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

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Abstract

Investment banks frequently hire credit analysts from rating agencies. A widely held view is that this "revolving door" undermines analysts' incentives to issue accurate ratings. Using a hand-collected dataset of the performance and career paths of 229 credit rating analysts between 2000 and 2010, I provide new evidence on the effects of revolving doors on analyst incentives. Analysts who eventually move to investment banks are on average more accurate than other analysts rating similar securities at the same point in time. A notable exception is the small fraction of securities underwritten by their future employers, where revolving analysts do not outperform. My empirical design ensures that the results are not driven by selection of smart individuals into investment banking jobs, or by endogenous matching of revolving analysts to securities. My findings suggest that investment banks are able to identify and systematically hire more accurate analysts, thereby strengthening their incentives to issue accurate ratings. This positive incentive effect may explain why revolving doors have remained open in many professions, despite the public criticism they have attracted.

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

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"The implication of Dodd-Frank is that if you can just clamp down on rogue and conflicted analysts, the credit-rating industry will be reformed."
William Harrington, former Moody's employee, in Wall Street Journal (2011)

1. Introduction

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

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

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

broadly, for understanding how concerns about future career prospects affect performance incentives.

The first challenge for empirical studies of revolving doors is that data on individual monitoring performance are scarce. The second challenge is identification because we do not observe how a monitor would have performed in the absence of revolving doors. The performance of non-revolving monitors provides a useful counterfactual, but such a comparison is complicated due to a number of potentially confounding factors. First, comparing the performance of monitors across time is problematic due to cohort effects and time-varying task environments. Second, even at the same point in time, monitors may be assigned to projects with different characteristics and levels of difficulty. Third, there could be unobserved heterogeneity across individuals. For example, we may observe that revolving monitors outperform not because they work harder but because they are inherently smarter.

This study overcomes these empirical challenges by assembling a novel hand-collected dataset that tracks the career paths of 229 credit rating analysts at Moody's and links them to 22,188 securitized finance securities they rate between 2000 and 2010. In particular, I identify which analysts join an investment bank following their employment at Moody's. This empirical setting is ideal for studying revolving door effects for several reasons. First, credit ratings represent a publicly observable and relatively frequent measure of monitoring output by individual analysts. Subsequent corrections of the initial ratings issued by these analysts provide a useful proxy for analyst (in)accuracy. An attractive institutional feature of Moody's organization is that subsequent rating adjustments are generally performed by a separate internal surveillance team and are therefore not under the influence of the initial analyst. Second, I can identify the revolving door effect by comparing the performance of revolving and non-revolving analysts rating *similar* securities *at the same point in time*, while controlling for a rich set of observable and unobservable differences in the characteristics of these securities. Non-revolving analysts at the same rating agency and the same point in time provide a useful counterfactual because they face the same organizational environment and similar tasks, objectives, and other career

concerns. Fourth, rating analysts produce relatively many output signals compared to other professions in the regulatory environment, such as lawyers, who usually work on few cases during their career. This feature of the data allows me to exploit changes in performance within the same individual and to separate incentive effects from the effect of time-invariant unobserved heterogeneity across analysts.

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

My findings are broadly consistent with the human capital view of revolving doors. Prior to their departure to investment banks, analysts are significantly more accurate than other analysts rating similar products at the same point in time. An important feature of my data is that I can remove time-invariant heterogeneity across analysts by including analyst fixed effects, which ensures that my results are not driven by the selection of smart individuals into investment banking jobs. In addition, my results are robust to alternative measures of analyst accuracy, different subperiods, and estimation methods. Further tests exploiting the cross-section of securities rated by revolving analysts show that the effect of the revolving door is not unambiguously positive. Consistent with a bias of revolving analysts in favor of their future employers (see Cornaggia, Cornaggia, and Xia (2015)), they do not outperform on the securities underwritten by their future employers. However, given that these securities represent less than 7% of all securities rated

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

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

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

by revolving analysts, they do not lead to economically sizable distortions in their aggregate performance.

A number of additional tests support the interpretation that revolving analysts outperform because of enhanced analyst effort. First, the outperformance of revolving analysts is larger for more complex securities, where one would expect analyst effort to matter more. Second, as opposed to a stable or gradual outperformance, I observe a sudden and strong improvement in the performance of revolving analysts during the last year prior to their departure. This performance improvement is unrelated to the analysts' tenure at the time of their exit, which makes an alternative explanation based on differential analyst learning unlikely. Third, I exploit variation in the supply of investment banking jobs as an exogenous shock to analysts' likelihood of moving to an investment bank. I find that positive shocks to the supply of investment banking jobs increase average analyst performance and, in the cross-section of analysts, affect more strongly analysts who are ex-ante more likely to switch career.

While my main tests are designed to address identification issues, Figure 1 shows that two important insights emerge even from the raw data. The figure plots the number of analyst departures to investment banks and the average outperformance of departing analysts for five subperiods. First, analysts who depart to investment banks issue ratings that require fewer subsequent adjustments than ratings issued by other analysts (ca. 0.4 notches on average). Second, in most subperiods the average outperformance of revolving analysts increases monotonically with the hiring intensity by investment banks as measured by the number of departing analysts. Hence, even the raw data are supporting the human capital view of revolving doors.

Overall, my findings suggest that revolving doors may *on average* lead to improved, rather than reduced monitoring performance. This may explain why, despite the frequently voiced concerns, revolving doors have remained open in most professions. My results also imply that conflicts of interest arising from revolving doors are unlikely to have been a major driver of poor ratings quality in securitized finance prior to the financial crisis, despite the claims by some regulators and the public press. On the contrary, they suggest that the option to switch to a

career in investment banking may represent a strong incentive for credit analysts to perform well, and that restricting the revolving door without changing other aspects of analyst compensation may lead to lower ratings quality. An excessive regulatory focus on conflicted *individual* analysts may further be detrimental if it shifts the regulator's attention away from addressing first-order drivers of poor ratings performance in securitized finance, as suggested in the opening quote of this paper.[5]

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 credit and equity analysts have focused on the collusion view. The study most closely related to mine is Cornaggia, Cornaggia, and Xia (2015), who find that credit rating analysts award inflated ratings to their future employers prior to the employment transfer. My study confirms their results on the subset of securities underwritten by transitioning analysts' future employers, but shows that this effect is dominated by their higher accuracy on other securities. For equity analysts, Cohen, Frazzini, and Malloy (2012) report that sell-side analysts who get appointed as independent directors are overly optimistic and poor relative performers, and Horton, Serafeim, and Wu (2015) document that sell-side analysts who issue more biased forecasts for potential future employers are rewarded with favorable career outcomes. Studies of revolving doors in other contexts report mixed results. Supporting the collusion view, Spiller (1990) finds that regulators who preside over more lenient regulatory periods are more likely to get jobs in the industry, and i Vidal, Draca, and Fons-Rosen (2012) show that revolving door lobbyists' main asset is selling access to powerful politicians rather than regulatory expertise. Other studies support the human capital view of revolving doors. For example, deHaan, Kedia, Koh, and Rajgopal (2015) show that private law firms hire harsher SEC lawyers, and Cohen (1986) finds that private firms hire

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

regulators who are generally less supportive of the industry. In addition, Lucca, Seru, and Trebbi (2014) document that gross worker outflows from the regulatory to the private sector are higher during times of higher enforcement activity, and Shive and Forster (2015) show that financial firms take significantly less risk after hiring former regulators.

2. Theoretical Framework and Empirical Strategy

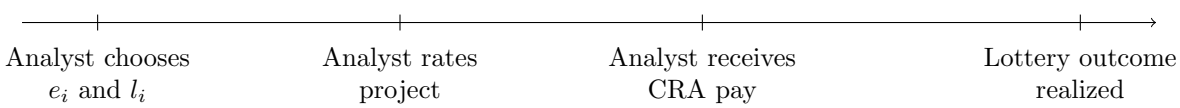
The goal of this section is twofold. First, I provide a parsimonious framework that illustrates the human capital view of revolving doors and that predicts the main effect that I document in this paper. The partial equilibrium model features heterogeneous analysts working at a credit rating agency and a revolving door between the rating agency and an investment bank. I show that the presence of a revolving door can have positive effects on the ex-ante incentives of analysts to exert effort while they are employed at the credit rating agency, as in Bar-Isaac and Shapiro (2011) and Che (1995). Second, I use the model to derive testable cross-sectional predictions and to point out some key empirical challenges.

2.1. Theoretical Framework

Consider a credit rating agency (CRA) that employs a group of heterogenous analysts who each rate a project during their term. Analyst i chooses to exert effort $e_i \in [0, 1]$, incurring a cost $e_i^2/2a_i$, where a_i denotes the innate ability of the analyst and is uniformly distributed over the interval $[\underline{a}, \bar{a}]$. The cost of effort is therefore increasing and convex in e_i, as in Bar-Isaac and Shapiro (2011), and decreasing in individual ability. If the rating by analyst i turns out to be accurate, which occurs with probability e_i, the CRA pays him w_{CRA}.

The analyst also decides whether he wants to participate in a lottery to be selected for a job at the investment bank (IB) after his term at the rating agency. The decision to participate in the lottery is indexed by l_i, which is equal to one if the analyst participates, and zero otherwise.

Conditional on participating in the lottery, the probability of being hired by the investment bank is $p \in [0, 1]$.[6] Switching career is assumed to be costly as in Bond and Glode (2014): analysts who choose to become investment bankers incur a fixed cost c.[7] The expected utility of post-CRA employment at the investment bank is equal to $e_i w_{IB}$, where w_{IB} represents the expected rent from the investment banking job. Analysts are risk-neutral and have a discount rate of zero. The sequence of events is depicted in the figure below:



To sum up, my simple model relies on the following assumptions.

Assumption 1. *Analysts are heterogenous in their innate ability, i.e.,* $\underline{a} < \bar{a}$.

Assumption 2. *Switching career to investment banking is costly, i.e.,* $c > 0$.

Assumption 3. *Analysts' expected utility in an investment banking job is increasing in* e_i. *Specifically, it is equal to* $e_i w_{IB}$.

Assumption 3 follows Bar-Isaac and Shapiro (2011) and can be justified by anecdotal evidence that expertise in rating securitized finance securities is very valuable to investment banks (see, for example, Financial Times (2007)). Assuming the expected utility in an investment banking job to be linear in e_i is convenient but can be relaxed: the option to switch to investment banking will have positive effects on analysts' ex-ante performance incentives as long as there is some positive correlation between the analyst's performance at the CRA and his expected utility in

[6] Following Bar-Isaac and Shapiro (2011), I assume that the probability of getting an investment bank job does not depend on e_i. Hence, my model reflects the possibility that investment banks may not directly observe analyst effort or performance. Alternatively, the probability of getting an investment banking job can be modeled to increase in the analyst's effort at the rating agency (see, for example, Che (1995)). This would be an alternative way of interpreting my results.

[7] The switching cost can be interpreted as a decrease in productivity, a direct disutility from relocating (see Bond and Glode (2014)), or a behavioral aversion against change or uncertainty. The implication of the switching cost is that not all analysts may prefer switching to investment banking after their employment at the CRA.

an IB job (see Che (1995)). The expected utility of analyst i is therefore:

$$U(e_i, a_i, l_i) = e_i w_{CRA} - e_i^2/2a_i + l_i(p(e_i w_{IB} - c)) \tag{1}$$

For each analyst i, the condition under which he chooses to participate in the lottery is given by $e_i w_{IB} > c$, implying the following optimal choice of l_i^*:

$$l_i^* = \begin{cases} 1 & \text{if } e_i > \frac{c}{w_{IB}}. \\ 0 & \text{if } e_i \leq \frac{c}{w_{IB}}. \end{cases} \tag{2}$$

Hence, only analysts with effort e_i above a certain threshold would choose to participate in the lottery. Analysts with effort e_i below the threshold would never benefit from switching careers, as their expected rent from the IB job would not be large enough to offset the switching cost c. These analysts would therefore never choose to enter the lottery irrespective of the probability of being selected. Maximizing equation (1) with respect to effort e_i yields the following optimal choice of e_i as a function of the analyst's innate ability a_i:

$$e_i^* = \begin{cases} w_{CRA} a_i & \text{for } l_i = 0 \\ (w_{CRA} + p w_{IB}) a_i & \text{for } l_i = 1 \end{cases} \tag{3}$$

Note that analysts who choose to enter the lottery systematically exert greater effort than those who choose not to enter the lottery, i.e., $(e_i^*(a_i)|l_i = 1) > (e_i^* a_i|l_i = 0)$. In addition, the optimal effort choice for those who choose to enter the lottery, $(e_i^*|l_i = 1)$, increases in the probability p of being hired by the investment bank.[8] This is the first positive effect of the revolving door. Combining equations (3) and (2) allows me to solve for the threshold ability level a_L above which analysts choose to participate in the lottery ($l_i^* = 1$) and exert relatively

[8]This claim is immediate on taking the derivative of $(e_i^*|l_i = 1)$ with respect to p.

more effort:

$$a_L \equiv \frac{c}{w_{IB}(w_{CRA} + pw_{IB})} \tag{4}$$

The threshold ability level increases in the switching cost c and decreases in the rent from the investment banking job w_{IB}. More importantly, it also decreases in the probability p of being hired by the investment bank. This is the second positive effect of the revolving door: more analysts exert a greater effort when the prospects of being hired by the investment bank are high.

2.2. Key Predictions and Empirical Approach

The main prediction arising from my theoretical framework above is that analysts at the CRA perform better in the presence of the revolving door, i.e., when they have the option to participate in the lottery for an investment banking job. In other words, the average causal effect of the revolving door on the performance of analysts who choose to enter the lottery ("the treated") is positive (as proven in Appendix A.1):

$$ATT = E(e_i|l_i = 1, a_i > a_L) - E(e_i|l_i = 0, a_i > a_L) = pw_{IB}0.5(a_L + \bar{a}) > 0 \tag{5}$$

The main challenge for empirical studies of revolving doors is that the counterfactual performance in the absence of the possibility to be selected for an IB job, $E(e_i|l_i = 0, a_i > a_L)$ in the above equation, is unobservable. Existing empirical studies have therefore resorted to using non-revolving monitors as a natural control group (see, for example, Cohen (1986), Spiller (1990), Cornaggia, Cornaggia, and Xia (2015), and deHaan, Kedia, Koh, and Rajgopal (2015)). However, comparing ex-post differences in performance between revolving and non-revolving analysts does not yield an unbiased estimate of the average causal effect of revolving doors (henceforth abbreviated as ATT). In the following, the event of becoming a revolving analyst is indexed by D_i, which is equal to one if the analyst is eventually selected for an IB job, and zero other-

wise. Observed differences in performance between revolvers and non-revolvers are linked to the average causal effect by the following equation (as proven in Appendix A.2):

$$\begin{aligned} E(e_i|D_i=1)-E(e_i|D_i=0) = & \underbrace{pw_{IB}0.5(a_L+\bar{a})}_{\text{ATT}} \\ & + \underbrace{w_{CRA}0.5(a_L+\bar{a}-\theta(a_L+\bar{a})-(1-\theta)(\underline{a}+a_L))}_{\text{Selection bias}} \\ & - \underbrace{\theta pw_{IB}0.5(a_L+\bar{a})}_{\text{Attenuation bias}}, \end{aligned} \tag{6}$$

where θ refers to the share of lottery entrants in the population of non-revolving analysts. The selection bias is driven by the fact that revolving analysts are not randomly drawn from the population of analysts. They have a higher average baseline ability and, hence, would have performed better than the average analyst in the control group even in the absence of revolving doors. This selection therefore creates an upward bias in the estimation of the ATT. Since the control group contains some "treated" analysts who also entered the lottery but were not selected for an IB job, there will also be some attenuation bias. Attenuation bias is not a major concern because it will bias the estimate of the ATT downward.

Once we are able to condition on individual baseline ability, observed differences in performance between revolving and non-revolving analysts provide a lower bound of the average causal effect of interest: $E(e_i|D_i=1,a_i)-E(e_i|D_i=0,a_i) \leq ATT$. In other words, we are only left with attenuation bias. Conditioning on individual baseline ability requires panel data, i.e., repeated observations on individual analysts. With panel data, we can remove the problem of selection bias by comparing the performance of revolving and non-revolving analysts while controlling for unobserved analyst heterogeneity through analyst fixed effects:

$$e_{it} = \lambda_i + \delta D_{i,t+h} + \epsilon_{it}, \tag{7}$$

where e_{it} is the performance of analyst i in period t, $D_{i,t+h}$ is an indicator equal to one if the

analyst is selected for an investment banking job within the next h periods, and λ_i are individual fixed effects. The human capital view predicts that δ in the above regression is positive, which is the focus of my main tests.

An alternative empirical approach to assess the effect of revolving doors on analyst performance is to exploit changes in the probability of being hired by an investment bank (parameter p). Consider, for example, a change in p from p_1 to p_2, where $p_2 > p_1$. In my theoretical framework, this change leads to a weakly positive average change in analyst performance, i.e., $E(e_i|p_2) - E(e_i|p_1) \geq 0$ (see Appendix A.3). However, changes in p that affect the performance of all analysts at the same point in time are empirically not separable from other unobserved time-varying factors also correlated with rating performance, such as the economic outlook, underwriting standards, product complexity, recruiting standards, etc. A suitable empirical analysis therefore requires variables that affect the prospects of *some* analysts to be hired by an investment bank, but not of others. In Section 5, I exploit the event of an investment bank entering a new segment of the securitized finance market as a proxy for an increase in the supply of investment banking jobs and for a positive shock to the probability of being hired by an investment bank for analysts working in that segment. In addition, I can test whether, in the cross-section, this change in supply affects some analysts more than others. Specifically, my theoretical framework predicts that there exits a group of low-ability analysts whose performance is insensitive to changes in p (see Appendix A.3).

3. Data

An important implication of the human capital view illustrated above is that revolving doors positively affect ex-ante analyst effort and, thus, the accuracy of 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 revolving doors on analyst performance. The reason is that *all* securities

benefit from revolving analysts building or showcasing their expertise, but potentially only *few* securities are helpful to curry favors to prospective employers. Hence, gauging the *net* effect of revolving doors requires analyzing the entire spectrum of securities rated by revolving analysts. In addition, the dataset should have two main features. First, as argued above, it needs to be a panel dataset with repeated performance measures at the individual analyst level in order to control for analyst heterogeneity. Such a dataset is not readily available, neither for monitors in general nor for credit analysts in particular.[9] To overcome this problem, I hand-collect a novel dataset that links individual analysts to the performance of the ratings they assign. Second, I need to be able to identify analysts who leave to investment banks after their employment at the rating agency. I collect this information from analysts' self-reported profiles on the professional networking website LinkedIn. 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 2010 that were initially rated by Moody's, because (i) data are sparse prior to 2000, (ii) my main measure of ratings accuracy requires three years of post-issuance performance data, and (iii) Moody's is the only rating agency that publicly discloses analyst names in the press release of a new rating on its website.[10] In addition to the name of the lead analyst responsible for the initial rating, 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, the identity of his first employer following the employment at Moody's, as well as information on previous employment, graduate, and undergraduate education. I am able to track a total of 229 analysts. As shown in Table 1, Panel B, 63 out of these 229 analysts subsequently go work for an investment bank

[9] 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 lead analyst responsible for the rating by a given rating agency.

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

that was ranked in the prestigious "The Bloomberg 20" ranking in the year prior to their exit,[11] 88 analysts leave to other employers, and 78 analysts have not left Moody's as of December 2013. The aforementioned investment banks also capture a large fraction of the underwriting market in securitized finance: they underwrite more than 80% of the securities in my sample (see Table 1, Panel C). As shown in Table 2, analysts with fewer years of prior work experience, no graduate degree, an undergraduate degree from an institution located in New York City, and a non-law undergraduate degree are more likely to leave to an investment bank. Interestingly, graduates from Ivy League institutions are less likely to subsequently work for an investment bank, although this relationship is not statistically significant.

As reported in Table 1, Panel A, my final dataset consists of 22,188 tranches from 4,520 securitized finance deals. All securities combined account for an aggregate issuance volume of ca. $2.5 trillion, which represents at least 35% 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).[12] Using similar categories as in Griffin, Lowery, and Saretto (2014), I classify securities depending on the type of the deal's underlying collateral into eight collateral groups and three main market segments (asset-backed securities (ABS), mortgage-backed securities (MBS), and collateralized debt or loan obligations (CDO/CLO)). Classifying all securities by collateral type is important for my empirical approach of comparing performance across analysts, which is described in further detail below.

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. While it is not uncommon that analysts rate securities underwritten by their future employers, the majority of analysts who get hired by

[11] 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 2 provides an overview of the top hiring banks in my sample. Table 4, Panel B, shows that my main findings are robust to alternative definitions of investment banks.

[12] Since SIFMA does not report agency asset-backed securities separately, I compute the aggregate deal volume as the sum of $4.6 trillion of non-agency mortgage-backed securities and $2.4 trillion of asset-backed securities (agency and non-agency). Hence, the 35% represent a lower bound estimate of the covered market share.

investment banks never rate securities of their future employers during their employment at Moody's (see Table 1, Panel B). As a result, securities underwritten by the future employer represent less than 7% of all securities rates by the average revolving analyst (see Table 1, Panel C).

3.1. Measuring and Comparing Analyst Performance

My main measure of rating (in)accuracy is the number of notches that the initial rating of a tranche has to be adjusted in the first three years after issuance, while controlling for observable tranche and deal characteristics. Defining accuracy based on subsequent rating actions is advantageous for two reasons. First, rating adjustments at Moody's are generally performed by a separate surveillance team and are therefore not under the influence of the analyst who assigned the initial rating.[13] Second, credit rating agencies claim that their ratings are designed to be long-term and forward-looking in the sense that they should anticipate ups and downs of the business cycle.[14] Rating actions within the first few years after issuance, as opposed to longer horizons, can therefore be attributed to trends or events that might have reasonably been anticipated by the analyst at the time of issuance. In addition, my empirical approach described below circumvents the problem that subsequent ratings adjustments may be driven by changes in the fundamentals of the underlying collateral that could not have possibly been foreseen by the analyst at issuance.

Comparing rating performance across analysts is non-trivial because of potential non-random assignment of analysts to securities. For example, analysts often specialize in one or few collateral types, which may exhibit different patterns in performance. Even within a given collateral

[13] Michael Kanef, former head of the Asset Backed Finance Rating Group at Moodys 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`.

[14] For example, Moody's writes the following about their approach to credit analysis: "As a rule of thumb, we are looking through the next economic cycle or longer. Because of this, our ratings are not intended to ratchet up and down with business or supply-demand cycles [...]" (available at `https://www.moodys.com/Pages/amr002003.aspx`).

type and date, analysts may be assigned to securities with special characteristics, e.g., complex subordination structures or poor collateral quality. To circumvent this difficulty, I use the following two-step procedure. In a first step, I compute for each security the "abnormal" level of subsequent rating adjustments after controlling for observable differences in tranche and deal characteristics:

$$Rating\,Adjustment_j = \beta_1' D_j + \beta_2' X_j + \eta_j, \tag{8}$$

where $Rating\,Adjustment_j$ is the absolute difference (in notches) between the initial rating of tranche j and the rating three years after issuance.[15] $D_j = (D_{Aaa}, D_{Aa1}, ..., D_C)$ is a vector of dummy variables indicating Moody's initial rating of the tranche, and X_j is a vector including tranche characteristics as well as characteristics of the corresponding deal. Tranche characteristics include the logarithm of the tranche principal value, level of subordination, weighted average life, coupon type, and an indicator equal to one if the tranche has an insurance wrap. Deal characteristics include 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 (2015), the level of overcollateralization, computed as the difference between the total collateral principal 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, the logarithm of the number of tranches in the deal, the logarithm of the average loan size (in USD), as well a vector of eight dummy variables marking the collateral type.[16] 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.

[15] In order to compute differences between ratings ("rating adjustments"), Moody's 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). In my robustness tests reported in Table 4, Panel A, I consider rating adjustments over alternative horizons (one and five years) and find similar effects.

[16] Since information on some tranche and deal characteristics (specifically, the level of subordination, the weighted average life, insurance wrap, geographical concentration, LTV ratio, credit score, and average loan size) are available only for a subset of my data, I replace missing observations and include additional indicators equal to one if information on a given variable is not available. My robustness test in Table 4, Panel C, shows that my approach of replacing and controlling for missing observations does not affect my results. In fact, they get stronger if I restrict my sample to tranches with information on characteristics that are most commonly available.

In a second step, I aggregate the residuals from the above regression into an (under)performance measure for each analyst i in a given collateral type z and semester t:

$$Inaccuracy_{izt} = \frac{1}{N} \sum_{j \in \mathcal{S}_{izt}} \widehat{\eta}_j \tag{9}$$

Defining performance (or inaccuracy) at the analyst $\times$ collateral type level instead of at the analyst level allows me to compare analyst performance on a subset of products that are more similar in their economic fundamentals and has three key advantages. First, despite the similar overall time-series pattern, there are notable differences in rating performance across different collateral types at the same point in time (see Appendix C, Figure C.1). For example, whereas other collateral types have largely recovered after 2007, RMBS and CMBS ratings continue to underperform. It is therefore important to control for differences in the ratings performance of the overall collateral type when comparing performance across different analysts at Moody's, because they may not be fully captured by the observable tranche and deal characteristics included in the first-step regression. Second, Moody's internal organization structure follows a similar division (see Appendix C, Figure C.2), which ensures that analysts rating securities of the same collateral type face similar incentives, rating methodologies, and management leadership. Third, it allows me to exploit variations in the supply of investment banking jobs across different collateral types and investigate how they affect analyst performance (see Section 5). I will implement the idea of comparing analysts rating securities of *the same underlying collateral type* at the *same point in time* by regressing my measure of analyst inaccuracy on collateral type $\times$ semester fixed effects (see equation (10)).[17]

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. Still, there may be good reasons to assume that investment banks observe signals about analyst performance that are unobservable to the econometrician but highly correlated with ex-post measures of

[17] While aggregating across all tranches rated by the same analyst in a given collateral type and semester 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 C.1, are both quantitatively and qualitatively very similar.

performance. First, underwriting investment banks directly interact with rating analysts during the ratings process. Second, they may receive signals through their social networks, e.g., other bankers who have directly worked with the analyst, former colleagues at Moody's, etc. While it is plausible that investment banks can observe signals of analyst performance, it is not a necessary condition to predict a positive incentive effect of revolving doors. As illustrated in my theoretical framework, assuming that the analyst's expected future pay at the investment bank is increasing in his skills as a credit rating analyst is sufficient for revolving doors to exert a positive influence on analysts' ex-ante incentives to enhance their qualifications.

Table 1, Panel C, reports descriptive statistics of my sample. Analyst inaccuracy, measured as the average abnormal 3-year rating adjustment of a given analyst in a given collateral type and semester, is roughly centered around zero and shows a substantial degree of variation, with a standard deviation of 4.3 notches.

3.2. Can Individual Analysts Influence Ratings?

A necessary condition for analyst incentives to play a role is that the ratings process for securitized finance products needs to provide sufficient room for individual analysts to affect the final rating of a security. This is not obvious given that the final rating decision is taken by a committee. Upon receiving a rating application from a potential customer, Moody's assigns a lead analyst to the ratings process. The lead analyst meets with the customer to discuss relevant information, which he subsequently analyzes with the help of Moody's analytical team. He then proposes a rating and provides a rationale to the rating committee, which consists of a number of credit risk professionals determined by the analyst. Once the rating committee has reached its decision, Moody's communicates the outcome to the customer and publishes a press release.[18] 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. Analysts guide meetings with

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

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.

How much individual analysts are able to influence ratings is ultimately an empirical question. Fracassi, Petry, and Tate (2015) show that individual analysts are important for corporate bond ratings: 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. Note that if individual analysts played no role in the ratings process, this would bias me against finding any significant differences in my across-analyst comparisons.

4. Main Results

This section presents my main results. I document that analysts who subsequently get hired by investment banks produce systematically more accurate ratings, as predicted by the human capital view of revolving doors. This difference in performance is robust to various measures of ratings accuracy, and is larger for complex securities where analyst effort should matter more. Additional tests confirm the interpretation that revolving analysts outperform because of enhanced effort.

4.1. Baseline Results

In order to gauge whether revolving doors strengthen or weaken analyst incentives to issue accurate ratings, I compare the performance of revolving and non-revolving analysts as follows. I first estimate analyst performance (or inaccuracy) in a given collateral type and semester using the two-step procedure described in Section 3.1. Then I regress this measure of analyst inaccuracy on an indicator equal to one if the analyst leaves to an investment bank within the next two

semesters ($IB\,Exit_{i,t+1yr}$):

$$Inaccuracy_{izt} = \lambda_i + \lambda_{zt} + \delta IB\,Exit_{i,t+1yr} + \beta' X_{izt} + \epsilon_{izt}, \tag{10}$$

where $Inaccuracy_{izt}$ stands for the average inaccuracy of all tranche ratings issued by analyst i in collateral type z and semester t. λ_i and λ_{zt} are analyst and collateral type $\times$ semester fixed effects, respectively, and X_{izt} represents a vector of additional controls. Specifically, X_{izt} comprises 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), the fraction of tranches underwritten by investment banks rated in "The Bloomberg 20" ranking,[19] as well as the average issuer market share.[20] All variables are defined in Appendix B. Note that since the dependent variable is analyst inaccuracy, the human capital view predicts $\delta < 0$ in the above regression. Standard errors are clustered at the analyst level.

Table 3, Panel A, reports the results. For comparison purposes, I also report results excluding analyst fixed effects in columns (1) and (2). Confirming the results from the simple sorts presented in Figure 1, analysts who leave Moody's to go work for an investment bank are on average 0.46 notches more accurate than other analysts rating tranches in the same collateral type and semester. When focusing on analyst performance during the last two semesters prior to the departure to the investment bank and including analyst fixed effects (columns (3) and (4)), the performance gap increases to 1.31 notches. This effect corresponds to 30% (= 1.310/4.34) of one standard deviation in analyst inaccuracy and is therefore economically sizable.

It is possible that, despite their aggregate outperformance, revolving analysts underperform on a subset of securities that are underwritten by their future employers. In order to test for the presence of such a potential bias, I interact the $IB\,Exit$ indicator with the fraction of tranches underwritten by the analyst's future employer. My coefficient estimates, reported in Panel B of

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

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

Table 3, imply that revolving analysts underperform by 0.53 notches in the extreme case where all tranches rated by the analyst are underwritten by his future employer (see column (4)).[21] This finding is consistent with evidence reported by Cornaggia, Cornaggia, and Xia (2015), who document that analysts give more favorable ratings to their future employers in the last quarters before their departure. However, securities underwritten by the future employer constitute less than 7% (see Table 1, Panel C) and therefore a small fraction of all securities rated by the average revolving analyst. Hence, this reduced accuracy is dominated by revolving analysts' outperformance on other securities. In addition, prior to the last year of their employment with Moody's, analysts who go work for investment banks are 1.36 notches more accurate on the securities of their future employers.

One may argue that an increase in performance prior to analyst departure is not specific to analyst transitions to investment banks but could be observed for any other employment transfer. To test this argument, I perform a placebo test using analysts who depart to other employers. In Panel C of Table 3, I find that analysts who depart to other employers perform, if anything, worse than other analysts during the last year of their employment at Moody's. This suggests that the possibility to go work for other employers is no perfect substitute for the possibility to be hired by an investment bank. A potential explanation is that credit rating skill may be particularly valuable for tasks required by the investment banks, such as structuring securitized finance deals ahead of public offerings, or that investment banks may have a superior skill in observing and evaluating the performance of analysts while they are employed at the rating agency.[22]

In sum, the results presented in this section show that analysts who subsequently get hired by investment banks systematically produce more accurate ratings, consistent with the human capital view of revolving doors. In the following, I show that these results are robust to alternative

[21] This point estimate is not statistically significantly different from zero.

[22] Such a special role of investment banks may be justified by the fact that rating analysts in securitized finance work very closely together with underwriting investment banks, as illustrated by Cetorelli and Peristiani (2012). When further refining the set of other employers, I observe an outperformance of similar magnitude for analysts who transfer to asset managers such as mutual funds or hedge funds (see Table C.2). However, given the small sample size of only 20 analyst transitions to asset managers, I cannot conclude that this outperformance is statistically significant.

measures of ratings accuracy and definitions of analyst departures to investment banks.

4.2. Robustness

Table 4 presents a number of robustness tests. Unless otherwise mentioned, I report results for the specification in Table 3, Panel A, column (4), and suppress all control variables for brevity. Panel A shows results for alternative measures of analyst performance. First, I aggregate tranches within each analyst and collateral type by value-weighting tranches by their principal amount instead of equal-weighting (see equation (9)), which produces economically similar estimates. As mentioned in the introduction, an attractive institutional feature of Moody's organization is that subsequent rating adjustments are performed by a separate surveillance team and are therefore not under the influence of the analyst who is responsible for the initial rating. In order to rule out potential exceptions to this rule, I compute a measure of analyst inaccuracy using only subsequent rating actions performed by different analysts than the one responsible for the initial rating.[23] The resulting estimates are very similar to my baseline, suggesting that the effect cannot be explained by bias in the ex-post adjustment of the initial ratings issued by revolving and non-revolving analysts. While effects are somewhat smaller if I look at rating adjustments over the first year of issuance only, they are similar when looking at a five-year horizon. Ratings by revolving analysts have both fewer downgrades and upgrades, but the effect is almost three times larger for downgrades. Hence, revolving analysts are not only more accurate, they also tend to be more pessimistic. I also see that securities rated by revolving analysts are less likely to be downgraded 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. Next, I measure ratings accuracy based on abnormal cumulative tranche losses over three years, which dramatically reduces the sample size but yields a result of similar economic magnitude. Abnormal cumulative tranche losses are computed as the absolute

[23]Since there are very few exceptions to the rule of assigning a separate surveillance analyst in my sample, I obtain a correlation coefficient of more than 98% between the two inaccuracy measures.

difference between the realized tranche loss and Moody's expected loss benchmark for the same rating category (see Moody's Investor Service (2001)). This result is very important for two reasons. First, since it does not rely on any adjustment for tranche characteristics, it shows that my results are not sensitive to the linear model for rating adjustments in equation (8). Second, cumulative tranche losses represent a measure of rating performance that does not require action on behalf of Moody's surveillance team. Finally, I also test two proxies of ratings accuracy that can be measured in real time. First, I use an indicator equal to one if the average tranche rated by the analyst has been rated by more than two rating agencies as a proxy for rating quality. The motivation for this measure is that tranche ratings by all three agencies are less likely to be shopped (see, for example, He, Qian, and Strahan (2015)). Consistent with my main finding that ratings issued by analysts who leave to investment banks are more accurate, they are also less likely to be shopped. Second, I show that the average initial yield of AAA-tranches rated by revolving analysts tend to be lower, suggesting that investors recognize their higher quality.

Panel B shows that I obtain very similar results if I consider alternative definitions for my key independent variable of interest, $IB\,Exit_{i,t+1yr}$. In the first two rows, I change the time horizon prior to the analyst's departure to six months and two years, respectively. The resulting estimates are very similar to my baseline coefficient. In the next two rows, I use alternative definitions for the set of hiring investment banks. Departures to investment banks in "The Vault Banking 50" ranking by prestige[24] and departures to the former five pure-play investment banks[25] yield similar, though statistically somewhat weaker results. In order to address potential concerns that my results may be specific to tranches issued during or shortly before the crisis, I show in Panel C that my findings survive if I only include tranche ratings issued before 2006. When I restrict my sample to tranches with complete information on the most commonly available tranche characteristics included in equation (8), the statistical significance of my results increases. Panel

[24]Since "The Vault Banking 50" ranking by prestige is available only from 2008 onwards and comparable rankings are fairly stable prior to that year, I use the 2008 ranking, which is available at `http://www.vault.com/company-rankings/banking/most-prestigious-banking-companies/?sRankID=162&rYear=2008`.

[25]The former five pure-play investment banks include Bear Stearns, Goldman Sachs, Lehman Brothers, Merrill Lynch, and Morgan Stanley.

D shows that my results are not sensitive to the estimation method. A propensity score matching approach yields very similar estimates.

To sum up, I conclude that my main result is robust to various measures of analyst performance and definitions of analyst departures to investment banks.

4.3. The Influence of Deal Complexity

If my previous results are driven by enhanced rating effort by analysts who aspire to work for an investment bank, then one would expect the marginal impact of their additional effort to be larger for deals that are harder to rate. This section tests this hypothesis by interacting my main independent variable of interest, $IB\,Exit_{i,t+1yr}$, with different measures of average deal complexity.

Table 5 reports the results for different proxies for deal complexity. The first proxy is the average fraction of loans with low documentation, since it is arguably more challenging to rate deals with less tangible information about the quality of the loans in the underlying collateral. The second measure is the absolute skewness of the credit score distribution of the underlying loans. Anecdotal evidence reported by Lewis (2011) suggests that one of the many factors why securitized finance ratings were off-target was that they focused too much on average credit scores rather than on their full distribution. More diligent analysts may have taken the skewness of the underlying credit score distribution into account in their rating recommendation. The third proxy is the deal complexity measure proposed by He, Qian, and Strahan (2015) ("HQS"), which is computed as the number of tranches in a deal divided by the combined principal amount of the tranches.

All measures indicate that revolving analysts outperform more when they rate more complex deals. A one-standard-deviation increase in the average fraction of low-documentation loans increases the outperformance of revolving analysts by 0.6 ($= -2.494 \times 0.24$) notches, and a one-standard-deviation increase in the average absolute skewness of the credit score distribution

increases their outperformance by 0.7 ($= -6.200 \times 0.12$) notches. While the interaction term in column (3) is not statistically significant, my estimates indicate that a one-standard-deviation increase in average deal complexity leads to an economically sizable increase in the performance gap of 1.1 ($= -2.595 \times 0.42$) notches. Overall, the results are consistent with the intuition that enhanced rating effort should matter more for deals that are harder to rate.

4.4. Alternative Explanations

My approach of comparing analyst performance both within the same analyst as well as across analysts rating similar securities at the same point in time rules out the impact of a number of potentially confounding factors suggested by the prior literature, most notably analyst baseline skill and non-random assignment of analysts to securities. In this section, I address two potential alternative explanations for my main result that analysts who get hired by investment banks outperform. First, could there be unobserved differences in learning across analysts? Second, could my results be driven by disincentives within Moody's organization rather than by positive incentives from revolving doors?

4.4.1. Unobserved Differences in Learning

Heterogeneity across analysts can lead to unobserved differences in the speed at which analysts learn. Hence, a potential concern could be that analysts who get hired by investment banks outperform because they have been learning at a faster rate than other analysts. Note that such a differential learning story would still be inconsistent with the collusion view and support the view of revolving doors as an economic mechanism that allocates skill to jobs with higher returns to skill. However, unlike the human capital view, it does not predict that rating analysts work harder in the presence of revolving doors. In this section, I present two pieces of evidence which are supportive of the human capital view and less consistent with unobserved differences in analyst learning.

First, a differential learning story would predict that revolving analysts gradually start to outperform over their tenure at the rating agency. To test this prediction, I split the observations of revolving analysts by the remaining time until their departure to the investment bank. Rather than a gradual improvement in performance, I observe a large and sudden increase in the performance of revolving analysts shortly before their departure (see Table 6, Panel A). There is no economically or statistically significant difference in performance during the early and middle stages of their tenure at Moody's. To further illustrate that revolving analysts outperform only shortly before their transition, I perform a placebo test where I replace the analyst's actual departure date with a random date between the start and end date of his employment at Moody's. Then I re-run the baseline regression presented in Table 3, Panel A, column (4), and obtain a placebo coefficient. Figure C.3 plots the distribution of placebo coefficients after 1,000 runs. The null hypothesis that the baseline coefficient is drawn from the distribution of placebo coefficients is rejected at the 1% level.

Second, if analysts get hired by investment banks because they have been on an accelerated learning path, then one would expect the outperformance of revolving analysts during their last year to be attenuated if their tenure at Moody's has been very long. The incentive story, on the other hand, would predict outperformance to increase prior to the analyst's departure irrespective of the analyst's tenure. To test these different predictions, I repeat the analysis presented in Table 3, Panel A, column (4), by categorizing revolving analysts based on their tenure at the time of their departure to the investment bank. As reported in Table 6, Panel B, the outperformance of revolving analysts during their last year of employment at Moody's remains high even for the quartile of analysts with the longest tenure at exit, who have been with Moody's for ca. fourteen years.

4.4.2. Disincentives at Moody's

A second potential concern could be that my results reflect disincentives within Moody's organization as opposed to positive incentives from revolving doors. For example, if Moody's

organization was strongly focused on expanding the company's market share, as suggested in the report by the Financial Crisis Inquiry Commission (2011),[26] it may have punished analysts who issued accurate ratings by not promoting them or by withholding their bonus. This interpretation cannot explain why accurate analysts may choose to seek employment elsewhere. However, it does not explain why only analysts hired by investment banks outperform and not the average analyst who transitions to other employers. The evidence reported in Table 3, Panel C, is therefore not consistent with this story.

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. The results, shown in Table 7, do not support the conjecture that Moody's punishes analysts for being accurate. Analyst who get promoted at Moody's are on average more accurate than other analysts rating similar securities at the same point in time. However, the relationship between performance and internal promotions is substantially weaker, both in economic and statistical terms, than the previously documented relationship between performance and departures to investment banks.

5. Variation in the Supply of Investment Banking Jobs

In this section, I provide additional evidence for the human capital view of revolving doors by exploiting how variation in the supply of investment banking jobs affects analyst performance. This complimentary approach is advantageous because changes in the supply of investment banking jobs provide exogenous shocks to the probability of an analyst to be hired by investment banks. Most importantly, they are unrelated to analysts' individual baseline skill, learning paths, and other career concerns.

I use the event of a new underwriting investment bank entering a collateral group as a shock

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

to the supply of investment banking jobs. This event is useful for identifying the effect of changes in the supply of investment banking jobs for two reasons. First, since an investment bank may choose to enter only one collateral group at a time and not others, I can compare how the performance of analysts in that collateral group changes relative to the performance of analysts in other collateral groups that are not affected. Second, I can exploit whether, in the cross-section of analysts within the same collateral group, analysts with certain characteristics are more affected by the event than others. Specifically, my theoretical framework predicts that low-ability analysts and, more generally, analysts who are ex-ante less likely to leave to investment banks should be less affected by fluctuations in the supply of investment banking jobs (see Section 2.2). Exploiting these cross-sectional differences is important in order to rule out that my findings are driven by unobservable factors that are driving both analyst performance and investment bank entry (e.g., economic fundamentals), 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 supply of investment banking jobs in the area of Student-loan ABS, both from Goldman as well as from other investment banks who may decide to follow, and thus the likelihood for analysts rating Student-loan ABS at Moody's to transition to an investment bank in the near future. In contrast, and by construction, the supply of investment banking jobs in Auto-loan ABS is not affected. I can therefore identify the impact of changes in the likelihood of being hired by an investment bank on analyst incentives by analyzing changes in the performance of analysts in Student-loan ABS and in Auto-loan ABS around the time of the investment bank entry.

To identify collateral group and semester observations where a new underwriting investment bank enters the 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 a collateral group to undergo an investment bank entry event in a given semester if an investment bank starts underwriting securities in that collateral group for the first time.[27] This yields 54 investment bank entry events. In order to verify that these events are indeed associated with an increase in the supply of investment banking jobs, I plot the difference in the average number of analysts who depart to investment banks between the event collateral group and the control collateral group in event time. As shown in Figure 3, the number of analyst departures jumps significantly in the semester where an investment bank enters a new collateral group and remains elevated for the three following semesters. This pattern suggests that the entry of an investment bank is indeed a good proxy for more aggressive hiring by investment banks.

Next, I look at how analyst performance changes around the event. To this end, I regress my main measure of ratings inaccuracy on a set of nine event-time dummy variables labeled $t-4$, $t-3$, ..., $t+3$, $t+4$, where my convention is that dummy t takes on the value one in the collateral group and semester in which an investment bank entry event occurs. 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, in addition to analyst fixed effects and the same control variables as in Table 3, Panel A. Table 8, Panel A, and the red line in Figure 3 show the results. Two things are worth noticing. First, analysts in the event group outperform those in the control group between semesters $t-3$ to $t+2$, but perform similarly at the very beginning and at the very end of the event window. Second, and consistent with analysts anticipating the investment bank entry and the associated higher chances to move to investment banking, analyst performance starts to increase a few semesters before the event, reaches its peak in $t-1$, and then falls back to normal levels.[28]

[27] I consider as investment bank underwriters all underwriters that at some point during my sample period appear in "The Bloomberg 20" investment bank ranking.

[28] The finding that analysts are able to anticipate the investment bank entry is not surprising. According to former rating analysts, it usually takes several months to complete a ratings process, which means that analysts at Moody's who are working on the new deal will know about the investment bank entry well in advance. In addition, analysts might learn about the plans of an investment bank to enter a new collateral group even before that, either through talks with investment bankers, or through the media.

Next, I investigate whether the increase in the likelihood of being hired by investment banks affects the performance of some analysts more than others. Specifically, the performance of analysts who are ex-ante less likely to move to an investment bank, such as analysts with low baseline ability, and analysts whose career path depends less on their ratings performance, should be less sensitive to changes in the outside option. In order to test this prediction, I use three criteria to separate analysts who should be ex-ante more or less likely to react to changes in the supply of investment banking jobs. The first proxy is a measure of analyst baseline ability, and is equal to the analyst's performance in the past two semesters. The intuition for this proxy is that, as discussed in my theoretical framework, analysts with low innate ability never choose to apply for investment banking jobs because their expected returns would never be high enough to cover their career switching cost. Next, 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 career. The third proxy looks at the analyst's professional network. My conjecture is that analysts with weak professional networks need to rely more on showcasing their skill in order to obtain a job in investment banking, compared to analysts with strong professional networks. I use an indicator equal to one for analysts at Moody's who are working in the same country as the country of their most recent educational institution as a proxy for strong professional networks.

I regress analyst inaccuracy on an indicator *IB Enters* $(-2, 0)$, which is equal to one in event semesters $t-2$ to $t=0$ in order to capture anticipation effects. Then I use the three proxies described above to perform sample splits. Table 8, Panel B, reports results. The results strongly confirm my hypothesis that the observed improvement in analyst performance is driven by enhanced analyst incentives due to better prospects of pursuing an investment banking career. First, as predicted by my theoretical framework, analysts with weak past performance do not outperform the control group (see columns (1) and (2)). Second, the outperformance is economically larger for analysts who are ex-ante more likely to leave to investment banking (columns (3) and (4)). Third, the outperformance is stronger for analysts with a weaker professional network,

who arguably need to rely more on signalling their expertise in order to advance their career (columns (5) and (6)).

6. Conclusion

My paper contributes to the ongoing debate whether revolving doors strengthen or distort monitoring incentives. I hand-collect a novel dataset that links 229 individual credit rating analysts at Moody's to their career paths and to the quality of the ratings they assign. In contrast with the generally negative view on revolving doors, I find that credit analysts who are subsequently hired by investment banks are more accurate than other analysts rating similar securities at the same point in time. A notable exception is the small subset of securities that are underwritten by their future employers where they do not outperform. The results suggest that, because only few ratings are helpful to curry favors to future employers, but almost all ratings are helpful in signaling skill or building expertise, the positive effects of revolving doors can be economically sizable. They may also explain why, despite the frequently voiced concerns, revolving doors have remained open in most professions.

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

While this paper focuses on the effects on performance incentives, revolving doors may affect monitoring quality through additional channels. For example, credit ratings quality may suffer if

rating agencies systematically lose their more experienced or talented staff to investment banks, reducing their incentives to train new analysts (see Bar-Isaac and Shapiro (2011)). In addition, former analysts may help investment banks to game the rating system once they have left the rating agency.[29] On the other hand, there may also be additional positive effects of revolving doors that I am not capturing in my analysis. For example, the option for rating analysts to move to investment banking may positively affect the quality of the pool of applicants for positions 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.

[29]Recent evidence reported by Jiang, Wang, and Wang (2015) 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 2010 with information identifying the lead analyst at issuance and information on the analyst's 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. Analyst performance is computed at the analyst × collateral type level in a two-step procedure using equations (8) and (9), i.e., one observation in my dataset refers to one analyst and collateral type and semester. A complete list of variable definitions is provided in Appendix B.

Panel A: Sample

	Number of Tranches	Number of Deals	Issuance Volume ($bn)
Segment: ABS			
ABS Auto	1,784	506	404
ABS Card	420	216	162
ABS Home	3,656	720	323
ABS Student	141	38	22
ABS Other	4,416	980	514
Segment: MBS			
CMBS	509	63	67
RMBS	10,361	1,726	914
Segment: CDO/CLO			
CDO	901	271	66
Total	22,188	4,520	2,473

Panel B: Number of Analysts By Subsequent Career Path

	All	No Exit	IB Exit	Other Exit			
				Other Bank	Asset Mgr.	Insurer	Other
Number of analysts	229	78	63	28	20	11	29
o/w rate future employer	26	0	26	0	0	0	0

Panel C: Descriptive Statistics

	N	Mean	Std. Dev.	0.25	Median	0.75
Dependent Variables						
Analyst Inaccuracy	1,476	0.60	4.34	-1.94	-0.76	1.18
Key Independent Variables						
IB Exit	1,476	0.29	0.45	0.00	0.00	1.00
IB Exit$_{t+1yr}$	1,476	0.08	0.27	0.00	0.00	0.00
Other Exit	1,476	0.34	0.47	0.00	0.00	1.00
Other Exit$_{t+1yr}$	1,476	0.08	0.27	0.00	0.00	0.00
Future Employer	1,476	0.02	0.11	0.00	0.00	0.00
Future Employer | IB Exit	427	0.07	0.20	0.00	0.00	0.00
Control variables						
Tenure	1,476	4.76	5.37	1.00	3.00	7.00
Number of deals	1,476	3.09	3.20	1.00	2.00	4.00
IB Underwriter	1,476	0.80	0.34	0.71	1.00	1.00
Issuer Market Share (in %)	1,476	0.65	0.99	0.00	0.30	0.87

Table 2: Predicting Analyst Departures to Investment Banks

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

	IB Exit	
	(1)	(2)
Female	-0.371	-0.630
	(-1.01)	(-1.40)
Prior Work Experience	-3.039	-3.508
	(-3.51)	(-2.94)
Graduate Degree	-0.941	-1.369
	(-2.44)	(-2.61)
NYC Undergrad	1.032	2.064
	(2.14)	(2.80)
Ivy League	-0.551	-0.735
	(-1.17)	(-1.15)
Law Degree	-0.832	-1.106
	(-1.42)	(-1.81)
Tech Degree	0.110	0.566
	(0.26)	(0.99)
Cohort dummies	No	Yes
N	93	73
Pseudo-R^2	0.252	0.339

Table 3: Analyst Performance and Departures to Investment Banks

The table reports results from regressing analyst inaccuracy on an indicator for analyst departures to investment banks. In columns (1) and (2), *IB Exit* is an indicator equal to one if the analyst eventually departs to an investment bank that was ranked in "The Bloomberg 20" ranking in the year prior to his departure. In columns (3) and (4), *IB* $Exit_{t+1yr}$ is an indicator equal to one only during the last two semesters of the analyst's employment at Moody's. Panel A presents baseline results. Panel B presents results for the interaction with the fraction of tranches that are underwritten by the analyst's future employer. Panel C reports results from a placebo test where *Other Exit* refers to analyst departures to other employers. All variables are defined in Appendix B. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

Panel A: Baseline

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
IB Exit	-0.456	-0.457		
	(-2.54)	(-2.52)		
IB Exit_{t+1yr}			-1.262	-1.310
			(-2.67)	(-2.76)
Tenure		0.011		0.491
		(0.11)		(1.38)
No. of deals		0.080		0.100
		(0.73)		(0.80)
IB underwriter		-0.067		-0.050
		(-0.24)		(-0.16)
Issuer market share		-0.058		-0.108
		(-0.69)		(-1.29)
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	1,476	1,476	1,476	1,476
R^2	0.675	0.675	0.764	0.764

Panel B: Interaction with Fraction of Tranches Underwritten by Future Employer

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
IB Exit	-0.459	-0.460		
	(-2.48)	(-2.45)		
IB Exit × Future Employer	0.057	0.044		
	(0.08)	(0.06)		
IB Exit_{t+1yr}			-1.312	-1.361
			(-2.74)	(-2.83)
IB Exit_{t+1yr} × Future Employer			1.850	1.892
			(1.55)	(1.57)
Future Employer			-1.339	-1.355
			(-1.84)	(-1.82)
Controls included	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	1,476	1,476	1,476	1,476
R^2	0.675	0.675	0.764	0.764

Panel C: Placebo Test with Departures to Other Employers

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
Other Exit	0.339	0.344		
	(1.79)	(1.81)		
Other Exit_{t+1yr}			0.496	0.447
			(1.23)	(1.10)
Controls included	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	1,476	1,476	1,476	1,476
R^2	0.674	0.674	0.762	0.762

Table 4: Robustness

The table presents robustness tests. The baseline regression refers to column (4) from Table 3, Panel A. For brevity I only report coefficients of interest and suppress control variables. Economic effects are calculated as the reported coefficient times the standard deviation of the independent variable, divided by the standard deviation of the dependent variable. Panel A tests alternative measures of ratings accuracy. In the first line, I value-weight tranches by their principal amount instead of using equal weights as in equation (9). In the second line, I exclude all subsequent rating adjustments that are performed by the analyst responsible for the initial rating. *1(5)-yr Abnormal Rating Adjustment* refers to rating adjustments over a one and five-year horizon, respectively. Securities are considered as in *default* when Moody's assigns a rating below Ca within the first three years after issuance. For the next two measures, I use only rating downgrades or upgrades as opposed to all rating adjustments. *Abnormal cumulative losses* are computed as the absolute difference between the tranche's cumulative losses after three years and Moody's expected loss benchmark for the initial tranche rating category. > 2 *Initial Ratings* is an indicator equal to one if the tranches rated by the analyst are on average rated by more than two of the three major rating agencies. *Initial yield* is computed following He, Qian, and Strahan (2015). In Panel B, I use alternative definitions for departures to investment banks. *IB* $Exit_{t+6m}$ and *IB* $Exit_{t+2yrs}$ refer to departures to "The Bloomberg 20" investment banks in the following 6 months or 2 years, respectively. *Exits to "The Vault 50" Investment Banks* are analyst departures to investment banks ranked in "The Vault 50" ranking by prestige in 2008. *Exits to 5 Pure-Play Investment Banks* refer to exits to Bear Stearns, Goldman Sachs, Lehman Brothers, Merrill Lynch, or Morgan Stanley. In Panel C, first line, I exclude tranche ratings issued after 2005. In the second line, I include only tranches with complete information on the tranche principal amount, level of subordination, weighted average life, overcollateralization, insurance wrap, number of bonds in the deal, and coupon type. In Panel D, I report results from a propensity score matching procedure that matches each analyst who departs to an investment bank in the next year to his three nearest neighbors who rate securities in the same collateral type and semester, using the control variables from Table 3. I also report results from the same matching procedure while adding the analyst's performance over the previous two semesters to the set of matching variables.

	Coefficient	t-statistic	N	Econ. Effect
Baseline	-1.310	(-2.76)	1,476	-30.2%
Panel A: Alternative Measures of Analyst (In)Accuracy				
Baseline, value-weighted	-1.013	(-2.11)	1,476	-24.4%
Baseline, excl. adjustments by initial analyst	-1.380	(-3.10)	1,476	-31.8%
1-yr Abn. Rating Adjustment	-0.096	(-1.85)	1,476	-10.6%
5-yr Abn. Rating Adjustment	-1.430	(-2.98)	1,476	-33.8%
3-yr Abn. Downgrades	-1.340	(-2.83)	1,476	-30.9%
3-yr Abn. Upgrades	-0.024	(-0.95)	1,476	-13.0%
3-yr Abn. Default	-0.056	(-2.30)	1,476	-24.6%
3-yr Abn. Cumulative Losses	-1.478	(-1.34)	412	-20.7%
$>$ 2 Initial Ratings	0.114	(1.71)	1,476	22.8%
Initial Yield on AAA Tranches	-0.127	(-1.04)	759	-14.2%
Panel B: Alternative Definitions of IB Exit				
IB Exit$_{t+6m}$	-1.174	(-1.91)	1,476	-27.1%
IB Exit$_{t+2yrs}$	-0.996	(-2.17)	1,476	-23.0%
Exits to "The Vault 50" Investment Banks	-1.205	(-2.04)	1,476	-27.8%
Exits to 5 Pure-Play Investment Banks	-1.305	(-1.82)	1,476	-30.1%
Panel C: Sample Restrictions				
Drop tranches issued after 2005	-0.831	(-2.55)	1,058	-19.2%
Drop tranches with missing deal characteristics	-1.364	(-3.69)	764	-23.9%
Panel D: Estimation Method				
Propensity score matching	-0.779	(-1.92)	1,476	-18.0%
Propensity score matching, incl. past performance control	-1.101	(-2.21)	952	-25.4%

Table 5: The Influence of Deal Complexity

The table presents results for interactions with proxies for average deal complexity. *Low Documentation* refers to the average percentage of loans with less than full documentation in the underlying collateral. *Absolute Credit Score Skewness* and refers to the absolute skewness of the credit score distribution of the loans in the underlying collateral. In column (3), deal complexity is computed as in He, Qian, and Strahan (2015) as the number of tranches in the deal divided by their combined principal amount. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

	Analyst Inaccuracy		
	Low Documentation	Abs. Credit Score Skewness	Deal Complexity (HQS)
	(1)	(2)	(3)
IB Exit$_{t+1yr}$	-0.001	-0.208	-0.895
	(0.00)	(-0.38)	(-1.74)
IB Exit$_{t+1yr}$ × Deal Complex.	-2.494	-6.200	-2.595
	(-2.04)	(-2.51)	(-1.42)
Deal Complexity	1.106	1.200	-0.449
	(1.69)	(0.90)	(-2.45)
Tenure	-0.146	-0.106	0.481
	(-0.30)	(-0.23)	(1.37)
No. of deals	0.357	0.502	0.105
	(2.03)	(2.88)	(0.84)
IB underwriter	-0.373	-0.171	-0.090
	(-0.72)	(-0.31)	(-0.29)
Issuer market share	-0.144	-0.012	-0.082
	(-1.22)	(-0.11)	(-0.99)
Collateral type × semester f.e.	Yes	Yes	Yes
Analyst f.e.	Yes	Yes	Yes
N	670	591	1,476
R^2	0.842	0.865	0.768

Table 6: Analyst Performance by Time Until Departure and Tenure at Departure

The table presents results for different subsamples of analysts who depart to investment banks. In Panel A, observations of departing analysts are grouped into quartiles based on the remaining time until their departure, and, in Panel B, based on their tenure at the time of departure. Quartiles are formed within a given calendar year. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

Panel A: Subsamples by remaining time until analyst departure

	Analyst Inaccuracy			
Quartile	Q1	Q2	Q3	Q4
Avg. time until departure (in years)	0.5	1.4	2.8	5.3
	(1)	(2)	(3)	(4)
IB Exit	-1.051	-0.368	-0.156	-0.011
	(-3.04)	(-1.06)	(-0.82)	(-0.04)
Tenure	0.082	0.143	0.170	0.169
	(0.67)	(1.11)	(1.40)	(1.37)
No. of deals	0.106	0.038	0.025	0.030
	(0.79)	(0.27)	(0.18)	(0.22)
IB underwriter	-0.211	-0.064	-0.163	-0.175
	(-0.62)	(-0.18)	(-0.48)	(-0.50)
Issuer market share	0.044	-0.004	-0.044	-0.037
	(0.49)	(-0.05)	(-0.47)	(-0.41)
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	No	No
N	1,197	1,128	1,152	1,146
R^2	0.664	0.688	0.687	0.698

Panel B: Subsamples by analyst tenure at time of departure

	Analyst Inaccuracy			
Quartile	Q1	Q2	Q3	Q4
Avg. tenure at departure (in years)	2.2	3.6	6.4	13.6
	(1)	(2)	(3)	(4)
IB Exit$_{t+1yr}$	-1.061	-2.246	-0.674	-1.337
	(-1.76)	(-2.56)	(-1.07)	(-2.58)
Tenure	0.658	0.575	0.746	0.584
	(1.41)	(1.24)	(1.63)	(1.28)
No. of deals	0.147	0.170	0.067	0.183
	(1.00)	(1.05)	(0.46)	(1.12)
IB underwriter	-0.048	-0.091	-0.036	-0.048
	(-0.12)	(-0.23)	(-0.09)	(-0.11)
Issuer market share	-0.031	-0.146	-0.169	-0.111
	(-0.32)	(-1.46)	(-1.90)	(-1.10)
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	Yes	Yes	Yes	Yes
N	1,189	1,149	1,194	1,091
R^2	0.769	0.770	0.770	0.769

Table 7: Analyst Performance and Promotions

The table presents results from regressing analyst inaccuracy on an indicator for analyst promotions, which are identified as follows. For all press releases from Moody's website mentioning a given analyst, I identify the analyst's job title. Matching job titles with salary information from www.glassdoor.com, I rank job titles from low to high average salary and classify an analyst as being promoted when his job title changes to a higher-salary category. In columns (1) and (2), *Promotion* is an indicator equal to one if the analyst gets promoted at least once during his tenure at Moody's. In columns (3) and (4), $Promotion_{t+1yr}$ is an indicator equal to one if the analyst gets promoted in the next year. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
Promotion	-0.362	-0.393		
	(-1.88)	(-2.04)		
Promotion_{t+1yr}			-0.233	-0.275
			(-0.98)	(-1.14)
Tenure		0.075		0.501
		(0.72)		(1.36)
No. of deals		0.072		0.086
		(0.65)		(0.69)
IB underwriter		-0.049		-0.019
		(-0.17)		(-0.06)
Issuer market share		-0.058		-0.098
		(-0.66)		(-1.18)
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	1,479	1,479	1,479	1,479
R^2	0.674	0.674	0.761	0.762

Table 8: Exploiting Shocks to the Supply of Investment Banking Jobs

The table presents results from my analysis of analyst inaccuracy around the event where an investment bank enters a new collateral group as an underwriter. Panel A compares the inaccuracy of analysts in the event collateral group (i.e., the collateral group entered by the investment bank) and the inaccuracy of analysts in other collateral groups in the same market segment (ABS, MBS, or CDO/CLO) around the event. Analyst inaccuracy is regressed on a set of nine event-time dummy variables labeled $t-4$, $t-3$, ..., $t+3$, $t+4$, where my convention is that dummy t takes on the value one in the collateral group and semester in which an investment bank entry event occurs. Each column reports the coefficient on one of the nine dummy variables. Panel B focuses on event semesters $t-2$ to t and shows how the performance gap between the event and the control group differs for analysts with different characteristics. *IB Enters* $(-2, 0)$ is an indicator equal to one in the two semesters prior to and including the event semester. *Past Performance* refers to the analyst's average inaccuracy in the collateral group during the previous two semesters, and is split into low and high groups within collateral type and date. $\widehat{Pr(IB\,Exit)}$ 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 a given calendar year. *Professional Network* is an indicator equal to one if the most recent educational institution attended by the analyst is located in the same country as his office location at Moody's. All regressions include segment × semester fixed effects, analyst fixed effects, and the same controls as in Table 3. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

Panel A: Analyst performance around investment bank entry

	Analyst Inaccuracy								
	$t-4$	$t-3$	$t-2$	$t-1$	t	$t+1$	$t+2$	$t+3$	$t+4$
IB Enters $t=0$	-0.033	-0.561	-0.913	-1.493	-0.940	-0.453	0.008	0.443	0.272
	(-0.14)	(-2.20)	(-3.31)	(-4.66)	(-3.47)	(-1.87)	(0.03)	(1.59)	(1.03)
Controls included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Segment × sem. f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Analyst f.e.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Analyst performance by subsample

	Analyst Inaccuracy					
	Past Performance		$\widehat{Pr(IB\,Exit)}$		Professional Network	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
IB Enters (-2,0)	0.716	-1.635	-0.050	-0.873	-3.015	-1.109
	(0.85)	(-2.61)	(-0.04)	(-1.44)	(-2.68)	(-2.46)
Controls included	Yes	Yes	Yes	Yes	Yes	Yes
Segment × sem. f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Analyst f.e.	Yes	Yes	Yes	Yes	Yes	Yes
Chi^2 statistic	6.02		0.45		4.77	
p-value	0.014		0.503		0.029	
N	437	515	312	262	81	867
R^2	0.851	0.770	0.787	0.863	0.926	0.698

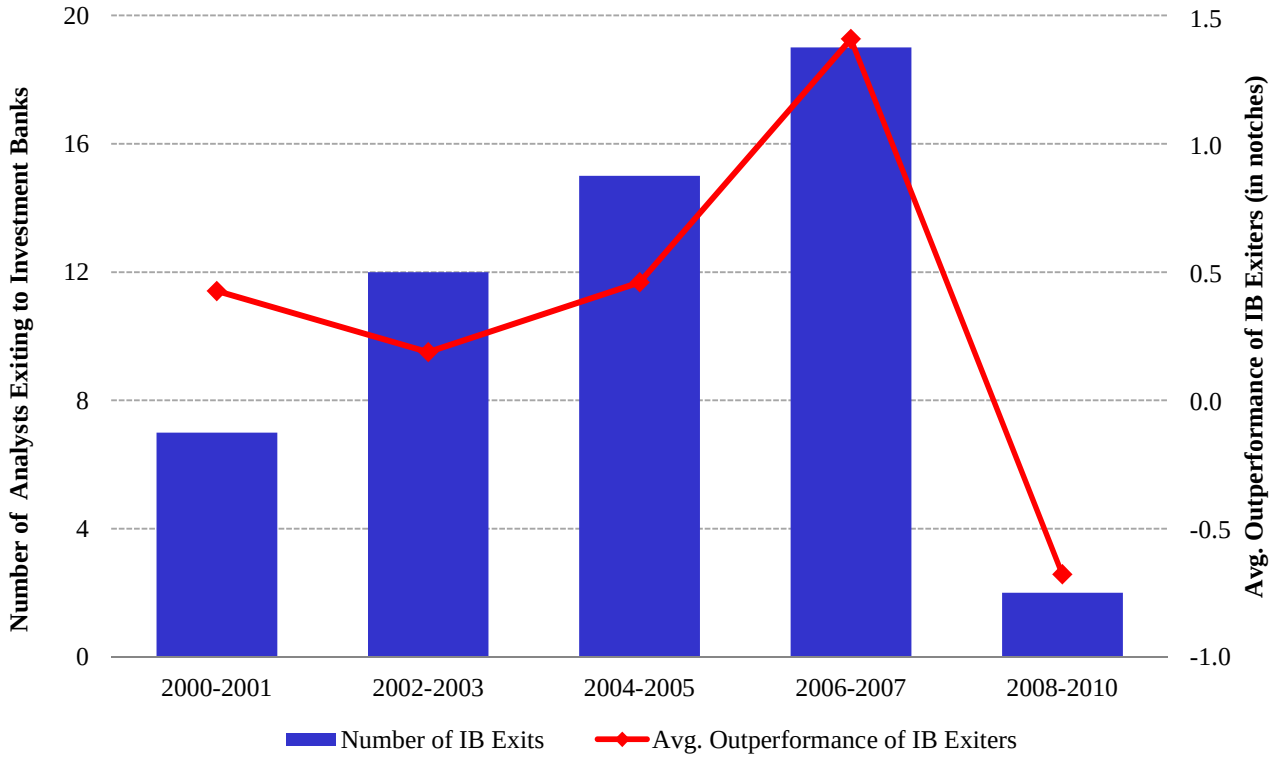


Figure 1: Departures to Investment Banks and Average Outperformance of Departing Analysts. The graph plots the number of analysts hired by investment banks and the average outperformance of departing analysts in each subperiod. Investment banks are all investment banks that were ranked in "The Bloomberg 20" ranking in the year prior to the analyst's exit. Outperformance is measured as minus one times the average abnormal absolute rating adjustment in the three years after rating issuance, following equations (8) and (9).

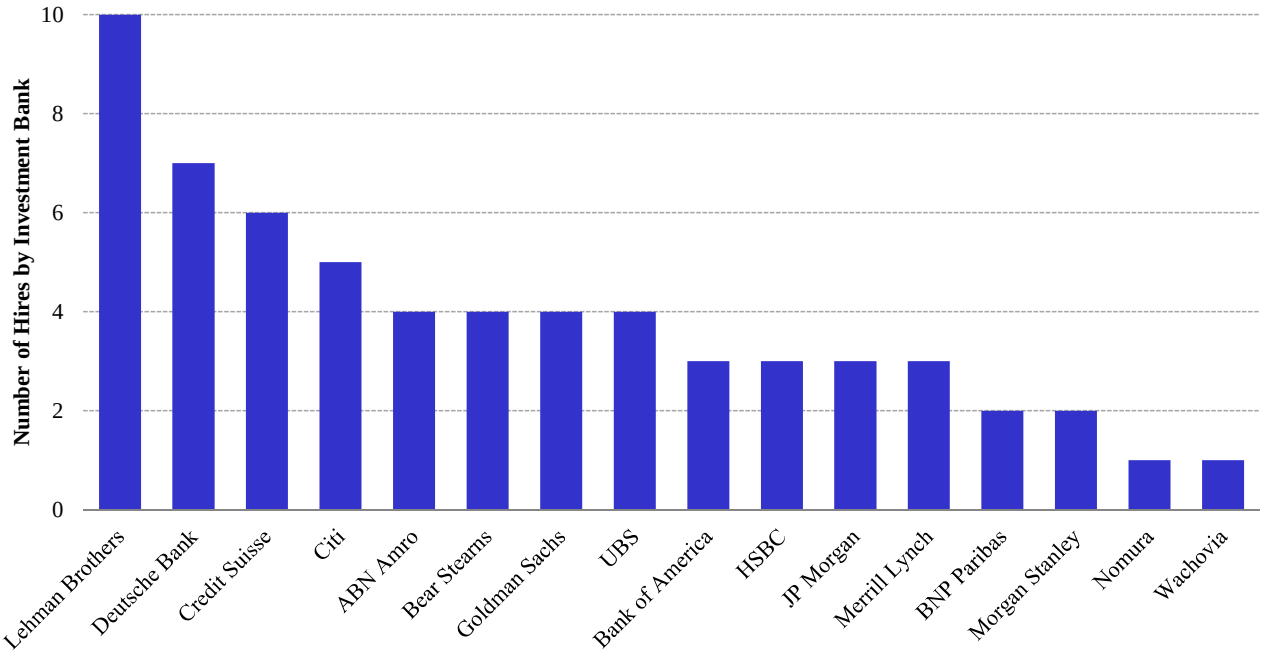


Figure 2: Number of Hires by Investment Bank. The graph plots the total number of Moody's analysts hired by each investment bank over the sample period. An analyst departure is classified as an exit to an investment bank if his subsequent employers was ranked in "The Bloomberg 20" ranking in the year prior to the analyst's departure.

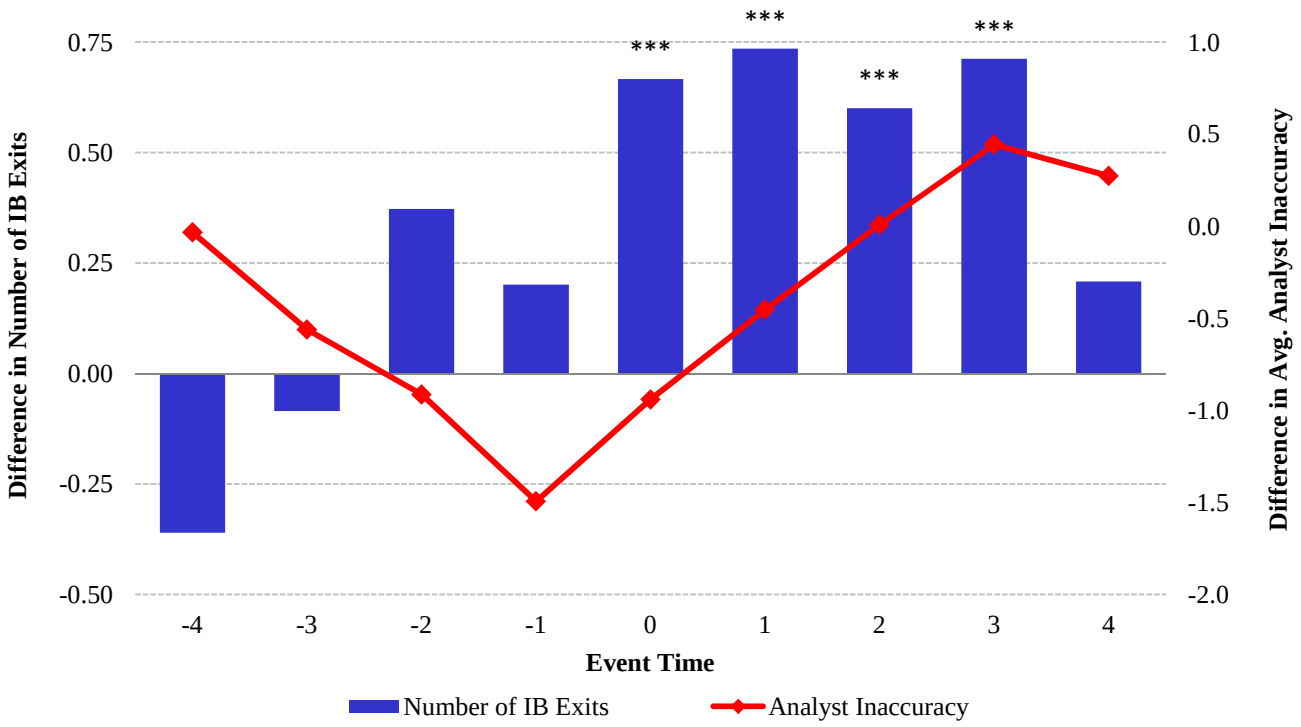


Figure 3: Event Study: Shocks to the Supply of Investment Banking Jobs. The graph plots the number of analysts departing to investment banks and average analyst inaccuracy around the event where an investment bank enters a new collateral group as an underwriter. The blue bars show the difference in the number of analysts who depart to investment banks between the event collateral group (i.e., the collateral group that the investment bank enters) and other collateral groups in the same market segment (ABS, MBS, or CDO/CLO) in the window $(-4, +4)$ around the event. For each collateral type and semester, the number of analysts who depart within the next year is regressed on a set of nine event-time dummy variables labeled $t-4$, $t-3$, ..., $t+3$, $t+4$, where my convention is that dummy t takes on the value one in the collateral group and semester in which an investment bank entry event occurs, as well as collateral type $\times$ semester fixed effects, analyst fixed effects, and the same controls as in Table 3. Each bar shows the coefficient on one of the nine dummy variables. The red line plots the coefficient estimates reported in Table 8, Panel A, i.e., the difference in analyst inaccuracy between the event and the control group, over the same event window. Asterisks ***, **, * indicate statistical significance on the 1%, 5%, and 10% level.

Appendix A. Proofs

Appendix A.1. Average causal effect of revolving doors

The average causal effect of revolving doors on the performance of analysts who choose to enter the lottery ("the treated") is given by:

$$\begin{aligned} ATT &= E(e_i|l_i = 1, a_i > a_L) - E(e_i|l_i = 0, a_i > a_L) \\ &= \int_{a_L}^{\bar{a}} (e^*(a_i)|l_i = 1, a_i > a_L)da - \int_{a_L}^{\bar{a}} (e^*(a_i)|l_i = 0, a_i > a_L)da \\ &= (w_{CRA} + pw_{IB})0.5(a_L + \bar{a}) - w_{CRA}0.5(a_L + \bar{a}) = pw_{IB}0.5(a_L + \bar{a}) \\ &= pw_{IB}0.5(\frac{c}{w_{IB}(w_{CRA} + pw_{IB})} + \bar{a}) \end{aligned} \tag{11}$$

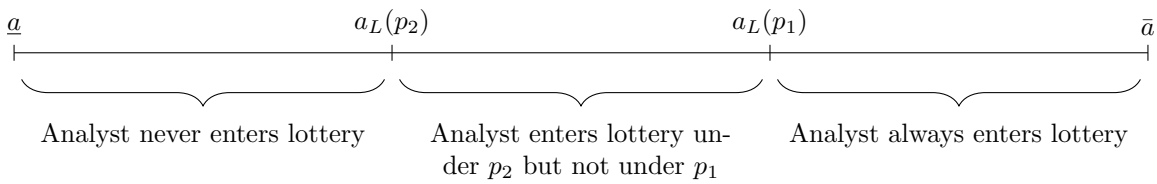
The above expression is larger than zero as long as the expected rent from an investment banking job and the switching cost are positive ($pw_{IB} > 0$ and $c > 0$).

Appendix A.2. Observed differences in performance

$$\begin{aligned} E(e_i|D_i = 1) - E(e_i|D_i = 0) &= (w_{CRA} + pw_{IB})0.5(a_L + \bar{a}) \\ &\quad - (\theta(w_{CRA} + pw_{IB})0.5(a_L + \bar{a}) + (1 - \theta)w_{CRA}0.5(\underline{a} + a_L)) \\ &= ATT + B - C, \\ where\ ATT &\equiv pw_{IB}0.5(a_L + \bar{a}), \\ B &\equiv w_{CRA}0.5(a_L + \bar{a} - \theta(a_L + \bar{a}) - (1 - \theta)(\underline{a} + a_L)), \\ C &\equiv \theta pw_{IB}0.5(a_L + \bar{a}), \\ \theta &\equiv \frac{(1 - p)(\bar{a} - a_L)}{(a_L - \underline{a}) + (1 - p)(\bar{a} - a_L)} \end{aligned} \tag{12}$$

Appendix A.3. Effect of a change in p

Consider a change in the probability of being hired by the investment bank, conditional on entering the lottery, from p_1 to p_2, where $p_1 < p_2$. Let $a_L(p_1)$ and $a_L(p_2)$ denote the two threshold levels of ability under the two scenarios p_1 and p_2, as defined in equation (4). The effect of a change from p_1 to p_2 on analyst performance differs for three groups of analysts, as depicted in the figure below:



The first group, analysts with ability $a_i < a_L(p_2)$, are analysts who choose not to enter the lottery in either scenario. The expected performance of these analysts is therefore insensitive to changes in the probability of being hired by investment banks.

$$\begin{aligned} &E(e_i|p_2, a_i < a_L(p_2)) - E(e_i|p_1, a_i < a_L(p_2)) \\ &= w_{CRA}0.5(\underline{a} + a_L(p_2)) - w_{CRA}0.5(\underline{a} + a_L(p_2)) \\ &= 0 \end{aligned} \tag{13}$$

The second group, analysts with ability $a_i > a_L(p_1)$, are analysts who choose to enter the lottery in either scenario. The expected change in the performance for this group of analysts is given by:

$$\begin{aligned} &E(e_i|p_2, a_i > a_L(p_1)) - E(e_i|p_1, a_i > a_L(p_1)) \\ &= (w_{CRA} + p_2 w_{IB})0.5(a_L(p_1) + \bar{a}) - (w_{CRA} + p_1 w_{IB})0.5(a_L(p_1) + \bar{a}) \\ &= (p_2 - p_1)w_{IB}0.5(a_L(p_1) + \bar{a}) \end{aligned} \tag{14}$$

The third group, analysts with ability $a_L(p_2) < a_i < a_L(p_1)$, are analysts who choose to enter the lottery in scenario p_2 but not in scenario p_1. The change in performance for this group of analysts is given by:

$$\begin{aligned} &E(e_i|p_2, a_L(p_2) < a_i < a_L(p_1)) - E(e_i|p_1, a_L(p_2) < a_i < a_L(p_1)) \\ &= (w_{CRA} + p_2 w_{IB})0.5(a_L(p_2) + a_L(p_1)) - w_{CRA}0.5(a_L(p_2) + a_L(p_1)) \\ &= p_2 w_{IB}0.5(a_L(p_2) + a_L(p_1)) \end{aligned} \tag{15}$$

First, note that the average change in performance in response to a positive change in p is either zero or positive for all three groups. Hence, the average change in analyst performance, which is a weighted average of the three groups, is weakly larger than zero (in other words, $E(e_i|p_2 - E(e_i|p_1) \geq 0$. Second, there may exist a group of low ability analysts, those with ability $a_i < a_L(p_2)$, whose performance is less sensitive to changes in p than that of analysts with higher ability.

Appendix B. Variable Descriptions

Table B.1: Variable descriptions

Variable	Description
Measures of Analyst (In)Accuracy	
Baseline	In a first step, the absolute difference (in notches) between Moody's initial rating of the tranche and the rating three years following the issuance is regressed on tranche and deal characteristics following equation (8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.
Baseline, value-weighted	In a first step, the absolute difference (in notches) between Moody's initial rating of the tranche and the rating three years following the issuance is regressed on tranche and deal characteristics following equation (8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by computing a weighted average where the weights are proportional to the tranche's principal amount. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.
1-yr Abn. Rating Adjustment	In a first step, the absolute difference (in notches) between Moody's initial rating of the tranche and the rating one year following the issuance is regressed on tranche and deal characteristics following equation (8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.
5-yr Abn. Rating Adjustment	In a first step, the absolute difference (in notches) between Moody's initial rating of the tranche and the rating five years following the issuance is regressed on tranche and deal characteristics following equation (8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.
3-yr Abn. Downgrades	Downgrades are computed as the absolute difference (in notches) between Moody's initial rating of the tranche and the rating three years following the issuance if the initial rating is higher (otherwise it is set to zero). In a first step, the number of downgrades is regressed on tranche and deal characteristics following equation (8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.

Continued on next page

Table B.1 – continued

Variable	Description
3-yr Abn. Upgrades	Upgrades are computed as the absolute difference (in notches) between Moody's initial rating of the tranche and the rating three years following the issuance if the initial rating is lower (otherwise it is set to zero). In a first step, the number of upgrades is regressed on tranche and deal characteristics following equation (8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.
3-yr Abn. Default	Tranches are considered in default when Moody's assigns a rating below Ca within the first three years after issuance. In a first step, this default indicator is regressed on tranche and deal characteristics following equation (8). In a second step, the residuals from the first-step regression are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Rating adjustments are obtained from Moody's website and tranche and deal characteristics are from SDC Platinum and Bloomberg.
3-yr Absolute Cumulative Losses	In a first step, the absolute difference between the cumulative tranche losses, i.e., the principal balance write offs due to default, and Moody's expected loss benchmark for the tranche's initial rating category is computed. In a second step, the absolute differences obtained in the first step are aggregated at the analyst × collateral type × semester level by taking the arithmetic mean. Cumulative tranche losses are obtained from Bloomberg and Moody's expected loss benchmarks are retrieved from Moody's website (available at `https://www.moodys.com/sites/products/productattachments/marvel_user_guide1.pdf`).
>2 Initial Ratings	An indicator function equal to one if the average deal is rated by more than two of the three major rating agencies (Moody's, S&P, Fitch). Initial ratings from the three major rating agencies are obtained from Bloomberg.
Initial Yield on AAA Tranches	Initial yields on AAA tranches are computed following He, Qian, and Strahan (2015). For tranches with floating coupon rates, the initial yield spread is equal to the spread (in basis points) over the benchmark specified at issuance as reported in Bloomberg. For tranches with fixed or variable coupon rates, the initial yield spread is computed as the difference between the coupon rate and the yield on a Treasury security whose maturity is closest to the tranche's weighted average life.
Key independent variables	
IB Exit	Indicator function equal to one if the analyst departs to an investment bank following his employment at Moody's. Investment banks are employers that were ranked in "The Bloomberg 20" ranking in the year prior to the analyst's departure. Post-Moody's employer information is obtained from public profiles on LinkedIn and web searches.
IB Exit$_{t+1yr}$	Indicator function equal to one during the last two semesters of the analyst's employment at Moody's before he departs to an investment bank. Investment banks are employers that were ranked in "The Bloomberg 20" ranking in the year prior to the analyst's departure. Post-Moody's employer information is obtained from public profiles on LinkedIn and web searches.

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Table B.1 – continued

Variable	Description
Other Exit	Indicator function equal to one if the analyst departs to an employer other than an investment bank following his employment at Moody's. Investment banks are employers that were ranked in "The Bloomberg 20" ranking in the year prior to the analyst's departure. Post-Moody's employer information is obtained from public profiles on LinkedIn and web searches.
Other Exit$_{t+1yr}$	Indicator function equal to one during the last two semesters of the analyst's employment at Moody's before he departs to a non-investment bank employer. 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.
Future Employer	Fraction of tranches that are underwritten by the analyst's future employer. Underwriter information is obtained from SDC Platinum and manually matched with information on the analyst's post-Moody's employer obtained from public profiles on LinkedIn and web searches.
Control variables	
Tenure	Logarithm of one plus the number of semesters since the begin of the analyst's employment at Moody's, which is the earlier date of the analyst's reported start date on LinkedIn and his first appearance in the dataset.
Number of deals	Logarithm of one plus the number of deals rated by the analyst in a given collateral type and semester.
IB Underwriter	The fraction of tranches 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. Underwriter information is obtained from SDC Platinum.
Issuer Market Share	The market share of the tranche issuer based on the dollar volume of deals across all collateral types originated in the previous calendar year.
Measures of Deal Complexity	
Low Documentation	The average percentage of loans with less than full documentation in the underlying collateral of the deal. The percentage of loans with full documentation is obtained from Bloomberg.
Abs. Credit Score Skewness	The absolute skewness of the credit score distribution of the loans in the underlying collateral of the deal. Skewness is computed in terms of quartiles of the credit score distribution using Bowley's formula. Quartiles of the credit score distribution are obtained from Bloomberg.
Deal Complexity (HQS)	Computed following He, Qian, and Strahan (2015) as the number of tranches in the deal divided by their combined principal amount.

Appendix C. Additional Evidence

Table C.1: Baseline Results – Regressions at the Deal Level

The table reports results from Table 3 when running regressions at the individual deal level. Specifically, I estimate the following regression:

$$Rating\,Adjustment_{kiz} = \lambda_i + \lambda_{zt} + \delta IB\,Exit_{i,t+1yr} + \beta' X_{ki} + \eta_{kiz}, \tag{16}$$

where $Rating\,Adjustment_{kiz}$ is the average absolute difference (in notches) between the initial rating and the rating three years after issuance across all tranches of deal k rated by analyst i. λ_i and λ_{zt} are analyst and collateral type × issuance semester fixed effects, respectively, and X_{ki} represents the same vector of additional controls as in equation (8). All variables are defined in Appendix B. In columns (1) and (2), *IB Exit* is an indicator equal to one if the analyst departs to an investment bank that was ranked in "The Bloomberg 20" ranking in the year prior to his departure. In columns (3) and (4), *IB* $Exit_{t+1yr}$ is an indicator equal to one in the last two semesters of the analyst's employment at Moody's before his departure to the investment bank. Panel A presents baseline results. Panel B presents results for the interaction with an indicator equal to one if the deal is underwritten by the analyst's future employer. Panel C reports results from a placebo test where *Other Exit* refers to analyst departures to other employers. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

Panel A: Baseline

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
IB Exit	-0.331	-0.384		
	(-2.10)	(-2.40)		
IB Exit_{t+1yr}			-0.844	-0.951
			(-2.47)	(-2.67)
Tenure		-0.078		0.516
		(-1.08)		(1.86)
No. of deals		0.237		0.193
		(2.42)		(1.76)
IB underwriter		-0.103		-0.060
		(-0.85)		(-0.58)
Issuer market share		0.084		0.097
		(1.85)		(2.38)
Deal Controls	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	4,515	4,507	4,515	4,515
R^2	0.782	0.788	0.814	0.814

Panel B: Interaction with Fraction of Tranches Underwritten by Future Employer

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
IB Exit	-0.319	-0.372		
	(-1.96)	(-2.24)		
IB Exit × Future Employer	-0.178	-0.190		
	(-0.75)	(-0.79)		
IB Exit$_{t+1yr}$			-0.855	-0.962
			(-2.46)	(-2.67)
IB Exit$_{t+1yr}$ × Future Employer			0.402	0.397
			(1.12)	(1.15)
Future Employer			-0.320	-0.290
			(-1.35)	(-1.23)
Deal and Other Controls	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	4,515	4,507	4,515	4,507
R^2	0.782	0.788	0.814	0.818

Panel C: Placebo Test with Departures to Other Employers

	Analyst Inaccuracy			
	(1)	(2)	(3)	(4)
Other Exit	0.373	0.332		
	(2.73)	(2.51)		
Other Exit$_{t+1yr}$			-0.049	-0.089
			(-0.25)	(-0.46)
Deal and Other Controls	No	Yes	No	Yes
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	No	No	Yes	Yes
N	4,517	4,509	4,517	4,509
R^2	0.782	0.787	0.813	0.817

Table C.2: Departures to Other Employers

The table presents results for analyst departures to employers other than investment banks. *Other banks* refer to employment analyst transitions to banks and brokers that are not listed in "The Bloomberg 20" ranking in the year prior to the transfer, *asset managers* include mutual funds and hedge funds, and *others* comprise all other employers (e.g., other rating agencies, regulators, or law firms). t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

	Analyst Inaccuracy			
	Other Banks	Asset Managers	Insurers	Others
	(1)	(2)	(3)	(4)
Exit_{t+1yr}	1.424	-1.303	0.891	0.197
	(2.66)	(-1.31)	(0.71)	(0.41)
Tenure	0.374	0.496	0.453	0.445
	(1.05)	(1.38)	(1.27)	(1.24)
No. of deals	0.091	0.096	0.086	0.088
	(0.74)	(0.76)	(0.69)	(0.71)
IB underwriter	-0.014	-0.019	-0.016	-0.013
	(-0.04)	(-0.06)	(-0.05)	(-0.04)
Issuer market share	-0.100	-0.102	-0.100	-0.100
	(-1.22)	(-1.22)	(-1.20)	(-1.20)
Collateral type × semester f.e.	Yes	Yes	Yes	Yes
Analyst f.e.	Yes	Yes	Yes	Yes
N	1,479	1,479	1,479	1,479
R^2	0.763	0.763	0.762	0.762

Table C.3: The Impact of Past Work Experience With Investment Banks

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

	Analyst Inaccuracy		
	(1)	(2)	(3)
Past IB	0.032	0.074	
	(0.16)	(0.35)	
Past IB × Past Employer		-0.884	-0.932
		(-0.99)	(-1.12)
Tenure	0.056	0.057	0.423
	(0.50)	(0.51)	(1.08)
No. of deals	0.087	0.084	0.124
	(0.76)	(0.73)	(0.96)
IB underwriter	-0.129	-0.112	-0.186
	(-0.39)	(-0.34)	(-0.50)
Issuer market share	-0.078	-0.078	-0.142
	(-0.95)	(-0.95)	(-1.76)
Collateral type × semester f.e.	Yes	Yes	Yes
Analyst f.e.	No	No	Yes
N	1,267	1,267	1,267
R^2	0.702	0.702	0.777

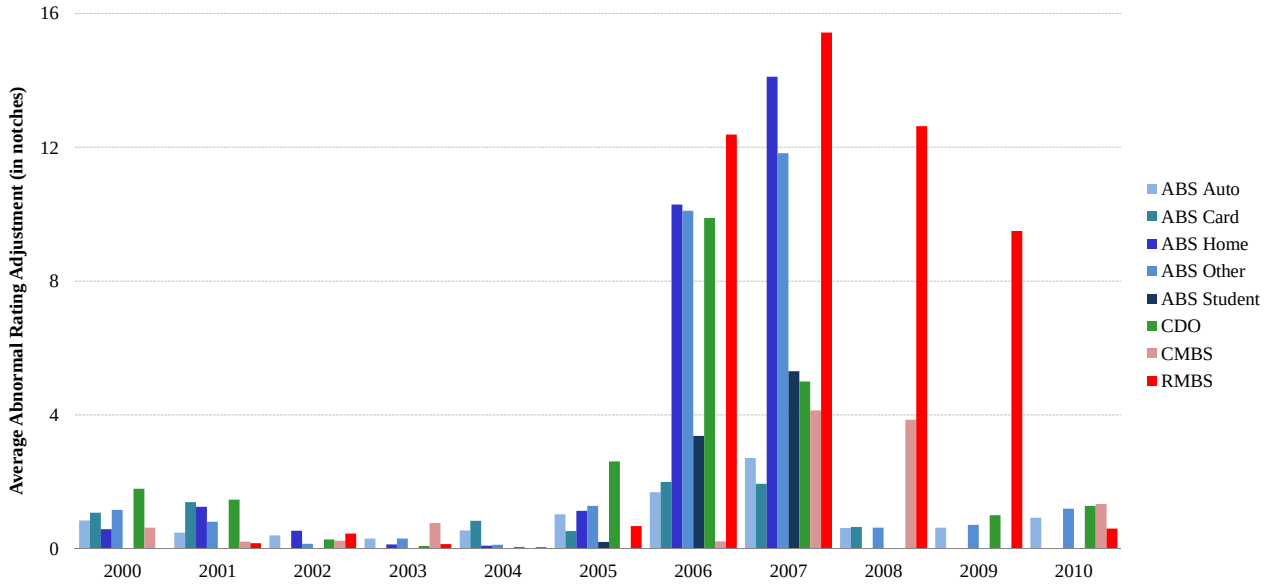

Figure C.1: Rating Performance by Collateral Type. The graph plots average rating adjustments across eight collateral types and over time. Rating adjustments are computed as the absolute difference (in notches) between a tranche's initial rating and the rating three years after issuance, and are averaged across all tranches issued in a given collateral type and calendar year.

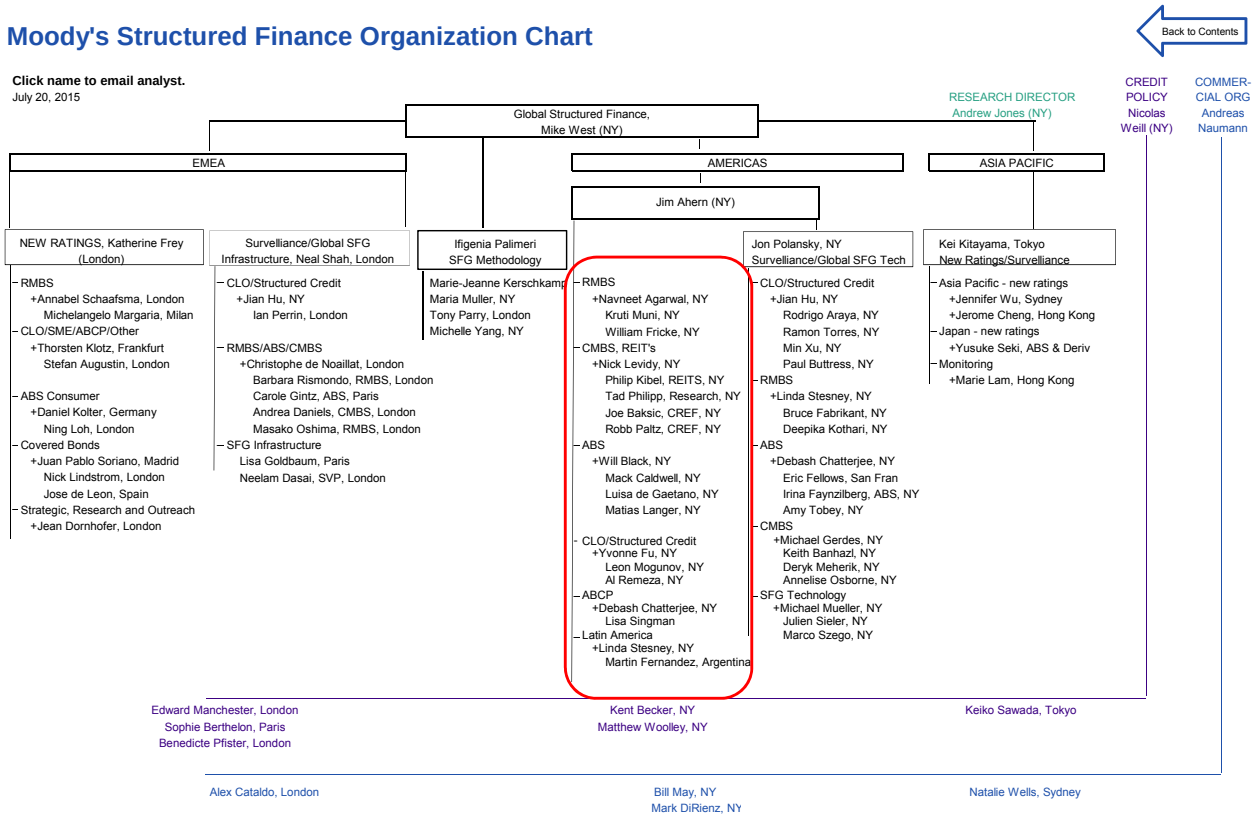

Figure C.2: Organizational Structure at Moody's. The chart shows the organizational structure of the Structured Finance team at Moody's as reported on Moody's website (available at `https://www.moodys.com/research/Structured-Finance-Ratings-Quick-Check-Newsletter--PBS_SF161380`). The red line highlights the division of interest for this paper, i.e., new ratings in the Americas region.

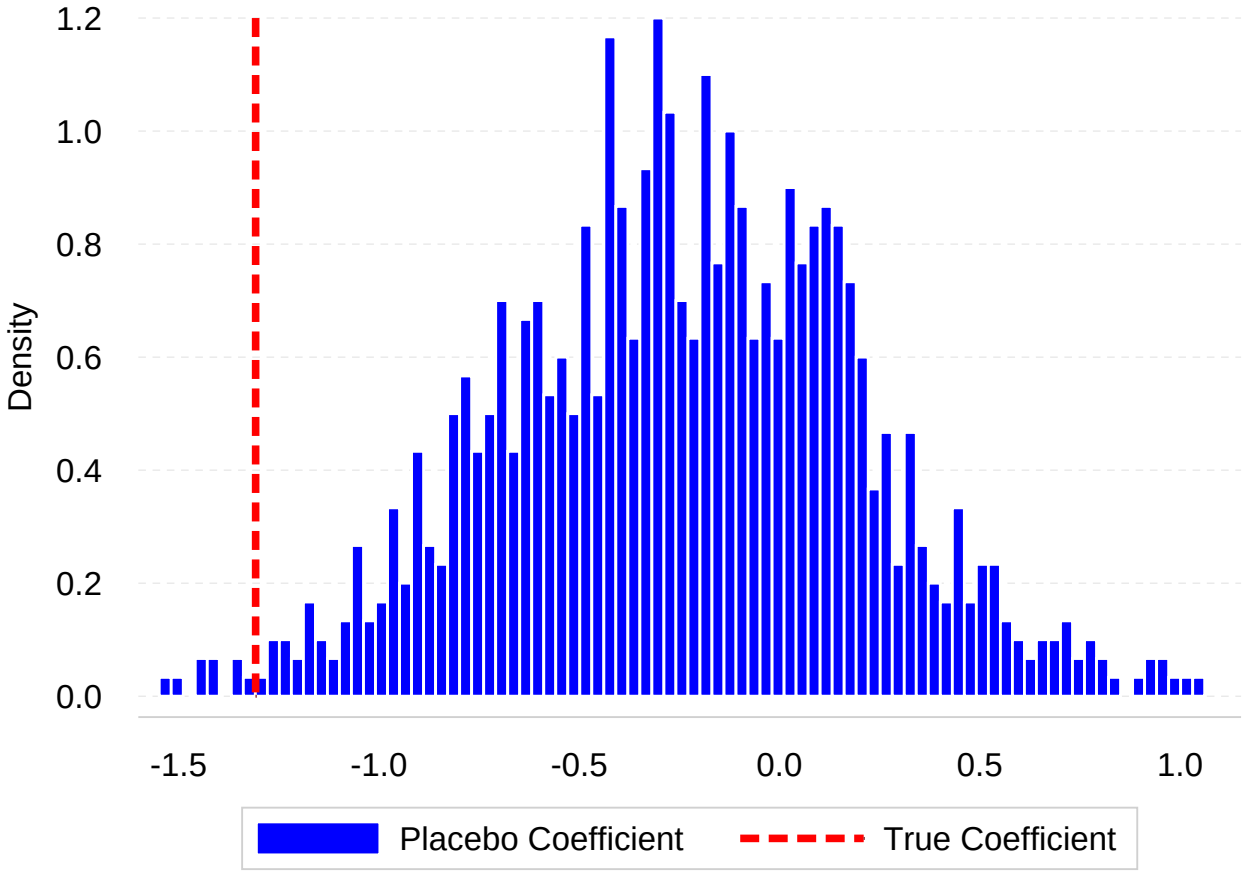

Figure C.3: Falsification test using placebo analyst departure dates. The figure illustrates the output from a falsification test where I replace the analyst's actual departure date to the investment bank with a random date between the actual start and end date of the analyst's employment with Moody's. Depicted is the histogram of the regression coefficients of $IB\ Exit_{t+1yr}$ estimated from 1,000 placebo runs.