# 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 registration records, we show that analysts who are not affiliated with the U.S. President's party are more likely to downward-adjust corporate credit ratings. 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.

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*“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 economy through a “partisan perceptual screen,”[1] i.e., their assessment and interpretation of economic conditions and economic policies depends on whether the White House is occupied by the party they support (e.g., Gaines, Kuklinski, Quirk, Peyton, and Verkuilen (2007); Gerber and Huber (2009); Curtin (2016); 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

[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.

provides an interesting setting, as market settings should reduce biases and drive economic behavior towards the rational model (e.g., Gentzkow, Glaeser, and Goldin (2004); Hong and Kacperczyk (2010)). *Ex ante*, it is therefore unclear whether the existing findings on household behavior translate to our setting.

Studying the role of political bias on professional decision-making 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 Fitch, Moody's, Standard and Poor's 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 U.S. president.

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 provide more room for bias 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 9.38% 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 $\times$ quarter fixed effects in the regressions. 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 conclusion that the above finding reflects partisan bias, we investigate whether the effect is 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. The effect of partisan bias is 75% larger when partisan conflict increases by one standard deviation. We also find larger effects for analysts who are more politically active, proxied by the frequency with which an analyst votes.

As a next step, we explore the mechanism through which partisan bias affects analyst behavior. We interpret the evidence in this paper as showing that analysts with opposing political views differ in their beliefs about how the economic policies of the U.S. president affect the future state of the economy and, as a result, the credit risk of firms in the economy. To support this interpretation, we show that analysts' alignment with the president's party has no effect on their ratings of foreign firms or of domestic firms with low political risk. Hence, their disagreement appears to be focused precisely on the set of firms whose fundamentals should be most affected by the economic policies of the U.S. president: domestic firms that face high political risk. This result suggests that the heterogeneous behavior of analysts with different party affiliations is likely driven by their ideologically different views of the economy (Meeuwis, Parker, Schoar, and Simester (2018)). It casts doubt on alternative potential mechanisms, such as analysts' emotional state when their affiliated party is not in power, as this would not predict a differential effect for foreign versus domestic firms, or for firms with high versus low political risk.

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 are less accurate.

We further show that rating actions by partisan analysts have non-negligible price and potential real effects. Consistent with prior studies, we find cumulative abnormal stock returns of ca. -1.8% in the three days around a rating downgrade, after removing concurrent earnings and M&A announcements. For upgrades, we only find very small abnormal returns. More importantly, we find little evidence that the stock price response to a downgrade differs significantly when the downgrade is announced by an analyst who is ideologically misaligned with the president. In other words, the stock market does not seem to correct analysts' ideological bias. To gauge the potential real effects of analysts' partisan bias on firm investment, we perform a back-of-the-envelope calculation that combines our estimates with the estimates by Almeida, Cunha, Ferreira, and Restrepo (2017), who exploit exogenous variation in corporate ratings due to rating agencies sovereign ceiling policies. The results suggest that replacing an analyst who is aligned with the president with an analyst who is misaligned leads to a difference in firm investment of ca. 2.8%.

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. By affecting the direction of rating changes, partisan bias also influences analyst 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.

[4]The accuracy of analysts' ratings has been shown to be a strong predictor of future career outcomes (e.g., Kisgen, Nickerson, Osborn, and Reuter (2017); Kempf (2018)).

## 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 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 self-reported consumption of non-durable goods rose more among conservatives around the 2016 presidential election. However, other studies have not found a significant 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) combine data on vehicle purchases and credit card spending with an estimated propensity to vote for the Republican candidate in presidential elections at the county and state level. They find a significant relationship between party affiliation and economic expectations, but not between party affiliation and household spending.[5] In addition to consumption, 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 among

[5]There are several factors that could explain the mixed findings when linking partisan bias to household consumption, such as using survey-based, self-reported consumption data versus administrative data, focusing on different settings in terms of countries or time periods or employing different methods to infer political affiliation.

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 these studies by focusing on finance professionals.

Our study also adds 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) and Cornaggia, Cornaggia, and Israelsen (2018) document that credit ratings are affected by analyst-level conflicts of interest and home bias, respectively. Adding to this research, our study explores the role of partisan bias as a source of distortion 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 correlates 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 campaign 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 recommendations (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 and Sample Construction

## 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 (S&P). These are obtained for S&P from S&P RatingXpress, for Moody's from Moody's Default and Recovery Database (DRD), and for Fitch from Mergent.[6] The time period spans the years from 2000 to 2015. We restrict the sample period to post 2000 because press releases with analyst information are sparse prior to 2000. Credit ratings are transformed into a cardinal scale, starting with 1 for AAA (Aaa) and ending with 21 for D (C), 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).

[6]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.

### 3.1.2 Political Affiliation

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, and committees, found on the Federal Election Committee (FEC) website,[8] the voter registration data cover a larger part of the population. In fact, according to a study by Hill and Huber (2017), less than 10% of registered U.S. voters are federal or state donors. Whereas political contributions allow us to infer party affiliation only for people who support a party financially, the voter registration records 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 the 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 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

[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/`

CEO-supported political candidates receive three times more money from employees. Political affiliation inferred from voter registration records is less likely to be subject to such influence. Third, 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. In Figure IA.1 in the Online Appendix, we plot the quarterly partisan conflict measure over time. Second, we obtain quarterly firm-level financial information from Compustat. 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[9] (see Ye, Han, Hu, Coskun, Liu, Qin, and Skiena (2017) for details). 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.[10]

## 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.[11] 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.[12] 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 449 analysts, covering 1,778 firms.

In order to put the resulting match rate of ca. 46% (=449/967) in context, consider the following statistics. The share of registered voters among the total voting-age population of individuals aged between 25 and 64 years with a Bachelor's degree or higher is ca. 75.6%, as of November 2016.[13] We lose analysts who work in New York City but reside in Connecticut

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

[10]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/`.

[11]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.

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

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

or other parts of the State of New York. According to New York City commuter data, approximately 12% of analysts should fall in this category.[14] Moreover, we 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.[15] Of course, these statistics have to be treated with caution, as we do not know how the population of credit rating analysts compares to the average population. We nevertheless find them useful because they suggest that a match rate with voter registration records of 46% is not unreasonable. Even though our analysis does not require a random sample, we would still like to understand the potential differences between our sample and the overall population of analysts and firms. First, we investigate whether analysts whom we are able to match to voter records rate different types of companies. The results, reported in Table IA.1 in the Online Appendix, show that analysts for whom we are able to obtain party affiliation rate firms that are similar along many observable dimensions. Three dimensions in which they differ are return on assets, R&D, and cash flow. 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 sample to analysts who are registered voters, even if these differ from the general population of 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 Table IA.2 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 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

[14] NYC Department of City Planning. Data available at `https://www1.nyc.gov/assets/planning/download/pdf/data-maps/nyc-population/acs/ctpp_p6_nyc_boro_06_10.pdf`.

[15] For 61 analysts, we are unable to determine a unique match out of multiple potential voter matches.

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 1 report summary statistics. In 36% of the analyst-firm-quarters, the analyst's party affiliation does not match the president's party (*"ideological mismatch"*). Figure 1a 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 1b shows that the percentage of Democratic analysts is higher in New York City (47%) than in Chicago (39%). Figure 1c compares the political affiliation of analysts in the three different rating agencies. At S&P, 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 2).

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 (Aaa) and ending with 21 for D (C), as in Fracassi, Petry, and Tate (2016), a positive rating change indicates a downgrade. The average credit rating change is 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 also consistent with the literature (e.g., Becker and Ivashina (2014)). We study rating changes instead of levels for two reasons. First, changes allow us to better isolate the decisions of the current analysts from other confounding factors, such as the influence of the previous analyst. Second, they allow for the possibility that partisan bias gets reflected in credit ratings gradually over time, as new information about economic policies and firm fundamentals arrives.

Figure 3 presents the average adjusted rating change for Democratic, Republican and unaffiliated 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 upward-adjust ratings more 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 agencies and party affiliation was randomly assigned to analysts, 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, 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 (see Figure 1a for the distribution of analyst party affiliation across sectors). 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, party affiliation is not randomly assigned to analysts, and may be correlated with other time-invariant characteristics of the analyst, such as upbringing, education, prior work experience, or attitudes towards certain industries or firms. Third, credit analysts may not be randomly assigned to rating agencies. As Figure 1c shows, the mix of Democratic versus Republican analysts varies substantially across agencies. If political cycles correlate with asset returns (see Pástor 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 Moody's rating methodology leads to more conservative ratings during a recession. Since Moody's 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.

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:

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

where $a$ denotes the rating agency and $j$ denotes the firm's industry identified by the first two digits of the SIC code. $Ideological\, Mismatch_{it}$ 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 $Ideological\,Mismatch_{it}$ 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.

In our baseline definition, *ideological mismatch* is equal to one for analysts whose party affiliation does not match the president's; it is zero for analysts whose party affiliation matches that of the president as well as for unaffiliated analysts. Since we include indicators for the analyst's party affiliation in all regressions, the coefficient on *ideological mismatch* will be effectively identified only based on analysts whose party switches at least once between aligned and misaligned with the president. The main benefit of including unaffiliated analysts in our baseline definition is that it allows us to estimate the fixed effects and coefficients on control variables more precisely. In our robustness tests in Table IA.3 in the Online Appendix, we show that we obtain very similar results if we define ideological mismatch for Democratic and Republican analysts only.

By including firm × 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 × industry × quarter fixed effects addresses concerns that different methodologies of credit agencies might be correlated with the political cycle during our sample period. Finally, our estimator of partisan bias is, by construction, orthogonal to the baseline effect of the analyst's political affiliation itself and, hence, to a number of unobserved analyst characteristics that may be correlated with party affiliation. Furthermore, we present robustness tests that include analyst × firm fixed effects (see Table 4), which addresses potential concerns that ideological mismatch could be correlated with time-invariant attitudes towards certain industries or firms. We cluster standard errors at the firm level throughout the analysis. In Table 4, we show that our results are robust to double clustering by firm and analyst as well as by firm and quarter.

## 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 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.[16] 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, Nickerson, Osborn, and Reuter (2017) and Kempf (2018) document that the rating decisions of

[16] 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.

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 analysts' rating decisions.

# 5. Partisan Bias and 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. We find no influence of partisan bias when analysts rate foreign firms as well as domestic firms with low political risk. 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. We include indicators for analysts' party affiliation in all regressions.

We begin by including agency fixed effects in addition to firm $\times$ quarter fixed effects, since Figure 1 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 0.0198 notches relative to analysts who are aligned with the president's party. The economic and statistical significance of the effect of partisan bias remains high as we tighten the identification to include agency $\times$ industry (column (2)) as well as agency $\times$ industry $\times$ quarter fixed effects (column (3)). The estimate in 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%.

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 $\times$ 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 $\times$ industry $\times$ 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 versus Republican administrations. It is not obvious what characteristics that might be. To still directly 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 Table IA.3 in the Online Appendix, we report the main specifications when we define ideological mismatch based on 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); Pástor and Veronesi (2018)). 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. In fact, 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 (Saad (2014)). Moreover, only 32% of registered voters prefer a one-party control of Congress (Jones (2014)). We leave the examination of the role of political alignment with 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 as well as by firm and quarter 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 younger 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 after sequentially excluding each GICS sector that represents at least 5% of our total observations. The coefficient estimate is remarkably consistent across all specifications, suggesting that our main result is not driven by a single sector. Overall, we conclude that our results are robust to a large set of different specifications and estimation approaches.

### 5.3 Heterogeneity Across Time and Analysts

The results presented in Table 2 suggest a causal relationship between partisan bias and analysts' rating actions. To further support this interpretation, we test for heterogeneous effects across time and analysts. Specifically, we investigate whether the effect 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. 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. In Table 5, column (1), we interact ideological mismatch with partisan conflict measured during the previous quarter. The estimates imply that the effect of partisan bias increases by 0.0091 (=0.0003 $\times$ 30.32) notches for a one-standard-deviation increase in partisan conflict, thereby increasing the baseline effect by 75% (=0.0091/0.0121). 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 Mechanism

As a next step, we investigate the mechanism through which partisan bias affects analyst behavior. If the difference in rating behavior documented above reflects differences in analysts' beliefs about the implications of the U.S. president's economic policies for the economy, then we should see smaller or no differences when analysts rate firms that are unlikely affected by the president's policies. We use two proxies to identify such firms: foreign firms, as well as domestic firms with low political risk. To identify domestic firms with low political risk, we use the firm-level measure provided by Hassan, Stephan, Lent, and Tahoun (2017), which is based on the share of the quarterly earnings conference calls that firms devote to political risks. We split firms into high and low political risk at the median. Table 7 reports the results. For foreign firms and domestic firms with low political risk, there is no difference in the rating behavior between analysts who are politically aligned versus misaligned with the White House. The effect of partisan bias appears to be concentrated in the group of domestic firms with high political risk.

These results suggest that the heterogeneous behavior of Democratic and Republican analysts is likely driven by their different assessment of economic conditions and efficacy of economic policies. They cast doubt on alternative potential mechanisms, such as analysts' overall emotional state when their affiliated party is not in power, as this would not predict a differential effect for foreign versus domestic firms, or for firms with high versus low political risk.

## 5.5 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 affiliated 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 downward-adjust by too little, or whether those who are misaligned downward-adjust by 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, Nickerson, Osborn, and Reuter (2017). 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 $\times$ quarter, we replace the firm $\times$ quarter fixed effects by industry $\times$ 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 8 reports the results. We vary the horizon over which the change in credit spreads is measured from 1, 2, 4 and 8 quarters. Across all horizons, ideological mismatch tends to be negatively associated with rating action accuracy. The economic magnitude and statistical significance of this negative relationship increases as the future change in credit spreads is measured over longer horizons. 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 8 suggest that analysts who do not support the president downgrade firms by more than what is 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 $\times$ quarter fixed effects, the results are more subject to concerns related to non-random matching of analysts to firms.

# 6. Price and Real Effects of Partisan Bias

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

## 6.1 Stock Price Effects

A number of studies have documented the reaction of common stock prices to credit rating changes. 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 potentially contaminating concurrent news releases. There is little evidence that upgrade announcements trigger significant 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 Figure IA.2 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.[17] 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 9, 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 analysts' partisan bias. In other words, is the stock market reaction to downgrades muted when they are issued by misaligned analysts?

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

Columns (3) to (6) in Table 9 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). Even conditional on the same magnitude of the rating change, 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 limited to purposes of political campaigns and education, the party affiliation of the analyst is not public information.[18] 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.[19]

### 6.2 Potential Real Effects: Back-of-the-envelope Calculation

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

[18]However, investors may still choose to purchase access to commercial voter files.

[19]See Sangiorgi and Spatt (2017) for an excellent review of the literature on the regulatory role of credit ratings.

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 an 8.9 percentage-point reduction in firm investment for treated firm, which corresponds to 24% of the pre-event investment level. Combining these estimates with our own estimates from Table 2 suggests that replacing an analyst who is ideologically aligned with the president with an analyst who is ideologically misaligned leads to a difference in bond yields of 5.23 ($= 61 \times 0.06/0.7$) basis points and a 0.76 ($= 8.9 \times 0.06/0.7$) percentage-point difference in firm investment, which represents 2.83% relative to the average investment level.

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 from both developed and emerging markets, 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 are not aligned with 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 rating actions by partisan analysts have price effects, and can therefore distort firms' financing and investment decisions.

Given the documented increase in political polarization, it is important to understand the potential implications of this trend for the U.S. economy. One potential channel how partisan bias can have real effects is through the actions of relevant economic agents. To the best of our knowledge, this is the first study to identify a significant effect of partisan bias among finance professionals.

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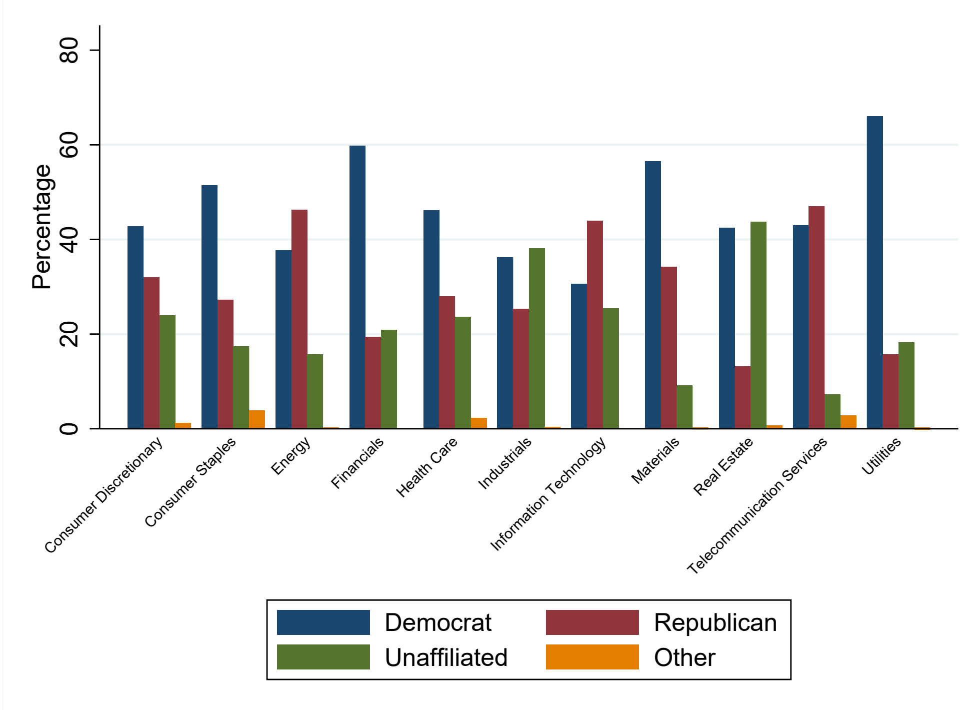
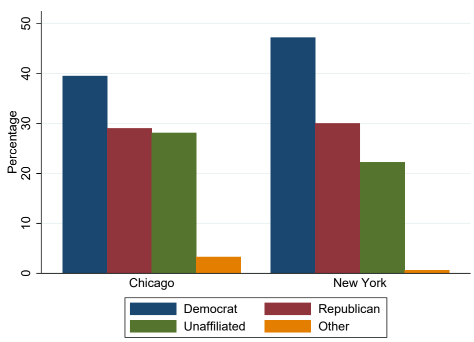
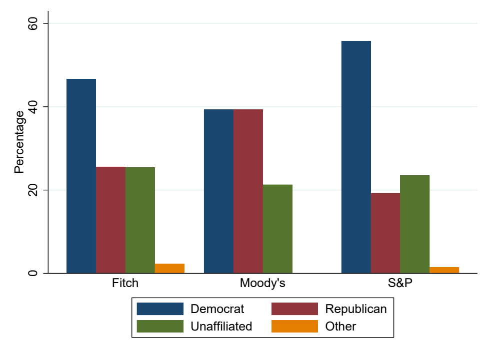



(a) Party Affiliation by GICS Sector




(b) Party Affiliation by City




(c) Party Affiliation by Rating Agency

Figure 1: **Party Affiliation Summary**

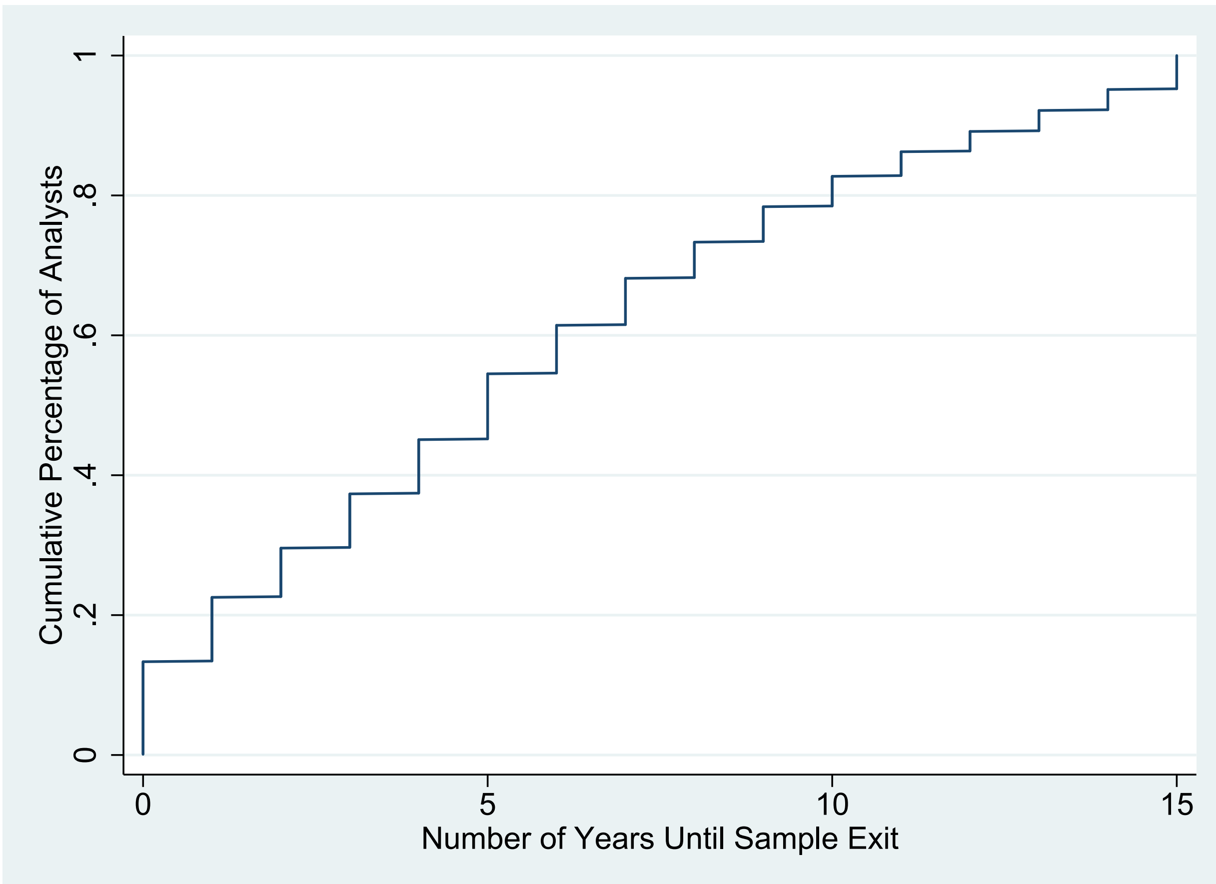



Figure 2: **Exit Probability.** The figure plots the cumulative percentage of analysts leaving the sample for a given number of years that they stay in the sample.

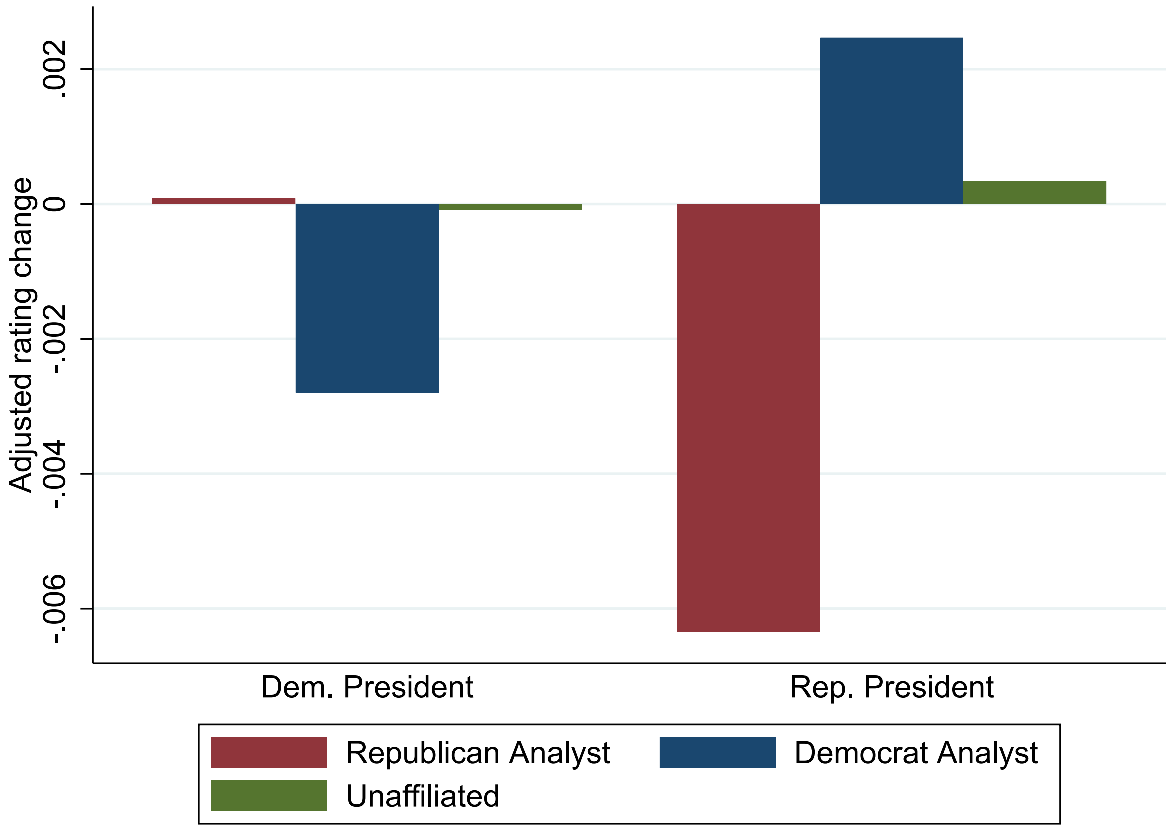



Figure 3: **Average Adjusted Rating Change by Analyst Party Affiliation.** The figure plots the average adjusted rating change separately for Democratic, Republican, and unaffiliated analysts under Democratic and Republican presidents. Adjusted rating changes are computed by taking the quarterly rating change and subtracting the average rating change within the same firm and quarter.

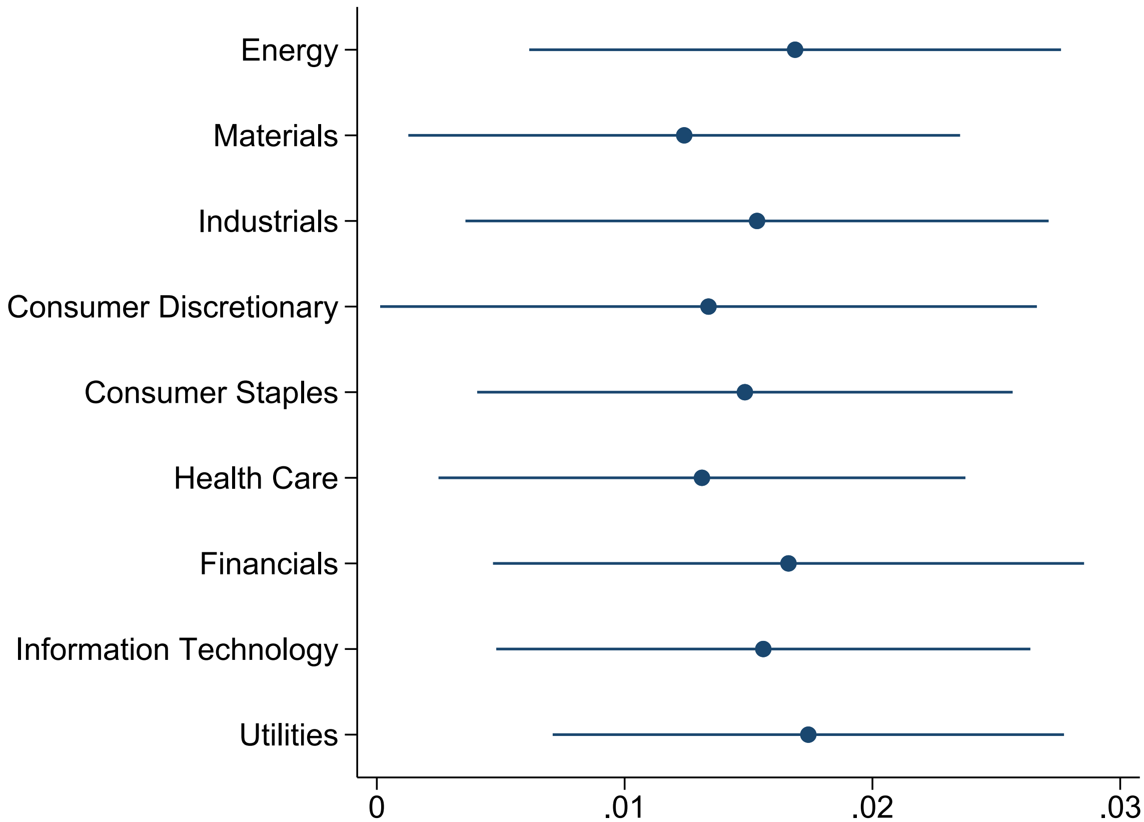



Figure 4: **Coefficient estimates after excluding each GICS sector.** The figure plots the coefficient estimate on ideological mismatch from the regression specification in Table 2, column (3), after excluding one GICS sector at a time. We also plot the corresponding 95% confidence intervals, based on standard errors that are clustered at the firm level.

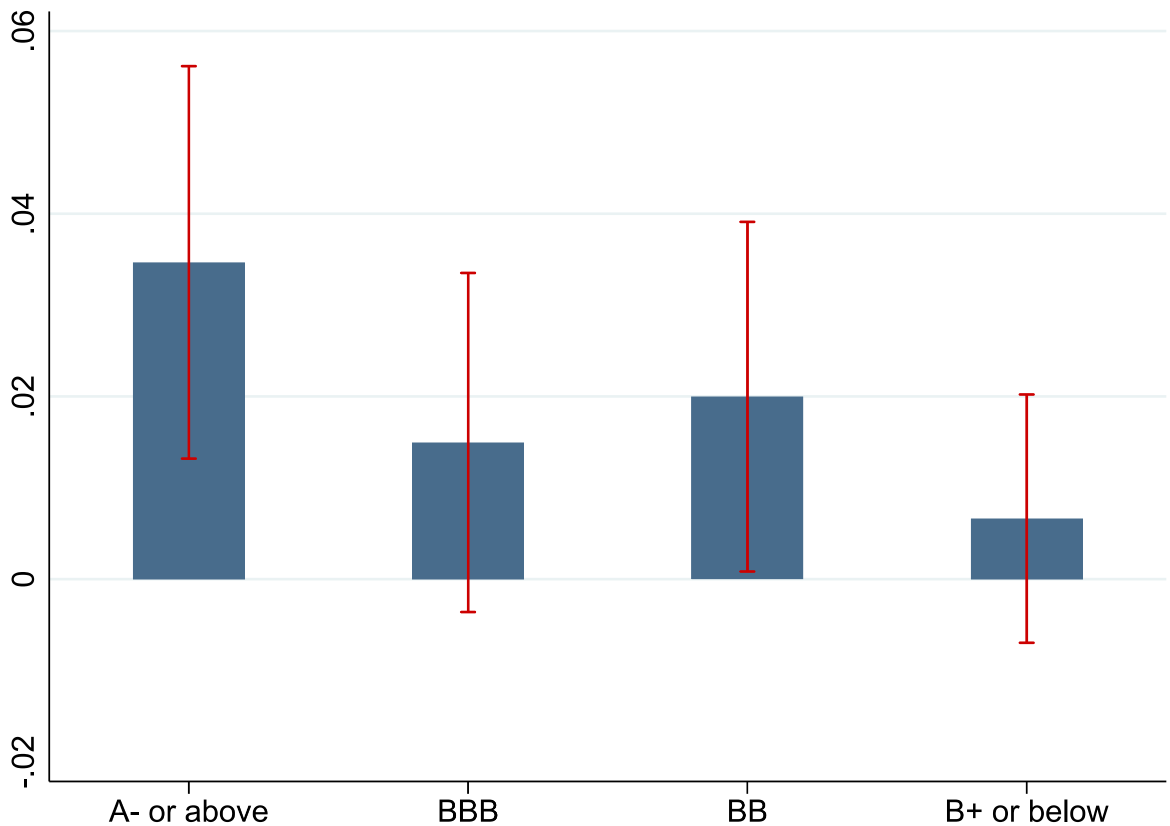



Figure 5: **Coefficient estimates by bond rating category.** The figure plots the coefficient estimate on ideological mismatch from the regression specification in Table 2, column (3), estimated separately for four different rating categories. We also plot the corresponding 95% confidence intervals, 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 by Fitch, Moody's, and S&P between 2000 and 2015, with available information on the analyst's political party affiliation. One observation is at the analyst-firm-quarter level. All variables are defined in Appendix A.1.

| | N | Mean | St.Dev. | 0.25 | Median | 0.75 |
|---|---|---|---|---|---|---|
| *Key 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 |
| *Key Independent Variables* | | | | | | |
| 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 |
| No. of firms covered | 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 |
| 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 Changes**

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. Coefficients on indicators for analysts' party affiliation are omitted for brevity. All variables are defined in Appendix A.1. $t$-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 Appendix A.1. $t$-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. In Panel C, *Double-clustered standard errors (analyst/quarter)* refers to double-clustering standard errors at the firm and analyst (quarter) level, respectively. *Firm-agency level* 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. $t$-statistics are based on standard errors that allow for clustering at the firm level (except for the value in rows *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 (analyst) | 0.0150 | 2.60 |
| Double-cluster standard errors (quarter) | 0.0150 | 2.72 |
| Firm-agency level | 0.046 | 2.27 |
| 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 presidential election quarters. *Partisan conflict* is the 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 Appendix A.1. $t$-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 quarter | | 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 ever votes 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 votes. *Age* refers to the analyst's age at quarter-end, inferred from the birth date in the voter record, less 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) |
| Mismatch × Age | -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: **Foreign Firms and Domestic Firms with Low Political Risk**

This table regresses quarterly rating changes on ideological mismatch using alternative samples. In column (1), we estimate our main regression on the subsample of non-U.S. firms. In columns (2) and (3), we split the sample of domestic firms into firms with low and high political risk, where political risk is defined as in Hassan, Stephan, Lent, and Tahoun (2017) and the sample is split at the median. $t$-statistics, reported in parentheses, are based on standard errors that allow for clustering at the firm level.

| | **Rating Change** | | |
|---|---|---|---|
| | Foreign Firms | Domestic Firms | |
| | | Low Pol. Risk | High Pol. Risk |
| | (1) | (2) | (3) |
| Ideological Mismatch | -0.0023 | -0.0002 | 0.0214 |
| | (-0.76) | (-0.04) | (2.52) |
| Tenure | 0.0020 | -0.0276 | 0.0112 |
| | (0.40) | (-0.98) | (0.33) |
| Tenure squared | 0.0002 | 0.0047 | -0.0015 |
| | (0.21) | (0.99) | (-0.25) |
| No. of firms covered | -0.0013 | -0.0045 | -0.0030 |
| | (-1.45) | (-2.00) | (-1.17) |
| Observations | 33,160 | 10,440 | 10,940 |
| $R^2$ | 0.956 | 0.944 | 0.922 |
| Firm × Quarter FE | Yes | Yes | Yes |
| Agency × Industry × Quarter FE | Yes | Yes | Yes |

Table 8: **Analyst Accuracy**

This table regresses 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 and 8 quarters, respectively. $t$-statistics, reported in parentheses, are based on standard errors that allow for clustering at the firm level.

| | **Accuracy** | | | |
|---|---|---|---|---|
| | 1Q | 2Q | 4Q | 8Q |
| | (1) | (2) | (3) | (4) |
| Ideological Mismatch | -0.0006 | -0.0010 | -0.0017 | -0.0022 |
| | (-1.01) | (-1.46) | (-2.08) | (-2.26) |
| Tenure | -0.0154 | -0.0160 | -0.0151 | -0.0197 |
| | (-1.03) | (-1.03) | (-0.91) | (-0.95) |
| Tenure squared | 0.0026 | 0.0027 | 0.0027 | 0.0037 |
| | (1.03) | (1.05) | (0.97) | (1.05) |
| No. of firms covered | -0.0002 | -0.0003 | -0.0003 | 0.0000 |
| | (-0.72) | (-0.84) | (-0.70) | (0.07) |
| Leverage | 0.0016 | -0.0013 | -0.0028 | -0.0090 |
| | (0.70) | (-0.44) | (-0.61) | (-1.32) |
| Size | -0.0001 | 0.0002 | 0.0006 | 0.0013 |
| | (-0.22) | (0.51) | (1.12) | (1.90) |
| Cash | 0.0059 | 0.0057 | 0.0037 | 0.0011 |
| | (1.04) | (0.89) | (0.53) | (0.11) |
| Avg. past rating | -0.0005 | -0.0004 | -0.0002 | -0.0001 |
| | (-1.73) | (-1.38) | (-0.66) | (-0.22) |
| Tobin's Q | -0.0008 | -0.0013 | -0.0014 | -0.0015 |
| | (-1.74) | (-2.17) | (-1.72) | (-1.15) |
| Revenue growth | -0.0009 | -0.0019 | -0.0036 | -0.0007 |
| | (-0.25) | (-0.46) | (-0.69) | (-0.11) |
| Asset growth | -0.0093 | -0.0079 | -0.0087 | -0.0137 |
| | (-1.03) | (-0.92) | (-0.76) | (-0.72) |
| Cash flow | 0.0361 | 0.1009 | 0.1127 | 0.1523 |
| | (1.37) | (2.36) | (2.09) | (1.67) |
| ROA | -0.0479 | -0.0506 | 0.0077 | 0.0456 |
| | (-1.66) | (-1.33) | (0.17) | (0.53) |
| R&D | -0.0260 | -0.0379 | -0.0641 | -0.0830 |
| | (-0.66) | (-0.83) | (-1.28) | (-1.15) |
| Capex | -0.0060 | -0.0140 | -0.0213 | -0.0004 |
| | (-1.02) | (-1.52) | (-1.40) | (-0.02) |
| Observations | 13,752 | 13,251 | 12,245 | 10,062 |
| $R^2$ | 0.242 | 0.250 | 0.273 | 0.309 |
| Agency FE | Yes | Yes | Yes | Yes |
| Industry × Quarter FE | Yes | Yes | Yes | Yes |

Table 9: **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) also include indicator variables for each rating category as additional controls (unreported for brevity). All variables are defined in Appendix A.1. $t$-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) |
| $\Delta$ Rating | | | | | -0.797 | -1.026 |
| | | | | | (-2.32) | (-2.38) |
| Mismatch $\times$ $\Delta$ Rating | | | | | 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 |
| Observations | 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) |
| $\Delta$ Rating | | | | | -0.225 | -0.106 |
| | | | | | (-1.83) | (-0.29) |
| Mismatch $\times$ $\Delta$ Rating | | | | | -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 |
| Observations | 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, as in Fracassi, Petry, and Tate (2016), starting with 1 for AAA and ending with 21 for D or lower, in the case of Fitch and S&P. For Moody's, the scale starts with 1 for Aaa and ends with 21 for C. Credit ratings are obtained for S&P 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 and 8 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 rated by the analyst in a given 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/`. |

*Continued on next page*

**Table A.1 – continued**

| **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. |
| Votes in midterm or primary | Indicator function equal to one if an analyst ever votes in a midterm or primary election, and zero otherwise. |
| No. of past elections | Total number of past elections in which an analyst has voted. |
| Avg. election gap | The average time gap (in quarters) between elections in which the analyst votes, measured across the full sample period. |
| 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, as in Fracassi, Petry, and Tate (2016), starting with 1 for AAA and ending with 21 for D or lower, in the case of Fitch and S&P. For Moody's, the scale starts with 1 for Aaa and ends with 21 for C. |
| 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. |

## A.2 Voter Registration Files

This section describes the voter registration files and merging procedure used to assign party affiliations to individual analysts. Table A.2 summarizes the the ratios of voter's party affiliations by election type for all three voter files.

### A.2.1 New York City

We obtain registered 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 Elections, Run-Off Elections, and Special Elections. 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. 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 each 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 obtain state-wide registered voter files and voter history files from the New Jersey Division of Elections. The information in the New Jersey voter records is very similar to the data from New York City. The main difference is the time period spanned by the dates of the covered elections, which ranges from 2007 to 2017. The party affiliation can be Democrat, Republican, other (e.g., Conservative, Libertarian, Green), or unaffiliated. The election types covered include General Elections, Primary Elections, Municipal Elections, and Special Elections. As with the New York City 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.

### A.2.3 Illinois

We obtain state-wide registered voter files and voter history files from the Illinois State Board of Elections. There are three main difference between the Illinois voter records and the records from New Jersey and New York City. First, we do not have date of birth information; instead, we have information on voter age, which is measured at the time where we requested the data (February 2018). Second, in terms of the time period, the dates of the covered elections range from 1976 to 2017. Third, the variable party affiliation is blank in all general elections. Hence, we can infer party information only based on primary elections. As a result, the rate of voters who switch between the Democratic and Republican party is higher for Illinois (see Table A.2). The party affiliation can be Democrat, Republican, or other (e.g., Libertarian, Independent, Green).

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 110,604 duplicates out of 7,080,218 observations.

### 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, after retaining all analysts whose offices are in New York or Chicago. Information on analysts' office locations is obtained from press releases published on the websites of Moody's and Fitch, and from S&P's 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.[20] Second, if the age criterion

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

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 from 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 step, we perform another merge by first and last name only. All other steps described above remain the same. We are able to match 449 out of 967 analysts to a unique voter record. We lose 61 analysts who match to multiple voters and for whom a unique match cannot be determined.

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.

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"**

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

This table regresses an indicator for analysts who are registered voters on characteristics of the rated firm. *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 | 0.0064 | 0.0088 |
| | (0.74) | (1.04) |
| Size | 0.0093 | 0.0006 |
| | (1.08) | (0.08) |
| Cash | -0.0026 | 0.0017 |
| | (-0.47) | (0.32) |
| Avg. past rating | 0.0027 | 0.0042 |
| | (0.32) | (0.52) |
| Tobin's Q | -0.0032 | -0.0051 |
| | (-0.55) | (-0.88) |
| Revenue growth | 0.0001 | 0.0016 |
| | (0.09) | (1.09) |
| Asset growth | -0.0008 | 0.0007 |
| | (-0.37) | (0.35) |
| Cash flow | 0.0337 | 0.0335 |
| | (2.81) | (2.74) |
| ROA | -0.0346 | -0.0354 |
| | (-3.18) | (-3.23) |
| R&D | 0.0101 | 0.0104 |
| | (1.82) | (1.83) |
| Capex | 0.0045 | 0.0003 |
| | (0.79) | (0.05) |
| 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 Appendix A.1. $t$-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 | -0.4035 | -0.3956 |
| | (-2.15) | (-2.15) |
| Prob. Black | 0.0292 | -0.0520 |
| | (0.08) | (-0.15) |
| Prob. Asian | -0.0249 | 0.0134 |
| | (-0.28) | (0.15) |
| Female | 0.0198 | 0.0190 |
| | (0.40) | (0.40) |
| Tenure | -0.0012 | 0.0367 |
| | (-0.02) | (0.52) |
| Tenure squared | 0.0148 | 0.0098 |
| | (0.84) | (0.57) |
| No. of firms covered | -0.0326 | -0.0452 |
| | (-1.12) | (-1.59) |
| Observations | 120,810 | 120,416 |
| $R^2$ | 0.465 | 0.525 |
| Firm × Quarter FE | Yes | Yes |
| Agency × Industry × Quarter FE | No | Yes |

*Continued on next page*

Panel B: Democratic vs. Republican Analysts

| | Democratic | |
|---|---|---|
| | (1) | (2) |
| Prob. Hispanic | -0.8689 | -0.7593 |
| | (-3.10) | (-2.02) |
| Prob. Black | 1.3539 | 1.3302 |
| | (5.83) | (4.32) |
| Prob. Asian | 0.1641 | 0.0715 |
| | (1.78) | (0.70) |
| Female | 0.4301 | 0.3705 |
| | (6.43) | (5.20) |
| Age | -0.0009 | -0.0015 |
| | (-0.29) | (-0.44) |
| Tenure | 0.1820 | -0.0002 |
| | (0.97) | (-0.00) |
| Tenure squared | -0.0366 | -0.0014 |
| | (-0.92) | (-0.04) |
| No. of firms covered | -0.0780 | -0.0946 |
| | (-1.64) | (-2.03) |
| Observations | 19,424 | 18,038 |
| $R^2$ | 0.590 | 0.697 |
| Firm × Quarter FE | Yes | Yes |
| Agency × Industry × 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 Appendix A.1. 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 |

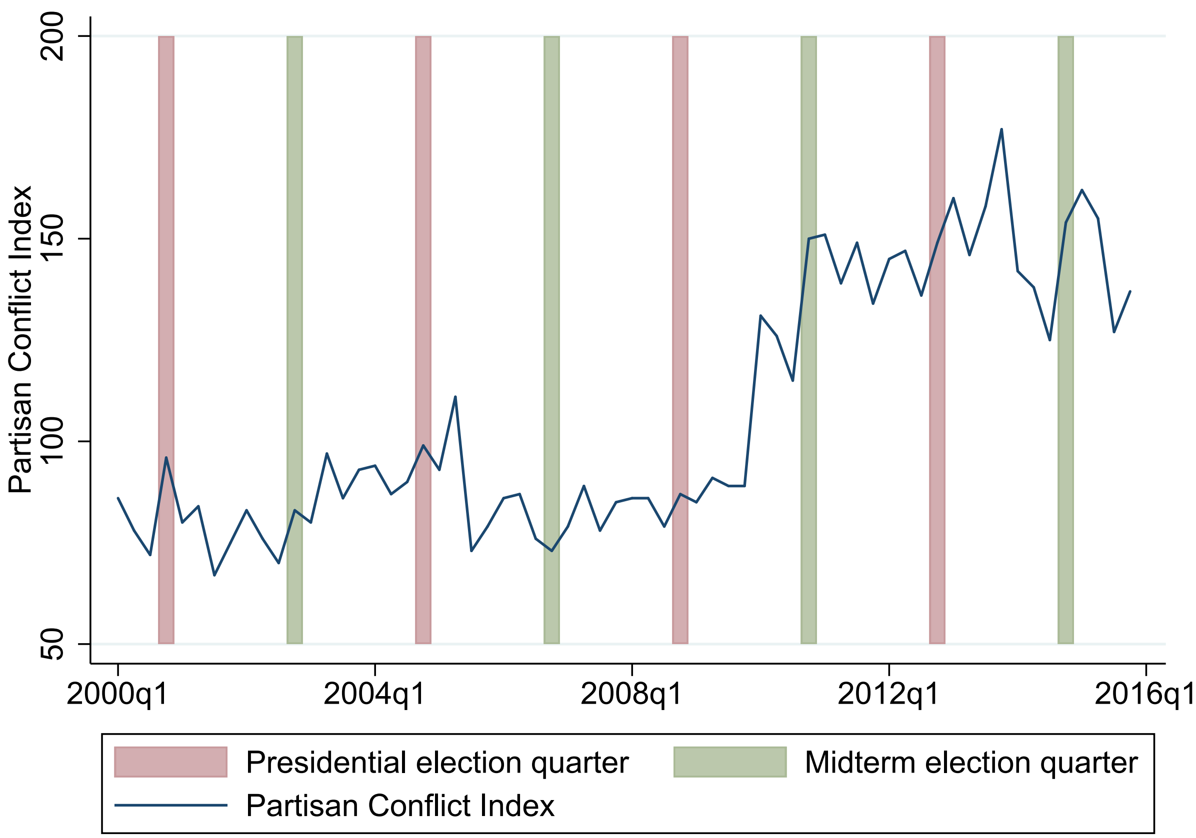



Figure IA.1: **Partisan conflict index by quarter.** The figure plots the Partisan Conflict Index, provided by the Federal Reserve Bank of Philadelphia, over time. The index is provided on a monthly basis and averaged across all months in a given quarter. Red bars indicate presidential election quarters and green bars indicate midterm election quarters.

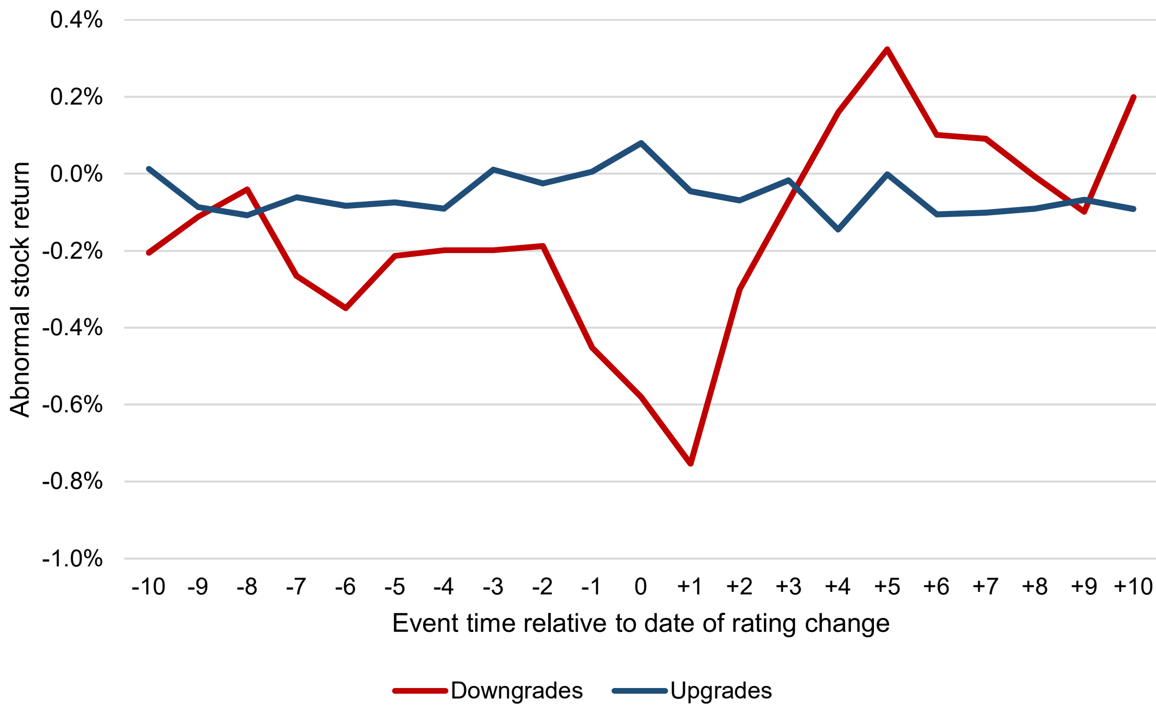



Figure IA.2: **Abnormal stock returns around rating change announcements.** The figure plots abnormal returns around downgrades and upgrades. Abnormal returns are calculated using the Fama and French (1993) and Carhart (1997) model estimated over trading days (-150,-50) relative to the event. We exclude all rating changes where a corporate earnings announcement or an MA announcement falls inside the (-10,+10) window around the rating change.