Fewer Lenders, Different Lenders: Equity Lending Fees and Financial Institution Mergers

Mohammad Alrukhais* David De Angelis† Hitesh Doshi‡ Parsa Pourghasem§

Abstract

Using 203 financial institution mergers from 2007 to 2023 as exogenous shocks to ownership structure, we show that consolidation among institutional owners raises equity lending fees by 5.3%. The increase does not reflect scarcity: utilization barely moves, and for the 85% of positions the merged institution keeps, supply remains essentially unchanged while fees rise by 4.6%. Passive acquirers raise fees more than active ones, 6.8% versus 3.8%, while withdrawing less supply, providing the first causal evidence that owner type, not just concentration, is priced. Consolidation also makes fees more volatile and slows price discovery: lender market power tightens short-sale constraints.

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*Mohammad.alrukhais@ku.edu.kw. College of Business Administration, Financial and Financial Institutions Department, Shadadiya, Kuwait University.

†deangelis@bauer.uh.edu. C.T. Bauer College of Business, University of Houston, 4250 Martin Luther King Boulevard, Houston, TX 77204, USA.

‡hdoshi@bauer.uh.edu. C.T. Bauer College of Business, University of Houston, 4250 Martin Luther King Boulevard, Houston, TX 77204, USA.

§ppourgha@cougarnet.uh.edu. C.T. Bauer College of Business, University of Houston, 4250 Martin Luther King Boulevard, Houston, TX 77204, USA.

1 Introduction

The equity lending market is an important part of modern financial markets, enabling short selling, hedging, and price discovery.[1] To sell a stock short, an investor must first borrow shares from a lender, typically a large institutional investor, and pay a lending fee. When that fee is high, short selling becomes more costly, which can slow the incorporation of negative information into prices (D'Avolio, 2002; Duffie et al., 2002; Jones and Lamont, 2002). A large literature examines how such short-sale constraints affect market efficiency, but causal evidence on the supply-side forces that generate them is still limited.

Lack of competition among lenders can be one such force. Simple summary statistics suggest that the equity lending market is not perfectly competitive: in our sample, the median stock has only about 6% of its total lendable inventory on loan, yet borrowers still pay a positive fee to obtain shares. Under competitive supply and a low cost of lending an idle share, unlent inventory should compete the fee down toward that cost. Consistent with this, fees remain above competitive levels while inventory sits unused (Chen et al., 2022), and identical shares of the same stock trade at materially different fees across lenders on the same day (Kolasinski et al., 2013).

What gives lenders this pricing power? Equity lending is an over-the-counter market in which borrowers and lenders negotiate bilaterally (Duffie et al., 2002), and borrowers cannot easily compare prices across lenders. A natural determinant is therefore how many independent lenders hold the stock: when several are present, a borrower can solicit competing offers and bargain the fee down, and consolidation should weaken that discipline. A second, less obvious determinant is the type of owner who controls the lendable shares. A loan request can signal that an informed trader is betting against the stock. An active owner can act on that signal—recall the loan and sell ahead of the borrower—so borrowers avoid its shares unless the fee is low enough to compensate; a passive owner poses no such threat and need

[1] Globally, equities on loan total about $1.3 trillion as of June 2025. See the International Securities Lending Association (ISLA), *23rd Edition Securities Lending Market Report, H1 2025* (https://www.islaemea.org/thought-leadership/23rd-edition-securities-lending-market-report-h1-2025/).

not offer the discount (Nurisso, 2026). This paper asks two questions. Does consolidation among institutional owners raise the price of borrowing stock? And does the type of owner who ends up holding the shares matter for how they are priced?

Causal evidence on these questions is limited. There is some evidence that fees tend to be higher and supply tighter when ownership is more concentrated, identified from activist 13D stakes and dual-class shares (Porras Prado et al., 2016). Yet these sources of variation are chosen by investors and firms and are therefore themselves functions of the stocks studied. To our knowledge, the ownership-type prediction has not been tested; testing it would require holding the characteristics of the stock fixed while varying the type of owner who controls its lendable shares.

We address these gaps using financial institution mergers as exogenous shocks to the ownership structure of the stocks the merging institutions hold in common. When two institutions that both held a stock are consolidated into a single entity, the number of independent lenders for that stock falls mechanically, while the underlying firm and the demand to short it are essentially unaffected. Crucially, mergers happen for reasons of their own: institutions combine to expand geographically or across product lines, to absorb a distressed rival during the crisis, or under regulatory pressure—not because of lending conditions in any particular stock they hold. The setting addresses the standard identification concerns directly: stock-level omitted variables are differenced out by within-stock comparisons inside narrow event windows; reverse causality is addressed because the merger is not a portfolio choice responding to the lending-fee dynamics of overlapping holdings; and simultaneity is broken by a sharp, discrete drop in the number of independent lenders at the announcement rather than a gradual re-matching. The same shock also transfers an existing block of shares to a new owner while the stock itself is unchanged, which is precisely the variation needed to test the ownership-type prediction in Nurisso (2026).

Our sample comprises 203 financial institution mergers announced between 2007 and 2023—mergers in which the target's 13(f) portfolio was subsequently absorbed, collapsed

to below half its pre-merger size, or converged toward the acquirer's holdings within two years of the announcement, so that the two lendable inventories plausibly combined. We estimate a stacked difference-in-differences design following Cengiz et al. (2019) and Baker et al. (2022), which is robust to the biases that arise in staggered settings with heterogeneous treatment effects (Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). Stocks held by both merging institutions before the announcement are treated; stocks held by exactly one are controls, restricted to *clean controls* that are not treated by any other merger within twelve months. Our preferred specification includes merger-by-firm, merger-by-month, and industry-by-month fixed effects, and standard errors are clustered at the merger level. In using financial institution mergers for identification we build on Friberg et al. (2024), who study the BlackRock–Barclays Global Investors merger of 2009 as a single-merger shock, and He and Huang (2017), who use a stacked design across mergers to study product-market outcomes; we apply the same insight to the equity lending market.

Ownership consolidation raises the price of borrowing stock. In our preferred specification, treated stocks experience a 5.3% increase in lending fees relative to controls following the merger; evaluated at the treated stocks' announcement-month mean fee of 64 basis points, this corresponds to a raw increase of roughly 3.4 basis points. The event-study coefficients are flat and statistically insignificant before the announcement, shift up at the announcement, and remain elevated throughout the following twelve months—a persistent tightening of short-sale constraints rather than an announcement blip. The estimates are essentially unchanged when we exclude the fifteen mergers announced during the 2008–2009 financial crisis, so the result is not an artifact of crisis-period disruptions in the lending market.

Fees rise; the question is how. Consolidation can raise the price of borrowing in two ways: the merged institution can withhold shares and make them scarce, or it can charge more for shares that remain on offer. Porras Prado et al. (2016) document the first in the cross-section—a one-standard-deviation increase in ownership concentration comes with

lending supply 3.0% below its mean and a fee 5.2 basis points above a mean of 71 basis points, which they read as the price of shares that concentrated owners withhold. We test that channel with the supply borrowers actually face, the shares owners actively offer for lending, and with active utilization, the share of that supply already on loan. At first glance the withholding story fits: active lending supply falls by 0.43 percentage points ($t = -4.78$). A closer look shows that it does not. Utilization barely moves—it rises by 0.43 percentage points, significant only at the 10% level, and the median stock still has just 6.9% of its active supply on loan—so the market is no tighter after the merger than before it.

Sorting treated stocks by when the merged institution sells the position separates the two channels: positions sold within a year see the largest response on both margins, a 9.7% fee increase and a withdrawal of 2.38 percentage points of active supply; positions sold during the second year see a 5.8% fee increase and lose 0.93 percentage points of supply; and the 85% of positions that are never sold see a 4.6% fee increase while their supply falls by just 0.6% of its level. The first two groups follow Nurisso (2026): an owner that intends to sell charges a higher fee and lends less, so that short sellers cannot push the price down before it sells. Owners are known to withdraw shares when lending conflicts with another goal—institutions recall shares in order to vote them (Aggarwal et al., 2015), and lending decisions respond to shareholder–creditor conflicts (Chu et al., 2025)—and holding status is an outcome rather than a predetermined characteristic, so we read this split cautiously. The last group is the one that matters for the mechanism: concentration raises the price of borrowing where the withdrawal of supply is trivial, so our evidence diverges from the cross-sectional relation and points to market power rather than scarcity.

We study the mechanism by exploring heterogeneity in the effect. Fees are set between short sellers, who want to borrow shares, and the institutional owners who supply them. When demand to short a stock is high, those owners compete for borrowers, and that competition holds the fee down. A merger removes one such owner: where competition was active, the remaining owners can now charge more; where no one was borrowing, there was

no competition to remove, so the fee barely moves. The data match this prediction. The fee increase is larger for high-volatility stocks and for more liquid stocks (higher turnover, lower Amihud illiquidity), rises—albeit with marginal statistical significance—with the fraction of lendable supply already on loan before the merger, and grows with the size of the overlapping stake that the merger consolidates. These are all features of active short-selling environments: consolidation raises fees most where competition among owners had been holding them down.

The type of acquirer matters in exactly the way the theory predicts. Measuring acquirer activeness by pre-merger portfolio churn (Gaspar et al., 2005) and evaluating the estimates one standard deviation below and above the mean, we find that a passive, buy-and-hold acquirer raises the fee by about 6.8% while cutting active lending supply by 0.27 percentage points, whereas an active acquirer raises the fee by 3.8% while cutting supply by 0.61 percentage points (estimates from the eight-quarter churn window; the twelve-quarter window is similar). The owner who keeps more shares available is the one who charges more for them—the joint price-and-quantity pattern predicted by Proposition 5 of Nurisso (2026), and the opposite of what a scarcity mechanism would produce. Classifying both merging parties points the same way: when an active acquirer absorbs a passive target, so that the block moves from a passive owner to an active one, the fee increase falls to an insignificant 1.7%.

Consolidation also makes borrowing less predictable and prices less efficient. Short sellers care not only about the level of the fee but about its variability (D'Avolio, 2002; Engelberg et al., 2018), and after the merger the within-month variance of the daily log fee rises by 11.5% for treated stocks relative to controls, with the gap widening over the first post-merger year rather than reverting. Prices respond to market-wide information more slowly as well: the price-delay measure of Hou and Moskowitz (2005) rises by 0.026, about 10% of its sample mean, while the same comparison one year earlier yields 0.0005. Higher and less predictable borrowing costs therefore translate into slower price discovery.

In summary, our analysis yields five main insights. First, ownership consolidation causes lending fees to rise: supply-side market structure is an important causal determinant of the cost of shorting, which supports the market-power view of the securities lending market in Chen et al. (2022) and extends the evidence that ownership concentration generates anticompetitive price effects (Azar et al., 2018) from product markets to financial markets. Second, the fee increase does not come from scarcity: fees rise even for the positions the merged institution keeps, whose active supply is barely reduced and whose utilization hardly moves, so the higher price reflects the pricing power of the consolidated owner rather than a contraction in the supply borrowers face. Third, the type of owner matters: passive acquirers raise fees more while withdrawing less supply, providing, to our knowledge, the first empirical support for the ownership-type predictions of Nurisso (2026). Fourth, the consequences reach beyond the level of fees: borrowing becomes less predictable and prices incorporate market-wide information more slowly, which to our knowledge is the first causal evidence that ownership concentration slows price discovery and links supply-side concentration to the limits-to-arbitrage evidence in Engelberg et al. (2018) and Saffi and Sigurdsson (2011). Fifth, the results identify a consequence of financial institution mergers that the literature has not examined: consolidation among owners spills over into the market for borrowing the stocks they hold, alongside the effects on stock-price fragility and firm financial policy documented by Friberg et al. (2024).

2 Related Literature

Our paper sits at the intersection of several literatures: work on ownership structure and equity lending outcomes, research on market power and strategic behavior by lenders, the broader literature on the functioning and costs of equity lending, studies that use financial institution mergers for identification, and the literature on common ownership. We discuss each in turn.

The paper most closely related to ours is Porras Prado et al. (2016), which examines how the concentration and composition of institutional ownership affect equity lending supply and fees in the cross-section of U.S. stocks. Using 13F and Markit lending data, they show that ownership structure matters for lending outcomes: stocks whose ownership is more concentrated have less lendable supply, more expensive borrowing, and tighter short-sale constraints. While our paper is similar in focus, it advances the literature by moving from correlation to causation. The cross-sectional design in Porras Prado et al. (2016) faces two identification challenges. The first is omitted variables: stocks with concentrated ownership may simply be inherently different—harder to value, less liquid, or more opaque—and these characteristics could independently reduce lending supply, making it impossible to isolate the effect of ownership concentration itself. Porras Prado et al. (2016) address this with propensity score matching, but matching controls only for observable stock characteristics, so unobserved differences between stocks remain. The second is reverse causality: concentrated ownership may not cause lower lending supply—instead, stocks with scarce lending supply may attract certain types of concentrated owners. They address this by exploiting SEC 13D filings as ownership shocks, but activists do not select target firms randomly; they target firms based on specific characteristics that may themselves be correlated with lending-market conditions, so the ownership shock is endogenous by construction. We address both concerns by exploiting financial institution mergers, which are driven by institution-level motives—cost synergies, regulatory considerations, strategic fit—that are unrelated to the lending-market conditions of the affected stocks. This allows us to credibly identify the causal effect of ownership concentration on lending supply and fees.

A second body of research examines the supply side of the lending market and how lenders use their market power to set lending fees. Chen et al. (2022) develop a dynamic model of asymmetric information and find that the lending market is highly concentrated: the top two lenders in a given stock control between 40% and 70% of the available supply. Using structural estimation, they show that lenders optimally hold excess inventory rather than

reduce fees, a form of rational rationing that helps maintain market power. Their counterfactual analysis further shows that the intermediated market structure may actually benefit informed short sellers in illiquid stocks relative to a perfectly competitive benchmark. Henderson et al. (2023) provide complementary evidence on the strategic behavior of individual lenders. They decompose observed lending fees into "intrinsic" and "abnormal" components and show that lenders systematically offer discounts on stocks with elastic demand (where volume can be stimulated) while maintaining premiums on stocks with inelastic, information-driven demand. This revenue-maximizing behavior implies that lenders possess significant information about the identity and motives of borrowers. Johnson and Weitzner (2025) model a conflict of interest that arises when the lending agent sits inside the fund family: because part of the fee income flows back to the manager, portfolios tilt toward stocks that are expensive to borrow—precisely the ones most likely to be overpriced. Using mandatory SEC disclosures, they show that funds retaining fee income indeed tilt toward special stocks, at a cost to their investors. Most directly related to our second research question, Nurisso (2026) models the informational side of the market: a loan request signals informed selling pressure, and a lender that can trade on that signal is tempted to recall the loan and sell first; to attract borrowers, such a lender must set a fee high enough that forfeiting it makes recall unprofitable. A passive lender faces no such temptation and, in the model's equilibrium, prices loans higher while lending more. We contribute to this literature in two ways: we show that supply-side concentration—driven by financial institution mergers—is itself a source of market power in the lending market, and, to our knowledge, we provide the first empirical test of the ownership-type predictions in Nurisso (2026).

A foundational line of research documents how the equity lending market functions and quantifies the costs it imposes on short sellers. D'Avolio (2002) provides an early, comprehensive characterization of the market for borrowing stocks using data from a major institutional lender. He shows that lending fees vary widely across stocks: about 9% of borrowed stocks are classified as "specials," with average annual borrowing fees above 4.3% and much higher

fees in some cases. He also finds that institutional ownership explains about 55% of the cross-sectional variation in lending supply. Saffi and Sigurdsson (2011) extend this analysis internationally, showing across 12,621 stocks in 26 countries that higher lending supply is associated with faster price discovery and lower return predictability, without raising the probability of extreme negative returns. Kaplan et al. (2013) study a natural experiment in which a large institutional manager randomly allocates a portion of its high-fee holdings to a treatment group available for lending; the supply shock reduces fees and increases short interest but leaves prices, volatility, and liquidity unaffected, suggesting that the overpricing caused by short-sale constraints may be overstated. Engelberg et al. (2018) find that the risk of unexpected fee increases or early loan recalls—"short-selling risk"—discourages arbitrage and contributes to mispricing. This literature establishes that borrowing costs in the equity lending market are economically important, but it leaves open what forces generate these costs. Our merger design supplies what this literature lacks: a causal source of variation in lending fees, namely changes in institutional ownership concentration.

Methodologically, we build on recent work that uses financial institution mergers as shocks to ownership structure: Friberg et al. (2024) exploit the BlackRock–Barclays Global Investors merger of 2009 to study stock price fragility and firms' financial policies, and He and Huang (2017) use a difference-in-differences design across mergers to study how institutional cross-ownership affects product-market outcomes. We bring this identification approach to the securities lending market, combining a stacked design across 203 mergers with single-merger analyses of selected cases.

Finally, our paper relates to the broader body of work on common ownership. Azar et al. (2018) demonstrate that cross-sectional variation in common institutional ownership in US airline markets is related to greater ticket prices and thus document common ownership's negative effect on product market competition. Although their research focuses on product markets rather than lending markets, the mechanism is analogous: investors holding shares in multiple firms in a given market may decrease competitive pressure in that market. In

our context, when two financial institutions merge, the new institution holds a larger block of shares in portfolio firms, reducing the number of independent lenders in the securities lending market and thus potentially decreasing competition in this market.

3 Data and Empirical Methodology

3.1 Data Sources

Financial Institution Mergers: We derive our financial institution merger sample using SDC's *Mergers and Acquisitions* database. For mergers announced between 2007 and 2023, reflecting the coverage of our securities lending data, we impose several restrictions. First, we include only mergers with the deal type *Merger* or *Acquisition of Assets*. Second, we require both the target and the acquirer, or their parent firms, to have filed a 13(f) report in the quarter preceding the announcement, since the design compares stocks held by both institutions with stocks held by only one. Third, we restrict attention to deals in which the acquirer is a financial institution, using SDC's mid-industry classification, and we exclude insurance targets. Fourth, we require the acquirer's reported 13(f) portfolio to exceed $1 billion at the quarter before the announcement, so that the merger involves an institution large enough for consolidation to affect lendable inventory materially; we impose no restriction on the transaction value, which is missing for many deals between privately held asset managers. Fifth, we require each merger to contribute at least ten treated and ten control stocks after the clean-control restriction described in Section 3.3.

A challenge we face is that the current SDC database does not contain identifiers such as CIK, 8-digit CUSIP, or GVKEY, which makes a direct merge with the 13(f) data difficult. Matching directly on the 6-digit CUSIP produces both incorrect matches and missing values. To overcome this, we first perform a preliminary text match and then verify each pair manually against the Thomson Reuters s34 master file. In performing our match we do not require an exact match on names; we allow a match on a parent firm where the parent is the

13(f) filer. For example, for Barclays Global Investors Ltd we use Barclays Bank PLC, and for Lehman-Invest Bkg Bus we use Lehman Brothers Inc.

Finally, we retain only mergers in which the two institutions' portfolios plausibly combined. Comparing the target's 13(f) holdings in the quarter before the announcement with the same holdings eight quarters later, we keep deals in which the target no longer reported any holdings (176 deals), still reported holdings but with a portfolio below half its pre-merger size (10 deals), or still reported holdings that converged toward the acquirer's, measured as an increase of more than ten percent in the Jaccard overlap between the two books (17 deals). We judge survival on holdings rather than on filing dates, because Thomson reuses manager identifiers: Merrill Lynch appears to file for thirty-three quarters after Bank of America acquired it, while its reported holdings are zero. This procedure leaves us with 203 mergers.

Securities Lending: We obtain security-level securities lending data from Markit Securities Finance from January 2006 to December 2024. This time period enables us to observe lending activity in the firm for up to a year before and after the merger announcement. Our dataset contains daily observations on the quantity of shares available for lending, quantity actively made available for lending, quantity and value of shares on loan, indicative lending fee, and utilization and active utilization rates. In order to handle multiple observations on the same CUSIP on the same date, we keep the observation with the highest value of shares on loan. We aggregate these daily observations to a monthly level by constructing a calendar month from the trading date and collapsing these data to a CUSIP-month level by taking the monthly mean of each lending measure. Finally, we focus on firms trading in the United States, using a market area classification provided by Markit. Our main dependent variable is the log lending fee, which represents the expected cost of borrowing a stock in the lending market. We compute it as the monthly average of the daily log indicative fee. Two further outcomes are active lending supply, the quantity of shares actively made available for lending as a percent of shares outstanding, and active utilization, the value of shares on loan as a

percent of active lendable value. We also use the daily data to compute the within-month variance of the daily log fee, requiring at least 15 daily observations. In the cross-sectional analysis we also use utilization, measured two months before the merger announcement and defined as the value of shares on loan divided by total lendable value; it captures how much of the firm's supply is already in use before the merger.

Financial Institution Holdings: Our data on institutional holdings come from the Thomson Reuters Institutional (13F) Holdings *s34 Master File*. We measure institutional holdings using data from the quarter before the merger announcement. Our treatment group is the set of securities held by both the target and the acquiring institution during the pre-announcement quarter. Our control group is the set of securities held by only one of the two institutions during the pre-announcement quarter. We also use the s34 data to measure each institution's portfolio churn in the quarters before the announcement, and to track whether the merged institution still holds each treated stock one and two years after the announcement.

Firm-Level Characteristics: We employ several firm-level characteristics to investigate cross-sectional variation in the treatment effect. From the CRSP database we obtain market capitalization, which we use as a control, and construct two measures of stock liquidity. The first is turnover, monthly trading volume scaled by the number of shares outstanding. The second is the Amihud (2002) illiquidity measure: for each stock-day, we compute the absolute value of the stock's daily return divided by its daily dollar trading volume (the absolute value of price times volume), and we average this ratio over each stock-month, requiring at least 10 trading days in the month. Finally, we use OptionMetrics to measure firm volatility as 365-day historical volatility. Beyond these characteristics, we use CRSP daily returns to construct weekly stock returns and value-weighted market returns for the price-delay measure of Hou and Moskowitz (2005).

Turnover, illiquidity, volatility, and utilization are measured two months prior to the merger announcement ($t = -2$), and each stock-merger pair carries that value over the full

event window. We then divide firms into terciles within each merger based on the pre-merger distribution of each characteristic. Tercile 1 consists of firms with low values of the firm characteristic, tercile 2 consists of firms with medium values, and tercile 3 consists of firms with high values. Market capitalization enters only as a control variable and not as a conditioning characteristic: we average log market capitalization over the twelve months preceding the announcement, and all monthly specifications include the interaction of this average with event time to absorb differential drift by firm size. Averaging over the full pre-period rather than a single month means size is available for every stock-merger pair with pre-announcement CRSP coverage, rather than only those observed in month $t = -2$.

3.2 Descriptive Statistics

Tables 1 and 2 describe the sample. The stacked sample runs from January 2006 to December 2024. After applying the data requirements discussed above, there are 2,390,043 stock-month observations over this time span. During this time, 203 financial institution mergers satisfy our screening criteria. Panel A of Table 2 indicates that the mean of the log lending fee in the stacked sample is -5.125 with a standard deviation of 1.240 and a median of -5.586. The interquartile range of the lending fees is relatively narrow, ranging from -5.828 at the 25th percentile to -5.301 at the 75th percentile. This corresponds to annualized lending fees of roughly 0.30% to 0.50%. These values are consistent with a sample dominated by securities that do not face substantial borrowing scarcity.

Panel A also reports two quantity measures of the lending market. Active lending supply—the quantity of shares actively made available for lending as a percent of shares outstanding—averages 23.9% with a median of 25.8%. Active utilization—the value of shares on loan as a percent of active lendable value—has a median of only 6.9%, consistent with the low fees, but a mean of 19.3%. This distribution is right-skewed: most stocks have ample supply relative to borrowing demand, while a subset of stocks already has a large share of its active supply on loan.

Table 1 provides a comparison of treated stocks and control stocks in the announcement month before the consolidation of ownership due to the merger. For the stacked sample, treated stocks have a mean log lending fee of -5.607, while control stocks have a mean of -4.753, a difference of -0.854 log points. Treated stocks also have more active lending supply (31.2% versus 18.6%) and lower active utilization (10.6% versus 26.3%). These pre-announcement gaps likely reflect a composition effect: stocks held by both the target and the acquirer—the treated group—tend to be widely held by institutions, the main source of lendable shares, so they are easier and cheaper to borrow than stocks held by only one party. These pre-merger differences are absorbed by the stock-merger fixed effects.

In Panel B of Table 2, volatility, turnover, utilization, and Amihud illiquidity are measured two months prior to the merger announcement, and market capitalization is the exponentiated mean of log market capitalization over the twelve months before the announcement. They are thus fixed for each stock-merger pair throughout the event window. Stocks in our sample have a mean market capitalization of $31.3 billion and a median market capitalization of $1.8 billion. As is well known, the distribution of market capitalization is right-skewed. Annualized historical volatility is on average 43.4%, with an interquartile range of 26.0%–53.1%. Monthly turnover is on average 20.4%. Pre-event utilization—the proportion of the value of shares already on loan to the total lendable value—is on average 14.0% with a median of 5.8%, so this distribution, too, is right-skewed: a subset of stocks starts the event window with lending supply already substantially utilized, while the majority start with much lower utilization rates. Amihud illiquidity is very low at the median and highly dispersed, with a mean of 0.677×10^{-6} and a standard deviation of 27.9×10^{-6}, which is in line with the fact that this sample consists mostly of liquid exchange-listed equities. The above statistics, taken together, suggest that this sample represents a broad cross-section of U.S. equities held by large financial institutions, with significant variation in volatility, liquidity, and pre-merger utilization—dimensions along which we examine the heterogeneity of the treatment effect.

3.3 Empirical Methodology

Our empirical strategy exploits variation in institutional ownership concentration generated by financial institution mergers as an exogenous shock to the ownership structure of affected stocks. The core identification assumption is that, absent the merger, stocks held simultaneously by both the target and the acquirer (treated firms) would have followed the same lending-market trajectory as stocks held by only one of the two institutions (control firms). Our specification is a *stacked* difference-in-differences estimator that pools information across all 203 mergers in our sample in a manner robust to heterogeneous treatment effects.

A concern when using a pooled approach with treated and control units across multiple merger events is the bias from heterogeneous treatment effects, as discussed in the staggered difference-in-differences literature (e.g., Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). Because the 203 mergers occur at different dates over the 2007–2023 sample period, a naive pooled two-way fixed effects estimator may be biased. This can happen if treatment effects differ across groups or if already treated stocks serve as implicit controls for other merger events.

To address these concerns, we follow the stacked regression strategy of Cengiz et al. (2019) and Baker et al. (2022). Specifically, for each merger m announced at time τ_m, we construct a separate event dataset containing: (i) all treated stocks, i.e., those held simultaneously by the acquirer and target in the pre-announcement quarter, and (ii) all clean control stocks, i.e., those held by exactly one of these two institutions in the pre-announcement quarter. Each event dataset includes observations over a $[-12, +12]$ month window centered on event time τ_m, so that each stock-merger pair contributes up to 25 monthly observations.

We stack these 203 sub-datasets into a single panel and estimate

$$\log(\mathit{Lending\ Fee}_{imt}) = \alpha + \beta\,(\mathit{Treat}_{im} \times \mathit{Post}_{mt}) + \gamma\,X_{imt} + \mu_{im} + \nu_{mt} + \lambda_{kt} + \varepsilon_{imt}, \tag{1}$$

where the unit of observation is stock i in merger event m in month t, and $\log(\textit{Lending Fee}_{imt})$ is the monthly average of the daily log fee. X_{imt} is a firm-size control, defined as the interaction of pre-period log market capitalization with event time. μ_{im} are stock-by-merger fixed effects, which compare each stock to itself within the same event window and control for any time-invariant differences between treated and control stocks. ν_{mt} are merger-by-month fixed effects, which control for shocks common to all stocks in a given merger event and month, e.g., general credit or equity market conditions at the time of the merger event. Finally, λ_{kt} are industry-by-calendar-month fixed effects, which control for trends in lending fees at the industry level. We estimate the same specification for active lending supply, active utilization, and fee volatility.

The size control X_{imt} is the product of $\overline{\log \textit{MktCap}}_{im}$, the mean of log market capitalization over the twelve months preceding the announcement, and event time measured relative to τ_m. Because it is constructed entirely from pre-announcement data, it is predetermined with respect to treatment and cannot itself be affected by the merger.

Throughout, we use a *clean control* sample. A stock is excluded from the control group for merger m if it is a treated stock in any other merger event announced within twelve months of τ_m, in either direction. The restriction is symmetric in event time: it removes stocks that are treated by another merger at any point inside the $[-12, +12]$ window, whether that merger precedes or follows τ_m. This ensures that no stock serves as a control for one merger while simultaneously experiencing consolidation from another, which is the contamination that motivates the stacked design. Treated stocks are never excluded on this basis, and mergers are retained only if at least ten treated and ten clean control stocks survive the restriction.

As a robustness check, we also re-estimate equation (1) on the subsample that excludes the fifteen mergers announced during 2008–2009, to verify that the results are not driven by the disruption of lending markets during the financial crisis. Finally, standard errors in equation (1) are clustered at the merger level. Because a treated stock can be treated by

more than one merger, the same stock can appear in more than one event dataset; the results are unchanged when standard errors are clustered by merger and by stock.

We verify the parallel trends assumption by estimating the event-study version of equation (1) using a full set of relative-time indicator interactions $Treat_{im} \times \mathbf{1}[t - \tau_m = k]$ for $k \in \{-12, \ldots, -2, 0, \ldots, +12\}$, omitting $k = -1$ as the reference period. Flat, statistically insignificant pre-event coefficients support the parallel trends assumption, and the post-announcement coefficients show whether the effect persists.

4 Empirical Results

4.1 Baseline Results for the Full Merger Sample

If consolidation weakens competition among the owners of lendable shares, treated stocks' fees should rise relative to controls after the announcement. Panel A of Table 3 presents the stacked difference-in-differences estimates. In all three models, the estimated effect is positive and statistically significant at the 1% level. In Column (1), which includes merger-by-firm, and industry-by-month fixed effects, the estimated coefficient is 0.045 ($t = 5.10$), or a 4.6% increase in lending fees. Column (2) instead includes firm, merger-by-month, and industry-by-month fixed effects and yields a coefficient of 0.051 ($t = 5.67$). In Column (3), our preferred specification, we include merger-by-firm, merger-by-month, and industry-by-month fixed effects, which tighten identification to within-merger variation; the estimated coefficient is 0.052 ($t = 5.48$), corresponding to a 5.3% increase in lending fees—about 3.4 basis points at the treated stocks' announcement-month mean fee of 64 basis points. All specifications restrict the control group to *clean controls*—stocks not treated in any other merger event within twelve months—and include the interaction of pre-period log market capitalization with event time. Using the richest fixed effects structure is not detrimental to the estimate, suggesting that the result is not driven by shocks common to stocks within a merger event.

Because fifteen of the mergers in our sample were announced during the 2008–2009 financial crisis, one may worry that the estimates reflect crisis-period disruptions in the lending market rather than the effect of ownership consolidation. Panel B re-estimates the three specifications excluding these mergers. The coefficients, ranging from 0.043 to 0.053, are nearly identical to the full-sample estimates and remain statistically significant at the 1% level. The fee increase of 4.4% to 5.4% after the merger is a meaningful tightening of the constraints on short sales for stocks jointly held by the two merging lenders.

Figure 1 presents the event-study estimates from the stacked design. Each point plots the coefficient on Treat $\times \mathbf{1}[t = k]$ for event months $k \in \{-12, \ldots, -2, 0, \ldots, 12\}$, with month -1 omitted as the reference period. All regressions include merger-by-firm, merger-by-month, and industry-by-month fixed effects, together with the interaction of pre-period log market capitalization with event time, and standard errors are clustered at the merger level.

As can be seen, the pre-treatment coefficients (months -12 to -2) do not show any systematic pattern relative to the month -1. Even though the estimates show some fluctuations around zero, no pre-merger divergence is evident between the treated and control stocks, which is in line with the parallel trends assumption. After the announcement, the coefficients are positive and economically significant. They do not quickly revert to zero, suggesting that the effect of the merger is not limited to a short-lived announcement response. Panel (b) shows that the same pattern holds when the fifteen mergers announced in 2008–2009 are excluded.

4.2 Heterogeneous Effects by Stock Characteristics

If the fee increase reflects lost competition among lenders, it should be strongest in stocks whose lending markets were most active before the merger—where competition for borrowers had been holding fees down. Table 4 examines this prediction by asking whether the effect varies systematically with pre-merger firm characteristics. In each regression, stocks are split into terciles within each merger based on the relevant characteristic, measured two months

before the announcement and held fixed over the entire event window. The *Treat* × *Post* coefficient represents the effect of the merger for stocks in the lowest tercile of the pre-merger characteristic, and the interaction terms report the differential effect for stocks in the second and third terciles of the characteristic. In all of these regressions, we use the clean-control sample and the full set of fixed effects.

Column (1) examines heterogeneity by utilization, measured as the value of shares on loan divided by total lendable value prior to the merger announcement. The coefficient on *Treat* × *Post* is 0.034 ($t = 3.43$) for the lowest utilization tercile, and the interaction terms for the second and third terciles are 0.012 ($t = 1.65$) and 0.033 ($t = 1.90$), both marginally significant at the 10% level. The point estimates thus suggest that the impact of consolidation is larger when the lending market is tight—when a larger fraction of lendable supply is already in use before the merger—although the tercile gradient is estimated somewhat imprecisely.

Column (2) presents results on stock volatility, measured using 365-day historical volatility before the merger. Here, we find a stronger effect for more volatile stocks. The coefficient on *Treat* × *Post* for the lowest tercile of stock volatility is 0.015 ($t = 1.38$) and statistically insignificant, while the interaction term for the highest tercile adds 0.044 ($t = 3.30$), statistically significant at the 1% level. This implies a total effect of 0.059 log points (roughly 6.1%) for the highest volatility tercile. This is consistent with the idea that mergers have a larger impact on lending fees for stocks with greater valuation uncertainty and stronger short-selling demand.

Columns (3) and (4) demonstrate how this effect is linked to stock liquidity. In Column (3), where we use turnover as a measure of trading activity, we find that the consolidation effect is stronger for more liquid stocks. The coefficient on *Treat* × *Post* equals 0.024 ($t = 2.59$) for the lowest turnover tercile, while the additional interaction effects for the second and third terciles are 0.029 ($t = 3.18$) and 0.057 ($t = 3.75$), both statistically significant at the 1% level, implying a total effect of 0.081 log points (roughly 8.4%) for the most liquid tercile. In Column (4), we replicate this finding using an alternative liquidity measure based

on the Amihud illiquidity ratio. Note that the two measures run in opposite directions: a higher tercile means greater liquidity in Column (3) but lower liquidity in Column (4). In Column (4), the coefficient on *Treat* × *Post* equals 0.071 ($t = 4.43$) for the most liquid (lowest illiquidity) tercile, while the interaction for the highest illiquidity tercile is −0.057 ($t = -3.28$), statistically significant at the 1% level. This implies a total effect of only 0.014 log points (about 1.4%) for the most illiquid stocks.

These findings suggest that the impact of consolidation is strongest for stocks with more liquid, and therefore more active, lending markets, where a reduction in independent lending supply is more likely to affect borrowing costs.

4.3 Active Lending Supply and Utilization

The baseline results show that mergers raise lending fees. The fee increase can have two sources. First, shares may become scarce, so borrowers compete for fewer shares. Second, the merged institution may gain pricing power and charge more even when shares remain plentiful. We distinguish between these explanations using two outcomes: active lending supply and active utilization.

Active lending supply is the quantity of shares that lenders actively offer for lending, as a percent of shares outstanding. It differs from total lendable supply, which also includes shares held in lending programs but not offered to borrowers. Borrowers cannot obtain those shares. Active supply therefore measures the supply that borrowers actually face. If the merged institution restricts supply, active supply should fall.

Active utilization is the value of shares on loan as a percent of the value of active supply. It measures how much of the available supply is already in use. If fees rise because shares become scarce, utilization should rise sharply as borrowers use up the available shares. If fees rise because of pricing power, utilization should change little, and most available shares should remain unused.

Table 5 reports the results. Active lending supply falls by 0.43 percentage points ($t =$

-4.78). The decline is small: less than 2% of the sample mean of 23.9%. Active utilization rises by 0.43 percentage points ($t = 1.91$), about 2% of its sample mean of 19.3%, and the increase is significant only at the 10% level. The market remains far from running out of shares: the median stock has only 6.9% of its active supply on loan.

Figure 2 shows the timing. Before the announcement, the coefficients for both outcomes are statistically insignificant. After the announcement, active supply declines gradually for about eight months and then levels off at roughly 0.7 percentage points below its pre-merger level. Utilization rises by about one percentage point in months three to five and remains positive, though imprecisely estimated, afterward.

These results suggest that fees do not rise because shares run out. Active supply falls by less than 2%, and most available shares remain unused. The decline in supply is nevertheless statistically significant, and it grows over the first year after the merger. Two questions follow. Which stocks lose supply, and why would the merged institution withdraw shares from the lending market? We turn to these questions in Section 4.4, which relates the supply response to the merged institution's later decisions to sell the stock.

4.4 Institutional Portfolio Response

The previous section shows that active lending supply declines after the merger. This section asks which positions lose supply and how the fee response relates to the merged institution's later decision to sell.

We divide treated stocks into three mutually exclusive groups according to when the merged institution's combined 13(f) position first reaches zero: within one year of the announcement quarter, during the second year, or not within two years. Most treated positions are kept: only 6.9% are sold within a year and 7.9% during the second year. Two features of this design matter for interpretation. First, fees and supply are measured over the twelve months after the announcement. Positions sold in the second year are still held throughout that window, whereas positions sold within a year are sold inside it. Second, holding status

is determined after the announcement. The split therefore describes where the response is concentrated; it does not identify a causal effect of selling.

Table 6 reports the results. Fees rise in all three groups. The increase is largest for positions sold within a year, at 9.7%, about twice the 4.6% for positions never sold. Positions sold in the second year fall in between, at 5.8%. Active supply shows a sharper pattern. It falls by 2.38 percentage points for positions sold within a year, about 8% of their pre-merger mean, and by 0.93 percentage points, or 3.4%, for positions sold in the second year. For positions never sold, it falls by only 0.19 percentage points, or 0.6%, and the decline is significant only at the 10% level. The supply decline documented in the previous section therefore comes mainly from positions that the merged institution later sells.

Each group reveals a different part of the response. Positions never sold account for 85% of treated positions. Their fees rise while their supply barely changes. This is the pricing-power effect of the previous section, operating on positions the institution continues to hold. Positions sold within a year show the largest responses in both fees and supply. For these positions, however, the sale occurs inside the measurement window. Part of their supply decline may simply reflect the sale, since sold shares leave the institution's lending pool, and part of their fee increase may follow from that loss of supply. Positions sold in the second year offer the cleanest timing. Their supply falls while the institution still holds them, so the withdrawal of shares precedes the sale, although some of it may reflect gradual trimming before the final sale.

Why would an institution stop lending shares it plans to sell? Two reasons are natural. First, shares on loan must be recalled before they can be sold, and recalls take time and can fail. Second, lending the shares helps short sellers, whose trades push the price down before the institution can sell. Nurisso (2026) formalizes the second reason. In the model, a lender with negative private information sets a fee at least as high as a lender with positive information, and strictly higher whenever it attracts short sellers (Corollary 3). The informed lender intends to sell, and short sellers compete with its sale.

The direction of causality requires care. One reading is that institutions raise fees and withdraw shares because they plan to sell. The opposite reading is that institutions sell stocks whose fees have risen, because high fees signal strong short-selling demand and low future returns (Jones and Lamont, 2002; Cohen et al., 2007). For positions sold within a year, the data cannot separate these readings. Positions sold in the second year are more informative. Their fees rise only slightly more than those of positions never sold, while their supply falls clearly before the sale. The evidence therefore supports the view that institutions withdraw shares from positions they plan to sell. It does not establish that they raise fees for that purpose. Both outcomes are also market-level aggregates: the Markit fee averages across lenders, and lendable supply sums all lenders' inventories. The merged institution's own fee-setting and lending decisions are thus inferred rather than directly observed.

These patterns contrast with evidence from mutual funds. Johnson and Weitzner (2025) find that fee-retaining funds overweight high-fee stocks, and Evans et al. (2017) find that funds with style restrictions lend rather than sell stocks with high short-selling demand. The banks and asset managers in our sample face fewer such restrictions. Rather than continuing to lend, they withdraw shares from the positions they exit, and these positions show the largest fee increases.

5 Active and Passive Owners

If institutions withdraw shares because they plan to sell them, the supply cut should be larger for institutions that trade more. An active institution trades often and needs its shares available to sell. A passive institution rarely trades and has little reason to withdraw shares. The type of institution may also matter for fees. Borrowers may pay more to borrow from a passive institution, because it is unlikely to recall the shares or trade against them. Nurisso (2026) formalizes this second prediction: in equilibrium, a passive lender charges a higher fee than an active lender and still attracts more short selling (Proposition 5).

Our mergers provide a test. A merger hands an existing block of shares to a new owner, while the stock itself stays the same. Comparing mergers by the type of acquirer therefore varies the lender while holding the stocks fixed. We measure how much an institution trades with the dollar-weighted churn ratio of Gaspar et al. (2005):

$$CR_{jq} = \frac{\sum_i |P_{iq}\,\Delta N_{ijq}|}{\frac{1}{2}\sum_i \big(N_{ijq}P_{iq} + N_{ij,q-1}P_{i,q-1}\big)}, \tag{2}$$

the dollar value the institution trades during the quarter divided by its average portfolio value, computed from Thomson/Refinitiv s34 holdings and CRSP quarter-end prices. We average the ratio over the eight or twelve quarters before the announcement, so the merger cannot affect it. We then winsorize it at the 1st and 99th percentiles and standardize it across deals. Low churn identifies a passive institution. In the eight-quarter window, the median acquirer trades 23% of its portfolio per quarter, and the standard deviation across deals is 12 percentage points. We estimate

$$y_{imt} = \alpha + \beta_1\big(\mathit{Treat}_{im}\times\mathit{Post}_{mt}\big) + \beta_2\big(\mathit{Treat}_{im}\times\mathit{Post}_{mt}\times\mathit{Churn}_{m}\big) + \gamma X_{imt} + \mu_{im} + \nu_{mt} + \lambda_{kt} + \varepsilon_{imt}, \tag{3}$$

where y_{imt} is the log lending fee or active lending supply, and Churn_m is the standardized churn ratio of the acquirer in merger m. As in the baseline specification, X_{imt} is the size control, and μ_{im}, ν_{mt}, and λ_{kt} are merger-by-firm, merger-by-month, and industry-by-month fixed effects. Standard errors are clustered by merger. Because churn is standardized, β_1 is the effect at mean acquirer churn and β_2 is the change per standard deviation of churn.

Table 7 reports the results. At mean churn, fees rise by 0.051 log points ($t = 5.89$) and active supply falls by 0.44 percentage points ($t = -4.56$). Both effects vary with the type of acquirer. For fees, the triple interaction is -0.014 ($t = -2.01$). An acquirer one standard deviation below mean churn raises the fee by 6.8%, whereas an acquirer one standard deviation above raises it by 3.8%. For supply, the triple interaction is -0.17 ($t = -1.76$). The passive acquirer cuts active supply by 0.27 percentage points, whereas the

active acquirer cuts it by 0.61 points. The supply result is significant only at the 10% level. The twelve-quarter window gives similar estimates.

Both patterns match the predictions. Active acquirers withdraw more shares, consistent with a need to keep shares available to sell. Passive acquirers raise fees more, consistent with borrowers paying a premium to borrow from owners that are unlikely to recall. The joint pattern also runs against scarcity. Under scarcity, the largest supply cuts should bring the largest fee increases. Here, the acquirers that cut supply the most raise fees the least. Table 7 classifies only the acquirer. Yet a merger changes the owner only of the target's block of shares; the acquirer's own block stays where it was. What should matter, therefore, is the type of owner the target's shares move from and to. Table 8 classifies both parties as passive or active at the sample median of churn. The indicator *Lost Passive* equals one when an active acquirer absorbs a passive target. In these mergers, the target's shares move from a passive owner to an active one. This is the case in 47 of the 199 mergers with churn data for both parties.

In all other mergers, fees rise by 0.060 log points ($t = 5.87$). When an active acquirer absorbs a passive target, the increase is 0.043 smaller ($t = -2.40$), leaving an insignificant 1.7% ($t = 1.07$). Active supply shows the opposite pattern. It falls by 0.38 percentage points in all other mergers ($t = -3.76$) and by 0.66 points when a passive lender is lost ($t = -3.06$), although the difference between the two is not statistically significant ($t = -1.16$). The twelve-quarter window gives nearly identical estimates.

6 Short-Selling Risk and Price Discovery

"... a short seller is concerned not only with the level of fees, but also with fee variability" (D'Avolio, 2002, p. 279). Short sellers keep their positions open for weeks or months, pay the fee each day, and face the chance that it rises. A lending fee is therefore not only a cost but also a risk. Engelberg et al. (2018) show that this risk limits arbitrage: stocks with more

volatile lending fees attract less short selling and are priced less efficiently. If consolidation weakens competition among lenders, it may make fees less predictable, not only higher. And if short selling becomes more costly and more risky, prices should incorporate information more slowly. We test both predictions.

We first measure fee volatility as the natural logarithm of the within-month variance of the daily log indicative fee. We use the log of the fee rather than its level. Because mergers raise the fee level, the variance of the fee in levels would rise mechanically: a fee that rises by 10% has a variance 21% higher even if its percentage fluctuations are unchanged. A proportional increase in the fee, however, leaves the variance of the log fee unchanged. An increase in our measure therefore means that fees fluctuate more in percentage terms, not merely that they are higher.

Table 9 reports the results. The estimate is positive and significant at the 1% level in all three specifications, ranging from 0.098 to 0.127. In our preferred specification, the variance of fees rises by 0.109 log points ($t = 2.92$), or 11.5%.

Figure 3 shows the timing. Each coefficient measures how much the gap in fee volatility between treated and control stocks differs from the gap in the month before the announcement. Before the merger, this gap is stable: the coefficients are small and statistically insignificant. After the merger, the gap widens gradually. The coefficients become significant in the fourth month, when the variance of daily fees of treated stocks is about 13% higher than that of control stocks. By the end of the first year, it is about 18% higher, which corresponds to daily fee fluctuations that are roughly 9% larger. The effect does not revert. Consolidation therefore makes borrowing not only more expensive but also less predictable.

We next ask whether prices become less efficient. Following Hou and Moskowitz (2005) and Engelberg et al. (2018), we measure how slowly each stock's price responds to market-wide news. For each stock i and calendar year y, we run two time-series regressions over

weeks w:

$$\text{Full:} \quad r_{i,w} = a_i + \beta_i R_{m,w} + \sum_{n=1}^{4} \delta_{i,n} R_{m,w-n} + \varepsilon_{i,w}, \tag{4}$$

$$\text{Restricted:} \quad r_{i,w} = a_i + \beta_i R_{m,w} + \varepsilon_{i,w}, \tag{5}$$

where $r_{i,w}$ is the weekly return of stock i, compounded from CRSP daily returns, and $R_{m,w}$ is the value-weighted market return, with weights based on market capitalization in the prior week. The full regression lets the stock respond to market news with up to four weeks of delay. The restricted regression allows only an immediate response. Price delay ($D1$) is

$$Delay_{i,y} = 1 - \frac{R^2_{\text{Restricted}}}{R^2_{\text{Full}}}. \tag{6}$$

The measure lies between zero and one. It equals zero when lagged market returns add no explanatory power, that is, when the price responds immediately. It approaches one when most of the response arrives with a delay. We require at least 40 weekly returns per stock-year.

Because the measure is annual, we compare the calendar year after the announcement year with the year before it, and we exclude the announcement year itself. We estimate

$$Delay_{imy} = \alpha + \beta\big(Treat_{im} \times Post_{my}\big) + \mu_{im} + \nu_{my} + \lambda_{ky} + \varepsilon_{imy}, \tag{7}$$

where μ_{im}, ν_{my}, and λ_{ky} are merger-by-firm, merger-by-year, and industry-by-year fixed effects.

Table 10 reports the results. Price delay rises by 0.026 for treated stocks relative to controls ($t = 3.70$), about 10% of the sample mean of 0.26. A placebo test repeats the comparison one year earlier, between the second and the first year before the announcement. The estimate is 0.0005 ($t = 0.13$). Treated and control stocks were therefore not already diverging before the merger.

Taken together, consolidation raises the level of lending fees, makes them more volatile, and slows the speed at which prices respond to market-wide information. These results are consistent with Engelberg et al. (2018) and Saffi and Sigurdsson (2011), who link the cost and risk of short selling to price efficiency. They also complement Porras Prado et al. (2016), who find that stocks with more concentrated ownership adjust more slowly to news. Our design adds a causal link: a merger that removes a competing lender slows price discovery in the stocks the merging institutions hold in common.

7 Additional Analysis

7.1 Treatment Intensity

If the fee increase reflects the consolidation of ownership, the effect should be stronger where the merger consolidates more ownership. Table 11 examines this prediction. The sample is restricted to treated stocks, and the dose of treatment is $\min(s^T, s^A)$, the smaller of the target's and the acquirer's pre-announcement stakes as a percent of shares outstanding.

The dose should measure how much competition the merger removes. Before the merger, borrowers can approach two separate owners; after it, only one. The larger of the two positions was already in place, and the merger adds the smaller one to it. The smaller stake is therefore the size of the competitor that the merger removes. Consider a stock in which one merging party holds 2% and the other holds 0.01%. The merger removes a competitor that held only 0.01%, so the dose should be small. The minimum gives exactly 0.01%. Two natural alternatives do not. The combined stake, $s^T + s^A$, equals 2.01%, because it includes the 2% position that existed before the merger and does not change. The target's stake, s^T, which is what the acquirer gains, equals 0.01% if the target is the smaller party but 2% if it is the larger one, even though the loss of competition is the same in both cases. Only the minimum measures the loss of competition regardless of which party is the acquirer. It also uses only holdings from before the announcement, so the merged institution's later trading

cannot affect it.

Because the comparison is across treated stocks within the same merger and month, the merger-by-month fixed effects absorb the average post-announcement change. The estimates therefore capture whether the effect grows with dose, while its level is reported in Table 3.

Fees rise more where the merger combines larger stakes. In Panel A, the coefficient on $\log(Dose) \times Post$ is 0.0033 ($t = 2.31$): a tenfold increase in dose raises the fee increase by about 0.8 percentage points. In Panel B, the fee increase for stocks in the highest dose tercile is 0.0121 log points larger than for stocks in the lowest tercile ($t = 2.59$). By contrast, active lending supply shows at most weak evidence of a dose gradient (Column (2)): the continuous dose term is insignificant, and only the highest dose tercile shows a decline, significant only at the 10% level. The dose gradient in fees is therefore unlikely to be explained by a larger supply contraction. This pattern links the fee increase to the amount of ownership the merger actually combines.

7.2 Case Studies of Two Mergers

If consolidation raises fees, the effect should also appear within a single merger, without pooling across deals. We examine two mergers from very different settings. The first is Bank of America's acquisition of Merrill Lynch in September 2008, at the peak of the financial crisis. The second is Mariner Wealth Advisors' acquisition of Baystate Wealth Management in November 2023, a recent merger outside any crisis. If the effect appears in both, it is neither an artifact of the crisis nor specific to one period.

For each merger we estimate the event-study version of our specification on that merger's own event window, with firm and industry-by-month fixed effects and the size control, and we cluster standard errors by stock. Figure 4 plots the coefficients, with month -1 omitted as the reference period.

The two cases reproduce the stacked result in isolation. In both mergers the coefficients are close to zero and statistically insignificant before the announcement, so treated and con-

trol stocks follow similar fee paths. After the announcement, fees of treated stocks rise relative to controls: the increase arrives within the first few months for Bank of America–Merrill Lynch and builds over the year for Mariner–Baystate. In both cases the post-announcement coefficients are larger than the stacked average, which pools 203 mergers of very different sizes. The pattern therefore holds in a crisis-era merger of two very large institutions and in a recent merger between wealth managers.

8 Conclusion

This paper asks whether consolidation among institutional owners raises the cost of borrowing stock, and why. We use 203 financial institution mergers announced between 2007 and 2023 as shocks to ownership structure: when two institutions that both hold a stock merge, the number of independent lenders falls and an existing block passes to a new owner, while the stock itself is unchanged. Stocks held by both merging institutions experience a sustained fee increase of 4.4% to 5.4% across specifications, 5.3% in our preferred specification, relative to stocks held by only one. The event-study estimates show no pre-trends, and the increase persists throughout the following year.

Our analysis yields four main findings. First, ownership consolidation causes lending fees to rise, and it does so most where competition among lenders was doing the most work: in volatile, liquid, actively shorted stocks, and in mergers that consolidate larger overlapping stakes. Second, the increase does not come from scarcity. Active lending supply falls by less than 2% of its mean, utilization barely moves, and the 85% of positions the merged institution keeps show a 4.6% fee increase with supply essentially unchanged. Third, the type of owner matters: passive acquirers raise fees by about 6.8% and active acquirers by 3.8%, and passive acquirers withdraw less supply. Fourth, the consequences extend beyond the level of fees. The within-month variance of the daily log fee rises by 11.5% and price delay rises by about 10% of its sample mean, so borrowing becomes less predictable and

prices incorporate market-wide information more slowly.

These findings have three broader implications. Supply-side market structure is a causal determinant of short-selling costs: previous work documents cross-sectional relationships between ownership characteristics and borrowing costs, whereas our design identifies the channel using plausibly exogenous variation in ownership concentration. Market power in a decentralized, bilaterally negotiated market operates through prices rather than through rationing, and the composition of the owner base, not only its concentration, shapes what borrowers pay. Finally, consolidation among owners spills over into the market for borrowing the stocks they hold, a consequence of financial institution mergers that the literature has not examined.

Antitrust review of financial institution mergers typically weighs effects on deposit and product markets. Our results point to a further one: the market for borrowing the stocks the merging institutions hold in common. Quantifying the welfare cost there would require a model of how borrowers substitute across lenders and across stocks. We leave it for future research.

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Appendix A: Variable Definitions

Variable	Definition	Source
Key Variables		
Log(Lending Fee)	Monthly average of the daily log indicative lending fee, Markit's estimate of the market cost of borrowing the stock.	Markit
Treat	Indicator equal to one if the stock is held by both the target and the acquirer in the quarter before the announcement, and zero if held by only one of them.	Thomson Reuters 13F s34
Post	Indicator equal to one for months on or after the merger announcement.	SDC M&A Database
Securities Lending Market Variables		
Lending Fee	Monthly average of the daily indicative lending fee, in basis points.	Markit
Active Lending Supply	Quantity of shares actively made available for lending, as a percent of shares outstanding.	Markit and CRSP
Active Utilization	Value of shares on loan, as a percent of the value of the active lendable supply.	Markit
Utilization	Value of shares on loan divided by total lendable value, measured two months before the announcement.	Markit
Log Fee Variance	Natural logarithm of the within-month variance of the daily log indicative fee; stock-months with at least 15 daily observations and a nonzero variance.	Markit
Stock Characteristics		
Size	Exponentiated mean of log market capitalization over the twelve months before the announcement.	CRSP
Volatility	365-day historical return volatility, measured two months before the announcement.	OptionMetrics
Turnover	Monthly trading volume divided by shares outstanding, measured two months before the announcement.	CRSP
Amihud Illiquidity	Monthly average of absolute daily return divided by daily dollar volume, following Amihud (2002), with at least 10 trading days; measured two months before the announcement.	CRSP

Continued

Variable	Definition	Source
Price Delay (D1)	One minus the ratio of the R^2 from regressing weekly returns on the contemporaneous value-weighted market return to the R^2 with four weekly market lags added, following Hou and Moskowitz (2005); estimated per stock and calendar year with at least 40 weekly returns.	CRSP
Ownership and Merger Variables		
Dose	Smaller of the target's and the acquirer's pre-announcement stakes in a treated stock, $\min(s^T, s^A)$, as a percent of shares outstanding.	Thomson Reuters 13F s34
Sold in Year 1, Sold in Year 2, Never Sold	Mutually exclusive indicators for treated stocks, by when the merged institution's combined 13(f) position first reaches zero: within one year of the announcement quarter, during the second year, or not within two years.	Thomson Reuters 13F s34
13(f) Assets	Value of the institution's 13(f) holdings in the quarter before the announcement, in billions of dollars.	Thomson Reuters 13F s34
Churn Ratio	Dollar-weighted portfolio churn ratio of Gaspar et al. (2005), averaged over the eight or twelve quarters before the announcement and winsorized at the 1st and 99th percentiles; standardized across deals in regressions.	Thomson Reuters 13F s34
Lost Passive	Indicator equal to one if the target's churn ratio is at or below the sample median and the acquirer's is above it.	Thomson Reuters 13F s34

Table 1: Treated and Control Stocks at the Announcement

This table compares treated and control stocks in the announcement month. Treated stocks are held by both merging institutions in the quarter before the announcement; control stocks are held by only one. *Difference* is the treated mean minus the control mean.

	Mean			N	
Variable	Treated	Control	Difference	Treated	Control
Log(Lending Fee)	−5.607	−4.753	−0.854	39,355	58,880
Lending Fee (bps)	64	429	−365	39,355	58,880
Active Lending Supply (%)	31.24	18.61	12.64	39,096	56,630
Active Utilization (%)	10.58	26.29	−15.71	39,254	58,533

Table 2: Summary Statistics

This table reports summary statistics for the stacked sample of 203 mergers, restricted to stock–merger pairs with pre-announcement market capitalization. Stock-months cover the $[-12, +12]$-month event windows, and stock-years cover event years −2, −1, and +1. Dose is for treated stocks only. Appendix A defines all variables.

Variable	Mean	Std. Dev.	p25	p50	p75	Obs.
Panel A: Stock-months						
Lending Fee (bps)	270.5	996.3	29.7	37.5	50.6	2,390,043
Log(Lending Fee)	−5.125	1.240	−5.828	−5.586	−5.301	2,390,043
Active Lending Supply (%)	23.85	14.57	11.76	25.78	34.78	2,328,398
Active Utilization (%)	19.33	27.16	1.56	6.90	24.31	2,388,849
Log Fee Variance	−4.116	1.639	−4.998	−4.089	−3.221	2,069,620
Panel B: Stock–merger pairs, before the announcement						
Size ($bil.)	31.28	131.63	0.42	1.82	11.35	99,600
Volatility (%)	43.40	25.47	25.96	36.61	53.06	76,582
Turnover (%)	20.41	84.15	7.36	12.73	22.11	99,421
Utilization (%)	13.95	18.91	1.35	5.75	18.64	97,698
Amihud Illiquidity ($\times 10^6$)	0.677	27.897	0.000	0.001	0.008	99,207
Dose (%)	0.0505	0.1729	0.0004	0.0031	0.0397	39,495
Panel C: Mergers						
Acquirer 13(f) Assets ($bil.)	43.68	203.46	4.10	7.35	25.13	203
Target 13(f) Assets ($bil.)	5.68	32.54	0.32	0.62	1.58	203
Acquirer Churn (%)	24.12	11.83	16.28	22.52	28.45	201
Target Churn (%)	23.23	18.91	11.23	18.18	27.62	201
Panel D: Stock-years						
Price Delay (D1)	0.262	0.265	0.060	0.164	0.380	380,451

Table 3: Mergers and Lending Fees

This table reports stacked difference-in-differences estimates of the effect of mergers on the log lending fee. Treated stocks are held by both merging institutions in the quarter before the announcement; control stocks are held by only one and are not treated by another merger within twelve months. Each merger contributes a $[-12, +12]$-month window, and *Post* equals one from the announcement month onward. Panel B excludes the fifteen mergers announced in 2008–2009. t-statistics based on standard errors clustered by merger are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Dependent Variable: Log(Lending Fee)		
	(1)	(2)	(3)
Panel A: Full sample			
Treat × Post	0.045***	0.051***	0.052***
	(5.10)	(5.67)	(5.48)
Observations	2,389,767	2,389,795	2,389,767
Adjusted R^2	0.887	0.816	0.887
Panel B: Excluding mergers announced in 2008–2009			
Treat × Post	0.043***	0.052***	0.053***
	(4.37)	(5.33)	(5.13)
Observations	2,022,512	2,022,523	2,022,512
Adjusted R^2	0.901	0.845	0.901
Controls			
Log(Market Cap)$_{pre}$ × Event Time	Y	Y	Y
Fixed effects			
Firm FE	N	Y	N
Industry×Month FE	Y	Y	Y
Merger×Month FE	N	Y	Y
Merger×Firm FE	Y	N	Y

Table 4: Heterogeneity by Stock Characteristics

This table reports how the merger effect on the log lending fee varies across within-merger terciles of stock characteristics measured two months before the announcement. The first tercile is omitted, so *Treat*×*Post* is the effect in the lowest tercile. A higher tercile means greater liquidity in column (3) but lower liquidity in column (4). Treated and control stocks are defined as in Table 3. t-statistics based on standard errors clustered by merger are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Dependent Variable: Log(Lending Fee)			
	(1) Utilization	(2) Volatility	(3) Turnover	(4) Amihud
Treat × Post	0.034***	0.015	0.024**	0.071***
	(3.43)	(1.38)	(2.59)	(4.43)
Treat × Post × Tercile 2	0.012*	−0.000	0.029***	−0.007
	(1.65)	(−0.04)	(3.18)	(−0.54)
Treat × Post × Tercile 3	0.033*	0.044***	0.057***	−0.057***
	(1.90)	(3.30)	(3.75)	(−3.28)
Observations	2,362,925	1,865,852	2,386,903	2,383,799
Adjusted R^2	0.888	0.838	0.887	0.887
Controls				
$\text{Log(Market Cap)}_{pre}$ × Event Time	Y	Y	Y	Y
Fixed effects				
Industry×Month FE	Y	Y	Y	Y
Merger×Month FE	Y	Y	Y	Y
Merger×Firm FE	Y	Y	Y	Y

Table 5: Mergers, Active Lending Supply and Active Utilization

This table reports stacked difference-in-differences estimates of the effect of mergers on active lending supply and active utilization. Treated and control stocks are defined as in Table 3. t-statistics based on standard errors clustered by merger are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	(1) Active Lending Supply (%)	(2) Active Utilization (%)
Treat × Post	−0.427***	0.431*
	(−4.78)	(1.91)
Observations	2,328,024	2,388,529
Adjusted R^2	0.963	0.768
Controls		
Log(Market Cap)$_{pre}$ × Event Time	Y	Y
Fixed effects		
Industry×Month FE	Y	Y
Merger×Month FE	Y	Y
Merger×Firm FE	Y	Y

Table 6: Fee and Supply Effects by When the Position Is Sold

This table reports merger effects separately for treated stocks grouped by when the merged institution's combined 13(f) position first reaches zero: within one year of the announcement quarter, during the second year, or not within two years. Each group is compared with the same control stocks, defined as in Table 3. Column (3) reports each group's share of the 39,182 treated stock–merger pairs with known holding status. t-statistics based on standard errors clustered by merger are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	(1) Log(Lending Fee)	(2) Active Lending Supply (%)	(3) Share of Treated (%)
Sold in Year 1 × Post	0.093***	−2.384***	6.9
	(4.23)	(−4.89)	
Sold in Year 2 × Post	0.056***	−0.930***	7.9
	(4.78)	(−4.32)	
Never Sold × Post	0.045***	−0.191*	85.2
	(4.13)	(−1.70)	
Observations	2,389,767	2,320,327	
Adjusted R^2	0.887	0.963	
Controls			
Log(Market Cap)$_{pre}$ × Event Time	Y	Y	
Fixed effects			
Industry×Month FE	Y	Y	
Merger×Month FE	Y	Y	
Merger×Firm FE	Y	Y	

Table 7: Heterogeneity by Acquirer Churn

This table reports how the merger effect varies with the acquirer's churn ratio (Gaspar et al., 2005), averaged over the eight (8Q) or twelve (12Q) quarters before the announcement. Churn is winsorized at the 1st and 99th percentiles and standardized across deals, so *Treat*×*Post* is the effect at mean churn and the triple interaction is the change per standard deviation of churn. Treated and control stocks are defined as in Table 3. t-statistics based on standard errors clustered by merger are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Log(Lending Fee)		Active Lending Supply (%)	
	(1)	(2)	(3)	(4)
	8Q	12Q	8Q	12Q
Treat × Post	0.052***	0.051***	−0.442***	−0.445***
	(5.89)	(5.89)	(−4.56)	(−4.64)
Treat × Post × Churn	−0.014**	−0.013**	−0.170*	−0.162*
	(−2.01)	(−2.05)	(−1.76)	(−1.67)
Observations	2,369,352	2,369,352	2,307,808	2,307,808
Adjusted R^2	0.887	0.887	0.963	0.963
Controls				
Log(Market Cap)$_{pre}$ × Event Time	Y	Y	Y	Y
Fixed effects				
Industry×Month FE	Y	Y	Y	Y
Merger×Month FE	Y	Y	Y	Y
Merger×Firm FE	Y	Y	Y	Y

Table 8: Losing a Passive Lender

This table reports whether the merger effect differs when an active acquirer absorbs a passive target (*Lost Passive*). Each party is passive if its churn ratio (Gaspar et al., 2005), averaged over the eight (8Q) or twelve (12Q) quarters before the announcement, is at or below the sample median, and active otherwise. *Treat*×*Post* is the effect in all other mergers, and the implied effect adds the triple interaction. Treated and control stocks are defined as in Table 3. *t*-statistics based on standard errors clustered by merger are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Log(Lending Fee)		Active Lending Supply (%)	
	(1) 8Q	(2) 12Q	(3) 8Q	(4) 12Q
Treat × Post	0.060***	0.060***	−0.378***	−0.377***
	(5.87)	(5.85)	(−3.76)	(−3.74)
Treat × Post × Lost Passive	−0.043**	−0.044**	−0.281	−0.284
	(−2.40)	(−2.54)	(−1.16)	(−1.15)
Implied effect in Lost Passive mergers				
Treat × Post + Triple	0.017	0.016	−0.659***	−0.661***
	(1.07)	(1.10)	(−3.06)	(−3.02)
Observations	2,363,054	2,363,054	2,301,869	2,301,869
Adjusted R^2	0.887	0.887	0.963	0.963
Mergers	199	199	199	199
Lost Passive mergers	47	47	47	47
Controls				
Log(Market Cap)$_{pre}$ × Event Time	Y	Y	Y	Y
Fixed effects				
Industry×Month FE	Y	Y	Y	Y
Merger×Month FE	Y	Y	Y	Y
Merger×Firm FE	Y	Y	Y	Y

Table 9: Mergers and the Volatility of Lending Fees

This table reports stacked difference-in-differences estimates of the effect of mergers on the log of the within-month variance of the daily log lending fee. Treated and control stocks are defined as in Table 3. t-statistics based on standard errors clustered by merger are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Dependent Variable: Log Fee Variance		
	(1)	(2)	(3)
Treat × Post	0.127***	0.098***	0.109***
	(3.25)	(2.80)	(2.92)
Observations	2,069,089	2,069,290	2,069,089
Adjusted R^2	0.366	0.301	0.366
Controls			
Log(Market Cap)$_{pre}$ × Event Time	Y	Y	Y
Fixed effects			
Firm FE	N	Y	N
Industry×Month FE	Y	Y	Y
Merger×Month FE	N	Y	Y
Merger×Firm FE	Y	N	Y

Table 10: Mergers and the Speed of Price Discovery

This table reports difference-in-differences estimates of the effect of mergers on the price delay measure $D1$ of Hou and Moskowitz (2005), estimated for each stock and calendar year; higher values indicate slower price discovery. Years are relative to the announcement year, which is excluded; column (2) is a placebo test. Treated and control stocks are defined as in Table 3. t-statistics based on standard errors clustered by merger are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Dependent Variable: Price Delay ($D1$)	
	(1)	(2)
	Years -1 vs. $+1$	Placebo: Years -2 vs. -1
Treat × Post	0.0262***	0.0005
	(3.70)	(0.13)
Observations	245,142	241,594
Adjusted R^2	0.518	0.531
Fixed effects		
Industry×Year FE	Y	Y
Merger×Year FE	Y	Y
Merger×Firm FE	Y	Y

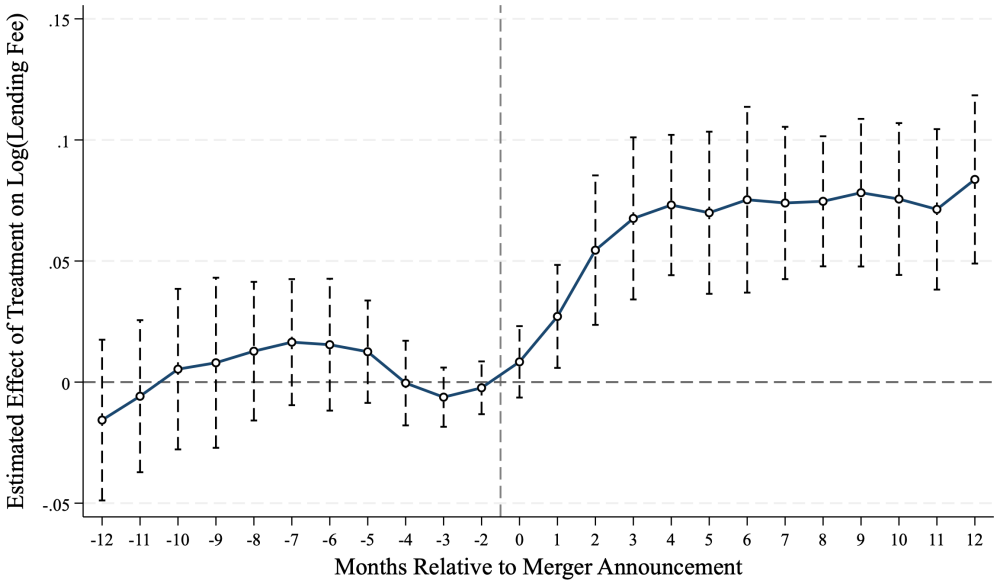
Table 11: Treatment Intensity

This table tests whether the merger effect grows with dose, $\min(s^T, s^A)$, the smaller of the target's and the acquirer's pre-announcement stakes as a percent of shares outstanding. The sample includes only treated stocks, so merger-by-month fixed effects absorb the average effect reported in Table 3. In Panel B, the first tercile is omitted. t-statistics based on standard errors clustered by merger are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

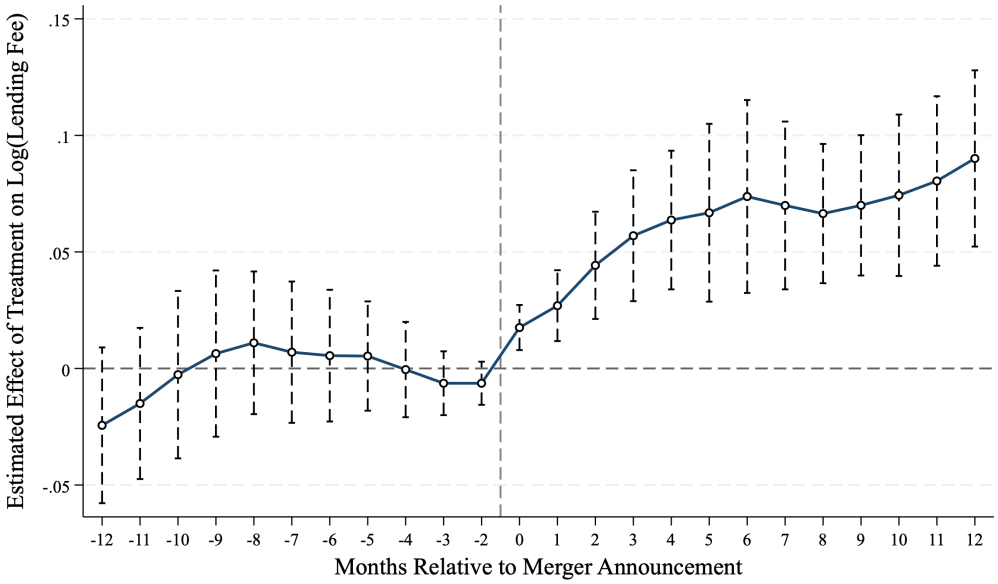
	(1) Log(Lending Fee)	(2) Active Lending Supply (%)
Panel A: Log dose		
log(Dose) × Post	0.0033**	−0.0266
	(2.31)	(−1.11)
Panel B: Dose terciles within merger		
Tercile 2 × Post	0.0094	−0.0226
	(1.53)	(−0.24)
Tercile 3 × Post	0.0121**	−0.1461*
	(2.59)	(−1.75)
Observations	970,666	964,741
Adjusted R^2	0.802	0.951
Controls		
Log(Market Cap)$_{pre}$ × Event Time	Y	Y
Fixed effects		
Industry×Month FE	Y	Y
Merger×Month FE	Y	Y
Merger×Firm FE	Y	Y

Figure 1: Mergers and Lending Fees: Event Study

This figure plots stacked event-study estimates of the effect of mergers on the log lending fee: coefficients on *Treat* interacted with event-month indicators, with month −1 as the omitted reference period. The specification follows column (3) of Table 3. Bars show 95% confidence intervals based on standard errors clustered by merger; the dashed vertical line marks the announcement.



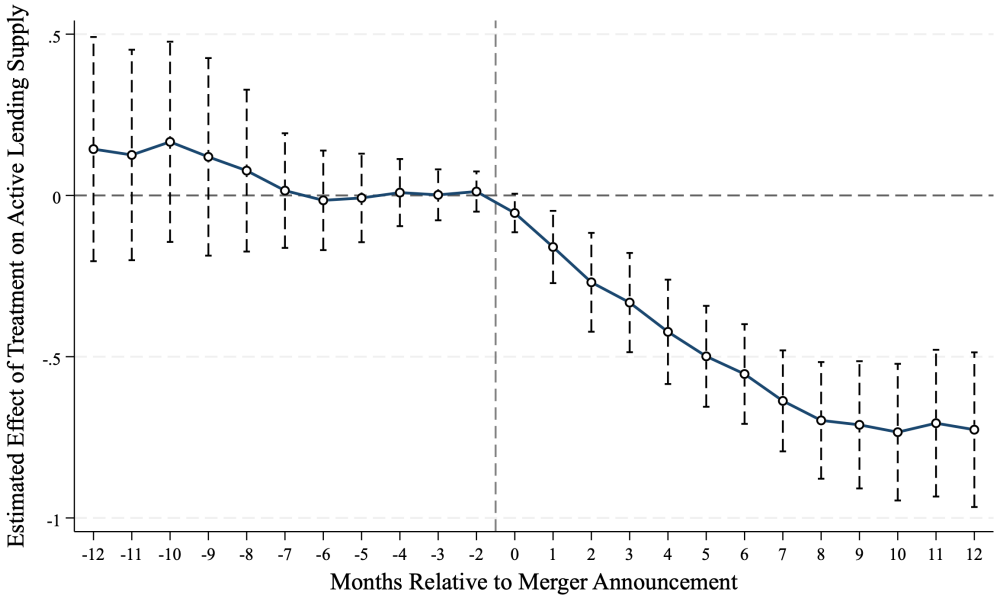
(a) Full sample



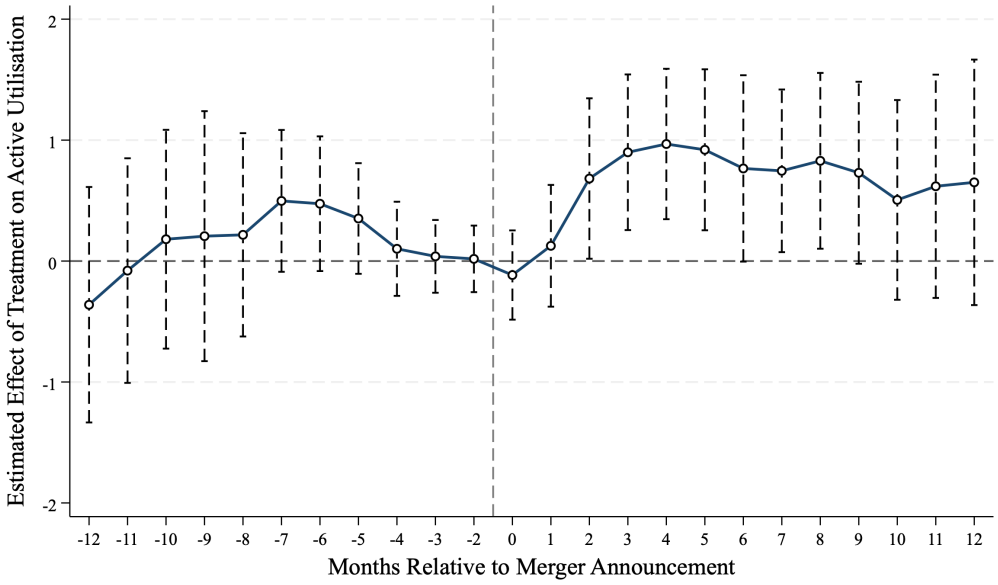
(b) Excluding mergers announced in 2008–2009

Figure 2: Active Lending Supply and Active Utilization: Event Study

This figure plots stacked event-study estimates of the effect of mergers on active lending supply and active utilization: coefficients on *Treat* interacted with event-month indicators, with month −1 as the omitted reference period. The specification follows Table 5. Bars show 95% confidence intervals based on standard errors clustered by merger; the dashed vertical line marks the announcement.



(a) Active lending supply



(b) Active utilization

Figure 3: Mergers and the Volatility of Lending Fees: Event Study

This figure plots stacked event-study estimates of the effect of mergers on the log of the within-month variance of the daily log lending fee: coefficients on *Treat* interacted with event-month indicators, with month -1 as the omitted reference period. The specification follows column (3) of Table 9. Bars show 95% confidence intervals based on standard errors clustered by merger; the dashed vertical line marks the announcement.

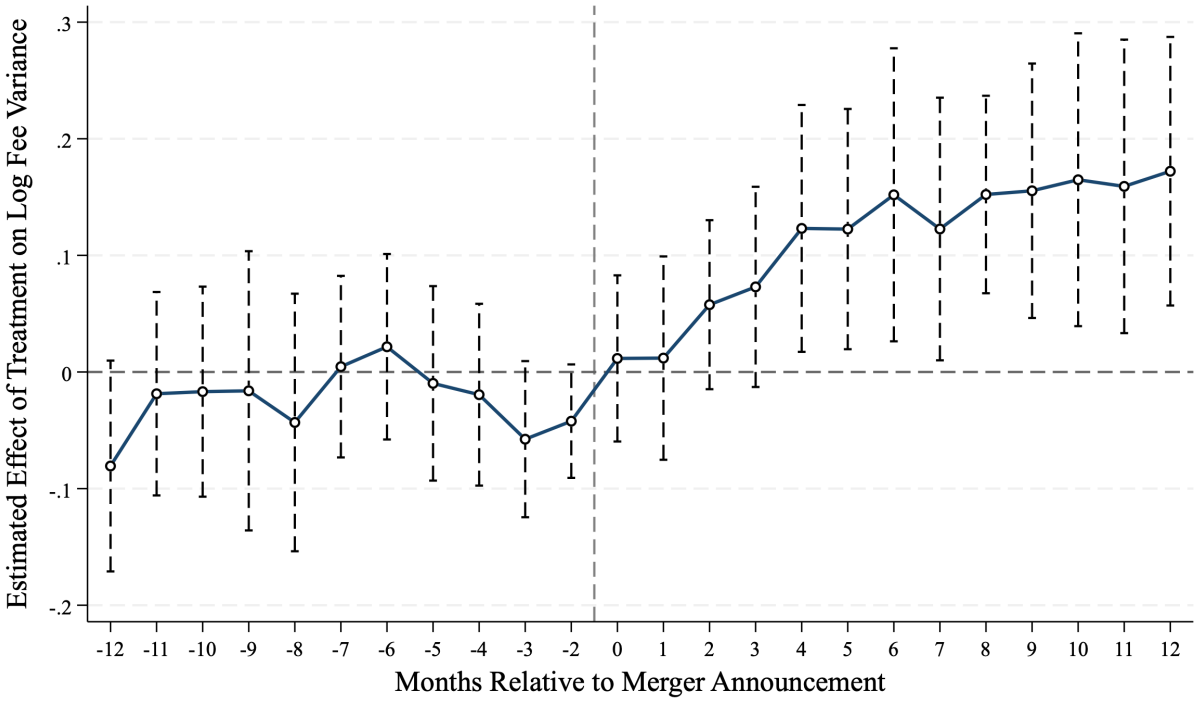
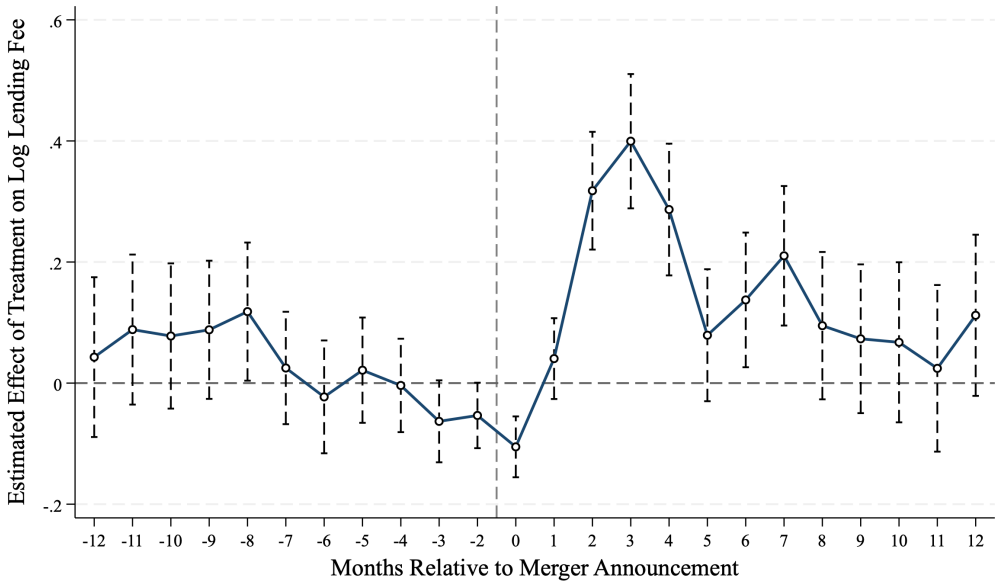
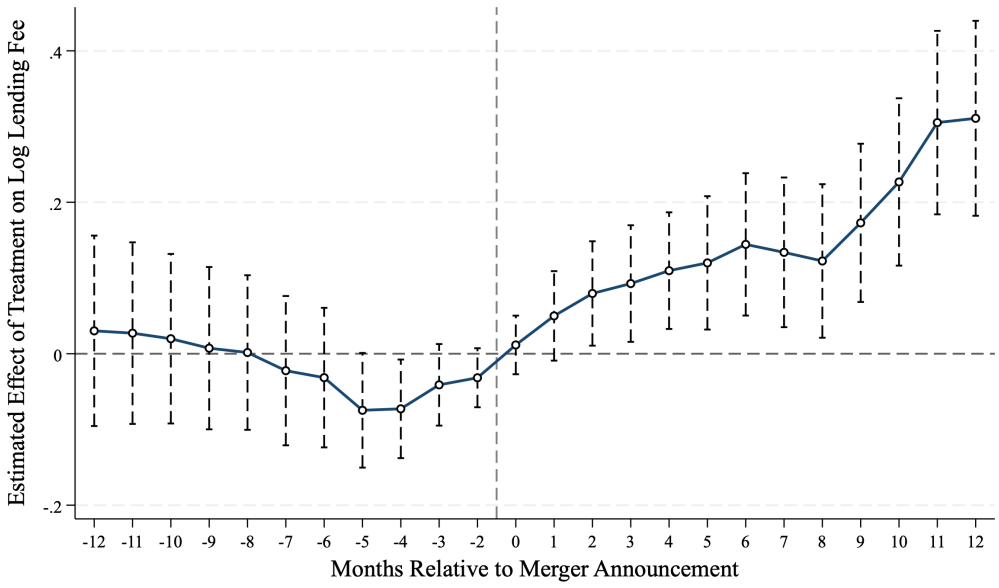


Figure 4: Case Studies of Two Mergers: Event Study

This figure plots event-study estimates of the effect of two individual mergers on the log lending fee: Bank of America's acquisition of Merrill Lynch (September 2008) and Mariner Wealth Advisors' acquisition of Baystate Wealth Management (November 2023). Coefficients are on *Treat* interacted with event-month indicators, with month −1 as the omitted reference period. Treated and control stocks are defined as in Table 3. All regressions include firm and industry-by-month fixed effects and the interaction of pre-period log market capitalization with event time. Bars show 95% confidence intervals based on standard errors clustered by stock; the dashed vertical line marks the announcement.



(a) Bank of America–Merrill Lynch (September 2008)



(b) Mariner–Baystate (November 2023)