Lending Fees and Ownership Structure

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Abstract

We study whether consolidation among institutional owners raises equity lending fees. Using 32 financial institution mergers announced between 2007 and 2024 and a stacked difference-in-differences design, we find that stocks held by both the acquirer and the target before the announcement experience a 5.2% to 8.4% increase in fees relative to controls. The effect is concentrated in larger, more volatile, and more liquid stocks, and in stocks with more liquid option markets. Three crisis-era mergers yield effects of 9.3% to 13.9%. Treated stocks also earn lower abnormal returns following the merger, consistent with tighter short-sale constraints. Supply-side market structure is a material determinant of short-selling costs and price efficiency.

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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. For stocks with strong borrowing demand or limited lendable supply, this fee can become quite high. When that happens, 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 has examined how such short-sale constraints affect asset prices, returns, and market efficiency, but causal evidence on the supply-side forces that generate them is still limited. In this paper, we study one such force: the ownership structure of the firms whose shares are lent. Specifically, we ask whether greater concentration in institutional ownership, by placing lendable shares in the hands of fewer independent lenders, causally increases the cost of borrowing stock.

Two papers motivate this question. Chen et al. [2022] show that the U.S. equity lending market is highly concentrated at the intermediary level. Lending is delegated to a small set of custodian lenders—financial institutions that hold clients' securities and act as lending agents—and for a typical stock the top two custodians command between 40% and 70% of the market. Fees stay above competitive levels even when many lendable shares are not being borrowed. Porras Prado et al. [2016] study a different layer of supply-side concentration: concentration among the underlying institutional owners of the stock. They find that this concentration is associated with higher lending fees and tighter lending supply. Together, the two papers points to the supply-side concentration being linked to higher lending fees. However, neither paper exploits variation in concentration that is exogenous to lending-market conditions.

A causal estimate is difficult to obtain because ownership concentration is endogenous to

[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/).

the very features of a stock that drive borrowing demand and lending supply. Three concerns stand out. The first is omitted variables. Stocks held by a small number of large institutions differ systematically from more dispersedly held stocks — in size, liquidity, volatility, and governance — and these characteristics independently affect lending fees. A cross-sectional association between concentration and fees may thus reflect common determinants rather than any direct effect of concentration itself. The second concern is reverse causality. Institutions typically receive a share of the fee income generated by lending their shares. This gives institutions an incentive to retain or accumulate positions in stocks that generate high lending revenues. If so, high fees attract concentrated ownership rather than the other way around. The third concern is simultaneity. In an over-the-counter market, fees and the identity of lenders are determined jointly in equilibrium. Hence, cross-sectional correlations between concentration and fees can be consistent with multiple interpretations.

We address these challenges by using financial institution mergers as a source of variation in ownership concentration that is plausibly exogenous to the lending fees of the stocks held in common. When two institutions that previously held the same 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, the decision to merge is driven by strategic considerations at the institution level — geographic and product-line expansion, distressed acquisitions during the financial crisis, regulatory pressure — rather than by the lending-market characteristics of any individual portfolio stock. This setting addresses each of the three identification concerns directly. Stock-level omitted variables are differenced out by within-stock, before/after comparisons inside narrow event windows. Reverse causality from fees to ownership is ruled out because the merger is not a portfolio choice in response to the lending-fee dynamics of overlapping holdings. And equilibrium simultaneity is broken by exploiting a sharp, discrete drop in the number of independent lenders that takes place at the merger announcement rather than gradually through search and re-matching.

Our identification strategy is related to recent work that uses financial institution mergers to generate exogenous variation in ownership structure. Friberg et al. [2024] study the BlackRock–Barclays Global Investors merger of 2009 as a single-merger shock and He and Huang [2017] use a stacked difference-in-differences design across multiple financial institution mergers. We apply the same insight to the securities lending market and use a comprehensive panel of 32 financial institution mergers announced between 2007 and 2024.

Our empirical design uses a stacked difference-in-differences approach following Cengiz et al. [2019] and Baker et al. [2022]. The stacked design aggregates information across all 32 mergers and is robust to the bias highlighted in the staggered difference-in-differences literature when treatment effects are heterogeneous [Callaway and Sant'Anna, 2021, De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2021]. Specifically, we define stocks held by both the acquiring and target institutions before the merger announcement as *treated* and stocks held by only one of the two merging institutions as *control*. We create a separate dataset for each of the 32 mergers and pool them to estimate the average treatment effect while controlling for merger-by-month, merger-by-firm, and industry-by-month fixed effects.

Our main hypothesis is that mergers between institutional owners raise lending fees for stocks they both hold. Equity lending is an over-the-counter market in which borrowers and lenders negotiate bilaterally [Duffie et al., 2002]. Borrowers cannot easily compare prices across lenders, so large institutional owners have some market power. When multiple independent owners hold the same stock, a borrower can solicit competing offers, which limits fees. When two such owners merge, this competition weakens. The alternative is that the merger has no effect on fees. This can happen if enough independent owners remain after the merger to keep fees competitive, or if borrowers can easily borrow the same stock from other owners. We test which prediction the data support.

Our baseline results strongly support the hypothesis that ownership consolidation leads to higher lending fees. In our stacked difference-in-differences specification with clean controls and all sets of fixed effects, the point estimate implies an 8.4% increase in lending fees for treated firms relative to controls following the merger—firms that were held in the portfolios of both the target and the acquiring institution. Evaluated at the sample mean lending fee of 147 basis points, this corresponds to a raw increase of about 12 basis points. Consistent with a market-power channel rather than a reduction in supply, lending supply and utilization do not change significantly after the merger.

The event-study plot also shows flat and statistically insignificant pre-event coefficients, consistent with the parallel trends assumption, along with a positive trend in lending fees starting around the announcement month and persisting throughout the twelve months following the merger. This positive trend following the merger represents an economically meaningful increase in lending fees and implies tighter short-selling constraints for stocks that were commonly owned by two institutional owners. We use a *clean control* sample in our preferred specification, where we restrict the set of control stocks to those not already treated in any prior merger event, thereby addressing a potential concern about contamination in the control group in our staggered design. To further support the causal nature of our estimates, we implement a placebo test. We randomly reassign treatment status in our clean control sample and re-run the baseline regression 1,000 times. The distributions of placebo coefficients and t-statistics are tightly clustered around zero, while our actual coefficient and t-statistic lie in the extreme right tail. This placebo test supports our conclusion that the magnitude and significance of our baseline coefficient are unlikely to be due to chance or mechanical features of our regression design and stacked data structure.

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. 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 big firms, high-volatility stocks, more liquid stocks (higher turnover, lower

Amihud illiquidity), and stocks with more liquid option markets. These are all features of active short-selling environments. The effect does not vary with pre-merger utilization. The pattern points to a market-power channel: consolidation raises fees most where competition among owners had been holding them down, not through a uniform cut in supply.

As complementary case-study evidence, we estimate difference-in-differences regressions for each of the three largest mergers associated with the financial crisis: Bank of America's acquisition of Merrill Lynch & Co., Barclays PLC's acquisition of the investment banking business of Lehman Brothers, and Wells Fargo & Co.'s acquisition of Wachovia Corporation. These events resulted from the intense stress of the 2008 financial crisis and were driven by regulatory and market pressures, not by strategic goals related to the lending market conditions of the stocks involved. We find consistent results across each of these events with an estimated fee increase between 9.3% to 13.9%. Event-study plots for each case visually demonstrate the lack of pre-trends and the continuation of post-announcement fee increases.

The merger-induced increase in lending fees has direct asset-pricing implications. If higher fees tighten short-sale constraints and slow the incorporation of negative information into prices, treated stocks should be temporarily overvalued at the announcement and then earn negative abnormal returns as prices correct, in line with the classical predictions of Miller [1977] and Diamond and Verrecchia [1987]. Our evidence supports this prediction. Treated stocks earn 10 to 40 basis points lower monthly Fama–French three-factor abnormal returns after the merger. Two-stage least squares regressions that use the merger event as an instrument for the log lending fee confirm that higher lending fees lead to lower abnormal returns. The dynamic pattern reinforces this interpretation: cumulative abnormal returns are statistically indistinguishable from zero in the six and twelve months *before* the announcement—consistent with the parallel-trends evidence on fees—but turn significantly negative thereafter, with a cumulative effect of approximately -2.55% over the twelve-month post-announcement window.

These asset-pricing results connect to a well-established literature linking short-sale constraints to subsequent stock underperformance [Jones and Lamont, 2002, Asquith et al., 2005, Cohen et al., 2007, Boehmer et al., 2008, Drechsler and Drechsler, 2014, Engelberg et al., 2018], and most closely to Porras Prado et al. [2016], who document a similar relationship between ownership concentration and subsequent returns in a cross-sectional setting. Our contribution is to identify the causal channel: merger-induced consolidation among lenders raises lending fees, and the higher fees translate into lower abnormal returns for treated stocks, consistent with overvaluation generated by tightened short-sale constraints.

This paper makes three contributions. First, we provide the first causal evidence on ownership concentration and lending fees from a shock that is exogenous to the individual stock. Porras Prado et al. [2016] document this relationship and use activist 13D stakes and dual-class shares to address endogeneity, but that variation is chosen by investors or firms and so is endogenous to the stocks studied. Financial-institution mergers instead change ownership for reasons unrelated to any single stock's lending conditions. Across 32 mergers, treated stocks experience an economically large and robust fee increase, which also supports the market-power view of the securities lending market in Chen et al. [2022]. Second, we sharpen the link between lending fees and returns. Prior work shows that high fees predict low returns [Jones and Lamont, 2002, Asquith et al., 2005, Engelberg et al., 2018], but its fee variation is endogenous. Because our shock instead raises fees for reasons unrelated to a stock's fundamentals or demand, the lower abnormal returns of treated stocks point to the constraint channel [Miller, 1977] rather than to fees signaling bad news. Third, our findings extend the evidence that ownership concentration generates anticompetitive price effects [Azar et al., 2018] from product markets to financial markets.

2 Related Literature

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 (i.e., fewer short-sale constraints) is associated with faster price discovery and lower return predictability, while constraints do not increase the probability of extreme negative returns.

Kaplan et al. [2013] use a unique natural experiment in which a large institutional manager randomly allocates a portion of its high-fee stock holdings to a treatment group that is made available for lending. This exogenous supply shock significantly reduces lending fees and increases short interest, but has no effect on stock prices, volatility, or liquidity, suggesting that the extent of overpricing caused by short-sale constraints may be overstated.

Engelberg et al. [2018] study the risks associated with short selling and find that the risk of unexpected fee increases or early loan recalls—what they call "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 paper contributes by identifying a causal source of variation in lending fees: changes in institutional ownership concentration induced by financial institution mergers.

The paper most similar to ours is Porras Prado et al. [2016], which examines the relationship between the composition and concentration of institutional ownership and equity lending supply and fees for U.S. stocks. The authors use 13F data and Markit lending data and find that greater concentration of institutional ownership, measured by the Herfindahl-Hirschman Index of institutional holdings, as well as the presence of institutional investors with restricted lending capacity, is associated with higher lending fees, lower lending activity, and lower price efficiency. Their paper establishes a direct cross-sectional relationship between ownership structure and short-sale constraints. While our paper is similar to Porras Prado et al. [2016] in focus and scope, it advances the literature by moving from correlation to causation. We do so by using financial institution mergers as a source of variation in ownership concentration and find that ownership consolidation causally increases lending fees by 8.4% in our preferred specification.

Our identification strategy is also related to recent work that uses financial institution mergers to generate exogenous variation in ownership structure. Friberg et al. [2024] study the BlackRock–Barclays Global Investors merger of 2009 as a single-merger shock and show that merger-induced changes in ownership composition affect stock price fragility and firms' financial policies. He and Huang [2017] use a difference-in-differences design across financial institution mergers to study how merger-induced changes in institutional cross-ownership affect product-market outcomes. These papers demonstrate that financial institution mergers provide a credible setting for identifying the effects of ownership structure. We apply the same insight to the securities lending market. Unlike existing work, our empirical analysis combines both a stacked difference-in-differences design across multiple mergers and separate single-merger analyses for selected cases, allowing us to identify the effect of ownership consolidation on lending fees in both a broad sample and individual settings.

A related 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. They show that 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] study an important conflict of interest in the market: mutual fund managers who retain a share of lending fee revenue through affiliated lending agents have an incentive to overweight high-fee stocks in their portfolios, even if those stocks are expected to underperform. Using mandatory SEC disclosures, they show that "fee-retaining" funds tilt their portfolios toward special stocks, generating underperformance for investors while earning implicit income for the manager.

Our paper contributes to this literature by showing that the concentration of institutional lenders on the supply side—driven by financial institution mergers—is itself a source of market power in the lending market. Mergers reduce the number of independent lenders competing for borrowers of commonly held stocks, and we find that this reduction translates into higher fees even after controlling for demand-side factors.

Our paper is also related 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 prove common ownership's negative effect on product market competition. Although their research is focused 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 has 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 2024, reflecting the coverage of our securities lending data, we impose four main restrictions. First, we include only mergers in which the target, the acquirer, or their parent firms filed a 13F report. Second, we include only mergers with the deal type *Merger* and *Acquisition of Assets*. Third, we retain only mergers with a transaction value above $1 billion. Fourth, we require at least one of the firms to be U.S.-based. A big challenge we face is that the current SDC Mergers and Acquisitions database does not contain information such as CIK, 8-digit CUSIP, or GVKEY. This makes it difficult to perform a direct merge with our 13F data. In addition, when we perform a direct match on the 6-digit CUSIP, we realize that we get incorrect matches and also some missing values. To overcome this challenge, we first perform a preliminary match on text. Then, we perform a manual match on the firms we have identified using the LSEG S34 master file. This enables us to determine if a firm or its parent firm has filed a 13F report. In performing our match, we did not require an exact match on names. Instead, we allowed a match on a parent firm if the parent firm was a 13F filer. For example, for Barclays Global Investors Ltd, we used Barclays Bank PLC, and for Lehman-Invest Bkg Bus, we used Lehman Brothers Inc. After performing these steps, we ended up with a total of 32 mergers. The list of mergers in our sample is contained in Appendix Table A2.

Securities Lending: We obtain security-level securities lending data from Markit Securities Finance from April 2006 to May 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 and value of shares on loan, indicative lending fee, and utilization rate. 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. The dependent variable is the log of the indicative lending fee, which represents the expected cost of borrowing a stock in the lending market. For our cross-sectional analysis, we will also use utilization two months before the merger announcement. Utilization is defined as the value of shares on loan divided by total lendable value and represents how much of the firm's supply is already utilized before the merger announcement.

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. This definition of the treatment and control groups means that they are similar with respect to institutional holdings, with the only difference being the overlap between the two sets of institutional holdings.

Firm-Level Characteristics: In the cross-sectional analysis, we employ several firm-level characteristics to investigate cross-sectional variation in the treatment effect. Using the CRSP database, our first variable is market capitalization, which is our measure of size. Next, we construct two different measures of stock liquidity. One is turnover, which is monthly trading volume scaled by the number of shares outstanding. The other is the Amihud [2002] illiquidity measure. For each stock-day, we calculate illiquidity as the absolute value of the stock's daily return divided by the stock's daily dollar trading volume, where dollar volume is calculated as the absolute value of the stock's price multiplied by trading volume. We then calculate the average of this illiquidity over each stock-month, which we call the firm's monthly illiquidity, based on the Amihud illiquidity formula. This requires us to have at least 10 trading days in the month. In addition, we use OptionMetrics to calculate firm volatility based on 365-day historical volatility. We use OptionMetrics option price data to calculate option liquidity. For each firm, we calculate the volume-weighted average of the normalized bid-ask spread across option contracts. The spread for each option contract is normalized by the mean of the bid and ask quote.

We calculate all firm characteristics two months prior to the merger announcement. For each stock-merger pair, we use the value of the firm characteristic from month $t = -2$, which we then use over the full sample period. We then divide firms into terciles within each merger based on the pre-merger distribution of each firm 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.

3.2 Descriptive Statistics

Tables 1 and 2 present the summary statistics for the stacked difference-in-differences sample and the three selected merger events in the crisis period. The stacked sample runs from July 2006 to May 2024. After applying the data requirements discussed above, there are 1,346,339 stock-month observations over this time span. During this time, 32 financial institution mergers satisfy our screening criteria. The range of these mergers in terms of deal size varies from \$1.1 billion (Aperio-BlackRock) to \$48.8 billion (Merrill Lynch-Bank of America). The three selected crisis-era merger events cover the peak of the crisis in late 2008. Collectively, these three merger events have more than 274,000 stock-month observations. Table 2, panel A, indicates that the mean of the log lending fee in the stacked sample is -5.338 with a standard deviation of 0.945 and a median of -5.591. The interquartile range of the lending fees is small, ranging from -5.831 at the 25th percentile to -5.432 at the 75th percentile. This corresponds to annualized lending fees of roughly 0.29% to 0.66%. These values are consistent with a sample dominated by securities that do not face substantial borrowing scarcity. The mean of the lending fees for the three individual merger samples is similar. Bank of America-Merrill Lynch, Barclays-Lehman IB, and Wells Fargo -Wachovia have mean log lending fees of -5.137, -5.084, and -5.094, respectively. These levels are all slightly higher than the mean of the pooled sample. This is consistent with the high levels of short-selling demand in the 2008 crisis period.

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 -5.259, while control stocks have a mean of -5.387, a difference of 0.128 log points. This pre-announcement gap likely represents the composition effect, where stocks held by both the target and the acquirer—the treated group—tend to be more heavily shorted than those held by only one party. This pre-merger difference is absorbed by the stock-merger fixed effects. The three crisis-era merger cases show similar patterns with slightly larger treated-control spreads, ranging from 0.34 to 0.41 log points. Again, this is consistent with the crisis conditions magnifying the cross-sectional dispersion in borrowing costs.

All the control variables are measured two months prior to the merger announcement. They are thus fixed for each stock-merger pair throughout the event window. Stocks in our sample have a mean market capitalization of \$15.6 billion and a median market capitalization of \$2.3 billion. As is well known, the distribution of market capitalization is right-skewed. Annualized historical volatility is on average 45.6%, with an interquartile range of 28.2% – 56.0%. Monthly turnover is on average 21.3%. Pre-event utilization—the proportion of the value of shares already on loan to the total lendable value—is on average 15.8% with a median of 7.4%. As can be seen, the distribution is right-skewed. A subset of stocks thus 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 1.035×10^{-6} and a standard deviation of 39×10^{-6}, which is in line with the fact that this sample is comprised largely of large, liquid exchange-listed equities. The volume-weighted normalized bid-ask spread in option markets for those firms with available option data averages 48.2% (median 36.7%), while 365-day historical volatility averages 45.6% (median 40.0%). 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 size, liquidity, and short interest—dimensions along which we will 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). We study this question using two complementary empirical designs. Our primary specification is a *stacked* difference-in-differences estimator that pools information across all 32 mergers in our sample in a manner robust to heterogeneous treatment effects. We complement this analysis with a set of *individual* difference-in-differences regressions for three crisis-period mergers that provide particularly clean identification.

3.3.1 Stacked Difference-in-Differences Design

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 32 mergers occur at different dates over the 2007–2024 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 has exactly 25 monthly observations.

We stack these 32 sub-datasets into a single panel and estimate

$$\log(\textit{Lending Fee}_{it}) \;=\; \alpha \;+\; \beta\,(\textit{Treat}_{im} \times \textit{Post}_{mt}) \;+\; \mu_{im} \;+\; \nu_{mt} \;+\; \lambda_{kt} \;+\; \varepsilon_{imt}, \qquad (1)$$

where the unit of observation is stock i in merger event m in month t. 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.

In addition to estimating equation (1) using the full sample, we also estimate this equation using a *clean control* sample. In this sample, we exclude any control stock in a merger event if it was treated in a previous merger event. In other words, a stock cannot be treated in one merger event and then appear in the control group in a later merger event. We report results using both the unrestricted and clean control samples. Finally, standard errors in equation (1) are clustered at the merger level.

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

As a further check on identification, we implement a placebo test using the clean-control sample. For each merger event, we randomly reassign treatment status across stocks while preserving the observed number of treated stocks within that event, and re-estimate equation (1) using this placebo treatment indicator. Repeating this procedure 1,000 times yields an empirical distribution of placebo coefficients and associated t-statistics. If our baseline estimates were driven by spurious features of the sample or by the stacked design itself, estimates of similar magnitude would arise frequently under random assignment.

3.3.2 Individual Merger Difference-in-Differences

We re-run our specification of equation (1) for three large mergers that were driven by the acute stress of the 2008 financial crisis: (i) Bank of America's acquisition of Merrill Lynch & Co. (announced September 2008), (ii) Barclays PLC's acquisition of the investment banking business of Lehman Brothers (announced September 2008), and (iii) Wells Fargo & Co.'s acquisition of Wachovia Corporation (announced October 2008). We find these three mergers particularly well-suited for causal inference because they were driven by the acute stress of the 2008 financial crisis. We estimate equation (1) for each of these mergers individually by limiting our sample to a $[-12, +12]$ month window around the announcement date and including firm fixed effects (μ_i) and industry-by-month fixed effects (λ_{kt}), and cluster our standard errors by CUSIP. An important advantage of these three mergers is that they were initiated by the acute stress of the 2008 financial crisis and not by strategic motivations related to the overlap of the portfolios of the merging firms. Hence, it is unlikely that the announcement of these mergers is endogenous to the lending conditions of the stocks in our data set. We also show event-study plots for each relative month. These plots help us see whether there are pre-trends and whether the treatment effect remains after the announcement.

4 Empirical Results

4.1 Baseline Results for the Full Merger Sample

Table 3 contains the key stacked difference-in-differences estimates of the effect of mergers on stock-level lending fees. In all four models, the estimated effect is positive and statistically significant at 1%. In Column (1), where we control for firm fixed effects and industry-by-month fixed effects, the estimated coefficient is 0.051 ($t = 3.69$), or a 5.2% increase in lending fees. In Column (2), where we add merger-by-month and merger-by-firm fixed effects, which further tighten identification to within merger variation, the estimated coefficient increases slightly.

In columns (3) and (4) we use the *clean-control* sample, which excludes control stocks if they were already treated in any earlier merger event. As can be seen, the estimate increases to 0.081 log points in both columns (3) and (4), with $t = 5.10$ and 4.44, respectively, corresponding to an 8.4% increase in lending fees. 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. For comparison, Table A3 reports the same specifications with standard errors clustered at the firm level.

These magnitudes are economically large. D'Avolio [2002] shows that the median stock loan rate in his sample is close to the general collateral rate, and the 90th percentile special fee is about 8-10 annualized basis points above the general collateral rate. The fee increase of 5.2% to 8.4% after the merger is a substantial tightening of the constraints on short sales for stocks jointly held by the two merging lenders' holdings. This result is consistent with the idea that ownership consolidation reduces the supply of shares available for lending and gives the remaining lender more power to charge higher fees [Chen et al., 2022, Henderson et al., 2023].

Figure 1 presents the event-study estimates from the stacked design. Each point plots the coefficient on $\text{Treat} \times \mathbf{1}[t = k]$ for event months $k \in \{-12, \ldots, -2, 0, \ldots, 12\}$, with month

-1 omitted as the reference period. All regressions include stock, merger-by-month, merger-by-firm, and industry-by-month fixed effects, with standard errors 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.

As a supplementary exercise, we report results using lending supply and utilization as dependent variables in Appendix Table 9. The estimates are broadly consistent with the fee results. The coefficients on lending supply are negative, while those on utilization are positive, although these effects are not statistically significant in our preferred specification. This pattern suggests that mergers affect borrowing costs even when the quantity effects are not estimated precisely.

4.1.1 Placebo Test

To further assess the causal interpretation of our difference-in-differences results, we conduct a placebo test based on the clean-control sample. This exercise examines whether the estimated effects could be driven by spurious patterns in the data rather than by a causal effect of mergers on lending fees.

In particular, for each merger event, we randomly reassign the treatment status among the stocks while keeping the number of treated observations the same as the original sample. Then, we re-run the baseline stacked DiD model with the new placebo treatment group. This process is repeated 1,000 times to obtain the distribution of the placebo coefficient estimates and the corresponding t statistics.

As shown in panel (a) of Figure 2, the distribution of placebo coefficients is tightly centered around zero. In contrast, the actual coefficient estimate from the baseline regression lies far to the right of this distribution. This suggests that the baseline estimate is unlikely to have arisen by chance.

Panel (b) shows the distribution of the placebo t-statistics. Again, the distribution is centered around zero, and the actual t-statistic from the baseline regression is far out in the right tail of this distribution. This suggests that the statistical significance of the baseline result is not due to the mechanical properties of the regression design or to random correlations in the data.

Taken together, these results provide strong support for the validity of our approach. If the estimated treatment effect were driven by spurious factors, such as time trends, composition effects, or the stacked structure of the data, we would expect similarly large effects to appear frequently in the placebo distribution. Instead, the actual estimate is an extreme outlier relative to what we observe under random assignment.

4.2 Heterogeneous Effects by Stock Characteristics

Table 4 investigates whether the effect of mergers on lending fees varies systematically by pre-merger firm characteristics. The goal of this analysis is to determine which types of stocks experience the largest effects of mergers on lending fees and thereby shed further light on the underlying channel of the initial finding. In each regression, stocks are split into terciles based on the relevant characteristic before the merger and held fixed over the entire sample period. The $Treat \times Post$ 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) studies heterogeneity by firm size, measured by market capitalization. The estimated effect is larger for larger firms: while the coefficient on $Treat \times Post$ is 0.050 in the smallest size tercile, the coefficient for firms in the highest tercile is 0.102 and statistically significant at the 5% level. This translates to a total effect of 16% for larger firms. One possible explanation for these findings is that ownership consolidation is more important for large-cap stocks, as these typically have more developed lending markets, so the loss of an independent lending source has a larger effect on equilibrium borrowing costs.

Column (2) examines heterogeneity by utilization, measured as the value of shares on loan divided by total lendable value prior to the merger announcement. The point estimates suggest that the impact of consolidation is larger when the lending market is tight, consistent with the idea that borrowing costs respond more when a larger fraction of lendable supply is already in use. However, the interaction terms are not statistically significant.

Column (3) 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 $Treat \times Post$ for the lowest tercile of stock volatility is 0.058, whereas the implied effect for the highest tercile is 0.068. The effect for the highest tercile is statistically significant at the 5% level and implies a total effect of 13%. 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 (4) and (5) demonstrate how this effect is linked to stock liquidity. In Column (4), 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 $Treat \times Post$ equals 0.057 for the lowest turnover tercile, while the additional effect for the highest tercile is 0.073 and statistically significant at the 10% level. In Column (5), we replicate this finding using an alternative liquidity measure based on the Amihud illiquidity ratio. In this column, we find that the consolidation effect is strongest for more liquid stocks and weakest for more illiquid stocks. The coefficient on $Treat \times Post$ equals 0.132 for the lowest illiquidity tercile, while the interaction for the highest tercile is -0.100 and statistically significant at the 5% level. This implies a total effect of only 0.032 log points 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.

Column (6) explores heterogeneity in the effect by options liquidity, which we measure using the volume-weighted average relative bid-ask spread across the firm's option contracts, with higher values indicating lower liquidity. We include options liquidity because options can provide an alternative way to take short positions, and because they may also capture how liquid and developed the broader trading environment is. The effect of the merger is largest for stocks with the most liquid options and declines as options become less liquid. The coefficient on $Treat \times Post$ is 0.186 for the most liquid tercile, while the interaction terms for terciles 2 and 3 are negative and statistically significant, implying total effects of 0.099 and 0.064, respectively. This pattern suggests that the effect of ownership consolidation on lending fees is strongest for stocks with more liquid and developed trading environments.

4.3 Evidence from Selected Merger Events

Table 5 presents the DiD estimates for each merger individually. We estimate three progressively richer specifications for each panel: Column (1) is the raw DiD without fixed effects; Column (2) adds firm fixed effects; and Column (3) further adds industry-by-month fixed effects, absorbing all industry-level time-series variation in lending fees.

The results are economically significant and remarkably consistent across all three mergers. For the Bank of America–Merrill Lynch merger (Panel A), the estimated treatment effect increases from 0.062 log points ($t = 3.06$) in Column (1) to 0.081 ($t = 4.13$) when firm fixed effects are included, and to 0.089 ($t = 4.49$) in the most saturated specification. For the Barclays–Lehman merger (Panel B) and the Wells Fargo–Wachovia merger (Panel C), in the model with all sets of fixed effects, the estimated effect is approximately 12.3% and 13.9% increase, respectively, and is statistically significant at the 1% level. The estimated effect in all three cases either increases or remains stable as we move from less saturated to more saturated specifications. This is the expected pattern if unobserved firm heterogeneity creates downward attenuation bias in the less saturated specifications.

These results are consistent with, and in some cases larger than, the estimates from the stacked sample (Section 4.1). This is unsurprising: the three events are between very large institutions, and the market for borrowing shares in large-institution portfolios is particularly active and price-sensitive. Chen et al. [2022] show that lenders with larger share inventories can exercise greater market power in fee setting, consistent with the notion that the surviving entity in each of these mergers—Bank of America, Barclays, and Wells Fargo—faced reduced competitive pressure from other potential lenders holding the same stocks.

Figure 3 shows graphical evidence on the pre-merger comparability condition. In all of the selected mergers, the treated and control groups move closely before the merger, suggesting no clear evidence of pre-merger divergence during the control periods. This is an important condition because the validity of the DiD approach rests on the treated and control stocks moving similarly before the merger, which is the identifying assumption in the analysis of the entire sample.

Figure 4 shows an event study for single mergers, which again supports the pre-merger patterns in Figure 3. In the pre-merger periods, the coefficients are stable, suggesting no clear evidence of pre-merger divergence between the treated and control groups, while the coefficients move upward in the post-merger periods, suggesting an upward movement in treated stocks relative to control stocks after the merger announcement. Moreover, the upward movement in treated stocks is persistent rather than transient, suggesting an effect of the merger on the lending market rather than an announcement effect.

4.4 Asset Pricing Implications

The results so far show that ownership consolidation raises lending fees. We now ask whether these higher fees matter for prices. The logic is simple. When borrowing a stock becomes more expensive, short selling becomes harder, and negative information is incorporated into prices more slowly. Treated stocks should therefore be temporarily overvalued around the merger and should then earn negative abnormal returns as prices correct. This is the classic prediction of Miller [1977] and Diamond and Verrecchia [1987]. We test it in two steps. We first ask whether the merger itself moves abnormal returns, and we then ask whether that movement is consistent with the higher lending fees it generates.

Table 6 reports the results. The dependent variable is the monthly Fama–French three-factor abnormal return. Panel A presents the reduced-form effect of the merger on abnormal returns, and Panel B presents two-stage least squares (2SLS) estimates in which we instrument the log lending fee with the merger treatment indicator, $Treat \times Post$. Columns (1) and (2) use the full stacked sample, while columns (3) and (4) use the preferred clean-control sample. Across columns, we progressively add fixed effects in the same way as in our baseline specification.

Panel A is our main asset-pricing result. Treated stocks earn lower abnormal returns after the merger. The coefficient on $Treat \times Post$ ranges from -0.001 in the full sample to -0.004 in the most saturated clean-control specification, and is statistically significant in every column. In economic terms, treated stocks earn roughly 10 to 40 basis points lower abnormal returns per month than control stocks following the merger. The effect is somewhat larger in the clean-control sample, consistent with the contamination concern that motivates that sample throughout the paper. These magnitudes are modest and in line with what one would expect from a moderate tightening of short-sale constraints.

Panel B asks whether this return effect is plausibly attributable to the higher lending fees, rather than to some other consequence of the merger. Because the merger is plausibly exogenous to any individual stock's lending conditions, it can serve as an instrument for the lending fee. We focus on our preferred specification in column (4), which uses the clean-control sample and the full set of fixed effects, and which has a strong first stage, with a Kleibergen–Paap F-statistic of about 20. The second-stage coefficient on the log lending fee is negative and statistically significant, confirming that the return effect runs in the direction predicted by the short-sale-constraint channel: higher fees are associated with lower abnormal returns. We read this estimate as corroborating the *sign* of the channel rather than as a precise structural magnitude. Because the instrument moves fees by only a modest amount—about 0.08 log points in the first stage—the implied per-log-point slope is large mechanically and is best understood through the merger-scaled reduced-form effect in Panel A rather than as a standalone elasticity. We therefore emphasize the reduced-form estimates in Panel A and treat the 2SLS result as supporting evidence on the direction of the effect.

Table 7 compares the cumulative abnormal returns (CARs) of treated and control stocks over fixed windows around the merger. For each stock–merger pair, we cumulate monthly abnormal returns over the relevant window, leaving a single CAR per stock, and then regress this CAR on the treatment indicator with merger and industry fixed effects. Columns (1) and (3) cover the pre-announcement windows, which serve as placebo tests, and columns (2) and (4) cover the post-announcement windows. The pre-announcement coefficients are economically small and statistically indistinguishable from zero over both the six-month and twelve-month windows, indicating that treated and control stocks do not differ in returns *before* the merger. This is the return-based counterpart to the absence of pre-trends in the lending-fee event studies. After the announcement, treated stocks underperform controls. The point estimate is negative but statistically insignificant over the six-month window (-1.22%), and becomes larger and significant at the 5% level over the twelve-month window, where treated stocks underperform controls by approximately -2.55%. The fact that the return gap is absent before the merger and emerges only afterward, growing with the horizon, is consistent with prices correcting as the tighter short-sale constraints take hold.

Taken together, these results connect our findings to a large literature linking short-sale constraints to subsequent stock underperformance [Jones and Lamont, 2002, Asquith et al., 2005, Cohen et al., 2007, Boehmer et al., 2008, Drechsler and Drechsler, 2014, Engelberg et al., 2018], and most closely to Porras Prado et al. [2016], who document a similar relationship between ownership concentration and returns in a cross-sectional setting. Our contribution is to identify the underlying causal channel: merger-induced consolidation among lenders raises lending fees, and the higher fees translate into lower abnormal returns for treated stocks, consistent with overvaluation generated by tightened short-sale constraints.

4.5 Institutional Portfolio Response

Having established that merger-induced ownership consolidation leads to higher lending fees, and that higher fees are associated with lower abnormal returns, we now examine how institutional investors respond to these fee increases in their portfolio decisions. Specifically, we ask whether the magnitude of the merger-induced fee increase predicts whether a stock is subsequently dropped from or retained in the institutional portfolio.

To conduct this analysis, we divide the treated stocks in our sample into two groups based on their post-merger holding status. Stocks that are dropped from the portfolio of the merged institution following the merger are classified as *Treated-Dropped*, while stocks that remain in the portfolio are classified as *Treated-Held*. We estimate the average fee increase for each group separately and test whether the two groups experienced significantly different fee increases following the merger. An important feature of this analysis is that our lending-fee data extend only to twelve months after the announcement. The fee increase for each group is therefore measured over this first post-merger year, while a stock's holding status is determined separately at the 1-, 2-, and 3-year horizons. For the 2- and 3-year horizons, this means we relate fee increases observed in the first year to dropping decisions that take place later, rather than observing fees at the time a stock is actually dropped.

Table 8 reports the results across three holding horizons: 1, 2, and 3 years after the merger announcement. Panel A presents the fee effects separately for each group and Panel B reports the difference between the two groups (Held minus Dropped). Both groups experience positive and statistically significant fee increases following the merger, consistent with the baseline results in Table 3. However, stocks that are subsequently dropped from the portfolio experience larger fee increases than stocks that are retained. At the 1-year horizon, the fee increase for the *Treated-Dropped* group is 0.151 log points, compared to 0.076 log points for the *Treated-Held* group, although the difference is not statistically significant at conventional levels. At the 2- and 3-year horizons, the difference (Held minus Dropped) reported in Panel B is negative and statistically significant at the 1% level, with estimates of -0.037 and -0.032, respectively. Because these fee increases are measured in the first post-merger year while the corresponding drops occur over a longer horizon, the pattern indicates that the stocks an institution eventually exits are those that had already exhibited the largest fee increases shortly after the merger.

We interpret this pattern cautiously. What the analysis establishes is descriptive: among treated stocks, those that the merged institution subsequently drops are precisely the ones that experienced the largest lending-fee increases after the merger. We are reluctant to read a particular motivation or direction of causality into this association. One possibility is that institutions exit these stocks in response to the higher fees and the lower abnormal returns documented in Section 4.4, treating the fee increase as a signal of weaker return prospects.

This finding offers a suggestive contrast with the mutual fund settings studied by Johnson and Weitzner [2025] and Evans et al. [2017]. Johnson and Weitzner [2025] find that fee-retaining mutual funds overweight high-fee stocks, as the fund family directly benefits from lending income and this distorts portfolio decisions. Evans et al. [2017] find that mutual funds subject to investment style restrictions set by the fund family tend to lend rather than sell stocks with high short-selling demand, precisely because they cannot act on the fee signal. The institutions in our sample — large financial institutions such as banks and asset managers — may face different incentive structures and fewer investment restrictions than the mutual funds studied in those papers, which could help explain why their portfolio response to rising fees differs. We leave a more complete investigation of this cross-sectional variation to future research.

4.6 Additional Analysis

The baseline results establish that mergers among financial institutions lead to a significant increase in lending fees for commonly held stocks. A natural question is whether this fee increase reflects a reduction in the supply of lendable shares or an increase in the market power of the merged institution in setting fees. To address this, Table 9 reports the effect of mergers on two additional lending market outcomes: lending supply and utilization. Lending supply is measured as lendable quantity — the stock inventory available for lending — divided by shares outstanding. Utilization is measured as the value of assets on loan divided by total lendable value, and captures what fraction of the available lending inventory is actually being borrowed at any point in time.

If the fee increase were driven by a reduction in the quantity of shares made available for lending, we would expect to see a significant decline in lending supply following the merger. If instead the fee increase reflects greater market power of the merged institution, we would expect lending supply to remain broadly unchanged while utilization increases, as borrowers continue to demand shares despite the higher cost.

In our preferred clean-control specification, the coefficients on lending supply are negative but statistically insignificant, suggesting that the merger does not lead to a meaningful reduction in the quantity of shares made available for lending. Similarly, the coefficients on utilization are positive but statistically insignificant in the preferred specification, although the point estimates are positive across all columns, consistent with continued borrowing demand following the merger. Taken together, these results suggest that the fee increases documented in Table 3 are not driven by a reduction in lending supply, and are more consistent with a pricing power channel than a simple supply restriction story, though we acknowledge that the quantity effects are not precisely estimated.

5 Conclusion

This paper examines how institutional ownership affects the cost of borrowing stock in the equity lending market. We use financial institution mergers as natural experiments. When two financial institutions merge, the number of potential lenders for stocks held by both the target and acquiring institutions declines. Using a stacked difference-in-differences approach that combines 32 mergers announced between 2007 and 2024, we find that stocks held by both merging institutions before the merger announcement experience a sustained increase in lending fees of 5.2% to 8.4% relative to stocks held by only one of the two institutions.

Three aspects of the analysis support a causal interpretation of this result. First, the event-study estimates do not reveal any pre-trends. The pre-announcement coefficients fluctuate around zero, taking both positive and negative values, while a clear upward shift appears in the announcement month and persists throughout the post-merger period. Second, using a control group consisting only of stocks not treated in any prior merger event—the "clean control group"—yields an estimated 8.4% increase for the treatment group, confirming that the baseline results are not driven by contamination of the control group. Third, a placebo exercise in which we randomly reassign treatment status 1,000 times shows that the actual coefficient and t-statistic lie in the far right tail of the placebo distribution. Finally, separate difference-in-differences estimates for three large financial institution mergers during the crisis—Bank of America-Merrill Lynch, Barclays-Lehman Brothers, and Wells Fargo-Wachovia—show even larger effects, ranging from 9.3% to 13.9%.

The cross-sectional heterogeneity in this treatment effect provides additional insight into the underlying mechanisms. In particular, ownership consolidation leads to larger increases in lending fees for stocks with greater market capitalization, higher historical volatility, higher turnover, lower Amihud illiquidity, and more liquid option markets. This pattern suggests that the merger effect is concentrated in stocks with deep, active, and competitive lending environments, precisely where the loss of a competing lender is most likely to affect the market equilibrium.

The fee increases also matter for prices. Treated stocks earn lower Fama–French three-factor abnormal returns after the merger, roughly 10 to 40 basis points per month, with cumulative abnormal returns near zero before the announcement and significantly negative afterward (about -2.55% over the following year). This is consistent with overvaluation arising when short-sale constraints tighten.

Examining the merged institutions' own holdings, we find that the treated stocks they later drop are those that experienced the largest fee increases in the first post-merger year. Because the fee increases precede these dropping decisions, the pattern is unlikely to be a mechanical effect of unwinding positions. We document this association without taking a stand on its motivation, as our data cannot separate a response to the fee signal from a common underlying cause.

Finally, neither lending supply nor utilization changes significantly after the merger. This suggests that the higher fees reflect greater pricing power of the merged institution rather than a contraction in lendable supply, consistent with the heterogeneity evidence that the effect is concentrated in the most active lending markets.

Our findings have several implications. First, they show that supply-side market structure, and in particular concentration among institutional lenders, is a first-order determinant of equity lending fees. While previous studies document cross-sectional relationships between ownership characteristics and borrowing costs, our design identifies the underlying causal channel using plausibly exogenous variation in ownership concentration. Second, these findings offer new insight into how market power operates in decentralized financial markets characterized by bilateral negotiation, limited transparency, and informational frictions. Even a modest reduction in lender competition, driven by mergers unrelated to the lending conditions of individual stocks, has economically meaningful and persistent effects on borrowing costs. Finally, the results suggest that consolidation among financial institutions can tighten short-sale constraints on a broad scale, which may in turn slow the incorporation of negative information into prices and increase the costs of arbitrage.

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Table 1: Sample Summary

This table reports summary statistics for the announcement month in the pooled stacked difference-in-differences sample and for three selected crisis-era merger case studies. Treated stocks are those held simultaneously by both the acquirer and the target in the quarter prior to the merger announcement, while control stocks are those held by exactly one of the two institutions in the same pre-announcement quarter. For each sample, the table reports the treated mean, control mean, treated-minus-control difference, and the numbers of treated and control observations for both Lending Fee and Log(Lending Fee).

Variable	Mean (Treated)	Mean (Control)	Difference	N(Treated)	N(Control)
A. Stacked DiD Sample					
Log(Lending Fee)	-5.259	-5.387	0.128	42,195	13,249
Lending Fee (bps)	170	100	70	42,195	13,249
B. Selected Crisis-Era Merger Cases					
Bank of America–Merrill Lynch					
Announcement Date: September 14, 2008					
Log(Lending Fee)	-4.737	-5.080	0.343	2,135	1,149
Lending Fee (bps)	220	140	80	2,135	1,149
Barclays–Lehman IB					
Announcement Date: September 16, 2008					
Log(Lending Fee)	-4.563	-4.973	0.411	1,889	2,368
Lending Fee (bps)	250	180	60	1,889	2,368
Wells Fargo–Wachovia					
Announcement Date: October 3, 2008					
Log(Lending Fee)	-4.576	-4.972	0.396	1,477	2,316
Lending Fee (bps)	250	160	100	1,477	2,316

Table 2: Summary Statistics

This table reports summary statistics for the pooled stacked difference-in-differences sample and for three selected crisis-era merger case studies. For each variable, the table reports the mean, standard deviation, 25th percentile, median, 75th percentile, and number of observations. Log(Lending Fee) is the logarithm of the monthly average of daily indicative lending fees for each stock. In the stacked DiD sample, firm-level characteristics are measured two months prior to the merger announcement date ($t = -2$) for each merger–CUSIP pair. Size is measured as market capitalization. Lending Supply is defined as the value of lendable shares available for borrowing divided by total shares outstanding, expressed as a percentage. FF3 Abnormal Return is the monthly return minus the return predicted by the Fama–French three-factor model, with factor loadings estimated over a trailing 36-month rolling window (minimum 24 months). Utilization is defined as the value of shares on loan divided by total lendable value. Volatility is measured as 365-day historical volatility. Turnover is defined as trading volume divided by shares outstanding. Stock illiquidity is measured using the Amihud illiquidity measure. Option liquidity is measured as the volume-weighted average relative bid–ask spread across a firm's option contracts, where each spread is scaled by the midpoint of the bid and ask quotes. Market capitalization is reported in $ billions. Amihud illiquidity is multiplied by 10^6 for readability.

Variable	Mean	Std. Dev.	p25	p50	p75	Obs.
A. Stacked DiD Sample						
Time period: 2006m7–2024m5						
Log(Lending Fee)	-5.338	0.945	-5.831	-5.591	-5.432	1,346,339
Lending Supply (%)	27.156	15.209	18.059	28.288	37.311	1,175,938
FF3 Abnormal Return	-0.002	0.111	-0.056	-0.004	0.047	1,105,533
Measured two months before the merger announcement ($t = -2$):						
Size ($ bil.)	15.576	69.252	0.564	2.266	8.897	48,772
Volatility (%)	45.6	25.0	28.2	40.0	56.0	39,955
Turnover (%)	21.3	45.1	8.7	14.300	24.1	48,861
Utilization (%)	15.761	19.717	1.868	7.439	22.020	54,934
Amihud Illiquidity ($\times 10^6$)	1.035	39.092	0.000	0.001	0.006	48,760
Option Liquidity (%)	48.2	35.6	23.8	36.7	60.7	39,781
B. Selected Crisis-Era Merger Cases						
Bank of America–Merrill Lynch						
Time period: 2007m9–2009m9						
Log(Lending Fee)	-5.137	0.950	-5.573	-5.471	-5.292	80,346
Barclays–Lehman Investment Banking Business						
Time period: 2007m9–2009m9						
Log(Lending Fee)	-5.084	0.979	-5.562	-5.442	-5.210	104,930
Wells Fargo–Wachovia						
Time period: 2007m10–2009m10						
Log(Lending Fee)	-5.094	0.996	-5.586	-5.463	-5.235	89,919

Table 3: DiD Test of the Effect of Institution Mergers on Lending Fees

This table reports stacked difference-in-differences estimates of the effect of financial institution mergers on stock-level lending fees. The dependent variable is the log of lending fees. For each merger event, treated stocks are those held simultaneously by both the target and the acquirer in the quarter prior to the merger announcement, while control stocks are those held by exactly one of the two institutions in that same pre-announcement quarter. The sample is constructed by stacking merger-specific event windows spanning the 12 months before and the 12 months after each merger announcement. *Post* equals one for months after the merger announcement within each event window. Columns (1) and (2) use the full stacked sample, while columns (3) and (4) use the clean-control sample, which excludes control observations for stocks that were already treated in any prior merger event. Specifically, once a stock first appears in the treated group, it is no longer allowed to serve as a control in later merger-event windows. Columns (1) and (3) include firm fixed effects and industry-by-month fixed effects. Columns (2) and (4) additionally include merger-by-month fixed effects and merger-by-firm fixed effects. t-statistics are reported in parentheses. Standard errors are clustered at the merger level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: Log(Lending Fee)			
	All		Clean Control	
	(1)	(2)	(3)	(4)
Treat × Post	0.051***	0.059***	0.081***	0.081***
	(3.69)	(3.60)	(5.10)	(4.44)
Firm FE	Y	Y	Y	Y
Industry×Month FE	Y	Y	Y	Y
Merger×Month FE	N	Y	N	Y
Merger×Firm FE	N	Y	N	Y
Observations	1,345,778	1,345,644	791,235	791,130
Adjusted R^2	0.734	0.819	0.747	0.803

Table 4: Cross-Sectional Analysis

This table reports stacked difference-in-differences estimates of the effect of financial institution mergers on stock-level lending fees, allowing the treatment effect to vary across terciles of pre-merger stock characteristics. The dependent variable is the log of lending fees. Treated stocks are those held simultaneously by both the target and the acquirer in the quarter prior to the merger announcement, while control stocks are those held by exactly one of the two institutions in that same pre-announcement quarter. The sample is constructed by stacking merger-specific event windows spanning the 12 months before and the 12 months after each merger announcement. *Post* equals one for months after the merger announcement within each event window. For each merger-stock pair, the interaction variable is measured two months before the announcement ($t = -2$) and then held fixed over the full event window. In column (1), size is measured as market capitalization. Column (2) uses utilization, defined as the value of shares on loan divided by total lendable value; column (3) uses 365-day historical volatility; column (4) uses share turnover, defined as trading volume divided by shares outstanding; column (5) uses stock illiquidity, measured by the Amihud illiquidity measure; and column (6) uses option liquidity, measured as the volume-weighted average relative bid-ask spread across a firm's option contracts, where each spread is scaled by the mean of the bid and ask quotes. All specifications use the clean-control sample, which excludes control observations for stocks that were already treated in any prior merger event. t-statistics based on standard errors clustered at the merger level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: Log(Lending Fee)					
	(1) Size	(2) Util.	(3) Vol.	(4) Turn.	(5) Amihud	(6) OptLiq
Treat × Post	0.050***	0.052**	0.058**	0.057***	0.132***	0.186***
	(3.369)	(2.102)	(2.372)	(3.184)	(3.769)	(3.344)
Treat × Post × Tercile 2	0.033	0.019	-0.011	-0.002	-0.048	-0.087**
	(0.817)	(0.894)	(-0.634)	(-0.100)	(-1.541)	(-2.310)
Treat × Post × Tercile 3	0.102**	0.066	0.068**	0.073*	-0.100**	-0.122**
	(2.420)	(1.439)	(2.091)	(1.722)	(-2.298)	(-2.320)
Firm FE	Y	Y	Y	Y	Y	Y
Industry×Month FE	Y	Y	Y	Y	Y	Y
Merger×Month FE	Y	Y	Y	Y	Y	Y
Merger×Firm FE	Y	Y	Y	Y	Y	Y
Observations	663,062	774,129	497,526	670,243	668,659	494,877
Adjusted R^2	0.791	0.799	0.781	0.794	0.793	0.782

Table 5: Case-Study Evidence from Three Major Crisis-Era Mergers

This table reports difference-in-differences estimates of the effect of three major crisis-era financial institution mergers on stock-level lending fees. The dependent variable is the log of lending fees. The three case studies are: Panel A, Bank of America's acquisition of Merrill Lynch & Co. (announced September 2008); Panel B, Barclays PLC's acquisition of the investment banking business of Lehman Brothers (announced September 2008); and Panel C, Wells Fargo & Co.'s acquisition of Wachovia Corporation (announced October 2008). For each merger, treated stocks are those held simultaneously by both the acquirer and the target in the quarter prior to the merger announcement, while control stocks are those held by exactly one of the two institutions in that same pre-announcement quarter. *Post* equals one for months after the merger announcement within the event window. Column (1) reports estimates without fixed effects. Column (2) adds firm fixed effects. Column (3) further adds industry-by-month fixed effects. t-statistics are reported in parentheses. Standard errors are clustered at the CUSIP level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Bank of America–Merrill Lynch			
	Dependent Variable: Log(Lending Fee)		
	(1)	(2)	(3)
Treat × Post	0.062***	0.081***	0.089***
	(3.062)	(4.130)	(4.490)
Firm FE	N	Y	Y
Industry×Month FE	N	N	Y
Observations	80,346	80,325	80,219
Adjusted R^2	0.022	0.709	0.736
Panel B: Barclays–Lehman Investment Banking Business			
	Dependent Variable: Log(Lending Fee)		
	(1)	(2)	(3)
Treat × Post	0.106***	0.098***	0.116***
	(5.641)	(5.327)	(6.245)
Firm FE	N	Y	Y
Industry×Month FE	N	N	Y
Observations	104,930	104,909	104,776
Adjusted R^2	0.039	0.714	0.740
Panel C: Wells Fargo–Wachovia			
	Dependent Variable: Log(Lending Fee)		
	(1)	(2)	(3)
Treat × Post	0.107***	0.102***	0.130***
	(4.869)	(4.813)	(6.264)
Firm FE	N	Y	Y
Industry×Month FE	N	N	Y
Observations	89,919	89,898	89,764
Adjusted R^2	0.048	0.715	0.737

Table 6: Asset Pricing Implications: Lending Fees and Abnormal Returns

This table reports asset pricing implications of the effect of financial institution mergers on stock-level lending fees. Panel A reports reduced-form estimates of the effect of mergers on Fama–French three-factor abnormal returns. Panel B reports second-stage estimates from a two-stage least squares (2SLS) regression in which the log of lending fees is instrumented by the merger treatment indicator (Treat × Post). For each merger event, treated stocks are those held simultaneously by both the target and the acquirer in the quarter prior to the merger announcement, while control stocks are those held by exactly one of the two institutions in that same pre-announcement quarter. The sample is constructed by stacking merger-specific event windows spanning the 12 months before and the 12 months after each merger announcement. *Post* equals one for months after the merger announcement within each event window. Columns (1) and (2) use the full stacked sample, while columns (3) and (4) use the clean-control sample, which excludes control observations for stocks that were already treated in any prior merger event. Columns (1) and (3) include firm fixed effects and industry-by-month fixed effects. Columns (2) and (4) additionally include merger-by-month fixed effects and merger-by-firm fixed effects. t-statistics are reported in parentheses. Standard errors are clustered at the merger level. KP F-statistic denotes the Kleibergen–Paap rk Wald F-statistic. Underidentification p-val reports the Kleibergen–Paap rk LM test p-value. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	All Sample		Clean Control	
	(1)	(2)	(3)	(4)
Panel A: Reduced Form				
Dependent Variable: FF3 Abnormal Return				
Treat × Post	−0.001***	−0.002***	−0.003***	−0.004**
	(−4.304)	(−2.749)	(−4.741)	(−2.417)
Firm FE	Y	Y	Y	Y
Industry×Month FE	Y	Y	Y	Y
Merger×Month FE	N	Y	N	Y
Merger×Firm FE	N	Y	N	Y
Adjusted R^2	0.096	0.079	0.085	0.061
Observations	1,105,014	1,104,945	596,697	596,642
Panel B: Second Stage (2SLS)				
Dependent Variable: FF3 Abnormal Return				
Log(Lending Fee)	−0.165	−0.053***	−0.085***	−0.054**
	(−0.986)	(−3.990)	(−2.919)	(−2.294)
Firm FE	Y	Y	Y	Y
Industry×Month FE	Y	Y	Y	Y
Merger×Month FE	N	Y	N	Y
Merger×Firm FE	N	Y	N	Y
KP F-statistic	0.957	10.559	9.324	20.017
Underidentification p-val	0.318	0.034	0.033	0.022
Observations	1,101,798	1,101,724	593,857	593,798

Table 7: Cross-Sectional Test of CAR Around Merger Events

This table reports cross-sectional estimates of cumulative abnormal returns (CARs) around merger events. The dependent variable is the cumulative Fama–French three-factor abnormal return over the indicated window. CAR $[-6, -1]$ and CAR $[-12, -1]$ are cumulative abnormal returns from six and twelve months before to one month before the merger announcement, respectively, and serve as placebo tests. CAR $[0, +6]$ and CAR $[0, +12]$ are cumulative abnormal returns from the merger announcement month to six and twelve months after the announcement, respectively. Treat equals one for stocks held simultaneously by both merging institutions in the quarter prior to the merger announcement, and zero for stocks held by exactly one of the two institutions. All specifications include merger fixed effects and industry fixed effects. *t*-statistics are reported in parentheses. Standard errors are clustered at the merger level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	CAR $[-6, +6]$		CAR $[-12, +12]$	
	Pre $[-6, -1]$	Post $[0, +6]$	Pre $[-12, -1]$	Post $[0, +12]$
	(1)	(2)	(3)	(4)
Treat	−0.0005	−0.0122	0.0046	−0.0255**
	(−0.06)	(−1.41)	(0.38)	(−2.26)
Merger FE	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
Observations	57,048	57,048	57,048	57,048
Adjusted R^2	0.0183	0.0126	0.0109	0.0132

Table 8: Fee Effects by Holding Duration

This table reports stacked difference-in-differences estimates of the effect of financial institution mergers on stock-level lending fees, distinguishing between stocks that are dropped from and stocks that are retained in the portfolio of the merged institution following the merger. The dependent variable is the log of lending fees. The sample is the clean-control sample, which excludes control observations for stocks that were already treated in any prior merger event. Treated stocks are divided into two groups based on their post-merger holding status. *Treated-Dropped* are treated stocks that are no longer held by the merged institution at the end of the indicated holding horizon (1, 2, or 3 years after the merger announcement). *Treated-Held* are treated stocks that remain in the portfolio of the merged institution at the end of the indicated holding horizon. Panel A reports the fee effect separately for each group. Panel B reports the difference in fee effects between the two groups (Held minus Dropped). All specifications include firm fixed effects, merger-by-month fixed effects, merger-by-firm fixed effects, and industry-by-month fixed effects. t-statistics based on standard errors clustered at the merger level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) 1 Year	(2) 2 Years	(3) 3 Years
Panel A: Fee Effects by Group			
Treated-Dropped × Post	0.151***	0.108***	0.103***
	(2.92)	(5.48)	(5.55)
Treated-Held × Post	0.076***	0.071***	0.071***
	(4.17)	(3.96)	(3.95)
Panel B: Difference (Held − Dropped)			
Held − Dropped	−0.075	−0.037***	−0.032***
	(−1.51)	(−3.30)	(−3.18)
Firm FE	Yes	Yes	Yes
Merger×Month FE	Yes	Yes	Yes
Merger×Firm FE	Yes	Yes	Yes
Industry×Month FE	Yes	Yes	Yes
Observations	791,130	791,130	791,130

Table 9: DiD Test of the Effect of Institution Mergers on Lending Supply and Utilization

This table reports stacked difference-in-differences estimates of the effect of financial institution mergers on stock-level lending supply and utilization. Lending supply is measured as lendable quantity divided by shares outstanding, expressed in percentage terms, and utilization is measured as the value of assets on loan divided by total lendable value. For each merger event, treated stocks are those held simultaneously by both the target and the acquirer in the quarter prior to the merger announcement, while control stocks are those held by exactly one of the two institutions in that same pre-announcement quarter. The sample is constructed by stacking merger-specific event windows spanning the 12 months before and the 12 months after each merger announcement. *Post* equals one for months after the merger announcement within each event window. Columns (1) and (2) report lending supply estimates for the full sample, columns (3) and (4) report lending supply estimates for the clean-control sample, columns (5) and (6) report utilization estimates for the full sample, and columns (7) and (8) report utilization estimates for the clean-control sample. The clean-control sample excludes control observations for stocks that were already treated in any prior merger event. Odd-numbered columns include firm fixed effects and industry-by-calendar-month fixed effects. Even-numbered columns additionally include merger-by-month fixed effects and merger-by-firm fixed effects. t-statistics are reported in parentheses. Standard errors are clustered at the merger level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: Lending Supply				Dependent Variable: Utilization			
	All		Clean Control		All		Clean Control	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat × Post	-0.192	-0.144	-0.361*	-0.293	1.207*	1.524**	1.052	1.203
	(-1.30)	(-0.70)	(-2.01)	(-1.22)	(2.01)	(2.20)	(1.33)	(1.36)
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y
Industry×Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Merger×Month FE	N	Y	N	Y	N	Y	N	Y
Merger×Firm FE	N	Y	N	Y	N	Y	N	Y
Observations	1,175,395	1,175,307	662,367	662,299	1,346,122	1,345,989	790,997	790,893
Adjusted R^2	0.868	0.931	0.856	0.906	0.633	0.772	0.667	0.754

Figure 1: Dynamic Effects of Institution Mergers on Lending Fees: Stacked Event-Study Evidence

This figure plots the event-study estimates from the stacked difference-in-differences design for the effect of institution mergers on stock-level lending fees in the clean-control sample. The dependent variable is the log of indicative lending fees. For each merger event, treated stocks are those held simultaneously by both the target and the acquirer in the quarter prior to the merger announcement, while control stocks are those held by exactly one of the two institutions in that same pre-announcement quarter. The clean-control sample excludes control observations for stocks that were already treated in any prior merger event. Specifically, once a stock first appears in the treated group, it is no longer allowed to serve as a control in later merger-event windows. We estimate the event-study version of the stacked DiD specification by replacing the interaction *Treat* × *Post* with a full set of interactions between *Treat* and indicators for event time from 12 months before to 12 months after the merger announcement. The Month before treatment (t = -1) is omitted as the reference period. The horizontal axis reports months relative to the merger announcement, and the vertical axis reports the estimated treatment effect on log lending fees. The dots represent the coefficient estimates, and the dashed vertical line marks the merger announcement month. All regressions include stock fixed effects, merger-by-month fixed effects, merger-by-stock fixed effects, and industry-by-month fixed effects. Standard errors are clustered at the merger level. The intervals around the dots represent 95% confidence intervals.

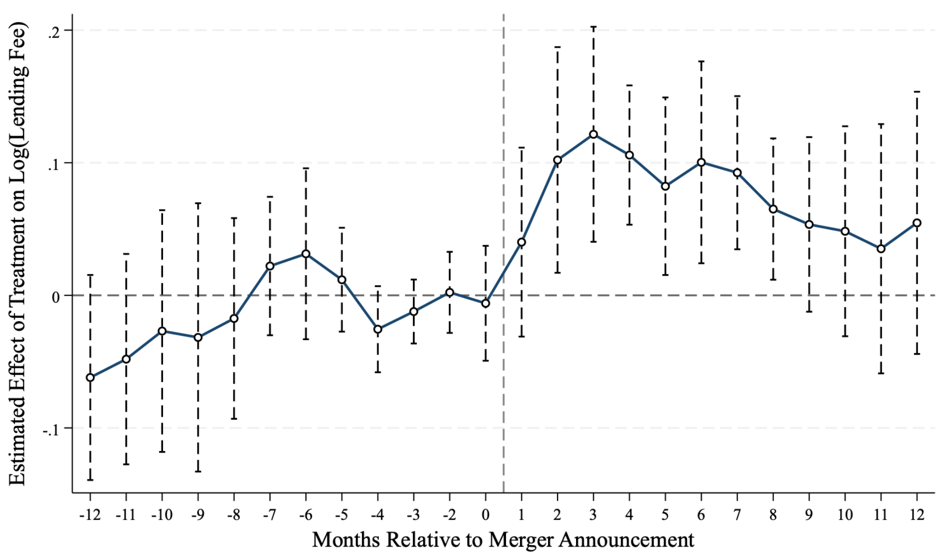
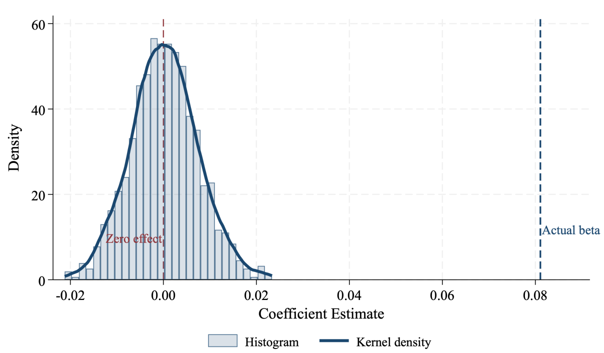
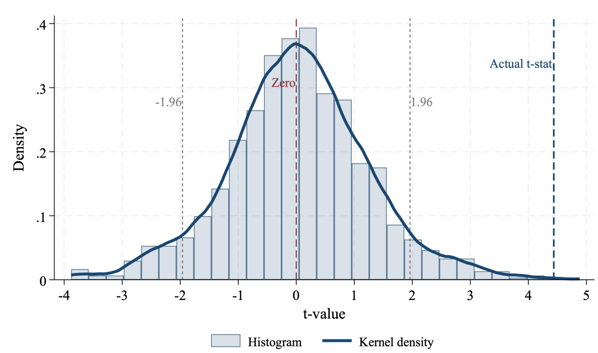


Figure 2: Placebo Tests

This figure reports placebo tests based on the clean-control sample from the stacked difference-in-differences design. In each placebo repetition, treatment status is randomly reassigned within each merger event while preserving the exact number of treated stocks observed in the actual data for that event. We then re-estimate the baseline stacked DiD specification using the placebo treatment indicator in place of the actual treatment indicator. Panel (a) shows the distribution of placebo coefficient estimates, and Panel (b) shows the distribution of placebo t-statistics, both based on 1,000 random assignments. In each panel, the navy dashed vertical line marks the actual estimate from the baseline regression, while the red dashed vertical line marks zero. In Panel (b), the additional gray dashed vertical lines at $t = \pm 1.96$ indicate conventional 5% significance thresholds. The dependent variable is the log of lending fees. All regressions include stock fixed effects, merger-by-month fixed effects, merger-by-stock fixed effects, and industry-by-month fixed effects. Standard errors are clustered at the merger level.



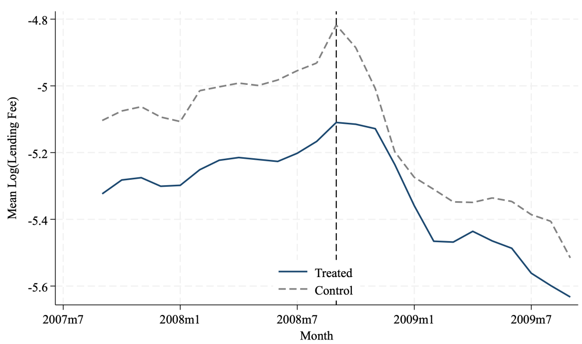
(a) Distribution of placebo coefficient estimates



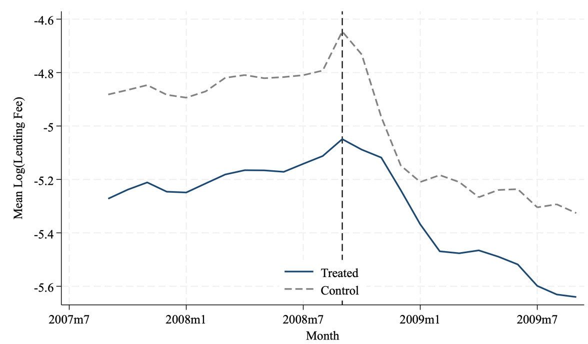
(b) Distribution of placebo t-statistics

Figure 3: Mean Lending Fees around Three Major Crisis-Era Mergers

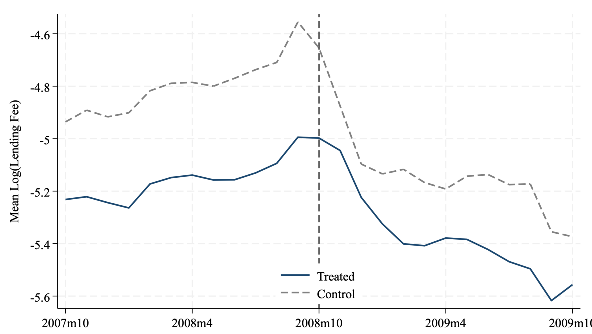
This figure plots the evolution of mean stock-level lending fees for treated and control stocks around three major crisis-era financial institution mergers. Panel A corresponds to Bank of America's acquisition of Merrill Lynch & Co. (announced September 2008); Panel B corresponds to Barclays PLC's acquisition of the investment banking business of Lehman Brothers (announced September 2008); and Panel C corresponds to Wells Fargo & Co.'s acquisition of Wachovia Corporation (announced October 2008). For each merger, treated stocks are those held simultaneously by both the acquirer and the target in the quarter prior to the merger announcement, while control stocks are those held by exactly one of the two institutions in that same pre-announcement quarter. The solid line denotes the treated group, and the dashed line denotes the control group. The vertical dashed line marks the merger announcement month. The horizontal axis reports calendar month, and the vertical axis reports mean of log(Lending Fee).



Panel A: Bank of America–Merrill Lynch



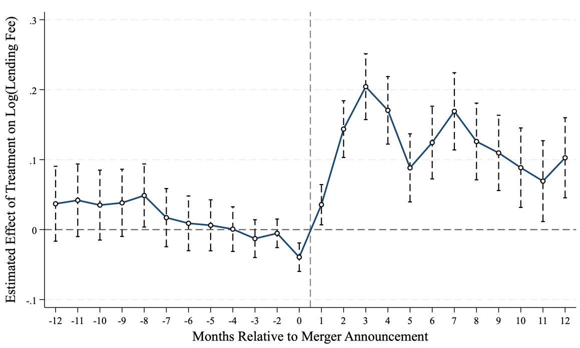
Panel B: Barclays–Lehman Investment Banking Business



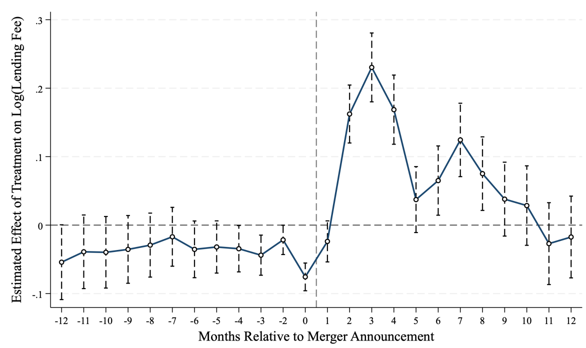
Panel C: Wells Fargo–Wachovia

Figure 4: Dynamic Effects of Three Major Crisis-Era Mergers on Lending Fees

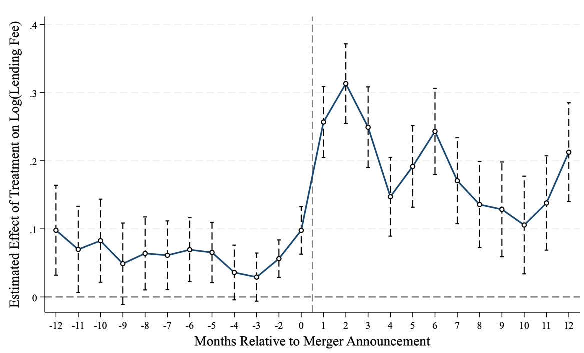
This figure plots event-study estimates of the effect of three major crisis-era financial institution mergers on stock-level lending fees. The dependent variable is the log of lending fees. Panel A corresponds to Bank of America's acquisition of Merrill Lynch & Co. (announced September 2008); Panel B corresponds to Barclays PLC's acquisition of the investment banking business of Lehman Brothers (announced September 2008); and Panel C corresponds to Wells Fargo & Co.'s acquisition of Wachovia Corporation (announced October 2008). For each merger, treated stocks are those held simultaneously by both the acquirer and the target in the quarter prior to the merger announcement, while control stocks are those held by exactly one of the two institutions in that same pre-announcement quarter. For each case, we estimate the event-study version of the difference-in-differences specification by replacing *Treat* × *Post* with a full set of interactions between *Treat* and indicators for event time from 12 months before to 12 months after the merger announcement. The Month before treatment (t = -1) is omitted as the reference period. The horizontal axis reports months relative to the merger announcement, and the vertical axis reports the estimated treatment effect on log lending fees. The dots represent the coefficient estimates, and the dashed vertical line marks the merger announcement month. All regressions include firm fixed effects and industry-by-month fixed effects. Standard errors are clustered at the CUSIP level. The intervals around the dots represent 95% confidence intervals.



Panel A: Bank of America–Merrill Lynch



Panel B: Barclays–Lehman Investment Banking Business



Panel C: Wells Fargo–Wachovia

Appendix

“Lending Fees and Ownership Structure”

Table A1: Variable Definitions

Variable	Definition	Source
Key variables		
Log(Lending Fee)	Monthly average of the log of daily indicative lending fees. The lending fee represents the expected cost of borrowing a stock on a given day. It is an indicative market rate constructed using borrowing costs between agent lenders, prime brokers, and hedge funds. This rate should be interpreted as an estimate of the standard market borrowing cost, not necessarily the exact rate quoted or charged by a prime broker.	Markit
Treat	Indicator equal to one if a stock is held by both the target and acquiring institutions in the quarter before the merger announcement, and zero if it is held by only one of the two institutions.	Thomson Reuters 13F s34
Post	Indicator equal to one for months after the merger announcement within each merger event window.	SDC M&A Database
Securities Lending Market Variables		
Utilization	The value of assets on loan divided by total lendable value.	Markit
Lending Supply	Lendable quantity divided by shares outstanding, expressed in percentage terms. Lendable quantity represents the stock inventory available for lending.	Markit and CRSP
Firm Characteristics		
Size	Firm market capitalization measured two months before the merger announcement.	CRSP
Volatility	365-day historical stock return volatility measured two months before the merger announcement.	OptionMetrics
Turnover	Monthly trading volume divided by shares outstanding.	CRSP
Amihud Illiquidity	Monthly average of the ratio of absolute daily stock return to daily dollar trading volume, following Amihud [2002].	CRSP
Option Illiquidity	Volume-weighted average normalized bid-ask spread across option contracts.	OptionMetrics

Continued

Variable	Definition	Source
Asset Pricing Variables		
FF3 Abnormal Return	Monthly abnormal return estimated using the Fama-French three-factor model. Factor loadings are estimated using a 36-month rolling window ending two months before the merger announcement and are held fixed throughout the event period.	
CAR [-6,-1]	Cumulative FF3 abnormal return from six months before to one month before the merger announcement.	
CAR [0,+6]	Cumulative FF3 abnormal return from the merger announcement month to six months after the announcement.	CRSP and Kenneth French Data Library
CAR [-12,-1]	Cumulative FF3 abnormal return from twelve months before to one month before the merger announcement.	
CAR [0,+12]	Cumulative FF3 abnormal return from the merger announcement month to twelve months after the announcement.	

Table A2: Sample of Financial Institution Mergers

This table reports the financial institution mergers included in our main sample that are matched to 13F filers. Transactions are drawn from SDC's *Mergers and Acquisitions* database and are restricted to deals announced between 2007 and 2024. We retain transactions classified as mergers or acquisitions of assets, with deal values above $1 billion and at least one U.S.-based party. Because SDC does not provide reliable identifiers for a direct merge to 13F data, merger participants are first matched using a text-based procedure and then manually verified using the LSEG *s34* master file, allowing links to parent entities when the parent is the relevant 13F filer.

No.	Date	Target	Acquiror	Type	$mil
1	Nov 2020	Aperio Group LLC	BlackRock Inc	Acq. Assets	1,050
2	Jan 2012	Morgan Keegan & Co Inc	Raymond James Financial Inc	Acq. Assets	1,180
3	May 2023	Putnam Investments LLC	Franklin Resources Inc	Merger	1,299
4	May 2010	GLG Partners Inc	Man Group PLC	Merger	1,544
5	Sep 2008	Lehman-Invest Bkg Bus	Barclays PLC	Acq. Assets	1,750
6	Aug 2015	National Penn Bancshares, PA	BB&T Corp	Merger	1,823
7	Aug 2021	NN Invest Partners Hldg NV	Goldman Sachs Group Inc	Acq. Assets	1,991
8	Oct 2020	CIT Group Inc	First Citizens Bancshares Inc	Merger	2,205
9	Nov 2014	Susquehanna Bancshares Inc	BB&T Corp	Merger	2,492
10	Jun 2021	First Midw Bancorp, Chicago, IL	Old National Bancorp	Merger	2,495
11	Apr 2021	Cadence Bancorp	BancorpSouth Bank	Merger	2,816
12	Apr 2018	Financial Engines Inc	Hellman & Friedman LLC	Merger	2,896
13	May 2023	Angelo Gordon & Co LP	TPG Operating Group II LP	Merger	3,100
14	Jan 2016	FirstMerit Corp	Huntington Bancshares Inc	Merger	3,338
15	Jun 2011	RBC Bank (USA), Raleigh, NC	The PNC Financial Services Gro	Merger	3,470
16	Oct 2016	Janus Capital Group Inc	Henderson Group PLC	Merger	3,782
17	Oct 2021	Oak Hill Advisors LP	T Rowe Price Group Inc	Merger	4,200
18	May 2018	MB Financial Inc	Fifth Third Bancorp	Merger	4,600
19	Dec 2020	TCF Financial Corp	Huntington Bancshares Inc	Merger	5,921
20	Apr 2014	Nuveen Investments Inc	TIAA-CREF I&I Services LLC	Acq. Assets	6,250
21	Feb 2020	Legg Mason Inc	Franklin Resources Inc	Merger	6,592
22	Oct 2020	Eaton Vance Corp	Morgan Stanley	Merger	6,859
23	Sep 2021	MUFG Union Bank NA	US Bancorp	Merger	7,971
24	Jun 2011	ING Direct USA	Capital One Financial Corp	Acq. Assets	8,876
25	Feb 2020	E*TRADE Financial Corp	Morgan Stanley	Merger	13,137
26	Jun 2009	Barclays Global Investors Ltd	BlackRock Inc	Merger	13,345
27	Oct 2008	Wachovia Corp, Charlotte, NC	Wells Fargo & Co	Merger	15,113
28	Dec 2021	Bank of the West Inc	BMO Harris Bank NA	Merger	16,300
29	Apr 2007	ABN AMRO North America Holding	Bank of America Corp	Acq. Assets	21,000
30	Feb 2019	SunTrust Banks Inc	BB&T Corp	Merger	28,282
31	Nov 2019	TD Ameritrade Holding Corp	Charles Schwab Corp	Merger	28,305
32	Sep 2008	Merrill Lynch & Co Inc	Bank of America Corp	Merger	48,766

Table A3: DiD Test of the Effect of Institution Mergers on Lending Fees

This table reports stacked difference-in-differences estimates of the effect of financial institution mergers on stock-level lending fees. The dependent variable is the log of lending fees. For each merger event, treated stocks are those held simultaneously by both the target and the acquirer in the quarter prior to the merger announcement, while control stocks are those held by exactly one of the two institutions in that same pre-announcement quarter. The sample is constructed by stacking merger-specific event windows spanning the 12 months before and the 12 months after each merger announcement. *Post* equals one for months after the merger announcement within each event window. Columns (1) and (2) use the full stacked sample, while columns (3) and (4) use the clean-control sample, which excludes control observations for stocks that were already treated in any prior merger event. Specifically, once a stock first appears in the treated group, it is no longer allowed to serve as a control in later merger-event windows. Columns (1) and (3) include firm fixed effects and industry-by-calendar-month fixed effects. Columns (2) and (4) additionally include merger-by-month fixed effects and merger-by-firm fixed effects. t-statistics are reported in parentheses. Standard errors are clustered at the CUSIP level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable: Log(Lending Fee)			
	All		Clean Control	
	(1)	(2)	(3)	(4)
Treat × Post	0.051***	0.059***	0.081***	0.081***
	(10.27)	(9.67)	(11.39)	(9.60)
Firm FE	Y	Y	Y	Y
Industry×Month FE	Y	Y	Y	Y
Merger×Month FE	N	Y	N	Y
Merger×Firm FE	N	Y	N	Y
Observations	1,345,778	1,345,644	791,235	791,130
Adjusted R^2	0.734	0.819	0.747	0.803