ESSAYS ON FINANCIAL INTERMEDIARIES

by

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ESSAYS ON FINANCIAL INTERMEDIARIES

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This dissertation consists of two essays that study issues related to financial intermediaries. In the first essay, I examine the effects of institutional common ownership on the use of peer benchmarking for CEO pay. By compensating managers based on their co-owned peers' performance as well as their own performance, blockholders can incentivize managers to avoid head-to-head competition with their co-owned peers while maximizing group performance. I find that CEOs' total compensation is positively sensitive to the stock performance of industry peers that share common blockholders. Furthermore, I document that firms sharing common blockholders tend to have more differentiated products, greater joint market share, and greater geographical overlap in business operations.

In the second essay, my coauthors and I examine the impact of the disclosure regulation under Regulation AB, which is the disclosure rule that requires all material risk factors applicable to the transaction or to the nature of the security to be disclosed. Specifically, we look at the responses of financial intermediaries to the regulatory changes on disclosure in the asset-backed securities market. We find an immediate jump in the percentage of deals with origination stakes just below the disclosure threshold following the implementation of Regulation AB. We also provide evidence that MBS underwriters deliberately keep lower-quality loans below the disclosure threshold to evade disclosure.

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CHAPTER 1

INTRODUCTION

Over the past few decades, the role of financial intermediaries has become increasingly more important due to the growth of financial markets. When financial intermediaries operate in different conditions, they have incentives to behave strategically in making economic decisions. In this dissertation, I explore how financial intermediaries behave in response to situations that influence their incentives.

In Chapter 2, I study the effects of institutional common ownership on the use of peer benchmarking for CEO pay. By compensating managers based on their co-owned peers' performance as well as their own performance, blockholders can incentivize managers to avoid head-to-head competition with their co-owned peers while maximizing group performance. I find that CEO compensation tends to be positively related to the performance of industry peers that share common blockholders. Using the merger between BlackRock and Barclays Global Investors as a quasi-natural experiment, I show that the BlackRock-Barclays merger leads to positive pay-performance sensitivity among industry rivals that are co-owned by the newly merged asset management company. I also document that firms sharing common blockholders tend to have more differentiated products, greater joint market share, and greater geographical overlap in business operations. My findings suggest that institutional common blockholders use CEO compensation contracts to mitigate competition and increase joint performance among rival portfolio firms.

In Chapter 3, my coauthors and I examine the impact of the disclosure regulation under Regulation AB, which is the disclosure rule that requires all material risk factors applicable to the transaction or to the nature of the security to be disclosed. Specifically, those originators who contribute more than 20% of the loans in a collateral pool are required to provide detailed information to the investor analysis of the collateral assets. We find a drastic jump in the percentage of deals with origination stakes just "below the disclosure

threshold" (BDT). More importantly, those deals where originators exhibit increased BDT stake occurrence suffer significantly larger losses than other deals, an effect that is only significant for deals issued after Regulation AB. Our loan-level analysis further demonstrates that the financial intermediaries underwriting securities intentionally placed poor-quality loans in BDT stakes to evade disclosure. Taken together, we find evidence suggesting that underwriters deliberately misrepresented asset quality.

CHAPTER 2

ESSAY 1: COMMON OWNERSHIP AND EXECUTIVE COMPENSATION

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2.1 Introduction

Institutional common ownership, where an investor owns large shares in multiple firms in the same industry, has become a prevalent phenomenon in the United States. As Figure 2.1 shows, the proportion of U.S. public firms that share common blockholders with industry peers increased from 35% in 1992 to 82% in 2013. Given the fast growth in assets under management (Gompers and Metrick, 2001), institutional investors inevitably become blockholders of many firms in the same industry. A natural question that arises is whether and how institutional common ownership affects the firms' competitive landscape. The existing industrial organization theory suggests that common ownership of same-industry firms can reduce competition (Gordon, 1990; O'Brien and Salop, 2000; Gilo, Moshe, and Spiegel, 2006). Consistent with this idea, recent research shows that common block ownership is associated with increased product pricing (Azar, Schmalz, and Tecu, 2015) and higher market share (He and Huang, 2017).

In this paper, I investigate the mechanism through which common blockholders influence competition among rival portfolio firms. As mentioned in Aggarwal and Samwick (1999), a positive pay sensitivity to rival firms' performance can incentivize managers to soften product market competition. By compensating managers based on their co-owned peers' performance as well as their own performance, blockholders can incentivize managers to avoid head-to-head competition with their co-owned peers and maximize group performance. Therefore, I hypothesize that CEOs' compensation exhibits additional positive sensitivity to the performance of industry peers that share common blockholders.

I test this hypothesis using a sample of U.S. public firms with available data on executive compensation from 1992 to 2013, and I define firms as co-owned peers if they share common blockholders in the past four quarters. I find that a CEO's total compensation is positively sensitive to the stock performance of industry peers that share common blockholders. The positive weight on co-owned peers' stock returns amounts to 9%-15% of the positive weight

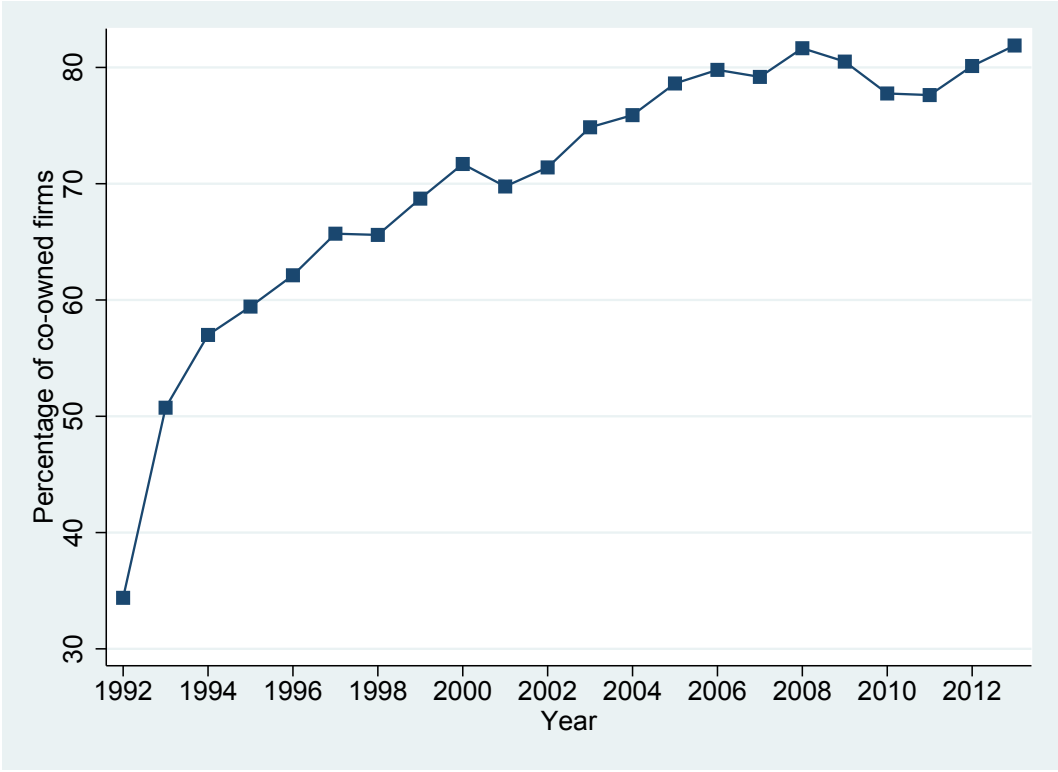

Figure 2.1. The percentage of co-owned firms

The plots in this figure represent the percentage of co-owned firms each year in Compustat/CRSP universe from 1992 to 2013.

on the firms' own stock returns, suggesting that the award based on the co-owned peers' performance is economically significant. In addition, I examine the effect of compensation benchmarking on CEO pay and find that CEOs' compensations are also positively sensitive to co-owned peers' compensation. Importantly, the pay sensitivity to co-owned peer performance remains significantly positive after accounting for compensation benchmarking. Further analysis on the components of CEO compensation reveals that only long-term incentives, such as stocks and options, have positive sensitivity to the performance of co-owned peers.

The above results are consistent with the prediction that blockholders incentivize managers to mitigate direct competition with their co-owned peers. However, as institutional

common ownership is endogenously formed, these results are subject to other non-causal interpretations. For example, investors could choose to cross-hold firms in the same industries that are known to not compete against one another. Moreover, there could be unobservable characteristics among these firms that cause both a positive pay-performance sensitivity and institutional common ownership.

To address these endogeneity concerns, I exploit the largest asset management firms' merger between BlackRock Inc. and Barclays Global Investors, which resulted in exogenous common ownership between firms that were previously owned separately by BlackRock and Barclays. Using a difference-in-differences (DiD) approach, I show that the BlackRock-Barclays merger led to positive pay-performance sensitivity among industry rivals that are co-owned by the newly merged asset management company. The effect is also economically significant: the increase in the positive weight to co-owned peer stock returns after the merger amounts to 142%-765% of the positive weight on the firms' own stock returns.

Turning to a potential mechanism through which common blockholders achieve positive pay performance sensitivity among co-owned firms, I examine the selection of performance benchmarking peers using firms' disclosure under the SEC's 2006 executive compensation disclosure rules. I find that same-industry peers that share common blockholders are less likely to be selected as RPE peers. This finding provides empirical evidence that firms select RPE peers based on whether peers share common blockholders.

Next, I examine the effect of institutional common ownership on future product market characteristics. I argue that the strategic incentive contracts offered by common blockholders can induce coordination among co-owned peers in the product market to reduce direct competition and enhance group performance. Consistent with my prediction, I find that, after having common ownership, firm pairs that share common blockholders tend to have more differentiated products, as reflected by the product descriptions in the annual reports. I also show that co-owned firm pairs tend to have greater joint market share and, subsequently,

greater geographical overlap in business operations. The results are consistent with the idea that managers under the incentives to cooperate can avoid direct competition through product differentiation, and thus, they can coexist in the same local market and enhance joint performance.

To investigate cases in which common blockholders are more likely to adopt the positive pay sensitivity to co-owned peer performance for managers, I conduct subsample tests based on product market characteristics, such as product market competition and combined market share. I find that the positive co-owned peer pay-performance sensitivity is more likely to occur in more competitive industries and among firm pairs with lower joint market share. This is consistent with the prediction in Aggarwal and Samwick (1999) that when competition is in strategic complements, the use of positive peer pay-performance sensitivity is more likely when the need to soften competition is greater.

I have conducted additional tests to ensure the robustness of my findings. In the main analysis, I define industry peers based on the three-digit level of Standard Industry Classification (SIC). I show that the results on the positive pay-performance sensitivity to co-owned peer performance still hold when I define industry peers using the four-digit SIC classification and the Text-Based Network Industry Classifications (TNIC) by Hoberg and Phillips (2010, 2016). Thus, the results are robust to alternative industry classifications.

This paper contributes to two strands of the literature. First, it contributes to the growing literature on cross-holding and common ownership in the same industry. The existing industrial organization theory has shown that cross-holding of same-industry firms can reduce the incentives of firms to compete with one another (Gordon, 1990; Hansen and Lott, 1996; O'Brien and Salop, 2000; Gilo, Moshe, and Spiegel, 2006). Empirical studies suggest that cross-holding among industrial firms can reduce competition and offer strategic benefits in product market relationships. For example, Allen and Phillips (2000) show that corporate block ownership adds significant value to target firms because of the strategic benefits

from product market relationships between target firms and corporate block owners. Fee, Hadlock, and Thomas (2006) find that customer firms' equity holding in supplier firms help alleviate the friction along the supply chain relationships. Nain and Wang (2016) show that acquisitions of a minority stake in competing firms lead to higher output prices and profit margins. Some recent studies focus on common ownership by institutional investors.[1] For example, He and Huang (2017) show that firms with common blockholders enjoy larger market share growth. Azar, Schmalz, and Tecu (2015) and Azar, Raina, and Schmalz (2016) conduct focused studies on the airline industry and banking industry, respectively, and show that common ownership induces collusive pricing and reduces competition. This paper adds to the literature by documenting a new effect of institutional common ownership on executive compensation and shedding light to the mechanism through which common ownership reduces competition.

Second, this study adds to the large empirical and theoretical literature on the executive compensation setting. The principal-agent theory suggests that the market-wide component of firm performance should be removed from the compensation package because executives have no control over market factors, and it is costly for them to bear market-wide risks (Holmstrom, 1982; Holmstrom and Milgrom, 1987). Relative performance evaluation (RPE), in which agents are compensated based on their performance relative to that of industry rivals, insulates agents from common risk and provides a more informative measure of the agents' performance. However, prior studies show mixed evidence on the use of RPE in CEO compensation (Antle and Smith, 1986; Gibbons and Murphy, 1990; Janakiraman, Lambert, and Larcker, 1992; Aggarwal and Samwick, 1999; Bertrand and Mullainathan, 2001; Garvey and Milbourn, 2006; Jenter and Kanaan, 2015).[2] Some studies argue that outside employment

[1]Matvos and Ostrovsky (2008) investigate the influence of institutional cross-holders in merger and acquisition. However, Harford, Jenter, and Li (2011) find that the impact of cross-holdings by active investors may be too small to matter.

[2]Please refer to Albuquerque (2009) for a nice summary of empirical findings on implicit approach in testing the use of RPE.

opportunities are a reason for not using RPE (Oyer, 2004; Rajgopal, Shevlin, and Zamora, 2006).[3] Others find strong evidence of RPE if appropriate peer groups are used (Albuquerque, 2009; Lewellen, 2015; Jayaraman, Milbourn, and Seo, 2015).[4] My study largely follows the argument by Joh (1999) and Aggarwal and Samwick (1999) that the use of RPE is limited by product market interaction and shows that the emergence of common ownership reinforces the role of product market considerations in a compensation setting.

The rest of the paper is organized as follows. Section 2.2 discusses hypotheses development. Section 2.3 describes the data and variable construction and reports summary statistics. Section 2.4 describes the empirical strategy and presents the results. Section 2.5 concludes.

2.2 Hypotheses development

Shareholders can link executive compensation to the peer firms' performance either positively or negatively. A negative pay sensitivity to peer-firm performance is consistent with the practice of relative performance evaluation (RPE). RPE provides a cost-effective way to incentivize risk-averse managers by filtering out the common shock to the industry. For this purpose, CEOs' compensation may be negatively linked to commonly owned peer-firm performance because common ownership may increase the stock price correlation between firms (Anton and Polk, 2014), and that RPE is more useful when firm performance is more correlated with that of the peers' (Holmstrom and Milgrom, 1987).

[3]Oyer (2004) develops a model where pay can appear to respond to luck when the outside opportunities of the manager are correlated with industry performance. Rajgopal, Shevlin, and Zamora (2006) empirically test Oyer's (2004) theory and find that the CEO's outside employment opportunities increase with his managerial talent, as proxied by the CEO's prior media mentions and his firm's industry-adjusted ROA.

[4]Albuquerque (2009) uses industry-size portfolio; Lewellen (2015) uses firm-specific industry portfolio; and Jayaraman, Milbourn, and Seo (2015) use Hoberg-Phillips Text-Based Network Industry Classification (TNIC).

However, RPE can also incentivize managers to behave aggressively in the product market to lower industry returns, which may not be in the interest of shareholders. As Aggarwal and Samwick (1999) argue, when firms compete in strategic complements, an optimal compensation contract with positive pay sensitivity to peer-firm performance can incentivize managers to soften product market competition; when firms compete in strategic substitutes, an optimal compensation contract with negative pay sensitivity to peer-firm performance can encourage managers to compete aggressively. Consistent with their argument for competition in strategic complements, they find that the pay sensitivity to peer-firm performance is positive and that the sensitivity is increasing in the degree of competition in the industry. Another closely related paper by Vrettos (2013) examines the competition in both strategic complements and strategic substitutes and finds evidence supporting the role of strategic interaction in CEO compensation setting.

Aggarwal and Samwick (1999) and Vrettos (2013) consider strategic interaction based on separate ownership, whereas I consider strategic interaction along with common ownership in a CEO compensation setting. When a common blockholder writes a compensation contract for a manager at a given portfolio firm, the common blockholder considers not only the profit of the particular firm but also the profits of the co-owned rival firms. Therefore, the optimal compensation contract should internalize the effect on profits of rival firms. I argue that the prevalence of a common ownership structure in the stock market reinforces the anti-competitive mechanism.

The objective of institutional investors is to maximize portfolio performance. An increase in the value of one firm at the cost of another portfolio firm is not a desirable outcome for these investors. In addition, common blockholders can provide anti-competitive incentive contracts simultaneously among the portfolio firms to ensure effective coordination between industry peers. Hence, I predict that the use of incentive contracts for softening competition is stronger with the presence of common ownership among industry rivals. I formally hypothesize the following:

Hypothesis 1: CEO compensation is more positively (or less negatively) sensitive to the performance of peers with common blockholders.

Following Hypothesis 1, if blockholders can provide anti-competitive incentives to firm managers with positive pay sensitivity to peer performance, then firms with common block ownership can internalize externalities and enhance group performance (Hansen and Lott, 1996). For instance, firms with common block ownership can differentiate their products to avoid direct competition with each other. While firms can focus on different geographical markets to avoid direct competition with each other, product differentiation allows them to cover more product ranges. As a result, these firms should enjoy higher growth in joint market share. Moreover, by preventing head-to-head competition in the product market, firms with common block ownership are also more likely to coexist in the same geographical area. Hence, my second hypothesis is the following:

Hypothesis 2: Pair of firms with common blockholders has more differentiated products, greater joint market share, and greater geographic overlap in business operations.

Similar to the argument by Aggarwal and Samwick (1999), when firms compete in strategic complements, common blockholders should have stronger incentives to curtail competition when the competition is already intense. Further, to set a positive pay sensitivity to the performance of co-owned peers, one has to exclude these peers from the list of rivals for RPE. In a concentrated industry where only a handful of major players are present, it might be hard to neglect any competitor in the RPE.[5] Also, considering that firms in concentrated industries are under great scrutiny by the Federal Trade Commission (FTC) and the Department of Justice (DoJ), any attempt to collude or coordinate among market players could trigger antitrust-related investigations. Hence, the positive pay sensitivity to

[5]Under the SEC's 2006 executive compensation disclosure rules, firms are required to provide details on how relative performance targets are used in setting executive pay.

commonly owned peer performance is also more feasible in competitive industries, where neither of the co-owned firms are a major player in the market. My third hypothesis is the following:

Hypothesis 3: CEO compensation is more likely to have more positive (or less negative) sensitivity to the performance of a co-owned peer when the firm is in a more competitive industry and has lower joint market share with the co-owned peer.

2.3 Sample selection and summary statistics

2.3.1 Sample selection

The sample contains U.S. public firms during the sample period 1992-2013. I obtain compensation data from Standard & Poor's ExecuComp database. List of self-identified performance benchmarking peers are obtained from Incentive Lab.[6] Institutional holdings data are obtained from the Thomson Reuters Institutional Holdings Database (13F filings). Stock return data are obtained from the Center for Research in Security Prices (CRSP). Industry classification and other financial statement items are from Compustat. I drop observations with non-positive or missing values for total compensation, total assets, market values, and common equity. I also exclude observations with no industry classification or stock return data. As the pay-performance sensitivity analysis is performed at the pairwise level, I create a firm-pair panel. I match each firm with its $n_j - 1$ peers in the same three-digit SIC industry (industry j with n_j firms) to construct firm pairs. The reason for using a three-digit SIC industry instead of a more refined four-digit SIC industry is that there are a number of four-digit SIC industries in which ExecuComp has only one firm.[7] As a result, for each

[6]Incentive Lab provides detailed data on peer group companies for relative performance awards as disclosed in proxy documents.

[7]In the robustness check Section 2.4.6, I use a four-digit SIC industry, and the results are qualitatively similar.

firm-year, I create $n_j - 1$ pairwise observations. For each year and for each industry j, there will be $n_j \times (n_j - 1)$ pairs of observations. Therefore, every pair of the same-industry firms will appear twice. For instance, consider firm A and firm B, which are in the same industry. These two firms will appear once as "Firm A, Firm B, Year X" and again as "Firm B, Firm A, Year X." In this example, firm A serves once as a focal firm and again as a peer firm for firm B. This sample selection process results in 740,780 firm-pair-years used in baseline regressions or on average 33,672 firm pairs per year.

2.3.2 Variable measurement

Measuring common ownership

For each quarter in the sample period, I obtain institutional holding information from Thomson Reuters (13F). Thomson Reuters data include ownership information by institutional investment managers with $100 million or more in assets under management. An institutional block-holding is defined as a holding by an institutional shareholder that is not less than 5% of the total shares outstanding. To identify the co-owned peers, I examine each institutional block-holding in each quarter and account for common ownership when a blockholder of a focal firm also has another block-holding in a peer firm (in the same three-digit SIC industry). I then match these quarterly data with Compustat data and aggregate over the four quarters prior to the fiscal year end date to obtain annual common ownership data.

To measure common ownership status at each firm-pair in any given fiscal year, I use a co-owned dummy variable as the main measure for common ownership. The co-owned dummy takes the value of one if the firm-pair is co-owned during the past year, and zero otherwise. The advantage of using a firm-pair panel in this study is that I can refine the common ownership measure to the specific firm-pair. This research design enables the study to identify different strategic incentive schemes for co-owned pairs and non-co-owned pairs.

Measuring CEO compensation

Each firm's executive is identified by ExecuComp as CEO given by the variable annual CEO flag (CEOANN) in ExecuComp. The annual CEO flag variable indicates that the executive served as CEO for all or most of the indicated fiscal year. Following the majority of the literature, I impose the requirement that the ExecuComp sample is limited to the value CEO for this variable because other executives may have an incentive to strategically influence each other to improve their own benefits (Holmstrom, 1982). I also delete observations for which there is more than one CEO per firm-year. Following Bertrand and Mullainathan (2001) and Albuquerque (2009, 2013), in the analysis I use the natural logarithm of total annual flow compensation (TDC1 in ExecuComp), which is the sum of salary, bonus, other annual compensation, total value of restricted stocks granted, total value of stock options granted, long-term incentive payouts, and all other compensation. I focus on the flows because they are representative of the actions taken by the board regarding executive compensation. Compensation committees usually make compensation decisions once a year, usually shortly after the end of the firm's fiscal year. This timing of compensation decisions is made so that stock returns and other accounting performance metrics can be observed.

Other variables

The stock return performance measure I use is annual compounded stock returns (including dividends). I measure annual stock returns for both the focal firm and its peer firm from the beginning of the fiscal year. I also measure accounting-based operating performance, return on asset (ROA), using earnings before interest, tax, depreciation, and amortization (EBITDA) divided by lagged total assets.

I include a set of control variables that could potentially affect CEO compensation. Following the literature, I control for firm size (natural logarithm of total assets), book leverage, cash-to-asset ratio, ownership of institutional investors, and measure of financial

constraints (Whited-Wu Index) for both focal firms and peer firms. I also control for the Herfindahl-Hirschman Index (HHI) and pairwise stock return correlation with industry peer. In addition to these controls, I include CEO characteristics, such as CEO age and CEO tenure.

2.3.3 Summary statistics

Table 2.1 presents the summary statistics for key variables used in this paper. Panel A provides summary statistics at the firm-year level. The mean (median) of total annual compensation is $4.53 million ($2.60 million). Since the total compensation is positively skewed, I take the natural logarithm of total compensation throughout analysis in the study. In my sample, 77% of the firm-year observations are co-owned by at least one institutional investor. In other words, these firms share at least one common blockholder with any number of same-industry peers. The institutional common ownership measure in the firm-year panel is for demonstration purpose. The actual variable of interest for institutional common ownership is measured between two firms. Figure 2.1 shows how prevalent institutional common ownership has become over the past decades. The percentage of co-owned firms rises from 35% in 1992 to 82% in 2013. I measure annual firm performance using both stock return and accounting-based operating performance. The average firm stock return is 7%, and the average ROA (defined as EBITDA/Asset) is 15%.

The rest of panel A in Table 2.1 summarizes the control variables at the firm-year panel. On average, a firm in the sample has a book value of asset of $8.5 billion, a cash-to-asset ratio of 17%, and a leverage of 35%. The average total institutional ownership is 68% of the outstanding shares. As for CEO characteristics, on average, CEO tenure is 8 years, and CEO age is 56 years.

Panel B of Table 2.1 summarizes the common ownership and correlation at the firm-pair-year level. In my sample, 35% of the firm-pair-years are co-owned by at least one

Table 2.1. Summary statistics

Variable	Mean	P25	P50	P75	St. Dev.
Panel A:					
Co-owned (d)	0.77	-	-	-	-
Total Compensation ($K)	4,531.95	1,239.37	2,602.32	5,548.5	5,428.29
Ln Total Compensation	7.87	7.12	7.86	8.62	1.06
Stock Return	0.07	-0.13	0.11	0.31	0.44
EBITDA/Asset	0.15	0.08	0.14	0.21	0.12
Asset ($MM)	8,532.79	462.05	1,422.43	5,096.31	24,880.22
Leverage	0.35	0.1	0.33	0.52	0.81
Cash	0.17	0.03	0.08	0.23	0.23
Whited-Wu Index	-0.36	-0.43	-0.36	-0.29	0.09
Institutional Ownership	0.68	0.54	0.7	0.84	0.22
CEO Tenure	7.81	3	6	10	7.1
CEO Age	55.68	51	56	60	7.17
SIC3-HHI	0.15	0.05	0.11	0.19	0.14
Panel B:					
Co-owned (d)	0.35	-	-	-	-
Correlation	0.33	0.17	0.32	0.49	0.22

This table contains summary statistics on the variables defined in Appendix A. Panels A and B present the summary statistics at firm level and firm-pair level, respectively. The statistics reported are Mean, the k^{th} percentile, (Pk for $k = 25, 50, 75$), and St. Dev. (standard deviation) of each variable. I use (d) to indicate that the variable is a dummy variable. I report only the mean for dummy variables.

institution. This average is much smaller than the average in the firm-year panel. This is expected because I increase the total number of observations when I duplicate each focal firm-year observation to the number of peers in each year, leading to a larger denominator of the fraction. Also, the average correlation between peer firms is 0.33.

2.4 Empirical analysis and results

Following previous literature on an implicit approach to test for the use of relative performance evaluation (RPE), I examine the compensation patterns among rival firms that share common blockholders. It would not be possible to expect firms to have compensation contracts that put a positive weight on co-owned peer-firms or any other firms' performance,

as this would be an outright collusion that could trigger antitrust investigations. On the other hand, placing a negative weight on peer-firms' performance can be simply achieved through the practice of RPE. Using explicit compensation contracts, Gong, Li, and Shin (2011) confirm that CEO compensation is negatively sensitive to contractual RPE peers' performance.

To achieve a positive pay sensitivity to co-owned peer firms' performance, common blockholders can potentially influence the compensation setting process to avoid listing co-owned firms as RPE peers. Therefore, one potential test is to directly examine whether co-owned peers are less likely to be listed as RPE peers.[8] However, there are several reasons necessitating a regression analysis. First of all, large part of CEO compensation is in the form of discretionary awards. De Angelis and Grinstein (2015) show that the discretionary awards are on average about half of total CEO compensation. Firms can implement the strategic compensation contracts implicitly through boards' subjective discretion, rather than precommitting to a formulaic explicit contract (Gong, Li, and Shin, 2011; Ferri, 2009). In addition, researchers do not observe the detailed contractual terms in the compensation contracts until 2006. Prior to 2006, the disclosure of the details on performance targets in executive compensation contracts in the United States had been voluntary (Carter, Ittner, and Zechman, 2009). In this study, I use an implicit approach for a test of RPE use to examine the additional pay performance sensitivity. This approach allows me to examine the compensation patterns even when the contractual terms are not available.

Despite the data limitation prior to 2006, the details of the executive compensation contract under the SEC's 2006 executive compensation rules can shed some light on a mechanism through which common blockholders achieve positive pay performance sensitivity among co-owned firms. I further examine whether firms exclude co-owned peers from being included

[8]Since RPE is mostly rank-based, firms that are trying to avoid competition with co-owned peers can include more "easy to beat" peers so that co-owned peers are listed as highest-ranked peers. This then leads to CEOs competing with middle-ranked peers.

in the performance benchmarking list. Using the self-identified performance benchmarking peer group available after the SEC's 2006 disclosure rules, I conduct additional analysis on the selection of performance benchmarking peers.

2.4.1 Performance and compensation benchmarking

To test whether the pay sensitivity to rival firms' performance varies with the presence of common ownership by institutional investors, I estimate the following pooled cross-sectional, time series regression model:

$$\begin{aligned}\text{Total Compensation}_{ijt} &= c + \eta_1 \text{Ret}_{it} + \eta_2 \text{Peer Ret}_{ijt} \\ &\quad + \eta_3 \text{Peer Ret}_{ijt} \times \text{Co-owned (d)}_{ijt} + \eta_4 \text{Peer Ret}_{ijt} \times \text{Correlation}_{ijt} \\ &\quad + \gamma \text{Control Var}_{t-1} + \text{Year}_t + \text{Firm}_i + \epsilon_{ijt}, \end{aligned} \tag{2.1}$$

where $\text{Total Compensation}_{ijt}$ is the total compensation of the CEO of firm i at time t. The performance of firm i at time t is measured by its annual stock return, Ret_{it}. Similar to Albuquerque (2009), the peer-firm performance of firm i at time t, Peer Ret_{ijt}, is measured by the annual stock return of the peer firm j in the same three-digit SIC industry as firm i. The co-owned status is represented by the dummy variable co-owned_{ijt}. The correlation between focal-firm and peer-firm stock return performance is represented by Correlation_{ijt}. Interaction between Peer Ret_{it} and Correlation_{ijt} is included as well. The pay-for-peer-performance sensitivity is allowed to vary with the correlation between the focal-firm and the peer-firm performance. To facilitate the interpretation of the results, I use the mean-centered value of correlation when computing the interaction term. As a result, the coefficient of Peer Ret_{it} is interpreted as peer-firm pay performance sensitivity when the stock return correlation is at the mean level. Other control variables capture variation in CEO pay that is not related to firm or industry performance. Year_t captures year fixed effects, and Firm_i

captures firm fixed effects. I cluster standard errors at the firm level. The variables have been discussed in Section 2.3.

I focus on change in firm value (i.e., stock return or total shareholder return [TSR]) as the main firm performance signal. Stock returns are not so easily manipulated as accounting variables such as return on assets (ROA) (Antle and Smith, 1986). Furthermore, CEOs are usually given stock options as part of their compensation whose value directly depends on the firm's future stock returns, creating incentives for CEOs to maximize firm value. With the use of stock options, firms provide CEOs with substantial rewards and penalties based on a long-run stock market value. Thus, stock returns are a reasonable performance measure of the firm and its peers.

Table 2.2 reports the results from estimating CEO compensation on own-firm and peer-firm stock returns in samples with different log asset distances, calculated as the difference between the asset of focal firms and that of peer firms. I measure the distance in assets using $|log(A) - log(B)|$, which is the absolute value of the difference between log(Assets) of firm A and that of peer firm B. Peer firms of similar size are more ideal benchmarks for the observed firm.[9] Columns (1) to (5) show that, consistent with CEOs being rewarded for better firm performance, CEO compensation is positively associated with own-firm stock return for each specification with a coefficient of approximately 0.15. At the average correlation level of 0.33, CEO compensation is negatively associated with peer-firm stock return.[10] The coefficient on the interaction between peer performance and pairwise stock return correlations is negative and significant. The results indicate that the CEO compensation is tied to the firm's performance measured against the performance of its peers provided that the observed firm has higher-than-average stock return correlation with its industry peers. This is consistent

[9] Albuquerque (2009) finds evidence of relative performance evaluation using industry-size peer groups.

[10] In unreported results, I include only own-firm stock return and peer-firm stock return and obtain similar results. The results are consistent with previous literature on mixed evidence on the use of RPE.

Table 2.2. Performance benchmarking using stock returns

Dependent variable:	Ln Total Compensation				
Log asset distance between focal- and peer- firms:	(1) $\leq 40\%$	(2) $\leq 50\%$	(3) $\leq 60\%$	(4) $\leq 70\%$	(5) All peers
Ln Peer Return × Co-owned (d)	0.042***	0.036***	0.035***	0.036***	0.026***
	(0.012)	(0.012)	(0.011)	(0.011)	(0.009)
Ln Firm Return	0.151***	0.153***	0.153***	0.154***	0.152***
	(0.024)	(0.024)	(0.024)	(0.023)	(0.022)
Ln Peer Return	-0.019*	-0.015	-0.016*	-0.019**	-0.012*
	(0.010)	(0.010)	(0.009)	(0.009)	(0.007)
Ln Peer Return × Correlation	-0.109***	-0.109***	-0.110***	-0.117***	-0.087***
	(0.035)	(0.034)	(0.034)	(0.033)	(0.027)
Correlation	0.104***	0.101***	0.105***	0.103***	0.113***
	(0.027)	(0.026)	(0.026)	(0.026)	(0.022)
Co-owned (d)	-0.001	-0.003	-0.001	-0.000	0.015**
	(0.009)	(0.008)	(0.008)	(0.008)	(0.007)
CEO Age	-0.271	-0.262	-0.275	-0.285	-0.166
	(0.195)	(0.193)	(0.190)	(0.190)	(0.183)
CEO Tenure	0.014	0.012	0.012	0.012	0.017
	(0.018)	(0.018)	(0.018)	(0.018)	(0.016)
SIC3 HHI	-0.423*	-0.447*	-0.463*	-0.458*	-0.284
	(0.243)	(0.242)	(0.238)	(0.237)	(0.197)
Institutional Ownership	0.529***	0.545***	0.553***	0.558***	0.582***
	(0.097)	(0.095)	(0.094)	(0.094)	(0.089)
Ln Asset	0.332***	0.332***	0.334***	0.333***	0.310***
	(0.039)	(0.037)	(0.036)	(0.035)	(0.036)
Leverage	-0.589***	-0.614***	-0.591***	-0.598***	-0.572***
	(0.122)	(0.117)	(0.113)	(0.115)	(0.103)
Cash	0.124*	0.126*	0.126*	0.127*	0.131*
	(0.075)	(0.075)	(0.075)	(0.075)	(0.069)
Whited-Wu Index	0.079	0.131	0.155	0.149	0.045
	(0.402)	(0.394)	(0.385)	(0.380)	(0.357)
Peer Institutional Ownership	-0.011	-0.011	-0.012	-0.011	-0.021***
	(0.016)	(0.015)	(0.014)	(0.013)	(0.007)
Peer Ln Asset	0.002	0.003	0.000	0.002	0.008**
	(0.010)	(0.008)	(0.007)	(0.006)	(0.004)
Peer Leverage	-0.012	-0.008	-0.022	-0.020	-0.018**
	(0.020)	(0.018)	(0.016)	(0.015)	(0.008)
Peer Cash	-0.009	-0.003	0.001	0.002	0.009
	(0.012)	(0.010)	(0.010)	(0.010)	(0.006)
Peer Whited-Wu Index	0.145	0.130	0.117	0.128	0.163**
	(0.101)	(0.095)	(0.091)	(0.089)	(0.075)

Table 2.2 continued

Dependent variable:	Ln Total Compensation				
Log asset distance between focal- and peer- firms:	(1) $\leq 40\%$	(2) $\leq 50\%$	(3) $\leq 60\%$	(4) $\leq 70\%$	(5) All peers
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.651	0.649	0.651	0.651	0.693
Observations	118,312	147,236	174,940	202,530	740,780
P-value ($\eta_2 + \eta_3$)	0.031	0.040	0.045	0.069	0.118

This table reports the results from regressing the natural logarithm of total CEO compensation on firm performance (measured by the natural logarithm of annual stock returns including dividends), peer-firm performance, and control variables. In columns (1) to (4), I restrict the samples to peer firms with a certain log asset distance. The distance in assets is calculated as the difference between the log asset of a focal firm and that of peer firms. Column (5) presents the results for the sample with all peers. Variable definitions are provided in Appendix A. The standard errors clustered by firm are reported in parentheses below each coefficient estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

with the prediction in Holmstrom and Milgrom (1987) that the use of relative performance evaluation (RPE) will be higher for firms with high performance correlation with industry peers. The variable of interest is the interaction between peer return and a dummy variable indicating whether focal firm shares common blockholder with peer firm. The coefficient on this interaction term is positive and significant (with coefficients of 0.026 to 0.042; t-statistics of 2.799 to 3.377). The results provide evidence that among the co-owned firm-pairs, firms put a more positive weight on peer-firm performance, which supports the argument that there is less relative performance evaluation in firms that share common blockholders with same-industry peers. To measure the observed firm's compensation sensitivity to co-owned peer, I sum up the coefficients for return and the interaction term between peer return and co-owned dummy. Since $\eta_2 + \eta_3 > 0$ (p-value < 0.05), this shows that the observed firm's CEO is rewarded positively if the co-owned peer firms are performing well. In terms of control variables, I find that firms of larger size, lower leverage, and higher percentage

of institutional ownership also reward CEOs with significantly higher total compensation. Overall, the findings are robust across samples with different asset distances. In further analysis, I show only the results for peers within 70% asset distance and all peers.[11]

Next, I examine the economic significance of institutional common ownership by comparing the percentage change in total compensation that occurs because of a shock to co-owned peer performance, measured as the percentage change by one standard deviation, to the one brought by a shock to own-firm performance. Table 2.2 column (1) shows the results based on peers within 40% asset distance. One standard deviation of stock return performance is 44% per year, as reported in Table 2.1. A one-standard-deviation increase (decrease) in co-owned peer performance leads to a 1.8% (equal to 0.042 $\times$ 0.44) increase (decrease) in total CEO compensation, all else being equal. Instead of looking at the absolute change in CEO compensation brought by shock to co-owned peer performance, I compare this percentage change in compensation with the one brought by shock to own-firm performance. A one-standard-deviation increase (decrease) in own-firm performance leads to a 6.6% (0.151 $\times$ 0.44) increase (decrease) in total CEO compensation, all else being equal. So the change to the firm's compensation brought by shock to co-owned peer is about 28% of the change brought by shock to its own-firm performance, while arguably, the own-firm performance is the most important determinant for executive compensation.

While the use of stock-based compensation can align CEOs' interests with stockholders' interests, stock prices are often a noisy measure because stock prices include movements caused by factors uncontrollable by the CEOs such as market-wide movements in equity values (Sloan, 1993). Earnings are less sensitive to market-wide noise in stock prices and therefore reflect factors that are more under CEOs' control. Since accounting earnings help shield top executive compensation from market-wide movement in equity values, they may

[11]I also run tests using other asset distances. The results with other asset distances are qualitatively similar and are not reported for brevity.

contain information that is useful for the purpose of performance evaluation beyond the information provided by stock returns. As a result, firms often include certain measures of accounting profit or market (stock return) performance into executive compensation contracts. In the baseline analysis, I use stock returns as the main performance measure. Also, I include return on assets (ROA) as another performance measure for further analysis.

Table 2.3 shows the results for the test estimating CEO compensation using stock returns as well as operating performance as performance measures. For both own firm and its peer firm, I include stock returns and return on assets (ROA), defined as the earnings before interest, tax, depreciation, and amortization (EBITDA) divided by lagged total assets. First, the coefficient of own-firm ROA is positive and statistically significant, consistent with CEOs being rewarded for better performance measured by ROA. In Table 2.3, columns (1) to (2) consider observations where the asset distances between focal firm and peer firm are within 70%. Columns (1) and (2) show that the coefficient of peer ROA is generally positive but insignificant, and the coefficient estimate of the interaction between peer ROA and a co-owned dummy variable in column (2) is positive and insignificant. The results suggest that peer ROA does not appear to be a performance measure target for co-owned peers in strategic compensation scheme. The coefficient on the interaction between peer stock return performance and a co-owned dummy variable is still positive and significant with the inclusion of peer ROA, indicating that the stock market return performance of peer is a dominant measure that common blockholders target in providing anti-competitive incentive contracts. In columns (3) to (4), I obtain similar results when I consider observations with all industry peers.

The role of the competitive labor market for CEO talent is reflected in the practice of paying CEOs for luck, that is, for performance outside the CEOs' control. Oyer (2004) argues that firms adjust the pay to employee in a way that is correlated with the outside options presented by the outside labor market rather than pay a fixed wage. When there

Table 2.3. Performance benchmarking using both stock returns and operating performance

Dependent variable:	Ln Total Compensation			
Log asset distance between focal- and peer- firms:	(1) ≤ 70%	(2) ≤ 70%	(3) All peers	(4) All peers
Ln Peer Return × Co-owned (d)	0.037***	0.034***	0.025***	0.023**
	(0.011)	(0.011)	(0.009)	(0.009)
Peer EBITDA/Asset × Co-owned (d)		0.055		0.040
		(0.039)		(0.024)
Ln Firm Return	0.109***	0.109***	0.109***	0.109***
	(0.023)	(0.023)	(0.021)	(0.021)
Ln Peer Return	-0.022**	-0.021**	-0.015**	-0.015**
	(0.009)	(0.009)	(0.007)	(0.007)
Ln Peer Return × Correlation	-0.107***	-0.107***	-0.080***	-0.080***
	(0.032)	(0.032)	(0.027)	(0.027)
EBITDA/Asset	1.335***	1.334***	1.192***	1.191***
	(0.148)	(0.148)	(0.140)	(0.140)
Peer EBITDA/Asset	0.020	-0.001	0.028	0.016
	(0.024)	(0.027)	(0.019)	(0.019)
Correlation	0.085***	0.084***	0.093***	0.093***
	(0.026)	(0.026)	(0.022)	(0.022)
Co-owned (d)	0.001	-0.007	0.014**	0.009
	(0.008)	(0.010)	(0.007)	(0.007)
CEO Age	-0.286	-0.286	-0.163	-0.163
	(0.190)	(0.190)	(0.183)	(0.183)
CEO Tenure	0.003	0.003	0.007	0.007
	(0.018)	(0.018)	(0.016)	(0.016)
SIC3 HHI	-0.530**	-0.529**	-0.369*	-0.369*
	(0.234)	(0.234)	(0.196)	(0.196)
Institutional Ownership	0.388***	0.388***	0.424***	0.424***
	(0.091)	(0.091)	(0.087)	(0.087)
Ln Asset	0.399***	0.399***	0.369***	0.369***
	(0.034)	(0.034)	(0.035)	(0.035)
Leverage	-0.540***	-0.541***	-0.524***	-0.524***
	(0.117)	(0.117)	(0.105)	(0.105)
Cash	0.148**	0.148**	0.145**	0.145**
	(0.071)	(0.071)	(0.067)	(0.067)
Whited-Wu Index	0.721**	0.719**	0.576*	0.574*
	(0.355)	(0.356)	(0.341)	(0.341)
Peer Institutional Ownership	-0.014	-0.013	-0.024***	-0.023***
	(0.013)	(0.013)	(0.007)	(0.007)
Peer Ln Asset	-0.001	-0.001	0.007	0.007
	(0.006)	(0.006)	(0.004)	(0.004)

Table 2.3 continued

Dependent variable:	Ln Total Compensation			
Log asset distance between focal- and peer- firms:	(1) $\leq 70\%$	(2) $\leq 70\%$	(3) All peers	(4) All peers
Peer Leverage	-0.013	-0.013	-0.014*	-0.014*
	(0.015)	(0.015)	(0.009)	(0.009)
Peer Cash	0.002	0.002	0.008	0.008
	(0.010)	(0.010)	(0.006)	(0.006)
Peer Whited-Wu Index	0.056	0.055	0.122	0.121
	(0.094)	(0.094)	(0.080)	(0.080)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.661	0.661	0.700	0.701
Observations	202,164	202,164	739,500	739,500

This table reports the results from regressing the natural logarithm of total CEO compensation on firm performance (measured by the natural logarithm of annual stock returns including dividends and by the firm return on assets [calculated as EBITDA divided by lagged total assets]), peer-firm performance, and control variables. In columns (1) and (2), I restrict the sample to peer firms with log asset distance less than or equal to 70%. Columns (3) and (4) present the results for the sample with all peers. Variable definitions are provided in Appendix A. The standard errors clustered by firm are reported in parentheses below each coefficient estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

is a high demand for managerial talent and CEO talent is scarce, firms adjust the pay to the CEO to minimize the chance that he will leave to another firm. Firms justify their CEOs' compensation by benchmarking their executive compensation against a peer group and rationalize the peer group by claiming that the firms compete for managerial talent with those selected companies. To determine the effects of peer-firm compensation benchmarking on CEO pay, I add peer-firm total compensation into the regression.[12] This is done based

[12]Bizjak, Lemmon, and Naveen (2008) find that peer group compensation benchmarking is common and significantly affects CEO compensation. Other studies that also focus on how the use of peer group may affect the compensation setting process are Albuquerque, De Franco, and Verdi (2013); Bizjak, Lemmon, and Nguyen (2011); Faulkender and Yang (2010, 2013).

on the premise that firms benchmark CEO compensation not only on peer performance but also on peer compensation to reflect the increased value of a CEO's outside options.

Table 2.4 presents the results from regressing CEO compensation on peer-firm performance and compensation. Similar to the results obtained in Table 2.2, the coefficients on the interaction between peer-firm performance and a co-owned dummy variable are positive and significant in all specifications. Consistent with the prediction in Oyer (2004) that CEO compensation is benchmarked to industry peers to retain talents, I find that CEO compensation is positively associated with peer-firm compensation. The variable of interest here is the interaction between peer-firm compensation and a co-owned dummy variable. Columns (2) and (4) show that the coefficient estimates on the interaction between peer-firm compensation and a co-owned dummy variable are positive and significant, suggesting that the observed firm's executive compensation exhibits additional sensitivity toward the co-owned peer firm's executive compensation. These results provide further evidence that the positive pay sensitivity to co-owned peer performance remains significant after controlling for peer compensation.

While compensation is given in the form of cash compensation and equity compensation (stocks and options awards), prior research (Hall and Liebman, 1998) examines pay-to-performance responsiveness that includes the change in the value of stocks and stock options in the measure; it also documents that changes in the value of stocks and options account for virtually all the sensitivity, whereas salary and bonus are quite insensitive to changes in firm performance. To examine the use of equity-based incentive compensation (stocks and options, in particular) to incentivize managers of co-owned portfolio firms, I decompose total compensation into the cash component, which includes salary and bonus, and the stocks and options component, which includes the total value of restricted stocks granted and the total value of stock options granted. I then examine whether short-term incentives (i.e., the cash component) and long-term incentives (i.e., the stocks and options component) exhibit additional sensitivity to the performance of co-owned peers.

Table 2.4. Performance and compensation benchmarking

Dependent variable:	Ln Total Compensation			
Log asset distance between focal- and peer- firms:	(1) ≤ 70%	(2) ≤ 70%	(3) All peers	(4) All peers
Ln Peer Return × Co-owned (d)	0.036***	0.032***	0.026***	0.024***
	(0.011)	(0.011)	(0.009)	(0.009)
Ln Peer Total Compensation × Co-owned (d)		0.020***		0.008***
		(0.005)		(0.002)
Ln Firm Return	0.154***	0.154***	0.152***	0.152***
	(0.023)	(0.023)	(0.022)	(0.022)
Ln Peer Return	-0.022**	-0.021**	-0.014*	-0.014*
	(0.009)	(0.009)	(0.007)	(0.007)
Ln Peer Return × Correlation	-0.115***	-0.115***	-0.086***	-0.086***
	(0.033)	(0.033)	(0.027)	(0.027)
Ln Peer Total Compensation	0.018***	0.011***	0.012***	0.009***
	(0.003)	(0.003)	(0.001)	(0.002)
Correlation	0.100***	0.100***	0.111***	0.111***
	(0.026)	(0.026)	(0.022)	(0.022)
Co-owned (d)	-0.001	-0.153***	0.015**	-0.050**
	(0.008)	(0.042)	(0.007)	(0.021)
CEO Age	-0.282	-0.283	-0.166	-0.166
	(0.190)	(0.190)	(0.183)	(0.183)
CEO Tenure	0.012	0.012	0.017	0.017
	(0.018)	(0.018)	(0.016)	(0.016)
SIC3 HHI	-0.459*	-0.452*	-0.284	-0.282
	(0.237)	(0.237)	(0.197)	(0.197)
Institutional Ownership	0.559***	0.561***	0.582***	0.582***
	(0.094)	(0.094)	(0.089)	(0.089)
Ln Asset	0.333***	0.334***	0.310***	0.310***
	(0.035)	(0.035)	(0.036)	(0.036)
Leverage	-0.597***	-0.597***	-0.572***	-0.572***
	(0.114)	(0.114)	(0.103)	(0.103)
Cash	0.126*	0.126*	0.130*	0.130*
	(0.075)	(0.075)	(0.069)	(0.069)
Whited-Wu Index	0.148	0.149	0.042	0.043
	(0.381)	(0.381)	(0.357)	(0.357)
Peer Institutional Ownership	-0.025*	-0.022*	-0.030***	-0.029***
	(0.014)	(0.014)	(0.007)	(0.007)
Peer Ln Asset	-0.007	-0.007	0.003	0.003
	(0.006)	(0.006)	(0.004)	(0.004)
Peer Leverage	-0.017	-0.017	-0.018**	-0.019**
	(0.015)	(0.015)	(0.008)	(0.008)

Table 2.4 continued

Dependent variable:	Ln Total Compensation			
Log asset distance between focal- and peer- firms:	(1) $\leq 70\%$	(2) $\leq 70\%$	(3) All peers	(4) All peers
Peer Cash	-0.005	-0.005	0.005	0.005
	(0.010)	(0.010)	(0.006)	(0.006)
Peer Whited-Wu Index	0.112	0.109	0.153**	0.151**
	(0.089)	(0.089)	(0.075)	(0.075)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.651	0.651	0.693	0.693
Observations	202,530	202,530	740,780	740,780

This table estimates the sensitivity of CEO compensation to its peer-firm performance and compensation. It reports the results from regressing the natural logarithm of total CEO compensation on firm performance (measured by the natural logarithm of annual stock returns including dividends), peer-firm performance, peer-firm compensation (measured by the natural logarithm of peer firm total CEO compensation), and control variables. In columns (1) and (2), I restrict the sample to peer firms with log asset distance less than or equal to 70%. Columns (3) and (4) present the results for the sample with all peers. Variable definitions are provided in Appendix A. The standard errors clustered by firm are reported in parentheses below each coefficient estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

To evaluate the compensation sensitivity to own-firm and peer-firm stock return performance, I estimate a similar specification as in Table 2.2. Table 2.5 presents the estimation results for the specifications where the dependent variables are either cash compensation or stocks and options compensation. While the coefficient on the interaction between peer-firm stock return and a co-owned dummy variable is not statistically significant when the dependent variable is cash compensation, it is positive and statistically significant when the dependent variable is stocks and options compensation. This relation provides some evidence of co-owned firms' board of directors using equity-based incentives to incentivize managers to mitigate direct competition with their co-owned peers.

Table 2.5. Compensation by components

Dependent variable:	Cash		Stocks and Options	
Log asset distance	(1)	(2)	(3)	(4)
between focal- and peer- firms:	$\leq 70\%$	All peers	$\leq 70\%$	All peers
Ln Peer Return × Co-owned (d)	0.009	-0.003	0.088**	0.065**
	(0.009)	(0.008)	(0.044)	(0.032)
Ln Firm Return	0.136***	0.122***	0.164**	0.183***
	(0.019)	(0.021)	(0.076)	(0.069)
Ln Peer Return	-0.019***	-0.004	-0.005	-0.012
	(0.006)	(0.006)	(0.037)	(0.026)
Ln Peer Return × Correlation	0.008	0.036*	-0.261**	-0.215**
	(0.023)	(0.022)	(0.128)	(0.100)
Correlation	0.015	0.016	0.215**	0.248***
	(0.018)	(0.017)	(0.095)	(0.076)
Co-owned (d)	0.020*	0.033***	-0.008	0.028
	(0.010)	(0.010)	(0.034)	(0.025)
CEO Age	-0.017	0.116	-2.228***	-1.822***
	(0.209)	(0.218)	(0.608)	(0.542)
CEO Tenure	0.051***	0.047***	-0.081	-0.098*
	(0.017)	(0.016)	(0.059)	(0.051)
SIC3 HHI	0.001	0.131	-0.824	-1.103*
	(0.244)	(0.184)	(0.812)	(0.651)
Institutional Ownership	0.271***	0.316***	1.389***	1.568***
	(0.077)	(0.076)	(0.309)	(0.291)
Ln Asset	0.129***	0.114***	0.526***	0.460***
	(0.030)	(0.030)	(0.120)	(0.104)
Leverage	-0.241**	-0.234**	-1.061**	-1.159***
	(0.105)	(0.093)	(0.425)	(0.392)
Cash	-0.056	-0.061	-0.275	-0.180
	(0.045)	(0.048)	(0.206)	(0.181)
Whited-Wu Index	-0.340	-0.076	-0.542	-0.746
	(0.308)	(0.330)	(1.218)	(0.992)
Peer Institutional Ownership	0.023*	-0.018**	-0.060	-0.050*
	(0.013)	(0.007)	(0.057)	(0.026)
Peer Ln Asset	-0.006	0.001	0.033	0.032***
	(0.005)	(0.004)	(0.039)	(0.012)
Peer Leverage	-0.018	-0.006	0.029	-0.015
	(0.019)	(0.008)	(0.068)	(0.029)
Peer Cash	-0.020**	-0.002	-0.022	-0.001
	(0.010)	(0.006)	(0.036)	(0.019)
Peer Whited-Wu Index	0.006	0.017	0.690*	0.562**
	(0.070)	(0.068)	(0.355)	(0.233)

Table 2.5 continued

Dependent variable:	Cash		Stocks and Options	
Log asset distance between focal- and peer- firms:	(1) $\leq 70\%$	(2) All peers	(3) $\leq 70\%$	(4) All peers
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.608	0.618	0.443	0.470
Observations	202,530	740,780	118,312	740,780

This table estimates the sensitivity of each component in CEO compensation to own-firm and peer-firm performance. The dependent variables are the cash component, which consists of salary and bonus, and the stocks and options component, which consists of the total value of restricted stocks granted and the total value of stock options granted. Columns (1) and (2) present the results for the estimation that uses cash component as the dependent variable when I restrict the sample to peer firms with log asset distance less than or equal to 70% and when I use the sample with all peers, respectively. Columns (3) and (4) present the results for the estimation that uses stocks and options component as the dependent variable when I restrict the sample to peer firms with log asset distance less than or equal to 70% and when I use the sample with all peers, respectively. Variable definitions are provided in Appendix A. The standard errors clustered by firm are reported in parentheses below each coefficient estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

2.4.2 Identification

A potential endogeneity concern is that omitted variables that are unobservable correlate with firms' institutional common ownership status and their compensation benchmarking practice. Blockholders may choose to invest in some same-industry firms because of their industry leadership status, and these firms could be correlated in certain ways. The correlation may determine the pay-performance sensitivity among these firms. As a result, the positive correlations between CEO compensation and co-owned peer performance from the baseline results could be biased. Also, institutional blockholders never randomly initiate a stake in a firm. It is possible that institutional blockholders seek to own firms when these firms are going to coordinate in the product market and exhibit positive benchmarking in compensation. Therefore, the baseline results in the beginning of the paper could be subjected

to omitted variables and reverse causality issues. In order to claim causality between the common ownership and positive compensation benchmarking, I resort to exogenous change to the number of common blockholders between firms.

To address the above-mentioned issues, I exploit a quasi-natural experiment involving asset management firms merger using difference-in-differences (DiD) approach. He and Huang (2017) are the first to use a similar identification strategy where they use all mergers between financial institutions. Specifically, I make use of arguably the largest asset management firms' merger, the merger between BlackRock Inc. ("BlackRock") and Barclays Global Investors (BGI), to create exogenous change in the number of common blockholders. (On June 11, 2009, BlackRock announced that it had agreed to acquire BGI from the UK-based bank Barclays PLC ["Barclays"]. The merger was completed on December 1, 2009.) According to the BlackRock press release, the main strategic reason of the acquisition was to bring together two market leaders to create the preeminent investment management firm, BlackRock Global Investors ("BlackRock") with the unique ability to combine active, quantitative and index strategies to develop investment solutions for institutional clients worldwide. Also, because each portfolio firm represents a very small fraction of the asset management firms' portfolio, I can safely assume that the motivation of the event is not related to the coordination in the product market and compensation benchmarking.

As the benchmarking analysis is done at the firm-pair-year panel, I identify treatment and control firm pairs instead of individual treatment and control firms. To identify a treatment firm pair, I require that the focal firm and peer firm be block-held separately by BlackRock and Barclays one year before the merger completion. These two blockholders in focal firm and peer firm cannot be the same institution, otherwise this firm-pair is considered a co-owned firm pair before the merger. After the merger, the focal firm and peer firm in a treatment firm pair will become co-owned because of the asset management firms' merger. Control group refers to control firm pairs where only the focal firm is owned by either BlackRock or

Barclays before and after the merger. The requirement for the control firm pairs helps to control for the stock-picking skills of the merging asset management firms in difference-in-differences (DiD) analysis. I use a seven-year window, which comprises three years before and three years after the event year and excludes the event year.

I estimate the following multivariate DiD model around the merger:

$$\begin{aligned}\text{Total Compensation}_{ijt} &= c + \beta_1 \text{Peer Ret}_{ijt} \times \text{Treat} \times \text{Post} + \beta_2 \text{Peer Ret}_{ijt} \times \text{Treat} \\ &\quad + \beta_3 \text{Peer Ret}_{ijt} \times \text{Post} + \beta_4 \text{Treat} \times \text{Post} + \beta_5 \text{Treat} + \beta_6 \text{Ret}_{it} \\ &\quad + \beta_7 \text{Peer Ret}_{ijt} + \beta_8 \text{Peer Ret}_{it} \times \text{Correlation}_{ijt} \\ &\quad + \gamma \text{Control Var}_{t-1} + \text{Year}_t + \text{Firm}_i + \epsilon_{ijt}, \end{aligned} \tag{2.2}$$

Table 2.6 reports the results from the DiD analysis. In different asset distance samples, the coefficients on Peer Ret × Treat × Post are positive and significant except for in column (5), suggesting that the focal firms whose number of common blockholder with peer firms increases because of the asset management firms' merger put additional positive pay sensitivity on the co-owned peer firms' stock return performance.

2.4.3 Potential mechanism: Choice of RPE peers

The main analysis so far shows that common ownership by institutional investors makes firms more likely to have positive pay sensitivity to co-owned peer performance. The next natural question is what the mechanism is. In this section, I examine a potential mechanism: choice of performance benchmarking peers, motivated by the rules on executive compensation disclosures that the SEC amended in 2006 as discussed in Gong, Li, and Shin (2011).

Table 2.6. BlackRock and Barclays Global Investors (BGI) merger

Dependent variable:	Ln Total Compensation				
Log asset distance between focal- and peer- firms:	(1) $\leq 40\%$	(2) $\leq 50\%$	(3) $\leq 60\%$	(4) $\leq 70\%$	(5) All peers
Ln Peer return × Treat × Post	0.329***	0.272**	0.196*	0.226**	0.101
	(0.112)	(0.107)	(0.099)	(0.098)	(0.090)
Ln Peer return × Treat	-0.108	-0.065	-0.048	-0.048	-0.055
	(0.089)	(0.086)	(0.080)	(0.082)	(0.076)
Ln Peer return × Post	-0.063	-0.033	0.007	-0.018	0.003
	(0.071)	(0.066)	(0.061)	(0.062)	(0.040)
Treat x Post	-0.045	-0.027	-0.034	-0.056	-0.031
	(0.083)	(0.078)	(0.072)	(0.072)	(0.068)
Treat	-0.023	0.009	0.017	0.039	0.009
	(0.060)	(0.060)	(0.051)	(0.050)	(0.043)
Ln Firm return	0.043	0.075	0.077	0.057	0.071
	(0.112)	(0.110)	(0.100)	(0.095)	(0.077)
Ln Peer return	0.035	0.007	-0.006	0.001	0.019
	(0.058)	(0.051)	(0.041)	(0.038)	(0.033)
Ln Peer return × Return correlation	-0.153	-0.043	-0.106	-0.099	-0.047
	(0.185)	(0.178)	(0.170)	(0.166)	(0.101)
Return correlation	-0.002	0.074	0.097	0.092	0.077
	(0.105)	(0.101)	(0.090)	(0.083)	(0.061)
Ln Age	0.130	0.187	0.287	0.310	0.149
	(0.770)	(0.747)	(0.709)	(0.660)	(0.482)
Ln Tenure	-0.019	-0.033	-0.027	-0.046	-0.068
	(0.076)	(0.075)	(0.072)	(0.068)	(0.062)
SIC3 HHI	-0.385	-0.487	0.214	-1.804	-2.119
	(3.154)	(3.124)	(3.013)	(2.363)	(1.646)
Institutional Ownership	0.987**	0.898**	0.851**	0.818**	0.648*
	(0.389)	(0.369)	(0.340)	(0.327)	(0.332)
Ln Asset	0.415**	0.366**	0.366**	0.380**	0.339**
	(0.164)	(0.166)	(0.153)	(0.155)	(0.159)
Leverage	-0.084	-0.034	-0.095	-0.070	-0.113
	(0.466)	(0.448)	(0.433)	(0.401)	(0.390)

Table 2.6 continued

Dependent variable:	Ln Total Compensation				
Log asset distance	(1)	(2)	(3)	(4)	(5)
between focal- and peer- firms:	$\leq 40\%$	$\leq 50\%$	$\leq 60\%$	$\leq 70\%$	All peers
Cash	0.225	0.199	0.165	0.187	0.139
	(0.179)	(0.174)	(0.166)	(0.169)	(0.165)
Whited-Wu Index	-2.434	-2.334	-2.139	-1.984	-1.804
	(2.045)	(1.899)	(1.797)	(1.739)	(1.349)
Peer Institutional Ownership	-0.025	-0.094	-0.079	-0.076	-0.020
	(0.080)	(0.069)	(0.054)	(0.050)	(0.015)
Peer Ln Asset	-0.022	0.028	0.034	0.032	0.006
	(0.065)	(0.041)	(0.030)	(0.024)	(0.006)
Peer Leverage	0.051	0.070	0.063	0.036	0.005
	(0.105)	(0.098)	(0.106)	(0.104)	(0.020)
Peer Cash	0.012	-0.006	-0.013	-0.010	0.005
	(0.036)	(0.044)	(0.031)	(0.027)	(0.004)
Peer Whited-Wu Index	0.067	0.101	0.142	0.061	0.144
	(0.426)	(0.354)	(0.297)	(0.268)	(0.119)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.829	0.841	0.835	0.841	0.865
Observations	1,184	1,472	1,778	2,065	10,831

This table reports the results from difference-in-differences (DiD) analysis. The dependent variable is the natural logarithm of total CEO compensation. *Treat* is an indicator variable that equals one for the firm pair that becomes co-owned because of the asset managers' merger, and zero otherwise. *Post* is an indicator variable that equals one for the post-merger period, and zero otherwise. *Ln Firm return* is measured by the natural logarithm of annual stock returns including dividends. *Ln Peer return* is measured by the natural logarithm of peer-firm annual stock returns including dividends. In columns (1) to (4), I restrict the samples to peer firms with a certain log asset distance to determine the closeness of the firm and its peers. The distance in assets is calculated as the difference between the log asset of a focal firm and that of peer firms. Column (5) presents the results for the sample with all peers. Variable definitions are provided in Appendix A. The standard errors clustered by firm are reported in parentheses below each coefficient estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Given that I am able to observe the actual RPE peers, I examine the selection of RPE peers using the following regression:

$$\begin{aligned}\text{RPE Peer}_{ijt}(d) \;=\; & f(c + \beta_1 \text{Co-owned (d)}_{ijt-1} + \beta_2 \text{Correlation}_{ijt-1} \\ & + \beta_3 \text{Co-owned (d)}_{ijt-1} \times \text{Correlation}_{ijt-1} \\ & + \beta_4 \text{Ln Asset Distance}_{ijt-1} + \beta_5 \text{Return Distance}_{ijt-1} \\ & + \text{Fixed effects}). \end{aligned} \tag{2.3}$$

Similar to the baseline regressions relying on the implicit approach to test the use of RPE, for each observed firm, I include all the same three-digit SIC industry peers as potential performance benchmarking peers. The data on performance benchmarking peers as disclosed in proxy documents allow me to clearly identify the actual peer group companies. Specifically, for each RPE firm, I examine whether the potential RPE peer is the actual performance benchmarking peer that the observed firm uses. I then use a dummy variable to indicate whether the potential peer is the actual peer used in the RPE contract. If the potential peer does appear on the RPE peer list, I set the value for the observation as 1, and 0 otherwise.

Table 2.7 reports results from estimating the RPE peers selection. Consistent with the results in Gong, Li, and Shin (2011), potential peers with higher correlation are more likely to be selected as performance benchmarking peers. Also, potential peers with similar return performance and size are more likely to be chosen as RPE peers. The variable of interest here is the co-owned dummy. I find that same-industry peers that share common block-holders are less likely to be selected as RPE peers. Furthermore, it is worth noting that the coefficient estimates of the interaction term between the co-owned dummy and the stock return correlation are all positive and statistically significant, suggesting that peers that are co-owned but with higher than average stock return correlation with observed firms are still more likely to be included as RPE peers. This result is consistent with a view of efficient contracting. In summary, the findings from the analysis on the choice of RPE peers suggest

Table 2.7. Choice of RPE peers

Dependent variable:	Ind-peer being chosen as RPE peer (d)							
	(1) LPM	(2) LPM	(3) LPM	(4) LPM	(5) LPM	(6) LPM	(7) Probit	(8) Probit
Co-owned (d)	-0.025**	-0.027***	-0.016*	-0.018**	-0.015*	-0.016*	-0.021**	-0.028***
	(0.011)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
Correlation	0.276***	0.154***	0.281***	0.208***	0.327***	0.248***	0.245***	0.195***
	(0.041)	(0.041)	(0.025)	(0.027)	(0.028)	(0.029)	(0.033)	(0.037)
Co-owned (d) × Correlation		0.268***		0.161***		0.180***		0.098***
		(0.042)		(0.034)		(0.036)		(0.033)
Ln Asset Distance	-0.077***	-0.079***	-0.080***	-0.081***	-0.079***	-0.080***	-0.103***	-0.104***
	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)
Return Distance	-0.032***	-0.036***	-0.020**	-0.022***	-0.024***	-0.027***	-0.051***	-0.052***
	(0.010)	(0.010)	(0.008)	(0.008)	(0.007)	(0.007)	(0.013)	(0.013)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other FE	-	-	Ind	Ind	Ind × Year	Ind × Year	-	-
Adjusted R^2	0.140	0.145	0.217	0.218	0.232	0.234		
Pseudo R^2							0.213	0.214
Observations	29,452	29,452	29,452	29,452	29,452	29,452	29,452	29,452

This table reports the results from the selection of performance benchmarking peers analysis. The dependent variable is the dummy variable indicating whether the potential peer is chosen as an actual RPE peer. In columns (1) to (6), I use a linear probability model. Columns (7) to (8) present the results for the probit regression. Variable definitions are provided in Appendix A. The standard errors clustered by firm are reported in parentheses below each coefficient estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

that firms with common ownership exclude peers that share common blockholders from the RPE peer group.

2.4.4 Implications of institutional common ownership for future product market characteristics

In this section, I investigate the effect of common ownership on future product market characteristics, such as product differentiation, combined market share, and geographic focus overlap, as a way to examine the effect of the anti-competitive incentive contracts. In the following study, I keep one observation for every two firm-pair-year observations (that comprise the same two firms) because the dependent and independent variables would be essentially the same for observations "Firm A, Firm B, Year X" and "Firm B, Firm A, Year X."

I first conduct an analysis based on future product similarity using Text-Based Network Industry Classifications (TNIC) pairwise data of Hoberg and Phillips (2010, 2016). The TNIC system is explicitly constructed based on the product similarities of firms. The higher the similarity score between two firms, the more likely these two firms are engaging in head-to-head competition in the product market. The TNIC pairwise data set a threshold on the similarity score; only the firm pairs that have a high-enough similarity score will appear in the TNIC network. Since TNIC is built from 10-K filings that firms file each year, the network for each firm evolves over time. Unlike the traditional standard industry classification where the relation between same-industry firms is static, TNIC assigns each firm to a different set of firms every year. This unique feature allows me to capture the dynamics of the product market relations between focal firms and peer firms. In other words, I can observe two firms that were previously in the same network but are not so in the next period. To examine whether firms with common block ownership differentiate their products to avoid direct competition with each other, I estimate the linear probability model (LPM) where the

dependent variables are TNIC linkage in year $t + 1$ and year $t + 2$. Table 2.8 presents the results for the implication of institutional common ownership for future product similarity. The results show that co-owned firm pairs are more likely to have product differentiation in the future. This suggests that co-owned firm pairs distance away from each other in the product market to avoid direct competition.

I conduct a further test as to whether co-owned firms are more likely to have a higher combined market share in the future. As argued earlier, I expect that firms with common block ownership will enjoy higher growth in joint market share, as firms differentiate their products if they want to avoid direct competition with each other. Table 2.9 presents the results of the specifications that use a dummy variable for co-owned as the independent variable, use a natural logarithm of a combined market share in one year (Ln CombinedMktShr$_{t+1}$) as the dependent variable in Models 1 and 2, and use a natural logarithm of a combined market share in two years (Ln CombinedMktShr$_{t+2}$) as the dependent variable in Models 3 and 4. I find that there is a strong positive association between being a co-owned firm-pair and future combined market share.

Lastly, I examine whether co-owned firms are more likely to have higher geographic focus overlap measured by overlapped state name count percentage in the future. In particular, I look at the relation between the co-owned dummy and future overlapped state name count percentage. Following Garcia and Norli (2012), I extract state name counts from annual reports filed with the Securities and Exchange Commission (SEC) on 10-K filings and calculate the percentage of state name counts for each firm in each state. To account for the overlap of geographic focus, if two firms have an overlapping state name count, I take the smallest percentage of each overlapped state and sum up across all the states to find the total state count percentage overlap. Table 2.10 presents the results from testing how co-owned firm pairs experience state overlap for one and two years in the future. I find no evidence of a strong relation between being a co-owned firm pair and having state overlap in one year, but

Table 2.8. Future product similarity

Dependent variable:	TNIC Peers$_{t+1}$		TNIC Peers$_{t+2}$	
Log asset distance	(1)	(2)	(3)	(4)
between focal- and peer- firms:	$\leq 70\%$	All peers	$\leq 70\%$	All peers
Co-owned (d)	-0.020***	-0.006***	-0.009*	-0.005**
	(0.005)	(0.002)	(0.006)	(0.002)
Institutional Ownership	0.006	-0.015*	0.003	-0.004
	(0.021)	(0.009)	(0.023)	(0.010)
Ln Asset	0.004	0.015***	0.010	0.012***
	(0.010)	(0.003)	(0.010)	(0.003)
Leverage	-0.020	-0.002	-0.040	-0.028**
	(0.025)	(0.010)	(0.027)	(0.012)
Cash	0.014	0.013***	0.001	0.009**
	(0.010)	(0.004)	(0.010)	(0.004)
Whited-Wu Index	0.173**	0.165***	0.115	0.159***
	(0.074)	(0.029)	(0.075)	(0.031)
Peer Institutional Ownership	-0.002	-0.003	0.012	0.001
	(0.020)	(0.008)	(0.022)	(0.009)
Peer Ln Asset	0.008	0.013***	0.005	0.014***
	(0.009)	(0.003)	(0.010)	(0.003)
Peer Leverage	0.003	0.002	0.002	-0.013
	(0.025)	(0.010)	(0.028)	(0.011)
Peer Cash	0.010	0.007*	0.019**	0.008**
	(0.009)	(0.004)	(0.009)	(0.004)
Peer Whited-Wu Index	0.145**	0.130***	0.106	0.137***
	(0.071)	(0.027)	(0.069)	(0.028)
Firm-pair FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.590	0.574	0.552	0.530
Observations	95,784	349,490	90,114	327,645

This table reports the linear probability model results from regressing firms' future product market relation (TNIC linkage) on the co-owned dummy variable, while controlling for variables expected to explain product market relation. TNIC Peers$_{t+1}$ is the one-year-ahead TNIC linkage, and TNIC Peers$_{t+2}$ is the two-year-ahead TNIC linkage. Columns (1) and (2) present the results for the estimation that uses TNIC Peers$_{t+1}$ as the dependent variable when I restrict the sample to peer firms with log asset distance less than or equal to 70% and when I use the sample with all peers, respectively. Columns (3) and (4) present the results for the estimation that uses TNIC Peers$_{t+2}$ as the dependent variable when I restrict the sample to peer firms with log asset distance less than or equal to 70% and when I use the sample with all peers, respectively. Variable definitions are provided in Appendix A. The standard errors clustered by firm are reported in parentheses below each coefficient estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.9. Future combined market share

Dependent variable:	LnMktShr_{t+1}		LnMktShr_{t+2}	
Log asset distance	(1)	(2)	(3)	(4)
between focal- and peer- firms:	$\leq 70\%$	All peers	$\leq 70\%$	All peers
Cross-held (d)	0.008*	-0.001	0.012***	0.005**
	(0.004)	(0.002)	(0.004)	(0.002)
Institutional Ownership	0.185***	0.083***	0.191***	0.121***
	(0.022)	(0.010)	(0.021)	(0.011)
Ln Asset	0.191***	0.154***	0.165***	0.123***
	(0.008)	(0.004)	(0.010)	(0.004)
Leverage	-0.071***	-0.055***	-0.084***	-0.057***
	(0.020)	(0.011)	(0.023)	(0.013)
Cash	-0.014***	-0.027***	-0.014***	-0.024***
	(0.005)	(0.002)	(0.004)	(0.002)
Whited-Wu Index	-0.067	-0.131***	0.087	-0.019
	(0.060)	(0.027)	(0.059)	(0.027)
Peer Institutional Ownership	0.155***	0.141***	0.183***	0.155***
	(0.024)	(0.013)	(0.023)	(0.014)
Peer Ln Asset	0.177***	0.200***	0.134***	0.160***
	(0.010)	(0.005)	(0.011)	(0.005)
Peer Leverage	-0.082***	-0.057***	-0.092***	-0.072***
	(0.024)	(0.013)	(0.026)	(0.015)
Peer Cash	-0.018**	-0.036***	-0.007	-0.023***
	(0.009)	(0.005)	(0.009)	(0.004)
Peer Whited-Wu Index	-0.270***	-0.279***	-0.118	-0.083**
	(0.082)	(0.036)	(0.074)	(0.037)
Firm-pair FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.976	0.971	0.976	0.970
Observations	92,225	338,794	82,787	305,267

This table reports the results from regressing firms' future combined market share on the co-owned dummy variable, while controlling for variables expected to explain combined market share. LnMktShr_{t+1} is the one-year-ahead combined market share, and LnMktShr_{t+2} is the two-year-ahead combined market share. Columns (1) and (2) present the results for the estimation that uses LnMktShr_{t+1} as the dependent variable when I restrict the sample to peer firms with log asset distance less than or equal to 70% and when I use the sample with all peers, respectively. Columns (3) and (4) present the results for the estimation that uses LnMktShr_{t+2} as the dependent variable when I restrict the sample to peer firms with log asset distance less than or equal to 70% and when I use the sample with all peers, respectively. Variable definitions are provided in Appendix A. The standard errors clustered by firm are reported in parentheses below each coefficient estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 2.10. Future state count percentage overlap

Dependent variable:	Ste Cnt % Overlap$_{t+1}$		Ste Cnt % Overlap$_{t+2}$	
Log asset distance	(1)	(2)	(3)	(4)
between focal- and peer- firms:	$\leq 70\%$	All peers	$\leq 70\%$	All peers
Co-owned (d)	0.001	0.001	0.003	0.005***
	(0.003)	(0.001)	(0.003)	(0.001)
Institutional Ownership	0.007	0.014***	0.014	0.003
	(0.013)	(0.005)	(0.014)	(0.006)
Ln Asset	0.005	0.002	0.001	0.002
	(0.005)	(0.002)	(0.006)	(0.002)
Leverage	-0.024	-0.019***	-0.017	-0.020***
	(0.015)	(0.006)	(0.017)	(0.007)
Cash	-0.015***	-0.007***	-0.003	-0.004*
	(0.006)	(0.002)	(0.006)	(0.002)
Whited-Wu Index	0.007	0.003	-0.039	-0.018
	(0.041)	(0.015)	(0.047)	(0.017)
Peer Institutional Ownership	0.010	0.013***	0.017	0.008
	(0.013)	(0.005)	(0.014)	(0.005)
Peer Ln Asset	-0.001	-0.000	0.002	0.002
	(0.006)	(0.001)	(0.006)	(0.002)
Peer Leverage	-0.020	-0.009	-0.007	-0.022***
	(0.015)	(0.006)	(0.017)	(0.006)
Peer Cash	-0.009*	-0.005**	-0.002	-0.003
	(0.005)	(0.002)	(0.005)	(0.002)
Peer Whited-Wu Index	-0.009	-0.020	0.020	0.011
	(0.042)	(0.014)	(0.042)	(0.015)
Firm-pair FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.794	0.763	0.803	0.773
Observations	39,490	143,450	32,051	116,043

This table reports the results from regressing firms' future state count percentage overlap on the co-owned dummy variable, while controlling for variables expected to explain state count percentage overlap. Ste Cnt % Overlap$_{t+1}$ is the one-year-ahead state count percentage overlap, and Ste Cnt % Overlap$_{t+2}$ is the two-year-ahead state count percentage overlap. Columns (1) and (2) present the results for the estimation that uses Ste Cnt % Overlap$_{t+1}$ as the dependent variable when I restrict the sample to peer firms with log asset distance less than or equal to 70% and when I use the sample with all peers, respectively. Columns (3) and (4) present the results for the estimation that uses Ste Cnt % Overlap$_{t+2}$ as the dependent variable when I restrict the sample to peer firms with log asset distance less than or equal to 70% and when I use the sample with all peers, respectively. Variable definitions are provided in Appendix A. The standard errors clustered by firm are reported in parentheses below each coefficient estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

there is a strong positive relation between being a co-owned firm and having state overlap in two years for the sample where I consider observations with all peers. The evidence shows that co-owned firm pairs are coordinating in geographic focus. Combining the results from product similarity and geographic focus, these co-owned firms are aware that they will not be in fierce competition with each other even if they are focusing on the same geographic product market. As a result, they are encouraged to go into each other's geographic market to jointly capture more market share.

2.4.5 Subsample tests based on product market characteristics

In this section, I study when common blockholders initiate the coordinating program through a compensation setting. I divide the sample into subsamples based on the product market characteristics, such as the industry Herfindahl-Hirschman Index (HHI) and combined market share.

First, I examine how firms in different levels of industry competition benchmark their CEO compensation to their peer firms' performance. I measure industry competitiveness using the Herfindahl-Hirschman Index (HHI), which is defined as the sum of squared percentage market shares in sales based on three-digit SIC industry classification. The HHI ranges from 0 to 1, moving from a large number of small firms to a single monopolistic firm. Next, I partition the sample into three subsamples based on the HHI and re-estimate the specification in column (5) of Table 2.2, where I consider all peer firms regardless of log asset distance in each subsample. Firm-pair-year observations that belong to the first, second, and third terciles of the HHI are classified as the low, medium, and high group, respectively. Table 2.11 reports the performance benchmarking results for the three HHI groups. The coefficient estimate of the interaction between peer performance and a dummy variable for co-owned firm pair is positive and statistically significant only in the low HHI subsample. This suggests that CEOs of firms in highly competitive industries are being rewarded if

Table 2.11. Subsample test by product market characteristics: HHI

Dependent variable:	Ln Total Compensation		
	(1)	(2)	(3)
HHI:	Low	Medium	High
Ln Peer Return × Co-owned (d)	0.035**	0.010	0.015
	(0.016)	(0.012)	(0.014)
Ln Firm Return	0.157***	0.108***	0.127***
	(0.036)	(0.030)	(0.040)
Ln Peer Return	-0.030**	0.004	-0.009
	(0.014)	(0.010)	(0.011)
Ln Peer Return × Correlation	-0.138***	-0.010	-0.067*
	(0.047)	(0.037)	(0.037)
Correlation	0.067*	0.058*	0.168***
	(0.035)	(0.030)	(0.029)
Co-owned (d)	0.004	0.022**	-0.001
	(0.013)	(0.009)	(0.008)
CEO Age	0.006	0.242	-0.614***
	(0.316)	(0.413)	(0.205)
CEO Tenure	0.048*	-0.017	0.014
	(0.029)	(0.026)	(0.022)
SIC3 HHI	6.709**	2.919	-0.251**
	(3.333)	(3.291)	(0.123)
Institutional Ownership	0.514***	0.736***	0.456***
	(0.150)	(0.144)	(0.132)
Ln Asset	0.253***	0.247***	0.338***
	(0.047)	(0.046)	(0.070)
Leverage	-0.621***	-0.478***	-0.582***
	(0.200)	(0.141)	(0.204)
Cash	0.294***	0.086	0.094
	(0.105)	(0.055)	(0.126)
Whited-Wu Index	-0.232	-0.831*	1.089
	(0.566)	(0.477)	(0.766)
Peer Institutional Ownership	-0.014	-0.024***	-0.024**
	(0.012)	(0.008)	(0.010)
Peer Ln Asset	0.025***	0.001	-0.007
	(0.008)	(0.002)	(0.008)
Peer Leverage	-0.045***	-0.007	-0.019*
	(0.010)	(0.005)	(0.011)
Peer Cash	0.027***	0.002	-0.005
	(0.008)	(0.003)	(0.007)
Peer Whited-Wu Index	0.465***	0.021	-0.090
	(0.143)	(0.038)	(0.158)

Table 2.11 continued

Dependent variable:	Ln Total Compensation		
	(1)	(2)	(3)
HHI:	Low	Medium	High
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Adjusted R^2	0.768	0.780	0.687
Observations	242,020	252,240	246,520

This table reports the results from regressing the natural logarithm of total CEO compensation on firm performance (measured by the natural logarithm of annual stock returns including dividends), peer-firm performance, and control variables. In columns (1) to (3), the full sample is divided into three subsamples based on the level of competitiveness measured by the Herfindahl-Hirschman Index (HHI). Variable definitions are provided in Appendix A. The standard errors clustered by firm are reported in parentheses below each coefficient estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

co-owned peer firms are performing well. These results are consistent with the findings in Aggarwal and Samwick (1999): the positive pay sensitivity to the performance of co-owned peers is used to soften product market competition in more competitive industries. On the other hand, considering the litigation risk of collusion/coordination in a concentrated market, it is arguably unnoticeable to carry out such a strategic incentive program in a competitive market where many small firms coexist.

Next, I examine whether firm pairs' current combined market share level has any implications for the implementation of the strategic incentive program. I divide the sample into three subsamples based on a combined market share. Firm-pair-year observations that belong to the first, second, and third terciles of the combined market share are classified as the low, medium, and high group, respectively. From the low group to the high group, the combined market power of the firm-pairs is increasing. Table 2.12 shows the performance benchmarking results where I estimate the same specification as in the baseline test for firm-pair-year observations with different levels of combined market share. The results show that

Table 2.12. Subsample test by product market characteristics: Combined market share

Dependent variable:	Ln Total Compensation		
	(1)	(2)	(3)
Combined market share:	Low	Medium	High
Ln Peer Return × Co-owned (d)	0.034***	0.014	0.010
	(0.013)	(0.014)	(0.016)
Ln Firm Return	0.143***	0.148***	0.173***
	(0.030)	(0.026)	(0.034)
Ln Peer Return	0.002	-0.019*	-0.019*
	(0.010)	(0.011)	(0.011)
Ln Peer Return × Correlation	-0.058*	-0.092**	-0.091**
	(0.035)	(0.041)	(0.036)
Correlation	0.108***	0.099***	0.120***
	(0.037)	(0.029)	(0.023)
Co-owned (d)	-0.004	0.022**	0.027***
	(0.011)	(0.010)	(0.007)
CEO Age	-0.206	-0.008	-0.211
	(0.284)	(0.294)	(0.130)
CEO Tenure	-0.030	0.009	0.047***
	(0.028)	(0.025)	(0.013)
SIC3 HHI	-2.029**	-0.115	-0.131
	(1.011)	(0.468)	(0.139)
Institutional Ownership	0.752***	0.486***	0.496***
	(0.139)	(0.108)	(0.070)
Ln Asset	0.348***	0.273***	0.253***
	(0.051)	(0.047)	(0.031)
Leverage	-0.611***	-0.526***	-0.508***
	(0.175)	(0.126)	(0.119)
Cash	0.086	0.229***	0.044
	(0.080)	(0.087)	(0.093)
Whited-Wu Index	0.147	0.113	-0.568**
	(0.492)	(0.455)	(0.234)
Peer Institutional Ownership	-0.008	-0.006	-0.046***
	(0.011)	(0.011)	(0.014)
Peer Ln Asset	-0.004	0.001	0.015**
	(0.004)	(0.006)	(0.006)
Peer Leverage	0.013	-0.010	-0.046***
	(0.009)	(0.012)	(0.015)
Peer Cash	0.002	0.009	0.016
	(0.006)	(0.009)	(0.015)
Peer Whited-Wu Index	0.051	0.192**	0.357***
	(0.075)	(0.092)	(0.110)

Table 2.12 continued

Dependent variable:	Ln Total Compensation		
	(1)	(2)	(3)
Combined market share:	Low	Medium	High
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Adjusted R^2	0.628	0.668	0.743
Observations	246,830	246,900	246,830

This table reports the results from regressing the natural logarithm of total CEO compensation on firm performance (measured by the natural logarithm of annual stock returns including dividends), peer firm performance, and control variables. In columns (1) to (3), the full sample is divided into three subsamples based on combined market shares of a firm pair. Variable definitions are provided in Appendix A. The standard errors clustered by firm are reported in parentheses below each coefficient estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

the coefficient estimate of the interaction between peer performance and a dummy variable for co-owned firm pair is positive and statistically significant only for the firm-pair-years with low combined market share, indicating that only CEOs of firms with low combined market share are rewarded when co-owned peer firms are performing well. This supports the argument that the strategic incentive program is more evident when firm pairs are in a competitive market and have lower joint market share.

Overall, the results provide strong evidence that common blockholders are strategically intervening compensation setting conditions on the co-owned portfolio firms' product market competition.

2.4.6 Alternative industry definitions

In the main analysis, I use the three-digit SIC industry to construct firm pairs. In this section, I re-estimate the baseline tests in Table 2.2 using alternative industry classifications: the four-digit Standard Industry Classification (SIC) and Hoberg-Phillips Text-based Network Industry Classification (TNIC). Columns (1) to (3) of Table 2.13 replicate the baseline results

Table 2.13. Robustness tests with alternative industry definitions

Dependent variable:	Ln Total Compensation					
Industry definition:	SIC4			TNIC		
Log asset distance between focal- and peer- firms:	(1) ≤ 40%	(2) ≤ 70%	(3) All peers	(4) ≤ 40%	(5) ≤ 70%	(6) All peers
Ln Peer Return × Co-owned (d)	0.046***	0.026**	0.025**	0.040***	0.040***	0.034***
	(0.016)	(0.013)	(0.010)	(0.013)	(0.011)	(0.009)
Ln Firm Return	0.172***	0.170***	0.181***	0.163***	0.162***	0.170***
	(0.024)	(0.024)	(0.021)	(0.017)	(0.017)	(0.017)
Ln Peer Return	-0.034**	-0.025**	-0.022**	-0.034***	-0.035***	-0.030***
	(0.014)	(0.012)	(0.009)	(0.010)	(0.009)	(0.008)
Ln Peer Return × Correlation	-0.155***	-0.149***	-0.123***	-0.152***	-0.144***	-0.124***
	(0.042)	(0.039)	(0.032)	(0.034)	(0.031)	(0.027)
Correlation	0.125***	0.116***	0.124***	0.104***	0.107***	0.115***
	(0.032)	(0.030)	(0.024)	(0.024)	(0.023)	(0.021)
Co-owned (d)	-0.013	-0.008	0.007	0.001	-0.003	0.008
	(0.010)	(0.010)	(0.007)	(0.008)	(0.007)	(0.006)
CEO Age	-0.221	-0.269	-0.160	-0.249	-0.270*	-0.229
	(0.197)	(0.186)	(0.162)	(0.161)	(0.157)	(0.149)
CEO Tenure	0.040**	0.039**	0.038**	0.048***	0.045***	0.044***
	(0.019)	(0.018)	(0.016)	(0.015)	(0.015)	(0.015)
HHI	-0.234	-0.215	-0.040	-0.014	-0.040	-0.080
	(0.195)	(0.183)	(0.144)	(0.081)	(0.077)	(0.072)
Institutional Ownership	0.538***	0.573***	0.559***	0.590***	0.597***	0.620***
	(0.099)	(0.094)	(0.085)	(0.085)	(0.082)	(0.078)
Ln Asset	0.318***	0.311***	0.299***	0.321***	0.313***	0.295***
	(0.037)	(0.034)	(0.030)	(0.029)	(0.027)	(0.026)
Leverage	-0.559***	-0.537***	-0.566***	-0.559***	-0.552***	-0.540***
	(0.114)	(0.109)	(0.101)	(0.093)	(0.089)	(0.089)

Table 2.13 continued

Dependent variable:	Ln Total Compensation					
Industry definition:	SIC4			TNIC		
Log asset distance between focal- and peer- firms:	(1) ≤ 40%	(2) ≤ 70%	(3) All peers	(4) ≤ 40%	(5) ≤ 70%	(6) All peers
Cash	0.159*	0.150	0.154**	0.280***	0.265***	0.236***
	(0.090)	(0.092)	(0.073)	(0.074)	(0.074)	(0.069)
Whited-Wu Index	0.159	0.118	-0.027	0.285	0.245	0.068
	(0.362)	(0.350)	(0.305)	(0.222)	(0.214)	(0.201)
Peer Institutional Ownership	0.001	-0.004	-0.023**	0.012	0.008	-0.008
	(0.020)	(0.016)	(0.009)	(0.013)	(0.011)	(0.007)
Peer Ln Asset	0.011	0.010	0.018***	0.001	0.001	0.008***
	(0.013)	(0.008)	(0.005)	(0.008)	(0.004)	(0.003)
Peer Leverage	-0.041	-0.042**	-0.047***	-0.016	-0.013	-0.027***
	(0.026)	(0.021)	(0.012)	(0.015)	(0.012)	(0.007)
Peer Cash	0.019	0.026	0.031***	0.031**	0.025*	0.030***
	(0.021)	(0.018)	(0.010)	(0.015)	(0.013)	(0.009)
Peer Whited-Wu Index	0.268**	0.205*	0.329***	0.069	0.027	0.157***
	(0.134)	(0.116)	(0.087)	(0.069)	(0.060)	(0.054)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.672	0.670	0.709	0.682	0.683	0.715
Observations	64,924	110,974	387,188	115,148	196,518	693,010

This table shows robustness of the results from Table 2.2 using alternative industry definitions: the four-digit Standard Industry Classification (SIC) in columns (1) to (3) and the Hoberg-Phillips Text-Based Network Industry Classification (TNIC) in columns (4) to (6). In columns (1), (2), (4) and (5), I restrict the samples to peer firms with a certain log asset distance. The distance in assets is calculated as the difference between the log asset of a focal firm and that of peer firms. Columns (3) and (6) present the results for the sample with all peers. Variable definitions are provided in Appendix A. The standard errors clustered by firm are reported in parentheses below each coefficient estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

using the four-digit SIC, and columns (4) to (6) of Table 2.13 replicate the baseline results using the Hoberg-Phillips TNIC. The results are robust to alternative industry classifications.

2.5 Conclusion

In this paper, I examine the effect of institutional common ownership on CEOs' compensation setting. Using a CEO pay model common in the literature, I find evidence that common ownership by institutional investors makes firms more likely to have positive pay sensitivity to co-owned peer performance (i.e., it gives firms more incentive to cooperate in the product market). I also find evidence that CEO pay in a co-owned firm is positively associated with co-owned peer compensation. A decomposition of total compensation reveals that only long-term incentives, such as stocks and options, have a positive sensitivity to the performance of co-owned peers.

To establish the causal effect of cross-ownership on firms' CEO compensation setting, I use a DiD approach that relies on the exogenous variation in common ownership generated by a merger between BlackRock Inc. and Barclays Global Investors. The evidence is consistent with the conjecture that firms linked through institutional common ownership have additional positive sensitivity to the co-owned peer performance.

Further analyses on the effect of institutional common ownership on future product market characteristics show that common ownership helps firm pairs avoid direct competition through product market differentiation, enhance joint market share, and thus can coexist in the same local market. An investigation into cases in which common blockholders are more likely to adopt the positive pay sensitivity to co-owned peer performance for managers shows that the positive peer pay-performance sensitivity is more likely to occur in more competitive industries and among firm pairs with lower joint market share. Overall, this study offers evidence that institutional common ownership uses CEO compensation contracts to mitigate competition and increase joint performance among rival portfolio firms.

CHAPTER 3

ESSAY 2: GAMING DISCLOSURE THRESHOLD BY FINANCIAL INTERMEDIARIES: EVIDENCE FROM REGULATION AB

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3.1 Introduction

Information disclosure in the non-agency mortgage-backed securities (MBS) market remains an under-explored area in the aftermath of the 2007 subprime mortgage crisis. Securitized residential mortgages accounted for a large fraction of the new issuance of securitized loans in the period leading up to the financial crisis.[1] Relative to corporate securities, asset-backed securities have minimal business or management that must be described, making truthful disclosure about asset pool quality of utmost importance to investors. Misreporting and misrepresentation differ from typical asymmetric information problems since investors may find it difficult to diversify such risk and thus withdraw from the market altogether. To better understand the causes of the 2007 financial crisis and guide future regulations and oversight, empirical evidence on the extent to which financial intermediaries failed to disclose material information in the asset-backed securities market is critical.

In contrast with agency markets where government-sponsored enterprises (GSEs) usually provide credit enhancements like guarantees on potential loan default, investors in non-agency markets are exposed to the risk of borrowers defaulting on their mortgages. As such, misrepresentation about the quality of the loan pool can cause investors large, unexpected losses. Moreover, financial intermediaries of non-agency securities have an incentive to distort or omit material information in order to facilitate sales. This is exacerbated by the insatiable demand from global investors seeking higher yields that are thus willing to purchase the risky asset-backed securities despite the information asymmetry. Unsurprisingly, there has been a growing list of SEC settlement cases against large financial intermediaries involved in the supply of non-agency MBS in the aftermath of the financial crisis.

[1] According to former International Monetary Fund chief economist Simon Johnson, the total volume of private mortgage-backed securities (excluding those issued by Ginnie Mae, Fannie Mae, and Freddie Mac) grew from $11 billion in 1984 to over $200 billion in 1994 to close to $3 trillion in 2007 (Johnson and Kwak, 2010).

Misrepresentation can take place at any point in the entire supply chain of MBS issuance, including by borrowers, lenders, and/or financial intermediaries underwriting mortgage-backed securities.[2] Each of these participants in the supply chain requires different kinds of market design and regulatory oversight. While Piskorski, Seru, and Witkin (2015) and Griffin and Maturana (2016) find strong evidence of borrowers and lenders misreporting the occupancy status of borrowers, the existence of second liens, and the appraisal values of the properties, their evidence is inconclusive on the role played by MBS underwriters. For instance, Piskorski, Seru, and Witkin (2015) find that the propensity to misrepresent by intermediaries involved in the sale of mortgages seems to be largely unrelated to measures of incentives for top management or the quality of risk management inside these firms. Additionally, Griffin and Maturana (2016) find that including the underwriter fixed effect can explain considerable variation in misreporting. In reality, any difference in misrepresentation among the underwriters may also be caused by factors other than an intent to deceive investors, such as different production technologies, time periods, or simply lack of due diligence in scrutinizing lenders. In other words, like investors, underwriters may also be kept in the dark by other parties in the MBS supply chain. In this paper, however, we take on the task of demonstrating that MBS underwriters intentionally misrepresented the quality of asset pools to investors. We accomplish this by examining changes in the behavior of MBS underwriters in response to a modification in disclosure regulation in the ABS market and its implications for asset pool quality.

To address the dearth of regulation explicitly targeting the distinguishing features of the asset-backed securities market, the U.S. Securities and Exchange Commission enacted Regulation AB (Reg AB) in January 2006. Reg AB stipulates different disclosure requirements

[2]The lender in the MBS supply chain is often referred to as the originator, who sells her loan portfolio to the sponsor. The sponsor works with the underwriter to form and sell the securities to investors. In practice, the issuer is usually an entity set up by the underwriter (although occasionally the sponsor does this). In this paper, we use the term "underwriters" in a general sense and its usage refers to financial intermediaries who sponsor and underwrite mortgage deals.

based on the percentage of loans included in a mortgage deal derived from each originator. While Reg AB generally requires disclosure on a principle-based system, it also contains specific, detailed disclosure requirements for certain items. In particular, Reg AB Item 1110 requires the disclosure of information regarding the size and composition of the originator's portfolio as well as information material to an analysis of the performance of the asset pool (such as the originator's credit-granting or underwriting criteria for the asset types being securitized, the total amount of delinquent assets as a percentage of the aggregate asset pool), if the originator or group of affiliated originators originated or is expected to originate 20% or more of the pool assets. From the underwriters' perspective, if they have information that a particular loan portfolio from a certain originator suffers from lax credit-granting criteria, they would have an incentive to keep the share of that portfolio below the 20% threshold in order to avoid disclosing the adverse information associated with that loan portfolio. This also makes discovery of wrongdoing more difficult in the future, thus reducing expected litigation risk for underwriters. Conversely, if the underwriters are truthful or unaware of the poor quality of a particular loan portfolio, we would not expect to see a discontinuity in loan quality around the disclosure threshold. Therefore, the introduction of the disclosure rule under Reg AB functions as a quasi-natural experiment setting for our investigation as we compare the percentage of shares pertaining to each originator in a given asset pool around the disclosure threshold and the corresponding loan quality before and after Reg AB's passage.

We collect publicly available information from mortgage deal prospectus supplements of privately securitized residential mortgages that took place between 2003 and 2007. For each mortgage deal, its prospectus supplement offers information on various performance-related characteristics, including FICO score, loan-to-value ratio, and the collateral's pool size. For our analysis in particular, it provides information on the composition of the mortgage loans from different originators. Using the First American CoreLogic LoanPerformance database,

we are also able to link individual loans to particular originators for a large portion of the examined deals. The CoreLogic data provide the name of the original lender for each loan while we hand-collect identity and affiliation information for the original lender of each loan to determine whether the original lender is affiliated with or is an originator for the mortgage deal. Using this information, we assign individual loans to the originators listed in the prospectus supplements. In this paper, we perform both deal-level analysis and loan-level analysis. The latter allows us to compare loan quality within deals and offers additional evidence of underwriter misrepresentation. For deal-level analysis, we examine the cumulative net loss, defined as the sum of all the losses suffered by the deal's principal up to a specific date divided by the total original balance of all of the mortgages. For loan-level analysis, we use the standard measure of 60 days or more delinquency within 24 months of the loan's origination.

Our main findings regarding the underwriting financial intermediaries' responses to the disclosure rule change under Reg AB are as follows: first, we find that the proportion of deals containing at least one origination stake below disclosure threshold (we refer to an origination stake below disclosure threshold as a "BDT stake" and a deal with one or more BDT stakes as a "BDT deal" hereafter) increases significantly after the implementation of Reg AB.[3] In fact, the proportion of BDT deals more than doubled after the passage of Reg AB. Second, BDT deals suffer significantly larger cumulative net losses than non-BDT deals (deals where none of the originators' stakes are below Reg AB's disclosure threshold). We show that such losses only occur when the deals are issued after Reg AB. Third, we track down originators with increased BDT stake occurrence after Reg AB and find that BDT deals with these particular originators have larger cumulative net losses. To the extent that increased BDT stake occurrence is associated with more involvement in gaming the disclosure

[3]We classify the deals issued before and after Reg AB the same way even though the threshold only matters after Reg AB.

threshold, this result suggests that the difference in losses between BDT deals and non-BDT deals surrounding Reg AB is the result of misrepresenting loan quality rather than due to probable latent variables that change from the pre-Reg AB period to the post-Reg AB period. An important policy implication for this finding is that it provides a practical approach to detect serious offenders of misrepresentation among financial intermediaries underwriting asset-backed securities.

Our findings are robust to the deal's cumulative net losses measured at different dates and to the inclusion of various controls on deal characteristics, issuing semester (a half-year) fixed effects and underwriter fixed effects. The latter takes into account the differences across underwriters, lending greater support to our postulation of active misrepresentation, because underwriters use BDT stakes when the loan quality from certain originators is poor, and such usage is independent of an underwriter's characteristics. This is consistent with Piskorski, Seru, and Witkin (2015), which documents how common measures of incentives for the management of intermediaries involved in mortgage sales are unrelated to the difference in the propensity to misrepresent across these intermediaries. Our results are also robust to the sample selection issue because the originators in our analysis appear in both BDT deals and non-BDT deals before and after Reg AB. Lastly, we take advantage of our loan-level data to compare loans *within* deals and demonstrate that securitized loans have higher delinquency when their originators have increased BDT stake occurrence after Reg AB. This effect is particularly strong for loans in BDT stakes and only so after Reg AB. Overall, our findings suggest that underwriters are aware of the quality differences among originators' loan portfolios and knowingly place them in BDT and non-BDT stakes accordingly.

By examining the disclosure rule change under Reg AB, we provide empirical evidence of misrepresentation by financial intermediaries underwriting mortgage-backed securities. As previously suggested, we believe these findings are critical, given the key role played by financial intermediaries in the supply chain of asset-backed securities and investors' reliance

on the information provided by these underwriters. Our paper contributes to several strands of research. First, by producing evidence of misrepresentation by the financial intermediaries underwriting non-agency mortgage-backed securities, we advance important ongoing research on misrepresentation taking place in the supply chain of non-agency MBS and ABS markets (see Piskorski, Seru, and Witkin (2015), Griffin and Maturana (2016)).[4] Misrepresentation at the underwriter level is arguably more damaging to this market because underwriters not only collect and verify information regarding the quality of the underlying collaterals but also are generally large, reputable financial intermediaries who are more sophisticated than the typical investor in this market. Our empirical strategy of scrutinizing the financial intermediary's reaction to a regulatory rule change is similar to the empirical strategy employed by Qian, Strahan, and Yang (2015), who study the impact of incentives and communication costs on information production by examining reforms of authority delegation in Chinese banks in response to a change in loan officer incentives to produce information.

Second, our paper contributes to the growing literature on the effects of regulation in the ABS/MBS market (see Keys, Mukherjee, Seru, and Vig (2009), Keys, Piskorski, Seru, and Vig (2013), Agarwal, Lucca, Seru, and Trebbi (2014), among others). Our analysis equips regulators with a valuable tool for investigating possible gaming of the threshold by underwriters. For example, regulators could examine the distribution of originator stakes around the disclosure threshold from time to time in order to detect any abrupt changes and/or low asset quality in BDT stakes. Third, our paper augments the literature on the economic consequences of financial reporting and disclosure regulation.[5] In particular, our

[4]Our paper is also related to a large literature on corporate fraud (see, for example, Burns and Kedia (2006), Kedia and Philippon (2009), Efendi, Srivastava, and Swanson (2007), Dyck, Morse, and Zingales (2010), and Kedia and Rajgopal (2011)).

[5]Many recent papers on disclosure regulation mainly focus on the impact of regulation changes under Regulation Fair Disclosure (Reg FD) and the Sarbanes-Oxley Act (SOX). Leuz and Wysocki (2016) provide a comprehensive review of the related studies; Granja (2013), among others, examines the effects of disclosure regulation in the commercial banking industry. There are also studies examining disclosure regulation on OTC bulletin board firms (Bushee and Leuz, 2005) and the JOBS Act (Chaplinsky, Hanley, and Moon, 2017).

study relates to the papers that explore the unintended consequences of regulation changes, such as the "going dark activities" after SOX.[6] Our findings shed light on the effects of mandatory disclosure on financial institutions and its implications for the quality of assets securitized by these financial institutions. Our evidence advances this literature in a new and important direction by bolstering our understanding of firms' reactions to disclosure regulation as well as developing greater insight into firms' avoidance strategies and the kinds of cost-benefit analyses that firms conduct when deciding whether or not to comply with disclosure regulation. Consequently, we also emphasize the serious considerations policy-makers must evaluate prior to passing regulatory rules (Leuz and Wysocki, 2016). Fourth, this paper advances the fast growing literature that explores the relation between mortgage securitization and subprime loan quality and is the first study to directly assess the impact of the Reg AB disclosure mandate on the MBS market.[7]

The rest of the paper is organized as follows. Section 3.2 describes information disclosure under Reg AB. Section 3.3 characterizes the data and provides summary statistics. In Section 3.4, we present and discuss our empirical findings at the deal level. In Section 3.5, we provide findings on our loan-level analysis. Section 3.6 concludes the paper.

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[6]For example, Gao, Wu, and Zimmerman (2009) provide evidence on the unintended consequences of Sarbanes-Oxley Act exemptions for small companies (i.e., firms with a public float of less than $75 million). They find that size-based exemptions provide incentives for firms to stay small by curbing growth in order to avoid crossing the compliance threshold. Leuz (2007) and Leuz, Triantis, and Wang (2008) show that "going dark" is associated with SOX-related events.

[7]For studies on various issues related to mortgage loans and mortgage-backed securities, see, e.g., Mian and Sufi (2009), Keys, Mukherjee, Seru, and Vig (2009), Loutskina and Strahan (2009), Keys, Mukherjee, Seru, and Vig (2010), Loutskina and Strahan (2011), Purnanandam (2011), He, Qian, and Strahan (2012), Ben-David (2011), Keys, Seru, and Vig (2012), Demiroglu and James (2012), Nadauld and Sherlund (2013), Demyanyk and Loutskina (2016), Stanton, Walden, and Wallace (2014), Piskorski, Seru, and Witkin (2015), Griffin and Maturana (2016), Loutskina and Strahan (2015), Garmaise (2015), Rajan, Seru, and Vig (2015), He, Qian, and Strahan (2015), among others.

3.2 The disclosure rule under Reg AB

The Securities and Exchange Commission defines asset-backed securities (ABS) as securities that are backed by a discrete pool of self-liquidating financial assets. The ABS market has experienced rapid growth in the last two decades.[8] In a basic securitization structure, a financial institution known as "sponsor" constructs a pool of financial assets, such as mortgage loans, that are either self-originated or acquired directly (or indirectly) through an affiliate. Securities that are backed by a pool of financial assets are then sold to investors by financial intermediaries (e.g., investment banks) known as "underwriters." Payment on the ABS depends primarily on the cash flows generated by the assets in the underlying pool and "credit enhancements," which are the other rights designed to ensure timely payment.

Asset-backed securities differ from corporate securities and operating companies in that "there is generally no business or management to describe in offering these securities. Instead, information about the transaction structure and the quality of the asset pool and servicing is often what is most important to investors."[9] According to the SEC, prior to Reg AB, many of its existing disclosure and reporting requirements (which were designed primarily for corporate issuers) did not elicit the information relevant for most ABS transactions. Regulation AB, which became effective in January 2006, thus represents a comprehensive treatment of ABS under the Securities Act of 1933 and the Securities Exchange Act of 1934. It consolidates and codifies the SEC's positions and industry practice that the SEC has done through no-action letters and the filing review process over time.

The new rules on disclosure under Reg AB represent the most dramatic changes in the ABS markets. Prior to Reg AB, there was no disclosure regulation specifically tailored to

[8]Bank One Capital Markets estimates that the annual issuance of US public non-agency ABS grew from $46.8 billion in 1990 to $416 billion in 2003. See Bank One Capital Markets, Inc., 2004 Structured Debt Yearbook. Thomson Media estimates that the new issuance for 2003 was at $800 billion. See Asset Securitization Report (published by Thomson Media Inc).

[9]See Securities and Exchange Commission Asset-Backed Securities Proposed Rule Release Nos. 33-8419; 34-49644.

ABS. In addition to eliminating boilerplate language and de-emphasizing unnecessary legal recitations about terminology, Reg AB requires financial intermediaries to disclose information material to an ABS transaction (such as the background, experience, performance, and roles of various transaction parties). Reg AB generally requires disclosure on a principal-based system regarding all material risk factors applicable to the transaction as a whole or to the nature of the security and also includes specific and detailed disclosure requirements for certain items (Walworth, Novomisle, and Wetzler, 2010). Specifically, Reg AB Item 1110 establishes progressive disclosure requirements based on the origination percentage of the pool assets by each originator. At the initial level of disclosure, the identification of any originator or group of affiliated originators is required if it originates, or expects to originate, 10% or more of the pool's assets. Furthermore, if the originator originates (or expects to originate) 20% or more of the pool's assets, the regulation requires "disclosure of information regarding the size and composition of the originator's origination portfolio as well as information material to an analysis of the performance of the pool assets, such as the originator's credit-granting or underwriting criteria for the asset types being securitized." Thus, a 20% or more stake of the pool's assets represents an important disclosure threshold that did not exist prior to Reg AB.

Unsurprisingly, the 20% threshold was a key point of contention during the commenting period of Reg AB. In the final ruling on Reg AB, the SEC stated that the initial proposed breakpoint for disclosure would be 10%. However, several commentators successfully argued for a higher disclosure threshold to lessen the burdens associated with disclosure, resulting in the SEC's adoption of a 20% disclosure threshold in the final rule of Reg AB.[10] In retrospect, it is interesting to note that, in our sample, most of the originators with BDT stakes in some deals also appear in non-BDT deals. Therefore, since these same originators simultaneously provide disclosure on larger loan portfolios, it is uncertain whether they hold their BDT

[10]We refer readers to the final ruling for more details: http://www.sec.gov/rules/final/33-8518.pdf.

stakes below the threshold as a result of the reduction in regulatory burden or not. Apparently, the disclosure mandate in Reg AB subjects MBS underwriters to more scrutiny and higher litigation risk in disclosing information on the originators and their loan portfolios. As a result, the underwriters may choose to place poor-quality loans in BDT stakes to avoid disclosing, thus constituting misrepresentation to investors under the disclosure regulations regarding materiality of information.

We aim to investigate whether MBS underwriters intentionally misrepresent the asset quality of securitized mortgage pools. This is accomplished by demonstrating that underwriters game the disclosure rule change under Reg AB by deliberately placing poor-quality loans in BDT deals. These underwriters must have known the undisclosed poor quality of loan portfolios from certain originators in order to take advantage of the disclosure threshold. Thus, in our empirical analysis, we first examine whether there is a jump in the proportion of BDT deals responding to the disclosure rule change. For example, we expect that before Reg AB, 18% or 22% origination stakes would occur randomly with respect to the relative size of a loan portfolio to the entire asset pool. After Reg AB, 18% and 22% origination percentages have become critical as they face very different disclosure requirements. Because of the consequences associated with having origination stakes just above 20%, the quantity of deals with origination stakes just below the disclosure threshold increases relative to those with stakes just above 20%. We then examine whether loans in these stakes suffer larger losses than other loans (controlling for all the disclosed characteristics), which serves as an indication of undisclosed quality problems. Note that, while underwriters attempting to reduce the burden of disclosure compliance can also increase the use of BDT deals, it does not necessarily subject BDT deals to greater undisclosed quality problems. Therefore, both the changes in the proportion and the quality of BDT deals in response to the rule change under Reg AB are critical for us to conclude that underwriters knowingly misrepresent asset quality. From the above discussion, we form the following testable hypotheses, which constitute the focus of our empirical analyses.

Hypothesis 1: All else equal, the proportion of BDT deals increases after Reg AB.

Hypothesis 2: All else equal, the loans in BDT stakes suffer larger losses and only do so in deals issued after Reg AB.

3.3 Data description and summary statistics

Our data come primarily from two sources: SEC EDGAR filings and First American CoreLogic LoanPerformance. We collect information on deal characteristics, the mortgage originators, and the underwriters from the deal prospectus supplements filed with EDGAR.[11] Our sample consists of publicly issued non-agency mortgage deals that are issued between 2003 and 2007, the period immediately preceding the financial crisis. Each deal in our database has detailed information on its characteristics at issuance. In the meantime, our loan-level data consist of information on securitized mortgages constructed by CoreLogic LoanPerformance. CoreLogic provides information on loan origination dates, the mortgage loan pools, the identities of the securitizers, the MBS where the loans are placed, the borrowers, and the characteristics of the loans. We also construct variables that account for regional housing and economic conditions because changes in house prices and the macroeconomic environment may have affected the mortgage performance. For deal-level controls, we use the house price index for the corresponding state reported by the Federal Housing Finance Agency (FHFA) and compute the weighted average change based on the geographic composition for each deal.

Deal prospectus supplements disclose the identities of the originators and the percentage of dollar principal that each originates for the deal. Origination information is not available for every deal, so this investigation focuses on a sample of 2,248 deals for which such infor-

[11] We use publicly issued non-agency mortgage deals because of data availability.

mation is available.[12] From the detailed origination information, we identify deals that have origination stakes in 10%-20% or below 20% of the asset pool from an originator and its affiliates. Considering the disclosure requirements of Reg AB, we use 10%-20% as the main measure of a BDT stake and use below 20% as an alternative measure.[13] We define a deal with one or more BDT stakes as a BDT deal. We also calculate the sum of BDT stakes for each deal as an alternative to the dummy variable for BDT deal.

Our deal-level asset quality measure is the cumulative net loss rate measured as the sum of all the losses of the principal suffered up to September 2014 divided by the total original balance of all the mortgages. As a robustness check, we also use the cumulative net loss rate measured as the sum of all the losses of the principal suffered up to December 2012. We use the deal characteristics as control variables, including original deal collateral balance, an indicator for high underwriter reputation following Griffin, Lowery, and Saretto (2014), the number of tranches, an indicator for higher-than-mean share of loans that have limited or no documentation in the collateral, weighted average FICO score, weighted average loan-to-value (LTV) ratio, percentage of adjustable rate mortgages in the deal, an indicator for the presence of negative amortization, percentage of purchase loans (as opposed to refinancing), percentage of loans for single family houses, percentage of loans for owner-occupied houses, percentage of loans for equity take out, percentage of loans for refinance, and percentage of second lien loans.

For the loan-level analysis, we first identify the link between each securitized loan and its originator in a deal with multiple originators. The CoreLogic database provides the name of the original lender for each loan, who could be either a direct lender or a mortgage broker.

[12]One potential concern is that the deals with missing originator information may have lower-quality loan pools. Consequently, missing this subsample in our analysis may bias our estimates. However, we find that the deals with missing originator information have similar losses as the deals with non-missing originator information. This fact alleviates the concern of sample selection issue in our final sample.

[13]Under Reg AB, originators contributing less than 10% to the collateral pool do not have to reveal their identities. This explicitly precludes using below 10% as a separate threshold in this analysis.

We collect identity and affiliation information for the original lender of each loan to determine whether the original lender is one of the mortgage deal's originators or is affiliated with one of the deal originators. When such a link can be made, we assign individual loans to the originators listed in the prospectus supplements. When the original lenders cannot be linked to any of the originators as is often the case with the loans acquired by the originators, we set the originator's information for these loans as missing and exclude them from our loan-level analysis. We then merge the deal-level originator variables with the loan-level data by an originator-deal pair. The definitions for all the variables at both the deal and the loan level are described in Appendix B.

We begin our investigation with the deal-level analysis. Table 3.1 reports the summary statistics for the deal-level variables. For our full sample, the average deal cumulative net loss is 13.1% with a standard deviation of 12.4%. Deals with 10%-20% (less than 20%) stakes from an originator and its affiliates are 18% (23%) of the sample. For the full sample, BDT stakes account for 4.8% (5.7% for less than 20% stakes) of pool assets with a standard deviation of 13% (14% for less than 20% stakes). The highest percentage of aggregate BDT stakes is 100% (in other words, a deal could consist entirely of BDT stakes in an extreme case). For deals with 10%-20% (less than 20%) BDT stakes, the percent of BDT stakes are on average 25.8% (24.5%) of the pool assets.

Table 3.2 reports the correlation coefficients on the main variables of interest at the deal level. The cumulative net loss is significantly and positively correlated with the existence of BDT stakes and the aggregate percentage of BDT stake loans in these deals. The results are very similar for both measures of BDT stakes (loan stakes within 10%-20% or below 20%). Consistent with the findings in the literature, the deal's cumulative net loss is negatively correlated with the average FICO score, which suggests that high credit worthiness of a borrower is associated with lower defaults. However, the deal's cumulative net loss is positively correlated with the average loan-to-value ratio, percentage of adjustable

Table 3.1. Summary statistics

Variable	Mean	St. Dev.	P5	P25	P50	P75	P95
Cumulative net loss	13.12	12.36	0.1	2.5	8.49	22.78	36.93
BDT Deal 10%-20%(d)	0.18	-	-	-	-	-	-
BDT Deal < 20%(d)	0.23	-	-	-	-	-	-
Total % of 10%-20%	4.78	12.87	0	0	0	0	30.5
Total % of <20%	5.65	14.05	0	0	0	0	34.11
Original collateral balance ($B)	0.82	0.52	0.24	0.43	0.71	1.02	1.87
No. of tranches	20.3	10.47	10	15	18	22	38
FICO	692.25	48.85	609	639	710	734	746
LTV	73.97	5.47	65	71	74.18	77	82
Adjustable rate mortgage (%)	60.33	38.96	0	0	71.2	100	100
Purchase loans (%)	44.05	14.23	19.01	36.3	43.35	53.32	68.38
Single family (%)	68.56	11.59	54.68	62.96	68.39	73.8	88.85
Owner occupied (%)	87.74	8.73	71.36	85.66	88.32	93.66	96.98
Equity take out (%)	36.02	14.81	13.32	26.65	35.26	44.04	63.46
Refinance (%)	18.92	13.33	3.01	10.37	18.97	21.41	48.47
Second lien (%)	0.62	1.75	0	0	0	0	4.61
House prices change	-8.3	10.95	-21.01	-15.77	-11.95	-3.06	16.86
House prices run-up	7.47	5.32	-2.15	2.73	9.06	11.62	14.11
Credit spread	0.88	0.11	0.68	0.82	0.9	0.92	1.11
10 Year Treasury	4.5	0.35	3.98	4.22	4.54	4.72	5.1
High reputation (d)	0.78	-	-	-	-	-	-
Low documentation (d)	0.47	-	-	-	-	-	-
Negative amortization (d)	0.08	-	-	-	-	-	-

This table presents the summary statistics on the deal and macro variables defined in Appendix B. The statistics reported are the Mean, St. Dev. (standard deviation), and k^{th} percentile (Pk for $k = 5, 25, 50, 75, 95$) of each variable. We use (d) to denote that the variable is a dummy variable. We also use (%) if the variable is in percentage. We report only the mean for dummy variables.

rate mortgages, presence of negative amortization loans, percentage of purchase loans, and percentage of loans with a second lien due to the higher default risk associated with these characteristics. Our correlation estimate also suggests that the deal's cumulative net loss is negatively correlated with the percentage of single family home loans.

Table 3.2. Correlation matrix

	Cum net loss	BDT Deal 10%-20%(d)	BDT Deal < 20%(d)	Total % of 10%-20%	Total % of <20%
Cumulative net loss	1.00				
BDT Deal 10%-20%(d)	0.14***	1.00			
BDT Deal < 20%(d)	0.14***	0.87***	1.00		
Total % of 10%-20%	0.10***	0.79***	0.69***	1.00	
Total % of <20%	0.10***	0.78***	0.75***	0.95***	1.00
Original collateral balance ($B)	0.17***	-0.06***	-0.05**	-0.06***	-0.05**
High reputation (d)	-0.03	-0.06***	-0.03	-0.05**	-0.04*
No. of tranches	-0.06***	-0.00	0.01	-0.01	-0.00
Low documentation (d)	0.03	0.02	0.03	0.01	-0.00
FICO	-0.55***	-0.03	-0.02	-0.02	-0.01
LTV	0.56***	0.04	0.04*	0.02	0.01
Adjustable rate mortgage (%)	0.26***	-0.02	-0.01	0.01	0.02
Negative amortization (d)	0.11***	-0.04*	-0.04*	-0.02	-0.03
Purchase loans (%)	0.00	0.00	-0.02	-0.00	-0.01
Single family (%)	-0.00	-0.08***	-0.11***	-0.06***	-0.07***
Owner occupied (%)	0.04*	-0.06***	-0.07***	-0.03	-0.03
Equity take out (%)	0.36***	0.06***	0.07***	0.04**	0.04*
Refinance (%)	-0.39***	-0.06***	-0.05**	-0.04**	-0.04
Second lien (%)	0.50***	-0.01	-0.03	-0.02	-0.02

This table presents the correlation coefficients between the main variables of interest and the other explanatory variables. Variable definitions are provided in Appendix B. Statistical significance levels of 1%, 5%, and 10% are indicated with ***, **, and *, respectively.

3.4 BDT stakes and loss of BDT deals after Reg AB

We start our empirical analysis by examining the change in BDT stake occurrence after the disclosure rule goes into effect under Reg AB. We then focus on investigating the implications of the change in BDT stake occurrence for the quality of the securitized mortgages at the deal level.

3.4.1 BDT stake occurrence after Reg AB

We first count the origination percentages within 10%-20% or below 20% of the pool assets from an originator and its affiliates. As previously mentioned, there is a sample selection issue since the disclosure on origination percentages below 10% is voluntary. Furthermore, we try to capture the underwriter's intention of capping the origination percentages from certain originators to below the disclosure threshold (for instance, from over 20% to right below), thus making 10%-20% a cleaner measure. We use both in our analysis as a robustness check and focus on 10%-20% when interpreting the results.

In Figure 3.1, we plot the number and the fraction of the deals with origination percentages below the disclosure threshold in our sample period. The top panels show the plots for deals with 10%-20% origination percentages before and after Reg AB. Both the number and the fraction of deals show similar patterns surrounding Reg AB. Specifically, the number of deals with origination percentages below the disclosure threshold shows a sharp increase from 121 before Reg AB to 303 after Reg AB, or an increase from 11% of all deals before Reg AB to 27% after Reg AB. Moreover, the bottom panels show that the fraction of deals with BDT stakes is relatively stable before Reg AB and that the sharp jump occurs right after Reg AB becomes effective and then remains high.

To demonstrate that the discontinuity in stake size change surrounding Reg AB indeed occurs at the disclosure threshold, we estimate the difference in the stake size density post- and pre-Reg AB. Figure 3.2 plots the estimation result. We observe a sharp increase in the

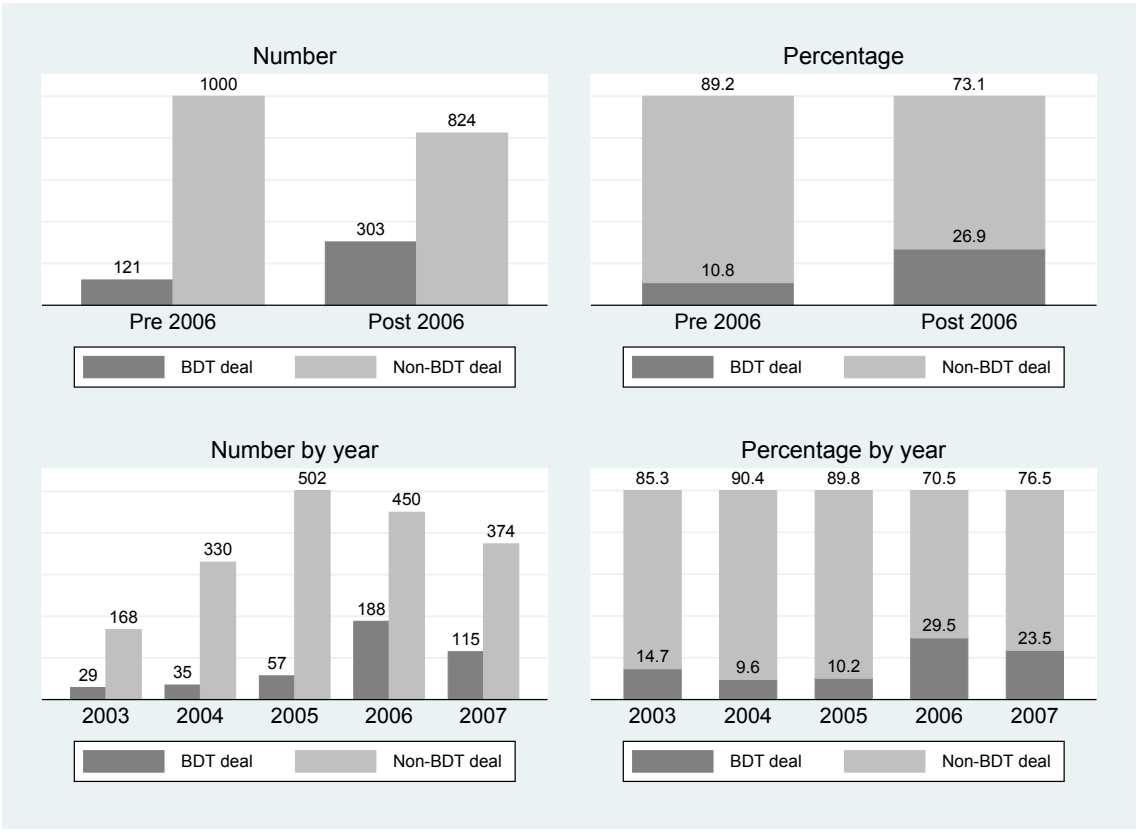

Figure 3.1. The use of BDT stakes before and after Reg AB

The bar plots in this figure represent the difference between the number (and percentage) of deals with originators in the [10,20)% (BDT Deals) and the number (and percentage) of deals without originators in this range (Non-BDT Deals). The top panel compares the corresponding measures before Reg AB (pre-2006) with after Reg AB (post-2006). The bottom panel plots these measures on an annual basis from 2003 to 2007.

stake density post-Reg AB relative to pre-Reg AB as the stake size approaches the disclosure threshold 20%. At the same time, we also observe a drop in the stake density as the stake size just exceeds the disclosure threshold. As the stake size increases further and moves away from the threshold, the stake density gradually recovers and flattens out. The change in stake density post- and pre-Reg AB clearly shows that the discontinuity occurs right at the disclosure threshold and lends support to the observation revealed in Figure 3.1.

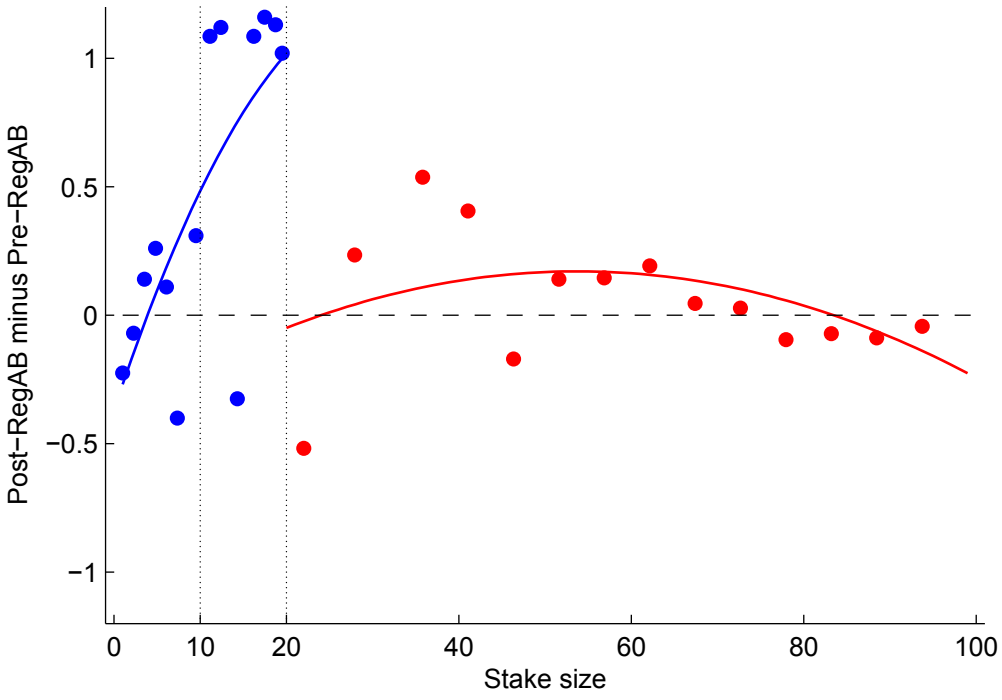

Figure 3.2. The change of stake size probability density from before to after Reg AB

This figure plots the probability density differences from before Reg AB to after Reg AB. We estimate the density function separately for the stake sizes below 20% and above 20% using quadratic functions.

The increase in the BDT stake occurrence is statistically significant. We apply probit regressions to evaluate this change by controlling for other factors that may affect such occurrence using the following specification:

$$\text{BDT Deal} = f(\beta \times \text{Post Reg AB} + \text{Deal and Macro controls} + \text{Fixed effects}), \qquad (3.1)$$

where BDT Deal is a dummy variable that represents the presence of one or more BDT stakes in a deal.

Table 3.3 reports the marginal effects from the probit regression estimates. Column (1) shows that there is an 18% increase in the faction of BDT deals after Reg AB. We find a similar result in column (2) when we use the alternative below 20% BDT stake measure.

Table 3.3. Determinants of the use of BDT stakes

	BDT Deal 10%-20%(d)	BDT Deal < 20%(d)
Post Reg AB	0.179***	0.193***
	(0.033)	(0.037)
Original collateral balance	-0.055***	-0.051**
	(0.019)	(0.021)
High reputation (d)	-0.017	0.016
	(0.037)	(0.041)
No. of tranches	-0.000	0.000
	(0.001)	(0.001)
Low documentation (d)	-0.030	-0.039*
	(0.020)	(0.022)
FICO	-0.000	-0.000
	(0.000)	(0.000)
LTV	-0.000	0.000
	(0.002)	(0.002)
Adjustable rate mortgage (%)	-0.000	-0.000
	(0.000)	(0.000)
Negative amortization (d)	-0.138***	-0.175***
	(0.038)	(0.042)
Purchase loans (%)	0.002	0.004
	(0.002)	(0.003)
Single family (%)	-0.002***	-0.004***
	(0.001)	(0.001)
Owner occupied (%)	-0.002	-0.002*
	(0.001)	(0.001)
Equity take out (%)	0.003	0.006**
	(0.002)	(0.003)
Refinance (%)	0.003	0.005*
	(0.002)	(0.003)
Second lien (%)	-0.013***	-0.016***
	(0.005)	(0.005)
House prices run-up	0.005**	0.001
	(0.002)	(0.003)
Credit spread	0.137	0.056
	(0.088)	(0.096)
10 Year Treasury	0.009	-0.004
	(0.037)	(0.041)
Lead-underwriter FE	Yes	Yes
Pseudo R^2	0.124	0.123
Observations	2,248	2,248

Table 3.3 continued

This table presents marginal effects from the probit regressions analyzing the determinants of the use of BDT stakes. Variable definitions are provided in Appendix B. The BDT Deal 10%-20% (d) and BDT Deal <20% (d) are regressed on other explanatory variables using probit regressions. The standard errors clustered by issue semester are reported in parentheses below each coefficient estimate. Statistical significance levels of 1%, 5%, and 10% are indicated with ***, **, and *, respectively.

In theory, MBS underwriters can have various reasons for pooling loans from different originators and issuing securities backed by the collateral. DeMarzo (2005) suggests the existence of a tradeoff between the reduction of asymmetric information problems with a diversified pool of loans from different lenders and the destruction of an underwriter's superior information on a particular portfolio during pooling. Gaur, Seshadri, and Subrahmanyam (2011) point out that securitization reduces market incompleteness by providing investors with the particular cash flow distribution that they value. Coval, Jurek, and Stafford (2009) suggest the potential for deal arrangers to deliver the cheapest possible set of assets obtainable with a high quality credit rating in order to cater to investors who rely solely on ratings. All these motives can potentially affect the composition of the pool's assets. Our sample consists of loan pools of various compositions, such as those with one originator making up the majority, those with two or three originators making up the majority, or loan pools with a mix of smaller origination shares. In this paper, we do not try to explore all the determinants for the originator composition of a pool, but rather examine whether the disclosure rule change affects a particularly relevant aspect of the composition. By focusing on a small neighborhood around the disclosure threshold where the effect of other determinants is likely continuous, we are able to minimize the impact of the potential omitted variables. This is similar in spirit to Keys, Mukherjee, Seru, and Vig (2009) which focus on a close neighborhood around a FICO score of 620 for loan securitization to assess laxing in the lending standard.

Next, we formally test whether or not there is a jump in the BDT stake occurrence surrounding Reg AB. The intuition is that the disclosure threshold creates a discontinuity around the 20% cutoff value while the effect of other determinants on the origination percentage is more likely to be continuous. In other words, while other determinants may affect the stake sizes within [18,20)% and [20,22)% more or less equally, the disclosure threshold would drastically increase the origination stakes within [18,20)% relative to [20,22)% surrounding Reg AB. In our analysis, we examine the difference between [10,20)% and [20,30)%, [15,20)% and [20,25)%, and [18,20)% and [20,22)%, respectively, and test whether there is a significant change in the origination stakes in the close neighborhood of the disclosure threshold pre- and post-Reg AB. While a narrower bracket is better suited for our purposes, there may not be enough observations within the bracket. We therefore choose the above three bracket sizes for robustness.

For each deal, we create a dummy variable to represent the presence of origination stake size just below 20% and another dummy variable representing the presence of origination stake size just above 20%. The difference between these two dummy variables is denoted as *diffA20B*, where [A,20) is the bracket just below 20% and [20,B) is the bracket just above 20%. This difference captures the relative magnitude around the threshold and is our main variable of interest in the following difference-in-differences (DiD) test. The combinations of {A,B} in our analysis include {10,30}, {15,25}, and {18,22}. We next compare this difference pre- and post-Reg AB. Indeed, we find a dramatic increase in the magnitude of 7.5% for the {10,30} combination, 5.8% for the {15,25} combination, and 2.9% for the {18,22} combination. All these increases are statistically significant at the 1% level.[14]

We also evaluate the increase of this differential in a regression model, controlling for the deal's characteristics, underwriters' reputation, macroeconomic variables, and underwriter

[14]These magnitudes decrease for smaller neighborhoods because a smaller proportion of sample falls into those neighborhoods.

fixed effect. Table 3.4 reports the results for the OLS estimation (panel A) and the ordered probit (panel B) regression analysis. Our OLS estimation shows that the increase of this differential from pre- to post-Reg AB is 15% for the {10,30} combination, 8% for the {15,25} combination, and 4% for the {18,22} combination. Given that the fractions of our sample in the [10,20)%, [15,20)%, and [18,20)% brackets before Reg AB are 10.8%, 5.2%, and 1.9%,

Table 3.4. Difference in origination stakes in brackets below and above 20%

Panel A: OLS regressions						
	diff102030	diff102030	diff152025	diff152025	diff182022	diff182022
	(1)	(2)	(3)	(4)	(5)	(6)
Post Reg AB	0.08***	0.15***	0.05***	0.08***	0.03***	0.04***
	(0.02)	(0.03)	(0.01)	(0.02)	(0.01)	(0.01)
Control variables	No	Yes	No	Yes	No	Yes
Lead-underwriter FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.012	0.018	0.007	0.008	0.007	0.007
Observations	2,248	2,248	2,248	2,248	2,248	2,248

Panel B: Ordered probit regressions						
	diff102030	diff102030	diff152025	diff152025	diff182022	diff182022
	(1)	(2)	(3)	(4)	(5)	(6)
Post Reg AB	0.07***	0.07***	0.06***	0.06***	0.03***	0.03***
	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)
Control variables	No	Yes	No	Yes	No	Yes
Lead-underwriter FE	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.0185	0.0282	0.0139	0.0213	0.0247	0.0384
Observations	2,248	2,248	2,248	2,248	2,248	2,248

This table presents the results of analyzing the difference between the percentage of deals with origination stakes in the bracket just below 20% and the percentage of deals with origination stakes in the bracket just above 20%. For each deal, we create dummy variables to represent the existence of origination stakes in a bracket just below 20% and just above 20%. The difference between these two dummy variables is denoted as *diffA20B*, where [A,20) is the bracket just below 20% and [20,B) is the bracket just above 20%. The combinations of {A,B} in our analysis include {10,30}, {15,25}, and {18,22}. Panel A reports the results of regressing this difference on the *Post Reg AB* dummy variable and other control variables using OLS regressions. Panel B reports the corresponding results using ordered probit regressions. The control variables are the same as in Table 3.3. The standard errors clustered by issue semester are reported in parentheses below each coefficient estimate. Statistical significance levels of 1%, 5%, and 10% are indicated with ***, **, and *, respectively.

respectively, our estimates translate into a relative increase of 136%, 140%, and 190% for the {10,30}, {15,25}, and {18,22} combinations, respectively, post-Reg AB. The ordered probit regression analysis produces qualitatively similar results.

3.4.2 Loss of BDT deals after Reg AB

Now that we have documented a significant increase in the fraction of BDT deals after Reg AB, we examine whether these deals indeed have lower quality than reported, a strong motive for the underwriters to avoid disclosure.

We regress the deal's cumulative net loss on variables that capture the presence of BDT stakes and their interactions with a post-Reg AB dummy variable, controlling for deal characteristics, macroeconomic conditions, and various fixed effects. The inclusion of the interaction term allows us to assess whether the use of BDT stakes has an incremental effect after Reg AB rather than before Reg AB. Specifically, we use the following specification for our regression analysis:

$$\begin{aligned} \text{Cumulative net loss} \;=\; & \alpha + \beta_1 \times \text{Post Reg AB} + \beta_2 \times \text{BDT Deal} \\ & + \beta_3 \times \text{Post Reg AB} \times \text{BDT Deal} \\ & + \text{Deal and Macro controls} + \text{Fixed effects}, \end{aligned} \tag{3.2}$$

where BDT Deal represents the presence of BDT stakes in mortgage deals. In addition to the BDT deal measure defined above, we also use the aggregate percentage of BDT stake loans in a deal for robustness check. We do so for low origination stakes within 10%-20% of a collateral pool and below 20% of a collateral pool, respectively. We include house price change, which we compute as the weighted average change in the house price associated with a deal from the quarter that the deal is issued to the third quarter of 2014. Using the contemporaneous house price changes permits a focus on the ex ante differences in the quality of the deals.

The results are reported in Table 3.5. Columns (1) to (4) present the findings for the 10%-20% stakes. It is clear that prior to Reg AB, there is little difference in the losses between BDT deals and non-BDT deals. However, after Reg AB, the BDT deals suffer significantly larger losses. Specifically, the estimate in column (2) indicates that BDT deals have 2.38 percentage points higher cumulative net loss. This represents 18% of the average cumulative net loss in our full sample period (2.38%/13.12%). When using the aggregate size of BDT stakes, our estimate shows that a one-standard-deviation increase in this aggregate size is associated with a 1.03% higher cumulative net loss. This represents 8% average cumulative net loss for our full sample (1.03%/13.12%). Our results are robust if we use the alternative measure of a BDT stake of less than 20% of the asset pool (see columns (5) to (8)).

Since the disclosure rule is only implemented after Reg AB, our finding that the disclosure threshold has no effect before Reg AB is expected and, more importantly, indicates that the BDT Deal dummy variable serves as a proxy for other undisclosed factors related to deal loss. The significant result of the interaction term suggests that BDT deals issued after Reg AB have larger losses than those issued before Reg AB; and among the deals issued after Reg AB, BDT deals suffer larger losses than the rest, controlling for the reported deal characteristics, contemporaneous housing price changes, issuing semester, and underwriter fixed effects. Since we are essentially comparing deals issued by the same underwriters, our findings are not driven by the differences among deal underwriters. Furthermore, the time fixed effect helps us alleviate the concern that changing market conditions rather than the regulation rule change affect the deal losses since we are comparing deals issued within the same period.

Nonetheless, it can still be argued that BDT deals and non-BDT deals may be affected by changing market conditions differently and that these changes are not included in our controls. We address this concern in the following sections by using a placebo test and by exploring the cross-sectional variation of the originators. To provide a placebo test for the

Table 3.5. The use of BDT stakes and cumulative net loss

	10%-20%				Below 20%			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BDT Deal 10%-20%(d)	1.04*	-0.53						
	(0.51)	(0.45)						
Post Reg AB × BDT Deal 10%-20%(d)		2.38**						
		(0.89)						
Total % of 10%-20%			0.02	-0.01				
			(0.02)	(0.01)				
Post Reg AB × Total % of 10%-20%				0.08**				
				(0.03)				
BDT Deal < 20%(d)					0.86	-0.38		
					(0.49)	(0.39)		
Post Reg AB × BDT Deal < 20%(d)						1.95**		
						(0.78)		
Total % of <20%							0.02	-0.01
							(0.01)	(0.01)
Post Reg AB × Total % of <20%								0.07***
								(0.02)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lead-underwriter and issue semester FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.789	0.790	0.789	0.790	0.789	0.790	0.789	0.790
Observations	2,105	2,105	2,105	2,105	2,105	2,105	2,105	2,105

We estimate linear regressions to examine the relation between the use of BDT stakes and the cumulative net loss as of September 2014 for deals completed between 2003 and 2007. Variable definitions are provided in Appendix B. The standard errors clustered by issue semester are reported in parentheses below each coefficient estimate. Statistical significance levels of 1%, 5%, and 10% are indicated with ***, **, and *, respectively.

effect of BDT stakes on the mortgage deal's cumulative net loss, we conduct a regression analysis that includes both the presence of 10%-20% stakes and 20%-30% stakes, a bracket just above the disclosure threshold. Table 3.6 reports the results of our analysis. We find that the 20%-30% stakes have no significant effect on the deal's cumulative net loss and the effect of the 10%-20% stakes remains. We include both brackets in our regression because they sometimes coexist in the same deal. The findings that the 20%-30% stake size has no relation with deal loss before and after Reg AB reassure us that our results are unlikely due to unobservable factors that are related to the size of stakes but unrelated to the disclosure regulation.

3.4.3 Cross-sectional variation in BDT stakes and BDT deal losses

Next, we explore cross-sectional variation among the originators in their BDT stakes and its relation to losses in BDT deals. Specifically, for each originator, we compute the percentage of its BDT stake occurrence before and after Reg AB, respectively, and then calculate the change in this percentage prior to and after Reg AB, denoted as ΔBDT. Our motivation here is that loan portfolios from originators with increased BDT stake occurrence are more likely used by financial intermediaries who deliberately evade disclosure; therefore, loans from these originators are more likely to have undisclosed quality problems. In particular, we sort originators by ΔBDT and compare deals with those originators that exhibit large increases in BDT stake occurrence with the rest of the deals. We refer to these originators as IBDT originators.

For each deal, we define the dummy variable, IBDT, as equal to one if the deal has one or more originators whose BDT stake occurrence increased more than the sample average, and equal to zero otherwise.[15] We use the following specification for our analysis on the

[15]For robustness, we also compare deals with originators whose changes in BDT occurrence are in the top and bottom quartiles and find even stronger results.

Table 3.6. Origination brackets [10,20), [20,30), and cumulative net loss

	(1)	(2)	(3)	(4)
BDT Deal 10%-20%(d)	1.03**	-0.53		
	(0.45)	(0.47)		
Post Reg AB × BDT Deal 10%-20%(d)		2.38**		
		(0.74)		
BDT Deal 20%-30%(d)	0.06	0.01		
	(0.56)	(0.42)		
Post Reg AB × BDT Deal 20%-30%(d)		0.00		
		(1.14)		
Total % of 10%-20%			0.02	-0.01
			(0.02)	(0.01)
Post Reg AB × Total % of 10%-20%				0.08***
				(0.02)
Total % of 20%-30%			0.00	-0.00
			(0.01)	(0.01)
Post Reg AB × Total % of 20%-30%				0.00
				(0.03)
Control variables	Yes	Yes	Yes	Yes
Lead-underwriter and issue semester FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.789	0.790	0.789	0.790
Observations	2,105	2,105	2,105	2,105

This table reports the results of analyzing the impact of [20,30) origination stakes on deal performance compared with the impact of [10,20) origination stakes. Variable definitions are provided in Appendix B. The standard errors clustered by issue semester are reported in parentheses below each coefficient estimate. Statistical significance levels of 1%, 5%, and 10% are indicated with ***, **, and *, respectively.

implication for the loan quality associated with the increased BDT stakes in mortgage deals:

$$\begin{aligned} \text{Cumulative net loss} \;\; &= \;\; \alpha + \beta \times \text{IBDT} \\ &\quad + \text{Deal and Macro controls} + \text{Fixed effects.} \end{aligned} \tag{3.3}$$

We report the results of this analysis for the full sample, the Pre-RegAB subsample, and the Post-RegAB subsample in columns (1) to (3) of Panel A in Table 3.7, respectively. Our estimate shows that deals with IBDT originators are on average associated with 1.94% higher

Table 3.7. Impact of BDT stakes and IBDT originator on deal loss

Panel A: BDT stakes, IBDT originators, and deal loss					
	(1)	(2)	(3)	(4)	(5)
	Full Sample	Pre RegAB	Post RegAB	Pre RegAB	Post RegAB
Deals with IBDT originators	1.94***	1.16**	2.89***		
	(0.49)	(0.37)	(0.30)		
BDT deals with IBDT originators				0.86	3.90***
				(0.43)	(0.49)
Non-BDT deals with IBDT originators				1.29**	2.62***
				(0.43)	(0.38)
BDT deals with Non-IBDT originators				1.36	1.39
				(1.01)	(0.79)
Control variables	Yes	Yes	Yes	Yes	Yes
Lead-underwriter and issue semester FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.788	0.748	0.776	0.749	0.777
Observations	2,038	1,042	996	1,042	996

Panel B: Difference in deal loss				
		Pre RegAB	Post RegAB	Post − Pre RegAB difference
Test 1	BDT deals v.s. Non-BDT deals for IBDT originators	-0.42	1.28	1.70
	p-value of the T-test	0.02	0.03	0.00
Test 2	BDT deals between IBDT and Non-IBDT originators	-0.49	2.51	3.01
	p-value of the T-test	0.53	0.01	0.00

Table 3.7 continued

We identify originators who increase the use of BDT stakes (10%-20%) from before Reg AB to after Reg AB and analyze the deal losses with the presence of these originators. For each deal, we define the dummy variable IBDT that equals 1 for originators with BDT stake usage above the average increase of BDT stake usage by all originators. We classify the deals into four groups based on their BDT and IBDT statuses. BDT deals with IBDT originators refer to deals with IBDT originators that originate the BDT stake, non-BDT deals with IBDT originators refer to deals with IBDT originators that originate the non-BDT stake, and BDT deals with non-IBDT originators refer to deals without IBDT originators but contain BDT loan stakes. Panel A columns (1) to (3) present the regressions that analyze the impact of IBDT originators on deal losses; Panel A columns (4) and (5) present the regressions that analyze the impact of the four groups formed by BDT deals and IBDT originators on deal losses, and the base group is non-BDT deals with non-IBDT originators. Panel B presents the tests for the differences between two groups of interests. Definitions of the other variables are provided in Appendix B. The standard errors clustered by issue semester are reported in parentheses below each coefficient estimate. Statistical significance levels of 1%, 5%, and 10% are indicated with ***, **, and *, respectively.

cumulative net loss than the non-IBDT deals (column (1)).[16] Next, we examine whether BDT deals with IBDT originators suffer larger losses after Reg AB. Our estimate shows that deals with IBDT originators are associated with 1.16% and 2.89% higher cumulative net loss than the rest before Reg AB and after Reg AB (column (2) and (3)), respectively.

Next, we classify all the deals into four groups based on the BDT stakes and IBDT originator involvement in each deal. Our purpose here is to connect, metaphorically, the "weapon of choice" to the "partners in crime" and compare the cases where the two are not connected. Specifically, *BDT deals with IBDT originators* refer to the deals with IBDT originators that originate the BDT stakes; *Non-BDT deals with IBDT originators* refer to the deals with IBDT originators originating the non-BDT stakes; *BDT deals with Non-IBDT originators* refer to the deals without IBDT originators but with BDT stakes; and *Non-BDT deals with Non-IBDT originators* refer to the deals without BDT stakes and without IBDT originators. Panel A columns (4) and (5) present the regression results that analyze the deal losses for these four groups with the *Non-BDT deals with Non-IBDT originators* as the base group.

As we expected, BDT deals with IBDT originators have largest losses among the four groups only after Reg AB. On the other hand, non-BDT deals with IBDT originators suffer larger losses both before and after Reg AB. In terms of magnitude, the loss increases from 0.86% before Reg AB to 3.90% after Reg AB for BDT deals with IBDT originators and from 1.29% before Reg AB to 2.62% after Reg AB for non-BDT deals with IBDT originators, relative to the base group. Furthermore, in Panel B, we conduct two difference-in-differences (DiD) tests to formally assess the above changes before and after Reg AB (BDT versus non-BDT deals for IBDT originators, and BDT deals with and without IBDT originators). These results suggest that low quality loans from IBDT originators are less likely to be associated with BDT stakes before Reg AB since the disclosure threshold is nonexistent before Reg

[16] A similar result is found when we use the alternative measure of below 20% origination stakes.

AB, and it is more economical for IBDT originators to place low quality loans in larger stake sizes and that BDT stakes comprise a larger proportion of low quality loans from IBDT originators after Reg AB.

Overall, we find that deals with IBDT originators suffer larger losses than those without, especially after Reg AB. This helps us establish the disclosure threshold effect on deal loss because we sort originators based on their loan stakes and compare deals cross-sectionally rather than over different sample periods.

To demonstrate that our findings are robust to the deal's cumulative net loss measured at different dates, we construct an alternative measure of cumulative net loss as of December 2012 that is scaled by the original collateral balance. Consistent with our findings reported here, the implications of the increased BDT stake occurrence for the deal's loan quality remain significant and qualitatively similar (unreported results). This finding indicates that larger cumulative net loss associated with increased BDT stake occurrence in mortgage deals is robust to different dates of computing the loss. It manifests that financial intermediaries underwriting MBS deals deliberately game the disclosure threshold under Reg AB. Their reactions shed light on their intent to misrepresent.

3.4.4 The implication of BDT stakes for deal yield spreads and credit enhancement

One important question is whether or not the higher cumulative net loss of BDT deals with IBDT originators is reflected in the initial yield spreads and credit enhancement of these deals. This is relevant for how investors evaluate the implications of the disclosure mandate for credit risk protection and deal pricing. We conduct two sets of analysis to address this question. First, we use the same specification for the yield spread and credit enhancement as for the cumulative net loss in Table 3.7 to assess whether the presence of BDT stakes and IBDT originators is reflected in these variables. Second, to quantitatively

assess the extent to which investors incorporate the information from BDT stakes and IBDT originators into pricing, we redo the analysis of Table 3.7 by controlling for the initial yields and credit enhancement. The rationale is that if investors fully incorporate the information of BDT stakes and IBDT originators into pricing, then they should be unrelated to deal loss after controlling for initial yields and credit enhancement. Otherwise, it suggests that the underwriters are successful in their use of BDT stakes to evade the Reg AB disclosure threshold and thus misrepresent underlying loan quality.

Table 3.8 reports the results of our first set of analysis. Panels A1 and A2 present the results on how BDT deals with IBDT originators affect a deal's initial yields.[17] First of all, we find that investors demand higher initial yields (14 basis points higher) for deals with IBDT originators than deals without IBDT originators after Reg AB (column (3)) but not before Reg AB (columns (2)). However, investors do not seem to be able to distinguish meaningfully between BDT and non-BDT deals with IBDT originators after Reg AB. For example, compared with the base group, investors demand 19 (14) basis points higher for BDT deals (non-BDT deals) with IBDT originators (column (5)). As reported in Panel A2, the difference of 5 basis points is not only small in magnitude but also statistically insignificant with a p-value of 0.32 (shown under the column Post RegAB). Our results suggest that although investors may be aware of certain problematic originators, they fail to detect the act of gaming the disclosure threshold, which is the key channel through which underwriters misrepresent the quality of the pool's assets after Reg AB.

Panels B1 and B2 present the results on how BDT deals with IBDT originators affect deal credit enhancement.[18] We find that deals with IBDT originators generally do not offer

[17]For deal yields, we use the initial average yield spread for all of the securities issued by the trustee of the mortgage deals. This is the difference between the average yield of all the securities issued by the trustee weighted by the face value of the securities and the yield on the 10-year Treasury bond. The former is calculated using the standards of the Bond Market Association.

[18]Credit enhancement is the subordination measured as the percentage of the face value of trust securities not rated AAA by Moody's or Standard & Poor's at the deal's close.

Table 3.8. Impact of BDT stakes and IBDT originator on yields and credit enhancement

Panel A1: BDT stakes, IBDT originators, and initial yield					
	(1)	(2)	(3)	(4)	(5)
	Full Sample	Pre RegAB	Post RegAB	Pre RegAB	Post RegAB
Deals with IBDT originators	0.04	-0.08	0.14**		
	(0.04)	(0.06)	(0.04)		
BDT deals with IBDT originators				-0.02	0.19**
				(0.16)	(0.05)
Non-BDT deals with IBDT originators				-0.06	0.14*
				(0.06)	(0.05)
BDT deals with Non-IBDT originators				0.30**	0.17
				(0.09)	(0.12)
Control variables	Yes	Yes	Yes	Yes	Yes
Lead-underwriter and issue semester FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.623	0.716	0.585	0.717	0.585
Observations	2,157	1,072	1,085	1,072	1,085

Panel A2: Difference in initial yield	Pre RegAB	Post RegAB	Post − Pre Reg AB
BDT deals v.s. Non-BDT deals for IBDT originators	0.04	0.05	0.01
p-value of the T-test	0.79	0.32	0.95
BDT deals between IBDT and Non-IBDT originators	-0.32	0.02	0.34
p-value of the T-test	0.09	0.85	0.03

Table 3.8 continued

Panel B1: BDT stakes, IBDT originators, and subordination					
	(1)	(2)	(3)	(4)	(5)
	Full Sample	Pre RegAB	Post RegAB	Pre RegAB	Post RegAB
Deals with IBDT originators	-0.58*	-0.73	-0.54		
	(0.31)	(0.53)	(0.30)		
BDT deals with IBDT originators				0.99**	-0.48
				(0.38)	(0.47)
Non-BDT deals with IBDT originators				-0.81	-0.50
				(0.50)	(0.39)
BDT deals with Non-IBDT originators				2.51	0.37
				(1.95)	(0.66)
Control variables	Yes	Yes	Yes	Yes	Yes
Lead-underwriter and issue semester FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.834	0.797	0.886	0.803	0.886
Observations	2,063	1,001	1,062	1,001	1,062

Panel B2: Difference in subordination	Pre RegAB	Post RegAB	Post − Pre Reg AB
BDT deals v.s. Non-BDT deals for IBDT originators	1.80	0.02	-1.78
p-value of the T-test	0.01	0.97	0.00
BDT deals between IBDT and Non-IBDT originators	-1.52	-0.86	0.67
p-value of the T-test	0.51	0.36	0.75

This table presents the results of analyzing the impact of BDT stakes and IBDT originator on deal initial yields and credit enhancement. The empirical design is the same as in Table 3.7 except that in Panels A1 and A2, we have deal initial yields as the dependent variable, and in Panels B1 and B2, we have credit enhancement as the dependent variable. For deal yields, we use the initial average yield spread of all the securities issued by the trust of mortgage deals. This is the difference between the average yield of all the securities issued by the trust weighted by the face value of the securities and the yield on the 10-year Treasury bond. Credit enhancement is the subordination measured as the percentage of the face value of trust securities not rated AAA by Moody's or Standard & Poor's at the deal's close. Definitions of the other variables are provided in Appendix B. The standard errors clustered by issue semester are reported in parentheses below each coefficient estimate. Statistical significance levels of 1%, 5%, and 10% are indicated with ***, **, and *, respectively.

more credit enhancement and that there is no significant difference between BDT deals and non-BDT deals with IBDT originators after Reg AB. The results here again lend support to the conclusion drawn above based on the initial yields of these deals.

Next, we quantitatively assess the extent to which investors incorporate the information from BDT stakes and IBDT originators into pricing by performing the analysis of Table 3.7 and controlling for the initial yields and credit enhancement. The results are presented in Table 3.9. These results are quite similar to the results in Table 3.7, where we do not control for the initial yields and credit enhancement. Combining these findings with the results on deal yield spreads and subordination provides evidence that investors may not have impounded the larger loss associated with BDT deals and IBDT originators into the yields and credit enhancement of these deals after Reg AB. This partly explains how financial intermediaries underwriting MBS sold many of these BDT deals of poor-quality loans without being detected by the investors.

Overall, our deal-level analyses utilizing the disclosure rule change under Reg AB uncover two important findings. First, underwriting financial intermediaries drastically increase the occurrence of BDT stakes in mortgage deals after Reg AB. Second, deals comprising loans from originators with a larger increase in BDT stake occurrence post-Reg AB are associated with higher cumulative net loss. These findings suggest that the increased BDT stake occurrence post-Reg AB is motivated by financial intermediaries' desire to evade disclosure on the pool's assets, resulting in their gaming of the disclosure threshold rule, which constitutes a serious form of misrepresentation by the underwriting financial intermediaries in the MBS securitization market.

3.5 Loan defaults in BDT stakes

In the previous sections, we compare the losses *across* deals and find that BDT deals issued after Reg AB suffer larger losses and the result is particularly strong for BDT deals

Table 3.9. Impact of BDT stakes and IBDT originator on deal loss controlling for yields and credit enhancement

Panel A: BDT stakes, IBDT originators, and deal loss					
	(1)	(2)	(3)	(4)	(5)
	Full Sample	Pre RegAB	Post RegAB	Pre RegAB	Post RegAB
Deals with IBDT originators	2.20***	1.32**	2.99***		
	(0.49)	(0.42)	(0.16)		
BDT deals with IBDT originators				0.62	3.96***
				(0.31)	(0.41)
Non-BDT deals with IBDT originators				1.48**	2.76***
				(0.47)	(0.27)
BDT deals with Non-IBDT originators				0.91	1.45
				(1.23)	(1.02)
Initial yield	0.74**	-0.22	1.09***	-0.23	1.06**
	(0.29)	(0.23)	(0.18)	(0.22)	(0.19)
Subordination	0.44***	0.25**	0.32**	0.26**	0.32**
	(0.06)	(0.09)	(0.08)	(0.09)	(0.08)
Control variables	Yes	Yes	Yes	Yes	Yes
Lead-underwriter and issue semester FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.804	0.758	0.784	0.759	0.785
Observations	1,945	971	974	971	974

Panel B: Difference in deal loss			
	Pre RegAB	Post RegAB	Post − Pre RegAB difference
BDT deals v.s. Non-BDT deals for IBDT originators	-0.86	1.20	2.06
p-value of the T-test	0.01	0.05	0.00
BDT deals between IBDT and Non-IBDT originators	-0.29	2.51	2.79
p-value of the T-test	0.80	0.04	0.02

Table 3.9 continued

This table presents the results of analyzing the impact of BDT stakes and IBDT originator on deal loss after controlling for the initial yields and credit enhancement. The empirical design is the same as in Table 3.7 except that we include deal initial yields and credit enhancement as additional control variables. For deal yields, we use the initial average yield spread of all the securities issued by the trust of mortgage deals. This is the difference between the average yield of all the securities issued by the trust weighted by the face value of the securities and the yield on the 10-year Treasury bond. Credit enhancement is the subordination measured as the percentage of the face value of trust securities not rated AAA by Moody's or Standard & Poor's at the deal's close. Definitions of the other variables are provided in Appendix B. The standard errors clustered by issue semester are reported in parentheses below each coefficient estimate. Statistical significance levels of 1%, 5%, and 10% are indicated with ***, **, and *, respectively.

comprising loans from originators with increased BDT stake occurrence after Reg AB. In sharpening our analysis, we compare loans *within* deals through an examination on whether loans associated with BDT stakes are more likely to default than other loans in the same deal. We first test whether loans made by originators with increased BDT stake occurrence after Reg AB indeed experience greater default rates than other loans. Next, we examine whether poor quality loans are more likely to be placed in BDT stakes after Reg AB to evade the disclosure rule. We include loan-level controls such as loan and borrower characteristics, housing price changes, regional and macroeconomics conditions, and origination time and deal fixed effects. Our loan-level analysis reaffirms our deal-level findings that those financial intermediaries underwriting MBS indeed game the disclosure threshold rule by concealing poor-quality loans from certain originators.

Following the standard practice in the literature, we use the securitized loan's delinquency, defined as 60 days or more past due within 24 months of the origination as the measure of our interest in the loan-level analysis. Detailed loan and borrower characteristics included in our analysis are listed in Appendix B. To control for the housing price changes, we compute the appreciation in house prices over the 24 months after the origination of a loan by using the house price index for the loan borrower's metropolitan statistical area (MSA) reported by the Federal Housing Financing Agency (FHFA). We also compute the change in the state-level unemployment rate over the 24 months after the loan origination using data reported by the Bureau of Economic Analysis and collect the median household income in 1999 for the borrower's zip code as reported by the U.S. Census Bureau in 2000. Additionally, we include the credit spread and the 10-year Treasury yield as macro control variables. To control for the different qualities of loans originated at different periods, we include the loan issue (origination) semester (half-year) fixed effect. Controlling the issue semester fixed effect mitigates both the vintage effect and other macroeconomic changes in the sample period not captured by our macro control variables. More importantly, we include the deal fixed

effect to enable us to compare loans within the deals from different originators and different stake sizes. One new variable in our loan-level regression is stake size, which represents an originator's share (same for all loans from the same originator in the same deal). We include this control variable because we expect that loans from vastly different stake sizes can have different qualities within the same deal.

A key variable in our loan-level analysis is the change in each originator's BDT stake occurrence surrounding Reg AB, that is, ΔBDT defined in Section 3.4.3. This is the same for all loans originated by the same originator. At the loan level, we can use ΔBDT directly without aggregating across originators in each deal. We use this continuous variable in two specifications in our loan-level regressions. First, we expect that loans from originators with increased BDT stake occurrence after Reg AB, that is, high ΔBDT originators, are more likely to be delinquent than loans from low ΔBDT originators within the same deal. This suggests that the frequency of loan delinquency should increase in ΔBDT. Second, we investigate whether the effect of ΔBDT on delinquency is stronger on loans in the 10%-20% stakes after Reg AB, an evidence supporting the hypothesis that underwriters placed poor quality loans in BDT stakes. This is the same intuition as in connecting the BDT deals with IBDT originators, except that here we compare loans within the deals. Since loans from vastly different stake sizes can have different qualities within the deal, we need an appropriate control group to show the difference in the effect of ΔBDT on delinquency. We choose to use loans from 20%-30% stakes as the control group because of their close proximity to the 10%-20% stake size and the fact that the disclosure threshold between these two groups is most relevant.

In the regression specification, we interact ΔBDT with a dummy variable for BDT stakes and expect this interaction term to be significantly positive if underwriters use the 10%-20% stake size to evade disclosure rather than the 20%-30% stake size. We conduct this analysis separately for the before and after Reg AB subsamples of loans and expect the interaction

term to be significantly positive after Reg AB and insignificant before Reg AB when the disclosure threshold was nonexistent. Merging the deal-level information on the originators with the loan-level data and excluding missing observations, we have more than 3.5 million loans in 1,603 deals. In Table 3.10, we report the summary statistics for the loan-level variables for the full sample and subsamples of loans in the 10%-20% and 20%-30% stakes, respectively. We observe that the sample averages for these variables are close between the whole sample and subsamples, and even closer between the two subsamples.

Table 3.10. Summary statistics for loans

		Originator's share in a deal	
Variables	All loans	[10,20)%	[20,30)%
Delinquency	0.23	0.25	0.23
FICO	638	654	645
Full Doc	0.59	0.50	0.52
CLTV	81.70	82.20	81.40
Investor	0.08	0.10	0.10
DTI	39.21	38.48	38.55
Miss DTI	0.18	0.15	0.15
Cash-Out	0.12	0.13	0.12
PrePayPen	0.64	0.58	0.62
Initial Rate	7.10	7.05	6.93
Margin	5.19	4.70	4.97
Rate Reset	27.77	34.36	33.83
Loan Amt.	232,299	257,756	248,703
ARM	0.07	0.06	0.07
Balloon	0.08	0.07	0.03
Hybrid2	0.45	0.35	0.39
Hybrid3	0.15	0.29	0.27
Int. Only	0.17	0.30	0.22
Local Income	47,772	48,485	48,252
Unemployment	0.10	0.26	0.16
Price Appr.	0.09	0.08	0.09

This table reports the mean values for the loan-level variables. We report these numbers for all the loans for which we can identify the originators at the deal level, as well as for the loans whose originators contributed loans to deals in the brackets of [10,20)% and [20,30)%.

Table 3.11 reports the marginal effects from the probit regression for the baseline model (column (1) and the model with our key variable ΔBDT (column (2)) and the subsamples with the variable ΔBDT for loans in the 10%-20% stakes (column (3)) and 20%-30% stakes (column (4)), respectively. Our estimation results in the baseline model are mostly as expected for the control variables, lower delinquency for higher FICO score, full documentation loans, lower loan-to-value ratio, owner-occupiers, lower debt-to-income ratio, among others. We also find that loans from larger stake sizes have lower delinquency. This finding makes it necessary for us to control for the stake size in our subsequent analysis and to use stake sizes close to BDT stakes as a control. Our main finding in this table is that ΔBDT is positively associated with delinquency, controlling for all other variables and the deal and issue semester fixed effects. The magnitude of the estimate is also economically significant. For one standard deviation change in ΔBDT (33.8% among all the originators in our sample), the delinquency rate increases by 7.3% relative to the sample average 23% delinquency rate (0.05*0.338/0.23). Further, this effect is concentrated in the subsample of loans in the 10%-20% stakes and is nonexistent in the subsample of loans in the 20%-30% stakes. This result suggests that loans from originators with increased BDT stake occurrence are significantly worse than their reported characteristics, in particular for loans in the 10%-20% stakes but not for loans in the 20%-30% stakes.

Next, we test whether the effect of ΔBDT is stronger for loans in BDT stakes after Reg AB using the subsample of loans from BDT stakes and the control group 20%-30% stakes. Table 3.12 presents the results of a probit regression for loans included in deals issued before and after Reg AB, respectively. Our estimation for the pre-Reg AB period shows that loan delinquency is not statistically significantly related to ΔBDT. Consistent with our expectation, we find no significant difference in the effect of ΔBDT on loan delinquency between BDT stakes and the control group pre-Reg AB. In contrast, our estimation for the post-Reg AB period shows that ΔBDT is strongly associated with higher delinquency for

Table 3.11. The use of BDT stakes and loan performance

	All loans	All loans	[10,20)% loans	[20,30)% loans
ΔBDT		0.05***	0.05**	-0.02
		(0.02)	(0.02)	(0.03)
Stake size	-0.04***	-0.03***	-0.05	0.04
	(0.00)	(0.01)	(0.15)	(0.15)
FICO	-0.09***	-0.09***	-0.10***	-0.10***
	(0.00)	(0.00)	(0.00)	(0.00)
Full Doc	-0.06***	-0.06***	-0.07***	-0.06***
	(0.00)	(0.00)	(0.01)	(0.00)
CLTV	0.06***	0.06***	0.07***	0.07***
	(0.00)	(0.00)	(0.00)	(0.00)
Investor	0.05***	0.05***	0.04***	0.06***
	(0.00)	(0.00)	(0.01)	(0.01)
DTI	0.02***	0.02***	0.02***	0.02***
	(0.00)	(0.00)	(0.00)	(0.00)
Miss DTI	0.03***	0.03***	0.06***	0.07***
	(0.01)	(0.01)	(0.01)	(0.02)
Cash-Out	0.00	0.00	-0.01*	0.00
	(0.00)	(0.00)	(0.00)	(0.01)
PrePayPen	0.05***	0.05***	0.06***	0.05***
	(0.00)	(0.00)	(0.00)	(0.00)
Initial Rate	0.03***	0.03***	0.03***	0.03***
	(0.00)	(0.00)	(0.01)	(0.01)
Margin	0.03***	0.03***	0.03***	0.02***
	(0.00)	(0.00)	(0.01)	(0.01)
Rate Reset	-0.02***	-0.02***	-0.01	-0.02***
	(0.00)	(0.00)	(0.01)	(0.00)
Loan Amt.	0.02***	0.02***	0.02***	0.02***
	(0.00)	(0.00)	(0.00)	(0.00)
ARM	0.02**	0.02**	0.00	0.03
	(0.01)	(0.01)	(0.03)	(0.03)
Balloon	0.04***	0.03***	0.01	0.03
	(0.01)	(0.01)	(0.02)	(0.02)
Hybrid2	0.01	0.01	-0.01	0.03*
	(0.01)	(0.01)	(0.02)	(0.02)
Hybrid3	0.01	0.00	-0.01	0.03*
	(0.01)	(0.01)	(0.02)	(0.02)
Int. Only	0.02***	0.02***	0.01	0.01
	(0.00)	(0.00)	(0.01)	(0.01)
Local Income	-0.02***	-0.02***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)

Table 3.11 continued

	All loans	All loans	[10,20)% loans	[20,30)% loans
Unemployment	-0.17***	-0.17***	-0.23***	-0.16***
	(0.01)	(0.01)	(0.02)	(0.03)
Price Appr.	-0.19***	-0.19***	-0.20***	-0.20***
	(0.00)	(0.00)	(0.01)	(0.01)
Deal and issue semester FE	Yes	Yes	Yes	Yes
Pseudo-R^2	0.240	0.240	0.290	0.239
N	3,531,107	3,531,107	99,108	150,317

This table reports the marginal effects from probit regressions analyzing the implication of the increased use of BDT stakes in mortgage deals on the performance of individual loans in the groups surrounding the disclosure threshold. We regress the loan *Delinquency* status on the origination change variable and other loan-level variables using probit regressions. The variable ΔBDT is defined as the change from before Reg AB to after Reg AB in the fraction of 10%-20% deals for each originator (same for all loans from the same originator). The *Stake size* is the share of the originator (same for all loans from the same originator in the same deal) in each mortgage deal. Definitions of the other variables are provided in Appendix B. The standard errors clustered by issue semester are reported in parentheses below each coefficient estimate. Statistical significance levels of 1%, 5%, and 10% are indicated with ***, **, and *, respectively.

Table 3.12. Loan performance in the brackets of [10,20) and [20,30)

	Pre-Reg AB [10,30)% loans	Post-Reg AB [10,30)% loans
ΔBDT	0.03	-0.24***
	(0.02)	(0.07)
ΔBDT × BDT Deal	-0.02	0.28***
	(0.03)	(0.08)
BDT Deal	0.02**	-0.02
	(0.01)	(0.03)
Stake size	0.08	-0.17
	(0.08)	(0.22)
FICO	-0.06***	-0.14***
	(0.00)	(0.01)
Full Doc	-0.03***	-0.13***
	(0.00)	(0.01)
CLTV	0.02***	0.11***
	(0.00)	(0.01)
Investor	0.02***	0.06***
	(0.01)	(0.01)

Table 3.12 continued

	Pre-Reg AB [10,30)% loans	Post-Reg AB [10,30)% loans
DTI	0.01***	0.04***
	(0.00)	(0.01)
Miss DTI	0.03***	0.13***
	(0.01)	(0.02)
Cash-Out	-0.01***	0.01*
	(0.00)	(0.01)
PrePayPen	0.03***	0.09***
	(0.00)	(0.01)
Initial Rate	0.01**	-0.01
	(0.00)	(0.01)
Margin	0.02***	0.05***
	(0.00)	(0.01)
Rate Reset	-0.02***	-0.03**
	(0.00)	(0.01)
Loan Amt.	0.02***	0.06***
	(0.00)	(0.01)
ARM	-0.04***	-0.04
	(0.01)	(0.05)
Balloon	-0.01	0.06**
	(0.01)	(0.03)
Hybrid2	0.01	0.01
	(0.01)	(0.03)
Hybrid3	0.01	0.02
	(0.01)	(0.03)
Int. Only	-0.01**	0.05***
	(0.00)	(0.02)
Local Income	-0.01***	-0.05***
	(0.00)	(0.00)
Unemployment	0.06***	-0.38***
	(0.01)	(0.01)
Price Appr.	-0.13***	-0.19***
	(0.00)	(0.01)
Deal and issue semester FE	Yes	Yes
Pseudo-R^2	0.313	0.326
N	139,316	109,181

This table reports the marginal effects from probit regressions analyzing the implication of increased BDT stake usage on individual loan performance in pre- and post-Reg AB periods. The ΔBDT and *Stake size* are defined in Table 3.11. For each deal, *BDT Deal* is a dummy variable that equals 1 if the deal has an originator that originated a 10%-20% BDT stake

Table 3.12 continued

(same for all loans from the same originator in the same deal), and 0 otherwise. Definitions of the other variables are provided in Appendix B. The standard errors clustered by issue semester are reported in parentheses below each coefficient estimate. Statistical significance levels of 1%, 5%, and 10% are indicated with ***, **, and *, respectively.

loans in BDT stakes relative to the control group. ΔBDT is negatively associated with delinquency for the control group (loans in non-BDT deals) after Reg AB. This is because gaming the disclosure threshold is more severe with high ΔBDT originators. Knowing the poor quality of a loan portfolio with stake size just above the disclosure threshold, the underwriting financial intermediary can adjust the composition of the pool assets to make the stake size below Reg AB's disclosure threshold. As a result of this adjustment, lower-quality loans end up in the BDT stakes and higher-quality loans in the control group for high ΔBDT originators. This only happens after Reg AB and results in a negative coefficient for ΔBDT and a positive coefficient for the interaction between ΔBDT and BDT Deal. Overall, our findings presented in this table support the hypothesis that low quality loans are placed in BDT stakes after Reg AB.

3.6 Conclusion

Information disclosure in the non-agency securitization market is complex due to the large number of participants involved in the process. When a loan is extended to a borrower, information on the loan's quality flows to investors along the entire supply chain of credit. Misrepresentation of critical information can occur at the borrower, lender, and/or security underwriter level. It is arguably more damaging for misrepresentation to occur at the underwriter level because the financial intermediaries underwriting securities collect and verify information regarding the quality of the underlying collaterals in the securitization process. They are also generally large and reputable financial intermediaries that are typically more

sophisticated than the investors in this market. Consequently, investors' reliance on underwriters renders them especially vulnerable to loan quality misrepresentation at this level. To investigate this issue, we take advantage of a regulatory change on disclosure rule under Reg AB as a quasi-natural experiment and document that underwriting financial intermediaries game the disclosure threshold and willingly misrepresent securitized loan quality.

The disclosure rule under Reg AB requires all material risk factors applicable to the transaction as a whole or to the nature of the security to be disclosed. Specifically, when an originator's loans comprise 20% or more of the collateral assets, the originator must disclose information, such as origination program, form of organization, and detailed information material to the investors' analysis of the collateral assets. The purpose of this requirement is to encourage transparency and, therefore, accountability. Using data on mortgage deals constructed before and after Reg AB, we find that MBS underwriters deliberately keep lower-quality loans from certain originators below the mandate threshold in order to evade disclosure under Reg AB. This leads to larger losses for investors who rely on reported deal characteristics for security analysis. Our findings are supported by both deal-level analysis and loan-level analysis. The latter uses more detailed controls for loan and borrower characteristics, thus sharpening our analysis.

Our study on how these regulations change market participants' behavior and the ensuing economic impact can shed light on future research and the policy making directed at the asset-backed securities market. Coincidentally, the recently adopted Regulation AB II has tightened the disclosure rule and now requires disclosure of material information on stakes from any originator comprising 10% or more of the pool assets.[19] We view this move as an important step toward curbing underwriters' evasion of material information disclosure and ultimately reducing misrepresentation in the asset securitization market.

[19]Regulation AB II adopted on August 27, 2014, requires that if the cumulative amount of pool assets originated by parties other than the sponsor or its affiliates is more than 10% of the total pool assets, then any originator that originates less than 10% of the pool assets must also be identified in the prospectus.

CHAPTER 4

CONCLUSION

With the growth of the financial services industry, the roles of financial intermediaries have become increasingly important. In this dissertation, I have examined the main strategic actions by financial intermediaries through institutional common ownership and during asset securitization process. In the first essay, I examine how institutional common ownership of same-industry firms influences competition among rival portfolio firms. I find that CEOs' total compensation is positively sensitive to the performance of peers that share common blockholders. In the second essay, my coauthors and I investigate the responses of financial intermediaries to the regulatory changes on disclosure in the asset-backed securities market under the Regulation AB, which covers the regulatory changes on disclosure in the asset-backed securities market. We find a drastic jump in the percentage of deals with origination stakes just below the disclosure threshold, following the implementation of Regulation AB. We further show that mortgage-backed securities underwriters deliberately keep lower-quality loans below the disclosure threshold to evade the disclosure under the regulation. The results from both essays demonstrate the important considerations that regulatory authorities must evaluate regarding the roles of financial intermediaries.

APPENDIX A

VARIABLE DEFINITIONS FOR CHAPTER 2

Variable	Definition
Total Compensation	The total annual compensation flow is calculated as the sum of salary, bonus, other annual compensation (e.g., perquisites and other personal benefits, tax reimbursements, above-market earnings on restricted stock, options, or deferred compensation paid during the year but deferred by the officer), total value of restricted stocks granted, total value of stock options granted (using the Black-Scholes formula), long-term incentive payouts, and all other compensation (e.g., payouts for cancellation of stock options, signing bonuses, 401(k) contributions, life insurance premiums).
Ln Total Compensation	The natural logarithm of the total annual compensation flow.
Cash Compensation	Cash compensation comprises the salary and bonus. Salary is the dollar value of the base salary (cash and non-cash) earned by the named executive during the fiscal year. Bonus is the dollar value of the bonus (cash and non-cash) earned by the named executive during the fiscal year.
Stocks and Options Compensation	Stocks and options compensation is the total value of restricted stocks granted and the total value of stock options granted.
Co-owned (d)	A dummy variable that equals 1 if the focal firm shares a common blockholder with a peer firm in any of the four quarters prior to the fiscal year end, and 0 otherwise.
Ln Firm Return	The natural logarithm of annual stock returns including dividends.
Ln Peer Return	The natural logarithm of peer-firm annual stock returns including dividends.
CEO Age	The natural logarithm of the CEO's age.
CEO Tenure	The natural logarithm of the CEO's tenure. Tenure is defined as the difference between the current fiscal year for which the CEO is still in office and the year in which the CEO assumed office (obtained from the variable BECAMECEO from ExecuComp).
SIC3 HHI	The Herfindahl-Hirschman Index (HHI) based on the Compustat three-digit SIC industry classification. This is based on the sum of squared percentage market shares in sales.

Variable	Definition
Institutional Ownership	The percentage of shares held by all institutional investors listed in 13F, calculated as a ratio of the total number of the firm's shares outstanding.
Size	The natural logarithm of total assets.
EBITDA/Asset	EBITDA/Asset is calculated as the earnings before interest, tax, depreciation, and amortization (EBITDA) divided by lagged total assets.
Correlation	Correlations were calculated using past one year firm-pair weekly returns.
Cash	The cash and short-term investments divided by lagged total assets.
Leverage	The sum of long-term debt and debt in current liabilities divided by beginning-of-year total assets.
Whited-Wu Index	The WW Index is equal to -0.091 $\times$ Cash Flow/Assets - 0.062 $\times$ I(Cash Dividend Dummy) + 0.021 $\times$ Long Term Debt/Assets - 0.044 $\times$ Ln(Assets) + 0.102 $\times$ 3-digit SIC Industry Sales Growth - 0.035 $\times$ Sales Growth.

APPENDIX B

VARIABLE DEFINITIONS FOR CHAPTER 3

Deal and Macro Variables:

Variable	Definition
Cumulative net loss	Historical percentages of cumulative loss on the underlying loans comprising the entire collateral that backs the deal, measured as of September 2014.
BDT Deal 10%-20% (d)	A dummy variable that equals 1 if a deal has (an) originator(s) that originate(s) a percentage of loans between 10% and 20%, and 0 otherwise.
BDT Deal $<$ 20% (d)	A dummy variable that equals 1 if a deal has (an) originator(s) that originate(s) loans below 20%, and 0 otherwise.
IBDT (d)	A dummy variable that equals 1 for originators with BDT stake usage above the average increase of BDT stake usage by all originators (based on a between 10% and 20% threshold), and 0 otherwise.
Total percentage of 10%-20%	Total percentage of loans that are in stakes between 10% and 20%.
Total percentage of $<$ 20%	Total percentage of loans that are in stakes below 20%.
Original collateral balance	The original balance of the underlying loans comprising the entire collateral.
High reputation (d)	A dummy variable that equals 1 if the deal has an underwriter whose IPO reputation score is greater than or equal to 8 (from Professor Jay Ritter's website), and 0 otherwise (detailed procedures of ranking are provided in Loughran and Ritter (2004)). This measure follows from Griffin, Lowery, and Saretto (2014).
No. of tranches	Number of securities in a deal.
Low documentation (d)	A dummy variable indicating higher than mean share of underlying loans with limited, as distinguished from full, documentation or no documentation.
FICO	Weighted average original credit score of the underlying loans.
LTV	Original loan to value percentage of the loan.
Adjustable rate mortgage	The percentage of the adjustable rate mortgage loans.
Negative amortization (d)	A dummy variable that equals 1 if the deal consists of mortgages with negative amortization features, and 0 otherwise.
Purchase loan	The percentage of the loan purpose (the reason for the loan) for purchase.

Variable	Definition
Single family	The percentage of single family mortgaged properties, the type of properties against which the loans were written.
Owner occupied	The percentage of the occupancy (the purpose of the property) for owner occupied.
Equity take out	The percentage of the loan purpose (the reason for the loan) for equity take out.
Refinance	The percent of the loan purpose (the reason for the loan) for refinance.
Second lien	The percentage of the loans comprising the collateral that are second liens.
House prices change	We compute the average house price changes from the issue's quarter to the third quarter of 2014 using the state-level Federal Housing Finance Agency's (FHFA) seasonally adjusted quarterly house price index. The weighted average for each deal is taken over the top 5 states by their mortgage balances assuming the remaining 45 states have equal representation.
House price run-up	We use the same data and method as in "House prices change" to calculate the weighted average price change associated with a deal during the four quarters preceding the quarter the deal was closed.
Credit spread	The spread between BBA and AAA corporate bond yields in the month of the issue.
10-Year Treasury	10-year treasury yield in the month of issue.

Loan-Level Variables:

Variable	Definition
Delinquency	A dummy variable that equals 1 if the loan payment is 60 days past due within the 24 months of origination, and 0 otherwise.
FICO	Fair, Isaac and Company (FICO) credit score at origination standardized with the sample mean and variance.
Full DOC	A dummy variable that equals 1 if the borrower has complete documentation on income and assets.
CLTV	Combined loan to value ratio for the first lien loan at origination. The ratio includes a second lien when it exists. The LTV ratio is in decimal (e.g., a 20% down payment = 0.80 LTV ratio).
Investor	A dummy variable that equals 1 if the borrower does not owner-occupy the property.

Variable	Definition
DTI	Back-end debt-to-income ratio, defined as the total monthly mortgage payment to monthly gross income at origination, in percent.
Miss DTI	A dummy variable that equals 1 if DTI is missing. Demyanyk and Van Hemert (2011) interpret a Miss DTI as a negative signal about borrower quality.
Cash-Out	A dummy variable that equals 1 if the purpose of the loan is for a cash-out refinance where the balance of the loan is increased to raise cash. As noted by Pennington-Cross and Chomsisengphet (2007), the most common reasons for a cash-out refinance are to consolidate debt and to improve property.
PrePayPen	A dummy variable that equals 1 when the loan has a prepayment penalty and/or is an option ARM or negative amortization loan.
Initial Rate	The initial mortgage interest rate in percent.
Margin	Margin (in percent) for an adjustable-rate or hybrid loan over an interest rate index, applicable after the first interest rate reset.
Rate Reset	Time period (in months) before the interest rate in an adjustable-rate loan starts to adjust.
Loan Amt.	Size of the loan at origination in dollars.
ARM	A dummy variable that equals 1 if the loan is an adjustable rate mortgage and the first interest rate reset period is less than or equal to 1 year from the date of origination.
Balloon	A dummy variable that equals 1 for a fixed rate or adjustable rate loan where the payments are lower over the life of the loan leaving a balloon payment at maturity.
Hybrid2	A dummy variable that equals 1 for an adjustable rate loan with the initial monthly payment fixed for the first two years. This is typically referred to as a 2/28 hybrid ARM.
Hybrid3	A dummy variable that equals 1 for a 3/27 hybrid ARM.
Int. Only	A dummy variable that equals 1 if the loan has an interest only feature. For example, a 30-year fixed rate or adjustable rate loan may permit the borrower to only pay interest for the first 60 months of the loan, but then the borrower must make payments in order to repay the loan in the final 25 years.
Local Income	Zip code-level median income in 1999 from the U.S. Census Bureau in 2000.

Variable	Definition
Unemployment	State-level change in the unemployment rate from loan origination to 24 months thereafter, reported by the Bureau of Economic Analysis.
Price Appr.	MSA-level house price index appreciation (in decimal) from loan origination to 24 months thereafter, reported by the Office of Federal Housing Enterprise Oversight (OFHEO).

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