Partisan Professionals:
Evidence from Credit Rating Analysts

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ABSTRACT

Partisan bias affects the decisions of financial analysts. Using a novel hand-collected dataset that links credit rating analysts to party affiliations from voter records, we show that analysts who are not affiliated with the President's party are more likely to downgrade firms. Our identification approach compares analysts with different party affiliations covering the same firm at the same point in time, ensuring that differences in the fundamentals of rated firms cannot explain the results. The effect is more pronounced in periods of high partisan conflict and for analysts who vote frequently. Our results suggest that partisan bias and political polarization create distortions in the cost of capital of U.S. firms.

"Like many people, I had and expressed personal political opinions during an extraordinary Presidential election. [...] But let me be clear, unequivocally and under oath: not once in my 26 years of defending my nation did my personal opinions impact any official action I took."
Peter Strzok, testimony to the House Judiciary and Oversight committees

1.   Introduction

Recent evidence suggests a large increase in polarization across political parties in the U.S. (e.g., Iyengar, Sood, and Lelkes (2012); Mason (2013); Mason (2015); Gentzkow (2016); Boxell, Gentzkow, and Shapiro (2017)). In particular, there is an increasing tendency of voters to view the world through a "partisan perceptual screen,"[1] i.e., their assessment and interpretation of economic conditions depends on whether the White House is occupied by the party they support (e.g., Gerber and Huber (2009) and Mian, Sufi, and Khoshkhou (2017)). In order to understand how partisan bias may affect the U.S. economy, it is important to establish whether and when partisan bias translates into differences in the behavior of economic agents. While researchers have documented that partisan bias affects U.S. households' assessment of future economic conditions, evidence on actual economic behavior is mixed.[2] Whether partisan bias influences the economic expectations and decisions of economic agents beyond households and, in particular, those of agents in professional environments, is an even more unexplored and frequently debated question, as illustrated in the opening quote.[3]

In this paper, we aim to fill this gap by investigating whether partisan bias affects the actions of an important set of finance professionals. Focusing on finance professionals provides an interesting setting, as competition (Gentzkow, Glaeser, and Goldin (2006), Hong and Kacperczyk (2010)) and career concerns should reduce biases. *Ex ante*, it is therefore unclear whether the existing findings on household behavior translate to our setting.

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[1]Campbell, Converse, Miller, and Stokes (1960) introduced the idea of the partisan perceptual screen: "Identification with a party raises a perceptual screen through which the individual tends to see what is favorable to his partisan orientation" (Campbell, Converse, Miller, and Stokes (1960), p. 133). In this paper, we will use "partisan perceptual screen" and "partisan bias" interchangeably.

[2]While Makridis (2017) documents a significant effect of partisan bias on household spending, McGrath (2017) and Mian, Sufi, and Khoshkhou (2017) find no significant effect. Focusing on households' investment decisions, Meeuwis, Parker, Schoar, and Simester (2018) show that political affiliation affects portfolio choice around the U.S. election of November 2016.

[3]Notable exceptions are Jelveh, Kogut, and Naidu (2015), who argue that political ideology affects economic research, and Posner (2008), McKenzie (2012), and Chen (2017), who document political biases among judges.

Studying political bias in professional contexts poses a number of empirical challenges. First, it requires observable actions at the level of the individual. Second, these individuals need to be linked to information about their political party affiliation. Third, in order to be able to rule out alternative explanations, it is necessary to compare the actions of individuals with different political affiliations on the same task and in the same environment.

We address these challenges by studying the decisions of credit rating analysts. We compile a novel hand-collected dataset that links credit rating analysts to the ratings they issue, as well as to information on party affiliation from voter registration records. Our sample consists of 449 corporate credit analysts with non-missing information on their party affiliation, working at Moody's, Standard and Poor's, and Fitch, between 2000 and 2015. These analysts cover a total of 1,778 U.S. firms. Motivated by the above-cited studies of U.S. households, the goal of our study is to test whether the rating behavior of credit rating analysts depends on their political alignment with the White House.

Credit rating analysts provide a suitable setting for a number of reasons. First, individual analysts have been documented to have substantial influence over credit ratings (Fracassi, Petry, and Tate (2016)). Since credit ratings affect firms' cost of capital and, in turn, their financial policy and investment decisions (Chernenko and Sunderam (2011); Almeida, Cunha, Ferreira, and Restrepo (2017)), partisan bias among this group of analysts can have real effects. Second, rating the creditworthiness of an issuer requires making long-term forecasts, which likely provides more room for partisan bias to materialize than short-term forecasts, such as one-quarter-ahead earnings. Third, by comparing analysts with different political affiliations, who analyze the *same firm* at the *same point in time*, we can control for differences in the task and information environment. Fourth, by comparing analysts' rating actions under Democratic and Republican presidents, we can separate the effect of partisan bias from other time-invariant individual characteristics.

We find that partisan bias affects credit ratings. Analysts who are not affiliated with the President's party are more likely to adjust ratings downward, relative to other analysts covering the same firm at the same point in time. Specifically, analysts who are not affiliated with

the President's party downward-adjust ratings more by 0.015 notches per quarter. This effect corresponds to 9.25% relative to the average absolute quarterly rating adjustment and is therefore economically sizable. Our identification strategy ensures that this result cannot be explained by several potential confounding factors. Most importantly, following Fracassi, Petry, and Tate (2016), we control for non-random matching of analysts to firms by including firm × quarter fixed effects in the regression. By comparing only analysts rating the same firm at the same point in time, we are able to rule out the possibility that Democratic analysts rate firms which tend to do well under the policies of Democratic presidents. Moreover, our empirical analysis allows us to control for differences in rating methodologies across rating agencies, as well as for unobserved time-invariant differences across analysts.

To further support our interpretation that the above result reflects partisan bias, we investigate whether our effects are more pronounced in periods with high partisan conflict. We use the Partisan Conflict Index provided by the Federal Reserve Bank of Philadelphia, which tracks the degree of political disagreement among U.S. politicians at the federal level by measuring the frequency of newspaper articles reporting disagreement in a given month. We find that the effect of partisan bias is roughly twice as large when partisan conflict increases by one standard deviation. Furthermore, we also find larger magnitudes during presidential election quarters, as well as for analysts who vote frequently.

While the above results show that analysts whose party affiliation does not match the President's are more likely to revise their ratings downward relative to analysts who are aligned, it is unclear which of these two groups is "right." In particular, we do not know whether analysts with ideological mismatch downward-adjust by too much, or whether analysts whose ideology matches the President's downward-adjust by too little, or both. To address this question, we estimate the effect of partisan bias on ratings accuracy, defined as the product of the quarterly ratings change and future changes in credit spreads. While our results on analyst accuracy are estimated less precisely relative to our main results on rating changes (which have a causal interpretation), they suggest that analysts who do not support the President's party downward-adjust by too much

and are less accurate.

To the best of our knowledge, this is the first study to identify a significant effect of partisan bias on the behavior of finance professionals. Our results suggest that partisan bias distorts the rating decisions of credit rating analysts; in particular, those of analysts who are not politically aligned with the White House. Since credit ratings have been shown to significantly affect firms' cost of capital (Fracassi, Petry, and Tate (2016)) and investment (Chernenko and Sunderam (2011); Almeida, Cunha, Ferreira, and Restrepo (2017)), distortions in credit rating decisions may have real effects. In addition, we show that partisan bias lowers ratings accuracy, which may create distortions in the analyst labor market. Finally, if partisan bias distorts the decisions of credit rating analysts, it may also affect the decisions of other relevant economic agents. Given that such bias prevails even in a setting where pecuniary and professional gains are at stake,[4] it may be even more pronounced in less competitive labor markets. This is a fruitful avenue for future research in our view.

The rest of this study proceeds as follows. In the next section, we discuss the related literature. Section 3 presents the data, the sample construction, and summary statistics. Section 4 describes the empirical strategy. Section 5 examines whether analysts' rating actions are influenced by partisan bias. Section 6 discusses how rating actions by partisan analysts may affect firms' cost of capital and investment, and Section 7 concludes.

2.   Related Literature

Our findings contribute to a growing literature on the effect of partisanship on economic behavior. Most of the existing studies have focused on partisan bias among households, and studies of consumption behavior have produced mixed results. In an early paper, Gerber and Huber (2009) demonstrate that consumption changes following a political election are correlated with whether or not the election was won by the preferred political party of the respondent. Gillitzer and

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[4]The accuracy of analysts' ratings has been shown to be a strong predictor of future career outcomes (e.g., Kisgen, Osborn, and Reuter (2016) and Kempf (2017)).

Prasad (2016), analyzing Australian elections, find that changes in sentiment around elections are also associated with future vehicle purchase rates. Benhabib and Spiegel (2017) document a positive relation between partisan-related sentiment and state-level GDP growth. Makridis (2017) uses individual-level data from Gallup and shows that consumption of non-durable goods rose more among conservatives during the 2016 Presidential election. However, other studies have not found a connection between partisanship and household consumption. McGrath (2017) extends the sample in Gerber and Huber (2009) and concludes that there is no evidence of an effect of partisan bias on spending. Mian, Sufi, and Khoshkhou (2017) using individual-, zip-, and country-level data, do not find a relationship between partisan bias and household spending. Furthermore, studies have examined partisanship and household asset allocation. Bonaparte, Kumar, and Page (2017) show that investors' portfolio allocation to risky assets is influenced by whether their preferred party is in power. Similarly, Meeuwis, Parker, Schoar, and Simester (2018) find that Republican investors actively increase the share of equity and the market beta of their portfolio relative to Democrats following the U.S. election of November 2016.

Moreover, our results contribute to studies that have investigated partisan bias in other groups of professionals. Hersh and Goldenberg (2016) find evidence of partisan bias among medical doctors, as doctors with different political affiliations recommend different treatment plans for politically sensitive health issues. Posner (2008), McKenzie (2012), and Chen (2017) document evidence of partisan biases among judges. Our work complements and adds to these studies by focusing on finance professionals.

Our study also contributes to the literature on behavioral biases of credit rating analysts. Fracassi, Petry, and Tate (2016) find evidence of systematic optimism and pessimism among credit analysts. Cornaggia, Cornaggia, and Xia (2016) document that analyst-level conflicts of interest can affect credit ratings. Adding to this research, our study explores the role of partisan bias as a source of distortions in credit ratings. Our paper also relates more broadly to the literature on the determinants of credit ratings (see, for example, Becker and Milbourn (2011), Kisgen and Strahan (2010), Xia (2014), Griffin and Tang (2012)).

Furthermore, our study adds to the literature that studies how political affiliation corre-lates with the behavior of financial analysts, sell-side equity analysts, corporate managers, and investment managers. Prior studies have shown that mutual fund managers who make cam-paign donations to Democrats hold less of their portfolios in companies that are deemed socially irresponsible (Hong and Kostovetsky (2012)), that sell-side equity analysts who give political contributions to the Republican Party are more likely to issue conservative forecasts and recom-mendations (Jiang, Kumar, and Law (2016)), and that Republican firm managers maintain more conservative corporate policies (Hutton, Jiang, and Kumar (2014)). These studies focus on the time-invariant attributes that characterize Democrats versus Republicans, whereas we focus on how the behavior of analysts changes depending on whether their preferred party is in power. We can therefore separate the effect of partisan bias from other time-invariant characteristics of the individual analyst.

3. Data, Sample Construction, and Empirical Strategy

3.1 Data

The main dataset used in the analysis is constructed from the combination of corporate bond credit ratings data; press releases with analyst information; and voter registration records. We also complement the data with a variety of other data sources. The datasets are described below, and further details can be found in Appendix A.2.

3.1.1 Credit Ratings Information

We collect rating actions on U.S. corporate debt issuers from all three major ratings agencies: Fitch, Moody's, and Standard and Poor's. These are obtained for Standard and Poor's from S&P RatingXpress, for Moody's from Moody's Default and Recovery Database (DRD), and for

Fitch from Mergent.[5] The time period spans the years from 2000 to 2015.[6] Credit ratings are transformed into a cardinal scale, starting with 1 for Aaa and ending with 21 for C (Moody's) or D (for S&P and Fitch), as in Fracassi, Petry, and Tate (2016). We match each rating action (i.e., new rating, downgrade, upgrade, affirmation, internal review, reinstatement, and withdrawal) to a press release that contains the name(s) of the analyst(s) covering the firm. The press releases are collected from Moody's and Fitch's websites and S&P's Global Credit Portal. They usually contain two names, one more junior employee (typically the lead analyst), and one more senior employee (typically the rating committee chair).

3.1.2   Political Affiliation and Measure of Ideological Mismatch

Our political affiliation measure comes from the voter registration records from the State of Illinois, the State of New Jersey, and New York City.[7] The voter registration records contain identifying information, such as voter names, date of birth, and mailing address, as well as information on the voter's party affiliation at the time of a given election. The elections covered are general, primary, and municipal elections during the period of 1983–2017 for New York City, 1976–2017 for Illinois, and 2007–2017 for New Jersey. In Appendix A.2, we describe the information available in the voter registration records of each state in more detail.

For the purposes of our study, the voter registration data have several advantages. First, relative to the commonly used financial contribution data to political parties, candidates, or committees, found on the Federal Election Committee (FEC) website,[8] the voter registration data cover a larger part of the population.[9] Whereas political contributions allow us to infer party affiliation only for people who support a party financially, the voter registration records

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[5]Since Mergent provides bond ratings rather than issuer ratings, we follow the procedure by Fracassi, Petry, and Tate (2016) and select a representative issuer rating after excluding bonds that are exchangeable, putable, convertible, pay-in-kind, subordinated, secured, or guaranteed, as well as zero coupon bonds and bonds with variable coupons.

[6]We restrict the sample period to post 2000 because press releases with analyst information are sparse prior to 2000.

[7]We use data from New York City as opposed to the State of New York, because the State of New York does not provide voter histories.

[8]https://www.fec.gov/

[9]According to a study by Hill and Huber (2017), less than 10% of registered U.S. voters are federal or state donors.

allow us to infer party affiliation for all individuals who are registered with a political party at the time of an election. While these differences in the sample restriction may not be as crucial when studying influence of political affiliation of high-profile individuals, such as CEOs and board members, they are increasingly important when looking at employees who are not at the highest level of the firm, such as credit analysts, who might not regularly contribute financially to political campaigns. Second, voter registration records are able to capture political beliefs separately from the intention of political influence and social pressure. The latter is a particularly important concern, given the evidence by Babenko, Fedaseyeu, and Zhang (2017) that CEOs influence the political contributions of their employees. They find that CEO-supported political candidates receive three times more money from employees. Political affiliation inferred from voter registration records is less likely to be influenced by employees' managers. Finally, party registration has been shown to be a good predictor of self-reported party identification. Igielnik, Keeter, Kennedy, and Spahn (2018) match commercial voter files, which are based on data from voter registration records, with a large-scale survey on political attitudes and voter behavior and show that, for more than two-thirds of the panelists, the party affiliation in the commercial voter file correctly infers the self-reported party identification. The accuracy is even higher for states with party registration, such as New York.

3.1.3 Additional Data Sources

We rely on a variety of complementary data sources. First, to measure partisan conflict, we use the Partisan Conflict Index provided by the Federal Reserve bank of Philadelphia. The index tracks the degree of political disagreement among U.S. politicians at the federal level by measuring the frequency of newspaper articles reporting disagreement in a given month. Higher index values indicate greater conflict among political parties, Congress, and the President. Azzimonti (2014) provides a detailed description of the measure. Given that our main analysis is performed at the quarterly level, we compute the average Partisan Conflict measure in a given quarter. Second, we obtain firm-level information from Compustat Quarterly. Third, we compute quarterly credit

spreads using bond transaction data from TRACE. Specifically, we follow Fracassi, Petry, and Tate (2016) and compute spreads by taking the yield to maturity and subtracting the benchmark Treasury yield. We then average the daily spreads within the same bond-quarter across all senior unsecured bonds. We aggregate the resulting quarterly bond-level credit spreads at the firm level by computing the weighted average, where the weights are proportional to the principal amount.

Finally, we further supplement the data with hand-collected biographical information from online searches. We also use analysts' first and last names to obtain additional characteristics. For example, we infer analysts' ethnicity from their first and last names, using a publicly available API[10] (see Ye, Han, Hu, Coskun, Liu, Qin, and Skiena (2017)). Moreover, we infer the gender of the analysts from their first name, using the publicly available API `genderize.io`, as well as manual online searches.[11]

3.2   Sample Construction

After focusing on rating actions by analysts who work in the offices of Chicago and New York City, our sample consists of rating actions on 2,317 issuers by 967 analysts.[12] Since we require information on political party affiliation, we further restrict the sample to analysts that can be matched to a voter registration record. We match analysts to voters as follows. In a first step, we merge analysts to voters using first name, middle initial, and last name, keeping only exact matches. In the case of duplicate matches, we try to determine the correct match based on voter age and zip code.[13] In a second step, we merge the remaining unmatched analysts to voter records using only their first name and last name. The merging procedure is described in more detail in Appendix A.2.4. Our final sample includes 448 analysts, covering 1,778 firms.

While a match rate of ca. 46% (=448/967) might seem low at first sight, there are good

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[10]See `http://name-prism.com/`.

[11]The API uses a large dataset of first names and known genders gathered from user profiles across major social networks in order to predict gender. See `http://api.genderize.io/`.

[12]When the press release does not provide any office information, we assume that the analyst is based in New York. Given that more than 85% of all analysts with non-missing office location are based in New York, we believe this is a reasonable assumption.

[13]Information on analyst age is obtained via manual online searches.

reasons to expect a match rate below 50%. First, the share of registered voters in the total population of New York and New Jersey is 59% and 61%, respectively, as of November 2016;[14] it is ca. 53% in New York City.[15] Second, we will lose analysts who work in New York City but reside in Connecticut or other parts of the State of New York.[16] Third, we will lose analysts who did not update their voter registration to the state of their work location, whose names are spelled differently in the press releases than in the voter records, and who match to multiple voters among which we cannot determine a single correct match.

An important consideration is how representative our final sample is to the average rated U.S. firm and to the average credit rating analyst. First, we investigate whether analysts whom we are able to match to voter records rate different types of companies relative to analysts that we cannot match, by regressing an indicator for missing party affiliation on firm characteristics. The results, reported in the Online Appendix, show that analysts for whom we are able to obtain party affiliation rate overall similar firms as the rest of the analyst population. Two exceptions are ROA and cashflow: analysts who are registered voters tend to rate firms with higher cash flow but lower ROA. Second, in terms of selection based on observable analyst characteristics, we do not expect analysts who are registered voters to be representative of the overall analyst population. Given the focus of our study, which is to estimate the importance of partisan bias on the decisions of finance professionals, restricting the analysis to the subset of partisan analysts, even if these differ from the population of non-partisan analysts, is justified. We provide a comparison of partisan analysts relative to the population of unregistered analysts, as well as a comparison of Democratic and Republican analysts, in the Online Appendix. The rating actions are converted into an analyst-firm-quarter panel by using the most recent rating at the end of

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[14]U.S. Census Bureau. Data available at `https://www.census.gov/data/tables/time-series/demo/voting-and-registration/p20-580.html`.

[15]The 53% ratio for New York City is obtained after dividing 4.6 million registered voters in 2017 (New York City Campaign Finance Board (2018)) by the total population of 8.5 million in 2016. Information on total population is obtained from the NYC Department of City Planning; available at New York City Campaign Finance Board (2018).

[16]According to data from the NYC Department of City Planning, 12% of the persons who work in New York City reside in Connecticut and other parts of New York State which are not covered by our voter records. Data available at `https://www1.nyc.gov/assets/planning/download/pdf/data-maps/nyc-population/acs/ctpp_p6_nyc_boro_06_10.pdf`.

a given quarter and the analyst information from the most recent press release for the firm. To minimize measurement error in the analyst assignment, we do not use analyst information from press releases that are older than five years as of quarter end, and we do not assign analysts to quarters beyond the date of the final report for a given agency-firm pair.

3.3   Summary Statistics

Table 1 and Figure 2 report summary statistics. 23% of the analysts who are registered voters are unaffiliated, 46% are registered Democrats, 30% are Republicans, and 1% are registered with a party other than Democrat or Republican. In 36% of the analyst-firm-quarters, the analyst's party affiliation does not match that of the President (*"ideological mismatch"*).[17] Figure 2a reports the average party affiliation by GICS sector. Some of the industries with the highest share of Democratic analysts are utilities (66%), financials (60%), and materials (56%). These patterns could be driven by factors such as the geography of where analysts grew up, which may influence both their party affiliation and the sectors they choose to cover. For example, analysts who grew up in New York (a "blue state") likely had more exposure to the financial sector, while analysts from Texas (a "red state") may be more familiar with energy companies. Figure 2b shows that the percentage of Democrat analysts is higher in New York City (47%) than in Chicago (39%). Figure 2c compares the political affiliation of analysts in the three different rating agencies. At Standard and Poor's, Fitch, and Moody's, 55%, 46%, and 39% of the analysts are Democrats, respectively. The median analyst is in the sample for approximately 5 years (see Figure 1).

Our main dependent variable is the quarterly change in the credit rating (measured in notches). Because credit ratings are transformed into a cardinal scale, starting with 1 for Aaa and ending with 21 for C (Moody's) or D (for S&P and Fitch), as in Fracassi, Petry, and Tate (2016), a positive rating change indicates a downgrade. The average credit rating change is

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[17]In our baseline definition, *ideological mismatch* is zero both for unaffiliated analysts and for analysts whose party affiliation matches that of the President. In our robustness tests in the Appendix, we show that we obtain very similar results if we restrict the sample to Democrat and Republican analysts only.

0.019 notches, confirming evidence from prior studies that downgrades are more common than upgrades (Dichev and Piotroski (2001); Hand, Holthausen, and Leftwich (1992); Holthausen and Leftwich (1986)). 11% of the observations in our sample have a rating change, which is consistent with evidence from prior literature (e.g., Becker and Ivashina (2014)). We study rating changes instead of levels for two reasons. First, they allow us to better isolate the decisions of the current analysts from other confounding factors, such as rating-agency or previous-analyst fixed effects, as well as the ideological mismatch of the previous analyst. Second, they allow for the possibility that differential rating actions may occur gradually over time as opposed to only at the beginning of a new administration.

Figure 3 presents the average adjusted rating change for Democratic, Republican and un-affiliated analysts under Democratic and Republican presidents, respectively. Adjusted rating changes are computed by taking the quarterly rating change and subtracting the average rating change within the same firm and quarter, averaged across all agencies rating the firm. This allows us to control for the possibility that the party affiliation of the analysts covering a given firm may correlate with the firm's fundamentals and investment opportunities under different administrations. Even in this univariate comparison, we observe a pattern that is very consistent with our main multivariate analysis: during a Democratic presidency, Democratic analysts are more likely to upgrade relative to Republican analysts. Under a Republican president, the sign of this difference reverses: Democratic analysts downward-adjust more relative to Republican analysts under a Republican president. Importantly, the rating behavior of unaffiliated analysts remains very similar under Democratic and Republican presidents.

4. Empirical Strategy

Measuring the influence of partisan bias on rating decisions by credit analysts is empirically challenging. If analysts were randomly assigned to firms and party affiliation was randomly assigned to analysts and rating agencies, we could measure the effect of partisan bias or ideology mismatch by comparing the rating actions of analysts who are aligned with the President's party

to the rating actions of analysts who are not aligned the President's party. However, such a comparison is complicated by a number of factors. First, analysts are unlikely randomly assigned to firms. It is conceivable that analysts with a certain political ideology specialize in sectors or firms whose fundamentals are affected by the party affiliation of the President. For example, it could be that Republican analysts are more likely to rate firms whose value increases under Republican presidents and decreases under Democratic presidents (e.g., oil and gas companies), and therefore downgrade more often under Democratic than under Republican administrations. In the presence of such non-random matching, the estimated average difference in the rating actions between analysts with different ideologies may not reflect the effect of partisan bias, but differences in the fundamentals of the firms they cover. Second, credit analysts may not be randomly assigned to rating agencies. As Figure 2c shows, the mix of Democratic versus Republican analysts varies substantially across agencies. If political cycles correlate with asset returns (see Pastor and Veronesi (2018)), and rating agencies' methodologies differ on how they incorporate economic variables into their models, then differences in ratings between agencies over the political cycles might not be due to political bias, but due to the non-random composition of political affiliations across agencies. Suppose, for example, that Moodys rating methodology leads to more conservative ratings during a recession. Since Moodys has more Republican analysts and most recession periods during our sample occur under a Democratic President, a positive coefficient on ideology mismatch might simply be a result of differences in rating methodology rather than differences in the political affiliation of the analyst. Third, party affiliation may be related to other time-invariant characteristics of the analyst, such as upbringing, education, prior work experience, or attitudes towards certain industries or firms.

Our identification strategy removes the above confounding factors by regressing the rating change for firm f rated by analyst i in quarter t on firm $\times$ quarter fixed effects as well as agency $\times$ industry $\times$ quarter fixed effects:

$$\Delta R_{ift} = \alpha_{ft} + \alpha_{ajt} + \beta Ideological\ Mismatch_{it} + \gamma' X_{it} + \epsilon_{ift}. \tag{1}$$

a denotes the rating agency and j denotes the firm's industry identified by the first two digits of the SIC code. $Mismatch_{it}$ is an indicator equal to one if the analyst's party affiliation does not coincide with the party of the elected President in quarter t. In a presidential election quarter, we define $Mismatch_{it}$ using the newly elected president, since the election results are known by the end of the quarter. X_{it} refers to a vector of controls that mainly includes indicator variables for the analyst's party affiliation and controls for analyst tenure. By including firm $\times$ quarter fixed effects, we are effectively comparing analysts who cover the same firm at the same point in time. Hence, our results cannot be driven by analysts with ideological mismatch covering different types of firms. Including agency $\times$ industry $\times$ quarter fixed effects addresses concerns that different methodologies of credit agencies might be correlated with the political cycle during our sample period. Finally, the coefficient of interest is the coefficient on $Mismatch$ (β), which interacts the political affiliation of the analyst with the political party of the president. Specifically, mismatch is defined as an indicator function equal to one if the analyst's party affiliation does not match the party of the President, and zero if the party either matches or if the analyst is unaffiliated. By construction, our estimator of partisan bias is orthogonal to the baseline effect of the analyst's political affiliation itself and, hence, to a number of unobserved analyst characteristics that may explain differences in party affiliation. Furthermore, we present robustness tests that include analyst $\times$ firm fixed effects, which address potential concerns that ideological mismatch could be correlated with time-invariant attitudes towards certain industries or firms, as well as with any static analyst characteristics. We cluster standard errors at the firm level.[18]

4.1 Can individual analysts influence ratings?

A necessary condition for analyst incentives to affect ratings quality is that the ratings process needs to provide sufficient room for individual analysts to affect the final rating of the security. This is not obvious given that the final rating decision is taken by a committee. Upon receiving a rating application from a potential customer, the rating agency typically assigns a lead analyst to

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[18]In the robustness tests presented in the appendix, we show that our results are robust to double clustering by firm and analyst.

the ratings process. The lead analyst meets with the customer to discuss relevant information, which she subsequently analyzes with the help of an analytical team. She then proposes a rating and provides a rationale to the rating committee, which consists of a number of credit risk professionals determined by the analyst in conjunction with the committee chair. Once the rating committee has reached its decision, the rating agency communicates the outcome to the customer and publishes a press release.[19] The ratings process therefore provides ample opportunities for individual analysts to influence the final rating, even if the final decision is taken by a committee. Lead analysts guide meetings with the customer, request and interpret information, and play a key role in the rating committee by proposing and defending a rating recommendation based on their own analysis. In addition, the rating committee chair serves a special role by influencing the composition of the rating committee and acting as the moderator.

How much individual analysts are able to influence ratings is ultimately an empirical question. Fracassi, Petry, and Tate (2016) attribute a substantial part of the variation in corporate bond ratings to individual analysts: they explain 30% of the within-firm variation in ratings. For securitized finance ratings, Griffin and Tang (2012) provide evidence that CDO ratings by a major credit rating agency frequently deviated from the agency's main model, reflecting room for subjectivity in the ratings process. In addition, Kisgen, Osborn, and Reuter (2016) and Kempf (2017) document that the rating decisions of lead analysts and committee chairs predict both internal promotions and external hiring by investment banks, suggesting that, by revealed preference, these parties see valuable information in the decisions by these analysts.

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[19]See Fracassi, Petry, and Tate (2016) and `https://www.moodys.com/sites/products/ProductAttachments/mis_ratings_process.pdf` for a description of the ratings process at Moody's, `https://www.standardandpoors.com/en_AU/delegate/getPDF?articleId=2053416&type=COMMENTS&subType=REGULATORY` for the ratings process at S&P, and `https://www.fitchratings.com/site/dam/jcr:b05b5bd2-0443-4338-815b-81a809840e65/Form%2025-101F1%20Item%205.pdf` for the ratings process at Fitch.

5. Partisan Bias and Analysts' Rating Actions

This section presents our main results. We first document that partisan bias has a significant influence on analysts' credit rating decisions, by showing that analysts whose party affiliation does not match the president's are more likely to adjust ratings downward. This result is robust to various measures of rating changes, sample restrictions, and estimation methods. We further show that the effect of partisan bias is more pronounced during quarters where partisan conflict is high, and for analysts who are regular voters. Finally, analysts who disagree with the president tend to be less accurate, suggesting that their increased propensity to adjust ratings downward is not justified by firm fundamentals.

5.1 Rating Changes

We estimate the regression from Equation 1 with different sets of fixed effects and report the results in Table 2. Specifically, we regress quarterly ratings changes on ideological mismatch, an indicator equal to one if the analyst's party does not match the current President's, and controls. Since we focus on credit rating analysts, we are identifying differences in analysts' expectations of firm fundamentals only to the extent that they affect credit risk.

We begin by including agency fixed effects in addition to firm × quarter fixed effects, since Figure 2 documents pronounced differences in political ideological across different agencies. The results, reported in column (1), suggest that partisan bias affects analysts' rating behavior. Specifically, analysts who do not support the President's party are more likely to adjust ratings downward by 0.0206 notches relative to analysts who are aligned with the President's party or unaffiliated. The economic and statistical significance of the effect of partisan bias remains high as we tighten the identification to include agency × industry fixed effects, where industries are defined based on 2-digit SIC codes. Our strictest specification, which includes both firm × quarter and agency × industry × quarter fixed effects (column (3)), suggests that an analyst who is misaligned with the President's party on average downward-adjusts ratings more by 0.0150 notches. Relative to the average absolute rating change of 0.16 notches, this is an economically

sizable increase of 9.38%. It is also informative to compare the effect to the distribution of the agency × industry × quarter fixed effects. Specifically, the effect of partisan bias corresponds to 41% of the interquartile range in the distribution of agency × industry × quarter fixed effects. In other words, ideological mismatch has an effect that is roughly equivalent to moving from the 25th to the 50th percentile of the agency-industry-quarter distribution.

We emphasize that our set of high-dimensional fixed effects eliminates a lot of potentially confounding variation and allows us to overcome some of the central challenges in empirical studies of partisan bias. Most importantly, the firm × quarter fixed effects address the possibility that the relationship between partisan bias and rating actions may be confounded by non-random matching of analysts to firms. In addition, by including agency × industry × quarter fixed effects, we can remove any differences in rating methodologies across rating agencies and industries.

A potential concern could be that our ideological mismatch variable picks up the effect of other analyst characteristics that may be correlated with party affiliation. Note that such unobservable characteristics would pose a threat to our identification only if they can explain differential behavior under Democratic vs. Republican administrations. It is not obvious what characteristics that might be. To nevertheless address this potential issue, Table 3 repeats the analysis from Table 2, while including additional analyst characteristics as well as their interaction with an indicator for Democratic Presidents. We include characteristics that are known to be important predictors of political affiliation: ethnicity, gender, and age. Across all three specifications, the coefficient estimate on ideological mismatch is, if anything, slightly larger when we include these additional control variables. Overall, we conclude that the effect of partisan bias is not subsumed by other analyst characteristics.

5.2   Robustness Tests

Table 4 presents additional robustness tests for the main result in Table 2. Unless otherwise mentioned, we report results for the specification in Table 2, column (3), and suppress all control variables for brevity.

In Panel A, we alter the definition of the dependent variable. In order to mitigate the concern that our main result could be driven by outliers in the dependent variable, we modify the rating change variable to take only three possible values: $+1$ for downgrades, 0 for no change, and -1 for upgrades. The result is very similar and the statistical significance, if anything, increases. When we separate the propensity to upgrade versus downgrade, we find that the effect is coming from both sides: analysts who are not aligned with the White House are both more likely to downgrade and less likely to upgrade.

Panel B shows results for a definition of mismatch that uses party affiliation only from presidential elections, as opposed to all elections. In addition, in the Online Appendix, we report the main specifications when we restrict the sample to Democrats and Republicans only. The estimates suggest that Democratic analysts downward-adjust more, relative to Republican analysts, under Republican presidents. Under Democratic presidents, the gap between the two groups closes to approximately zero. We also add unaffiliated analysts, consisting of registered voters who are unaffiliated as well as analysts who are not registered voters. On average, the group of unaffiliated analysts seems to behave more like Democratic analysts than Republican analysts.

In unreported regressions (available upon request), we also test whether ideological mismatch with the party that controls the U.S. Senate or the U.S. House of Representatives matters for rating decisions. Since those results are insignificant, we do not report them in the paper. The insignificance of the party in control of Congress is consistent with previous studies of political cycles and stock returns (Santa-Clara and Valkanov (2003), Pastor and Veronesi (2017)). We do not have a complete explanation for why disagreement with the President matters, while disagreement with Congress does not. We speculate that this result could be driven by the party of the U.S. President being more salient to analysts than the identity of the party that controls Congress. For example, according to a Gallup poll from 2014, only 41% of the surveyed registered voters are able to correctly identify the majority in both the Senate and the House (Gallup (2014a)). Moreover, only 32% of registered voters prefer a one-party control of Congress (Gallup (2014b)). We leave the examination of the role of Congress to future research.

In Panel C, we assess the robustness of our results with respect to alternative estimation methods. Double-clustering by firm and analyst yields very similar results. We also verify that we find similar results if we estimate the regression in Equation (1) at the agency-firm-quarter level rather than at the analyst-firm-quarter level, i.e., we compute the average ideological mismatch across all analysts of the same rating agency covering the same firm at the same point in time. Next, we estimate a weighted regression, where weights are proportional to the total book assets of the rated firm. We also show that our results are robust to including analyst × firm instead of agency × industry × quarter fixed effects. The latter result is important, because it reinforces our argument that we are capturing the effect of partisan bias separately from other time-invariant characteristics of the analyst, which may be correlated with party affiliation.

In Panel D, we test whether our results are robust to two sample restrictions: we exclude analysts who are young than 22 or older than 65 at the time of the rating (indicating potential false matches), and we exclude the years 2008 and 2009 from our analysis. The latter addresses the potential concern that our baseline results reflect a differential response of Democratic and Republican analysts to the financial crisis, as opposed to a differential response to different White House administrations. Under both sample restrictions, the estimated magnitude of the effect becomes, if anything, larger.

Finally, Figure 4 plots our coefficient estimates separately for different GICS sectors.[20] For all sectors, the sign of the coefficient estimate for ideological mismatch is positive. The effect is statistically significant in utilities, information technology, industrials, materials, and energy. Overall, we conclude that our main result is robust to a large set of different specifications and estimation approaches.

5.3 The Influence of Partisan Conflict and Political Activeness

The results presented in Table 2 suggest a causal relationship between partisan bias and analysts' rating actions. To further support this interpretation, we investigate whether the effect

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[20]We exclude sectors that represent less than 5% of our total observations (Real Estate and Telecommunication) in order to avoid very noisy coefficient estimates.

of partisan bias is more pronounced during periods of greater partisan conflict, and for analysts who are more politically active.

To measure partisan conflict, we use the Partisan Conflict Index provided by the Federal Reserve Bank of Philadelphia. This index tracks the degree of political disagreement among U.S. politicians at the federal level by measuring the frequency of newspaper articles reporting disagreement in a given month. In Table 5, columns (1) and (3), we interact ideological mismatch with partisan conflict measured during the previous quarter. The estimates in column (3) imply that the effect of partisan bias increases by 0.0123 ($=0.0004 \times 30.63$) notches for a one-standard-deviation increase in partisan conflict, thereby roughly doubling the baseline effect. We also hypothesize that the effect is stronger during presidential election quarters, when party affiliation is particularly salient and new information about the election outcome is revealed. We therefore interact ideological mismatch with an indicator for presidential election quarters in columns (2) and (3). Given that we only have four presidential elections during our sample period, the coefficient estimate is very noisy and only marginally statistically significant in column (3), where we simultaneously control for partisan conflict. It is nevertheless economically sizable: the effect of ideological mismatch is 0.0248 notches higher during election quarters.

Next, we investigate whether partisan bias is more pronounced among analysts who are politically active. In Table 6, we test three distinct proxies for political activeness, based on how frequently the analyst votes. First, we identify analysts who vote in midterm or primary elections (column (1)). Next, we count the number of past elections in which the analyst has voted (column (2)). Third, we compute the average time gap (in quarters) between the elections in which the analyst has voted (column (3)). Since some of these measures are strongly correlated with voter age, we also control for the interaction between voter age and ideological mismatch. All three measures indicate that the effect of partisan bias is substantially stronger for analysts who vote more frequently.

Overall, the importance of partisan conflict and voting frequency strongly support the interpretation that our results are capturing the effect of partisan bias, and further raises the bar for

alternative explanations.

5.4   Analyst Accuracy

The results so far suggest that analysts make different rating adjustments depending on whether the White House is controlled by the party they are registered with. What we cannot disentangle based on the existing results is which of the two groups of analysts, those with and without ideological mismatch, is "right" or "wrong," or whether the rating actions of both groups are equally distorted. In other words, it remains unclear whether the analysts who are aligned with the President's party downgrade too little, or whether those who are misaligned downgrade too much, or both.

In order to shed light on this question, we estimate the effect of ideological mismatch on rating action accuracy. We propose a measure of accuracy that builds on existing work by Fracassi, Petry, and Tate (2016) and Kisgen, Osborn, and Reuter (2016). Specifically, we measure the accuracy of the rating action on firm f in quarter t by analyst i as the current-quarter rating change times the future change in credit spreads (s):

$$Accuracy_{ift} = \Delta R_{ift} \times (s_{f,t+h} - s_{ft}). \tag{2}$$

Intuitively, if the analyst issues a downgrade ($\Delta R_{ift} > 0$) and subsequently credit spreads on the firm's bonds increase (decrease), she is coded as being more accurate (inaccurate). We then regress this measure of accuracy on ideological mismatch and controls. Because future changes in credit spreads do not vary within firm × quarter, we replace the firm × quarter fixed effects by industry × quarter fixed effects, and include additional firm-level controls. Specifically, we control for lagged firm size, leverage, cash ratio, average rating, Tobin's Q, past revenue and asset growth, cash flow, ROA, R&D, and Capex.

Table 7 reports the results. We vary the horizon over which the change in credit spreads is measured from 1, 2, 4, 8, and 12 quarters. Ideological mismatch tends to be negatively associated with rating action accuracy. The economic magnitude of this negative relationship increases as

the future change in credit spreads is measured over longer horizons, at least up an horizon of eight quarters. The magnitude decreases again for a horizon of twelve quarters. Although the sign is consistent across horizons, only accuracy measured over horizons of four and eight months is statistically significant. The point estimate in column (4) suggests that ideological mismatch reduces analyst accuracy by 0.22 percentage points, which corresponds to 4.6% (=0.0022/0.048) of one standard deviation in analyst accuracy measured over eight quarters.

Overall, the results presented in Table 7 suggest that analysts who do not support the President downgrade firms by more than what could be justified by firm fundamentals. This is an informative result, because the previously documented differences in the average rating adjustment cannot speak to which group of analysts exhibits a greater distortion in their rating decisions. One caveat of the accuracy analysis is that, since we cannot include firm × quarter fixed effects, the results are more subject to concerns related to non-random matching of analysts to firms.

6. Consequences of Analysts' Partisan Bias

The results in this paper show that partisan bias affects corporate credit ratings. The goal of this section is to gauge the magnitude of the consequences of the documented rating changes for firms' cost of capital and investment. First, we document that rating changes by partisan analysts trigger a significant stock price reaction in the case of downgrades. Second, we perform a back-of-the-envelope calculation to gauge the potential real effects of analysts' partisan bias, by combining our estimates with causal estimates of the effect of rating changes on firm investment from Almeida, Cunha, Ferreira, and Restrepo (2017).

6.1 Effect of Rating Changes on Common Stock Prices

A number of studies have documented the effect of rating changes on common stock prices. The general conclusion of this literature, starting with the work by Holthausen and Leftwich

(1986), is that downgrades are associated with significant negative abnormal stock returns, even after eliminating observations that contain concurrent, potentially contaminating, news releases. There is little evidence that upgrade announcements trigger abnormal returns.

We replicate these findings for our sample of rating change announcements. Retaining all rating actions by analysts whom we can match to a voter record, we compute abnormal stock returns around the date of the rating change reported in Moody's DRD, S&P RatingXpress, and Mergent, respectively. Abnormal returns are calculated using the Fama and French (1993) and Carhart (1997) model estimated over trading days (-150,-50) relative to the rating change. In the Online Appendix, we plot the abnormal returns in the 21 days around the upgrade and downgrade announcements in our sample. We remove any rating changes where the firm makes an earnings announcement or M&A announcement inside the (-10,+10) window.[21] Consistent with the evidence from prior studies, we find large negative abnormal returns in the three days around rating downgrades, and very small abnormal returns around upgrades. Table 8, Panel A, columns (1) and (2), further shows that the average three-day cumulative abnormal return (CAR) around rating downgrades is ca. -1.8% and statistically significant, even after removing concurrent announcements. The three-day cumulative abnormal return around rating upgrades, on the other hand, is insignificant (Panel B).

Given that we have established that analysts whose party affiliation does not match the President's downward-adjust more frequently, and likely by too much, a remaining question is how well the market is able to "correct" for the bias of these analysts. In other words, is the stock market reaction to downgrades muted when they are issued by misaligned analysts?

Columns (3) to (6) in Table 8 examine this question. Specifically, we regress three-day CARs on an indicator equal to one if the analysts who issued the rating action are politically misaligned with the President, and zero otherwise. Columns (3) and (4) in Panel A indicate that abnormal returns around downgrades by mismatched analysts are somewhat more positive, but the difference is not statistically significant. Columns (5) and (6) control for the magnitude of the rating change (in notches). Conditional on the same magnitude of the rating change,

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[21]Earnings announcement dates are obtained from IBES and M&A announcement dates from SDC Platinum.

downgrades by mismatched analysts do not appear to trigger statistically different returns.

Why do stock prices not differentiate more between the downgrades by partisan analysts? While a complete answer to this question is beyond the scope of this paper, we speculate there could be at least two reasons. First, since access to registered voter lists is often limited to purposes of political campaigns and education, the party affiliation of the analyst is not public information. This argument is somewhat incomplete, in so far as investors may still choose to purchase commercial voter files. Second, in the presence of rating-based regulatory frictions, downgrades may affect the supply of capital to firms, even if the rating change itself does not reveal any new information to the market. Overall, we conclude that rating actions by partisan analysts are associated with sizable changes in stock prices.

6.2 Back-of-the-envelope Calculation for Potential Real Effects

Since credit ratings have been shown to significantly affect firms' cost of capital and, in turn, their financial policies and investment decisions (Chernenko and Sunderam (2011), Almeida, Cunha, Ferreira, and Restrepo (2017)), distortions in analysts' rating decisions may have real effects. We gauge the magnitude of the potential real effects by combining our estimates of the sensitivity of changes in credit ratings with respect to analysts' partisan bias and estimates of the sensitivity of firms' cost of capital and investment with respect to changes in credit ratings from Almeida, Cunha, Ferreira, and Restrepo (2017).

In Table 2, we find that partisan bias can explain a 0.06-notch ($= 0.0150 \times 4$) differential in the annual credit rating change for the average firm in our sample. Exploiting exogenous variation in corporate ratings due to rating agencies' sovereign ceiling policies, Almeida, Cunha, Ferreira, and Restrepo (2017) show that a 0.7-notch decrease in a credit rating is associated with a 61 basis-point increase in bond yields and a 6.4 percentage-point reduction in firm investment, which corresponds to 24% of the pre-event investment level for treated firms. Combining these estimates with our own estimates from Table 2 suggests that analysts' partisan bias may lead to a difference in bond yields of 5.23 ($= 61 \times 0.06/0.7$) basis points and a 0.55 ($= 6.4 \times 0.06/0.7$)

percentage-point difference in firm investment.

Obviously, these numbers are coarse and need to be taken with a grain of salt. The estimates from Almeida, Cunha, Ferreira, and Restrepo (2017) are based on an international sample of corporate bond issuers and represent local estimates for firms around the sovereign bound, which tend to be firms of the highest credit quality that may have easy access to alternative sources of capital. We find the effect of partisan bias to be strongest for highly rated issuers (Figure 5), suggesting that the use of real-effect estimates for highly rated firms is appropriate. However, to the extent that the real effects of credit rating changes differ across countries and time, the true real effects of partisan bias may be different than in our back-of-the-envelope calculation above.

7. Conclusion

We show that partisan bias affects the decisions of financial analysts. Using a novel dataset that links credit rating analysts to party affiliations from voter registration records, we show that analysts who do not support the President's party are more likely to adjust ratings downward. Our identification approach compares analysts with different party affiliations covering the same firm at the same point in time, ensuring that differences in firm fundamentals cannot explain the observed differences in rating actions. We further show that the effect is more pronounced in periods of increased partisan conflict and during election quarters. Moreover, we find that analysts who do not support the President's party are less accurate, suggesting that they downward-adjust by too much.

To the best of our knowledge, this is the first study to identify a significant effect of partisan bias on the behavior of individuals in a professional environment. Given the documented strong increase in political polarization in the United States, it is important to understand the potential implications of this trend for the U.S. economy. One potential channel for partisan bias to matter could be through the actions of relevant economic agents. We look forward to future research exploring this issue.

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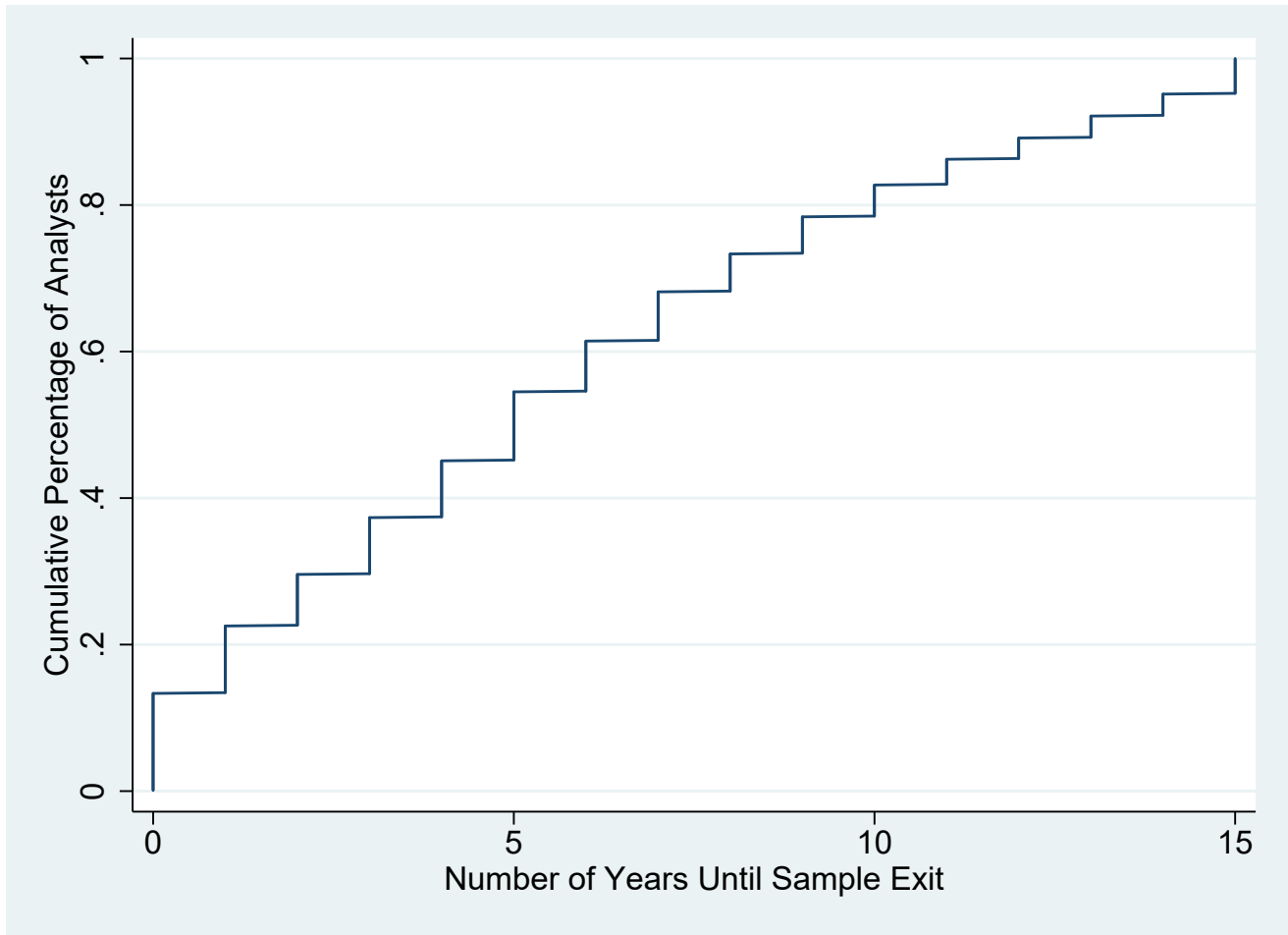
Figure 1: **Exit Probability.** The figure plots the cumulative percentage of analysts leaving the sample for a given number of years that they stay in the sample.
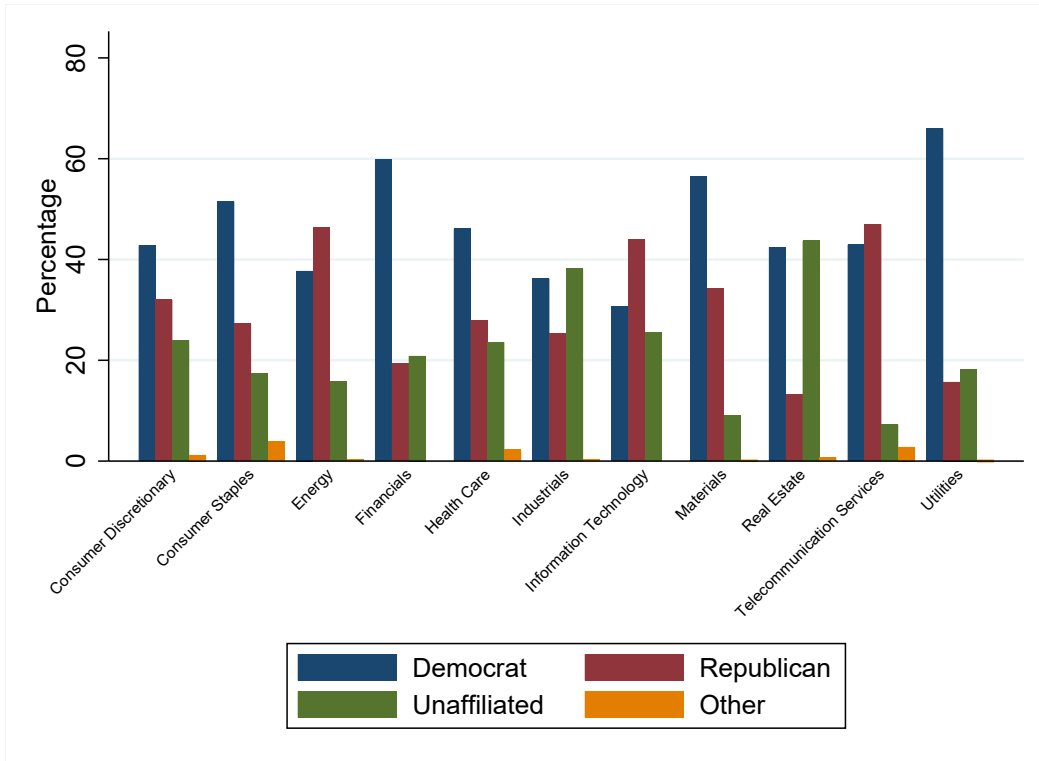
(a) Party Affiliation by GICS Sector
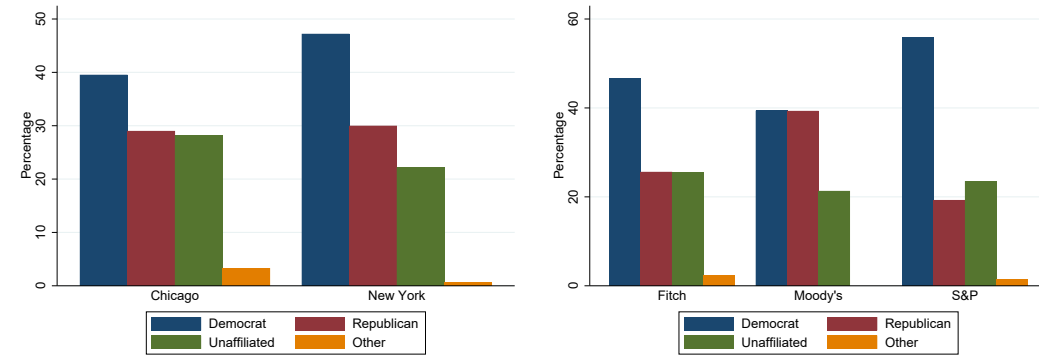
(b) Party Affiliation by City

(c) Party Affiliation by Rating Agency

Figure 2: **Party Affiliation Summary**
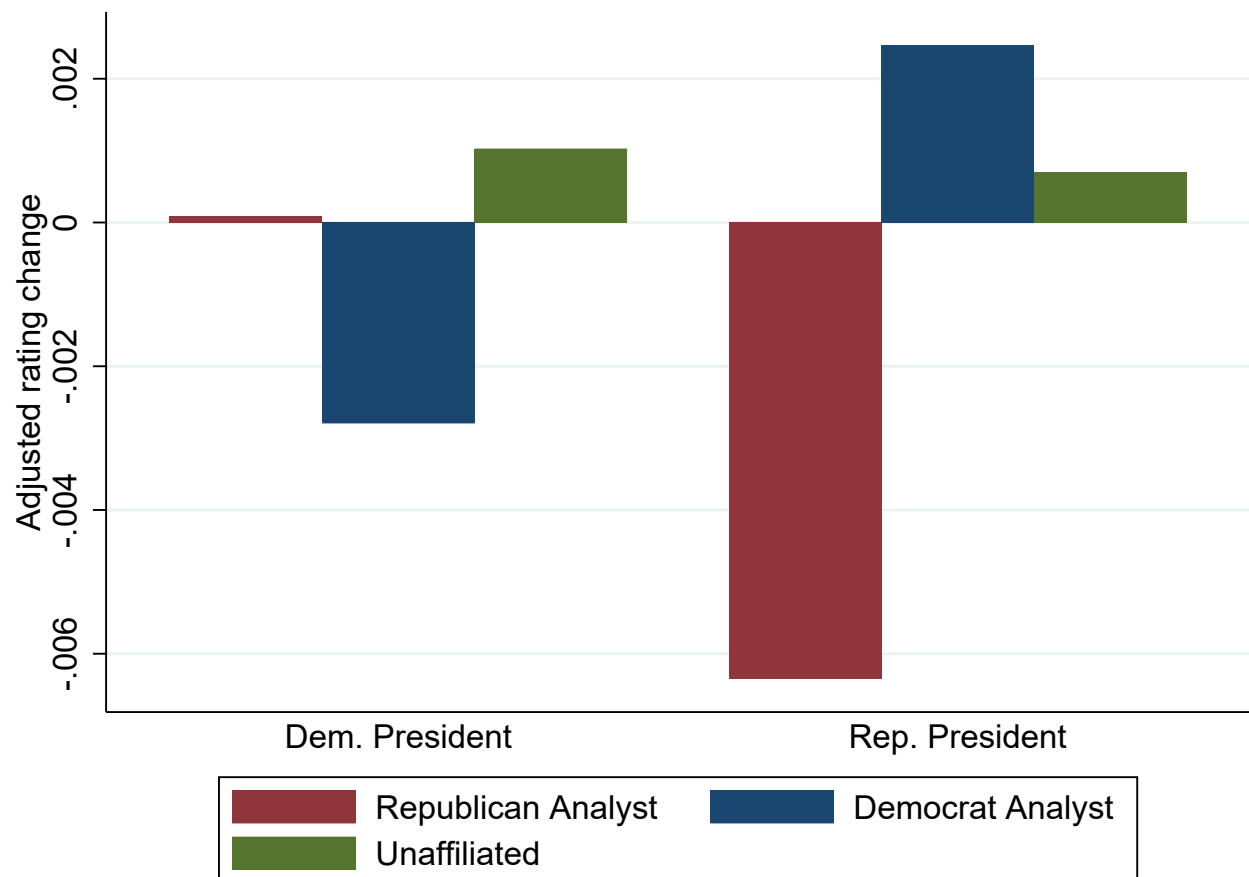
Figure 3: **Average Adjusted Rating Change by Analyst Party Affiliation.** The figure plots the average adjusted rating change separately for Democratic, Republican, and unaffiliated analysts under Democratic and Republican presidents. Adjusted rating changes are computed by taking the quarterly rating change and subtracting the average rating change within the same firm and quarter.
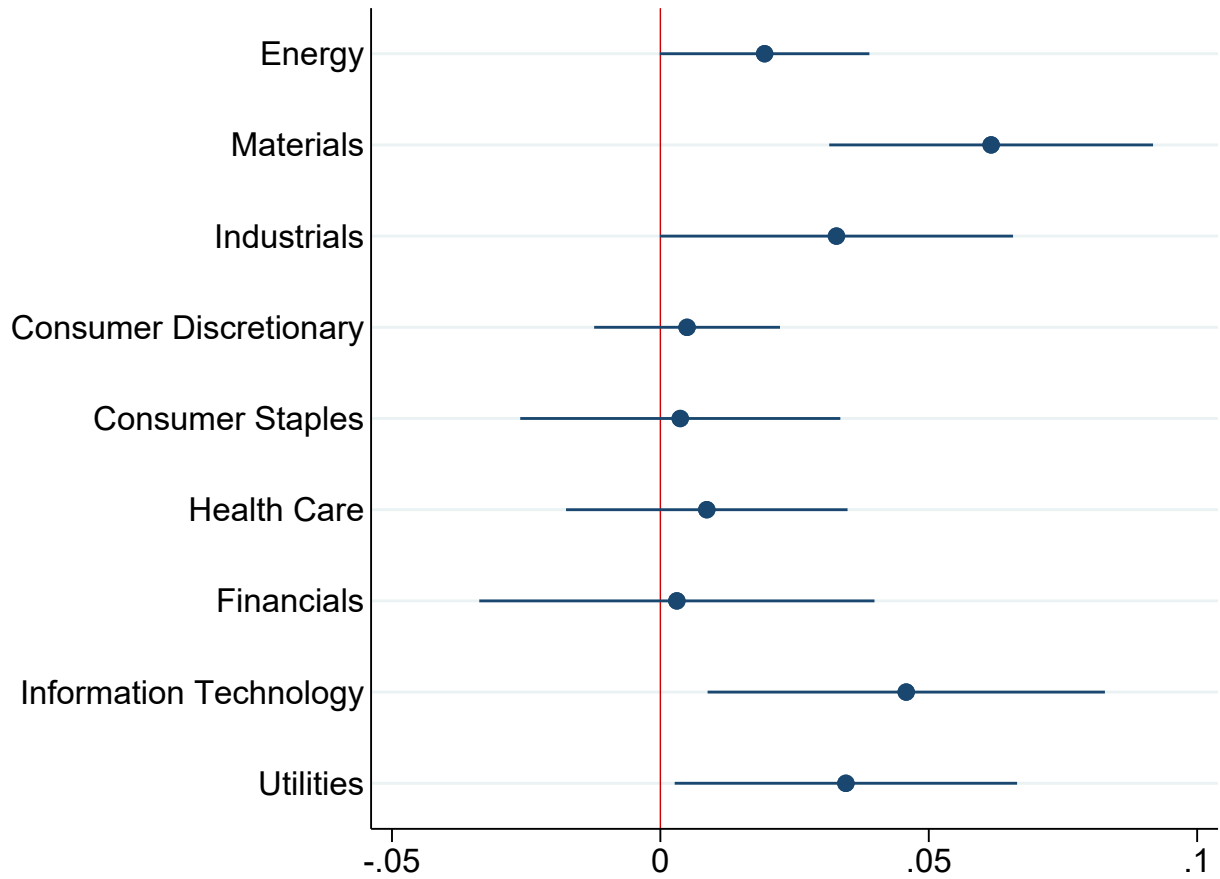
Figure 4: **Coefficient estimates by GICS sector.** The figure plots the coefficient estimate on ideological mismatch from the regression specification in Table 2, column (3), estimated separately for each GICS sector. We also plot the corresponding 95% confidence intervals, based on standard errors that are clustered at the firm level.
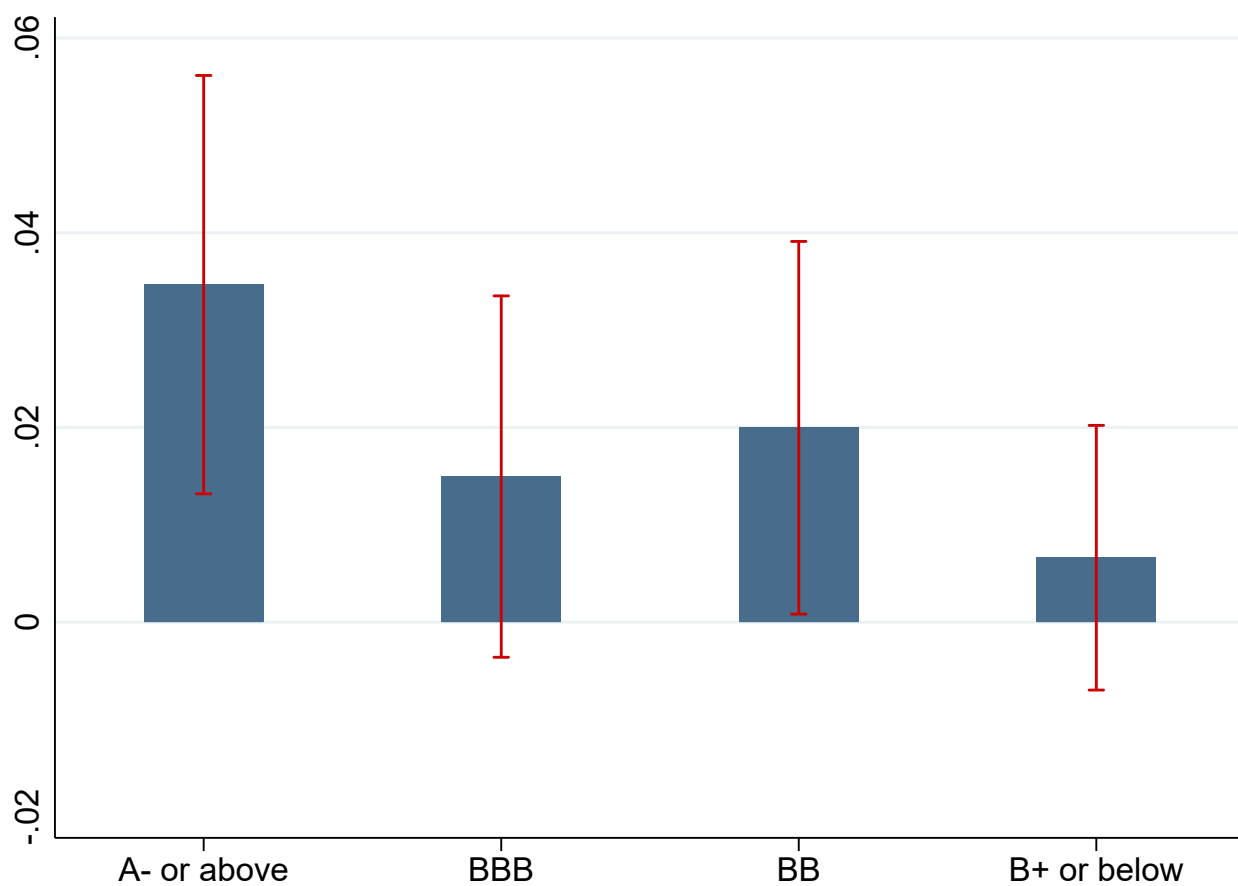
Figure 5: **Coefficient estimates by bond rating category.** The figure plots the coefficient estimate on ideological mismatch from the regression specification in Table 2, column (3), estimated separately for four different rating categories. We also plot the corresponding 95% confidence intervals, based on standard errors that are clustered at the firm level.

Table 1: **Summary Statistics**

This table presents summary statistics for our key variables. The sample consists of all rating changes for U.S. corporate bond issuers issued by Fitch, Moody's, and Standard and Poor's between 2000 and 2015, with available information on the analyst's political party affiliation. Party affiliation is obtained from voter registration records from Illinois, New Jersey, and New York City. One observation is at the analyst-firm-quarter level. All variables are defined in the Appendix.

	N	Mean	Std. Dev.	0.25	Median	0.75
Dependent Variables						
Rating change	48,493	0.019	0.597	0.000	0.000	0.000
Accuracy 1Q	19,454	0.000	0.022	0.000	0.000	0.000
Accuracy 2Q	18,749	-0.000	0.027	0.000	0.000	0.000
Accuracy 4Q	17,340	-0.001	0.038	0.000	0.000	0.000
Accuracy 8Q	14,379	-0.002	0.048	0.000	0.000	0.000
Accuracy 12Q	11,669	-0.001	0.050	0.000	0.000	0.000
Ideological Mismatch						
Ideological Mismatch	49,757	0.364	0.481	0.000	0.000	1.000
Control Variables						
Tenure	49,757	3.186	0.710	2.833	3.401	3.714
Tenure squared	49,757	10.654	3.925	8.027	11.568	13.791
Tenure firm	49,757	2.193	0.987	1.609	2.197	2.639
Prob. White	49,757	0.882	0.249	0.922	0.972	0.991
Prob. Hispanic	49,757	0.016	0.073	0.002	0.003	0.008
Prob. Black	49,757	0.029	0.069	0.001	0.008	0.031
Prob. Asian	49,757	0.071	0.238	0.001	0.002	0.006
Female	49,757	0.288	0.453	0.000	0.000	1.000
Age	49,757	48.821	10.135	42.000	49.000	56.000
Partisan Conflict	49,757	118.645	31.319	87.271	126.183	147.903
Presidential election quarter	49,757	0.066	0.248	0.000	0.000	0.000
Votes in midterm or primary	49,757	0.763	0.426	1.000	1.000	1.000
No. of past elections	49,757	4.836	3.343	2.000	4.000	7.000
Avg. election gap	46,982	5.011	2.511	3.333	4.250	5.800
Leverage	47,894	0.332	0.227	0.185	0.292	0.430
Size	47,935	8.630	1.544	7.550	8.576	9.636
Cash	47,926	0.081	0.093	0.016	0.048	0.112
Avg. past rating	48,795	11.392	3.736	8.500	11.000	14.500
Tobin's Q	39,069	1.487	0.672	1.053	1.280	1.697
Revenue growth	46,275	0.019	0.138	-0.047	0.013	0.081
Asset growth	46,828	0.008	0.044	-0.015	0.006	0.029
Cash flow	43,671	0.017	0.025	0.009	0.019	0.029
ROA	46,885	0.006	0.026	0.001	0.008	0.017
R&D	47,638	0.002	0.007	0.000	0.000	0.000
Capex	46,479	0.032	0.042	0.007	0.018	0.041

Table 2: **Partisan Bias and Rating Actions**

This table regresses quarterly rating changes on ideological mismatch, an indicator equal to one for analysts whose party affiliation does not match the President's party, and zero otherwise. All variables are defined in the Appendix. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the firm level.

| | **Rating Change** | | |
	(1)	(2)	(3)
Ideological Mismatch	0.0198	0.0201	0.0150
	(2.90)	(2.79)	(2.77)
Tenure	0.0081	0.0125	0.0149
	(0.27)	(0.40)	(0.66)
Tenure squared	-0.0003	-0.0015	-0.0026
	(-0.05)	(-0.28)	(-0.67)
No. of firms covered	0.0003	-0.0013	-0.0009
	(0.13)	(-0.59)	(-0.61)
Observations	30,739	30,738	29,474
R^2	0.857	0.859	0.912
Firm × Quarter FE	Yes	Yes	Yes
Agency FE	Yes	No	No
Agency × Industry FE	No	Yes	No
Agency × Industry × Quarter FE	No	No	Yes

Table 3: **Additional Controls**

This table repeats the analysis in Table 2, after adding additional analyst-level controls, as well as their interaction with an indicator for Democratic presidents (*DemPresident*). The coefficients on the non-interacted analyst controls are suppressed for brevity. All variables are defined in the Appendix. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the firm level.

	Rating Change		
	(1)	(2)	(3)
Ideological Mismatch	0.0221	0.0225	0.0159
	(3.16)	(2.99)	(2.79)
Tenure × DemPresident	0.0285	0.0274	0.0275
	(0.20)	(0.19)	(0.16)
Tenure squared × DemPresident	-0.0105	-0.0102	-0.0079
	(-0.39)	(-0.37)	(-0.25)
No. of firms covered × DemPresident	0.0032	0.0060	0.0002
	(0.48)	(0.83)	(0.03)
Prob. Hispanic × DemPresident	-0.1713	-0.1499	-0.1089
	(-1.83)	(-1.50)	(-2.21)
Prob. Black × DemPresident	0.0065	0.0109	-0.0126
	(0.12)	(0.21)	(-0.21)
Prob. Asian × DemPresident	0.0235	0.0391	0.0039
	(0.87)	(1.20)	(0.09)
Female × DemPresident	0.0015	-0.0002	-0.0065
	(0.11)	(-0.02)	(-0.42)
Age × DemPresident	0.0006	0.0004	0.0001
	(1.18)	(0.73)	(0.22)
Observations	30,739	30,738	29,474
R^2	0.857	0.859	0.912
Firm × Quarter FE	Yes	Yes	Yes
Agency FE	Yes	No	No
Agency × Industry FE	No	Yes	No
Agency × Industry × Quarter FE	No	No	Yes
Analyst Characteristics	Yes	Yes	Yes

Table 4: **Robustness**

This table presents robustness tests. The baseline regression refers to specification (3) from Table 2. For brevity, we only report coefficients of interest and suppress control variables. In Panel A, we test alternative definitions of the dependent variable. *Rating change indicator* is equal to zero if the quarterly rating change is zero; minus one if the rating change is negative (i.e., upgrade); plus one if the rating change is positive (i.e., downgrade). *Downgrade* (*Upgrade*) is an indicator equal to one if the quarterly rating change is positive (negative), and zero otherwise. In Panel, B we use an alternative definition of ideological mismatch, using party affiliation from presidential elections only. Panel C reports the results for alternative estimation methods. *Double-clustered standard errors* refers to double-clustering the standard errors at the firm and analyst level. *Firm-agency level, with firm-quarter and agency-firm FE* refers to a regression of quarterly rating changes on average ideological mismatch, after collapsing the data at the firm-agency-quarter level and averaging ideological mismatch across all analysts rating the same firm for the same rating agency in the same quarter. *Weighted least squares* refers to a weighted least squares regression, where weights are proportional to the total book assets of the rated firm. *Analyst × Firm FE* replaces the agency × Industry × quarter fixed effects by analyst × firm fixed effects. Panel D shows the results for different sample restrictions. *Exclude analysts younger than 22 or older than 65* removes analysts who are older than 65 or younger than 22 years old, and *Exclude year 2008 and 2009* excludes rating changes in 2008 and 2009, respectively. *t*-statistics are based on standard errors that allow for clustering at the firm level (except for the value in row *Double-cluster standard errors*).

	Coeff	*t*-statistic
Baseline	0.0150	2.77
Panel A: Alternative dependent variables		
Rating change indicator (-1; 0; +1)	0.0125	3.08
Downgrade	0.0047	1.83
Upgrade	-0.0078	-2.68
Panel B: Alternative definitions of ideological mismatch		
Use only party affiliation from presidential elections	0.0186	2.74
Panel C: Estimation		
Double-cluster standard errors	0.0150	2.60
Firm-agency level, with firm × quarter and agency × firm FE	0.0279	2.37
Weighted least squares	0.0142	2.52
Analyst × Firm FE	0.0272	2.30
Panel D: Sample restrictions		
Exclude analysts younger than 22 or older than 65	0.0166	2.95
Exclude years 2008 and 2009	0.0203	3.17

Table 5: **Interaction with Partisan Conflict and Election Quarters**

This table regresses quarterly rating changes on ideological mismatch as well as interactions with partisan conflict and election quarters. *Partisan conflict* is a lagged value of the Partisan Conflict Index, provided by the Federal Reserve Bank of Philadelphia, less 100. *Election quarter* refers to quarters with a presidential election. All variables are defined in the Appendix. *t*-statistics, reported in parentheses, are based on standard errors that allow for clustering at the firm level.

	Rating Change		
	(1)	(2)	(3)
Ideological Mismatch	0.0121	0.0141	0.0103
	(2.17)	(2.55)	(1.79)
Mismatch × Partisan Conflict	0.0003		0.0004
	(2.33)		(2.75)
Mismatch × Election Qtr		0.0167	0.0248
		(1.12)	(1.67)
Tenure	0.0161	0.0146	0.0159
	(0.71)	(0.64)	(0.70)
Tenure squared	-0.0028	-0.0025	-0.0028
	(-0.73)	(-0.66)	(-0.72)
No. of firms covered	-0.0011	-0.0009	-0.0011
	(-0.73)	(-0.60)	(-0.72)
Observations	29,474	29,474	29,474
R^2	0.912	0.912	0.912
Firm × Quarter FE	Yes	Yes	Yes
Agency × Industry × Quarter FE	Yes	Yes	Yes

Table 6: **Interaction with Voting Frequency**

This table regresses quarterly rating changes on ideological mismatch as well as interactions with measures of voting frequency. *Votes in midterm or primary* is an indicator equal to one if the analyst has ever voted in a midterm or primary election. *No. of past elections* is the total number of past elections in which the analyst has voted. *Avg. election gap* is the average time gap (in quarters) between elections in which the analyst has voted, measured across the full sample period. *Age* refers to the analyst's age at quarter-end, inferred from the birth date in the voter record, minus 20. *t*-statistics, reported in parentheses, are based on standard errors that allow for clustering at the firm level.

	Rating Change		
	(1)	(2)	(3)
Ideological Mismatch	-0.0126	0.0247	0.0424
	(-0.93)	(2.08)	(2.48)
Votes in midterm or primary	-0.0023		
	(-0.46)		
Mismatch × Votes in midterm or primary	0.0326		
	(2.85)		
No. of past elections		-0.0002	
		(-0.43)	
Mismatch × No. of past elections		0.0031	
		(2.13)	
Avg. election gap			0.0005
			(1.05)
Mismatch × Avg. election gap			-0.0036
			(-1.65)
Age	-0.0004	-0.0001	-0.0002
	(-1.74)	(-0.59)	(-0.92)
Age × Mismatch	-0.0001	-0.0008	-0.0005
	(-0.33)	(-1.78)	(-1.24)
Tenure	0.0119	0.0132	0.0338
	(0.46)	(0.59)	(1.42)
Tenure squared	-0.0015	-0.0022	-0.0058
	(-0.35)	(-0.57)	(-1.43)
No. of firms covered	-0.0028	-0.0008	-0.0004
	(-1.44)	(-0.48)	(-0.21)
Observations	24,328	29,474	26,752
R^2	0.919	0.912	0.908
Firm × Quarter FE	Yes	Yes	Yes
Agency × Industry × Quarter FE	Yes	Yes	Yes

Table 7: **Analyst Accuracy**

This table regresses measures of rating action accuracy on ideological mismatch. Accuracy is computed as the current quarter's rating change multiplied by future changes in credit spreads, measured over the following 1, 2, 4, 8 and 12 quarters, respectively. All variables are defined in the Appendix. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the firm level.

| | Rating Accuracy | | | | |
| | 1Q | 2Q | 4Q | 8Q | 12Q |
	(1)	(2)	(3)	(4)	(5)
Ideological Mismatch	-0.0006	-0.0010	-0.0017	-0.0022	-0.0010
	(-1.01)	(-1.46)	(-2.08)	(-2.26)	(-1.01)
Tenure	-0.0154	-0.0160	-0.0151	-0.0197	-0.0210
	(-1.03)	(-1.03)	(-0.91)	(-0.95)	(-0.76)
Tenure squared	0.0026	0.0027	0.0027	0.0037	0.0042
	(1.03)	(1.05)	(0.97)	(1.05)	(0.90)
Tenure firm	-0.0002	-0.0003	-0.0003	0.0000	-0.0011
	(-0.72)	(-0.84)	(-0.70)	(0.07)	(-1.13)
Leverage	0.0016	-0.0013	-0.0028	-0.0090	-0.0135
	(0.70)	(-0.44)	(-0.61)	(-1.32)	(-1.52)
Size	-0.0001	0.0002	0.0006	0.0013	0.0012
	(-0.22)	(0.51)	(1.12)	(1.90)	(1.10)
Cash	0.0059	0.0057	0.0037	0.0011	0.0151
	(1.04)	(0.89)	(0.53)	(0.11)	(0.94)
Avg. Past rating	-0.0005	-0.0004	-0.0002	-0.0001	-0.0003
	(-1.73)	(-1.38)	(-0.66)	(-0.22)	(-0.39)
Tobin's Q	-0.0008	-0.0013	-0.0014	-0.0015	-0.0024
	(-1.74)	(-2.17)	(-1.72)	(-1.15)	(-1.40)
Revenue growth	-0.0009	-0.0019	-0.0036	-0.0007	-0.0080
	(-0.25)	(-0.46)	(-0.69)	(-0.11)	(-1.00)
Asset growth	-0.0093	-0.0079	-0.0087	-0.0137	-0.0039
	(-1.03)	(-0.92)	(-0.76)	(-0.72)	(-0.19)
Cash flow	0.0361	0.1009	0.1127	0.1523	0.1317
	(1.37)	(2.36)	(2.09)	(1.67)	(1.00)
ROA	-0.0479	-0.0506	0.0077	0.0456	0.0087
	(-1.66)	(-1.33)	(0.17)	(0.53)	(0.08)
R&D	-0.0260	-0.0379	-0.0641	-0.0830	-0.1574
	(-0.66)	(-0.83)	(-1.28)	(-1.15)	(-1.80)
Capex	-0.0060	-0.0140	-0.0213	-0.0004	-0.0115
	(-1.02)	(-1.52)	(-1.40)	(-0.02)	(-0.56)
Observations	13,752	13,251	12,245	10,062	8,039
R^2	0.242	0.250	0.273	0.309	0.290
Agency FE	Yes	Yes	Yes	Yes	Yes
Industry × Quarter FE	Yes	Yes	Yes	Yes	Yes

Table 8: **Abnormal Stock Returns Around Rating Change Announcements**

This table regresses cumulative abnormal returns around downgrades (Panel A) and upgrades (Panel B) on ideological mismatch. Cumulative abnormal returns (CARs) are calculated using the Fama and French (1993) and Carhart (1997) model estimated over trading days (-150,-50) and are measured over a (-1,+1) event window. In columns (2), (4), and (6), we exclude all rating changes where a corporate earnings announcement or M&A announcement falls inside the (-10,+10) window around the rating change. Columns (3) to (6) include indicator variables for each rating category as additional controls. All variables are defined in the Appendix. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the event-month level.

Panel A: Downgrades

	Cumulative abnormal return (-1,+1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Ideological mismatch			0.109	0.295	0.144	-0.289
			(0.20)	(0.40)	(0.17)	(-0.30)
Δ Rating					-0.797	-1.026
					(-2.32)	(-2.38)
Δ Rating $\times$ Mismatch					0.005	0.437
					(0.01)	(0.77)
Constant	-2.161	-1.777				
	(-7.84)	(-5.69)				
Agency FE	No	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes	Yes
Excluding corporate events	No	Yes	No	Yes	No	Yes
N	1,992	1,215	1,992	1,215	1,992	1,215
R^2	0.000	0.000	0.059	0.060	0.067	0.070

Panel B: Upgrades

| | **Cumulative abnormal return (-1,+1)** | | | | | |
	(1)	(2)	(3)	(4)	(5)	(6)
Ideological mismatch			0.073	0.192	0.002	0.060
			(0.30)	(0.63)	(0.00)	(0.10)
Δ Rating					-0.225	-0.106
					(-1.83)	(-0.29)
Δ Rating $\times$ Mismatch					-0.035	-0.088
					(-0.17)	(-0.19)
Constant	0.120	0.028				
	(1.04)	(0.20)				
Agency FE	No	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes	Yes
Excluding corporate events	No	Yes	No	Yes	No	Yes
N	1,508	975	1,508	974	1,508	974
R^2	0.000	0.000	0.028	0.044	0.031	0.045

A. Appendix

A.1 Variable Definitions

Table A.1: **Variable descriptions**

Variable	Description
Dependent variables	
Rating change	The quarterly change (measured in notches) in the credit rating of a given firm by a given rating agency. 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). Credit ratings are obtained for Standard and Poor's from S&P RatingXpress, for Moody's from Moody's Default and Recovery Database, and for Fitch from Mergent.
Accuracy	Quarterly rating change multiplied by future changes in credit spreads. Credit spreads are computed following Fracassi, Petry, and Tate (2016). Changes in credit spreads are computed over the subsequent 1, 2, 4, 8, and 12 quarters.
Main independent variables	
Ideological mismatch	Indicator function equal to one if the analyst's party affiliation does not match the party of the President in a given quarter, and zero if the party either matches or if the analyst is unaffiliated. Information on party affiliation is obtained after merging analysts to voter records from Illinois, New Jersey, and New York City. Appendix A.2 provides additional details regarding the voter files and the merging procedure.
Control variables	
Tenure	Logarithm of one plus the number of quarters since the analyst's first rating action for a given rating agency.
Tenure squared	The square of tenure.
No. of firms covered	Logarithm of the number of firms analysts cover per each quarter.
Prob. White	The probability that the analyst's race/ethnicity is white, inferred based on the analyst's first and last names using the API `http://www.name-prism.com/`.
Prob. Hispanic	The probability that the analyst's race/ethnicity is Hispanic or Latino, inferred based on the analyst's first and last names using the API `http://www.name-prism.com/`.
Prob. Black	The probability that the analyst's race/ethnicity is black or African American, inferred based on the analyst's first and last names using the API `http://www.name-prism.com/`.
Prob. Asian	The probability that the analyst's race/ethnicity is Asian or Native Hawaiian or Other Pacific Islander, inferred based on the analyst's first and last names using the API `http://www.name-prism.com/`.

Variable	Description
Female	Indicator equal to one if the analyst is female, and zero otherwise. Gender is inferred based on the analyst's first name, using the API `http://api.genderize.io/`, as well as from manual online searches.
Age	Age of the analyst as of the end of the quarter. Analysts' birth dates are obtained from voter registration records.
Partisan conflict	The lagged value of the Partisan Conflict Index, obtained from the Federal Reserve Bank of Philadelphia and averaged across all months in a given quarter.
Election quarter	Indicator function equal to one if a Presidential Election is held during the quarter, and zero otherwise.
Leverage	The lagged ratio of the firm's total long-term debt to total assets from Compustat.
Size	The lagged logarithm of the firm's total assets from Compustat.
Cash	The lagged ratio of the firm's cash and short-term investments to total assets from Compustat.
Avg. Past Rating	The lagged average rating across all rating agencies rating the firm. 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).
Tobin's Q	The lagged ratio of the firm's quarterly market value to book value of total assets from Compustat.
Revenue growth	The lagged value of the firm's growth rate in total revenue from Compustat.
Asset growth	The lagged value of the firm's growth rate in total book assets from Compustat.
Cash flow	The lagged ratio of the firm's income before extraordinary items and depreciation to property, plant, and equipment from Compustat.
ROA	The lagged ratio of the firm's net income to the lagged value of total asset from Compustat.
R&D	The lagged ratio of the firm's research and development (R&D) expense to the lagged value of total asset from Compustat, set to zero if R&D expense is missing.
Capex	The lagged ratio of the firm's capital expenditures to the value of total asset from Compustat.
IVY League	An indicator equal to one if analyst went to one of the IVY League universities as undergraduate or graduate institution, and zero otherwise.
Missing IVY League	An indicator equal to one if IVY League is missing, and zero otherwise.
Tenure firm	Logarithm of one plus the number of quarters since the analysts first rating action for a given firm.

A.2 Voter Registration Files

This section describes the voter registration files and merging procedure used to assign party affiliations to individual analysts.

A.2.1 New York City

We obtained voter files and voter history files from the Board of Elections in the City of New York. The New York City voter records contain two types of datasets. One is the voting history, which contains the history of voting records for a given voter ID, including election type, election date, and party affiliation. The second dataset contains information regarding the full name, address, gender, date of birth, registration date, and voter status for each voter ID. The party affiliation can be Democrat, Republican, Other (e.g., Conservative, Liberal, Independent), or blank. We treat blank observations as *unaffiliated*. The dates of the covered elections range from 1983 to 2017. The election types covered include General Elections, Primary, Run-Off Election, Special Election, and Presidential Primary. We take the following steps to clean the NYC voter data:

- We merge the dataset that contains the individual voting histories with the static information on the voters' demographics, address, date of birth, etc., using the voter ID. The voter address refers to his/her most recent address.

- We remove duplicates by first name, middle name, last name, and date of birth in order to obtain a dataset where each observation is uniquely identified by full name and date of birth. We include this procedure in order to get the unique voter dataset by names and date of birth. There are 1,279 duplicates out of 3,780,569 observations. We drop all duplicate observations because the majority of the duplicates does not have the same voting history.

Following the two steps above, we obtain a cleaned NYC voter dataset with static voter information as well as information on the voter's voting history. Each voter is uniquely identified by first name, middle name, last name, and date of birth.

A.2.2 New Jersey

We obtained state-wide voter history files from the New Jersey Division of Elections. The New Jersey voter records contain individual voting histories, including election type, election date, and party affiliation for each voter ID. Observations are uniquely identified by voter ID, election type, and election date. The dataset also includes additional voter characteristics, from which we select first name, middle name, last name, gender, date of birth, address, registration date, and voter status. The data files are separated for each county, and we aggregate all counties into one

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large voter file. The party affiliation can be Democrat, Republican, Other (e.g., Conservative, Libertarian, Green), or unaffiliated. The dates of the covered elections range from 2007 to 2017. The election types covered include General Election, Primary, Municipal Election, and Special Election. We take the following steps to clean the NJ Voter data:

- We remove duplicates by first name, middle name, last name, and age in order to obtain a dataset where each observation is uniquely identified by full name and age. There are 2,945 duplicates out of 5,715,810 observations. We drop all duplicate observations because the majority of the duplicates does not have the same voting history.

Following the two steps above, we obtain a cleaned New Jersey voter dataset with voter characteristics as well as information on the voter's voting history. Each voter is uniquely identified by first name, middle name, last name, and date of birth.

A.2.3 Illinois

We obtained state-wide voter files and voter history files from the Illinois State Board of Elections. The Illinois voter records contain two datasets. One is the voting history, which contains the history of voting records for a given voter ID, including election type, election date, and party affiliation. The second dataset contains information regarding the full name, address, gender, age, registration date, and voter status for each voter ID. The party affiliation can be Democrat, Republican, Other (e.g., Libertarian, Independent, Green), non-partisan, or blank. We treat both blank and non-partisan observations as *unaffiliated*. The dates of the covered elections range from 1976 to 2017. The election types covered include General Primary, Consolidated Election, General Election, Nonpartisan Election, Consolidated Primary, Special Election and Special Primary. A notable feature of the Illinois records is that voters' party affiliation is blank in all general elections. Hence, we obtain party information only from primary elections. We take the following steps to clean the Illinois voter data:

- We merge the dataset that contains the individual voting histories with the static information on the voters' demographics, address, date of birth, etc., using the voter ID. The voter address refers to his/her most recent address.

- We remove duplicates by first name, middle name, last name, and date of birth in order to obtain a dataset where each observation is uniquely identified by full name and date of birth. We include this procedure in order to get the unique voter dataset by names and date of birth. There are 110,604 duplicates out of 7,080,218 observations. We drop all duplicate observations because the majority of the duplicates does not have the same voting history.

Following the two steps above, we obtain a cleaned Illinois voter dataset with static voter information as well as information on the voter's voting history. Each voter is uniquely identified by first name, middle name, last name, and date of birth.

Table A.2 summarizes the the ratios of voter's party affiliations by election types for all three voter files. *Total Count* refers to the number of voting records by election types.

A.2.4   Merging Analyst Data with Voter Registration Files

We merge the analyst-firm-quarter panel dataset with the cleaned voter records from New York City, New Jersey, and Illinois. Specifically, we first retain all analysts whose offices are either in New York or Chicago. Analyst locations are obtained from the press releases published on the websites of Moody's and Fitch, and from S&P Credit Portal. For analysts with missing office location, we assume that they are based in New York. We then match analysts located in New York with voter records from New York City and New Jersey, and analysts whose office is in Chicago with voter records from Illinois. We use the following method to match each analyst to an individual voter.

We first merge the analyst dataset and voter lists by first name, middle initial, and last name. In case of multiple matches, we apply the following criteria to determine the correct unique match. First, we retain the match with the smallest age difference between the analyst and the voter, conditional on the absolute age difference being three years or less. Information on analysts' age is obtained from online searches.[22] Second, if the age criterion does not allow us to determine a unique match, we use the distance between the zip code of the analyst's office location and the zip code of the voter address as a criterion. Specifically, we define a correct unique match if (i) the voter lives within a 50 miles radius from the rating agency and (ii) the second-nearest voter match is located more than 50 miles further away from the rating agency than the first voter. Third, for remaining analysts located in New York who match both to voter records from New York City and to New Jersey, we keep the match from New York City. Fourth, if the analyst matches to multiple voters who always have the same party affiliation, we keep the voter with the longest history. For those analysts who are not matched in the first merge by first name, middle initial, and last name, we do another round of merging by first and last name only (all other steps remain the same). We are able to match 449 out of 967 analysts to a voter record.

In the merged analyst-firm-quarter dataset, we define the analyst's party affiliation at the end of a given quarter as the most recent non-blank party affiliation in the matched voter record (using all elections). If the matched voter never had a non-blank party affiliation, we set the affiliation to *unaffiliated*.

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[22]We are able to find age information for ca. 62% of the analysts with duplicate matches to voter records.

Table A.2: **Summary Statistics – Voter Records**

This table summarizes party affiliation for all registered voters in the New York City, New Jersey and Illinois voter files, by election type. *Other* refers to all voters who are affiliated with parties other than Democratic and Republican. *Total Count* shows the total number of voters by election types. *Switch between Democratic and Republican* shows the ratio of voters who have switched at least once from Democratic to Republican, or vice versa.

New York City	Democrat	Republican	Other	Unaffiliated	Total Count
General Elections	0.725	0.116	0.029	0.129	21,800,991
Primary Elections	0.869	0.097	0.006	0.028	3,663,031
Other Elections	0.950	0.040	0.004	0.007	5,749,550
Total	0.784	0.100	0.022	0.095	31,213,572
Switch between Democratic and Republican				0.028	

New Jersey	Democrat	Republican	Other	Unaffiliated	Total Count
General Elections	0.443	0.299	0.002	0.255	21,048,052
Primary Elections	0.625	0.374	0.000	0.000	6,471,747
Other Elections	0.417	0.291	0.003	0.288	5,068,883
Total	0.475	0.313	0.002	0.210	32,588,682
Switch between Democratic and Republican				0.015	

Illinois	Democrat	Republican	Other	Unaffiliated	Total Count
General Elections	0.000	0.000	0.000	1.000	33,567,464
Primary Elections	0.487	0.371	0.002	0.141	18,368,420
Other Elections	0.000	0.000	0.000	1.000	11,936,881
Total	0.140	0.107	0.000	0.753	63,872,765
Switch between Democratic and Republican				0.120	

Online Appendix to

"Partisan Professionals:
Evidence from Credit Rating Analysts"

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Table IA.1: **Predicting Registered Voter Status with Firm Characteristics**

This table regresses an indicator for registered voters on firm characteristics. *Registered Voter* is an indicator equal to one for analysts who can be matched to a voter registration record, and zero otherwise. All independent variables are standardized to have a mean of zero and a standard deviation of one. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the firm level.

	Registered Voter	
	(1)	(2)
Leverage	0.0133	0.0183
	(0.61)	(0.92)
Book assets	0.0192	0.0013
	(0.76)	(0.06)
Cash ratio	-0.0053	0.0035
	(-0.36)	(0.23)
Avg. Past rating	0.0056	0.0088
	(0.21)	(0.36)
Tobin's Q	-0.0066	-0.0106
	(-0.49)	(-0.86)
Revenue growth	0.0003	0.0034
	(0.08)	(1.07)
Asset growth	-0.0016	0.0015
	(-0.33)	(0.33)
Cash flow	0.0698	0.0693
	(2.55)	(2.51)
ROA	-0.0717	-0.0733
	(-2.96)	(-3.03)
R&D	0.0209	0.0214
	(1.19)	(1.22)
Capex	0.0093	0.0006
	(0.55)	(0.04)
Observations	87,825	87,568
R^2	0.216	0.312
Industry × Quarter FE	Yes	No
Agency × Industry × Quarter FE	No	Yes

Table IA.2: **Predicting Registered Voter Status and Party Affiliation with Analyst Characteristics**

This table regresses indicators for registered voters (Panel A) and Democratic analysts (Panel B) on analyst characteristics. *Registered Voter* is an indicator equal to one for analysts who can be matched to a voter registration record, and zero otherwise. *Democratic* is an indicator equal to one for analysts who are are registered with the Democratic Party, and zero for analysts who are registered with the Republican Party. All variables are defined in the Appendix. *t*-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

Panel A: Registered Voters vs. Non-Registered Voters

	Registered Voter	
	(1)	(2)
Prob. Hispanic	-0.3290	-0.3167
	(-1.80)	(-1.77)
Prob. Black	0.0337	-0.1420
	(0.09)	(-0.46)
Prob. Asian	-0.0212	0.0109
	(-0.23)	(0.12)
Female	0.0155	0.0100
	(0.31)	(0.21)
Tenure	-0.0169	0.0339
	(-0.25)	(0.51)
Tenure squared	0.0163	0.0083
	(0.97)	(0.51)
No. of firms covered	-0.0307	-0.0424
	(-1.03)	(-1.51)
Observations	133,742	133,510
R^2	0.217	0.309
Industry × Quarter FE	Yes	No
Agency × Industry × Quarter FE	No	Yes

Panel B: Democratic vs. Republican Analysts

	Democratic	
	(1)	(2)
Prob. Hispanic	-0.8859	-0.9032
	(-2.89)	(-2.50)
Prob. Black	1.3966	1.5018
	(5.05)	(4.19)
Prob. Asian	0.1333	0.0998
	(1.50)	(1.23)
Female	0.3979	0.3220
	(6.94)	(5.41)
Age	-0.0019	-0.0021
	(-0.55)	(-0.63)
Tenure	0.1473	0.0395
	(1.19)	(0.33)
Tenure squared	-0.0282	-0.0065
	(-0.97)	(-0.23)
No. of firms covered	-0.0860	-0.1016
	(-2.03)	(-2.64)
Observations	37,409	36,178
R^2	0.382	0.544
Industry $\times$ Quarter FE	Yes	No
Agency $\times$ Industry $\times$ Quarter FE	No	Yes

Table IA.3: **Alternative Definition of Ideological Mismatch**

This table repeats the analysis from Table 2 while using alternative measures of ideological mismatch. Panel A estimates the regression on the subsample of Democratic and Republican analysts only. We regress quarterly rating changes on *Democrat*, an indicator equal to one for analysts who are affiliated with the Democratic Party and zero for Republican analysts, as well as an interaction with an indicator for Democratic presidents (*DemPresident*). Panel B repeats the analyst, but adds unaffiliated analysts, defined as all analysts who are either classified as unaffiliated in the voter records or do not match to a voter record. The coefficients on *Democrat* and *Republican* capture the difference relative to the base group of unaffiliated analysts. All variables are defined in the Appendix. t-statistics, reported in parentheses, are based on standard errors that allow for clustering at the firm level.

Panel A: Democratic vs. Republican Analysts

	Rating Change		
	(1)	(2)	(3)
Democrat	0.0389	0.0314	0.0155
	(2.78)	(2.22)	(1.55)
Democrat × DemPresident	-0.0361	-0.0344	-0.0194
	(-2.38)	(-2.12)	(-1.87)
Tenure	0.0310	0.0356	0.0265
	(0.70)	(0.78)	(0.86)
Tenure squared	-0.0044	-0.0057	-0.0046
	(-0.59)	(-0.74)	(-0.87)
No. of firms covered	-0.0010	-0.0015	-0.0007
	(-0.36)	(-0.55)	(-0.36)
Observations	18,993	18,992	17,629
R^2	0.859	0.862	0.914
Firm × Quarter FE	Yes	Yes	Yes
Agency FE	Yes	No	No
Agency × Industry FE	No	Yes	No
Agency × Industry × Quarter FE	No	No	Yes

Panel B: Democratic and Republican vs. Unaffiliated Analysts

	Rating Change		
	(1)	(2)	(3)
Democrat	0.0036	0.0012	-0.0002
	(0.89)	(0.30)	(-0.06)
Democrat × DemPresident	-0.0082	-0.0064	-0.0025
	(-1.59)	(-1.23)	(-0.56)
Republican	-0.0143	-0.0132	-0.0072
	(-2.86)	(-2.65)	(-1.84)
Republican × DemPresident	0.0122	0.0123	0.0094
	(1.99)	(2.05)	(1.98)
Tenure	0.0163	0.0156	0.0078
	(1.31)	(1.25)	(0.81)
Tenure squared	-0.0022	-0.0021	-0.0011
	(-0.99)	(-0.92)	(-0.64)
No. of firms covered	-0.0006	-0.0008	-0.0014
	(-0.53)	(-0.69)	(-1.61)
Observations	116,323	116,323	115,932
R^2	0.835	0.835	0.873
Firm × Quarter FE	Yes	Yes	Yes
Agency FE	Yes	No	No
Agency × Industry FE	No	Yes	No
Agency × Industry × Quarter FE	No	No	Yes

Table IA.4: **Interaction with Analyst Education and Experience**

This table regresses quarterly rating changes on ideological mismatch as well as interactions with measures of analysts' education and experience. *Ivy League* is an indicator equal to one if the analyst has obtained a degree at an Ivy League university, and zero otherwise. *Missing Ivy League* is an indicator equals to one if education information is missing, and zero otherwise. *Tenure* is the logarithm of one plus the number of quarters since the analysts first rating action for a given rating agency. *Tenure firm* is logarithm of one plus the number of quarters since the analysts first rating action for a given firm. All variables are defined in the Appendix. *t*-statistics, reported in parentheses, are based on standard errors that allow for clustering at the analyst level.

|	Registered Voter		
	(1)	(2)	(3)
Ideological Mismatch	0.0350	0.0148	0.0288
	(3.19)	(0.60)	(2.19)
Ivy League	0.0066		
	(1.09)		
Mismatch × Ivy League	-0.0235		
	(-2.08)		
Mismatch × Tenure		0.0001	
		(0.01)	
Tenure firm			0.0041
			(1.23)
Mismatch × Tenure firm			-0.0062
			(-1.33)
Missing Ivy League	0.0072		
	(0.69)		
Mismatch × Missing Ivy League	-0.0452		
	(-2.21)		
Tenure	0.0152	0.0149	0.0141
	(0.67)	(0.66)	(0.62)
Tenure squared	-0.0027	-0.0026	-0.0027
	(-0.69)	(-0.67)	(-0.69)
No. of firms covered	-0.0007	-0.0009	-0.0007
	(-0.42)	(-0.61)	(-0.45)
Observations	29,474	29,474	29,474
R^2	0.912	0.912	0.912
Firm × Quarter FE	Yes	Yes	Yes
Agency × Industry × Quarter FE	Yes	Yes	Yes
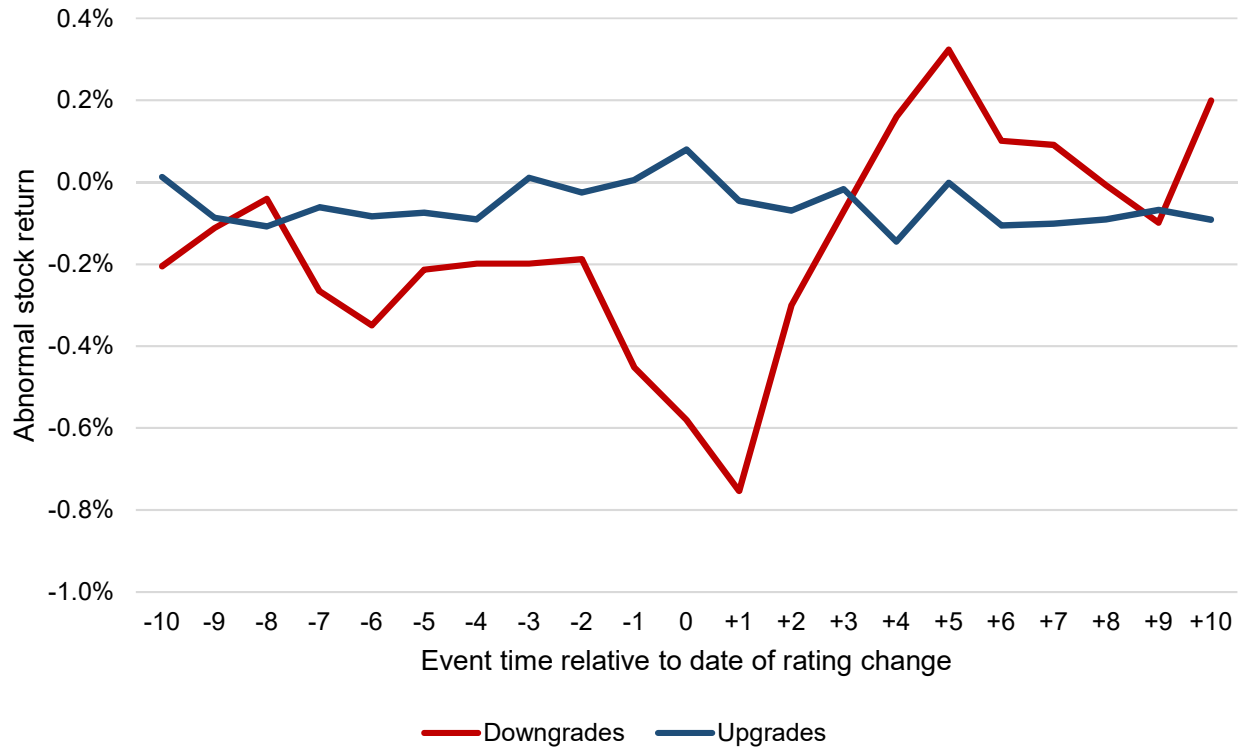
Figure IA.1: **Abnormal stock returns around rating change announcements.** The figure plots abnormal returns around downgrades and upgrades. Abnormal returns are calculated using the Fama and French (1993) and Carhart (1997) model estimated over trading days (-150,-50) relative to the event. We exclude all rating changes where a corporate earnings announcement or an MA announcement falls inside the (-10,+10) window around the rating change.