The Job Rating Game: The Effects of Revolving Doors on Analyst Incentives

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Abstract

Investment banks frequently hire credit analysts from rating agencies. A widely held view is that this "revolving door" undermines analysts' incentives to issue accurate ratings. Using a hand-collected dataset of the performance and career paths of 245 credit rating analysts between 2000 and 2009, I show that the ratings by analysts who eventually move to investment banks are on average more accurate than the ratings by other analysts who rate similar securities at the same point in time. A notable exception is the small fraction of securities underwritten by their future employers, where revolving analysts do not outperform. Overall, my findings suggest that the revolving door may, *on average*, strengthen rather than distort analysts' incentives to issue accurate ratings.

Keywords: Revolving Door; Credit Ratings; Securitized Finance

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1. Introduction

Revolving doors – the possibility for monitors to be hired by the firms they monitor – are widespread in financial markets. Financial regulators join banks they oversee, risk-controllers join trading floors they monitor, and analysts join entities they evaluate. Despite their common occurrence, revolving doors are often seen as a source of governance failure, rather than as an efficient economic mechanism. A commonly voiced concern is that revolving doors make monitors overly sympathetic to the interests of the monitored. For example, Barney Frank claims that *"the notion that you would be critical of some entity and then hope they hire you goes against what we know about human nature"* (Wall Street Journal (2011)). The public's critical stance on revolving doors is further underscored by recent regulatory efforts aimed at reducing their potential adverse effects: the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 ("Dodd-Frank") requires credit rating agencies to disclose analyst transfers to entities they helped rate.[1]

While many observers view revolving doors as an economic distortion, ex-ante their net effect on monitoring performance is ambiguous. If monitors get hired as a quid pro quo for favors to their future employers or for their ability to influence their former colleagues (the "quid pro quo" view), they may be willing to give their future employers favorable treatment, or focus too much on building their network at the expense of their monitoring performance (Eckert (1981)). In contrast, if monitors are hired primarily for their expertise (the "human capital" view), they will have a greater incentive to invest in their industry qualifications or to signal their expertise during their employment as monitors (Che (1995), Salant (1995), and Bar-Isaac and Shapiro (2011)). Whether the human capital view or the quid pro quo view dominates is an empirical question. The answer has important implications for determining the optimal regulatory response, and more broadly, for understanding how concerns about future career prospects affect performance incentives.

[1]See section 932 of Dodd-Frank, which adds a new paragraph to section 15E(h)(5) of the Securities Exchange Act of 1934. Available on the SEC's website at `https://www.sec.gov/divisions/marketreg/ratingagency/wallstreetreform-cpa-ix-c.pdf`.

The main difficulty for empirical studies of revolving doors is that monitoring performance in the absence of the revolving door is unobservable. The performance of non-revolving monitors provides a useful counterfactual, but this cross-sectional approach poses additional challenges. First, it requires data on individual monitoring performance, which are generally scarce. Second, performance differences between revolving and non-revolving monitors can be confounded by unobserved factors. For example, comparing the performance of monitors across time is problematic due to cohort effects and time-varying task environments. In addition, even at the same point in time, monitors may be assigned to projects with different characteristics and levels of difficulty. Finally, there could be unobserved heterogeneity across individuals. For example, we may observe that revolving monitors outperform not because they work harder but because they are inherently smarter.

This study overcomes these empirical challenges by assembling a novel hand-collected dataset that tracks the career paths of 245 credit rating analysts at Moody's and links them to 24,406 ratings of securitized finance securities issued between 2000 and 2009, which covers the period prior to the Dodd-Frank regulation. In particular, I identify which analysts join an investment bank following their employment at Moody's. This empirical setting is ideal for studying revolving door effects for several reasons. First, credit ratings represent a publicly observable and relatively frequent measure of output quality by individual analysts. Subsequent corrections of the initial ratings issued by these analysts provide a useful proxy for analyst (in)accuracy. Another attractive institutional feature of Moody's organization is that subsequent rating adjustments are performed by a separate internal surveillance team and are therefore not under the influence of the analyst who assigned the initial rating. Having access to reliable measures of output quality is crucial for studying revolving door effects, and represents an important advantage of studying credit rating analysts compared to many public-sector functions, where individual output quality is difficult to assess. Second, I can analyze the net revolving door effect by comparing the performance of revolving and non-revolving analysts rating *similar* securities *at the same point in time*, while controlling for a rich set of observable and unobservable differences in the characteristics of these securities. Non-revolving analysts working at the same rating agency at the same point in time provide a useful counterfactual because they face the same organizational environment and similar tasks, objectives, and other career concerns. Third, rating analysts produce relatively more output signals than other professions, such as lawyers, who usually work on few cases during their career. I can therefore exploit changes in performance within the same individual over time in order to remove the influence of time-invariant heterogeneity across analysts.

Studying revolving doors in the context of credit analysts in securitized finance is economically relevant for two main reasons. First, the market for securitized finance is of first-order economic importance with more than $10 trillion of outstanding debt in the U.S. by the end of 2012, which is 1.4 times the size of the U.S. corporate bond market.[2] Distortions in the incentives of analysts rating these securities could therefore have economically sizable consequences. Second, inflated credit ratings of securitized finance products have been identified as being at the root of the last financial crisis,[3] and have at least partially been attributed to the revolving door between rating agencies and investment banks.[4]

My findings are broadly consistent with the human capital view of revolving doors. Analysts who subsequently depart to investment banks are significantly more accurate than other analysts rating similar products at the same point in time. This outperformance is stronger as revolving analysts get closer to their transition and as they rate more complex deals. My results are robust to various alternative measures of ratings accuracy, including a measure based on realized tranche losses. Further tests exploiting the cross-section of securities rated by revolving analysts show that the effect of the revolving door is not unambiguously positive. Consistent with a bias of revolving analysts in favor of their future employers (see Cornaggia, Cornaggia, and Xia (2016)), they underperform on the securities underwritten by their future employers during the last year of their employment. However, given that these cases are relatively rare and future employer

[2] Securities Industry and Financial Markets Association (SIFMA); reports available at `http://www.sifma.org`.

[3] The Financial Crisis Inquiry Commission (2011) concluded that "the failures of credit rating agencies were essential cogs in the wheel of financial destruction. The three credit rating agencies were key enablers of the financial meltdown. The mortgage-related securities at the heart of the crisis could not have been marketed and sold without their seal of approval."

[4] See, for example, Wall Street Journal (2011) and Bloomberg News (2015).

securities represent less than 10% of all securities rated by revolving analysts, they do not lead to economically sizable distortions in their aggregate performance.

If the likelihood of getting an investment banking job is positively related to overall analyst performance, does this induce analysts to exert greater effort while they are at the rating agency? A second test suggests it does. In this test, I exploit time-variation in the expected availability of investment banking jobs as an exogenous shock to analysts' likelihood of being hired by an investment bank. More specifically, I study announcements of future expansions in the set of underwriting investment banks across different collateral groups. Consistent with the prediction that aspiring investment bank analysts work harder when opportunities in this sector arise, average analyst performance improves around these announcements. Moreover, in the cross-section of analysts, the improvement in performance is concentrated among analysts who are ex-ante more likely to switch careers. These pronounced cross-sectional differences rule out the possibility that the change in performance could be induced by changes in the fundamentals of the affected collateral group, which would affect the performance of all analysts.

Overall, my findings suggest that revolving doors may *on average* lead to improved, rather than reduced, monitoring performance. This may explain why, despite the frequently voiced concerns, revolving doors have remained open in most professions. My results also imply that conflicts of interest arising from revolving doors are unlikely to have been a major driver of poor ratings quality in securitized finance prior to the financial crisis, despite the claims by regulators and the public press. On the contrary, they suggest that the option to switch to a career in investment banking may represent a strong incentive for credit analysts to perform well, and that restricting the revolving door without changing other aspects of analyst compensation may lead to lower ratings quality. An excessive regulatory focus on conflicted *individual* analysts may further be detrimental if it shifts the regulator's attention away from addressing first-order drivers of poor ratings performance in securitized finance.[5]

[5]The academic literature has, for example, pointed to distortions created by the "issuer pays" business model of credit rating agencies, such as an excessive focus on issuer relationships (He, Qian, and Strahan (2012), Efing and Hau (2015)), rating shopping (Benmelech and Dlugosz (2009), Mathis, McAndrews, and Rochet (2009), He, Qian, and Strahan (2016)), and rating catering (Griffin, Nickerson, and Tang (2013), He, Qian, and Strahan

There is surprisingly little systematic evidence on revolving doors, given the public interest and regulatory concern for the topic. The few existing studies on the career concerns of financial analysts have focused on the quid pro quo view. The study most closely related to mine is Cornaggia, Cornaggia, and Xia (2016), who document that corporate bond ratings of companies who hire former credit rating analysts are inflated prior to the employment transfer. My study is consistent with their results on the subset of securities that are underwritten by transitioning analysts' future employers, but shows that (i) potentially conflicted transitions represent a relatively small subset of all transitions to underwriting investment banks, (ii) future employer securities represent a small share of all securities rated, and (iii) revolving analysts exhibit a significantly higher accuracy on the large share of securities that are unrelated to their future employer. For sell-side equity analysts, Cohen, Frazzini, and Malloy (2012) report that analysts who get appointed as independent directors are overly sympathetic to management and poor relative performers, and Lourie (2014) finds that analysts who get hired by a firm they cover become more optimistic about their future employer, while becoming more pessimistic about other firms. Horton, Serafeim, and Wu (2015) document that banking analysts exhibit a stronger pattern in issuing optimistic forecasts at the beginning of the year and pessimistic forecasts at the end of the year when they are forecasting earnings of potential future employers. Studies of revolving doors in other contexts report mixed results. Whereas existing evidence supports the quid pro quo view for other regulators (e.g., Blanes i Vidal, Draca, and Fons-Rosen (2012), Bertrand, Bombardini, and Trebbi (2014)), the first studies of revolving doors for financial regulators are inconsistent with quid pro quo, documenting that regulatory lenience is associated with reduced prospects of finding employment in the financial sector (Agarwal, Lucca, Seru, and Trebbi (2014), Lucca, Seru, and Trebbi (2014), deHaan, Kedia, Koh, and Rajgopal (2015)). In addition, Forster and Shive (2016) show that financial firms take significantly less risk after hiring former regulators, consistent with the human capital view of revolving doors.

(2016)). In addition, interactions of the business model with the lack of investor sophistication (Skreta and Veldkamp (2009), Bolton, Freixas, and Shapiro (2012)), regulatory arbitrage (Opp, Opp, and Harris (2013)), and the business cycle (Bar-Isaac and Shapiro (2013)) have been identified as potential drivers of poor ratings quality in securitized finance.

2. Theory, Empirical Approach, and Data

2.1. Theoretical Framework and Key Predictions

To fix ideas, this section describes a theoretical framework that illustrates the human capital view of revolving doors and predicts the main effect that I document in this paper.[6]

2.1.1. Theoretical framework

The starting point for my theoretical framework is a partial equilibrium model featuring heterogeneous analysts who work at a credit rating agency and a revolving door between the rating agency and an investment bank. In the absence of the revolving door, analysts choose their effort based on a trade-off between the expected payoff at the rating agency and the cost of exerting effort, as in the standard principal-agent framework (Berle and Means (1932), Jensen and Meckling (1976)).

Following Che (1995), the revolving door is modeled as a positive probability that, after their term at the rating agency, analysts can be hired by the investment bank. Under at least two different circumstances, the revolving door has a positive effect on the ex-ante incentives of analysts to exert effort while they are employed at the credit rating agency (see Che (1995)). In the first case, qualifications acquired at the rating agency may increase analysts' expected payoff in a future investment banking career, as in Bar-Isaac and Shapiro (2011). This generates a greater incentive for rating analysts who want to pursue an investment banking career to invest in such qualifications during their employment at the rating agency. The assumption can be justified by anecdotal evidence that expertise in rating securitized finance securities is very valuable to investment banks (see, for example, Financial Times (2007)). In the second case, analysts can increase the probability of being hired by an investment bank through signaling, which also increases their ex-ante incentives to exert effort, as in Che (1995).[7]

[6] I provide a more formal model in the Internet Appendix.

[7] In order to reflect the possibility that investment banks may not be able to observe analyst performance, the

2.1.2. Key predictions

The above two cases predict that analysts perform better at the rating agency in the presence of the revolving door. A main challenge for empirical studies of revolving doors is that the counterfactual – analyst performance in the absence of the revolving door – is unobservable. Existing empirical studies have therefore resorted to using non-revolving monitors as a natural control group (see, for example, Cohen (1986), Spiller (1990), Cornaggia, Cornaggia, and Xia (2016), and deHaan, Kedia, Koh, and Rajgopal (2015)).[8] Following this literature, testing the average difference in the performance of revolving and non-revolving monitors will be the focus of my main tests. A central prediction of the human capital view, contrary to quid pro quo, is that revolving analysts outperform their non-revolving counterparts. Section 2.2 discusses the empirical implementation of this first set of tests and Section 3 presents the results.

In addition to the relative performance of revolving and non-revolving analysts, the human capital view also makes a prediction regarding how changes in the probability of being hired by the investment bank affect analyst performance. Specifically, an increase in the likelihood of being hired by the investment bank increases analysts' expected utility in a subsequent investment banking job. As I derive more formally in the Internet Appendix, this induces analysts to exert additional effort at the rating agency, thereby leading to an improvement in average analyst performance. Since my theoretical framework features analysts with heterogeneous ability, I can further derive which type of analysts are expected to react more strongly in their performance to changes in the investment banking opportunity set. In particular, there exists a group of low-ability analysts whose performance is insensitive to changes in investment banking opportunities, as they find it unattractive to pursue an investment banking career regardless of the scenario.

theoretical framework presented in the Internet Appendix follows the first scenario, as in Bar-Isaac and Shapiro (2011), by assuming that analysts' expected payoff in an investment banking job, but not their probability of being hired, increases in their effort at the rating agency.

[8]This cross-sectional approach implicitly assumes that analysts are heterogeneous. An obvious source of heterogeneity that is also featured in my model in the Internet Appendix is dispersion in innate ability. If, in addition, switching careers is costly as in Bond and Glode (2014), not all analysts may find it optimal to move to investment banking after their term at the rating agency, generating cross-sectional differences in the revolving door effect.

A test that exploits this second set of predictions is presented and explained in more detail in Section 4.

2.2. Empirical Approach

Testing differences in performance between revolving and non-revolving analysts poses at least two main challenges. First, it requires reliable measures of output quality at the individual analyst level. Second, differences in performance across analysts may be confounded due to potential non-random assignment of analysts to securities. This section describes how my empirical strategy addresses these two key issues.

2.2.1. Measuring rating performance

An important advantage of the rating-agency context is that individual output, i.e., ratings issued by the analysts and their subsequent performance, are observable. However, traditional metrics of ratings accuracy, such as average default rates by rating category or accuracy ratios (see Cornaggia, Cornaggia, and Hund (2016)), rely on a large number of sample events in order to be meaningful. Considering that a given analyst only rates a limited number of securities in each period and defaults are infrequent events, these measures may not be very reliable in gauging analyst-level performance. To circumvent this difficulty, I propose to exploit updated assessments of the expected future default probability by Moody's surveillance team as an alternative to realized defaults.[9] Specifically, I focus on instances where the surveillance team concludes that the initially assigned rating no longer reflects the expected default probability going forward, and adjusts the rating accordingly. The absolute difference between the initial rating and the subsequent rating by the surveillance team is therefore my main measure of ratings (in)accuracy.

Measuring ratings accuracy based on deviations between the initial rating and subsequent ratings has important advantages. First, it allows me to capture smaller changes in the expected

[9] The surveillance team is in charge of the ongoing monitoring of ratings at Moody's.

default probability that may not always lead to a default. Second, a very attractive feature of Moody's organization in structured finance is that rating surveillance is performed by a separate team.[10] Subsequent ratings are therefore very unlikely to be biased by the analyst who rated the security at issuance. It is worth noting that systematic mistakes by the surveillance team would affect the performance of *all* ratings and cannot bias my cross-sectional comparison. In addition, to the extent that rating updates are driven by the arrival of new fundamental information that is orthogonal to the analyst's information set at issuance, this would introduce noise in the measurement of analyst inaccuracy and bias me against finding cross-sectional differences in my subsequent analysis.[11]

A potential concern about defining ratings accuracy based on subsequent adjustments is that it represents an ex-post measure of performance and cannot be observed in real time. First, I show in the Internet Appendix that my main results are robust to measuring subsequent rating updates over various horizons, including short horizons such as one year. Second, there are good reasons to assume that investment banks may observe signals about analyst performance that are unobservable to the econometrician but highly correlated with ex-post measures of performance. For example, underwriting investment banks may, from direct interactions with the analyst during the ratings process, learn about his skill by observing the level of preparation, the type of information requested, and the quality of the questions raised. Even if investment banks do not directly interact with an analyst, they may still receive signals about his quality through their professional network, e.g., from conversations with other bankers who have directly worked with the analyst, his colleagues at Moody's, etc. Third, while it is plausible that investment banks are able to observe signals of analyst performance, it is not a necessary condition to predict

[10]Michael Kanef, former head of the Asset Backed Finance Rating Group at Moody's Investors Service, testified before the U.S. Senate in 2007 that "monitoring is performed by a separate team of surveillance analysts who are not involved in the original rating of the securities, and who report to the chief credit officer of the Asset Finance Ratings Group." His testimony is available on the website of the U.S. Senate at `http://www.banking.senate.gov/public/index.cfm?FuseAction=Files.View&FileStore_id=e9c1a464-a73b-417a-a384-41c15315f8c2`.

[11]In order to rule out the possibility that analysts may move between the ratings issuance and ratings surveillance functions, I also compute a measure of analyst performance using only subsequent rating actions that are performed by different analysts than the one responsible for the initial rating. Since there are very few exceptions to Moody's rule, I obtain a correlation coefficient of more than 98% between the two performance measures and a very similar baseline coefficient.

a positive incentive effect of the revolving door. As illustrated in my theoretical framework, a positive correlation between the skills acquired at the rating agency and analysts' expected future pay at the investment bank is sufficient to generate a positive revolving door effect, even if investment banks are not able to observe performance.

2.2.2. Comparing rating performance

Comparing ratings accuracy across analysts is non-trivial because they may be rating different types of products. For example, analysts often specialize in deals of one or a few collateral types, which may exhibit different trends in fundamentals and may be correlated with hiring intensity by investment banks.[12] In order to be able to compare analyst performance on a subset of securities that are similar in their economic fundamentals, I aggregate ratings (in)accuracy at the analyst × collateral type level instead of at the analyst level. This has the additional advantage that Moody's internal organization structure follows a similar division (see Appendix B), which ensures that analysts who rate securities of the same collateral type face similar incentives, rating methodologies, and management leadership. In sum, I define analyst inaccuracy as the absolute difference (in notches) between the initial rating and the subsequent surveillance rating, averaged across the set of securities $\mathcal{S}_{izt}$ rated by analyst i in collateral type z and semester t:[13]

$$Inaccuracy_{izt} = \frac{1}{N} \sum_{j \in \mathcal{S}_{izt}} |R_{j,t+h} - R_{j,t}| \,, \tag{1}$$

where $R_{j,t}$ refers to the initial rating of security j issued by analyst i in semester t, and $R_{j,t+h}$ refers to the rating of the same security at some future point in time, $t+h$. In my baseline definition,

[12] As shown in the Internet Appendix, there is a lot of heterogeneity in average rating performance across different collateral types.

[13] While aggregating across all securities rated by the same analyst in a given collateral type and semester reflects the idea that individual analysts are the main research subjects in this study and has the advantage of reducing the influence of outliers, it is also possible to run my subsequent analysis at the individual deal level. The results, reported in Table 4, are both quantitatively and qualitatively very similar. In addition, my results are robust to computing a value-weighted performance measure, where the weights are proportional to the security's principal amount (see Internet Appendix).

h will be equal to three calendar years.[14] Credit ratings are transformed into a cardinal scale, starting with 1 for Aaa and ending with 21 for C, as in Jorion, Liu, and Shi (2005). I can then implement the idea of comparing analysts rating securities of *the same underlying collateral type* at the *same point in time* by regressing my measure of analyst inaccuracy on collateral type × semester fixed effects (see Equation (2) below).

Even within a given collateral type and date, analysts may be assigned to securities with different characteristics, e.g., belonging to deals with complex subordination structures or poor collateral quality. In addition to collateral type × semester fixed effects, I therefore control for a rich set of observable tranche and deal characteristics. Specifically, I control for the logarithm of the combined principal value of all tranches in the deal ("deal size"); the geographical concentration of the collateral, measured as the sum of the squared shares of the top five U.S. states in the deal's collateral as in He, Qian, and Strahan (2016); the level of overcollateralization, computed as the difference between the total collateral value and the combined principal value of the tranches as in Efing and Hau (2015); the weighted average loan-to-value (LTV) ratio and the weighted average credit score of the underlying collateral at issuance; the logarithm of the average loan size; the weighted average life; the fraction of tranches with an insurance wrap; and the logarithm of the number of tranches in the deal. These characteristics are averaged across all securities rated by the analyst in a given collateral type and period.[15] Controlling for this rich set of tranche and deal characteristics takes into account that some securities might be harder to rate and systematically face larger rating adjustments than others.

In sum, for my main analysis the following regression is estimated:

$$Inaccuracy_{izt} = \lambda_{zt} + \delta IB\ Exit_i + \beta' X_{izt} + \epsilon_{izt}, \tag{2}$$

[14] In the robustness tests reported in the Internet Appendix, I consider rating adjustments over alternative horizons (one and five years) and find similar effects.

[15] Since information on some tranche and deal characteristics (specifically, the weighted average life, insurance wrap, geographical concentration, LTV ratio, credit score, and average loan size) are available only for a subset of my data, I follow He, Qian, and Strahan (2016) by replacing missing observations and including additional indicators equal to one if information on a given variable is not available. My robustness test in the Internet Appendix shows that the approach of replacing and controlling for missing observations does not materially affect my results.

where $Inaccuracy_{izt}$ stands for analyst inaccuracy as computed in Equation (1), λ_{zt} for collateral type × semester fixed effects, and $IB\,Exit_i$ is an indicator equal to one for analysts who subsequently join an investment bank. Vector X_{izt} includes the average tranche and deal characteristics listed above. Note that since the dependent variable is analyst inaccuracy, the human capital view predicts $\delta < 0$ in the above regression.

2.2.3. Can individual analysts influence ratings?

A necessary condition for revolving doors to affect analyst performance is that the ratings process for securitized finance products needs to provide sufficient room for individual analysts to affect the final rating of a security. This is not obvious given that the final rating decision is taken by a committee. Upon receiving a rating application from a potential customer, Moody's assigns a lead analyst to the ratings process. The lead analyst meets with the customer to discuss relevant information, which he subsequently analyzes with the help of Moody's analytical team. He then proposes a rating and provides a rationale to the rating committee, which consists of a number of credit risk professionals determined by the analyst in conjunction with the committee chair. Once the rating committee has reached its decision, Moody's communicates the outcome to the customer and publishes a press release.[16] The ratings process at Moody's therefore provides ample opportunities for individual analysts to influence the final rating, even if the final decision is taken by a committee. Lead analysts guide meetings with the customer, request and interpret information, and play a key role in the rating committee by proposing and defending a rating recommendation based on their own analysis. In addition, the rating committee chair serves a special role by influencing the composition of the rating committee and acting as the moderator.

How much individual analysts are able to influence ratings is ultimately an empirical question. Fracassi, Petry, and Tate (2016) attribute a substantial part of the variation in corporate bond ratings to individual analysts: they explain 30% of the within-firm variation in ratings. For

[16]See `https://www.moodys.com/sites/products/ProductAttachments/mis_ratings_process.pdf` for a description of the ratings process at Moody's.

securitized finance ratings, Griffin and Tang (2012) provide evidence that CDO ratings by a major credit rating agency frequently deviated from the agency's main model, reflecting room for subjectivity in the ratings process. Note that if individual analysts played no role, this would bias me against finding any significant differences across analysts.

2.3. Data

An important implication of the human capital view illustrated in Section 2.1 is that the revolving door positively affects ex-ante analyst effort and, hence, *all* ratings issued by revolving analysts. Focusing on the performance of revolving analysts in interactions with their future employers only, an approach used in some previous studies, may therefore underestimate the positive effects of the revolving door on analyst performance. The reason is that *all* securities may benefit from revolving analysts signaling or investing in their expertise, but potentially only *few* securities are helpful to curry favors to prospective employers. Hence, gauging the *net* effect of revolving doors requires analyzing the entire spectrum of securities rated by revolving analysts. In addition, the dataset should have two main features. First, it needs to be a dataset with performance measures at the individual analyst level. Such a dataset is not readily available, either for monitors in general or for credit analysts in particular.[17] To overcome this problem, I hand-collect a novel dataset that links individual analysts to the performance of the ratings they issue. Second, it is necessary to identify analysts who leave to investment banks after their employment at the rating agency. I collect this information from analysts' self-reported profiles on the professional networking website LinkedIn and from web searches. The full dataset is described in more detail below.

My sample consists of all non-agency securitized finance securities issued in the U.S. and reported in SDC Platinum. Additional deal and tranche information is manually collected from Bloomberg. I restrict my sample to all issues between 2000 and 2009 that were initially rated

[17]Standard databases on corporate and securitized finance credit ratings (e.g., Mergent FISD, Bloomberg, or SDC Platinum) do not provide the identity of the individual analysts responsible for a given rating.

by Moody's, because (i) data are sparse prior to 2000, (ii) my goal is to study the analyst labor market prior to the Dodd-Frank regulation, and (iii) Moody's publicly discloses analyst names in press releases of a new rating action on its website.[18] Most of the times, Moody's press releases list two names, one more junior employee (typically the lead analyst), and one more senior employee (typically the rating committee chair).[19] In addition to the analyst names, I also collect data on subsequent rating changes for each security from Moody's website.

The securitized finance data are complemented with hand-collected biographical information from web searches, in the vast majority of cases from analysts' public profiles on LinkedIn. In particular, I gather information on the date when the analyst left Moody's and the identity of his first employer following his employment at Moody's, as well as information on previous employment, graduate, and undergraduate education. I am able to track the career paths of 245 out of 268 analysts. As shown in Table 1, Panel B, 67 out of these 245 analysts subsequently go work for an investment bank that was ranked in the prestigious "The Bloomberg 20" ranking in the year prior to their exit,[20] 94 analysts leave to other employers, and 84 analysts have not left Moody's as of December 2015. The number of analyst departures to investment banks increases steadily before the crisis, but declines dramatically after 2007 (see Figure 2, lower graph). The "Bloomberg 20" investment banks also capture a large fraction of the underwriting market in securitized finance: they underwrite 88% of the securities in my sample (see Table 1, Panel C). As shown in Table 2, analysts with fewer years of prior work experience, no graduate degree, an undergraduate degree from an institution located in New York City, and a non-law undergraduate degree are more likely to leave to an investment bank. Interestingly, graduates from Ivy League institutions are less likely to subsequently work for an investment bank, although this relationship is not statistically significant.

[18]I am able to find corresponding analyst information from Moody's website in 71% of the cases.

[19]As I show in the Internet Appendix, my results are not sensitive to dropping the rating committee chairs from the analysis and focusing only on the lead analysts.

[20]Since the ranking is only available from 2004 onwards and the composition of the ranked investment banks is fairly stable prior to 2008, I use the 2004 ranking to classify analyst exits prior to 2004. Figure 1 provides an overview of the top hiring banks in my sample. In the Internet Appendix, I show that my main findings are robust to alternative definitions of investment banks.

As reported in Table 1, Panel A, my final dataset consists of 24,406 tranches from 4,979 securitized finance deals. All securities combined account for an aggregate issuance volume of ca. \$2.7 trillion, which represents at least 40% and therefore a sizable fraction of the aggregate U.S. non-agency securitized finance deal volume over this period reported by the Securities Industry and Financial Markets Association (SIFMA).[21] Using similar categories as in Griffin, Lowery, and Saretto (2014), I classify securities depending on the type of the underlying collateral into eight collateral groups and three broad market segments (asset-backed securities (ABS), mortgage-backed securities (MBS), and collateralized debt obligations (CDO)). Classifying all securities by collateral type is important for my empirical approach of comparing analyst performance within collateral type and date.

I also identify instances where analysts rate securities underwritten by their future employers by manually matching the name of the analyst's subsequent employer to the lead underwriting banks of the security reported in SDC Platinum. Half of the analysts who join an investment bank rate their future employer at some point during their employment at Moody's, but only 21% (= 14/67) do so during their last year at the rating agency (see Table 1, Panel B). Overall, future employer securities represent only 8.7% of all securities rated by the average revolving analyst.[22]

Table 1, Panel C, reports descriptive statistics of my sample. On average, ratings issued during my sample period are adjusted by 2.69 notches over a three-year horizon. There is a high degree of heterogeneity in rating adjustments, with a median of zero and a standard deviation of 4.95 notches. As shown in the Internet Appendix, I find very little evidence of statistically significant differences between the securities rated by revolving and non-revolving analysts along the tranche and deal characteristics in Equation (2). This reduces potential concerns that the securities rated by these two groups of analysts may differ on some unobserved dimension.

[21] Since SIFMA does not report agency asset-backed securities separately, I compute the aggregate deal volume as the sum of \$4.5 trillion of non-agency mortgage-backed securities and \$2.3 trillion of asset-backed securities (agency and non-agency). Hence, the 40% represent a lower bound estimate of the covered market share.

[22] Conditional on rating the future employer, the share of future employer securities increases to 17.7%. I obtain very similar shares when computing the fraction of the total principal amount rated by the analyst that is underwritten by his future employer.

While my main tests below are designed to address identification issues, the upper graph of Figure 2 shows that two important insights emerge even from the raw data. The figure plots the average outperformance of analysts who depart to investment banks for five subperiods. Two important insights emerge from this graph. First, analysts who depart to investment banks issue ratings that require fewer subsequent adjustments than ratings issued by other analysts (ca. 0.9 notches on average). Second, the observed outperformance of revolving analysts is strongest, in absolute terms, in the subperiod from 2006 to 2007, when the dispersion in ratings quality was the highest and departures to investment banks occurred most frequently. Hence, even the raw data are supporting the human capital view of revolving doors.

3. Main Results

This section presents my main results. I document that analysts who subsequently get hired by investment banks produce systematically more accurate ratings, as predicted by the human capital view of revolving doors. This relative outperformance gets stronger as analysts get closer to the transition, is robust to various measures of ratings accuracy, and is larger for complex securities where analyst skill should matter more.

3.1. Baseline Results

In order to compare the performance of revolving and non-revolving analysts, I first compute analyst inaccuracy in a given collateral type and semester as the average number of adjustments that are made to the ratings issued by analyst i in collateral type z and semester t (see Equation (1)). Then I estimate the regression in Equation (2). In addition to average deal characteristics, I also control for the logarithm of the total number of deals rated by analyst i in collateral type z and semester t, the logarithm of one plus the analyst's tenure at Moody's (in semesters), and the fraction of tranches underwritten by investment banks rated in "The Bloomberg 20" ranking,[23] as well as the average issuer market share.[24] All variables are defined in Appendix A. Standard errors are clustered at the analyst level.

Table 3, Panel A, reports the results. Confirming the results from the simple sorts presented in Figure 2, analysts who leave Moody's to go work for an investment bank are on average 0.54 notches more accurate than other analysts rating securities of the same collateral type and in the same semester (see column (2)). In columns (3) and (4), I focus on revolving analysts' last year of employment at the rating agency, as this is the period where conflicts of interest may have been most likely to occur. Inconsistent with this concern, I find that the outperformance of transitioning analysts increases to 1.0 notches in the last year. This effect corresponds to 39% (= 1.036/2.69) of the average analyst inaccuracy and is therefore economically sizable.

One might argue that the possibility to go work for other attractive employers could be a perfect substitute for the possibility to be hired by an investment bank if the revolving door was restricted, with the additional advantage of ruling out potential conflicts of interest. To test this conjecture, I also study the relative performance of analysts who depart to other employers. As shown in Panel B of Table 3, I find that analysts who depart to other employers do not compare as favorably to their peers in terms of performance. This could be explained by the fact that credit rating skill may be particularly valuable for tasks required by investment banks, such as structuring securitized finance deals ahead of public offerings (see Bar-Isaac and Shapiro (2011)), or that investment banks may have superior access to information about analysts' performance while they are employed at the rating agency.[25]

In sum, the results presented in this section show that analysts who subsequently get hired by investment banks systematically produce more accurate ratings, consistent with the human

[23]Griffin, Lowery, and Saretto (2014) show that securities issued by high-reputation investment banks have higher default rates.

[24]He, Qian, and Strahan (2012) show that a larger issuer market share is associated with worse tranche performance.

[25]Such a special role of investment banks may be justified by the fact that rating analysts in securitized finance work very closely together with underwriting investment banks, as illustrated by Cetorelli and Peristiani (2012).

capital view of revolving doors. In the following, I show that these results are robust to alternative measures of ratings accuracy, including a measure based on realized losses.

3.2. Robustness

Table 4 presents robustness tests for the main results presented in Table 3, Panel A, columns (2) and (4). Panel A investigates alternative measures of ratings accuracy and separates positive and negative rating adjustments. First, I measure ratings accuracy based on excess tranche losses, which dramatically reduces the sample size but still yields results of similar economic magnitude. Excess losses are computed as the absolute difference between the realized tranche loss and Moody's expected loss benchmark for the initial rating category (see Moody's Investor Service (2001)). While the statistical significance of the results is weak when excess losses are measured over a three-year horizon, it gets much stronger for a horizon of five years. This result is important because it uses a measure of rating performance that does not require any action on behalf of Moody's surveillance team, suggesting that the documented effect cannot be explained by subjectivity in the ex-post adjustment of ratings. Next, I show that ratings by revolving analysts experience both fewer downgrades and upgrades. The economic effect being symmetric for upgrades and downgrades is consistent with the conjecture that observed performance differences are driven by skill as opposed to bias. For downward adjustments, I can focus on the particular cases where securities are downgraded all the way to default – a rating action that is typically tied to hard events such as covenant violations (see Griffin, Lowery, and Saretto (2014)) and therefore less subjective than other rating adjustments. Securities rated by revolving analysts are significantly less likely to be downgraded to default.

Panel B studies different subperiods. Dividing the main sample period into two subperiods shows that revolving analysts significantly outperform non-revolving analysts both during the earlier and the later part of my sample period. Interestingly, they no longer outperform in the post-Dodd-Frank period (2010 to 2012). This result may hint at the possibility that the Dodd-Frank regulation or the associated public debate which stigmatized job transitions between rating agencies and investment banks has had adverse effects on analysts' performance incentives. However, an important caveat is that this period experienced very low issuance volumes, which makes it difficult to detect statistically significant differences in performance.

Panel C shows the results for alternative estimation methods. Specifically, the richness of my data allow me to augment the baseline specification in column (4) of Table 3, Panel A, by analyst fixed effects. The point estimate is virtually unchanged, suggesting that the documented performance difference between revolving and non-revolving analysts is not driven by time-invariant heterogeneity across analysts, such as baseline ability. Instead, time-varying differences across analysts, such as differences in analyst effort, are more likely explanations.[26] Next, I analyze the performance difference between revolving and non-revolving analysts at the individual deal level. The resulting estimates are very similar to my baseline.

I conduct additional robustness checks in the Internet Appendix. In sum, I conclude that my main results are robust to a large set of alternative measures of ratings accuracy and estimation methods.

3.3. Future Employers

It is possible that, despite their aggregate outperformance, revolving analysts underperform on a subset of securities that are underwritten by their future employers. In order to test for the presence of such a potential bias, I interact the *IB Exit* indicator with the fraction of tranches underwritten by the analyst's future employer. The results, reported in Table 5, suggest that, *on average*, revolving analysts do not perform differently when rating their future employers (see columns (1) and (2)). However, their performance starts to diverge during the last year of their employment at Moody's. The estimates in column (4) imply that in the extreme case where all securities are underwritten by the future employer, revolving analysts underperform by ca. 1.0 notches during the last two semesters.[27] This result is consistent with evidence reported by

[26]Section 4 explores analyst effort as a source of outperformance in more depth.
[27]This point estimate is not statistically different from zero.

Cornaggia, Cornaggia, and Xia (2016), who find that analysts are biased in favor of their future employers in the last quarters before their departure. However, my data suggest that this negative effect may not necessarily lead to economically sizable distortions. First, the underperformance on the future-employer securities is limited to the last year of the analysts' employment at the rating agency. Prior to the last year, analysts are significantly more accurate when rating their future employer (see column (4)). Second, instances where analysts rate their future employer shortly before their transition are relatively rare (14 out of 67 transitions, see Table 1, Panel B). Third, even conditional on these few cases, securities underwritten by the future employer represent the smaller share (17.7%) of all securities rated by the analysts. Hence, the reduced accuracy is largely outweighed by revolving analysts' outperformance on other securities.

3.4. The Influence of Deal Complexity

If revolving analysts outperform on average due to greater skill, then one would expect performance differences to become larger for deals that are more difficult to rate. This section tests this hypothesis by interacting my main independent variable of interest, $IB\,Exit$, with different measures of average deal complexity.

Table 6 reports the results for different proxies for deal complexity. All observations are sorted into quartiles by average deal complexity, and an indicator variable *High Deal Complexity* is defined as equal to one for observations in the top quartile, and zero otherwise. Following Furfine (2014), the first measure of deal complexity is deal size, measured as the total principal amount aggregated across all tranches of a given deal. The second measure is the number of tranches in the deal, as in He, Qian, and Strahan (2016) and Furfine (2014). The third is the number of loans in the underlying collateral.

All three measures are associated with significantly worse subsequent rating performance. More importantly, all measures also indicate that revolving analysts outperform more when they are rating more complex deals: high deal complexity increases their relative outperformance by between 0.3 to 1.2 notches. This is an economically sizable increase relative to the average outperformance of ca. 0.4 notches on non-complex deals. Overall, the results are consistent with the intuition that differences in skill should matter more for deals that are harder to rate.

4. Exploiting Changes in the Supply of Investment Banking Jobs

The results reported so far support the human capital view of revolving doors by showing that analyst performance is positively related to departures to investment banking jobs. While there is evidence of reduced ratings accuracy on the securities of analysts' future employers, potentially consistent with quid pro quo behavior, such conflicts are infrequent and affect a relatively small fraction of securities rated by transitioning analysts.

An important implication of the human capital view is that analysts have an incentive to exert additional effort in the presence of the revolving door. Hence, at least part of the observed outperformance prior to the transition to an investment bank could be driven by analysts working harder to develop their skills or to signal their ability. However, other explanations are conceivable. For example, it could be that revolving analysts outperform due to luck, or because they have been learning at a faster rate than their peers. It is worth pointing out that all of these alternative explanations are inconsistent with quid pro quo, and distinguishing among them may not be relevant from a policy perspective (see deHaan, Kedia, Koh, and Rajgopal (2015)). They all imply that open revolving doors are associated with better or at least equal rating quality.

To establish the existence of a positive effect on analyst effort, I exploit variation in the availability of investment banking jobs as an exogenous shock to analysts' likelihood of being hired by an investment bank. Most importantly for my analysis, changes in the investment banking opportunity set are likely orthogonal to individual analysts' baseline skill, learning paths, and other career concerns. As discussed in Section 2.1.2, my theoretical framework predicts that analyst performance should react positively to news about improved prospects of joining an investment bank. I use the announcement of a new investment bank starting to underwrite securities in a given collateral group as a positive shock to the supply of investment banking jobs in this particular market.[28] This provides a useful event for at least three reasons. First, prospectus filings are publicly observable and the entry of a new underwriting bank represents a discontinuous event that may signal heightened interest by investment banks in a given product area. Second, the event affects the employment prospects of analysts who rate securities in that particular collateral group disproportionally more than those of analysts who rate other products. I can therefore study how the performance of analysts in the affected collateral group changes relative to the performance of the control group. Third, it allows me to test whether, in the cross-section of analysts within the same collateral group, analysts with certain characteristics are more affected by the event than others. Specifically, my theoretical framework predicts that low-ability analysts and, more generally, analysts who are ex-ante less likely to leave to investment banks, should be less affected by fluctuations in the supply of investment banking jobs (see Internet Appendix). Exploiting these cross-sectional differences is important in order to rule out that my findings are a result of unobservable factors that are driving both investment bank entry and the overall rating performance in a collateral group, or by other changes that are directly induced by the entry of a new investment bank (e.g., underwriter competition, average analyst work load).

The following thought experiment illustrates my empirical approach. Consider two collateral groups, Student-loan ABS and Auto-loan ABS. Suppose now that an investment bank – called Goldman – starts to underwrite securities in Student-loan ABS but remains absent in Auto-loan ABS. My conjecture is that this event is going to increase the future employment prospects in investment banking jobs for analysts rating Student-loan ABS.[29] In contrast, and by construction,

[28] As announcement dates, I use the filing dates of prospectuses that list an investment bank as lead underwriter that has not previously been underwriting securities in this particular collateral group.

[29] Improved employment prospects may be driven by hiring by the entering investment bank – Goldman in the above example – as well as from other investment banks who may decide to follow or to defend their market share.

the prospects of future employment in Auto-loan ABS is not affected. I can therefore identify the impact of changes in the likelihood of being hired by an investment bank on analyst incentives by analyzing changes in the performance of analysts in Student-loan ABS and in Auto-loan ABS around the announcement of the investment bank entry.

To identify collateral group and semester observations where a new investment bank enters the underwriting market, I use the following approach. Using all non-agency U.S. securitized finance securities reported in SDC Platinum and assigning them to the eight collateral groups listed in Table 1, Panel A, I consider as an event all collateral group-semester observations where a prospectus is filed that lists as lead underwriter an investment bank that has not previously been underwriting securities in that collateral group.[30] This yields 18 investment bank entry events in 7 collateral groups. In order to verify that these events are indeed associated with increased prospects to be hired by an investment bank, Figure 3 plots the difference in the frequency of analyst departures to investment banks between the event group and the control group. The frequency of analyst departures jumps significantly following the announcement, suggesting that the entry of a new underwriter is indeed a good proxy for more aggressive hiring.

Next, I investigate whether average analyst performance reacts positively to the news about improved future employment prospects in investment banking. The following regression is estimated:

$$Inaccuracy_{izt} = \lambda_{st} + \lambda_i + \sum_{\tau=-3}^{\tau=+3} \delta_\tau I(New\,IB\,{}^{\tau}_{zt}) + \beta' X_{izt} + \epsilon_{izt}, \tag{3}$$

where $Inaccuracy_{izt}$ stands for analyst inaccuracy as computed in Equation (1), and λ_{st} and λ_i are market segment $\times$ semester and analyst fixed effects, respectively.[31] $I(New\,IB\,{}^{\tau}_{zt})$ is a set of seven event-time dummy variables labeled $\tau = -3$, $\tau = -2$, ..., $\tau = +2$, $\tau = +3$, where my convention is that dummy $\tau = 0$ takes on the value one in the collateral group and semester where a prospectus lists a new underwriting investment bank. Vector X_{izt} includes the same

[30] I restrict the set of potential new underwriters to all investment banks that appear in "The Bloomberg 20" investment bank ranking in the majority of the sample period.

[31] Since the event-time dummies do not vary within the same collateral type and semester, I only include market segment $\times$ semester fixed effects in this part of the analysis. See Table 1, Panel A, for the list of market segments.

set of control variables as in Table 3. If analyst incentives respond positively to variation in investment banking opportunities, then one would expect $\delta_\tau < 0$ for $\tau = 0$, and possibly for periods shortly following the announcement.

Table 7, Panel A, and the dotted line in Figure 3 report the results. Two things are worth noticing. First, analysts in the event group and in the control group perform very similarly in the pre-event window, alleviating potential concerns about unobserved differences between these two groups. Second, and more importantly, the performance of analysts in the event group reacts strongly and positively to the announcement of the investment bank entry, creating a gap of 0.8 to 1.0 notches vis-à-vis the control group in semesters $\tau = 0$ and $\tau = +1$. This pattern is consistent with analysts exerting additional effort when opportunities in the investment banking sector arise.

In order to rule out the possibility that the improvement in average performance is driven by other unobserved factors, I investigate whether the performance of some analysts reacts more strongly to the event than that of others. I use two criteria to identify analysts whose performance should be more sensitive to changes in investment banking opportunities under the human capital view. The first proxy uses the predicted values from the Probit regression of *IB Exit* on ex-ante analyst characteristics presented in Table 2, column (2), as a measure of the analyst's ex-ante likelihood of switching careers.[32] The second proxy is a measure of analyst baseline ability, constructed based on the analyst's average performance in the previous two years. The intuition for this proxy is that analysts with low innate ability never choose to apply for investment banking jobs because their expected returns would never be high enough to cover their career-switching cost.

I regress analyst inaccuracy on the indicator *New IB*, which is equal to one in the event semester $\tau = 0$. Then I use the two proxies described above to perform sample splits. Table 7, Panel B, reports results. First, the improvement in performance is concentrated among analysts

[32] I find very similar results if I use the predicted values from column (1) in Table 2, i.e., without controlling for cohort effects.

who are ex-ante more likely to leave to investment banking (columns (1) and (2)). Second, analysts with very weak past performance do not outperform the control group (columns (3) and (4)). The fact that the improvement in performance is concentrated in these subsets of analysts makes an omitted variable bias very unlikely. Overall, the results confirm my hypothesis that the observed improvement in analyst performance is driven by greater analyst effort in anticipation of employment opportunities in investment banking.

5. Extensions

This section investigates two potentially remaining concerns. First, could senior employees be more corrupted by the revolving door? Second, are analysts influenced by former work experience at investment banks?

5.1. The Role of Analyst Seniority

Despite the fact that the *average* revolving analyst outperforms, there could be substantial heterogeneity across analyst ranks. In particular, it would be problematic if outperformance by junior analysts were masking underperformance by the most senior members of Moody's organization. Given that Moody's press releases disclose both the name of the lead analyst and the name of a more senior member of the rating committee (typically the committee chair), my sample consists of Moody's employees of various ranks. This allows me to look at the relative performance of analysts prior to their departure to an investment bank as a function of their last job title at Moody's.

Table 8 reports the results. While the magnitude and statistical significance of the outperformance of revolving analysts varies across ranks, its sign always remains the same. In particular, I do not observe that the sign switches when I look at more senior ranks such as Senior Vice President or Managing Director. I therefore conclude that my main results are not specific to junior analysts.

5.2. Inbound Revolvers

In my last test, I study whether analysts with former ties to investment banks ("inbound revolvers") may be conflicted. Relative to the debate about potentially conflicted outbound revolvers, the role of inbound revolvers has attracted less attention in the context of rating agencies. Yet, it is not uncommon for Moody's to employ analysts with former work experience at investment banks: I identify 57 cases. Out of these 57 inbound revolvers, 22 rate their former employer at some point during my sample period.

Analogously to the analysis in Table 5 for outbound revolvers, I regress analyst inaccuracy on an indicator variable equal to one if the analyst has previously worked at a top investment bank (*Past IB*), and zero otherwise; the fraction of securities rated by the analyst that are underwritten by his former employer (*Past Employer*); an interaction term; and controls. Table 9 presents results. Analysts with former investment banking experience do not perform differently from other analysts. This rules out the possibility that the revolving door may negatively affect analyst incentives through inbound revolvers.

6. Conclusion

My paper contributes to the ongoing debate on whether revolving doors strengthen or distort monitoring incentives. I hand-collect a novel dataset that links 245 individual credit rating analysts at Moody's to their career paths and to the quality of the ratings they assign. In contrast with the generally negative view of revolving doors, I find that credit analysts who are subsequently hired by investment banks are more accurate than other analysts rating similar securities at the same point in time. A notable exception is the small subset of securities that are underwritten by their future employers where they do not outperform. The results suggest that, because only few ratings may be helpful to curry favors to future employers, but almost all ratings are helpful in signaling skill or building expertise, the positive effects of revolving doors can be economically sizable. They may also explain why, despite the frequently voiced concerns, revolving doors have remained open in most professions.

My paper also contributes to the debate about the sources of poor performance of securitized finance ratings prior to the financial crisis. Many observers have identified conflicted individual analysts as one of the drivers of poor ratings accuracy, and regulators have responded by imposing enhanced disclosure requirements on rating agencies in cases where employees transfer to a previously rated entity. My results imply that conflicts at the *individual* analyst level were unlikely a main driver of poor ratings performance and that, if anything, analysts may have performed better because of the possibility to be hired by an investment bank. Restricting the revolving door may therefore have the undesirable effect of discouraging rating analysts from developing and showcasing their expertise while employed at the rating agency.

While this paper focuses on the effects on performance incentives, the revolving door may affect rating quality through additional channels. For example, credit ratings quality may suffer if rating agencies systematically lose their more experienced or talented staff to investment banks, reducing their incentives to train new analysts (see Bar-Isaac and Shapiro (2011)). In addition, former analysts may help investment banks to game the rating system once they have left the rating agency.[33] On the other hand, there may be other positive aspects of revolving doors that I am not capturing in my analysis. For example, the option to move to investment banking may positively affect the quality of the pool of applicants at rating agencies, and many motivated applicants may no longer apply if career mobility is reduced. I leave the exploration of these additional channels to future research.

[33]Recent evidence reported by Jiang, Wang, and Wang (2016) supports this possibility.

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Table 1: Summary Statistics

The table presents summary statistics for my sample, which comprises all U.S. non-agency securitized finance deals rated by Moody's between 2000 and 2009 with information identifying the analyst(s) at issuance and information on the their post-Moody's employment status. Panel A shows the breakdown of securities by collateral type. Panel B provides an overview of the subsequent career paths of the analysts in my sample and the number of analysts who, at some point during their employment at Moody's, rate securities underwritten by their future employers. Panel C reports descriptive statistics of key variables. A complete list of variable definitions is provided in Appendix A.

Panel A: Sample

	Number of Tranches	Number of Deals	Issuance Volume ($bn)
Market Segment: ABS			
ABS Auto	1,929	539	433.06
ABS Card	487	246	184.55
ABS Home	4,540	891	406.93
ABS Other	4,826	1,089	566.80
ABS Student	146	40	23.15
Market Segment: MBS			
CMBS	537	65	71.67
RMBS	11,036	1,839	977.29
Market Segment: CDO			
CDO	905	270	66.68
Total	24,406	4,979	2,730.14

Panel B: Number of Analysts By Subsequent Career Path

				Other Exit			
	All	No Exit	IB Exit	Other Bank	Asset Mgr.	Insurer	Other
Number of Analysts							
Full sample	245	84	67	30	21	11	32
Rate future employer	33	0	33	0	0	0	0
Rate future employer in last year	14	0	14	0	0	0	0
Avg. Share of Future Employer Securities							
Full sample	3.6%	0.0%	8.7%	0.0%	0.0%	0.0%	0.0%
Conditional on rating future employer	n.a.	n.a.	17.7%	n.a.	n.a.	n.a.	n.a.

Panel C: Summary Statistics

	N	Mean	Std. Dev.	0.25	Median	0.75
Dependent Variables						
Avg. rating adjustments (in notches)	1,859	2.69	4.95	0.00	0.00	2.19
Avg. excess losses (in %)	513	6.34	6.83	1.02	3.99	9.56
Avg. downgrades (in notches)	1,859	2.61	4.99	0.00	0.00	2.10
Avg. defaults (in %)	1,859	6.63	16.84	0.00	0.00	0.00
Avg. upgrades (in notches)	1,859	0.05	0.21	0.00	0.00	0.00
Key Independent Variables						
IB Exit	1,859	0.24	0.43	0.00	0.00	0.00
IB Exit$_{t+1yr}$	1,859	0.07	0.26	0.00	0.00	0.00
Future employer	1,859	0.02	0.12	0.00	0.00	0.00
Control variables						
Tenure (in semesters)	1,859	5.93	6.07	2.00	4.00	9.00
Number of deals	1,859	4.86	9.06	1.00	2.00	5.00
IB Underwriter	1,806	0.88	0.27	0.93	1.00	1.00
Issuer market share (in %)	1,859	0.61	0.96	0.00	0.25	0.86
Weighted avg. life (in years)	1,748	4.97	2.43	3.21	4.44	6.15
Geographical HHI	990	0.34	0.06	0.30	0.33	0.36
Weighted avg. credit score	739	673.58	49.82	628.32	678.79	719.97
Weighted avg. LTV (in %)	925	67.41	11.63	64.33	69.25	72.88
Insurance wrap	1,746	0.02	0.07	0.00	0.00	0.00
Avg. loan size	896	15.83	79.78	0.98	2.46	5.82
Avg. deal size (in $m)	1,859	150.82	191.76	58.25	98.51	174.69
Number of tranches in the deal	1,859	7.86	5.45	4.77	7.00	9.71
Overcollateralization	1,859	0.06	0.08	0.00	0.00	0.00

Table 2: Predicting Analyst Departures to Investment Banks

The table reports the characteristics of analysts who depart to investment banks. *IB Exit* is an indicator equal to one if the analyst departs to an investment bank that was ranked in "The Bloomberg 20" ranking in the year prior to his departure, and is regressed on various analyst characteristics using a Probit model. *Prior Work Experience* refers to the logarithm of one plus the number of years of prior work experience, *Graduate Degree* is an indicator equal to one if the analyst has obtained a graduate degree prior to joining Moody's, *NYC Undergrad* indicates whether the analyst has obtained his undergraduate degree from an institution located in New York City, and *Ivy League* indicates whether the analyst has obtained his most recent degree prior to joining Moody's at an Ivy League institution. *Law Degree* and *Tech Degree* are indicators if the analyst's undergraduate degree is in law or in a technical field (mathematics / engineering / physics / computer science), respectively. In column (2), dummies indicating the calendar year of the beginning of the analyst's employment with Moody's are included. Robust t-statistics are reported in parentheses.

	IB Exit	
	(1)	(2)
Female	-0.306	-0.514
	(-0.89)	(-1.36)
Prior Work Experience	-2.913	-2.820
	(-3.56)	(-2.73)
Graduate Degree	-0.800	-1.094
	(-2.20)	(-2.57)
NYC Undergrad	0.844	1.186
	(2.00)	(2.01)
Ivy League	-0.592	-0.509
	(-1.41)	(-1.01)
Law Degree	-0.812	-1.067
	(-1.40)	(-1.91)
Tech Degree	0.041	0.342
	(0.11)	(0.71)
Cohort fixed effects	No	Yes
N	98	79
Pseudo-R^2	0.216	0.284

Table 3: Analyst Performance and Subsequent Career Outcomes

The table reports results from regressing analyst inaccuracy, measured as the average number of rating adjustments, on indicators for analysts who eventually depart from Moody's. Panel A presents results for departures to investment banks. In columns (1) and (2), *IB Exit* is an indicator equal to one if the analyst eventually departs to an investment bank that was ranked in "The Bloomberg 20" ranking in the year prior to his departure. In columns (3) and (4), *IB* $Exit_{t+1yr}$ is an indicator equal to one only during the last two semesters of the analyst's employment at Moody's. Panel B repeats the regression shown in Panel A, column (4) for analyst departures to other employers. All variables are defined in Appendix A. A table with coefficients for the complete set of control variables is shown in the Internet Appendix. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

Panel A: Departures to Investment Banks

	Avg. Rating Adjustments			
	(1)	(2)	(3)	(4)
IB Exit	-0.490	-0.540		
	(-2.71)	(-3.15)		
IB Exit_{t+1yr}			-0.961	-1.036
			(-2.29)	(-2.59)
Tenure	-0.262	-0.152	-0.241	-0.126
	(-2.54)	(-1.71)	(-2.39)	(-1.43)
Number of deals	0.108	-0.122	0.111	-0.121
	(1.64)	(-1.50)	(1.77)	(-1.53)
IB underwriter	-0.095	0.314	-0.106	0.307
	(-0.33)	(1.03)	(-0.37)	(1.01)
Issuer market share	0.298	0.124	0.299	0.124
	(4.20)	(1.83)	(4.12)	(1.81)
Deal controls	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
N	1,806	1,806	1,806	1,806
R^2	0.749	0.759	0.750	0.760

Panel B: Departures to Other Employers

	Avg. Rating Adjustments			
	Post-Moody's Employer			
	Other Bank	Asset Manager	Insurer	Other
	(1)	(2)	(3)	(4)
Exit_{t+1yr}	0.667	-0.369	0.534	-0.301
	(1.89)	(-0.41)	(1.16)	(-0.36)
Tenure	-0.131	-0.137	-0.139	-0.134
	(-1.48)	(-1.55)	(-1.58)	(-1.53)
Number of deals	-0.085	-0.096	-0.091	-0.096
	(-1.11)	(-1.24)	(-1.18)	(-1.22)
IB underwriter	0.335	0.327	0.335	0.334
	(1.09)	(1.09)	(1.09)	(1.09)
Issuer market share	0.115	0.114	0.114	0.115
	(1.65)	(1.64)	(1.64)	(1.65)
Deal controls	Yes	Yes	Yes	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
N	1,806	1,806	1,806	1,806
R^2	0.758	0.757	0.757	0.757

Table 4: Robustness

The table presents robustness tests. The baseline regression refers to columns (2) and (4) from Table 3, Panel A. For brevity, I only report coefficients of interest and suppress control variables. Economic effects are calculated as the reported coefficient divided by the mean of the dependent variable. Panel A tests alternative measures of analyst inaccuracy. *Excess losses* are computed as the absolute difference between the tranche's cumulative losses after three (five) years and Moody's expected loss benchmark for the initial tranche rating category. The next lines look at rating upgrades and downgrades separately. Securities are considered to be in *default* when Moody's assigns a rating below Ca within three years after issuance. Panel B presents coefficient estimates when restricting the data to different subperiods. Panel C tests alternative estimation methods. First, the specification presented in column (4) of Table 3, Panel A, is augmented by analyst fixed effects. In the second line, the main regressions are estimated at the individual deal level instead of at the analyst × collateral type × semester level. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

	IB Exit				**IB Exit$_{t+1yr}$**			
	Coeff	t-statistic	Econ. Effect	N	Coeff.	t-statistic	Econ. Effect	N
Baseline	-0.540	(-3.15)	-20.1%	1,806	-1.036	(-2.59)	-38.5%	1,806
Panel A: Alternative Measures of Analyst Inaccuracy								
Avg. excess losses (3 years)	-0.877	(-2.06)	-13.8%	511	-0.849	(-1.40)	-13.4%	511
Avg. excess losses (5 years)	-1.455	(-3.50)	-15.8%	655	-1.815	(-3.50)	-19.6%	655
Avg. downgrades (3 years)	-0.543	(-3.16)	-20.8%	1,806	-0.996	(-2.48)	-38.2%	1,806
Avg. defaults (3 years)	-1.290	(-1.90)	-19.5%	1,806	-2.450	(-1.83)	-37.0%	1,806
Avg. upgrades (3 years)	-0.019	(-1.85)	-34.4%	1,806	-0.033	(-3.05)	-60.4%	1,806
Panel B: Subperiods								
Pre-Dodd Frank I: 2000-2004	-0.263	(-2.94)	-66.5%	1,001	-0.389	(-3.13)	-98.3%	1,001
Pre-Dodd Frank II: 2005-2009	-0.755	(-2.32)	-13.4%	805	-1.360	(-1.70)	-24.2%	805
Post-Dodd Frank: 2010-2012	0.212	(0.48)	39.8%	143	0.802	(1.02)	151.0%	143
Panel C: Estimation Method								
Analyst fixed effects	n.a.	n.a.	n.a.	n.a.	-1.064	(-2.28)	-39.6%	1,757
Deal-level	-0.422	(-2.38)	-14.3%	4,332	-1.101	(-2.72)	-37.4%	4,332

Table 5: Rating Future Employers

The table reports results when analysts rate their future employers. The regression presented in Table 3, Panel A, is augmented by an interaction of *IB Exit* with the fraction of securities rated by the analyst that are underwritten by his future employer. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

	Avg. Rating Adjustments			
	(1)	(2)	(3)	(4)
IB Exit	-0.501	-0.536		
	(-2.52)	(-2.88)		
IB Exit × Future Employer	0.127	-0.046		
	(0.28)	(-0.10)		
IB Exit$_{t+1yr}$			-1.080	-1.165
			(-2.34)	(-2.71)
IB Exit$_{t+1yr}$ × Future employer			1.862	2.207
			(1.88)	(2.19)
Future employer			-0.663	-0.964
			(-1.44)	(-1.90)
Controls included	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
N	1,834	1,834	1,834	1,834
R^2	0.748	0.759	0.749	0.760

Table 6: The Influence of Deal Complexity

The table presents results for interactions with proxies for high deal complexity. Average deal complexity is measured as the average combined principal amount of the deal, the average number of tranches in the deal, and the average number of loans in the underlying collateral. All observations are sorted into quartiles by average deal complexity, and an indicator variable, *High Deal Complexity*, is defined as equal to one for observations in the top quartile, and zero otherwise. In column (3), an additional indicator is defined for observations with a missing average deal complexity measure. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

	Avg. Rating Adjustments		
	Deal size	Number of Tranches	Number of Loans
	(1)	(2)	(3)
IB Exit	-0.385	-0.451	-0.372
	(-2.08)	(-2.38)	(-1.80)
IB Exit × High Deal Complex.	-0.599	-0.332	-1.226
	(-1.94)	(-1.25)	(-2.17)
High Deal Complexity	0.707	0.680	1.441
	(3.34)	(2.58)	(3.77)
IB Exit × High Deal Complex. Missing			-0.655
			(-1.47)
High Deal Complexity Missing			-0.929
			(-1.05)
Tenure	-0.146	-0.151	-0.141
	(-1.66)	(-1.72)	(-1.63)
Number of deals	-0.120	-0.131	-0.132
	(-1.50)	(-1.63)	(-1.68)
IB underwriter	0.274	0.332	0.290
	(0.92)	(1.09)	(0.94)
Issuer market share	0.090	0.124	0.115
	(1.40)	(1.87)	(1.81)
Deal controls	Yes	Yes	Yes
Collateral type × semester f.e.	Yes	Yes	Yes
N	1,806	1,806	1,806
R^2	0.761	0.760	0.763

Table 7: Exploiting Shocks to the Supply of Investment Banking Jobs

The table presents results from analyzing analyst inaccuracy, measured as the average number of subsequent rating adjustments, around the event where an investment bank enters a new collateral group as a lead underwriter. Panel A compares the inaccuracy of analysts in the event collateral group (i.e., the collateral group where the investment bank enters) and the inaccuracy of analysts in other collateral groups in the same market segment (control group) around the event. Analyst inaccuracy is regressed on a set of seven event-time dummy variables labeled $\tau = -3$, $\tau = -2$, ..., $\tau = +2$, $\tau = +3$, where my convention is that dummy $\tau = 0$ takes on the value one in the collateral group and semester in which an investment bank is listed as lead underwriter in a prospectus for the first time. Each column reports the coefficient on one of the seven dummy variables. Panel B focuses on the effect during the event semester $\tau = 0$ and performs sample splits. $New\,IB^{\tau=0}$ is an indicator equal to one for the event group in the event semester $\tau = 0$. $\widehat{Pr(IB\,Exit)}$ refers to the analyst's ex-ante predicted probability of leaving to an investment bank, estimated as the predicted values from the Probit model in Table 2, column (2), and is split at the median across all analysts in my sample. *Past Performance* is low if the analyst's average inaccuracy during the previous two years falls into the bottom quartile of all analysts in a given collateral type and semester. All regressions include market segment × semester fixed effects, analyst fixed effects, and the same controls as in Table 3. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

Panel A: Analyst Performance Around Investment Bank Entry

	Avg. Rating Adjustments						
	Event-time (τ)						
	-3	-2	-1	0	+1	+2	+3
New IB$^{\tau}$	-0.046	-0.273	-0.158	-0.807	-0.979	-0.051	-0.144
	(-0.12)	(-0.73)	(-0.50)	(-2.22)	(-3.18)	(-0.15)	(-0.47)
Controls included	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Segment × semester f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Analyst f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Analyst Performance by Subsample

	Avg. Rating Adjustments			
	$\widehat{Pr(IB\,Exit)}$		Past Performance	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
New IB$^{\tau=0}$	0.595	-1.183	0.920	-0.825
	(0.80)	(-2.06)	(0.85)	(-2.42)
Controls included	Yes	Yes	Yes	Yes
Segment × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	Yes	Yes	Yes	Yes
Chi^2 statistic	5.70		5.94	
p-value	0.017		0.015	
N	329	268	264	1,012
R^2	0.810	0.747	0.897	0.767

Table 8: Analyst Seniority at Departure

The table presents results for different subsamples of analysts who depart to investment banks, sorted by their most recent job title at the time of departure. The regression presented in Table 3, Panel A, column (4), is estimated with a modification to the variable *IB Exit*$_{t+1yr}$. In each column, *IB Exit*$_{t+1yr}$ is an indicator equal to one during the last two semesters of the analyst's employment at Moody's, conditional on the analyst having reached a given job title by the time of his departure. Job titles are obtained from press-releases on Moody's website. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

	Avg. Rating Adjustments				
	Analyst Job Title at Exit				
	Associate Analyst	Analyst / AVP	Vice President	Senior Vice President	Managing Director
	(1)	(2)	(3)	(4)	(5)
IB Exit$_{t+1yr}$	-0.477	-2.346	-1.511	-0.787	-0.300
	(-1.29)	(-2.95)	(-1.54)	(-2.22)	(-0.59)
Tenure	-0.147	-0.107	-0.116	-0.125	-0.120
	(-1.36)	(-0.92)	(-1.02)	(-1.13)	(-0.98)
Number of deals	-0.132	-0.164	-0.183	-0.168	-0.172
	(-1.65)	(-1.90)	(-2.10)	(-1.94)	(-1.97)
IB underwriter	0.272	0.276	0.295	0.238	0.243
	(0.80)	(0.72)	(0.80)	(0.65)	(0.62)
Issuer market share	0.110	0.201	0.167	0.180	0.183
	(1.55)	(2.68)	(2.26)	(2.48)	(2.48)
Deal controls	Yes	Yes	Yes	Yes	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes	Yes
Analyst f.e.	Yes	Yes	Yes	Yes	Yes
N	1,533	1,456	1,515	1,424	1,390
R^2	0.773	0.761	0.751	0.764	0.763

Table 9: Inbound Revolvers

The table presents results from regressing analyst inaccuracy on past investment banking experience. *Past IB* is an indicator equal to one if the analyst has worked for an investment bank prior to his employment with Moody's. *Past Employer* refers to the fraction of tranches that are underwritten by the analyst's past employer. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

	Avg. Rating Adjustments			
	(1)	(2)	(3)	(4)
Past IB	0.141	0.100	0.141	0.100
	(0.73)	(0.51)	(0.73)	(0.51)
Past IB × Past Employer	0.247	0.193	0.247	0.193
	(0.31)	(0.25)	(0.31)	(0.25)
Tenure	-0.254	-0.147	-0.254	-0.147
	(-2.36)	(-1.56)	(-2.36)	(-1.56)
Number of deals	0.157	-0.060	0.157	-0.060
	(2.21)	(-0.68)	(2.21)	(-0.68)
IB underwriter	-0.011	0.386	-0.011	0.386
	(-0.04)	(1.18)	(-0.04)	(1.18)
Issuer market share	0.281	0.113	0.281	0.113
	(3.67)	(1.61)	(3.67)	(1.61)
Deal controls	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
N	1,564	1,564	1,564	1,564
R^2	0.761	0.771	0.761	0.771

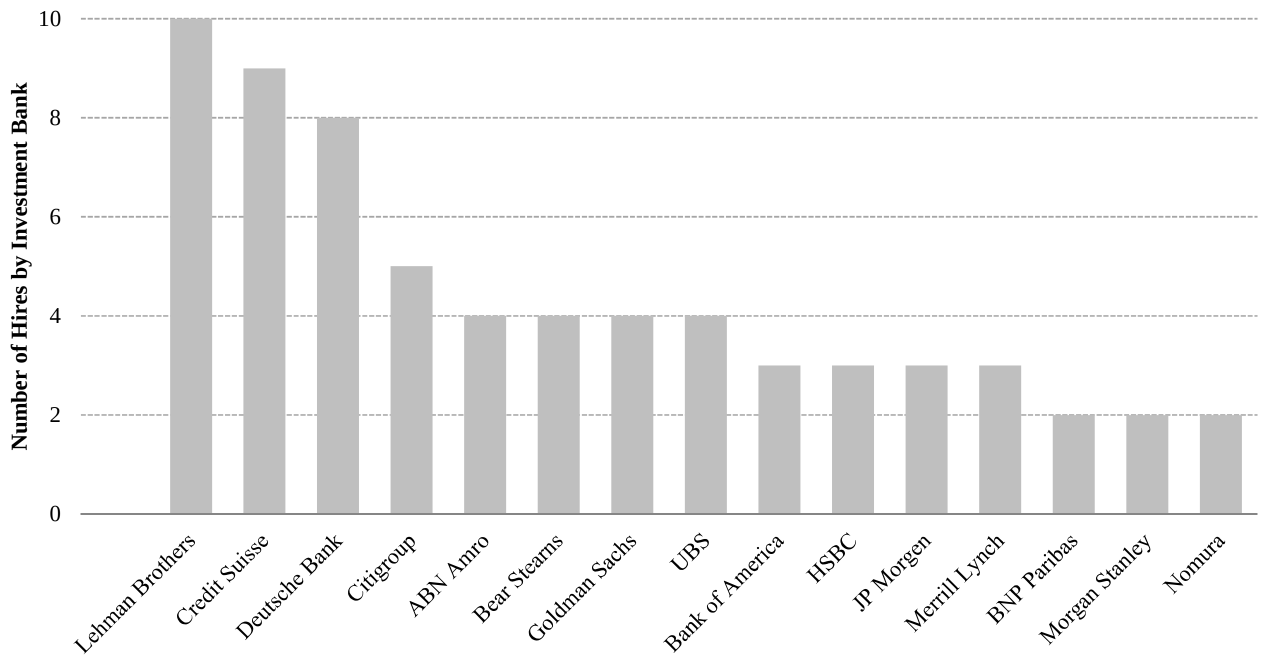

Figure 1: Number of Hires by Investment Bank. The graph plots the total number of Moody's analysts hired by each investment bank over the sample period. An analyst departure is classified as an exit to an investment bank if his subsequent employers was ranked in "The Bloomberg 20" ranking in the year prior to the analyst's departure.

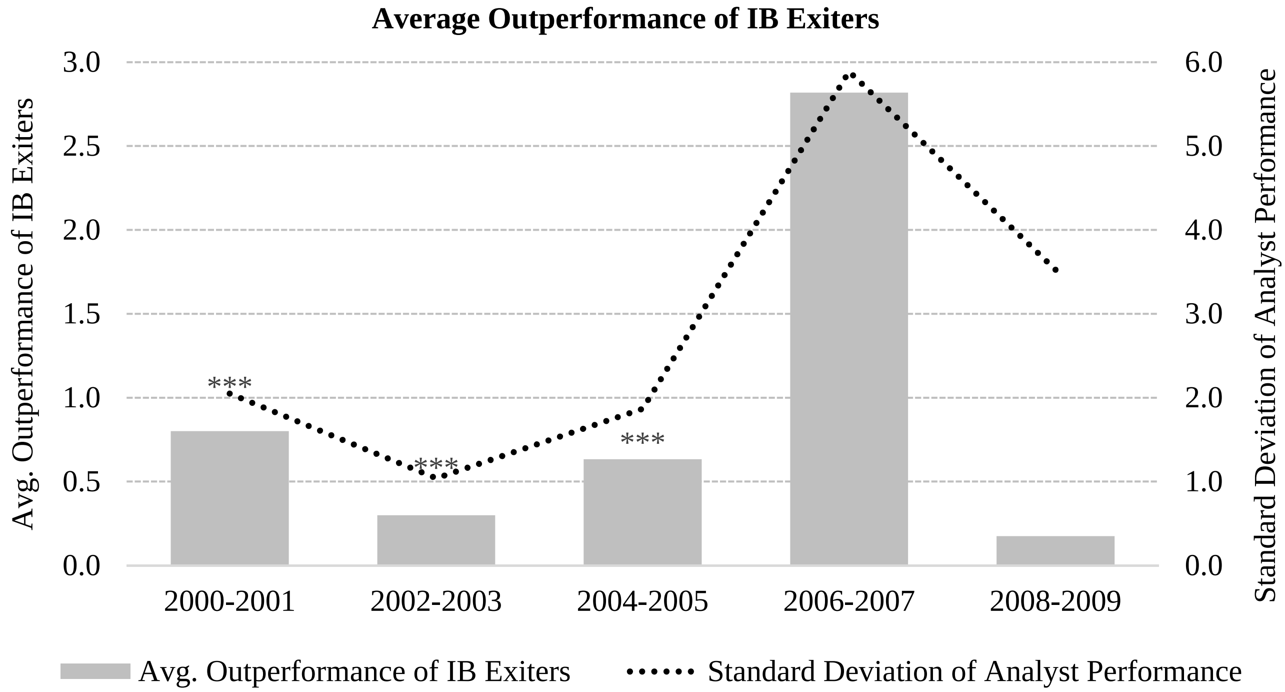

Figure 2: Departures to Investment Banks and Average Outperformance of Departing Analysts. The graphs plot the total number of analysts hired by investment banks (lower graph) and the average outperformance of departing analysts (upper graph) in each subperiod. Investment banks are all investment banks that were ranked in "The Bloomberg 20" ranking in the year prior to the analyst's exit. Outperformance is measured as minus one times the average difference in analyst inaccuracy, measured as in Equation (1).

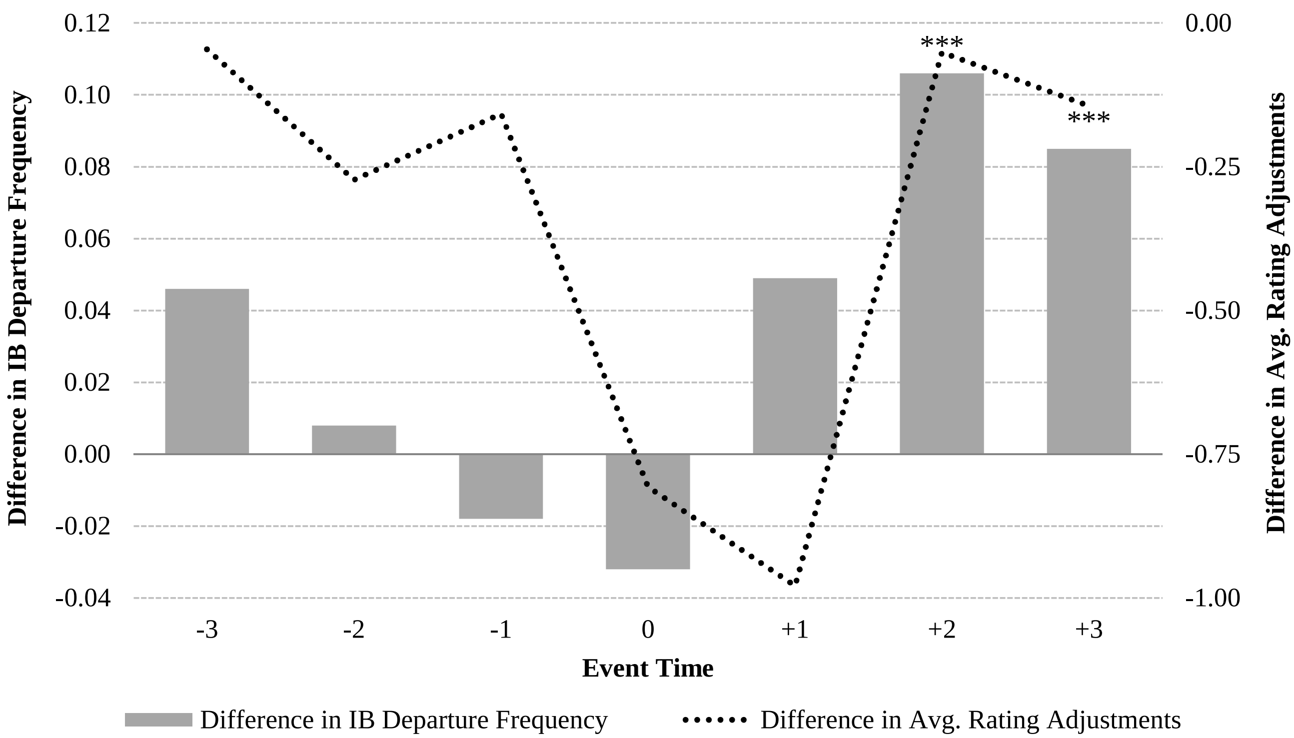

Figure 3: Event Study: Exploiting Variation in the Expected Supply of Investment Banking Jobs. The graph plots the frequency of analyst departures to investment banks and average analyst inaccuracy around the event where an investment bank is listed as lead underwriter in a given collateral group for the first time. The grey bars show the difference in the frequency of analyst departures to investment banks between the event group (i.e., the collateral group where the investment bank enters) and the control group in the window $(-3, +3)$ around the event. For each collateral type and semester, the departure frequency, measured as the number of analysts who depart to an investment bank within the next two semesters divided by the average number of analysts in the previous two semesters, is regressed on a set of seven event-time dummy variables labeled $\tau = -3$, $\tau = -2$, ..., $\tau = +2$, $\tau = +3$, where my convention is that dummy $\tau = 0$ takes on the value one in the collateral group and semester in which the investment bank entry occurs. Each column reports the coefficient on one of the seven dummy variables and asterisks ***, **, * indicate statistical significance on the 1%, 5%, and 10% level. The dotted line plots the coefficient estimates reported in Table 7, Panel A, i.e., the difference in the average number of rating adjustments between the event and the control group, over the same event window.

Appendix A. Variable Descriptions

Table A.1: Variable descriptions

Variable	**Description**
Measures of Analyst (In)Accuracy	
Avg. rating adjustments	The average absolute difference (in notches) between Moody's initial rating of the security and the rating three years following the issuance, averaged across all ratings issued by a given analyst in a given collateral type and semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website.
Avg. excess losses	The average absolute difference between the cumulative tranche losses, i.e., the principal balance write-offs due to default, after three years following the issuance and Moody's expected loss benchmark for the tranche's initial rating category, averaged across all ratings issued by a given analyst in a given collateral type and semester level by taking the arithmetic mean. Cumulative tranche losses are obtained from Bloomberg and Moody's expected loss benchmarks are retrieved from Moody's website (available at `https://www.moodys.com/sites/products/productattachments/marvel_user_guide1.pdf`).
Avg. downgrades	The average absolute difference (in notches) between Moody's initial rating of the security and the rating three years following the issuance if the initial rating is higher (otherwise it is set to zero), averaged across all ratings issued by a given analyst in a given collateral type and semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website.
Avg. upgrades	The average absolute difference (in notches) between Moody's initial rating of the security and the rating three years following the issuance if the initial rating is lower (otherwise it is set to zero), averaged across all ratings issued by a given analyst in a given collateral type and semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website.
Avg. defaults	The average fraction of securities in default within three years after issuance, averaged across all ratings issued by a given analyst in a given collateral type and semester level by taking the arithmetic mean. Securities are considered in default when Moody's assigns a rating below Ca. Rating adjustments are obtained from Moody's website.
Key independent variables	
IB Exit	Indicator function equal to one if the analyst departs to an investment bank following his employment at Moody's. Investment banks are employers that were ranked in "The Bloomberg 20" ranking in the year prior to the analyst's departure. Post-Moody's employer information is obtained from public profiles on LinkedIn and web searches.
IB Exit$_{t+1yr}$	Indicator function equal to one during the last two semesters of the analyst's employment at Moody's before he departs to an investment bank. Investment banks are employers that were ranked in "The Bloomberg 20" ranking in the year prior to the analyst's departure. Post-Moody's employer information is obtained from public profiles on LinkedIn and web searches.

Continued on next page

Table A.1 – continued

Variable	Description
Future employer	Fraction of tranches that are underwritten by the analyst's future employer. Lead underwriter information is obtained from SDC Platinum and manually matched with information on the analyst's post-Moody's employer obtained from public profiles on LinkedIn and web searches.
Control variables	
Tenure	Logarithm of one plus the number of semesters since the beginning of the analyst's employment at Moody's, which is the earlier date of the analyst's reported start date on LinkedIn and his first appearance in the dataset.
Number of deals	Logarithm of one plus the number of deals rated by the analyst in a given collateral type and semester.
IB Underwriter	The fraction of tranches rated by the analyst in a given collateral type and semester that are underwritten by an investment bank that was rated in "The Bloomberg Top 20" ranking in the year prior to ratings issuance. For ratings issued prior to 2005, the Bloomberg ranking from 2004 is used. Lead underwriter information is obtained from SDC Platinum.
Issuer market share	The average market share of the tranche issuer based on the dollar volume of deals across all collateral types originated in the previous calendar year, averaged across all tranches rated by the analyst in a given collateral type and semester.
Weighted avg. life	The number of years that are expected to elapse from the closing date until each dollar of the tranche's principal is repaid to the investor, averaged across all tranches rated by the analyst in a given collateral type and semester.
Geographical HHI	The average sum of the squared shares of the collateral within a deal across each of the five U.S. states with the largest aggregate amount of loans, with the aggregation of all the other states as the sixth category, averaged across all tranches rated by the analyst in a given collateral type and semester.
Weighted avg. credit score	The weighted average FICO score of the borrowers in the underlying collateral at issuance, averaged across all tranches rated by the analyst in a given collateral type and semester.
Weighted avg. LTV	The weighted average loan-to-value ratio of the loans in the underlying collateral at issuance, averaged across all tranches rated by the analyst in a given collateral type and semester.
Insurance wrap	The fraction of tranches rated by the analyst in a given collateral type that have a financial guaranty insurance.
Avg. loan size	The simple average of the loan amounts in the underlying collateral, averaged across all tranches rated by the analyst in a given collateral type and semester.
Deal size	The principal amount of all tranches belonging to a given deal at issuance, averaged across all tranches rated by the analyst in a given collateral type and semester.
Number of tranches	The number of tranches belonging to the same deal, averaged across all tranches rated by the analyst in a given collateral type and semester.
Overcollateralization	The difference between total collateral value and the combined principal value of the tranches at issuance, averaged across all tranches rated by the analyst in a given collateral type and semester.

Appendix B. Moody's Organizational Structure

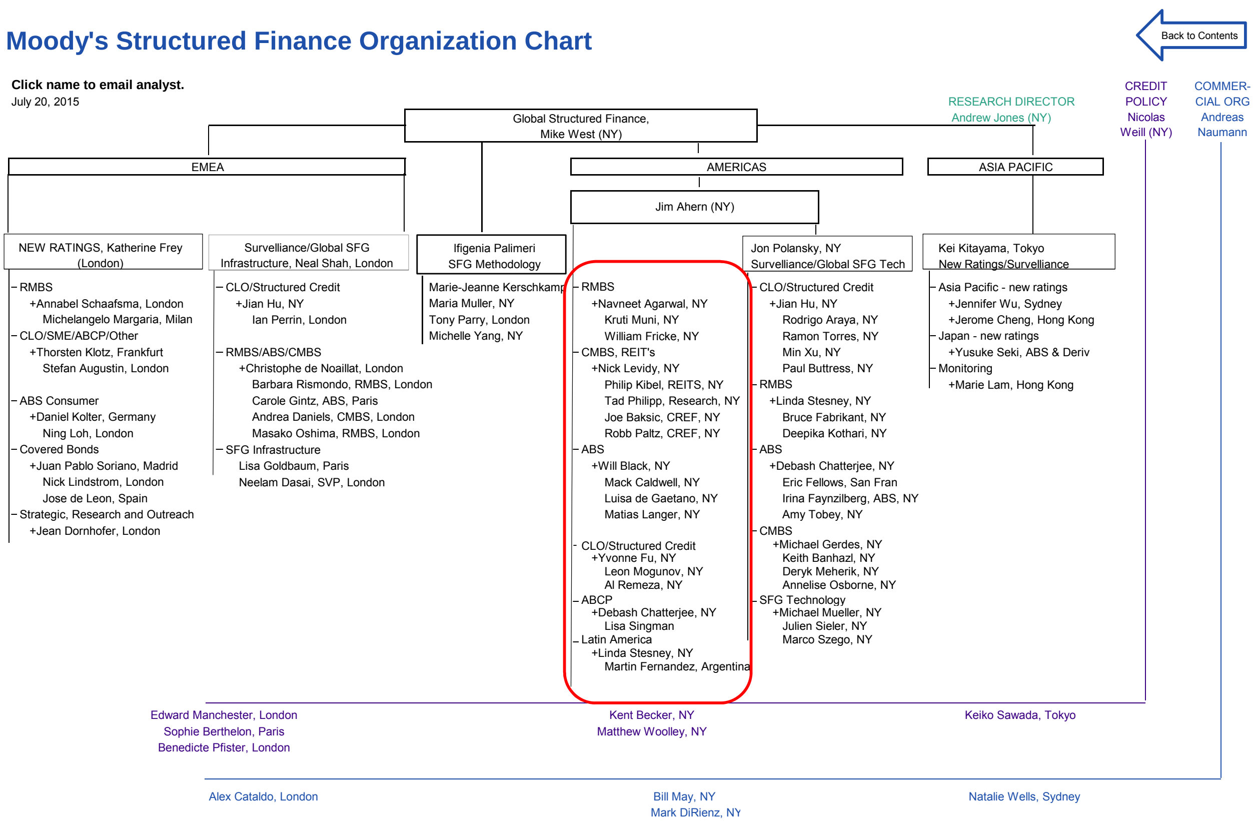

Figure B.1: Moody's Organizational Structure in Structured Finance. The chart shows the organizational structure of the Structured Finance team at Moody's as reported on Moody's website (available at `https://www.moodys.com/research/Structured-Finance-Ratings-Quick-Check-Newsletter--PBS_SF161380`). The red line highlights the division of interest for this paper, i.e., new ratings in the Americas region.