Competition, Visibility, and Partisan Bias in Sell-Side Equity Research

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April 7, 2026

Abstract

Prior work documents that partisan identity distorts the economic judgments of financial professionals in private settings, including credit ratings, loan pricing, and mutual fund allocation, where accuracy is difficult to observe contemporaneously and competitive pressure from peers is limited. I ask whether partisan bias survives in a setting with the opposite institutional features, using voter registration data matched to sell-side equity analyst forecasts for S&P 500 firms from 1992 to 2020. Consistent with prior work, misaligned analysts issue more pessimistic earnings forecasts. But the bias is systematically disciplined along every competitive margin I measure. Analysts in battleground states exhibit no measurable bias, with misaligned pessimism and aligned optimism compressed symmetrically toward the center. Long-run political diversity among analysts covering a firm attenuates individual bias, while persistent unanimous coverage amplifies it. Within brokerages, contemporaneous diversity among coworkers disciplines bias and persistent institutional homogeneity amplifies it. Elevated aggregate political conflict attenuates rather than amplifies the bias, the reverse of the pattern documented in private credit markets. The results locate a boundary condition for the partisan-bias-in-finance literature: the bias is real, but its magnitude depends on whether the institutional setting exposes it to competitive correction.

Keywords: partisan bias, sell-side analysts, earnings forecasts, competition, institutional visibility, political polarization, peer effects

JEL Codes: G14, G24, D72, D83

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1 Introduction

Partisan identity distorts the economic judgments of financial professionals, but the magnitude and direction of that distortion appear to depend on where the professionals work. Credit rating analysts issue more negative ratings when misaligned with the president (Kempf and Tsoutsoura, 2021), loan officers charge higher spreads (Dagostino, Gao, and Ma, 2023), corporate executives overinvest when aligned (Rice, 2023), and mutual fund managers tilt portfolios toward cyclical industries under same-party presidents (Cassidy and Vorsatz, 2024). What unites these settings is that they are all largely private: decisions are made in bilateral negotiations or within the walls of a single institution, accuracy is difficult to observe contemporaneously, and competitive pressure from peers producing rival judgments is limited or absent. This paper asks whether partisan bias survives in a setting with the opposite institutional features — one in which judgments are public, attributable, continuously benchmarked against peers, and subject to reputational consequences — and whether the competitive visibility of that setting disciplines the bias.

I study sell-side equity analysts, for whom these institutional features are defining. Analysts issue named, publicly disseminated earnings forecasts that are compared against realized earnings and against the forecasts of competing analysts covering the same firm. Accuracy is tracked, reported by commercial data vendors, and tied to career outcomes. If the private-setting findings in prior work reflect a universal behavioral pull of partisan identity, that same pull should produce measurable bias in analyst forecasts. If instead the competitive and visible structure of the setting disciplines partisan judgment, the bias should be attenuated where competition and visibility are strongest and amplified where they are weakest. I test this proposition using voter registration data from LexisNexis Public Records matched to analyst earnings forecasts for S&P 500 firms from 1992 to 2020, assembling a dataset of 10,027 forecasts issued by 224 analysts at 109 brokerage houses covering 669 firms.

Consistent with Fu, Ma, and Peng (2024), who document partisan bias in analyst forecasts using an independent L2 voter registration sample, I find that analysts whose party affiliation does not match the president's party issue significantly more pessimistic earnings forecasts. In the fully saturated specification with firm $\times$ fiscal quarter, broker, year-quarter, and analyst fixed effects, the coefficient on misalignment is -0.159 and significant at the 1% level, indicating that misaligned analysts produce forecast errors approximately 0.16 percentage points lower than aligned analysts evaluating the same firm in the same fiscal quarter. That the two papers reach the same qualitative conclusion using independently constructed samples — Fu, Ma, and Peng (2024) through automated L2 matching, and mine through a hand-collected validation procedure combining LexisNexis records with LinkedIn profiles, broker websites, the WayBack Machine, and SEC licensing — substantially strengthens confidence in the underlying phenomenon. The contribution of this paper, however, is not to redocument the baseline effect but to ask a different question: given that partisan bias exists in analyst forecasts, what features of the institutional environment discipline it?

I argue that the answer is competitive visibility, and I test three channels through which it operates. The first is between-party competition in the analyst's political geography. The second is between-broker competition among analysts covering the same firm. The third is within-broker competition among colleagues inside the same institution. Each channel isolates a different competitive margin, and each produces evidence consistent with the hypothesis that visible competition disciplines partisan bias.

My first and strongest result concerns geographic political competition. Analysts located in battleground states — states in which the most recent presidential election margin was five percentage points or less — exhibit no measurable partisan bias. Under the Electoral College system, presidential campaigns concentrate advertising, appearances, and mobilization in these competitive states (Hill, Rodriguez, and Wooden, 2010), producing an information environment in which residents are exposed to sustained cross-partisan messaging from both sides (Martin and Yurukoglu, 2017; DellaVigna and Kaplan, 2007). The interaction between misalignment and battleground state residence is 0.273 and significant at the 1% level, fully offsetting the baseline misalignment effect. A symmetric test using an alignment indicator produces an interaction of -0.168, also significant, indicating that the attenuation operates on both sides of the partisan distribution: battleground residence disciplines aligned optimism and misaligned pessimism in equal measure. This finding is consistent with Becker (1971), who argues that competitive environments reduce discrimination, and it provides the cleanest evidence in the paper that exposure to political competition disciplines partisan economic judgment.

Second, I examine whether the political composition of the analysts covering a particular firm disciplines individual bias. Heterogeneous groups are predicted to outperform homogeneous groups by bringing varied perspectives to shared problems (Hong and Page, 2004), but in the analyst setting the mechanism operates through competitive observation rather than collaborative deliberation. Sell-side earnings forecasts submitted to I/B/E/S are point estimates without accompanying written justifications, so analysts cannot learn from one another's reasoning; they can only observe one another's forecast levels and, critically, the realized accuracy of those forecasts once actual earnings are announced. Learning in this setting is therefore Bayesian updating about peer accuracy, which by construction requires repeated observation over time. I classify a firm's coverage as politically diverse if at least one Democratic and one Republican analyst cover the firm, and as politically unified if all partisan analysts belong to the same party. At the annual frequency, the interaction between misalignment and diversified coverage is small and insignificant, consistent with learning from peers being a cumulative rather than instantaneous process: a single year of cohabitation provides insufficient signal about which forecasts were ultimately right. Measured over the firm's entire coverage history, however, the interaction becomes positive and significant (0.146, $t = 2.36$), indicating that sustained exposure to the forecasts of politically different peers attenuates individual bias. Politically unanimous coverage, by contrast, substantially amplifies the misalignment effect, consistent with an echo-chamber dynamic in which shared partisan priors compound rather than correct each other (Cookson, Engelberg, and Mullins, 2020; Iyengar, Sood, and Lelkes, 2012). The asymmetry between annual and long-run diversity is itself informative: it indicates that the mechanism is Bayesian learning from observed peer accuracy, not instantaneous social influence.

Third, I conduct analogous tests at the brokerage level, where the competitive margin operates among colleagues inside a single institution. Analysts at the same brokerage share office space, attend the same meetings, and are subject to common institutional norms, so the time horizon over which diversity matters differs from the firm-coverage case. At the annual frequency, politically diverse brokerage environments attenuate the misalignment effect, consistent with contemporaneous cross-partisan exposure among coworkers limiting the amplification of partisan priors. At the sample-wide frequency, persistent brokerage homogeneity substantially amplifies the effect (-1.897, $t = -2.97$), indicating that a brokerage consistently composed of analysts from a single party develops an institutional informational culture in which partisan priors go unchallenged. The inversion between the firm-coverage and brokerage-coverage horizons — long-run diversity matters at the firm level, short-run diversity matters at the brokerage level — is consistent with the underlying mechanism: firm-level competition operates through observation of past forecast accuracy, which accumulates over years, while brokerage-level competition operates through direct daily interaction, which acts contemporaneously.

Fourth, I examine how aggregate political conflict interacts with analyst bias, using the

Partisan Conflict Index maintained by the Federal Reserve Bank of Philadelphia (Azzimonti, 2018). I define a high-conflict indicator for months in which the PCI exceeds the median level observed within the current presidential administration, isolating periods of unusually elevated political conflict relative to the baseline environment of the same presidency. The interaction between misalignment and the high-PCI indicator is positive and significant across specifications that control for market volatility (VIX) and economic policy uncertainty (Baker, Bloom, and Davis, 2016), indicating that elevated political conflict *attenuates* partisan bias in analyst forecasts. This result is the most direct contrast with Dagostino, Gao, and Ma (2023), who find that partisan bias in loan pricing *increases* during high-conflict periods. The institutional settings differ on precisely the dimensions this paper emphasizes: loan officers operate in private bilateral negotiations in which accuracy is not publicly evaluated and competitive pressure from peers on the same application is absent, while sell-side analysts operate in a public, competitive forecasting environment in which accuracy is observable and reputationally consequential. During periods of elevated political salience, the visibility of analyst performance appears to sharpen rather than dull competitive pressure, pulling partisan judgment back toward accuracy.

Beyond these three competitive channels, I examine whether partisan bias in analyst forecasts extends beyond the analyst's own political identity to the broader information environment surrounding the covered firm. Using Federal Election Commission donation data, I construct measures of partisan misalignment at the firm CEO, firm employee, and employing brokerage levels. CEO and employee political misalignment each independently predict more pessimistic analyst forecasts, controlling for the analyst's own misalignment. The aggregate political lean of the employing brokerage, by contrast, produces a precisely estimated null (coefficient 0.014, $t = 0.32$). The contrast across these four channels — own identity, firm CEO, firm employees, and employing brokerage — is itself informative about the mechanism: partisan bias travels through channels in which the analyst directly interacts with partisan actors, and not through the broad institutional orientation of the employer, which lacks a direct pathway to influence any specific earnings forecast. That the analyst-CEO interaction examined in a separate specification is directionally consistent with the broader pattern but statistically weaker is also consistent with the competitive-visibility thesis: the analyst-CEO dyad lacks the competitive structure that disciplines bias in the geographic, firm-coverage, and brokerage-coverage settings.

Identification relies on firm × fiscal quarter fixed effects, which compare forecasts from misaligned and aligned analysts evaluating the same firm's earnings in the same quarter. I progressively add broker, year-quarter, and analyst fixed effects; the misalignment coefficient nearly doubles when analyst fixed effects are included, underscoring the importance of controlling for permanent analyst heterogeneity. The baseline result is robust to excluding unaffiliated analysts, restricting the sample to Democrats and Republicans, and including party, voter state, firm × brokerage, and analyst × firm fixed effects. Across all specifications, the coefficient remains significant at the 1% level and close in magnitude to the baseline. Standard errors are double-clustered by analyst and firm throughout.

This paper makes three contributions. First, I provide evidence that the institutional visibility of a professional setting disciplines partisan bias in economic judgment. Prior work on partisan bias in finance has examined settings in which judgments are largely private (Kempf and Tsoutsoura, 2021; Dagostino, Gao, and Ma, 2023; Rice, 2023; Cassidy and Vorsatz, 2024), documenting that bias persists and, in some cases, intensifies under political stress. I show that in a public, competitive, attributable setting the bias is still present on average but is systematically attenuated along every competitive margin I can measure — geographic, firm-coverage, brokerage-coverage, and time-series. This supports Becker's (1971) prediction that competitive environments reduce discrimination and locates a boundary condition for the partisan-bias-in-finance literature: the bias is real, but its magnitude depends on whether the institutional setting exposes it to competitive correction.

Second, I distinguish between two temporal mechanisms through which political heterogeneity disciplines individual bias. At the firm-coverage level, where analysts at different brokerages do not directly interact, the disciplining effect emerges only over long horizons, consistent with Bayesian learning from observed peer accuracy accumulated across many forecast cycles. At the brokerage level, where analysts work alongside colleagues daily, the disciplining effect operates contemporaneously, consistent with direct social influence among coworkers. The contrast between long-horizon and contemporaneous effects is itself informative: it indicates that diversity disciplines bias through different channels at different institutional levels, and it suggests that the effectiveness of diversity as a corrective depends on whether the setting allows for direct interaction or only observational learning. This nuance complements Evans, Porras Prado, Rizzo, and Zambrana (2025), who find that ideologically diverse mutual fund teams outperform homogeneous teams through improved monitoring, and it sharpens the broader literature on diversity in professional judgment (Hong and Page, 2004; Adams and Ferreira, 2009) by distinguishing the horizons over which heterogeneity matters.

Third, I provide the sharpest available contrast between public and private settings in the partisan-bias literature. The same political-conflict variable that amplifies bias in private credit markets (Dagostino, Gao, and Ma, 2023) attenuates it in public equity research. Because the PCI variable and the outcome construction are comparable across the two papers, the difference cannot be attributed to measurement; it reflects a genuine institutional contrast. This finding supports a broader claim about institutional design: the observability of professional judgments and the presence of peer competition are not just features of analyst research but load-bearing disciplines on the accuracy of expert forecasts in politically polarized environments.

The remainder of the paper is organized as follows. Section 2 describes the data sources and sample construction. Section 3 presents the main results, organized around the three competitive channels and the public–private contrast. Section 4 concludes.

2 Data and Sample Construction

2.1 Earnings Forecasts

I collect earnings forecasts for S&P 500 firms between 1992 and 2020 from the Thomson Reuters Eikon database. For each quarterly earnings period of each firm, I obtain the analyst's forecast of earnings per share, the actual reported earnings per share, and the date the forecast was issued. The database provides the analyst's first name, last name, and middle initial, as well as the name of the employing brokerage house. I manually collect office phone numbers from the analyst reports and use the area codes to identify the city and state of the analyst's office location, which is required to search for the analyst in the voter registration data described below.

For each earnings forecast, I compute *Forecast Error* as the difference between the forecast earnings per share and the actual reported earnings per share, scaled by the stock price as of the previous month-end, multiplied by 100. A positive (negative) forecast error indicates a more optimistic (pessimistic) forecast relative to actual earnings. I construct a set of analyst and forecast characteristics as of the forecast issuance date: the natural logarithm of the number of quarters since the analyst's first earnings forecast for a given broker (*Ln(Tenure)*), the natural logarithm of the number of firms covered by the analyst in a given quarter (*Ln(Num Covered)*), and the natural logarithm of the number of days between the forecast issue date and the fiscal period end date (*Ln(Distance)*).

2.2 Analyst Political Affiliation

I identify analysts' political affiliations using voter registration records from the LexisNexis Public Records database, which is used in prior studies of partisan bias in financial markets (Dagostino, Gao, and Ma, 2023; Kempf and Tsoutsoura, 2021).[1] LexisNexis Public Records aggregates information from a broad set of public record sources, including voter registration records, historical residential addresses across states, professional licenses, employment records, and educational histories. Voter registration coverage spans the 23 states listed in the appendix and the District of Columbia. Analysts residing in states not covered by the LexisNexis voter registration database are excluded from the sample.

I search for each analyst by first name, last name, middle initial, and city in LexisNexis, using the location of the analyst's office derived from the area code of the office phone number. For many analysts, common names generate a large number of potential matches. The names Yun Kim and Michael Lewis, for example, produced over 500 and 663 matches in New York, respectively. To resolve these ambiguities, I supplement the LexisNexis search with information gathered from LinkedIn, broker websites, financial news websites, the WayBack Machine, and Google. The key data points collected from these sources include educational history, employment history, and age.

LexisNexis provides age as a search parameter, which proves to be the most valuable data point for narrowing matches. When an analyst's exact birth year is unavailable, I estimate age from educational history: assuming the analyst was approximately eighteen years old when starting college, I compute an expected age as the difference between the search year and the first year of college, plus eighteen, and restrict the LexisNexis search to a two-year window around this value. This substantially reduces the number of potential matches.

[1] Dagostino, Gao, and Ma (2023) verify the quality of voter registration data in LexisNexis Public Records via FOIA requests with the New York State Board of Elections and find complete overlap of party affiliation between LexisNexis and the information provided by New York State.

I construct a multi-dimensional validation procedure to assess match quality. For each potential match, I record indicators for the following criteria: (i) *LinkedIn Match*, equal to one if the LinkedIn profile and LexisNexis record exhibit strong similarities in employment or educational history; (ii) *Email*, equal to one if LexisNexis lists an email from the analyst's brokerage firm, a former employer, or a college; (iii) *Address*, equal to one if LexisNexis lists a home address within 50 miles of the brokerage firm, a former employer, or an educational institution; (iv) *Employment Match*, equal to one if LexisNexis lists any firm from the analyst's employment history; (v) *SEC License*, equal to one if a securities license is listed; and (vi) *Education*, equal to one if the analyst's educational institution appears on the LexisNexis profile or if a residential address is near the institution. When multiple addresses in different parts of the country match between the LexisNexis record and external sources, I assign a high certainty score. I also request LinkedIn connections with every identified analyst; confirmed mutual connections with other analysts in the sample provide additional validation, as analysts are likely to know one another through conferences and professional networks.

LexisNexis occasionally lists the same person under multiple identification numbers. I record the primary ID with the voting record and note alternative IDs for the same person. For difficult determinations, I maintain detailed notes documenting the reasons for the assigned certainty score. I assign an overall *Certainty* score ranging from 10% to 99% based on the totality of the evidence. This hand-collected validation procedure complements the automated matching approaches used in prior work. Fu, Ma, and Peng (2024) identify analyst political affiliations using L2 voter registration data matched by name and geographic proximity between the analyst's residential address and the brokerage address. The LexisNexis approach provides access to a richer set of public records, including historical addresses across states, professional licenses, and employment records, enabling the cross-referencing that is particularly valuable for common names.

I collect the analyst's most recent party registration for each earnings forecast. Following Dagostino, Gao, and Ma (2023) and Kempf and Tsoutsoura (2021), I determine the analyst's political affiliation as of each U.S. presidential general election and update the affiliation within a presidential term if the analyst switches parties. I define *Misalign Analyst* as a dummy variable equal to one if the analyst's party affiliation does not match the president's party. Following Dagostino, Gao, and Ma (2023) and Kempf and Tsoutsoura (2021), I assign a value of zero to *Misalign Analyst* for analysts who are unaffiliated with any political party. In Table 4, I verify that the baseline result is robust to excluding unaffiliated analysts entirely and to restricting the sample to Democrats and Republicans only.

2.3 Firm, Employee, and Brokerage Political Alignment

To measure the political orientation of the firms covered by analysts and of the analysts' employing brokerages, I obtain political donation data from the Federal Election Commission (FEC). For each two-year federal election cycle, I collect the total amounts donated to Democratic and Republican candidates by three categories of donors: the firm's CEO, the firm's non-CEO employees, and the employees of the analyst's brokerage. An entity is classified as leaning Democratic (Republican) if Democratic (Republican) donations exceed Republican (Democratic) donations within the cycle. Observations with no donations or missing data are excluded, as the partisan direction of the entity cannot be determined.

I construct three FEC-based misalignment variables. *Misalign Firm CEO* equals one if the CEO's donation-based political lean does not match the party of the U.S. president. *Misalign Firm Employees* equals one if the firm's non-CEO employee donations lean toward the party opposing the president. *Misalign Brokerage* equals one if the aggregate employee donations of the analyst's brokerage lean toward the party opposing the president. These three variables differ from the analyst's own *Misalign Analyst* in that they measure partisan alignment in the broader information environment surrounding the forecast rather than the political identity of the forecaster. They also differ from one another in the directness of the channel through which the political alignment could plausibly influence any specific earnings forecast: CEO and employee alignment characterize the firm being covered and the individuals with whom the analyst directly interacts, whereas brokerage alignment characterizes the employer and has no direct pathway to any particular forecast. Table 9 exploits this distinction to test whether partisan bias travels through channels of direct interaction rather than through broad institutional orientation.

Sample sizes for the FEC-based variables are substantially smaller than for *Misalign Analyst* because donation data are not available for all CEOs, firms, and brokerages in all election cycles. CEO-level donations produce the smallest subsample (2,743 forecasts) because many CEOs do not personally donate. Employee-level donations are more widely available (5,874 forecasts) because rank-and-file employees collectively donate even when the CEO does not. Brokerage-level donations fall in between (6,370 forecasts). The reduced sample sizes do not undermine the core finding: the analyst's own misalignment coefficient remains statistically significant in every subsample, indicating that the baseline result is not an artifact of the firms or brokerages for which FEC data happen to be available.

2.4 Political Environment Variables

I construct several variables to characterize the competitive political environment in which analysts operate. *Battleground State* is a dummy variable equal to one if the analyst's voter registration state experienced a presidential election margin of five percentage points or less in the most recent election. Under the Electoral College system, presidential campaigns concentrate resources in electorally competitive states (Hill, Rodriguez, and Wooden, 2010), exposing residents to substantially higher levels of cross-partisan political messaging.

I construct measures of political composition at both the firm and brokerage levels using the LexisNexis-based party affiliations of the analysts in the sample. At the firm level, I classify analyst coverage as politically diverse if the firm is covered by at least one Democratic and at least one Republican analyst, and as politically unified if every analyst covering the firm is a registered partisan of the same party. The two classifications treat unaffiliated analysts differently by construction: an unaffiliated analyst does not affect diversity because diversity requires only the presence of both Democrats and Republicans, but an unaffiliated analyst breaks unanimity because unified coverage requires that no analyst belong to any other category. Composition measures require at least two analysts covering the firm, so firms with a single analyst are excluded from the interaction tests. I define these measures at both the annual (firm-year) frequency and over the firm's entire coverage history in the sample. I construct analogous measures at the brokerage level, classifying a brokerage as diverse or unified based on the partisan composition of its analyst workforce at the annual and sample-wide frequencies. These LexisNexis-based composition measures are distinct from the FEC-based *Misalign Brokerage* variable described above: the composition measures count the partisan identities of individual analysts, while *Misalign Brokerage* measures the aggregate donation behavior of the brokerage's employees.

I obtain the Partisan Conflict Index (PCI) from the Federal Reserve Bank of Philadelphia (Azzimonti, 2018). The PCI measures the degree of political disagreement among U.S. politicians at the federal level using a search-based algorithm that tracks the frequency of newspaper articles reporting lawmakers' policy disagreements. I define *PCI Dummy* as an indicator equal to one for months in which the PCI exceeds the median level observed within the current presidential administration. This within-president definition isolates periods of unusually high political conflict relative to the baseline of the same presidency, rather than conflating conflict with shifts in the overall political environment across administrations. I also obtain the CBOE Volatility Index (VIX) and the Economic Policy Uncertainty (EPU) indexes developed by Baker, Bloom, and Davis (2016), including the news-based index (*EPU (News)*) and the three-component index (*EPU (Three-Component)*), which combines the news-based measure with tax code expiration data and forecaster disagreement.

2.5 Sample Construction

The initial sample from Thomson Reuters Eikon contains 23,793 earnings forecasts issued by sell-side analysts covering S&P 500 firms between 1992 and 2020. The primary sample restriction retains only the most recent forecast issued by each analyst for each firm-fiscal quarter combination, since when analysts revise their forecasts as additional information becomes available during the quarter, earlier forecasts are superseded. This restriction eliminates approximately half of the initial observations. The remaining attrition reflects the geographic coverage of the LexisNexis voter registration database: analysts whose office locations fall outside the 23 covered states and the District of Columbia cannot be matched to voter registration records and are excluded. A small number of additional observations are lost to singleton group absorption in the most saturated fixed effects specifications. The final sample contains 10,027 earnings forecasts issued by 224 unique analysts employed at 109 brokerage houses covering 669 firms.

2.6 Summary Statistics

Table 1 reports summary statistics for the main variables. The mean forecast error is 0.058, indicating a slight optimistic bias on average. Approximately 38% of forecast observations are issued by misaligned analysts. The average analyst is 45 years old with a tenure of approximately 13 quarters at the current broker and covers 6 firms per quarter. The average distance between the forecast issue date and the fiscal period end date is 135 days. Among analysts with identified party affiliations, 28% are Democrats, 34% are Republicans, 5% are affiliated with other parties, and 33% are unaffiliated. Among the subsample with identified gender, approximately 9% of analysts are female.

Table 2 presents the geographic distribution of analyst party affiliations by voter registration state. New York accounts for the largest share of forecasts (36.4%), consistent with the concentration of the financial services industry, followed by Texas (18.4%) and Connecticut (17.3%). The distribution of party affiliations varies substantially across states. New York leans Democratic among affiliated analysts, with Democrats outnumbering Republicans roughly two to one, while Connecticut and Texas are heavily Republican. Several states, including Louisiana, Minnesota, and Wisconsin, contain only unaffiliated analysts in the sample.

Figure 2 presents the overall distribution of analyst party affiliation. Republicans and unaffiliated analysts each account for roughly one-third of observations, with Democrats comprising just under 30% and other-party affiliations representing a small share. The relatively even split between Democrats and Republicans is important for identification, as the misalignment variable switches direction for each group when the presidency changes parties.

Figure 3 presents the distribution by presidential administration. The early years of the sample contain relatively few observations: Clinton's first term accounts for less than 1% of forecasts, and Clinton's second term for less than 3%. This reflects two features of the underlying data. First, I/B/E/S analyst coverage of S&P 500 firms was substantially sparser in the early 1990s than in later years. Second, identifying the office location of an early-1990s analyst requires locating broker reports from that period, which are less systematically archived than more recent reports. The Biden administration is similarly small because the sample ends in 2020, capturing only the first year of that presidency. Across the administrations with substantial observation counts, the share of analysts registered as Democrats declines and the share registered as unaffiliated rises. I do not take a position on the source of this trend, which could reflect changes in the partisan composition of the analyst workforce, changes in how analysts register to vote, or both; the identification strategy relies on within-analyst variation in presidential alignment rather than on cross-administration comparisons of sample composition.

Figure 4 presents the distribution of analyst party affiliation by one-digit SIC industry of the covered firm. The sample is heavily concentrated: manufacturing alone accounts for roughly 35% of forecast observations, and finance, mining, and services together account for another 47%, leaving the remaining five industries with less than 20% of the sample combined. The partisan composition of analysts varies meaningfully across these industries. This concentration is a feature of the S&P 500 universe rather than of the matching procedure, and the identification strategy accommodates it directly: the baseline misalignment effect is estimated within firm × fiscal quarter cells, so comparisons are made between misaligned and aligned analysts covering the same firm in the same quarter, not across industries. The robustness results in Table 4 further confirm that the effect survives specifications that absorb firm × brokerage and analyst × firm variation, neither of which would hold if the result were being driven by industry-level partisan sorting.

3 Empirical Results

3.1 Specification

I estimate the following specification:

$$\textit{Forecast Error}_{i,j,q,t} = \beta\, \textit{Misalign Analyst}_{i,t} + \Gamma Z_{i,j,q,t} + \alpha_{j,q} + \eta_i + \theta_b + \tau_t + \varepsilon_{i,j,q,t}, \qquad (1)$$

where $Forecast\ Error_{i,j,q,t}$ is the forecast error associated with analyst i's forecast of firm j's earnings for fiscal quarter q, issued at time t. The key independent variable is $Misalign\ Analyst_{i,t}$, a dummy variable equal to one if analyst i's party affiliation does not match the president's party at time t. $Z_{i,j,q,t}$ is a vector of control variables including $Ln(Tenure)$, $Ln(Num\,Covered)$, and $Ln(Distance)$. I include firm × fiscal quarter fixed effects ($\alpha_{j,q}$), analyst fixed effects (η_i), broker fixed effects (θ_b), and year-quarter fixed effects (τ_t). Standard errors are double-clustered by analyst and firm.

The firm × fiscal quarter fixed effects are central to the identification strategy. They absorb both observable and unobservable time-varying firm characteristics, ensuring that I compare forecasts from misaligned and aligned analysts evaluating the same firm's earnings in the same quarter. This approach follows Kempf and Tsoutsoura (2021) and Dagostino, Gao, and Ma (2023). Year-quarter fixed effects account for macroeconomic conditions common to all analysts in a given period, ensuring that the misalignment effect is not driven by aggregate shocks such as recessions or policy changes. Analyst fixed effects control for time-invariant individual characteristics, including innate ability, career incentives, and baseline optimism or pessimism. Broker fixed effects absorb institutional differences across brokerage houses.

3.2 Baseline Results

Table 3 reports the baseline estimates. Column (1) includes only firm × fiscal quarter and broker fixed effects. The coefficient on *Misalign Analyst* is −0.089 ($t = -2.10$), indicating that misaligned analysts issue significantly more pessimistic earnings forecasts. Column (2) adds year-quarter fixed effects; the coefficient is −0.080 ($t = -2.74$). Column (3) replaces year-quarter fixed effects with analyst fixed effects, and the coefficient nearly doubles to −0.159 ($t = -2.43$), underscoring the importance of controlling for permanent analyst heterogeneity. Column (4), the fully saturated specification, includes all four sets of fixed effects and produces a coefficient of -0.159 ($t = -3.26$), significant at the 1% level.

The economic magnitude is meaningful. A coefficient of -0.159 indicates that a misaligned analyst's forecast error is 0.159 percentage points of stock price lower than that of an aligned analyst covering the same firm in the same quarter. This effect represents approximately 18% of the sample standard deviation of forecast error (0.877) and nearly the full interquartile range (0.165). Relative to the median forecast error of -0.026, the misalignment effect shifts an analyst from the center of the distribution well below the 25th percentile (-0.121).

The control variables behave as expected: $Ln(Distance)$ is positive and significant, consistent with greater information uncertainty at longer horizons producing more optimistic forecasts; $Ln(Num\ Covered)$ is positive, suggesting analysts with larger coverage portfolios issue slightly more optimistic forecasts; and $Ln(Tenure)$ is negative, indicating more experienced analysts issue more pessimistic forecasts.

3.3 Robustness

Table 4 subjects the baseline result to a battery of alternative specifications. A first concern is that the coding of unaffiliated analysts mechanically drives the result, since unaffiliated analysts are assigned a value of zero for *Misalign Analyst*. Unaffiliated analysts may hold private partisan views that are not reflected in their voter registration, introducing measurement error that attenuates the estimated effect. In column (1), I exclude all unaffiliated analysts, reducing the sample to 5,254 observations. The coefficient strengthens to -0.207 ($t = -3.54$), consistent with the removal of misclassified partisans who dilute the signal. In column (2), I further restrict the sample to Democrats and Republicans only (4,542 observations), and the coefficient increases to -0.249 ($t = -3.86$). The monotonic pattern is expected: as the sample is progressively restricted to analysts with clearly identified partisan affiliations, the contrast between aligned and misaligned groups sharpens.

The remaining columns introduce additional fixed effects to address specific identification concerns. Column (3) adds party fixed effects to absorb time-invariant differences across party affiliations, such as the possibility that Republicans are systematically more conservative in their forecasts regardless of alignment. The coefficient is -0.156 ($t = -2.87$), virtually unchanged from the baseline. Column (4) adds voter state fixed effects to control for geographic differences in political culture that may correlate with both analyst registration and forecasting behavior; the coefficient is -0.159 ($t = -3.18$). Column (5) adds firm $\times$ brokerage fixed effects, which control for the time-invariant match between a particular analyst's brokerage and a particular firm. Column (6) adds analyst $\times$ firm fixed effects, which compare the same analyst covering the same firm across different presidential administrations. This is the tightest within-analyst specification and mirrors the fixed effects structure used by Fu, Ma, and Peng (2024); the coefficient is -0.188 ($t = -3.45$), slightly larger than the baseline. Column (7) combines all additional fixed effects simultaneously; the coefficient is -0.180 ($t = -3.18$). Across these specifications, the misalignment coefficient ranges from -0.156 to -0.188 and remains significant at the 1% level.

As a complementary test, column (8) re-estimates the baseline specification replacing *Misalign Analyst* with *Align Analyst*, a dummy variable equal to one if the analyst's party affiliation matches the president's party. The coefficient on *Align Analyst* is 0.136 ($t = 2.77$), significant at the 1% level. This result is the mirror image of the baseline: aligned analysts exhibit excess optimism of comparable magnitude to the excess pessimism of misaligned analysts. The symmetry of the two effects becomes relevant when interpreting the battleground state results in Section 3.5.

3.4 Three Competitive Channels

Having established the baseline misalignment effect and its robustness, I now turn to the central question of the paper. If the private-setting findings in prior work reflect a universal behavioral pull of partisan identity, that pull should produce measurable and uniform bias in analyst forecasts regardless of the analyst's immediate competitive environment. If instead the competitive and visible structure of sell-side research disciplines partisan judgment, the bias should be attenuated where competition and visibility are strongest and amplified where they are weakest. The remainder of this section tests three competitive channels through which visibility could discipline bias. The first is between-party competition in the analyst's political geography. The second is between-broker competition among analysts covering the same firm. The third is within-broker competition among colleagues inside a single institution. After presenting the three channels, I test a fourth prediction at the aggregate time-series level using the Partisan Conflict Index, which also provides the sharpest contrast with evidence from private-market settings. Finally, I extend the analysis beyond the analyst's own political identity to examine whether partisan alignment in the firm's broader information environment independently predicts forecast pessimism.

3.5 Battleground States

I begin with the cleanest test of the competitive-visibility thesis: between-party competition in the analyst's political geography. Table 5 reports the results. *Battleground State* equals one if the analyst's voter registration state experienced a presidential election margin of five percentage points or less in the most recent election. Under the Electoral College system, presidential campaigns concentrate advertising, appearances, and mobilization in these competitive states (Hill, Rodriguez, and Wooden, 2010), producing an information environment in which residents are exposed to sustained cross-partisan messaging.

In column (1), I interact *Battleground State* with *Misalign Analyst*. The baseline misalignment effect is -0.197 ($t = -3.80$), and the interaction is positive and significant at the 1% level (0.273, $t = 3.13$), fully offsetting the baseline effect: for misaligned analysts in battleground states, the net effect is $-0.197 + (-0.132) + 0.273 = -0.056$, close to zero. In column (2), I interact *Battleground State* with *Align Analyst*. The baseline alignment effect is 0.171 ($t = 3.11$), and the interaction is negative and significant (-0.168, $t = -2.42$). For aligned analysts in battleground states, the net effect is $0.171+(-0.027)+(-0.168) = -0.024$, also close to zero.

The two columns together reveal that battleground states discipline both sides of the partisan gap. Misaligned analysts become less pessimistic, and aligned analysts become less optimistic. Both groups compress toward the center, and the gap between aligned and misaligned forecasts largely disappears. This symmetric compression is consistent with Becker's (1971) prediction that competitive environments reduce discrimination: the disciplining effect does not favor one group over the other but disciplines both. The mechanism does not require that analysts in battleground states hold different underlying views about politics or the economy; it requires only that sustained exposure to cross-partisan messaging (Martin and Yurukoglu, 2017; DellaVigna and Kaplan, 2007) attenuates the filtering of economic information through partisan priors, and the symmetric compression in the two columns is consistent with that interpretation.

The battleground state result is the strongest single piece of evidence in the paper for the competitive-visibility thesis. The interaction term more than offsets the baseline effect, the symmetric test produces a matching result on the alignment side, and the theoretical mechanism is well-established in prior work on media exposure and belief formation. The remaining tests in this section examine whether analogous competitive structures at the firm-coverage and brokerage levels produce comparable disciplining effects.

3.6 Political Composition of Firm-Level Analyst Coverage

I next examine whether the political composition of the analysts covering a particular firm disciplines individual bias. Table 6 reports the results. A firm's coverage is classified as politically unified if every analyst covering the firm is a registered partisan of the same party, which means the presence of even a single unaffiliated or other-party analyst breaks unanimity. Coverage is classified as politically diverse if at least one Democratic and one Republican analyst cover the firm. The two classifications are not mirror images: diversity requires the presence of both partisan groups, while unanimity requires the absence of every other group. The main effects of the composition variables are absorbed by firm × fiscal quarter fixed effects, so identification comes from the interaction terms, which exploit within-firm variation across analysts. I construct both classifications at the annual (firm-year) frequency and over the firm's entire coverage history in the sample. The contrast between these two horizons is substantively informative: sell-side earnings forecasts submitted to I/B/E/S are point estimates without accompanying written justifications, so analysts cannot learn from one another's reasoning. They can only observe one another's forecast levels and the realized accuracy of those forecasts once actual earnings are announced. Learning about peer accuracy therefore requires repeated observation over many forecast cycles, and any disciplining effect of diversity should emerge at long horizons rather than instantaneously.

Columns (1) and (2) report the results for politically unified coverage. In column (1), defined over the firm's entire coverage history, the interaction between *Misalign Analyst* and total unified coverage is large, negative, and significant (-1.533, $t = -2.11$), indicating that persistent partisan homogeneity substantially amplifies the misalignment effect. When a firm has been covered exclusively by analysts of a single party throughout the sample, misaligned analysts exhibit much stronger forecast pessimism than in firms with mixed or non-unanimous coverage. In column (2), the annual-frequency analogue produces a similar but smaller effect (-1.124, $t = -2.00$), consistent with homogeneity compounding partisan priors over any horizon in which it is sustained.

Columns (3) and (4) report the results for politically diverse coverage. In column (3), the interaction between *Misalign Analyst* and total diversified coverage is positive and significant (0.146, $t = 2.36$), indicating that sustained exposure to the forecasts of politically different peers attenuates individual bias. In column (4), the annual-frequency analogue is small and insignificant (-0.011, $t = -0.19$). This asymmetry between the annual and long-run diversity measures is itself the main finding of the table. A single year of cohabitation with diverse peers provides insufficient signal about which forecasts were ultimately right; the disciplining effect of diversity requires the accumulation of ex-post accuracy feedback over multiple forecast cycles before partisan priors can be updated.

The combined pattern of results, unanimity amplifies bias at both horizons while diversity disciplines it only over long horizons, is consistent with Bayesian learning as the underlying mechanism. Homogeneity is a strong poison that requires no accumulation: when all analysts share the same partisan lean, the absence of dissenting forecasts reinforces the shared bias continuously. Diversity is a weaker corrective that operates through observed peer accuracy, which takes time to accumulate. The magnitudes are consistent with this asymmetry: the total-unified interaction coefficient (-1.533) is roughly ten times the magnitude of the total-diverse interaction (0.146).

A potential concern with these results is that coverage composition may be endogenous to forecast behavior. If firms with particular partisan characteristics attract or repel analysts of particular political affiliations, the observed correlation between coverage composition and the misalignment effect could reflect selection rather than discipline. Several features of the research design mitigate this concern. The main effects of coverage composition are absorbed by firm × fiscal quarter fixed effects, so the interaction estimates are not identified from differences in the types of firms that attract diverse versus unified coverage. The total composition measures are determined by the full history of analyst coverage, which predates many of the individual forecasts in the sample and is therefore less susceptible to contemporaneous feedback. The endogeneity concern cannot be fully eliminated, but the most plausible selection story, that politically homogeneous analyst teams sort into firms whose realized earnings are systematically surprising in a partisan direction, would need to operate through within-firm variation across analysts and across time, which the fixed effects structure is designed to absorb.

3.7 Political Composition of Brokerage-Level Analyst Coverage

Table 7 conducts analogous tests at the brokerage level, where the competitive margin operates among colleagues inside a single institution. Analysts at the same brokerage share office space, attend the same meetings, and are subject to common institutional norms, so the time horizon over which diversity disciplines bias differs from the firm-coverage case. Where firm-level competition operates through observation of past forecast accuracy, which accumulates over years, brokerage-level competition operates through direct daily interaction, which should act contemporaneously.

Column (1) reports the interaction between *Misalign Analyst* and total unified brokerage coverage. The coefficient is large, negative, and highly significant (-1.897, $t = -2.97$), indicating that persistent brokerage homogeneity substantially amplifies the misalignment effect. When a brokerage has been consistently composed of analysts from a single party throughout the sample, the institutional informational culture embeds partisan priors that go unchallenged by colleagues, and misaligned analysts exhibit much stronger forecast deviations. The magnitude is larger than the corresponding firm-level estimate (-1.533), consistent with direct daily interaction among coworkers biting harder than observational learning across brokerages.

Column (2) reports the annual-frequency analogue for unified brokerage coverage. The interaction is positive and insignificant (0.294, $t = 1.25$). Unlike the firm-coverage case, where homogeneity amplifies bias at both horizons, brokerage-level homogeneity appears to matter only when it is persistent. A single year in a unified brokerage does not produce measurable amplification, which is consistent with the interpretation that the disciplining mechanism at the brokerage level requires the development of an institutional culture rather than the immediate presence of ideologically similar coworkers.

Columns (3) and (4) report the diverse-coverage results. In column (3), the interaction between *Misalign Analyst* and total diverse brokerage coverage is small and insignificant (0.050, $t = 0.43$). Former brokerage employees who are no longer at the institution do not appear to affect current analysts, consistent with the interpretation that brokerage-level discipline operates through direct interaction rather than observational learning. In column (4), the annual-frequency diversity measure produces a positive and marginally significant interaction (0.157, $t = 1.84$), indicating that contemporaneous cross-partisan exposure among coworkers limits the amplification of partisan priors. This is the opposite horizon from the firm-coverage diversity result: at the firm level, long-run diversity matters because analysts learn from the accumulated record of peer accuracy; at the brokerage level, short-run diversity matters because coworkers directly influence each other's contemporaneous judgments.

The combined pattern across Tables 6 and 7 is internally consistent once the two horizons are interpreted through the lens of how competition operates in each setting. Firm-level competition is observational and requires accumulation, so long-run diversity matters and annual diversity does not. Brokerage-level competition is interactional and operates in real time, so annual diversity matters and long-run diversity (which captures former employees no longer at the institution) does not. Both homogeneity effects persist, but they emerge at different rates: firm-level homogeneity amplifies bias at both horizons, while brokerage-level homogeneity requires persistence to develop the institutional culture through which it operates.

3.8 Partisan Conflict and Forecast Bias

I next examine how aggregate political conflict interacts with the misalignment effect. Table 8 reports the results. *PCI Dummy* equals one for months in which the Partisan Conflict Index (Azzimonti, 2018) exceeds the median level observed within the current presidential administration. This within-president definition isolates periods of unusually high political conflict relative to the baseline of the same presidency, rather than comparing across presidencies that may differ in their overall conflict levels. Because PCI is a time-series variable measured at the monthly frequency, I do not include year-quarter fixed effects in these specifications; doing so would mechanically absorb the variation the test is designed to exploit.

Column (1), with firm $\times$ fiscal quarter, broker, and analyst fixed effects, reports an interaction between misalignment and the PCI indicator of 0.097 ($t = 2.17$), indicating that the negative effect of partisan misalignment is attenuated during periods of elevated political conflict within a given presidency. Columns (2) through (6) progressively add controls for market volatility (*VIX*), news-based economic policy uncertainty (*EPU (News)*), and the three-component economic policy uncertainty index (*EPU (Three-Component)*). The interaction remains positive and significant across these specifications, with coefficients ranging from 0.087 to 0.097. The stability of the interaction term after controlling for *VIX* and the two *EPU* measures indicates that the attenuating effect of partisan conflict is distinct from general uncertainty or macroeconomic conditions.

This result is the most direct contrast in the paper with Dagostino, Gao, and Ma (2023), who find that partisan bias in loan pricing *increases* during high-conflict periods. The two papers use the same partisan conflict measure and construct partisan bias from the same type of underlying variable, so the difference in sign cannot be attributed to measurement. It reflects a difference in institutional setting. Loan officers operate in private bilateral negotiations in which accuracy is not publicly evaluated and competitive pressure from peers producing rival judgments on the same application is absent. Sell-side analysts operate in a public, competitive forecasting environment in which accuracy is observable and reputationally consequential. During periods of elevated political salience, the visibility of analyst performance appears to sharpen rather than dull competitive pressure, pulling partisan judgment back toward accuracy. This interpretation is consistent with the battleground state results: both tests suggest that competitive environments, whether geographic or temporal, discipline partisan bias rather than amplify it.

3.9 Partisan Misalignment in the Firm's Broader Information Environment

The three competitive channels examined so far test whether visibility and competition discipline the analyst's own partisan bias. I now extend the analysis to ask whether partisan alignment in the firm's broader information environment, beyond the analyst's own identity, independently predicts forecast errors. Table 9 reports the results. Column (1) reproduces the baseline. Columns (2) through (4) each add a second misalignment variable measured at a different level of the information environment surrounding the forecast: the firm's CEO, the firm's non-CEO employees, and the analyst's employing brokerage.

In column (2), *Misalign Firm CEO* equals one if the firm's CEO political donations lean toward the opposing party from the president. The analyst's own misalignment remains significant (-0.206, $t = -2.07$), and CEO misalignment is independently significant (-0.127, $t = -2.17$). Analysts regularly interact with CEOs through earnings calls, investor meetings, and conferences, and the CEO's partisan orientation plausibly shapes managerial guidance and the analyst's interpretation of that guidance.

In column (3), *Misalign Firm Employees* measures the aggregate political lean of the firm's non-CEO employees. The coefficient is -0.120 ($t = -2.36$), and the analyst's own misalignment remains significant in this subsample (-0.095, $t = -2.36$). The firm's rank-and-file political orientation independently predicts forecast pessimism, extending the partisan information channel beyond top management.

In column (4), *Misalign Brokerage* measures the aggregate political lean of the analyst's employing brokerage. The coefficient is 0.014 ($t = 0.32$), a precisely estimated null, while the analyst's own misalignment remains strong (-0.191, $t = -2.82$). This null is not a failed test but a discriminating one. Partisan bias operates through channels in which the analyst directly interacts with partisan actors: the analyst's own identity, the CEO, and the employees of the firm being covered. It does not operate through the broad institutional orientation of the employer, which has no direct pathway to influence any specific earnings forecast. Under a generic "partisan environment" interpretation of the results in columns (2) and (3), any aggregate measure of partisan environment should predict forecast pessimism; under a channel-specific interpretation, only measures capturing direct interaction should. The precisely estimated null in column (4) is consistent with the latter and inconsistent with the former.

The contrast between the brokerage-level null in column (4) and the brokerage-level coverage composition results in Table 7 sharpens the channel-specific interpretation further. The two tables measure two different things at the same institutional level. Table 9 measures the brokerage's aggregate partisan lean through the political donations of its employees, most of whom are not equity analysts. Table 7 measures the partisan composition of the specific analysts working at the brokerage through their individual voter registrations. The donation-based measure in Table 9 produces a precisely estimated null. The analyst-composition measure in Table 7 produces economically large and statistically significant interactions with the misalignment effect. The difference is not about where the partisan signal is measured but about whether the measured population is in competition with the analyst whose forecast is being explained. Analysts at the same brokerage produce rival forecasts on the same firms, their accuracy is publicly tracked and individually attributed, and their career outcomes depend on their relative performance. This is the competitive structure that the thesis of the paper identifies as the disciplining force: when politically different peers are visible, benchmarked, and in direct competition with the analyst, their presence tempers partisan priors. The support staff, traders, compliance officers, and other non-analyst employees whose donations define *Misalign Brokerage* share none of these features in relation to any particular forecast. They may outnumber the analysts in any given brokerage by a large margin, but they do not produce rival forecasts on the covered firms and their performance is not benchmarked against the analyst's. The null in Table 9 is therefore not evidence that brokerage-level partisan environments are inert. It is evidence that the active ingredient at the brokerage level is peer competition for accuracy among the analysts themselves, which *Misalign Brokerage* in Table 9 does not measure and *Politically Unified Coverage* in Table 7 does.

Sample sizes in columns (2) through (4) are smaller than the baseline because the FEC-based misalignment variables require donation data that are not available for all firms and brokerages in all election cycles. The analyst's own misalignment coefficient remains statistically significant across all subsamples, indicating that the baseline result is not sensitive to the composition of firms or brokerages for which FEC data happen to be available.

3.10 Analyst-CEO Partisan Alignment

Table 10 examines whether the partisan alignment between an analyst and a covered firm's CEO interacts with the baseline misalignment effect. This test provides a final piece of evidence on the competitive-visibility thesis, from a direction that contrasts with the previous tests. Unlike the geographic, firm-coverage, and brokerage-coverage channels, the analyst-CEO dyad lacks a competitive structure in which opposing partisan views are publicly contested and benchmarked against realized outcomes. The analyst observes the CEO's forecasts only indirectly, through managerial guidance, and the CEO does not observe the analyst's forecasts in a way that produces reputational consequences for either party. If the competitive-visibility thesis is correct, the disciplining mechanism should be correspondingly muted in this channel.

In column (1), I interact *Misalign Analyst* with *Analyst Aligned with CEO*, which equals one if the analyst's party affiliation matches the partisan lean of the firm's CEO. The interaction is negative (-0.104) but statistically insignificant ($t = -0.83$). In column (2), I interact *Misalign Analyst* with *Analyst Misalign with CEO*. The interaction is positive (0.140) and also insignificant ($t = 1.07$). Neither interaction achieves statistical significance, though both are directionally consistent with the broader pattern in the paper: political homogeneity between agents tends to reinforce partisan bias, and political heterogeneity tends to attenuate it.

The weaker results in this table are consistent with the thesis rather than a challenge to it. The analyst-CEO dyad is the one channel tested in the paper in which no competitive structure disciplines the interaction between the two parties. There is no public benchmark against which the analyst's forecast and the CEO's implicit views are continuously compared, no peer competition among multiple analysts and multiple CEOs resolving the same question, and no reputational feedback that would incentivize either party to update its partisan priors. Under the competitive-visibility thesis, exactly this pattern, directional consistency without statistical significance, is what one would expect in a channel that lacks the features the thesis identifies as load-bearing. Additional power from a larger CEO-donation subsample (the current subsample is 2,743 observations, compared to 10,027 for the baseline) might produce significant estimates in the same direction, but statistical significance is not what the table is designed to establish. The table is designed to test whether the disciplining mechanism is present in a channel where the thesis predicts it should be weak, and the weak results are consistent with that prediction.

4 Conclusion

Partisan identity distorts the economic judgments of financial professionals in settings that have mostly been studied in isolation from one another. Credit rating analysts, loan officers, corporate executives, and mutual fund managers all exhibit partisan bias in their professional decisions, and those decisions share an institutional structure that is largely private, bilateral, and difficult to benchmark contemporaneously against peers. This paper asks whether the same bias survives in a setting with the opposite institutional features, and whether the competitive visibility of that setting disciplines the bias. Using voter registration data from LexisNexis Public Records matched to sell-side equity analyst earnings forecasts for S&P 500 firms from 1992 to 2020, I find that the baseline partisan bias documented in prior work is present in analyst forecasts but is systematically disciplined along every competitive margin the data allow me to measure.

The strongest evidence comes from the comparison of analysts in battleground and non-battleground states. Under the Electoral College system, presidential campaigns concentrate advertising, appearances, and mobilization in states with close presidential elections, producing sustained cross-partisan exposure that is not present in safer states. Analysts in battleground states exhibit no measurable partisan bias: the interaction between misalignment and battleground residence fully offsets the baseline effect, and a symmetric test on the alignment side confirms that the disciplining operates on both sides of the partisan distribution. Misaligned pessimism and aligned optimism are compressed toward the center by comparable amounts. This is the cleanest test in the paper of the prediction that competitive environments reduce discrimination.

Two institutional tests at the firm-coverage and brokerage-coverage levels produce complementary evidence. At the firm level, long-run political diversity among the analysts covering a firm attenuates individual bias, while persistent politically unanimous coverage amplifies it. The disciplining effect of diversity emerges only over long horizons, consistent with Bayesian learning from observed peer accuracy: sell-side forecasts are point estimates without accompanying reasoning, so analysts cannot learn from their peers' logic and can only learn from which peers get the number right over time, which requires the accumulation of many forecast cycles. At the brokerage level, by contrast, the disciplining effect of diversity operates contemporaneously, because analysts at the same brokerage interact daily and compete directly for individual performance outcomes. The inversion of the relevant time horizon between the two settings, long-run at the firm level and contemporaneous at the brokerage level, is itself informative about how competition disciplines bias: observational learning accumulates, and direct interaction bites immediately.

The time-series test using the Partisan Conflict Index produces the sharpest available contrast with evidence from private-market settings. In the private credit market studied by Dagostino, Gao, and Ma (2023), elevated partisan conflict *amplifies* partisan bias in loan pricing. In the public equity research setting studied here, the same variable *attenuates* partisan bias in earnings forecasts. Because the conflict measure and the outcome construction are comparable across the two papers, the difference in sign cannot be attributed to measurement. It reflects a difference in institutional design: elevated political salience sharpens rather than dulls competitive pressure in a setting where performance is continuously observed and individually attributed.

The analysis also extends the partisan information channel beyond the analyst's own identity. The political orientation of the firm's CEO and employees independently predicts forecast pessimism even after controlling for the analyst's own misalignment. The aggregate political lean of the analyst's employing brokerage, however, produces a precisely estimated null. This contrast is not a failed test but a discriminating one, and it is particularly sharp when read alongside the brokerage-level coverage composition results. The brokerage's employees, most of whom are not analysts, do not produce rival forecasts on the covered firms, and their political orientation therefore has no channel through which to influence any specific forecast. The analysts at the same brokerage, by contrast, do produce rival forecasts and are benchmarked directly against each other. Partisan bias operates through channels of direct interaction and peer competition for accuracy, not through the broad institutional orientation of the employer.

Taken together, these results locate a boundary condition for the growing literature on partisan bias in finance. The bias is real, and the settings in which it has been documented are not idiosyncratic. But its magnitude depends on whether the institutional setting exposes it to competitive correction. Where peer competition is visible, benchmarked, and individually attributed, partisan bias is disciplined along multiple margins and, in the most competitive settings, eliminated entirely. Where those features are absent, bias persists and can be amplified by political stress. This has implications for how the partisan-bias-in-finance literature is aggregated into a single narrative about polarization and expert judgment: the universal claim is that bias is present across many settings; the more precise claim is that bias is present in proportion to the degree to which institutional design insulates expert judgment from competitive correction.

Several questions remain for future research. The mechanisms through which political diversity disciplines bias at the firm and brokerage levels — whether through direct discussion among colleagues, observational learning from peer forecasts, reputational concern, or formal performance incentives — are consistent with the aggregate pattern in the data but cannot be fully distinguished with the measures used here. The interaction between political diversity and other dimensions of professional diversity, including gender, education, and pre-analyst industry experience, is a promising direction for understanding how heterogeneity shapes expert judgment more broadly. And the competitive-visibility thesis has implications beyond the analyst setting: it predicts that any professional environment in which expert judgments are publicly attributed, continuously benchmarked, and subject to reputational consequences should exhibit similar disciplining effects, while environments that lack these features should exhibit persistence or amplification of partisan bias. Testing that prediction in other public, competitive settings — portfolio managers with publicly reported returns, economic forecasters whose predictions are tracked in surveys, or auditors whose opinions are disclosed — would sharpen the boundary the current paper locates.

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Figure 1: Partisan Conflict Index, 12-Month Rolling Average

This figure plots the 12-month rolling average of the Partisan Conflict Index (PCI) maintained by the Federal Reserve Bank of Philadelphia (Azzimonti, 2018). The PCI measures the degree of political disagreement among U.S. politicians at the federal level using a search-based algorithm that tracks the frequency of news reports on lawmakers' policy disagreements. Higher values indicate greater partisan conflict.

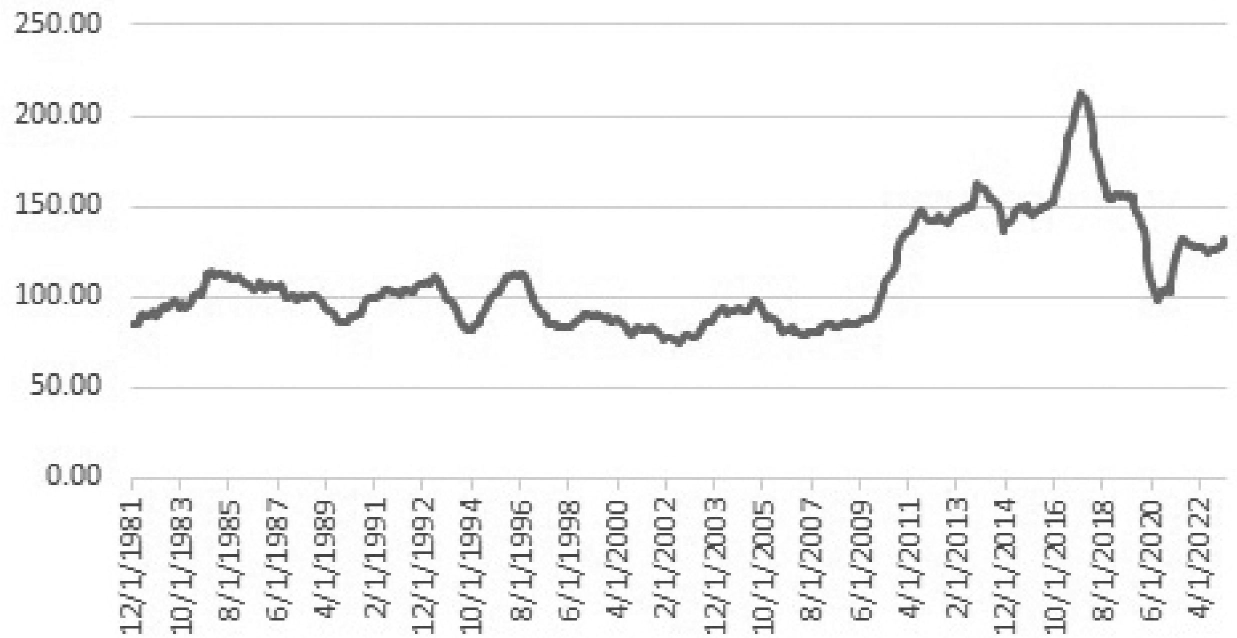

Figure 2: Distribution of Analyst Party Affiliation

This figure presents the overall distribution of analyst party affiliation in the sample. Each analyst-forecast observation is classified as Democrat, Republican, Other, or Unaffiliated based on voter registration records obtained from LexisNexis Public Records. The sample consists of 10,027 analyst-quarter-firm forecast observations from 1992 to 2020.

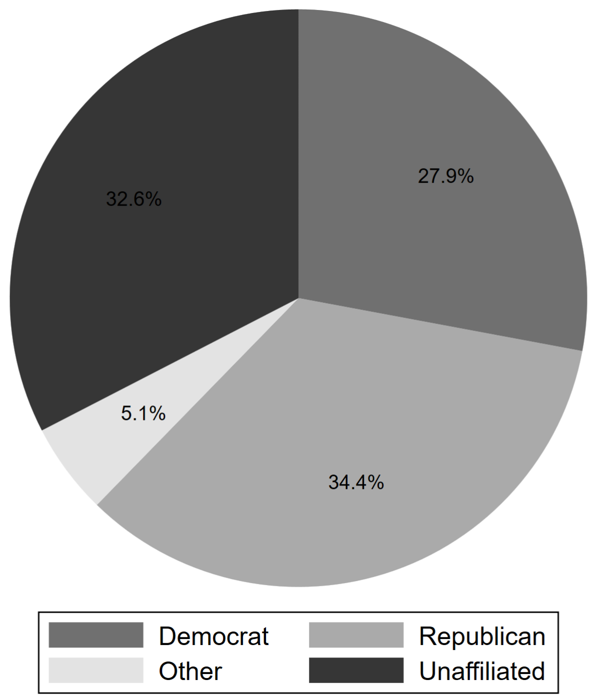

Figure 3: Distribution of Analyst Party Affiliation by Presidential Administration

This figure presents the distribution of analyst party affiliation by presidential administration. Each bar shows the percentage of forecast observations attributed to Democrat, Republican, Other, and Unaffiliated analysts during each presidential term. Administrations are ordered chronologically from right to left, beginning with Clinton's first term and ending with the Biden administration.

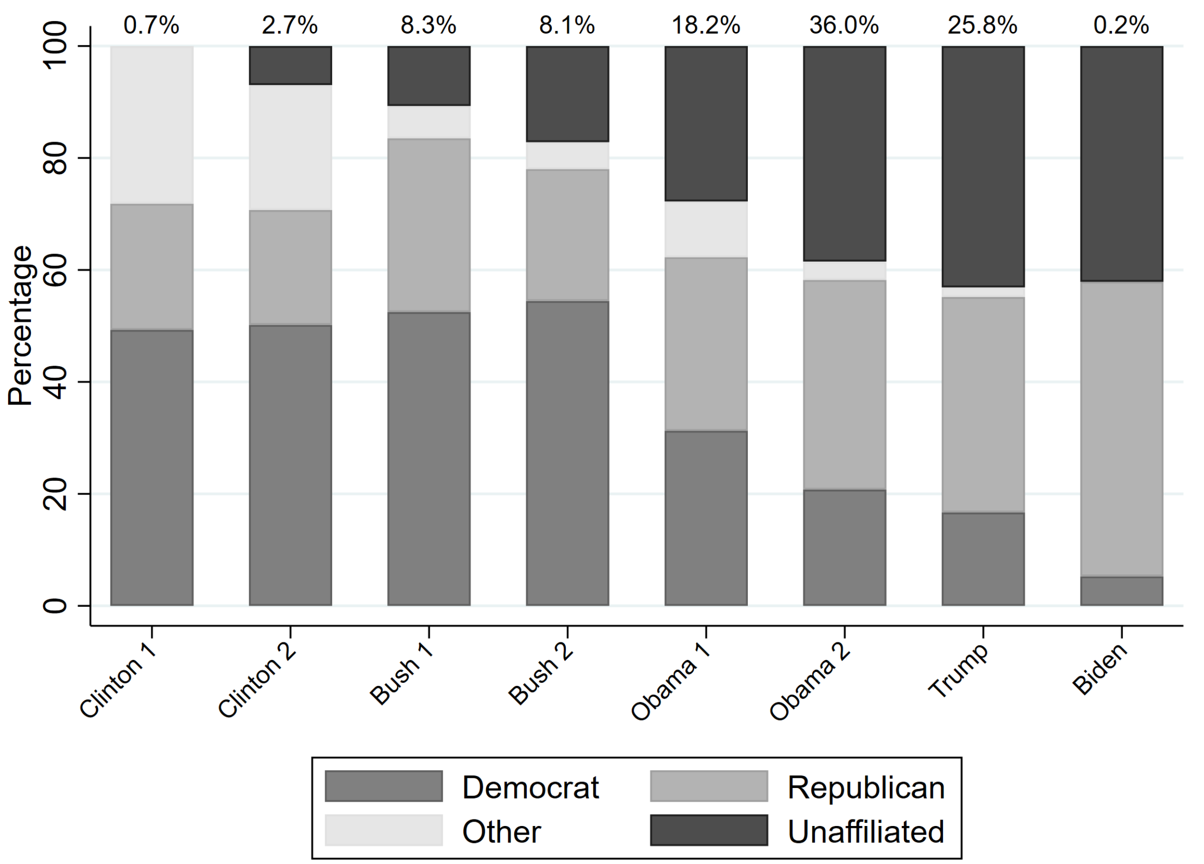

Figure 4: Distribution of Analyst Party Affiliation by Industry

This figure presents the distribution of analyst party affiliation by one-digit SIC industry classification. Each bar shows the percentage of forecast observations attributed to Democrat, Republican, Other, and Unaffiliated analysts within each industry. Industry classifications are based on the CRSP Standard Industrial Classification Code (SICCD) of the covered firm.

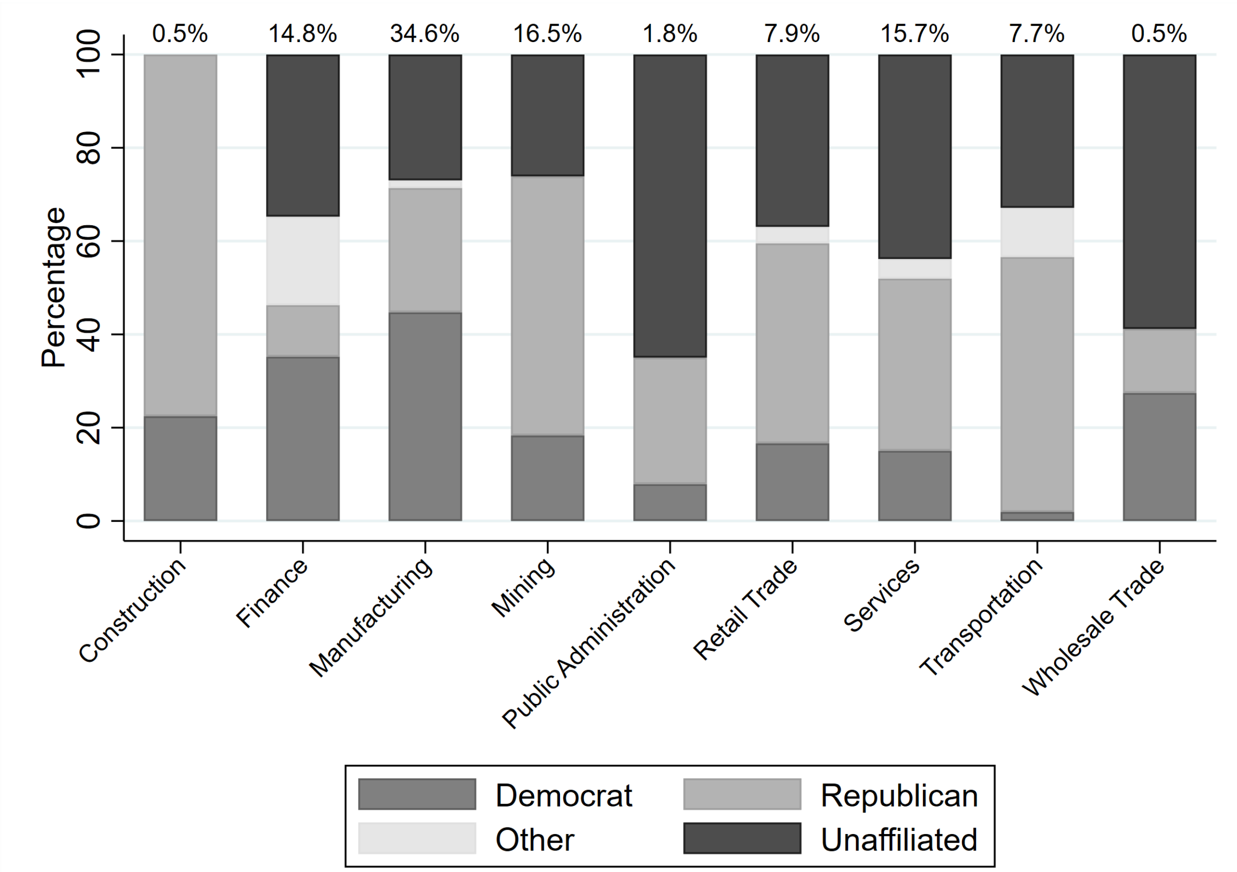

Table 1: Descriptive Statistics

This table reports summary statistics for the main variables used in the analysis. *Forecast Error* is the difference between the forecast earnings and the actual earnings, scaled by the stock price as of the previous month-end, multiplied by 100. *Misalign Analyst* is a dummy variable equal to one if the analyst's party affiliation does not match that of the president. *Tenure* is the number of quarters since the analyst's first earnings forecast for a given broker. *Num Covered* is the number of firms covered by the analyst in a given quarter. *Distance* is the number of days between the forecast issue date and the fiscal period end date. *Age* is the analyst's age in years. *Democrat*, *Republican*, *Other Party*, and *Unaffiliated* are indicator variables for the analyst's party registration. *Female* and *Male* are indicator variables for analyst gender. Variable definitions are in Appendix Table A1.

	N	Mean	Std. Dev.	25^{th} Pctl.	50^{th} Pctl.	75^{th} Pctl.
Forecast Error	10,027	0.058	0.877	−0.121	−0.026	0.044
Misalign Analyst	10,027	0.381	0.486	0.000	0.000	1.000
Tenure (quarters)	10,027	12.849	10.745	4.000	11.000	19.000
Num Covered	10,027	6.076	3.486	3.000	5.000	8.000
Distance (days)	10,027	134.692	137.028	56.000	70.000	166.000
Age	9,803	45.321	9.632	38.000	44.000	52.000
Democrat	10,027	0.279	0.449	0.000	0.000	1.000
Republican	10,027	0.344	0.475	0.000	0.000	1.000
Other Party	10,027	0.051	0.221	0.000	0.000	0.000
Unaffiliated	10,027	0.326	0.469	0.000	0.000	1.000
Female	8,245	0.090	0.286	0.000	0.000	0.000
Male	8,245	0.910	0.286	1.000	1.000	1.000

Table 2: Party Affiliation Distribution by State

This table presents the distribution of party affiliations of the analysts by voter registration state. Since analysts can change party affiliation during the sample period, the distribution is reported at the forecast level. *% Forecasts* reports the share of total forecasts attributable to analysts registered in each state. States are those covered by the LexisNexis Public Records voter registration database. Analysts in states not covered by LexisNexis are excluded from the sample.

State	% Forecasts	Democrat	Republican	Other	Unaffiliated
Arkansas	4.76%	97	94	97	189
Colorado	2.90%	0	291	0	0
Connecticut	17.30%	112	904	67	652
District of Columbia	0.46%	0	46	0	0
Florida	3.72%	145	171	0	57
Louisiana	0.76%	0	0	0	76
Minnesota	8.32%	0	0	0	834
New Mexico	0.26%	26	0	0	0
New York	36.37%	1,817	866	350	614
Ohio	1.46%	46	0	0	100
Pennsylvania	4.97%	190	212	0	96
Texas	18.43%	368	862	0	618
Wisconsin	0.30%	0	0	0	30

Table 3: Partisan Misalignment and Forecast Error

This table reports OLS regression results of the effect of analyst partisan misalignment on earnings forecast error. The dependent variable is *Forecast Error*, the difference between the forecast earnings and the actual earnings, scaled by the stock price as of the previous month-end, multiplied by 100. The key independent variable is *Misalign*, a dummy variable equal to one if the analyst's party affiliation does not match that of the president. Variable definitions are in Appendix Table A1. The t-statistics are reported in parentheses. Standard errors are double-clustered by analyst and firm. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

	(1)	(2)	(3)	(4)
Misalign	−0.089**	−0.080***	−0.159**	−0.159***
	(−2.104)	(−2.740)	(−2.427)	(−3.262)
Ln(Tenure)	−0.030*	−0.019	−0.085**	−0.048*
	(−1.734)	(−1.596)	(−2.572)	(−1.719)
Ln(Num Covered)	0.068**	0.040*	0.115***	0.061**
	(2.494)	(1.736)	(3.008)	(2.024)
Ln(Distance)	0.138***	0.075***	0.131***	0.072***
	(5.153)	(3.709)	(4.600)	(3.530)
Firm × Fiscal Quarter FE	Yes	Yes	Yes	Yes
Broker FE	Yes	Yes	Yes	Yes
Year-Quarter FE		Yes		Yes
Analyst FE			Yes	Yes
Observations	10,039	10,037	10,029	10,027
Adjusted R^2	0.643	0.715	0.663	0.730
Sample Mean				
Dep. Variable	0.058	0.058	0.058	0.058

Table 4: Robustness: Partisan Misalignment and Forecast Error

This table reports OLS regression results of the effect of partisan misalignment on forecast error under alternative specifications. The dependent variable is *Forecast Error*. The key independent variable is *Misalign* in columns (1)–(7) and *Align* in column (8), defined as in Table 3. Column (1) excludes analysts who are unaffiliated with any political party. Column (2) includes only analysts affiliated with the Democratic or Republican parties. Column (3) adds party fixed effects to absorb time-invariant differences across party affiliations. Column (4) adds voter state fixed effects to control for geographic differences in political culture. Column (5) adds firm × brokerage fixed effects, which control for the time-invariant match between a particular analyst's brokerage and a particular firm. Column (6) adds analyst × firm fixed effects, which compare the same analyst covering the same firm across different presidential administrations; analyst fixed effects are subsumed by analyst × firm fixed effects in this specification. Column (7) combines all additional fixed effects simultaneously. Columns (5)–(7) additionally cluster standard errors by brokerage. Column (8) replaces *Misalign* with *Align* under the baseline specification. Variable definitions are in Appendix Table A1. Controls are the same as in Table 3. Unless otherwise noted, all specifications include firm × fiscal quarter, broker, year-quarter, and analyst fixed effects. The t-statistics are reported in parentheses. Standard errors are double-clustered by analyst and firm unless otherwise noted. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Exclude Unaffil.	Only Dem/Rep	Party FE	State FE	Firm × Broker FE	Analyst × Firm FE	All FE	Align
Misalign (Align)	−0.207***	−0.249***	−0.156***	−0.159***	−0.185***	−0.188***	−0.180***	0.136***
	(−3.537)	(−3.862)	(−2.865)	(−3.181)	(−3.525)	(−3.448)	(−3.177)	(2.774)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × Fiscal Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Broker FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Analyst FE	Yes	Yes	Yes	Yes	Yes			Yes
Party FE			Yes				Yes	
Voter State FE				Yes			Yes	
Firm × Brokerage FE					Yes		Yes	
Analyst × Firm FE						Yes	Yes	
Observations	5,254	4,542	10,027	10,027	9,898	9,929	9,884	10,027
Adjusted R^2	0.829	0.834	0.729	0.729	0.724	0.741	0.671	0.729
Sample Mean								

Table 5: Partisan Misalignment and Battleground States

This table reports OLS regression results examining whether the effect of partisan alignment varies across political environments. The dependent variable is *Forecast Error*. *Battleground State* is a dummy variable equal to one if the analyst's voter registration state experienced a presidential election margin of five percentage points or less. Column (1) interacts *Battleground State* with *Misalign Analyst*. Column (2) interacts *Battleground State* with *Align Analyst*, where *Align Analyst* equals one if the analyst's party affiliation matches the president's party. Controls are the same as in Table 3. All specifications include firm × fiscal quarter, broker, year-quarter, and analyst fixed effects. The t-statistics are reported in parentheses. Standard errors are double-clustered by analyst and firm. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

	(1)	(2)
Misalign Analyst	−0.197***	
	(−3.804)	
Align Analyst		0.171***
		(3.109)
Battleground State	−0.132**	−0.027
	(−2.376)	(−0.464)
Battleground State × Misalign	0.273***	
	(3.134)	
Battleground State × Align		−0.168**
		(−2.418)
Controls	Yes	Yes
Firm × Fiscal Quarter FE	Yes	Yes
Broker FE	Yes	Yes
Year-Quarter FE	Yes	Yes
Analyst FE	Yes	Yes
Observations	10,027	10,027
Adjusted R^2	0.730	0.730
Sample Mean		
Dep. Variable	0.058	0.058

Table 6: Political Composition of Firm-Level Analyst Coverage

This table reports OLS regression results examining whether the political composition of the analysts covering a particular firm moderates the effect of partisan misalignment on forecast error. The dependent variable is *Forecast Error*. *Politically Unified Coverage* is an indicator equal to one if all partisan analysts covering the firm belong to the same party. *Politically Diversified Coverage* is an indicator equal to one if the firm is covered by at least one Democrat and one Republican analyst. Columns (1) and (3) define composition over the firm's entire coverage history in the sample, capturing sustained exposure to politically homogeneous or diverse peer forecasts. Columns (2) and (4) define composition at the annual (firm-year) level, capturing contemporaneous exposure. The contrast between the total and annual horizons is informative about the mechanism: if analysts discipline their forecasts by learning from the observed accuracy of politically different peers, the effect should emerge over long horizons rather than instantaneously. The main effects of coverage composition are absorbed by firm × fiscal quarter fixed effects. Controls are the same as in Table 3. All specifications include firm × fiscal quarter, broker, year-quarter, and analyst fixed effects. The t-statistics are reported in parentheses. Standard errors are double-clustered by analyst and firm. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

	Unified		Diverse	
	(1) Total	(2) Annual	(3) Total	(4) Annual
Misalign Analyst	−0.130***	−0.115***	−0.247***	−0.154**
	(−3.392)	(−2.848)	(−3.331)	(−2.540)
Misalign × Unified	−1.533**	−1.124**		
	(−2.108)	(−2.003)		
Misalign × Diversified			0.146**	−0.011
			(2.355)	(−0.192)
Controls	Yes	Yes	Yes	Yes
Firm × Fiscal Quarter FE	Yes	Yes	Yes	Yes
Broker FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Analyst FE	Yes	Yes	Yes	Yes
Observations	10,027	10,027	10,027	10,027
Adjusted R^2	0.734	0.733	0.730	0.730
Sample Mean DV	0.058	0.058	0.058	0.058

Table 7: Political Composition of Brokerage-Level Analyst Coverage

This table reports OLS regression results examining whether the political composition of analysts within a brokerage moderates the effect of partisan misalignment on forecast error. The dependent variable is *Forecast Error*. *Politically Unified Coverage* is an indicator equal to one if all partisan analysts at the brokerage belong to the same party. *Politically Diversified Coverage* is an indicator equal to one if the brokerage employs at least one Democrat and one Republican analyst. Columns (1) and (3) define composition over the brokerage's entire sample history, capturing persistent institutional homogeneity or diversity. Columns (2) and (4) define composition at the annual (brokerage-year) level, capturing contemporaneous cross-partisan exposure among colleagues sharing office space, meetings, and institutional norms. The contrast with Table 6 is informative about the mechanism: brokerage-level competition operates through direct daily interaction among colleagues, which acts contemporaneously, whereas firm-coverage competition operates through observation of past forecasts, which accumulates over longer horizons. The main effects of coverage composition are absorbed by fixed effects where applicable. Controls are the same as in Table 3. All specifications include firm × fiscal quarter, broker, year-quarter, and analyst fixed effects. The t-statistics are reported in parentheses. Standard errors are double-clustered by analyst and firm. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

	Unified		Diverse	
	(1) Total	(2) Annual	(3) Total	(4) Annual
Misalign Analyst	−0.148***	−0.149	−0.184	−0.190*
	(−2.836)	(−1.583)	(−1.629)	(−1.848)
Misalign × Unified	−1.897***	0.294		
	(−2.965)	(1.247)		
Misalign × Diversified			0.050	0.157*
			(0.434)	(1.839)
Controls	Yes	Yes	Yes	Yes
Firm × Fiscal Quarter FE	Yes	Yes	Yes	Yes
Broker FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Analyst FE	Yes	Yes	Yes	Yes
Observations	8,218	5,343	8,218	5,343
Adjusted R^2	0.732	0.732	0.732	0.732
Sample Mean DV	0.068	0.086	0.068	0.086

Table 8: Partisan Misalignment and the Partisan Conflict Index

This table reports OLS regression results examining how aggregate political conflict interacts with the effect of partisan misalignment on forecast error. The dependent variable is *Forecast Error*. *PCI Dummy* is an indicator equal to one for months in which the Partisan Conflict Index (Azzimonti, 2018) exceeds the median level observed within the current presidential administration. *VIX* is the CBOE Volatility Index. *EPU (News)* is the news-based Economic Policy Uncertainty index. *EPU (Three-Component)* is the three-component Economic Policy Uncertainty index. Controls are the same as in Table 3. *Baseline FE* refers to firm × fiscal quarter, broker, and analyst fixed effects, which are included in all columns. Unlike the specifications in Tables 3–7, I omit year-quarter fixed effects because PCI is itself a time-series variable measured at the monthly frequency; including year-quarter fixed effects would mechanically absorb the variation the test is designed to exploit. The t-statistics are reported in parentheses. Standard errors are double-clustered by analyst and firm. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Misalign Analyst	−0.223***	−0.203**	−0.202**	−0.194**	−0.196**	−0.190**
	(−2.618)	(−2.496)	(−2.429)	(−2.382)	(−2.407)	(−2.365)
PCI Dummy	−0.100**	−0.098***	−0.049	−0.040	−0.067*	−0.052
	(−2.586)	(−2.641)	(−1.415)	(−1.206)	(−1.857)	(−1.554)
Misalign × PCI Dummy	0.097**	0.090**	0.088*	0.088**	0.087**	0.087**
	(2.170)	(2.101)	(1.958)	(1.980)	(1.984)	(1.986)
VIX		−0.020***			−0.014**	−0.010**
		(−3.024)			(−2.507)	(−2.103)
EPU (News)			−0.002***		−0.001***	
			(−3.234)		(−2.901)	
EPU (Three-Component)				−0.004***		−0.003***
				(−3.367)		(−3.238)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Baseline FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,029	10,029	10,029	10,029	10,029	10,029
Adjusted R^2	0.664	0.672	0.671	0.676	0.674	0.677
Sample Mean						
Dep. Variable	0.058	0.058	0.058	0.058	0.058	0.058

Table 9: Partisan Misalignment Across Channels of the Information Environment

This table examines whether partisan misalignment in the firm's broader information environment independently predicts analyst forecast pessimism beyond the analyst's own misalignment. The four columns isolate different channels through which partisan information could influence forecasts. Column (1) reproduces the baseline result with analyst-level misalignment from Table 3. Column (2) adds *Misalign Firm CEO*, a dummy variable equal to one if the firm's CEO political donations lean toward the opposing party from the president, constructed from Federal Election Commission data. Column (3) adds *Misalign Firm Employees*, defined analogously using aggregate employee political donations. Column (4) adds *Misalign Brokerage*, defined using the brokerage's aggregate political donations. The contrast across columns is informative about the channels through which partisan bias operates: a significant effect in columns (2) and (3) combined with a null in column (4) would indicate that the bias travels through direct analyst-firm interaction rather than through the broad institutional orientation of the analyst's employer. Sample sizes vary across columns because FEC donation data are not available for all CEOs, firms, and brokerages in all election cycles. Controls are the same as in Table 3. All specifications include firm × fiscal quarter, broker, year-quarter, and analyst fixed effects. The t-statistics are reported in parentheses. Standard errors are double-clustered by analyst and firm. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

	(1)	(2)	(3)	(4)
Misalign Analyst	−0.159***	−0.206**	−0.095**	−0.191***
	(−3.262)	(−2.066)	(−2.364)	(−2.824)
Misalign Firm CEO		−0.127**		
		(−2.173)		
Misalign Firm Employees			−0.120**	
			(−2.356)	
Misalign Brokerage				0.014
				(0.320)
Controls	Yes	Yes	Yes	Yes
Firm × Fiscal Quarter FE	Yes	Yes	Yes	Yes
Broker FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Analyst FE	Yes	Yes	Yes	Yes
Observations	10,027	2,743	5,874	6,370
Adjusted R^2	0.730	0.757	0.691	0.744
Sample Mean DV	0.058	0.036	0.021	0.085

Table 10: Analyst-CEO Partisan Alignment and Forecast Error

This table tests whether the partisan alignment between an analyst and a covered firm's CEO moderates the baseline misalignment effect. Unlike the geographic, firm-coverage, and brokerage-coverage channels examined in Tables 5–7, the analyst-CEO dyad lacks a competitive structure in which opposing partisan views are publicly contested and benchmarked against realized outcomes, and the disciplining mechanism emphasized in this paper is correspondingly muted. The dependent variable is *Forecast Error*. *Analyst Aligned with CEO* is a dummy variable equal to one if the analyst's party affiliation matches the partisan lean of the firm's CEO, as measured by Federal Election Commission donation data. *Analyst Misalign with CEO* is defined analogously. The sample is restricted to firms with identifiable CEO political donations. Controls are the same as in Table 3. All specifications include firm × fiscal quarter, broker, year-quarter, and analyst fixed effects. The t-statistics are reported in parentheses. Standard errors are double-clustered by analyst and firm. Significance at the 1%, 5%, and 10% levels is indicated by ***, **, and *, respectively.

	(1)	(2)
Misalign Analyst	−0.176	−0.269**
	(−1.413)	(−2.439)
Analyst Aligned with CEO	0.132	
	(1.145)	
Misalign Analyst × CEO Aligned	−0.104	
	(−0.830)	
Analyst Misalign with CEO		−0.154
		(−1.252)
Misalign Analyst × CEO Misalign		0.140
		(1.069)
Controls	Yes	Yes
Firm × Fiscal Quarter FE	Yes	Yes
Broker FE	Yes	Yes
Year-Quarter FE	Yes	Yes
Analyst FE	Yes	Yes
Observations	2,743	2,743
Adjusted R^2	0.758	0.758
Sample Mean DV	0.036	0.036

Table A1: Variable Definitions

This table defines all variables used in the analysis. Panel A defines the dependent variable. Panel B defines the key independent variables. Panel C defines analyst and forecast characteristics used as controls. Panel D defines variables used in the political environment and diversity tests. Panel E defines coverage composition variables. Panel F defines aggregate political and economic conditions.

Panel A: Dependent Variable	
Forecast Error	The difference between the analyst's forecast earnings per share and the actual reported earnings per share, scaled by the absolute value of the stock price as of the previous month-end, multiplied by 100. A positive (negative) value indicates a more optimistic (pessimistic) forecast relative to actual earnings.
Panel B: Key Independent Variables	
Misalign	A dummy variable equal to one if the analyst's party affiliation does not match the party of the U.S. president at the time the forecast is issued. Following Dagostino, Gao, and Ma (2023) and Kempf and Tsoutsoura (2021), analysts who are unaffiliated with any party are assigned a value of zero. Labeled *Misalign Analyst* in specifications that include additional misalignment variables.
Align	A dummy variable equal to one if the analyst's party affiliation matches the party of the U.S. president. Unaffiliated and third-party analysts are assigned a value of zero. Labeled *Align Analyst* in specifications that include interaction terms.
Panel C: Analyst and Forecast Characteristics	
Tenure	The number of quarters since the analyst's first earnings forecast for a given broker. In regressions: Ln(Tenure).
Num Covered	The number of firms covered by the analyst in a given quarter. In regressions: Ln(Num Covered).
Distance	The number of days between the forecast issue date and the fiscal period end date. In regressions: Ln(Distance).
Age	The age of the analyst in years.
Democrat	Equals one if the analyst is affiliated with the Democratic party.
Republican	Equals one if the analyst is affiliated with the Republican party.
Other Party	Equals one if the analyst is affiliated with a party other than the Democratic or Republican parties.
Unaffiliated	Equals one if the analyst is not affiliated with any political party.
Female	Equals one if the analyst is female.
Male	Equals one if the analyst is male.

Table A1: Continued

Panel D: Political Environment Variables	
Misalign Firm CEO	Equals one if the political lean of the firm's CEO, measured by the direction of FEC political donations within a two-year federal election cycle, does not match the party of the U.S. president. Missing if both donation amounts are zero or data are unavailable.
Misalign Firm Employees	Equals one if the aggregate political lean of the firm's CEO and employees, measured by the direction of FEC donations, does not match the party of the U.S. president.
Misalign Brokerage	Equals one if the aggregate political lean of the analyst's brokerage house, measured by the direction of FEC donations, does not match the party of the U.S. president.
Battleground State	Equals one if the analyst's voter registration state experienced a presidential election margin of five percentage points or less.
Analyst *Aligned with CEO*	Equals one if the analyst's party affiliation matches the partisan lean of the firm's CEO, as measured by FEC donations.
Analyst *Misaligned with CEO*	Equals one if the analyst's party affiliation does not match the partisan lean of the firm's CEO, as measured by FEC donations.
Panel E: Coverage Composition Variables	
Diverse Coverage *Annual, Firm*	Equals one if the firm is covered by at least one Democratic and one Republican analyst in a given year. A firm is not classified as diversified if all partisan analysts belong to the same party, even if unaffiliated analysts are also present.
Unified Coverage *Annual, Firm*	Equals one if all partisan analysts covering the firm in a given year belong to the same party. The presence of any unaffiliated analyst prevents classification as unified.
Diverse Coverage *Total, Firm*	Equals one if the firm has been covered by at least one Democratic and one Republican analyst at any point over the entire sample period.
Unified Coverage *Total, Firm*	Equals one if all partisan analysts who have ever covered the firm across the entire sample period belong to the same party.
Diverse Coverage *Annual, Brokerage*	Equals one if the brokerage employs at least one Democratic and one Republican analyst in a given year.
Unified Coverage *Annual, Brokerage*	Equals one if all partisan analysts at the brokerage in a given year belong to the same party.
Diverse Coverage *Total, Brokerage*	Equals one if the brokerage has employed at least one Democratic and one Republican analyst at any point over the entire sample period.
Unified Coverage *Total, Brokerage*	Equals one if all partisan analysts who have ever been employed at the brokerage across the entire sample period belong to the same party.
Panel F: Aggregate Political and Economic Conditions	
PCI Dummy	Equals one for months in which the Partisan Conflict Index (Azzimonti, 2018) exceeds the median level observed within the current presidential administration.
VIX	The monthly average of the CBOE Volatility Index.
EPU (News)	The news-based Economic Policy Uncertainty index.
EPU *Three-Component*	The three-component Economic Policy Uncertainty index, which combines the news-based measure with tax code expiration data and forecaster disagreement about government spending and CPI.