vs. Fixed Dollar Deals: Acquisitions Over Time

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



Appendix-Figure C.2. 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 6.1 for additional details on the construction of the sunk cost period.



Appendix D 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.[D1]

Construction of Schoenfeld Residuals. The Cox (1972) model assumes that the effect of covariates on the hazard rate is constant across time. 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[D2] 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 $\hat{\theta}_k = \frac{\sum_{s=1}^{D} (t_s - \bar{t}) \left(r_{s,k}^{scaled} - \bar{r}_k^{scaled} \right)}{\sum_{s=1}^{D} (t_s - \bar{t})^2} = \frac{\sum_{s=1}^{D} (t_s - \bar{t})\, r_{s,k}^{scaled}}{\sum_{s=1}^{D} (t_s - \bar{t})^2}$ 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 $T_k(\hat{\theta}) = \frac{\hat{\theta}_k^2}{\text{Var}\left(\hat{\theta}_k\right)}$, 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 Appendix-Tables D.1 and D.2 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 (unreported but available upon request), 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 3 that have a p-value of 0.15 or less in Appendix-Table D.1 or D.2, 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

[D1] Some of the discussion of Schoenfeld residuals is based on material by Dan Dillen, available at ics.uci.edu/dgillen/STAT255/Handouts/lecture10.pdf.

[D2] To be precise, one uses a scaled version of these values, weighted by the inverse of the covariance matrix of $\hat{\boldsymbol{\beta}}$.

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.

Appendix-Table D.1. 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 3. The definitions of ρ and T are provided on page 62. 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

Appendix-Table D.2. 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 6. The definitions of ρ and T are provided on page 62. Appendix A provides variable definitions.

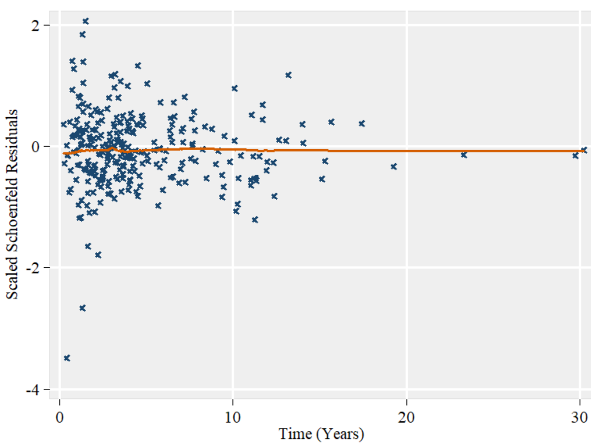
	ρ	T	p-Value
ΔC	−0.019	0.18	0.67
CAR < 0	−0.122	8.49	0.00
Deal Value (ln)	0.062	2.33	0.13
Aquirer Size (ln)	−0.065	2.51	0.11
Diversifying Deal	−0.037	0.77	0.38
Geo-Diversifying Deal	−0.066	3.12	0.08
Public Target	0.111	6.33	0.01
All-Stock Deal	0.110	6.49	0.01
Beta	−0.039	1.28	0.26
12-Month Return	0.003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

As described above, another useful visual Schoenfeld test is to plot the Schoenfeld residuals against a function of time. Appendix-Figure D.1 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 Appendix-Table D.2. For the cost change variable in Panel D.1a, 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 D.1b, instead, the smoothed line fluctuates over time, supporting the inclusion of time interactions for this variable.

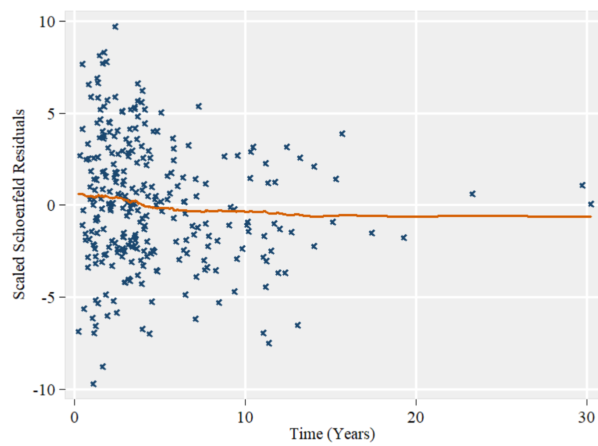
Appendix-Figure D.1. Schoenfeld Residuals Against Time

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 62 for additional details on the construction of Schoenfeld residuals.

(a) ΔC



(b) CAR < 0



Appendix E Two-Stage Control Function Approach

This appendix 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 3 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 3 (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).

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

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 E.1 presents the second-stage results. The results corroborate those presented in the main part of the 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.

Appendix-Table E.1. Two-Stage Control Function Approach

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 3. Column (2) corresponds to Column (4) of Table 3. 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 65, 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	
Number of Deals	558		558	
Observations	4,461		4,461	