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

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**Abstract**

Investment banks frequently hire analysts from rating agencies. While many observers argue this "revolving door" undermines analysts' incentives to issue accurate ratings, this paper shows it more likely improves accuracy at the rating agencies. Using an original dataset that links employee performance and career paths, I find that credit analysts who issue more accurate ratings are more likely to be hired by investment banks. Optimism does not significantly improve analysts' prospects to be hired, except by investment banks whose issues they have recently rated. Overall, investment banks appear to reward analysts mainly for accuracy rather than favors.

Keywords: Revolving Door; Analysts; Credit Ratings; Securitized Finance

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# 1. Introduction

Revolving doors – the frequency at which monitors are hired by the firms they monitor – are widespread in financial markets. Bank regulators join institutions 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 economic distortion. A commonly voiced concern is that they make monitors overly sympathetic to the interests of the monitored.[1] One type of organization that has been criticized particularly heavily for being captured by the revolving door is credit rating agencies. In response, the Dodd-Frank Act of 2010 introduced new provisions that require credit rating agencies to disclose analyst transfers to entities they helped rate and to implement mandatory look-back reviews.[2] Using these new disclosures, recent research supports the revolving door concerns by showing that the credit ratings of firms who hire credit rating analysts are inflated prior to the employment transfer (see Cornaggia, Cornaggia, and Xia (2016)).

However, this may only be half the story. On the one hand, if monitors mainly 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), then they may indeed 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)). On the other hand, 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)).

Thus, the net effect of the revolving door is ex-ante ambiguous. Whether the human capital view or the quid pro quo view dominates is ultimately an empirical question. The answer has

[1]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)).

[2]See section 932 of the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 ("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`.

important implications for determining the optimal regulatory response, and more broadly, for understanding how concerns about future career prospects affect performance incentives.

I study whether rating analysts are hired by investment banks based on accuracy ("expertise") in the context of the securitized finance market. The market for securitized finance instruments is of first-order economic importance with more than $10 trillion of outstanding debt in the U.S. by the end of 2016, which is 1.2 times the size of the U.S. corporate bond market.[3] Analysts at rating agencies assess the relative credit risk of these products and are shown to have substantial influence on the final rating outcome (Fracassi, Petry, and Tate (2016)). Their decisions therefore have the potential to affect investor demand, e.g., through regulatory reliance on credit ratings (White (2010), Acharya and Richardson (2009), Acharya, Schnabl, and Suarez (2013)), prices (He, Qian, and Strahan (2012), Hand, Holthausen, and Leftwich (1992)), and investment decisions (Becker and Ivashina (2015)). In addition, the rating inflation of securitized finance products, documented by Benmelech and Dlugosz (2009) and Ashcraft, Pinkham, and Vickery (2010), has been identified as being at the root of the last financial crisis,[4] and has at least partially been attributed to the revolving door between rating agencies and investment banks.[5]

In particular, I compile a dataset that links the career paths of 245 credit rating analysts at Moody's to 24,406 ratings of securitized finance securities issued between 2000 and 2009.[6] Ca. 27% of the analysts in my sample join a prestigious investment bank immediately following their employment at Moody's. The dataset allows me to circumvent one of the most important difficulties for empirical studies of revolving doors: the availability of micro data on employee performance and career paths. 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 in-

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

[4]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."

[5]See, for example, Cornaggia, Cornaggia, and Xia (2016), Wall Street Journal (2011) and Bloomberg News (2015).

[6]The sample ends in 2009 in order to restrict the analysis to the period prior to the Dodd-Frank provisions.

stitutional 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 over many public-sector functions, where individual output quality is difficult to assess.[7] Even with micro performance data, a second empirical challenge is that observed performance may be confounded by differences in the task environment faced by different individuals. By comparing performance across analysts at the same rating agency for the *same type of product* and at the *same point in time*, my research design ensures that the compared individuals face the same organizational environment, task difficulty, objectives, and internal career concerns.

I find that the human capital view dominates. Analysts who are one-standard-deviation more accurate are 78% (5.5 percentage points) more likely to be hired by a prestigious investment bank than the average analyst who rates similar products at the same point in time. This result is robust to various alternative measures of ratings accuracy, including a measure based on realized tranche losses. In the large majority of cases, investment banks hire analysts with whom they did not have a direct interaction during the last year prior to the hire. Yet, when investment banks do hire analysts following a recent interaction, they appear to place a significant weight on optimism in addition to overall accuracy. The former could be indicative of leniency towards future employers and is consistent with the findings by Cornaggia, Cornaggia, and Xia (2016), who document an increase in rating optimism immediately prior to an employment transfer. However, investment banks continue to place a relatively higher weight on overall accuracy even when they hire analysts who have recently rated their products. In addition, given that analysts are almost four times more likely to be hired in the near future by an investment bank whose products they are not rating, the incentives to be accurate likely dominate incentives to inflate ratings.

[7]For example, while it is possible to link lawyers and judges to individual cases, or regulators and examiners to individual decisions, the optimal level of harshness or leniency is unknown. This makes it challenging to distinguish whether revolvers are hired on the basis of skill versus bias.

The main result documented in this paper is consistent with a model where investment banks hire analysts primarily based on observed rating performance. As long as being hired by an investment bank carries a reward and analyst effort positively effects performance, the revolving door will strengthen incentives at the credit rating agency. Still, a remaining question is how much of the observed performance is driven by ability versus effort. While fully disentangling the two effects may be less relevant from a policy perspective and is beyond the scope of this paper, initial evidence suggests that the effect of the revolving door on ex-ante effort may be sizable. For this purpose, I exploit the prediction of a simple hiring model that performance incentives increase when the probability of being hired by an investment bank increases, as long as this probability is not too high.[8] More specifically, I use announcements of future expansions in the set of underwriting investment banks across different collateral groups as a proxy for an increase in the probability of being hired by an investment bank. Consistent with the prediction, 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.

My findings suggest that the revolving door may *on average* lead to improved, rather than reduced, rating 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 the revolving door 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

[8] As further explained in Section 2, this prediction is valid as long as the hiring rate is below 1/2 (see Gibbs (1995)). Given the low average probability of being hired by an investment bank in a given period in my sample ($p = 7\%$), it is reasonable to focus on this particular case.

ratings quality.[9] 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.[10]

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 results are consistent with their findings when I restrict the definition of revolving analysts to analysts who have rated their future employers shortly prior to the transfer. However, this approach neglects the fact that, unconditionally, analysts are much more likely to join investment banks with whom they have not recently interacted, and that being accurate significantly increases their overall chances of advancing their careers. 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.

This paper is also related to a growing literature on regulatory capture (Stigler (1971), Peltz-

[9] Whereas Kisgen, Osborn, and Reuter (2016) find that internal promotions at Moody's are positively related to ratings accuracy, internal promotions in structured finance appear not to be significantly related to ratings accuracy.

[10] 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 (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.

man (1976), Shleifer and Vishny (1993)). Most empirical work on revolving doors in the public sector finds no or limited evidence of capture. For example, Agarwal, Lucca, Seru, and Trebbi (2014) and Lucca, Seru, and Trebbi (2014) find that regulatory lenience is associated with reduced prospects of finding employment in the financial sector. In addition, Forster and Shive (2016) show that financial firms take significantly less risk after hiring former regulators, consistent with the view that they are hired for their expertise in risk management. deHaan, Kedia, Koh, and Rajgopal (2015) find that harsher SEC lawyers are more likely to b hired by private law firms. On the other hand, recent research by Tabakovic and Wollmann (2017) documents evidence of capture by showing that U.S. patent examiners grant more patents to firms that hire them and extends this leniency to other firms in close geographical proximity. Finally, related work on lobbyists suggests that networks are valued more than expertise (Blanes i Vidal, Draca, and Fons-Rosen (2012), Bertrand, Bombardini, and Trebbi (2014)).

The results presented in this paper may also advance our understanding of why the above studies come to different conclusions regarding the sign of the revolving door effect. Studies which look at employment transitions without conditioning on an explicit interaction between the individual and the future employer (such as Agarwal, Lucca, Seru, and Trebbi (2014), Lucca, Seru, and Trebbi (2014), deHaan, Kedia, Koh, and Rajgopal (2015)) tend to find a positive revolving door effect. On the other hand, studies which condition on an explicit interaction (e.g., Cornaggia, Cornaggia, and Xia (2016), Tabakovic and Wollmann (2017), Cohen, Frazzini, and Malloy (2012), Lourie (2014)) tend to find evidence of capture. My study is one of the first to document the presence of both effects and to show that the human capital effect is likely of higher-order economic importance in the context studied in this paper.

# 2. Theoretical Framework and Key Predictions

## 2.1. Theoretical framework

To fix ideas, this section describes a parsimonious theoretical framework that illustrates the human capital view of revolving doors and derives the main predictions tested in this paper. The model reflects the idea formulated by Che (1995) that the revolving door may improve ex-ante performance incentives if future employers hire based on observed performance. The setup closely follows the promotion model by Gibbs (1995) and features heterogeneous analysts who work at a credit rating agency (CRA). The revolving door to the investment bank (IB) is modeled as an external promotion that entails a positive and permanent increase in the expected lifetime earnings of the analyst.

Analysts combine ability $\alpha$ and effort $e$ to rate projects. The cost of effort is $C(e)$, which is increasing in $e$ at an increasing rate so that $C'$ and $C'' \geq 0$. The performance measure is $y = \alpha e + \epsilon$, where $y$ is observed ratings accuracy (i.e., how accurately the analyst predicts the default probabilities of the projects she rates) and $\epsilon$ is measurement error, which is assumed to be distributed normally with mean zero, $\epsilon \sim N(0, \sigma_\epsilon^2)$. Ability is assumed to be distributed normally with expected ability $\bar{\alpha}$, $\alpha \sim N(\bar{\alpha}, \sigma_\alpha^2)$. Neither the CRA nor the IB nor the analysts know their ability ex-ante.

The investment bank would like to hire the analysts with the highest ability from the CRA and learns over time about abilities. This selection is modeled in a simplified way, by assuming that the IB sets a performance standard, $z$, and promotes all analysts whose observed performance measure exceeds this standard.[11] For now, it is assumed that all analysts who are offered a job at the IB are willing to switch because it is associated with an increase in expected lifetime utility equal to $\Delta EU$. In other words, I assume that other employers (including the CRA) are unable to compete with the IB on salaries. The probability of being offered a job at the IB, $p(e)$, is

[11] As discussed by Gibbs (1995) and Gibbs (1994), the implications of the performance standard rule are identical to those of a tournament version of the model where top performers are hired regardless of the level of their performance.

therefore equal to:

$$Prob(IBExit) = p(e) = Prob(\alpha e + \epsilon > z) = 1 - F(z), \tag{1}$$

where $F$ denotes the cumulative distribution of measured performance, which depends on the distributions of ability and measurement error.

The CRA pays a base salary $w_0$ but offers no pay-for-performance.[12] The expected utility maximized by the analyst is thus $U(e) = w_0 + p(e)\Delta EU - C(e)$, which leads to the following first-order condition:

$$C'(e^*) = p'(e^*)\Delta EU \tag{2}$$

Equation (2) illustrates the main positive effect of the revolving door. Analyst incentives at the CRA increase in the expected increase in lifetime earnings $\Delta EU$ as well as in the marginal effect of effort on the probability of being hired $p'(e^*)$. The second-order sufficient condition, $-C'' + p''\Delta EU \leq 0$, holds as long as probability of exit does not exhibit too large increasing returns to effort. Ideally, the IB would like to isolate signals about analyst ability and base the hiring decision only on those signals. However, in the absence of less coarse signals the IB is forced to hire based on the noisy performance measure, which makes it difficult to disentangle ability and effort. As long as being hired by the IB carries an increase in expected lifetime utility, the revolving door will increase analyst incentives, because exerting effort positively affects the chance of getting a job offer in Equation (1). Hence, by hiring high-performing analysts the IB strengthens incentives at the CRA, even if it did not intend this effect. This could potentially explain why pay for performance at the CRAs seems low-powered.

The marginal effect of effort on the probability of being hired is equal to the density function of observed performance $p' = f(z)$. Given normality assumptions, this can be written as

[12] According to former credit ratings analysts and self-reported compensation data from `www.glassdoor.com`, the use of performance pay at CRAs is very low, especially for individuals at the lower levels of the corporate hierarchy. Note that the revolving door can have positive incentive effects even in the presence of performance pay, as long as the investment bank is able to offer a compensation increase.

$\bar{\alpha}\phi(\cdot)/\sigma_y$, where $\phi(\cdot)$ is the standard Gaussian distribution. As discussed by Gibbs (1995), since the standard Gaussian distribution has a maximum at the 50th percentile, and is symmetric, $p'$ is largest for a hiring rate of 1/2. Intuitively, when the probability of being hired is exactly 1/2, an analyst is ex-ante on the margin of staying at the CRA versus being hired by the IB, and effort has the highest marginal effect on the chance of career advancement. If less than half of all analysts get hired by the IB, $p'$ increases in the probability of being hired, and the reverse is true if more than half of the analysts get hired. In the data studied in this paper, the hiring rate is always well below 1/2 (it is 7% on average in any given period). When deriving testable empirical predictions, I will therefore focus on the the case in which $p'$ is increasing in the hiring rate.

So far I have assumed that all analysts who move to the investment bank receive the same increase in lifetime earnings. However, it is possible that some analysts do not gain as much from moving to the investment bank (i.e., $\Delta EU$ is lower), either because they are more advanced in their career or because switching careers is more costly for them.[13] If analysts differ in their $\Delta EU$, then incentives are muted for analysts who are ex-ante expected to gain less from the employment transfer.

## 2.2. Key predictions

To summarize, the assumptions and results of this simple model lead to the following testable predictions.

**Assumption 1.** *Average lifetime expected earnings increase substantially after being hired by the investment bank.*

**Assumption 2.** *Higher observed performance increases the probability of being hired by the investment bank.*

[13] See Bond and Glode (2014) for a theoretical model where switching career is costly. Bond and Glode (2014) interpret the switching cost as a decrease in productivity, a direct disutility from relocating, or a behavioral aversion against change or uncertainty. The implication of heterogeneous switching costs is that not all analysts may prefer switching to investment banking after their employment at the CRA.

**Prediction 1.** *If the hiring rate is less than one half, an increase in the hiring rate will increase incentives and observed performance will be higher.*

**Prediction 2.** *Incentives are lower for analysts who are ex-ante expected to gain less from the employment transfer.*

Due to the lack of compensation data, Assumption 1 cannot be formally tested, but anecdotal evidence supports this view. For example, The New York Times (2010) reports that "at the height of the mortgage boom, companies like Goldman offered million dollar pay packages to workers like Mr. Yukawa who had been working at much lower pay at the rating agencies, according to several former workers at the agencies." In addition, working for the IB may also make the analysts eligible for even more higher-paying jobs that would be unattainable coming out of the CRA. Assumption 2 will be the focus on my main tests. Contrary to quid pro quo, the human capital view of the revolving door predicts that higher performance should increase the probability of being hired by an investment bank. Section 3.1 discusses the empirical implementation of this first set of tests and Section 4 presents the results.

Section 5 tests Predictions 1 and 2 by studying changes in the expected employment opportunities at investment banks. Given the low average probability of being hired by an investment bank in my sample ($p = 7\%$), the model predicts that improved employment prospects with investment banks lead to stronger incentives and higher observed performance on average. In the cross-section of analysts, incentive effects should be muted for analysts who are ex-ante expected to benefit less from switching careers.

# 3. Empirical Approach and Data

## 3.1. Empirical Approach

Relating observed rating performance to the likelihood of being hired by an investment bank poses at least two main challenges. First, it requires reliable measures of on-the-job performance 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.

### 3.1.1. Measuring rating performance

An important advantage of the rating-agency context is that individual output, i.e., the ratings issued by a given analyst and their subsequent performance, is 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.[14] 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. 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 default probability that may not always lead to a default. Second, a very attractive feature of Moody's organization in structured finance is that ratings surveillance is performed by a separate team.[15] Subsequent ratings are therefore 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

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

[15] 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`.

affect the performance of *all* ratings and cannot bias my cross-sectional comparisons. 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.[16]

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, and to using a proxy for rating shopping that can be observed in real time. 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 her 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 her quality through their professional network, e.g., from conversations with other bankers who have directly worked with the analyst, her colleagues at Moody's, etc. Finally, since rating reports are publicly available on Moody's website, investment banks may infer rating quality from the provided rating rationale and characteristics of the deal.

### 3.1.2. 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

[16]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 ratings accuracy 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 ratings accuracy measures and a very similar baseline coefficient.

types, which may exhibit different trends in fundamentals and be correlated with hiring intensity by investment banks.[17] 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 $\times$ collateral type level instead of at the analyst level. This has the additional advantage that Moody's internal organization 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 $t$:[18]

$$Inaccuracy_{izt} = \frac{1}{N} \sum_{j \in \mathcal{S}_{izt}} \left| R_{j,t+h} - R_{j,t} \right|, \tag{3}$$

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, $h$ will be equal to three calendar years.[19] 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 including collateral type $\times$ semester fixed effects in my regressions (see Equation (4) below).

Even within a given collateral type and date, analysts may be assigned to securities with different characteristics, e.g., deals with complex subordination structures or poor collateral quality. In addition to collateral type $\times$ semester fixed effects, I therefore control for a rich set

[17]As shown in the Internet Appendix, there is substantial heterogeneity in average ratings accuracy across different collateral types.

[18]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).

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

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 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.[20] 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:

$$IB\ Exit_i = \lambda_{zt} + \delta Inaccuracy_{izt} + \beta' X_{izt} + \epsilon_{izt}, \tag{4}$$

where $IB\ Exit_i$ is an indicator equal to one for analysts who join an investment bank after their employment at Moody's, $Inaccuracy_{izt}$ stands for analyst inaccuracy as computed in Equation (3), and $\lambda_{zt}$ for collateral type $\times$ semester fixed effects. Vector $X_{izt}$ includes the average tranche and deal characteristics listed above. Note that since the performance measure is analyst *inaccuracy*, the human capital view predicts $\delta < 0$ in the above regression.

[20] Since information on some tranche and deal characteristics (specifically, the weighted average life, insurance wrap, geographical concentration, LTV ratio, and credit score) is 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.

### 3.1.3. 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 the 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 she subsequently analyzes with the help of Moody's analytical team. She 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.[21] 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 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.

[21] See Fracassi, Petry, and Tate (2016) and `https://www.moodys.com/sites/products/ProductAttachments/mis_ratings_process.pdf` for a description of the ratings process at Moody's.

## 3.2. Data

An important implication of the human capital view illustrated in Section 2 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* ratings may be helpful to signal expertise, but potentially only *few* securities may be helpful to curry favors to the prospective employer. Hence, gauging the *net* effect of the revolving door requires analyzing the entire spectrum of securities rated by revolving analysts. The dataset should thus 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, neither for monitors in general nor for credit analysts in particular.[22] To overcome this problem, I hand-collect an original dataset that links individual analysts to the performance of the ratings they issue. Second, it is necessary to identify analysts who leave to underwriting 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 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.[23] Most of the times, Moody's press releases list two names, one more junior employee (typically the lead analyst), and one more

[22]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.

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

senior employee (typically the rating committee chair).[24] 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 her first employer following her 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, 66 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,[25] 94 analysts leave to other employers, and 84 analysts have not left Moody's as of December 2015. 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% (= 14/66) do so during their last year at the rating agency (Table 1, Panel B). Conditional on analysts who leave to an investment bank, the average duration of employment at Moody's is 3.5 years (Figure 1, Panel B). 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 (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. Overall, this pattern is consistent with the prediction that analysts who are ex-ante expected to

[24] As I show in the Internet Appendix, my results are economically and statistically stronger for the group of lead analysts.

[25] 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, Panel A 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.

benefit more from moving to an investment bank are more likely to switch, presumably because of a higher expected increase in lifetime earnings (e.g., younger analysts without graduate degrees), or because of lower switching costs (e.g., analysts with ties to New York City and non-law degrees).

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).[26] 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.

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 4.96 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 included in Equation (4). This reduces potential concerns that the securities rated by revolving analysts may differ on some unobserved dimension.

While my main tests below are designed to address identification issues, Figure 2 shows that two important insights emerge even from the raw data. In each collateral type and semester, analysts are sorted into quartiles based on their inaccuracy (computed as in Equation (3)). The graph plots the average relative inaccuracy as well as the average exit rate to investment banks

[26]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.

by inaccuracy quartile. First, there is substantial variation in analyst performance even within a given collateral type and semester. Ratings issued by analysts in the lowest inaccuracy quartile have to be adjusted by more than 3 notches less compared to ratings issued by the average analyst in the highest inaccuracy quartile. Second, the probability of being hired by a top investment bank is more than twice as high in the bottom inaccuracy quartile than in the top inaccuracy quartile.[27] Hence, even the raw data are supporting the human capital view of revolving doors.

# 4. Main Results

This section presents my main results. I document that ratings accuracy is a strong predictor of departures to investment banks, as predicted by the human capital view of revolving doors. In contrast, relative optimism does not significantly affect the chance of career advancement, except when the employment transfer is to a bank the analyst has recently rated.

## 4.1. Baseline Results

In order to test whether investment banks primarily hire analysts based on observed performance, 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 (3)). Then I estimate the regression in Equation (4) using a linear probability model.[28] 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,[29] as well as the average issuer market

[27]The difference is significant at the 1% level.

[28]As shown in Table 4, my results are robust to using a conditional logit model instead.

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

share.[30] All variables are defined in Appendix A. Standard errors are clustered at the analyst level.

Table 3 reports the results. Confirming the results from the simple sorts presented in Figure 2, more accurate analysts are significantly more likely to be hired by an investment bank than less accurate analysts rating securities of the same collateral type in the same semester. A one-standard-deviation decrease in inaccuracy increases the probability of being hired by an investment bank by 7.9 percentage points, or 33% (= $0.016 \times 4.96/0.24$) relative to the mean. The economic effect is larger when I focus on the likelihood of departing to an investment bank in the near future (columns (3) and (4)). A one-standard-deviation decrease in inaccuracy increases the probability of being hired by an investment bank by the end of the following semester by 78% (= $0.011 \times 4.96/0.07$), equivalent to 5.5 percentage points. Overall, these results are consistent with an efficient analyst labor market that allocates skilled analysts to jobs with higher returns to skill. Note that if analysts got attractive jobs in investment banking because they inflated their future employers' ratings, then one would not predict greater overall ratings accuracy.

In sum, accurate analysts are significantly more likely to be hired by investment banks, consistent with the human capital view of revolving doors. In the following, I show that this result is robust to alternative measures of ratings accuracy, including a measure based on realized losses.

## 4.2. Robustness

Table 4 presents robustness tests for the main results presented in Table 3, columns (2) and (4). Panel A investigates alternative measures of ratings (in)accuracy. First, I measure inaccuracy based on excess tranche losses, which dramatically reduces the sample size but still yields results of similar 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

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

Investor Service (2001)). While the statistical significance of the results is at best marginal when excess losses are measured over a three-year horizon, it is stronger for excess losses computed over a five-year horizon. Next, I focus on 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. These results are important because they use measures of ratings accuracy that are impossible or less likely to be influenced by Moody's surveillance team, suggesting that the documented effect cannot be explained by subjectivity in the ex-post adjustment of ratings.

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. Estimating Equation (4) using a conditional logit model yields results that are in line with the linear probability model. Finally, I run the baseline regressions at the individual deal level rather than at the collapsed analyst $\times$ collateral type $\times$ semester level, which also produces the same pattern.

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

## 4.3. Future Employers

It is possible that, while investment banks generally prefer to hire accurate analysts, they place more weight on optimism when hiring analysts who have directly rated their products. In order to test for the presence of such a bias, I study how the effects of ratings inaccuracy and relative pessimism on the probability of being hired change for hires that occur following an interaction between the analyst and the hiring investment bank.

In order to implement this idea, I estimate the following specification at the individual deal level:

$$This\ IB\ Exit_{ikzt} = \lambda_{zt} + \delta Inaccuracy_{ikzt} + \beta' X_{ikzt} + \epsilon_{ikzt}, \tag{5}$$

where $This\ IB\ Exit_{ikzt}$ is an indicator equal to one if one of the analysts $i$ rating deal $k$ is eventually hired by one of the deal's lead underwriting banks, and zero if they stay at the rating agency. $Inaccuracy_{ikzt}$ is the average inaccuracy of the analysts $i$ rating deal $k$, measured across all deals they rate in the same collateral type $z$ and semester $t$. Inaccuracy on a given deal is computed as the absolute difference (in notches) between the initial rating and the rating three years after issuance across all tranches. $\lambda_{zt}$ are collateral type $\times$ issuance semester fixed effects, respectively, and $X_{ikzt}$ represents the same vector of additional controls as in Equation (4). Coefficient $\delta$ in the above regression can then be tested against the same coefficient in an alternative specification where the left-hand-side variable is an indicator equal to one if an analyst rating deal $k$ joins an investment bank that is *not* the underwriter of the particular deal. The human capital view still predicts $\delta < 0$ in the above regression, i.e., investment banks should care about the overall accuracy of the analyst even if she has rated them.

Table 5, Panel A, reports the results. For reasons of easier comparison, all variables are standardized to have zero mean and a standard deviation of one. The probability of being hired by one of the lead underwriters of the deal is still sensitive to the analysts' average inaccuracy, although somewhat weaker than the probability of being hired by any other investment bank (16.8% vs. 35.1% relative to one standard deviation). This also holds for the probability of being hired by a lead underwriter of the deal by the end of the following semester (columns (3) and (4)). Thus, even investment banks who hire analysts shortly following an interaction appear to place a substantial weight on the analyst's overall accuracy.

While the results above suggest that investment banks hire analysts whose ratings are on average more accurate, it is possible that they have a simultaneous preference for analysts who are optimistic when rating their own issues. Following Hong and Kubik (2003), I therefore include a measure of relative rating pessimism while controlling for overall inaccuracy:

$$This\ IB\ Exit_{ikzt} = \lambda_{zt} + \delta_1 Inaccuracy_{ikzt} + \delta_2 Pessimism_k + \beta' X_{ikzt} + \epsilon_{ikzt}, \tag{6}$$

where $Pessimism_k$ is computed for a particular deal $k$ as (minus) one if the rating of a tranche in the deal is more pessimistic (optimistic) relative to the average of S&P and Fitch, averaged across all tranches of the deal. Table 5, Panel B reports results. Assigning optimistic ratings generally does not increase the prospects of being hired (columns (1) and (3)), except by investment banks whose issues analysts are currently rating (columns (2) and (4)).[31] The latter result is statistically significant only if the hire occurs shortly following the deal (column (4)). However, the economic significance of the effect of optimism is modest and remains several magnitudes smaller than the effect of overall accuracy (3.0% vs. 12.1% relative to one standard deviation, respectively).

The increasing importance of optimism when rating deals of the future employer shortly before the transition is consistent with evidence reported by 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 effect may not necessarily lead to economically sizable distortions. First, analysts are almost four times more likely to be hired in the near future by an underwriter whose products they are currently not rating (see Table 1, Panel B). The fact that investment banks mainly hire analysts who have not recently rated their products is inconsistent with the view that they mainly hire as a reward for favors. Second, even

[31] My conclusions regarding the economic significance of the effect of pessimism are unchanged if I do not simultaneously control for inaccuracy.

in the rare case that investment banks do hire analysts following a recent interaction, they still place a large weight on the analysts' overall accuracy. From the perspective of the analysts, the ex-ante expected payoff for being accurate thus appears to largely dominate the expected payoff for being optimistic.

# 5. Analyst Performance and Variation in Hiring Rates

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 when analysts rate their future employers, potentially consistent with quid pro quo behavior, such conflicts are very infrequent and affect a relatively small fraction of securities rated by transitioning analysts.

One implication of the simple model presented in Section 2 is that as long as effort has a positive effect on performance, analysts will exert greater effort in the presence of the revolving door. Disentangling the effect of ability, effort, and luck on performance is difficult, and may be less relevant in the context of the question asked in this paper, whether revolving doors should be regulated (see deHaan, Kedia, Koh, and Rajgopal (2015)). So far, my results suggest that regulating the revolving door between credit rating agencies and investment banks has at best a limited, if not a negative effect on incentives.

In order to provide some initial evidence regarding the existence of a positive effect on analyst effort, I exploit variation in the availability of investment banking jobs as a discontinuous increase in the 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 2.2, my theoretical framework predicts that analyst performance should respond positively to news about improved prospects of joining an investment bank. I use the announcement of a *new* lead investment bank starting to underwrite securities of a particular collateral type as a positive shock to the supply of investment banking jobs in this market.[32] 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 a control group. Third, it allows me to test whether, in the cross-section of analysts within the same collateral group, analysts with certain characteristics respond more strongly to the event than others. Specifically, my theoretical framework predicts that analysts who are ex-ante less likely to leave to investment banks (because of a higher switching cost or reduced expected lifetime benefits) should respond less to fluctuations in the supply of investment banking jobs. 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.[33] In contrast, and by construction, 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

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

[33] 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.

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.[34] 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 increases 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:

$$Inaccuracy_{izt} = \lambda_{st} + \lambda_i + \sum_{\tau=-3}^{\tau=+3} \delta_\tau I(New\,IB_{zt}^{\,\tau}) + \beta' X_{izt} + \epsilon_{izt}, \tag{7}$$

where $Inaccuracy_{izt}$ stands for analyst inaccuracy as computed in Equation (3), and $\lambda_{st}$ and $\lambda_i$ are market segment $\times$ semester and analyst fixed effects, respectively.[35] $I(New\,IB_{zt}^{\,\tau})$ 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 set of control variables as in Table 3. If analyst incentives respond positively to improvements in investment banking opportunities, then one would expect $\delta_\tau < 0$ for $\tau = 0$, and possibly for periods shortly following the announcement.

[34] 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.

[35] 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.

Table 6, column (1), 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 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 0.9 to 1.1 notches vis-à-vis the control group in semesters $\tau = 0$ and $\tau = +1$, respectively. 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 the predicted values from the Probit regression of $IB\,Exit$ on ex-ante analyst characteristics presented in Table 2, column (1), as a measure of the analyst's ex-ante likelihood of switching careers.[36] Intuitively, analysts who are ex-ante more likely to switch to investment banking should be more sensitive to changes in investment banking opportunities under the human capital view.

Table 6, columns (3) to (6), reports results. The improvement in performance is concentrated among analysts who are ex-ante more likely to leave to investment banking (columns (5) and (6)), whereas the performance of analysts who are ex-ante less likely to leave does not show a significant reaction (columns (3) and (4)). The fact that the improvement in performance is concentrated in this subset of analysts raises the bar for alternative explanations. 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.

[36] I find very similar results if I use the predicted values from column (2) in Table 2, i.e., with controls for cohort effects.

# 6. Extensions

This section investigates three potentially remaining concerns. First, could senior employees be more corrupted by the revolving door? Second, could my results be driven by Moody's punishing accurate analysts as opposed to by investment banks hiring based on observed performance? Third, are analysts influenced by *former* work experience at investment banks?

## 6.1. 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 investment banks' hiring of accurate junior analysts were masking distorted hiring decisions at a more senior level. Since 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 performance sensitivity of investment bank hiring as a function of analyst seniority.

I define three proxies for senior analysts, identifying analysts with a job title of Vice President, Senior Vice President, and Managing Director or higher, respectively. Then I estimate the baseline regression from Equation (4) while adding an interaction term between analyst inaccuracy and the indicator for senior analysts. Table 7 reports the results. Overall, the likelihood to depart to an investment bank is as sensitive to ratings inaccuracy for senior analysts as it is for junior analysts (columns (1) to (3)). However, all three proxies indicate that, for senior analysts, the likelihood to be hired by an investment bank by the end of the following semester is less sensitive to recent performance than for junior analysts. This pattern is consistent with investment banks learning about an analyst's ability over time and placing a lower weight on the most recent performance signal if they have observed the analyst over a longer period of time. Importantly, however, the point estimates suggest that the sign of the relationship between inaccuracy and likelihood of departure does not change for senior analysts, i.e., even for more senior analysts inaccuracy negatively predicts a transfer to an investment bank.

## 6.2. Disincentives at Moody's

A second potential concern could be that my results reflect disincentives within Moody's organization as opposed to investment banks hiring based no observed performance. For example, if Moody's was strongly focused on expanding its market share, as suggested by the Financial Crisis Inquiry Commission (2011),[37] it may have punished analysts who issued accurate ratings by not promoting them. This interpretation could explain why accurate analysts may choose to seek employment elsewhere. However, it would not explain why analysts hired by investment banks outperform and not the average analyst who transitions to other employers. The evidence reported in the Internet Appendix, where I show that departures to other types of employers are at best weakly negatively correlated with inaccuracy, is therefore not consistent with this story. The finding that hiring by investment banks is more strongly related to rating performance than hiring by other outside employers can be justified 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 ability to access or interpret information about analysts' performance.[38]

To further investigate this potential concern, I look at the relationship between analyst performance and internal promotions at Moody's. I identify promotions based on changes in the analyst's job title mentioned in the press releases from Moody's website, which are classified into same categories as in Kisgen, Osborn, and Reuter (2016). The results, shown in Table 8, do not support the conjecture that Moody's punishes analysts for being accurate. Consistent with the findings by Kisgen, Osborn, and Reuter (2016) who study promotions of corporate bond analysts at Moody's, accurate analysts in structured finance are also more likely to be promoted.

[37] The Financial Crisis Inquiry Commission (2011) reports that "a strong emphasis on market share was evident in employee performance evaluations" at Moody's.

[38] Cetorelli and Peristiani (2012) report that rating analysts in securitized finance work very closely with the underwriting investment banks. These close interactions may provide investment banks with better access to the rating analyst network and possibly a greater ability to judge rating skill.

However, the relationship between performance and internal promotions is substantially weaker, both in economic and in statistical terms,[39] than the previously documented relationship between performance and departures to investment banks. It is slightly stronger for more junior analysts, i.e., analysts below the rank of Vice President (columns (3) and (4)).

## 6.3. 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 53 cases. Out of these 53 inbound revolvers, 22 rate their former employer at some point during my sample period.

As the test on future employers in Section 4.3, the analysis is run at the individual deal level in order to be able to distinguish deals that involve a former employer from other deals. Analyst inaccuracy and relative pessimism are regressed on an indicator variable equal to one if the analyst has previously worked at a top investment bank (*Past IB*), and zero otherwise, as well as on an interaction term between *Past IB* and an indicator equal to one if one of the lead underwriting banks of the deal if the former employer of one of the analysts involved in the deal (*Past Employer*). Table 9 presents results. Analysts with former investment banking experience do not perform statistically differently from other analysts (columns (1) and (2)). However, they are 18% (= 0.497/2.69) more accurate when rating deals underwritten by their former employers (columns (3) and (4)). This result also holds with analyst $\times$ collateral type $\times$ semester fixed effects (column (5)). This pattern is consistent with an informational advantage of analysts who rate their former employers, and inconsistent with conflicts of interest. While the ratings on deals by former employers tend to be slightly more optimistic relative to S&P and Fitch (see

[39] A one-standard-deviation decrease in inaccuracy is associated with a 33% increase in the promotion probability ($= 0.004 \times 4.96/0.06$).

Panel B), this does not appear to hurt overall accuracy. In sum, I do not find evidence of sizeable distortions in the performance of inbound revolvers.

## 7. Conclusion

My paper contributes to the ongoing debate on whether revolving doors strengthen or distort monitoring incentives. I compile a dataset that links individual credit rating analysts at Moody's to their career paths and to the quality of the ratings they issue. In contrast with the generally negative view of revolving doors, I find that more accurate analysts significantly more likely to be hired by investment banks than other analysts rating similar securities at the same point in time. A notable exception to this relationship are the few cases where analysts depart to underwriters whose issues they have recently rated. 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 concerns about their outside labor market options. 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.

This paper focuses on the effects on performance incentives and is silent on other channels through which the revolving door may affect rating quality. 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 game the rating system once they have left the rating agency.[40] 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.

[40] 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 | 85 | 66 | 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 |

Panel C: Summary Statistics

| | N | Mean | Std. Dev. | 0.25 | Median | 0.75 |
|---|---|---|---|---|---|---|
| *Dependent Variables* | | | | | | |
| IB Exit | 1,859 | 0.24 | 0.43 | 0.00 | 0.00 | 0.00 |
| IB Exit$_{t+1}$ | 1,859 | 0.07 | 0.25 | 0.00 | 0.00 | 0.00 |
| Promotion$_{t+1}$ | 1,703 | 0.06 | 0.24 | 0.00 | 0.00 | 0.00 |
| | | | | | | |
| *Key Independent Variables* | | | | | | |
| Inaccuracy (baseline) | 1,859 | 2.69 | 4.96 | 0.00 | 0.00 | 2.19 |
| Avg. 3-yr excess losses (in %) | 513 | 6.34 | 6.83 | 1.02 | 3.99 | 9.56 |
| Avg. 5-yr excess losses (in %) | 660 | 9.24 | 9.33 | 1.71 | 5.03 | 15.83 |
| Avg. 3-yr defaults (in %) | 1,859 | 6.63 | 16.83 | 0.00 | 0.00 | 0.00 |
| | | | | | | |
| *Control variables* | | | | | | |
| Tenure (in semesters) | 1,859 | 5.92 | 6.07 | 2.00 | 4.00 | 8.00 |
| 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) | 1,750 | 4.97 | 2.43 | 3.22 | 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 | 1,747 | 0.02 | 0.07 | 0.00 | 0.00 | 0.00 |
| Deal size (in $m) | 1,859 | 658.35 | 557.21 | 341.58 | 535.20 | 849.86 |
| Number of tranches in the deal | 1,859 | 7.86 | 5.45 | 4.77 | 7.00 | 9.71 |
| Overcollateralization | 1,859 | 0.01 | 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 her departure, and is regressed on various analyst characteristics using a Probit model. *Ivy League* indicates whether the analyst has obtained her 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. *NYC Undergrad* indicates whether the analyst has obtained her undergraduate degree from an institution located in New York City, and *Graduate Degree* is an indicator equal to one if the analyst has obtained a graduate degree prior to joining Moody's. *Prior Work Experience* refers to the logarithm of one plus the number of years of prior work experience. 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) |
| Ivy League | -0.595 | -0.509 |
| | (-1.42) | (-1.01) |
| Law Degree | -0.812 | -1.067 |
| | (-1.40) | (-1.91) |
| Tech Degree | 0.038 | 0.342 |
| | (0.10) | (0.71) |
| NYC Undergrad | 0.845 | 1.186 |
| | (2.00) | (2.01) |
| Graduate Degree | -0.801 | -1.094 |
| | (-2.20) | (-2.57) |
| Prior Work Experience | -2.923 | -2.820 |
| | (-3.59) | (-2.73) |
| Female | -0.308 | -0.514 |
| | (-0.89) | (-1.36) |
| Cohort fixed effects | No | Yes |
| N | 99 | 79 |
| Pseudo-$R^2$ | 0.220 | 0.284 |

## Table 3: Analyst Performance and Departures to Investment Banks

The table reports results from regressing indicators for analyst departures to investment banks on rating accuracy. In columns (1) and (2), *IB Exit* is an indicator equal to one if at the end of her employment with Moody's the analyst departs to an investment bank that was ranked in "The Bloomberg 20" ranking in the year prior to her departure. In columns (3) and (4), *IB* $Exit_{t+1}$ is an indicator equal to one if the analyst departs to an investment bank by the end of the following semester. Analyst inaccuracy is measured as the average number of notches by which the ratings issued by the analyst need to be adjusted within three years of issuance. 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.

| | **IB Exit** | | **IB Exit**$_{t+1}$ | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Inaccuracy | -0.014 | -0.016 | -0.010 | -0.011 |
| | (-2.76) | (-3.19) | (-2.26) | (-2.50) |
| Tenure | -0.034 | -0.031 | 0.003 | 0.008 |
| | (-0.81) | (-0.78) | (0.25) | (0.63) |
| Number of deals | -0.045 | -0.055 | -0.019 | -0.027 |
| | (-2.37) | (-2.64) | (-2.66) | (-3.11) |
| IB underwriter | -0.064 | -0.033 | -0.044 | -0.022 |
| | (-0.88) | (-0.44) | (-1.41) | (-0.66) |
| Issuer market share | 0.026 | 0.016 | 0.015 | 0.009 |
| | (1.34) | (0.86) | (1.58) | (0.90) |
| Deal controls | No | Yes | No | Yes |
| Collateral type × semester f.e. | Yes | Yes | Yes | Yes |
| N | 1,806 | 1,806 | 1,806 | 1,806 |
| $R^2$ | 0.116 | 0.138 | 0.109 | 0.119 |

## Table 4: Robustness

The table presents robustness tests. The baseline regression refers to columns (2) and (4) from Table 3. For brevity, I only report coefficients of interest and suppress control variables. Economic effects are calculated as the reported coefficient multiplied by the standard deviation of the key independent variable, 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. 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, I test a conditional logit model instead of a linear probability model using bootstrapped standard errors after 500 replications. Odds ratios are reported as the economic effect. 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**$_{t+1}$ | | | |
|---|---|---|---|---|---|---|---|---|
| | Coeff | $t$-stat | Econ. Effect | $N$ | Coeff. | $t$-stat | Econ. Effect | $N$ |
| Baseline | -0.016 | (-3.19) | -33.1% | 1,806 | -0.011 | (-2.50) | -77.9% | 1,806 |
| *Panel A: Alternative Measures of Analyst Inaccuracy* | | | | | | | | |
| Avg. 3-yr excess losses | -0.009 | (-1.71) | -27.0% | 511 | -0.003 | (-1.33) | -30.1% | 511 |
| Avg. 5-yr excess losses | -0.015 | (-3.06) | -59.6% | 655 | -0.006 | (-2.64) | -86.0% | 655 |
| Avg. 3-yr defaults | -0.152 | (-1.91) | -10.8% | 1,805 | -0.108 | (-1.69) | -26.3% | 1,805 |
| *Panel B: Subperiods* | | | | | | | | |
| Pre-Dodd-Frank I: 2000-2004 | -0.024 | (-3.17) | -16.6% | 734 | -0.013 | (-2.73) | -31.3% | 734 |
| Pre-Dodd-Frank II: 2005-2009 | -0.014 | (-2.51) | -33.6% | 991 | -0.010 | (-1.83) | -78.7% | 991 |
| Post-Dodd-Frank: 2010-2012 | 0.014 | (0.35) | 31.8% | 80 | 0.021 | (1.29) | 220.3% | 80 |
| *Panel C: Estimation* | | | | | | | | |
| Conditional logit | -0.107 | (-3.16) | 89.8% | 1,706 | -0.171 | (-3.00) | 84.3% | 1,271 |
| Deal level | -0.011 | (-2.45) | -19.8% | 4,343 | -0.010 | (-2.84) | -61.4% | 4,343 |

**Table 5: Analyst Performance and Departures to Investment Banks Following an Interaction**

The table reports results from deal-level regressions of indicators for analyst departures to investment banks on measures of rating accuracy and rating bias. *Other IB Exit* is an indicator equal to one if an analyst who rates the deal is hired by an investment bank that is not among the lead underwriters of the deal, and zero if the analyst stays at the rating agency. *This IB Exit* is an indicator equal to one if an analyst who rates the deal is hired by one of the lead underwriters of the deal, and zero if the analyst stays at the rating agency. *Other IB Exit*$_{t+1}$ and *This IB Exit*$_{t+1}$ are defined analogously, but are equal to one only if the analysts get hired by the end of the following semester. Analyst inaccuracy is measured as the average number of notches by which the ratings of the tranches of a deal need to be adjusted within three years of issuance, and is averaged across all deals rated by the analysts in a given collateral type and semester. Relative pessimism is equal to (minus) one if the rating of a tranche in the particular deal is more pessimistic (optimistic) relative to the average of S&P and Moody's, and zero otherwise, and is averaged across all tranches in the deal. For reasons of easier comparison, all variables are standardized to have zero mean and a standard deviation of one. $t$-statistics, reported in parentheses, are based on standard errors that allow for clustering at the lead analyst level.

Panel A: Inaccuracy only

| | Other IB Exit | This IB Exit | Other IB Exit$_{t+1}$ | This IB Exit$_{t+1}$ |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Avg. Inaccuracy | -0.351 | -0.168 | -0.512 | -0.126 |
| | (-3.00) | (-2.30) | (-3.64) | (-1.68) |
| Tenure | 0.019 | -0.049 | 0.134 | -0.018 |
| | (0.26) | (-1.51) | (2.25) | (-0.41) |
| Number of deals | -0.017 | 0.027 | -0.147 | -0.070 |
| | (-0.23) | (0.60) | (-2.30) | (-1.09) |
| IB underwriter | -0.024 | 0.063 | -0.050 | 0.029 |
| | (-0.58) | (3.00) | (-1.64) | (2.35) |
| Issuer market share | 0.064 | -0.020 | 0.084 | 0.000 |
| | (1.18) | (-1.17) | (1.68) | (-0.25) |
| Deal controls | Yes | Yes | Yes | Yes |
| Coll. type × semester f.e. | Yes | Yes | Yes | Yes |
| N | 4,214 | 3,060 | 3,281 | 2,977 |
| $R^2$ | 0.179 | 0.186 | 0.296 | 0.255 |

Panel A: Inaccuracy and Relative Pessimism

| | Other IB Exit | This IB Exit | Other IB Exit$_{t+1}$ | This IB Exit$_{t+1}$ |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Avg. Inaccuracy | -0.352 | -0.172 | -0.527 | -0.121 |
| | (-2.93) | (-2.29) | (-3.68) | (-1.72) |
| Relative pessimism | -0.003 | -0.031 | -0.005 | -0.030 |
| | (-0.17) | (-1.34) | (-0.25) | (-1.80) |
| Tenure | 0.022 | -0.049 | 0.137 | -0.018 |
| | (0.28) | (-1.53) | (2.28) | (-0.43) |
| Number of deals | -0.020 | 0.027 | -0.152 | -0.059 |
| | (-0.29) | (0.67) | (-2.34) | (-1.02) |
| IB underwriter | -0.023 | 0.063 | -0.050 | 0.029 |
| | (-0.55) | (3.01) | (-1.60) | (2.38) |
| Issuer market share | 0.064 | -0.020 | 0.084 | 0.000 |
| | (1.16) | (-1.19) | (1.67) | (-0.27) |
| | | | | |
| Deal controls | Yes | Yes | Yes | Yes |
| Coll. type × semester f.e. | Yes | Yes | Yes | Yes |
| N | 4,172 | 3,024 | 3,245 | 2,941 |
| $R^2$ | 0.179 | 0.189 | 0.299 | 0.259 |

## Table 6: Exploiting Variation in the Expected 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. The inaccuracy of analysts in the event collateral group (i.e., the collateral group where the investment bank enters) is compared to 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. Columns (3) to (6) show the same regression for two subgroups of analysts. $\widehat{Pr(IB\,Exit)}$ 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 (1), and is split at the median across all analysts in my sample. 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.

| | **Inaccuracy** | | | | | |
|---|---|---|---|---|---|---|
| | All Analysts | | Low $\widehat{Pr(IB\,Exit)}$ | | High $\widehat{Pr(IB\,Exit)}$ | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| New IB ($\tau = -3$) | -0.118 | | 0.497 | | -0.338 | |
| | (-0.30) | | (0.84) | | (-0.53) | |
| New IB ($\tau = -2$) | -0.271 | | -0.172 | | -0.185 | |
| | (-0.77) | | (-0.37) | | (-0.34) | |
| New IB ($\tau = -1$) | -0.287 | | -0.146 | | -0.396 | |
| | (-0.93) | | (-0.31) | | (-0.98) | |
| New IB ($\tau = 0$) | -0.935 | -0.592 | 0.227 | 0.063 | -1.612 | -0.936 |
| | (-2.63) | (-2.26) | (0.45) | (0.23) | (-3.63) | (-2.53) |
| New IB ($\tau = +1$) | -1.055 | | -0.368 | | -1.267 | |
| | (-3.62) | | (-0.77) | | (-3.60) | |
| New IB ($\tau = +2$) | -0.069 | | 0.578 | | -0.344 | |
| | (-0.21) | | (0.93) | | (-0.78) | |
| New IB ($\tau = +3$) | -0.187 | | 0.697 | | -0.660 | |
| | (-0.60) | | (1.29) | | (-1.49) | |
| Controls included | Yes | Yes | Yes | Yes | Yes | Yes |
| Segment × quarter f.e. | Yes | Yes | Yes | Yes | Yes | Yes |
| Analyst f.e. | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 1,752 | 1,806 | 850 | 864 | 817 | 853 |
| $R^2$ | 0.800 | 0.799 | 0.856 | 0.855 | 0.717 | 0.712 |

## Table 7: The Role of Analyst Seniority

The table presents results interactions with three different measures of analyst seniority. The regressions presented in Table 3, columns (2) and (4), are estimated with an interaction term between rating performance and an indicator for senior analysts. In columns (1) to (3), *IB Exit* is an indicator equal to one if at the end of her employment with Moody's the analyst departs to an investment bank that was ranked in "The Bloomberg 20" ranking in the year prior to her departure. In columns (4) to (6), *IB* $Exit_{t+1}$ is an indicator equal to one if the analyst departs to an investment bank by the end of the following semester. Analyst inaccuracy is measured as the average number of notches by which the ratings issued by the analyst need to be adjusted within three years of issuance. All variables are defined in Appendix A. In columns (1) and (3) ((2) and (4)) [(3) and (6)], senior analysts are defined as analysts with a title of Vice President (Senior Vice President) [Managing Director] or higher. Analyst job titles are obtained from press releases on Moody's website and categorized as in Kisgen, Osborn, and Reuter (2016). $t$-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

| | | **IB Exit** | | | **IB** $\textbf{Exit}_{t+1}$ | |
|---|---|---|---|---|---|---|
| | Definition of Analyst Seniority | | | | | |
| | ≥VP | ≥SVP | ≥MD | ≥VP | ≥SVP | ≥MD |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Inaccuracy | -0.020 | -0.020 | -0.016 | -0.017 | -0.017 | -0.014 |
| | (-2.54) | (-2.54) | (-2.48) | (-3.37) | (-3.38) | (-2.93) |
| Senior | -0.224 | -0.223 | -0.130 | -0.079 | -0.080 | -0.065 |
| | (-3.74) | (-3.77) | (-3.77) | (-2.46) | (-2.55) | (-3.12) |
| Inaccuracy × Senior | 0.007 | 0.006 | 0.001 | 0.009 | 0.009 | 0.006 |
| | (0.81) | (0.78) | (0.08) | (2.35) | (2.39) | (1.64) |
| Tenure | -0.015 | -0.015 | -0.036 | 0.012 | 0.012 | 0.006 |
| | (-0.38) | (-0.38) | (-0.96) | (0.99) | (0.99) | (0.49) |
| Number of deals | -0.051 | -0.051 | -0.048 | -0.026 | -0.026 | -0.025 |
| | (-2.68) | (-2.68) | (-2.52) | (-3.14) | (-3.14) | (-2.97) |
| IB underwriter | -0.010 | -0.011 | -0.027 | -0.016 | -0.016 | -0.019 |
| | (-0.13) | (-0.15) | (-0.37) | (-0.50) | (-0.50) | (-0.59) |
| Issuer market share | 0.010 | 0.010 | 0.009 | 0.008 | 0.007 | 0.006 |
| | (0.52) | (0.52) | (0.51) | (0.84) | (0.84) | (0.70) |
| Deal Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Collateral type × semester f.e. | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 1,806 | 1,806 | 1,806 | 1,806 | 1,806 | 1,806 |
| $R^2$ | 0.176 | 0.177 | 0.157 | 0.132 | 0.132 | 0.129 |

## Table 8: Analyst Performance and Promotions

The table presents results from regressing an indicator for analyst promotion on measures of analyst rating performance. $Promotion_{t+1yr}$ is an indicator equal to one if the analyst is internally promoted at Moody's (i.e., moves to a higher job title) during the following semester. Analyst job titles are obtained from press releases on Moody's website and categorized as in Kisgen, Osborn, and Reuter (2016). Analyst inaccuracy is measured as the average number of notches by which the ratings issued by the analyst need to be adjusted within three years of issuance. In columns (1) and (2), the regression is estimated using all analysts. In columns (3) and (4), only analysts with a job title below Vice President are included. $t$-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

| | **Promotion**$_{t+1}$ | | | |
|---|---|---|---|---|
| | All Analysts | | Analysts <VP | |
| | (1) | (2) | (3) | (4) |
| Inaccuracy | -0.004 | -0.004 | -0.008 | -0.008 |
| | (-1.41) | (-1.61) | (-1.71) | (-1.79) |
| Tenure | 0.004 | 0.005 | 0.019 | 0.019 |
| | (0.62) | (0.64) | (0.75) | (0.77) |
| Number of deals | -0.020 | -0.022 | -0.003 | -0.004 |
| | (-3.88) | (-3.92) | (-0.25) | (-0.30) |
| IB underwriter | 0.003 | 0.002 | 0.016 | 0.013 |
| | (0.20) | (0.12) | (0.63) | (0.46) |
| Issuer market share | 0.007 | 0.005 | 0.008 | 0.004 |
| | (0.99) | (0.62) | (0.89) | (0.50) |
| Deal controls | No | Yes | No | Yes |
| Collateral type × semester f.e. | Yes | Yes | Yes | Yes |
| N | 1,656 | 1,656 | 648 | 648 |
| $R^2$ | 0.132 | 0.136 | 0.209 | 0.220 |

## Table 9: Inbound Revolvers

The table reports results from deal-level regressions of ratings performance on an indicator for past investment banking experience. *Past IB* is an indicator equal to one if one of the analysts rating the deal has worked for an investment bank that was ranked in "The Bloomberg 20" ranking prior to her employment at Moody's. *Past Employer* is an indicator equal to one if one of the lead underwriting banks of the deal is the former employer of one of the analysts involved in the rating. In Panel A, the dependent variable is analyst inaccuracy, measured as the average number of notches by which the ratings of the deal's tranches need to be adjusted within three years of issuance. In Panel B, the dependent variable is relative pessimism, which is equal to (minus) one if the rating of a tranche in the deal is more pessimistic (optimistic) relative to the average of S&P and Moody's, and zero otherwise, and is averaged across all tranches in the deal. $t$-statistics, reported in parentheses, are based on standard errors that allow for clustering at the lead analyst level.

Panel A: Inaccuracy

| | **Inaccuracy** | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Past IB | 0.096 | 0.117 | 0.118 | 0.146 | |
| | (0.60) | (0.74) | (0.73) | (0.91) | |
| Past IB × Past Employer | | | -0.391 | -0.497 | -0.707 |
| | | | (-1.69) | (-2.13) | (-2.37) |
| Tenure | -0.241 | -0.201 | -0.241 | -0.199 | -0.325 |
| | (-1.74) | (-1.49) | (-1.74) | (-1.48) | (-2.08) |
| Number of deals | 0.129 | 0.055 | 0.129 | 0.053 | 0.090 |
| | (1.82) | (0.70) | (1.81) | (0.67) | (0.59) |
| IB underwriter | -0.130 | -0.128 | -0.121 | -0.116 | -0.095 |
| | (-0.91) | (-0.88) | (-0.85) | (-0.81) | (-0.96) |
| Issuer market share | 0.128 | 0.103 | 0.129 | 0.104 | 0.083 |
| | (2.89) | (2.22) | (2.90) | (2.22) | (2.02) |
| Deal controls | No | Yes | No | Yes | Yes |
| Collateral type × semester f.e. | Yes | Yes | Yes | Yes | Yes |
| Analyst × coll. type × sem. f.e. | No | No | No | No | Yes |
| N | 3,836 | 3,836 | 3,836 | 3,836 | 3,836 |
| $R^2$ | 0.797 | 0.800 | 0.797 | 0.800 | 0.800 |

Panel B: Relative pessimism

| | Relative pessimism | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Past IB | 0.005 | 0.007 | 0.007 | 0.010 | |
| | (0.60) | (0.87) | (0.78) | (1.05) | |
| Past IB × Past Employer | | | -0.034 | -0.037 | -0.021 |
| | | | (-1.40) | (-1.58) | (-0.62) |
| Tenure | -0.014 | -0.011 | -0.014 | -0.011 | 0.006 |
| | (-2.04) | (-1.56) | (-2.03) | (-1.55) | (0.51) |
| Number of deals | 0.027 | 0.022 | 0.027 | 0.022 | 0.019 |
| | (4.42) | (3.85) | (4.42) | (3.82) | (2.10) |
| IB underwriter | -0.002 | -0.005 | -0.001 | -0.004 | -0.007 |
| | (-0.13) | (-0.45) | (-0.06) | (-0.37) | (-0.52) |
| Issuer market share | -0.002 | -0.004 | -0.002 | -0.004 | 0.000 |
| | (-0.75) | (-1.26) | (-0.73) | (-1.25) | (-0.06) |
| Deal controls | No | Yes | No | Yes | Yes |
| Collateral type × semester f.e. | Yes | Yes | Yes | Yes | Yes |
| Analyst × coll. type × sem. f.e. | No | No | No | No | Yes |
| N | 3,800 | 3,800 | 3,800 | 3,800 | 3,800 |
| $R^2$ | 0.082 | 0.091 | 0.083 | 0.091 | 0.091 |

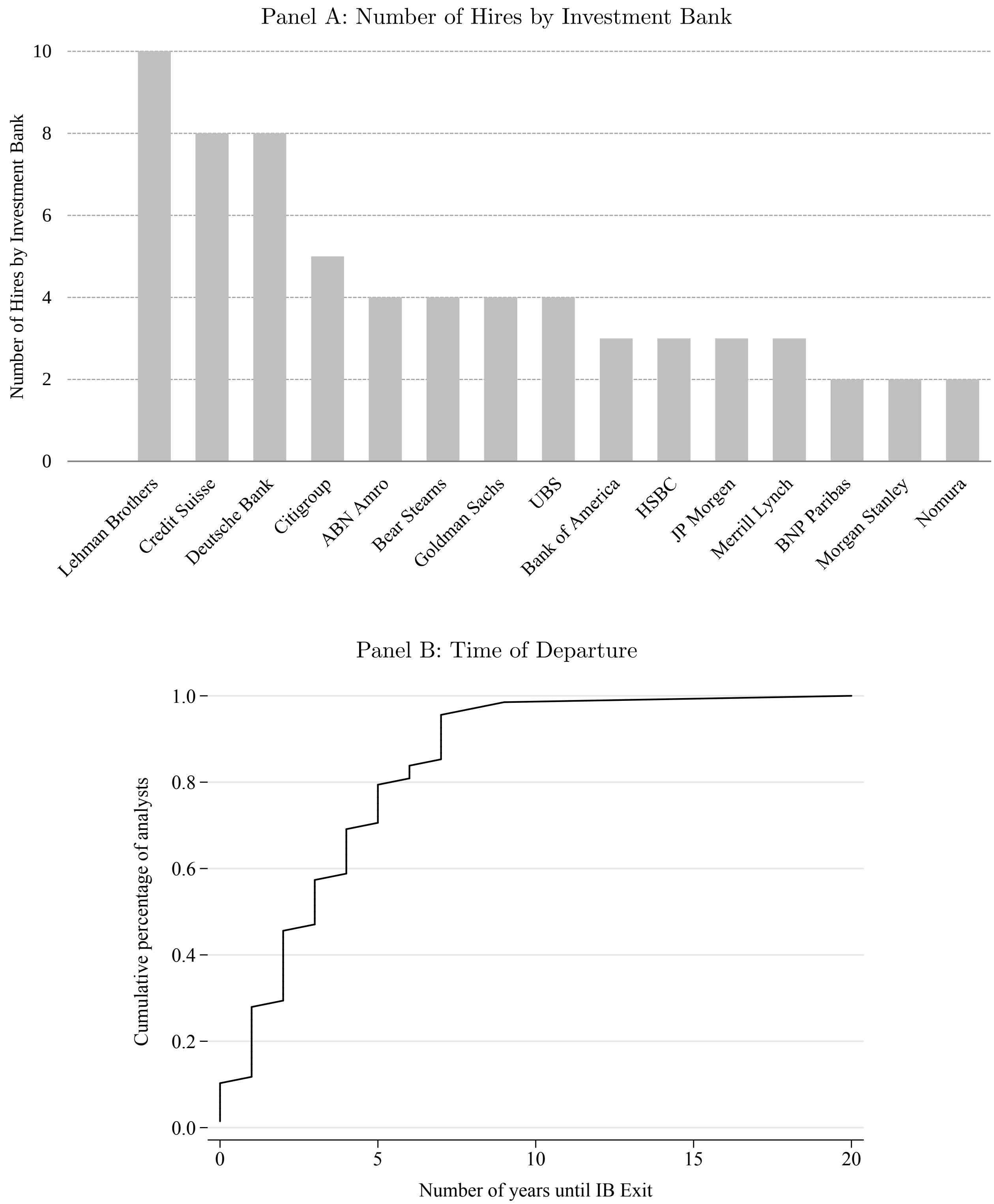


**Figure 1: Summary Plots.** Panel A plots the total number of Moody's analysts hired by each investment bank over the sample period. An analyst departure is classified as an exit to an investment bank if the subsequent employer was ranked in "The Bloomberg 20" ranking in the year prior to the departure. Panel B plots the cumulative percentage of analysts departing to an investment bank for a given number of years of employment at Moody's.

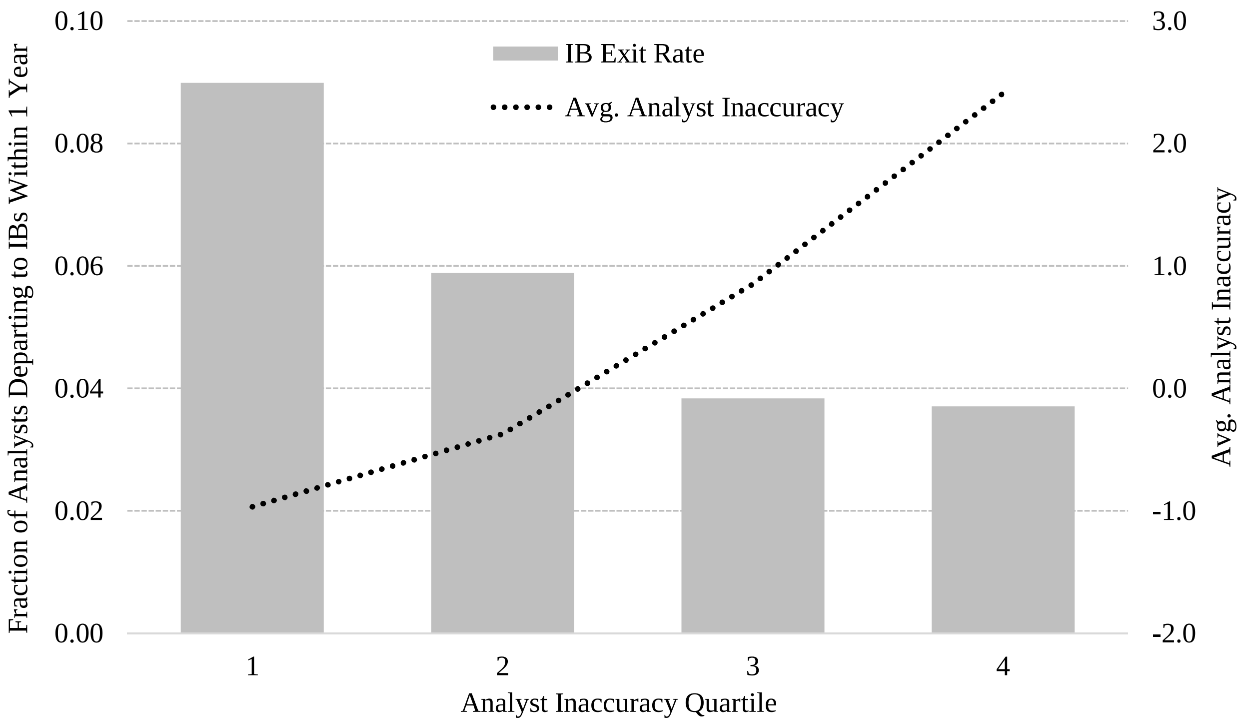


**Figure 2: Analyst Performance and Departures to Investment Banks.** The graph plots by analyst inaccuracy quartile the average analyst inaccuracy and the fraction of analysts who leave to investment banks. Analyst inaccuracy is computed as the average number of notches that ratings issued by a given analyst in a given collateral type and semester need to be adjusted within three years after issuance (see Equation (3)). Using this measure, analysts are sorted into quartiles within a given collateral type and semester.

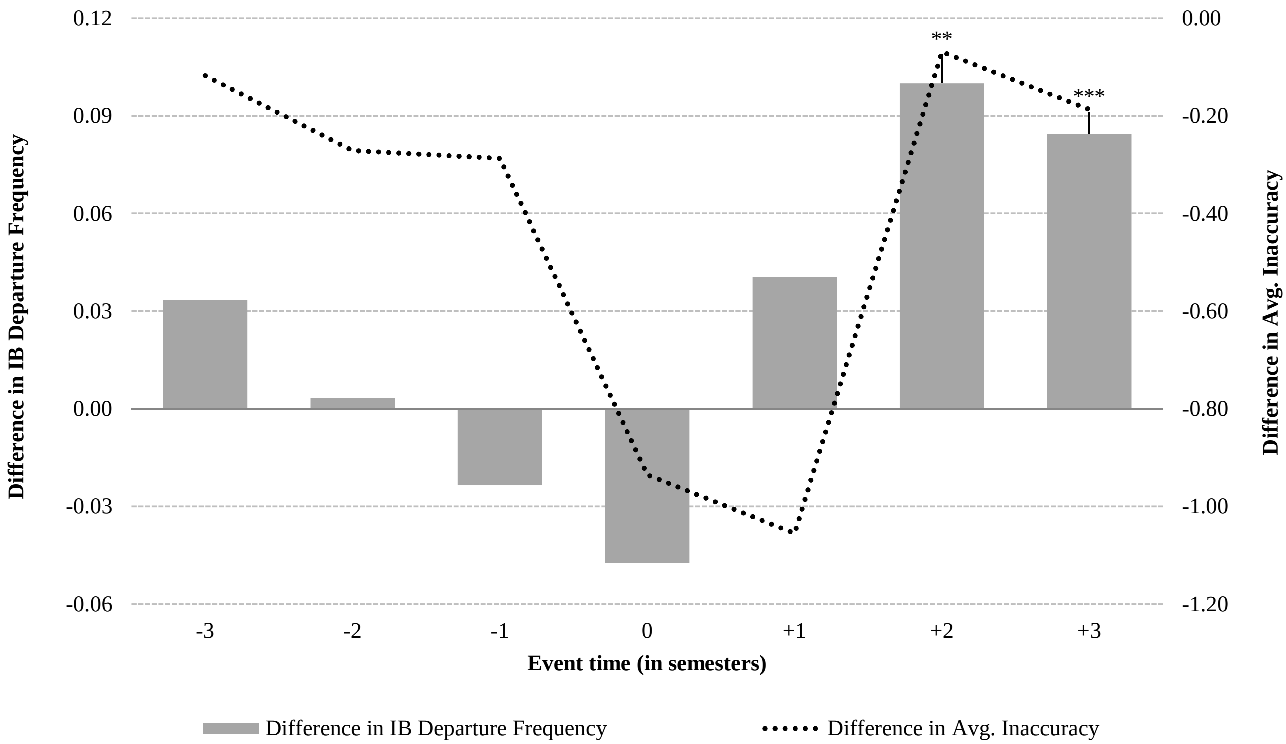


**Figure 3: Exploiting Variation in the Expected Supply of Investment Banking Jobs.** The graph plots the frequency of analyst departures to investment banks and average analyst inaccuracy around the event where an investment bank is listed as lead underwriter in a given collateral group for the first 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, where standard errors are calculated following the method of Newey–West (1987) with four lags. The dotted line plots the coefficient estimates reported in Table 6, column (1), 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** |
|---|---|
| *Key dependent variables* | |
| IB Exit | Indicator function equal to one if the analyst departs to an investment bank following her 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$_{t+1}$ | Indicator function equal to one if the analyst departs to an investment bank by the end of the following semester. 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. |
| Promotion$_{t+1}$ | Indicator function equal to one if the analyst is internally promoted (i.e., moves to a higher job title) in the following semester. Analyst job titles are obtained from press releases on Moody's website and categorized as in Kisgen, Osborn, and Reuter (2016). |
| *Measures of Analyst (In)Accuracy* | |
| Inaccuracy (baseline) | 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 by taking the arithmetic mean. Rating adjustments are obtained from Moody's website. |
| Avg. 3(6)-yr excess losses | The average absolute difference between the cumulative tranche losses, i.e., the principal balance write-offs due to default, after three (six) 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 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. 3-yr 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 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. |
| *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 her 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. |

*Continued on next page*

**Table A.1 – continued**

| Variable | Description |
|---|---|
| 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. |
| 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. |
| *Other variables* | |
| Relative pessimism (deal level) | Variable equal to (minus) one if Moody's rating is more pessimistic (optimistic) than the average of S&P and Fitch, and zero otherwise, averaged across all tranches in a given deal. Initial ratings are obtained from Bloomberg. |

# Appendix B. Moody's Organizational Structure

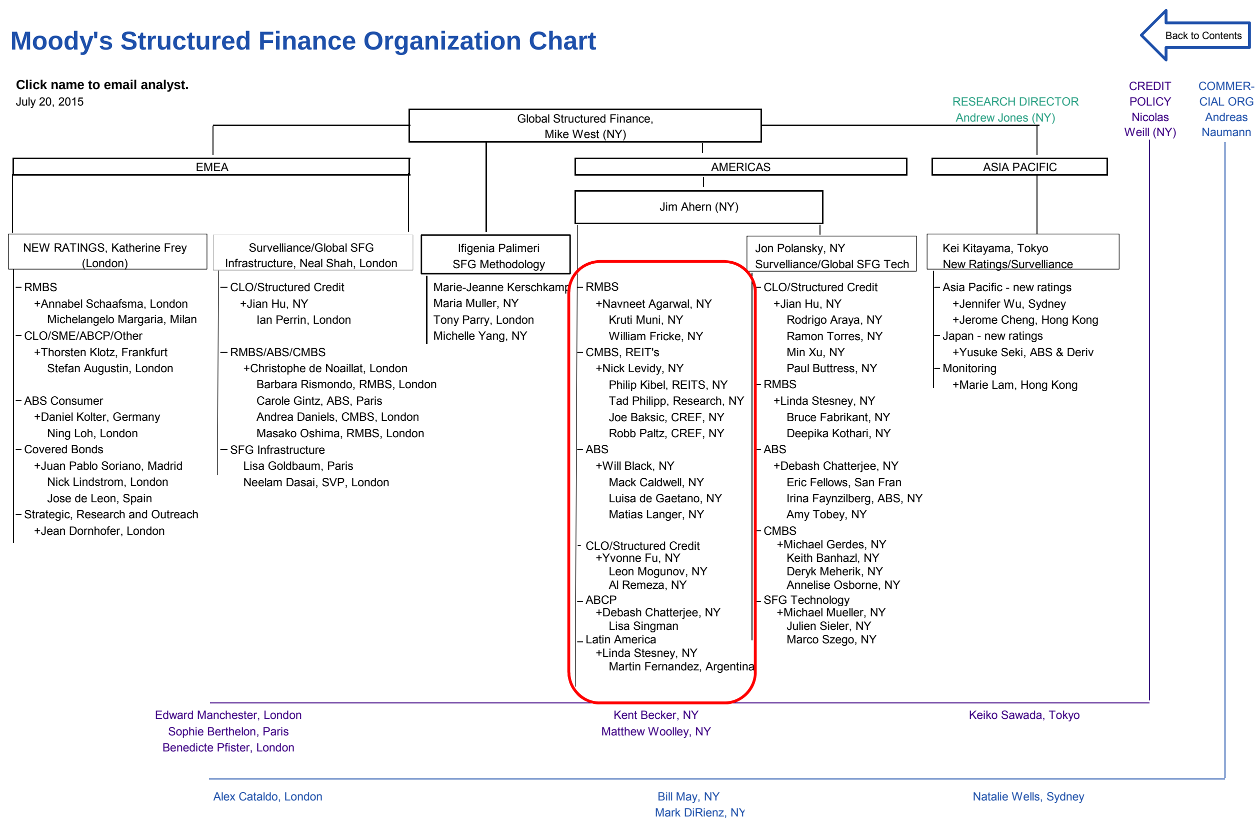


**Figure B.1: Moody's Organizational Structure in Structured Finance.** The chart shows the organizational structure of the Structured Finance team at Moody's as reported on Moody's website (available at `https://www.moodys.com/research/Structured-Finance-Ratings-Quick-Check-Newsletter--PBS_SF161380`).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