NBER WORKING PAPER SERIES

# PARTISAN PROFESSIONALS: EVIDENCE FROM CREDIT RATING ANALYSTS

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Working Paper 25292
http://www.nber.org/papers/w25292

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
November 2018, Revised May 2019

We thank Pat Akey, John Barrios, Marianne Bertrand, Steve Davis, Carola Frydman (discussant), Jiekun Huang (discussant), Emir Kamenica, Anil Kashyap, Stefan Lewellen (discussant), Raghuram Rajan, Jesse Shapiro, Chester Spatt, Amir Sufi, Vikrant Vig, and seminar participants at Aalto University, Bocconi University, Chicago Booth, Cornell University, Dartmouth College, the 2018 FRA Conference, Imperial College, MIT Sloan, New York Fed, Rice University, Stockholm School of Economics, the 2019 UNC/Duke Corporate Finance Conference, University of Illinois at Chicago, University of Luxembourg, University of Minnesota Carlson, UVA Darden, Vanderbilt University, and Yale University for valuable comments. Kempf gratefully acknowledges financial support from the James S. Kemper Foundation, the Initiative on Global Markets, and the Fama-Miller Center for Research in Finance at the University of Chicago, Booth School of Business. We thank Yu Gao, Ariza Gusti, Dong Ryeol Lee, Tianshu Lyu, Michael Schwartz, Pan Yingru, and Dongchen Zou for excellent research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 25292
November 2018, Revised May 2019
JEL No. G14,G24,G41

**ABSTRACT**

Partisan perception affects the actions of professionals in the financial sector. Using a novel dataset linking credit rating analysts to party affiliations from voter records, we show that analysts who are not affiliated with the U.S. president's party downward-adjust corporate credit ratings more frequently. By comparing analysts with different party affiliations covering the same firm in the same quarter, we ensure that differences in firm fundamentals cannot explain the results. We also find a sharp divergence in the rating actions of Democratic and Republican analysts around the 2016 presidential election. Our results suggest partisan perception has implications for firms' cost of capital.

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# 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); (Lott and Hassett 2014); 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., Bartels (2002), Gaines, Kuklinski, Quirk, Peyton, and Verkuilen (2007); Gerber and Huber (2009); Curtin (2016); Mian, Sufi, and Khoshkhou (2017)). In order to understand how partisan perception may affect the U.S. economy, it is important to establish whether and when it translates into differences in the behavior of economic agents. While researchers have documented partisan bias in households' assessment of future economic conditions, evidence on actual economic behavior is mixed.[2] Moreover, it has remained an open question to what extent partisan perception influences the economic expectations and actions of individuals with greater economic sophistication, and in high-stake professional environments.[3]

We aim to fill this gap by investigating whether partisan perception affects the actions of an important set of professionals in the financial sector: credit rating analysts. Focusing on credit analysts provides an interesting setting, since their expertise and career concerns should reduce biases and drive their behavior towards the rational model (e.g., Gentzkow, Glaeser, and Goldin (2004); Hong and Kacperczyk (2010)). At the same time, if partisan

[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", "partisan perception", 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 document partisan bias in economic research, and Posner (2008), McKenzie (2012), and Chen (2017), who document partisan bias among judges.

perception affects credit rating actions, then this likely has implications for firms' cost of financing (Fracassi, Petry, and Tate (2016)), as well as their financial policy and investment decisions (Chernenko and Sunderam (2011); Almeida, Cunha, Ferreira, and Restrepo (2017)).

In order to identify the effect of partisan perception, we test whether the rating actions of credit analysts depend on their political alignment with the U.S. president. This test poses a number of empirical challenges. First, it requires observable actions at the level of the individual analyst. Second, analysts need to be linked to information about their political affiliation. Third, it is necessary to compare the actions of analysts with different political affiliations on the same task and in the same information environment. Fourth, we need to separate the effect of political alignment with the president from time-invariant characteristics of Democratic and Republican analysts.

To address these challenges, 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 557 corporate credit analysts with non-missing information on their party affiliation, working at Fitch, Moody's, and Standard and Poor's (S&P) between 2000 and 2018. These analysts cover a total of 1,984 U.S. firms. By comparing rating actions of analysts who rate the same firm at the same point in time, we ensure that our results cannot be driven by differences in the fundamentals of rated firms (i.e., we can compare analysts on the same "task").

We find that partisan perception 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 0.013 notches per quarter. This effect corresponds to 11.4% relative to the average absolute quarterly rating adjustment and is therefore economically sizable. Our empirical 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. Thus, we can rule out the possibility that Democratic analysts rate firms which tend to do well under the policies of Democratic presidents. Our empirical strategy also allows us to control for differences in rating methodologies across rating agencies via agency $\times$ quarter fixed effects. Finally, we control for unobserved time-invariant differences across analysts with different party affiliations via party affiliation fixed effects. We therefore focus on how the behavior of analysts changes depending on whether their preferred party is in power, as opposed to static differences between Democratic and Republican analysts.

To further support our conclusion that the above finding reflects partisan perception, we investigate whether the effect is more pronounced in periods when views of economic conditions are more polarized across political parties. We use the absolute difference in the views of economic conditions between Democrats and Republicans from the Gallup Daily Survey as a measure of political polarization in economic views. The effect of political alignment with the president is 83% larger when polarization increases by one standard deviation. We also find substantially larger effects for analysts who are more politically active, proxied by the frequency with which an analyst votes. Moreover, in an event study around the 2016 presidential election, we find a sizable divergence in the rating actions of Democratic and Republican analysts (see Figures 2 and 5). The 2016 election provides a particularly clean setting because the outcome was unexpected and the two candidates had very different views on economic policy (Meeuwis, Parker, Schoar, and Simester (2018)).

Regarding the economic mechanism, 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 credit risk of firms in the economy. One important advantage of our setting for isolating belief disagreement from other factors is that the rating actions of analysts are unlikely to be driven by how the election of their preferred candidate affects analysts' personal economic condition. To further support our interpretation, we explore one potential object of analysts' belief disagreement—the state of the economy. We show that analysts' alignment with the president's party has no effect on their ratings of foreign firms as well as of domestic firms with low market betas. Hence, the disagreement appears to be focused precisely on the set of firms whose fundamentals should be most affected by changing aggregate economic conditions. Moreover, we conduct an online survey of credit rating analysts and also find striking differences in the assessment of current economic conditions by Democrats and Republicans, consistent with the existing evidence from the household setting.

Finally, we show that rating actions by partisan analysts have non-negligible price as well as potential real effects. Consistent with prior studies, we find cumulative abnormal stock returns of -1.9% 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 leaning. As a result, analysts' partisan perception has economically sizable consequences for the value of rated firms. Replacing an analyst who is aligned with the president with an analyst who is misaligned leads to a difference in the firm's market capitalization of 0.51%–0.73%, or $90–$128 million, over a single presidential term. As we argue below, this likely represents a lower bound for the true effect of analysts' partisan perception.

To gauge the potential real effects of analysts' partisan perception 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. 1.7%.

This is the first study to identify a significant effect of partisan perception on the actions of finance professionals; specifically, on the rating actions of credit analysts. Since credit ratings have been shown to significantly affect firms' cost of capital (Kisgen and Strahan (2010), Baghai, Servaes, and Tamayo (2014), Fracassi, Petry, and Tate (2016)), capital structure decisions (Kisgen (2006)), and investment (Chernenko and Sunderam (2011); Begley (2015), Almeida, Cunha, Ferreira, and Restrepo (2017)), partisan perception influencing credit rating decisions will likely have real effects.

By affecting rating actions, partisan perception also influences analyst accuracy, which can create distortions in the analyst labor market.[4] Finally, if partisan perception affects the decisions of credit rating analysts, it may also affect the decisions of other relevant economic agents. Given that the effect of partisan perception prevails even in a setting where pecuniary and professional gains are at stake,[5] 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 perception. Section 6 discusses how rating actions by partisan analysts may affect firms' cost of capital and investment, and Section 7 concludes.

## 2. Related Literature

Our findings contribute to a growing literature on the connection between partisanship and economic behavior. Most of the existing studies have focused on partisan perception 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 pre-

[4]Our findings regarding analyst accuracy are reported in the Internet Appendix.

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

ferred 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 ideology 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.[6] In addition to consumption, studies have examined partisanship and household asset allocation. Addoum and Kumar (2016) shows that the industry-level composition of investor portfolio changes when the party in power changes. 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 the effect of partisan ideology among 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.

[6]There are several factors that could explain the mixed findings when linking partisan ideology to household consumption, such as using survey-based, self-reported consumption data versus administrative data, studying different countries and time periods, as well as employing different methods to infer political affiliation.

Our work complements these studies by focusing on financial experts.

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 perception as a source of distortion in credit ratings. Our paper also relates more broadly to the literature on the determinants and consequences of credit ratings (see, for example, Becker and Milbourn (2011); Kisgen and Strahan (2010); Xia (2014); Griffin and Tang (2012); Cornaggia, Cornaggia, and Israelsen (2017); Cunha, Ferreira, and Silva (2015)).

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, investment managers and investors. Prior studies have documented that mutual fund managers who make campaign donations to the Democratic party hold less of their portfolios in companies that are deemed socially irresponsible (Hong and Kostovetsky (2012)), left-wing voters are less likely to invest in stocks (Kaustia and Torstila (2011)), sell-side equity analysts who make political contributions to the Republican Party are more likely to issue conservative forecasts and recommendations (Jiang, Kumar, and Law (2016)), and 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 perception from unobserved time-invariant characteristics of individuals with different political affiliations.

Finally, our findings relate to prior literature that supports that agents do not interpret public information identically (e.g. Harris and Raviv (1993), Kandel and Pearson (1995), Bamber, Barron, and Stober (1999), (Meeuwis, Parker, Schoar, and Simester 2018)).Our setting allow us to provide direct evidence that Democratic and Republican analysts provide different rating recommendations in response to similar public information.

# 3. Data and Sample Construction

## 3.1 Data

The main dataset used in the analysis is constructed from the combination of credit ratings on corporate debt issuers, 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 Internet Appendix IA.A.1.

### 3.1.1 Corporate Credit Ratings

We collect rating actions on U.S. corporate debt issuers from all three major ratings agencies: Fitch, Moody's, and 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.[7] The time period spans the years from the first quarter of 2000 to the first quarter of 2018. 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 from S&P's Global Credit Portal. They usually contain two names, one more junior employee (typically the lead analyst), and one more senior

[7]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.

employee (typically the rating committee chair).

### 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.[8] 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 Internet Appendix IA.A.1, 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,[9] 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. 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 are less likely to 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. 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.

[8]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.

[9]`https://www.fec.gov/`

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 political polarization in the views of economic conditions in the general public, we use the Daily Survey by Gallup, Inc. The Gallup Daily data cover around 1,000 individuals every day for years 2008 to 2017. We require two main variables from the survey in order to measure the polarization in economic views: (i) a measure of an individual's view on the current economic conditions, and (ii) a measure of her political affiliation. To measure the views on current economic conditions, the Gallup survey asks the following question: "How would you rate economic conditions in this country today — as excellent, good, only fair, or poor?". The responses to this question are converted into a numerical scale that ranges from 1 (poor) to 4 (excellent). Moreover, the Gallup survey contains two question about political affiliation, which allows to classify survey respondents into Democrats, Republicans, or Independents. Our measure of political polarization in economic views is the absolute difference in the average economic views of Democrats and Republicans in a given calendar quarter.

For additional robustness tests we use an alternative measure of the views of economic conditions based on the Michigan Survey. Specifically, we use the Current Economic Conditions Index from the Michigan Survey. We provide more details on the two surveys in the Internet Appendix and we plot the time-series of both measures of polarization in economic views in Internet Appendix Figure IA.1.

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 the API `name-prism.com` (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 2,402 issuers by 1,211 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 the Internet Appendix. Our final sample includes 557 analysts, covering 1,984 firms.

In order to put the resulting match rate of ca. 46% (=557/1,211) in context, consider the

[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.

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 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 U.S. 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.3 in the Internet Appendix, show that analysts for whom we are able to obtain party affiliation rate firms that are slightly larger, have a higher Tobin's Q and a lower return on assets. 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 political alignment with the president on the decisions of financial experts, 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.4 in

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

[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 65 analysts, we are unable to determine a unique match out of multiple potential voter matches.

the Internet 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 three years as of quarter end, and we do not assign analysts to quarters beyond the date of the final report for a given agency-firm pair.

### 3.3 Summary Statistics

Table 1 and Figure 1 report summary statistics. 26% of the analysts who are registered voters are unaffiliated, 39% are registered Democrats, 34% are Republicans, and 1% are registered with a party other than the Democratic or Republican party. In 37% 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 (64%), consumer staples (50%), and telecommunication services (49%). 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 (44%) than in Chicago (27%). Figure 1c compares the political affiliation of analysts in the three different rating agencies. At S&P, Fitch, and Moody's, 54%, 34%, and 33% of the analysts are Democrats, respectively. The median analyst is in the sample for approximately 5 years (unreported for brevity).

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 0.026 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)). 15% 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 political perception gets reflected in credit ratings gradually over time, as new information about economic policies and firm fundamentals arrives.

Figure 2 presents the average adjusted rating change for Democratic, Republican, and unaffiliated analysts during the five presidential terms in our sample period. 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 Republican presidency, Republican analysts upward-adjust ratings more relative to Democratic analysts. Under Obama's presidency, the sign of this difference reverses: Republican analysts downward-adjust more relative to Democratic analysts. An additional pattern that emerges from Figure 2 is that the disagreement between Republican and Democratic analysts is greater during Republican administrations. This is consistent with existing survey evidence on households, where the partisan divide in views of the economy is also stronger under Republican presidents (e.g., Pew Research Center (2019)). Finally, the Trump presidency is particularly interesting because the outcome of the 2016 election was unexpected, it lead to the Republican party controlling both the Senate and the White House, and the two candidates involved in the election had very different views on economic policy. The disagreement between Democratic and Republican analysts is particularly large during the Trump presidency, with Democratic analysts downward-adjusting more relative to Republicans. Importantly, the rating behavior of unaffiliated analysts is relatively similar under Democratic and Republican presidents.

# 4. Empirical Strategy

Measuring the influence of political alignment with the president 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 perception 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, Republican analysts could be 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 party affiliations may not reflect the effect of partisan perception, 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 partisan perception, but due to the non-random composition of political affiliations across agencies.

Our analysis does not require a random sample. 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, political affiliation fixed effects, as well as agency $\times$ quarter fixed effects:

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

where $j$ denotes the rating agency, $p$ denotes the analyst's political affiliation (Democrat, Republican, Unaffiliated, and Other), and $ideological\,mismatch_{it}$ is an indicator equal to one if the analyst's party affiliation does not match 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 includes a control for analyst tenure and the number of firms rated.

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 party affiliation fixed effects in all regressions, the coefficient on *ideological mismatch* will be identified only based on Republican and Democratic analysts, because their party switches at least once from aligned to misaligned with the president.[16] The main benefit of including unaffiliated analysts in our baseline definition is that it allows us to estimate the fixed effects

[16]Since unaffiliated analysts and analysts with other party affiliations never change from aligned to misaligned, whether we code them as aligned or misaligned does not affect out estimate of $\beta$ in Equation (1).

and coefficients on control variables more precisely. In Table 3, we show that we obtain very similar results if we only keep Democratic and Republican analysts.

By including firm $\times$ quarter fixed effects, we are effectively comparing analysts who cover the same firm at the same point in time. Hence, our results cannot be driven by analysts with ideological mismatch covering different types of firms. Including agency $\times$ quarter fixed effects addresses concerns that different methodologies of credit agencies might be correlated with the political cycle during our sample period. Furthermore, we present specifications were we include agency $\times$ industry instead of agency $\times$ quarter fixed effects to address any concerns related to agencies with different composition of Democratic and Republican analysts having different rating models for different industries. Finally, our estimator of partisan perception 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. Thus, our estimator of partisan perception captures how the behavior of Democratic and Republican analysts changes depending on whether their preferred party occupies the White House, and not a time-invariant characteristic correlated with party affiliation. We also present robustness tests that include analyst fixed effects (see Table 4), which leaves the magnitude of the estimated coefficient virtually unchanged. We double-cluster standard errors at the analyst and firm level throughout the analysis. In Table 4, we show that our results are robust to triple clustering by firm, analyst, 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.[17] 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 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.

[17] 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.

## 4.2 Why do corporate credit analysts provide a useful setting?

Corporate credit rating analysts provide a suitable setting to study the effect of partisan perception on economic forecasts in a high-stake environment for a number of reasons. First, it is one of the few settings where professional economic forecasts can be linked to the identity of the individual forecaster. Second, credit ratings have been shown to affect firms' cost of financing (Fracassi, Petry, and Tate (2016)), as well as their financial policy and investment decisions (Chernenko and Sunderam (2011); Almeida, Cunha, Ferreira, and Restrepo (2017)). Hence, the views and actions of credit rating analysts have real consequences for the rated firms. Third, rating the creditworthiness of an issuer requires making forecasts over long horizons, where differences in beliefs should matter more than for shorter horizons (Patton and Timmermann (2010)).[18] Fourth, since multiple analysts can rate the *same firm* at the *same point in time*, we are able to control for differences in the type of firms that analysts rate. Finally, one of the challenges of addressing our question is that we need to match individual analysts to voter registration data. The geographical location of credit analysts is very concentrated in New York City and Chicago, which reduces problems related to heterogeneous data availability and data quality in the voter registration records across different U.S. states.

# 5. Analysts' Partisan Perception and Credit Rating Actions

This section presents our main results. We first document that partisan perception 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 estima-

[18] While equity analysts also provide long-term growth forecasts, those forecasts have been shown to have low predictive power for realized growth rates over longer horizons (e.g., La Porta (1996); Chan, Karceski, and Lakonishok (2003); Barniv, Hope, Myring, and Thomas (2009)).

tion methods. We further show that the effect of partisan perception is more pronounced when views of economic conditions are politically polarized, as well as for analysts who are frequent voters. We find no influence of partisan perception when analysts rate foreign firms as well as domestic firms with low market betas.

## 5.1 Main Results

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 party affiliation fixed effects in all regressions.

We begin by including agency fixed effects in addition to firm $\times$ quarter fixed effects and analyst's party affiliation fixed effects, since Figure 1 documents pronounced differences in political ideological across different agencies. The results, reported in column (1), suggest that partisan perception affects analysts' rating behavior. Specifically, analysts who do not support the president's party are more likely to adjust ratings downward by 0.0168 notches relative to analysts who are aligned with the president's party. The economic and statistical significance of the effect of partisan perception remains high as we tighten the identification to include agency $\times$ industry (column (2)) as well as agency $\times$ 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 0.0134 notches. Relative to the average absolute rating change of 0.118 notches, this is an economically sizable increase of 11.4%.

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 behavior. Most importantly, 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. Second, by including agency $\times$ quarter fixed effects, we can remove any differences in rating methodologies across rating agencies. Third, by including party affiliation fixed effects, we are removing any time-invariant differences across individuals with different party affiliations, and can isolate changes in their rating behavior as the president changes.

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 IA.5 in the Internet Appendix 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 remarkably stable when we include these additional control variables.

In Table 3 we repeat our main analysis but we report the effects separately for Democratic and Republic analysts (Panel A) as well as for Democratic, Republican, and unaffiliated analysts (Panel B). The estimates in Panel A suggest that Democratic analysts downward-adjust ratings more, relative to Republican analysts, under Republican presidents. Under Democratic presidents, the gap between the two groups reverts sign and Democratic analysts upward-adjust more. Consistent with Figure 2, we find that the difference between Democratic and Republican analysts is more pronounced under Republican presidents.[19]

In Panel B we also add unaffiliated analysts, such that the coefficients on *Democrat* and *Republican* capture the difference relative to the base group of unaffiliated analysts. Rel-

[19]The results from an $F$-test that assesses whether the difference between Republican and Democratic analysts is statistically significant under Democratic presidents, reported at the bottom of the table, reveals that the difference between Republican and Democratic analysts is mostly insignificant under Democratic presidents.

ative to unaffiliated analysts, Republican analysts upward-adjust significantly more under Republican presidents and downward-adjust more under Democratic presidents. Democratic analysts behave more similarly to unaffiliated analysts, since the difference between Democratic and unaffiliated analysts is almost never statistically significant. Qualitatively, they downgrade more relative to unaffiliated analysts under Republican presidents, and the gap closes to roughly zero under Democratic presidents. At the bottom of the table, we report the results from an $F$-test that assesses whether the difference between Republican and Democratic analysts is statistically significant under Democratic and Republican presidents, respectively, which confirms the results from Panel A.[20]

## 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 alternative definitions of mismatch. For example, we use a definition of mismatch that uses only party affiliation information from presidential elections, as opposed to all elections. We also test a definition of mismatch that complements our party affiliation from voter registration records with party information from analysts'

[20]In Internet Appendix Table IA.6 we also add unregistered analysts and group them together with the unaffiliated voters. The results are qualitatively very similar to the ones reported in Table 3.

financial contributions to political parties, candidates, and committees.[21] Both alternative definitions of ideological mismatch yield results that are similar to our baseline.

In Panel C, we assess the robustness of our results with respect to alternative estimation methods. Triple-clustering by firm, analyst, and quarter leads to smaller standard errors. We also verify that we find similar results if we estimate the regression in Equation (1) at the agency-firm-quarter level instead of at the analyst-firm-quarter level; i.e., we compute the average ideological mismatch across all analysts from 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 fixed effects. The latter result is important, because it reinforces our argument that we are capturing the effect of partisan perception separately from time-invariant characteristics of the analyst, which may be correlated with party affiliation. Furthermore, our results do not change when we introduce NBER recession interacted with party affiliation fixed effects. The latter addresses the potential concern that our baseline results may reflect a differential response of Democratic and Republican analysts to recessions, as opposed to a differential response to different White House administrations.

In Appendix Table A.2, 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. In column (1), we find that analysts misaligned with the party majority in the Senate downward-adjust ratings more. We do not find a significant effect from misalignment with the party majority in the House of Representatives (column (2)). When we add misalignment with the president as an additional control in columns (3) and (4), neither ideological mismatch with the Senate nor with the House of Representatives has incremental explanatory power. This insignificance of the party in control of Congress is consistent with existing studies of political cycles and stock returns (Santa-Clara and Valkanov (2003); Pástor and Veronesi (2018)), as well as with prior studies of political cycles and GDP growth (Blinder

[21] Data on financial contributions are obtained from Stanford's Database on Ideology, Money in Politics, and Elections (DIME) (see Bonica (2016)). We are able to match 57 analysts to a federal or state-level contribution. In the Internet Appendix we provide additional information regarding the merging procedure.

and Watson (2016)). We do not have a complete explanation for why disagreement with the president matters, while disagreement with Congress does not matter incrementally. 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. 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.

Figure 3 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. Finally, we find the effect of partisan perception to be fairly consistent across issuers of different credit quality (Figure 4). We conclude that our main result is robust to a large set of different specifications and estimation approaches.

## 5.3 2016 Presidential Election

To further support the causal relationship between partisan perception and analysts' rating actions, we conduct an event study around the 2016 presidential election. We focus on the 2016 election since it is unique — the outcome was unexpected and the two candidates had very different views on economic policy (Meeuwis, Parker, Schoar, and Simester (2018)). Moreover, the timing of the 2016 election did not overlap with any major economic events, and it lead to a change of majority in the Senate.

Figure 2 already suggests that the Trump presidency is special because the disagreement between Republican and Democratic analysts is particularly pronounced during this period. Zooming in more closely around the election further strengthens this conclusion. Figure 5 plots the difference in the average rating adjustment between Democratic and Republican analysts around the 2016 election. We observe that the difference in the average rating adjustment between Democrats and Republicans is close to zero prior to the election. The difference then spikes during the election quarter, indicating that Democratic analysts downward-adjusted firms more.[22]

Following the election, the average difference between Democratic and Republican analysts continues to be positive. Although the coefficient for each individual quarter is not statistically significant at the 95% level due to data constraints, the differences are still economically sizable. We test in Appendix Table A.3 whether Democratic analysts on average downward-adjust credit ratings more than Republicans in the post-Trump period relative to the pre-Trump period. We find that they do. Despite the data limitations related to this event study and the fact that rating changes are infrequent events, the results indicate a significant change in the rating behavior between Democratic and Republican analysts around the 2016 election. Due to the unexpected outcome of the election, this further supports the causal interpretation of our main results.

## 5.4 Heterogeneity Across Time and Analysts

We next test for heterogeneous effects across time and analysts. Specifically, we investigate whether the effect of partisan bias is more pronounced during periods in which the views of economic conditions are more politically polarized, as well as among analysts who are more political.

To measure political polarization in the views of economic conditions we use data from the Gallup Daily survey. To measure the views on current economic conditions, the Gallup survey asks the following question: "How would you rate economic conditions in this country today — as excellent, good, only fair, or poor?". The responses to this question are converted into a numerical scale that ranges from 1 (poor) to 4 (excellent). Our measure of polarization is the absolute difference in the average economic views between Democrats and Republicans in a given calendar quarter. We standardize the measure to have a mean

[22]Switching to monthly frequency reveals that most of the divergence occurs in November 2016. Results are unreported for brevity.

of zero and a standard deviation of one.

In Table 5, we interact ideological mismatch with the polarization in economic views *Econ. Polarization Gallup*. The estimates in column (1) imply that the effect of misalignment with the president increases by 0.0087 notches for a one-standard-deviation increase in partisan conflict, thereby increasing the baseline effect by 83% (=0.0087/0.0105). The result is robust when we add agency $\times$ industry fixed effects (column (2)) and agency $\times$ quarter fixed effects (column (3)). Internet Appendix Table IA.7 shows that we find similar results if we construct our measure of polarization in economic views based on the Michigan Survey.

Next, we examine the importance of analyst heterogeneity and investigate whether the effect of political alignment with the president is more pronounced among analysts who are politically active. In Table 6 we test three alternative proxies for political activeness, based on how frequently the analyst votes. First, we identify analysts who have voted in a primary or midterm election. Second, we compute the average time gap (in quarters) between the elections in which the analyst votes, and define an indicator *Low avg. election gap* equal to one for analysts in the bottom quartile within a given quarter (column (2)). Third, we create an indicator variable *Frequent voter* that is equal to one for analysts who either have voted in a midterm or primary election, or for whom the *Low avg. election gap* indicator is equal to one (column (3)). Since some of these measures are 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 political polarization in economic views and voting frequency strongly support the interpretation that our results reflect differences in partisan perception, and further raises the bar for alternative explanations.

## 5.5 Economic Mechanism: Belief Disagreement

We interpret our main result as evidence that analysts with different party affiliations differ in their views on how the economic policies of the U.S. president will affect the credit risk of firms in the economy. An important advantage of our setting is that rating actions are unlikely driven by differences in how analysts' personal economic situation changes when their preferred party gains power, which represents a key challenge for studies of household behavior. To further support the interpretation that our results reflect differences in economic beliefs, this section explores one potential object of analysts' belief disagreement—the state of the economy—and provides two additional pieces of evidence. First, we show that differences in rating behavior disappear when analysts rate firms whose fundamentals are less correlated with the U.S. economy. Second, we conduct an online survey of credit rating analysts and document more pessimistic views on current economic conditions among credit rating analysts who are not aligned with the president.

### 5.5.1 Firm Heterogeneity

Prior studies have documented striking differences in economic expectations across households with different partisan identities. If the main result in this paper is also driven by credit analysts with different party affiliations having different beliefs about the state of the economy under different presidents, then we should see smaller differences in rating behavior when analysts rate firms whose fundamentals are less correlated with the U.S. economy. We use two proxies to identify such firms: foreign firms, as well as domestic firms with low market betas. We estimate market betas using the Fama and French (1993) and Carhart (1997) model, monthly return data, and a 5-year rolling window. We split firms into high and low market beta firms at the median within a given quarter. Table 7 reports the results. For foreign firms and domestic firms with low market betas, there is no difference in the rating behavior between analysts who are politically aligned versus misaligned with the White House. The effect of partisan perception thus appears to be concentrated in the group of domestic firms with high exposure to the U.S. market.

These results suggest that the heterogeneous behavior of Democratic and Republican analysts could be driven by their different assessment of economic conditions and 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 market betas.

#### 5.5.2 Survey Evidence

We also present survey evidence to directly support our interpretation that credit analysts with different partisan identities differ in their beliefs about the state of the economy. Between November 2018 and March 2019, we conducted an online survey of current and former credit rating analysts to ask them about their views on current economic conditions. We invited alumni of Cornell University Johnson School of Management and of the University of Chicago Booth School of Business who work or used to work at credit rating agencies, as well as members of the LinkedIn group "Credit Rating Agency Alumni + Friends" to participate in the survey. Overall, we invited 365 individuals and received 70 responses (i.e., we have a response rate of 19.2%). We drop 12 individuals who indicate that they have never worked as credit rating analysts.

To infer analysts' views on current economic conditions, we ask the following question: "How would you rate economic conditions in this country today?", with possible answers of "excellent", "good", "fair", and "poor". We code the answers with numerical values from 1 to 4, with 1 indicating least favorable view and higher values representing more favorable views. In our analysis, we standardize the measure to have a mean of zero and a standard deviation of one. In addition to the question on economic views, we also collect demographic characteristics, such as the number of years of experience, age, gender, ethnicity, and residence. To infer the political affiliation of the analysts, we use the following question:

"In politics, as of today, do you consider yourself a Republican, a Democrat, an Independent or Unaffiliated?". We classify a respondent's political affiliation as *Republican*, *Democrat*, or *Independent* (if she responded Independent or Unaffiliated). Since 13 respondents did not finish the survey, our final sample of individuals that responded to both questions consists of 45 responses.

Table IA.2 in the Internet Appendix presents summary statistics on the respondents. The vast majority of the respondents has more than 10 years of work experience as credit rating analysts, with forty percent of the respondents having more than fifteen years of experience. Regarding their political affiliation, forty percent identify as Democrat, twenty-seven percent identify as Republican, and thirty-three percent identify as Independent. Consistent with the dataset for our main analysis, the majority of the respondents is white and male.

Appendix Table A.4 presents the analysis of analysts' self-reported views on the state of the economy. We observe that Republicans have more a positive view of current economic conditions relative to Independents, while Democrats are statistically indistinguishable from Independents. The result is robust to including controls for analysts' demographic characteristics (gender, age, and ethnicity) and years of experience. The difference between Democrats and Republicans is statistically significant at the 1% level in column (1) and at the 10% level in columns (2) and (3). Since the survey was conducted under a Republican administration, the results confirm the findings from prior household surveys that individuals' views of economic conditions depend on whether the White House is occupied by their preferred political party (Bartels (2002), Gaines, Kuklinski, Quirk, Peyton, and Verkuilen (2007); Gerber and Huber (2009); Curtin (2016); Mian, Sufi, and Khoshkhou (2017)). It is remarkable that this finding survives in a sample of finance professionals that likely has a much higher level of economic sophistication than the average U.S. household. Moreover, the result provides direct evidence for our interpretation that the differences in credit rating actions documented above are driven by differences in economic beliefs.

# 6. Price and Real Effects of Partisan Perception

The results in this paper show that partisan perception affects corporate credit ratings. The goal of this section is to gauge the potential consequences of the main result on firms' cost of financing and investment decisions. First, regarding price effects, we document that firms which are rated by partisan analysts experience greater losses in their market capitalization. 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 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 (-300,-50) relative to the rating change. In Figures IA.2 and IA.3 in the Internet Appendix, we plot the abnormal returns in the 21 days around the upgrade and downgrade announcements in our sample. We also remove any rating changes where the firm makes an earnings announcement or M&A announcement inside the (-10,+10) window.[23] Consistent with

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

prior studies, we find large negative average abnormal returns (-1.9%) in the three days around rating downgrades, and very small abnormal returns (0.2%) around upgrades after excluding concurrent events.

Given that we have established that analysts whose party affiliation does not match the president's downward-adjust more frequently, a remaining question is how well the market is able to "correct" for analysts' partisan perception. In other words, is the stock market reaction to downgrades muted when they are issued by misaligned analysts? Figures IA.3 and IA.2 in the Internet Appendix suggest this is not the case. The overall pattern in the cumulative abnormal returns is very similar for analysts who are aligned and misaligned with the president.[24]

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 the purpose of political campaigns and education, the party affiliation of the analyst is not public information and may therefore not be fully reflected in the stock price. 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.[25]

Using the cumulative abnormal returns estimated above as a measure of the change in the firm's market capitalization induced by rating changes, we next examine whether firms rated by misaligned analysts experience on average a greater reduction in their market capitalization. We aggregate the three-day and seven-day cumulative abnormal returns around rating changes within a given rating agency, firm, and quarter, and replace them by zero when there is no rating change in a given quarter. We then reestimate the regression in Equation (1), after replacing the dependent variable with the cumulative abnormal returns

[24] While we do find statistically higher cumulative abnormal returns for misaligned analysts when we focus on a 3-day window around rating downgrades, the difference disappears once we control for agency and calendar month effects (see Internet Appendix Table IA.8).

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

around rating changes during the quarter.

Table 8 presents the results for cumulative abnormal returns measured over two event windows, (–1,+1) (columns (1) and (2)) and (–3,+3) (columns (3) and (4)), respectively. In columns (2) and (4), we exclude rating changes with a concurrent corporate event in the windows (–1,+1) and (–3,+3) around the event, respectively. We find that, in any given quarter, firms rated by misaligned analysts lose 0.032%–0.046% more of their market capitalization when they are rated by a misaligned analyst, depending on whether cumulative abnormal returns are measured over a (–1,+1) or a (–3,+3) event window. Since these losses accumulate over time, they are economically sizable: over a four-year political cycle, a firm rated by a misaligned analyst loses 0.51% (=0.0321×16) to 0.73% (=0.0458×16) more of its market capitalization if it is rated by a misaligned analyst as opposed to by an analyst who is politically aligned with the president. For the average-sized firm in our sample, this amounts to a total dollar loss of $90–$128 million.

We believe that the estimated losses above likely represent an underestimation of the true losses in market capitalization due to analysts' partisan perception. First, while voter registration records provide a very useful proxy for analysts' political party affiliation, they surely come with measurement error. Such measurement will bias our estimates of the effect of partisan bias downward. Second, if rating changes are partially anticipated by the market, as Figures IA.3 and IA.2 in the Internet Appendix may suggest, then the cumulative abnormal returns during the three or seven days around the rating change will underestimate the actual change in the firm's market capitalization due to a rating change. Our estimates above should therefore be interpreted as a lower bound on the magnitude of the price effect of analysts' partisan perception.

## 6.2 Firm Investment: 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 firm 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 0.05-notch ($= 0.0134 \times 4$) differential in the annual credit rating change for the average firm in our sample. Exploiting exogenous variation in corporate ratings due to rating agencies' sovereign ceiling policies, Almeida, Cunha, Ferreira, and Restrepo (2017) show that a 0.7-notch decrease in a credit rating is associated with an 8.9 percentage-point reduction in firm investment for the treated firms, which corresponds to a 24% decrease relative to the pre-event level of investment. 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 firm investment of 0.64 ($= 8.9 \times 0.05/0.7$) percentage points, which represents 1.7% 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 perception to be fairly consistent across issuers (Figure 4), suggesting that the use of real-effect estimates for highly rated firms may even lead to a downward bias. However, to the extent that the real effects of credit rating changes differ across countries and time, the true real effects of partisan perception may be different than in our back-of-the-envelope calculation above.

# 7. Conclusion

We show that partisan perception 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 polarization can have real effects is through the economic beliefs and actions of relevant economic agents. To the best of our knowledge, this is the first study to quantify the influence of partisan perception on the actions of an important set of finance professionals: credit rating analysts. Given that the effect of partisan perception prevails even in a setting where pecuniary and professional gains are at stake, it may influence the behavior of many other relevant economic agents, such as firm managers and investment managers. This is a fruitful avenue for future research in our view.

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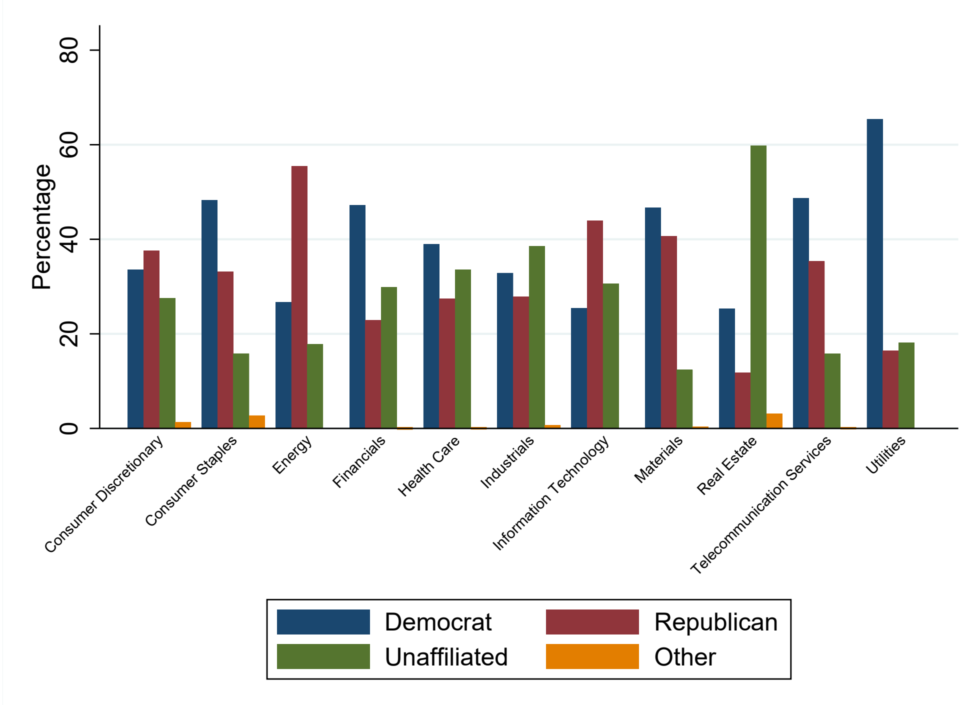
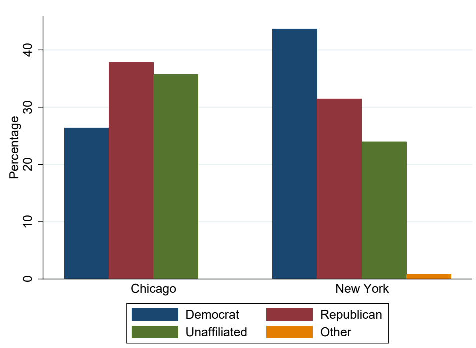
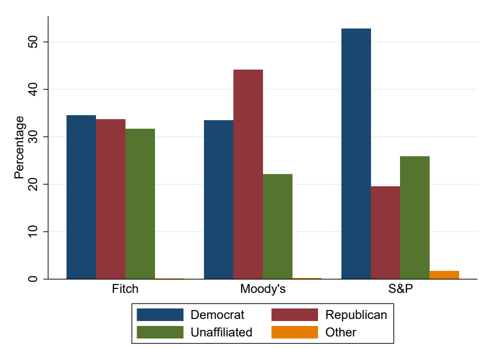



(a) Party Affiliation by GICS Sector




(b) Party Affiliation by City




(c) Party Affiliation by Rating Agency

Figure 1: **Party Affiliation Summary**

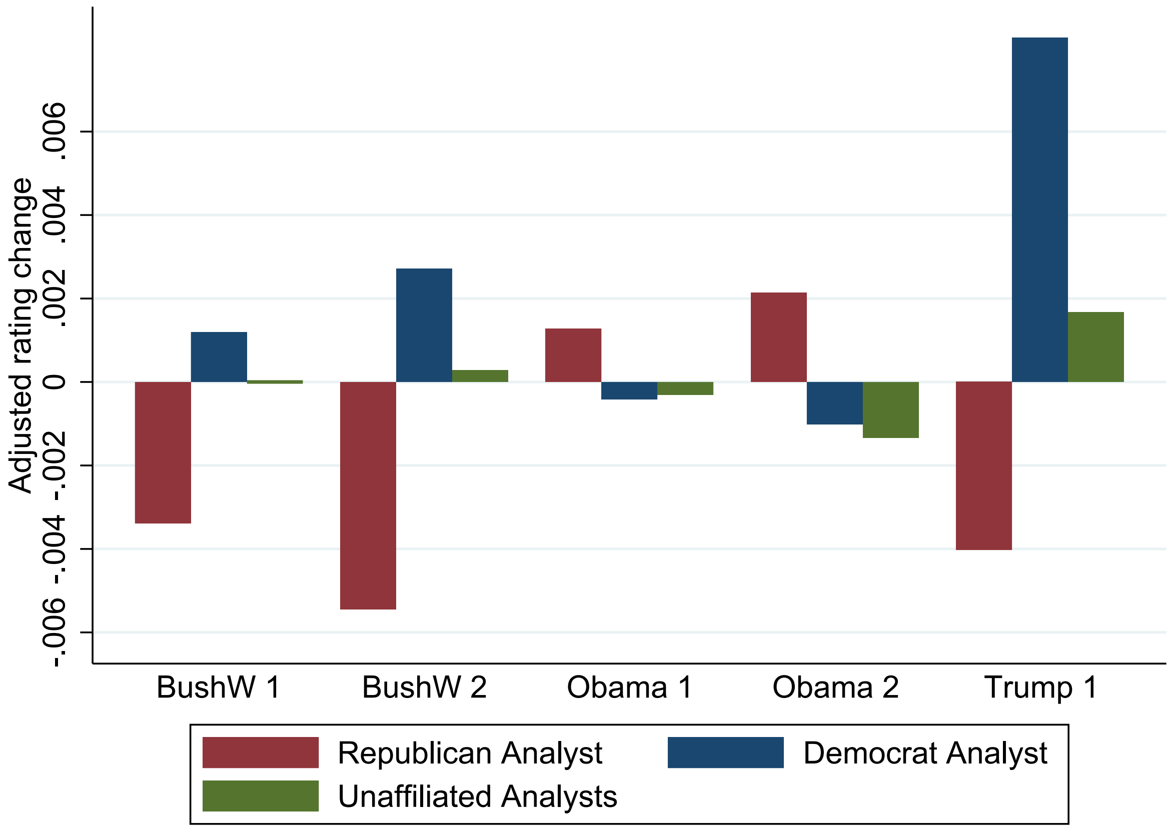



Figure 2: **Average Rating Changes by Analyst Party Affiliation and Presidency.** The figure plots the average adjusted rating change separately for Democratic and Republican analysts under each presidency during our sample period. Adjusted rating changes are computed by taking the quarterly rating change and subtracting the average rating change within the same firm and quarter.

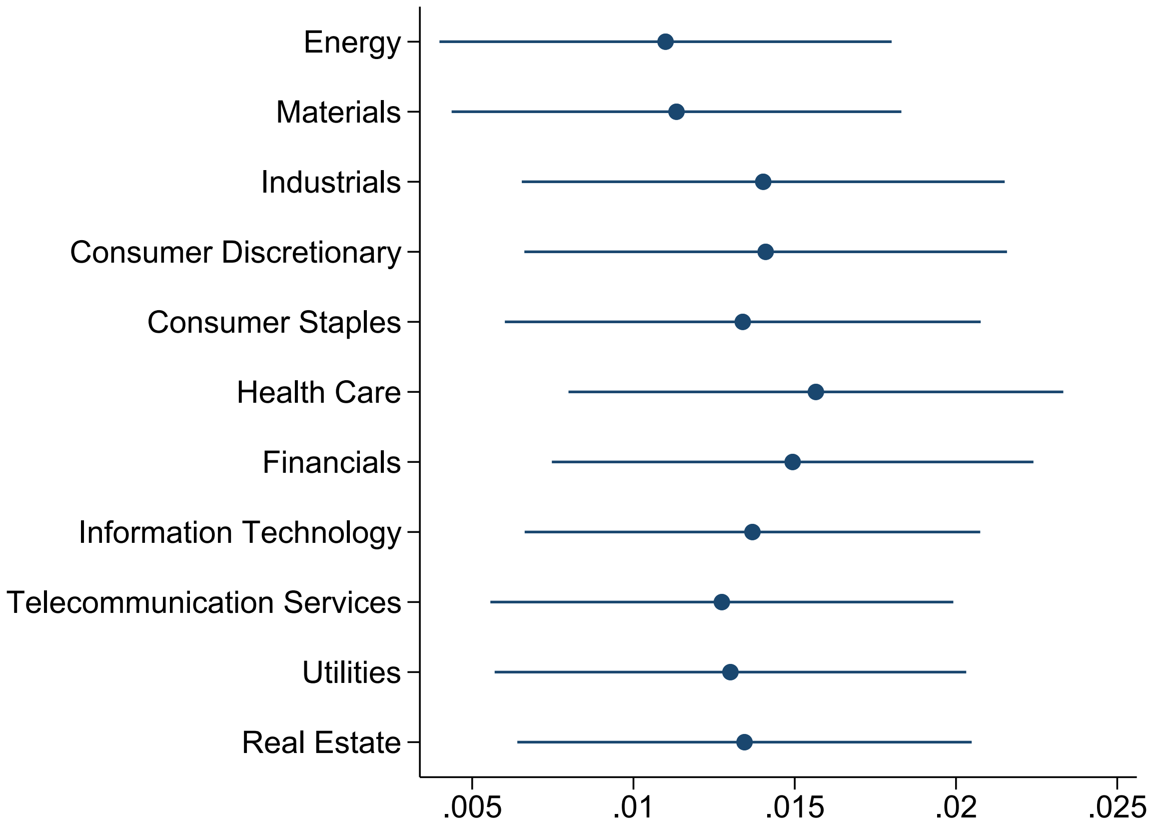



Figure 3: **Coefficient estimates after excluding each GICS sector.** The figure plots the coefficient estimate on ideological mismatch from the regression specification in Table 2, column (3), after excluding one GICS sector at a time. We also plot the corresponding 95% confidence intervals, based on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

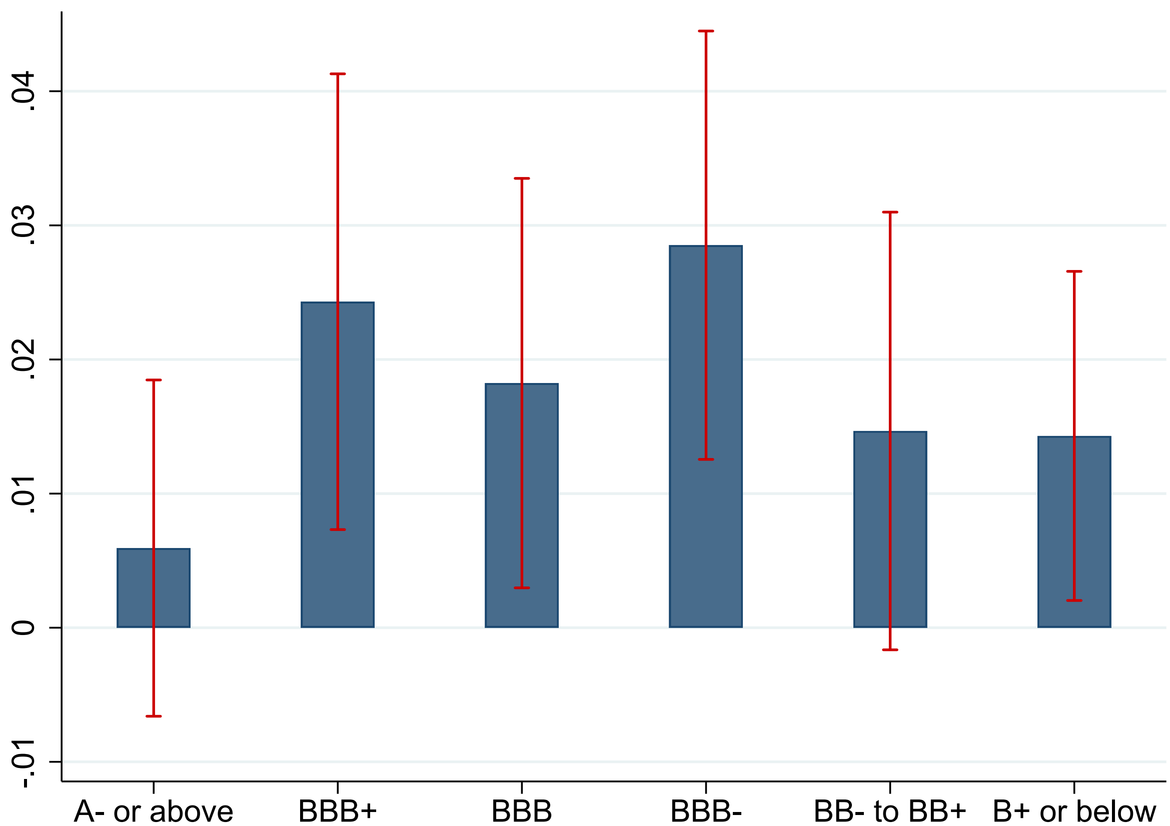



Figure 4: **Coefficient estimates by initial rating category.** The figure plots the coefficient estimate on the interaction term between ideological mismatch and six different rating categories. The regression specification is otherwise the same as in Table 2, column (3). Rating categories refer to the credit rating at the end of the quarter prior to the ratings change. We also plot the corresponding 95% confidence intervals, based on standard errors that are double-clustered at the firm and analyst level.

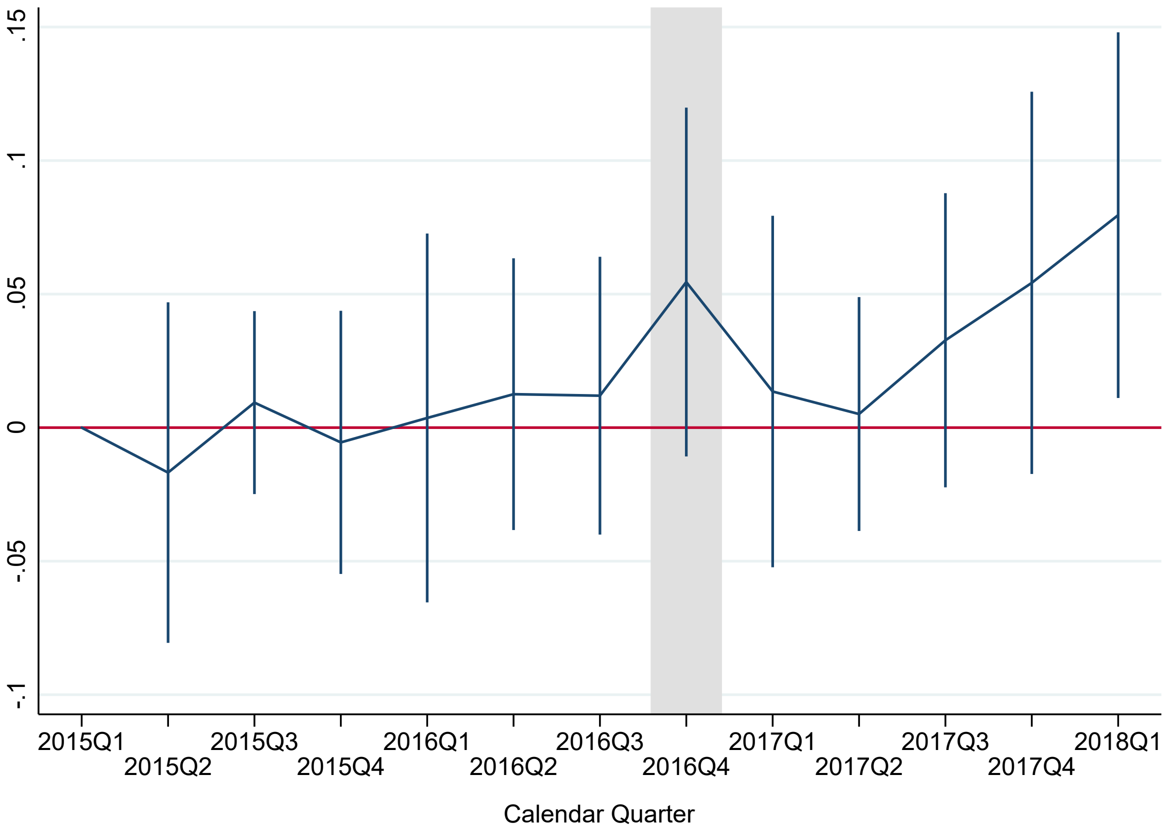



Figure 5: **Event study around the 2016 presidential election.** The figure plots the difference in the average rating adjustment between Democratic and Republican analysts around the 2016 presidential election. We plot the coefficient estimate $\beta_\tau$ from the regression below against calendar time:

$$\Delta R_{ift} = \alpha_{ft} + \alpha_j + \sum_{\tau=2015Q1}^{\tau=2018Q1} \beta_\tau Democrat_{it} \times D_t^\tau + \gamma' X_{it} + \epsilon_{ift}, \tag{3}$$

where $Democrat_{it}$ refers to an indicator equal to one for analysts who are affiliated with the Democratic Party and zero for Republican analysts, and $D_t^\tau$ stand for calendar quarter dummies. $\alpha_{ft}$ and $\alpha_j$ refer to firm $\times$ quarter and agency fixed effects, respectively. Vector $X_{it}$ includes dummies for the analyst's party affiliation as well as an interaction with an indicator for the first quarter of each calendar year. The corresponding 95% confidence intervals are based on standard errors that are double-clustered at the firm and analyst 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 2000Q1 and 2018Q1, 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 | 72,732 | 0.026 | 0.387 | 0.000 | 0.000 | 0.000 |
| CAR(−1,+1) | 71,631 | -0.064 | 1.587 | 0.000 | 0.000 | 0.000 |
| CAR(−1,+1) excl. other events | 70,927 | -0.056 | 1.411 | 0.000 | 0.000 | 0.000 |
| CAR(−3,+3) | 71,631 | -0.106 | 2.346 | 0.000 | 0.000 | 0.000 |
| CAR(−3,+3) excl. other events | 70,108 | -0.078 | 1.931 | 0.000 | 0.000 | 0.000 |
| *Key Independent Variables* | | | | | | |
| Ideological mismatch | 76,969 | 0.370 | 0.483 | 0.000 | 0.000 | 1.000 |
| *Control Variables* | | | | | | |
| Tenure | 76,969 | 3.234 | 0.761 | 2.833 | 3.466 | 3.807 |
| No. of firms covered | 76,969 | 2.418 | 0.978 | 1.792 | 2.398 | 2.996 |
| Econ. Polarization Gallup | 62,015 | 0.001 | 0.986 | -0.866 | 0.114 | 0.615 |
| Low avg. election gap | 74,678 | 0.263 | 0.440 | 0.000 | 0.000 | 1.000 |
| Frequent voter | 76,969 | 0.782 | 0.413 | 1.000 | 1.000 | 1.000 |
| Age | 76,969 | 3.852 | 0.204 | 3.738 | 3.892 | 4.007 |
| Market beta | 49,626 | 1.179 | 0.669 | 0.709 | 1.093 | 1.537 |
| Leverage | 65,677 | 0.330 | 0.231 | 0.179 | 0.291 | 0.431 |
| Size | 65,705 | 8.820 | 1.607 | 7.707 | 8.765 | 9.826 |
| Cash | 65,692 | 0.084 | 0.095 | 0.017 | 0.052 | 0.117 |
| Avg. past rating | 74,925 | 10.918 | 3.824 | 8.000 | 10.000 | 14.000 |
| Tobin's Q | 53,993 | 1.541 | 0.741 | 1.065 | 1.316 | 1.764 |
| Revenue growth | 63,297 | 0.017 | 0.135 | -0.048 | 0.011 | 0.079 |
| Asset growth | 64,356 | 0.008 | 0.044 | -0.016 | 0.006 | 0.028 |
| Cash flow | 59,878 | 0.017 | 0.025 | 0.009 | 0.018 | 0.029 |
| ROA | 64,430 | 0.006 | 0.026 | 0.001 | 0.008 | 0.017 |
| R&D | 65,333 | 0.002 | 0.007 | 0.000 | 0.000 | 0.000 |
| Capex | 63,978 | 0.031 | 0.040 | 0.006 | 0.017 | 0.039 |

Table 2: **Partisan Perception 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. All variables are defined in Appendix A.1. $t$-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the analyst and firm level.

| | **Rating Change** | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Ideological mismatch | 0.0168 | 0.0159 | 0.0134 |
| | (4.18) | (3.96) | (3.79) |
| Tenure | 0.0001 | 0.0004 | -0.0004 |
| | (0.03) | (0.15) | (-0.15) |
| No. of firms covered | -0.0001 | 0.0000 | -0.0000 |
| | (-0.10) | (0.02) | (-0.02) |
| Observations | 49,792 | 49,792 | 49,790 |
| $R^2$ | 0.804 | 0.806 | 0.808 |
| Firm × Quarter FE | Yes | Yes | Yes |
| Agency FE | Yes | No | No |
| Agency × Industry FE | No | Yes | No |
| Agency × Quarter FE | No | No | Yes |
| Party Affiliation FE | Yes | Yes | Yes |

Table 3: **Democratic vs. Republican vs. Unaffiliated Analysts**

This table repeats the analysis from Table 2 after replacing ideological mismatch with indicators for the analyst's party affiliation and an indicator for Democratic presidents. 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 analysis, but adds unaffiliated analysts, defined as all analysts who are classified as unaffiliated in the voter records. The coefficients on *Democrat* and *Republican* capture the difference relative to the base group of unaffiliated analysts. At the bottom of the table we report the results from an $F$-test that assesses whether the difference between Republican and Democratic analysts is statistically significant under Democratic and Republican presidents, respectively. $t$-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the analyst and firm level.

Panel A: Democratic vs. Republican Analysts

| | **Rating Change** | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Democrat | 0.0313 | 0.0267 | 0.0268 |
| | (3.88) | (3.44) | (3.79) |
| Democrat × DemPresident | -0.0365 | -0.0341 | -0.0299 |
| | (-3.85) | (-3.46) | (-3.60) |
| Tenure | 0.0060 | 0.0066 | 0.0058 |
| | (1.58) | (1.61) | (1.59) |
| No. of firms covered | 0.0001 | -0.0003 | -0.0002 |
| | (0.04) | (-0.20) | (-0.12) |
| Observations | 29,348 | 29,347 | 29,346 |
| $R^2$ | 0.798 | 0.800 | 0.803 |
| DemPresident $F$-stat | 1.48 | 2.56 | 0.58 |
| DemPresident $p$-value | 0.225 | 0.110 | 0.447 |
| Firm × Quarter FE | Yes | Yes | Yes |
| Agency FE | Yes | No | No |
| Agency × Industry FE | No | Yes | No |
| Agency × Quarter FE | No | No | Yes |

Panel B: Democratic and Republican vs. Unaffiliated Analysts

| | Rating Change | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Republican | -0.0162 | -0.0151 | -0.0138 |
| | (-2.88) | (-2.61) | (-2.52) |
| Republican × DemPresident | 0.0228 | 0.0207 | 0.0200 |
| | (3.32) | (2.92) | (3.00) |
| Democrat | 0.0117 | 0.0079 | 0.0089 |
| | (1.84) | (1.29) | (1.49) |
| Democrat × DemPresident | -0.0103 | -0.0104 | -0.0066 |
| | (-1.39) | (-1.41) | (-0.94) |
| Tenure | 0.0005 | 0.0009 | 0.0001 |
| | (0.21) | (0.35) | (0.02) |
| No. of firms covered | -0.0003 | -0.0003 | -0.0002 |
| | (-0.25) | (-0.20) | (-0.16) |
| Observations | 49,316 | 49,316 | 49,314 |
| $R^2$ | 0.806 | 0.808 | 0.810 |
| RepPresident $F$-stat | 16.39 | 12.20 | 13.71 |
| RepPresident $p$-value | 0.000 | 0.001 | 0.000 |
| DemPresident $F$-stat | 2.01 | 4.91 | 1.30 |
| DemPresident $p$-value | 0.157 | 0.027 | 0.254 |
| Firm × Quarter FE | Yes | Yes | Yes |
| Agency FE | Yes | No | No |
| Agency × Industry FE | No | Yes | No |
| Agency × Quarter FE | No | No | 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, *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); and 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), respectively, and zero otherwise. In Panel B, we use alternative definitions of ideological mismatch, using party affiliation from presidential elections only (first row), and adding party affiliation information from analysts' political contributions. In Panel C, *Triple clustering by analyst, firm, and quarter* refers to triple-clustering standard errors at the analyst, firm, and quarter level. *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 lagged book assets of the rated firm. *Add Analyst FE* adds analyst fixed effects. *Add NBER Recession* $\times$ *Party affiliation FE* replaces party affiliation fixed effects with party affiliation $\times$ recession dummy fixed effects. Recessions are defined following the National Bureau of Economic Research (NBER). $t$-statistics are based on standard errors that allow for double-clustering at the analyst and firm level (except for the value in row *Firm-agency level*, where standard errors are clustered at the firm level).

| | Coeff. | $t$-statistic |
|---|---|---|
| **Baseline** | 0.0134 | 3.79 |
| | | |
| **Panel A: Alternative dependent variables** | | |
| Rating change indicator | 0.0110 | 3.88 |
| Downgrade | 0.0064 | 3.24 |
| Upgrade | -0.0046 | -2.21 |
| | | |
| **Panel B: Alternative definitions of ideological mismatch** | | |
| Use only party affiliation from presidential elections | 0.0144 | 2.75 |
| Add party affiliation from political contributions | 0.0135 | 4.36 |
| | | |
| **Panel C: Estimation** | | |
| Triple-cluster standard errors (analyst, firm, and quarter) | 0.0134 | 4.33 |
| Firm-agency level | 0.0298 | 3.82 |
| Weighted least squares | 0.0125 | 3.35 |
| Add Analyst FE | 0.0108 | 2.40 |
| Add NBER Recession $\times$ Party affiliation FE | 0.0150 | 4.03 |

Table 5: **Interaction with Polarization of Economic Views**

This table regresses quarterly rating changes on ideological mismatch as well as interactions with a measure of political polarization of economic views *Econ. Polarization Gallup*. To measure political polarization in the views of economic conditions we use data from the Gallup Daily survey. To measure the views on current economic conditions, the Gallup survey asks the following question: "How would you rate economic conditions in this country today—as excellent, good, only fair, or poor?". The responses to this question are converted into a numerical scale that ranges from 1 (poor) to 4 (excellent). Based on this question our measure of polarization of economic views *Econ. Polarization Gallup* is the absolute average quarterly difference between Democrats and Republicans. We standardize the measure 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 double-clustering at the analyst and firm level.

| | | **Rating Change** | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Ideological mismatch | 0.0105 | 0.0099 | 0.0085 |
| | (2.02) | (1.96) | (1.89) |
| Mismatch × Econ. Polarization Gallup | 0.0087 | 0.0083 | 0.0053 |
| | (2.96) | (2.72) | (1.93) |
| Tenure | -0.0004 | -0.0003 | -0.0003 |
| | (-0.15) | (-0.09) | (-0.12) |
| No. of firms covered | -0.0001 | 0.0002 | 0.0003 |
| | (-0.08) | (0.16) | (0.22) |
| Observations | 45,041 | 45,041 | 45,041 |
| $R^2$ | 0.805 | 0.807 | 0.808 |
| Firm × Quarter FE | Yes | Yes | Yes |
| Agency FE | Yes | No | No |
| Agency × Industry FE | No | Yes | No |
| Agency × Quarter FE | No | No | Yes |
| Party Affiliation 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 for analysts who have voted for midterm or primary election in the past, and zero otherwise. *Low avg. election gap* is the indicator equal to one for analysts who are below the bottom quartile of average time gap (in quarters) between elections in which the analyst votes, and zero otherwise. *Frequent voter* is an indicator equal to one for analysts for whom *Low avg. election gap* is equal to one or *Votes in midterm or primary* is equal to one, and zero otherwise. *Age* is logarithm of age of the analyst as of the end of the quarter, and it is 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 double-clustering at the analyst and firm level.

| | | **Rating Change** | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Ideological mismatch | 0.0014 | 0.0091 | 0.0003 |
| | (0.21) | (2.24) | (0.04) |
| Votes in midterm or primary | 0.0008 | | |
| | (0.25) | | |
| Mismatch × Votes in midterm or primary | 0.0140 | | |
| | (2.21) | | |
| Low avg. election gap | | 0.0007 | |
| | | (0.22) | |
| Mismatch × Low avg. election gap | | 0.0138 | |
| | | (2.54) | |
| Frequent voter | | | -0.0013 |
| | | | (-0.40) |
| Mismatch × Frequent voter | | | 0.0151 |
| | | | (2.33) |
| Age | -0.0001 | -0.0009 | 0.0001 |
| | (-0.08) | (-0.57) | (0.05) |
| Mismatch × Age | -0.0007 | -0.0009 | -0.0009 |
| | (-0.25) | (-0.30) | (-0.33) |
| Tenure | -0.0004 | -0.0000 | -0.0004 |
| | (-0.15) | (-0.02) | (-0.15) |
| No. of firms covered | -0.0006 | -0.0002 | -0.0005 |
| | (-0.45) | (-0.13) | (-0.39) |
| Observations | 49,790 | 47,454 | 49,790 |
| $R^2$ | 0.808 | 0.808 | 0.808 |
| Firm × Quarter FE | Yes | Yes | Yes |
| Agency × Quarter FE | Yes | Yes | Yes |
| Party Affiliation FE | Yes | Yes | Yes |

Table 7: **Foreign Firms and Domestic Firms with Low Market Beta**

This table regresses quarterly rating changes on ideological mismatch using alternative samples. In column (1), we estimate our main regression on the sample of non-U.S. firms. In columns (2) and (3), we split the sample of domestic firms into firms with low and high market beta, where the market beta is estimated using the Fama and French (1993) and Carhart (1997) four-factor model, using monthly return data from CRSP with 5-year rolling window. $t$-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the analyst and firm level.

| | **Rating Change** | | |
|---|---|---|---|
| | Foreign Firms | Domestic Firms | |
| | | Low Beta | High Beta |
| | (1) | (2) | (3) |
| Ideological mismatch | 0.0016 | 0.0017 | 0.0176 |
| | (0.88) | (0.30) | (2.81) |
| Tenure | 0.0002 | 0.0024 | -0.0036 |
| | (0.18) | (0.67) | (-0.80) |
| No. of firms covered | -0.0001 | -0.0023 | 0.0016 |
| | (-0.21) | (-1.30) | (0.70) |
| Observations | 55,111 | 17,456 | 16,283 |
| $R^2$ | 0.936 | 0.770 | 0.815 |
| Firm $\times$ Quarter FE | Yes | Yes | Yes |
| Agency $\times$ Quarter FE | Yes | Yes | Yes |
| Party Affiliation FE | Yes | Yes | Yes |

Table 8: **Cumulative Abnormal Returns Around Rating Changes**

This table regresses cumulative abnormal stock returns around rating changes on ideological mismatch. Cumulative abnormal returns (CARs) are measured in percent and calculated using the Fama and French (1993) and Carhart (1997) model estimated over trading days (-300,-50) and are measured over an event window of (-1,+1) (columns (1) and (2)) and (-3,+3) (columns (3) and (4)), respectively. In quarters with no rating change, the dependent variable is set to zero. In columns (2) and (4), we exclude rating changes where a corporate earnings announcement or M&A announcement falls inside the event window. All variables are defined in Appendix A.1. $t$-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the analyst and firm level.

| | **CAR(−1,+1)** | | **CAR(−3,+3)** | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Ideological mismatch | -0.0321 | -0.0322 | -0.0458 | -0.0386 |
| | (-1.98) | (-2.29) | (-2.04) | (-2.10) |
| Tenure | -0.0037 | -0.0047 | -0.0153 | -0.0071 |
| | (-0.28) | (-0.40) | (-0.76) | (-0.49) |
| No. of firms covered | 0.0003 | 0.0028 | -0.0016 | -0.0032 |
| | (0.06) | (0.52) | (-0.18) | (-0.42) |
| Observations | 48,147 | 47,580 | 48,147 | 46,934 |
| $R^2$ | 0.744 | 0.750 | 0.755 | 0.766 |
| Firm × Quarter FE | Yes | Yes | Yes | Yes |
| Agency × Quarter FE | Yes | Yes | Yes | Yes |
| Excluding corporate events | No | Yes | No | Yes |
| Party Affiliation FE | Yes | Yes | Yes | Yes |

# 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 for S&P and Fitch. 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. The variable is winsorized at the top and bottom 1% level. |
| CAR(–1,+1); CAR(–3,+3) | Cumulative abnormal returns (CARs) during trading days (–1,+1) and (–3,+3) around a rating change, computed using the Fama and French (1993) and Carhart (1997) model estimated over trading days (-300,-50) relative to the event date. CARs are aggregated over all rating changes for a given agency, firm, and quarter, and are set to zero when there is no rating change. We exclude rating actions with missing stock returns in the (–3,+3) window around the event. The variable is measured in percent and is winsorized at the top and bottom 1% level. |
| CAR(–1,+1) excl. other events; CAR(–3,+3) excl. other events | Cumulative abnormal returns are computed as described above, but we exclude rating changes where an earnings announcement or M&A announcement falls inside the respective event window. Earnings announcement dates are obtained from IBES and M&A announcement dates from SDC Platinum. |
| *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. Internet Appendix IA.A.1 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. |
| No. of firms covered | Logarithm of the number of firms rated by the analyst in a given quarter. |
| Votes in midterm or primary | An indicator equal to one for analysts who have voted for midterm or primary election in the past, and zero otherwise. |

*Continued on next page*

**Table A.1 – continued**

| Variable | Description |
|---|---|
| Low avg. election gap | The indicator equal to one for analysts who are below the bottom quartile of average time gap (in quarters) between elections in which the analyst votes, and zero otherwise. |
| Frequent voter | An indicator equal to one for analysts for whom *Avg. election gap* is below the bottom quartile or *Votes in midterm or primary* is equal to one, and zero otherwise. |
| Age | Logarithm of age of the analyst as of the end of the quarter. Analysts' birth dates are obtained from voter registration records. |
| Market beta | Beta estimated using the Fama and French (1993) and Carhart (1997) four-factor model, using monthly return data from CRSP with 5-year rolling window. |
| 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 for S&P and Fitch. 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. |

Table A.2: **Mismatch with Congress**

This table regresses quarterly rating changes on *Mismatch with Senate*, an indicator equal to one for analysts whose party affiliation does not match the party majority in the Senate, and zero otherwise; *Mismatch with House*, an indicator equal to one for analysts whose party affiliation does not match the party majority in the House, and zero otherwise; and *Mismatch with President*, our baseline definition of ideology mismatch based on the party affiliation of the president. $t$-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the analyst and firm level.

| | **Rating Change** | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Mismatch with Senate | 0.0071 | | 0.0000 | |
| | (2.55) | | (0.01) | |
| Mismatch with House | | 0.0031 | | 0.0014 |
| | | (0.90) | | (0.39) |
| Mismatch with President | | | 0.0134 | 0.0132 |
| | | | (3.14) | (3.71) |
| Tenure | -0.0004 | -0.0003 | -0.0004 | -0.0004 |
| | (-0.14) | (-0.13) | (-0.15) | (-0.14) |
| No. of firms covered | 0.0001 | 0.0001 | -0.0000 | -0.0000 |
| | (0.05) | (0.09) | (-0.02) | (-0.01) |
| Observations | 49,790 | 49,790 | 49,790 | 49,790 |
| $R^2$ | 0.808 | 0.808 | 0.808 | 0.808 |
| Firm × Quarter FE | Yes | Yes | Yes | Yes |
| Agency × Quarter FE | Yes | Yes | Yes | Yes |
| Party Affiliation FE | Yes | Yes | Yes | Yes |

Table A.3: **Event Study Around the 2016 Presidential Election**

This table regresses 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 its interaction with *Post Trump*, an indicator equal to one after the presidential election in 2016Q4, and zero otherwise. The sample is restricted to Democratic and Republican analysts, as well as to the time period from 2015Q1 to 2018Q1. $t$-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the analyst and firm level.

| | **Rating Change** | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Democrat | -0.0040 | -0.0156 | -0.0003 |
| | (-0.58) | (-2.32) | (-0.04) |
| Democratic × Post Trump | 0.0323 | 0.0370 | 0.0245 |
| | (2.12) | (2.47) | (1.99) |
| Tenure | 0.0013 | 0.0017 | 0.0013 |
| | (0.21) | (0.31) | (0.23) |
| No. of firms covered | 0.0031 | -0.0006 | 0.0033 |
| | (1.10) | (-0.22) | (1.15) |
| Observations | 7,916 | 7,908 | 7,916 |
| $R^2$ | 0.813 | 0.823 | 0.819 |
| Firm × Quarter FE | Yes | Yes | Yes |
| Agency FE | Yes | No | No |
| Agency × Industry FE | No | Yes | No |
| Agency × Quarter FE | No | No | Yes |

Table A.4: **Survey of Credit Rating Analysts**

This table shows the results from our online survey of credit rating analysts. We infer analysts' views on current economic conditions by asking the following question: "How would you rate economic conditions in this country today?", with possible answers of "excellent", "good", "fair", and "poor". We code the answers with numerical values from 1 to 4, with 1 indicating least favorable view and higher values representing more favorable views. The measure is standardized to have a mean of zero and a standard deviation of one. To infer the political affiliation of the analysts, we ask: "In politics, as of today, do you consider yourself a Republican, a Democrat, an Independent or Unaffiliated?" We classify a respondent's political affiliation as *Republican*, *Democrat*, or *Independent* (if she responded Independent or Unaffiliated). *Demographic controls* include gender, age and ethnicity. *Experience controls* is the self-reported number of years of work experience as a credit rating analyst. $t$-statistics, reported in parentheses, are based on robust standard errors.

| | **Current Economic Conditions** | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Democrat | 0.1778 | 0.1835 | 0.1913 |
| | (0.96) | (0.90) | (0.92) |
| Republican | 0.7333 | 0.6903 | 0.6877 |
| | (3.81) | (3.06) | (3.00) |
| Constant | 2.6000 | 2.6053 | 2.5616 |
| | (19.86) | (6.65) | (6.06) |
| Observations | 45 | 43 | 43 |
| $R^2$ | 0.251 | 0.302 | 0.303 |
| Demographic controls | No | Yes | Yes |
| Experience controls | No | No | Yes |

## Internet Appendix to

## “Partisan Professionals: Evidence from Credit Rating Analysts”

This internet appendix presents additional results to accompany the paper “Partisan Professionals: Evidence from Credit Rating Analysts.” The contents are as follows:

**Internet Appendix IA.A** describes the voter registration files used to obtain information on analysts’ political affiliation.

**Internet Appendix IA.B** provides summary statistics for the online survey of credit rating analysts that we conducted in the winter of 2018/2019.

**Internet Appendix IA.C** describes the measures of political polarization in the views of economic conditions used in the paper.

**Internet Appendix IA.D** presents additional analyses to accompany our main empirical results.

**Internet Appendix IA.E** presents our findings regarding analyst accuracy.

# IA.A. Information on Political Affiliation

## IA.A.1 Voter Registration Files

This section describes the voter registration files and merging procedure used to assign party affiliations to individual analysts. Table IA.1 summarizes the the ratios of voter's party affiliations by election type for all three voter files.

### IA.A.1.1 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.

### IA.A.1.2 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.

#### IA.A.1.3 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 IA.1). 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.

#### IA.A.1.4 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.[26] Second, if the age criterion does not allow us to determine a unique match, we use the distance between the zip code of the analyst's office location and the zip code of the voter address as a criterion. Specifically, we define a correct unique match if (i) the voter lives within a 50 miles radius from the rating agency and (ii) the second-nearest voter match is located more than 50 miles further away from the rating agency than the first voter. Third, for remaining analysts located in New York who match both to voter records from New York City and 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.

After removing analysts who match to multiple voters and for whom a unique match cannot be determined, as well as analysts whose implied age at the time of the rating is younger than 22 or older than 65 according to the age information in the voter record, we are able to match 557 analysts to a unique voter record.

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

### IA.A.2 Political Contributions

This section describes the political contribution data and merging procedure used to obtain additional information on the political leaning of individual analysts.

#### IA.A.2.1 Stanford's Database on Ideology, Money in Politics, and Elections (DIME)

Political contributions are obtained from Stanford's Database on Ideology, Money in Politics, and Elections (DIME) database, which contains local, state and federal-level contributions from individuals and organizations between 1979 and 2014. The DIME database includes information about contributors' zip codes as well as their employer and occupation. It relies on data from the Federal Election Commission (FEC), the National Institute on Money in State Politics, the New York City Campaign Finance Board, the Center for Responsive Politics, and the Internal Revenue Service. We restrict the sample to federal contributions. For a more detailed description of the DIME data set, see Bonica (2016).

[26] We are able to find age information for ca. 65% of the analysts with duplicate matches to voter records.

#### IA.A.2.2 Merging Analyst Data with Political Contributions

Analysts are matched to political contributions using a procedure similar to the one described in Hong and Kostovetsky (2012). Specifically, credit analysts are matched to contributions using first and last names applying the following filters:

1. Exclude contributions with contradicting middle names.

2. Exclude contributions outside the metropolitan area of the rating agency (i.e., more than 100 miles away from the credit rating agencys office).

3. Exclude contributions where the employer and occupation is unrelated to finance or rating agencies. We search for related strings such as "credit", "rating", "finance", "wealth", as well as for names of the main rating agencies and other large financial firms.

4. Exclude analysts that are matched to more than three different contributors, according to the contributor identifier provided in DIME.

Following the merging procedure described above, we are able to identify a federal-level contribution for 57 analysts. Using this information we create a quarterly panel of analysts' party affiliation, where Democratic analysts are defined as individuals who made most of their contributions to federal Democratic candidates. Republicans are defined analogously. An analyst is considered unaffiliated if none of the contributions by the analyst can be attributed to a specific political party.

In 72% of the analyst-quarter observations where we have a non-missing party affiliation from both the voter records and the contributions data, the party affiliations from both sources agree. Moreover, once we condition on Democratic and Republican affiliations only, the two sources agree in 96% of the cases.

Table IA.1: **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 | |

# IA.B. Online Survey of Credit Rating Analysts

Table IA.2: **Summary Statistics – Credit Analyst Survey**

This table summarizes the responses from our online survey of credit rating analysts. The sample consists of 58 responses from individuals who indicate that they have worked as credit rating analysts. The survey is described in more detail in Section **5.5.2** in the main paper.

| | N | Mean | SD |
|---|---|---|---|
| *Dependent Variables* | | | |
| Current Economic Conditions | 51 | -0.012 | 1.028 |
| *Years of Experience as a Credit Rating Analyst* | | | |
| <5 | 58 | 9% | |
| 5–10 | 58 | 26% | |
| 10–15 | 58 | 26% | |
| >15 | 58 | 40% | |
| *Party Affiliation* | | | |
| Democrat | 45 | 40% | |
| Republican | 45 | 27% | |
| Independent | 45 | 33% | |
| *Age Group* | | | |
| 25–34 | 45 | 4% | |
| 35–44 | 45 | 24% | |
| 45–54 | 45 | 29% | |
| 55–64 | 45 | 36% | |
| 65–74 | 45 | 4% | |
| 75–84 | 45 | 2% | |
| *Ethnic Origin* | | | |
| Asian | 44 | 16% | |
| African American | 44 | 2% | |
| White | 44 | 80% | |
| Other | 44 | 2% | |
| *Gender* | | | |
| Female | 44 | 30% | |
| Male | 44 | 70% | |

## IA.C. Measures of Political Polarization in the Views of Economic Conditions

Our main measure of political polarization in the views of economic conditions is based on the Gallup Daily survey by Gallup, Inc. We also construct an alternative measure based on the Thomson Reuters University of Michigan Survey of Consumers. The Gallup Daily survey is nationally representative and covers around 1,000 individuals every day for years 2008 to 2017. To measure the views on current economic conditions, the Gallup survey asks the following question: "How would you rate economic conditions in this country today — as excellent, good, only fair, or poor?". The responses to this question are converted into a numerical scale that ranges from 1 (poor) to 4 (excellent). Moreover, the Gallup survey contains two question about political affiliation, which allows to classify survey respondents into Democrats, Republicans, or Independents. The first question asks: "In politics, as of today, do you consider yourself a Republican, a Democrat, or an Independent?". If the individual answers *Republican* or *Democrat*, no further question is asked regarding her party affiliation. If the individual answers *Independent*, *Other*, or refuses to answer, then she is asked a second question: "As of today, do you lean more to the Democratic Party or the Republican Party?". The individual can answer *Republican* or *Democrat* to this question. We follow Mian, Sufi, and Khoshkhou (2017) and classify an individual as Republican if the individual answers either of these questions *Republican*, and Democrat if the individual answers either of these questions *Democrat*. The remaining individuals are classified as Independents. Our measure of political polarization in economic views is the absolute difference in the average economic views of Democrats and Republicans in a given calendar quarter.

The Michigan Survey covers around 500 individuals every month and is nationally representative. We use the Current Economic Conditions Index from the Michigan Survey to capture views of economic conditions. This index is a slightly adjusted average of the answers to two different questions meant to capture the views of individuals on current economic conditions. The first question is: "We are interested in how people are getting along financially these days. Would you say that you (and your family living there) are better off or worse off financially than you were a year ago?". The second question is: "About the big things people buy for their homes such as furniture, a refrigerator, stove, television, and things like that. Generally speaking, do you think now is a good time or a bad time for people to buy major household items?". To classify the political affiliation of the respondents, the survey asks the following question: "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent or what?", with possible answers being "Democrat", "Republican", or "Independent". However, the survey only asked the question on political affiliation in the following months: June 1980, January 1984, July 1984, January 1985, April 1985, May 1985, September through November 2006, March 2008 through June 2009, March 2010 through November 2010, April 2012, May 2012, September through November 2012, June 2014, June 2015, June through October 2016, and February and March of 2017. Given that the Michigan Survey does not ask the political affiliation between years 1985 and 2006, we use data from the Michigan Survey starting in 2006. For quarters with one or two consecutive missing values, we impute them by using the average of two non-missing quarters around these missing quarters. As with the Gallup Survey, our main measure of political polarization is the absolute difference in the average Current Economic Conditions Index between Democrats and Republicans in a given calendar quarter.

We standardize both variables to have a mean of zero and a standard deviation of one. We plot the time-series of our two measures of polarization in economic views in Figure IA.1.

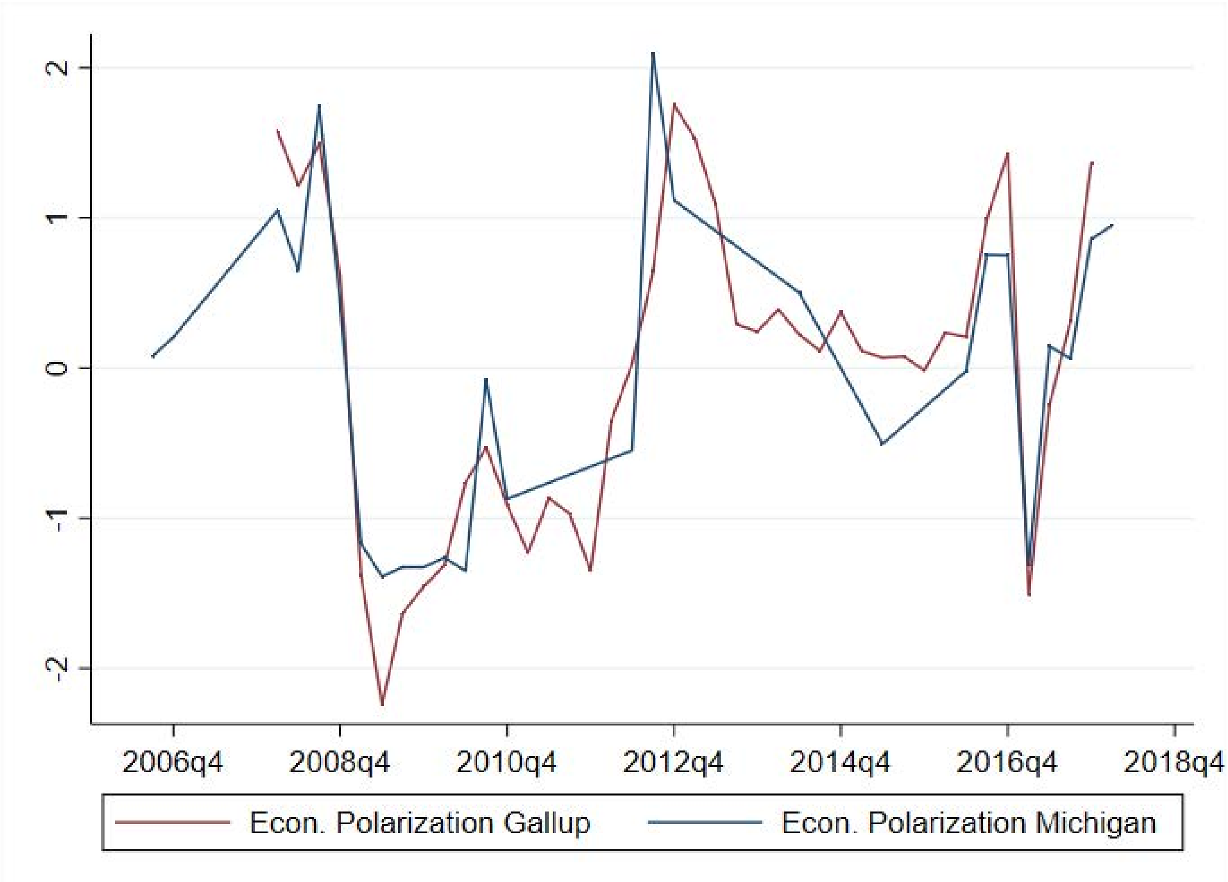



Figure IA.1: **Political Polarization in the Views of Economic Conditions.** The figure plots the time-series of our two measures of political polarization in economic views. *Econ. Polarization Gallup* refers to the difference in economic views between Democrats and Republicans based on data from the Gallup Daily Survey. *Econ. Polarization Michigan* is based on the Current Economic Conditions Index from the Michigan Survey. Both measures are standardized to have a mean of zero and a standard deviation of one.

# IA.D. Additional Analyses

Table IA.3: **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 analyst and firm level.

| | **Registered Voter** | |
|---|---|---|
| | (1) | (2) |
| Leverage | 0.0011 | -0.0026 |
| | (0.12) | (-0.19) |
| Size | 0.0239 | 0.0242 |
| | (2.07) | (1.87) |
| Cash | -0.0044 | -0.0045 |
| | (-0.82) | (-0.64) |
| Avg. past rating | -0.0088 | 0.0133 |
| | (-0.58) | (1.08) |
| Tobin's Q | 0.0084 | 0.0206 |
| | (1.41) | (2.09) |
| Revenue growth | -0.0008 | -0.0005 |
| | (-0.70) | (-0.26) |
| Asset growth | -0.0009 | -0.0040 |
| | (-0.48) | (-1.49) |
| ROA | -0.0343 | -0.0382 |
| | (-2.64) | (-2.13) |
| R&D | -0.0043 | -0.0171 |
| | (-0.54) | (-1.72) |
| Capex | 0.0066 | 0.0069 |
| | (0.95) | (0.49) |
| Cash flow | 0.0331 | 0.0343 |
| | (2.36) | (1.78) |
| Tenure | 0.0565 | 0.0826 |
| | (3.08) | (3.87) |
| No. of firms covered | -0.0318 | -0.0440 |
| | (-1.10) | (-1.43) |
| Observations | 113,953 | 114,074 |
| $R^2$ | 0.188 | 0.135 |
| Industry × Quarter FE | Yes | No |
| Agency × Quarter FE | No | Yes |

Table IA.4: **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. *Prob. Hispanic*, *Prob. Black* and *Prob. Asian* are the probabilities that the analyst's race/ethnicity is Hispanic or Latino, black or African American, and Asian or Native Hawaiian or other Pacific Islander, respectively. The probability is inferred based on the analyst's first and last names using the API `name-prism.com`. *Female* is an 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 `api.genderize.io`, as well as from manual online searches. $t$-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 | -0.4299 | -0.4084 |
| | (-2.36) | (-2.29) |
| Prob. Black | 0.0574 | 0.0324 |
| | (0.17) | (0.10) |
| Prob. Asian | -0.1004 | -0.0925 |
| | (-1.20) | (-1.09) |
| Female | -0.0268 | -0.0287 |
| | (-0.56) | (-0.59) |
| Tenure | 0.0684 | 0.0844 |
| | (3.16) | (3.55) |
| No. of firms covered | -0.0412 | -0.0467 |
| | (-1.60) | (-1.78) |
| Observations | 178,985 | 178,985 |
| $R^2$ | 0.423 | 0.430 |
| Industry × Quarter FE | Yes | No |
| Agency × Quarter FE | No | Yes |

*Continued on next page*

Panel B: Democratic vs. Republican Analysts

| | Democratic | |
|---|---|---|
| | (1) | (2) |
| Prob. Hispanic | -0.4886 | -0.4242 |
| | (-1.30) | (-1.31) |
| Prob. Black | 1.2096 | 1.1935 |
| | (7.35) | (6.81) |
| Prob. Asian | 0.1720 | 0.1492 |
| | (1.73) | (1.64) |
| Female | 0.4236 | 0.3918 |
| | (6.13) | (5.75) |
| Age | 0.0016 | 0.0034 |
| | (0.44) | (0.84) |
| Tenure | -0.0434 | -0.0476 |
| | (-1.16) | (-1.25) |
| No. of firms covered | -0.0750 | -0.0580 |
| | (-1.55) | (-1.14) |
| Observations | 30,569 | 30,569 |
| $R^2$ | 0.607 | 0.635 |
| Industry × Quarter FE | Yes | No |
| Agency × Quarter FE | No | Yes |

Table IA.5: **Baseline Result: 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. $t$-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the analyst and firm level.

| | **Rating Change** | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Ideological mismatch | 0.0145 | 0.0134 | 0.0124 |
| | (3.49) | (3.19) | (3.23) |
| No. of firms covered | -0.0020 | -0.0025 | -0.0013 |
| | (-0.57) | (-0.73) | (-0.41) |
| Tenure × DemPresident | -0.0130 | -0.0135 | -0.0115 |
| | (-2.22) | (-2.31) | (-2.02) |
| No. of firms covered × DemPresident | 0.0029 | 0.0036 | 0.0019 |
| | (0.76) | (0.96) | (0.55) |
| Prob. Hispanic × DemPresident | 0.0057 | 0.0072 | 0.0096 |
| | (0.16) | (0.22) | (0.30) |
| Prob. Black × DemPresident | -0.0408 | -0.0490 | -0.0388 |
| | (-1.20) | (-1.40) | (-1.18) |
| Prob. Asian × DemPresident | -0.0018 | -0.0014 | 0.0051 |
| | (-0.13) | (-0.09) | (0.39) |
| Female × DemPresident | -0.0094 | -0.0093 | -0.0045 |
| | (-1.17) | (-1.19) | (-0.60) |
| Age × DemPresident | 0.0008 | 0.0008 | 0.0007 |
| | (2.38) | (2.44) | (2.09) |
| Observations | 49,232 | 49,232 | 49,230 |
| $R^2$ | 0.801 | 0.803 | 0.805 |
| Firm × Quarter FE | Yes | Yes | Yes |
| Agency FE | Yes | No | No |
| Agency × Industry FE | No | Yes | No |
| Agency × Quarter FE | No | No | Yes |
| Party Affiliation FE | Yes | Yes | Yes |
| Analyst Characteristics | Yes | Yes | Yes |

Table IA.6: **Baseline Result: Include Unregistered Analysts**

This table repeats the analysis from Table 3, Panel B, after adding unregistered analysts and treating them as unaffiliated. The coefficients on *Democrat* and *Republican* capture the difference relative to the base group of unaffiliated and unregistered analysts. At the bottom of the table we report the results from an $F$-test that assesses whether the difference between Republican and Democratic analysts is statistically significant under Democratic and Republican presidents, respectively. $t$-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the analyst and firm level.

| | **Rating Change** | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Republican | -0.0096 | -0.0090 | -0.0076 |
| | (-3.16) | (-2.83) | (-2.71) |
| Republican × DemPresident | 0.0138 | 0.0124 | 0.0113 |
| | (3.24) | (2.91) | (2.86) |
| Democrat | 0.0030 | 0.0006 | 0.0033 |
| | (0.94) | (0.18) | (1.08) |
| Democrat × DemPresident | -0.0036 | -0.0024 | -0.0027 |
| | (-0.89) | (-0.61) | (-0.69) |
| Tenure | 0.0002 | 0.0004 | 0.0000 |
| | (0.11) | (0.25) | (0.02) |
| No. of firms covered | -0.0002 | -0.0005 | -0.0002 |
| | (-0.32) | (-0.74) | (-0.35) |
| Observations | 167,161 | 167,161 | 167,159 |
| $R^2$ | 0.793 | 0.793 | 0.795 |
| RepPresident $F$-stat | 8.52 | 4.78 | 7.45 |
| RepPresident $p$-value | 0.004 | 0.029 | 0.007 |
| DemPresident $F$-stat | 2.96 | 4.33 | 1.34 |
| DemPresident $p$-value | 0.086 | 0.038 | 0.247 |
| Firm × Quarter FE | Yes | Yes | Yes |
| Agency FE | Yes | No | No |
| Agency × Industry FE | No | Yes | No |
| Agency × Quarter FE | No | No | Yes |

Table IA.7: **Interaction with Polarization of Economic Views: Alternative Measure**

This table repeats the analysis presented in Table 5 using an alternative measure of political polarization in the views of economic conditions. *Econ. Polarization Michigan* refers to the difference in economic views between Democrats and Republicans based on the Current Economic Conditions Index from the Michigan Survey. The index is standardized to have a mean of zero and a standard deviation of one. Section IA.C provides more details on the survey questions. $t$-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the analyst and firm level.

| | **Rating Change** | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Ideological mismatch | 0.0122 | 0.0107 | 0.0111 |
| | (2.40) | (2.20) | (2.42) |
| Mismatch × Econ. Polarization Michigan | 0.0085 | 0.0091 | 0.0057 |
| | (2.04) | (2.06) | (1.35) |
| Tenure | 0.0012 | 0.0014 | 0.0014 |
| | (0.36) | (0.41) | (0.43) |
| No. of firms covered | -0.0014 | -0.0013 | -0.0011 |
| | (-0.63) | (-0.66) | (-0.50) |
| Observations | 25,107 | 25,103 | 25,107 |
| $R^2$ | 0.813 | 0.817 | 0.815 |
| Firm × Quarter FE | Yes | Yes | Yes |
| Agency FE | Yes | No | No |
| Agency × Industry FE | No | Yes | No |
| Agency × Quarter FE | No | No | Yes |
| Party Affiliation FE | Yes | Yes | Yes |

Table IA.8: **Cumulative abnormal stock returns around rating-change announcements**

This table regresses cumulative abnormal stock returns around downgrades (Panels A and B) and upgrades (Panels C and D) on ideological mismatch. Cumulative abnormal returns (CARs) are measured in percent and calculated using the Fama and French (1993) and Carhart (1997) model estimated over trading days (-300,-50) and are measured over an event window of (-1,+1) (Panels A and C) and (-3,+3) (Panels B and D), respectively. In columns (3) and (4), we exclude rating changes where a corporate earnings announcement or M&A announcement falls inside the event window. In all regressions we control for the log of the firm's total book assets, leverage, Tobin's Q, and cash holdings, as well as for the analysts' party affiliations. All variables are defined in Appendix A.1. $t$-statistics, reported in parentheses, are based on standard errors that allow for clustering at the calendar date level.

Panel A: CAR(–1,+1) for Downgrades

| | **CAR(–1,+1)** | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Ideological mismatch | 0.0085 | 0.0073 | 0.0069 | 0.0069 |
| | (1.48) | (1.09) | (0.97) | (0.97) |
| Rating change | -0.0101 | -0.0102 | -0.0108 | -0.0108 |
| | (-2.52) | (-2.25) | (-2.31) | (-2.31) |
| Observations | 1,871 | 1,741 | 1,561 | 1,561 |
| $R^2$ | 0.140 | 0.256 | 0.271 | 0.271 |
| Month FE | Yes | No | Yes | No |
| Agency FE | Yes | No | Yes | No |
| Agency × Month FE | No | Yes | No | Yes |
| Excluding corporate events | No | No | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |

Panel B: CAR(–3,+3) for Downgrades

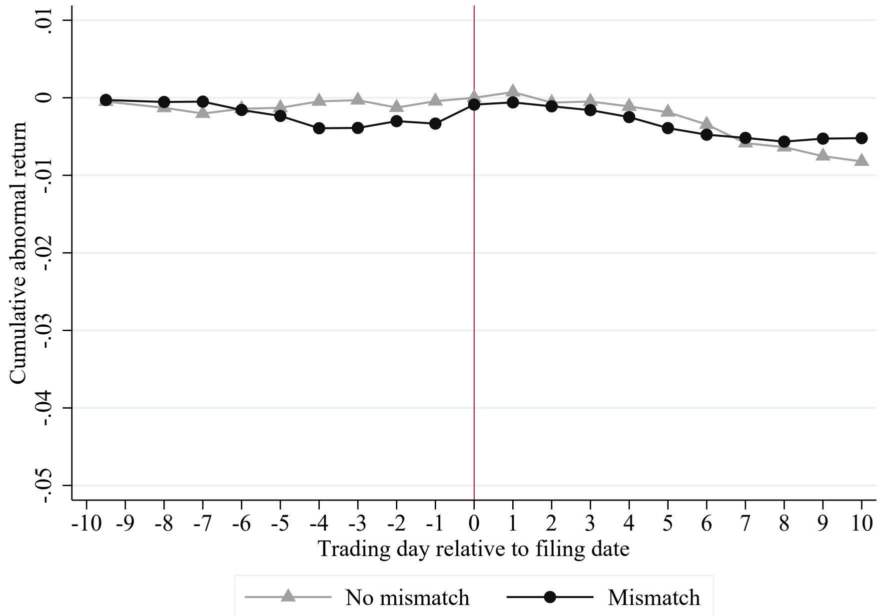
| | **CAR(–3,+3)** | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Ideological mismatch | -0.0012 | -0.0019 | -0.0069 | -0.0093 |
| | (-0.15) | (-0.19) | (-0.78) | (-0.83) |
| Rating change | -0.0177 | -0.0165 | -0.0214 | -0.0191 |
| | (-3.09) | (-2.46) | (-3.36) | (-2.53) |
| Observations | 1,871 | 1,741 | 1,543 | 1,412 |
| $R^2$ | 0.154 | 0.236 | 0.167 | 0.245 |
| Month FE | Yes | No | Yes | No |
| Agency FE | Yes | No | Yes | No |
| Agency × Month FE | No | Yes | No | Yes |
| Excluding corporate events | No | No | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |

Panel C: CAR(−1,+1) for Upgrades

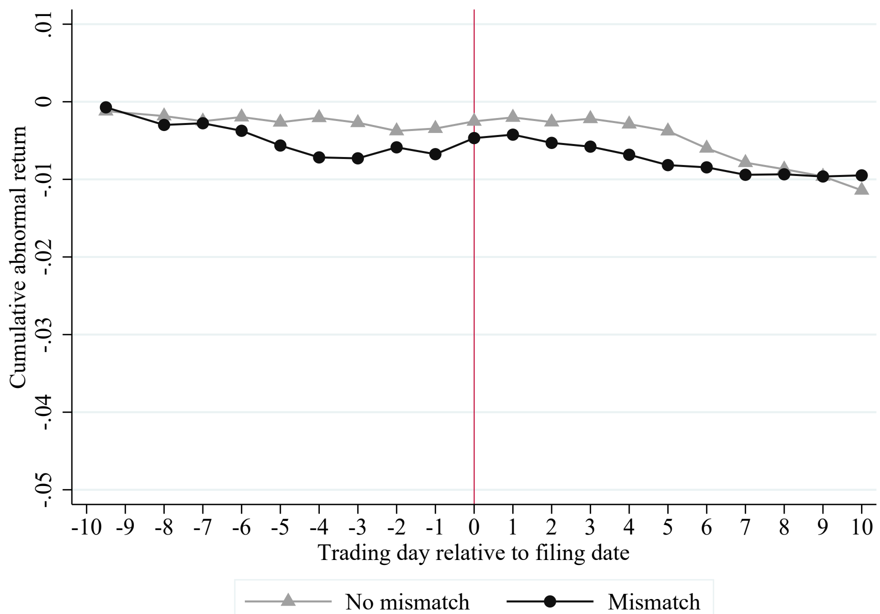
| | CAR(−1,+1) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Ideological mismatch | 0.0011 | 0.0001 | 0.0013 | -0.0003 |
| | (0.41) | (0.04) | (0.48) | (-0.10) |
| Rating change | 0.0020 | -0.0020 | 0.0013 | -0.0015 |
| | (0.54) | (-0.48) | (0.36) | (-0.37) |
| Observations | 1,438 | 1,326 | 1,363 | 1,248 |
| $R^2$ | 0.182 | 0.270 | 0.174 | 0.266 |
| Month FE | Yes | No | Yes | No |
| Agency FE | Yes | No | Yes | No |
| Agency × Month FE | No | Yes | No | Yes |
| Excluding corporate events | No | No | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |

Panel D: CAR(−3,+3) for Upgrades

| | CAR(−3,+3) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Ideological mismatch | 0.0024 | 0.0007 | 0.0054 | 0.0032 |
| | (0.54) | (0.14) | (1.16) | (0.57) |
| Rating change | -0.0024 | -0.0058 | -0.0030 | -0.0038 |
| | (-0.47) | (-0.89) | (-0.56) | (-0.55) |
| Observations | 1,438 | 1,326 | 1,266 | 1,144 |
| $R^2$ | 0.165 | 0.245 | 0.168 | 0.245 |
| Month FE | Yes | No | Yes | No |
| Agency FE | Yes | No | Yes | No |
| Agency × Month FE | No | Yes | No | Yes |
| Excluding corporate events | No | No | Yes | Yes |
| Control variables | Yes | Yes | Yes | Yes |

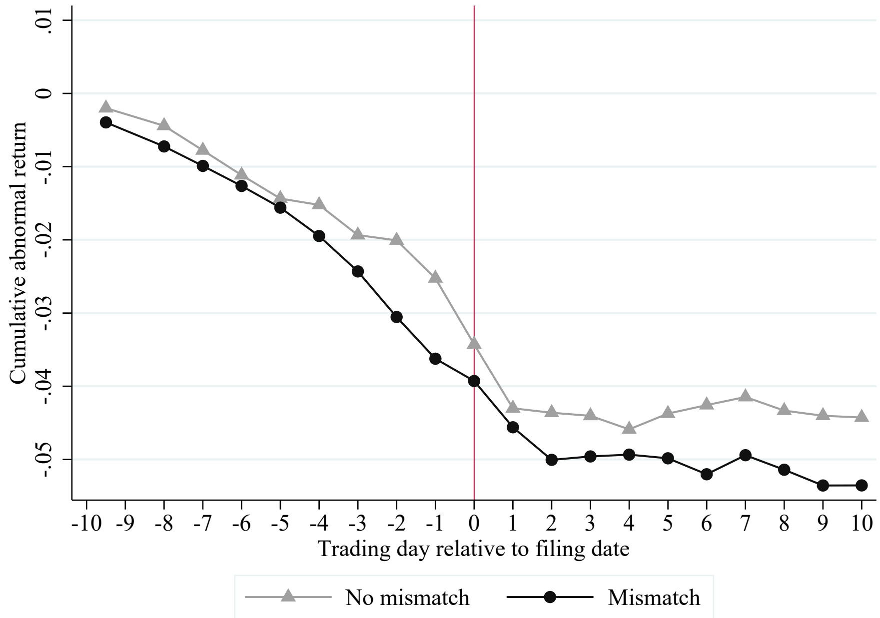



(a) All Upgrades

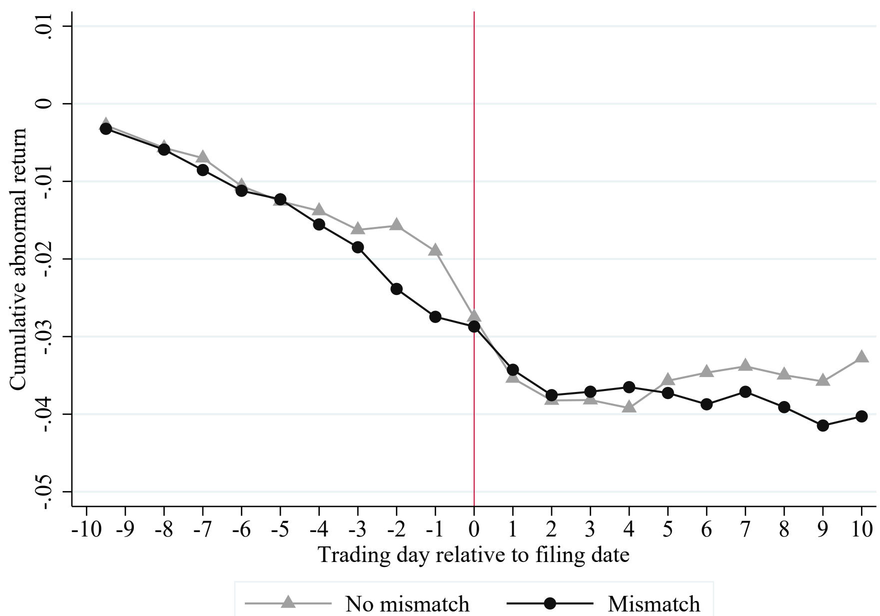



(b) Excluding Concurrent Earnings and M&A Announcements

Figure IA.2: **Cumulative Abnormal Stock Returns Around Upgrades**. The figure plots cumulative abnormal returns around credit rating upgrades. Abnormal returns are calculated using the Fama and French (1993) and Carhart (1997) model estimated over trading days (-300,-50) relative to the event date. In Panel (b), we exclude all rating changes where a corporate earnings announcement or an M&A announcement falls inside the (-10,+10) window around the rating change.




(a) All Downgrades




(b) Excluding Concurrent Earnings and M&A Announcements

Figure IA.3: **Cumulative Abnormal Stock Returns Around Downgrades**. The figure plots cumulative abnormal returns around credit rating downgrades. Abnormal returns are calculated using the Fama and French (1993) and Carhart (1997) model estimated over trading days (-300,-50) relative to the event date. In Panel (b), we exclude all rating changes where a corporate earnings announcement or an M&A announcement falls inside the (-10,+10) window around the rating change.

## IA.E. Analyst Accuracy

The results in the main paper suggest that analysts make different rating adjustments for the same firm at the same point in time depending on whether the White House is controlled by the party they are affiliated with. One consequence of this result is that partisan perception affects credit rating accuracy, since these rating differences are not driven by firm fundamentals. Rating accuracy, in turn, is important because it has been shown to determine analysts' labor market outcomes (e.g., Kisgen, Nickerson, Osborn, and Reuter (2017); Kempf (2018)).

Empirically estimating the degree of distortion in analyst accuracy is challenging for a number of reasons. First, there is no clear benchmark of analysts who are not subject to partisan perception. Second, measuring analyst accuracy is challenging because the firm's true probability of default and expected loss given default are unobservable. Third, determining systematic differences across analysts in their ex-post rating accuracy requires a long time-series of election cycles.

In order to at least partially address these challenges, we use the group of unaffiliated and unregistered analysts as an unbiased benchmark and propose a measure of rating 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 ($s$):

$$Accuracy_{ift} = \Delta R_{ift} \times (s_{f,t+h} - s_{ft}). \tag{4}$$

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 analysts' party affiliation 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 growth and asset growth, cash flow, ROA, R&D, and Capex.

Table IA.9 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 estimates in column (4) suggests that

Republicans are 0.09 percentage points more accurate than unaffiliated and unregistered analysts during Republican presidencies, and 0.10 (=0.0009–0.0019) percentage points less accurate during Democratic presidencies. This corresponds to 5.26% (=0.0009/0.0171) and 5.85% (=0.0010/0.0171) relative to one standard deviation in analyst accuracy measured over eight quarters, respectively.

Overall, the results reported in Table IA.9 indicate that analysts are less accurate when they are misaligned with the president. Moreover, consistent with the findings in Table IA.6, we find again that the differences between Republicans and unaffiliated/unregistered are larger than the differences between Democrats and unaffiliated/unregistered analysts.

While we find this test informative, it comes with a number of caveats. First, it relies on the assumption that unaffiliated and unregistered analysts represent a group that is not influenced by partisan perception. This assumption is likely violated because some analysts may be unregistered or registered as unaffiliated and still hold partisan views. Therefore, it is difficult to disentangle whether the accuracy of Republican analysts changes more with the identity of the president than that of Democrats, or whether the benchmark of unaffiliated and unregistered analysts behaves more similarly to Democrats. Second, our measure of accuracy is subject to the caveat that credit spreads might be directly affected by the rating change. In the test above, we are implicitly assuming that eventually credit spreads will revert if the rating action that triggered an initial change in credit spreads turns out to be inaccurate ex-post. Third, since we are no longer able to include firm $\times$ quarter fixed effects, we cannot fully address the problem of non-random matching of analysts to firms.

Table IA.9: **Analyst Accuracy**

This table regresses rating action accuracy on indicators for the analyst's party affiliation and an indicator for Democratic presidents. 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. *Republican* (*Democrat*) is an indicator equal to one for analysts who are affiliated with the Republican (Democratic) Party, and zero otherwise. The omitted base group is the group of unaffiliated and unregistered analysts. *Firm Characteristics* include leverage, size, cash holdings, Tobin's Q, revenue growth, asset growth, cash flow, ROA, R&D expense and Capex. $t$-statistics, reported in parentheses, are based on standard errors that allow for double-clustering at the analyst and firm level.

| | **Accuracy** | | | |
|---|---|---|---|---|
| | 1Q | 2Q | 4Q | 8Q |
| | (1) | (2) | (3) | (4) |
| Republican | 0.0008 | 0.0013 | 0.0012 | 0.0009 |
| | (2.35) | (2.34) | (2.31) | (1.35) |
| Republican × DemPresident | -0.0007 | -0.0015 | -0.0020 | -0.0019 |
| | (-2.02) | (-2.54) | (-3.65) | (-2.64) |
| Democrat | 0.0000 | 0.0001 | -0.0002 | -0.0003 |
| | (0.28) | (0.53) | (-0.68) | (-0.66) |
| Democrat × DemPresident | -0.0001 | -0.0001 | 0.0003 | 0.0005 |
| | (-0.34) | (-0.56) | (0.57) | (0.80) |
| Tenure | -0.0002 | -0.0003 | 0.0000 | 0.0007 |
| | (-1.45) | (-1.23) | (0.11) | (1.99) |
| No. of firms covered | 0.0001 | 0.0002 | 0.0002 | 0.0001 |
| | (1.54) | (1.60) | (1.80) | (0.93) |
| Observations | 37,557 | 36,144 | 33,509 | 28,577 |
| $R^2$ | 0.209 | 0.230 | 0.251 | 0.244 |
| Agency FE | Yes | Yes | Yes | Yes |
| Industry × Quarter FE | Yes | Yes | Yes | Yes |
| Firm Characteristics | Yes | Yes | Yes | Yes |