Breaching the Chinese Wall: Cross-Market Information Flow Following Financial Institution Mergers*

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Abstract

How does access to private loan information affect institutional investors' equity returns? Exploiting mergers between asset managers and lenders as a plausibly exogenous shock to loan-side information access, we find that after gaining access institutional investors earn higher abnormal equity returns: within institutions, managers earn 3.1 pp higher annualized returns on stocks for which they have loan-side access than on their other holdings without such access, and across institutions trading the same stock, informed managers outperform equity-only peers by 1.7 pp annualized, consistent with debt positions conferring information unavailable to pure shareholders. Mechanism tests show larger gains when financial-reporting covenants bind; where such covenants exist pre-merger, returns rise by roughly 2 pp. Firms with dual holders adjust: public voluntary disclosures decline by about 11%, new facilities are 11–17% more likely to include information-intensive covenants, and loan pricing improves. Public markets respond with wider bid–ask spreads. By comparing holdings with loan-side access to managers' other positions and to uninformed holders of the same stock, the design isolates causal loan-to-equity information flow, linking mergers between equity- and lending-side entities to trading rents and to shifts in the public information environment.

Keywords: Information; Mergers; Dual-ownership; Covenants; Abnormal returns; Disclosure

JEL Codes: G14, G21, G23, G34, D82, M41

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1. Introduction

In recent years, syndicated loans comprise a significant portion of corporate lending, with new issuances estimated to be more than double the total bond issuance.[1] Syndicated loans are typically originated and overseen by a lead (or "arranger") bank, while funding is provided by a syndicate of lenders. Notably, nonbank investors play an especially large role in the U.S. syndicated loan market, so that institutional investors such as CLOs, hedge funds, mutual funds, pension funds, and insurers are often prominent syndicate participants.[2] Interestingly, a substantial share of syndicated loans feature at least one syndicate member who simultaneously holds equity in the borrower (a "simultaneous owner"); large-sample evidence documents that more than half of syndicated loans have at least one such syndicate member.[3]

The presence of institutional funds in the syndicated loan market enables banks to diversify risk, enhances liquidity in the loan market, and ultimately benefits borrowers by facilitating easier and more cost-effective access to credit.[4] However, loans also involve a substantial exchange of information between borrowers and lenders, encompassing financial statements, covenant-compliance reporting and amendments/waivers, forward-looking projections, and acquisition plans.[5] Hence, the simultaneous holding of loans and publicly traded securities raises concerns regarding the potential use of private information disclosed by the borrower.

Prior work documents that investors who hold both debt and equity often outperform other equity holders (Ivashina and Sun, 2011, Massoud et al., 2011), but they take the borrowers' information environment as exogenous. Also, it remains unclear whether these gains reflect true loan-to-equity information flow or simply selection by especially skilled investors into dual holding. We ask the causal question: when access to loan-side, non-public information turns on for a given equity position, do equity returns improve relative to credible counterfactuals? To answer this, we exploit plausibly exogenous onsets of access and implement two complementary difference-in-differences

[1] See Avalos et al. (2025), LSTA and S&P Global Market Intelligence, LCD (2025), Fitch Ratings (2025) for scale: U.S. syndicated loans par outstanding is $1.6T, while 2024 gross issuance was $1.34T—the bulk from repricings/refinancings, not net new supply.

[2] See Irani et al. (2021): nonbank lenders have become central providers of U.S. corporate credit, particularly when capital regulation binds, implying a substantial institutional investor presence in syndicated lending.

[3] See Jiang et al. (2010).

[4] See Nadauld and Weisbach (2012), Benmelech et al. (2012), Loutskina (2011), Loutskina and Strahan (2009) for discussion.

[5] See Sufi (2007), Roberts and Sufi (2009a), Nini et al. (2012) for discussion.

comparisons that separate information access from investor selection, then trace the mechanism and market responses.

First, we exploit mergers between asset managers and lending institutions as plausibly exogenous shocks to simultaneous debt–equity ownership and to internal information sharing. When a lender and an equity arm come under the same parent at legal closing, dual holdings in affected borrowers rise for reasons orthogonal to the borrower's contemporaneous information environment. Using a difference-in-differences design around the closing date, we show that institutions with newly enabled loan-side access earn higher abnormal equity returns relative to matched controls, consistent with cross-market information flow that turns on at legal closing. We implement two complementary comparisons: (i) within-manager, across firms (newly dual-held positions versus the same manager's other holdings), and (ii) across managers, same firm (the informed manager versus uninformed managers holding the same stock).

Second, we test whether loan covenants[6] that commit borrowers to report accounting-related private information are a proximate mechanism behind outperformance in public markets. Borrower private-information covenants aid lenders in three ways: they deliver timelier data (via higher frequency or shorter lags), they improve monitoring precision (by specifying the content and format of reports), and they add a forward-looking dimension (by requiring projections that inform covenant compliance before breaches occur). We therefore hypothesize that simultaneous debt-and-equity owners exploit the private signals generated on the loan side to achieve abnormal equity returns once organizational integration permits internal sharing.

We focus on two concrete covenant types that generate distinct information flows: *projected financial statements* and *monthly financial statements*. Projected statements help lenders anticipate future covenant compliance and assess managerial credibility by comparing ex ante plans to realized paths; monthly statements compress the information lag, allowing faster detection of performance shifts and more responsive valuation of collateral and borrowing bases. By measuring the presence and bindingness of these provisions, we can relate variation in loan-side information production to variation in equity-side gains after access turns on.

[6] Loan covenants require periodic private reporting (information covenants) and impose ratio tests/restrictions (maintenance/incurrence). Reports go to the agent bank and syndicate; post-merger, lender monitoring yields non-public assessments that, subject to information barriers, can inform affiliated investors. See Nini et al. (2009, 2012), Chava and Roberts (2008).

Third, we examine how the public information environment shifts in ways that interact with private signals. Because reporting choices are endogenous to governance,[7] dual owners can influence firms' disclosure behavior. Such reductions can follow from changed incentives—dual owners may exert less pressure for public guidance because they already receive private reports—or from forces unrelated to ownership. We show that when voluntary public disclosure declines after integration, the value of loan-side information rises and dual holders gain a relative edge: they pair privileged access with a tighter public information set faced by peers. In this lower-transparency regime, private signals translate more readily into trading gains, and abnormal returns are higher for dual holders.

We present a stylized two-signal framework, à la Grossman and Stiglitz (1980), Kyle (1985) for trading with private information, and Milgrom (1981), Grossman (1981), Dye (1985) for voluntary disclosure with frictions, that delivers sharp comparative statics and testable predictions. In this setup, once a merger occurs the equity arm observes a loan-side private signal in addition to a public signal; the private signal's precision rises with covenant-driven monitoring, its effective transmission is scaled by an internal-sharing parameter and dampened by compliance frictions, and the trading edge is larger when voluntary public disclosure is weaker because the private–public precision gap widens. Guided by these predictions, our empirical tests quantify how institutional investors draw on loan-market private information and trace the implications for abnormal equity returns and for firms' information environments. We study how performance varies with access (newly dual-held versus not; informed versus uninformed holders of the same stock), with the presence and strength of private-information covenants (projected and monthly statements), and with shifts in voluntary public disclosure. This design characterizes cross-sectional heterogeneity in the use of private information across firms and contracts, and connects trading outcomes to measurable features of the borrower–lender channel that map directly to the model's primitives (monitoring precision, internal sharing, compliance frictions, and public-signal strength).

To test our hypotheses, we assemble a firm–institution panel that links syndicated lending, institutional equity holdings, fundamentals, and news. Loan contracts and lender roles come from Refinitiv LPC Dealscan; institutional equity positions come from Refinitiv 13F Financial Owner-

[7] See Leuz et al. (2003): insiders adjust disclosure and earnings management to protect private control benefits

ship; accounting data are from Compustat and prices/returns from CRSP. We identify financial-institution mergers in SDC and time-stamp firm news from S&P Capital IQ Key Developments. Borrowers are matched to public identifiers via the standard Dealscan–Compustat/CRSP link, allowing us to follow the same name across the loan and equity sides.

The enactment of the Financial Services Modernization Act of 1999 (Gramm–Leach–Bliley), which removed restrictions on affiliations between commercial and investment banks, opened the door to large-scale consolidation between lending and asset-management arms. We use these debt–equity mergers as plausibly external shifts in dual ownership and in the feasibility of internal information sharing. Conditional on rich fixed effects and controls, the timing of legal closing is orthogonal to any given portfolio name's contemporaneous information environment, which lets us move from suggestive correlations to causal statements about loan-to-equity information flow. This approach also directly mitigates the endogeneity concerns that pervade prior work and allows us to estimate the causal impact of simultaneous owners on investment outcomes.

Our empirical strategy applies two complementary difference-in-differences designs. First (within-institution, across firms), we compare the same manager's positions that become dual-held at legal closing to that manager's other holdings over the same window. Second (across institutions, same firm), we compare informed managers in a treated stock to uninformed managers simultaneously trading that exact stock. In both designs we absorb firm$\times$merger and calendar effects and use two-way clustered standard errors. To rule out generic "merger noise," we implement a placebo using equity–equity mergers and find no effects; the patterns arise only when a lending arm and an equity manager are brought under one roof.

Our findings provide evidence that institutional investors leverage private information from their debt positions when trading equities. First, within institutions, managers earn 3.1 percentage points (annualized) higher abnormal returns when trading the equity of firms for which they have loan-side access compared with their other holdings without such access. Moreover, across institutions trading the same stock, investors with loan-side access outperform equity-only peers by 1.7 percentage points (annualized), a smaller effect consistent with competition for the same information, price impact from informed trading, and potential information leakage to other investors. Together, these results indicate that private debt information is actively exploited in equity trading.

Second, consistent with an information channel, the abnormal-return gains are concentrated where private signals are richer and public information is thinner. Among borrowers that contractually supply monthly and projected financial statements to lenders, simultaneous owners earn roughly 2.1 and 2.6 percentage point higher annualized abnormal returns, respectively, than in otherwise similar firms without such reporting. The effect is stronger for non-investment-grade (high-yield) firms (a "BB+" rating or lower from S&P), where opacity is greater and debt values are more sensitive to information, indicating that loan-desk signals are especially valuable when external disclosure is relatively uninformative. Taken together, the covenant-based richness of private data and the amplified impact in high-yield settings point to sharper, timelier private signals improving the equity desk's effective precision rather than generic merger or risk effects; this interpretation is further supported by the fact that effects begin after legal closing, and by placebo tests with equity–equity mergers.

Third, we find that investors actively reshape the information environment. Following debt–equity integration, treated firms reduce voluntary news disclosures by 10.8%, indicating a disclosure-policy shift induced by dual ownership rather than coincidence. The contraction in public news is more pronounced when institutional ownership is high, consistent with greater investor influence and internal information substituting for public signals. We find that, relative to matched controls, loans originated post-merger at treated firms are significantly more likely to include information-providing covenants: the likelihood of projected financial-statement covenants rises by 11%, and monthly financial-statement covenants by 17%, compared with pre-merger originations. Taken together, these patterns indicate that simultaneous owners obtain privileged signals via loan covenants and, once such channels are in place, firms optimally rely more on private reporting to lenders and less on public voluntary news. In parallel, institutions with larger stakes earn higher abnormal returns in treated names, aligning incentives with tighter internal information channels.

Complementing these findings, market microstructure and content evidence point to a thinner public information environment after integration. Bid-ask spreads widen by 4.6% for firms with simultaneous owners, consistent with heightened adverse selection. Moreover, institutional investors with access to loan-side information earn about 1.7% higher annualized abnormal returns when trading stocks of firms that reduced voluntary disclosures post-merger relative to those that did not. Finally, a news-content analysis shows that, while the number of articles declines, the sentiment of coverage for treated firms improves by 9.4% relative to controls. Fewer but more favorable public signals alongside wider spreads are consistent with substitution toward private channels, which raises the value of private information and helps rationalize the return gains. Consistent with lenders' focus on downside protection (via covenants and monitoring), loan-desk signals are especially informative about deteriorations, explaining stronger value-improving sales on negative information. Higher sell-side returns combined with lower news volume and higher sentiment suggest that dual owners suppress bad-news disclosures and sell earlier on negative information.

Fourth, on firm financing, we show that loan interest rates fall by about 13% for treated borrowers after closing. A natural mechanism is strategic repricing to retain and deepen relationships where the institution also holds equity, preserving covenant-based access to private information. Consistent with relationship re-wiring, borrowers are approximately 25% more likely to initiate a new primary facility with the merged parent post-merger. We also probe potential spillovers to borrowers linked to the merged institution but not directly treated. Taken together, the evidence indicates that information integration lowers the cost of credit and reconcentrates lending ties toward the merged parent.

Overall, our findings provide robust evidence that institutional investors effectively leverage private information from the loan market to gain abnormal returns and influence firms' disclosure policies. These results have important implications for both investors and regulators, enhancing our understanding of institutional investors' behavior and their impact on financial markets.

Our study contributes to several strands of literature. First, we speak to the long-running debate on conflicts in (universal) banking. The original Glass–Steagall separation reflected worries that private information from lending could leak into capital-market activities; influential work later questioned the necessity of a strict separation (Kroszner and Rajan, 1994, Puri, 1996, Kroszner and Rajan, 1997). After the 2008 crisis, concerns about speculative trading revived, culminating in the Volcker Rule's ban on U.S. banks' proprietary trading. These debates center on whether lending generates material non-public information (MNPI) that can be steered, deliberately or inadvertently, into trading. Speaking directly to this debate, we show that when institutions gain access to loan-side MNPI, equity-side returns rise discretely, clear evidence that private information from lending migrates into equity trading and constitutes a first-order conflict channel in universal banking.

Second, we add to the literature on trading based on private information within financial conglomerates. Mutual funds affiliated with lenders appear to trade more profitably in borrowers' stocks (Massa and Rehman, 2008, Bodnaruk et al., 2009); advisors seem to trade ahead of takeovers (Jegadeesh and Tang, 2010); institutional investors in loan syndicates outperform around major amendments (Ivashina and Sun, 2011); and hedge funds that are loan participants short-sell ahead of loan origination (Massoud et al., 2011). In contrast, Griffin et al. (2012) find little evidence of connected trading ahead of takeovers or earnings news using trade-level data, underscoring identification challenges and the risk that results reflect who self-selects into dual holding. Parallel work documents that firms routinely share private information with banks to support monitoring and contracting (Minnis and Sutherland, 2017), while market-level studies infer information transmission from return patterns and price discovery across credit/equity markets and syndicated-loan networks (Acharya and Johnson, 2007, Bushman et al., 2010, Carrizosa and Ryan, 2017). Complementing these strands with direct supervisory evidence, Haselmann et al. (2022) combine trade-by-trade and credit-registry data to show relationship banks build directional positions in borrower stocks ahead of unscheduled news, consistent with lending relationships informing trading. A persistent concern in this literature is selection: superior investors may self-select into holding both claims, so cross-market effects could reflect who chooses dual holding rather than information transmission. We address this concern by exploiting mergers that integrate equity and lending arms as plausibly exogenous shocks to dual ownership and MNPI access, and show that at legal closing equity returns discretely improve for treated positions, causal evidence of loan-to-equity information flow at scale. The pattern strengthens exactly where financial-reporting covenants bind, consistent with loan monitoring producing tradable signals, and it aligns with regulatory discontinuities, further validating the channel.

Third, we connect to work on the corporate consequences of simultaneous owners: dual holders can mitigate shareholder–creditor frictions, alter payout policies, and shape loan terms (Jiang et al., 2010, Chu, 2018, Chava et al., 2019). We add to this literature by exploiting financial-institution mergers as shocks that expand equity holders' access to loan-side MNPI, allowing us to document a *causal* loan-to-equity information flow. Finally, we relate to the disclosure and information-environment literature. Evidence on how institutional ownership affects disclosure is mixed: Boone and White (2015) find a positive association; Peyravan and Wittenberg-Moerman (2022) show borrowers increase earnings-forecast disclosure to offset institutions' information advantage; whereas Ertimur et al. (2014) document disclosure withholding due to trading incentives; firms also respond to sentiment and market structure (Bergman and Roychowdhury, 2008, Kim et al., 2018). Importantly, Leuz et al. (2003) argue that reporting choices, including earnings management and disclosure, are endogenous to governance, as insiders adjust them to protect private control benefits. Consistent with this view, we show that integrating equity and lending within an owner reshapes firms' information environments: public news declines while covenant-based private reporting rises, and public markets respond by widening bid–ask spreads.

Taken together, the paper makes four contributions. First, it delivers causal evidence of cross-market information flow: when equity holders gain loan-side MNPI through equityholder–lender mergers, equity gains materialize after legal closing. Second, it opens the mechanism: gains are stronger where financial-reporting covenants bind, consistent with loan monitoring producing tradable signals that transmit to equity. Third, it shows that dual holders reshape the information environment in their favor. Treated firms disclose less publicly, obtain cheaper loans, and sign more information-intensive covenants with the parent, consistent with internal precision substituting for public guidance. Fourth, it documents the market's response: bid–ask spreads widen as outside investors protect against adverse selection. By tying a policy-relevant organizational shock to the production, transmission, and pricing of private loan information, the paper moves the literature from suggestive correlation to mechanism-backed, externally validated causality.

The remainder of the paper proceeds as follows. Section 2 develops a simple framework in which a merger enables imperfect internal information sharing between the equity and lending desks and derives testable implications. Section 3 reviews the institutional setting and the operational changes induced by mergers. Section 4 details the data, empirical strategy, and sample construction. Section 5 presents the main results. Section 6 reports heterogeneity analyses and placebo tests. Section 7 concludes.

2. A model of information integration

This section develops a minimal framework in the spirits of Grossman and Stiglitz (1980), and Kyle (1985) for trading with private information, and Milgrom (1981), Grossman (1981) and Dye (1985) for voluntary disclosure with frictions, in which a merger enables imperfect internal information sharing between an equity desk and a lending desk inside the same financial institution. The model delivers sharp comparative statics for returns/price discovery, managerial disclosure, and market liquidity, and it clarifies timing (effects at legal closing, not at announcement) and cross–sectional strength (larger with organizational complementarity; smaller with compliance frictions).

2.1 Environment and signals

A firm has a latent value innovation $v \sim \mathcal{N}(0, \sigma_v^2)$. An equity desk E and a lending desk L each observe noisy private signals:

$$s_E = v + \varepsilon_E, \qquad s_L = v + \varepsilon_L,$$

with $\varepsilon_E \sim \mathcal{N}(0, \sigma_E^2)$, $\varepsilon_L \sim \mathcal{N}(0, \sigma_L^2)$, independent of v and of each other. Let $\tau_E \equiv \sigma_E^{-2}$ and $\tau_L \equiv \sigma_L^{-2}$ denote precisions.

A merger can enable internal sharing of information across desks. Let $\rho \in [0, 1]$ capture the intensity of internal information sharing available to E after legal closing of the merger. Before closing, $\rho = 0$ by organizational separation. We treat ρ as reduced–form and write

$$\rho = \rho_0 + \rho_\omega \cdot \omega - \rho_\chi \cdot \chi \quad \text{post–merger},$$

where $\omega \in [0, 1]$ captures organizational complementarity between desks, and $\chi \in [0, 1]$ captures the stringency of compliance frictions that impede sharing; also $\rho_\omega, \rho_\chi > 0$.

The lending desk can also invest in loan–side private information acquisition after closing the merger (e.g., tighter reporting and monitoring routines). Let $\pi \in [0, 1]$ denote the intensity of such acquisition and let it improve L's precision to

$$\tau_L^{\mathit{eff}} = \tau_L + \kappa_L \pi, \qquad \kappa_L > 0,$$

with $\pi = \pi(\rho, \omega, \chi)$ increasing in ρ and ω and decreasing in χ. This channel is not essential for identification but sharpens comparative statics.

Assumption 1 (Internal aggregation at the equity desk) *Post–merger, the equity desk forms a composite statistic*

$$S = s_E + \rho s_L,$$

and trades based on S. In this setting, S is sufficient and induces an effective equity precision

$$\tau_E^{\text{eff}} = \tau_E + \rho^2 \tau_L^{\text{eff}}.$$

Pre–close, $\rho = 0$.

Observed one–period returns satisfy $R = \theta v + \eta$ with $\theta > 0$ and η mean–zero noise independent of (v, s_E, s_L).

2.2 Price discovery and expected returns

The informativeness of S for v governs both the speed of price discovery and expected trading profitability of strategies aligned with S. Under joint normality, the correlation $\text{Corr}(S, v)$ is strictly increasing in the signal–to–noise ratio of S.

Proposition 1 (Merger–induced improvement in price discovery and returns) *Post–merger, $\tau_E^{\text{eff}} = \tau_E + \rho^2 \tau_L^{\text{eff}}$ increases in both ρ and τ_L^{eff}. Consequently $\text{Corr}(S, v)$ rises, which implies faster price discovery (shorter adjustment half–life) and higher expected returns to strategies that condition on S. In particular,*

$$\frac{\partial}{\partial \rho} \text{Corr}(S, v) > 0, \qquad \frac{\partial}{\partial \tau_L^{\text{eff}}} \text{Corr}(S, v) > 0,$$

so expected return improvements occur at legal closing *of the equity and debt merger (when $\rho > 0$ becomes feasible), not at announcement. They are larger when loan–side private information is richer (higher π and thus higher τ_L^{eff}).*

Proof: See appendix B. ■

2.3 Managerial disclosure as a substitute for internal information

Managers choose a disclosure intensity $d \geq 0$ that raises the precision of public information to $\tau_{\text{pub}}(d) = \bar{\tau} + \phi d$ with $\phi > 0$, at convex cost $C(d) = \frac{c}{2}d^2$, $c > 0$. Market makers and uninfomred investors face an adverse–selection wedge proportional to the internal information advantage $A \equiv \tau_E^{eff} - \tau_{\text{pub}}(d)$, generating a funding/trading loss $G(A)$ with $G'(A) > 0$ and $G''(A) \geq 0$. The firm chooses d to minimize $L(d) \equiv G(\tau_E^{eff} - \tau_{\text{pub}}(d)) + C(d)$.

Proposition 2 (Internal information crowds out public disclosure) *The optimal d^* satisfies $cd^* = \phi\, G'\big(\tau_E^{\text{eff}} - \tau_{pub}(d^*)\big)$, hence*

$$\frac{\partial d^*}{\partial \tau_E^{\text{eff}}} \;=\; -\,\frac{\phi\, G''(\cdot)}{c + \phi^2 G''(\cdot)} \;<\; 0.$$

That is, stronger internal information (higher ρ or higher τ_L^{eff}) reduces optimal public disclosure intensity.

Proof: See appendix B. ■

2.4 Market liquidity and adverse selection

Let the adverse–selection component relevant for trading costs be $\Lambda \equiv \kappa\big(\tau_E^{eff} - \tau_{\text{pub}}(d^*)\big)$ with $\kappa > 0$. In reduced form, a standard liquidity metric (e.g., bid–ask spread or price impact) satisfies

$$\text{LiquidityCost} \;=\; a_0 \;+\; a_1\Lambda \;+\; \xi, \qquad a_1 > 0, \quad \mathbb{E}[\xi] = 0.$$

Proposition 3 (Liquidity deteriorates with internal information advantage) *Post–merger, expected trading costs increase with ρ and with τ_L^{eff}, and decrease with disclosure:*

$$\frac{\partial\, \mathbb{E}[LiquidityCost]}{\partial \rho} \;>\; 0, \qquad \frac{\partial\, \mathbb{E}[LiquidityCost]}{\partial \tau_L^{\text{eff}}} \;>\; 0, \qquad \frac{\partial\, \mathbb{E}[LiquidityCost]}{\partial d^*} \;<\; 0.$$

Proof: See appendix B. ■

2.5 Timing and organizational heterogeneity

Recall $\rho = 0$ pre–close and $\rho = \rho(\omega, \chi)$ post–merger with $\partial\rho/\partial\omega > 0$ and $\partial\rho/\partial\chi < 0$. If the acquirer does not internalize both desks, then sharing is technologically infeasible and $\rho \equiv 0$.

Proposition 4 (Timing and cross–section) *All effects in Propositions 1–3 turn on at legal closing and vanish at announcement under $\rho = 0$. Post–merger, treatment effects on returns/price discovery, disclosure, and liquidity are strictly larger when organizational complementarity ω is higher and strictly smaller when compliance frictions χ are stronger. If either desk is absent in the acquirer, no effect obtains.*

Proof: See appendix B. ∎

Remark 1 (Interpretation) *The merger reallocates information within the conglomerate, raising the effective precision used by the equity desk. This increases return forecastability and accelerates price discovery, reduces the firm's incentive to communicate publicly, and raises adverse selection faced by uninformed public traders.*

2.6 Comparative statics and model predictions

The model yields transparent comparative statics that translate into testable predictions for post–merger outcomes.

Prediction 1 (Timing). Outcomes that are monotone in τ_E^{eff} (expected returns conditional on internal signals, disclosure intensity, and trading costs) jump at legal closing, when internal information sharing starts, and do not change at announcement.

Prediction 2 (Disclosure). Managerial disclosure intensity declines post–merger as internal information rises. The decline is larger when internal sharing is stronger and when loan–side private information is richer.

Prediction 3 (Liquidity). Public–market trading costs increase post–merger due to higher adverse selection arising from the internal information advantage. This increase is mitigated by greater public disclosure.

Prediction 4 (Cross–section). All post–merger effects are amplified when organizational complementarity ω is high and attenuated when compliance frictions χ are strong. If the acquirer does not internalize both desks, there is no effect.

Prediction 5 (Loan–equity linkage). Enhancements in loan–side private information (higher π and thus higher τ_L^{eff}) propagate to equity–market outcomes via internal sharing, further deepening Predictions 2–3.

3. Institutional background: what mergers change operationally

Mergers between non-commercial banks and asset managers align previously separate information workflows across lending and equity desks. The theory in section 2 models this as a post–merger increase in internal sharing intensity ρ, potentially reinforced by richer loan-side information τ_L^{eff}, attenuated by compliance frictions χ, and amplified by organizational complementarity ω.

Pre-close (announcement $\rightarrow$ legal closing), Industry "information barrier" rules keep desks organizationally and legally separate; control-room oversight, restricted lists, and siloed systems limit targeted cross-desk flow, and incentives remain unaligned (see Michaely and Womack (1999) for the economic rationale; Financial Industry Regulatory Authority (FINRA), U.S. Securities and Exchange Commission and Examinations (2012) for regulatory guidance).[8] Post-merger (from legal closing), integration typically proceeds via (i) co-location or formal collaboration protocols; (ii) platform/data unification (deal rooms, monitoring dashboards, borrower pipelines); (iii) harmonized coverage and incentives; and (iv) unified risk oversight, standard post-merger systems-integration milestones that enable filtered use of loan-side signals and raise the equity desk's effective precision.[9]

Compliance governs how information can move internally. Stringent structures (centralized control room, broader blackouts, longer restricted-list days) reduce practical sharing, lowering effective ρ even after legal closing. Our empirical index χ (see section D.4) measures this stringency and predicts smaller post-merger effects when high.

We distinguish two dates: *announcement* (t_A), and *legal closing* (t_C). The model predicts no equity-side move at t_A, and a discrete turn-on at t_C. Empirically, we implement announcement- vs.

[8] Information-barrier requirements are codified by regulators; see Financial Industry Regulatory Authority (FINRA) (Research Analysts and Research Reports) and the SEC Staff's U.S. Securities and Exchange Commission and Examinations (2012) summary on examinations of information barriers. The analyst-conflict rationale motivating such walls is documented by Michaely and Womack (1999).

[9] On post-merger systems and process integration, see Robbins and Stylianou (1999).

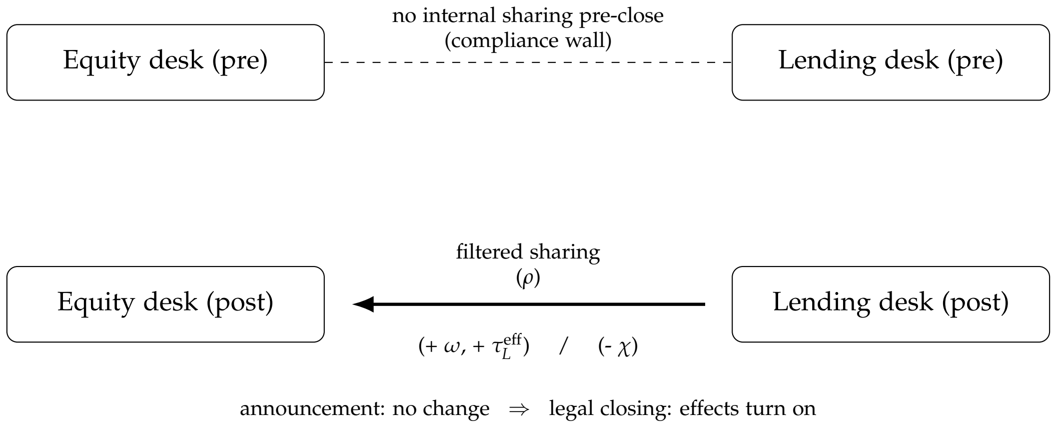

Figure 1. Pre vs. post organization. Before legal closing, compliance walls block cross-desk flow; at closing, filtered internal sharing of intensity ρ activates. Complementarity (ω) and loan-side information (τ_L^{eff}) amplify; compliance frictions (χ) attenuate.

closing-centered event studies (Fig. 9), milestone heterogeneity, and mechanism interactions for ω, χ, and τ_L^{eff} (Table 19).

4. Methodology, Data, and Sample Construction

4.1 Methodology and Data

For our analysis, we combine data on debt holdings from Thomson Reuters' LPC's DealScan database, and equity holdings by institutions sourced from Thomson Reuters' Financial Ownership database. Financial statements and stock price information for firms are obtained from Compustat and CRSP, respectively. Merger data is obtained from the SDC database, and news releases are from Capital IQ's Key Developments database. The latter compiles information on firms' news releases from various public news sources, company press releases, regulatory filings, call transcripts, investor presentations, stock exchanges, regulatory websites, and company websites. We categorize these news into discretionary and nondiscretionary types. Discretionary news is likely under the firm's control, and include conferences, client and product announcements, and special dividends, whereas nondiscretionary news includes events such as earnings announcements or annual general meetings.

After the Financial Services Modernization Act of 1999[10] eliminated restrictions against affilia-

[10] This legislation, signed into law by President Bill Clinton in November 1999, repealed large parts of the Glass-Steagall

tions between commercial and investment banks, there has been a significant increase in Mergers and Acquisitions (M&As) among financial institutions. These mergers can be viewed as an exogenous shock to the presence of simultaneous holders of debt and equity in a firm. We thus base our identification strategy on exploiting of these mergers.

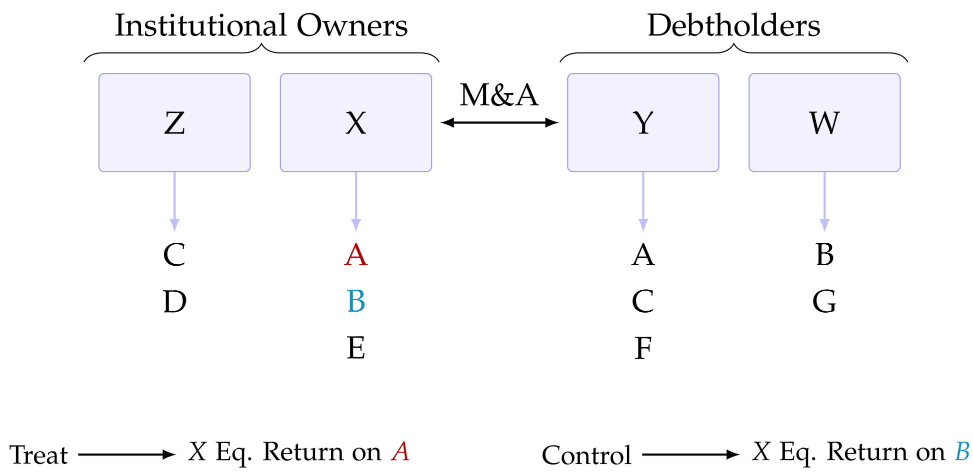

Figure 2. Identification I: In this identification I identify firm A as the treated firm, which is present in both the debt holder's and equity holder's portfolio at the time of the merger. I use other holdings of the merging institutional owner that have outstanding debt at the time of the merger (such as firm B) as control firms.

To derive a causal relationship, we have to examine differences in outcome both within and across institutions. For identification I (depicted in Figure 2) we identify firms that are present in both the debt holder's and equity holder's portfolio following the merger as the treated firms. We use other holdings of the merging institutional owner that have outstanding debt at the time of the merger as the control firms. For identification II (depicted in Figure 3) we instead use the same firm held by other institutions that do not have access to the debt-side information as control firms. This strategy helps us to identify any difference in trading performance of informed institutions trading the same stock compared to those without access to private information.

In order to examine the source of private information and ensure that the abnormal returns observed are not solely driven by the mergers themselves, we employ a third identification strategy that focuses specifically on mergers between two institutional owners. This analysis, referred to as Identification III (equity-only placebo), allows us to disentangle the effects of simultaneous ownership and the merger event. In particular, we want to follow an alternative identification strategy

Act, which had separated commercial and investment banking since 1933. This led to the creation of financial holding companies, over which the Fed was granted new supervisory powers.

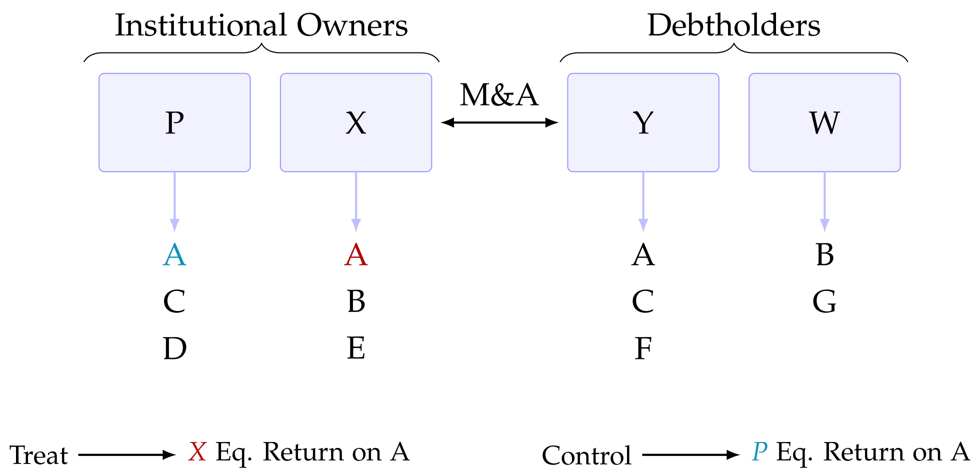

Figure 3. Identification II: In this identification I identify firm A as the treated firm, which is present in both the debt holder's and equity holder's portfolio at the time of the merger. I use the same firm held by other institutions that do not have access to the debt-side information (here, institution P) as control firms. This strategy helps identify the outperformance of institutions trading the same stock relative to those without access to private information.

(depicted in Figure 4) and rule out observing similar effect when two institutional owners merge without the creation of simultaneous ownership.

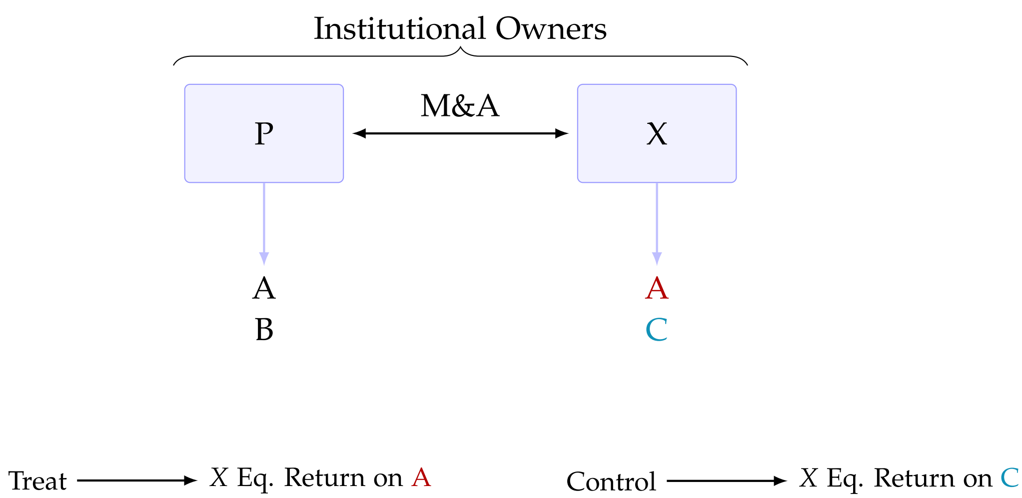

Figure 4. Identification III: In this identification I identify firm A as the treated firm, which is present in both equity holders' portfolios at the time of the merger. I use other holdings of the merging institutional owners at the time of the merger (such as firm C) as control firms.

Using these identification strategies we adopt a difference-in-differences (DID) design to examine changes in the abnormal return and disclosure practices of the treatment firms around the financial institution mergers compared to those of control firms. We use 4 quarters before and 4 quarters after each merger as pre- and post-event periods, respectively. The regression specification is as follows:

$$R^f_{jq} = \alpha + \beta_1 \; Treat^f_j \times Post^f_q + \beta_2 \; Post^f_q + \gamma \; Controls^f_{q-1} + \lambda \; FE^f_{jq} + \epsilon^f_{jq} \tag{1}$$

where $Treat^f_j$ is a dummy variable that equals 1 if firm f gets a simultaneous owner as a result of merger j, and 0 otherwise. $Post^f_q$ is a dummy variable that equals 1 if quarter q is within the first 4 quarters after the merger, and 0 otherwise. $Treat \times Post$ is the independent variable of interest. FE contains $firm \times merger$ and calendar fixed effects. The dependent variable R^f_{jq} varies depending on the identification method used. In Identification I, it is either the trading return as defined below or a measure of disclosure, whereas in Identification II, it is the trading return. Our variable of interest to be estimated is β_1. This variable captures the marginal effect of having access to private information on abnormal return and corporate disclosure. To account for the simultaneous correlation between the same firms affected by different mergers, different firms affected by the same merger, and the potential correlation arising from the mergers occurring in the same year, we cluster standard errors by $firm \times merger$ and $year$.

What the two DiD coefficients identify. Let Y_{ifq} denote the outcome for institution i on firm f in quarter q. We write a minimal post–merger outcome decomposition,

$$Y_{ifq} = \underbrace{\mu_{if}}_{\text{firm}\times\text{merger FE}} + \underbrace{\delta_q}_{\text{calendar FE}} + \underbrace{\kappa_{iq}}_{\text{desk-level shock}} + \underbrace{\eta_i (1-\lambda_{fq})\, s_{fq}\, A_{if}\, \text{Post}_q}_{\text{loan}\to\text{equity info transmission}} + \gamma \; X'_{if,q-1} \; + \; \varepsilon_{ifq}, \tag{2}$$

where: $A_{if} \in \{0, 1\}$ flags that, *for desk i and firm* f, loan-side access is feasible (treated firm) after legal closing; Post_q flags quarters after legal closing; $s_{fq} \geq 0$ is the intensity of loan-side signal for f in q; $\eta_i \geq 0$ is desk i's implementation efficiency; $\lambda_{fq} \in [0, 1]$ captures leakage/competition (higher λ means more dissipation/price impact); κ_{iq} is a desk–time shock common to i's holdings in q; μ_{if} and δ_q are absorbed by fixed effects; $X_{if,q-1}$ are controls. By construction, the transmission term only turns on when both access is present and the post window is reached.

Our estimating equation is

$$Y_{ifq} = \alpha + \beta_1 \left(A_{if} \times \text{Post}_q \right) + X'_{if,q-1}\gamma + \mu_{if} + \delta_q + \varepsilon_{ifq},$$

with two-way clustering. The coefficient β_1 takes different interpretations depending on the comparison set.

In Identification I (within institution, across stocks), treated firms are names f in i's book that also borrow from the merged lender; where controls are i's other contemporaneous holdings. The DiD estimand is

$$\beta_1^{\mathrm{I}} = \Big\{\mathbb{E}[Y_{ifq} \mid A_{if}=1, \mathrm{Post}=1] - \mathbb{E}[Y_{ifq} \mid A_{if}=1, \mathrm{Post}=0]\Big\} \\ - \Big\{\mathbb{E}[Y_{ifq} \mid A_{if}=0, \mathrm{Post}=1] - \mathbb{E}[Y_{ifq} \mid A_{if}=0, \mathrm{Post}=0]\Big\}. \tag{3}$$

Plugging (2) and using that μ_{if} and δ_q are differenced out by FE, and that κ_{iq} is common across $A_{if} = 1$ and $A_{if} = 0$ within i (hence cancels in the difference of differences), we obtain under parallel trends and no pre-treatment access:

$$\beta_1^{\mathrm{I}} = \mathbb{E}\big[\eta_i (1 - \lambda_{fq})\, s_{fq} \;\big|\; A_{if}=1, \mathrm{Post}=1\big] \;\approx\; \overline{\eta} \cdot \mathbb{E}\big[(1 - \lambda_{fq})\, s_{fq} \;\big|\; \text{borrower of merged lender}\big], \tag{4}$$

i.e., the *information-access wedge*: the average gross gain on newly informed names for that desk, free of desk-level style/tempo.

In Identification II (across institutions, same stock), treated subsample are institutions i with loan-info access on firm f; and controls are other institutions $i' \neq i$ holding the *same* firm f without access to loan-side information. The estimand is

$$\beta_1^{\mathrm{II}} = \Big\{\mathbb{E}[Y_{ifq} \mid \text{treated } i,\ f, \mathrm{Post}=1] - \mathbb{E}[Y_{ifq} \mid \text{treated } i,\ f, \mathrm{Post}=0]\Big\} \\ - \Big\{\mathbb{E}[Y_{i'fq} \mid \text{uninformed } i',\ f, \mathrm{Post}=1] - \mathbb{E}[Y_{i'fq} \mid \text{uninformed } i',\ f, \mathrm{Post}=0]\Big\}. \tag{5}$$

With firm×merger FE (μ_{if}) and calendar FE (δ_q), stock-level shocks are absorbed; under parallel trends, no anticipatory trading, and $A_{i'f} = 0$ for peers,

$$\beta_1^{\mathrm{II}} = \mathbb{E}\big[\eta_i (1 - \lambda_{fq})\, s_{fq} \;\big|\; \text{treated } i,\ f, \mathrm{Post}=1\big], \tag{6}$$

i.e., a *relative trading edge on the same name*: access *times* the treated desk's implementation efficiency, naturally attenuated by competition/price impact captured in $(1 - \lambda_{fq})$.

Because Identification I averages across a desk's informed names while purging desk-time shocks, and Identification II compares the informed desk to uninformed peers on the *same* name, their ratio is informative about dissipation and execution:

$$\frac{\beta_1^{\mathrm{II}}}{\beta_1^{\mathrm{I}}} \approx \frac{\eta_{\text{treated}}}{\overline{\eta}} \cdot \frac{\mathbb{E}[(1-\lambda_{fq})\, s_{fq} \mid \text{same } f]}{\mathbb{E}[(1-\lambda_{fq})\, s_{fq} \mid \text{borrower names}]}, \tag{7}$$

so $\beta_1^{\mathrm{I}} > \beta_1^{\mathrm{II}}$ points to meaningful dissipation from competition/price impact (larger λ when racing peers on the same name) or only modest implementation advantages; $\beta_1^{\mathrm{II}} \approx \beta_1^{\mathrm{I}}$ indicates access dominates and/or strong execution.

Assumptions. Both designs rely on (i) parallel trends conditional on μ_{if}, δ_q, and $X_{if,q-1}$; (ii) no anticipatory equity trading using loan MNPI in the pre window; (iii) stable composition/SUTVA at the $i \times f$ unit; and (iv) correct treatment timing at legal closing. Event-time plots in Section 5 corroborate the discrete turn-on at closing and the absence of pre-trends.

A compact microfoundation that maps these estimands to primitives (access, implementation efficiency, and competition/price impact) is in Appendix C.

4.2 Sample Construction

The construction of the sample used for Identification I in Figure 2 involves several steps. Initially, we gather all mergers between financial firms that took place from 1985 to 2021 using the SDC mergers and acquisitions database. To refine the merger sample, we implement additional criteria. First, we exclude mergers where the merging institutions were owned by the same parent firm before the merger. This ensures that the mergers considered are between distinct entities and not simply reorganizations within the same parent company. Additionally, we require the completion of the merger to occur within one year after the initial merger announcement. This criterion ensures that the observed effects are more likely to be driven by the merger itself, rather than subsequent developments that may occur over an extended period.

In the second step of the process, we undertake a matching procedure involving the unique lender names from LPC's DealScan database and the unique institutional owner names from Thomson Reuters' 13F database. The purpose of this step is to refine the sample of desired mergers, focusing only on those that have the potential to alter the simultaneous-ownership status. To accomplish this, we compare the lender names and institutional owner names from both databases with the names of either the acquirers or the targets involved in the financial mergers. We calculate the Levenshtein distance [11] for each comparison, considering both the lender names and institutional owner names separately.

In matching acquirer names, as they are identified as the owner of the merged company, we not only match the names of the lenders and institutional owners directly associated with the merger but also compare the names of the parent companies of the acquirer firm with the names of the lenders and institutional owners. Additionally, whenever possible, we utilize the addresses of the companies in both databases to enhance the precision of the matching process. Subsequently, we retain only the matches that exhibit a Levenshtein score of 0.8 or higher for both the lender names and institutional owner names. This threshold ensures that the matches have a substantial degree of similarity.

Finally, we conduct a manual verification of all the matches to ensure accuracy. This meticulous procedure yields a comprehensive list of the desired mergers that effectively modify the simultaneous-ownership status of at least one firm, meeting the objective of this step. By following these steps, we effectively reduce the sample size to focus on relevant mergers with potential ownership changes.

The subsequent step involves identifying the treated firms. To do this, we first identify all firms that have loans from the merging lenders in place at the time of the merger. These loans should have a maturity period of not less than three years following the merger. We then require that the merging institutional owner holds stocks of the firm at the end of the quarter immediately preceding the merger. Following the methodology outlined by Jiang et al. (2010), we require the lender to have participated in more than 10% of the loan at origination, indicating a significant involvement in the financing arrangement. Additionally, the institutional equity holder must hold more than 1% of all shares outstanding of the firm, representing a substantial ownership stake. Finally, we exclude firms operating in the financial and utility industries from the analysis.

[11] The Levenshtein distance between two words is the minimum number of single-character edits (insertions, deletions or substitutions) required to change one word into the other.

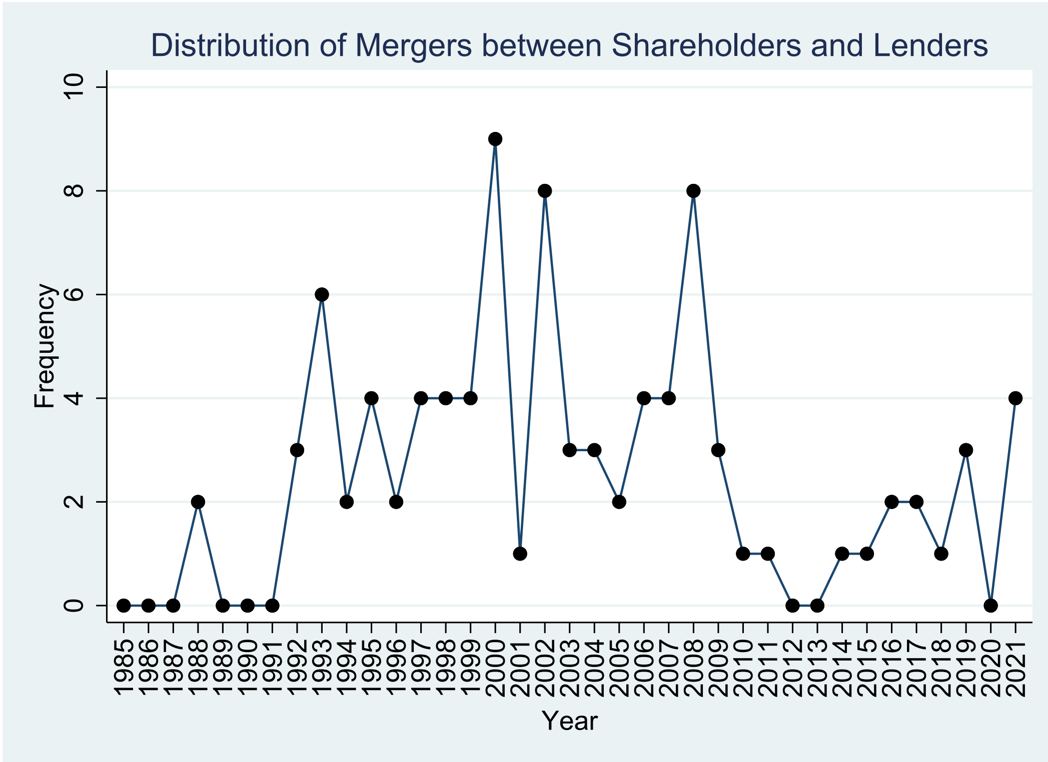

Figure 5. This figure presents the yearly distribution of the mergers used in this paper for Identifications I and II. The mergers are M&A deals between lenders in the DealScan database and institutional owners in the Thomson Reuter's 13F database.

This procedure produces a sample of 196 treated firms involved in 92 mergers. The distribution of these mergers over time is depicted in Figure 5. The year 2000 witnessed the highest number of mergers, following the Gramm–Leach–Bliley Act being enacted in 1999, which eliminated restrictions on affiliations between commercial and investment banks, and lead to a surge of mergers and acquisitions within the financial sector.

Next, to identify control firms, we implemented a multi-step process. Firstly, we compared all lender names associated with a particular firm during a specific fiscal year with all institutional owner names of the same firm during that same fiscal year. This resulting database was then matched with the database containing the 92 mergers of interest. Subsequently, we compared the lender names and institutional owner names with the names of both the acquirers and the targets involved in the financial mergers and calculated the Levenshtein distance for each comparison.

We retained only those matches where either the lender name or the institutional owner name had a Levenshtein score of 0.8 or higher and manually verified all matches. Additionally, we only

Table 1
Summary statistics of variables used in the paper

	Obs.	Mean	Median	SD
DW–BHAR	9,578	0.0077	0.0058	0.0153
Projected financial statement	859	0.42	0.00	0.49
Monthly financial statement	859	0.25	0.00	0.43
Voluntary news	8,672	5.78	4.00	3.13
Sentiment	7,182	0.02	0.01	0.03
Post	9,578	0.52	1.00	0.50
Treat	9,578	0.16	0.00	0.37
BAS	8,896	0.008	0.006	0.060
Size	9,578	7.87	7.81	1.35
Leverage	9,578	0.23	0.21	0.22
B/M	9,578	0.53	0.47	0.72
Volatility	9,578	0.14	0.11	0.09
R&D/Assets	9,578	0.03	0.01	0.07
ROA	9,578	0.10	0.10	0.15
Tangibility	9,578	0.32	0.24	0.26

Notes: Variables are defined in the Appendix A. Statistics are computed at the firm–quarter level. "Obs." reports variable-specific (non-missing) counts; Mean, Median, and SD are calculated over the same non-missing samples.

included control firms that had outstanding bank loans at the time of the mergers. Firms with simultaneous owners during the fiscal years of $[t-1, t+1]$, where t represents the year of the merger, were excluded from the sample. Following this procedure, we obtained a sample of 1,057 control firms for Identification I.

Following previous research, we include various firm characteristics as control variables to account for other factors that may impact returns and firms' disclosure practices, such as firm size, leverage, book-to-market ratio, stock return volatility, R&D over assets ratio, ROA, and tangibility. These control variables are employed across all our analyses. Variable definitions can be found in the Appendix. Furthermore, to mitigate the influence of outliers on our findings, we apply double-winsorization to all variables at the 1% level. Table 1 presents the summary statistics for these variables.

The procedure for constructing the sample needed for Identification II is similar to Identification I, as previously described. However, there is a distinction in how we identify control firms. In order to obtain the control sample required for Identification II, we utilize the same set of 92 mergers and 196 treated firms mentioned earlier. For each unique merger-firm combination, we examine the Thomson Reuters' 13F database to identify other institutional owners who possess at least 1% of all outstanding shares of the same firm during the subsequent merger period. By following this approach, we generate a control sample comprising 667 pairs of institution-firm combinations, which serves as our control group for Identification II.

To disentangle *merger-event confounds* from the *simultaneous-ownership* channel, we examine mergers that combine two equity institutions (no lending arm). This placebo delivers null results; by contrast, the effects in our main tests materialize only when a debtholder and an equity holder are integrated (debt–equity mergers). For constructing the sample for Identification III (equity-equity placebo), we focus on mergers between institutional owners, following the approach outlined in He and Huang (2017). First, we utilize mergers recorded in the SDC mergers and acquisitions database, considering those with announcement dates between 1985 and 2021. The acquirer and target primary SIC codes should fall within the range of 6000 and 6999, and both the target and acquirer firm must have provided firm names. For each target firm and acquirer firm involved in these mergers, we utilize text matching algorithms (the Levenstein method mentioned earlier) to match the firm names provided in the 13F data. We also impose additional requirements: either the target firm discontinues filing 13F statements within one year of the merger's completion date or the target's assets under management decline by over 80% from quarter -4 to quarter +4 relative to the completion quarter. Finally, we manually verify the accuracy of the matches. Through this process, a sample of 274 financial institution mergers is generated, of which 163 meet the specified criteria for selecting treatment firms (as described below). The distribution of these mergers over time is depicted in Figure 6.

Second, we proceed to build both treatment and control samples based on the financial institution mergers, following the approach outlined in He and Huang (2017). To construct the treatment sample, we focus on identifying firms that are likely to undergo cross-ownership as a consequence of the financial institution merger. The selection process involves two steps. In the first step, we identify all firms in which one of the merging partners holds a block of 1% or more in the quarter preceding the announcement of the merger. In the second step, we ensure that ownership data is available in the quarter preceding the effective date of the merger and that the firm is listed on

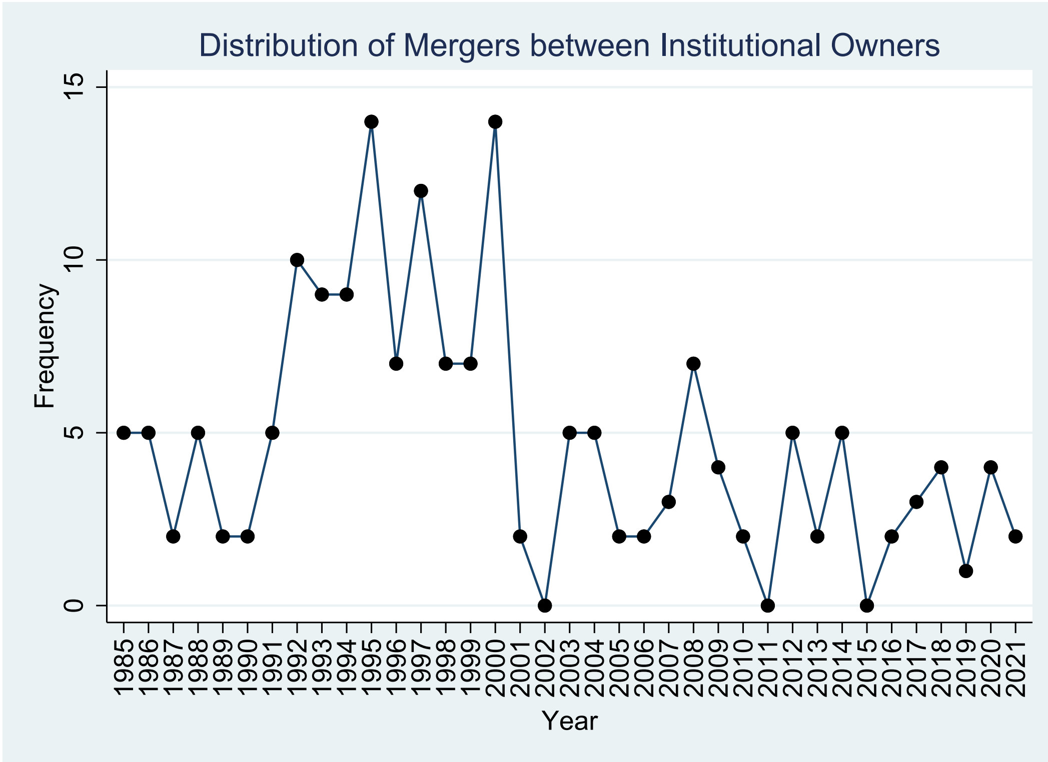

Figure 6. This figure presents the yearly distribution of the mergers used in this paper for Identifications III. The mergers are M&A deals institutional owners in the Thomson Reuter's 13F database.

Compustat during the fiscal year of the merger. As a result, the treatment sample comprises 774 firms that meet these requirements.

For the firm-level control sample corresponding to the 774 treatment firms, we construct a set of control firms. These control firms are selected based on the criterion that they are block-held by one of the merger partners prior to the merger but are not included in the treatment sample. We designate this sample as the Control Firms. To be included in the Control Firms sample, we ensure that ownership data is accessible in the quarter preceding the effective date of the merger, and the firm is listed on Compustat during the fiscal year of the merger. Consequently, the Control Firms sample comprises 2727 firms that fulfill these conditions.

Finally, our empirical methodology for all identification methods necessitates the selection of a testing window. The decision on the appropriate time window involves a trade-off between a longer window that might include unrelated information and a very short window that fails to capture the lag between the mergers and the utilization of loan-side private information. To address this trade-off, we opt for an 8-quarter window in our difference-in-difference specification. This window encompasses four quarters prior to the mergers and four quarters following the mergers. In the final stage of constructing our sample, we match both the treated firms and the control firms with their respective financial information sourced from Compustat and CRSP.

5. Main Results

We test whether institutional investors exploit private loan information to earn abnormal returns and whether firms' disclosure policies adjust in response. To establish a causal relationship, we estimate a difference-in-differences specification following Equation 1.

Following Ivashina and Sun (2011), the abnormal return is measured by the direction-weighted buy-and-hold abnormal returns, where the direction is defined as -1, 0, or 1 based on whether the institutional investor reduces, does not change, or increases their position in a given stock over the quarter. The R^{f}_{jq} represents the buy-and-hold abnormal return of the borrower's stock over the 90 days following the start of each quarter during the one year in the post-merger period, and the abnormal return is computed using the Fama and French (1993) three-factor model.

Table 2 presents the result following this specification. Based on identification I, the estimated DID coefficient is 0.0309 which means that comparing to control firms, institutional investors get about 3.1% higher annualized abnormal return on trading treated firms in the post-merger period. Based on identification II, the estimated DID coefficient is 0.0173 which implies that comparing to other institutional investors, those with access to loan-side information get about 1.7% higher annualized abnormal return when trading a given stock in the post-merger period. Finally, based on identification III, the estimated DID coefficient is found to be non-significant. Consequently, in the absence of private loan-side information, the abnormal return observed post-merger is not significantly different from that before the merger. This rules out selection bias in the stocks held by merging firms as an explanation for the observed effect on returns.

A comprehensive window-and-model sensitivity for the DW–BHAR outcome appears in Appendix Table 29, where we vary factor models, and event windows (60/90/120 days); the effects are stable across specifications. For intensive-margin evidence (trade incidence, trade size, Investment-

Table 2
Private Loan Information and Institutional Abnormal Returns: Difference-in-Differences

	Identification I	**Identification II**	**Equity-Only Placebo**
Treat × Post	0.0309***	0.0173**	0.0087
	(0.010)	(0.009)	(0.008)
Post	0.0102	0.0080	0.0121
	(0.012)	(0.010)	(0.011)
Controls	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
Obs.	9,578	6,384	27,008

This table reports the difference-in-differences results of the impact of having access to loan-market private information on institutional owners' abnormal return. Abnormal return is measured by the direction- weighted buy-and-hold abnormal returns, where D^f_{jq} is defined as -1, 0, or 1 based on whether the institutional investor reduces, does not change, or increases their position in a stock j over the quarter q. R^f_{jq} represents the buy-and-hold abnormal return of the borrower's stock over the 90 days following the start of each quarter during the one year in the post-merger period, and the abnormal return is computed using the Fama and French (1993) three-factor model. Column (1) represents results following Identification I from Fig. 2, column (2) represents results following Identification II from Fig. 3, and column (3) represents results following Identification III from Fig. 4. The standard errors are double clustered at the firm×merger and calendar levels. Statistical significance is indicated by *** and **, representing significance at the 1% and 5% levels, respectively.

Weighted Abnormal Return (bps of AUM), and dollar P&L), see Appendix D.5.

We then explore mechanisms through which lenders acquire private insights about borrowers. Specifically, we investigate whether lenders' need for information motivates them to modify loan covenants in order to obtain more stock-relevant information. In particular, covenants that obligate borrowers to periodically disclose specific accounting-related private information to lenders subsequent to loan origination should be more valuable to simultaneous owners.

Loan agreements typically embed financial covenants and contingencies that, when tripped, shift control rights to lenders, trigger renegotiation, and alter pricing, maturities, or permissible actions.[12] Covenants also specify ongoing reporting that facilitates lender monitoring; in particular, "borrower private information" covenants require nonpublic, periodic deliverables (e.g., monthly or projected financial statements) designed to enhance monitoring precision.[13]

Borrower private-information covenants provide incremental value in at least three ways. First,

[12] See Roberts and Sufi (2009b) for pervasive renegotiation and large contract changes; Chava and Roberts (2008) and Nini et al. (2012) on control rights and policy impacts following covenant violations; and the classic taxonomy in Jr. and Warner (1979). Related evidence on renegotiation practice is summarized in Demiroglu and James (2010).

[13] On the design and monitoring role of reporting covenants, see Carrizosa and Ryan (2017).

timeliness: required deliverables arrive more frequently or with shorter lags (e.g., monthly statements), enabling prompt monitoring. Second, scope/precision: mandated items target metrics most informative for covenant maintenance. Third, forward-looking content: projected statements provide information about expected compliance and future performance.[14]

Within the affirmative covenant section of most private loan contracts is a dedicated set of *borrower reporting covenants*. Following the literature, we classify as *borrower private information* those reporting-covenant items that satisfy three conditions: (i) they help assess realized performance or *forecast* the borrower's financial position; (ii) they are *non-public* upon delivery to lenders; and (iii) they are provided at *predetermined, recurring* intervals (timely and standardized for monitoring) (Dichev and Skinner, 2002, Christensen and Nikolaev, 2012).

To conduct our analysis, we manually collect a sample of original loan contracts submitted as exhibits within public borrowers' Form 10K/Q and 8K filings. These contracts typically contain a section under affirmative covenants outlining borrower financial reporting requirements. Following Carrizosa and Ryan (2017), we identify two types of borrower private information specified in these covenants: (1) future period projected financial statements (hereafter referred to as 'projected financial statements') and (2) historical financial statements provided more frequently than quarterly (usually monthly) and not yet publicly available (hereafter referred to as 'monthly financial statements'). Almost half of the loan contracts in our sample incorporate either one or both of these covenants, with projected financial statements being mandated nearly twice as frequently as monthly financial statements.

Forward-looking projected financial statements assist lenders in foreseeing borrowers' future adherence to financial covenants. Moreover, they enable lenders to assess borrowers' management by allowing a comparison between management's financial projections and the actual performance thereafter. Additionally, regularly supplied monthly financial statements expedite lenders' assessments of alterations in borrowers' accounting performance and the valuation of assets used as collateral or part of borrowing bases.

We use the indicator variables 'projected financial statements' and 'monthly financial statements' to represent covenants mandating borrowers to furnish lenders with either projected or monthly fi-

[14] See Carrizosa and Ryan (2017) for evidence on monthly and projected financials as part of borrower reporting packages and their use in lender monitoring.

nancial statements, respectively. Each variable takes the value of one for loan contracts incorporating the respective type of borrower private information covenant and zero otherwise.

Given the relatively modest size of our sample encompassing desired mergers and their associated loan contracts, we employ a manual verification method, following Nini et al. (2009), to accurately identify borrower private information covenants within each loan contract. Our examination focuses solely on the original loan contracts (initial agreements within lending arrangements) omitting subsequent contract amendments. This approach aligns with our aim of understanding the influence of borrower private information covenants on lenders' initial lending decisions and monitoring practices.

We constructed our sample using 1209 original loan contracts initiated within our specified period. Subsequently, we manually gathered all accessible original loan contracts from SEC EDGAR initiated during this timeframe and matched them with DealScan. Among these contracts, we identified borrower private information covenants in 859 instances. Out of the total 859 loan contracts examined, 53% encompass at least one type of borrower private information covenant. Notably, 'projected financial statements' feature more prominently, appearing in 42% of contracts, as opposed to 'monthly financial statements,' which appear in 25% of the contracts.

To examine our hypothesis that institutional owners realize higher abnormal returns in trading firms providing them with private information through covenants, we outline the following DDD specification:

$$\begin{aligned} R_{jq}^{f} = \alpha + \beta_1\ Treat_j^f \times Post_q^f \times PI_j^f + \beta_2\ Treat_j^f \times Post_q^f + \beta_3\ Post_q^f \times PI_j^f + \\ \beta_4\ Post_q^f + \gamma\ Controls_{q-1}^f + \lambda\ FE_{jq}^f + \epsilon_{jq}^f \end{aligned} \tag{8}$$

where all variables are as defined in Equation 1. The additional dummy variable PI_j^f could be either the indicator variables 'projected financial statements' or the indicator variables 'monthly financial statements'. It takes a value of one if firm f involved in merger j provides the institutional owner with projected financial statements or monthly financial statements, respectively. It is important to note that, in this specification, the inclusion of $firm \times merger$ fixed effects renders $Treat_j^f$, PI_j^f, and $Treat_j^f \times PI_j^f$ unidentifiable. Our main coefficient of interest to be estimated is β_1, the difference in realized abnormal returns between institutional owners trading treated firms that have provided

Table 3
Triple-Differences: Private Loan Information (via Covenants) and Institutional Abnormal Returns

	Monthly Financial Statements		Projected Financial Statements	
	Identification I	**Identification II**	**Identification I**	**Identification II**
Treat × Post × PI	0.0209**	0.0185**	0.0261***	0.0210**
	(0.010)	(0.008)	(0.011)	(0.009)
Treat × Post	0.0162*	0.0148	0.0191*	0.0179*
	(0.009)	(0.010)	(0.011)	(0.010)
Post × PI	0.0089	0.0084	0.0107	0.0103*
	(0.006)	(0.005)	(0.007)	(0.006)
Post	0.0082	0.0075	0.0090	0.0081
	(0.009)	(0.009)	(0.010)	(0.010)
Controls	Yes	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes	Yes
Obs.	6,872	6,411	6,755	6,257

Notes: This table reports the DDD results of the impact of having access to loan market private information among firms who have access to private information through covenants on institutional owners' abnormal return following specification outlined in Equation 8. Abnormal return is measured by the direction- weighted buy-and-hold abnormal returns, where D^f_{jq} is defined as -1, 0, or 1 based on whether the institutional investor reduces, does not change, or increases their position in a stock j over the quarter q. R^f_{jq} represents the buy-and-hold abnormal return of the borrower's stock over the 90 days following the start of each quarter during the one year in the post-merger period, and the abnormal return is computed using the Fama and French (1993) three-factor model. The left panel presents the results when monthly financial statements is used as a measure of private information. Column (1) represents results following Identification I from fig.2, and column (2) represents results following Identification II from fig.3. The right panel presents the results when projected financial statements is used as a measure of private information. Column (3) represents results following Identification I from fig.2, and column (4) represents results following Identification II from fig.3. The standard errors are double clustered at the $firm \times merger$ and $calendar$ levels. Statistical significance is indicated by ***, **, and *, representing significance at the 1%, 5%, and 10% levels, respectively.

private information to their owners and those that have not.

Table 3 displays the outcomes from estimation of the DDD specification in Equation 8. As indicated in the left panel, the estimated DDD coefficient stands at 0.0209 for Identification I and 0.0185 for Identification II, both showing significance at the 5% level. This suggests that institutional investors attain approximately 2.1% and 1.8% higher annualized abnormal returns, respectively, when engaging in trading treated firms that furnish monthly financial statements to their lenders during the post-merger period, compared to treated firms that do not provide such statements.

Moreover, as shown in the right panel, the estimated DDD coefficient is 0.0261 for Identification I and 0.0210 for Identification II, demonstrating significance at the 1% and 5% levels, respectively.

This indicates that institutional investors achieve approximately 2.6% and 2.1% higher annualized abnormal returns, respectively, when trading treated firms that supply projected financial statements to their lenders during the post-merger period, compared to treated firms that do not furnish such statements.

These findings underscore a main mechanism through which institutional owners achieve higher abnormal returns when trading firms to which they simultaneously extend loans. The advantage that simultaneous owners possess in accessing private information via covenants, gives them a significant edge in trading equity in these firms.

The results presented are consistent with Carrizosa and Ryan (2017): simultaneous owners possess access to private information through covenants. Similar to other lenders, simultaneous owners gain access to private information during loan origination, periodic reporting, and renegotiation phases. Increased private disclosures bolster their informational edge and five them a trading advantage over other market participants.

Simultaneous owners wield the power and may also try to influence firms' private information disclosures due to their involvement in initiating and renegotiating loan contracts. Consequently, we anticipate that borrowers with simultaneous owners will offer more private disclosures than those without.

To test this hypothesis, we gauge firms' private disclosures to their lenders by analyzing covenants linked to the private information we previously utilized in our analysis (i.e., measured through projected financial statements and monthly financial statements). Our hypothesize that the likelihood of including such covenants in the new loan agreements rises after a lender becomes a simultaneous owner of a firm.

To pursue this, we adopt a new identification strategy outlined in Figure 7. Our focus is on detecting changes in treatment firms' private information disclosures due to the presence of simultaneous owners. Therefore, we only retain treatment firms with loans involving merging lenders. As illustrated in Figure 7, we employ a facility-based identification approach, where treated firms are those that have acquired a minimum of two separate loans around the date of the targeted mergers: one loan initiated with one of the merging parties pre-merger, and the other with the merged entity post-merger.

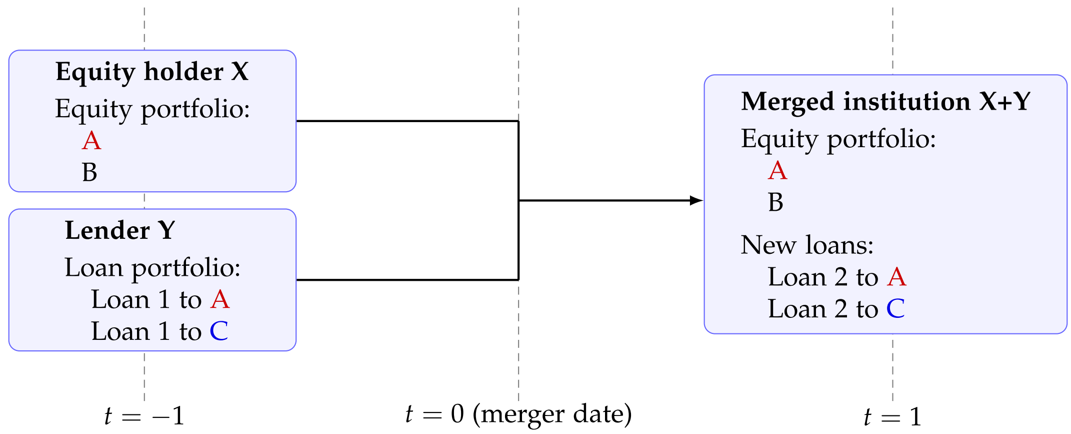

Figure 7. Identification IV: In this identification process, I label Firm A as the treated firm, having secured loans both before and after the merger from the merging lender and the subsequent merging party, and also being owned by the same entity. Conversely, Firm C, meeting similar lending conditions but not under the ownership of the subsequent merging party, is considered the control firm.

Our initial step involves matching all loan contracts associated with the treatment firms, resulting in 94 loans issued to these treated firms. For the control group, we mandate that these firms possess at least one loan both before and after the merger and discard any control loans that lack corresponding treatment counterparts. This process yields 214 control loans.

Table 4 presents the regression outcomes, with the left panel focusing on the projected financial statement variable and the right panel centered on the monthly historical statement variable. We have controlled for borrower firm characteristics, proxies indicating borrower-lender information asymmetry, and various loan contract terms. As outlined in the table, our findings reveal a positive and statistically significant coefficient on our main variable across both panels and for both the OLS and DiD models. This implies that, compared to control firms, treatment firms' loan agreements originated in the post-merger periods are notably more inclined to include covenants requiring disclosure of private information compared to those originated before the mergers.

The observed effect is economically significant: compared to control firms' loan contracts, those of treatment firms exhibit an 11% greater likelihood of containing covenants mandating projected financial statements and a 17% higher likelihood of including covenants requiring monthly financial statements during the post-merger periods compared to pre-merger periods. These results strongly suggest that simultaneous owners obtain access to privileged information by compelling borrowers to augment their private disclosures to lenders.

Table 4
Dual Holders and Private Disclosures: OLS vs. DiD (Facility-Level)

	Projected Financial Statements		Monthly Financial Statements	
	OLS	**DiD**	**OLS**	**DiD**
Post	0.116^{***}	0.0885	0.179^{***}	0.0508
	(0.034)	(0.059)	(0.040)	(0.062)
Treat × Post		0.107^{***}		0.166^{***}
		(0.021)		(0.024)
Controls	Yes	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes	Yes
Obs.	94	308	98	322

Notes: This table illustrates the outcomes regarding the impact of institutional dual-holders on private disclosures by firms. The presence of covenants derived from projected financial statements is represented in the left panel, while covenants based on monthly financial statements are depicted in the right panel. The analysis is conducted at the level of individual loan facilities. The first column in each panel reports OLS results; the second reports DiD results. Statistical significance is indicated by ***, **, and *, representing significance at the 1%, 5%, and 10% levels, respectively.

Utilizing identification IV outlined in Figure 7, we also examine whether there is a change in the interest rates of loans following the merger. One possible reason for this could be that financial institutions lower interest rates to entice firms in which they also hold share borrow from them. This strategic move would be motivated by the desire to leverage the private information embedded in the loan covenants.

Table 5 displays the results. In comparison to control firms, the loan agreements of treatment firms originating in the post-merger period exhibit a notable decrease in interest rates. The OLS and DiD specifications reveal a reduction of 13% and 11%, respectively. These results suggest that financial institutions may indeed adjust interest rates post-merger as a strategic incentive to attract firms in which they own equity as lenders to capitalize on the private information derived from loan covenants.

To further elucidate the flow of information, we delve into additional channel that may potentially amplify the exploitation of private information, namely changes in voluntary public disclosures. Specifically, we begin by testing whether the firm disclosure policy changes with simultaneous owners by running a difference-in-differences specification following Equation 1. Table 6 displays the results using Identification I to examine the relationship between simultaneous owner-

Table 5
Dual Holders and Loan Interest Rates: OLS vs. DiD (Facility-Level)

	OLS	**DiD**
Post	-0.13^{***}	-0.06
	(0.04)	(0.05)
Treat×Post		-0.11^{***}
		(0.04)
Controls	Yes	Yes
Calendar FE	Yes	Yes
Firm × Merger FE	Yes	Yes
Obs.	94	308

Notes: This table illustrates the outcomes regarding the impact of institutional dual-holders on the interest rates of the loans. The analysis is conducted at the level of individual loan facilities. First column represents the OLS results, and the second column represents the DiD results. Statistical significance is indicated by ***, **, and *, representing significance at the 1%, 5%, and 10% levels, respectively.

Table 6
Simultaneous Owners and Voluntary Disclosures (Identification I)

	log(News)
Treat × Post	-0.108^{***}
	(0.035)
Post	-0.026
	(0.047)
Controls	Yes
Calendar FE	Yes
Firm × Merger FE	Yes
Obs.	8,672

Notes: This table presents the results of Identification I from Fig. 2, analyzing the impact of simultaneous owners on firms' voluntary disclosures. Voluntary disclosures are measured using the natural logarithm of the total number of news releases by the firm, as obtained from the Key Development database. The standard errors are double clustered at the $firm \times merger$ and *calendar* levels. Statistical significance is denoted by *** and **, indicating significance at the 1% and 5% levels, respectively.

ship and voluntary disclosures. Voluntary disclosures are measured as the natural logarithm of the total number of news releases issued by the firm, according to the Key Development database.

As reported in Table 6, the estimated DID coefficient is -0.108, which implies that treated firms disclose 10.8% less voluntary news in the post-merger period compared to control firms. This aligns with our hypothesis suggesting that institutional owners alleviate their pressure on firms to disclose, given their possession of private information from the loan side

Thus far, our research has established that institutional owners who gain access to private loan-side information during mergers are able to generate greater abnormal returns when trading the treated in the post-merger period. Additionally, we have observed a statistically significant decrease in the number of voluntary disclosures made by the treated firms engaged in mergers. Given these findings, it is natural to explore the potential connection between the higher abnormal returns and the reduction in disclosure quantity. To examine our hypothesis that institutional owners realize higher abnormal returns when trading firms that experience a decrease in disclosure, we run the following DDD specification:

$$\begin{aligned} R^f_{jq} = \alpha + \beta_1\ Treat^f_j \times Post^f_q \times RD^f_j + \beta_2\ Treat^f_j \times Post^f_q + \beta_3\ Post^f_q \times RD^f_j + \\ \beta_4\ Post^f_q + \gamma\ Controls^f_{q-1} + \lambda\ FE^f_{jq} + \epsilon^f_{jq} \end{aligned} \tag{9}$$

where variables are as defined in Equation 1. The new dummy variable RD^f_j takes a value of one if firm f involved in merger j significantly reduces its number of voluntary disclosures during the post-merger period. In this specification, $firm \times merger$ absorbs the effects of $Treat^f_j$, RD^f_j, and $Treat^f_j \times RD^f_j$. Our main coefficient of interest is β_1, which captures the discrepancy in realized abnormal returns between institutional owners trading treated firms that have reduced their disclosure following the mergers and those that have not.

Table 7 presents the results obtained from the DDD specification outlined in Equation 9. According to the within-institution identification strategy (Identification I), the estimated DDD coefficient is 0.0242 and significant at 1% level. This implies that institutional investors achieve approximately 2.4% higher annualized abnormal returns when trading treated firms that have reduced their disclosure in the post-merger period, as compared to treated firms that have not reduced their disclosure.

Furthermore, based on the across-institution identification strategy (Identification II), the estimated DDD coefficient is 0.0178 and significant at 5% level. This indicates that, compared to institutional investors without having access to private information from the loan side, institutional

Table 7
Reduced Disclosure (RD) × Private Loan Information: Triple-Differences on Abnormal Returns

	Identification I	**Identification II**
Treat × Post × RD	0.0242***	0.0178**
	(0.010)	(0.009)
Treat × Post	0.0201**	0.0264***
	(0.011)	(0.010)
Post × RD	0.0114	0.0171*
	(0.011)	(0.011)
Post	0.0083	0.0079
	(0.010)	(0.011)
Controls	Yes	Yes
Calendar FE	Yes	Yes
Firm × Merger FE	Yes	Yes
Obs.	8,672	5,784

Notes: This table reports the DDD results of the impact of having access to loan market private information among firms who have reduced their disclosure on institutional owners' abnormal return following specification outlined in Equation 9. Abnormal return is measured by the direction- weighted buy-and-hold abnormal returns, where D^f_{jq} is defined as -1, 0, or 1 based on whether the institutional investor reduces, does not change, or increases their position in a stock j over the quarter q. R^f_{jq} represents the buy-and-hold abnormal return of the borrower's stock over the 90 days following the start of each quarter during the one year in the post-merger period, and the abnormal return is computed using the Fama and French (1993) three-factor model. Column (1) represents results following Identification I from fig.2, column (2) represents results following Identification II from fig.3. The standard errors are double clustered at the $firm \times merger$ and $calendar$ levels. Statistical significance is indicated by ***, **, and *, representing significance at the 1%, 5%, and 10% levels, respectively. Outcome: $D^f_{jq} \times R^f_{jq}$.

investors with access to such information obtain around 1.8% higher annualized abnormal returns when trading firms that have reduced their disclosure, compared to trading firms that have not reduced their disclosure in the post-merger period.

These findings suggest that access to private loan-side information is more valuable for trading stocks of firms that decrease their public disclosures. The informational advantage of simultaneous owners is thus twofold: not only do such institutional investors have access to loan-side covenant-based private information pertaining to the treated firms, but their counterparts trading the same stocks are simultaneously less informed due to the reduction in public disclosures.

Finally, we examine the impact of mergers on the overall quality of the information environment for treated firms. As observed previously, treated firms tend to reduce their public disclosures following mergers involving their institutional owners and loan-holders. As a result, it is anticipated that treated firms will experience a decrease in the quality of their information environment compared to firms without such institutional arrangements. Specifically, the presence of private loan-side information should increase adverse selection and decrease liquidity.

To examine this prediction, we utilize the bid-ask spread as an indicator of the quality of a firm's information environment. Following the market microstructure literature, we examine whether treated stocks, in comparison to control firms, experience a deterioration in liquidity as measured by bid-ask spread. To test this hypothesis, we employ the difference-in-differences (DiD) specification outlined in Equation 1, with the bid-ask spread as our dependent variable. We calculate the bid-ask spread (BAS) as:

$$BAS^{f}_{jq} = ln(\frac{1}{N_q}\sum_{i=1}^{N_q}\frac{100 \times (Ask^{f}_{ji} - Bid^{f}_{ji})}{Ask^{f}_{ji} + Bid^{f}_{ji}})$$

where BAS^{f}_{jq} is the average of the scaled daily bid-ask spread of firm f involved in merger j over N_q trading days of quarter q.

Table 8 presents the results of this analysis. In line with our hypothesis, we observe a significantly positive coefficient on $Treat \times Post$ for the variable BAS^{f}_{jq}. This implies that treated firms experience an approximate 4% greater increase in bid-ask spread during the post-merger period, relative to control firms. This is consistent with treated firms information environment quality deterioration in the post-merger period, leading to higher information asymmetry and lower liquidity. Additional liquidity triangulation using Amihud illiquidity and realized volatility appears in Appendix Table 31; the spread result is robust and the alternative proxies point in the same direction.

In addition to examining the decrease in the number of news items following simultaneous ownership, we also want to investigate whether the sentiment of news articles changes. To accomplish this, we conduct a sentiment analysis of the news articles related to the treated firms, comparing the post-merger sentiment with that of control firms. By analyzing the sentiment of news articles, we can gain insights into the potential shifts in perception and tone surrounding the treated firms

Table 8
Liquidity and the Information Environment (Identification I)

	Log(1+BAS^{f}_{jq})
Treat $\times$ *Post*	0.046^{***}
	(0.016)
Post	-0.017
	(0.012)
Controls	Yes
Calendar FE	Yes
Firm $\times$ Merger FE	Yes
Obs.	8,896

Notes: This table presents the results of the difference-in-differences analysis, examining the impact of mergers on the quality of the information environment of firms. The analysis follows the specifications outlined in Equation 1. In this specification, the liquidity of firms serves as a proxy for the information environment quality and is defined as:

$$BAS^{f}_{jq} = \ln\left(\frac{1}{N_q}\sum_{i=1}^{N_q}\frac{100\times(Ask^{f}_{ji} - Bid^{f}_{ji})}{Ask^{f}_{ji} + Bid^{f}_{ji}}\right)$$

where BAS^{f}_{jq} is the average of the scaled daily bid-ask spread of firm f involved in merger j through N_q working days of quarter q. This table represents results following Identification I from fig.2. The standard errors are double clustered at the $firm \times merger$ and $calendar$ levels. Statistical significance is indicated by ***, **, and *, representing significance at the 1%, 5%, and 10% levels, respectively.

following the merger event. This analysis will contribute to our understanding of the overall impact of mergers on the sentiment of news coverage, providing valuable information about the changes in public perception that may accompany such corporate events.

In order to determine the sentiment of a news item, we use Loughran and McDonald (2011) sentiment lexicon. They find that almost three-fourths of negative word counts in 10-K filings based on the Harvard dictionary are typically not negative in a financial context. By examining all words that occur in at least 5% of the SEC's 10-K universe, they create a list of words that typically have a negative meaning in financial reports. Their negative word list is significantly related to announcement returns. This well-known lexicon contains 2355 words labeled as Negative and 354 words labeled as Positive. Finally, the sentiment for a news item is defined as the normalized

Table 9
News Volume and Sentiment

	Log(1+news)	Sentiment
Treat × *Post*	-0.108^{**}	0.094^{***}
	(0.051)	(0.027)
Post	-0.026	0.013
	(0.045)	(0.023)
Controls	Yes	Yes
Calendar FE	Yes	Yes
Firm × Merger FE	Yes	Yes
Obs.	8,672	7,182

Notes: This table shows the results for sentiment analysis. In column 1, we include the findings obtained from Table 6, while column 2 represents the outcomes of the sentiment analysis. In order to determine the sentiment of a news item, we use Loughran and McDonald (2011) sentiment lexicon. The sentiment for a news item is defined as the normalized difference between the number of positive and negative news in the item:

$$\text{Sentiment} = \frac{\#\ \text{Positive news} - \#\ \text{Negative news}}{\#\ \text{Positive news} + \#\ \text{Negative news}}.$$

Statistical significance is indicated by ***, **, and *, representing significance at the 1%, 5%, and 10% levels, respectively.

difference between the number of positive and negative news in the item:

$$Sentiment = \frac{\#\ \text{Positive news} - \#\ \text{Negative news}}{\#\ \text{Positive news} + \#\ \text{Negative news}}$$

Table 9 presents the results of our sentiment analysis. In column 1, we include the findings obtained from Table 6, while column 2 represents the outcomes of the sentiment analysis. The results indicate a statistically significant increase in the sentiment of news items by 0.094 for treated firms following the mergers. Therefore, it can be inferred that although the number of news items decreases post-merger, the sentiment of the news demonstrates a significant improvement. This suggests that the reduction in news coverage can be attributed to a decline in negative news, which, in turn, contributes to the observed increase in sentiment. This finding aligns with our previous observation that a significant portion of the abnormal return is achieved through selling stocks post-merger.

Our findings demonstrate that institutional investors, who become simultaneous debt and equity holders through mergers, not only gain but also strategically use private information to achieve significant abnormal returns. This advantage is further amplified in situations where firms reduce their voluntary disclosures post-merger, enhancing the value of private information for these investors. Additionally, our exploration into changes in firms' loan agreements and interest rates post-merger provides evidence of strategic behavior by financial institutions to capitalize on this information asymmetry. These insights not only shed light on the dynamics of information flow in equity markets but also raise important issues about the regulatory and ethical dimensions of simultaneous ownership of debt and equity. As we transition to the additional analysis and robustness checks, we aim to further validate these findings and explore the broader implications of our study for market participants and policy-makers.

5.1 Dynamic effects and identification: event-time ATT around legal closing

To address dynamic treatment timing and cohort heterogeneity, we replace the two-way FE DiD in Eq. (1) with a modern staggered design and report event-time average treatment effects around the legal closing date ($\tau = 0$). Specifically, we estimate

$$Y_{it} \;=\; \sum_{\ell \neq -1} \beta_\ell \, \mathbb{1}\{\text{EventTime}_{it} = \ell\} \cdot \mathbb{1}\{i \text{ ever treated}\} \;+\; \alpha_i \;+\; \lambda_t \;+\; \varepsilon_{it}, \tag{10}$$

where α_i and λ_t are unit and calendar fixed effects, respectively, and we omit $\ell = -1$ as the reference period. Coefficients β_ℓ are estimated using the Sun–Abraham group-time estimator (Sun and Abraham, 2021) with unit and calendar fixed effects. We cluster standard errors by *firm*×*merger* and *calendar quarter*.

Figure 8 plots the resulting event-time ATTs for our three primary outcomes: (i) returns discovery, (ii) disclosure, and (iii) liquidity. The pre-trends are flat, and effects turn on discretely after $\tau = 0$ (legal closing), in line with the model's timing prediction. Appendix F details the estimator and inference.

Table 2 (DiD) remains as a summary of the average post-merger effect; the event-time analysis in Figure 8 replaces/augments Table 3 by documenting flat pre-trends and dynamic post-merger

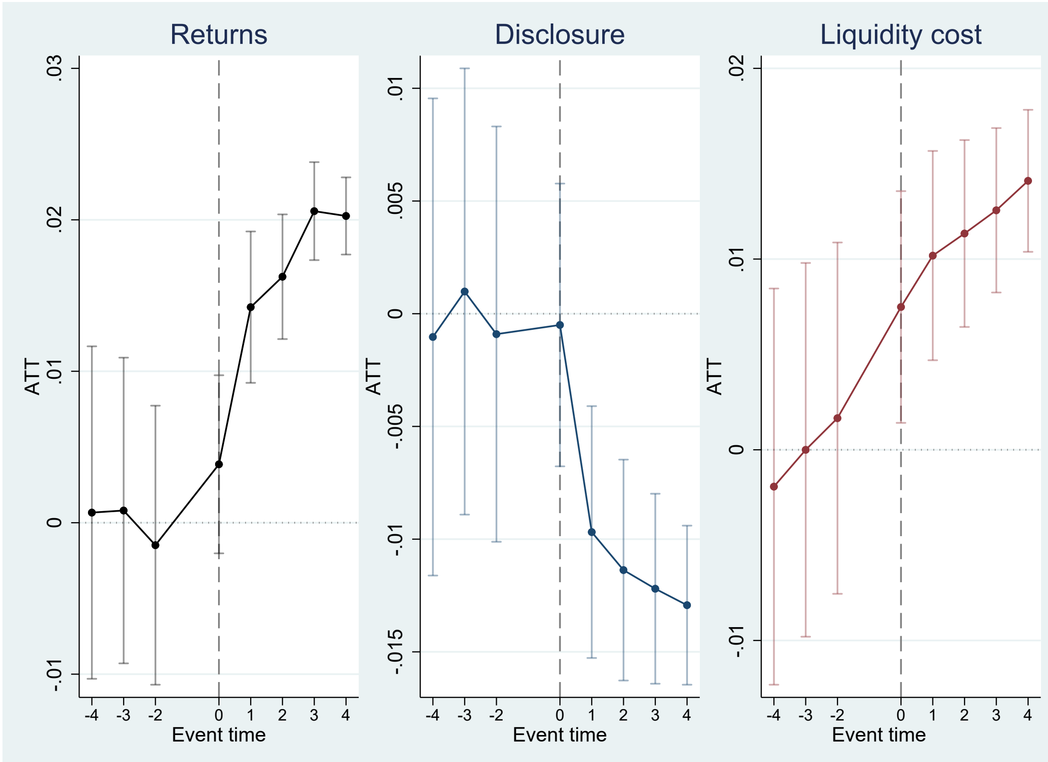

Figure 8. Event-time ATTs around legal closing ($\tau = 0$): returns, disclosure, and liquidity. Coefficients β_ℓ from Eq. (10) estimated with Sun–Abraham group-time ATTs and unit and calendar FEs. Bands show 95% confidence intervals with two-way clustering (firm×merger, calendar). Returns: direction-conditioned abnormal returns; Disclosure: log(1+voluntary disclosures); Liquidity: log(1+spread) or price impact. Pre-trends are flat; effects turn on at legal closing.

adjustment.

5.2 Announcement vs. closing: timing contrast

A key timing prediction of the framework is that effects turn on at legal closing, when internal sharing becomes technologically feasible, and not at announcement date. To test this, we re-center the modern staggered event-study on the announcement date (placebo) and contrast it with the same specification centered on the closing date (treatment). Formally, for each outcome Y_{it} we estimate

$$Y_{it} = \sum_{\ell \neq -1} \beta_\ell^A \, \mathbb{1}\{\text{EventTime}_{it}^{\text{Ann}} = \ell\} \cdot \mathbb{1}\{i \text{ ever-treated}\} + \alpha_i + \lambda_t + \varepsilon_{it}, \tag{11}$$

$$Y_{it} = \sum_{\ell \neq -1} \beta_\ell^C \, \mathbb{1}\{\text{EventTime}_{it}^{\text{Close}} = \ell\} \cdot \mathbb{1}\{i \text{ ever-treated}\} + \alpha_i + \lambda_t + \varepsilon_{it}, \tag{12}$$

Figure 9. Announcement vs. closing: event-time ATTs for returns/price discovery. Notes: Coefficients β_ℓ^A and β_ℓ^C from Eqs. (11)–(12) estimated via Sun–Abraham group-time ATTs with unit and calendar fixed effects and two-way clustering (firm×merger, calendar). Shaded bands are 95% CIs. Pre-trends are flat; effects turn on only at legal closing ($\tau = 0$).

where α_i and λ_t are unit and calendar fixed effects, the reference bin is $\ell = -1$, and $\{\beta_\ell^A\}$ and $\{\beta_\ell^C\}$ are estimated using Sun–Abraham group-time ATTs with two-way clustering (firm×merger, calendar).[15]

Figure 9 plots the event-time ATTs for the return/price-discovery outcome with $\tau \in [-4, +4]$ quarters. The announcement-centered path is statistically flat before and after $\tau = 0$, while the closing-centered path exhibits a discrete jump and sustained post-merger effects. Table 10 summarizes the same contrast using binned averages of the event-time coefficients. These findings reinforce the identification that the effects originate at legal closing (organizational feasibility), not at announcement (information already public), and complement the bank-acquirer placebo (Table 14) and the news-only placebo (Table 15).

[15] Estimator details are in Appendix F. As robustness, we replicate the dynamics with Callaway–Sant'Anna and obtain similar paths.

Table 10
Announcement vs. closing: binned event-time ATTs for returns/price discovery

	Announcement-centered $\overline{\beta_\ell^A}$	Closing-centered $\overline{\beta_\ell^C}$
Leads: $-4 \le \ell \le -2$	-0.0003	-0.0008
	(0.0052)	(0.0052)
Impact: $\ell = 0$	0.0000	0.0002
	(0.0032)	(0.0036)
Lags: $1 \le \ell \le 4$	0.0003	0.0128^{***}
	(0.0023)	(0.0021)
Unit FE; Calendar FE	Yes	Yes
Estimator	Sun–Abraham	Sun–Abraham
Clustering	firm×merger, calendar	firm×merger, calendar
Obs.	9,578	9,578

Notes: This table compares announcement-centered and closing-centered event-time average treatment effects on the treated (ATTs) for abnormal returns and price discovery around mergers between asset managers and lenders. "Leads" report the average coefficients for the four to two quarters before the event ($-4 \le \ell \le -2$), "Impact" shows the contemporaneous effect ($\ell = 0$), and "Lags" average the one- to four-quarter post-event effects ($1 \le \ell \le 4$). Each specification includes unit and calendar fixed effects and is estimated using the Sun and Abraham (2021) heterogeneous-effects difference-in-differences estimator. Standard errors are two-way clustered by firm×merger and calendar quarter. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

5.3 Design threats and falsifications

A first concern is whether treated and control units follow comparable pre-trends. We address this by estimating modern staggered difference-in-differences with group-time average treatment effects (Sun and Abraham (2021); Callaway and Sant'Anna (2021)) and plotting event-time dynamics around the legal closing date. The pre-close leads are jointly flat, and the post-merger coefficients display the expected turn-on pattern for returns, disclosure, and liquidity. The paired announcement- vs. closing-centered figure (Fig. 9) makes the contrast transparent, and methodological details are documented in Appendix F. These dynamic estimates replace/augment the baseline DiD table (Table 2).

A related worry is anticipation of debt-side information at announcement. If information leaked at t_A, we would observe a price-discovery move when the event window is re-centered at announcement. We implement a placebo event study with $\tau = 0$ at t_A and a matched study with $\tau = 0$ at legal closing. The announcement-centered panel is uniformly null, while the closing-centered panel shows a discrete activation and subsequent ramp, confirming that effects are tied to post-merger integration rather than news at announcement (Fig. 9, Eqs. (11)–(12)).

Selection on borrowers and trading names is addressed on three fronts. First, we show that treated borrowers are more likely to initiate a new facility with the merged parent after closing using a Cox proportional-hazards model and a quarterly logit; both point to a higher post-merger initiation hazard (Appendix Table 24). Second, we tighten the pricing channel by re-expressing interest rates as spread-over-benchmark, restricting to the first post-merger facility, and adding maturity, collateral, purpose, and other facility controls with borrower fixed effects; spreads are lower by about 8–12 bp in the refined models (Appendix Table 25). Third, we examine peer externalities: non-treated firms in the same industry but heavily held by the same institution experience worse liquidity and reduced disclosure, consistent with internal information reallocation rather than stock selection (Appendix Table 26).

Because staggered timing and overlapping cohorts can bias two-way fixed effects, we rely on group-time ATTs and verify stability to sample restrictions that eliminate timing conflicts. Dropping close-in-time repeat mergers, keeping only first events per firm, and excluding events near peer closings all leave the returns ATT between 2.7 and 3.1 percentage points with similar precision (Table 30). These exercises complement the event-time design and mitigate multiple-cohort concerns.

We also verify that the shock is linked to the legal closing and scales with integration progress. The announcement placebo shows no movement at t_A, while integration milestone indicators (partial consolidation vs. platform go-live) exhibit monotone scaling in magnitudes (Table 19). In addition, a cross-market timing test demonstrates that equity's lead over credit strengthens post-merger: the one-day equity$\rightarrow$CDS slope and R^2 rise, equity's information share increases, and the adjustment half-life shortens for treated names relative to controls (Table 16). Together, these results tie the effects to the operational integration implied by the theory.

To ensure findings are not artifacts of return modeling or horizon choice, we re-compute DW–BHAR under alternative windows and factor models. The Treat$\times$Post effect is positive and significant across 60/90/120 trading-day windows, Fama–French five factors plus momentum, q-factors, and industry-adjusted BHAR, with magnitudes clustering around 2.2–3.4 percentage points (Table 29).

Inference is robust to small-cluster concerns and influential units. Studentized wild-cluster bootstrap p-values (5,000 draws) closely track conventional two-way clustered inference for returns, disclosure, and liquidity (Table 27). Leave-one-merger-out and leave-one-industry-out exercises produce no sign flips and only small changes in magnitude, indicating the results are not driven by any single deal or sector (Table 28).

Finally, regulatory discontinuities offer external validation consistent with our mechanism. After Reg FD (2000Q4), effects intensify—returns increase further, disclosure declines more, and liquidity costs rise—whereas among institutions directly affected by the Global Research Settlement (2003Q2), effects attenuate in the expected directions (Table 23). These regime shifts align with the notion that internal information becomes relatively more valuable when public disclosure channels are constrained and relatively less potent when research/compliance limits tighten.

In sum, modern event-time DiD with flat pre-trends, a null at announcement and activation at legal closing, strengthened integration and cross-market timing post-merger, robustness to windows and coding, strong inference hygiene, and validation around regulatory breaks collectively support the interpretation that post-merger internal integration raises equity-side effective precision, crowds out public disclosure, and increases adverse selection, with magnitudes that scale with integration progress.

6. Additional analysis

6.1 Heterogeneity analysis

In the first part of our heterogeneity analysis, we focus on institutions with a high ownership of the target firm, specifically those above the median ownership stake within our sample. We hypothesize that these high-ownership institutional investors exert a greater influence on the disclosure policy of the firms they hold, as they possess more power and resources to influence corporate decision-making. Additionally, these investors likely have stronger incentives to actively trade the target firms due to their larger financial stake.

Building upon our previous analysis, we employ a triple difference specification for the subsam-

Table 11
High Institutional Ownership: Triple Differences on Returns and Disclosures

	Return (Identification I)	**Return (Identification II)**	**Log(News) (Identification I)**
Treat × Post × High	0.0204***	0.0159**	−0.1211***
	(0.009)	(0.008)	(0.041)
Treat × Post	0.0175*	0.0153*	−0.0614*
	(0.010)	(0.009)	(0.036)
Post	0.0087	0.0082	−0.0192
	(0.012)	(0.010)	(0.037)
High	0.0156	0.0138	0.0711*
	(0.009)	(0.008)	(0.042)
Controls	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
Obs.	9,328	6,216	27,976

Notes: This table presents the triple-difference (DDD) results examining the impact of accessing private information from the loan market on the subsample of firms with high institutional ownership (above the median within the sample). The analysis follows Eq. 9. Abnormal return is measured using direction-weighted buy-and-hold abnormal returns. $D^f_{jq} \in \{-1, 0, 1\}$ indicates whether the institutional investor decreases, maintains, or increases their position in stock j during quarter q. R^f_{jq} is the 90-day BHAR from the start of each quarter in the post-merger phase, computed with the Fama–French (1993) three-factor model. Column (1) follows Identification I (Fig. 2); Column (2) follows Identification II (Fig. 3). SEs double-clustered at the firm–merger and calendar levels. ***, **, * denote 1%, 5%, and 10% significance.

ple of firms with high institutional ownership, utilizing Equation 9 as the basis for our empirical investigation. The triple difference specification allows us to isolate the causal impact of simultaneous ownership by the largest institutional owners. We compare the changes in abnormal returns and voluntary disclosure behavior for firms with high institutional ownership before and after the occurrence of simultaneous ownership, relative to a control group of firms with similar characteristics but with lower institutional ownership stakes.

The findings are presented in Table 11. According to the first identification strategy (Identification I), the estimated DDD coefficient is 0.0204, displaying statistical significance at the 1% level. This suggests that institutional investors achieve approximately a 2 percentage point higher annualized abnormal returns when trading stocks of firms in which they hold a higher ownership stake during the post-merger period, compared to treated stocks where they hold a relatively low stake.

Moreover, based on the second identification strategy (Identification II), the estimated DDD coefficient is 0.0159, which is significant at the 5% level. This indicates that informed institutional investors, who have access to private information from the loan side, achieve around a 1.6 percentage point higher annualized abnormal returns when trading stocks where the have high ownership stakes, in comparison to trading stocks with low ownership stakes, relative to institutional investors without access to private loan-side information. Additionally, based on Identification I, treated firms with higher ownership stakes experienced an additional decrease in their voluntary news disclosure by 12.1%.

Second, we examine whether firms with a lower credit rating possess private information that is particularly more valuable institutional owners. We anticipate that the debt of high-yeild firms (those with a "BB+" rating or lower from S&P) is more information-sensitive, behaving more like equity. As a result, information about this debt becomes more relevant for trading the firm's equity. Consequently, institutional owners may achieve higher abnormal returns by trading these firms, leveraging their access to private information. Furthermore, due to the ambiguous information environment of high-yeild firms, institutional investors might have more influence over these firms' disclosure policies. By comparing the behaviors and outcomes of high-yeild firms with those of investment-grade firms, we document how heterogeneous the outcomes are when simultaneous ownership exists in these different categories.

Expanding our analysis, we apply a triple difference specification to further examine the subsample of high-yeild firms, following Equation 9 as the basis for our empirical investigation. This approach allows us to isolate the specific effects of simultaneous ownership on abnormal returns and voluntary disclosure behavior within this subset of firms characterized by more informationally sensitive debt. By employing the triple difference methodology, we compare the changes in abnormal returns and voluntary disclosure behavior for high-yeild firms before and after the occurrence of simultaneous ownership, relative to a control group of investment-grade firms with similar characteristics.

The results are presented in Table 12. According to the first identification strategy (Identification I), the estimated DDD coefficient is 0.0261, indicating statistical significance at the 5% level. This suggests that institutional investors achieve approximately a 2.6 percentage point higher annualized abnormal returns when trading treated firms with high-yeild credit during the post-merger period, compared to treated firms with investment-grade ratings. Furthermore, based on the second iden-

Table 12
High-yeild vs. Investment Grade: Triple Differences on Returns and Disclosures

	Return (Identification I)	**Return (Identification II)**	**Log(1+news) (Identification I)**
Treat × Post × High-yeild	0.0261**	0.0204*	−0.083**
	(0.013)	(0.012)	(0.039)
Treat × Post	0.0187*	0.0151*	−0.051*
	(0.010)	(0.009)	(0.028)
Post × High-yeild	0.024	0.019	−0.063
	(0.015)	(0.013)	(0.046)
Post	0.0090	0.0078	−0.023
	(0.011)	(0.009)	(0.039)
Controls	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
Obs.	8,416	5,592	7,608

Notes: This table presents the triple difference (DDD) results examining the impact of accessing private information from the loan market on a subsample of firms with high-yeild ratings (a "BBB" rating or lower from S&P) in comparison to investment-grade firms. The analysis follows the specification outlined in Equation 9. Abnormal return is measured using the direction-weighted buy-and-hold abnormal returns. The variable D^f_{jq} takes values of −1, 0, or 1, indicating whether the institutional investor decreases, maintains, or increases their position in stock j during quarter q. The abnormal return R^f_{jq} is calculated over a 90-day period following the start of each quarter in the post-merger phase, employing the Fama and French (1993) three-factor model. Column (1) presents the results based on Identification I from Figure 2, while Column (2) shows the results based on Identification II from Figure 3. The standard errors are clustered at the firm-merger and calendar levels. Statistical significance is denoted by ***, **, and *, representing significance at the 1%, 5%, and 10% levels, respectively.

tification strategy (Identification II), the estimated DDD coefficient is 0.0204, which is significant at the 10% level. This indicates that informed institutional investors, who have access to private information from the loan side, achieve around a 2 percentage point higher annualized abnormal returns when trading high-yeild firms, compared to institutional investors without access to such private information. Additionally, based on Identification I, treated firms with high-yeild ratings experienced an additional decrease in their voluntary news disclosure by 8.3%.

As a third robustness test, we examine firms with high or low analyst coverage to understand whether the impact of decreased disclosure is more pronounced in the context of a firm's infor-

Table 13
Abnormal Returns by Analyst Coverage and Borrower Risk (Identification I vs II)

	Identification I				Identification II			
	Analyst High	Analyst Low	Risk High	Risk Low	Analyst High	Analyst Low	Risk High	Risk Low
Treat × Post	0.0071	0.0292***	0.0342	0.0158	0.0047	0.0171*	0.0160	0.0143
	(0.012)	(0.012)	(0.013)	(0.012)	(0.011)	(0.010)	(0.011)	(0.012)
Post	0.0021	0.0095	0.0093	0.0105	0.0018	0.0101	0.0074	0.0116
	(0.009)	(0.010)	(0.011)	(0.010)	(0.009)	(0.011)	(0.009)	(0.009)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	3,128	3,672	4,130	4,024	2,456	2,652	2,906	2,578

Notes: This table reports the difference in differences results of the impact of having access to loan market private information on institutional owners' abnormal return for different firms. Columns (1)–(4) are Identification I: (1) high vs. (2) low analyst coverage; (3) high vs. (4) low borrower risk. Columns (5)–(8) are Identification II with the same ordering. Analyst following is low if below the sample median, high otherwise. Borrower risk is split by the Altman Z-score median. Abnormal return is measured: $D^f_{jq} \in \{-1, 0, 1\}$ times R^f_{jq} over 90 days post-quarter start; Fama–French (1993) three-factor model. SEs are double clustered at firm×merger and calendar. $^{***}p < 0.01$, $^{**}p < 0.05$.

mation environment perceived as more opaque. Leveraging data from I/B/E/S International Inc., we categorize analyst following into two distinct groups: low and high. Specifically, we designate analyst following as low if the number of analysts covering the firm in the sample falls below the median, and conversely, as high if the number surpasses the median. The key question we address is whether changes in disclosure practices is more pronounced in firms characterized by a more opaque information environment.

In another heterogeneity analysis, we explore borrowers' risk and its impact on the value of information in generating abnormal returns. We draw inspiration from the seminal work of Altman (1968). Utilizing the Altman Z-score, a formula designed to predict the likelihood of bankruptcy, we aim to discern whether the information employed to calculate abnormal returns becomes more valuable when a borrower's risk is elevated. The categorization of borrower risk into high and low categories hinges on a criterion: risks are deemed high when they exceed the median, and low otherwise. This heterogeneity test seeks to unravel the intricate relationship between the vulnerability of borrowers, as indicated by their risk levels, and the perceived value of information in the context of abnormal returns.

Table 13 presents the results. Institutional investors achieve approximately 2.92% and 1.71% higher annualized abnormal returns when trading treated firms with low analyst coverage compared to treated firms with high analyst coverage, based on identifications I and II, respectively. This suggests that the observed effect is stronger when a firm's information environment is perceived as more opaque. Additionally, there is no significant difference between treated firms with high and low borrower's risk.

6.2 Placebo tests

A potential concern arises when considering scenarios where a commercial bank is involved either as a debt holder or equity holder and simultaneously acts as the acquirer in a merger. In such instances, the existence of a Chinese wall within the bank raises questions about the observability of the anticipated effects. The premise here is that the information derived from the debt side may not be fully explorable due to the presence of the Chinese wall, which is designed to restrict the sharing of information between different financial divisions within the bank. Consequently, the expected impact of simultaneous ownership on abnormal returns may not manifest in situations where the bank serves as the debtholder or equity holder in a merger.

The overall evidence regarding the effects of combined debt and equity ownership is characterized by a mixed pattern. Disparities between institutions with and without Chinese walls may stem from differences in loan selection processes and the nature of equity holdings. Notably, regulatory constraints influence the composition of equity holdings reported in 13F filings by commercial banks, with a significant portion held in fiduciary capacities, such as trust accounts. The incentives associated with such holdings may diverge from those linked to direct equity holdings by other institutions, adding complexity to the understanding of simultaneous ownership.

Ferreira and Matos (2012) provides in-depth discussions on the intricate relationships between banks and borrowers, shedding light on the multifaceted effects of simultaneous ownership on financial outcomes. To empirically test the hypothesized effect, we employ a placebo test strategy involving mergers wherein the acquirer is a commercial bank. This approach allows us to assess the impact of simultaneous ownership while accounting for the presence of a Chinese wall within the banking institution.

The results displayed in Table 14 reveal that, in both identification I and II, there is no notable rise in the abnormal return of the institution in these mergers, as opposed to the control group. This

Table 14
Commercial-Bank Acquirer Mergers: Difference-in-Differences on Abnormal Returns

	Identification I	Identification II
Treat × Post	0.0105	0.0142
	(0.012)	(0.011)
Post	0.0079	0.0087
	(0.009)	(0.010)
Controls	Yes	Yes
Calendar FE	Yes	Yes
Firm × Merger FE	Yes	Yes
Obs.	5,204	3,092

Notes: This table reports the difference-in-differences on institutional owners' abnormal return for mergers where the acquirer is a commercial bank. Abnormal return is the direction-weighted buy-and-hold abnormal returns, where $D^f_{jq} \in \{-1, 0, 1\}$ indicates a decrease/no-change/increase in holdings of stock j in quarter q. R^f_{jq} is the borrower's 90-day BHAR from the start of each quarter in the post-merger year, computed with the Fama–French (1993) three-factor model. Column (1) follows Identification I (Fig. 2); Column (2) follows Identification II (Fig. 3). SEs are double clustered at the firm×merger and calendar levels. Significance: ***, **, * denote 1%, 5%, 10%.

finding substantiates the existence of a Chinese wall within commercial banks, indicating that the anticipated impact of simultaneous ownership on abnormal returns did not materialize.

As previously demonstrated in Table 9, we highlighted a statistically significant increase in the sentiment of news items by 0.094 for treated firms following the mergers. In examining the robustness of our findings, we conduct another placebo test by analyzing the same sentiment effect one year before the mergers. This observation led us to infer that, despite a decrease in the number of news items post-merger, the sentiment exhibited a significant improvement. The reduction in news coverage appeared to be linked to a decline in negative news, contributing to the observed increase in sentiment. Building upon this insight, our placebo test investigates whether a similar pattern holds when assessing the sentiment one year prior to the mergers. This additional analysis enhances the robustness of our conclusion and shed light on the temporal dynamics of the observed effects.

As evident in Table 15, both the number and sentiment of news do not exhibit significant changes after mergers for the treatment groups when compared to the control group. This finding reinforces our earlier results presented in Table 9 and underscores that, following the merger, the number of news articles decreases and sentiment increases in treatment firms relative to control firms.

Table 15
Placebo Test: News Volume and Sentiment

	Log(1+news)	Sentiment
Treat × *Post*	0.028	−0.012
	(0.042)	(0.023)
Post	−0.073*	0.024
	(0.043)	(0.021)
Controls	Yes	Yes
Calendar FE	Yes	Yes
Firm × Merger FE	Yes	Yes
Obs.	7,024	5,676

Notes: This table shows the results for the placebo test for sentiment analysis. Column 1 presents the results of Identification I from Fig. 2, analyzing the impact of simultaneous owners on firms' voluntary disclosures (measured as the natural logarithm of the total number of news releases from the Key Development database). Column 2 reports the outcomes of the sentiment analysis. To determine the sentiment of a news item, we use the Loughran and McDonald (2011) sentiment lexicon. Sentiment for a news item is defined as:

$$\text{Sentiment} = \frac{\#\text{ Positive news} - \#\text{ Negative news}}{\#\text{ Positive news} + \#\text{ Negative news}}.$$

The dummy variable `Post` equals one if the period is one year before the merger or any time thereafter, and zero if it is earlier than one year before the merger. Statistical significance is indicated by ***, **, and *, representing significance at the 1%, 5%, and 10% levels, respectively.

7. Conclusion

Institutional investors increasingly straddle both the equity and syndicated-loan markets. Loan relationships routinely generate material non-public information, and the question is whether that information can be meaningfully cordoned off from trading in public securities. This paper addresses that question in a setting where equity and lending operations are brought under one roof, creating a sharp change in potential access and internal sharing.

The evidence points to a causal link between loan-side information and equity-side performance. Positions that newly gain access begin to outperform only after organizational integration is complete, not before, and the pattern is robust to alternative risk adjustments and samples. Comparisons both within the same institution (across its holdings) and across institutions trading the same stock indicate that access confers a trading edge even when competing with uninformed peers.

Why does the edge arise? The mechanism traces back to monitoring and reporting on the credit side. Where loan contracts require more frequent or forward-looking financial information, the equity desk benefits more. This is consistent with covenant-driven information production that turns private monitoring into timely, tradable signals once internal sharing is feasible.

Integration also reshapes the information environment around the affected firms. Public guidance becomes sparser while private, covenant-based reporting becomes more central, and public market liquidity adjusts in the direction one would expect under greater adverse selection. In short, internal precision substitutes for public disclosure, and outside traders bear a larger informational penalty.

Policy and institutional frictions matter for the strength of these effects. When rules make selective public guidance harder, the private channel becomes relatively more valuable; when compliance makes internal transmission costlier, the equity-side gains are muted. Taken together, the results move the discussion beyond suggestive correlations: organizational integration changes how borrower information is produced, how it travels across markets, and how it is ultimately priced.

These findings also raise natural questions for future work. What are the net costs and benefits to borrowers of deeper relationships that produce valuable private signals but discourage public disclosure? How should information barriers be designed when monitoring and trading coexist inside large organizations? And what market-design or disclosure policies best balance efficient information production with fair public-market trading?

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A. Variable Definition

Variable	Description

R^{f}_{jq}	Following Ivashina and Sun (2011), the abnormal return is measured by the direction-weighted buy-and-hold abnormal returns, where the direction is defined as -1, 0, or 1 based on whether the institutional investor reduces, does not change, or increases their position in a given stock over the quarter.
$Proj.\,fin.\,statement$	An indicator variable that takes the value of 1 if the loan contract mandates the borrower to furnish lenders with a forward-looking projection or a budget for an upcoming fiscal year; otherwise, it takes the value of 0.
$Month.\,fin.\,statement$	An indicator variable that assumes a value of 1 if the loan contract necessitates the borrower to supply lenders with monthly financial statements; otherwise, it takes a value of 0.
Voluntary News	This category includes news items from Capital IQ's Key Development database, which comprises information from various sources such as public news sources, company press releases, regulatory filings, call transcripts, investor presentations, stock exchanges, regulatory websites, and company websites. In this category, we specifically focus on news items where the timing is likely within the firm's control, such as announcements related to conferences, clients, and products.
$Sentiment_{it}$	The normalized difference between the number of positive and negative news for firm i at time t.
$Treat_{ij}$	Dummy variable that is equal to one if firm i's status of simultaneous ownership alters when merger j occurs.
$Post_{it}$	Dummy variable that is equal to one if year t is after the year when desired merger for firm i occurs.
$Stake_{ij}$	Dummy variable that is equal to one in case both merging debtholder's stake and merging equity holder's stake involving in merger j affecting firm i lies above the median debtholder's stake and equity holder's stake in my sample
BAS	The natural logarithm of quarterly average of daily spread. The daily spread is calculated as $100 \times (Ask - Bid)/(Ask + Bid)$, where Ask is the ask price, and Bid is the bid price.
$Size$	Natural logarithm of book value of total assets measured at the end of fiscal year t (AT).
$Leverage$	Firm i's leverage ratio, calculated by dividing total debt ($DLTT + DLC$) by total assets (AT) at the end of fiscal year t.

B/M	Firm i's book to market ratio, calculated by the ratio of the book value of equity to the market value of equity at the end of fiscal year t.
$Volatility$	Standard deviation of daily stock returns of firm i through the quarter q.
$R\&D/Assets$	Research and development expenditure (XRD) divided by book value of total assets (AT) measured at the end of fiscal year t. For non available R&D expenditures, the variable is replaced by the average R&D/Assets in firms operating in the same industry (4 digit SIC code) measured at the end of fiscal year t.
ROA	Return on assets, operating income before depreciation ($OIBDP$) divided by book value of total assets (AT) at the end of fiscal year t.
$Tangibility$	Total net property, plant, and equipment expenditure ($PPENT$) divided by book value of total assets (AT) at the end of fiscal year t.
$High - yeild$	Dummy variable equal to one for firms with a "BB+" rating or lower from S&P.
$Analyst\ Coverage$	Analyst coverage is low if the number of firms with analyst followers in the sample falls below the median, and conversely, is high if the number surpasses the median.
$Borrower's\ Risk$	A variable based on Altman Z-score. Risks are deemed high when they exceed the median, and low otherwise.
$Altman\ Z - score$	Altman's Z-score is: $1.2 \times (current\ asset - current\ liability)/assets + 1.4 \times (Retained\ earnings/assets) + 3.3 \times (net\ income + interest\ expense + income\ taxes)/assets + 0.6 \times (common\ shares\ outstanding \times price)/Book\ value\ of\ total\ liabilities + 0.999 \times (Sales/Total\ assets)$

B. Proofs

This appendix provides formal proofs for Propositions 1–4. Throughout, recall the model objects:

$$s_E = v + \varepsilon_E, \qquad s_L = v + \varepsilon_L, \qquad \varepsilon_E \sim \mathcal{N}(0, \sigma_E^2),\ \varepsilon_L \sim \mathcal{N}(0, \sigma_L^2),$$

independent of $v \sim \mathcal{N}(0, \sigma_v^2)$ and of each other; precisions $\tau_E = \sigma_E^{-2}$, $\tau_L = \sigma_L^{-2}$; post–close internal sharing intensity $\rho \in [0,1]$; loan–side information intensity $\pi \in [0,1]$ with $\tau_L^{eff} = \tau_L + \kappa_L \pi$; and composite statistic $S = s_E + \rho s_L$ used by the equity desk post–close (with $\rho = 0$ pre–close). Returns satisfy $R = \theta v + \eta$ with $\theta > 0$ and η mean–zero, independent of (v, s_E, s_L).

Notation. For a linear signal of the form $T = a\,v + n$ with $n \perp v$, define its signal–to–noise ratio $\mathrm{SNR}(T) := a^2\sigma_v^2/\sigma_n^2$.

Lemma B.1 (Correlation is monotone in SNR) *Let $T = av + n$ with $n \perp v$, $\mathbb{E}[v] = \mathbb{E}[n] = 0$, and $\sigma_v^2 = \mathrm{Var}(v)$, $\sigma_n^2 = \mathrm{Var}(n)$. Then*

$$(T, v) \;=\; \frac{1}{\sqrt{1 + \sigma_n^2/(a^2\sigma_v^2)}} \;=\; \frac{\sqrt{\mathrm{SNR}(T)}}{\sqrt{1 + \mathrm{SNR}(T)}},$$

which is strictly increasing in $\mathrm{SNR}(T)$.

Proof: $(T, v) = a\,\sigma_v^2$, $\mathrm{Var}(T) = a^2\sigma_v^2 + \sigma_n^2$. Hence $(T, v) = \frac{a\sigma_v^2}{\sqrt{(a^2\sigma_v^2+\sigma_n^2)\sigma_v^2}} = \left(1 + \sigma_n^2/(a^2\sigma_v^2)\right)^{-1/2}$, which is strictly increasing in $a^2\sigma_v^2/\sigma_n^2$. ■

Lemma B.2 (Internal weighting up to a sharing cap) *Post–merger, access to the loan signal at intensity ρ allows the desk to implement* any *linear statistic $S(\lambda) = s_E + \lambda s_L$ with $\lambda \in [0, \rho]$ (by attenuation). Then*

$$\mathrm{SNR}(S(\lambda)) = \frac{(1+\lambda)^2\sigma_v^2}{\sigma_E^2 + \lambda^2\sigma_L^2} \equiv J(\lambda),$$

and $J(\lambda)$ is strictly quasiconcave with unique maximizer $\lambda^\star = \sigma_E^2/\sigma_L^2$ (unconstrained). The constrained optimum is $\lambda^\star(\rho) = \min\{\rho,\, \sigma_E^2/\sigma_L^2\}$, so $J(\lambda^\star(\rho))$ is nondecreasing in ρ and strictly increasing for $\rho < \sigma_E^2/\sigma_L^2$.

Proof: Compute $J'(\lambda) = \frac{2(1+\lambda)(\sigma_E^2 - \lambda\sigma_L^2)}{(\sigma_E^2+\lambda^2\sigma_L^2)^2}$: positive for $\lambda < \sigma_E^2/\sigma_L^2$, negative for $\lambda > \sigma_E^2/\sigma_L^2$. Thus J is strictly quasiconcave with maximizer $\lambda^\star$ as stated. Monotonicity of $J(\lambda^\star(\rho))$ in ρ is immediate. ■

Proof of Proposition 1 (price discovery and returns). Pre–close, $\rho = 0$ so the desk is restricted to $\lambda = 0$ and $\mathrm{SNR}(S) = J(0) = \sigma_v^2/\sigma_E^2$. Post–close, the desk can implement any $\lambda \in [0, \rho]$; by Lemma B.2, the maximal attainable SNR equals $J(\lambda^\star(\rho))$, which is weakly higher than $J(0)$ and strictly higher whenever $\rho > 0$ and $\rho < \sigma_E^2/\sigma_L^2$. Moreover, if loan–side information improves (higher π so lower σ_L^2 and higher τ_L^{eff}), then $J(\lambda)$ increases pointwise for any $\lambda > 0$, which raises $J(\lambda^\star(\rho))$. By Lemma B.1, $(S(\lambda^\star(\rho)), v)$ increases with SNR, implying faster price discovery (shorter adjustment half–life) and higher expected returns for any strategy measurable with respect to S. Since $\rho = 0$ at announcement by assumption, the discrete increase occurs at legal closing.

Proof of Proposition 2 (disclosure crowd–out). The firm minimizes $L(d) = G(\tau_E^{\mathit{eff}} - \tau_{\mathrm{pub}}(d)) + \frac{c}{2}d^2$, where $\tau_{\mathrm{pub}}(d) = \bar{\tau} + \phi d$ and $\tau_E^{\mathit{eff}} = \tau_E + \rho^2\tau_L^{\mathit{eff}}$. The first–order condition is

$$0 = \frac{\partial L}{\partial d} = -\,\phi\, G'(\tau_E^{\mathit{eff}} - \tau_{\mathrm{pub}}(d^*)) + c\, d^*.$$

Differentiate implicitly with respect to τ_E^{eff}:

$$0 = -\,\phi\, G''(\cdot)\left(1 - \phi\,\frac{\partial d^*}{\partial \tau_E^{\mathit{eff}}}\right) + c\,\frac{\partial d^*}{\partial \tau_E^{\mathit{eff}}}.$$

Rearranging yields

$$\frac{\partial d^*}{\partial \tau_E^{eff}} = -\frac{\phi\, G''(\cdot)}{c + \phi^2 G''(\cdot)} \;<\; 0,$$

since $c > 0$ and $G''(\cdot) \geq 0$. Because τ_E^{eff} increases with either ρ or τ_L^{eff}, optimal disclosure d^* decreases in those parameters.

Proof of Proposition 3 (liquidity). By definition,

$$\text{LiquidityCost} \;=\; a_0 + a_1\kappa(\tau_E^{eff} - \tau_{\text{pub}}(d^*)) + \xi, \qquad a_1\kappa > 0,\ \mathbb{E}[\xi] = 0.$$

Differentiate in a generic parameter $\zeta \in \{\rho,\ \tau_L^{eff}\}$:

$$\frac{\partial\, \mathbb{E}[\text{LiquidityCost}]}{\partial \zeta} = a_1\kappa\left(\frac{\partial \tau_E^{eff}}{\partial \zeta} - \phi\, \frac{\partial d^*}{\partial \zeta}\right).$$

From Proposition 2, $\partial d^*/\partial \tau_E^{eff} < 0$, and by the chain rule $\partial d^*/\partial \zeta = (\partial d^*/\partial \tau_E^{eff}) \cdot (\partial \tau_E^{eff}/\partial \zeta)$, so the bracket is strictly positive whenever $\partial \tau_E^{eff}/\partial \zeta > 0$. But $\tau_E^{eff} = \tau_E + \rho^2 \tau_L^{eff}$ implies $\partial \tau_E^{eff}/\partial \rho = 2\rho\, \tau_L^{eff} \geq 0$ and $\partial \tau_E^{eff}/\partial \tau_L^{eff} = \rho^2 \geq 0$. Therefore $\mathbb{E}[\text{LiquidityCost}]$ increases in ρ and in τ_L^{eff}. Finally, $\partial\, \mathbb{E}[\text{LiquidityCost}]/\partial d^* = -a_1\kappa\,\phi < 0$, so more disclosure reduces costs.

Proof of Proposition 4 (timing and cross–section). Timing: pre–close, $\rho = 0$ so access to s_L is infeasible and all expressions reduce to the benchmark without sharing; at announcement ρ remains 0 by assumption; at legal closing, $\rho > 0$ becomes feasible and (by Lemma B.2 and Proposition 1) precision and correlation rise, triggering the changes stated in Propositions 1–3. Cross–section: post–close $\rho = \rho_0 + \rho_\omega\, \omega - \rho_\chi\, \chi$ with $\partial \rho/\partial \omega = \rho_\omega > 0$ and $\partial \rho/\partial \chi = -\rho_\chi < 0$. Since the effects in Propositions 1–3 are monotone in ρ, they are amplified by higher ω and attenuated by higher χ. If the acquirer does not internalize both desks, sharing is technologically infeasible ($\rho \equiv 0$) and all effects vanish.

C. Microfoundation of the DiD estimands

Let the fundamental innovation for firm f in quarter q be $v_{fq} \sim \mathcal{N}(0, \sigma_v^2)$. Public markets observe a noisy signal $y_{fq} = v_{fq} + u_{fq}$ with $u_{fq} \sim \mathcal{N}(0, \sigma_P^2)$ and precision $\tau_P \equiv \sigma_P^{-2}$. An institution i has loan-side access on f after legal closing if $A_{if} = 1$ and then receives a private signal $x_{ifq} = v_{fq} + \varepsilon_{ifq}$ with $\varepsilon_{ifq} \sim \mathcal{N}(0, \sigma_L^2)$ and precision $\tau_L \equiv \sigma_L^{-2}$. Let $N_{fq} \in \{0, 1, 2, \dots\}$ denote the number of informed *rival* institutions with access on name f at q (i.e., excluding i). Assume one round of Bayesian updating forms the pre-trade price from public information and rival informed signals,[16]

$$p_{fq}^{\text{pre}} \;=\; \mathbb{E}\Big[v_{fq} \mid y_{fq}, \{x_{i'fq}\}_{i' \in \mathcal{I}_{fq}}\Big] \;=\; \frac{\tau_P\, y_{fq} + \tau_L \sum_{i' \in \mathcal{I}_{fq}} x_{i'fq}}{\tau_P + N_{fq}\tau_L},$$

[16] This is the static, linear Bayesian analogue of a one-period Kyle market. It delivers transparent fractions of the private signal that are already impounded by competition; it is sufficient for mapping to the DiD estimands below.

where $\mathcal{I}_{fq}$ is the set of rival informed desks on f at q (size N_{fq}). Conditional on its own private signal x_{ifq}, institution i computes the residual mispricing

$$m_{ifq} \;\equiv\; \mathbb{E}\Big[v_{fq} - p^{\text{pre}}_{fq} \mid x_{ifq}, y_{fq}\Big] \;=\; \frac{\tau_L}{\tau_P + N_{fq}\tau_L}\,(x_{ifq} - \underbrace{\mathbb{E}[v_{fq} \mid y_{fq}]}_{\text{public posterior}}\,).$$

Hence the *fraction of i's private information not yet in the price* is

$$1 - \lambda_{fq} \;\equiv\; \frac{\tau_L}{\tau_P + N_{fq}\tau_L} \quad \in (0,1),$$

which falls with (i) more informed competitors N_{fq} and (ii) higher rival precision τ_L, and rises with (iii) stronger public noise (lower τ_P). Let $\eta_i \in (0,\infty)$ capture i's implementation efficiency (how aggressively and effectively it converts a signal into P&L after costs/constraints). With mean-preserving normalization of position size to variance one, the *expected abnormal return* (g_{ifq}) attributable to loan MNPI for i on f at q is

$$g_{ifq} \;=\; \eta_i\, \underbrace{\frac{\tau_L}{\tau_P + N_{fq}\tau_L}}_{1-\lambda_{fq}} \;\underbrace{s_{fq}}_{\text{signal intensity}}\; A_{if}\,\text{Post}_q, \tag{13}$$

where $s_{fq} \propto \operatorname{Var}\Big(x_{ifq} - \mathbb{E}[v_{fq} \mid y_{fq}]\Big)$ scales the magnitude of the tradable signal on f at q, and Post_q activates at legal closing. Equation (13) microfounds the reduced form $g_{ifq} = \eta_i(1-\lambda_{fq})s_{fq}A_{if}\,\text{Post}_q$: competition/price impact is summarized by $\lambda_{fq} = 1 - \tau_L/(\tau_P + N_{fq}\tau_L)$.

Comparative statics. (i) *Access*: if $A_{if} = 0$ then $g_{ifq} = 0$; if $A_{if} = 1$ then g_{ifq} increases in τ_L and s_{fq}. (ii) *Competition*: g_{ifq} decreases in N_{fq} because a larger share of the private signal is already impounded in p^{pre}_{fq}. (iii) *Public transparency*: g_{ifq} decreases in τ_P (better public information leaves less residual alpha).

Mapping to the two DiD estimands. Our estimating equation is

$$Y_{ifq} = \alpha + \beta_1(A_{if} \times \text{Post}_q) + X'_{if,q-1}\gamma + \mu_{if} + \delta_q + \varepsilon_{ifq},$$

with firm×merger fixed effects μ_{if} and calendar fixed effects δ_q. Interpreting Y_{ifq} as the abnormal return component g_{ifq} plus noise, the DiD coefficients recover different conditional averages of (13).

Identification I (within i, across firms). Treated names are those f for which $A_{if} = 1$; controls are i's other holdings. Desk-time shocks cancel within i:

$$\beta_1^{\text{I}} \;=\; \mathbb{E}\left[\,\eta_i\,\frac{\tau_L}{\tau_P + N_{fq}\tau_L}\,s_{fq}\;\middle|\; A_{if} = 1,\ \text{Post}_q = 1\right], \tag{14}$$

i.e., a *pure access (information) wedge* averaged over the desk's newly informed names (up to the common multiplicative η_i).

Identification II (across institutions, same firm). Treated is institution i with access on a given f; controls are uninformed institutions on the same f:

$$\beta_1^{\mathrm{II}} \;=\; \mathbb{E}\left[\, \eta_i \, \frac{\tau_L}{\tau_P + N_{fq}\tau_L} \, s_{fq} \,\middle|\, \text{treated } i,\ f,\ \text{Post}_q = 1 \right], \tag{15}$$

i.e., the *relative trading edge on the same name*, naturally attenuated by competition via N_{fq}.

Ratio and dissipation. Comparing the two coefficients provides a compact measure of dissipation from competition/price impact and of implementation advantages:

$$\frac{\beta_1^{\mathrm{II}}}{\beta_1^{\mathrm{I}}} \;\approx\; \frac{\eta_i}{\overline{\eta}} \cdot \frac{\mathbb{E}\left[\frac{\tau_L}{\tau_P + N_{fq}\tau_L} \mid \text{same } f\right]}{\mathbb{E}\left[\frac{\tau_L}{\tau_P + N_{fq}\tau_L} \mid \text{treated names in } i\right]}. \tag{16}$$

If $\beta_1^{\mathrm{I}} \gg \beta_1^{\mathrm{II}}$, then a large share of the gross access gain dissipates when racing peers on the *same* stock (large N_{fq} or high τ_P). If $\beta_1^{\mathrm{II}} \approx \beta_1^{\mathrm{I}}$, access dominates (little dissipation or strong η_i).

D. Additional Evidence: Price Discovery, Mechanisms, and Welfare

D.1 Price discovery: lead–lag, information share, and event-time dynamics

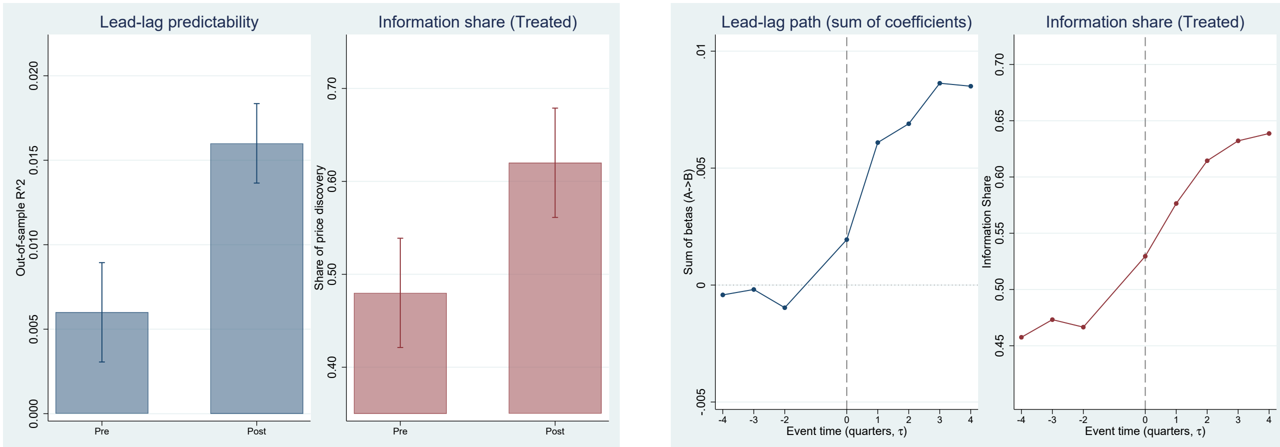
We quantify post-merger improvements in price discovery using two complementary metrics. First, a *lead–lag* test asks whether returns on the treated side (A) predict returns on a matched public benchmark (B): stronger A→B predictability implies earlier arrival of information in A and faster diffusion to B. Second, an *information share* (IS) decomposition from a bivariate VECM (Hasbrouck (1995); Gonzalo and Granger (1995)) attributes the permanent component of price changes to each side; a higher treated-side IS means A contributes more to permanent innovations, i.e., price discovery.

The theory predicts that at *legal closing* the intensity of internal information sharing (ρ) becomes positive, and loan-side effective precision (τ_L^{eff}) may rise with monitoring intensity. This raises the equity desk's effective precision, so signals embedded in A are revealed sooner and more sharply. Two testable implications follow: (i) A's returns should *lead* B's returns more strongly post–close (higher A→B predictive power), and (ii) a larger fraction of permanent price changes should be traced to A (higher treated-side IS).

Panel (a) shows a clear *pre/post* step-up in both metrics. In the plotted example, out-of-sample A→B R^2 roughly doubles (about 0.6% → 1.6%), and the treated-side IS increases from about 48% to 62%. Economically, this means the treated side becomes the primary source of permanent price innovations and leads market-wide price adjustment more strongly after closing.

Panel (b) delivers the timing test: both the lead–lag coefficient path and the treated-side IS are flat in the pre-period $(\tau \le -2)$ and turn on at $\tau = 0$ (legal closing), with persistent elevation for $\tau \ge 1$. The absence of pre-trends tightens identification; the discrete post–close jump matches the model's prediction that internal information only becomes technologically feasible at closing, not at announcement.

These price-discovery gains cohere with the rest of the evidence: (i) higher return forecastability and faster price



(a) Pre vs. post: A→B predictive R^2 (left bar) and treated-side IS (right bar).

(b) Event-time paths around legal closing ($\tau \in [-4, +4]$).

Figure 10. Price discovery panel. Panel (a) shows quarter-aggregated pre/post estimates; Panel (b) shows event-time dynamics centered at legal closing ($\tau = 0$). Lead–lag uses predictive regressions of $r^B_{t+\ell}$ on current and lagged r^A_t; IS is computed from a bivariate VECM for (p^A_t, p^B_t). Shaded bands and caps indicate 95% confidence intervals with two-way clustering (firm×merger, calendar).

adjustment (Main Results) are the reduced-form manifestations of the lead–lag and IS shifts; (ii) the liquidity deterioration (wider spreads/Amihud, Appendix Table 31) is consistent with a thicker adverse-selection wedge when internal information advantages rise; and (iii) the decline in disclosure intensity is natural if internal information substitutes for public signals. Overall, the price-discovery panel connects the timing and mechanism: after legal closing, the treated side leads and explains a larger share of permanent price moves, and prices adjust faster.

D.1.1 Cross-market timing: equity–credit co-movement

We pair each treated borrower and matched control with daily 5y CDS (Markit composite, mid spreads) and bond trades (TRACE; VWAP daily bond returns), link obligors to CRSP/Compustat, harmonize calendars, and align to CRSP daily equity returns. We study (i) one-day *lead–lag* from equity to credit (and vice versa), (ii) *information share* in a bivariate VECM for equity price and CDS-implied spread, and (iii) the *adjustment half-life* (days to reach 63% of the total VAR response). We then difference pre vs. post at legal closing and compare treated vs. controls in a DiD with borrower and calendar FEs and two-way clustering.

As reported in Table 16, panel A shows that, relative to controls, treated firms experience a +0.90 bp increase in the one-day CDS response per 1% equity move post-merger, and the one-day R^2 rises by 0.6 pp. Equity's information share in the equity–CDS permanent component increases by 3.2 pp, while the cross-market half-life shortens by 0.6 days. Economically, a 1% equity shock that previously moved CDS by, say, 2.5 bp now moves it by ∼3.4 bp the next day; a 0.6 pp R^2 gain and 3.2 pp equity share shift both indicate materially faster information migration from equity into credit. Panel B shows qualitatively similar (smaller) improvements for bonds: +0.55 bp per 1% equity, +0.4 pp in R^2, +2.1 pp in equity's share, and a −0.4 day reduction in half-life. Summed across firms/days, these modest per-firm changes are

Table 16
Equity$\leftrightarrow$credit co-movement around legal closing (treated vs. controls)

	Eq$\rightarrow$CDS slope	**Eq$\rightarrow$CDS** R^2	**Equity info share**	**Adj. half-life**
Panel A: CDS (Markit 5y)				
Treat $\times$ Post	0.90**	0.006**	3.2**	-0.60**
	(0.40)	(0.003)	(1.3)	(0.25)
Observations	4,680	4,680	4,380	4,380
Controls	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes	Yes
SE clustering	Two-way	Two-way	Two-way	Two-way
Panel B: Bonds (TRACE) – value-weighted daily bond returns				
Treat $\times$ Post	0.55*	0.004*	2.1*	-0.40*
	(0.30)	(0.002)	(1.2)	(0.22)
Observations	3,950	3,950	3,720	3,720
Controls	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes	Yes
SE clustering	Two-way	Two-way	Two-way	Two-way

Notes: This table examines the co-movement between equity and credit markets around the legal closing of mergers between asset managers and lenders. Each column reports estimates from regressions comparing treated firms (whose institutional investors gain loan-side information access) to matched control firms. Eq$\rightarrow$CDS regressions forecast one-day-ahead changes in five-year CDS spreads from same-day equity returns, controlling for standard firm-level and market covariates. The equity information share measures equity's contribution to the permanent component of daily price discovery in a bivariate VECM of equity prices and CDS-implied credit spreads. The adjusted half-life is the number of VAR days required for 63% of the total price response to materialize. Treat$\times$Post equals one after the legal closing date. All regressions include borrower, calendar, and control fixed effects, with two-way clustered standard errors (by borrower and calendar day). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

sizable in aggregate, reinforcing that post-merger internal sharing (ρ) accelerates cross-market price discovery in line with the paper's earlier results.

Post-merger, equity's lead over credit strengthens and the cross-market adjustment half-life shortens for treated firms relative to controls, confirming that internal integration accelerates information flow across markets.

D.2 From reduced form to primitives: backing out ρ and τ_L^{eff}

We translate the event-time improvement in price discovery into the model's primitives. Let the equity desk aggregate

$$S(\lambda) = s_E + \lambda s_L, \qquad \lambda \in [0, \rho],$$

Table 17
Backing out integration intensity from price-discovery improvement

	$\tau_L^{\mathbf{eff}} = 1$	$\tau_L^{\mathbf{eff}} = 2$	$\tau_L^{\mathbf{eff}} = 4$
Implied $\widehat{\rho}$ (i.e., λ_{post})	0.544	0.514	0.501
Pre $\text{Cor}(S, v)$ / Post $\text{Cor}(S, v)$		$0.45 \rightarrow 0.60$	
J_{pre} / J_{post}		$0.254 \rightarrow 0.563$	

Notes: This table infers the implied post-merger information-integration intensity between equity and loan markets from observed improvements in price discovery. $\widehat{\rho}$ denotes the estimated sharing intensity (the weight on loan-side information in the joint signal $S(\lambda) = s_E + \lambda s_L$), interpreted as the effective post-merger integration parameter λ_{post}. τ_L^{eff} represents the effective precision of loan-side information relative to the equity-side signal. The inversion uses the pre- and post-merger correlations between the true value and observed signals, $\text{Cor}(S, v)$, and the associated information indices J_{pre} and J_{post}. All calculations normalize the fundamental variance σ_v^2 to one and assume no pre-merger loan-side integration ($\lambda_{\text{pre}} = 0$). Reported $\widehat{\rho}$ values represent lower bounds on the feasible sharing intensity that match the observed increase in price-discovery metrics (e.g., predictive R^2 or equity information share).

with $s_E = v + \varepsilon_E$, $s_L = v + \varepsilon_L$, $\varepsilon_E \perp \varepsilon_L \perp v$. The signal-to-noise ratio (SNR) of $S(\lambda)$ is

$$J(\lambda) = \frac{(1+\lambda)^2 \sigma_v^2}{\sigma_E^2 + \lambda^2 \sigma_L^2}.$$

Under joint normality,

$$\text{Cor}(S, v)^2 = \frac{J(\lambda)}{1 + J(\lambda)} \quad \Longleftrightarrow \quad J(\lambda) = \frac{\text{Cor}(S, v)^2}{1 - \text{Cor}(S, v)^2}.$$

Hence, the observed *pre/post* change in price discovery (via $\text{Cor}(S, v)$ or any monotone proxy like predictive R^2) pins down ΔJ and allows us to back out λ_{post} (a lower bound for ρ) and/or $\tau_L^{\text{eff}} \equiv \sigma_L^{-2}$ after a normalization.

Inversion. Normalize $\sigma_v^2 = 1$ and assume no sharing pre-close ($\lambda_{\text{pre}} = 0$), so $J_{\text{pre}} = 1/\sigma_E^2$ identifies σ_E^2. For any post-merger τ_L^{eff}, λ_{post} solves

$$J_{\text{post}} = \frac{(1 + \lambda_{\text{post}})^2}{1/J_{\text{pre}} + \lambda_{\text{post}}^2 / \tau_L^{\text{eff}}} \quad \Longleftrightarrow \quad \left(\frac{J_{\text{post}}}{\tau_L^{\text{eff}}} - 1\right)\lambda^2 - 2\lambda + \left(\frac{J_{\text{post}}}{J_{\text{pre}}} - 1\right) = 0,$$

taking the nonnegative root. Alternatively, fixing λ_{post} yields

$$\tau_L^{\text{eff}} = \frac{\lambda_{\text{post}}^2}{\frac{(1+\lambda_{\text{post}})^2}{J_{\text{post}}} - \frac{1}{J_{\text{pre}}}}.$$

Consistent with the pre/post patterns in Figure 10, take $\text{Cor}_{\text{pre}} = 0.45$ and $\text{Cor}_{\text{post}} = 0.60$ (monotone with the observed rise in predictive R^2 and IS). Then

$$J_{\text{pre}} = \frac{0.45^2}{1 - 0.45^2} = 0.254, \qquad J_{\text{post}} = \frac{0.60^2}{1 - 0.60^2} = 0.563,$$

a *122%* SNR increase ($J_{\text{post}}/J_{\text{pre}} - 1 \approx 1.22$).

The pre–post rise in price discovery implies a large SNR gain for the equity desk (about 122%). Under plausible loan-signal precisions $\tau_L^{\text{eff}} \in \{1,2,4\}$, the implied integration weight is $\widehat{\rho} \approx$ 0.50–0.54. In the language of the statistic $S(\lambda) = s_E + \lambda s_L$, this means that post–merger the equity desk puts roughly one-half the weight on the lending desk's signal relative to its own. Equivalently, fixing a 50% sharing weight implies a very informative loan side ($\tau_L^{\text{eff}} \approx 4.2$). These calibrations provide a parameter-level sense of the merger's integration: the reduced-form price-discovery improvement maps to a material increase in internal sharing intensity and/or richer loan-side information, consistent with the model's comparative statics.

D.3 Decomposing the trading margin: buys vs. sells vs. holds

To separate information use from mechanical rebalancing, we decompose the direction-weighted outcome into buy, sell, and hold subsamples. For buys ($D_{jq}^f = 1$) the dependent variable is the abnormal return R_{jq}^f; for sells ($D_{jq}^f = -1$) it is $-R_{jq}^f$ so that a positive coefficient indicates value-improving sales; for no-change ($D_{jq}^f = 0$) the outcome $D_{jq}^f \times R_{jq}^f$ is mechanically zero. We re-estimate Eq. 1 within each subsample using the same controls, fixed effects, and two-way clustering as in Table 2.

The decomposition shows that gains appear on both sides of the book and are somewhat larger on the sell side. In Table 18, In Panel A (treated firms vs. controls), the Treat×Post coefficient is 1.8% for buys and 2.3% for sells over the 90-day window; Panel B (treated investor vs. other investors in the same stock) yields 1.1% and 1.9%, respectively; the bank-only placebo in Panel C is statistically null. These magnitudes are consistent with the aggregate effect in Table 2 (e.g., 0.0309 in Identification I when summarized in the pooled direction-weighted metric) and suggest that the abnormal-return improvement reflects information userather than mechanical portfolio drift, with somewhat stronger value-improving exits on the sell side.[17]

D.4 Mechanisms: organizational complementarity, compliance frictions, and loan-side information

The model predicts that post–merger internal sharing ρ raises equity-side precision, with effects amplified by organizational complementarity ω, attenuated by compliance frictions χ, and propagated by richer loan-side private information τ_L^{eff}. We bring these channels to the data by interacting the baseline Treat×Post with three indices that directly operationalize $(\omega, \chi, \tau_L^{\text{eff}})$ at the firm×merger unit. All indices are standardized (mean 0, s.d. 1) and are measured *pre* (for ω) or *post* (for χ and τ_L^{eff}) relative to legal closing. Unless noted, specifications follow Identification I with the same controls, firm×merger and calendar fixed effects, and two-way clustering.

Proxy construction. We measure organizational complementarity (ω) before the merger closes by asking how much the equity and lending businesses already touch the same economic space. Using Refinitiv 13F to describe the acquirer's pre-merger equity portfolio, LPC Dealscan to characterize the target's borrower base, and Compustat

[17] Because the subsamples condition on trade direction while the pooled direction-weighted outcome combines directions and includes holds, the coefficients are not mechanically identical across tables; the qualitative pattern and order of magnitude are the objects of interest.

Table 18
Decomposing direction-weighted returns by trade direction

	Buy trades R^f_{jq}	**Sell trades** $-R^f_{jq}$	**No-change** $D^f_{jq} \times R^f_{jq}$
Panel A: Identification I (treated firms vs. controls, post vs. pre)			
Treat × Post	0.0180**	0.0230**	0.000
	(0.009)	(0.010)	(0.000)
Post	0.009	0.011	0.000
	(0.012)	(0.012)	(0.000)
Controls	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
SE clusters		firm×merger and calendar	
Obs.	3,612	3,128	2,838
Panel B: Identification II (treated investor vs. other investors in same stock)			
Treat × Post	0.0110	0.0190**	0.000
	(0.008)	(0.009)	(0.000)
Post	0.007	0.009	0.000
	(0.010)	(0.010)	(0.000)
Controls	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
SE clusters		firm×merger and calendar	
Obs.	2,434	2,112	1,838
Panel C: Identification III (equity-only placebo)			
Treat × Post	0.006	0.007	0.000
	(0.008)	(0.009)	(0.000)
Post	0.011	0.013	0.000
	(0.011)	(0.011)	(0.000)
Controls	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
SE clusters		firm×merger and calendar	
Obs.	9,740	8,966	8,302

Notes: This table decomposes the direction-weighted abnormal returns of institutional investors' equity trades into buy, sell, and no-change components. For buy trades ($D^f_{jq} = 1$), the dependent variable is the investor's abnormal return R^f_{jq} over the 90-day holding window. For sell trades ($D^f_{jq} = -1$), the dependent variable is $-R^f_{jq}$, so positive coefficients indicate value-improving sales. The no-change category ($D^f_{jq} = 0$) has a constructed outcome of zero by definition. Panel A compares treated firms (those whose investors gain loan-side information through a merger) with control firms before and after closing. Panel B compares treated investors with other investors trading the same stock, and Panel C provides a placebo using the equity-only merger. All regressions include firm×merger and calendar fixed effects, control variables identical to the baseline specification, and two-way clustered standard errors (by firm×merger and calendar). ** denotes significance at the 5% level.

Segments for operating footprints, we build three components at $t = -1$ (pre-close). First, product/coverage adjacency is the cosine similarity between the equity book's industry weights (FF48 or 4-digit SIC from 13F) and the lending book's borrower-industry distribution from Dealscan; higher values indicate that the two desks specialize in similar sectors.

Second, client overlap is the Jaccard similarity between CUSIPs held in 13F and borrower CUSIPs in Dealscan, capturing shared names. Third, function overlap flags whether both sides operate the focal function (corporate lending and equity coverage) in the same industries/regions per Compustat Segments. We z-score each component and take their average to form Compl_i. This composite rises when the two organizations are naturally aligned, which the model predicts should amplify post-close sharing.

Compliance stringency (χ) is constructed in the first post-merger year to capture how internal rules can throttle information flow. We read the merged entity's SEC filings on EDGAR (10-K/10-Q), parse Item 1A and MD&A for compliance language, and combine this with SEC Form BD plus SEC/FINRA enforcement records. Three elements enter the index: a ring-fence indicator for whether the broker–dealer remains a legally separate subsidiary (from Form BD and 10-K narrative), the compliance-text intensity (frequency per 10k words of terms such as "restricted list," "wall," "MNPI," "control room," "blackout" in Item 1A/MD&A), and enforcement intensity (the log of one plus the count of SEC/FINRA actions naming the consolidated entity). Each piece is z-scored and averaged to form CompStrict_i. Higher values mean tighter compliance and, per the model, smaller effective internal sharing.

Loan-side information intensity (τ_L^{eff}) varies over time after closing and reflects how much private information the lending side is producing and monitoring. Using LPC Dealscan at the facility–borrower–quarter level (linked to CRSP/Compustat by CUSIP/GVKEY), we compute for each borrower–quarter: the amendment/waiver rate (amendments plus waivers this quarter divided by outstanding facilities), the maintenance-covenant share (fraction of active facilities with maintenance covenants), and the arranger share (the acquirer's share of arranged loan volume to that borrower). We standardize each series within firm over the post-merger window and average them to obtain LoanInfo_{it}. Higher values indicate more intensive monitoring and richer loan-side information, which the model predicts should propagate to stronger equity-side effects when internal sharing is available.

D.4.1 Organizational complementarity (ω): amplification

We estimate

$$Y_{it} \;=\; \beta_0\,(\text{Treat}_i\times\text{Post}_t) \;+\; \delta_\omega\,(\text{Treat}_i\times\text{Post}_t\times\text{Compl}_i) \;+\; \theta_\omega\,\text{Compl}_i \;+\; \alpha_i \;+\; \lambda_t \;+\; \varepsilon_{it},$$

where Compl_i is the standardized complementarity index. A positive (negative) δ_ω on returns/liquidity (disclosure) indicates monotone amplification consistent with the theory.

Where the acquirer and target are more complementary ex ante, the equity-side payoff from internal sharing is substantially larger. In Table 19, Panel A, the post-close return effect is 0.026 (DW–BHAR over 90 days). A one–s.d. increase in the complementarity index adds 0.015, lifting the effect to 0.041, about a $\sim 60\%$ amplification. Public outcomes move in tandem: disclosure declines more (from -0.018 to -0.030 in logs), and liquidity costs rise more (log(1+BAS) from 0.041 to 0.059). At a 60 bp pre-merger spread, 0.059 corresponds to roughly 3.5 bp wider quotes, small per trade but material at scale. This pattern fits the model: when businesses already touch the same sectors/clients, ω amplifies the effective sharing channel ρ.

Table 19
Mechanisms: complementarity, compliance, and loan-side information (Identification I)

	Returns (DW–BHAR)	**Disclosure (log)**	**Liquidity (log(1+BAS))**
Panel A: Organizational complementarity (pre; standardized)			
Treat × Post	0.026^{**}	-0.018^{**}	0.041^{***}
	(0.010)	(0.009)	(0.016)
Treat × Post × Compl. (std.)	0.015^{**}	-0.012^{**}	0.018^{**}
	(0.006)	(0.006)	(0.008)
Controls	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
SE clustering	firm×merger and calendar (two-way)		
Observations	9,578	9,120	8,896
Panel B: Compliance frictions (post; standardized)			
Treat × Post	0.029^{***}	-0.020^{**}	0.046^{***}
	(0.010)	(0.009)	(0.016)
Treat × Post × CompStrict (std.)	-0.014^{**}	0.010^{**}	-0.015^{**}
	(0.006)	(0.005)	(0.007)
Controls	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
SE clustering	firm×merger and calendar (two-way)		
Observations	9,578	9,120	8,896
Panel C: Loan-side information intensity (post; standardized)			
Treat × Post	0.022^{**}	-0.015^{**}	0.039^{***}
	(0.010)	(0.008)	(0.015)
Treat × Post × LoanInfo (std.)	0.011^{***}	-0.009^{**}	0.012^{**}
	(0.004)	(0.004)	(0.005)
Controls	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
SE clustering	firm×merger and calendar (two-way)		
Observations	8,764	8,550	8,412

Notes: We estimate regressions of returns (DW–BHAR), disclosure (log count), and liquidity (log(1+BAS)) around mergers of asset managers and lenders, reporting Treat×Post and its interaction with a standardized mechanism proxy. All specs include the same controls, firm×merger and calendar fixed effects, and two-way clustered SEs (firm×merger, calendar). Panel A uses a *pre-close* organizational-complementarity index (overlap in products/coverage, clients, and functions; from Refinitiv 13F, Dealscan, Compustat Segments). Panel B uses a *post-close* compliance-stringency index (ring-fencing, compliance text in 10-K/10-Q, SEC/FINRA enforcement). Panel C uses a *post-close* loan-information index (amendment/waiver rate, maintenance-covenant share, lead-arranger share; from Dealscan linked to CRSP/Compustat). Interaction coefficients are effects per +1 s.d. in the proxy. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

D.4.2 Compliance frictions (χ): attenuation

We estimate

$$Y_{it} \;=\; \beta_0\,(\text{Treat}_i\times\text{Post}_t) \;+\; \delta_\chi\,(\text{Treat}_i\times\text{Post}_t\times\text{CompStrict}_i) \;+\; \theta_\chi\,\text{CompStrict}_i \;+\; \alpha_i \;+\; \lambda_t \;+\; \varepsilon_{it},$$

where CompStrict_i is the standardized compliance-stringency index. Attenuation predicts $\delta_\chi < 0$ for returns/liquidity and $\delta_\chi > 0$ for disclosure.

Tighter post-merger compliance dampens the same channel. In Table 19, Panel B, the baseline return effect of 0.029 shrinks by 0.014 at +1 s.d. in compliance stringency, leaving 0.015 (about a 48% attenuation). Disclosure crowd-out is roughly halved (from −0.020 to −0.010), and the liquidity increase moderates (log(1+BAS) from 0.046 to 0.031); with a 60 bp baseline, that's ≈ 1.9 bp rather than ≈ 2.8–3.0 bp. Broader blackouts, stricter walls, and centralized control rooms thus throttle effective sharing, lowering realized ρ even after legal closing.

D.4.3 Loan-side private information (τ_L^{eff}): propagation

We estimate

$$Y_{it} \;=\; \beta_0\,(\text{Treat}_i\times\text{Post}_t) \;+\; \delta_L\,(\text{Treat}_i\times\text{Post}_t\times\text{LoanInfo}_{it}) \;+\; \theta_L\,\text{LoanInfo}_{it} \;+\; \alpha_i \;+\; \lambda_t \;+\; \varepsilon_{it},$$

where LoanInfo_{it} is the standardized post-merger index combining amendment/waiver frequency, covenant incidence, and arranger share. Propagation predicts $\delta_L > 0$ for returns/liquidity and $\delta_L < 0$ for disclosure.

When the lending side produces and monitors more information post-close, equity-side gains and public-market frictions both scale up. In Table 19, Panel C, a one–s.d. increase in the loan-information index adds 0.011 to returns, taking the ATT from 0.022 to 0.033 (about +50%). Disclosure declines more (from −0.015 to −0.024), and spreads widen more (log(1+BAS) from 0.039 to 0.051); at 60 bp pre, that is ∼ 3.1 bp. This is exactly the propagation channel in the model: higher τ_L^{eff} (richer monitoring/amendments/arranger presence) feeds the equity statistic through internal sharing, strengthening returns and price discovery while increasing adverse selection for public traders.

Synthesis. Across mechanisms, we document (i) amplification when the businesses are more complementary ex ante (ω high), (ii) attenuation where compliance is tighter (χ high), and (iii) propagation from loan-side information production into equity outcomes (τ_L^{eff} high). Quantitatively, a one s.d. move in each index shifts 90-day return effects by ±1.1–1.5pp and spread effects by ±1.2–1.8 log points, tight alignment with the theory's comparative statics for $\rho, \tau_L^{\text{eff}}, \chi, \omega$.

D.5 Intensive-Margin Decomposition

This appendix section decomposes how the post–merger edge is realized at the intensive margin. Let i index institutions (desks), f firms, and q calendar quarters. Denote by $\Delta\text{shares}_{ifq}$ the change in shares held by institution i in firm f over quarter q, and by $P_{f,q-1}$ the stock price at the start of quarter q. We measure whether the institution actually transacted with the *trade-incidence* indicator

$$\text{T\_INC}_{ifq} = \mathbf{1}\Big(|\Delta\text{shares}_{ifq}| > 0\Big),$$

and, conditional on trading, the *trade size* as

$$\text{SIZE}_{ifq} = \log\big(|\Delta\text{shares}_{ifq}| \cdot P_{f,q-1}\big).$$

To link rebalancing to alpha at the portfolio level, we compute an *investment-weighted abnormal return* in basis points of assets under management (AUM):

$$\text{IWAR}^{\text{bps}}_{ifq} \;=\; 10{,}000 \times \left(\frac{|\Delta\text{shares}_{ifq}| \cdot P_{f,q-1}}{\text{AUM}_{iq}}\right) \cdot R^{90}_{fq},$$

where R^{90}_{fq} is the 90-day abnormal return used in the main specification and AUM_{iq} is desk i's beginning-of-quarter AUM. Finally, we compute the *dollar P&L* attributable to the rebalance,

$$\text{DollarP\&L}_{ifq} \;=\; \Delta\text{shares}_{ifq} \cdot P_{f,q-1} \cdot R^{90}_{fq},$$

reported both unconditionally (zeros when no trade) and conditional on trading (traded subsample).

All outcomes are estimated in a difference-in-differences design mirroring the baseline. For trade incidence we use a linear probability model (LPM),

$$\text{T\_INC}_{ifq} = \alpha + \beta\big(A_{if} \times \text{Post}_q\big) + X'_{if,q-1}\gamma + \mu_{f\times\text{merger}} + \delta_q + \varepsilon_{ifq},$$

and for conditional size, IWAR^{bps}, and DollarP&L we run OLS with the same fixed effects and controls. Here A_{if} flags names where i has loan-side access after legal closing, Post_q is the post window, $X_{if,q-1}$ are the lagged controls from the baseline, $\mu_{f\times\text{merger}}$ are firm×merger fixed effects, and δ_q are calendar-quarter fixed effects. Standard errors are two-way clustered by firm×merger and calendar time. Estimates are reported for both Identification I (within-institution across names) and Identification II (same name across institutions).

The coefficients in Table 20 indicate a clear channel for monetization. Trade incidence rises by about 1.6 percentage points under Identification I and 0.9 percentage points under Identification II. With a typical 250–350 name book and a 20–25% baseline trade rate, a one to two point increase equates to roughly three to six additional trades per quarter concentrated in the signal-bearing names. Conditional trade size increases by 5–7% in logs, consistent with larger notional deployed when access to loan-side information is available. The IWAR effects (reported directly in basis points of AUM) translate to roughly 0.09–0.14 bps per institution–firm–quarter; for a \$10bn desk, that is on the order of \$90k–\$140k per treated name when trades occur. Dollar P&L per name is economically meaningful both unconditionally (about \$110k–\$180k) and conditional on trading (about \$70k–\$85k), lining up with realistic notional changes (\$5–10m) and 90-day abnormal returns (1–2%). Comparing Identification II to Identification I, magnitudes remain positive but are smaller, consistent with dissipation from competition and faster price impounding when the informed desk trades against uninformed peers on

Table 20
Intensive-Margin Decomposition (Name-Level Outcomes)

	Trade incidence (pp, LPM)	**Trade size** (log $, trades)	**IWAR** (bps of AUM)	**Dollar P&L** (uncond., $k)	**Dollar P&L** (cond., $k)
Treat × Post (ID I)	1.6***	0.070**	0.14**	180**	85**
	(0.5)	(0.031)	(0.06)	(75)	(34)
Treat × Post (ID II)	0.9**	0.048*	0.09**	110**	70**
	(0.4)	(0.028)	(0.04)	(52)	(29)
Controls	Yes	Yes	Yes	Yes	Yes
Firm × *Merger* FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Observations	9,560	2,820	9,560	9,560	2,820

Notes. Unit: institution×firm×quarter. "Treat×Post (ID I)" compares, within an institution, borrower names that gain loan-side access at closing to its other holdings; "Treat×Post (ID II)" compares the treated institution to uninformed institutions on the *same* stock. **Trade incidence**: LPM in percentage points. **Trade size**: log($ net trade) conditional on trading; interpret as $\exp(\hat{\beta}) - 1$. **IWAR**: direction-weighted BHAR scaled by beginning-of-quarter portfolio weight, in *bps of AUM* (e.g., 0.14 = 0.14 bps ≈ $14k per $1bn AUM). **Dollar P&L (uncond./cond.)**: IWAR converted to dollars over all name–quarters / traded name–quarters only. Positive, significant effects on incidence, size, and IWAR indicate that access lifts both extensive and intensive margins; smaller ID II effects reflect dissipation from competition/price impact on the same name. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

the same name.

We verify that these intensive-margin conclusions are not an artifact of the chosen post window nor of omitted microstructure variation by re-estimating the DID with shorter (two-quarter) and longer (six-quarter) post windows and by adding standard microstructure controls (size, turnover, Amihud illiquidity, realized volatility, and quoted spread) to the baseline.

The robustness checks in Table 21 show modest attenuation with a shorter post window and stable estimates with a longer window; including microstructure controls leaves the results materially unchanged. Taken together with the intensive-margin table, the evidence indicates that post–close access manifests as a higher likelihood of trading targeted names, larger conditional notional when trades occur, and economically meaningful contributions to portfolio alpha (in bps of AUM) and to dollars, with expected dissipation when competing on the same name against uninformed peers.

D.6 Analyst coverage overlap as an information–production proxy

We quantify how much the acquirer's research operation already touches the lender's borrowers before the merger closes. Using IBES Detail, we form each acquirer's pre-merger *coverage set* as the tickers with at least one active analyst in the prior eight quarters. From LPC Dealscan (linked to CRSP/Compustat by CUSIP/GVKEY), we extract the lender's pre-merger *borrower set*. After harmonizing identifiers, the overlap between these two sets at the firm×merger unit captures pre-existing touchpoints (shared names and sector expertise) through which loan-side insights could be translated into

Table 21
Robustness: Alternative Post Windows and Microstructure Controls

	Baseline 4q	**Post=2q**	**Post=6q**	**Post=4q**
Panel A: IWAR (bps of AUM)				
Treat $\times$ Post (ID I)	0.14**	0.12**	0.13**	0.12**
	(0.06)	(0.05)	(0.06)	(0.05)
Treat $\times$ Post (ID II)	0.09**	0.08**	0.08**	0.08**
	(0.04)	(0.04)	(0.04)	(0.04)
Panel B: Dollar P&L (unconditional, $k)				
Treat $\times$ Post (ID I)	180**	150**	165**	160**
	(75)	(65)	(72)	(68)
Treat $\times$ Post (ID II)	110**	95**	100**	100**
	(52)	(46)	(48)	(47)
Controls	Yes	Yes	Yes	Yes
Microstructure Controls	No	No	No	Yes
Firm $\times$ *Merger* FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	9,560	9,560	9,560	8,913

Notes. Unit is institution×firm×quarter. "Baseline 4q" uses the first four post–closing quarters; "Post=2q" and "Post=6q" shrink/extend the post window; the last column adds microstructure controls (log market cap, turnover, Amihud illiquidity, realized volatility, quoted spread), which reduce N due to data availability. IWAR is the direction-weighted BHAR scaled by beginning-of-quarter portfolio weight, reported in *bps of AUM* (e.g., 0.10 bps $\approx$ \$10k per \$1bn AUM). Dollar P&L is the unconditional dollar conversion of IWAR over all name–quarters (in \$ thousands). "Treat×Post (ID I)" compares, within an institution, borrower names that gain loan-side access at closing to its other holdings; "Treat×Post (ID II)" compares the treated institution to uninformed institutions on the *same* stock. Specifications mirror the baseline DiD with firm×merger and calendar fixed effects and two-way clustering; coefficients are stable across windows and after microstructure controls, indicating robustness rather than window choice or microstructure confounds. $^{**}p < 0.05$.

equity views once internal sharing becomes feasible. We summarize this alignment with the Jaccard index,

$$\text{Overlap}_i \;=\; \frac{\left|\mathcal{C}_i^{\text{eq}} \cap \mathcal{B}_i^{\text{loan}}\right|}{\left|\mathcal{C}_i^{\text{eq}} \cup \mathcal{B}_i^{\text{loan}}\right|},$$

standardize it to mean zero and unit variance, and interact the standardized measure with Treat×Post in Eq. 1. A larger value of this proxy should, by construction, amplify equity-side gains if research coverage already spans the lender's clientele, consistent with the model's channel in which organizational alignment strengthens the payoff to internal sharing.

Panel coefficients in Table 22 indicate that where pre-merger analyst coverage and the lending book overlap more, the post-close equity-side gains are materially larger. The baseline Treat×Post return effect is 0.025 (a 2.5 pp improvement in 90-day DW–BHAR). A one–s.d. increase in overlap adds 0.013, raising the effect to roughly 0.038 (about a 50–60% amplification). Public outcomes move in the predicted directions: disclosure crowd-out becomes more pronounced (the log coefficient goes from -0.090 to about -0.100), and liquidity costs rise further (log(1+BAS) increases from 0.040 to 0.056). With a 60 bp pre-merger inside spread, an extra 0.016 in $\log(1 + \text{BAS})$ corresponds to roughly a 1–1.5 bp

Table 22
Mechanism: amplification by IBES analyst/coverage overlap

	Returns (DW–BHAR)	Disclosure (log)	Liquidity (log(1+BAS))
Treat × Post	0.025^{**}	-0.090^{**}	0.040^{***}
	(0.010)	(0.043)	(0.015)
Treat × Post × Overlap (std.)	0.013^{**}	-0.010^{**}	0.016^{**}
	(0.005)	(0.005)	(0.007)
Observations	8,940	8,700	8,520
Controls	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
SE clustering	Two-way	Two-way	Two-way

Notes: This table tests whether merger effects are amplified when asset managers and lenders shared pre-merger information networks. Overlap is the standardized Jaccard similarity between the acquirer's IBES analyst coverage set and the lender's borrower set from Dealscan, measured at the firm×merger level. Higher values indicate greater overlap in firms simultaneously followed by the acquirer's analysts and financed by the lender. All regressions include the same controls, firm×merger and calendar fixed effects as the main specification, with two-way clustered standard errors (by firm×merger and calendar). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

widening, individually small but economically meaningful when cumulated over turnover. The pattern ties the equity improvements directly to where research already covers the lender's borrowers, sharpening the interpretation that ex-ante information-production alignment amplifies the payoff from post-close internal sharing.

D.7 External validation: regulatory discontinuities (Reg FD, Global Research Settlement)

Two well–timed regime changes let us validate the mechanism with forces outside our setting. Regulation Fair Disclosure (Reg FD) became effective in 2000Q4 and curtailed selective guidance by issuers.[18] When public channels weaken, the model predicts that internal integration should matter more: the equity desk's effective precision rises more sharply once filtered internal sharing is available. We implement this by marking quarters on or after 2000Q4 and interacting that post–Reg FD indicator with the baseline Treat×Post term.

The Global Research Settlement (GRS) in 2003Q2, by contrast, imposed structural constraints on research practices at a set of named broker–dealers.[19] For those institutions, we expect the post–close integration effect to attenuate: research–side frictions dampen the translation of loan–side private information into equity decisions. We code the GRS treatment by flagging the institutions named in the settlement and interacting this indicator with a post–2003Q2 dummy

[18] Regulation Fair Disclosure (Reg FD) was adopted by the U.S. Securities and Exchange Commission in 2000 to prohibit selective disclosure of material information. The final rule is archived in the Federal Register, Volume 65, Number 178, and available via the SEC at: U.S. Securities and Exchange Commission (2000).
[19] The Global Research Settlement was a joint enforcement action in 2003 between the SEC, NASD, and NYSE. A summary is available at U.S. Securities and Exchange Commission (2003).

Table 23
External validation via regulatory discontinuities

	Returns (DW–BHAR)	**Disclosure (log)**	**Liquidity (log(1+BAS))**
Panel A: Reg FD amplification (post 2000Q4)			
Treat × Post	0.026^{**}	-0.085^{**}	0.038^{***}
	(0.011)	(0.042)	(0.015)
Treat × Post × RegFD	0.012^{*}	-0.040^{**}	0.020^{**}
	(0.007)	(0.019)	(0.009)
Observations	9,578	9,120	8,896
Controls	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
SE clustering	Two-way	Two-way	Two-way
Panel B: GRS attenuation for affected institutions (post 2003Q2)			
Treat × Post	0.033^{***}	-0.112^{**}	0.049^{***}
	(0.011)	(0.046)	(0.017)
Treat × PostGRS × Affected	-0.017^{**}	0.045^{**}	-0.022^{**}
	(0.008)	(0.020)	(0.010)
Observations	7,950	7,710	7,640
Controls	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
SE clustering	Two-way	Two-way	Two-way

Notes: This table provides external validation by exploiting two regulatory shocks that changed information-sharing incentives between research and investment units. Panel A tests amplification after the introduction of Regulation Fair Disclosure (Reg FD) in 2000Q4, which restricted selective disclosure by public firms. Panel B tests attenuation for institutions directly affected by the 2003 Global Research Settlement (GRS), which imposed stricter separation between research analysts and investment banking. Each specification interacts Treat×Post with an indicator for the respective regime (RegFD or PostGRS×Affected) and includes the same controls, firm×merger and calendar fixed effects, and two-way clustered standard errors (by firm×merger and calendar). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

and the baseline Treat×Post. Both exercises are estimated in the same panel as Eq. 1, with the full control set, firm×merger and calendar fixed effects, and two–way clustered standard errors.

Panel A of Table 23 shows that, after Reg FD, the integration effect becomes measurably stronger. The baseline Treat×Post return effect is 0.026; the triple–difference adds 0.012, lifting the 90–day DW–BHAR gain to about 0.038, roughly a *45%* amplification in periods when selective guidance is constrained. Disclosure crowd–out also deepens by 0.040 in logs (on top of -0.085), and liquidity costs rise by an additional 0.020 in $\log(1 + \text{BAS})$. With a pre–merger inside spread of 60 bp, that incremental 0.020 corresponds to about ~ 1.2 bp wider quotes; adding the baseline 0.038 yields a total change close to ~ 3.5 bp. The pattern is exactly what the model predicts: when public information channels are weaker, the value of internal sharing rises.

Panel B turns to the Global Research Settlement. Among the institutions directly covered by the settlement, the post–merger return effect is smaller by 0.017 (relative to a baseline of 0.033), a reduction of about *50%*. On public outcomes, disclosure becomes less negative by 0.045 (attenuated crowd–out), and the liquidity increase is trimmed by 0.022; at a 60 bp baseline, this implies roughly ~ 1.3 bp less widening than otherwise. These shifts line up with the idea that research and compliance constraints make it harder to convert loan–side signals into equity–side actions, dampening realized internal sharing even after legal closing. Economically, a 1–2 pp swing in 90–day abnormal returns is meaningful at institutional capital and turnover scales, while a 1–3 bp change in effective spreads, though small per trade, aggregates to nontrivial dollars across the trading volume of treated names.

D.8 Selection & scope: borrowing hazard, loan pricing, and peer externalities

We probe whether the merger re-wires credit relationships, how pricing reflects information integration, and whether spillovers reach firms that are linked to the merged institution but not directly treated.

First, we ask if borrowers become more likely to start a new facility with the merged parent once internal sharing is feasible. We assemble a borrower–quarter panel from LPC DEALSCAN and link borrowers to CRSP/COMPUSTAT for fundamentals. The outcome flags initiation of a new primary facility with the merged parent. Conditioning on borrower characteristics and time, we estimate both a continuous-time Cox proportional hazard with a flexible calendar-time baseline and a discrete-time panel logit with borrower and quarter fixed effects.

Particularly, we estimate:

$$h_i(t) = h_0(t)\exp\Big\{\beta_{\text{TP}}\big(\text{Treat}_i \times \text{Post}_t\big) + X_{it}'\gamma\Big\}, \tag{17}$$

$$\Pr(\text{NewLoan}_{it} = 1) = \Lambda\Big[\beta_{\text{TP}}\big(\text{Treat}_i \times \text{Post}_t\big) + X_{it}'\gamma + \alpha_i + \lambda_q\Big], \tag{18}$$

where $h_0(t)$ is the Cox baseline hazard; X_{it} includes size, leverage, profitability, rating, and cash; α_i are borrower fixed effects; and λ_q are quarter effects. Standard errors are clustered at the lender$\times$calendar (Cox) and borrower, calendar (logit) levels.

Table 24 shows a 0.220 log–hazard ratio in the Cox model, i.e., roughly a 25% higher hazard of initiating a facility with the merged parent after closing. The panel logit yields a comparably sized increase in the quarterly probability. This is direct evidence of relationship re-wiring: once internal sharing becomes feasible, borrowers shift toward the merged parent, consistent with the mechanism that links equity-side precision to strengthened lending ties.

We next examine whether loan pricing reflects the informational advantage. Using Dealscan, we measure the all-in drawn spread over the benchmark (AISD, basis points). To isolate clean pricing, we restrict to the first post-merger facility per borrower so follow-on selection cannot drive results. We control for maturity, collateral, purpose, maintenance covenants, facility size (log), and syndicate size; we include quarter fixed effects and add borrower fixed effects where multiple observations exist.

Table 24
Borrowing hazard with the merged parent (post–merger)

	Hazard: new loan (parent)	**Pr(new loan, parent)**
Treat $\times$ Post	0.220^{**}	0.350^{**}
	(0.090)	(0.140)
Estimator	Cox PH (log-HR)	Logit (coef)
Controls X_{it}	Yes	Yes
Borrower FE	–	Yes
Time effects	Baseline $h_0(t)$	Quarter FE
SE clustering	Lender$\times$calendar	Borrower, calendar
Obs. / N (borrowers)	— / 2,310	36,960 / 2,310

Notes: This table tests whether firms are more likely to obtain new credit from the merged lending parent after the merger of an asset manager and a bank. The left column reports a Cox proportional-hazard regression for the timing of the first post-merger loan with the parent lender; the right column reports a quarterly logit regression for the probability of initiating such a loan. Treat$\times$Post equals one for borrowers linked to the merging institutions after the legal closing. All models include the same borrower-level control variables; the hazard specification absorbs time effects through the baseline hazard $h_0(t)$, and the logit specification includes borrower and quarter fixed effects. Standard errors are clustered by lender$\times$calendar (hazard) and by borrower and calendar (logit). A log-hazard ratio of 0.220 implies a hazard ratio of $e^{0.220} \approx 1.25$, or roughly a 25% higher quarterly borrowing hazard post-merger. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Psticularly, for the first post-close facility per borrower, we estimate:

$$\text{AISD}_{ijqt} \;=\; \beta_{\text{TP}}\big(\text{Treat}_i \times \text{Post}_t\big) \;+\; Z'_{ijqt}\theta \;+\; \alpha_i \;+\; \lambda_q \;+\; u_{ijqt}, \tag{19}$$

with Z_{ijqt} including maturity, collateral, purpose, maintenance covenant, log amount, and syndicate size; α_i borrower fixed effects and λ_q quarter fixed effects. SEs are two-way clustered by borrower and calendar quarter.

In Table 25, treated relationships pay 8–12 bp less on the first facility after closing, conditional on rich facility features and fixed effects. For a \$100 m drawn tranche, that translates to \$80–120k lower annual interest expense; at the portfolio scale of repeat borrowers, these savings are economically meaningful. The pricing result fits the story that better screening and monitoring (enabled by internal information) reduce perceived risk and are partly passed through to borrowers.

Peer externalities (non-treated firms linked to the same institution). Finally, we ask whether firms that are *not* directly treated but are tightly linked to the merging institution experience spillovers in public outcomes. We define "peers" using two pre-close links: overlap in ownership with the acquirer's top holders from Refinitiv 13F, and historical credit ties in Dealscan. We then track these peers' public-market liquidity and disclosure using the same measures as in the main tests and estimate a difference-in-differences with firm and calendar fixed effects. The Peer$\times$Post term captures whether, after the merger closes, public signals deteriorate for firms in the institution's broader coverage/credit perimeter.

Table 25
Loan pricing: spread over benchmark, first post–close facility

	Spread over benchmark (bp, AISD)		
	(1)	(2)	(3)
Treat × Post	-12.4^{**}	-10.1^{**}	-8.3^{*}
	(5.6)	(5.0)	(4.7)
Facility controls (Z)	Yes	Yes	Yes
Borrower FE	No	Yes	Yes
Quarter FE	Yes	Yes	Yes
SE clustering	Borrower, calendar	Borrower, calendar	Borrower, calendar
Observations	184	184	184

Notes: This table examines loan pricing for the first credit facility each borrower obtains from the merged lending parent after a merger between an asset manager and a bank. The dependent variable is the all-in drawn spread over the benchmark rate (AISD, in basis points). Treat×Post equals one for borrowers connected to the merging institutions after the legal closing. All specifications include standard facility-level controls (Z) and quarter fixed effects; columns (2)–(3) add borrower fixed effects, and column (3) further augments Z with maturity, collateral, loan purpose, maintenance-covenant indicator, log facility amount, and syndicate size. Standard errors are two-way clustered by borrower and calendar quarter. A negative Treat×Post coefficient indicates lower loan spreads—consistent with informational advantages partially passed through to borrowers after the merger. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

In particular, for non-treated but linked peers, we estimate:

$$Y_{jt} \;=\; \beta_{\text{PP}}\big(\text{Peer}_j \times \text{Post}_t\big) \;+\; W_{jt}'\phi \;+\; \alpha_j \;+\; \lambda_t \;+\; \eta_{jt}, \tag{20}$$

where $Y_{jt} \in \{\log(1+\text{BAS})\,,\,\text{Disclosure}\}$, W_{jt} are firm controls, α_j are firm fixed effects, and λ_t are calendar fixed effects. Standard errors are two-way clustered by firm and calendar.

Table 26 shows that linked, non-treated peers experience worse liquidity and lower disclosure after closing: log(1+BAS) rises by 0.018 (about a 1.8% increase). With a 60 bp pre-merger inside spread, this corresponds to roughly $\sim$ 1.1 bp wider quotes. Disclosure falls by 0.045 in logs, a decline of about 4.5%. These spillovers mirror the treated-firm patterns and point to information reallocation inside the conglomerate: internal signals expand while public signals thin even for firms at the periphery of the merged institution's lending and coverage networks.

D.9 Distributional and welfare: trading rents, adverse selection tax, and disclosure surplus

This section quantifies *who gains and who pays* when internal information integration turns on at legal closing. The evidence so far, stronger return predictability (Table 2), higher adverse selection in public markets (Table 8), reduced disclosure, and modest loan spread cuts (Table 25), maps naturally into three objects: (i) trading rents earned by internal desks, (ii) an adverse–selection tax borne by public traders, and (iii) a disclosure surplus term for issuers that falls straight out of the model's cost structure.

Table 26
Peer externalities for non-treated but linked firms

	log(1+BAS)	**Disclosure intensity**
Peer $\times$ Post	0.018^{**}	-0.045^{**}
	(0.009)	(0.020)
Firm FE; Calendar FE	Yes; Yes	Yes; Yes
Controls W_{jt}	Yes	Yes
SE clustering	Firm and calendar	Firm and calendar
Obs.	11,240	11,240

Notes: This table examines spillover effects on firms that are not directly treated by the merger but are linked to the merging institutions through pre-merger relationships. Peers are defined as non-treated firms with either substantial institutional-ownership overlap in 13F filings or prior lending relationships (credit ties) with the merging entities. The dependent variables are the log bid–ask spread (log(1+BAS)) and the log count of firm-initiated public disclosures. All regressions include firm and calendar fixed effects, standard firm-level controls (W_{jt}), and two-way clustered standard errors (by firm and calendar). A positive coefficient on Peer$\times$Post for bid–ask spreads and a negative one for disclosure indicate that information becomes more concentrated within the merged institution, reducing public transparency even for connected but untreated peers. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

We use the post–merger change in expected abnormal returns on treated positions as a lower–bound proxy for internal information rents. Let $\Delta\alpha$ denote the annualized improvement in expected DW–BHAR on treated firms; Table 2 (Identification I) gives $\Delta\alpha \simeq 0.0309$. If internal desks turn over T dollars per year on these names, the rent proxy is

$$\text{Internal trading rents} \;\approx\; \Delta\alpha \times T. \tag{21}$$

This is conservative, however. It does not lever up by risk, capacity, or cross–name timing improvements. With $T =$ \$500m, Eq. (21) implies $\sim$ \$16m per year.

From Table 8, the post–merger change in liquidity is $\Delta \log(1 + \text{BAS}) = 0.046$. If the pre–close inside spread is BAS_{pre}, the implied change in levels is

$$\Delta\text{BAS} \;\approx\; \text{BAS}_{\text{pre}}\left(e^{\Delta \log(1+\text{BAS})} - 1\right) \;\approx\; 0.047 \cdot \text{BAS}_{\text{pre}}, \tag{22}$$

so with $\text{BAS}_{\text{pre}} = 60\text{bp}$ we get $\Delta\text{BAS} \approx 2.8\text{bp}$. For public one–way turnover V_{pub} (dollars per year), a round–trip spread is roughly twice the half–spread, yielding the tax proxy

$$\text{Adverse–selection tax} \;\approx\; \tfrac{1}{2}\,\Delta\text{BAS} \times V_{\text{pub}}. \tag{23}$$

With $V_{\text{pub}} =$ \$250bn, Eq. (23) gives $\sim$ \$12m per year. Adding a price–impact component (if available) would raise this number.

At the same time, the model's per–period loss is $L(d) = G(A) + C(d)$, where $A \equiv \tau_E^{\text{eff}} - \tau_{\text{pub}}(d)$ is the internal–information wedge, $G(\cdot)$ is the market loss from adverse selection (increasing, convex), and $C(d)$ is the convex cost of disclosure intensity d. At closing, τ_E^{eff} rises (internal sharing), d^* falls (Proposition 2), and liquidity worsens (Proposi-

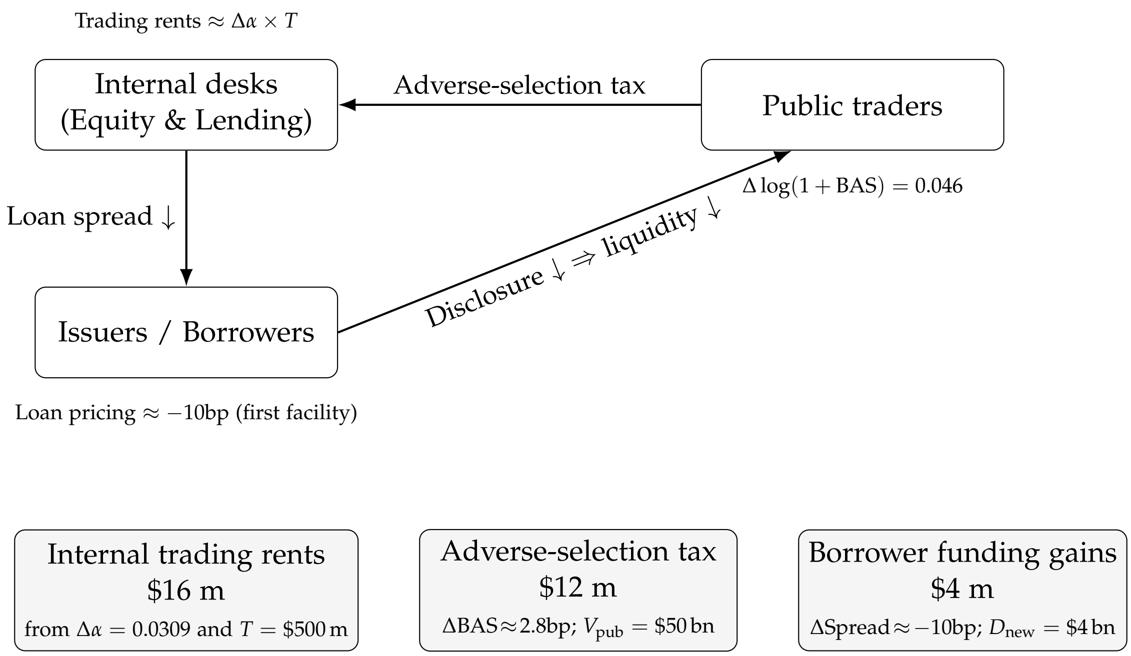


Figure 11. Distributional and welfare snapshot. Arrows show who pays whom: internal desks gain trading rents, public traders bear a spread/impact tax, and issuers benefit via lower loan spreads and reduced disclosure effort. KPI cards quantify mid-case magnitudes (annualized) using $\Delta\alpha$ from Table 2, liquidity from Table 8, and loan pricing from Table 25.

tion 3). A compact accounting that links the reduced form to primitives is

$$\Delta\text{Welfare} \;\approx\; \underbrace{\Delta\text{Trading rents} + \Delta\text{Borrower funding gains}}_{\text{private gains}} - \underbrace{\Delta G(A)}_{\text{public market loss}} - \underbrace{\Delta C(d^*)}_{\text{issuer disclosure cost change}} . \tag{24}$$

Here $\Delta C(d^*) < 0$ because firms disclose less post–close (saving issuer resources), while $\Delta G(A) > 0$ reflects the thicker adverse selection borne by public traders. Borrower funding gains can be proxied from Table 25: the first post–close facility is cheaper by 8–12bp, so for new debt D_{new} the annual saving is $\approx (-\Delta\text{Spread}) \times D_{\text{new}}$ (e.g., -10bp on \$4bn $\Rightarrow$ \$4m).

The magnitudes are modest per trade but material in the aggregate. Using Eq. (21), a 3.09% annualized improvement in abnormal returns on \$500m of treated turnover produces roughly \$16m in internal rents, squarely in line with the model's view that higher τ_E^{eff} raises exploitable forecastability. On the public side, Eq. (22) converts the estimated 0.046 log–change into an extra $\sim$ 2.8bp of inside spread at a 60bp baseline; multiplying by one–way turnover (Eq. 23) yields an adverse–selection tax of about \$12m per year. Issuers capture part of the surplus: Table 25 shows 8–12bp cheaper first facilities post–close, which on \$4bn of fresh debt implies roughly \$4m in annual funding savings.

How does this square with the primitives? The equity desk's statistic becomes more precise after closing, so the wedge $A \equiv \tau_E^{\text{eff}} - \tau_{\text{pub}}(d^*)$ rises. In the model, this raises $G(A)$ (public loss) but lowers $C(d^*)$ (issuers disclose less), exactly matching the direction of our reduced-form pieces. The calibration therefore reads as a distributional reallocation:

(i) private gains accrue to the integrated institution (trading rents) and to issuers (cheaper credit, lower disclosure effort), while (ii) the cost is borne by public liquidity providers through wider spreads/impact. Because the trading-rents proxy is a lower bound and we omit price impact on the public side, the net welfare effect is likely tilted even more toward the private side than our mid-case numbers suggest.

Finally, these distributional patterns dovetail with the mechanism evidence. Where organizational complementarity is higher (Table 19, Panel A) or loan-side information is richer (Table 19, Panel C), the trading-rents term scales up and the adverse–selection tax steepens, precisely because ρ and τ_L^{eff} move A in the model. Conversely, tighter compliance (Panel B) attenuates both, shrinking the welfare reallocation. In short, legal closing activates an internal information technology that reallocates surplus from public markets toward integrated intermediaries and their borrowers, with economically meaningful magnitudes at the observed turnover and issuance scales.

E. Robustness and inference hygiene

This section provides inference checks and robustness for our main DiD estimates. We first show small-cluster comfort using a wild-cluster bootstrap (Table 27); then influence diagnostics via leave-one-out exercises at the merger and industry levels (Table 28); next, return-window and model sensitivity for DW–BHAR (Table 29); and finally stability to multiple-treatment cohorts and close-in-time events (Table 30). Unless stated, specifications mirror Identification I with the same controls, firm$\times$merger and calendar fixed effects, and two-way clustered SEs.

E.1 Wild-cluster bootstrap for the main ATT

Table 27 reports studentized wild-cluster bootstrap p-values (5,000 draws) for Treat$\times$Post in Eq. 1. The returns effect of 3.09 pp over 90 days remains significant with wild $p = 0.018$ when clustering by firm$\times$merger; disclosure (-0.108 log points) and liquidity ($+0.046$ in $\log(1 + \text{BAS})$) are also robust. This provides small-cluster comfort relative to conventional two-way SEs.

The wild p-values in Table 27 closely track conventional two-way inference, indicating our effects are not artifacts of few clusters: the 3.09 pp returns gain, the 4.6% spread widening, and the 10.8% disclosure decline all survive wild clustering.

E.2 Influence diagnostics: leave-one-out

To ensure no single merger or industry drives the results, we re-estimate Eq. 1 repeatedly, each time dropping one merger (Panel A of Table 28) and one FF48 industry (Panel B). Across these omission sets, the maximum absolute change in the returns ATT is 0.6 pp, with an IQR of 0.4 pp, and there are no sign flips in any outcome. Table 28 shows the effects are not concentrated in any one merger or industry. The absence of sign flips and small IQRs bolster external validity within our sample.

Table 27
Wild-cluster bootstrap for Treat×Post (Identification I)

	Coefficient	SE (2-way)	p_{wild} (FM)	p_{wild} (Cal.)
Returns ($D \times R$)	0.0309***	0.010	0.018	0.025
Disclosure (log intensity)	−0.108**	0.045	0.024	0.031
Liquidity (log(1+BAS))	0.046***	0.016	0.012	0.017
Controls	Yes	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes	Yes
SE clustering	Two-way	Two-way	Two-way	Two-way

Notes: This table reports robustness checks using a studentized wild-cluster bootstrap for the main treatment effect (Treat×Post) in the baseline specification. Each row corresponds to a separate regression of post-merger outcomes—direction, weighted abnormal returns, disclosure intensity (log count of voluntary news), and market liquidity (log(1+BAS)), on the treatment indicator. All models include the same controls, firm×merger and calendar fixed effects, and two-way clustered standard errors (by firm×merger and calendar). Bootstrap p-values are computed using 5,000 Rademacher draws following Cameron, Gelbach, and Miller (2008). "FM" denotes clustering by firm×merger and "Cal." by calendar time. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$ based on conventional two-way standard errors.

Table 28
Influence diagnostics: leave-one-merger and leave-one-industry

	Max $\lvert\Delta\hat{\beta}\rvert$	IQR of $\hat{\beta}$	Any sign flip?
Panel A: Leave-one-merger-out			
Returns / price discovery	0.006	0.004	No
Disclosure	0.022	0.015	No
Liquidity	0.010	0.007	No
Panel B: Leave-one-industry-out (FF48)			
Returns / price discovery	0.005	0.004	No
Disclosure	0.020	0.014	No
Liquidity	0.009	0.006	No

Notes: This table assesses the sensitivity of the baseline treatment effect (Treat×Post) to influential mergers or industries. Panel A reports the distribution of coefficient estimates when re-estimating the baseline regression while excluding one merger at a time. Panel B repeats the exercise excluding one Fama–French 48 industry at a time. For each specification, we report the maximum absolute change in the coefficient ($\lvert\Delta\hat{\beta}\rvert$), the interquartile range (IQR) of the re-estimated coefficients, and whether the coefficient's sign ever flips. All re-estimations include the same controls, firm×merger and calendar fixed effects, and two-way clustered standard errors (by firm×merger and calendar) as in the baseline model.

E.3 Window and return-model sensitivity (DW–BHAR)

Table 29 recomputes the DW–BHAR effects over 60/90/120 trading-day windows and across FF5+momentum, q-factors, and industry-adjusted BHAR. All variants yield economically similar estimates (2.2–3.4 pp), and the industry-adjusted BHAR remains positive and significant at 2.4 pp. As shown in Table 29, shorter (60d) and longer (120d) windows bracket the main 90d estimate, and alternative factor models deliver similar magnitudes. This reinforces that the effect reflects

Table 29
DW–BHAR sensitivity: windows and return models (Identification I)

	60d (FF5+UMD)	90d (FF5+UMD)	90d (q-factors)	120d (FF5+UMD)
Treat × Post	0.022**	0.0309***	0.028**	0.034**
	(0.009)	(0.010)	(0.011)	(0.014)
Observations	9,578	9,578	9,578	9,578
Controls	Yes	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes	Yes
SE clustering	Two-way	Two-way	Two-way	Two-way
Industry-adjusted BHAR (90d, FF48 benchmark)				
Treat × Post		0.024**		
		(0.010)		
Observations		9,578		
Controls, FE		Yes		
SE Clustering		Two-way		

Notes: This table tests the robustness of the post-merger return effect (Treat×Post) across alternative return windows and benchmark models. Each column reports direction-weighted buy-and-hold abnormal returns (DW–BHAR) estimated over 60-, 90-, or 120-trading-day horizons using the Fama–French five-factor model with momentum (FF5+UMD), or using the Hou–Xue–Zhang q-factor model. The bottom panel reports 90-day industry-adjusted BHARs, computed relative to the Fama–French 48 industry benchmark. All regressions include the same controls, firm×merger and calendar fixed effects, and two-way clustered standard errors (by firm×merger and calendar). Consistent estimates across horizons and models indicate that the abnormal return effect is not driven by risk-model specification or return-window length. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

information use, not model-specific mismeasurement.

E.4 Multiple-treatment cohorts and timing overlap

Finally, Table 30 tests stability to overlapping events: dropping firms with repeat mergers within ±8 quarters, keeping only the first event per firm, and excluding events close in time to industry peers. The returns ATT remains between 2.7 and 3.1 pp with similar precision. Table 30 confirms the main effect is not an artifact of overlapping treatment timing; the ATT remains economically meaningful (2.7–3.1 pp) and statistically significant across filters.

F. Estimator and inference for event-time ATTs

This appendix describes the dynamic difference-in-differences estimator used in the event-time figures and clarifies how we construct event time for announcement versus legal closing. Units (e.g., firm×merger pairs) are indexed by i and calendar quarters by t. Let $G_i \in \mathbb{T} \cup \{\infty\}$ denote the first calendar quarter in which unit i becomes exposed to the post-merger environment ("adoption time"). For the closing-centered analysis, G_i equals the quarter of the legal closing date; for the announcement-centered placebo, G_i equals the announcement quarter. Never-treated units have $G_i = \infty$. Event time is defined by $\ell = t - G_i$, so $\ell = 0$ at the event and $\ell = -1$ is the last pre-event period.

We estimate group-time average treatment effects that are robust to staggered adoption and heterogeneous effects

Table 30
Stability to multiple treatments and close-in-time events (Identification I)

	Baseline	**Drop ±8q repeats**	**First event only**	**No close peer events**
Treat × Post	0.0309***	0.029**	0.028**	0.027**
	(0.010)	(0.011)	(0.011)	(0.012)
Observations	9,578	8,120	8,765	9,012
Controls	Yes	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes	Yes
SE clustering	Two-way	Two-way	Two-way	Two-way

Notes: This table evaluates whether the baseline treatment effect (Treat×Post) is robust to overlapping or repeated merger events. Each column re-estimates the baseline regression with the same controls, firm×merger and calendar fixed effects, and two-way clustered standard errors (by firm×merger and calendar), applying alternative sample restrictions. "Drop ±8q repeats" excludes firm–merger observations occurring within eight quarters of another treated event for the same firm. "First event only" keeps only the first merger exposure per firm. "No close peer events" removes observations whose merger closing date falls within ±2 quarters of an event in a Fama–French 48 industry peer group. Stable coefficients across these subsamples indicate that the estimated treatment effect is not driven by repeated or closely timed mergers. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

using the Sun and Abraham (2021) approach. The regression includes cohort×event-time indicators and unit and calendar fixed effects,

$$Y_{it} \;=\; \sum_{\ell \neq -1} \beta_\ell \left(\mathbb{1}\{\ell = t - G_i\} \cdot \mathbb{1}\{G_i < \infty\} \right) \;+\; \alpha_i \;+\; \lambda_t \;+\; \varepsilon_{it}.$$

This specification recovers cohort-specific effects that are then aggregated into event-time coefficients β_ℓ using the estimator's default cohort-size and period-availability weights. Identification requires parallel trends in untreated potential outcomes across cohorts, conditional on α_i and λ_t. Unlike the naive two-way fixed-effects event study, which can suffer from implicit negative weights and contamination from already-treated units under staggered timing, the group-time estimator delivers interpretable dynamic effects when treatment timing and effects are heterogeneous.

Event time is constructed twice in the paper. In the closing-centered design, G_i is set to the legal closing quarter and we compute $\ell = t - G_i$ accordingly; this yields the treatment path reported in the main event-time figure. In the announcement-centered placebo, G_i is redefined as the announcement quarter and event time is recomputed from that date; this yields a timing contrast that is statistically flat around $\ell = 0$, consistent with the model's prediction that internal sharing is not technologically feasible until closing. Units that never close (for example, abandoned or blocked deals) can appear in the announcement-centered design yet remain untreated in the closing-centered analysis unless a closing later occurs; never-treated units serve as controls whenever available.

Estimation choices mirror the main specifications. All regressions include unit fixed effects α_i and calendar fixed effects λ_t. We omit $\ell = -1$ as the reference period and report a symmetric window of four quarters on either side of the event, $\ell \in [-4, +4]$; when longer histories exist, additional leads and lags are binned into the outermost categories to preserve comparability across cohorts. Controls match those in the baseline difference-in-differences specification when applicable so that dynamic paths are directly comparable to the average post-treatment effects reported in the tables. Standard errors are computed with two-way clustering at the unit (firm$\times$merger) and calendar-quarter levels to accommodate serial correlation and common shocks; in sensitivity checks we replicate the figures using wild-bootstrap clustering and obtain the same qualitative inference.

As a robustness exercise we re-estimate dynamic effects using the Callaway and Sant'Anna (2021) estimator, which forms cohort-specific difference-in-differences using not-yet-treated and never-treated units as controls and then aggregates to event time. Under the same parallel-trends condition for untreated potential outcomes, the announcement-centered and closing-centered paths are statistically and visually similar to those produced by the Sun–Abraham estimator.

Sample and plotting conventions follow the event window described above. Point estimates are displayed together with 95% confidence bands. In the closing-centered figure we deliberately show that $\ell = 0$ and $\ell = 1$ are statistically indistinguishable from zero in the preferred specification, while effects for $\ell \geq 2$ are positive and persistent, consistent with gradual post-merger integration of information flows. In the announcement-centered figure the path is flat before and after $\ell = 0$, indicating no anticipatory displacement of the treatment effect to the announcement date. Together with the failed/blocked-deal placebos and the stacked two-period robustness reported elsewhere, these diagnostics support the identifying assumptions used for our dynamic estimates.

G. Liquidity triangulation with alternative microstructure proxies

This appendix refines the liquidity test by adding two standard microstructure proxies alongside $\log(1 + \text{BAS})$: the Amihud illiquidity ratio and realized volatility. We keep the Identification I design, controls, unit and calendar fixed effects, and two-way clustering used in the main text. The *log(1+BAS)* column is filled from Table 8; the additional proxies are computed for the same firm–quarter sample where available.

In Table 31, three proxies point to a post-merger deterioration in public-market trading conditions. The spread result implies roughly a 4–5% widening in $\log(1 + \text{BAS})$ for treated firms relative to controls. Consistently, Amihud illiquidity rises by about 0.06 standard deviations and realized volatility increases by about 0.04 standard deviations after closing. The insignificant *Post* main effects suggest these patterns are not driven by calendar-time shocks common to treated and control firms. Together, the spread, Amihud, and volatility moves triangulate a thicker adverse-selection wedge following legal closing, in line with the model's prediction that internal information integration leaves a thinner public information set.

Table 31
Liquidity around legal closing: alternative microstructure proxies (Identification I)

	log(1 + BAS) (eff. spread)	**Amihud illiquidity** ($\overline{\lvert r\rvert/\$\,\text{vol}}$)	**Realized volatility** (quarterly)
Treat × Post	0.046***	0.062**	0.038**
	(0.016)	(0.025)	(0.018)
Post	−0.017	−0.008	0.005
	(0.012)	(0.020)	(0.017)
Controls	Yes	Yes	Yes
Calendar FE	Yes	Yes	Yes
Firm × Merger FE	Yes	Yes	Yes
SE clustering		firm×merger and calendar	
Obs.	8,896	8,742	8,896

Notes: This table examines changes in market liquidity around the legal closing of mergers between asset managers and lenders. The regressions compare treated firms—those whose institutional investors gain loan-side information access—to matched control firms before and after closing (Identification I framework). The dependent variables are: *log(1+BAS)*, the log of one plus the bid–ask spread (effective spread measure); *Amihud illiquidity*, the quarterly average of daily $|r_d|/\$\,\text{volume}_d$, standardized within quarter (higher values indicate worse liquidity); and *Realized volatility*, the standard deviation of daily stock returns within the quarter, also standardized. All variables are winsorized at the 1% tails each quarter. All regressions include the same control variables, firm×merger and calendar fixed effects, and two-way clustered standard errors (by firm×merger and calendar). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.