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Essays in Accounting and Financial Institutions

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Essays in Accounting and Financial Institutions

by
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Dedicated to my family.

ABSTRACT OF THE DISSERTATION

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Professor Xiumin Martin, Chair

Financial institutions, e.g., mutual funds and banks, are important entities in the capital markets. They make decisions based on accounting information. Further, their activities influence information production on the firm side, because interests are aligned between institutional investors and firms either through contracting or through market price pressure. Within this context, my dissertation focuses on the interactions between financial institutions and firms. In Chapter 1, I study how institutional investors' myopia affects firms' voluntary disclosures. This research question is motivated by the concurrent debate in accounting theory about whether myopia leads to more or less voluntary disclosures. I explore the SEC regulation that required mutual funds to mandatorily report holdings more frequently as a natural experiment that increases institutional investors' myopia, and study its impact on portfolio firms' issuance of earnings guidance. I find firms that experience larger increase in investors' myopia are more likely to issue earnings guidance. In particular, they issue earnings guidance that are below the analysts' consensus, which helps them to better meet-or-beat the quarter end earnings target.

In Chapter 2 with Ran Duchin, Roni Michaely and Xiumin Martin, we study how personal connections matter in loan granting decisions. Previous theoretical and empirical studies find borrowers connected to lenders are more likely to receive loans, either due to lower information asymmetry, or favoritism, and are unlikely to distinguish between the two mechanisms. We explore the Paycheck Protection Program (PPP) as part of the Coronavirus Aid, Relief, and Economic Security Act (CARES Act). Banks carry minimum screening responsibilities in granting the PPP loans, since the funding is supported by the government and borrowers do not need to repay the debt as long as certain criteria are met. The unique feature of the PPP mutes the lower information channel in loan decisions and we are able to provide a cleaner estimate of how favoritism matters.

In Chapter 3 with Xiumin Martin, Xiaoxiao Tang and Yifang Xie, we employ an unsupervised machine learning algorithm to identify the decentralized network among lenders in the syndicate market. We study how the network reduces coordination costs and promotes collective actions among lenders when a new syndicate is formed. We find with more participant lenders coming from the same clique as the lead lender does, the syndicate is more likely to go through renegotiations before maturity. Since renegotiations required voting from both lead lenders and participant lenders and are considered as a Pareto improvement to both the borrowing firms and the lending institutions, our findings suggest that the decentralized networks can serve to improve coordination and contracting efficiency.

Chapter 1: Pass Through Myopia: Institutional Investors and Voluntary Disclosure

Theories debate over whether myopia increases or reduces voluntary disclosure. The disagreement is mainly driven by the different assumptions about whether managers' reputation of being uninformed or forthcoming affects disclosing decisions. I exploit the SEC regulation that requires mutual funds to report holdings more frequently as an exogenous shock which increases institutional investors' short-term focus, and study whether and how the issuance of managerial guidance changes. With a differences-in-differences research design, firms with higher affected mutual fund ownership are more likely to issue guidance after the regulatory change. The guidance is more likely to walk down the target at quarter end and increase the probability to meet-or-beat. Validation evidence support the assumption that both the mutual funds and the firms become more myopic: fund flows become more responsive to short-term performance, CEO wealth is more related to current stock price, and firms are more likely to cut R&D when the net income is negative. The increase in voluntary disclosure is of larger magnitude when mutual fund ownership is more transient, when the business environment is more volatile, and when the CEO is more short-viewed. This study demonstrates a causal effect of institutional investors' tunnel vision on portfolio firms' disclosure policy which helps to explain how managers' reputational concern affects disclosures, and has implications for regulations about transparency and mutual funds.

1.1 Introduction

Institutional investors' focus on short-term performance can pressure investee firm managers to meet the short-term targets and affect their decisions, including investment, earnings management, executive compensation, and corporate governance (Bushee, 1998; Matsumoto, 2002; Cadman and Sunder, 2014; Borochin and Yang, 2017). Short-term focus (i.e., myopia) arises due to asymmetric information and agency frictions, driving agents to emphasize on short-term performance even with the cost of long-term value (Stein, 1988; Stein, 1989). It exists in the relationship between firm managers and shareholders, as well as between fund managers and their clients (Ma, Tang, and Gomez, 2019). Fund managers can pick stocks more likely to generate positive returns in the short run, while avoid stocks likely to outperform in the long-term (Shleifer and Vishny,1990; Bushee, Goodman and Sunder, 2019). This paper provides causal evidence of whether and how institutional investors' myopia affects investee firms' voluntary disclosure, by exploiting the SEC regulation requiring mutual funds to report holdings more frequently as an exogenous shock which increases institutional investors' short-term focus.

Recent accounting theories debate whether myopia increases or reduces voluntary disclosures. The disagreement in predictions mainly stems from whether the reputation of being forthcoming or the reputation of being uninformed raises the expected stock price in the future and affects managers' disclosing decisions. In anticipation of how disclosures affect their reputation, managers choose disclosing policy strategically. Reputation of being forthcoming suggests the manager discloses the information whenever they receive it. Reputation of being uninformed indicates when the manager makes no disclosure, it is because he does not have the information. The implications illustrate that either the reputation of being forthcoming or uninformed potentially conveys the concept that the managers are less likely to withhold information. On the one hand, myopia reduces voluntary disclosures (Beyer and Dye, 2012; Aghamolla and An, 2021). Assuming the reputation of being forthcoming raises the future prices, managers with longer-view voluntarily disclose more. More disclosures help to build the reputation of being forthcoming. Myopic managers benefit less from the higher future prices and thus are less likely to disclose. On the other hand, myopia could encourage voluntary disclosures (Einhorn and Ziv, 2008; Bertomeu et al., 2022). In their model, voluntary disclosures are viewed as an implicit commitment to the disclosing policy, as managers face price drop if they voluntarily disclosed earlier and remain silent now.[1] Thus, the reputation of being uninformed raises the expected price, as it indicates the manager does not have the information in either earlier period or now. Reputation of being uninformed is achieved through non-disclosure. Myopic (patient) managers voluntarily disclose more (less), as they benefit less (more) from the reputation of being uninformed. In this scenario, change in disclosure is costly as the market considers the manager more likely to be withholding, and discounts the expected price if she voluntarily disclosed earlier and remains silent now. Myopic managers are less concerned with the discount in future stock price due to change in disclosure, and are more likely to disclose when they are informed. Thus, it is ex-ante a theoretical debate over how the shift in investors' myopia affects voluntary disclosures.

Earlier empirical studies also present conflicting association evidence over the relation between myopia and voluntary disclosures. Cheng, Subramanyam and Zhang (2005) claim myopia is related to more disclosure. They find firms that issue managerial guidance more often tend to outperform short-term targets, and have lower future growth rates. Chen, Matsumoto and Rajgopal

[1] The underlying assumption is that the probability for the manager to be endowed with the information is positively correlated across different time periods. Disclosure in the past suggests higher probability for the manager to have the information at the current stage. If the manager disclosed before and remains silent for now, the market will infer the manager is more likely to withhold the information, which negatively impacts the stock price.

(2011) contend the other direction, by showing firms with more short-term investors are more likely to stop issuing guidance, i.e., voluntarily disclose less. In May 2004, the SEC began requiring mutual funds to report their holdings on quarterly basis. Previously, mutual funds were only required to disclose their holdings semi-annually (Agarwal et al., 2015).[2] The more transparent reporting requirement imposes greater pressure on short-term performance for mutual fund managers, as investors of the funds have more frequent access to the information which they need to assess fund managers and make trading decisions. The increase in transparency motivates fund managers to care more about short-run performance, and to focus more on choosing stocks likely to outperform in the short-term (Bhojraj and Libby, 2005; Gigler et al., 2014; Kraft, Vashishtha, Venkatachalam, 2018). The increase in fund managers' myopia further impacts corporate managers' horizon and decision making in the same direction. Managers of firms with higher mutual fund ownership are likely to behave more myopically (Agarwal, Vashishtha, Venkatachalam, 2018).

To capture voluntary disclosures, I use quarterly issuance of managerial guidance from May 2003 to May 2005 as the primary measure. With certain probabilities, managers tend to be endowed with the information to make earnings guidance across different fiscal periods. Managerial guidance contains information that is most directly related to the forecast of cash flow and firm value, and explains a significant part of the variation of stock returns (Beyer et al., 2010).[3] The main analysis adopts a differences-in-differences research design with firm and year-quarter-

[2] Before the regulation, some funds voluntarily disclose their holdings information through SEC and MorningStar Direct (Ge and Lu, 2006; Schwarz and Potter, 2016). These funds are considered as unaffected by the regulation and are excluded from the main analysis.
[3] Table 1 in Beyer et al. (2010) shows management forecasts on average explains 15.67% of the variation of quarterly cumulative abnormal returns, which ranks the top among information events, including analyst forecasts(6.14%), pre-earnings announcements (3.21%), earnings announcements (2.32%), and SEC filings (1.03%).

post fixed effects around the SEC regulation in May 2004.[4] Firms with higher mutual fund ownership are considered as the firms with larger treatment effects from increase in institutional investor's myopia, as the regulation has larger impact on the firms through a higher percentage of institutional ownership. I find firms with higher mutual fund ownership are more likely to issue and issue more managerial guidance in the post-regulation period, which is consistent with the hypothesis that the reputation of being uninformed impacts disclosing decisions (Einhorn and Ziv, 2008; Bertomeu et al., 2022).

As the most straightforward implication from myopia is firms' tendency to deliver satisfying short-term performance (Stein, 1989; Graham, Harvey and Rajgopal, 2005), my additional analysis investigates whether more affected firms use voluntary disclosures strategically to achieve the short-term targets. Findings suggest that more affected firms become more active in managing market expectations. They tend to issue EPS guidance that falls below the prevailing consensus, i.e., bad news, which increase the probability for the firms to meet-or-beat the quarterly EPS target. In addition, firms with higher mutual fund ownership are more likely to issue guidance that follow the long-term optimistic and short-term pessimistic pattern (hereafter, O-P pattern) after the regulation.

The validity of the research design relies on the assumption that the SEC regulation of more frequent mutual funds reporting increases myopia on the fund side, which is further transmitted to the firm side. To support the assumption, I conduct the following empirical analysis. Sirri and

[4] I include year-quarter-post fixed effects, instead of year-quarter fixed effects. This is due to the regulation was announced on 10 May 2004, and some firms have fiscal quarter ended at 30 Apr 2004. For these firms, their fiscal quarter belong to the second calendar quarter, whereas the pre-regulation stage. I include year-quarter-post fixed effects to better control for the firms with fiscal quarter ended on 30 April 2004. My results are quantitively similar with year-quarter fixed effects.

Tufano (1998) find with lowered search costs, mutual fund flows become more sensitive to past performance. The SEC regulation in May 2004 offers mutual fund investors more convenient access to fund holding information. If mutual fund investors' trading decisions rely more on prior performance, this motivates mutual fund managers to emphasize on short-term performance as they prefer larger total assets under management (Huang, Wei and Yan, 2007; Ma, Tang and Gomez, 2019). Therefore, my first validation analysis explores and verifies that fund flows become more sensitive to the prior relative performance after the regulation. Second, CEO compensation contract is used to align interests between the principal and the agent. For firms with more transient investors, CEO compensations are more related to current stock price (Shin, 2008; Dikolli, Kulp and Sedatole, 2009). If the increase in institutional investors' myopia drives manager to make more short-term decisions, the CEO compensation is expected to have a higher delta. My second validation analysis investigates and confirms that CEO compensation becomes more closely tied to current stock price for the more affected firms. Lastly, firms with more transient institutional investors are more likely to cut R&D to avoid deterioration in performance (Bushee, 1998). If firms make more short-term decisions after the regulation, they are more likely to cut R&D when the performance falls below the expectation. My last validation analysis finds that more affected firms tend to cut their R&D expenditure when the net income is negative after the regulation.

The observed changes in managerial guidance support the hypothesis that higher institutional investors' myopia leads to more voluntary disclosures. Further cross-sectional analysis is adopted to confirm that the increase in firm-side managerial myopia is the underlying mechanism. Bushee (1998) finds institutional investors impose capital market pressure and drive short-term decisions when they are transient. If so, the change in voluntary disclosure should be larger for firms with fund ownership that is more sensitive to changes in earnings. Therefore, the first cross-sectional test examines whether the results vary across whether the affected investors are transient or not. Next, Einhorn and Ziv (2008) predict that the relation between myopia and voluntary disclosure is more pronounced when the business environment is more volatile, and when the CEO is more short-termed. The impact of myopia on voluntary disclosure is intensified with higher business volatility because uncertainty reduces the correlation of the probability of information endowment across different time periods. The lowered correlation further decreases the cost of changing disclosing policy. More short-termed CEOs will increase their disclosures to a larger extent, because they benefit more from the short-term price and are less concerned with lower expected future price. Consistent with the cross-sectional predictions in Einhorn and Ziv (2008), I find the positive relation between myopia and voluntary disclosure is of larger magnitude when the stock price is more volatile, and when the CEO's firm-specific tenure is shorter or when the CEO is not the founder of the firm.

In the robustness check, to mitigate the concern that the May 2004 regulation affects corporate disclosing decisions through purely high institutional ownership rather than increased myopia from the affected mutual funds, I investigate whether the voluntary disclosure is affected by the ownership of mutual funds with reporting frequency unaffected by the regulation (i.e., unaffected mutual funds), and index-tracking mutual funds that do not impose short-term pressure on the portfolio firms (Agarwal et al., 2015; Agarwal et al., 2018). The ex-ante prediction is that neither the unaffected mutual funds nor the index-tracking mutual funds would have change in myopia due to the regulatory change. Thus, ownership by the unaffected or the passive mutual funds should have no impact on corporate voluntary disclosure. Consistent with the prediction, the issuance of managerial guidance does not significantly increase due to the ownership of either the unaffected or the passive funds.

This study makes several contributions. First, it enhances our understanding of how institutional investor's myopia causally affects managers' decisions, with a focus on disclosure. Myopia is one of the unavoidable economic phenomena related to agency frictions, and could be caused by financial reporting (Kraft et al., 2018; Roychowdhury, Shroff and Verdi, 2019). Short-termism has a significant impact on macro economy and is considered the "first-order problem faced by the modern firm" (Edmans, 2009; Terry, 2022). Earlier studies reveal the potential impacts of investors' myopia on management decisions, including lower level of investment, CEO compensation of shorter horizon, more misvaluation and less effective governance, lower level of innovation and more active patenting.[5] This paper extends this literature to study one of the consequences of myopia by showing causal evidence of how investors' myopia influences firms' disclosing policy. Second, this paper deepens our understanding of the determinants of voluntary disclosure, and helps to distinguish which reputational concern (forthcoming or uninformed) matters in disclosing decisions. As noted by the analytical models, voluntary disclosures are affected not only by the assessment of benefits and costs in current period, but also by the discounted benefits and costs upon disclosing decisions in the future, and the inter-temporal probability for the manager to be informed (Einhorn and Ziv, 2008; Beyer and Dye, 2012; Aghamolla and An, 2021; Bertomeu et al., 2022). Theoretically, either the reputation of being forthcoming or the reputation of being uninformed reduces the probability of the manager withholding information from investors, and raise the expected price. Findings in this paper empirically support that the reputation of being uninformed influences managers' disclosures. Lastly, results in this paper highlight how institutional investors affect corporate decisions, and

[5] Related papers include Bushee (1998), Cadman and Sunder (2014), Borochin and Yang (2017), Agarwal, Vshishtha and Venkatachalam (2018) and Glaeser, Michels and Verrecchia (2020).

how regulations on institutional investors have externality on the portfolio firms. It relates to prior research about mutual fund regulations by demonstrating that regulatory change aiming to improve funds transparency also affects corporate information production, CEO compensation, and investment decisions. Collectively, the evidence is useful for regulators to evaluate whether more frequent mandatory reporting is desirable or not, especially on the investor side.

The rest of the paper proceeds as follows. Section 2 discusses related literature and hypotheses. Section 3 presents the sample construction and research design. Section 4 contains empirical results and additional analysis, and Section 5 concludes.

1.2 Literature Review and Hypothesis Development

1.2.1 Mandatory Reporting Frequency and Institutional Investors' Myopia

Myopia stems from the information asymmetry between management and shareholders, capital market pressure, and agents' emphasis on short-term performance. Consequences from agents' short-term focus cannot be ignored, as earlier literature describes myopia as a prevailing phenomenon, the first-order problem for modern firms and creates distortions that slows down the economy growth and lowers social welfare (Graham, Harvey and Rajgopal, 2005; Edmans, 2009; Terry, 2022). Myopia exists even in fully efficient markets (Stein, 1989) and is one of the unavoidable consequences of financial reporting. The review paper by Roychowdhury et al. (2019) summarizes that "*financial reporting can induce myopia.*"

Evidenced by theoretical, experimental, and empirical studies, higher mandatory reporting frequency exacerbates the short-term focus of the agent. Gigler et al. (2014) develop an analytical model to study the benefits and costs associated with required reporting frequency, in which they find more frequent financial reporting brings the benefit for the principals to better detect projects with negative NPV, as well as the cost of higher probability of the agent to emphasize short-term performance. In an experimental setting, Bhojraj and Libby (2005) change the reporting frequency and study how it impacts experienced financial managers' choice between available projects. They find managers are more likely to switch to projects that generate higher short-term earnings and give up projects with higher total cash flows when they are required to report more frequently. Kraft, Vashishtha and Venkatachalam (2018) explore the regulation for US firms to change mandatory financial reporting from annual to semi-annual and then to quarterly, and find firms reduce investments significantly with the increased reporting frequency.

In the mutual fund industry, information asymmetry exists between fund clients and fund managers as fund clients do not possess the same information as fund managers do and cannot perfectly observe managers' actions. Fund managers could focus on metrics that are different from maximizing investors' aggregated long-term wealth, to maximize their compensation or get promoted, e.g., total assets under management and fund flows (Ma, Tang and Gomez, 2019). Mutual fund managers could behave myopically due to the separation of ownership and control, information asymmetry and their incentives to achieve certain short-term targets. In particular, because of the holding costs and the time to correct mispricing, myopic fund managers are more willing to long (sell) stocks with future returns realized in more (less) timely manner (Shleifer and Vishny, 1990; Bushee, Goodman and Sunder, 2019).

In May 2004, aiming to enhance transparency and reduce fraud in the mutual fund industry, the SEC began requiring mutual funds to disclose their holding portfolio on a quarterly basis. Before the regulation, fund managers were only required to report their holdings semi-annually, through Form N-30D. After the regulation, mutual funds need to report their holdings on a quarterly basis with Form N-CSR and Form N-Q. Fund clients have more frequent access to the holdings in the portfolio. Short-term clients, who are performance sensitive, may use the information to evaluate whether the mutual fund managers are capable to satisfy their demand. The shortened reporting interval creates pressure for fund managers to switch to strategies that are more likely to deliver quick results. The more frequent mandatory reporting requirement incentivizes mutual fund managers to emphasize on short-term performance. Fund manager become more likely to pick stocks with a higher probability to be the winner for the short run. Portfolio firms could behave myopically due to the increase in investors' myopia (Gigler et al., 2014; Agarwal et al., 2018).

1.2.2 Institutional Investors' Myopia and Corporate Managerial Myopia

Mutual fund managers' higher emphasis on short-term performance exerts pressures for corporate managers to behave myopically and to signal their ability for superior performance in the short term (Bushee, 1998; Agarwal et al., 2018). This is because mutual fund managers could affect managerial decisions either with being active in management, or with a threat to "vote with their feet" when they are bearish with investees' performance. Compared with holding currently losing stocks for long-term return to wait for price correction, rebalancing portfolios to winning stocks serves as a less costly way for mutual fund managers to maximize profits (Shleifer and Vishny, 1990). Institutional investors with shorter horizon reduce holdings of value stocks due to holding costs and are more likely to pick past winning stocks (Bushee, Goodman and Sunder, 2019).[6]

[6] Bushee et al. (2019) define value stock as stocks with high book-to-market value.

Facing the change in institutional investors' time horizon, corporate managers become more myopic and would change their decision-making and operational policy accordingly. Prior papers find that managerial myopia is more pronounced when institutional ownership is more sensitive to changes in earnings, i.e., transient. When institutional investors are more transient, managers are more likely to cut R&D to avoid earnings decline and engage in earnings management to meet and beat fiscal period targets (Bushee, 1998; Matsumoto, 2002). These empirical findings are further supported with the survey paper by Graham, Harvey and Rajgopal (2005), in which the authors reveal that myopia is a common feature for corporations. The majority of the CFOs interviewed admit that they may sacrifice long-term growth to achieve short-term targets. One of the most important reasons is to deliver fiscal-end performance which does not disappoint investors and avoid decline in the stock market.

Investor horizon also impacts firm valuation, corporate governance, and compensation contract design. Yan and Zhang (2009) find short-horizon institutional investors' reactions to new information largely explain why institutional ownership is positively related to future stock returns. More active trading activities from short-term investors also predict better probability of meeting-or-beating. Cadman and Sunder (2014) find IPO firms with venture capitalists grant short-horizon incentives to CEO compensation. Venture capitalists, who are short-termed as they are more likely to sell their shares after IPO, are capable of influencing compensation structure and benefit from managerial actions to increase the short-term stock price. Borochin and Yang (2017) find transient institutional investors increase future firm misvaluation and are more likely to be related to higher realized volatility and higher executive compensation, compared to dedicated investors.

In short, the May 2004 SEC regulation that requires the mutual funds to report their holdings more frequently could be viewed as an exogenous shock that increases institutional investors' myopia. Due to investors' ability to influence managerial decisions of the investee firms, those with higher ownership by the mutual funds with mandatory reporting frequency affected face larger increases in managerial myopia (Agarwal et al., 2018).

1.2.3 Managerial Myopia and Voluntary Disclosure

Corporate managers become more myopic as institutional investors focus more on short-term performance due to the regulation. How will firm-side voluntary disclosures change? Earlier empirical studies provide association evidence upon the relation. However, the results are mixed, and casual evidence is missing.

On the one hand, managerial myopia could be positively related to the voluntary disclosures. Cheng, Subramanyam and Zhang (2005) find that firms with higher institutional ownership tend to issue managerial guidance more frequently. Firms that issue guidance more frequently behave more myopically as they spend less in R&D, meet or beat quarterly street EPS targets more frequently, and have lower long-term growth.[7] Brochet, Loumioti and Serafeim (2015) predict managers' horizons will be reflected with the time proxies revealed in conference calls. They find firms with less long-term institutional ownership are more likely to emphasize short-term prospectus in conference calls. Higher concentration on short-term prospectus during the conference call is further accompanied with more active earnings management activities, including discretionary accruals, small positive earnings surprises, and loss avoidance. Cadman,

[7] Cheng et al. (2005) noted the endogenous nature in managerial myopia and disclosure policy, as they carefully point out that managerial myopia could simultaneously determines the cut in investment and the frequent provision of earnings guidance.

Heinle and Macciocchi (2022) take newly public firms and firms with CEOs close to retirement as more short-termed and find these firms voluntarily disclose more.

On the other hand, prior empirical studies also document the negative association between myopia and voluntary disclosure. Call, Chen, Miao and Tong (2014) find firms that issue short-term quarterly earnings guidance more frequently are less active in earnings management, either in abnormal accruals, or in discretionary revenues. The relation holds even for firms close to raising external capital. Researchers also look into firms that stopped issuing guidance and study whether the decisions to reduce or cease voluntary disclosures are related to myopia. Huston, Lev and Tucker (2010) do not find significant association between long-term horizon and the decision to stop issuing managerial guidance – firms that stopped issuing managerial guidance do not significantly increase capital expenditure or R&D expense. Chen, Matsumoto and Rajgopal (2011) classify the stopping firms into "quiet stoppers" and "public announcers."[8] Public announcement represents a certain commitment for the firm to stick to the non-disclosure policy. They find with a decrease in long-term investors, firms are more likely to stop issuing managerial guidance.[9] In addition, with more long-term investors, firms would voluntarily make the public announcement that they would stop issuing managerial guidance. Taken together, the findings in Chen, Matsumoto and Rajgopal (2011) suggest investors' myopia reduces voluntary disclosures.[10]

[8] "Public announcers" stop issuance of earnings guidance with voluntary public announcements, while "quiet stoppers" stop issuance of earnings guidance without public announcement.
[9] The result suggests that more intensified short-term focus discourages voluntary disclosures (i.e., issuance of managerial guidance), although the authors interpret the results as managers reduce managerial guidance to attract long-term investors.
[10] Combining the primary results in Houston, Lev and Tucker (2010) and Chen, Matsumoto and Rajgopal (2011), the leading reason for the firms to stop issuing guidance is prior poor performance, and lack of anticipation of good news in the future.

Recent theoretical studies adopt an intertemporal model to study how myopia affects voluntary disclosures, and the opinions diverge. In the analytical model, there are two periods, current period t_0, and future period t_1. The manager's utility U is measured as

$$U = \alpha P_0 + (1 - \alpha)E(P_1)$$

where P_0 represents current price, P_1 represents future price, and α measures myopia. A larger (smaller) α means the manager is more myopic (patient), as his/her utility relies more (less) on current stock price. In each period, the manager learns the information about the firm's earnings with certain probability, and decides whether to disclose or to withhold. In choosing to disclose or not, the managers assess not only how the disclosure changes the current stock price, but also how the market participants update their belief about whether manager is endowed with the information and how the future stock price will be affected as well.

On the one hand, myopia leads to less voluntary disclosure. Beyer and Dye (2012) adopt a dynamic setting in which the probability for the managers to receive the information is independent across different periods, and managers value the reputation of being *forthcoming*.[11] The reputation of being forthcoming helps to raise the expected price in the future. As the market updates the expected future price and the perceived probability for the manager to be forthcoming based on whether the manager discloses or not in current period, patient (myopic) managers would like to disclose more (less) to boost up the expected price in the future. This is because disclosure in the current period helps to build the reputation of being forthcoming.[12] Agamolla and An (2021) build

[11] According to Beyer and Dye (2012), forthcoming refers to managers "*disclosing value-relevant information in a timely manner*."
[12] Beyer and Dye (2012) also predict that managers with a higher probability to behave strategically in the future are more likely to disclose today, especially for bad news. The reason is that strategic managers benefit from the perception of being forthcoming. With the setting of mutual funds increase reporting frequency that exacerbates

a dynamic model where the firm value evolves over time and managers' disclosing policy affects the evolving process. They find that by making voluntary disclosure in the current period, market participants raise their expectation over the non-disclosure price in the future. Thus, non-myopic (myopic) managers are more (less) likely to make voluntary disclosures. Motivated by Beyer and Dye (2012) and Agamolla and An (2021), my first hypothesis is,

H1: Firms with higher affected mutual fund ownership *decrease* voluntary disclosures after the regulation which requires mutual funds to mandatorily report holdings more frequently.

On the other hand, theory predicts that myopia leads to more voluntary disclosures. Einhorn and Ziv (2008) adopt an intertemporal dynamic setting to study the incentives for firms to make voluntary disclosures in which the probability of managers being informed in the current period *t* is positively correlated with the probability of being informed in the future period $t + 1$. When the probability of endowment is positively correlated across different time periods, disclosure in the current period acts as implicit commitment to disclose in the future. This is because the market will assign a higher probability of endowment in the future when they receive disclosure in current period, compared to the case when the firm makes no disclosure. Thus, changing disclosure policies is costly, because the stock price is lower if the firm discloses now and remains silent in the future, compared to the case when the firm remains silent in both periods. Einhorn and Ziv (2008) assume managers benefit from the reputation of being *uninformed*.[13] Non-disclosure helps the manager to build the reputation of being *uninformed*, as impersistent disclosing policy

managerial myopia, the strategic managers' incentive to disclose in current period also decreases. This is because when managers are more myopic, current price is more important in the manager's utility. The benefit for the strategic manager to manipulate the reputation, which comes from the higher expectation of future price, also decreases. Thus, the strategic manager is less likely to issue forecasts when short-term pressure is higher.

[13] Uninformed refers to the managers do not withhold private information that reduces shareholder value.

increases the market's assessment of the manager withholding information, which negatively impacts the price. In anticipation of the potential market updates about the probability of withholding information, patient (myopic) managers voluntarily disclose less (more). Similarly, Bertomeu et al. (2022) also predict that managerial myopia induces less concealment and encourages voluntary disclosures, as myopic managers are less concerned with reputational damage caused by impersistent disclosing policies. Therefore, my second hypothesis is,

H2: Firms with higher affected mutual fund ownership *increase* voluntary disclosures after the regulation which requires mutual funds to mandatorily report holdings more frequently.

1.3 Regulatory Background, Data and Research Design

1.3.1 Regulatory Background

As an amendment to the Securities Act of 1933, the Securities Exchange Act of 1934, and the Investment Company Act of 1940, the May 2004 regulation requires mutual funds to mandatorily report their holdings on a quarterly basis with Form N-CSR (every semi-annual) and Form N-Q (at the end of first and third fiscal quarters). Before the amendment, mutual funds were only required to report their portfolio twice every year, with Form N-30D. The amendment was proposed on Dec 11, 2003, with the intention to deliver "*more streamlined, useful, and understandable*" information to investors.[14] The final rule was adopted on Feb 27, 2004, and became effective starting May 10, 2004.[15] Figure 1 shows the timeline with the regulation.

[14] https://www.sec.gov/rules/proposed/ic-25870.htm
[15] https://www.sec.gov/rules/final/33-8393.htm#IA

After the amendment was proposed, it triggered criticism that the new rule would emphasis too much on short-term performance, although it enhances transparency. In his comment letter, James G. Curtis, who was an experienced long-term mutual fund client, strongly opposed the rule. The reasons include that the change is detrimental to the long-term value maximization.[16] The potential costs of short-term focus driven by the more frequent mandatory reporting seems to be overlooked, or outweighed by the benefits from the perspective of the SEC. The SEC approved the quarterly reporting requirement in Dec 2003 with effective date on May 10, 2004. To further enhances transparency in mutual fund industry, SEC announced another regulation to require funds to report holdings every month, in Oct 2016.[17] Under the most recently updated regime, mutual funds need to report their full portfolio to SEC on monthly basis through Form N-PORT. Only the one filed by the third month of the quarter is publicly available.

1.3.2 Data

I collect data from several public sources, including mutual fund related data from Thomson Reuter S12, CRSP Mutual Fund Database, SEC EDGAR, MFLINKS and firm related data from I/B/E/S, Quarterly Compustat, CRSP, and Compustat ExecuComp.

1.3.2.1 Change in Mandatory Reporting Frequency of Mutual Funds

Following the method described in Agarwal et al. (2015), I obtain mutual funds' date of report holdings from Thomson Reuter S12, CRSP Mutual Fund, and SEC EDGAR, and identify the funds with mandatory reporting frequency changed due to the regulation in May 2004.[18] As

[16] https://www.sec.gov/rules/proposed/s75102/jgcurtis1.txt
[17] https://www.sec.gov/rules/final/2016/33-10231.pdf
[18] I use the holding information from the CRSP Mutual Fund Database, instead of Morningstar as described in Agarwal et al. (2015). This is essentially the same, as noted by Schwarz and Potter (2016) that, "*prior to the fourth quarter of 2007, CRSP obtained its portfolios from Morningstar*."

noted by Ge and Zheng (2006) and Schwarz and Potter (2016), some funds voluntarily report their holdings on a quarterly basis to the data vendors (e.g., S12, CRSP mutual fund, and Bloomberg) before the SEC mandatory requirement in May 2004. These funds are considered unaffected and are excluded from the sample construction. After dropping mutual funds that voluntarily disclose on a quarterly basis before regulation, I rely on the disclosure dates from SEC EDGAR to identify funds with mandatory reporting frequency increased due to the regulation, i.e., affected funds.

I first merge the mutual fund data from the Thomson S12 database and CRSP Mutual Fund database, using MFLINKS. I only retain the funds that are actively managed, open-to-invest, equity-based and report holding information on a semi-annual basis in both data vendors before May 2004. To obtain the mandatory reporting frequency in the pre-stage, I combined the above-mentioned data with the disclosing date from SEC EDGAR by matching the mutual fund ticker from S12 and the ticker from SEC EDGAR. I also manually check the fund name and fund family name in S12 and SEC EDGAR to ensure the quality of matching and exclude the poorly matched observations. I further identify the change in mandatory reporting frequency with the disclosure date reported in SEC EDGAR, i.e., funds reporting holdings on a semi-annual basis before May 2004. The final sample consists of 1,514 affected mutual funds with mandatory report frequency increased due to the regulation, and 555 unaffected mutual funds that voluntarily disclosed on a quarterly basis before the regulation.[19] To further calculate the shares retained by affected mutual funds, I use mutual fund ownership data from S12.

[19] The sample used in Agarwal et al. (2015) contains 1,459 affected funds and 604 unaffected funds. Possible reasons that might explain the difference are the updates with Thomson Reuters S12, CRSP Mutual Fund, and MFLINKS.

1.3.2.2 Managerial Guidance and Firm Characteristics

Managerial guidance data is collected from I/B/E/S, spanning from May 2003 to May 2005. Firm characteristics data is collected from Quarterly Compustat, CRSP, and Compustat ExecuComp. The final sample contains 3,372 unique firms and 25,886 firm-quarter observations.

1.3.2.3 Summary Statistics

MF_Own is the percentage of shares held by the mutual funds with reporting frequency affected by the regulation in May 2004. It is calculated by averaging the firm-quarter shares held by the final sample of 1,514 funds with mandatory reporting frequency increased, scaled by the total shares outstanding of the stock at the corresponding period, from May 2003 to May 2004. When the mutual fund does not provide the shares for the quarter, I use the most recently available holding. *MF_Own* in the main sample has an average (median) of 6.15% (4.70%).[20]

The average likelihood for the firm to issue EPS guidance for the current quarter (any guidance targeting at any fiscal period end) is 21.4% (41.1%). Before the regulation, the average of the probability for the firm to issue current quarter EPS guidance (any guidance targeting at any fiscal period end) is 20.5% (37.5%). After the regulation, the probability is 22.3% (44.6%). Throughout the sample, about 58.9% of the firm-quarter observations issue no guidance, 28.6% issue one guidance, and 12.5% issue more than one guidance.

[20] Compared to the same statistics in Agarwal et al. (2015), the average (median) is 6.60% (4.96%).

1.4 Research Design and Empirical Results

1.4.1 Research Design

I estimate the effect of myopia of voluntary disclosure with the following specification:

$$Guidance_{i,t} = \beta MF\ Own_i \times Post_{i,t} + Control + \alpha_i + \gamma_t + \varepsilon \qquad (1)$$

where *i* represents the firm and *t* represents quarter. *Guidance* is measured by (1) *QEPS*: a dummy equal to one if the firm provides EPS guidance for the current quarter, and zero otherwise; (2) *Any_Guide*: a dummy equal to one if the firm provides any guidance targeting at any fiscal period end during the quarter, and zero otherwise; (3) *Num_Guide*: the number of guidance provided by the firm during the quarter.[21] *MF_Own* is the percentage of shares outstanding held by the mutual funds with reporting frequency affected by the regulation, calculated as the average over one year before the regulation. The sample period spans from May 2003 to May 2005, i.e., one year before and one year after the regulation.[22] *Post* is a dummy variable equal to one if the date of the fiscal quarter end is after May 10, 2004, and zero otherwise. α_i is firm fixed effects, and γ_t is year-quarter-post fixed effects. Standard errors are clustered by firm.[23] With the firm fixed effects, the empirical results capture the within-firm change in the voluntary disclosure from the pre-stage to the post-stage. With the year-quarter-post fixed effects, the model controls for the potential time-

[21] I stack multiple managerial guidance issued on the same day as one guidance.

[22] I use the continuous variable *MF_Own* to capture the differential impact on the firm due to the different level of institutional ownership. My results are robust using an alternative measure by splitting the sample based on whether the institutional ownership is above or below the sample median. Also, I follow Agarwal et al (2015) and use sample period of two years. My results are robust when changing the sample period to eight years, i.e., four years before and four years after the regulatory change.

[23] The empirical results are robust and quantitatively similar with 2-digit SIC fixed effects, and/or standard errors clustered at the 2-digit SIC level.

series changes, which may affect firms with higher mutual fund ownership and firms with lower mutual fund ownership differently, which is irrelevant to the regulation.

The major coefficient of interest is the coefficient β on the interaction term, $MF\ Own_i \times Post_{i,t}$. The coefficient captures the incremental change on firms' voluntary disclosures caused by the higher mutual fund ownership, due to the SEC regulation. In other words, it is the differences-in-differences estimate of how the increase in mandatory fund reporting frequency affects corporate disclosing decisions, through the mutual fund ownership. I control for firm characteristics that are likely to affect managers' voluntary disclosures, including log(total assets), MTB, leverage, ROA, RD dummy, and the number of analysts following. Firms that are larger, make higher profits, have higher MTB, invest in R&D, and with more analysts following tend to make more voluntary disclosures. All the control variables are from last quarter.

1.4.2 Main Results

1.4.2.1 Managerial Guidance

I start with the main analysis focusing on how the more frequent reporting of mutual fund holdings impacts corporate voluntary disclosure differently based on the affected mutual fund ownership. The coefficient of interest is on the interaction term *MF_Own* × *Post*. Based on the predictions in H1 and H2, if institutional myopia makes managers less (more) likely to make voluntary disclosures, the coefficient is predicted to be significantly negative (positive). Table 2 provides the results from estimating equation (1). In column (1) and (2), the dependent variable *QEPS* measures the likelihood for the firm to issue an EPS guidance for the current quarter. In column (1), I present the estimate of the model with no control variables. In column (2), I include variables to control for firm characteristics. In both columns, the coefficient captures the incremental effect on the likelihood of issuing EPS guidance for the current quarter due to higher mutual fund ownership, after the regulation. The estimates in both column (1) and (2) are positive, statistically significant at the 1% level, and are close in magnitude. Looking into column (2), the coefficient is 0.200 with p-value < 0.01. The economic magnitude suggests that, in the post period, one standard deviation increase in mutual fund ownership is associated with a 5.51% increase in the likelihood of issuance EPS guidance for the current quarter. The results indicate that the increase in the likelihood to issue quarterly EPS guidance after the regulation is positively related to the mutual fund ownership. That is, higher level of institutional investors' myopia leads to more voluntary disclosures through intensified managerial myopia, which is the same as the prediction in Einhorn and Ziv (2008).

In column (3), the dependent variable is *Any_Guide*, which represents the likelihood for the firm to issue any type of guidance (including but not limited to EPS, e.g., net income, CAPEX, etc.) aiming at any horizon during the quarter. The coefficient on the interaction term *MF_Own* × *Post* captures the incremental likelihood for the firm to issue any guidance after the regulation caused by higher mutual fund ownership. The estimate of the coefficient is equal to 0.599 and statistically significant at the 1% level. In column (4), the dependent variable is *Num_Guide*, which is the count of guidance issued during the quarter by the firm. Multiple guidance issued by a firm on the same day are stacked and counted as one. The coefficient of interest captures the incremental change on the number or frequency for the firm to issue guidance during the quarter. It is estimated to be 1.548 and statistically significant at the 1% level.

Within Table 2, the estimates of the coefficient of interest are consistently positive and statistically significant at the 1% level for all the outcome variables across all the columns. In the untabulated tests, I (1) change firm fixed effects to 2-digit SIC industry fixed effects, or (2) change the standard error clustering to 2-digit SIC or year-quarter level, and the results remain robust and quantitatively similar. The findings suggest that higher mutual fund ownership is associated with higher likelihood to issue guidance, as well as larger number of guidance, in the post-regulation stage compared to the pre-regulation stage. The findings are consistent with the prediction that myopia encourages more voluntary disclosures from the managers, i.e., H2. By exploring the setting with higher mutual fund reporting frequency and managerial guidance, I find managers' reputational concern of being uninformed affect disclosing decisions.

The identification of the differences-in-differences research design requires the parallel assumption to hold. That is, without the SEC regulation, firms with different levels of mutual fund ownership should have similar trends in providing voluntary disclosures, after considering control variables and fixed effects. I conduct an empirical test by modifying the equation (1) to test if the assumption holds. To do so, I construct four *Pre* indicators to represent the periods before the regulation, and four *Post* indicators to represent the period after the regulation. *Pre_4 (Pre_3, Pre_2, Pre_1)* indicates firm-quarter observations with fiscal periods ending from May 31, 2003 to June 30, 2003, (July 31, 2003 to Sep 30, 2003 Oct 31, 2003 to Dec 31, 2003, Jan 31, 2004 to Apr 30, 2004).[24] *Post_1* (*Post_2*, *Post_3*, *Post_4*) indicates the fiscal period ending from May 31, 2004 to June 30, 2004 (July 31, 2004 to Sep 30, 2004, Oct 31, 2004 to Dec 31, 2004, Jan 31, 2005 to Apr 30, 2005). I include each of the fiscal period end indicators in equation (1), together with the interaction term of each indicator with the *MF_Own*, i.e., *MF_Own* × *Pre* and *MF_Own* ×

[24] This is because the SEC regulation was in May 2004. Similar reason applies to the Post_1, which identifies fiscal periods ending on May 31, 2004 to June 30, 2004.

Post. Coefficient estimates related to Pre_4 are dropped due to multi-collinearity, and are used as benchmarks.

Results are reported in Appendix B. Column (1) presents the estimates of the coefficients with firm fixed effects, but not with time fixed effects.[25] Column (2) presents the estimates of the coefficients with both firm fixed effects and time fixed effects. Figure 2 plots the coefficient estimates from results in Appendix B, i.e., the differences-in-differences coefficient estimates of the likelihood of issuing quarterly EPS guidance in each of the pre- and post-regulation periods with 95% confidence interval. As shown in Appendix B and Figure 2, the coefficient on each of the interaction term *MF_Own* × *Pre* is not statistically or economically significant. There is not a clear and significant trend of voluntary disclosure due to the difference in affected mutual fund ownership in the pre-regulation stage. The results are consistent with the assumption that issuance of managerial guidance follows the parallel trend before the regulation. One may notice that the coefficient estimates on the interaction term *MF_Own* × *Pre_1* is equal to 0.267, and statistically significant at 5% level. Due to the SEC amendment was first proposed in Dec, 2003, approved in Feb, 2004 and finally became effective in May 2004. The positive coefficient reflects the anticipation effect during the first quarter of 2004. Yet, there is not a clear trend in the voluntary disclosure conditional on *MF_Own* in the pre-regulation stage. More importantly, each of the estimates on coefficient on the separate item *MF_Own* × *Post* is statistically positive. The results suggest that the increased involuntary disclosure is significant and persistent following the regulation.

[25] Please notice that in this specification the estimate on each of the *Pre* and *Post* indicator are not shown for simplicity.

1.4.2.2 Good News, Bad News and Neutral News

The main results show that due to increase in institutional investors' myopia, firms issue more voluntary disclosures. Prior studies show that managerial preference closely affects whether the voluntary disclosure is optimistic or pessimistic. For example, when it is close to insiders buying shares, forecasts tend to be bad-news based to lower the price (Cheng and Lo, 2006). On the other hand, Lang and Lundholm (1993) show that firms' disclosures are more active and more optimistic prior to equity offerings. With short-term focus transmitted from institutional investors to investee firms, meeting short-term targets to boost stock prices becomes a more critical issue for managers (Graham, Harvey and Rajgopal, 2005). With the increase in voluntary disclosure, will firms exploit voluntary disclosures to increase the likelihood of meeting-or-beating quarter-end target?

So far, how institutional investors' myopia affects the good news or bad news has not been fully answered by current literature.[26] Edmans, Fang and Lewellen (2017) find firms that are myopic driven by CEO compensation incentive tend to issue good news, which facilitate managers to sell options at favorable prices when the option become exercisable. Whereas, myopic managers care more about meeting short-term targets and firms significantly increase the probability of meeting-or-beating by releasing earnings guidance that are below consensus (Matsumoto, 2002; Cotter, Tuna and Wysocki, 2006). Ex-ante, it is unclear whether firms are more likely to issue good news or bad news due to increased institutional investors' myopia.

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[26] A recent theory paper by Jia and Menon (2022) assumes that shareholders have the power to control the firm and determine disclosing decisions and studies how managers use voluntary disclosures to avoid over-intervention from shareholders. They predict that when shareholders are "*not too short-termed,*" managers disclose both good news and bad news. When the shareholders are *"highly short-termed,"* managers only disclose good news and withhold bad news.

To answer this question, I investigate whether managers become more optimistic or more pessimistic in their guidance for the current quarter after the regulation. I rely on whether the guidance is above or below the prevailing consensus. To be more specific, *Good_News* (*Bad_News*, *Neutral_News*) is a dummy variable equal to one if the last EPS guidance for the current quarter before the earnings announcement is above (below, indistinguishable from) the prevailing consensus, and zero otherwise. It reflects manager's incentive to influence the street consensus forecasts by the analysts before the actual performance of the quarter is released. If the manager prefers a target that is more beatable, he/she is more likely to use the most recent guidance to walk down the expectations.

Results about the last guidance being optimistic or pessimistic are shown in Table 3. Column (1) contains the result with *Good_News* as the dependent variable. The coefficient on the interaction term is positive yet statistically insignificant at a conventional level. Column (2) contains the result with *Bad_News* as the dependent variable. The coefficient of interest is equal to 0.141 and statistically significant with p-value smaller than 0.01. The result suggests that, after the regulation firms with higher mutual fund ownership are significantly more likely to issue a guidance that is below the prevailing consensus, which could walk down analysts' expectations. Column (3) presents the results with *Neutral_News* as the dependent variable. The estimate of the coefficient is statistically indifferent from zero. The result suggests that the probability for the firm to issue guidance that is indistinguishable from the consensus is unaffected by the regulation. Taken together the earlier findings about the issuance of managerial guidance and O-P pattern, myopic managers become more strategic in choosing voluntary disclosures.

1.4.2.3 Meet-or-Beat Analysis

Does the higher likelihood of issuing managerial guidance or bad news increase the probability for the firm to satisfy current period performance? In this subsection, I explore the consequences of the issuance of managerial guidance by examining whether the firm meets-or-beats quarterly EPS target. In conjunction with the findings in the previous sections that myopic managers become more likely to issue EPS guidance for the current quarter, especially pessimistic EPS guidance before the announcement of the actual earnings. This analysis helps to better understand managers' motives in changing disclosing decisions. To investigate whether the issuance of guidance affects the chances for managers to achieve the EPS target, I use the model below:

$$MB_{i,t} = \beta_1 \times MF\ Own_i \times Post_{i,t} \times Guidance_{i,t} + \beta_2 \times MF\ Own_i \times Post_{i,t} + \beta_3 \times MF\ Own_i \times Guidance_{i,t}$$
$$+ \beta_4 \times Post_{i,t} \times Guidance_{i,t} + \beta_5 \times Guidance_{i,t} + Control + \alpha_i + \gamma_t + \varepsilon \quad (2)$$

The dependent variable, *MB*, is a dummy equal to one if the actual EPS of the quarter is higher than or equal to the street consensus before the earnings announcement, and zero otherwise. $Guidance_{i,t}$ takes four formats, $QEPS_{it}$, $Good\_News_{it}$, Bad_News_{it}, and $Neutral\_News_{it}$ which correspond to the scenarios of whether the firm offers EPS guidance for the current quarter, and whether the last EPS guidance for the current quarter is above, below or indifferent from the prevailing street consensus. The coefficient of interest is β_1 on the triple interaction term, $MF\ Own_i \times Post_{i,t} \times Guidance_{i,t}$, which measures that relative to firms that do not issue the certain type of guidance, the incremental probability to meet-or-beat the EPS target for firms that issue the guidance, due to the difference in mutual fund ownership after the regulation.

Table 4 presents the results from estimating equation (2). Column (1) is about the change in the likelihood to meet or beat the EPS target when the firm issues EPS guidance for the current quarter. The coefficient on the triple interaction $MF\ Own_i \times Post_{i,t} \times QEPS_{i,t}$ term is positive and equal to 0.378, yet marginally insignificant at a conventional level with p-value equal to 0.11. Column (2) and (3) are about the change in the likelihood to meet or beat the EPS target when the firm issues the most recent current-quarter EPS guidance which is above the consensus or below the consensus, separately. The coefficient on the triple interaction term $MF\ Own_i \times Post_{i,t} \times Good\_News_{i,t}$ in column (2) is 0.041 with p-value equal to 0.918. This suggests that, by issuing good news right before the earnings announcement, there is no significant change in the probability of meeting or beating the EPS target. Looking at column (3), the coefficient on the interaction term $MF\ Own_i \times Post_{i,t} \times Bad\_News_{i,t}$ is 0.716 with p-value equal to 9.8%. The finding suggests that by issuing a pessimistic guidance before the earnings announcement, firms with higher mutual fund ownership are more likely to meet-or-beat the street target of the current quarter after the regulation. Column (4) presents the evidence on meeting-or-beating when the most recent quarterly EPS is indifferent from the prevailing consensus. The coefficient on the interaction term $MF\ Own_i \times Post_{i,t} \times Neutral\_News_{i,t}$ is 0.176 and statistically indifferent from zero.

In short, the results about issuance of good news or bad news and meeting-or-beating suggest that firms with higher mutual fund ownership are more likely to issue pessimistic guidance and walk down market expectation before revealing the actual performance. This strategy significantly increases the probability for them to meet-or-beat the short-term market expectations.

1.4.2.4 Optimistic-Pessimistic Pattern

The results in earlier subsections reveal that more intensified mutual fund myopia drives firms to make more voluntary disclosures, as well as bad news to increase the meet-or-beat probability. In this subsection, I further investigate whether myopic managers use the guidance as a strategy to influence analysts' and market participants' expectations. To be more specific, are the guidance more likely to follow the long-term optimistic short-term pessimistic (O-P) pattern? Previous studies show that managers have the incentive to provide optimistic forecasts of longer terms to raise the stock price and provide pessimistic forecasts of shorter terms to increase the probability of meeting and beating the targets (Richardson et al, 2004; Ke and Yu, 2006). Myopic managers are expected to be more likely to issue guidance that tend to follow the O-P pattern for at least two reasons. First, compared to patient managers, their benefits from a higher current stock price are higher. Second, myopic managers have higher incentives to have a beatable target as they are more concerned about delivering short-term fiscal performance that satisfies investors' expectations.

To capture the phenomenon, I construct two variables *Pattern_Qtr* and *Pattern_Ann*. *Pattern_Qtr* (*Pattern_Ann*) is a dummy variable equal to one if the quarterly (annual) EPS guidance of the longest horizon is above the quarterly (annual) EPS guidance of the shortest horizon, and zero otherwise.[27] *Pattern_Qtr* (*Pattern_Ann*) equal to one means that the managers tend to be more strategic in their guidance, raising the market expectations of performance for the further future, while making the closer EPS target more beatable. I employ *Pattern_Qtr* and *Pattern_Ann* as the dependent variables in the analysis the same as equation (1). If myopic

[27] To construct the measure, I require the guidance targeting at the longest horizon and the shortest horizon made on the same day.

managers are more likely to follow the O-P pattern in their guidance, the coefficient β is expected to be positive.

Empirical results are presented in Table 5. Column (1) shows the results with *Pattern_Qtr* as the dependent variable. The coefficient on the interaction term *MF_Own* × *Post* is equal to 0.091, with p-value smaller than 0.01. The finding suggests that after the regulation, quarterly EPS guidance from firms with higher mutual funds is significantly more likely to follow the O-P pattern. In column (2), the dependent variable is *Pattern_Ann*, which captures the possible O-P pattern in annual EPS forecasts. The coefficient on the interaction term is positive and equal to 0.015, whereases not statistically significant at a conventional level. In column (3), I create the dummy variable *Pattern_Any*, which takes the value of one if any of the *Pattern_Qtr* and *Pattern_Ann* is equal to one, and zero otherwise. The estimate of the coefficient on the interaction term is equal to 0.0850, with p-value equal to 0.015. The result shows that firms with higher mutual fund ownership become significantly more likely to issue guidance that follows the O-P pattern after the regulation.

1.4.3 Validation Analysis

The validation of my hypotheses relies on both mutual fund managers and firm managers become more short-term focused with the regulation requires mutual fund to mandatorily report holdings more frequently. In this section, I provide evidence to validate the argument, by investigating how the regulation affects mutual fund flow sensitivity to relative performance, corporate CEO compensation design, and firms' likelihood to cut R&D. Empirical evidence in this section enhances our understanding. Empirical evidence in this subsection aims to provide support on whether fund managers become more myopic after the regulation, and whether the incentives for corporate managers change in the similar direction when institutional investors become more short-term focused.

1.4.3.1 Changes in Mutual Funds Flows Sensitivity to Relative Performance

My first validation analysis studies whether mutual fund flows become more responsive to short-term relative performance after the regulation. Sirri and Tufano (1998) find reduced search costs enhances the positive correlation between fund prior performance and flows. The May 2004 regulation reduces search costs for fund investors. If fund flows become more sensitive to prior short-term relative performance, fund managers are more likely to pursue short-term returns for larger assets under management and better compensation. To empirically detect how the higher reporting frequency affects the degree to which fund investors respond to short-term fund performance, I construct the sensitivity of fund flows to prior relative performance following Huang, Wei and Yan (2007) and study how the sensitivity changes due to the regulation.

My sample is constructed at the fund-month level. For each fund-month, I investigate how the monthly fund flow correlates to the relative performance over the last month. Mutual fund flow, *Flow*, is calculated as $(TNA_{i,t} - TNA_{i,t-1} \times (1 + R_t))/TNA_{i,t-1}$, where i represents the fund and t represents the month. TNA is the total net asset under management. R_t is the monthly return for the fund. Fund relative performance, *Return Rank,* is calculated as the percentage ranking of funds' prior monthly return within the respective investment objective categories This is a continuous value from zero (worst) to one (best).[28] Funds are classified into terciles based on

[28] I use the Lipper Objective Code in CRSP Mutual Fund Database.

the *Return Rank*. The variable, *Performance*, is constructed based on the tercile of the *Return Rank*, which takes value equal to one (three) when the *Return Rank* falls into the lowest (highest) tercile.

I begin the analysis with estimating the relation with the following equation, separately for funds with reporting frequency affected by the regulation (i.e., *treated funds*), as well as funds unaffected by the regulation (i.e., *control funds*):

$$Flow_{i,t} = \beta \times Performance_{i,t} \times Post_{i,t} + Control + \alpha_i + \gamma_t + \varepsilon \qquad (3)$$

I include variables from the last month including fund management fee, total net assets under management, turnover ratio, and prior fund flow. I control for fund (α_i) and year-month (γ_t) fixed effects in the analysis. Standard errors are clustered at the fund level. The variable, *Post*, is equal to one for observations on and after May 2004, and zero otherwise. The coefficient of interest is β, which estimates the change in how mutual fund flow reacts to relative performance in the post period, compared to the pre period. The observations are at the fund-month level and span from May 2003 to May 2005. If investors of mutual funds become more transient after the regulation, β is expected to be positive.

Table 6 presents the results of how the relative performance affects mutual fund flow changes before and after the regulation. Column (1) shows the results for mutual funds with mandatory reporting frequency increased by the regulation i.e., the affected funds. The coefficient on the interaction term *Performance* × *Post* is 0.46%, and significant at 1%. This suggests that for the funds with mandatory reporting frequency affected, monthly fund flows become more sensitive to the relative performance from the prior month after the regulation. Investors' trading of the mutual funds become more transient and more responsive to the short-term performance. They are more likely to purchase the funds with better relative performance, and sell funds with unsatisfying relative performance. This provides supporting evidence that affected fund managers become more concerned with short-term performance after the regulation, as one of the most important determinants of their compensation is the total net assets under management.

In column (2), I investigate whether similar results hold for the funds that already reported their holdings on a quarterly basis before the regulation, i.e., mutual funds with reporting frequency unaffected. The coefficient is 0.12% and statistically insignificant at the conventional level (p-value equal to 0.42). The results suggest that for funds that have already been reporting their holdings on a quarterly basis before the regulation, the mandatory reporting regulation does not induce investors of the mutual funds to be more sensitive to the short-term performance.

In column (3), the sample contains both the affected funds and the unaffected funds. I use the triple interaction *Performance* × *Treat* × *Post* to study whether the change in fund flow sensitivity to relative performance is significantly different between the treated funds and the control funds. Although the coefficient on the triple interaction term is positive at 0.24%, it is not statistically significant at a conventional level (p-value equal to 0.22). The result suggests that although the increase in sensitivity is higher for the treated funds, it is not statistically different from the change in the unaffected mutual funds.

In column (4), I conduct an analysis similar to column (3) focusing on funds without institutional shares, i.e., funds that only have retail shares. Compared to institutional investors, retail investors' focus on short-term performance is considered to be more affected by the regulation change in transparency. There are potentially at least two reasons why retail investors are more responsive to short-term performance after the regulation. First, institutional fund investors may have access to fund holdings on more frequent basis before the regulation. Second, retail investors are considered to be more short-sighted and more likely to make investment decisions based on past short-term performance (Barber, Odean, and Zhu, 2009). Thus, the shift in myopia is larger for retail investors relatively to the shift for institutional investors. I rely on fund names from the CRSP Mutual Fund database to identify whether the share class is an institutional share or not.[29] Retail funds are the funds with no share class identified as belonging to the institutional shares. Results in column (4) find that, within the funds with no retail shares, the change in the sensitivity for the affected mutual funds is significantly larger than the change for the unaffected mutual funds. The coefficient on the triple interaction term *Performance* × *Treat* × *Post* is 0.43% and statistically significant at the 10% level. To briefly sum up, the results suggest that the investors of the affected mutual funds become more myopic after the regulation, especially for the funds that only have retail shares. The increase in fund investors' myopia is supported by the empirical evidence that fund flows are more sensitive to the short-term relative performance after the regulation. This finding provides corroborating evidence to support the argument that the mutual fund managers become more myopic after the regulation – they would like to boost short-term performance to attract higher fund inflows.

1.4.3.2 Change in CEO Compensation

My second validation analysis investigates how CEO compensation structure changes when investors are required to report holdings more frequently. Institutional investors influence CEO compensation design to ensure the interests are better aligned. Firms with transient institutional ownership tend to have CEO compensation more closely related to current stock price

[29] I identify funds with institutional share class if the name contains keywords including institutional class, class I, class Y, institutional share, institutional plus, institutional select.

(Shin, 2008; Dikolli, Kulp and Sedatole, 2009). If the increase in institutional investors' myopia is transmitted to the firm side, firms with higher mutual fund ownership is expected to have CEO's wealth to be more reliant to current stock price. The change in compensation also suggests that managerial decisions would be more short-viewed in the post stage. Empirically, a higher compensation delta (Core and Guay, 2002; Coles, Daniel and Naveen, 2006) means CEO's wealth is more sensitive to current stock price. When manager's compensation delta is higher, manager is more likely to adopt discretionary earnings to keep the current stock price high and reduce risk-taking (Cheng and Warfield, 2005; Bergstresser and Philippon, 2006; Brockman, Martin, and Unlu, 2010). If change in institutional investors drive corporate managers to become more myopic after the regulation, CEO compensation is expected to have higher delta to better reflect the change in shareholder's interest.

I use the equation below to estimate the change in CEOs' compensation before and after the SEC regulation in May 2004 based on the affected mutual fund ownership:

$$Delta_{i,t} = \beta \times MF\ Own_i \times Post_{i,t} + Control + \alpha_i + \gamma_t + \varepsilon \qquad (4)$$

Delta is a 1,000-dollar change in the executive's wealth for a 1% change in stock price, following the construction in Core and Guay (2002) and Coles et al. (2006). The unit of observation is firm-year, and the sample period spans from May 2000 to May 2008, i.e., four years before the regulation and four years after the regulation. Coefficient β captures how CEO compensation *Delta* changes due to mutual fund ownership after the regulation. The coefficient β is expected to be positive as managers' wealth will be more sensitive to current stock price.

The results are presented in Table 7. The estimate of the coefficient β is equal to 1.617 and statistically significant at the 1% level. The finding is consistent with the expectation that CEO compensation becomes more closely related to current stock price due to the increase in shareholder myopia. The change in compensation structure supports the argument that an increase in institutional investors' myopia would affect managers in the similar way through compensation contract design.

1.4.3.3 Cut in R&D

The third validation analysis looks into whether firms with higher mutual fund ownership become more likely to cut R&D expense when the performance is unsatisfying after the regulation change. Reduction in R&D is considered as a reflection of managerial myopia. Bushee (1998) find when the institutional investors are more transient, the firm is more likely to cut R&D to avoid decline in earnings. Due to the low coverage of R&D in the quarterly Compustat, I focus on the change in R&D at the firm-year level. The dependent variable of interest is *Cut_RD*, which is a dummy variable equal to one if the R&D expense scaled by the total assets for current year is lower than that of last year, and zero otherwise. I capture higher performance of the firm with variable, *Neg*, which is a dummy variable equal to one if the net income is negative, and zero otherwise. I use the following model to estimate the change in likelihood for the firms with higher mutual fund ownership to reduce their annual R&D expense after the regulation:

$$Cut\_RD_{i,t} = \beta_1 MF\ Own_i \times Post_{i,t} \times Neg_{i,t} + \beta_2 MF\ Own_i \times Post_{i,t} + \beta_3 MF\ Own_i \times Neg_{i,t} + \beta_4 Post_{i,t} \times Neg_{i,t} + \beta_5 Neg_{i,t} + Control + \alpha_i + \gamma_t + \varepsilon \qquad (5)$$

The observations are at the firm-year level. Control variables include firm size, profit, BTM, R&D dummy, price and number of analysts following from the prior year. I include firm (α_i) and year (γ_t) fixed effects. Standard errors are clustered at the firm level. The coefficient of interest is β_1, which estimates the difference in likelihood for firms with higher mutual fund ownership to cut the R&D expenditure when the net income is negative, after the regulation.

Table 8 presents the results from estimating equation (5). Column (1) shows the results of the sample period from May 2003 to May 2005. Column (2) looks at a longer period, from May 2000 to May 2008, four years before the regulation and four years after the regulation. Both columns show that the estimated β_1 is significantly negative and statistically significant at conventional levels. Take column (1) for example, the coefficient is equal to 1.566, with p-value equal to 2.9%. The results suggest that, after the regulation, firms with higher mutual fund ownership are more likely to cut their R&D expenditures when the net income is negative. The increased likelihood of cutting R&D supports the prediction that firms with higher mutual fund ownership tend to focus more on the short-term performance after the regulation, even at the cost of sacrificing long-term profitability.

1.4.4 Cross-Sectional Analysis

Preliminary findings in section 4.1 supports the prediction in Einhorn and Ziv (2008) and Bertomeu et al. (2022), that more myopic institutional investors encourage the firms to make more voluntary disclosures, through the more intensified corporate manager's myopia. Analysis in section 4.2 presents the findings that both fund managers and corporate managers become more short-term focused after the regulation. In this section, I conduct three sets of cross-sectional tests to provide additional evidence to confirm that the increase in voluntary disclosure is driven by the increase in corporate managerial myopia. The intuition behind the cross-sectional analysis is that if corporate managerial myopia is the underlying mechanism that encourages voluntary disclosure, the effect should be of larger magnitude when the firm is expected to be more responsive to institutional investors' myopia caused by the mandatory reporting frequency regulation.

I use a model as below to conduct the cross-sectional analysis:

$$QEPS_{i,t} = \beta_1 \times MF\ Own_i \times Post_{i,t} \times CX_i + \beta_2 \times Post_{i,t} \times CX_i + \beta_1 \times MF\ Own_i \times Post_{i,t} + Control + \alpha_i + \gamma_t + \varepsilon \qquad (6)$$

where CX_i refers to the cross-sectional variables measured prior to the regulation and will be discussed in detail in the each of following subsections. The coefficient of interest is β_1 on the triple interaction term $MF\ Own_i \times Post_{i,t} \times CX_i$, which estimates the incremental change in voluntary disclosures due to higher mutual fund ownership from before to after the regulation, when the firm is expected to be more responsive to the increase in myopia. In short, β_1 is the triple-difference estimate and shows how much larger effect the increased mutual fund mandatory reporting has on corporate voluntary disclosures for the more sensitive firms after the regulation.

1.4.4.1 Capital Market Pressure

My first cross-sectional analysis focuses on the short-term capital market pressure imposed by transient institutional investors. Managers are likely to put a higher weight on short-term performance when the institutional ownership is more sensitive to changes in earnings (Stein, 1989; Bushee, 1998). Later empirical evidence also finds transient investors are related to managers' incentive to avoid missing the EPS target (Matsumoto, 2002) and the association between CEO compensation horizon and earnings management (Chen et al., 2015). If transient investors play a more important role in shortening managers' horizons, I expect the change in voluntary disclosure to be stronger for firms with mutual fund ownership that is more sensitive to change in earnings.

Following Bushee (1998), I proxy for capital market pressure with *Sensitivity*, which relates to the change in firm-level mutual fund ownership due to change in earnings.[30] *Sensitivity* is a dummy variable equal to one if the mutual fund ownership is more related (i.e., above the median) to the changes in the current quarter actual EPS compared to last quarter, and zero otherwise.[31]

Column (1) in Table 9 presents the result of using *Sensitivity* as the cross-sectional variable. The coefficient on the triple interaction term is expected to be positive, as firms with mutual fund ownership more sensitive to changes in earnings are predicted to be more responsive to increases in investors' myopia. The estimate of the coefficient on the triple interaction term $MF\ Own_i \times Post_{i,t} \times CX_i$ is 0.26 and statistically significant at the 10% level. It suggests that when the mutual fund ownership is more sensitive to changes in earnings, firms with higher mutual fund ownership are more aggressive in increasing voluntary disclosures after the regulation.

1.4.4.2 Business Environment

In finding myopia leads to more voluntary disclosures, Einhorn and Ziv (2008) also find that the cost of changing the voluntary disclosure policy is lower when the firm operates in a more volatile business environment. When the environment is more volatile, the positive correlation of

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[30] I only focus on the ownership of mutual funds with reporting frequency affected by the regulation.
[31] Averaged across the four quarters before May 10, 2004.

the probability for the managers to be endowed with the information across different periods is lower. The market assigns a relatively lower cost when the firm changes its disclosing policy. This is because, compared to the scenario when the business is stable (i.e., the probability of being endowed is highly positively correlated), a non-disclosure during volatile times is more likely to be driven by being uninformed, instead of withholding. Thus, the positive relation between myopia and voluntary disclosure is stronger when the operation is more volatile. To test this prediction, I construct the variable *Vol*, which is the standard variation of monthly stock return measured over the 24-month window before the regulation. *Vol* captures the business environment uncertainty. For firms with higher stock return volatility, they are expected to be more responsive to the increase in myopia. Thus, β_1 is predicted to be positive.

Column (2) of Table 9 presents the results of equation (6), with *Vol* as the cross-sectional variable. The coefficient on the triple interaction term is 2.53 and statistically significant at 5%. The result suggests that when the firms operate in a more volatile environment, firms with more shares held by the mutual fund increase their voluntary disclosures to a larger degree after the regulation. The finding is consistent with Einhorn and Ziv (2008) and provides support that managerial myopia is the underlying mechanism by which higher institutional myopia increases the likelihood of voluntary disclosure.

1.4.4.3 CEO Characteristics

Einhorn and Ziv (2008) further find the myopia has a larger impact on voluntary disclosure when the manager has a shorter horizon. A more myopic manager cares more about current stock price, and is less concerned with the discount of the future non-disclosure price. To measure CEO incentive, I use CEO *Short_Tenure* and *Non_Founder* to capture the likelihood for the CEO to be short-termed. *Short_Tenure* is a dummy variable equal to one if the tenure of the CEO is lower than the sample median, and zero otherwise.[32] For a CEO with shorter tenure and less firm-specific experience, there is more uncertainty about his/her ability, and he/she faces a higher probability of being punished or replaced for poor performance (Dikolli, Mayew and Nanda, 2014). Thus, CEOs with shorter tenure are predicted to be more active in increasing voluntary disclosures than CEOs with longer tenure. β_1 is expected to be positive. *Non_Founder* is a dummy variable equal to one if the current CEO is not the founder of the firm, and zero otherwise. Firms run by founders are perceived to be more visionary, as they invest more in R&D (Fahlenbrach, 2009), suffer less from agency problems, and have higher firm value (Villalonga and Amit, 2006). Therefore, the relation is predicted to be stronger when the CEO is not the founder of the firm, i.e., β_1 is predicted to be positive. Both *Short_tenure* and *Non_founder* are measured with the most recent data before the regulation in May 2004, from ExecuComp.[33]

Column (3) in Table 9 presents the result from estimating equation (6) with whether the CEO is of shorter tenure as the cross-sectional variable. The estimate on the coefficient β_1 is equal to 0.524, with p-value equal to 5.0%. Column (4) presents the cross-sectional result on whether the CEO is a non-founder of the firm. The estimate is equal to 0.996 and p-value is equal to 6.7%. Collectively, the findings suggest that, after the regulation, the increase in the likelihood of issuing quarterly EPS guidance is larger when the CEO is more likely to short-termed. The higher level of

[32] In this test, the sample median of CEO tenure is five years.
[33] The number of observations drops due to coverage in ExecuComp.

myopia could be either due to higher concern about CEO's ability or due to lack of a long-term view.

In brief, findings in section 4.3 point out that the relation between mandatory portfolio reporting and corporate voluntary disclosures is more pronounced when the firm is perceived to be more affected by the increase in myopia. That is, when the mutual fund ownership is more sensitive to changes in earnings, when the stock returns are more volatile, and when the CEO is more short-termed. Evidence in this section supports the argument that increase in corporate myopia is the reason why more frequent mutual fund reporting drives more firm-side voluntary disclosures.

1.4.5 Changes in Characteristics of Managerial Guidance

In this section, I investigate the how characteristics of managerial guidance (including the precision and the horizon of guidance) change after the regulation due to the different levels of mutual fund ownership. Investing guidance characteristics aims to provide more evidence and comprehensive description on how the content of guidance changes in the post-regulation stage when mutual fund ownership is higher. As motivated and suggested by Leuz and Wysocki (2016) and Roychowdhury et al. (2019), current literature do not have an in-depth understanding of the determinants of the characteristics of managerial guidance.

1.4.5.1 Managerial Forecast Precision

In this subsection, I study how the forecast precision changes due to mutual fund ownership and the SEC regulation. Managers provide more accurate guidance to improve the credibility of the information and improve transparency (Armstrong, Core and Guay, 2014).

For each EPS guidance, I assign the precision measure equal to four if it is a point estimate, equal to three if it is a range estimate, equal to two if it is an open-end estimate, equal to one if it is a qualitative estimate, and zero if no forecast (Armstrong, Guay and Weber, 2014; Chen and Vashishtha, 2017). *Presicion_Qtr* (*Precision_Ann*) is the average of the precision value for quarterly (annual) EPS guidance provided by the managers during the quarter.[34] I use the equation (1) and switch the dependent variable to *Presicion_Qtr* (*Precision_Ann*) to estimate the change in precision due to the change in myopia.

The results are presented in Table 10. Column (1) and (2) are about the precision for quarterly and annual EPS guidance, separately. The findings are consistently similar. In column (1), the coefficient on the interaction term is equal to 0.605 and statistically significant at 1%. In column (2), it is equal to 0.502 and statistically significant at 5%. The results suggest that for firms with higher mutual fund ownership, managers tend to provide more precise guidance after the SEC regulation. The improvement in precision holds for EPS guidance at both the quarterly level and the annual level. Taken together with the main results that both the extensive margin and the intensive margin of managerial guidance increase for more affected firms after the regulation, induced myopia promotes the transparency of the information environment.

[34] I also construct the measure to look at the maximum of the precision value for the quarterly and annual EPS guidance. The results are quantitatively and qualitatively similar.

1.4.5.2 Managerial Guidance Horizon

In this subsection, I investigate the change with respect to the horizon of managerial guidance. Brochet, Loumioti and Serafeim (2015) find firms with more myopic executives are more likely to discuss short-term performance in their conference calls. [35] The review paper by Beyer et al. (2010) concludes that firms that are short-term oriented could use long-term disclosures as a strategy of cheap talk to hide moral hazard problems. Based on the earlier papers, there is no agreed upon opinion on whether myopia will introduce more short-term guidance or more long-term guidance. Analysis in this section aims to provide more evidence to solve this disagreement.

Horizon_QEPS (*Horizon_Qtr*) is the average forecast horizon of quarterly EPS guidance (any type of quarterly guidance) issued during the quarter, measured by the number of quarters between the date when the forecast is made and the date which the forecast targets. *Horizon_AEPS* (*Horizon_Ann*) is the average forecast horizon of annual EPS guidance (any type of annal guidance) issued during the quarter, measured by the number of years between the date when the forecast is made and the date which the forecast targets.[36] As the summary statistics in Table 1 show, on average, the quarterly EPS targets at about 0.13 quarter when the guidance is issued, and the annual EPS targets at 0.15 year. If managerial guidance become more short-term (long-term) focused, the coefficient β on the interaction term is expected to be negative (positive).

[35] Brochet et al. (2015) measure capital market pressure using (1) the difference of shares held by dedicated and quasi-index investors, (2) the number of earnings guidance issues in the prior quarter, and (3) the number of analysts. They define executive myopia with stock-based compensation.
[36] Empirical results are similar using the maximum of the forecast horizon.

Table 11 presents the results. Column (1) shows that in the post-regulation stage, firms with higher mutual ownership extend their horizon of the quarterly EPS guidance. The coefficient is equal to 0.109,with the p-value equal to 3.6%. Column (2) presents the results for the average horizon of any type of quarterly guidance as the dependent variable. Similar to the finding in column (1), the coefficient is positive and statistically significant at the 1% level. Column (3) and (4) present the results with the average horizon for annual EPS and any type of guidance as the dependent variables. The results are consistent with the horizon for the quarterly guidance. Column (3) shows that, for the annual EPS guidance, the coefficient is equal to 0.071 and p-value equal to 10.7%. In column (4), the coefficient is 0.456 and statistically significant at the 1% level. The findings in Table 11 reveal that, on average, firms tend to issue managerial guidance targeting at longer horizon when they become more myopic. The findings support the argument in Beyer et al. (2010), that myopic firms may use their voluntary disclosures strategically to provide more information about the farther future, which is relatively less verifiable in the current stage.

1.4.6 Robustness Check

In this section, I provide a robustness check on my findings to illustrate that the above findings are driven by the more frequent mandatory reporting of affected mutual funds, instead of concurrent events which may change both the treated funds and the control funds. To address this concern, I investigate whether the ownership of (1) unaffected mutual funds, whose reporting frequency is not changed by the regulation, or (2) index-tracking mutual funds, incrementally affect firms' voluntary disclosures. To do so, I adopt the model as below:

$$QEPS_{i,t} = \beta \times MF\ Own_i \times Post_{i,t} + \delta \times Alternative_i \times Post_{i,t} + Control + \alpha_i + \gamma_t + \varepsilon \quad (7)$$

where $Alternative_i$ takes two formats, *Unaffected* and *Index*. *Unaffected* is the percentage of shares owned by the actively managed mutual funds that voluntarily disclose on a quarterly basis before the regulation. *Index* is the percentage of shares owned by the passive mutual funds. Both *Unaffected* and *Index* are measured with the average value from May 2003 to May 2004, same as *MF_Own*. The coefficient δ on the interaction term *Unaffected* × *Post* (*Index* × *Post*) captures the effect of high institutional ownership by funds whose reporting frequency was not affected by the regulation (passive mutual fund) has on the corporate voluntary disclosures.

The results are presented in Table 12. As shown in column (1) and (2), the estimates of the coefficient on the interaction term $MF\ Own_i \times Post_{i,t}$ are significantly positive at the 1% level. Also, the magnitude of the estimates (0.201, 0.197) is similar to the results in Table 2 (0.200). Both the estimate of the coefficient on the interaction term $Unaffectd_i \times Post_{i,t}$ and $Index_i \times Post_{i,t}$ are not significantly different from zero. The findings suggest that the regulation in May 2004 increases corporate voluntary disclosures of quarterly EPS guidance through the ownership of mutual funds with reporting frequency increased, instead of through the mutual funds' ownership with reporting frequency unaffected, or through higher passive ownership.[37] With the robustness tests, the concern that concurrent events impact the corporate voluntary disclosures through higher institutional ownership is less likely to be valid.

[37] In untabulated tables, I explore *Any_Guide* and *Num_Guide* as dependent variables with the same design as shown in Table 12. The results are consistently similar.

1.5 Conclusion

By exploring the mandatory reporting frequency change in the mutual fund industry, this paper provides causal evidence on institutional investors' myopia increases corporate voluntary disclosures, which contributes empirical evidence to the theoretical debate over whether myopia induces more disclosures or less. When mutual funds are required to report their holdings more frequently, portfolio firms with higher mutual fund ownership are more likely to issue managerial guidance. Firms also become more strategic with their disclosure to boost short-term performance – they are more likely to issue short-term pessimistic guidance to facilitate meeting-or-beating of current targets, and issue guidance that follows the optimistic-pessimistic pattern. Consistent with earlier empirical findings and theoretical predictions, the relation is stronger when the mutual fund ownership is more transient, when the business environment is more volatile, and when the CEO is more likely to be short-termed.

Validation analysis suggests both mutual fund managers and corporate managers become more myopic after the regulation. Fund flows of the treated funds become significantly more sensitive to short-term relative performance, especially for the funds that have no institutional shares. The more affected firms tend to have CEO compensation more closely related to current stock performance, and are more likely to cut R&D when the net income is negative are after the regulation.

Findings in this paper help to complete the understanding of the firm-side consequences from institutional investors' myopia. I present empirical findings regarding the causal link and theoretical debate about myopia and voluntary disclosures. The results support the prediction in the prior theoretical framework that short-term focus encourages voluntary disclosures (Einhorn and Ziv, 2008; Bertomeu et al., 2022). The evidence helps to separate whether the reputation of being forthcoming or the reputation of being uninformed affects disclosing decisions. In addition, this paper helps to assess how the regulation on mutual fund industry transparency influences corporate information production. Finally, evidence in this paper also contributes to the debate over the benefits and costs of enhanced reporting requirements proposed by the regulator, especially for the rules in the mutual fund industry.

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Appendix 1.A Variable Definitions

Variable	Description	Data Source
Managerial Guidance		
QEPS	Dummy equal to one if the firm issues managerial guidance for current quarter EPS, and zero otherwise.	I/B/E/S
Any_Guide	Dummy equal to one if the firm issues any type of managerial guidance targeting at any fiscal period end, and zero otherwise.	I/B/E/S
Num_Guide	The number of managerial guidance issued during the quarter. Multiple guidance issued on the same day are counted as one	I/B/E/S
Pattern_Qtr (Pattern_Ann)	A dummy equal to one if the quarterly (annual) EPS guidance of the longest horizon is above the quarterly (annual) EPS guidance of the shortest horizon, and zero otherwise.	I/B/E/S
Pattern_Any	A dummy equal to one if any of the *Pattern_Qtr* and *Pattern_Ann* is equal to one, and zero otherwise.	I/B/E/S
Good_News (Bad_ News, Neutral_News)	A dummy equal to one if the firm last EPS guidance before the earnings announcement for the current quarter is above (below, indistinguishable from) the street consensus by the time the guidance is issued, and zero otherwise.	I/B/E/S
Presicion_Qtr (Precision_Ann)	The average of precision value for quarterly (annual) EPS managerial guidance issued during the quarter. Precision value equal to four for point forecasts, three for range forecasts, two for open-ended forecasts, one for qualitative forecasts, and zero for no forecasts.	I/B/E/S
Horizon_QEPS (Horizon_Qtr)	The average of forecast horizon of quarterly EPS guidance (any type of quarterly guidance) issued during the quarter, measured by the number of quarters between the date when the forecast is made and the date when the forecast targets.	I/B/E/S
Horizon_AEPS (Horizon_Ann)	The average of forecast horizon of annual EPS guidance (any type of annal guidance) issued during the quarter, measured by the number of years between the date when the forecast is made and the date which the forecast targets.	I/B/E/S
Firm Characteristics		
MF_Own	Percentage of shares held by the mutual funds with mandatory reporting frequency increased due to the SEC regulation in May 2004. The value is calculated as the average over the one year before to the regulation.	S12

Variable	Description	Data Source
Unaffected	Percentage of shares held by the mutual funds with reporting frequency *unaffected* by the SEC regulation in May 2004. The value is calculated as the average over the one year before to the regulation.	S12
Index	Percentage of shares held by the index-tracking mutual funds. The value is calculated as the average over the one year before the regulation in May 2004.	S12
Post	Dummy equal to one if the firm-quarter observation is after May 10, 2004, and zero otherwise	Compustat
Total Assets	Book value of total assets.	Compustat
Profit	Net income scaled by total assets	Compustat
BTM	Total assets, scaled by the sum of the firm's market capitalization and book value of debt.	Compustat
Leverage	Long-term debt scaled by total assets.	Compustat
RD dummy	Dummy equal to one if the R&D expenditure is above zero, and zero otherwise.	Compustat
Price	Stock price at the quarter end.	Compustat
Num_Analyst	Number of analysts following the firm.	I/B/E/S
Sensitive	Dummy equal to one if the change in mutual fund ownership is more responsive (above sample median) to the change in earnings, and zero otherwise.	S12, Compustat
Vol	Standard deviation of the monthly stock return of 24 months before the SEC regulation in May 2004.	CRSP
Short_Tenure	Dummy equal to one if the CEO of the firm is of shorter tenure with the firm (below sample median), and zero otherwise.	ExecuComp
Non_Founder	Dummy equal to one if the CEO is not the founder of the firm, and zero otherwise.	ExecuComp
Meet-or-Beat	Dummy equal to one if the firm's actual EPS is greater than or equal to the street consensus, and zero otherwise.	I/B/E/S
RD_Cut	Dummy equal to one if the current year R&D scaled by total assets is lower than R&D scaled by total assets for the prior year, and zero otherwise.	Compustat

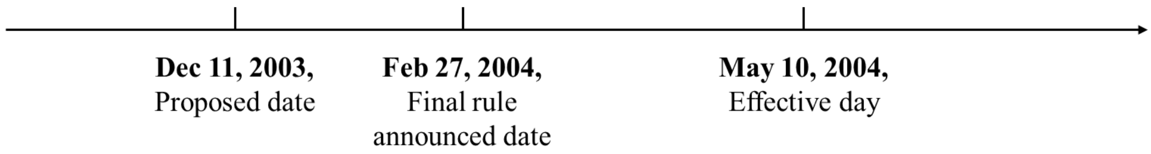
Variable	Description	Data Source
Neg	Dummy equal to one if the net income is negative and zero otherwise.	Compustat
Delta	CEO pay-performance sensitivity, which is a 1,000-dollar change in the CEO's wealth for a 1% change in stock price (Core and Guay, 2002; Coles et al., 2006).	ExecuComp
<u>Mutual Fund</u>		
Flow	Percentage change in the net total assets under management of the fund, after adjusting for fund returns during the quarter.	CRSP Mutual Fund
Return_Rank	Percentage of the return raking within the same category of mutual funds.	CRSP Mutual Fund
Performance	Tercile value assigned to the funds based on *Return_Rank*. Performance is equal to one for funds in the lowest tercile, equal to two for funds in the middle tercile, and equal to three for the highest tercile.	CRSP Mutual Fund
TNA	Total net assets under management of the fund.	CRSP Mutual Fund
Exp_ratio	Fund's annual expense ratio as reported.	CRSP Mutual Fund

Appendix 1.B Parallel Trend

This table reports regression estimates the likelihood of issuing earnings guidance before and after the regulation in May 2004. *Pre_4 (Pre_3*, *Pre_2*, *Pre_1*) indicates firm-quarter observations with fiscal periods ending from May 31, 2003 to June 30, 2003, (July 31, 2003 to Sep 30, 2003 Oct 31, 2003 to Dec 31, 2003, Jan 31, 2004 to Apr 30, 2004). *Post_1* (*Post_2*, *Post_3*, *Post_4*) indicates the fiscal period ending from May 31, 2004 to June 30, 2004 (July 31, 2004 to Sep 30, 2004, Oct 31, 2004 to Dec 31, 2004, Jan 31, 2005 to Apr 30, 2005). Coefficient estimates related to *Pre_4* are dropped due to multi-collinearity. Estimates of each period indicators are not shown in col (1) for simplicity. *QEPS* is a dummy equal to one if the firm issues EPS guidance for the current quarter, and zero otherwise. All other variables are defined in Appendix A. The unit of observation is firm-quarter. The t-statistics are given in parentheses. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

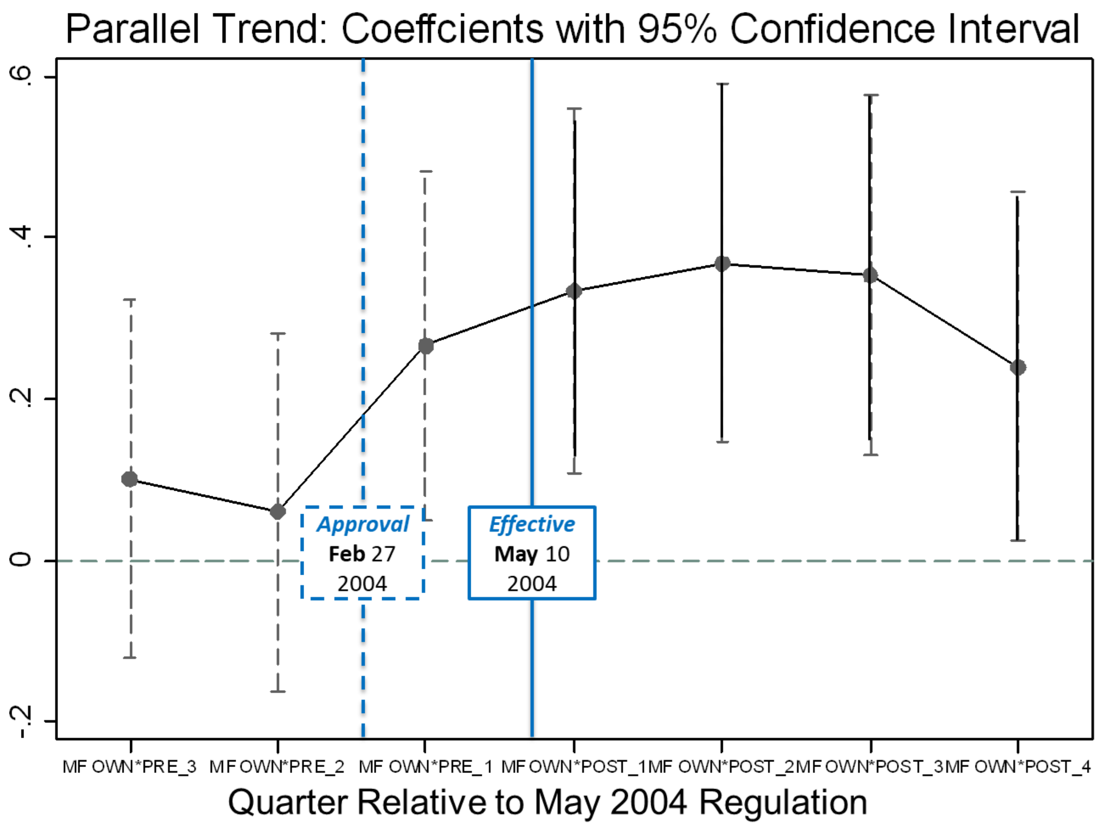
	(1)	(2)
	QEPS	
*MF_Own*Pre_3*	0.100	0.100
	(0.888)	(0.889)
*MF_Own*Pre_2*	0.0603	0.0604
	(0.533)	(0.533)
*MF_Own*Pre_1*	0.267**	0.266**
	(2.424)	(2.408)
*MF_Own*Post_1*	0.334***	0.334***
	(2.883)	(2.882)
*MF_Own*Post_2*	0.368***	0.368***
	(3.243)	(3.243)
*MF_Own*Post_3*	0.353***	0.353***
	(3.117)	(3.117)
*MF_Own*Post_4*	0.239**	0.239**
	(2.161)	(2.165)
Log(Total Assets)	0.0490***	0.0489***
	(3.970)	(3.961)
Profit	0.209***	0.208***
	(3.182)	(3.168)
BTM	-0.0431*	-0.0429*
	(-1.847)	(-1.840)
Leverage	-0.0105	-0.0106
	(-0.348)	(-0.349)
RD dummy	0.0177	0.0177*
	(1.639)	(1.645)
Log(price)	0.0119	0.0118
	(1.060)	(1.048)
Log(1+Num_Analyst)	0.0191***	0.0191***
	(2.900)	(2.898)
Constant	-0.826***	-0.831***
	(-3.556)	(-3.556)
Observations	25,886	25,886
R-squared	0.668	0.668
Sample	Firm-Quarter	
Period	2003 May to 2005 May	
Std Error Clustered	No	No
Year-Qtr-Post F.E.	No	Yes
Firm F.E.	Yes	Yes

Figure 1.1 Timeline of the Regulation



This figure presents the timeline of the regulation that requires mutual fund to increase the frequency of mandatory reporting. The rule was proposed on Dec 11, 2003. The final rule was announced on Feb 27, 2004. The effective date is May 10, 2004.

Figure 1.2 Likelihood of Issuing Quarterly Managerial Guidance Before and After the Regulation



This figure presents the likelihood of issuing quarterly EPS guidance for quarterly periods before and after the SEC regulation in May 2004. The dotted line and box denote the relative date when the final rule was approved on Feb 27, 2004. The solid line and box denote the relative date when the final rule became effective on May 10, 2004. The dots represent the differences-in-differences coefficient estimates for each quarter relative to the regulation change. The vertical lines represent the 95% confidence interval for each of the estimates. The solid (hollow) dot represents the coefficient is significant (insignificant) at 5%. The plot is based on the coefficient estimates from column (2) in Appendix B.

Table 1. 1 Summary Statistics

This table provides summary statistics on managerial guidance, firm characteristic, and mutual fund characteristics used in each analysis. All variable definitions are given in Appendix A.

Variable	N	Mean	Std. Dev	P10	Median	P90
Managerial Guidance						
QEPS	25,886	0.21	0.41	0	0	1
Any_Guide	25,886	0.41	0.49	0	0	1
Num_Guide	25,886	0.59	0.88	0	0	2
Pattern_Qtr	25,886	0.01	0.11	0	0	0
Pattern_Ann	25,886	0.02	0.12	0	0	0
Pattern_Any	25,886	0.03	0.16	0	0	0
Good_News	25,886	0.03	0.18	0	0	0
Bad_News	25,886	0.06	0.24	0	0	0
Neutral_News	25,886	0.11	0.32	0	0	1
Precision_Qtr	25,886	0.67	1.29	0	0	3
Precision_Ann	25,886	0.75	1.33	0	0	3
Horizon_QEPS	25,886	0.13	0.30	0	0	0.67
Horizon_Qtr	25,886	0.17	0.34	0	0	0.70
Horizon_AEPS	25,886	0.15	0.30	0	0	0.69
Horizon_Ann	25,886	0.19	0.33	0	0	0.79
Firm Characteristics						
MF_Own	25,886	0.06	0.06	0	0.05	0.14
Unaffected	25,886	0.01	0.02	0	0.01	0.03
Index	0.04	0.03	0.00	0.04	0.08	0.04
Total Assets ($M)	25,886	6,575	25,292	55	642	10,312
Profit	25,886	0	0.04	-0.03	0.01	0.03
BTM	25,886	0.69	0.29	0.29	0.72	1.01
Leverage	25,886	0.17	0.19	0	0.12	0.44
RD dummy	25,886	0.35	0.48	0	0	1
Price	25,886	22.97	18.32	4.17	18.85	46.37
Num_Analyst	25,886	5.52	6.19	0	3	14
Sensitive	24,083	0.5	0.5	0	0	1
Vol	21,731	0.14	0.09	0.06	0.12	0.26
Short_Tenure	9,999	0.57	0.5	0	1	1
Non_Founder	10,638	0.94	0.24	1	1	1
MB	18,925	0.7	0.46	0	1	1
Delta	10,685	0.83	1.79	0.04	0.27	1.85
RD_Cut	13,527	0.38	0.49	0	0	1
Neg	13,527	0.34	0.47	0	0	1
Mutual Fund Characteristics						
Flow	32,603	0.06	0.24	-0.07	0.01	0.21
Return_Rank	32,603	0.51	0.3	0.09	0.51	0.92
TNA	32,603	745	2823	10	125	1310
Exp_ratio	32,603	0.01	0.01	0.01	0.01	0.02
Turnover	32,603	1.18	1.92	0.14	0.68	2.09

Table 1. 2 Main Results: Issuance of Managerial Guidance

This table reports regression estimates from differences-in-differences models explaining the change in the likelihood of issuing managerial guidance after the regulation. *QEPS* (*Any_Guide*) is a dummy equal to one if the firm issues EPS guidance for the current quarter (any type of guidance), and zero otherwise. *Num_Guide* is the number of guidance issued during the quarter. *MF_Own* is the percentage of shares held by mutual funds with reporting frequency affected. *Post* is a dummy variable equal to one for observations on and after May 10, 2004, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-quarter. The standard errors are clustered by firm. The t-statistics are given in parentheses. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1) ***QEPS***	(2) ***QEPS***	(3) ***Any_Guide***	(4) ***Num_Guide***
MF_Own*Post	**0.179*** **	**0.200*** **	**0.599*** **	**1.548*** **
	(2.698)	**(3.023)**	**(7.355)**	**(11.71)**
Log(Total Assets)		0.0488***	0.0348*	0.0358
		(3.196)	(1.836)	(1.172)
Profit		0.209***	0.307***	0.471***
		(3.065)	(3.613)	(3.687)
BTM		-0.0414*	-0.0700**	-0.0272
		(-1.697)	(-2.168)	(-0.563)
Leverage		-0.00973	0.0322	0.0154
		(-0.252)	(0.753)	(0.229)
RD dummy		0.0179*	-0.00200	0.0107
		(1.658)	(-0.161)	(0.503)
Log(price)		0.0119	0.0352**	0.0692***
		(0.975)	(2.321)	(2.827)
Log(1+Num_Analyst)		0.0191***	0.0420***	0.0523***
		(2.670)	(4.752)	(4.018)
Constant	0.208***	-0.825***	-0.435	-0.467
	(102.2)	(-2.757)	(-1.196)	(-0.805)
Observations	25,886	25,886	25,886	25,886
R-squared	0.667	0.668	0.687	0.667
Sample		Firm-Quarter		
Period		2003 May to 2005 May		
Std Error Clustered	Firm	Firm	Firm	Firm
Year-Qtr-Post F.E.	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes

Table 1. 3 Good News, Bad News and Neutral News

This table reports regression estimates from differences-in-differences models explaining the change in the likelihood of the firm to issue good news, bad news and neutral news. *Good_News* (*Bad_News*, *Neutral_News*) is a dummy equal to one if the firm issues the most recent EPS guidance before the earnings announcement that is above (below, indistinguishable from) the street consensus, and zero otherwise. *MF_Own* is the percentage of shares held by mutual funds with reporting frequency affected. *Post* is a dummy variable equal to one for observations on and after May 10, 2004, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-quarter. The standard errors are clustered by firm. The t-statistics are given in parentheses. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1) ***Good_News***	(2) ***Bad_News***	(3) ***Neutral_News***
MF_Own*Post	**0.0528**	**0.141*** **	**-0.0108**
	(1.293)	**(2.754)**	**(-0.176)**
Log(Total Assets)	-0.0111	0.0517***	0.00918
	(-1.290)	(4.453)	(0.655)
Profit	0.0135	0.0864	0.107*
	(0.364)	(1.518)	(1.674)
BTM	-0.00553	0.00518	-0.0348*
	(-0.429)	(0.296)	(-1.694)
Leverage	0.0258	0.0318	-0.0771***
	(1.382)	(1.082)	(-2.795)
RD dummy	0.00384	-0.00226	0.0146
	(0.432)	(-0.213)	(1.286)
Log(price)	-0.000657	0.0202**	-0.00342
	(-0.0902)	(2.269)	(-0.335)
Log(1+Num_Analyst)	-0.00516	0.0214***	0.00352
	(-1.247)	(3.748)	(0.585)
Constant	0.266	-1.091***	-0.0360
	(1.569)	(-4.737)	(-0.130)
Observations	25,886	25,886	25,886
R-squared	0.273	0.292	0.466
Sample		Firm-Quarter	
Period		2003 May to 2005 May	
Std Error Clustered	Firm	Firm	Firm
Year-Qtr-Post F.E.	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes

Table 1. 4 Meet or Beat

This table reports estimates from differences-in-differences models explaining the change in the meeting-or-beating after the regulation, conditional on issuing certain type of guidance. *MB* is a dummy equal to one if the firm meets or beats its earnings target of the current quarter, and zero otherwise. *MF_Own* is the percentage of shares held by mutual funds with reporting frequency affected. *Post* is a dummy variable equal to one for observations on and after May 10, 2004, and zero otherwise. *QEPS* is a dummy equal to one if the firm issues managerial guidance for the current quarter EPS, and zero otherwise. *Good_News* (*Bad_News*, *Neutral_News*) is a dummy equal to one if the firm issues the most recent EPS guidance before the earnings announcement that is above (below, indistinguishable from) the street consensus, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-quarter. The standard errors are clustered by firm. The t-statistics are given in parentheses. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1) ***MB***	(2) ***MB***	(3) ***MB***	(4) ***MB***
Guidance	***QEPS***	***Good_News***	***Bad_News***	***Neutral_News***
MF_Own*Post*Guidance	**0.378**	**0.0412**	**0.716***	**0.176**
	(1.585)	**(0.103)**	**(1.655)**	**(0.695)**
*MF_Own*Post*	-0.161	-0.0705	-0.106	-0.0901
	(-1.140)	(-0.591)	(-0.893)	(-0.708)
*MF_Own*Guidance*	-0.235	-0.493	-0.386	-0.0505
	(-0.951)	(-1.540)	(-1.106)	(-0.231)
*Post*Guidance*	-0.0384	0.0112	-0.0684	-0.0125
	(-1.528)	(0.260)	(-1.511)	(-0.440)
Guidance	0.0248	0.127***	-0.0706**	0.0501**
	(1.002)	(3.932)	(-1.986)	(2.090)
Log(total assets)	-0.0912***	-0.0900***	-0.0835***	-0.0917***
	(-3.198)	(-3.163)	(-2.944)	(-3.221)
Profit	1.987***	1.979***	1.999***	1.981***
	(11.03)	(10.96)	(11.08)	(11.02)
BTM	-0.0920*	-0.0900*	-0.0924*	-0.0909*
	(-1.759)	(-1.720)	(-1.765)	(-1.742)
Leverage	0.204***	0.203***	0.211***	0.210***
	(3.260)	(3.244)	(3.374)	(3.351)
RD dummy	0.0170	0.0169	0.0168	0.0168
	(0.801)	(0.796)	(0.795)	(0.795)
Log(price)	-0.0126	-0.0125	-0.0103	-0.0128
	(-0.540)	(-0.536)	(-0.440)	(-0.550)
Log(1+Num_analysts)	-0.0150	-0.0139	-0.0130	-0.0153
	(-1.038)	(-0.963)	(-0.895)	(-1.055)
Constant	2.669***	2.633***	2.504***	2.669***
	(4.890)	(4.838)	(4.614)	(4.900)
Observations	18,830	18,830	18,830	18,830
R-squared	0.299	0.302	0.300	0.303
Sample	Firm-Quarter			
Period	2003 May to 2005 May			
Std Error Clustered	Firm	Firm	Firm	Firm
Year-Qtr-Post F.E.	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes

Table 1. 5 Optimistic-Pessimistic Pattern

This table reports regression estimates from differences-in-differences models explaining the change in the likelihood for the managerial guidance to follow the long-term optimistic, short-term pessimistic pattern, after the regulation. *Pattern_Qtr* (*Pattern_Ann*) is a dummy variable equal to one the quarterly (annual) EPS guidance of the longest horizon is above the quarter (annual) EPS guidance of the shortest horizon, and zero otherwise. *MF_Own* is the percentage of shares held by mutual funds with reporting frequency affected. *Post* is a dummy variable equal to one for observations on and after May 10, 2004, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-quarter. The standard errors are clustered by firm. The t-statistics are given in parentheses. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1) ***Pattern_Qtr***	(2) ***Pattern_Ann***	(3) ***Pattern_Any***
MF_Own*Post	**0.0908*****	**0.0153**	**0.0850****
	(3.593)	**(0.617)**	**(2.445)**
Log(Total Assets)	0.00721	0.0104	0.0196**
	(1.181)	(1.591)	(2.197)
Profit	0.0176	0.00929	0.0243
	(0.762)	(0.472)	(0.816)
BTM	0.00422	-0.0102	-0.00601
	(0.511)	(-1.289)	(-0.530)
Leverage	0.00471	0.000143	0.00405
	(0.419)	(0.0113)	(0.256)
RD dummy	0.00680	-0.00781	0.00101
	(1.266)	(-1.602)	(0.138)
Log(price)	0.0119***	0.00245	0.00996
	(2.660)	(0.494)	(1.521)
Log(1+Num_Analyst)	0.000145	-0.00508**	-0.00345
	(0.0662)	(-2.077)	(-1.095)
Constant	-0.177	-0.188	-0.395**
	(-1.469)	(-1.473)	(-2.255)
Observations	25,886	25,886	25,886
R-squared	0.214	0.202	0.223
Sample		Firm-Quarter	
Period		2003 May to 2005 May	
Std Error Clustered	Firm	Firm	Firm
Year-Qtr-Post F.E.	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes

Table 1. 6 Fund Flow Sensitivity to Relative Performance

This table reports regression estimates from differences-in-differences models explaining the change in the fund flow over fund relative performance, after the regulation. *Flow* is the quarterly change in total net assets under management, after adjusting to the quarterly return. *Performance* is the tercile indicator based on the relative performance of the fund within its objective category. *Post* is a dummy variable equal to one for observations on and after May 10, 2004, and zero otherwise. *Low* is dummy equal to one if the relative return is lower than the median, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-quarter. The standard errors are clustered by firm. The t-statistics are given in parentheses. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)
	Flow	***Flow***	***Flow***	***Flow***
Performance*Post	**0.00455*****	**0.00123**	0.00190	0.000962
	(3.903)	**(0.803)**	(1.193)	(0.480)
Performance*Post*Treat			**0.00243**	**0.00431***
			(1.235)	**(1.722)**
*Performance*Treat*			-0.000472	-0.00152
			(-0.325)	(-0.839)
*Treat*Post*			-0.00915**	-0.0143**
			(-2.082)	(-2.491)
Performance	0.000713	0.00164	0.00129	0.00254*
	(0.800)	(1.493)	(1.131)	(1.817)
Fee	-3.158***	-0.416	-2.366***	-1.976*
	(-3.187)	(-0.309)	(-3.055)	(-1.898)
Log(MTNA)	-0.0631***	-0.0376***	-0.0579***	-0.0659***
	(-12.44)	(-7.192)	(-13.89)	(-11.59)
Turnover	0.00256	-0.00263	0.00208	0.00338
	(1.264)	(-1.302)	(1.153)	(1.590)
Flow (lagged)	0.0217	0.0379	0.0226	0.0108
	(1.062)	(1.258)	(1.239)	(0.525)
Constant	0.358***	0.185***	0.321***	0.349***
	(11.89)	(6.654)	(13.41)	(10.86)
Observations	32,950	9,655	42,605	29,771
R-squared	0.236	0.254	0.238	0.223
Sample	Fund-Month			
Period	2003 May to 2005 May			
Subsample	Treated Funds	Control Funds	All Funds	Only retail shares
Std Error Clustered	Fund	Fund	Fund	Fund
Year-Month F.E.	Yes	Yes	Yes	Yes
Fund F.E.	Yes	Yes	Yes	Yes

Table 1. 7 CEO Compensation

This table reports regression estimates from differences-in-differences-in-dif models explaining the change in CEO compensation after the regulation. *Delta* is CEO pay-performance sensitivity, which is a 1,000-dollar change in the CEO's wealth for a 1% change in stock price. *MF_Own* is the percentage of shares held by mutual funds with reporting frequency affected. *Post* is a dummy variable equal to one for observations on and after May 10, 2004, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-year. The standard errors are clustered by firm. The t-statistics are given in parentheses. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1) ***Delta***
MF_Own*Post	**1.617***** **(3.135)**
Log(total assets)	-0.915 (-1.064)
Profit	1.175 (0.798)
BTM	0.0974 (0.0624)
Leverage	-1.327 (-0.933)
RD dummy	-0.800 (-0.428)
Log(price)	3.738*** (4.975)
Log(1+Num_analysts)	0.0128 (0.0385)
Constant	15.41 (0.958)
Observations	10,685
R-squared	0.765
Sample	Firm-Year
Period	2000 May to 2008 May
Std Error Clustered	Firm
Year F.E.	Yes
Firm F.E.	Yes

Table 1. 8 Cut in R&D

This table reports regression estimates from differences-in-differences-in-dif models explaining the change in the likelihood of for the firm to cut R&D when the net income is negative after the regulation. *RD_Cut* is a dummy variable equal to one if the firm annual R&D scaled by annual total assets is lower than that of the prior year, and zero otherwise. *MF_Own* is the percentage of shares held by mutual funds with reporting frequency affected. *Post* is a dummy variable equal to one for observations on and after May 10, 2004, and zero otherwise. *Neg* is a dummy equal to one if the annual net income is negative, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-year. The standard errors are clustered by firm. The t-statistics are given in parentheses. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1) ***RD_Cut***	(2) ***RD_Cut***
MF_Own*Post*Neg	**1.566****	**0.608***
	(2.186)	**(1.921)**
*MF_Own*Post*	-0.670**	-0.465***
	(-2.068)	(-2.627)
*MF_Own*Neg*	-0.366	-0.415
	(-0.491)	(-1.548)
*Post *Neg*	-0.131**	-0.0568**
	(-2.271)	(-2.153)
Neg	-0.127*	-0.145***
	(-1.865)	(-6.317)
Log(total assets)	-0.492***	-0.202***
	(-7.475)	(-15.23)
Profit	-0.428***	-0.352***
	(-4.576)	(-12.13)
BTM	0.0417	0.0351
	(0.451)	(1.082)
Leverage	-0.368**	-0.0537
	(-2.472)	(-1.198)
RD dummy	-0.0241	-0.00196
	(-0.473)	(-0.147)
Log(price)	0.0267	0.0397***
	(0.773)	(3.737)
Constant	10.25***	4.367***
	(8.366)	(18.22)
Observations	3,144	13,527
R-squared	0.637	0.338
Sample	Firm-Year	Firm-Year
Period	2003 May to 2005 May	2000 May to 2008 May
Std Error Clustered	Firm	Firm
Year F.E.	Yes	Yes
Firm F.E.	Yes	Yes

Table 1. 9 Cross-Sectional Analysis

This table reports regression estimates from differences-in-differences models explaining the change in the likelihood of issuing managerial guidance after the regulation, conditional on different cross-sectional variables (C-X Var). *QEPS* is a dummy equal to one if the firm issues EPS guidance for the current quarter, and zero otherwise. *MF_Own* is the percentage of shares held by mutual funds with reporting frequency affected. *Post* is a dummy variable equal to one for observations on and after May 10, 2004, and zero otherwise. *Sensitive* is a dummy equal to one if the mutual fund ownership is more responsive to changes in earnings, and zero otherwise. *Vol* is the standard deviation of the monthly stock return from the 24 months before the regulation. *Short_Tenure* is a dummy equal to one if the tenure of the firm is lower than or equal to the sample median, and zero otherwise. *Non_Founder* is a dummy variable equal to one if the CEO is not the founder of the firm, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-quarter. The standard errors are clustered by firm. The t-statistics are in parentheses. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1) ***QEPS***	(2) ***QEPS***	(3) ***QEPS***	(4) ***QEPS***
C-X Var	***Sensitive***	***Vol***	***Short_Tenure***	***Non_Founder***
MF_Own*Post*C-X Var	**0.260***	**2.530****	0.524**	**0.996***
	(1.781)	**(2.338)**	(1.964)	**(1.832)**
*Post*C-X Var*	0.00836	-0.00795	-0.0416	-0.136**
	(0.727)	(-0.140)	(-1.580)	(-2.103)
*MF_Own*Post*	0.0245	-0.0545	0.0142	-0.604
	(0.279)	(-0.334)	(0.0762)	(-1.147)
Log(Total Assets)	0.0575***	0.0551***	0.126***	0.136***
	(3.333)	(3.140)	(3.489)	(3.743)
Profit	0.233***	0.197***	0.363**	0.302*
	(3.092)	(2.624)	(2.277)	(1.861)
BTM	-0.0473*	-0.0441	-0.129*	-0.123*
	(-1.727)	(-1.578)	(-1.820)	(-1.804)
Leverage	-0.0110	-0.0132	-0.0283	-0.0103
	(-0.262)	(-0.293)	(-0.387)	(-0.134)
RD dummy	0.0191	0.0178	0.0236	0.0233
	(1.623)	(1.476)	(1.312)	(1.360)
Log(price)	0.0121	0.00908	-0.0122	-0.0140
	(0.894)	(0.648)	(-0.461)	(-0.544)
Log(1+Num_Analyst)	0.0203***	0.0195**	0.0470***	0.0406***
	(2.661)	(2.233)	(2.925)	(2.624)
Constant	-0.992***	-0.919***	-2.363***	-2.494***
	(-2.935)	(-2.717)	(-3.199)	(-3.345)
Observations	24,083	21,731	9,999	10,638
R-squared	0.665	0.666	0.668	0.664
Sample	Firm-Quarter			
Period	2003 May to 2005 May			
Std Error Clustered	Firm	Firm	Firm	Firm
Year-Qtr-Post F.E.	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes

Table 1. 10 Precision

This table reports regression estimates from differences-in-differences models explaining the change in the precision in issuing managerial guidance. *Precision_Qtr (Precision_Ann)* is the average of the precision value for the quarterly (annual) EPS guidance issued during the quarter. *MF_Own* is the percentage of shares held by mutual funds with reporting frequency affected. *Post* is a dummy variable equal to one for observations on and after May 10, 2004, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-quarter. The standard errors are clustered by firm. The t-statistics are given in parentheses. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1) ***Precision_Qtr***	(2) ***Precision_Ann***
MF_Own*Post	**0.605*** **	**0.502** **
	(2.874)	**(2.313)**
Log(Total Assets)	0.164***	0.0415
	(3.445)	(0.815)
Profit	0.678***	0.434**
	(3.219)	(2.113)
BTM	-0.140*	-0.0261
	(-1.902)	(-0.371)
Leverage	-0.0291	0.0355
	(-0.248)	(0.307)
RD dummy	0.0845**	-0.0238
	(2.449)	(-0.751)
Log(price)	0.0417	0.107***
	(1.128)	(2.861)
Log(1+Num_Analyst)	0.0567**	0.0829***
	(2.562)	(3.663)
Constant	-2.823***	-0.520
	(-3.022)	(-0.533)
Observations	25,886	25,886
R-squared	0.665	0.684
Sample	Firm-Quarter	
Period	2003 May to 2005 May	
Std Error Clustered	Firm	Firm
Year-Qtr-Post F.E.	Yes	Yes
Firm F.E.	Yes	Yes

Table 1. 11 Horizon

This table reports regression estimates from differences-in-differences models explaining the change in the forecast horizon of managerial guidance after the regulation. *Horizon_QEPS* (*Horizon_Qtr*) is the average horizon of the quarterly EPS (ant type of) guidance issued during the quarter. *Horizon_AEPS* (*Horizon_Ann*) is the average horizon of the annual EPS (ant type of) guidance issued during the quarter. *MF_Own* is the percentage of shares held by mutual funds with reporting frequency affected. *Post* is a dummy variable equal to one for observations on and after May 10, 2004, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-quarter. The standard errors are clustered by firm. The t-statistics are given in parentheses. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1) ***Horizon_QEPS***	(2) ***Horizon_Qtr***	(3) ***Horizon_AEPS***	(4) ***Horizon_Ann***
MF_Own*Post	**0.109****	**0.399*****	**0.0709**	**0.456*****
	(2.101)	**(6.129)**	**(1.613)**	**(7.973)**
Log(Total Assets)	0.0320***	0.0389***	0.0190	0.0242
	(2.726)	(3.013)	(1.514)	(1.630)
Profit	0.0438	0.128*	0.0193	0.0892
	(0.677)	(1.784)	(0.356)	(1.426)
BTM	-0.0391**	-0.0668***	-0.00536	-0.00229
	(-2.293)	(-3.197)	(-0.333)	(-0.109)
Leverage	0.0231	0.0402	-0.0350	-0.0392
	(0.900)	(1.374)	(-1.223)	(-1.199)
RD dummy	-0.000456	-0.00142	0.0807***	0.0969***
	(-0.0425)	(-0.140)	(6.974)	(7.371)
Log(price)	-0.00114	-0.0100	0.0168*	0.0239**
	(-0.131)	(-0.963)	(1.908)	(2.135)
Log(1+Num_Analyst)	0.00654	0.0128**	0.0200***	0.0136**
	(1.289)	(2.077)	(3.772)	(2.244)
Constant	-0.508**	-0.589**	-0.337	-0.427
	(-2.166)	(-2.319)	(-1.392)	(-1.499)
Observations	25,886	25,886	25,886	25,886
R-squared	0.620	0.613	0.580	0.543
Sample		Firm-Quarter		
Period		2003 May to 2005 May		
Std Error Clustered	Firm	Firm	Firm	Firm
Year-Qtr-Post F.E.	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes

Table 1. 12 Robustness Check

This table reports robustness-check regression estimates from differences-in-differences models explaining the change in the likelihood of issuing managerial guidance after the regulation. *QEPS* (*Any_Guide*) is a dummy equal to one if the firm issues EPS guidance for the current quarter (any type of guidance), and zero otherwise. *Num_Guide* is the number of guidance issued during the quarter. *MF_Own* is the percentage of shares held by mutual funds with reporting frequency affected. *Unaffected* is the percentage of shares held by actively managed mutual funds with reporting frequency unaffected. *Index* is the percentage of shares held by index-tracking funds. *Post* is a dummy variable equal to one for observations on and after May 10, 2004, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-quarter. The standard errors are clustered by firm. The t-statistics are given in parentheses. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1) ***QEPS***	(2) ***QEPS***
*MF_Own*Post*	0.201***	0.197***
	(2.736)	(2.866)
Unaffected * Post	**-0.0100**	
	(-0.0434)	
Index * Post		**0.0164**
		(0.132)
Log(Total Assets)	0.0488***	0.0489***
	(3.198)	(3.201)
Profit	0.209***	0.209***
	(3.065)	(3.064)
BTM	-0.0414*	-0.0413*
	(-1.697)	(-1.695)
Leverage	-0.00977	-0.00980
	(-0.253)	(-0.254)
RD dummy	0.0179*	0.0179*
	(1.657)	(1.659)
Log(price)	0.0119	0.0119
	(0.974)	(0.973)
Log(1+Num_Analyst)	0.0191***	0.0192***
	(2.670)	(2.677)
Constant	-0.825***	-0.826***
	(-2.760)	(-2.764)
Observations	25,886	25,886
R-squared	0.668	0.668
Sample	Firm-Quarter	
Period	2003 May to 2005 May	
Std Error Clustered	Firm	Firm
Year-Qtr-Post F.E.	Yes	Yes
Firm F.E.	Yes	Yes

Chapter 2: Concierge Treatment from Banks: Evidence from the Paycheck Protection Program

We use loans that were extended to public firms through the Paycheck Protection Program (PPP) as a laboratory to separate between favoritism and informational advantages in interpersonal ties between banks and firms. Because PPP loans are guaranteed by the government and banks do not need to carefully screen borrowers, this setting reduces information frictions, allowing us to quantify the effect of favoritism. We find that firms with personal ties to banks are more likely to obtain PPP loans. The role of personal ties weakens when firms are less opaque, but does not vary with banks' corporate governance. We also find that connected firms are more likely to return their loans to avoid regulatory scrutiny. Overall, we offer clean estimates of the role of favoritism in bank lending and highlight the unintended consequence of government programs that use the banking system to allocate capital.

2.1 Introduction

A large body of research in economics and finance studies the role of agency frictions and information asymmetries in capital allocation. An important mechanism that can exacerbate or ameliorate these effects is the relationship between the involved parties. Political economy research has focused on the agency role of relationships in exacerbating favoritism in the access to government capital (e.g., Sapienza (2004), Dinç (2005), Faccio, Masulis, and McConnell (2006), Duchin and Sosyura (2012)). In contrast, the banking literature has focused on the role of relationships in ameliorating information asymmetries between lenders and borrowers (e.g., Lummer and McConnell (1989), Petersen and Rajan (1994), Berger and Udell (1995), Engelberg, Gao, and Parsons (2012)). In this paper, we provide novel evidence on the role of relationships in a unique setting that involves both political economy and banks – the Paycheck Protection Program – where the government uses the banking system to allocate capital and where informational frictions are unimportant since participating banks are not required to screen borrowers and are not exposed to credit risk, allowing us to isolate the effects of favoritism.

The Paycheck Protection Program (PPP) is a central piece of the 2020 Coronavirus Aid, Relief, and Economic Security (CARES) Act, which extends government-guaranteed, forgivable bank loans to businesses to cover payroll, utilities, mortgage, and rent costs.[38] We argue that information frictions are unimportant in the PPP for several reasons. First, PPP loans are fully guaranteed by the Small Business Administration (SBA). Hence, participating banks have little exposure to credit risk due to these loans. Second, PPP loans are forgivable, reducing the risk of default and rendering lenders' reputational concerns about borrowers' defaults irrelevant

[38] See https://www.sba.gov/funding-programs/loans/coronavirus-relief-options/paycheck-protection-program.

(Gopalan, Nanda, and Yerramilli (2011)).[39] Third, PPP eligibility requirements only demand a good faith certification by the borrower, and the SBA does not require participating banks to collect soft information nor to screen borrowers. In particular, lenders are not required to assess the credit worthiness of the borrowers and are only subject to basic Bank Secrecy Act (BSA) and Know Your Customer (KYC) requirements, which are much less stringent. Consequently, the PPP generates a setting in which informational advantages associated with connections are less relevant: participating banks have little incentives to screen borrowers and are not required to do so.

On the other hand, the PPP setting is susceptible to favoritism – a form of agency frictions that results from connections between banks and borrowers. The Covid-19 crisis was an economy-wide shock, with millions of companies simultaneously applying for PPP loans, leading to credit rationing.[40] As such, banks played an important role in allocating loans and prioritizing borrowers, giving rise to possible favoritism in originating loans to borrowers with connections to lenders. If connected borrowers are less qualified to receive PPP loans, the allocation of capital can also deviate from the stated goals of the PPP, and the costs will be internalized by taxpayers and small businesses that fail to receive government aid.

Indeed, anecdotal evidence suggests that favoritism played a role in the origination of PPP loans. For example, an article in *Bloomberg* argued that "JPMorgan Chase & Co. provided loans to virtually all of its commercial banking customers that sought financing through the small

[39] Per the SBA's guidelines, "borrowers may be eligible for loan forgiveness if the funds were used for eligible payroll costs, payments on business mortgage interest payments, rent, or utilities during either the 8- or 24-week period after disbursement."

[40] This observation is consistent with Li, Strahan and Zhang (2020), who show that during the last three weeks of March 2020, commercial banks witnessed the largest increase in credit demand ever observed. Similarly, Erel and Liebersohn (2020) argue that Covid-19 induced "tremendous stress on financial institutions, with an unprecedented demand for their services."

business relief program, while the lender's smallest customers were almost entirely shut out."[41] Similarly, an article in the *Wall Street Journal* stated that "Companies with existing loans at big banks fared well in coronavirus-relief effort."[42] Following reports that the program "favored large, well-funded companies over struggling small businesses in underserved communities," the House Select Subcommittee on the Coronavirus crisis launched an investigation into the PPP. The preliminary analyses "revealed significant potential fraud, waste, and abuse."[43]

To investigate whether and how favoritism impacts loan origination, we focus on personal connections between directors or executives from the borrowing firm and the lending financial institution. We measure personal connections based on shared education, previous employment, and nonprofit backgrounds of firms' and banks' executives and board members. The role of such social connections has been studied across a wide range of economic activities, including investments by mutual fund managers (Cohen, Frazzini, and Malloy (2008)), external finance (Engelberg, Gao, and Parsons (2012)), internal capital allocation (Duchin and Sosyura (2013)), and mergers and acquisitions (Schmidt (2015)). We focus on personal connections because they provide a cleaner setting to study the role of favoritism in the allocation of PPP loans compared to other measures of connections, such as relationship-lending, which could reflect assortative matching between borrowers and lenders, as well as the goals of program administrators to expedite the allocation of loans by tapping into banks' existing customer base.

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[41] See "JPMorgan's Small Business Loans Instead Went to Its Biggest Customers" by David McLaughlin and Michelle Davis, https://www.bloomberg.com/news/articles/2020-04-22/jpmorgan-commercial-clients-beat-out-smaller-ones-for-sba-loans.
[42] See "In Race for Small-Business Loans, Winning Hinged on Where Firms Bank" by Ruth Simon and Peter Rudegeair, https://www.wsj.com/articles/in-race-for-small-business-loans-winning-hinged-on-where-firms-bank-11587410421.
[43] See https://coronavirus.house.gov/sites/democrats.coronavirus.house.gov/files/2020-09-01.PPP%20Interim%20Report.pdf

The empirical analyses focus on public firms that obtained PPP funds. While only a small fraction of PPP loans has been granted to publicly traded firms, this setting offers two critical advantages for our study. First, it offers a natural sample to detect favoritism since public firms are less likely to qualify for a PPP loan than small private firms due to their size and access to capital markets. Second, focusing on public firms allows us to obtain relevant data on PPP recipients and their matched control firms that did not receive a PPP (these data are not available for private firms). Importantly, focusing on a small sample of public PPP borrowers does not allow us to assess the overall efficacy of the PPP.

We collect information on PPP loans from S&P Global Market Intelligence, FactSquared, corporate press releases, 8-K filings, 10-Q/K filings, and the SBA, and merge it with firm-level information from Compustat and data on director/executive biographies and personal connections from BoardEx. In the sample, the average loan size is $2.4 million, and 43.7% of recipients have personal connections to top executives in the lending financial institution.

We start by investigating the effect of personal connections on the likelihood of obtaining a PPP loan. We match each public PPP borrower to its closest public nonborrower based on industry, size, the number of employees, and credit ratings, because PPP eligibility was based on industry-specific small business size standards, payroll size, and access to financing. Hence, this design mitigates concerns that the results are driven by differences in eligibility criteria between PPP and non-PPP firms. We also include in the regressions industry fixed effects, location fixed effects, and controls for firm size, firm leverage, firm social network size, local severity of the Covid-19 shock, local government policies in response to Covid-19, and past lending relationships. This design addresses selection concerns that the effects are driven by assortative matching or by economic indicators that are correlated with credit demand and the severity of the Covid-19 crisis.

The estimates suggest that within a set of comparable firms, a personal connection between the borrower and the lender increases the likelihood of obtaining a PPP loan by 8.0-9.3%, depending on the specification. The effect is statistically significant at the 1% level and robust to the inclusion of fixed effects and control variables. Since information frictions are unimportant in this setting, the observed effects of connections on PPP allocation can be attributed to favoritism.

To provide additional evidence on the role of favoritism in PPP loan provision, we collect information on PPP loans that were returned to the government ex-post, an indication of allocative deviations from the goals and guidelines of the PPP. Using these data, we investigate the effect of connections on the likelihood of returning PPP loans. We find that connected borrowers are 7.8-8% more likely to return their PPP loan. These estimates provide additional evidence that banks deviated from the stated objectives/guidelines of the program by favoring connected borrowers.

Next, we provide cross-sectional analyses that explore the effects of opaqueness and corporate governance. First, we investigate whether the degree of opaqueness, as measured by the number of analysts following a firm or its media coverage, affects the provision of PPP loans to connected borrowers. We conjecture that more visibility will curtail favoritism to avoid public criticism and outcry. We find evidence consistent with this conjecture: the effect of personal connections is weaker among firms with more analysts following or greater media coverage.

Second, we examine the effect of corporate governance. We argue that since PPP loans are guaranteed by the government, bank performance and shareholder value are unaffected by loan performance, and hence originating loans to connected borrowers does not represent an agency conflict between shareholders and managers. Consequently, corporate governance should not have an effect on the role of personal connections in PPP loan provision. Consistent with our hypothesis, we do not find a significant effect of corporate governance, measured by board independence or the E-index (Bebchuk, Cohen and Ferrell (2009)).

Overall, our paper contributes to a growing literature on the role of connections in credit allocation. La Porta, Lopez-de Silanes and Zamarripa (2003) examine how common ownership of banks and firms affects lending decisions in Mexico. Guiso, Sapienza and Zingales (2004) find that Italian banks allocate more credit to socially connected firms. Khwaja and Mian (2005) study the impact of political connections on credit allocation in Pakistan. Engelberg, Gao and Parsons (2012) show that interpersonal connections between banks and firms result in lower interest rates and better performance. Haselmann, Schoenherrr, and Vig (2018) find that social connections among members of an elite club in Germany affect banks' credit allocation decisions. We extend this literature by studying the role of personal connections in the allocation of emergency government capital through the U.S. banking system in response to the Covid-19 crisis. Unlike prior estimates that capture the joint effect of informational advantages and agency costs, we exploit the design of the PPP, which mutes information frictions, to isolate the role of favoritism in credit allocation.

Our paper is also related to the literature on favoritism in the allocation of government capital. Existing studies focus on firms' political connections to the government. For example, Sapienza (2004) and Dinc (2005) study the allocation of credit by government-owned banks, and Faccio, Masulis, and McConnell (2006) and Duchin and Sosyura (2012) study favoritism in bailing out politically connected firms. We extend these studies by investigating the role of connections between firms and banks rather than between firms and politicians. These connections are important in our setting because the PPP disbursed government capital through banks rather than directly to firms. While such a program design leverages private banks' existing distribution networks and can help disburse government aid quickly and cost-effectively, our estimates suggest that it also gives rise to favoritism in the allocation of government capital due to the additional layer of connections between banks and borrowers. As such, our analyses reveal the tradeoff between agency costs, speed, and cost effectiveness that result from a program design where banks do not have any skin in the game and are unexposed to credit risks that may arise from their allocation decisions.

Our study is also related to the literature on regulatory arbitrage. Prior studies (e.g., Acharya, Schnabl and Suarez (2013) and Arteta, Carey, Correa, and Kotter (2013)) show that banks exploit capital regulations by engaging in excessive risky lending. We show that banks exploit the allocation of PPP loans to strengthen their personal relationships with large firms amid the Covid-19 crisis. Coupled with the origination fees that banks collected, the lack of exposure to the credit risk of these loans, and the deviation from the stated goals of the program, these benefits amount to a net transfer from taxpayers to banks.

Finally, our paper is related to several contemporaneous studies of the PPP. Autor et al. (2020) examine the impact of PPP on employment using administrative payroll data; Granja, Makridis, Yannelis, and Zwick (2020) study the allocation of PPP loans across regions, and show that it did not correlate with employment shocks nor had a significant effect on local economic conditions; Erel and Liebersohn (2020) focus on the role of FinTech in the provision of PPP loans. Cororaton and Rosen (2020) characterize the universe of public PPP borrowers; Li and Strahan (2020) show that PPP loan supply at the bank-level decreases in bank size and increases in pre-existing credit lines, commitment lending, and core deposits; Balyuk, Prabhala and Puri (2020) study the relationship between the allocation of PPP loans and various firm attributes, including financial constraints. Berger et al. (2020) find that following the onset of the Covid-19 crisis, relationship borrowers obtain worse loan terms, including higher interest rates, on their non-PPP loans; Amiram and Rabetti (2020) focus on firms that obtain PPP loans, and show that at the intensive margin, borrowers' business relationships with their lenders increase the size of PPP loans. We augment these studies by focusing on the role of personal connections and showing that they affect the allocation of PPP loans at the extensive margin, comparing between PPP recipients and nonrecipients.

2.2 The Paycheck Protection Program (PPP)

The Paycheck Protection Program (PPP) is a centerpiece $659 billion business loan program established by section 1102 of the Coronavirus Aid, Relief, and Economic Security (CARES) Act, which authorized the Small Business Administration (SBA) to distribute loans to support payroll and overhead expenses to eligible small businesses through its nationwide network of lenders.

PPP loans were guaranteed by the SBA and loan applicants did not need to provide any collateral or personal guarantees to apply or to be approved for a PPP loan. Participating lenders were not required to evaluate or screen borrowers and did not assume meaningful credit risks by originating PPP loans. Thus, information frictions and adverse selection played little role in banks' approval process of PPP loans. Participating lenders earned an upfront origination fee proportional to the amount of the loan: 5% for loans under $350k, 3% for loans between $350k and $2 million, and 1% for loans above $2 million.[44]

The terms of PPP loans are highly attractive for borrowers. First, the principal of a PPP loan can be either partially or fully forgiven based on the usage of the loan proceeds. Second, even

[44] See: https://www.sba.gov/sites/default/files/2020-07/5000-20036-508.pdf

if not forgiven, PPP loans carry a low interest rate of one percent. Third, both the principal and interest payments are deferred until the loan is forgiven or, if the borrower does not apply for loan forgiveness, ten months after the end of the 24-week covered period.[45] Consequently, millions of businesses in the U.S immediately applied for PPP loans, which were accepted, approved, and disbursed on a first-come first-served basis, leading to credit rationing and generating a setting susceptible to favoritism.[46]

The first round of the PPP commenced on April 3, 2020. Within two weeks, on April 16, 2020, the entire first round of $349 billion was depleted.[47] A bill to add $310 billion of funding was passed by Congress and signed into law by President Trump on April 24, and the SBA began accepting new applications from lenders on April 27. The PPP was due to expire at midnight on June 30 with funds remaining, but just hours before the expiration of the program Congress authorized an extension through August 8. This date passed without a second extension to the program, and the SBA stopped accepting applications. Figure 1 describes the timeline of the PPP. By August 08, 2020, the SBA had disbursed over $525 billion of the $659 billion appropriated by Congress to this program[48].

In a press briefing on April 22, 2020, Treasury Secretary at the time, Steven Mnuchin, warned of "severe consequences" for businesses that received PPP funds not according to the

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[45] The SBA initially required that at least 75% of the loan be used for payroll, rent, mortgage interest, and utilities to be forgiven at the end of 8 weeks. On June 5, President Trump signed the PPP Flexibility Act, which reduced the proportion needed to be spent on payroll to 60% and extended the time period to use the funds from 8 to 24 weeks.

[46] While the SBA did not release information about the number of PPP applications or application approval rates, it reported a total of 4.67 million loans disbursed by June 20, 2020.

[47] See, for example, the article "Small business rescue loan program hits $349 billion limit and is now out of money," by Thomas Franck and Kate Rogers, published on CNBC on April 16: https://www.cnbc.com/2020/04/16/small-business-rescue-loan-program-hits-349-billion-limit-and-is-now-out-of-money.html

[48] https://www.sba.gov/sites/default/files/2020-08/PPP_Report%20-%202020-08-10-508.pdf

guidelines.[49] Following Mnuchin's press briefing, the SBA instituted a "safe harbor" for the return of PPP funds by those businesses. On April 28, the Treasury and the SBA issued a joint statement that they would retroactively examine all loans over $2 million to certify that program qualifications were met.[50] Consequently, 99 PPP loans by public firms, totaling more than $469 million, were returned. We conjecture that favoritism played a particularly strong role in the subset of returned loans, and provide a comparative analysis of the role of connections in these loans relative to unreturned PPP loans.

2.3 Data and Variables

2.3.1 Sample construction and variables

We begin our sample construction by identifying all public companies that received a PPP loan. We collect these data from S&P Global Market Intelligence, FactSquared, corporate press releases, 8-K filings, 10-Q/K filings, and the SBA. As shown in Appendix B, we start with 971 firms receiving PPP loans over the period of April 13, 2020 – August 8, 2020. We end the sample period on August 8, 2020 because the SBA stopped accepting applications on this date.[51] The matching procedure and data availability lead to a final sample of 652 unique PPP borrowers, 213 unique lenders, and 687 unique PPP firm-lender pairs, corresponding to 692 PPP loans. Table 1 shows that the mean and median loan sizes are $2.4 million and $1.1 million, respectively.

[49] See https://www.businessinsider.com/treasury-mnuchin-consequences-big-companies-taking-ppp-small-business-loans-2020-4

[50] See https://factba.se/sba-loans for the list of public PPP borrowers, including those that subsequently returned the funds. The full PPP loan-level data can be found here: https://home.treasury.gov/policy-issues/cares-act/assistance-for-small-businesses/sba-paycheck-protection-program-loan-level-data.

[51] While the SBA reopened the PPP on January 11, 2021, we did not find additional public firms that received a PPP loan after August 8, 2020.

We merge these data with firm-level financial information from Compustat, data on director and executive biographies, personal connections and network sizes from BoardEx, data on syndicated loans from the LPC Dealscan database, data on Covid-19 cases from the New York Times, and county-level policy responses to Covid-19 from the National Association of Counties (NACo). For our cross-sectional analyses, we also collect data on the number of analysts covering a firm from I/B/E/S, data on media coverage from Ravenpack, and data on the board of directors from ISS.

We measure personal connections based on the social ties between executives or directors from the firm and executives or directors from the lending financial institution who are likely to influence the allocation of credit.[52] The conjecture that personal connections may affect credit allocation decisions is supported by earlier work, which shows that social networks influence corporate outcomes, such as executive compensation (Hwang and Kim (2009)), financial policy (Fracassi (2017)), governance (Fracassi and Tate (2012)), access to capital (Cohen, Frazzini and Malloy (2008), Hochberg, Ljungqvist, and Lu (2007), Engelberg, Gao, and Parsons (2012)), incidence of fraud (Chidambaran, Kedia, and Prabhala (2011)), earnings management (Hwang and Kim (2012)), and acquisition activity (Ishii and Xuan (2014), Schmidt (2015), Shue (2013), Cai and Sevilir (2012)).

We consider three types of personal connections: connections via education (*Educational*), connections via previous employment (*Previous employment*), and connections via nonprofit

[52] Specifically, we consider directors and executives of financial institutions serving in the following roles: CEO, CFO, Deputy CFO, Independent director, Chief Corporate Banking Officer, Director-Corporate Banking, Vice President - Corporate Banking, Head of Corporate Banking, Senior VP - Corporate Business, and Executive VP-Corporate Banking. Online Appendix 1 shows that our results continue to hold if we measure personal connections based on *all* directors and executives of the lender.

organizations (*Nonprofit organizations*). Educational connections foster a sense of belonging to a common group, which is evidenced by alumni clubs, donations to the home school, and college sports. We define two executives or directors as connected via an *Educational* tie if they earned the same degrees from the same educational institutions. Table 1 shows that 32.7% of the firms' executives or directors are connected to executives and directors in the financial institution via educational ties.

We define two executives or directors as connected via prior employment (*Previous employment*) if they worked together or served on the same board of directors at a third-party firm at the same time in the past. Table 1 shows that 12.4% of the executives or directors share this connection with executives or directors in the financial institution. Lastly, two executives or directors are connected via nonprofit organizations (*Nonprofit organizations*) if they share membership in the same nonprofit organization. These organizations typically include social clubs, religious organizations, philanthropic foundations, industry associations, and other nonprofit institutions defined in BoardEx as a manager's other activities. In our sample, 21.5% of the executives and directors share a nonprofit connection with executives and directors in the financial institution (Table 1).

We also construct an aggregate measure, *Personal connections index*, that encompasses all three types of personal connections. The aggregation of personal connections formed via various networks into a summary measure is widely used in the social networks literature (e.g., Hwang and Kim (2009), Fracassi and Tate (2012), Schmidt (2015), Fracassi (2017)). In particular, if a firm has any of the three types of personal connections with executives and directors in the financial institution, it is considered connected to the lender. We set the indicator variable *Personal connections index* equal to one if the firm is personally connected to the lender and zero otherwise.

In our sample, 43.7% of the firms have personal connections with the lender. The indicator variable *Personal connections index* captures the extensive margin, that is, the existence of personal connections. In Online Appendix 2, we also consider the intensive margin by measuring the intensity of personal connections and exploring whether the effect of personal connections strengthens as the number of connection types increases. The estimates in Online Appendix 2 suggest that personal connections affect the allocation of PPP loans both at the extensive margin and the intensive margin.

2.3.2 Research Design

To test whether favoritism played a role in the allocation of PPP loans, we construct a matched sample by identifying a group of non-PPP firms (i.e., firms that did not receive a PPP loan) for each PPP borrower. Since only one PPP firm has a credit rating, we restrict the matched non-PPP firms to US public firms that do not have a credit rating. To construct the matched sample, we employ the Mahalanobis distance approach (Rubin (1979) and Patton and Weller (2020)), with replacement, to find matching non-PPP firms in the same 2-digit NAICS or 1-digit NAICS industry group with the closest total assets and number of employees.[53] We match on these attributes because they determine firms' eligibility for PPP loans.[54] Total assets and number of employees are measured based on the most recent quarter available before the onset of the PPP. If the number of employees is missing, we linear-extrapolate it based on the firm's size. If we cannot find matched control firms within the same 2-digit NAICS industry, we look for matching firms within

[53] We require that the difference in total assets and the number of employees between the PPP firm and the matched non-PPP firm does not exceed 50% of the total assets and number of employees of the PPP firm, respectively.
[54] See: https://www.sba.gov/funding-programs/loans/coronavirus-relief-options/paycheck-protection-program.

the same 1-digit NAICS industry. If we still cannot find control firms, we exclude the firm from the sample. Altogether, we are able to match 581 (89% out of the 652 unique PPP borrowers) using 2-digit NAICS codes and additional 71 firms using 1-digit NAICS codes.

Since a borrower can obtain multiple PPP loans in each of the two funding rounds, we collapse all loans obtained by the same borrower from the same lender in the same PPP funding round into a single observation. This procedure yields a matched sample comprising a total of 1,384 firm-lender-loan observations, of which 692 are in the treatment group (PPP firm-lender-loan) and 692 are in the control group (non-PPP firm-lender-loan).

To assess the matching quality, Panel A of Table 2 reports difference-in-means estimates and the corresponding p-values for several characteristics of PPP and non-PPP firms. Based on the point estimates, PPP firms are comparable in their asset size ($1,638 million vs. $1,462 million in total assets), leverage (0.78 vs. 0.58), market-to-book ratios (4.5 vs. 5.1), employees (2,402 vs. 2,368 employees), and profitability, as measured by ROA (-0.12 vs. -0.13). Importantly, the above differences between the two groups are statistically insignificant at conventional levels, as shown by the p-values of the difference-in-means estimates reported in the last column of Panel A. Moreover, we show that the two groups have essentially identical mean values by presenting the absolute standardized mean differences, defined as the mean differences divided by the standard deviation of the differences, which are small across all variables. These estimates suggest that the matching procedure yields observationally similar PPP and non-PPP firms. Lastly, in Panel B of Table 2, we show that the composition of stock exchanges is similar across PPP and non-PPP firms.

We investigate the effect of connections on the allocation of PPP loans by estimating the following linear probability model:

$$\text{Prob}\ (Treatment_{i,l,k} = 1)\ = \beta_0 + \ \beta_1 Connections_{i,l} + \beta_2 Controls_{\boldsymbol{i}} + \mu_{MSA} + \eta_{industry} + \varepsilon_{i,l,k} \quad (1)$$

where *i* indexes firms, *l* indexes lenders, and *k* indexes loans offered to firm *i* by lender *l* in either funding round. We include size and leverage as control variables because they are likely to be correlated with credit demand and because PPP borrowers are smaller and are more likely to have debt compared to eligible non-PPP firms (see Cororaton and Rosen (2020)). We also control for the borrower's social network size to mitigate concerns about alternative mechanisms, such as borrower sophistication or access to information, driving the effects. *Network size indicator* is a dummy variable equal to one if the average of the log(network size) is above the sample median, and zero otherwise. We include Metropolitan Statistical Area (MSA) fixed effects based on the location of firm *i*'s headquarters, as well as industry fixed effects. These fixed effects mitigate concerns that the effect of connections is driven by unobservable local, or industry-level economic factors correlated with credit demand. Lastly, we control for the variation in the severity of the Covid-19 pandemic, measured by the number of positive cases at the state, MSA, or county levels, since the goal of the PPP was to help maintain employment in businesses hit by the pandemic. We cluster the standard errors at the industry level.

2.4 Results

2.4.1 The Role of Connections in the Allocation of PPP Loans

We begin by providing regression evidence on the role of personal connections in the allocation of PPP loans. The regressions follow the matched-sample specification in equation (1) and the results are reported in Table 3. The key variables of interest are the measures of interpersonal connections: *Educational*, *Previous employment*, *Nonprofit organizations*, and *Personal connections index,* which are discussed in the previous section. The coefficient estimates capture the effect of interpersonal connections on the likelihood of receiving a PPP loan.

In columns (1) and (2), we investigate the effect of interpersonal connections through shared educational backgrounds. The estimates in column (1) suggest that the existence of an educational connection increases the likelihood of receiving a PPP loan by 9.8%. This effect is statistically significant at the five percent level and holds after controlling for MSA and industry fixed effects. In column (2), we include network size, firm size, and leverage as additional control variables, and obtain similar results. The coefficient on *Network size indicator* is positive but statistically insignificant, suggesting that a larger social network does not materially increase the likelihood of receiving a PPP loan.

In columns (3) and (4), we investigate the role of interpersonal connections via prior employment. The estimates suggest that the existence of personal connections via prior employment (*Previous employment*) increases the likelihood of receiving a PPP loan by 8.7% to 9.1%. This effect is also highly statistically significant (t-statistic=2.2 or 2.1) and holds after controlling for large network size, firm size, leverage, and the same set of fixed effects. In columns (5) and (6), we investigate the roles of interpersonal connections via nonprofit organizations (*Nonprofit organizations*). Similarly, this type of connections increases the likelihood of receiving a PPP loan by about 5.8%, and the effect is statistically significant at the ten percent level. While statistically significant, the economic impact of connections via nonprofit organizations is smaller compared with the other two types of connections. This result is consistent with prior evidence (e.g., Fracassi (2017)) that connections via employment and education play a stronger role than connections via nonprofit organizations in influencing firms' policies. A possible interpretation is that memberships in nonprofit organizations, which are often broad and passive, represent a weaker type of personal connections.

In columns (7) and (8), we investigate the role of interpersonal connections using an aggregate measure of connections, *Personal connections index*. The coefficient estimates on *Personal connections index* are highly statistically significant at the one percent level, and the economic magnitude is within the range of those reported in columns (1)-(6). Overall, the evidence presented in Table 3 suggests that personal connections played a role in the allocation of PPP loans.

2.4.2 Robustness

In this section, we conduct two robustness tests. First, we consider the effects of the local severity of, and response to, the pandemic. Second, we disentangle between the effects of interpersonal connections and business connections (measured by past lending relationships).

4.2.1 Local Covid-19 Severity and Response

The regression specifications in Table 3 include MSA fixed effects, which absorb demographic and economic differences across MSAs, as well as differences in the local severity of the Covid-19 pandemic and the government's response to it. Nevertheless, to directly quantify the relation between regional exposure (or policy response) to Covid-19 and the allocation of PPP loans, we drop the MSA fixed effects and augment equation (1) with the following four variables. The first variable, *Log (1+ # of Covid-19 Cases),* is defined as the logarithm of the number of positive Covid-19 cases as of April 3, 2020 (when the PPP was launched) in the state, MSA, or county where the firm is headquartered. The other three variables represent county-level policy responses to Covid-19, which likely had an effect on local businesses: (1) Declarations of a state of emergency, (2) Business closures, and (3) Safer-at-home policies. To conserve space, we only report results using *Personal connections index* as the measure of connections. However, all the other individual connection measures yield similar results.

We report these results in Panel A of Table 4. Overall, the estimates provide two main takeaways. First, the effects of the variation in local exposures and responses to the Covid-19 pandemic are not robust, flipping signs across specifications, and are statistically insignificant at conventional levels, suggesting that the allocation of PPP loans was not systematically affected by differences in the impact of the Covid-19 pandemic across businesses and regions. These findings are consistent with the evidence in Granja, Makridis, Yannelis, and Zwick (2020). Second, the effects of *Personal connections index* remain highly statistically significant and economically similar after controlling for these effects.

4.2.2 Past Lending Relationships

Contemporaneous research shows that lending relationships increase the likelihood of receiving PPP loans (Li and Strahan (2020)) and obtaining bigger loans (Amiram and Rabetti (2020)). In this subsection, we augment our regression specification with the presence of past lending relationships to disentangle between the effect of interpersonal connections and lending relationships.

We define an indicator variable, *Past lending relationships*, equal to one if the firm obtained a non-PPP loan from its PPP lender that matured within the past five years (on or after 2015), and zero otherwise[55]. If the loan is syndicated by a group of banks, we require the PPP

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[55] We use Dealscan-Compustat Link Data constructed by Chava and Roberts (2008).

lender to serve as a lead bank. We choose a five-year window because prior research commonly uses five years as the threshold to identify relationship lending (e.g., Gopalan, Nanda and Yerramilli (2011)). The intuition behind this cutoff is that borrowers are likely to still have an active relationship with the lender if their loan matured within the last five years. As shown in Table 1, about 4% of the observations have past lending relationship between the PPP lender and the firm.

We report the results in Panel B of Table 4. Consistent with prior research, we find that lending relationships played a significant role in the allocation of PPP loans. Nevertheless, we find that interpersonal connections are significantly positively associated with receiving a PPP loan even after controlling for past lending relationships. The magnitude of the coefficients in Panel B is comparable with those reported in Table 3, suggesting that interpersonal connections play a distinct role from past lending relationships in the allocation of PPP loans.

2.4.3 Returned Loans

The estimates in the previous subsections suggest that personal connections played a role in the allocation of PPP loans. We argue that such connections capture the effect of favoritism on the allocation of loans, and hence, likely represent deviations from the stated guidelines and objectives of the PPP. As such, we predict that PPP borrowers with personal connections to banks would be more likely to return their PPP loans in response to public pressures and to avoid prosecution.

To test this prediction, we estimate the impact of the three individual measures of personal connections (*Educational*, *Previous employment*, *Nonprofit organizations*), as well as the aggregate index (*Personal connections index*), on the likelihood of returning the PPP loan. The returned loan analyses focus solely on PPP borrowers (i.e., excluding the matched non-PPP firms) and compare the likelihood of returning PPP loans across connected and unconnected firms. In these analyses, we include lender fixed effects, and cluster standard errors at the industry level.

The results are reported in Panel A of Table 5. The dependent variable in the regressions is an indicator variable that equals one if the PPP borrower publicly announced returning its PPP loans and zero otherwise. The main explanatory variables of interest are *Educational* (column (1)), *Previous employment* (column (2)), *Nonprofit organizations* (column (3)), and *Personal connections index* (column (4)).

Across all the columns and measures of connections, we find that personally connected firms are considerably more likely to return their loans. These findings are economically large relative to the sample mean of 15% loan return rate, and statistically significant at conventional levels. Borrowers with *Educational* (*Previous employment*, *Nonprofit organizations*) connections are 6.8% (7.8%, 8.4%) more likely to return their PPP loans. Further, the coefficient on *Personal connections index* in column (4) reveals that the existence of at least one of the connections increases the likelihood of loan returns by 8.0%.

In Panel B of Table 5, we include *Past lending relationships* as an additional explanatory variable. These analyses aim to distinguish between the effects of interpersonal connections and businesses relationships on the likelihood of returning PPP loans. The estimates in Panel B reveal similar effects of interpersonal connections and insignificant effects of past lending relationships on the likelihood of returning PPP loans. These estimates suggest that while both interpersonal connections and lending relationships increased the likelihood of receiving PPP loans, interpersonal connections are associated with the incidence of returning the loans whereas lending relationships are not. Interpreted more broadly, these estimates suggest that banks' reliance on their existing business networks of borrowers was likely consistent with the stated guidelines and goals of the program to disburse emergency loans quickly and cost-effectively. Hence, these loans were no more likely to be returned. On the other hand, the provision of loans to personally connected borrowers likely deviated from the program's stated guidelines, and, consequently, personally connected borrowers returned their loans to avoid prosecution and public criticism.

2.4.4 Cross-sectional Analyses

In this subsection, we investigate the heterogenous effects of interpersonal connections on the allocation of PPP loans. In particular, we examine two dimensions of cross-sectional variation. First, we consider the opacity of the borrowing firms. We conjecture that less opaque firms are subject to more public scrutiny, which makes favoritism easier to detect. As a result, we expect the effect of interpersonal connections on the allocation of PPP loans to be weaker for less opaque firms. Second, we investigate the impact of banks' corporate governance. If originating PPP loans to personally connected firms were to erode banks' profitability and shareholder value, we would expect well-governed banks to be less likely to lend personally connected borrowers. However, since PPP loans are guaranteed by the government, banks are not exposed to credit risk. Hence, their performance and value should not be affected by PPP loan performance. Thus, we do not expect the relation between personal connections and PPP loans to vary with banks' governance.

We use the following two variables to estimate the opaqueness of the borrower: (1) *Log(1+# of analysts)*, which is the natural logarithm of one plus the number of analysts following the firm in 2019; (2) *Media*, which is the natural logarithm of number of news articles covering the firm in 2019.[56] Columns (1) and (2) in Table 6 present the results. The key variables of interest are the interaction terms between opaqueness and interpersonal connections, which capture the variation in the effect of connections on the allocation of PPP loans across firms with different levels of opaqueness.

As shown in column (1), the coefficient on the interaction term *Personal connections index * Log (1+# of analysts)* is significantly negative, consistent with our prediction that the effect of *Personal connections index* is weaker for less opaque firms. Personal connections increase the likelihood of obtaining a PPP loan by 13.6% at firms with zero analyst coverage. This likelihood declines by 3.42% to 10.18% for firms with an average level of analysts following. Similarly, in column (2), the coefficient on the interaction term *Personal connections index * media* is significantly negative. For firms with zero media coverage, personal connections increase the likelihood of obtaining a PPP loan by 34.3%. This likelihood drops to 11.29% for firms with an average level of media coverage. Taken together, this evidence suggests that banks are less likely to provide preferential treatment to personally connected borrowers when they are more visible and hence more likely to be detected. Therefore, stronger monitoring and public scrutiny serve as disciplinary devices that deter borrowers and lenders from engaging in favoritism.

To measure banks' corporate governance, we construct the following two variables: (1) *Independent directors* is a dummy variable equal to one if the number of independent directors scaled by the total number of directors is above the sample median, and zero otherwise; (2) *E-*

[56] We only include news from Dow Jones Newswires, and exclude news articles identified as "press-releases" since those are likely initiated by the firms. We only keep news with a relevance score equal to 100 in Ravenpack. Based on the definition in Ravenpack, a relevance score of 0 means the entity was passively mentioned while a score of 100 means the entity was prominent in the news story. We obtain qualitatively similar results by restricting relevance score greater or equal to 75 (values above 75 are considered significantly relevant).

index is the entrenchment index from Bebchuk, Cohen and Ferrell (2009). Columns (3) and (4) in Table 6 present the results of whether the governance of the lending institution matters. In both columns, the coefficients on the interaction terms *Personal connections index * Corporate governance* are statistically indistinguishable from zero, indicating that personal connections are equally important in the allocation of PPP loans across well-governed and poorly governed lenders. This evidence suggests that lenders' corporate governance is unrelated to favoritism in the allocation of PPP loans, arguably because banks do not have any "skin in the game" since the loans are guaranteed by the government.

2.5. Concluding Remarks

This paper exploits the unique features of the Paycheck Protection Program (PPP) to provide novel estimates of the role of interpersonal connections in preferential access to credit. Unlike other government aid programs that allocated capital directly to their end-users, PPP capital was disbursed by banks that, effectively, were not required to screen borrowers and were not meaningfully exposed to credit risk. Consequently, the PPP provides an ideal setting to isolate the role of favoritism from information frictions in bank lending.

We focus on PPP loans that were extended to publicly traded firms, and find that personal connections between borrowers and financial institutions play an important role in the allocation of loans as personally connected firms are considerably more likely to obtain loans. These effects hold in regression specifications that control for local exposures and government responses to the Covid-19 crisis, the overall size of the social networks of the borrowers, as well as a restrictive system of fixed effects. We also find that personal connections play a distinctive role from that of past lending relationships in the allocation of PPP loans.

We provide several analyses that further isolate the role of favoritism in the provision of PPP loans. First, we show that personally connected borrowers were more likely to return their PPP loans, whereas relationship-lending borrowers were not. These estimates suggest while relationship lending likely allowed banks to rely on their existing borrower networks and disburse government aid quickly and cost effectively, personal connections led to deviations from the stated guidelines and goals of the PPP, resulting in loan returns. Collectively, these findings highlight the tradeoffs and unintended consequences of relying on the banking system to disburse government subsidies.

Second, we show that the role of personal connections in the allocation of PPP loans is weaker among less opaque firms, suggesting that public monitoring mitigates banks' preferential treatment in disbursing government subsidies. We do not find that banks' corporate governance has had a measurable influence on the role of personal connections, likely because banks did not have any "skin in the game", and, hence, their performance and value were unaffected by the performance of PPP loans.

All in all, our findings provide some of the cleanest estimates, to date, of the role of personal connections in fostering favoritism in loan provision by financial institutions. Furthermore, they highlight the conflicts of interest resulting from the design of the Paycheck Protection Program, emphasizing the importance of oversight and aligned incentives in financial intermediation. At the same time, it is important to note that our findings do not aim to evaluate the overall efficacy of the PPP since we focus only on the loans that were extended to public firms, which do not represent the complete universe of PPP loans.

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Figure 2. 1 Timeline of the PPP

This figure describes the timeline of the two funding rounds of the Paycheck Protection Program and the accompanying guidance for public firms.

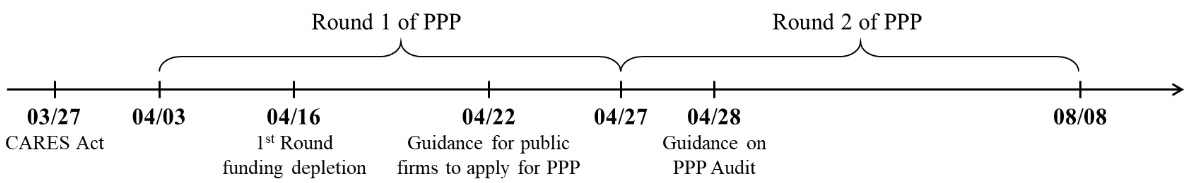

Table 2. 1 Summary Statistics

This table provides summary statistics for the sample of PPP loans, public firms that received PPP loans, and matched public firms that did not receive PPP loans. It summarizes loan characteristics, the connections between borrowers and lenders, firm-level attributes, and local exposures and responses to the Covid-19 crisis. All variables definitions are given in Appendix A.

Variable	N	Mean	Std. Dev	p25	Median	p75
PPP Loan Characteristics						
PPP loan Amount ($M)	692	2.37	3.50	0.37	1.07	2.94
PPP loan Amount/Total Assets	692	0.056	0.122	0.011	0.031	0.061
PPP loan return dummy	692	0.15	0.358	0	0	0
Firm-lender connections						
Personal connections index	1,384	0.437	0.496	0	0	1
Educational	1,384	0.327	0.469	0	0	1
Previous employment	1,384	0.124	0.330	0	0	0
Nonprofit organizations	1,384	0.215	0.411	0	0	0
Past lending relationships	1,384	0.040	0.195	0	0	0
Firm Characteristics						
Average network size	1,384	1055.3	873.4	277.5	977.9	1594.4
Total assets ($M)	1,384	1918.7	8661.4	15.0	51.3	218.2
Leverage	1,384	0.67	2.79	0.07	0.21	0.47
Market-to-book ratio	1,342	4.64	13.50	1.12	1.70	3.57
ROA	1,323	-0.12	0.46	-0.13	-0.01	0.02
Number of Employees	1,384	3172	17214	37	123	459
Media	1,159	44.91	41.78	20	34	54
# of analysts	1,384	3.24	5.57	0	1	4
Lender Governance						
Independent director	880	0.69	0.46	0	1	1
E-index	880	0.77	1.45	0	0	0
Covid-19						
MSA Covid-19 Cases	1,384	16428.4	27887.3	3,312	8,420	12,569
State Covid-19 Cases	1,384	8104.0	18042.1	528	1,634	5658
County Covid-19 Cases	1,384	1279.2	1961.9	249	711	1,156
County Emergency Declaration	1,384	0.798	0.402	1	1	1
County Business Closure Policy	1,384	0.079	0.271	0	0	0
County Safer-at-Home Policy	1,384	0.376	0.484	0	0	1

Table 2. 2 Covariate Balance

This table provides comparative statistics on the matched sample of public PPP loan recipients and nonrecipients. Panel A provides difference-in-means estimates of firm-level attributes across PPP recipients and their matched nonrecipients. In panel A, all variables are measured based on the most recent available quarterly report of 2019 except the number of employees, which is measured based on the most recent available annual report since 2018. Panel B compares between the stock exchanges where recipients and nonrecipients are listed. All variable definitions are given in Appendix A.

Panel A: Firm Characteristics

	treatment firms (N = 652)		Matched control firms (N = 652)		Difference (Treatment - Control)	
	Mean	SD	Mean	SD	Absolute Standardized Mean Difference	P-value of Test of PPP firms = non-PPP firms
Total Assets ($M)	1,638	345.7	1,462	301.0	0.021	†0.7003
Leverage	0.78	0.13	0.58	0.09	0.072	0.1963
Market-to-book ratio	4.47	0.62	5.12	0.47	-0.047	0.4067
ROA	-0.12	0.02	-0.13	0.02	0.032	0.5717
Number of employees	2,402	606.9	2,368	648.1	0.002	††0.9693

†The p-value of testing the difference of log(Total Assets) between PPP firms and non-PPP firms is 0.5761

††The p-value of testing the difference of log(Number of employees) between PPP firms and non-PPP firms is 0.6267

Panel B: Stock Exchanges

Table 2 Panel B shows the comparison of listed exchanges between the PPP firms and their matched non-PPP firms. Data is collected from Compustat.

	PPP firms (N = 652 firms)		Matched non-PPP firms (N = 652 firms)	
Exchange	N	%	N	%
AMEX	59	9.05	44	6.75
NASDAQ	369	56.60	360	55.21
NYSE	68	10.43	65	9.97
OTC	155	23.77	179	27.45
Others	1	0.15	4	0.61
Total	652	100	652	100

Table 2. 3 The Role of Connections in the Allocation of PPP Loans

This table reports regression estimates from linear probability models explaining the likelihood of receiving a PPP loan. *Educational (Previous employment, Nonprofit organizations)* is an indicator variable that equals one if the firm's directors or executives shares an education (previous employment, nonprofit) connection with directors or executives at the lending financial institution, and zero otherwise. *Personal connections index* is an indicator variable that equals one if any of the firm's directors or executives shares an education, employment, or nonprofit connection with directors or executives at the lending financial institution, and zero otherwise. *Average log(network) dummy* is a dummy variable equal to one if the average of log (network) is above median, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-lender-loan triplet. The standard errors are clustered by the NAICS2. The t-statistics are given in parenthesis. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Prob (treatment = 1)				
Educational	0.0975**	0.0979**						
	(2.704)	(2.619)						
Previous employment			0.0907**	0.0874**				
			(2.170)	(2.074)				
Nonprofit organizations					0.0582*	0.0574*		
					(1.783)	(1.749)		
Personal connections index							0.0925***	0.0930***
							(3.249)	(3.262)
Network size indicator	0.00811	0.00291	0.0228	0.0187	0.0212	0.0162	0.00697	0.00257
	(0.224)	(0.0671)	(0.718)	(0.511)	(0.658)	(0.439)	(0.215)	(0.0693)
Log(total assets)		0.00544		0.00478		0.00534		0.00496
		(0.764)		(0.785)		(0.884)		(0.816)
Leverage		0.00707*		0.00631*		0.00679*		0.00714**
		(1.988)		(1.722)		(1.830)		(1.982)
Constant	0.457***	0.357***	0.470***	0.382***	0.470***	0.372***	0.449***	0.357***
	(17.19)	(3.292)	(27.52)	(3.842)	(27.42)	(3.764)	(24.04)	(3.592)
Observations	1,320	1,320	1,320	1,320	1,320	1,320	1,320	1,320
R-squared	0.158	0.160	0.154	0.156	0.153	0.155	0.158	0.159
Std Error Clustered				NAICS2				
Firm NAICS2 fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 2. 4 Robustness

This table reports regression estimates from linear probability models explaining the likelihood of receiving a PPP loan. Panel A considers the effect of the exposure and policy responses to Covid-19 on the allocation of PPP loans by including the following four variables: (1) Log (1+ # of Covid-19 Cases), defined as the logarithm of the number of positive Covid-19 cases as of April 3, 2020 (when the PPP was launched) in the state, MSA, or county where the firm is headquartered; (2) Declarations of a state of emergency, (3) Business closures, and (4) Safer-at-home policies. In Panel B, we consider past lending relationships between borrowers and lenders. *Past lending relationships* is a dummy variable equal to one if the firm borrows at least a loan that ends on and after 2015 from the PPP lender that acts as lead lender of the loan, and zero otherwise. *Personal connections index* is an indicator variable that equals one if any of the firm's directors or executives shares an education, employment, or nonprofit connection with directors or executives at the lending financial institution, and zero otherwise. *Educational (Previous employment, Nonprofit organizations)* is an indicator variable that equals one if the firm's directors or executives shares an education (previous employment, nonprofit) connection with directors or executives at the lending financial institution, and zero otherwise. *Average log(network) dummy* is a dummy variable equal to one if the average of log (network) is above median, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-lender-loan triplet. The standard errors are clustered by NAICS2 sectors. The t-statistics are given in parenthesis. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

Panel A: Local Exposures and Policy Responses to Covid-19

	(1)	(2)	(3)	(4)	(5)	(6)
	Prob (Treatment = 1)					
	State-level #cases	MSA-level #cases	County-level #cases	County Emergence Declaration	County Business Closure policy	County Safer-at-home policy
Personal connections index	0.0800***	0.0812***	0.0803***	0.0831***	0.0809***	0.0814***
	(3.190)	(3.236)	(3.198)	(3.129)	(3.112)	(3.131)
Network size indicator	-0.0150	-0.0149	-0.0181	-0.0165	-0.0126	-0.0139
	(-0.314)	(-0.327)	(-0.386)	(-0.357)	(-0.258)	(-0.297)
log(1+ # Covid-19 Cases)	0.0128	0.00200	0.0107			
	(1.101)	(0.218)	(1.127)			
County-level policy				-0.0517	0.0743	0.0258
				(-1.172)	(1.314)	(0.526)
Log(total assets)	0.00803	0.00762	0.00843	0.00730	0.00800	0.00774
	(1.471)	(1.414)	(1.652)	(1.281)	(1.571)	(1.333)
Leverage	0.00837*	0.00850*	0.00870*	0.00878**	0.00876**	0.00846**
	(2.001)	(2.092)	(2.077)	(2.166)	(2.102)	(2.105)
Constant	0.207	0.314***	0.249***	0.374***	0.314***	0.315***
	(1.644)	(3.853)	(4.033)	(3.549)	(4.204)	(3.297)
Observations	1,382	1,382	1,382	1,382	1,382	1,382
R-squared	0.027	0.026	0.028	0.028	0.028	0.027
Std Error Clustered	NAICS2					
Firm NAICS2 fixed effect	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Past Lending Relationship

	(1)	(2)	(3)	(4)
		Prob (Treatment = 1)		
Educational	0.0870**			
	(2.159)			
Previous employment		0.0712*		
		(1.732)		
Nonprofit organizations			0.0458**	
			(2.408)	
Personal connections index				0.0833***
				(3.073)
Past lending relationships	0.250***	0.258***	0.262***	0.252***
	(4.060)	(4.436)	(4.745)	(4.251)
Network size indicator	0.00311	0.0175	0.0156	0.00262
	(0.0746)	(0.347)	(0.322)	(0.0600)
Log(total assets)	-0.000625	-0.00128	-0.000917	-0.00112
	(-0.0964)	(-0.199)	(-0.147)	(-0.165)
Leverage	0.00636*	0.00569	0.00608	0.00642*
	(1.739)	(1.576)	(1.629)	(1.807)
Constant	0.460***	0.484***	0.478***	0.461***
	(4.624)	(4.782)	(4.846)	(4.481)
Observations	1,320	1,320	1,320	1,320
R-squared	0.167	0.164	0.163	0.167
Std Error Clustered		NAICS2		
Firm NAICS2 fixed effects	Yes	Yes	Yes	Yes
Firm MSA fixed effects	Yes	Yes	Yes	Yes

Table 2. 5 Returning PPP Loans

This table reports regression estimates from linear probability models explaining the likelihood of returning a PPP loan. Panels A and B present the results without and with controlling for past lending relationships, respectively. *Educational (Previous employment, Nonprofit organizations)* is an indicator variable that equals one if the firm's directors or executives shares an education (previous employment, nonprofit) connection with directors or executives at the lending financial institution, and zero otherwise. *Personal connections index* is an indicator variable that equals one if any of the firm's directors or executives shares an education, employment, or nonprofit connection with directors or executives at the lending financial institution, and zero otherwise. *Average log(network) dummy* is a dummy variable equal to one if the average of log (network) is above median, and zero otherwise. All variables are defined in Appendix A. The unit of observation is a firm-lender-loan triplet. The standard errors are clustered by the NAICS2. The t-statistics are given in parenthesis. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

Panel A: Baseline Effects

	(1)	(2)	(3)	(4)
	PPP loan return dummy			
Educational	0.0678*			
	(1.945)			
Previous employment		0.0776*		
		(1.993)		
Nonprofit organizations			0.0842**	
			(2.432)	
Personal connections index				0.0795**
				(2.336)
Network size indicator	-0.00280	0.00947	0.00313	-0.00438
	(-0.0864)	(0.308)	(0.124)	(-0.175)
Log(total assets)	0.0384***	0.0373***	0.0375***	0.0380***
	(4.250)	(4.265)	(3.891)	(3.937)
Leverage	0.00283	0.00214	0.00303	0.00297
	(1.117)	(0.917)	(1.207)	(1.206)
Constant	-0.570***	-0.541***	-0.552***	-0.577***
	(-3.696)	(-3.636)	(-3.457)	(-3.652)
Observations	560	560	560	560
R-squared	0.199	0.198	0.200	0.201
Sample	Only PPP firm-lender-loan			
Std Error Clustered	NAICS2			
Lender fixed effects	Yes	Yes	Yes	Yes

Panel B: Past Lending Relationship

	(1)	(2)	(3)	(4)
	PPP loan return dummy			
Educational	0.0647*			
	(1.914)			
Previous employment		0.0746*		
		(1.905)		
Nonprofit organizations			0.0810**	
			(2.486)	
Personal connections index				0.0777**
				(2.530)
Past lending relationships	0.106	0.108	0.106	0.108
	(1.183)	(1.162)	(1.152)	(1.217)
Network size indicator	-0.000537	0.0112	0.00503	-0.00245
	(-0.0159)	(0.350)	(0.193)	(-0.0949)
Log(total assets)	0.0335***	0.0323***	0.0327***	0.0329***
	(3.120)	(3.018)	(2.902)	(2.914)
Leverage	0.00207	0.00139	0.00226	0.00220
	(0.810)	(0.579)	(0.899)	(0.901)
Constant	-0.489**	-0.459**	-0.472**	-0.494**
	(-2.690)	(-2.560)	(-2.543)	(-2.703)
Observations	560	560	560	560
R-squared	0.203	0.202	0.205	0.205
Sample	Only PPP firm-lender-loan			
Std Error Clustered	NAICS2			
Lender fixed effects	Yes	Yes	Yes	Yes

Table 2. 6 Cross-sectional Analyses

This table reports the cross-sectional regression estimates from linear probability models explaining the likelihood of receiving a PPP loan. *Personal connections index* is an indicator variable that equals one if any of the firm's directors or executives shares an education, employment, or nonprofit connection with directors or executives at the lending financial institution, and zero otherwise. *Average log(network) dummy* is a dummy variable equal to one if the average of log (network) is above median, and zero otherwise. *# of analysts* is the number of analysts following the firm during 2019. *Media* is calculated as log(1+ the number of news about the firm with relevance =100 in year 2019). *Independent directors* is a dummy equal to one if the share of independent directors is above the sample median. *E-index* is constructed following Bebchuk et al. (2009). All variables are defined in Appendix A. The unit of observation is a firm-lender-loan triplet. The standard errors are clustered by the NAICS2. The t-statistics are given in parenthesis. *, **, *** represent significance at the 10%, 5% and 1% level, respectively.

	(1)	(2)	(3)	(4)
C-S variable:	Log(1+ # of analysts)	Media	Independent directors	E-index
Personal connections index	0.136***	0.343**	0.0234	0.0961
	(3.758)	(2.195)	(0.426)	(1.510)
Personal connections index * c-s variable	**-0.0495****	**-0.0665***	**0.101**	**0.00310**
	(-2.493)	**(-1.748)**	**(1.014)**	**(0.266)**
c-s variable	0.0591**	0.0625**	0.000298	0.000130
	(2.483)	(2.394)	(0.00413)	(0.0127)
Network size indicator	-0.0126	-0.0231	-0.0186	-0.0197
	(-0.245)	(-0.397)	(-0.454)	(-0.484)
Log(total assets)	-0.00509	-0.0160	0.0118	0.0120
	(-0.554)	(-1.371)	(1.291)	(1.262)
Leverage	0.00658*	0.00361	0.00849	0.00819
	(1.771)	(0.279)	(1.235)	(1.273)
Constant	0.500***	0.551***	0.238	0.234
	(3.245)	(3.710)	(1.630)	(1.635)
Observations	1,320	1,097	830	830
R-squared	0.163	0.180	0.161	0.158
Firm NAICS2 fixed effect	Yes	Yes	Yes	Yes
Firm MSA fixed effect	Yes	Yes	Yes	Yes

Appendix 2.A Variable definition

Variable	Description	Data Source
Treatment	An indicator variable equal to one if the firm receives a PPP loan after April 3, 2020, and zero otherwise.	8-K, 10-Q/K, S&P Market Intelligence, FactSquared, SBA website
Connections		
Personal connections index	An indicator variable equal to one if any directors or executives of the firm has educational, previous employment, or non-profit organization connection with any directors or executives who have influence on credit allocation of the PPP lender, and zero otherwise.	BoardEx
Educational connection	An indicator variable equal to one if any directors or executives of the firm and any directors or executives who has influence on credit allocation of the PPP lender graduated from the same educational institution with the same degree, and zero otherwise.	BoardEx
Previous employment connection	An indicator variable equal to one if any directors or executives of the firm and any directors or executives who has influence on credit allocation of the PPP lender worked in the same place or served on the same board at the same time before, and zero otherwise.	BoardEx
Nonprofit organizations connection	An indicator variable equal to one if any directors or executives of the firm and any directors or executives who has influence on credit allocation of the PPP lender participated in the same social organizations on and after Jan 01, 2019, and zero otherwise.	BoardEx
Past lending relationships	An indicator variable equal to one if the firm borrows at least a loan that ends on and after 2015 from the PPP lender that acts as lead lender of the loan, and zero otherwise.	DealScan
Firm Characteristics		
Network size	The network size of an individual (number of overlaps through employment, other activities, and education)	BoardEx
Network size indicator	An indicator variable equal to one if the average of the log(network size) of all the individuals (board members, or disclosed earners) of the firm is above the sample median, and zero otherwise	BoardEx

Variable	Description	Data Source
Total Assets	Book value of total assets, from the most recent available observation of 2019.	Quarterly Compustat
Leverage	Current liabilities plus total long-term debt scaled by book value of total assets, from the most recent available observation of 2019.	Quarterly Compustat
Market-to-Book	Quarterly close price times common shares outstanding minus total common equity, scaled by total assets, from the most recent available observation of 2019	Quarterly Compustat
ROA	Operating Income Before Depreciation scaled by total assets, from the most recent available observation of 2019	Quarterly Compustat
Number of employees	Number of Employees, from the most recent available observation after 2018	Annual Compustat Footnote
Media	Log(1+ the number of news about the firm with relevance =100 in year 2019). Only news from Dow Jones Newswires, and those that are not "Press release" are included.	Ravenpack
Log(1+ # of analysts)	# of analysts is the number of analysts following the firm during 2019	IBES
Lender Characteristics		
Independent directors	A dummy equal to one if the share of independent directors is above the sample median. Share of independent directors is calculated as the number of independent directors, scaled by the total number of directors.	ISS
E-index	Entrenchment index constructed following Bebchuk et al. (2009) based on 6 antitakeover provisions.	ISS
Covid-19		
State Covid-19 Cases	The number of Covid-19 positive cases in the state of the firm headquarter, as of April 03, 2020	New York Times
MSA Covid-19 Cases	The number of Covid-19 positive cases in the MSA are of the firm headquarter, as of April 03, 2020	New York Times
County Covid-19 Cases	The number of Covid-19 positive cases in the county of the firm headquarter, as of April 03, 2020	New York Times
County Emergency Declaration	An indicator variable equal to one headquarter county of the firm has the Emergency Declaration, and zero otherwise	National Association of Counties

Variable	Description	Data Source
County Business Closure Policy	An indicator variable equal to one headquarter county of the firm has the Business Closure policy, and zero otherwise	National Association of Counties
County Safer-at-Home Policy	An indicator variable equal to one headquarter county of the firm has the Safer-at-home policy, and zero otherwise	National Association of Counties
Other		
PPP loan return dummy	An indicator variable equal to one if the PPP firm announce to return or have returned the PPP loan to the lender, and zero otherwise	8-K, 10-Q/K, S&P Market Intelligence, FactSquared

Appendix 2.B Sample Construction

		Number of PPP firms	Number of PPP firm-lender	Number of PPP firm-lender-loan
	All firms disclose approvals of PPP loans from 8-K, 10-Q/K, S&P Market Intelligence, FactSquared, and SBA website	971		
Require 1	With Ticker available	940		
Require 2	Matched to 2019 quarterly Compustat with total assets and leverage, headquarter and listed in the US	740		
Require 3	Matched to BoardEx	738		
Require 4	With identified lender and loan approval date	679	716	722
Require 5	Matched to one non-PPP firms in the same 2-digit NAICS or 1-digit NAICS industry, with the difference in total assets and leverage less than 50%	652	687	692
	Final	652	687	692

Chapter 3: Lender Coordination and Loan Renegotiation

This paper employs an unsupervised machine learning technique to detect informal groups, termed as cliques, capturing the tight multidimensional interconnections among lenders. We hypothesize that the sharing of information about members within a clique can foster collective actions, thereby reducing coordination costs. Consistent with this prediction, we find an increased likelihood and intensity of loan renegotiation when a higher percentage of participant lenders belong to the same clique as the lead lender. The results are robust to controlling for the bilateral relationships between each pair of participant and lead lenders, the centrality of a lead arranger in the entire loan market, and borrower-year fixed effects. We also use the proposal of Basel III as a plausible exogenous shock to validate that our results are not driven by unobservable time-varying lender characteristics. Cross-sectionally, our analysis reveals that this positive association is more pronounced when the value of renegotiation rises, or when coordination costs increase. Specifically, we find a more pronounced positive association when (1) the economic uncertainty of the borrowing firm is higher, (2) the lead lender has less experience, and (3) the number of lenders in a syndicate increases. Importantly, we find these tight interconnections can improve loan outcomes via renegotiation. However, the dissemination of borrower information within cliques can be costly for borrowers with high proprietary costs. Overall, our results show that the tight multidimensional interconnections can serve as a mechanism to reduce the coordination costs among lenders, and consequently improve the contracting efficiency.

3.1 Introduction

Incomplete contract theory, pioneered by Grossman and Hart (1986) and subsequently extended to various corporate finance areas, underscores the critical role played by the allocation of control rights and renegotiation.[57] Since loan contracts are inherently incomplete, renegotiation serves as a vital mechanism for allocating control rights across different states (Garleanu and Zwiebel, 2009). Recent empirical research (e.g., Roberts and Sufi 2009; Roberts 2015; Nikolaev 2018) shows that renegotiation is prevalent in syndicated loan markets.[58] In a standard syndicated loan, multiple lenders collectively provide credit, and amendments of loan provisions require consensus among the majority of lenders (Caskey, Huang, and Saavedra, 2021).[59] The heterogeneity in lenders' incentives gives rise to coordination issues in the renegotiation process. Research (Asquith et al., 2005) suggests when coordination costs are high, performance pricing grid provision can help reduce the necessity for ex-post renegotiation. Yet, to the best of our knowledge, no study has explored whether any ex-ante mechanism exists to lower coordination costs, which could facilitate renegotiation leading to Pareto-improving outcomes.[60] This is the focus of our study.

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[57] Refer to Bolton and Scharfstein (1996) and Bolton and Freixas (2000; 2006) for financial contracts; Aghion and Bolton (1992) and Maskin and Tirole (1999) for capital structure; and Diamond and Rajan (2000, 2001, 2005) for banking.

[58] Robert (2015) shows that each loan experiences 3.5 renegotiations over an average stated maturity at origination of 51 months. Nikolaev (2018) finds similar statistics with a more comprehensive sample of renegotiation.

[59] Roberts and Sufi (2009) show the mean and median numbers of lenders are 8.6 and 6, respectively, for loans experiencing renegotiation.

[60] Asquith et al. (2005) find that renegotiation costs, measured by whether a loan is syndicated or not, are positively associated with the inclusion of interest-decreasing pricing grid, suggesting that performance pricing grid is one contracting mechanism to avoid debt renegotiation costs. However, Roberts and Sufi (2009) show the inclusion of pricing grid is *not* associated with the occurrence of renegotiation, suggesting that pricing grid inclusion does not affect renegotiation.

This paper, grounded in network theory, employs a machine-learning technique to construct *cliques*, capturing the *tightness of the multidimensional interconnections* among lenders. A clique represents a group of lenders whose members are interconnected through repeated interactions in the syndicated loan market (Figure 1.A). We propose that the tight interconnections within a clique can act as a mechanism to reduce coordination costs among syndicate lenders, thereby facilitating renegotiation and ultimately improving loan performance.

Previous research on syndicated loans has primarily focused on bilateral relationships, that is, one-to-one connections between a participant lender and a lead lender, to assess their interest alignments (e.g., Ivashina, 2009), or on lead lender centrality to understand their informational advantage (e.g., Pena Romera, 2019). In contrast, our study employs cliques to gauge the coordination cost for a given loan. This approach is grounded in social network literature that highlights the advantages of cliques, where all members are interconnected, in promoting cooperation and collective actions over alternative community structures (e.g., Coleman, 1988; Ali and Miller, 2010; Jackson et al., 2012; Crane et al., 2019). The superiority of cliques lies in two aspects.

First, multidimensional interconnections within a clique can enhance information sharing and communication among members. Information can travel through the entire clique, without relying on a central player, and consequently lower the information asymmetry between *any* pair of clique members. In the syndicated loan setting, when more participant lenders are part of the same clique as the lead lender (hereafter, lead-lender clique), the likelihood of adverse selection, i.e., selecting a less capable lead lender, decreases. This can reduce coordination challenges during the loan renegotiation.

Second, the sharing of information about members within a clique can alleviate moral hazard concerns, fostering collective actions in a syndicated loan. Clique members face the risk of losing multiple relationships if they fail to cooperate in any given relationship. For instance, if a member deviates from agreed-upon collective behaviors in interactions with any other member, such information can spread throughout the entire clique, potentially threatening the relationships with other members. This aspect differs from the networks with one central player, where information may not be shared among non-central players who are not interconnected. As shown in Figure 1.B, illustrating a traditional community structure centered around Player A, if Player A misbehaves with Player B, the information may not propagate to Players C, D, E, F, and G. Therefore, in the context of a syndicated loan, we expect information sharing among clique members can discipline lead lenders by improving their monitoring effort, which in turn increases participant lenders' cooperative incentives.

Due to the superiority of cliques in fostering collective actions, we first hypothesize that a higher proportion of participating lenders sharing the same clique with the lead lender reduces the coordination costs, leading to a higher likelihood and a greater intensity of renegotiation. [61] Second, if information sharing through multidimensional interconnections within a clique improves lead lenders' monitoring efforts, we expect the loan to perform better.

To empirically identify lender cliques, we employ an unsupervised machine learning technique, i.e., the Louvain algorithm, following prior studies (e.g., Blondel et al., 2008; Crane et al., 2019).

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[61] For 43% of our sample loans, the share of credit contributed by clique lenders exceeds the threshold of lenders' ownerships required for loan amendment, reinforcing the view that lender interconnectedness can facilitate renegotiation.

[62] To capture the tight multidimensional interconnections, the algorithm identifies cliques within which any pair of lenders is connected with each other. We consider two lenders to be connected in a given year if they have shared at least one syndicated loan every year for the past three consecutive years. This empirical approach is motivated by network theory, which suggests repeated social interactions provide a strategic foundation to form a clique (e.g., Raub and Weesie, 1990; Ali and Miller, 2012). Although cliques identified by the Louvain algorithms are constructed based on one-to-one connections, they require the interconnections between any pair of lenders, which sets them apart from both bilateral relationships and networks centered on a single central player, as shown in Figure 1.

To examine whether the proportion of participant lenders belonging to the same clique as the lead arranger for a given loan is associated with lower coordination cost, we focus on syndicated loans with data available in DealScan. We use the likelihood and frequency of renegotiations to gauge coordination costs. Following Nikolaev (2018), we collect the data on renegotiations from SEC filings, supplemented by facility amendments in DealScan. Our final sample consists of 23,983 facilities, corresponding to 16,861 unique loan packages and 3,116 unique borrowers, from 1995 to 2017.[63] For each syndicate, we calculate the percentage of participant lenders (p-lender) from the same clique as the lead lender, *Same_Clique%*. We focus on the cliques of lead

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[62] Unsupervised learning is a type of machine learning technique commonly utilized to extract meaningful patterns from data (Segal et al., 2005; Abbe et al., 2017; Li et al., 2020). In our study, we employ network analysis with graphical models as our specific unsupervised machine learning tool. Network analysis provides a comprehensive framework for understanding the intricate and high-dimensional relationships among a group of subjects. This technique enables us to identify patterns and trends in the relationships between individuals in a system, shedding light on how these relationships influence the behavior and structure of the system.

[63] The sample ends in 2017, which is determined by the availability of DealScan-Compustat Link provided by Chava and Roberts (2008).

lenders, because prior literature shows that lead lenders play the primary role of forming a syndicate and monitoring the borrower (e.g., Ball, Bushman, and Vasvari, 2008; Ivashina, 2009).

We begin our analysis with examining whether the likelihood and the frequency of renegotiation increase with *Same_Clique%*. Consistent with our hypothesis, we find a higher likelihood and frequency of renegotiations for syndicates with a higher *Same_Clique%*. In economic terms, a one-standard-deviation increase in *Same_Clique%* is associated with an approximately 5% higher likelihood of renegotiations, which is both statistically and economically significant considering the unconditional mean of 55%. Our results are robust to controlling for the percentage of p-lenders that have bilateral relationships with the lead arranger. A higher percentage of this number implies more p-lenders are from the network in which the lead lender holds a central position. Thus, the impact of multilateral relationships among lenders in a syndicate, as shown in our main results, cannot be solely due to the intensity of the bilateral relationships. To further corroborate our main results, we also examine the time leading to the first renegotiation for each syndicate, and find the first renegotiation occurs earlier when the loan has higher *Same_Clique%*. A one-standard-deviation increase in *Same_Clique%* is associated with the first renegotiation occurring approximately half of a month earlier, which is a 2% reduction relative to the unconditional mean of 30 months. Collectively, our findings support that syndicating loans with clique members have lower coordination costs.

One potential endogeneity concern arises as the lead lender can choose the syndicate structure (i.e., *Same_Clique%*). Unobservable lender characteristics that influence this choice can be correlated with the likelihood and intensity of renegotiation, thereby explaining the positive relation we find. To alleviate this concern, we employ a regulation shock that provides plausible exogenous variation in our variable of interest, *Same_Clique%*. Following Irani et al. (2021), we use Basel III proposed in 2012 Q2 in the U.S. as a shock to the capital constraints of commercial banks.[64] Gopalan et al. (2011) suggest that banks, when experiencing negative shocks, are more likely to syndicate loans with lenders with whom they have established previous relationships. The rationale is that previous lending relationships reduce information asymmetry between the lead and participants, and improves lead's incentive to monitor borrowers, which in turn results in better loan performance and improves regulatory capital. Thus, we conjecture that the unexpected rise in capital requirements due to the proposed Basel III increases the propensity of commercial banks to syndicate loans with clique members, particularly for lender cliques with a higher percentage of commercial banks, since they are more likely to be affected by the regulation change. Consistent with this prediction, we find that following the proposal on raising capital requirements, the propensity of a lead lender to syndicate loans with clique members increases significantly when the lead lender is from a clique with more commercial banks. This evidence empirically validates our maintained assumption of using the exogenous variation in *Same_Clique%* caused by Basel III to re-examine the main hypothesis. Next, we employ a research design that resembles a difference-in-difference analysis, and investigates whether the relationship of both the likelihood and frequency of renegotiations with the ratio of commercial banks in a lead lender's clique increases in the post-period. We find confirmative results. In addition, our results are also insensitive to the inclusion of lender fixed effects. Taken together, these findings demonstrate the robustness of our main results, alleviating the concern that the

[64] Following Irani et al. (2013), we focus on the proposal of Basel III, as banks began responding to the proposed regulation by increasing their regulatory capital requires even before the official implementation took place. The implementation of Basel III in the U.S. began in January 2014.

main results are solely due to correlated omitted unobservable time-varying lender characteristics.

Next, we conduct three cross-sectional tests to provide further support for our hypothesis that lender cliques facilitate renegotiation by reducing coordination costs. First, we explore cross-sectional variation in borrower uncertainty. Prior research (Grossman and Hart 1986; Roberts 2015) suggests that, due to uncertainty, debt contracts are incomplete, and renegotiation plays an important role in restoring equilibrium. Therefore, we expect our results to be more pronounced for borrowers with higher uncertainty. Consistent with this prediction, we find that in the presence of greater borrower uncertainty, i.e., when the volatility of the borrower's operating cash flows increases by 1%, *Same_Clique%* increases the renegotiation likelihood by an additional 13%. The finding suggests that lender cliques play a more important role in facilitating loan renegotiation to improve debt efficiency, when borrowers operate with greater uncertainty.

Second, we investigate whether our results vary with lead-lender experience. Lead lenders play a crucial role in drafting loan contracts. An inexperienced lead lender may face challenges in designing an optimal contract that accurately and adequately reflects borrower credit risk through loan terms. This scenario renders ex-post renegotiation more valuable to make up for lead-lender inexperience. As such, we expect our results to be stronger for inexperienced lead lenders. We find that *Same_Clique%* increases the renegotiation likelihood by an additional 12% when the lead lenders are less experienced.

Third, as prior studies suggest coordination costs increase with the number of lenders (e.g., Bergman and Callen 1991; Brunner and Krahnen 2008), we examine whether the main results are stronger when a lending syndicate involves a larger number of lenders. We find that *Same_Clique%* increases the renegotiation likelihood by an additional 5% for syndicates with a larger group of lenders, i.e., above the sample median of seven. The result is consistent with the view that the benefits of lender cliques are manifested when the coordination cost would otherwise be high. Overall, our cross-sectional evidence buttresses our main findings, reinforcing that multidimensional relationships in a lending syndicate facilitate coordination among lenders, and is pivotal for the occurrence of renegotiation to achieve Pareto-improvement outcomes.

Next, we test the second hypothesis of the positive relation between *Same_Clique%* and loan performance. We measure loan performance based on borrowers' credit ratings within loan maturity. We find that borrowers of loans with higher *Same_Clique%* have a lower probability of being downgraded to a default rating before the loan matures. In addition, we find this relation mainly concentrates on loans that have at least one renegotiation before maturity, suggesting that the benefits of loans having a higher proportion of clique lenders mainly operate through renegotiation. Taken together, the evidence from testing the two hypotheses suggests strong multidimensional relationships among lenders play an important role in reducing coordination costs, as they alleviate information frictions and moral hazard; as a result, renegotiations take place more frequently and timely, and loans are less likely to default.

While our evidence demonstrates that interconnections among lenders improve loan performance, potentially benefiting borrowers, a question remains: are there any associated costs? We seek to answer this question by exploring the potential costs from the borrowers' perspective. Carrizosa and Ryan (2017) suggest borrowers bear proprietary costs when lending institutions disseminate borrowers' private information. Since a larger clique size facilitates the dissemination of borrowers' private information widely, we expect that borrowers with high proprietary costs are less likely to borrow from lead lenders with large clique size. Consistent with this prediction, we find that the size of the lead-lender clique decreases by approximately 12%, when the borrower has R&D expenses and intangible assets above the sample median and also redacts contracts from the SEC. This finding thus highlights one downside of lending cliques – information exchange can facilitate the dissemination of borrowers' private information, increasing borrowers' proprietary costs.

Finally, we conduct batteries of additional tests to demonstrate the robustness of our findings. First, we attempt to rule out the possibility that our findings are due to our variable of interest, *Same_Clique%*, capturing the centrality of lead lenders. We follow Pane Romera (2019) and construct three alternative measures for the centrality of a lead lender and show that our main findings are robust to including any of these measures. Second, we replace our main independent variable *Same_Clique*% with an alternative measure, *Same_Clique_Share*, defined as the percentage of loan amount contributed by same-clique p-lenders following Caskey, Huang, and Saavedra (2021). We find our main results are robust to this alternative measure. Third, we find our main results continue to hold when including the interaction of borrower and facility start year fixed effects in our main analysis, which eliminates the possibility that certain time-varying borrower characteristics can drive our main results. Fourth, we restrict our sample to the largest facility within a package, to address the concern that the serial correlation of multiple facilities within a loan package drives our findings. The main results are robust to this subsample.

Our paper contributes to the literature in three ways. First, it offers an insight into the mechanism for lender coordination in the syndicated loan market. While the incomplete contracting theory (Aghion and Bolton 1992; Hart and Moore 1994; 1998; and Bolton and Scharfstein 1990; 1996) highlights the important role of renegotiation in improving contracting efficiency, and extant empirical research demonstrates the prevalence of renegotiations in the private debt market (Roberts and Sufi 2009; Robert 2015; Nikolaev 2018), how lenders coordinate to address renegotiation costs still remains a puzzle. This is particularly perplexing, given that a syndicate typically involves seven to eight lenders and requires majority voting for loan amendments. We argue that tight multilateral interconnections among lenders constitute one important mechanism for lenders to coordinate. We empirically show that syndicated loans with a greater number of lenders who have established tight multilateral interconnections tend to have more timely and frequent renegotiations, supporting our hypothesis that these interconnections reducing coordination costs.

Second, we contribute to the empirical syndicated loan research by developing a measure capturing multilateral economic relationships among lenders. Existing empirical work largely treats connections between the lead and participant lenders as bilateral (i.e., one-to-one connection), rather than multidimensional. For example, prior research shows that the number of p-lenders that have bilateral relationships with the lead lender is associated with loan pricing (Ivashina, 2009) and the voting rules for loan renegotiations (Caskey, Huang, and Saavedra, 2021), and that more central lead arrangers have access to more information about borrowers (Pena Romera, 2019). Diverging from these studies, we focus on the clique formed by tight multidimensional interconnections among lenders, an aspect underexplored in debt contracting literature. Our empirical evidence reveals that syndicating loans with clique members results in a higher likelihood and intensity of renegotiation, and improves loan performance. More importantly, this effect cannot be substituted by bilateral relationships. Our findings align with previous economic and finance studies, which suggest that cliques are superior in enhancing coordination among lenders, since deviations from any agreed-upon collective actions within a clique can result in forfeiture of gains from future cooperation with clique members (e.g., Bagwell and Staiger, 1996; Maggi, 1999; Crane, Koch, and Michenaud, 2019).

Lastly, we contribute to the literature examining the potential proprietary costs for borrowers when lending institutions possess their private information. Kim et al. (2015) and Carrizosa and Ryan (2017) suggest that lender trading can disseminate borrower information to the market. Our evidence that borrowers with high proprietary costs are less likely to syndicate loans with lead lenders from large cliques implies that borrowers are concerned that information exchange among interconnected lenders, resulting in significant proprietary costs. Moreover, this finding can be informative for bank regulators. While banking regulations that promote interconnectedness in lending markets can improve both bank and borrower performance, they may adversely affect borrower welfare. Consequently, regulators should carefully weigh this trade-off when designing policies.

The rest of the paper proceeds as follows. Section 2 presents the literature review and hypothesis development. Section 3 presents how cliques are empirically constructed. Section 4 describes our data. Section 5 presents the empirical results. Section 6 concludes.

3.2 Literature Review and Hypothesis Development

Renegotiation is viewed as an action taken by parties involved in a contract when there exists an ex-post surplus under the revised terms of the contract (Roberts and Sufi, 2009). Renegotiation would be unnecessary in a frictionless world, where the initial contract is complete. However, in real world, it is almost impossible for borrowers and lenders to nail down any possibly onerous or restrictive terms in the initial contract. Frictions arising from bounded rationality, transaction costs, and non-verifiability of information can result in incomplete contracts (Hart, 1995). Such contractual incompleteness gives rise to the opportunities of Pareto-improving renegotiation, when new information arrives and/or outside options change (e.g., Bolton, 1990; Roberts, 2015). Therefore, renegotiation is an important mechanism for dynamically completing contracts and resolving ex-post inefficient outcomes.

However, renegotiations can be costly for syndicated loans due to the existence of multiple lenders. After the original financing of a syndicated loan, each lender retains the authority to vote on any changes to the governing agreement during the life of the loan. As Taylor and Sansome (2006, p.360) explains, the basic rule of voting in a credit agreement is that the "majority" or "required" lenders must approve any modification, waiver, or supplement to any provision of the credit agreement. "Majority" or "required" typically means lenders holding more than 50 percent of the aggregate credit exposure. As to changes in loan terms such as interest rate, amortization and amount of commitment, it commonly requires unanimous consent of any affected syndicated members. Therefore, consensus is important or even necessary under certain circumstances in the renegotiation process. The presence of multiple lenders and their heterogeneous incentives can make consensus costly and difficult to achieve. With an increase in the number of lenders in a syndicate, a single lender is less likely to be pivotal in renegotiation (Bergman and Callen, 1991), and decision-making takes longer time which increases opportunity costs (e.g., options foregone) and reduces the probability of workout success (Brunner and Krahnen, 2008). Moreover, Van den Steen (2010) suggests that, even holding the number of investors fixed, dispersion in preferences among creditors can lead to costly coordination.

Given the costly coordination involved in the renegotiation process with multiple lenders, we build upon prior studies in network literature and hypothesize that cliques informally formed by tight multidimensional interconnections among lenders can reduce coordination costs during renegotiation, thereby improving loan performance. Unlike groups where every member is linked to one central player, cliques are characterized by interactions between any two members within a clique, resulting in multilateral relationships. The clique structure can foster coordination through two mechanisms.

First, multilateral information sharing among clique members can reduce ex-ante adverse selection problems by alleviating information asymmetry between participant lenders and lead arrangers. Therefore, when more p-lenders belong to the same clique as the lead lender, the likelihood of p-lenders initially choosing a less reliable lead lender with hidden actions decreases. Consequently, the willingness of cooperation among lenders is enhanced, which in turn facilitates reaching an agreement during the renegotiation process.

Second, information exchanges among clique members can align collective actions and mitigate the moral hazard problems ex post in syndicated loans. As suggested by theoretical network coordination models (e.g., Coleman, 1988; Ali and Miller, 2010; Jackson et al., 2012), the threat of losing gains from future cooperation with other clique members encourages individual members to conform to the collective behaviors of the group. Specifically, if a member deviates from agreed-upon collective behaviors, information about the deviation can circulate throughout the entire clique, potentially jeopardizing the member's relationships with all other members in the same clique. Prior empirical work in economics and finance supports these theoretical predictions, showing that cliques foster coordination in multilateral trade policies in international trade settings (e.g., Bagwell and Staiger, 1996; Maggi, 1999) and that institutional investors from the same clique are more likely to vote together on proxy terms (Crane, Koch, and Michenaud, 2019). Building upon these studies, we argue that interconnections within a lender clique can reduce moral hazard by disciplining lead lenders to monitor borrowers effectively and boosting p-lenders' cooperative incentives, which ultimately lowers coordination costs. The reduced coordination costs can prompt p-lenders from the lead-lender clique to vote together with the lead arranger, facilitating the loan renegotiation process.

Based on the two advantages of clique structures, we propose our first hypothesis:

H1: The proportion of participant lenders from the same clique as the lead lender is positively associated with the likelihood of renegotiation and the frequency of renegotiations.

If the interconnections among clique members lead to more Pareto-improving renegotiation and encourage lead lenders to engage in effective monitoring, loans with a higher percentage of participant lenders from the same clique as the lead lender are likely to perform better. This prediction leads to our second hypothesis, stated as follows:

H2: The percentage of participant lenders from the same clique as the lead lender is positively associated with loan performance.

3.3 Clique construction

This section discusses how we empirically capture the high-density interconnections among lenders by constructing cliques in a network. A clique refers to a group (or a community) of individuals such that every two distinct individuals are connected, essentially yielding a highly connected and decentralized system. One important feature of cliques that is different from independent one-to-one connections is that any pair of lenders in the clique are connected to each other. The difference is illustrated in Figure 1. The relationship depicted in Panel A of Figure 1 is an ideal example of clique. For all seven lenders, any two randomly picked lenders are connected. By contrast, the relationship in Panel B is not a clique, because not every two lenders are connected.

We take two steps to empirically identify cliques formed in the syndicated loan market in a given year *t*. First, we construct an adjacency matrix for lenders in year *t* based on our definition of pairwise connections. We define a pair of lenders as connected in a year *t* if they co-fund at least one syndicated loan every year in the past three consecutive years. This empirical construction is motivated by prior studies suggesting repeated interactions play a vital role in the enforcement of cooperative strategies in a network (e.g., Raub and Weesie, 1990; Ali and Miller, 2012). After defining the connection between a pair of lenders, we then construct an $N_t \times N_t$ adjacency matrix of lender-to-lender relationship for all lenders in the market for each given year, where N_t is the total number of lenders in year *t*. The off-diagonal elements take a value of one if the corresponding pair of lenders are identified as connected, and zero otherwise. The diagonal entries of the adjacency matrix are set as zero by default in network analysis. Note that the dimension N_t of the adjacency matrix corresponds to the total number of lenders in our case and thus could be ultra-high dimensional.

In the second step, we apply a community-detection algorithm to identify cliques in year *t*, based on the adjacency matrix constructed in step one. A variety of community-detection algorithms are developed in the past decades which enable us to solve the clique-detection problem approximately.[65] Our study adopts the widely used Louvain algorithm based on a prevalent degree-corrected stochastic block model (e.g., Blondel et al., 2008; Karrer and Newman, 2011; Chen, Li and Xu, 2018; Crane et al., 2019). The employed Louvain algorithm assigns a lender to one specific clique in year *t*. The algorithm determines the clique assignment by maximizing the

[65] A perfect clique detection remains rather challenging in network analysis due to the significant computational difficulty, and would be infeasible given the size and the complexity of the considered network of lenders coordination. Therefore, empirical results obtained from all community-detection algorithms are approximations.

corresponding modularity, measured as the density of connections inside the cliques relative to connections outside the cliques. Meanwhile, the adopted degree-corrected stochastic block model enforces the individuals in the same clique to share similar connectivity pattern and thus pursues a decentralized structure within the clique.[66] The number of cliques and the size of each clique are completely determined by the data. These two steps are then repeated for each year in our sample. Figure 2 illustrates an example of cliques constructed with our data in a randomly picked year.

3.4. Data

3.4.1 Sample Selection

We collect data on syndicated loans from DealScan, which provides extensive loan details such as start and end dates, loan amounts, lender composition, and pricing and non-pricing terms. We only include loans with at least one lead and one participant lender, i.e., excluding sole-lender loans, as we are interested in investigating the coordination among lenders in a syndicated loan. To identify lead lender, we follow Ivashina (2009) and classify a lender titled administrative agent as the lead lender. If administrative agent is missing in a syndicate, we classify agent, arranger, bookrunner, lead arranger, lead bank, or lead manager as lead lender. To construct lender cliques, we start with syndicate loans from 1992 onwards, as data before then are of low quality. Our final sample starts in 1995 because, to identify the clique membership of lenders in each syndicate loan, we require each lender's syndicate records from the previous three years.

[66] One special case could be a lender is connected to two cliques in an identical way. For example, a lender is fully connected to all lenders in both Clique A and Clique B. In this special scenario, the algorithm assigns the lender to a clique with members that have similar relationships with the rest of lenders on the market (i.e., those that are neither in Clique A or Clique B) as the lender of interest.

The sample ends in 2017, which is determined by the availability of DealScan-Compustat Link provided by Chava and Roberts (2008).

Next, we obtain facility-level renegotiation data from two sources: (1) SEC EDGAR filings, following Nikolaev (2018), and (2) DealScan amendments. To identify when a facility experienced amendment, we first search for keywords related to amendments or renegotiations in forms 8-K, 10-K and 10-Q of the borrowing firm. To ensure data accuracy, we collect renegotiation data for both syndicates and sole-lender loans in DealScan, though our main analysis focuses on syndicates. This step allows us to compare the data with Nikolaev (2018). Combining the two types of loans, we find 57,532 renegotiations for 16,983 facilities between 1995 and 2017. On average, a facility experiences approximately three renegotiations, the same as the average number (i.e., 3.06 = 12,451/40,558) in Nikolaev (2018). To ensure completeness, we further supplement the renegotiation data with facility amendments in DealScan. We find 15,216 more renegotiations for 7,401 facilities in addition to the ones identified in the SEC filings.

Last, we merge in borrower characteristics for all syndicated loans in our sample, using Compustat annual database via the DealScan-Compustat Link provided by Chava and Roberts (2008). This step reduces our sample size to 3,116 unique borrowing firms, 4,228 unique lenders and 23,983 facilities from 1995 to 2017. We note that the sample sizes can vary across analyses due to the data availability. For all continuous variables, we winsorize them at the 1th and 99th percentiles.

3.4.2 Descriptive Statistics

Table 1 presents the summary statistics of main variables used in our main analysis. The unit of observation is at the facility level. With the focus on syndicated loans, our sample consists of borrowing firms and loans that are relatively large. The average (median) firm in the sample owns $8,396 (1,559) million in assets, has an average (median) asset growth of 22% (7%), and an average (median) leverage of 29% (26%). These firms are larger than the ones in Nikolaev (2018), which includes both sole lender and syndicated loans, with an average (median) firm size of $2,560 (251) million. This leads to higher sample means and medians for both the likelihood and intensity of renegotiations. Specifically, around 55% of facilities experienced at least one renegotiation in our sample, with a median (mean) of four (nine) renegotiations per facility.

Figure 3 presents the time-series characteristics of cliques. In Panel A, the blue line represents the total number of lenders in the syndicated loan market for each year, while the red line shows the number of lenders belonging to a clique. Three observations are worth noting. First, we find that the fraction of lenders in cliques has increased over time, suggesting greater collaboration in clique fashion among lending institutions. Second, there is a rise in the total number of lenders in the earlier sample period, consistent with deregulations in the banking industry at that time, allowing both commercial and non-commercial banks easier access to the syndicated loan market (Jiang, Li, and Shao, 2011). Moreover, we observe a significant decrease in the number of lenders starting in 2007, coinciding with the onset of the financial crisis. Finally, the sharp decline in the number of lenders in 2017 is due to data availability issues, as the DealScan and Compustat mapping does not provide complete coverage for loans originated in that year.

In Panel B of Figure 3, the blue line depicts the number of syndicates originated each year throughout our entire sample period, while the red line represents the number of syndicates that have at least one p-lender belonging to the same clique as the lead lender. We find that the proportion of syndicates that have at least one same-clique p-lender has grown over time, suggesting an increasing tendency for lenders to syndicate with same-clique p-lenders. As with

Panel A, the sharp decline in 2017 is due to data availability issues. Taken together, Figure 3 suggests that there is an increasing trend of lenders collaborating with each other, and forming syndicates with fellow clique members.

Figure 4 further delves into the characteristics of clique membership within a syndicated loan. The x-axis shows the number of participant lenders in a syndicate. The left y-axis displays the number of same-clique p-lenders, with the red line representing their average count for a given number of p-lenders in a syndicate. The right y-axis indicates the frequency of syndicates with specific numbers of p-lenders, illustrated by the blue bars. We observe a decreasing frequency of syndicates as the number of p-lenders increases, consistent with previous studies suggesting that coordination costs rise with the total number of lenders in a loan (e.g., Bergman and Callen 1991; Brunner and Krahnen 2008). More interestingly, we find the number of same-clique p-lenders exhibits a linear relation with the total number of p-lenders in a syndicate, which provides preliminary evidence that lead lenders are more likely to syndicate loans with same-clique p-lenders when the coordination costs are expected to be high.

3.5. Empirical Analyses

3.5.1 Lender cliques and renegotiations

We begin our analysis by studying how the likelihood and frequency of renegotiations are associated with the percentage of p-lenders in the same clique with the lead lender. We estimate the following regression model using our main sample:

$$
\begin{aligned}
&Renegotiation_{i,j,t} \\
&\quad = b_1 * Same\ Clique\%_{i,t-1} + b_2 * One\ to\ One_{i,t-3\ to\ t-1} + b_3 \\
&\quad * Rev\ One\ to\ One_{i,t-3\ to\ t-1} + \boldsymbol{b}' * Borrower\ Char_{j,t-1} + \boldsymbol{c}' * Facility\ Char_{j,t} \\
&\quad + Industry\ FE + Start\ \ Year\ \ FE + \varepsilon \qquad\qquad (1)
\end{aligned}
$$

where *i* represents a facility originated in year *t* to the borrowing firm *j*. The dependent variable *Renegotiation* is measured in two ways: (1) *Reneg_Dummy*, defined as an indicator variable taking the value of one if facility *i* experiences at least one renegotiation, and zero otherwise; (2) *Ln_Reneg_Count*, defined as the natural logarithm of one plus the total number of renegotiations that facility *i* experiences. The main independent variable of interest, *Same_Clique%*, is measured as the percentage of p-lenders that are part of the same clique as the lead lender. For example, a syndicate with five participants and one of them is part of the same clique as the lead lender, the value of *Same_Clique%* is 0.2. Cliques are identified in year *t-1*, based on bilateral relationships observed from *t-3* to *t-1*. For facilities with multiple lead lenders, we average over all lead lenders within a facility.

Turning to the control variables, we first include two proxies to capture the bilateral relationships between a lead lender and each p-lender, following Ivashina (2009). The first proxy, *One_to_One*, is defined as the percentage of p-lenders in the focal facility *i* who also participated in other loans arranged by that facility's lead lender. The second proxy, *Rev_One_to_One*, is defined as the percentage of p-lenders that arranged syndicated loans in the past three years, and the lead lender in the focal facility *i* was a participant in those loans (i.e., lead banks and p-lenders switched their roles). For facilities with multiple lead lenders, we average over all lead lenders within a facility. As shown in Figure 1.B, these one-to-one connections around a lead lender can form a core-periphery network structure, placing the lead lender in a central position.

Thus, higher values of *One_to_One* and *Rev_One_to_One* can capture the percentage of p-lenders that are from a network where the lead lender holds a central position.

Furthermore, we include controls for facility characteristics and time-varying borrower characteristics. Facility characteristics include *Log(Amount)*, *Log(Maturity)*, *Revolver*, and *BtoK*. *Revolver* indicates if a facility is a revolving loan, which poses higher risks for lenders compared to term loans, as revolving loan borrowers can continually borrow and repay funds (e.g., Asquith et al. 2005; Berlin et al., 2020). *BtoK* indicates if a facility belongs to Term B through Term H. These facilities are intended for sale to nonbank institutional investors, where lead lenders lose control rights after the transaction (Taylor and Sansome, 2006; Nini, 2008). This might weaken the incentives to renegotiate. For borrower characteristics, we include *Log(Total_Assets)*, *ROA*, *MTB*, *R&D_Dummy*, *Asset_Growth*, and *Asset_Intensity*, following Roberts and Sufi (2009) and Nikolaev (2018). All borrower characteristics are measured in the most recent fiscal year-end before the facility starts. Last, we include facility start-year fixed effects, and industry fixed effects defined by two-digit SIC codes. Standard errors are clustered at the package level.

Table 2 presents the results estimated from equation (1). Column (1) shows the empirical results without any fixed effects included, while column (2) adds fixed effects specified above. In both specifications, we find the main independent variable, *Same_Clique%*, loads with a positive and statistically significant coefficient. In economic terms, a one-standard-deviation increase in the percentage of p-lenders from the same clique as the lead lender corresponds to a 5% higher likelihood of renegotiations, which is economically meaningful relative to the unconditional mean of 55%. Moving to columns (3) and (4), we replace the binary outcome variable, *Reneg_Dummy*, with the continuous one, *Ln_Reneg_Count*, and the results continue to hold. A one-standard-deviation increase in *Same_Clique%* leads to a 1% increase in the number of renegotiations. This is economically largerelative to the unconditional mean of nine.

Turning to the coefficients on control variables, the two proxies for bilateral relationships between p-lenders and each lead lender do not show positive relations with the occurrence of renegotiations. There are two potential explanations. First, the two proxies have a high correlation with each other (approximately 0.5). Second, for more than half of the facilities in our sample, all p-lenders have bilateral relationships with the lead lender, as shown in Table 1. Therefore, there is limited variation in this variable. For facility characteristics, we find loan maturity to be positively associated with the likelihood and intensity of renegotiations, consistent with the findings in Robert and Sufi (2009). For borrower characteristics, *R&D_Dummy* exhibits a statistically significant negative relation with the likelihood and intensity of renegotiations, consistent with the results in Nikolaev (2018). This finding implies that borrowers with R&D investment opportunities are less likely to structure debt contracts in ways that trigger frequent renegotiations because of shareholder-creditor conflicts over investment decisions.

Collectively, we find results consistent with the argument that more p-lenders from the same clique as the lead lender reduces the coordination costs during loan renegotiations. Notably, the results are robust when controlling for one-to-one connections between each p-lender and the lead lender, suggesting the role of lender cliques is incremental to the independent bilateral relationships between each p-lender and the lead lender.

3.5.2 Endogeneity concern

One potential endogeneity concern arises as our main results can be driven by unobservable time-varying lender characteristics, since the lead lender can choose the syndicate structure. To mitigate this concern, we explore the Basel III regulation proposed in 2012. This regulation provides an opportunity to explore the plausibly exogenous variations in same-clique lenders syndicating loans together, i.e., our main independent variable in equation (1) *Same_Clique%*. On June 7, 2012, U.S. federal banking agencies announced the proposed adjustments to the types of capital counted in Tier 1 capital and risk-weights on numerous real estate exposures. These adjustments resulted in increased capital requirements for commercial banks in the U.S. (Berrospide and Edge, 2016).[67] Irani et al. (2021) investigate how banks react to the regulation proposal and find that commercial banks indeed increase their regulatory capital by a great amount in a short time period right after the proposed adjustments were announced. Their evidence suggests this regulation change is a negative shock to commercial banks. Prior studies find that, when faced with negative shocks, banks are more likely to syndicate loans with lenders with whom they have established previous relationships (e.g., Gopalan et al., 2011). The reasoning behind this is that prior lending relationships can reduce the information asymmetry between the lead and participant lenders. This increases the lead lender's incentive to monitor borrowers, subsequently improving loan performance and regulatory capital. Building upon these findings, we conjecture that commercial banks (CBs) are more likely to fund loans together with clique members after the regulation change, relative to the pre-period. Therefore, we expect to see members in a clique with a higher percentage of CBs are more likely to collaborate with each other in the post-period. In other words, facilities set up by lead lenders in cliques with a higher percentage of CB are more likely to include participant lenders from the same clique, i.e., higher *Same_Clique%*. As Irani et al. (2021) find that CBs react to the proposal shortly after its

[67] www.federalreserve.gov/newsevents/pressreleases/bcreg20120607a.htm

announcement, we focus on a short window around it, i.e., two years before and after 2012, and estimate the following regression model:

$$
\begin{aligned}
Same\_Clique_t\% \\
&= b_1 * CB\%_{i,t-1} * Post_t + b_2 * CB\%_{i,t-1} + b_3 * Post_t + b_4 \\
&* One\_to\_One_{i,t-3\ to\ t-1} + b_5 * Rev\_One\_to\_one_{i,t-3\ to\ t-1} + \boldsymbol{c}' * Facility_{Char_{i,t}} \\
&+ Industry\ FE + Start\ \ Year\ \ FE + \varepsilon \qquad (2).
\end{aligned}
$$

CB% represents the percentage of commercial banks within the clique that the lead lender belongs to. Commercial banks are identified by the classification of financial institutions in DealScan following Jiang, Li, and Shao (2010). *Post* is an indicator variable that takes the value of one for years after 2012, and zero otherwise. We exclude facilities initiated in year 2012 for this analysis to eliminate any potential anticipation impact right before the proposal announcement.

Column (1) of Table 3 presents results. We find the interaction term, *CB%*Post*, loads with a positive coefficient that is statistically significant at the 10% level. This result is consistent with our prediction – lead lenders from cliques with a higher percentage of CBs are more likely to syndicate loans with clique members in the post-period. More importantly, this evidence lends empirical support to our underlying assumption that the Basel III proposal can generate exogenous variation in *Same_Clique%*, allowing us to re-examine the main hypothesis.

Relying on the results in column (1), we then conduct an analysis that resembles the differences-in-differences research design to exploit the exogenous variation in *Same_Clqiue%* and test its impact on loan renegotiations. Specifically, we estimate the following regression model:

$$Regenegotiation_{i,j,t} = b_1 * CB\%_{i,t-1} * Post_t + b_2 * One\_to\_One_{i,t-3\ to\ t-1} + b_3 * Rev\_One\_to\_one_{i,t-3\ to\ t-1} + \boldsymbol{b}' * Borrower\_Char_{j,t-1} + \boldsymbol{c}' * Facility\_Char_{i,t} + Industry\ FE + Start\ \ Year\ \ FE + \varepsilon \quad (3)$$

Columns (2) and (3) of Table 3 present the results. In both columns, we find that the interaction term, *CB%*Post*, loads with a positive coefficient that is statistically significant at the 5% level. The coefficient on *Post* is subsumed by year fixed effects. Further, the variable *CB%* loads with an insignificant coefficient, mitigating the concern that our results are driven by the pre-period trend. In addition, in columns (4) to (6), we find that the results are robust to including lender fixed effects.

Collectively, these findings mitigate the concerns about our main results being driven by unobservable lender characteristics and lend further support to our main hypothesis. That is, the likelihood and frequency of renegotiations both increase when lead lenders form syndicates with a high percentage of same-clique p-lenders, suggesting that more p-lenders from the same clique reduce coordination costs in syndicated loans.

3.5.3 Cross-sectional analyses

Next, we conduct cross-sectional analyses to investigate whether the heightened likelihood and occurrence of renegotiations indeed reflect lower coordination costs due to a higher proportion of same-clique p-lenders. Specifically, we explore whether the main results in Table 2 vary across (1) the uncertainty of borrowers' future performance, (2) the experience of the lead lenders, and (3) the number of lenders in syndicated loans.

5.3.1 Uncertainty of a borrower's future performance

Increased uncertainty in a borrower's future performance can complicate the prediction of their ability to fulfill loan payments, resulting in a higher demand for ex-post renegotiation (Grossman and Hart 1986; Roberts 2015). Moreover, borrower uncertainty can exacerbate the information disadvantage faced by p-lenders, resulting in higher coordination costs (Sufi, 2007; Ivashina, 2009). Therefore, we predict that the positive relation between renegotiation and the percentage of same-clique p-lenders is more pronounced when a borrower has greater uncertainty. To capture borrower's uncertainty, we use the variable *CFO_Volatility*, defined as the volatility of a borrower's cash flow from operations, measured over a five-year window preceding the loan initiation (Nikolaev, 2018). To address potential outlier concerns, we convert this variable into a percentile rank. In the regression analysis, we interact *CFO_Volatility* with all our independent variables and fixed effects in equation (1). The sample size for this analysis is smaller than that of Table (2) due to the availability of the five-year operation cash flow data.

Table 4, Columns (1) and (2) present the results of the likelihood and occurrence of renegotiations, respectively. In both columns, the main interaction term of interest, *Same_Clique%*CFO_Volatility*, loads with positive and statistically significant coefficients. In terms of the economic magnitude, when the volatility of the borrower's operating cash flows increases by 1%, *Same_Clique%* increases the likelihood of renegotiation by an additional 13% and the number of renegotiations by an additional 0.37.[68] These results highlight that lender

[68] *CFO_Volatility* is converted into a percentile rank in this regression analysis. When *CFO_Volatility* is increased by 1%, *Same_Clique%* increases the dependent variable, log(1+ *Reneg_Count*) by an additional 0.317. Using a natural logarithm transformation, we find an additional increase in *Reneg_Count*, i.e., the number of renegotiation, by 0.37.

cliques play a vital role in facilitating the renegotiation process, particularly in circumstances characterized by heightened borrower uncertainty, resulting in greater debt efficiency.

5.3.2 Experiences of lead lenders

Our second cross-sectional analysis investigates how the value of having clique members varies with the experience of lead lenders in arranging syndicates. In a syndicated loan, the experience of lead lenders can be pivotal, as they are typically responsible for coordinating the syndicate, including but not limited to negotiating the loan terms with the borrower, communicating with the group of lenders, and overseeing the disbursement of funds (Sufi, 2007). Less experienced lead lenders may design loan contracts that are less comprehensive. In such cases, renegotiation could become more crucial to address any unforeseen circumstances. Therefore, we predict syndicating with same-clique p-lenders can have a greater value for inexperienced lead lenders, as it reduces the communication costs with p-lenders. Thus, we anticipate the positive association between renegotiation and the percentage of same-clique p-lenders to be more pronounced for those loans arranged by inexperienced lead lenders. To test our prediction, we first create a dummy variable *Inexp_Lead_Lender* that identifies lead lenders with three years or less of experience in acting as lead lenders in the syndicate market. We then interact *Inexp_Lead_Lender* with all independent variable and fixed effects in equation (1).

Table 5, Columns (1) and (2) present the results of the likelihood and occurrence of renegotiations, respectively. In both columns, the main interaction term of interest, *Same_Clique%*Inexp_Lead_Lender*, loads with positive coefficients statistically significant at the 1% level. In economic terms, for less experienced lead lenders, *Same_Clique%* increases the likelihood of renegotiation by an additional 12%, and the number of renegotiations by an additional 0.27. Our findings of stronger results for loans arranged by inexperienced lead lenders supports the notion that lender cliques can help less experienced lead lenders to navigate the challenges in arranging syndicated loans by reducing the coordination costs.

5.3.3 Number of lenders in syndicated loans

Prior theoretical and empirical studies both suggest that coordination costs are positively associated with the number of lenders in a syndicate. This is because the more the lenders the greater the heterogeneity in their interests, and thus the longer the time to coordinate for decision-making and the lower the probability of workout success (e.g., Bergman and Callen 1991; Brunner and Krahnen 2008). Built upon this line of reasoning, we conjecture that the value of syndicating loans with clique members is greater when there are more lenders funding a syndicated loan. To test this prediction, we first create a dummy variable *Large_Group_Lenders* , which indicates whether the number of lenders in a given syndicated loan exceeds the sample median value of seven. We then interact *Large_Group_Lenders* with all independent variables and fixed effects in equation (1).

Table 6,Columns (1) and (2) present the results of the likelihood and occurrence of renegotiations, respectively. We find the main interaction term of interest, *Same_Clique%* Large_Group_Lenders*, loads with a positive coefficient that is statistically significant at the 10% level. In economic terms, *Same_Clique%* increases the likelihood of renegotiation by an additional 5% when the number of lenders is above the sample median. This finding supports our argument that having clique members is more beneficial when the coordination cost is likely to be higher, underscoring the .

3.5.4 Lender cliques and loan performances

Our results so far have supported our first hypothesis that multidimensional relationships within a lending syndicate can facilitate the renegotiation process. The next question is whether enhanced coordination can further improve loan outcomes. As posited in our second hypothesis, we expect the enhanced coordination can potentially lead to renegotiations that benefit both the lenders and borrowers, manifested by improved loan outcomes. Furthermore, involving more clique members in syndicated loans would motivate lead lenders to exercise greater diligence in monitoring borrowers throughout the loan's duration, which is also expected to contribute to improved loan outcomes. To empirically test the prediction, we create the dependent variable, *DG_to_Default*, defined as a dummy variable indicating whether the borrowing firm has ever downgraded to a default rating during the facility existing period. Following Cornaggia, Krishnan and Wang (2017), we define ratings of "D" and "SD" as default rating. We obtain credit ratings from S&P and drop those observations with missing credit ratings. This step leads to a significant decrease in the sample size. We then replace the dependent variable in equation (1) with *DG_to_Default* and estimate the regression model.

Table 7 presents the results. We find that borrowers involved in loans with a higher percentage of p-lenders from the same clique as the lead lender are less likely to experience a downgrade in their default rating throughout the loan's lifespan. In economic terms, an increase of one-standard-deviation in the percentage of same-clique prime lenders reduces the probability of being downgraded to default by 12% relative to the unconditional mean of 7%. To ensure that our results are not driven by ex-ante operating performance of the borrowing firm, we control for *ROA* in the most recent fiscal period end prior to the loan origination. We find *ROA* has a statistically significant negative relation with the likelihood of being downgraded to default rating. This result is consistent with the view that firms with better operating performance are less likely to be downgraded to a default rating. More importantly, our results on loan performance are not affected by controlling for the ex-ante borrower operating performance.

To further investigate the importance of renegotiation for the improved loan performance, we partition the sample into two groups based on whether renegotiation happens or not before the syndicate matures. If renegotiation matters for the better performance when the number of same-clique p-lender is larger in the syndicate, we expect our results in column (1) to be mainly driven by the sub-sample with at least one renegotiation. Columns (2) and (3) in Table 7 presents the results. We find the negative relation between *Same_Clique%* and *DG_to_Default* is only statistically significant when renegotiation happens, as shown in column (2). The findings highlight the positive impact of participating lenders from the same clique on loan performance, achieved through an increased likelihood of renegotiation, which further underscores the value of lender cliques.

Taken together, our findings from testing the two hypotheses suggest that multidimensional relationships among lenders play a vital role in lowering coordination costs, which facilitates the renegotiation process and consequently improves loan performance.

3.5.5 Additional Analyses and Robustness Check

In this section, we delve deeper into renegotiations by investigating other dimensions related to the main analysis and perform robustness tests to strengthen the validity of our main findings. In section 5.5.1, we examine how the time to first renegotiation is associated with the participation of clique members. In section 5.5.2, we test to see if our main findings are robust while controlling for different measures of the centrality of a lead lender. In section 5.5.3, we examine whether our main results are robust to (1) an alternative of our main independent variable--the percentage of loan amounts contributed by same-clique p-lenders, (2) including borrower-year fixed effects, and (3) restricting our sample to the largest facility in each package.

5.5.1 Time to the first renegotiation

To corroborate the results in Table 2, we investigate how the time to the first renegotiation is associated with the percentage of same-clique p-lenders in a facility. We anticipate syndicated loans with a higher percentage of clique members can not only reduce coordination costs but also motivates the lead lender to put more effort into post-origination monitoring. Therefore, a facility is likely to experience the first renegotiation sooner after its origination, when a higher percentage of p-lenders are from the same clique as the lead lender. To empirically test this prediction, we follow Nikolaev (2018) to employ a proportional hazard duration model (Cox, 1972):

$$\begin{aligned} h(t) = h_0(t)exp(b_1 * Same\_Clique\%_{i,t-1} + b_2 * One\_to\_One_{i,t-3\ to\ t-1} + b_3 \\ * Rev\_One\_to\_one_{i,t-3\ to\ t-1} + \boldsymbol{b}' * Borrower\_Char_{j,t-1} + \boldsymbol{c}' \\ * Facility\_Char_{i,t} + Industry\ FE + Start\ \ Year\ \ FE + \varepsilon \qquad (4) \end{aligned}$$

where $h_0(t)$ is a non-parametric baseline hazard function and t is the time to the first renegotiation in months. We include the same set of control variables and fixed effects as those in equation (1). A higher hazard rate implies a shorter time to the first renegotiation.

Table 8 presents the results. To facilitate interpretation, we present the marginal effects, which are inversely related to the duration of time leading up to the first renegotiation. The main independent variable of interest, *Same_Clique%*, loads with a positive and statistically significant coefficient. This result indicates that the percentage of same-clique p-lenders exhibit a positive association with the renegotiation hazard rate, which translates into a shorter time to the first renegotiations. In economic terms, a one-standard-deviation increase in *Same_Clique%* is associated with the first renegotiation occurring approximately half of a month earlier. This magnitude is considered economically meaningful, given that the unconditional mean is roughly 30 months. This evidence is in line with our main results, suggesting that coordination costs decrease when more p-lenders are from the same clique as the lead lender.

5.5.2 Does the percentage of same clique lenders merely capture the centrality of a lead lender?

In this subsection, we conduct additional analysis to examine whether clique membership differs from lead lender centrality, a network feature studied by Pena Roma (2019). Conceptually, network with one central player, i.e., the core-peripheral network, usually consists of non-central players all connected to one central player, but they do not interconnect with each other. This feature can result in difficulties in aligning the interests of all members in the network (e.g., Hojman and Szeidl, 2009). While a lead lender in a more central position tends to have greater information advantage over other lenders (Pena Roma, 2019), the information may not reach all members of the network. This is because non-central lenders are not interconnected with one another in the core-peripheral network. For example, as illustrated in Figure 1.B, if Player A behaves poorly in the interaction with Player B, the information may not be shared with Players C, D, E, F, and G. By contract, clique enforces discipline among all players and promotes coordination through a mechanism that relies on the sharing of information among all members, so that any deviation from the collective actions can result in losing future cooperation opportunities with clique members. Therefore, the centrality of a lead lender is *not* expected to impact coordination and loan outcomes through the same mechanism as lender cliques.

Empirically, we test whether our findings are robust to controlling for the centrality of the lead lender.

To empirically differentiate lender cliques from the centrality of a lead lender, we include two controls for the bilateral relationships, i.e., *One_to_One* and *Rev_One_to_One* in all our main analyses. As discussed in Section 5.1, these two variables capture the percentage of p-lenders from the network in which the lead lender has a central role. Our main results show that the impact of lender cliques is incremental to these two controls for one-to-one connections. In this subsection, we create three additional measures to capture the centrality of lead lenders, following Pena Romera (2019). The first measure is *Degree*, which captures the number of connections each lender has. The second measure is *Betweenness*, which captures the number of times that the lender lies on the shortest path between two other lenders. The third measure is *Eigenvector*, which captures the influence of a lender in a network and is calculated based on the relative importance of other lenders that the focal lender connected with.

We first test to see if our results on renegotiation are robust to adding these three measures of centrality. Panel A of Table 9 presents the results. To make sure the measures are comparable, we create the annual percentile rank of our independent variable and each of the centrality measure. We consistently find a positive coefficient on *Same_Clique*% that is significant at the 1% level, when each of the three measures for centrality is added in the regression analysis, respectively. Moreover, including the proxies for centrality does not reduce the magnitude of coefficients on *Same_Clique%*. These results, along with our main findings, indicate that the impact of lender cliques on reducing coordination cost cannot be absorbed by the centrality of lead lenders.

Second, we investigate whether the impact of lender cliques on loan outcomes is robust to adding in the three measures of centrality. To test this, we regress *DG_to_Default* on the annual percentile rank of *Same_Clique%* and each proxy for centrality, i.e., degree, betweenness and Eigenvector. Panel B of Table 9 presents the results. In Columns (1) to (3), we consistent find *Same_Clique%* has a statistically significant negative relation with *DG_to_Default*, when different proxies for centrality are included in the regression analysis. More importantly, we find the coefficients loaded on *Same_Clique%* are larger than those on the centrality measures. In untabulated results, we validate our constructions of centrality measures. We find consistent results with Pena Romer (2019). That is, when *Same_Clique%* is not included in the regression analysis, the variable for centrality loads with a negative and statistically significant coefficient.

5.5.3 Additional Robustness analyses

In this subsection, we conduct three additional sets of robustness analyses to lend further support to our main results in Table 2. First, we create an alternative measure for our main independent variable. While our primary measure for *Same_Clique%* is the number of same-clique p-lenders relative to the total number of p-lenders in a loan, in this subsection, we explore the percentage of loan amount contributed by lenders following Caskey, Huang, and Saavedra (2021). Specifically, we replace *Same_Clique%* in equation (1) with *Same_Clique_Shares*, measured as the percentage of loan amount contributed by same-clique p-lenders. Columns (1) and (2) of Table 10 present the results. We find that *Same_Clique_Shares* has a statistically significant positive relation with both the likelihood and the frequency of renegotiations. In addition, this analysis includes the control variable, *Threshold*, measured as the minimal percent of loan amount required for loan amendment. *Threshold* has a statistically significant negative relation with the likelihood and the frequency of renegotiations, consistent with the view that higher amendment thresholds lead to higher coordination costs in the renegotiations. We note that the sample size for these two tests is much smaller than that of our main analysis, due to the data availability issue when we require a facility to have data on renegotiations, shares contributed by lenders, and the amendment threshold. Overall, the evidence suggests that our main findings are insensitive to the measure of the degree of clique lenders participating in a loan.

Second, to eliminate the possibility that certain time-varying borrower characteristics serve as correlated omitted variables that drive our main results, we include the interaction of borrower and facility start year fixed effects in equation (1) and re-estimate the regression model. The sample for this analysis is smaller than that for our main analysis, since we drop singletons for all regression analysis. Borrower-year pairs with only one observation in the sample are eliminated. Columns (3) and (4) of Table 11 presents the results. All the control variables for borrower characteristics listed in equation (1) are dropped, as they are subsumed by the borrower-year fixed effects. We find the main independent variable of interest, *Same_Clique*%, still has a statistically significant positive relation with the likelihood and the number of renegotiations after adding borrower-year fixed effects. These findings suggest that our main results cannot be fully driven by any time-varying borrower characteristics.

Third, we only retain the facility with the largest loan amount within a package in our analysis, and re-estimate equation (1). Since our main sample is at the facility level, doing so helps us to address the concern that loan deals with multiple facilities drive our findings. Columns (5) and (6) of Table 11 present the results. Our results on both the likelihood and the number of renegotiations are robust when we only focus on the largest facility from each package.

3.5.6 Cost of Lender cliques

So far, our empirical analysis shows that with a higher proportion of p-lenders from the same clique, the syndicate has a higher probability to experience renegotiation and better loan performance. Our findings have revealed the benefits of lender cliques, and have demonstrated the results are robust to different specifications. In this section, we explore the cost of lender cliques. The improved information sharing among lenders can raise concerns about the spread of borrower proprietary information. In selecting which group of lenders to borrow from, firms face the trade-off to balance the need of better financing terms and the need to protect its proprietary information (Boone, Floros, and Johnson, 2016). We hypothesize that the costs of borrowing from lead lender that belongs to a larger clique involves costs of higher likelihood of disseminating proprietary information. Thus, we expect a negative relation between the size of the lead lender's clique and the proprietary cost of the borrower. To test this conjecture, we estimate the following regression model:

$$\begin{aligned} Log(Clique_{Size} + 1)_{i,j,t} \\ &= b_1 * Proprietary\ Cost_{i,t-1} + \boldsymbol{b}' * Borrower\_Char_{j,t-1} + \boldsymbol{c}' \\ &* Facility\_Char_{i,t} + Industry\ FE_j + Start\ \ Year\ \ FE_t + \varepsilon \qquad (5) \end{aligned}$$

To capture the borrower's proprietary cost, we use following measures. *R&D_cont* is the ratio of R&D expenditure, scaled by the total assets (Boone, Floros, and Johnson, 2016). *Redaction_dummy* is a dummy variable which indicates whether the firm has redacted material information from its financial statement or not (Chen, Tian and Yu, 2022). *Asset_Intensity*, which is calculated as the ratio of net property, plant, and equipment divided by total assets, is a reversed measure of proprietary information (Wyatt, 2005). We also create a comprehensive measure of proprietary information cost, *High_Proprietary*, which equal to one if the firm has positive R&D and redacts in the financial statement and asset intensity is below the sample median, and zero otherwise.

Table 12 presents the results estimated from equation (5). We find the clique size that the lead lender belongs to is negatively correlated with R&D expenditure and redaction, and negatively correlated with asset intensity. In addition, it is negatively correlated with our comprehensive measure, *High_Proprietary*. The findings are statistically significant at conventional level, and are consistent with the view that borrowers face the trade-off between lower borrowing cost and the risk of dissemination of proprietary information.

3.6 Conclusion

Building upon network theory, this study develops a novel measure to capture the tight multidimensional interconnections among lenders (cliques). We hypothesize that these tight interconnections can facilitate information sharing, serving as a mechanism to foster collective actions among clique members in syndicates. The enhanced information sharing can mitigate coordination issues among syndicate lenders, leading to improved loan outcomes.

Empirically, we use the Louvain algorithm, an unsupervised machine learning technique, to identify cliques. We find that a higher percentage of same-clique p-lenders in a syndicate is positively associated with the likelihood and intensity of renegotiation, and leads to a more timely first-time renegotiation. The results are robust to controlling for the bilateral relationships between the lead arrangers and p-lenders, and the centrality of the lead arranger in the entire loan market. Furthermore, the results continue to hold when we exploit a plausible exogenous variation in the percentage of same-clique p-lenders in a syndicate. Our findings are also robust when we include borrower-year fixed effects, and restrict our sample to the largest facility in each package.

In the cross-section, we find that the benefits of lender cliques are amplified when the value of ex-post renegotiation is high, or the coordination costs are heightened: our results concentrate on loans with more lenders involved, borrowers with more uncertain future performances, and inexperienced lead arrangers. Lastly, we find that these tight multidimensional relationships within a syndicate improve loan outcomes, achieved through renegotiations.

Besides the benefits of multidimensional relationships, we also explore the costs. We demonstrate that borrowers with greater proprietary costs are less likely to engage with syndicates arranged by lead lenders from large cliques. The findings suggest that borrowers trade off the benefits of raising funds from clique members against the cost of the potential increase in proprietary costs due to the information sharing within cliques.

Overall, our study helps understand the seemingly puzzling phenomenon that syndicated loans, typically involving multiple lenders with heterogeneous interests, are frequently renegotiated. Our findings highlight that tight multidimensional interconnections that are developed through repeated interactions in the lending market can improve lender coordination and significantly reduce renegotiation costs. As a result, contracting efficiency rises. This value is incremental to the bilateral relationships studied in prior studies (e.g., Ivashina, 2009; Caskey, Huang, and Saavedra, 2021).

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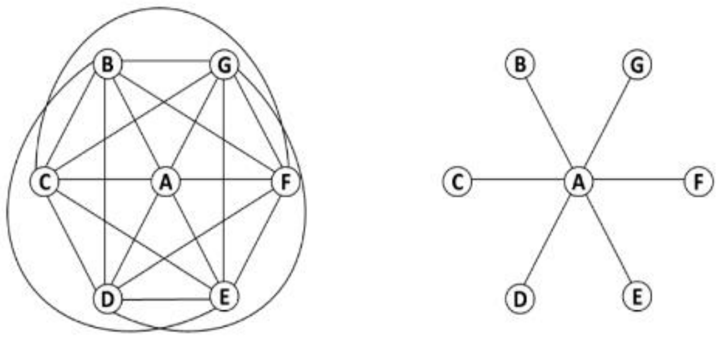
Figure 3. 1 Cliques vs. Network with a central player



A. Clique B. Network with a central player, not a clique

Panel A of Figure 1 illustrates the structure of a clique. Clique refers to a group of players (e.g., lending institutions) of which any pair of players within the group are connected. Panel B of Figure 1 illustrates a centralized network that is not a clique.

Figure 3. 2 Examples of lender cliques

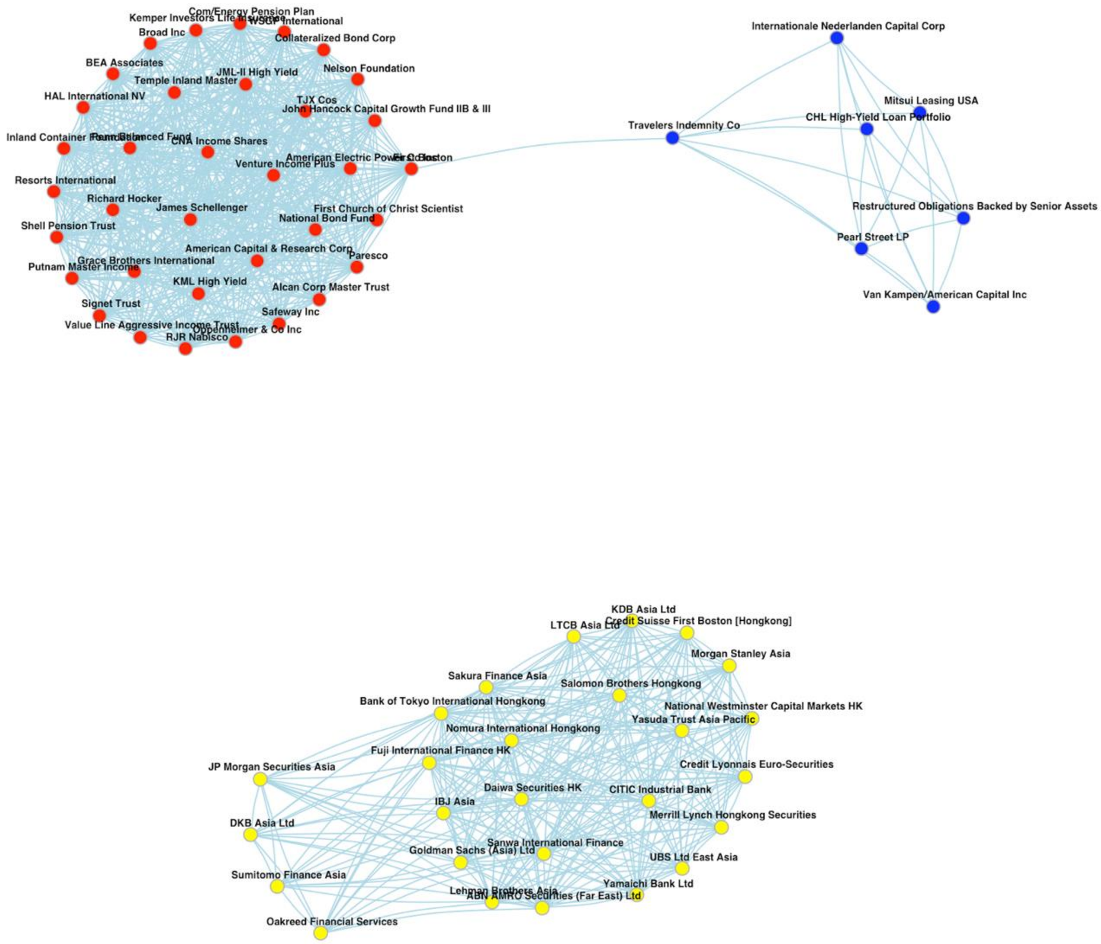
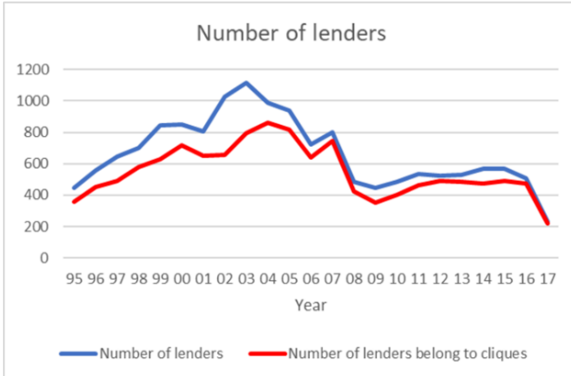


Figure 2 is an example of three real cliques of lenders constructed in our data. In each clique, every pair of lenders are connected.

Figure 3. 3 Time-series characteristics of cliques

Panel A Number of lenders belong to cliques in the syndicated market



Panel B Number of syndicates with at least one p-lender from the same clique as the lead lender

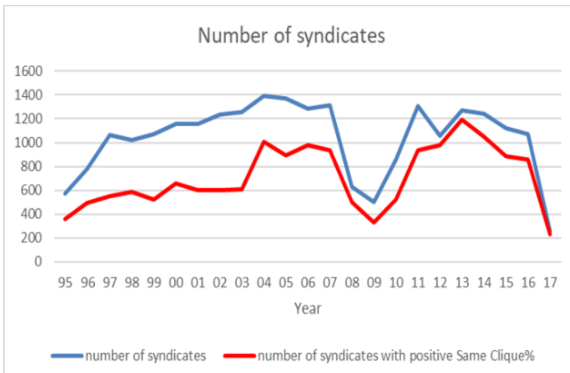


Figure 3 Panel A describes the yearly trend of the number of lenders in the syndicate market. The blue line represents the total number of lenders. The red line focuses on the total number of lenders that belong to a clique. Figure 3 Panel B describes the yearly trend of the number of syndicates initiated. The blue line represents the total number of syndicates initiated during the year. The red line focuses on the total number of syndicates with at least one participant lender that belongs to a clique.

Figure 3. 4 Clique membership within a syndicated loan

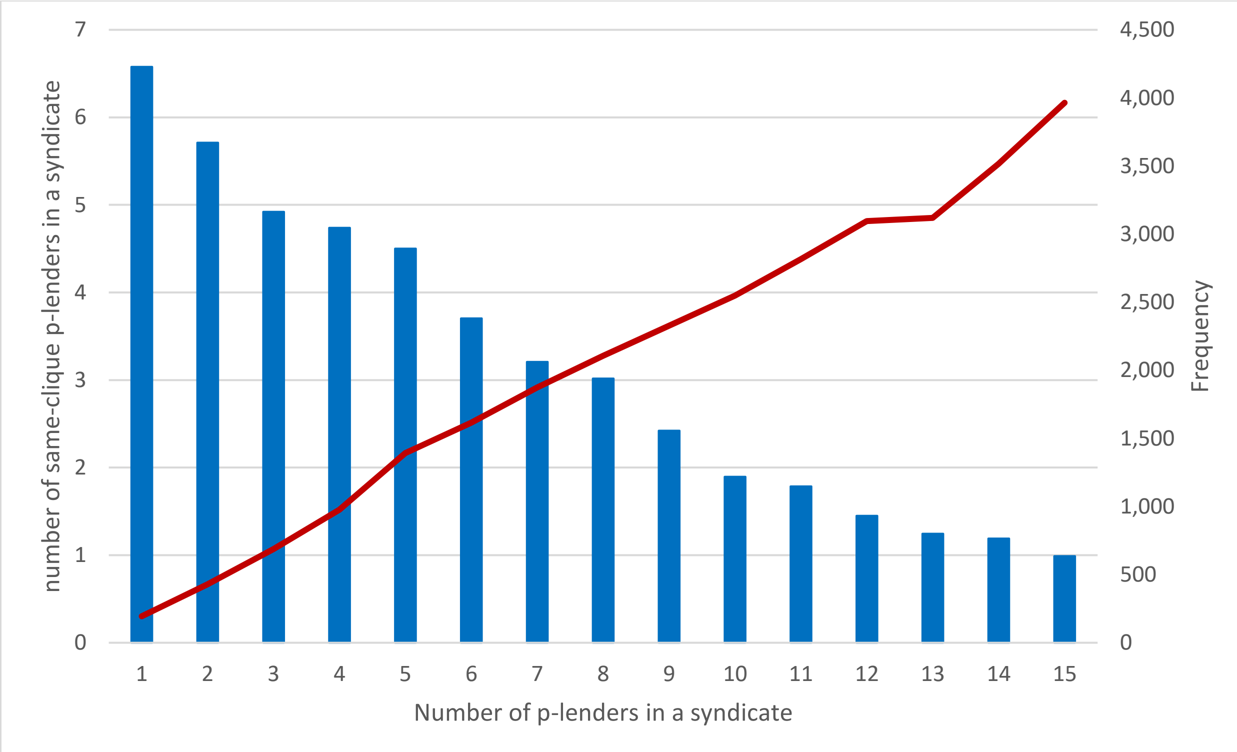


Figure 4 describes the number of same-clique p-lenders in a syndicate, and the frequency for the syndicates with a specific number of lenders in a Syndicate. The x-axis is the number of participant lenders in a syndicate. The left y-axis is the average of p-lenders that are in the same clique as the lead lender, represented by the red line. The right y-axis is the frequency of the syndicates with the specific number of p-lenders, represented by the blue bar.

Table 3. 1 Summary Statistics

Table 3.1 reports the summary statistics of variables in our analyses. See the detailed definitions of all variables in Appendix A.

Variable	N	mean	sd	p25	p50	p75
Outcome variables:						
Reneg_Dummy	23,983	0.55	0.50	0	1	1
Reneg_Count	23,983	9.54	15.06	0	4	12
Time_to_First_Reneg (in month)	23,983	30.47	25.61	9.87	18.63	59.83
DG_to_Default	9,333	0.07	0.26	0	0	0
Independent variables:						
Same_Clique%	23,983	0.38	0.35	0	0.33	0.67
Total Assets (in $Million)	23,983	8396	37945	505	1559	5088
ROA	23,983	0.03	0.08	0.01	0.04	0.07
Leverage	23,983	0.29	0.21	0.13	0.26	0.41
MTB	23,983	1.33	0.90	0.78	1.09	1.61
R&D_Dummy	23,983	0.31	0.46	0	0	1
Asset_Growth	23,983	1.22	0.53	0.99	1.07	1.23
Asset_Intensity	23,983	0.31	0.25	0.10	0.24	0.49
Amount (in $Million)	23,983	460	667	100	215	500
Maturity (in Month)	23,983	49.54	22.67	36	60	60
Revolver	23,983	0.58	0.49	0	1	1
BtoK	23,983	0.10	0.31	0	0	0
One_to_One	23,983	0.84	0.27	0.80	1	1
Rev_One_to_One	23,983	0.64	0.31	0.48	0.70	0.92
CB%	4,598	0.76	0.21	0.67	0.80	0.89

Table 3. 2 Lender cliques and loan renegotiation

This table presents results on whether renegotiation is associated with the percentage of p-lenders from the same clique as the lead lender. Columns (1) and (2) present the results with the likelihood of renegotiation as the dependent variable. Columns (3) and (4) present the results with the natural logarithm of one plus the number of renegotiations as the dependent variable. We control for characteristics of borrowing firms and loans. Standard errors are clustered by package. In columns (2) and (4), we include facility start year and firm industry fixed effects. Appendix A contains detailed variable definitions. *, **, and *** represent the statistical significance at 10%, 5%, and 1%, respectively (two-tailed). T-statistics are presented beneath the coefficient estimates in parentheses.

	(1)	(2)	(3)	(4)
VARIABLES	Reneg_Dummy	Reneg_Dummy	Ln_Reneg_Count	Ln_Reneg_Count
Same_Clique%	0.0681***	0.0349**	0.182***	0.0874**
	(5.539)	(2.505)	(5.225)	(2.229)
Log(Amount)	0.0257***	0.0272***	0.0819***	0.0850***
	(5.606)	(6.007)	(6.550)	(6.935)
Log(Maturity)	0.104***	0.114***	0.570***	0.607***
	(16.76)	(17.53)	(35.64)	(35.52)
Revolver	0.0508***	0.0435***	0.0890***	0.0755***
	(7.038)	(6.153)	(4.453)	(3.868)
BtoK	0.0398***	0.0230*	0.175***	0.135***
	(3.179)	(1.854)	(4.606)	(3.596)
Log(Total_Assets)	-0.0403***	-0.0479***	-0.134***	-0.143***
	(-10.51)	(-11.85)	(-12.73)	(-12.89)
ROA	-0.236***	-0.208***	-0.750***	-0.739***
	(-4.119)	(-3.551)	(-4.546)	(-4.437)
Leverage	-0.0571***	-0.0289	-0.122*	-0.0361
	(-2.605)	(-1.256)	(-1.908)	(-0.536)
MTB	-0.00227	-0.00335	-0.0266*	-0.0194
	(-0.440)	(-0.633)	(-1.823)	(-1.310)
R&D_Dummy	-0.0286***	-0.0192	-0.0969***	-0.112***
	(-3.030)	(-1.469)	(-3.653)	(-2.971)
Asset_Growth	0.00619	0.0187**	0.0671***	0.0793***
	(0.756)	(2.242)	(2.610)	(3.048)
Asset_Intensity	-0.0167	0.0318	0.0710	0.162**
	(-0.988)	(1.250)	(1.458)	(2.123)
One_to_One	-0.0113	-0.0199	-0.0666	-0.0741
	(-0.611)	(-1.084)	(-1.226)	(-1.383)
Rev_One_to_One	-0.0892***	-0.0722***	-0.255***	-0.214***
	(-5.494)	(-4.479)	(-5.506)	(-4.638)
Constant	0.560***	0.629***	0.642***	0.598***
	(8.069)	(8.286)	(3.277)	(2.797)
Observations	23,983	23,983	23,983	23,983
R-squared	0.050	0.081	0.114	0.146
Std Error Clustered	Package	Package	Package	Package
Facility start Year F.E.	No	Yes	No	Yes
SIC2 F.E.	No	Yes	No	Yes

Table 3. 3 Addressing the endogeneity concern

This table presents results with the proposal Basel III as a plausible exogenous shock to the percentage of p-lenders from the same clique for a syndicated loan. Columns (1) to (3) include facility start year and firm industry fixed effects, whereas columns (4) to (6) add one more set of fixed effects—the lead-lender fixed effects. Columns (1) and (4) present the validation evidence that the proposal of Basel III affects the percentage of p-lenders from the same clique. Columns (2) to (3) and (5) to (6) present the results of the impacts on renegotiations. We control for characteristics of borrowing firms and loans. Standard errors are clustered by package. Appendix A contains detailed variable definitions. *, **, and *** represent the statistical significance at 10%, 5%, and 1%, respectively (two-tailed). T-statistics are presented beneath the coefficient estimates in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Same_Clique%	Reneg_Dummy	Ln_Reneg_Count	Same Clique%	Reneg_Dummy	Ln_Reneg_Count
CB%	-0.110**	-0.0123	-0.0932	0.0331	-0.0323	-0.205
	(-2.434)	(-0.243)	(-0.641)	(0.747)	(-0.486)	(-1.067)
CB_ratio*Post 2012	0.663***	0.262**	0.810**	0.454***	0.471**	1.349***
	(6.921)	(2.066)	(2.538)	(6.010)	(2.574)	(3.315)
Log(Total_Assets)	0.00447	-0.0419***	-0.150***	0.00476	-0.0400***	-0.140***
	(0.805)	(-4.453)	(-5.893)	(0.868)	(-3.979)	(-5.146)
ROA	0.0231	-0.448***	-1.520***	0.00128	-0.468***	-1.495***
	(0.266)	(-3.711)	(-4.363)	(0.0170)	(-3.682)	(-4.158)
Leverage	-0.0244	-0.0379	0.0391	0.0125	-0.0565	0.0126
	(-0.701)	(-0.733)	(0.267)	(0.395)	(-1.026)	(0.0811)
MTB	0.00734	0.0142	0.0109	0.00628	0.0208	0.0227
	(0.793)	(1.076)	(0.293)	(0.778)	(1.586)	(0.611)
R&D_Dummy	0.0396**	-0.0146	-0.0686	0.00910	-0.0216	-0.0885
	(1.993)	(-0.492)	(-0.821)	(0.477)	(-0.716)	(-1.042)
Asset_Growth	-0.00441	0.0341	0.142**	0.00561	0.0356	0.146**
	(-0.392)	(1.617)	(2.215)	(0.410)	(1.599)	(2.161)
Asset_Intensity	0.119***	-0.0647	0.0216	0.0373	-0.0894	-0.0539
	(3.040)	(-1.120)	(0.124)	(1.086)	(-1.504)	(-0.303)
Log(Amount)	0.0139**	0.0314***	0.110***	0.00657	0.0279***	0.105***
	(2.523)	(3.026)	(4.103)	(1.226)	(2.645)	(3.832)
Log(Maturity)	0.00182	0.180***	0.822***	0.00712	0.180***	0.822***
	(0.148)	(9.506)	(19.40)	(0.636)	(9.593)	(19.25)
Revolver	0.0149	-0.0165	-0.0633	0.0128	-0.0209	-0.0683
	(1.482)	(-0.974)	(-1.362)	(1.436)	(-1.272)	(-1.521)
BtoK	-0.0244	-0.111***	-0.258***	-0.0170	-0.115***	-0.274***
	(-1.277)	(-3.685)	(-2.965)	(-0.989)	(-3.764)	(-3.191)

One_to_One	0.146***	-0.158***	-0.433***	0.137***	-0.213***	-0.552***
	(3.682)	(-3.265)	(-3.097)	(3.003)	(-3.717)	(-3.208)
Rev_One_to_One	0.217***	0.0292	0.0100	0.204***	-0.0134	-0.0960
	(7.790)	(0.754)	(0.0923)	(7.372)	(-0.314)	(-0.798)
Constant	-0.384***	0.225	-0.499	-0.249*	0.263	-0.564
	(-2.932)	(1.185)	(-0.989)	(-1.819)	(1.234)	(-1.005)
Observations	4,598	4,598	4,598	4,502	4,502	4,502
R-squared	0.305	0.078	0.128	0.475	0.120	0.168
Sample period	2010-2014	2010-2014	2010-20115	2010 2011 2013 2014	2010 2011 2013 2014	2010 2011 2013 2014
Std Error Clustered	Package	Package	Package	Package	Package	Package
Facility start Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
SIC2 F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Lead-lender F.E.	No	No	No	Yes	Yes	Yes

Table 3. 4 Cross-sectional analysis: Borrower uncertainty

This table presents how the association between same-clique p-lenders and renegotiation varies with the borrower uncertainty. The variable *CFO_Volatility* is measured as the volatility of the borrower's operating cash flows in the five years previous to the loan initiation. We transform this variable into a percentile rank to mitigate the impacts of outliers. We include the same set of controls as those in Table 2. We interact *CFO_Volatility* with all control variables, facility start year fixed effects, and firm industry fixed effects. Standard errors are clustered by package. Appendix A contains detailed variable definitions. *, **, and *** represent the statistical significance at 10%, 5%, and 1%, respectively (two-tailed). T-statistics are presented beneath the coefficient estimates in parentheses.

	(1)	(2)
VARIABLES	Reneg_Dummy	Ln_Reneg_Count
Same_Clique%	-0.0142	-0.0351
	(-0.390)	(-0.367)
Same_Clique%*CFO_Volatility	0.130**	0.317*
	(2.093)	(1.919)
Controls*CFO_Volatility included	Yes	Yes
Controls included	Yes	Yes
Observations	17,549	17,549
R-squared	0.493	0.547
Std Error Clustered	Package	Package
Facility start Year FE*CFO_Volatility	Yes	Yes
SIC2 FE*CFO Volatility	Yes	Yes

Table 3. 5 Cross-sectional analysis: Inexperienced lead lenders

This table presents how the association between same-clique p-lenders and renegotiation varies with lead-lender experience. The indicator variable *Inexp_Lead_Lender* takes the value of one if the facility has a number of lenders above the sample median, and zero otherwise. We include the same set of controls as those in Table 2. We interact *Inexp_Lead_Lender* with all control variables, facility start year fixed effects, and firm industry fixed effects. Standard errors are clustered by package. Appendix A contains detailed variable definitions. *, **, and *** represent the statistical significance at 10%, 5%, and 1%, respectively (two-tailed). T-statistics are presented beneath the coefficient estimates in parentheses.

VARIABLES	(1) Reneg_Dummy	(2) Ln_Reneg_Count
Same_Clique%	0.00383	0.0312
	(0.222)	(0.640)
Same_Clique%*Inexp_Lead_Lender	0.121***	0.244***
	(4.190)	(3.014)
Controls* Inexp_Lead_Lender	Yes	Yes
Controls included	Yes	Yes
Observations	23,983	23,983
R-squared	0.093	0.156
Std Error Clustered	Package	Package
Facility start Year F.E.* Inexp_Lead_Lender	Yes	Yes
SIC2 F.E.* Inexp_Lead_Lender	Yes	Yes

Table 3. 6 Cross-sectional analysis: The number of lenders in a syndicated loan

This table presents how the association between same-clique p-lenders and renegotiation varies with the number of lenders in a facility. The indicator variable *Large_Group_Lenders* takes the value of one if the facility has a number of lenders above the sample median, and zero otherwise. We include the same set of controls as those in Table 2. We interact *Large_Group_Lenders* with all control variables, facility start year fixed effects, and firm industry fixed effects. Standard errors are clustered by package. Appendix A contains detailed variable definitions. *, **, and *** represent the statistical significance at 10%, 5%, and 1%, respectively (two-tailed). T-statistics are presented beneath the coefficient estimates in parentheses.

VARIABLES	(1) Reneg_Dummy	(2) Ln_Reneg_Count
Same_Clique%	0.00433	0.0291
	(0.250)	(0.591)
Same_Clique%*Large_Group_Lenders	0.0494*	0.0762
	(1.758)	(0.970)
Controls* Large_Group_Lenders	Yes	Yes
Controls included	Yes	Yes
Observations	23,983	23,983
R-squared	0.104	0.167
Std Error Clustered	Package	Package
Facility start Year FE* Large_Group_Lenders	Yes	Yes
SIC2 F.E.* Large_Group_Lenders	Yes	Yes

Table 3. 7 Loan Performance

This table presents results on whether loan performance is associated with the percentage of p-lenders from the same clique as the lead lender. The outcome variable *DG_to_Default* is an indicator equal to one if the firm rating is downgraded to default before the facility matures, and zero otherwise. We control for characteristics of borrowing firms and loans. Standard errors are clustered by package. We include facility start year and firm industry fixed effects. Appendix A contains detailed variable definitions. *, **, and *** represent the statistical significance at 10%, 5%, and 1%, respectively (two-tailed). T-statistics are presented beneath the coefficient estimates in parentheses.

	(1)	(2)	(3)
	Whole sample	With at least one renegotiation	Without any renegotiation
VARIABLES	DG_to_Default	DG_to_Default	DG_to_Default
Same_Clique%	-0.0265**	-0.0496**	-0.0136
	(-2.191)	(-2.024)	(-0.784)
Log(Total_Assets)	-0.00108	0.0111	0.00956*
	(-0.393)	(1.525)	(1.827)
ROA	-0.446***	-0.312***	-0.246**
	(-6.236)	(-2.634)	(-2.258)
Leverage	0.110***	0.0968**	0.125***
	(4.502)	(2.460)	(3.125)
MTB	-0.0136***	-0.0241***	-0.00788
	(-2.740)	(-2.716)	(-0.972)
R&D_Dummy	0.00768	0.0216	0.0304
	(0.640)	(1.010)	(1.542)
Asset_Growth	0.0140*	0.0116	0.0193*
	(1.905)	(0.909)	(1.749)
Asset_Intensity	0.0831***	0.0694*	0.0648*
	(3.463)	(1.775)	(1.695)
Log(Amount)	0.00474*	-0.00605	0.000278
	(1.662)	(-0.832)	(0.0470)
Log(Maturity)	0.0297***	0.0268*	0.0335***
	(4.730)	(1.775)	(3.907)
Revolver	-0.00436	-0.00781	-0.0216**
	(-0.718)	(-0.723)	(-2.277)
BtoK	-0.0111	-0.00372	-0.0640***
	(-1.071)	(-0.217)	(-3.812)
One_to_One	-0.0490***	-0.0455	-0.0545*
	(-2.839)	(-1.455)	(-1.813)
Rev_One_to_One	0.00521	0.00318	-0.0121
	(0.364)	(0.108)	(-0.567)
Constant	-0.118	-0.124	-0.285**
	(-1.586)	(-0.850)	(-2.359)
Observations	9,333	3,597	2,837
R-squared	0.138	0.172	0.170
Std Error Clustered	Package	Package	Package
Facility start Year F.E.	Yes	Yes	Yes
SIC2 F.E.	Yes	Yes	Yes

Table 3. 8 Additional Analysis: Time to the first renegotiation

This table presents results on whether the timing of the first renegotiation is associated with the percentage of p-lenders from the same clique with the lead lender. The outcome variable *Time_to_First_Reneg* is the number of months between the loan initiation to the first renegotiation. The coefficients from Cox analysis are listed. We control for characteristics of borrowing firms and loans. Standard errors are clustered by package. We include facility start year and firm industry fixed effects. Appendix A contains detailed variable definitions. *, **, and *** represent the statistical significance at 10%, 5%, and 1%, respectively (two-tailed). T-statistics are presented beneath the coefficient estimates in parentheses.

	(1)
VARIABLES	Time_to_First_Reneg
Same_Clique%	0.103***
	(2.652)
Log(Total_Assets)	-0.143***
	(-12.55)
ROA	-0.764***
	(-4.644)
Leverage	-0.0663
	(-1.038)
MTB	-0.00294
	(-0.191)
R&D_Dummy	-0.0931**
	(-2.552)
Asset_Growth	0.0802***
	(3.305)
Asset_Intensity	0.109
	(1.514)
Log(Amount)	0.0704***
	(5.479)
Log(Maturity)	-0.235***
	(-10.38)
Revolver	0.127***
	(6.172)
BtoK	0.110***
	(3.106)
One_to_One	-0.0799
	(-1.567)
Rev_One_to_One	-0.212***
	(-4.820)
Observations	23,983
Std.Error	Package
Facility start year Fixed effects	Yes
SIC2 fixed effects	Yes

Table 3. 9 Additional Analysis: Controlling for lead-lender centrality

Panel A Likelihood and intensity of renegotiation

This table presents results on whether renegotiation is associated with the percentage of p-lenders from the same clique as the lead lender, controlling for lead-lender centrality. We transform *Same_Clique*% and the three proxies for lead-lender centrality into percentile ranks to allow for the comparisons of the coefficients. We include the same set of controls as those included in Table 2. Standard errors are clustered by package. In columns (2) and (4), we include facility start year and firm industry fixed effects. Appendix A contains detailed variable definitions. *, **, and *** represent the statistical significance at 10%, 5%, and 1%, respectively (two-tailed). T-statistics are presented beneath the coefficient estimates in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Reneg_Dummy	Reneg_Dummy	Reneg_Dummy	Log(1+# renegotiation)	Log(1+# renegotiation)	Log(1+# renegotiation)
Same_Clique%_per	0.0540***	0.0562***	0.0531***	0.143***	0.149***	0.139***
	(4.196)	(4.380)	(4.134)	(3.968)	(4.168)	(3.886)
Proxies for lead-lender centrality:						
Degree_per	0.0336**			0.128***		
	(2.092)			(2.828)		
Betweenness_per		0.0226			0.0954**	
		(1.413)			(2.120)	
Eigenvector_per			0.0395**			0.149***
			(2.512)			(3.354)
Controls included	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,983	23,983	23,983	23,983	23,983	23,983
R-squared	0.082	0.082	0.082	0.147	0.147	0.147
Std Error Clustered	Package	Package	Package	Package	Package	Package
Facility start Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
SIC2 F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Panel B Loan performance

This table presents results on whether loan performance is associated with the percentage of p-lenders from the same clique as the lead lender, controlling for lead-lender centrality. We transform *Same_Clique%* and the three proxies for lead-lender centrality into percentile ranks to allow for the comparisons of the coefficients. We include the same set of controls as those in Table 7. Standard errors are clustered by package. We include facility start year and firm industry fixed effects. Appendix A contains detailed variable definitions. *, **, and *** represent the statistical significance at 10%, 5%, and 1%, respectively (two-tailed). T-statistics are presented beneath the coefficient estimates in parentheses.

VARIABLES	(1) DG_to_Default	(2) DG_to_Default	(3) DG_to_Default
Same_Clique%_per	-0.0259**	-0.0261**	-0.0263**
	(-2.341)	(-2.362)	(-2.380)
Proxies for lead-lender centrality:			
Degree_per	-0.000152		
	(-0.0110)		
Betweenness_per		0.000804	
		(0.0582)	
Eigenvector_per			0.00200
			(0.146)
Controls included	Yes	Yes	Yes
Observations	9,333	9,333	9,333
R-squared	0.138	0.138	0.138
Std Error Clustered	Package	Package	Package
Facility start Year F.E.	Yes	Yes	Yes
SIC2 F.E.	Yes	Yes	Yes

Table 3. 10 Alternative measure: shares held by same-clique p-lenders

This table presents results re-estimated from our main model after replacing the main independent variable *Same_Clique%* with the *Same_Clique_Share*, measured as the percentage of shares contributed by same-clique p-lenders. We include the same set of controls as those in Table 2. In addition, we control the loan amendment threshold (*Required_Lender*), following Caskey et al. (2021). Standard errors are clustered by package. Facility start year and firm industry fixed effects are included. Appendix A contains detailed variable definitions. *, **, and *** represent the statistical significance at 10%, 5%, and 1%, respectively (two-tailed). T-statistics are presented beneath the coefficient estimates in parentheses.

VARIABLES	(1) Reneg_Dummy	(2) Log(1+# renegotiation)
Same_Clique_Shares	0.0706**	0.152*
	(2.187)	(1.708)
Required_Lenders	-0.00159	-0.00442
	(-1.536)	(-1.540)
Log(Amount)	-0.00839	-0.0140
	(-0.967)	(-0.603)
Log(Maturity)	0.116***	0.716***
	(9.267)	(22.11)
Revolver	-0.000878	-0.0505
	(-0.0604)	(-1.263)
BtoK	-0.0162	0.107
	(-0.401)	(0.816)
Log(Total_Assets)	-0.0287***	-0.105***
	(-3.733)	(-5.076)
ROA	-0.329***	-1.193***
	(-3.142)	(-3.967)
Leverage	-0.0531	-0.0499
	(-1.225)	(-0.409)
MTB	0.00808	0.00254
	(0.886)	(0.102)
R&D_Dummy	-0.00671	-0.0949
	(-0.285)	(-1.420)
Asset_Growth	0.0113	0.0838**
	(0.764)	(1.978)
Asset_Intensity	-0.00221	0.0898
	(-0.0461)	(0.667)
One_to_One	-0.0532*	-0.135
	(-1.699)	(-1.489)
Rev_One_to_One	0.0401	0.117
	(1.359)	(1.374)
Constant	1.065***	1.642***
	(6.284)	(3.669)
Observations	6,105	6,105
R-squared	0.099	0.193
Std Error Clustered	Package	Package
Facility start Year F.E.	Yes	Yes
SIC2 F.E.	Yes	Yes

Table 3. 11 Additional robustness analysis

This table presents results on two sets of robustness analyses. Columns (1) and (2) presents the results re-estimated from the main regression model with borrower-facility start year fixed effects included. Thus, the borrower characteristics are subsumed. Columns (3) and (4) present the results re-estimate from the main regression model for the sample of the largest facilities in each package. Standard errors in all tests are clustered by package. Appendix A contains detailed variable definitions. *, **, and *** represent the statistical significance at 10%, 5%, and 1%, respectively (two-tailed). T-statistics are presented beneath the coefficient estimates in parentheses.

	(1)	(2)	(3)	(4)
	Whole sample; Including Borrower*Facility start-year F.E.		Largest facility for each package	
VARIABLES	Reneg_Dummy	Log(1+# renegotiation)	Reneg_Dummy	Log(1+# renegotiation)
Same_Clique%	0.0767**	0.227***	0.0483***	0.119***
	(2.401)	(2.670)	(3.753)	(3.470)
Log(Amount)	0.0169***	0.0455***	0.0490***	0.157***
	(4.388)	(4.484)	(9.513)	(11.43)
Log(Maturity)	0.0901***	0.529***	0.0916***	0.507***
	(10.19)	(22.50)	(13.39)	(29.61)
Revolver	0.0354***	0.109***	0.112***	0.246***
	(6.369)	(7.265)	(11.34)	(9.107)
BtoK	-0.00882	0.0521*	0.0584***	0.213***
	(-0.873)	(1.892)	(3.503)	(4.251)
One_to_One	0.0613	0.196*	-0.0418**	-0.138***
	(1.572)	(1.832)	(-2.426)	(-2.871)
Rev_One_to_One	-0.116***	-0.319***	-0.0761***	-0.187***
	(-3.613)	(-3.589)	(-4.996)	(-4.546)
Log(Total_Assets)			-0.0571***	-0.178***
			(-14.38)	(-16.73)
ROA			-0.271***	-0.892***
			(-5.067)	(-6.076)
leverage			-0.0559***	-0.0804
			(-2.599)	(-1.336)
MTB			-0.000615	-0.0186
			(-0.126)	(-1.448)
R&D_Dummy			-0.0201*	-0.0918***
			(-1.656)	(-2.781)
Asset_Growth			0.0155**	0.0642***
			(2.050)	(2.873)
Asset_Intensity			0.0260	0.162**
			(1.103)	(2.469)
Constant	-0.120	-1.482***	0.439***	0.163
	(-1.490)	(-6.978)	(5.766)	(0.818)
Observations	15,429	15,429	16,734	16,734
R-squared	0.801	0.838	0.096	0.155
Std Error Clustered	Package	Package	Package	Package
Borrower *Facility Start-year F.E.	Yes	Yes	No	No
Facility start Year F.E.	No	No	Yes	Yes
SIC2 F.E.	Yes	Yes	Yes	Yes

Table 3. 12 Downside of the cliques: Borrower's proprietary costs

This table presents results on whether borrowers with high proprietary costs are concerned about information dissemination within a clique. We regress the natural logarithm of one plus clique size on four proxies for borrower's proprietary costs. We include the same set of controls as those in Table 2. Standard errors are clustered by package. In columns (2) and (4), we include facility start year and firm industry fixed effects. Appendix A contains detailed variable definitions. *, **, and *** represent the statistical significance at 10%, 5%, and 1%, respectively (two-tailed). T-statistics are presented beneath the coefficient estimates in parentheses.

	(1)	(2)	(3)	(4)
VARIABLES	Log(clique size+1)	Log(clique size+1)	Log(clique size+1)	Log(clique size+1)
R&D_cont	-23.88*			
	(-1.947)			
Redaction_dummy		-0.0428*		
		(-1.941)		
Asset_Intensity			0.109*	
			(1.662)	
High_Proprietary				-0.120***
				(-2.669)
Log(Total_Assets)	0.0219**	0.0281***	0.0283***	0.0285***
	(2.413)	(3.173)	-3.194	(3.227)
ROA	0.0196	0.0142	0.039	0.0126
	(0.112)	(0.0806)	(0.222)	(0.0717)
Leverage	-0.0949	-0.0784	-0.0935	-0.0845
	(-1.484)	(-1.230)	(-1.449)	(-1.326)
MTB	0.016	0.0145	0.0141	0.0161
	(1.091)	(0.982)	(0.954)	(1.093)
Asset_Growth	-0.0243	-0.0222	-0.0202	-0.0205
	(-1.147)	(-1.051)	(-0.955)	(-0.970)
Log(Amount)	0.0255**	0.0284***	0.0271***	0.0281***
	(2.468)	(2.745)	(2.617)	(2.717)
Log(Maturity)	0.0291*	0.0291*	0.0297*	0.0289
	(1.651)	(1.651)	(1.683)	(1.636)
Revolver	-0.00186	-0.00122	-0.00249	-0.00144
	(-0.101)	(-0.0658)	(-0.135)	(-0.0778)
BtoK	-0.00691	-0.00782	-0.0059	-0.00811
	(-0.231)	(-0.261)	(-0.198)	(-0.271)
Constant	4.004***	3.818***	3.786***	3.805***
	(19.03)	(18.61)	(18.33)	(18.55)
Observations	28,307	28,307	28,307	28,307
R-squared	0.144	0.144	0.144	0.144
Std Error Clustered	Package	Package	Package	Package
Facility start Year F.E.	Yes	Yes	Yes	Yes
SIC2 F.E.	No	No	No	No

Appendix 3 Variable Definitions

Variable	Definition	Source
Outcome variables (listed in alphabetical order):		
DG_to_Default	A dummy variable equal to one if the borrowing firm's rating falls into the default grades (D or SD) before the loan matures, and zero otherwise.	S&P Credit rating
Reneg_Count	The number of renegotiation that the facility experiences before the maturity.	SEC EDGAR, DealScan
Reneg_Dummy	A dummy variable equal to one if the facility experiences one or more renegotiations before the maturity, and zero otherwise.	SEC EDGAR, DealScan
Time_to_First_Reneg	The number of month between the facility start date and the date of the first renegotiation.	SEC EDGAR, DealScan
Clique size	The number of lenders in the clique.	DealScan
Main independent variable:		
Same_Clique%	The number of participant lenders that are in the same clique with the lead lender, scaled by the total number of participant lenders in the clique. For facilities with multiple lead lenders, the value is calculated as average across all the lead lenders.	DealScan
Other independent variables (listed in alphabetical order):		
Amount (in $M)	Total amount of the facility.	DealScan
Asset_Growth	Current period total assets divided by the previous year total assets.	Compustat
Asset_Intensity	Ratio of net property, plant, and equipment divided by total assets.	Compustat
Betweenness_per	Percentile rank of the betweenness. Betweenness is a centrality measure which is calculated as the number of times the lender is on the shortest path between two other lenders in the market.	DealScan
BtoK	Indicator variable that takes the value of one if the classification of facility belongs to type B to type H, representing institutional term loans participated by nonbanks.	DealScan
CB%	The number of lenders that are classified as commercial bank in the clique, scaled by the total number of lenders in the clique.	DealScan
CFO_Volatility	The percentile rank of CFO volatility. CFO is the ratio of operating cash flow, scaled by total assets. CFO volatility is calculated based on the quarterly data for the 5-year window before the facility starts, (max of the CFO-min of the CFO)*2/(max of the CFO + min of the CFO).	Compustat
Degree_per	Percentile rank of the degree. Degree is a centrality measure which is calculated as the number of lenders the focal lender is connected with.	DealScan

Variable	Definition	Source
Eigenvector per	Percentile rank of the Eigenvector. Eigenvector is a centrality measure which is calculated as the relative scores to all the lenders in the market based on the concept that connections to high-scoring lenders contribute more to the Eigenvector score of the focal lender than equal connections to low-scoring lenders.	DealScan
High_Proprietary	A dummy equal to one if R&D > 0 and redaction dummy = 1 and Asset_Intensity < sample median, and zero otherwise.	SEC EDGAR, Compustat
Inexp_Lead_Lender	A dummy variable equal to one if the lead lender has less than or equal to three-year experience as a lead lender. For facilities with multiple lead lenders, the value is calculated as the max of all the lead lenders.	DealScan
Large_Group_Lenders	A dummy variable equal to one if the number of lenders in the facility is above sample median, and zero otherwise.	DealScan
Leverage	The ratio of long-term debt divided by total assets.	Compustat
Maturity (in Month)	Stated maturity (in months) of the facility.	DealScan
MTB	The market value of equity plus the value of total liabilities divided by total assets.	Compustat
One_to_One	The number of participant lenders that have the one-to-one connection with the lead lender, scaled by the total number of participant lenders in the clique. One-to-one connection means that the lead (participant) lender acted as lead (participant) lender in the previous facilities. For facilities with multiple lead lenders, the value is calculated as average across all the lead lenders.	DealScan
R&D_cont	The ratio of R&D expense divided by total assets.	Compustat
RD_Dummy	Indicator variable that takes a value of one when the R&D expense is positive, and zero when it is zero or missing. Note that a missing R&D expense indicates that the R&D amount is not material and therefore is not separately disclosed.	Compustat
Redaction_dummy	A dummy equal to one if the fim redacts information in its annual report, and zero otherwise.	SEC EDGAR
Required_Lenders	The required voting percentage of all lenders to release of any lien on collateral associated with a deal.	DealScan
Rev_One_to_One	The number of participant lenders that have the reversed one-to-one connection with the lead lender, scaled by the total number of participant lenders in the clique. One-to-one connection means that the lead (participant) lender acted as participant (lead) lender in the previous facilities. For facilities with multiple lead lenders, the value is calculated as average across all the lead lenders.	DealScan

Variable	Definition	Source
Revolver	Indicator variable that takes the value of one if the facility is a revolving loan.	DealScan
ROA	Earnings before extraordinary items divided by average total assets.	Compustat
Same_Clique_Shares	Percentage of loan shares contributed by p-lenders from the same clique as the lead lender	DealScan
Total Assets (in $M)	Total assets of the borrowing firm.	Compustat

Curriculum Vitae

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EDUCATION

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Ph.D. in Accounting 2023 (expected)

Duke University Durham, NC
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Financial accounting, voluntary disclosure, syndicate loan, debt contract, sell-side analyst, mutual fund

PUBLICATION

Duchin, Ran, Xiumin Martin, Roni Michaely, and Hanmeng Wang. "**Concierge treatment from banks: Evidence from the Paycheck Protection Program.**" *Journal of Corporate Finance* 72 (2022): 102124.

WORK IN PROGRESS

1. "Pass through Myopia: Institutional Investor and Corporate Voluntary Disclosure"

- Solo-authored job market paper
- Dissertation Committee: Xiumin Martin (chair), Richard Frankel, Jeremy Bertomeu
- Presented at Washington University in St. Louis, AAA Doctoral Consortium, Dopuch Conference (poster session), AAA Midwest Region Meeting, AAA Mid-Atlantic and Northeast Regions, AAA Rookie Camp
- Abstract: This paper studies how institutional investors' myopia affects corporate voluntary disclosures. Earlier theories debate upon whether myopia leads to more or less voluntary disclosures (Einhorn and Ziv, 2008; Beyer and Dye, 2012; Aghamolla and An, 2021; Bertomeu et al., 2022). I exploit the SEC regulation that requires mutual funds to report holdings more frequently as an exogenous increase to institutional investors' short-term focus, and study how issuance of managerial guidance changes after the regulation. With a difference-in-differences research design, firms with higher mutual fund ownership are more likely to issue managerial guidance after the regulation. The effect is stronger when the mutual fund ownership is more transient, when the business environment is more volatile, and when the CEO is more short-termed. This study demonstrates the causal effect of institutional investors' tunnel vision on firms' voluntary disclosure policy, which helps to disentangle whether the reputation of being uninformed or the reputation of being forthcoming affects managers' disclosing decisions. It also documents the spillover effects of the SEC regulation and has regulatory implications.

2. “Do two wrongs make a right? Strategically forecasting EPS through inaccurate share forecasts.”

- With Zachary Kaplan, Nathan Marshall and Jerry Mathis
- Presented at Washington University in St. Louis, George Washington University*, 2022 Conference on Financial Economics and Accounting*, 2022 Utah Winter Accounting Conference*, 2022 AAA Annual Meeting*, 2023 FARS Midyear
- Under review at *Review of Accounting Studies*
- Abstract: We decompose EPS forecasts into the numerator and denominator and show that share forecasts serve as a strategic ‘plug’, allowing the analyst to convert street earnings into the desired EPS number, rather than providing novel information about the share count to clients. First, analysts use share forecasts to herd EPS to the consensus, as share forecast errors have the same sign as street earnings forecast errors. Second, analysts use share forecasts to cater to management, as share forecasts exhibit bias facilitating firms’ ability to meet or beat EPS benchmarks (MB) and inter-temporal variation consistent with managerial preferences. Third, analysts’ share forecasts are significantly less accurate than naïve share forecasts; however, these inaccurate share forecasts tend to make EPS forecasts more accurate, further highlighting their strategic role. Finally, our evidence that analysts do not rapidly update share forecasts with new information suggests firms consistently repurchasing shares might MB more frequently because of biased expectations rather than managerial opportunism. Consistent with this, we show that the association between repurchases and MB (documented in prior work) is driven by predictable rather than unpredictable repurchases.

3. “Lenders’ Coordination and Syndicate Debt Contract.”

- With Xiumin Martin, Xiaoxiao Tang and Yifang Xie
- Presented at Washington University in St. Louis
- Revising draft
- Abstract: Building upon theories about incomplete contract and network, we study how interconnection among lenders reduces coordination costs in syndicate loans. We identify network of inter-connected lenders using machine learning algorithm, and examine how the participation by lenders from the same network affects renegotiations. Different from previous literature about lenders’ one-to-one connections or centrality measures, we focus on a loan-specific measure, which captures the interconnections among lenders in the syndicate. We find with more participate lenders connected to the lead lender through the network, coordination costs are reduced, and syndicates are more likely to experience renegotiations. The time to reach a consensus is also shortened, as the first renegotiation happens closer to loan initiation. In addition, borrowers tend to experience better performance when coordination costs are lower, which is consistent with the concept that renegotiation is a pareto improvement to both lenders and borrowers.

4. “Solo-managed and Team-managed Funds during the Pandemic.”

- With Yanrong Jia and Xiumin Martin
- Presented at Washington University in St. Louis
- Revising draft

5. “ESG and Mutual Fund Disclosures”

- With Richard Frankel and Xiumin Martin
- Presented at Washington University in St. Louis*, Seoul National University*

** Presented by a co-author*

TEACHING

Teaching Assistant	Accounting Policy and Research by Professor Xiumin Martin	2020 – 2022
Teaching Assistant	Accounting in the Digital Age by Professor Edwige Cheynel	Fall 2022

CONFERENCE AND WORKSHOP

FARS Midyear (scheduled, reviewer×3)	2023
AAA Midwest Region Meeting (presenter, moderator)	2022
AAA Mid-Atlantic and Northeast Regions (reviewer, presenter)	2022
AAA Annual Meeting (discussant)	2022
Annual Labor and Accounting Group Conference	2022
Junior Accounting Theory Conference and Summer School	2020
Web-scraping and Data-cleaning for Research	2020
Ph.D. summer paper series at Washington University in St. Louis (presenter)	2019 – 2022
Nicholas Dopuch Accounting Conference (poster presenter in 2022)	2018 – 2022

HONOR AND REWARD

AAA/Deloitte Foundation/J. Michael Cook Doctoral Consortium Fellow	2022
AAA/FARS Section Doctoral Consortium Fellow	2022
Doctoral Fellowship, Olin Business School, Washington University in St. Louis	2018 – Present
M.A. Merit Scholar Award, Department of Economics, Duke University	2018
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