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THE POLITICAL POLARIZATION OF CORPORATE AMERICA

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ABSTRACT

U.S. executives are increasingly segregating by political party. Using voter registration records for top executives of S&P 1500 firms between 2008 and 2022, we document that while the executive pool shifted toward Democrats, partisan segregation across firms increased sharply. Intensifying segregation offsets 74% of the decline in team-level homogeneity implied by changing party composition. Turnover is the driving force: politically misaligned executives are more likely to depart, and executives who join top teams increasingly match the team's partisan majority. The trend accelerates after the 2016 presidential election, marking an inflection point in the political polarization of corporate America.

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1. Introduction

Americans are increasingly divided across partisan lines (e.g., Iyengar, Sood, and Lelkes, 2012; Mason, 2013; Lott and Hassett, 2014; Mason, 2015; Gentzkow, 2016; Boxell, Gentzkow, and Shapiro, 2017; Iyengar, Lelkes, Levendusky, Malhotra, and Westwood, 2019). Party identification is now a more significant predictor of Americans' core political values than any other social or demographic divide (Pew Research Center, 2017). Social relationships, including marriages, are becoming more politically homogeneous (e.g., Iyengar, Konitzer, and Tedin, 2018; Huber and Malhotra, 2017). However, comparatively little is known about partisan segregation in the workplace, how it has evolved over time, and the factors driving it.[1]

To fill this gap, we study partisan segregation among key decision-makers in firms: executive teams. Top executives in publicly listed firms offer an ideal setting for several reasons. First, SEC disclosure requirements make their identities publicly available, enabling us to link them to voter registration records and determine their party affiliations. Second, executives are responsible for designing and executing important corporate decisions (Bertrand and Schoar, 2003), so partisan segregation within executive teams could have significant implications for firm outcomes. Consistent with this view, recent research shows that political partisanship shapes the economic expectations and investment decisions of both households (e.g., Mian, Sufi, and Khoshkhou, 2023; Meeuwis, Parker, Schoar, and Simester, 2022) and economically sophisticated agents in high-stakes environments (e.g., Kempf and Tsoutsoura, 2021; Dagostino, Gao, and Ma, 2023; Cassidy and Vorsatz, 2025).

Whether partisan segregation among top executives mirrors trends observed in other social contexts is not clear a priori. If partisan segregation is inefficient, competitive pressures may limit its extent in the workplace. Consistent with this view, the workplace has historically been more politically diverse and provided more opportunities for cross-party interactions than families, neighborhoods, or voluntary associations (Mutz and Mondak, 2006; Hertel-Fernandez, 2020). Moreover, investors, regulators, and stock exchanges have pressured firms to increase demographic diversity in the C-suite and on boards of directors (e.g., Wall Street Journal, 2021), potentially

[1] Notable exceptions include Gift and Gift (2015) and McConnell, Margalit, Malhotra, and Levendusky (2018), who explore the role of partisanship in labor market experiments, and contemporaneous work by Colonnelli, Neto, and Teso (2025), who show firm owners in Brazil are more likely to hire employees who share their political affiliation.

fostering broader diversity.

Using ExecuComp matched to voter registration records for top executives at S&P 1500 firms from 2008—2022, we first document a substantial shift in the partisan composition of the executive pool: the Democratic share among partisan executives rises by 4.5 percentage points (pp), from 29.5% in 2008 to 34.1% in 2022, and increases even more sharply among new joiners after 2016. Despite this shift, average team-level partisan homogeneity remains relatively flat over time, and the share of perfectly homogeneous teams even increases. We reconcile these facts by showing that assortative matching on party rises sharply after 2016, offsetting 74% of the decline in average team-level homogeneity that the compositional change alone would imply. We measure assortative matching by comparing the observed share of same-party executive pairs within firms to the share expected under random assignment given the overall party mix and team sizes. The difference, which we label excess homogeneity, is therefore a measure of assortative matching on party.[2]

We complement this approach with dyadic regressions, which treat each possible executive pair as an observation and allow us to simultaneously control for matching on other executive characteristics (gender, race, age). Executives sharing the same political party are 22.3% more likely to work at the same firm, with this effect approximately doubling between 2016 and 2022. Partisan matching intensifies even within the same metropolitan statistical area (MSA) and sector, implying it is not driven by increased partisan sorting into these areas. The pattern of increased partisan matching post 2016 is robust across alternative samples, including a balanced sample of firms present in all years, and when using political contributions instead of voter registrations.

A decomposition analysis shows that more than 90% of team-level changes in party composition are driven by executive turnover, with the remainder explained by new registrations and executives switching parties. We therefore investigate executive turnover as a driver of partisan segregation and find that partisan alignment plays an increasingly important role in both retention and entry into the top executive team. Teams with a Democratic majority are significantly more likely to add a new executive who is a Democrat, compared to teams with a Republican

[2]Throughout this paper, we use the terms partisan segregation and partisan matching of executives interchangeably. We use the title phrase *political polarization of corporate America* to describe the increase in across-firm partisan segregation.

majority, and this gap increases by approximately 50% in the post-2016 period. This increase in assortative matching on party among new joiners is more pronounced in teams that are already highly homogeneous. Complementing these team entry patterns, we observe a growing role of partisan alignment in predicting executive departures. Executives whose party affiliation differs from their team's majority are 2.8 pp more likely to depart on average, representing a 23.6% increase over the unconditional turnover probability of 12%, and the effect more than triples after 2016 relative to earlier years. We estimate the effect of partisan alignment on departure rates while including firm $\times$ year fixed effects, thus controlling for time-varying firm-level factors that might influence departure decisions.

Why did partisan segregation among U.S. executives intensify when it did? We provide direct evidence that rising partisan disagreement among executives may have contributed to this trend. We measure revealed disagreement by examining executives' personal portfolio choices around highly salient political events: presidential elections. By comparing the propensity to sell company stock among executives within the same firm but with different party affiliations, we assess their disagreement about how election outcomes affect their company's fundamentals. Democratic and Republican executives exhibit significant trading differences around the 2016 and 2020 elections, but not around the elections in 2008 or 2012. Given prior research linking executive beliefs to corporate financing and investment decisions (e.g., Malmendier and Tate, 2005), this suggests that partisan executives may increasingly disagree on how to manage their firms. Consistent with this interpretation, greater partisan disagreement within the same firm during the 2016 and 2020 elections is associated with larger gaps in subsequent departure rates between misaligned and aligned executives within the same firm.

The rest of this study proceeds as follows. In the next section, we discuss the related literature. Section 3 presents the data, sample construction, and summary statistics. Section 4 studies aggregate trends in the partisan composition of U.S. executive teams. Section 5 studies partisan matching at the executive-pair level using dyadic regressions, and Section 6 analyzes executive turnover. Section 7 explores partisan disagreement as a potential mechanism behind rising partisan segregation. Section 8 discusses our results, and Section 9 concludes.

2. Related Literature

We contribute to a growing literature on the connection between political partisanship and economic decisions. Much of the existing research has focused on households, examining the link between partisanship and household consumption (Gerber and Huber, 2009; McGrath, 2017; Gillitzer and Prasad, 2018; Mian, Sufi, and Khoshkhou, 2023), real estate transactions (McCartney, Orellana-Li, and Zhang, 2024), and portfolio allocation (Addoum and Kumar, 2016; Bonaparte, Kumar, and Page, 2017; Meeuwis, Parker, Schoar, and Simester, 2022; Giglio, Maggiori, Stroebel, and Utkus, 2021). More recently, studies have shown that partisanship also explains the economic decisions of more sophisticated individuals in high-stakes environments, such as credit analysts (Kempf and Tsoutsoura, 2021), loan officers (Dagostino, Gao, and Ma, 2023), entrepreneurs (Engelberg, Guzman, Lu, and Mullins, 2024), mutual fund managers (Cassidy and Vorsatz, 2025; Kempf, Luo, Schäfer, and Tsoutsoura, 2023), and judges (Gormley, Kaviani, and Maleki, 2026). Recent work also explores the role of political ideology and partisanship in corporate decisions and firm outcomes. For example, Duchin, Farroukh, Harford, and Patel (2023) find that the political distance between firms helps explain M&A activity and merger success; Hoang, Ngo, and Zhang (2025) show that CEOs in firms with polarized boards exhibit a lower forced turnover-performance sensitivity, and Rice (2024) investigates the relationship between executives' political partisanship and firms' investment decisions. To the best of our knowledge, our study is the first to document a rise in partisan segregation among executive teams in the United States.

Our paper also contributes to the literature on diversity in executive teams and boards of directors. Previous research has explored the role of similarities in demographics, skills, and past experience (e.g., Adams and Ferreira, 2009; Westphal and Zajac, 1995; Bernile, Bhagwat, and Yonker, 2018; Fos, Jiang, and Nie, 2025) in the selection of board members and executives, as well as their relationship with firm performance. A key difference between these studies and ours is that we focus on political diversity, which is less frequently discussed in the academic and public debate about diversity within corporate leadership. Political affiliation, however, increasingly predicts differences in social attitudes across individuals, as shown by Bertrand and Kamenica (2023).

In addition to our study, a few other papers have examined political alignment within a

firm's leadership. Lee, Lee, and Nagarajan (2014) use political contributions to measure political alignment between CEOs and board members and find that higher alignment is associated with less board independence, greater managerial entrenchment, and lower firm value. Bonica (2016a) documents substantial heterogeneity in the political preferences of directors both across and within firms. Expanding beyond top leadership, Bermiss and McDonald (2018) focus on the U.S. private equity industry and show that employees whose political ideology diverges from the dominant ideology of their organization are more likely to leave.[3] Our study differs in several ways. We focus on corporate executives rather than board members, use voter registration records instead of political contributions to infer partisan leanings, and, most importantly, investigate the time trend in partisan segregation among executives.

3. Data Sources and Sample Description

3.1. ExecuComp

We obtain information on the firm's top-earning executives from the ExecuComp database, maintained by Standard & Poor's (S&P). ExecuComp covers all companies in the S&P 1500 index. It uses compensation data from firms' annual proxy statements (form DEF 14A), in which firms are required to report compensation data for chief executive officers, chief financial officers, and the three other most highly compensated executives. In addition to compensation information, ExecuComp contains the full names of the executives, their gender, age, and role in the firm. ExecuComp is frequently used to define executives in the economics and finance literature (e.g., Bertrand and Schoar, 2003; Matsa and Miller, 2011). The coverage starts in 1992, but we restrict the sample to 2008–2022, as this period offers better alignment with the voter registration data used to infer party affiliation (see below). After restricting the sample to the above time period and to firms headquartered in the U.S., the ExecuComp database spans 31,142 executives in 2,627 firms.

To infer executives' race from their first and last names, we use the name-prism.com API (see Ye, Han, Hu, Coskun, Liu, Qin, and Skiena, 2017, for details). We have verified the accuracy

[3] A large literature examines the benefits and costs of team homogeneity, starting with the seminal work of Becker (1957). Marx, Pons, and Suri (2021) offer an excellent summary. See Evans, Prado, Rizzo, and Zambrana (2025) and Vorsatz (2022) for examples of recent work studying the relationship between political diversity and team performance in the financial sector.

of the API using voter registration data from North Carolina, which contain information on voters' race. Among the executives that we were able to match to voter records from North Carolina, the accuracy of the API in predicting race (white vs. non-white) is 93%.

An important feature of ExecuComp is that, in addition to covering CEOs and CFOs, it defines other members of top management teams based on their compensation. The advantage of this approach is that it provides a clear and objective criterion for executives' inclusion in the sample that is applicable across all industries. The main disadvantage is that, when an executive enters or leaves the sample, we cannot immediately distinguish whether this entry or exit is due to changes in her employment status at the firm or due to changes in her compensation rank at the firm. We view this disadvantage as relatively minor for two reasons. First, instances where an executive starts or ceases to be a top earner constitute an interesting economic outcome. Second, in the vast majority of cases, disappearances from ExecuComp are due to 'real' departures; i.e., executives leaving their firms.[4]

3.2. Political Affiliation Data

We infer executives' political affiliations from voter registration records, which we obtain from two sources. Our first source are voter registration records obtained directly from the boards of election in California (Contra Costa, Marin, Napa, San Francisco, San Mateo, Santa Clara, Sonoma), Colorado, Illinois, Massachusetts (Boston, Cambridge), North Carolina, New Jersey, New York (New York City), Ohio, and Texas. We restrict the sample to these locations because the boards of election in other states either do not directly share voter registration records with academics, do not track voters' party affiliations over time, or do not provide voter histories going back to at least 2008.[5] In Section IA.A of the Internet Appendix, we describe in more detail the information available in the voter registration records of each location and how they were obtained.

The second source of voter records is commercial data provided by L2 Inc, which is described in more detail in Internet Appendix IA.A.2. The L2 data is commonly used by political campaigns

[4]In a randomly selected sample of 100 executive disappearances from ExecuComp, we find that 85% of those departures represented a termination of the executive's employment at the firm.

[5]We use county-level data for California and city-level data for New York City, Boston, and Cambridge, because the statewide data for California, New York, and Massachusetts do not contain historical party affiliations.

and has also been employed in the academic literature of economics and political science (e.g., Allcott, Braghieri, Eichmeyer, and Gentzkow, 2020; Spenkuch, Teso, and Xu, 2023). It contains information on registered voters in all 50 states (and DC). For 34 states and DC, L2 assigns political affiliation based on party registration. For six states, it uses information on primaries. In the remaining states, it uses a combination of information on primaries, demographics, and commercial lifestyle data. We only use political affiliations inferred from party registrations or primaries. For the purpose of our study, L2 has two important advantages. First, it covers the entire United States, allowing us to substantially increase the share of firms and executives that we can cover. Second, because L2 provides year-by-year snapshots, the L2 sample is immune to concerns about potential data purging in voter registration files (Kim and Fraga, 2022). However, it has the disadvantage of starting only in 2014. We therefore combine the data from L2 and the boards of election in a procedure described in Internet Appendix IA.A.3.

The combined voter data contains identifying information, such as the voter's name, date of birth, and mailing address, as well as the voter's party affiliation at the time of a given election and an indicator for the election(s) in which the individual has voted. The elections covered are general and primary elections, and in some cases, municipal elections. Whenever possible, we infer political affiliation based on the voter's registration status at a given point in time. When registration status is unavailable, we infer political affiliation based on the primaries in which the individual has voted. For example, if a voter has most recently voted in a Republican primary, we will classify her as Republican. For voters in L2, we backfill the first observed party affiliation in order to increase our data coverage prior to 2014.

For the purpose of our study, voter registration data have important advantages over the more commonly used data on financial contributions to political parties, candidates, and committees, such as those found on the Federal Election Commission (FEC) website.[6] First, voter registrations are more likely to reflect individuals' personal political views than are their political contributions, which could be made for other reasons. An ongoing debate among political scientists concerns the extent to which political contributions reflect consumption or investment motives, that is, the extent to which individuals donate in order to derive a consumption benefit

[6]See https://www.fec.gov/.

or to influence political outcomes (e.g., Gordon, Hafer, and Landa, 2007). Political donations may also be influenced by social pressures (e.g., Babenko, Fedaseyeu, and Zhang, 2019, provide evidence that CEOs influence the political contributions of other employees) or by the business interests of the individual's employer, with the latter being particularly relevant for corporate executives. Second, a notable share of executives (39% in our sample) contribute to both parties, making inferring a clear party preference difficult. Third, party registration has been shown to be a good predictor of self-reported party identification. Pew Research Center (2018) matches commercial voter files, which are based on data from voter registration records, with a large-scale survey on political attitudes and voter behavior and shows that, for more than two-thirds of the panelists, the party affiliation in the commercial voter file correctly infers the self-reported party identification. The accuracy is even higher for states with party registration, such as New York. That said, we show in our robustness tests that our main finding—the increasing partisan segregation of executives—also obtains if we use political contributions of executives rather than voter records to infer party affiliations.

3.3. Insider-Trading Data

Sections 10(b) and 16(b) of the Securities Exchange Act of 1934 serve as the base for regulating insider trading. We obtain data on insider trading from the Thomson Reuters Insider Filing Data Files, which include data on insider trading activities by corporate executives. We use information from Table 1 of the Thomson Reuters Insider Filing Data Files, which discloses transactions at the insider-security level, and primarily focus on security sales, because the vast majority of insider transactions constitute sales of shares obtained as part of the executive's compensation package. We merge the insider-trading data to our sample of corporate executives from ExecuComp using company names and executives' first and last names.

3.4. Additional Data Sources

We collect financial information and Global Industry Classification Standard (GICS) codes for the companies in our sample from Compustat. Throughout the paper, we define industries based on GICS sectors. To obtain the address of the firm's historical headquarters, we use the

information found in the header section of the firm's 10-K/Q filings.[7] When location data from historical filings are unavailable, we use address information from Compustat.

To track the location of executives' residences, we use the Infutor dataset. Infutor provides address histories for more than 160 million U.S. residents, covering up to 10 addresses or 30 years of address history for each individual. Their data are aggregated from various public sources, such as phone connects and disconnects, real estate deed and property data, mover-reported address changes, and professional registries. In addition to address histories, Infutor also contains individuals' first and last names, year of birth, and gender. In Internet Appendix IA.A.4, we describe in detail how we link the executives in our sample to address histories from Infutor.

3.5. Sample Construction

We match executives to the combined voter data as follows. We use first name, middle initial, and last name to merge executives with voters, keeping only exact matches with age gaps less than or equal to three years whenever the executive's age is available. For executives who are matched to multiple voter records within the same state, we apply additional criteria, such as a criterion based on the geographical distance between the matched voters and the executive's residence or company location. Our merging procedure is described in more detail in Internet Appendix IA.A.4.

We are able to match 16,681 (=53.6% of) executives to a unique voter.[8] Out of the 16,681 executives that we match, 12,632 executives working in 2,529 firms are at some point affiliated with either the Democratic or Republican party. As shown in Panel A of Internet Appendix Figure IA.I, the share of executives who can be matched to a unique voter record varies between 52% and 62%, and the share of all executives with a Democratic or Republican party affiliation varies between 41% and 46% depending on the year.

[7] We thank Bill McDonald for making these data available on the University of Notre Dame's Software Repository for Accounting and Finance at https://sraf.nd.edu/data/augmented-10-x-header-data/.

[8] In one of our robustness tests discussed in Section 5, we follow Spenkuch, Teso, and Xu (2023) and repeat the matching procedure allowing for less stringent matching requirements on middle names and location, resulting in linking about 64.8% of all executives in ExecuComp to a unique voter record. This match rate is comparable to Spenkuch, Teso, and Xu (2023), who are able to link 67.5% of U.S. federal bureaucrats to a unique voter in L2. The results are consistent across our main sample and the extended sample constructed using laxer matching requirements.

Throughout most of our analysis, we further restrict the sample to firms with at least two partisan executives, reducing the sample to 2,300 unique firms. This reduced sample of firms still constitutes 69% of all ExecuComp firms on average, with coverage ranging between 63% and 71%, depending on the year (see Internet Appendix Figure IA.I, Panel B). Moreover, we show in Sections 4 and 5 that our main findings replicate also in a balanced sample of firms.

To better understand the potential differences between our sample and the overall population of executives and firms in the ExecuComp database, we first investigate whether executives whom we are able to match to a voter record run different types of companies. The results, reported in Internet Appendix Table IA.I, show that executives with a voter match run firms that are slightly larger and have lower leverage and cash holdings than firms run by executives without a matching voter record. Second, in terms of selection based on observable executive characteristics, we do not expect executives who are registered voters to be representative of the overall population of U.S. executives. A comparison of matched and nonmatched executives, also reported in Internet Appendix Table IA.I, reveals that older and white executives, as well as executives with longer tenure and in more prominent positions (e.g., CEO, CFO) are more likely to be matched to a voter record. Moreover, the executives in our sample are more likely to make political contributions and, conditional on contributing, their share of Democratic donations tends to be lower than that of unmatched executives.

3.6. Summary Statistics

Table 1 reports summary statistics for the five samples used in our subsequent analysis. Across all panels, we restrict the sample to executive teams with at least two partisan executives. Panel A reports statistics for the firm-year-level variables. The average share of Democratic and Republican executives among the partisan executives is 29.7% and 70.3%, respectively, with a standard deviation of 30.3%. The average partisan homogeneity (*Partisanship*), which we measure as the probability that two randomly drawn executives from the same team belong to the same party and which we discuss in more detail below, is equal to 63.4% (see Appendix A.1 for variable definitions). The average number of executives per team is 5.5, of which 3.8 can be matched to a voter record and 3.1 are affiliated with either the Democratic or the Republican party. The samples used in Panels B to E are described in more detail in the corresponding sections below.

[Insert Table 1 here]

4. The Partisan Composition of U.S. Executive Teams

This section provides a transparent accounting of how the partisan composition of U.S. executive teams has evolved over time. We begin by documenting trends in the composition of the overall executive pool, and then analyze team-level partisan homogeneity. We distinguish between changes in the team homogeneity driven by (i) shifts in the partisan composition of the executive pool and (ii) changes in partisan matching; i.e., whether executives are increasingly likely to work at the same firm as co-partisans. We also examine the relative importance of different channels through which within-firm team composition changes.

4.1. The Partisan Composition of the Executive Pool

Figure 1 illustrates the distribution of party affiliations at the executive level over time. Panel A reports the shares of executives who are registered as Democrats and Republicans among the partisan executives; i.e., the sample is restricted to executives who are either Republican or Democratic-affiliated in that year. The share of Republican executives increases slightly, from 70.5% in 2008 to 72.7% in 2016, followed by a striking decline of almost seven percentage points, to 65.9% in 2022. When comparing 2022 to 2008, the pool of executives has thus become substantially more politically balanced (less Republican-leaning).

[Insert Figure 1 here]

Because only about 10% of executives turn over in a given year, the changes in partisan composition of the overall pool mask more dramatic composition changes among joiners. In Panel C, we therefore plot the Democratic share not only among the entire pool of executives, but also among joiners, defined as executives who appear for the first time at a given firm in ExecuComp. Among joiners, the Democratic share increases even more dramatically in the second half of our time period, from 30.5% in 2016 to 39.4% in 2022. Between 2016 and 2019, the Democratic share among joiners increased by more than 10 pp.

In Panels B and D, we plot again the trends in the partisan composition of the executive pool, after including all executives whom we can link to a voter record (i.e., we add unaffiliated

and those affiliated with other parties). In Panel B, we observe a decrease in the share of unaffiliated executives over time. This result is partly mechanical, because in some states, we infer party affiliation from primary elections, and the likelihood of having voted in at least one primary election increases over time for each executive. To ensure our results are not driven by changes in the fraction of unaffiliated voters, we restrict most of our main analysis to Democrat and Republican executives and report results including unaffiliated voters in the robustness tests. Another important reason for excluding unaffiliated executives is that many self-declared independents have strong partisan allegiances (e.g., Abramowitz, 2018).

The dominance of the Republican party among U.S. corporate executives is consistent with Cohen, Hazan, Tallarita, and Weiss (2019), who find the majority of CEOs in S&P 1500 companies donate primarily to the Republican party. Bonica (2016a) finds similar evidence. We also use the cumulative donation amounts of the executives to infer party affiliation from political contributions and report these graphs in Internet Appendix Figure IA.III. Data on political contributions are obtained from Stanford's Database on Ideology, Money in Politics, and Elections (DIME) (see Bonica, 2016b), and an executive is classified as a Democrat (Republican) if she has made the majority of her cumulative contributions to the Democratic (Republican) party, respectively. The main difference between the contributions and the voter registration data is that the executive population as a whole is more politically homogeneous (i.e., more Republican) in the voter registration data than in the contributions data. Among executives who donate to a political party, the average share of executives who donate primarily to the Republican (Democratic) party is 56% (44%), respectively (see Internet Appendix Figure IA.III, Panel A). We also observe an increase in the share of executives donating primarily to the Democratic party after 2016, aligning with the trend that we observe in the voter registration data.

In Internet Appendix Figure IA.IV, Panels A and B, we plot the distribution of party affiliations inferred from political contributions separately for executives who are registered Democrats and registered Republicans. The two panels show that party affiliation inferred from voter registration records and from campaign donations are correlated: executives who are registered Democrats (Republicans) tend to give primarily to the Democratic (Republican) party, respectively. However, the correlation is not perfect, as can be seen from the sizable share of executives who have donated more to the party they are not registered with.

Finally, Internet Appendix Figure IA.II reports the shares of Democrats and Republicans by executive role and state of the firm's headquarters, respectively. In Panel A, we observe the lowest share of Democratic executives among COOs (24.0%), followed by CFOs (24.9%) and CEOs (25.4%). The share of Democrats is substantially larger among executives in general counsel / chief legal officer positions (48.5%). In Panel B, we restrict to the 15 states with the largest number of executives in our sample. The three states with the highest share of Democrats are Washington, New York, and Massachusetts, and the three states with the lowest share of Democrats are North Carolina, Florida, and Ohio. Internet Appendix Table IA.II further shows that female and non-white executives are more likely to be Democrats.

4.2. Aggregate Trends in Team-Level Partisan Homogeneity

Next, we investigate to what extent the increased Democratic representation among executives has translated into a decline in the partisan homogeneity of executive teams. We begin by examining how the distribution of executive teams by partisan dominance has evolved over time. Panel A of Figure 2 plots the fraction of teams falling into four categories, defined by the share of the dominant party: balanced (50–60%), moderately homogeneous (60–70%), highly homogeneous (70–99%), and perfectly homogeneous (100%). We restrict the sample to firm-years with at least two partisan executives.

Two striking patterns emerge. First, throughout our sample, approximately half of all executive teams are perfectly homogeneous. In contrast, only about one fifth of teams are politically balanced (50–60%). Second, the distribution remains surprisingly stable despite the substantial shift in the partisan composition of the executive pool. Between 2008 and 2022, the Democratic share among executives rose from 29.5% to 34.1%, moving the pool substantially toward partisan balance. Yet the share of perfectly homogeneous teams *increased* from 45.8% to 47.4%, and the share of balanced teams rose only moderately, from 18.5% to 21.3%.

[Insert Figure 2 here]

To provide intuition for the expected magnitude of these changes, consider a back-of-the-envelope calculation for the median team size of three executives. Under random assignment, when the pool's Democratic share is 29.5%, the probability that all three executives share the same party is 37.7%. When the pool's Democratic share is 34.1%, this probability falls to 32.6%,

representing an expected decline of 5.1 pp. The observed 1.6-pp increase in the share of perfectly homogeneous teams thus moves in the opposite direction from random matching, suggesting that partisan matching increased enough to offset the 5.1-pp implied decline.

This interpretation is reinforced by examining the most recent years. Between 2019 and 2022, the Democratic share of the pool continued to rise from 32.3% to 34.1%, yet the share of perfectly homogeneous teams actually increased by a striking 3.7 pp. This increase in extreme homogeneity while the pool became more balanced suggests that partisan matching has intensified precisely when compositional forces would otherwise have reduced team-level homogeneity.

To assess whether the stability we observe in Panel A of Figure 2 indeed reflects intensified partisan matching, rather than other factors such as a changing distribution of team size or sampling variation, we construct a summary measure of team-level partisan homogeneity that we can subsequently compare against a random-matching benchmark.

We measure partisan homogeneity of firm f's executive team in year t as the degree to which executives within the team are concentrated in a single party, using a size-adjusted Herfindahl index:

$$\widehat{Partisan}_{ft} = \frac{N_{ft} \times Partisan_{ft} - 1}{N_{ft} - 1}, \tag{1}$$

where $Partisan_{ft} = (\frac{D_{ft}}{D_{ft}+R_{ft}})^2 + (\frac{R_{ft}}{D_{ft}+R_{ft}})^2$ is the raw Herfindahl index, and D_{ft} and R_{ft} denote the number of Democratic and Republican executives in firm f in fiscal year t, respectively. N_{ft} is the total number of partisan executives in the team ($N_{ft} = D_{ft} + R_{ft}$), which we refer to as team size.

The raw Herfindahl index has a natural interpretation: it equals the probability that two executives drawn at random with replacement from the same team share the same party affiliation. The size-adjusted measure in equation (1) instead corresponds to the probability that two executives drawn without replacement share the same party affiliation; that is, the share of same-party pairs among all distinct pairs in the team. Following Hall, Jaffe, and Trajtenberg (2001), we size-adjust the raw Herfindahl index to correct for small-sample bias, ensuring that the measure provides an unbiased estimate of average homogeneity across teams of different sizes (see Internet Appendix IA.D.1). This is particularly important given that executive teams are small (typically between two and five partisan executives per team). Our measure is inversely

related to the fractionalization index proposed by Easterly and Levine (1997), which has been widely used to study the ethnic, linguistic, and religious diversity in populations (e.g., Alesina, Devleeschauwer, Easterly, Kurlat, and Wacziarg, 2003). In all subsequent analysis, *partisanship* and *partisan homogeneity* refer to the size-adjusted measure in equation (1).

Panel B of Figure 2 plots the average partisan homogeneity over time (solid line). Consistent with the distributional evidence in Panel A, average partisan homogeneity remains relatively flat between 2008 and 2022, declining by only 0.9 pp between the two endpoints despite the 4.5-pp increase in Democratic representation. This disconnect between pool composition and team-level homogeneity is more pronounced in recent years: between 2019 and 2022, average homogeneity increases by 1.0 pp even as the share of Democratic executives continues to rise.

To assess how much the observed level and evolution of partisan homogeneity reflects systematic sorting beyond random chance, we construct a counterfactual benchmark using Monte Carlo simulations.[9] For each firm-year, we keep the team size fixed and then simulate a counterfactual "random team" by assigning each executive a party label using the Democratic and Republican share observed in that year's executive pool.[10] We repeat this procedure 1,000 times, generating a distribution of partisan homogeneity under random matching for each firm-year and for the annual average across all firms. See Internet Appendix IA.D.2 for a detailed description of our simulation steps.

The dashed line in Panel B of Figure 2 plots the average partisan homogeneity under random matching for each year. For each of the 1,000 simulation runs, we compute the mean homogeneity across all firms in that year; the plotted line shows the average of these means across all simulations. The dotted lines show the 5th and 95th percentiles of this distribution of average homogeneity in each year. Three patterns stand out. First, the simulated average homogeneity inversely tracks the mean Democratic share, rising from 59.0% to 60.7% in 2017, then declining to 55.5% in 2022. Under random matching, the 4.5-pp increase in Democratic representation between 2008 and 2022 should have thus reduced average partisan homogeneity

[9] Our approach mirrors that in other studies of workplace segregation, which measure segregation above and beyond that which would occur if workers were matched randomly to firms or establishments (e.g., Hellerstein and Neumark, 2008; Boisso, Hayes, Hirschberg, and Silber, 1994).

[10] The partisan composition of the restricted sample used in our simulations (all partisan executives in firms with at least two partisan executives) is nearly identical to that of the full partisan executive population shown in Figure 1, differing by less than 1 pp in all years.

by 3.5 pp. Second, in all years, actual partisan homogeneity far exceeds the 95th percentile of the simulated distribution, allowing us to reject the null hypothesis of random matching with high confidence. Third, the gap between actual and expected homogeneity widens over time, especially after 2016. Specifically, the excess homogeneity (actual minus expected) increased from 3.3 pp in 2008 to 5.9 pp in 2022. In other words, intensifying partisan matching offsets approximately 74% of the decline in team homogeneity that should have occurred due to increased Democratic representation.

Panel C of Figure 2 converts this excess homogeneity into an intuitive metric. For each year, we ask: how lopsided would the executive pool need to be for random assignment to generate the observed level of team homogeneity? The difference between this hypothetical majority-party share and the actual majority-party share measures the composition-equivalent effect of partisan sorting; that is, the amount of "diversity erased by sorting." The solid line shows annual values; the dashed line plots a three-year moving average. In 2008, partisan sorting was equivalent to erasing 4.6 pp of minority-party representation; by 2022, this number increased to 8.1 pp. Both the raw excess homogeneity (Panel B) and the diversity-erased measure (Panel C) thus indicate that partisan sorting nearly doubled between 2008 and 2022 (Internet Appendix Figure IA.VI, Panel A, indexes both series to 2008=100 and plots them together).

As a robustness check, Internet Appendix Figure IA.VI, Panel B, presents an alternative measure of excess homogeneity: the share of firms with large absolute binomial z-scores, which test whether the observed number of Democratic executives in a given firm-year significantly deviates from the expectation under random assignment. For each firm-year, we compute:

$$z_{ft} = \frac{D_{ft} - N_{ft} \times p_D}{\sqrt{N_{ft} \times p_D \times (1 - p_D)}}, \tag{2}$$

where p_D is the population share of Democrats in the relevant population of executives in year t. Consistent with our main results, the share of firms with $|z| > 1.64$ increased from about 8% in 2015 to about 13% of all firms in 2022.

Taken together, the distributional evidence (Panel A) and the summary homogeneity measure (Panels B and C) provide consistent evidence that assortative matching on party has intensified substantially, offsetting most of the decline in team-level homogeneity that would have

occurred due to the substantially increased Democratic representation in the executive pool. Moreover, the increase in partisan matching occurs in the second half of our sample period.

4.3. Firm-Level Evidence

To test whether the increase in partisan matching is statistically significant and robust to controlling for firm characteristics, we estimate firm-level panel regressions. The dependent variable is standardized excess partisan homogeneity, defined as the difference between the team's actual and expected partisan homogeneity under random matching, divided by the standard deviation of the team's simulated homogeneity across 1,000 simulations. This standardization makes the measure interpretable across team sizes: it captures how many standard deviations a team's composition deviates from random matching, accounting for the fact that sampling variation differs mechanically with team size.

The descriptive evidence in Section 4.2 suggests a change in the dynamics of partisan segregation around 2016. We therefore use the 2016 presidential election as a natural breakpoint and formally test for a change in trend by estimating separate linear time trends before and after 2016. Specifically, we regress firm-level excess partisan homogeneity on fiscal year minus 2008, a post-2016 dummy (*Post-16*, which includes 2016 in the post period), and their interaction (*Year*×*Post-16*). We include team-size fixed effects (number of partisan executives) throughout. While our partisan homogeneity measure incorporates the Hall, Jaffe, and Trajtenberg (2001) bias correction to ensure unbiased estimation of average homogeneity across team sizes, and we further standardize by the simulation standard deviation, these adjustments do not fully eliminate the measurement challenges posed by very small teams. Homogeneity in two-person teams can only take two values (0% or 100%), while larger teams have more continuous distributions. The team-size fixed effects absorb mechanical differences across team sizes and ensure that time-trend estimates are not confounded by changes in the distribution of team sizes over time. However, the inherent discreteness of two-person teams means that statistical power remains limited for this subsample, as we discuss below.

Table 2, Panel A, reports the results, separated by minimum team size and clustering standard errors at the firm level. Columns (1)–(2) report the results for the full sample (i.e., all firms with at least two partisan executives), while columns (3)–(4) restrict the sample to teams

with at least three partisan executives. Two findings emerge. First, for teams with three or more executives, the *Year*×*Post-16* interaction coefficient of 0.0236 in column (3) indicates that excess partisan homogeneity increased by an additional 2.36% of a standard deviation per year after 2016, relative to a roughly flat pre-2016 trend (the *Year* coefficient is –0.0061). This estimate implies a cumulative difference of 14.2% (=2.36%×6) of a standard deviation over six years.

[Insert Table 2 here]

Second, and importantly for addressing concerns about compositional changes, the post-2016 acceleration becomes even larger and more statistically significant when we add firm fixed effects and time-varying firm controls in column (4). The *Year*×*Post-16* coefficient increases to 0.0322, indicating that even within the same firm over time and controlling for variation in firm size, leverage, cash holdings, profitability, investment, Tobin's Q, and revenue growth, executive teams become more politically homogeneous relative to the random-matching benchmark. This within-firm evidence rules out the possibility that our results are driven by changes in the composition of public firms (e.g., shifts toward firms in more homogeneous industries or locations).

In contrast, while the post-2016 increase for two-person teams (columns (1)–(2)) is economically meaningful (a cumulative increase of approximately 4.7% of a standard deviation), it is statistically insignificant, reflecting the fundamental discreteness of two-person team outcomes despite our measurement adjustments. We report both samples for transparency, but focus our interpretation on the $N \geq 3$ results given their higher statistical power.

Panel B restricts the sample to a balanced panel of firms present in all 15 years, directly addressing concerns about whether entry and exit of firms drives our results. The estimates are qualitatively similar and, if anything, stronger. For $N \geq 3$ teams, the *Year*×*Post-16* coefficient is 0.0466 without firm controls (column (3)) and 0.0395 with firm fixed effects and controls (column (4)), both highly statistically significant. This confirms that our findings are not driven by changes in the composition of firms over time. Internet Appendix Table IA.IV corroborates these findings using an indicator for firms with large binomial $|z|$-scores as an alternative measure of excess homogeneity.

In sum, the post-2016 increase in excess partisan homogeneity of U.S. executive teams is not only economically but also statistically significant, especially when we focus on larger teams

and a balanced panel of firms. Moreover, the within-firm and balanced-sample estimates indicate that the increase in partisan matching does not reflect changes in the types of firms that are represented in our sample. Instead, assortative matching on party intensifies within the same firms.

4.4. Drivers of Changes in Team Composition

The evidence thus far establishes that partisan matching has intensified substantially. A natural question is: through which margins does team composition change? More concretely, when a team's partisan composition shifts, is this driven by executives changing their party affiliation, by new voter registrations, or by executive turnover (entry and exit)?

To answer this question, we decompose within-firm changes in teams' party composition into three components: executives registering with a party for the first time, executives switching parties, and executive turnover (i.e., executives joining or leaving the team). We closely follow Brown, Cantoni, Enos, and Pons (2023), who use a decomposition approach based on partial derivatives to analyze shifts in the share of Democratic voters within a given geography. Details are provided in Internet Appendix IA.D.3.

Internet Appendix Figure IA.V presents the results from this decomposition for firm-years in which the Democratic share of executives increases (Panel A) or decreases (Panel B), respectively. Across both panels, executive turnover accounts for the vast majority of changes in team composition. Specifically, executive turnover accounts for approximately 92% of within-firm increases in the Democratic share, while new registrations and party switching account for only 2% and 6%, respectively. We obtain similar shares when we decompose within-firm decreases rather than within-firm increases in the Democratic share (see Panel B). Within-firm changes in partisan composition are thus driven overwhelmingly by entry and exit. Given the central role of turnover in shaping team composition, we investigate the role of partisan alignment in team entry and exit in Section 6 below.

5. Dyadic Regression Analysis

Sections 4.2 and 4.3 document rising excess partisan homogeneity in executive teams relative to a random-matching benchmark. While these team-level measures are transparent, they

are noisy in very small teams and cannot flexibly control for matching on other executive characteristics. We therefore complement the team-level evidence with a dyadic regression approach in the spirit of Colonnelli, Neto, and Teso (2025) that estimates the effect of shared party affiliation on the likelihood of two executives working at the same firm. This pair-level framework offers three advantages.

First, dyadic regressions allow us to flexibly control for shared demographics (age, gender, and race), ensuring that partisan matching is not confounded by assortative matching on these other characteristics. Second, the dyadic framework allows us to assess partisan matching within industries and geographies, helping us identify the level at which partisan segregation is most pronounced. Third, by analyzing executive pairs rather than teams, it addresses the measurement challenges posed by small teams in a different way. At the team level, two-person teams contribute substantial noise due to their discrete outcomes (only 0% or 100% homogeneity). At the pair level, every dyad contributes information regardless of team size, and the outcome variable (working at the same firm) is not mechanically constrained by team size.

Importantly, the dyadic and team-level approaches are directly linked. The dyadic regressions estimate how much more likely same-party executive pairs are to be co-employed at the same firm. The firm-level excess homogeneity measure captures how much more likely a pair of executives *employed by the same firm* shares a party relative to a randomly selected pair of executives. By Bayes' rule, a positive same-party matching effect mechanically implies higher within-firm same-party rates and therefore a larger gap between actual and simulated partisan homogeneity. We formalize this mapping in Internet Appendix IA.E.1.

5.1. Empirical Specification

We construct a sample of all hypothetical executive pairs for each fiscal year. Panel B of Table 1 reports summary statistics for this dyadic sample, in which the unit of observation is an executive pair-year. The sample is restricted to firm-years with at least two partisan executives, as well as to Democratic and Republican executives. The unconditional probability that two executives work at the same firm is 5.8 basis points (bps), and the probability that they share the same party affiliation is 59.0%. We then estimate:

$$SFirm_{ikt} = \alpha_t + \beta^{SP} SParty_{ikt} + \beta^{SG} SGender_{ik} + \beta^{SR} SRace_{ik} + \beta^{SA} SAge_{ik} + \epsilon_{ikt}, \qquad (3)$$

where $SFirm_{ikt}$ is an indicator equal to one if executives i and k work at the same firm in year t, and zero otherwise. We multiply the dependent variable by 10,000, so coefficients are interpretable as basis points. $SParty_{ikt}$ is an indicator equal to one if executives i and k have the same party affiliation in year t, and zero otherwise; $SGender_{ik}$, $SRace_{ik}$, and $SAge_{ik}$ are analogous indicators for shared gender, race (white vs. non-white), and age proximity (defined as a five-year age gap or less). α_t are year fixed effects, and standard errors are clustered at the executive pair level. We consider alternative variance estimators in our robustness tests below.

As shown in Internet Appendix IA.E.1, the coefficient β^{SP} captures partisan matching beyond what would arise under random assignment. As a validation, we conduct a placebo test by randomly assigning political affiliations using each year's observed party shares. Under this placebo, the estimated coefficients on $SParty_{ikt}$ are consistently close to zero and almost always statistically insignificant (see Internet Appendix Figure IA.VII, Panels A and B). The same is true when we allow the effect of shared party affiliation to vary over time (see Panels C and D), confirming that our dyadic regression estimates reflect excess same-party co-employment beyond random chance.

5.2. Results

To assess whether the degree of assortative matching on party has changed over time, we estimate equation (3) separately for each fiscal year in our sample. Figure 3 plots the estimated coefficient β^{SP} for each year. The results show a sharp increase in partisan assortative matching in the post-2016 period, with the estimated β^{SP} approximately twice as large in 2022 compared to 2016. While shared party affiliation was statistically significant even before 2016, its effect remained relatively stable between 2008 and 2016. These patterns suggest that intensified partisan matching within the C-suite is a relatively recent phenomenon, emerging around the 2016 presidential election.

[Insert Figure 3 here]

To formalize whether intensified same-party co-employment after 2016 is statistically sig-

nificant, we estimate separate linear time trends for the pre- and post-2016 periods:

$$SFirm_{ikt} = \beta_0 SParty_{ikt} + \beta_1 SParty_{ikt} \times Post\text{-}16_t + \beta_2 SParty_{ikt} \times Year_t \quad (4)$$
$$+ \beta_3 SParty_{ikt} \times Year_t \times Post\text{-}16_t + \gamma' X_{ikt} + \alpha_t + \epsilon_{ikt},$$

where $Post\text{-}16_t$ is an indicator equal to one for 2016 onward, and zero otherwise. $Year_t$ is defined as fiscal year minus 2008 in the $SParty_{ikt} \times Year_t$ term, and as fiscal year minus 2016 in the triple interaction. This specification implies that β_0 captures the effect of shared party affiliation in 2008, β_1 captures the discrete shift in 2016, β_2 measures the pre-2016 trend, and β_3 measures the change in trend post 2016. X_{ikt} includes controls for shared gender, race, and age proximity.

Table 3 reports the results. Column (1) estimates equation (3) without controls. Executives who share the same party are 1.29 bps more likely to work at the same firm, representing a 22.3% increase over the unconditional co-employment probability of 5.8 bps. To interpret the dyadic estimate in the same units as our firm-level analysis, we translate it into an implied (pair-weighted) excess partisan homogeneity, defined as the difference between the probability that two executives employed at the same firm share a party and the corresponding probability under random matching. Using sample average and the Bayes-rule mapping in Internet Appendix IA.E.1, the baseline dyadic coefficient implies an excess partisan homogeneity of about 5.4 pp, which matches the pair-weighted average excess homogeneity we see in our firm-year panel data. Given roughly 5,213 co-employed executive pairs per year, this corresponds to approximately 280 ($\approx$5,213$\times$0.054) excess same-party co-employment pairs relative to the random-matching benchmark.

[Insert Table 3 here]

Column (2) of Table 3 adds an interaction between shared party affiliation and the post-2016 indicator. The effect of shared party affiliation is 0.42 bps higher after 2016 compared to the pre-2016 average effect of 1.14 bps. Column (3) estimates the full specification in equation (4), allowing for different linear time trends before and after 2016. Consistent with Figure 3, we find no significant time trend before 2016, but the post-2016 acceleration is positive and highly significant. The coefficient of 0.1543 implies the same-party matching effect increased by an additional 0.93 bps (=0.1543$\times$6) between 2016 and 2022 relative to the pre-2016 trend,

representing a 16% increase relative to the unconditional mean of the dependent variable and an 80% increase relative to the estimated same-party effect in 2008.

Columns (4) and (5) add controls for matching on other observable executive characteristics: shared gender, race, and age proximity. Column (5) further allows these effects to vary over time by interacting each control with fiscal year and the post-2016 indicator. The estimated effect of shared party affiliation is virtually unchanged; if anything, the post-2016 acceleration becomes slightly more pronounced. This indicates that the trend toward greater same-party co-employment is distinct from and not confounded by assortative matching on other observable traits.

Internet Appendix Table IA.V reports the full set of coefficients. Unlike party affiliation, there is no evidence of assortative matching on gender at any point in the sample period. We do observe assortative matching on race, but its strength weakens after 2016. The only other characteristic for which assortative matching increases in the post-2016 subsample is age, although the effect is only marginally significant and smaller in magnitude than the increase for shared party affiliation.

5.3. Asymmetry in Partisan Matching Across Parties

Our baseline analysis pools Democrat–Democrat and Republican–Republican pairs under a single same-party indicator. We now relax this restriction to investigate whether partisan matching differs across parties and whether time trends vary by party. Specifically, we replace the pooled $SParty$ indicator with separate indicators for Democrat–Democrat pairs ($SPartyDem$) and Republican–Republican pairs ($SPartyRep$), with mixed-party pairs as the omitted group.

Internet Appendix Table IA.VI reports the results. Column (1) estimates party-specific matching effects without time trends; columns (2)–(4) progressively add controls, linear time trends, and post-2016 interactions. The analysis delivers two main insights. First, column (1) reveals substantial baseline asymmetry: Democrats are 2.15 bps more likely to share a firm with co-partisans, while Republicans are 1.15 bps more likely to do so. Second, columns (2) to (4) show that Republicans experience a significant increase in same-party co-employment after 2016, as can be seen from the positive and significant coefficient on $SPartyRep \times Year \times Post - 16$, whereas Democrats did not.

These findings refine our understanding of partisan assortative matching in executive labor markets. We document a clear asymmetry: Democrats exhibit higher rates of co-partisan co-employment than Republicans. After 2016, the dynamics are likewise asymmetric: Republicans, who start from lower baseline co-partisan co-employment, experience a pronounced increase, whereas Democrats remain at a relatively high but stable level. Taken together, the post-2016 increase in partisan sorting is driven primarily by a within-party change among Republicans, rather than by shifts in the overall partisan composition of the executive sample.

5.4. Robustness

Internet Appendix Table IA.VII reports robustness checks across alternative samples and measures of party affiliation. To facilitate comparison, all coefficients are standardized by dividing by the mean of the dependent variable. The post-2016 increase in partisan matching is robust to: (i) restricting to a balanced sample of firms present throughout 2008–2022 (Panel A); (ii) using only L2 data (Panel B) or only government voter files (Panel C); (iii) employing less stringent name and location matching criteria, thereby covering 65% of ExecuComp (Panel D); (iv) including unaffiliated executives (Panel E) or treating all unmatched executives as unaffiliated (Panel F);[11] (v) defining a static measure of party affiliation using each executive's modal affiliation over the full sample period (Panel G); and (vi) inferring party affiliation from political contributions rather than voter records (Panel H). The estimated magnitudes are, if anything, slightly larger than in the baseline specification.

Our baseline specification clusters standard errors at the executive-pair level to account for serial correlation within pairs. We also evaluate robustness to alternative variance estimators that account for dependence across dyads. First, we implement two-way clustering by the two executives forming each pair, allowing for arbitrary correlation across dyads that share a common executive (Cameron, Gelbach, and Miller, 2011). Standard errors change negligibly and statistical significance is unaffected (see Internet Appendix Table IA.VIII, Panel A). Second, we implement the dyad-robust variance estimator of Aronow, Samii, and Assenova (2015), designed specifically for dyadic data (see Internet Appendix Table IA.VIII, Panel B). Finally, we implement the sparse-network variance estimator of Graham (2024), which accounts for the fact that in sparse networks,

[11] Due to the large sample size in Panel F, we estimate regressions on a random 20% subsample.

effective sample size scales with the number of nodes (executives) rather than dyads (executive-pairs). Because Graham (2024) develops the estimator for cross-sectional settings, we estimate the model separately by year, as in Figure 3. Standard errors on average increase by 11% relative to baseline (see Internet Appendix Figure IA.VIII). Importantly, across all these tests, our inference remains unchanged.

In sum, the post-2016 increase in partisan matching is robust to alternative samples, party affiliation measures, and variance estimators designed for dyadic data.

5.5. Partisan Matching Within Industries and Geographies

We next investigate potential sources of the increasing partisan segregation among U.S. executives. Specifically, we examine whether partisan sorting into industries and geographies contributes to the trends documented above. If Democratic and Republican executives are increasingly working in different sectors or states, this could help explain the rise in partisan segregation. To test this hypothesis, we re-estimate our dyadic regression while restricting the sample to executive pairs who work in the same industry (i.e., GICS sector), state, or MSA. If the effect of shared party affiliation remains positive and continues to increase after imposing these restrictions, it would suggest that partisan segregation is also occurring within industries and geographies, rather than being solely driven by cross-industry or cross-state sorting.

Table 4 presents the results of this analysis. Column (1) replicates our baseline result from Table 3, column (5), but scales all coefficients by the mean of the dependent variable for easier comparability across subsamples. In column (2), we restrict the sample to executives working within the same GICS sector. The results show that significant partisan matching persists within industries, as indicated by the positive and significant coefficient on $SParty$, with an economic magnitude similar to that of the full sample.[12] The same is true for the triple interaction term, suggesting that increased partisan sorting into industries does not explain the rise in partisan segregation after 2016. The increase in partisan assortative matching after 2016 is most pronounced in the utilities sector, as well as in consumer discretionary, industrials, and energy (see Internet Appendix Figure IA.IX, Panel B).

[12] As shown in Internet Appendix Figure IA.IX, Panel A, most sectors already exhibited significant within-sector partisan matching in 2008.

[Insert Table 4 here]

In column (3) of Table 4, we restrict the sample to executives working for firms headquartered in the same state. Once again, we find a significant increase in partisan matching post-2016, with economic magnitudes similar to those in the full sample. As shown in Internet Appendix Figure IA.X, Panel B, the within-state increase in partisan matching varies substantially across states, with the strongest effects observed in Virginia, Washington, Ohio, and North Carolina. Column (4) of Table 4 further refines the analysis by testing for partisan matching within MSA. Even within MSA, we observe both a statistically significant level of partisan matching in 2008, and a strong post-2016 increase.

Taken together, the results in Table 4 indicate that the rise in same-party co-employment after 2016 is not driven by increased geographical or industry sorting of executives. The effect remains equally strong within sectors, states, and MSAs.

6. Executive Turnover

Our results so far indicate that, since 2016, executives have shown an increasing tendency to work with other executives who share their party affiliation. Moreover, in Section 4.4 we document that executive turnover is the primary driver of changes in a team's partisan composition. To further investigate the importance of political alignment in executive-team formation, our third empirical approach focuses on inflows and outflows; i.e., team entry and exit. Specifically, we test whether partisan alignment with a team's majority predicts who joins the team and who departs from it.

6.1. Partisan Alignment and Team Entry

We begin our analysis of executive turnover by constructing an executive-firm-year panel using data on executives' employment spells from ExecuComp. For the analysis of team entry, we focus on executive-firm-year observations in which an executive appears in a given firm for the first time in ExecuComp. We further restrict the sample to joiners whose party affiliation is Democratic or Republican and to destination firm-years with at least two partisan executives and a clear partisan majority.

We then predict the party affiliation of executive i joining firm f in year t using the destination team's existing partisan majority:

$$DemJoiner_{ift} = \alpha_{rt} + \alpha_{jt} + \gamma Majority\,Democrat_{f,t-1} + \delta' X_{it} + \varepsilon_{ift}, \tag{5}$$

where $DemJoiner_{ift}$ is an indicator equal to one (zero) if executive i is Democrat (Republican), respectively, and $Majority\,Democrat_{f,t-1}$ is an indicator equal to one (zero) if the dominating party of firm f's team in $t-1$ is Democrat (Republican), respectively. In our baseline specification, we define a firm to have a "clear" partisan majority if the team's lagged partisan majority exceeds the minority by more than one executive; we consider alternative definitions below. α_{rt} denote role (r) $\times$ year fixed effects and α_{jt} denote firm characteristic j (either headquarters state or GICS sector) $\times$ year fixed effects. These fixed effects flexibly control for time-varying demand and supply for specific executive roles as well as across geographies and sectors, thereby absorbing any shocks common to, for example, CFO positions or executive positions in California in a given year. Vector X includes observable executive characteristics (gender, race, and age). Under random matching, we would expect $\gamma = 0$; under positive assortative matching on party, we would expect $\gamma > 0$. Standard errors are clustered at the firm level. Summary statistics for the joiner sample are reported in Panel C of Table 1.

Table 5 reports the estimates of our coefficient of interest, γ. For brevity, we suppress the coefficients on the control variables and report the full set of coefficients in Internet Appendix Table IA.IX. Across all specifications, firms with a clear Democratic majority are significantly more likely to add a Democratic executive than firms with a Republican majority. Columns (1) and (2) use the full sample and differ in whether we control for sorting into geographies or industries. Column (1) includes role $\times$ year and sector $\times$ year fixed effects, along with individual controls for age, gender, and race. The estimate implies that a Democratic-majority team is 17.3 pp more likely to add a Democrat than a Republican-majority team, a 56% increase relative to the 31% unconditional mean. Column (2) replaces sector $\times$ year with state $\times$ year fixed effects. The coefficient declines to 10.1 pp, indicating that sorting into states explains part of the alignment gap, but a sizable and statistically significant effect remains even within the same state and year (a 32% increase).

[Insert Table 5 here]

Columns (3)–(6) split the sample into the subperiods 2008–2015 and 2016–2022. Consistent with the dyadic evidence, partisan alignment among executives who join a top team becomes substantially stronger after 2016. Relative to 2008–2015, the effect of *Majority Democrat* increases from 14.4 pp to 21.7 pp within sectors (about a 50% increase) and from 8.2 pp to 12.8 pp within states (about a 56% increase). Thus, the post-2016 intensification reflects stronger within-sector and within-state partisan alignment rather than increased partisan sorting into industries or geographies.

We also examine whether partisan alignment among new joiners is stronger in more polarized teams. To do so, we re-estimate our team-entry regression under alternative definitions of a "clear" partisan majority, varying the required majority margin. Panel A of Figure 4 plots the coefficient on $Majority\,Democrat_{f,t-1}$ from equation (5) separately for the pre-2016 and post-2016 periods under three alternative majority definitions: (i) a narrow definition requiring only that the majority party exceeds the minority (majority margin > 0); (ii) our baseline definition (majority margin > 1); and (iii) a stringent definition capturing highly homogeneous teams (majority margin > 2). Across all three majority definitions, the point estimates are larger post-2016 than pre-2016, indicating that partisan alignment in top-team entry strengthened broadly after 2016. Moreover, the increase is strongest for highly homogeneous teams. While teams with large majority margins exhibit little evidence of positive assortative matching prior to 2016, they display the sharpest increase in aligned team-entry in the post-2016 period.

[Insert Figure 4 here]

Finally, we convert the estimated partisan alignment among joiners into the intuitive "diversity erased" metric introduced in Section 4.2. Figure 5 asks, for each year, what joiner party composition would be required to generate the observed joiner-to-team alignment rate under random assignment. The difference between the dominant party share in this counterfactual composition and the dominant party share in the actual joiner composition captures the composition-equivalent effect of partisan sorting among joiners. We again report results separately for the three majority definitions and, to abstract from year-to-year volatility, plot the

three-year moving average for each series. Consistent with Figure 4, all three series trend upward, indicating an increase in aligned top-team-entry across majority definitions. Moreover, the increase is strongest for teams that are already highly homogeneous: early in the sample, aligned team-entry is stronger for teams with narrow majorities, but by 2022 the series converge, reflecting a disproportionate post-2016 acceleration in aligned entry among the most lopsided teams.

In terms of magnitude, focusing on the solid line (majority margin>1), partisan sorting erased about 3.6 pp of minority-party representation among new joiners in 2008; by 2022, this number increased to more than 15 pp. The implied diversity erased among new joiners thus increased more than fourfold, substantially exceeding the increase we observe in the stock of executives in Panel C of Figure 2, as expected given that the stock adjusts more slowly than the flow of new executives.

[Insert Figure 5 here]

6.2. Partisan Alignment and Team Exit

For the analysis of team exits, we keep the full employment spell for each executive and define the dependent variable, *Executive Departure*, as an indicator equal to one in the final year an executive is listed among the top earners of a given firm in ExecuComp, and zero otherwise. Panel D of Table 1 presents the summary statistics for this sample, which is restricted to Democratic and Republican executives, as well as to firm-years with at least two partisan executives and a clear partisan majority (as defined above). The unconditional probability of an executive departure is 12%. The average tenure in the current position is 8 years, 9.7% of executives are older than 65 years, 91% of executives are white, and 8.3% are women. Relative to the joiner sample, the stock of executives is thus slightly older, with a lower representation of women and minorities.

We then estimate the following regression:

$$Executive\,Departure_{ift} = \alpha_{ft} + \alpha_{rt} + \alpha_p + \beta Misaligned_{if,t-1} + \delta' X_{if,t-1} + \varepsilon_{ift}, \qquad (6)$$

where f, i, and t index firms, executives, and years, respectively, and p denotes the executive's party affiliation (Democratic or Republican). *Misaligned* is an indicator equal to one if the

executive's party affiliation differs from that of the team's majority as of the previous year-end, and zero otherwise. A team is classified as having a Democratic majority if the number of Democratic executives exceeds the number of Republican executives by more than one at the previous year-end, with a Republican majority defined analogously. The vector X includes time-invariant and time-varying individual-level control variables, such as tenure (both linear and squared), gender, race, age, and an indicator for executives older than 65. The time-varying controls are lagged. Because we focus on the entire stock of executives in this analysis, we are able to include firm $\times$ year fixed effects (α_{ft}), which absorb both time-invariant and time-varying firm characteristics, eliminating the need for additional firm-level control variables in the regression. We further include role $\times$ year fixed effects to control for time-variation in departure rates across executive roles.

Our coefficient of interest is β, which captures the difference in the likelihood of departure between executives who are misaligned with the team's majority versus those who are aligned. Because executive-party-affiliation fixed effects (α_p) are included in all regressions, β reflects the effect of having a different (as opposed to the same) party affiliation from the majority, while controlling for potential differences in average turnover rates between Republican and Democratic executives.

Table 6 presents the estimates of our coefficient of interest, β. For brevity, we suppress the coefficients on control variables, with the full set reported in Internet Appendix Table IA.X. The results indicate that politically misaligned executives are more likely to exit the team than their aligned counterparts. In column (1), which includes year, firm, role $\times$ year and party affiliation fixed effects, as well as individual-level controls, the coefficient implies that misaligned executives have a 3.5-pp higher probability of leaving the team than aligned executives.

[Insert Table 6 here]

In the stricter specification, reported in column (2), we control for time-varying shocks at the firm level by exploiting variation within the same firm and year. We find that, within the same firm-year, executives whose party affiliation differs from that of the team's majority have a 2.8-pp higher probability of leaving the firm compared to executives whose party affiliation matches that of the majority. This effect represents a 23.6% increase relative to the unconditional turnover

probability of 12%. In columns (3) to (6), we investigate how the effect varies across subperiods. In columns (3) and (4), which focus on the years 2008–2015, the coefficient on *Misaligned* is between 1.6 and 2.7 pp but statistically insignificant when we include firm × year fixed effects in column (4). During the 2016–2021 period (columns (5) and (6)), the coefficient estimate is greater than 5 pp and statistically significant, aligning qualitatively with the findings from our dyadic regressions and team-entry analysis. The departures analysis ends in 2021 because departures are defined as an executive's last appearance in ExecuComp, which requires observing the subsequent year.

Internet Appendix Table IA.XI reports qualitatively similar findings when we include unaffiliated executives, for whom *Misaligned* is coded as 0.5. Since unaffiliated executives never switch from matching the majority to not matching the majority, the coefficient on *Misaligned* remains unaffected by how we code unaffiliated executives.

We provide two sets of heterogeneity tests related to our main executive departure analysis. Internet Appendix Table IA.XII examines potential asymmetries in the effect of partisan alignment based on whether the executive team has a Democratic or Republican majority. We interact the *Misaligned* variable with another dummy variable, *Majority Democrat*, which takes the value one if the number of Democratic executives exceeds the number of Republican executives by more than one at the previous year-end, and zero otherwise. The coefficient on the interaction term reveals that the effect of misalignment on departure is stronger for teams with Democratic majorities.

We also examine whether the effect of partisan alignment is stronger in more polarized teams. To this end, we repeat the main departure analysis while varying the majority margin of the firm. Panel B of Figure 4 plots the coefficient on $Misaligned_{if,t-1}$ from equation (6) separately for the pre-2016 and post-2016 periods under three alternative majority definitions: (i) a narrow definition requiring only that the majority party exceeds the minority (majority margin > 0); (ii) our baseline definition (majority margin > 1); and (iii) a stringent definition capturing highly homogeneous teams (majority margin > 2). Across all three majority definitions, the point estimates are larger in the post-2016 period than in the pre-2016 period, indicating that the partisan alignment effect on departures strengthened broadly after 2016. Moreover, consistent with the evidence from our team-entry analysis, the increase is most striking for highly

homogeneous teams.

To summarize, our analysis of team entry and exit highlights the significant and growing power of partisan alignment in explaining the composition of executive teams. Misaligned executives, whose political views differ from the majority of the team, are more likely to depart. Additionally, we find that the partisan composition of an existing executive team increasingly predicts the party affiliation of new executives, with firms more likely to add executives whose party affiliation aligns with the existing team's majority. Across both analysis, the effect of partisan alignment intensifies particularly strongly in already homogeneous teams.

7. Potential Mechanism: Partisan Disagreement

We now turn to the question of why partisan segregation among executives increased when it did. A growing body of literature highlights the role of political affiliation in explaining individuals' economic views. In particular, alignment with the president has been shown to be a powerful predictor of households' perceptions of economic conditions (e.g., Gerber and Huber, 2009; Mian, Sufi, and Khoshkhou, 2023), as well as those of professionals in high-stakes environments, such as credit analysts (Kempf and Tsoutsoura, 2021), loan officers (Dagostino, Gao, and Ma, 2023), and professional asset managers (Cassidy and Vorsatz, 2025). Therefore, one potential channel contributing to the rise in assortative matching on party affiliation is that Democratic and Republican executives increasingly disagree on economic issues. This economic disagreement across partisan lines can create friction within executive teams and make partisan leanings more salient in corporate settings, thereby raising the influence of political alignment in executive-team formation. This section explores this mechanism.

7.1. The Partisan Gap in Executives' Insider Trades

To measure economic disagreement between Democratic and Republican executives, we study their decisions to sell their company's stock ("insider selling") around highly salient political events: presidential elections. We obtain data on insider trades for the firms in our sample from Thomson Reuters, as described in Section 3, and restrict the sample to trades occurring between three months prior to three months following the 2008, 2012, 2016, and 2020 presidential elections, respectively. Table 1, Panel E, reports descriptive statistics for the insider-trading sample, with

the unit of observation being an insider-month. The sample is restricted to firm-years with at least two executives who can be matched to a voter record. The average monthly propensity of an insider to sell company stock is 12.0% during our event windows. We focus on sales rather than purchases of company stock because purchases are much more rare (about 1.2% of all insider-months).

To study the relationship between an insider's political affiliation and her trading decisions, we estimate the following regression:

$$\text{Insider Sell}_{ifet} = \alpha_{fet} + \alpha_{ie} + \beta \text{Election}_{et} \times \text{PartyWins}_{ie} + \gamma \text{Election}_{et} \times \text{Unaff.}_{ie} + \varepsilon_{ifet}, \quad (7)$$

where $Insider\ Sell_{ifet}$ equals one if insider i sells shares of firm f in year-month t around presidential election e, and $Election_{et}$ equals one for the month of the presidential election (November), and zero otherwise. $PartyWins_{ie}$ equals one if insider i is a registered Republican, and zero otherwise, in the 2016 election; it equals one if insider i is a registered Democrat, and zero otherwise, in the 2008, 2012, and 2020 elections, and it equals 0.5 for unaffiliated executives. $Unaff._{ie}$ is an indicator equal to one for unaffiliated executives, and zero for partisan insiders. Note that whether we code unaffiliated insiders as aligned or misaligned does not affect our estimate of β in equation (7). The main benefit of including unaffiliated executives in our insider trading analysis is that it allows us to estimate the fixed effects more precisely. α_{fet} and α_{ie} refer to firm $\times$ month and insider $\times$ election fixed effects, respectively. The main coefficient of interest is β, which captures the change in the relative propensity to sell company stock during the election month between executives whose party aligns with the newly elected president versus those whose party does not align. If a presidential election induces greater optimism about the firm's future performance among executives who support the president than among executives who do not support the president, we would expect a relative decrease in the likelihood of insider selling for executives whose party wins the election (i.e., $\beta < 0$).

Panel A of Table 7 reports the results for the 2016 and 2020 elections. In column (1), we find that the coefficient estimate of β is negative and significant, suggesting that executives with different party affiliations have divergent views on how the election outcome will influence their company's future stock performance. Executives aligned with the newly elected president's party

are about 4.5 pp less likely to sell shares during the election month compared to their misaligned counterparts. In other words, executives who support the new president become significantly more optimistic about their company's future stock performance relative to those with opposing party affiliations. This is a large effect relative to the unconditional likelihood of an insider sale of 12.0%.[13] Notably, these results are obtained using firm × month fixed effects, ensuring that the estimates are not influenced by unobservable, time-varying characteristics of the firm.

[Insert Table 7 here]

Columns (2) and (3) report the results for two key alternative specifications. Column (2) shows that the point estimate is virtually unchanged when we allow the selling propensity of unaffiliated executives to also vary around the election. In column (3), we find that our estimates are again very similar when we include executive × election fixed effects, which control for any differences in the average propensity to sell company stock across executives. These fixed effects also largely absorb the slow-moving time-varying executive characteristics used in our executive departure analysis in Table IA.X. Finally, Internet Appendix Table IA.XIII demonstrates that the results are robust when we use a measure of abnormal insider selling, defined as the difference between the insider selling indicator and the insider trading indicator lagged by twelve months.

Panel B of Table 7 presents the results for the 2008 and 2012 presidential elections. The estimates of β in this sample are much smaller in magnitude and statistically insignificant.[14] Together, these findings suggest that executives with different party affiliations increasingly hold divergent views on how the political environment affects their companies' fundamentals. Given existing studies linking executive optimism to corporate financing and investment decisions (e.g., Malmendier and Tate, 2005), it is likely that executives with opposing party affiliations may profoundly disagree on how their firms should be run.

[13] We acknowledge that the magnitudes may be challenging to interpret. For instance, executives in teams where party affiliation is very homogeneous (e.g., almost all executives are Democrats or Republicans) might behave differently from those in more politically mixed teams. Future research could explore this heterogeneity in more detail.

[14] The coefficient on $PartyWins$ cannot be separately identified in Panel B because the same party won the presidential election in both 2008 and 2012.

7.2. The Partisan Trading Gap and Executive Departures

The above results highlight a growing economic disagreement between Democratic and Republican executives. The next question is whether this disagreement also predicts executive departures. To test this, we examine the relationship between the documented partisan gap in executives' insider trades around presidential elections and the gap in the departure likelihood between misaligned versus aligned executives. Specifically, we measure the gap in executives' insider trades as the difference between the average selling propensity of executives whose party lost the most recent presidential election and those whose party won, measured within the same firm during the election month. After defining the variable *Disagreement* as equal to one if this trading gap is positive, and zero if it is negative, zero, or missing, we re-estimate the executive departure regression from equation (6), after interacting the *Misaligned* variable with *Disagreement*. We account for observations where the partisan gap in insider trades cannot be measured (e.g., because partisan executives cannot be matched to the insider trading data) via a *Missing Disagreement* variable. To focus on a time period during which partisan disagreement around presidential elections is most likely to influence executive departures, we restrict the sample to fiscal year-ends within 365 days following the 2016 and 2020 presidential elections.

The results of this estimation are reported in Table 8. In column (1), we find that the coefficient on the interaction between *Misaligned* and *Disagreement* is positive and statistically significant, indicating that misaligned executives are more likely to leave the firm when the partisan trading gap among the firm's executives is positive. The result remains consistent in column (2), where we include firm $\times$ year fixed effects, controlling for time-varying firm characteristics such as performance and investment opportunities. The interaction coefficient of *Misaligned* and *Missing Disagreement* is also positive and marginally significant. This result is intuitive, as it suggests that when information on executives' insider trades is missing, the effect of political misalignment is stronger compared to when partisan executives' trades agree. Overall, the results suggest that misaligned departures are more likely when there is greater disagreement among the partisan executives within the firm. However, we note that this analysis is based on a relatively small number of executive-firm-years and departures, as it focuses on fiscal year-ends within 365 days following the 2016 and 2020 presidential elections. Consequently, the observed economic effects appear somewhat larger than expected, which may be partly due to the limited sample

size and could affect the generalizability of these findings.

[Insert Table 8 here]

Combined, the results in this section suggest that partisan disagreement over economic issues may have contributed to the rising departure rate of misaligned executives. However, it is important to note that other factors may also play a role. For instance, partisan differences in economic views could be correlated with partisan differences in other social and political issues, which are increasingly discussed publicly by large corporations (e.g., Cassidy and Kempf, 2025).

8. Discussion

Partisan animosity has increased substantially over the last 20 years. According to Pew Research Center (2014), the share of individuals with a highly negative view of the opposing party has more than doubled since 1994 for both parties. Most of these intense partisans believe the opposing party's policies "are so misguided that they threaten the nation's well-being." This finding raises the question of whether the polarized environment in the U.S., with tensions between the two major parties at historically high levels, affects the ability of individuals to collaborate across partisan lines in the workplace. We provide novel evidence showing that U.S. executives are increasingly segregating across partisan lines, leading to a political polarization of corporate America.

The increase in partisan segregation is concentrated after the 2016 presidential election, indicating that the growing political polarization of corporate executives is a relatively recent phenomenon, starting around or shortly after the 2016 presidential election. We also explore the mechanisms behind the increasing partisan segregation of executives. For example, we show that the post-2016 rise in partisan matching occurs within both geographies and sectors, suggesting that it is not simply driven by increased partisan sorting into these areas.

Changes in the political composition of executive teams likely have profound effects on how firms are run. We find that Democratic and Republican executives differ fundamentally in their optimism about the future of the *same* company depending on which party is in control of the White House. Executive optimism, in turn, has been linked to important firm decisions, such as corporate investment and financing (e.g., Malmendier and Tate, 2005).

Interpreting the rise in partisan segregation requires distinguishing between supply-side and demand-side forces in the executive labor market. On the demand side, boards, CEOs, or senior managers may increasingly prefer politically aligned executives, either because alignment improves coordination and reduces conflict (e.g., Donaldson, Malenko, and Piacentino, 2020), or because party affiliation is perceived to proxy for differences in beliefs about regulation, ESG, labor relations, or corporate strategy. On the supply side, executives may increasingly prefer to work with politically like-minded colleagues. Since the descriptive design used in this paper is not based on an identified source of variation that separates changes in demand from changes in the supply of executives, we interpret the evidence as describing an equilibrium shift in assortative matching rather than a change driven by a particular side of the market.

This distinction matters for how one interprets the welfare and governance implications of our findings. From a corporate-governance perspective, boards and CEOs have fiduciary responsibilities to appoint competent managers independent of political affiliation. Through the lens of assignment-market models with scarce top managerial talent (e.g., Gabaix and Landier, 2008; Tervio, 2008), our results raise a natural question: how can political affiliation become increasingly predictive of executive team formation without materially affecting the quality of appointed executives? One possibility is that the supply of high-quality executives is sufficiently elastic within each party, so that increased partisan sorting does not come at a large cost in managerial quality. Another possibility is that partisan considerations increasingly enter hiring and retention decisions in ways that generate inefficiencies.[15] Distinguishing between these interpretations is an important direction for future research.

More broadly, what does matching on political affiliation among executives reflect? It may

[15]To provide some initial evidence on this question, we study how shareholders view the value implications of executive departures by analyzing abnormal stock returns around the departure announcements. We obtain the announcement dates for executives' departures by screening the firm's 8K filings, as described in Internet Appendix IA.H, and study abnormal stock returns around departure announcements. The results, reported in Internet Appendix Table IA.XV, indicate a more negative stock price reaction to the departures of misaligned executives. The magnitude of the difference is large: the announcement of a misaligned executive's departure is, on average, associated with a 1.75% larger stock price decline in trading days (0,+1) relative to the announcement, compared to the announcement of an aligned executive's departure. Although the exact economic mechanism behind this striking difference in the stock price reactions—in particular, whether it reflects differences in managerial ability or a causal effect of a change in political group composition—remains unclear, it indicates that shareholders of public U.S. firms may have reasons to be concerned about the trend toward greater partisan homogeneity in executive teams. Shareholder proposals and discussions about ideological diversity, such as the one at Apple's annual shareholder meeting in 2019 (CNET, 2019) or recent activism at Salesforce (Wall Street Journal, 2023), may thus become a more common phenomenon.

either indicate a causal effect of party affiliation, where executives are increasingly drawn to members of their own team (Mason, 2015), or it could suggest that party affiliation is becoming a more powerful indicator of ideological differences that extend beyond ethnicity, gender, and age, offering strong predictive power for executives' labor market outcomes and trading decisions. While our analysis, presented in Panel C of Internet Appendix Figure IA.IV, shows that executives' ideological positions, as measured by the positions of the candidates they financially support, have not become more extreme during the post-2016 time period, further research is needed to distinguish these possibilities.

Finally, our results raise the question of whether policymakers should be concerned about diversity of political views and political discrimination in the workplace, even in the absence of any economic inefficiencies. Traditionally, discussions about discrimination in the workplace have focused on gender, race, sexual orientation, and age.[16] By contrast, U.S. federal law and many state laws do not prohibit private employers from discriminating against employees on the basis of political views.

9. Conclusion

This paper establishes a new stylized fact, namely, increased partisan segregation among U.S. executives, leading to a political polarization of corporate America. We establish this fact by linking top executives at S&P 1500 firms between 2008 and 2022 to party affiliations from voter registration records. We document rising segregation using complementary evidence from (i) simulation-based measures of excess team-level partisan homogeneity relative to a random-assignment benchmark, (ii) dyadic regressions showing that shared party affiliation increasingly predicts which executives work at the same firm, and (iii) turnover analyses indicating that politically misaligned executives are more likely to depart and that executives who join top teams increasingly align with the team's existing partisan composition. Across approaches, we observe an inflection point around the 2016 presidential election.

The results in this paper raise many important questions that provide fruitful avenues for

[16] Under Title VII of the Civil Rights Act of 1964, it is illegal for employers to make job decisions based on race, color, national origin, religion, and sex. Moreover, the Age Discrimination Act, the Americans with Disabilities Act, and the Genetic Information Nondiscrimination Act prohibit discrimination based on age, disability, and genetic information.

future research. First, it is important to further understand how the political composition of the executive team affects important corporate decisions. Second, while our paper focuses on the top-earning executives in publicly listed U.S. firms, it remains an open question whether other parts of the workforce and employees in privately held companies exhibit trends similar to those of the executives we study. Finally, while our evidence shows that party affiliation has become increasingly predictive of executive team formation, our design does not separately identify shifts in labor supply from shifts in labor demand. We look forward to future research exploring these questions.

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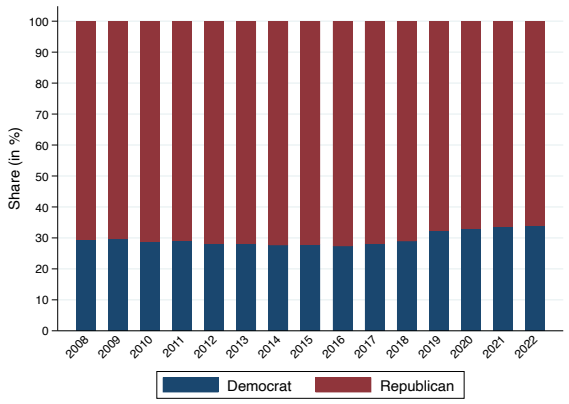
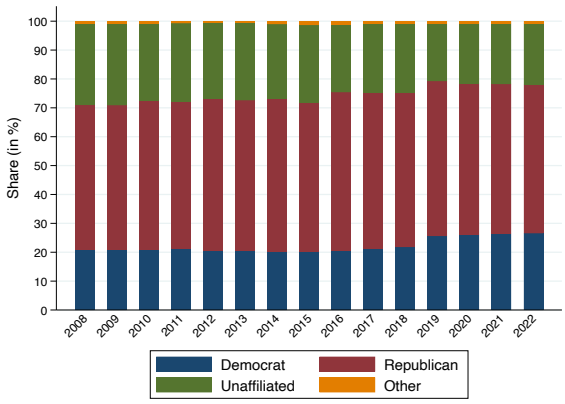
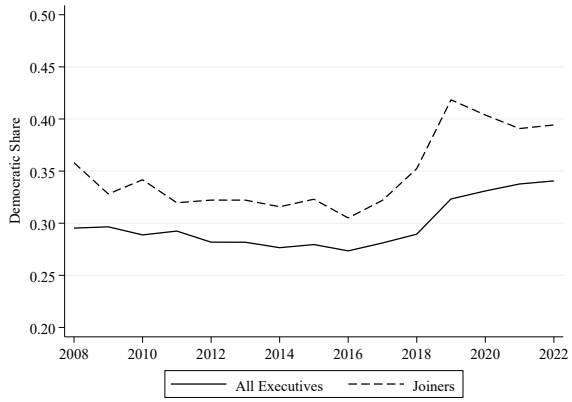


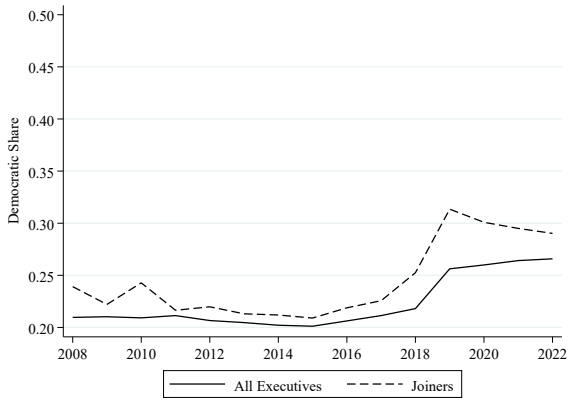
(A) Share of Dem./Rep. Executives



(B) Share of Dem./Rep./Unaff. Executives



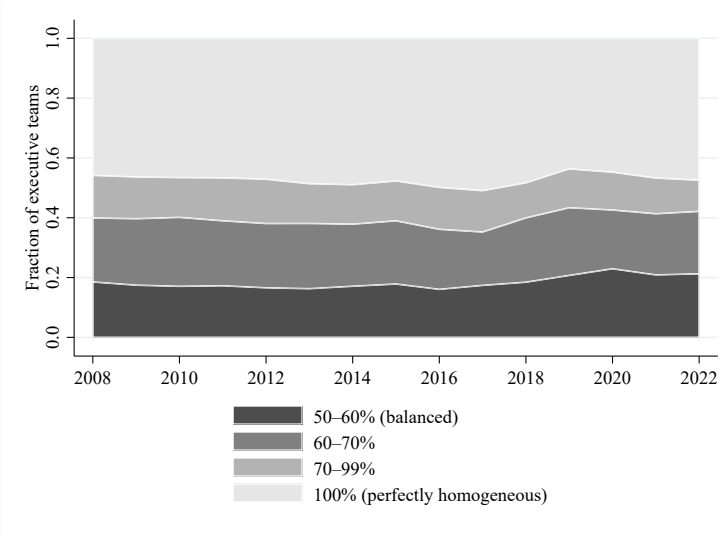
(C) Joiners vs. All Executives



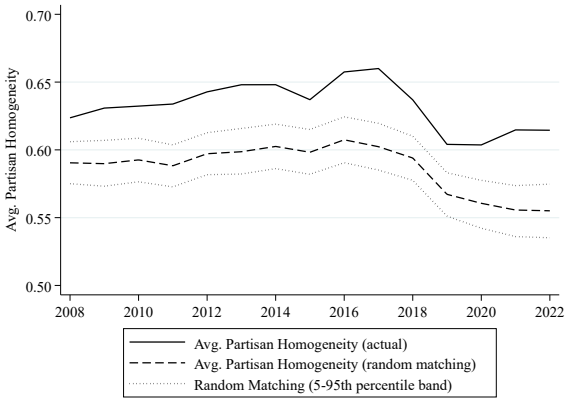
(D) Joiners vs. All Executives (incl. Unaff.)

Figure 1: Party Affiliations of U.S. Executives

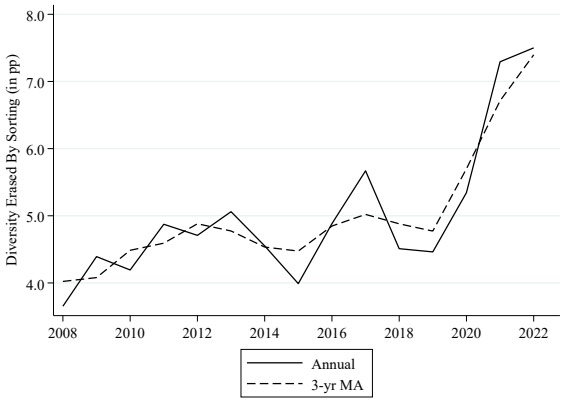
The figure shows the distribution of executives' party affiliations inferred from voter registration records over time. Panels A and C restrict the sample to partisan executive (Democrat or Republican), whereas Panels B and D use all executives who are registered voters. Joiners are defined as executives who appear for the first time at a given firm in ExecuComp.



(A) Distribution of Teams By Partisan Dominance



(B) Actual vs. Expected Homogeneity



(C) Diversity Erased by Sorting

Figure 2: (Excess) Partisan Homogeneity of U.S. Executive Teams

The figure shows trends in the (excess) partisan homogeneity of U.S. executive teams. Panel A shows the distribution of executive teams by the dominant party's share over time. Teams are classified into four categories based on the share of the majority party: balanced (50–60%), moderately homogeneous (60–70%), highly homogeneous (70–99%) and perfectly homogeneous (100%). Panel B plots the average partisan homogeneity of executive teams (solid line) and the level expected under random matching (dashed line). The random-matching benchmark is constructed via a Monte Carlo simulation that preserves each firm-year's executive team size and randomly assigns party affiliations using the year-specific partisan composition of the executive pool; the dotted lines show the 5th and 95th percentiles of the simulated distribution of average homogeneity. In Panel C, we compute the hypothetical executive pool's party composition that would be required to generate the observed level of homogeneity under random matching. The difference between the actual and the hypothetical majority-party share yields a composition-equivalent shift toward the pool's partisan majority implied by sorting, which we label "diversity erased by sorting." The solid line shows annual values; the dashed line shows a three-year moving average.

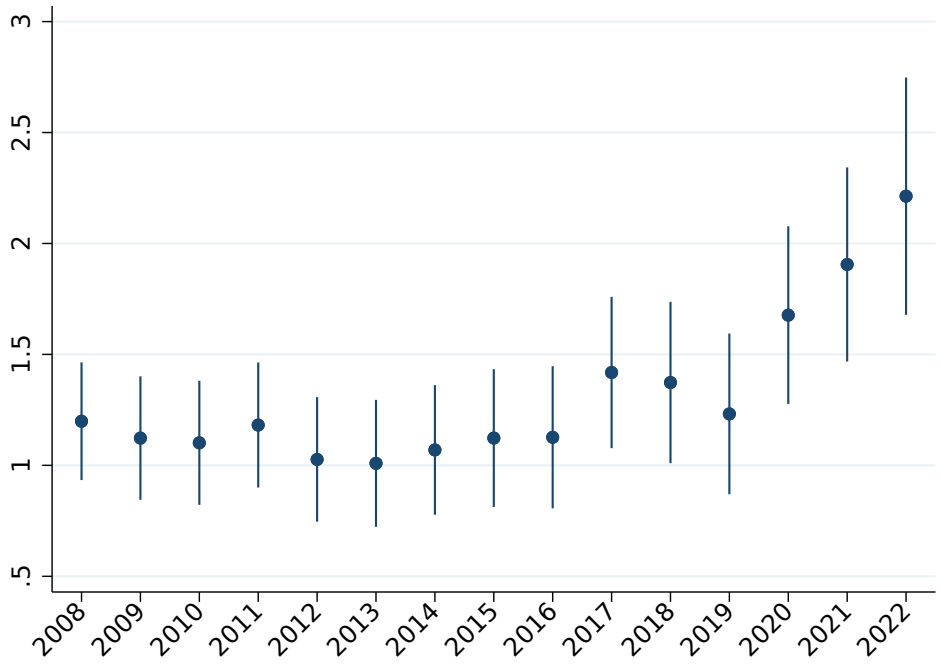
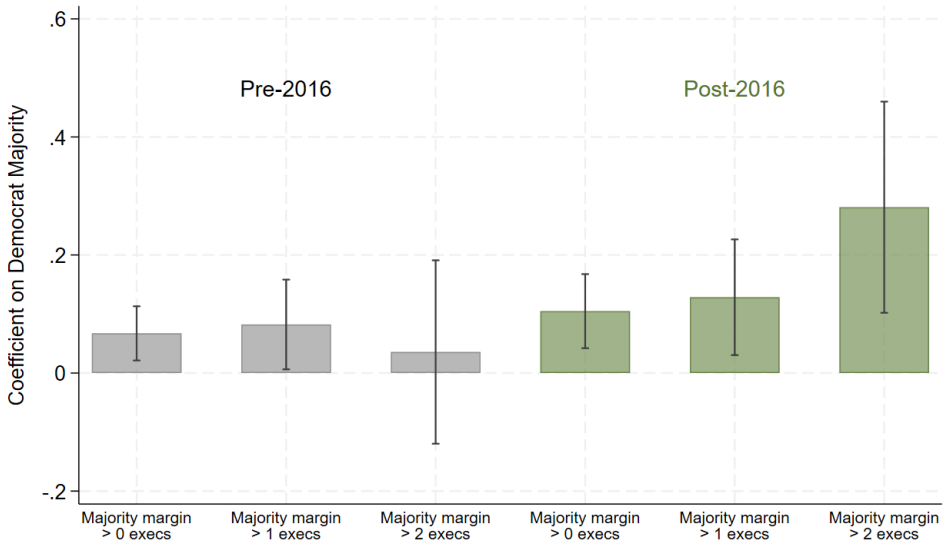
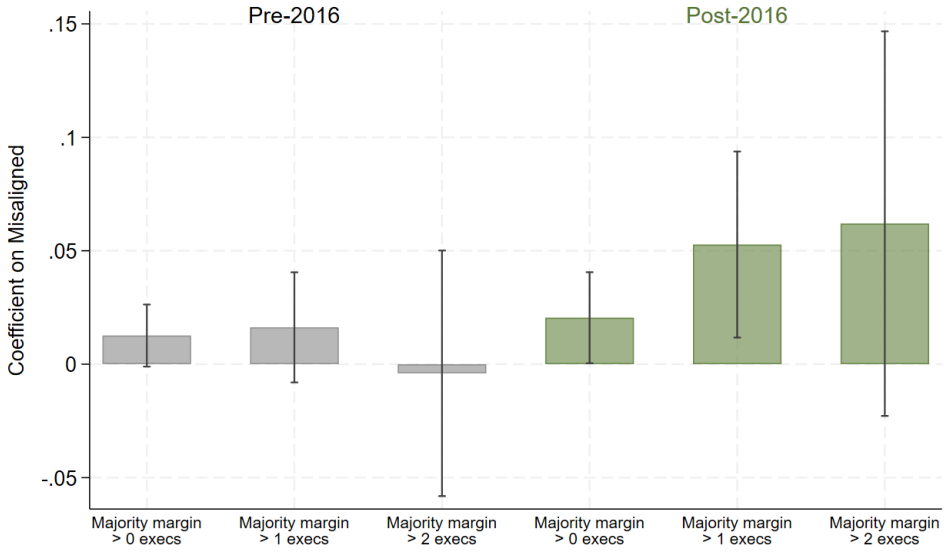


Figure 3: Dyadic Regression by Year

The figure presents the results from estimating our dyadic regression in equation (3) separately for each year. We plot the estimated coefficient β^{SP} of the variable $SParty$, an indicator equal to one if both executives share the same party, and zero otherwise, together with the corresponding 95% confidence intervals.



(A) Partisan Alignment and Executive Hiring



(B) Partisan Alignment and Executive Departures

Figure 4: Partisan Alignment and Executive Turnover

This figure illustrates the role of partisan alignment in executive team entry and departures over time. Panel A plots the coefficient on *Majority Democrat* from equation (5), estimated separately for the pre-2016 (grey) and post-2016 (green) periods. The partisan majority threshold varies across specifications: an executive team is classified as having a clear Democrat (Republican) majority if Democrats (Republicans) outnumber the other party by more than zero, one, or two executives, respectively. Panel B plots the coefficient on *Misaligned* from equation (6), also separately for the pre-2016 and post-2016 periods and for the same majority definitions. The 95% confidence intervals are based on standard errors clustered at the firm level.

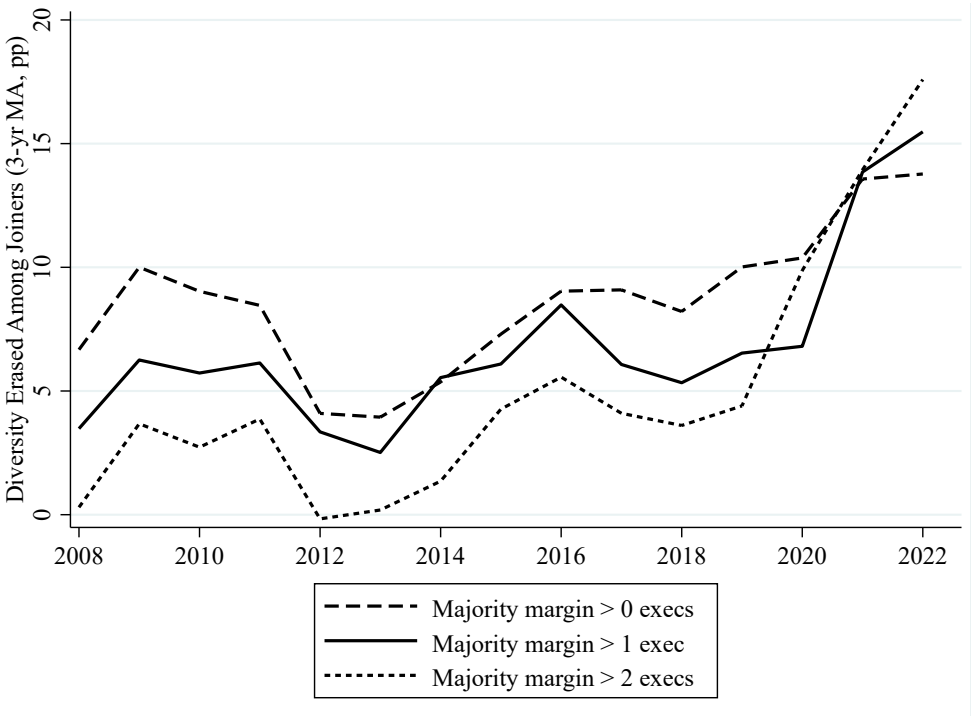


Figure 5: Diversity Erased By Partisan Sorting of Joiners

The figure plots partisan sorting among newly appearing executives by translating the observed share of joiners who align with their destination team's partisan majority into a composition-equivalent measure of "diversity erased." For each year t, we compute the partisan composition of joiners that would be required to generate the observed alignment rate under random assignment of joiners; the plotted series reports the implied percentage-point shift in joiners' party composition toward the incumbent majority relative to the actual joiner composition. We plot the three-year moving average separately by the strength of the incumbent partisan majority, requiring the lagged partisan majority to exceed the minority by more than zero, one, or two executives, respectively.

Table 1: Summary Statistics

This table presents summary statistics for the five samples used in our analysis. Panel A reports statistics for the firm-year panel, Panel B for the dyadic executive-pair sample, Panel C for the sample of executive joiners, and Panel D for the executive-firm-year panel used to study departures. Panel E reports summary statistics for the insider-trading sample. Variable definitions are provided in Appendix A.1.

	N	Mean	St.Dev.	0.25	Median	0.75
Panel A: Firm-Year Panel						
Partisanship	20,319	0.634	0.376	0.333	0.600	1.000
Excess partisan homogeneity (std.)	20,319	0.150	1.038	-0.819	0.022	1.121
Democratic share	20,319	0.297	0.303	0.000	0.250	0.500
Number of executives	20,319	5.472	1.116	5.000	5.000	6.000
Number of matched executives	20,319	3.765	1.205	3.000	4.000	5.000
Number of partisan executives	20,319	3.102	1.081	2.000	3.000	4.000
Panel B: Dyadic Sample						
SFirm ($\times$ 10,000)	134,663,972	5.801	240.774	0.000	0.000	0.000
SParty	134,663,972	0.590	0.492	0.000	1.000	1.000
SGender	134,663,972	0.815	0.388	1.000	1.000	1.000
SRace	134,663,972	0.802	0.399	1.000	1.000	1.000
SAge	134,663,972	0.407	0.491	0.000	0.000	1.000
Panel C: Joiner Sample						
Democratic joiner	2,750	0.315	0.464	0.000	0.000	1.000
White	2,750	0.819	0.385	1.000	1.000	1.000
Age	2,750	51.133	7.012	46.000	51.000	56.000
Female	2,750	0.165	0.372	0.000	0.000	0.000
Majority Democrat	2,750	0.139	0.346	0.000	0.000	0.000
Panel D: Executive-Firm-Year Panel for Departures						
Executive departure	30,868	0.120	0.325	0.000	0.000	0.000
Misaligned	30,868	0.080	0.271	0.000	0.000	0.000
Tenure	30,868	7.970	5.272	4.000	7.000	11.000
White	30,868	0.910	0.287	1.000	1.000	1.000
Age	30,868	55.274	7.641	50.000	55.000	60.000
Age $\geq$ 65	30,868	0.097	0.296	0.000	0.000	0.000
Female	30,868	0.083	0.276	0.000	0.000	0.000
Majority Democrat	30,868	0.125	0.331	0.000	0.000	0.000
Panel E: Insider Trading Sample						
Insider sell	56,775	0.121	0.326	0.000	0.000	0.000
PartyWins	56,775	0.427	0.428	0.000	0.500	1.000
Unaffiliated	56,775	0.247	0.431	0.000	0.000	0.000
Election	56,775	0.143	0.350	0.000	0.000	0.000

Table 2: Excess Partisan Homogeneity Over Time (Firm-Level)

This table reports trends in excess partisan homogeneity using firm-level statistics. The unit of observation is a firm × year. The dependent variable is the standardized excess partisan homogeneity, defined as the difference between the team's actual homogeneity and expected homogeneity under random matching, divided by the standard deviation of the team's simulated homogeneity across 1,000 Monte Carlo runs. In columns (1)–(2) ((3)–(4)), the sample is restricted to teams with at least two (three) partisan executives, respectively. All specifications include team size fixed effects (number of partisan executives). Columns (2) and (4) include firm fixed effects as well as controls for lagged firm characteristics (logarithm of the firm's total book assets, leverage, cash holdings, cash flow, investment rate, Tobin's Q, and revenue growth). Panel A reports results for all firms with at least two partisan executives and Panel B for a balanced panel of firms present in all 15 years. Standard errors, reported in parentheses, are clustered at the firm level.

Panel A: All Firms

	Standardized Excess Partisan Homogeneity			
	$N \geq 2$		$N \geq 3$	
	(1)	(2)	(3)	(4)
Year	0.0004	-0.0080	-0.0061	-0.0136**
	(0.0048)	(0.0055)	(0.0059)	(0.0068)
Post-16	0.0093	-0.0186	0.0473	-0.0153
	(0.0261)	(0.0277)	(0.0329)	(0.0332)
Year × Post-16	0.0078	0.0143	0.0236**	0.0322***
	(0.0089)	(0.0097)	(0.0114)	(0.0123)
N	20,319	16,952	13,037	10,918
R^2	0.007	0.571	0.006	0.636
Mean dep. var.	0.150	0.150	0.196	0.196
Team Size FE	Yes	Yes	Yes	Yes
Firm FE + Controls	No	Yes	No	Yes

Panel B: Balanced Sample

	Standardized Excess Partisan Homogeneity			
	$N \geq 2$		$N \geq 3$	
	(1)	(2)	(3)	(4)
Year	-0.0120	-0.0180*	-0.0165*	-0.0177*
	(0.0076)	(0.0093)	(0.0087)	(0.0103)
Post-16	-0.0277	-0.0379	-0.0229	-0.0506
	(0.0399)	(0.0436)	(0.0435)	(0.0448)
Year × Post-16	0.0341**	0.0323**	0.0466***	0.0395**
	(0.0136)	(0.0148)	(0.0159)	(0.0171)
N	7,049	6,156	5,776	5,060
R^2	0.015	0.553	0.013	0.590
Mean dep. var.	0.242	0.242	0.277	0.277
Team Size FE	Yes	Yes	Yes	Yes
Firm FE + Controls	No	Yes	No	Yes

Table 3: Dyadic Regressions

This table reports results from dyadic regressions, where the unit of observation is an executive-pair × year. The dependent variable is an indicator equal to one if both executives work at the same firm, and zero otherwise, multiplied by 10,000 (i.e., the coefficients can be interpreted as the effect in basis points). *SParty* is an indicator equal to one if both executives have the same political affiliation, and zero otherwise. Column (1) reports estimates of the dyadic regression in equation (3) without control variables, and column (2) adds an interaction between *SParty* and an indicator for fiscal years 2016 onwards (*Post-16*). Columns (3) to (5) report estimates for equation (4), sequentially expanding the set of control variables. In the interaction term *SParty* × *Year*, *Year* refers to the fiscal year minus 2008, whereas in the triple interaction term, *Year* refers to the fiscal year minus 2016. The sample is restricted to Republican and Democratic executives and to firms with at least two partisan executives. The economic effects are calculated as the coefficient on *SParty* (*SParty* × *Year* × *Post-16*) divided by the mean of the dependent variable, respectively. *Static controls* include controls for shared race, age, and gender, as defined in Appendix A.1. *Dynamic controls* include the static controls interacted with fiscal year and the post-2016 indicator. The full sets of coefficients are reported in Internet Appendix Table IA.V. Standard errors, reported in parentheses, are clustered at the executive-pair level.

	Same Firm				
	(1)	(2)	(3)	(4)	(5)
SParty	1.2938***	1.1410***	1.1721***	1.1608***	1.1576***
	(0.0967)	(0.1065)	(0.1372)	(0.1373)	(0.1373)
SParty × Post-16		0.4174***	0.0841	0.0732	0.0711
		(0.1500)	(0.1665)	(0.1665)	(0.1669)
SParty × Year			-0.0092	-0.0158	-0.0156
			(0.0274)	(0.0274)	(0.0274)
SParty × Year × Post-16			0.1543***	0.1633***	0.1646***
			(0.0588)	(0.0588)	(0.0589)
N	134,663,972	134,663,972	134,663,972	134,663,972	134,663,972
Effect SParty (in %)	22.3	19.7	20.2	20.0	20.0
Effect SParty × Year × Post-16 (in %)			2.7	2.8	2.8
Fixed Effects and Controls:					
Year FE	Yes	Yes	Yes	Yes	Yes
Static Controls	No	No	No	Yes	Yes
Dynamic Controls	No	No	No	No	Yes

Table 4: Dyadic Regressions: Partisan Matching Within Sectors and Geographies

This table explores the role of partisan sorting into sectors and geographies. Column (1) repeats our baseline specification from column (5) of Table 3, while standardizing all coefficients by dividing them by the mean of the dependent variable. Column (2) repeats the baseline specification, after restricting to executive pairs who work within the same GICS sector. Column (3) ((4)) restricts to executive pairs who work for companies headquartered in the same state (MSA), respectively. The sample is restricted to Republican and Democratic executives and to firms with at least two partisan executives. Standard errors, reported in parentheses, are clustered at the executive-pair level.

	Same Firm			
	(1)	(2)	(3)	(4)
SParty	0.1996***	0.1804***	0.2656***	0.2973***
	(0.0237)	(0.0229)	(0.0225)	(0.0227)
SParty × Post-16	0.0123	-0.0037	-0.0360	-0.0851***
	(0.0288)	(0.0294)	(0.0291)	(0.0283)
SParty × Year	-0.0027	-0.0044	-0.0035	-0.0030
	(0.0047)	(0.0047)	(0.0046)	(0.0045)
SParty × Year × Post-16	0.0284***	0.0299***	0.0324***	0.0271***
	(0.0102)	(0.0102)	(0.0102)	(0.0101)
N	134,663,972	15,666,390	7,864,791	3,227,123
Fixed Effects and Controls:				
Year FE	Yes	Yes	Yes	Yes
Static Controls	Yes	Yes	Yes	Yes
Dynamic Controls	Yes	Yes	Yes	Yes
Sample	All	Same Sector	Same State	Same MSA

Table 5: Partisan Alignment and Executive Team Entry

This table reports the relationship between the party of a newly appearing executive and the destination team's existing partisan majority, by estimating equation (5). The unit of observation is an executive joiner, defined as the first year an executive is reported among the top earners of a given firm in ExecuComp. The dependent variable, *Democratic Joiner*, is an indicator equal to one if the joining executive is a Democrat, and zero if she is Republican. *Majority Democrat* is a lagged binary indicator equal to one (zero) if, in the prior year, the firm's Democratic (Republican)-affiliated executive team members exceeded the Republican (Democratic)-affiliated members by more than one executive, respectively. Control variables, defined in Appendix A.1, include race, gender, and age of the joining executive. Columns (1) and (2) are based on the full sample, columns (3) and (4) are based on the years 2008–2015, and columns (5) and (6) are based on years 2016–2022. Standard errors are clustered at the firm level. The full set of coefficients is reported in Internet Appendix Table IA.IX.

	Democratic Joiner					
	Full Sample		2008–2015		2016–2022	
	(1)	(2)	(3)	(4)	(5)	(6)
Majority Democrat	0.1727***	0.1011***	0.1444***	0.0823**	0.2168***	0.1284**
	(0.0269)	(0.0307)	(0.0346)	(0.0388)	(0.0441)	(0.0500)
N	2,750	2,634	1,638	1,579	1,112	1,055
R^2	0.165	0.268	0.134	0.240	0.212	0.312
Controls Included	Yes	Yes	Yes	Yes	Yes	Yes
Role × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector × Year FE	Yes	No	Yes	No	Yes	No
State × Year FE	No	Yes	No	Yes	No	Yes

Table 6: Partisan Alignment and Executive Team Exit

This table reports the relationship between an executive's political alignment with the team's majority and her likelihood of leaving the team, by estimating equation (6). The unit of observation is an executive × firm × year. The dependent variable, *Executive Departure*, is an indicator equal to one in the last year an executive is reported among the top earners of a given firm in ExecuComp, and zero otherwise. *Misaligned* is an indicator equal to one if the executive's party affiliation differs from the team's majority party affiliation at the end of the prior year, and zero otherwise. The sample is restricted to firms with a clear partisan majority, defined as teams in which the lagged partisan majority exceeds the minority by more than one executive. We include controls for the executive's age, tenure, tenure squared, CEO status, and indicators for whether the executive is above the age of 65, female, white, and whether the firm's executive team has a Democratic versus Republican majority. Control variables are defined in Appendix A.1. Columns (1) and (2) are based on the full sample, columns (3) and (4) are based on the years 2008–2015, and columns (5) and (6) are based on years 2016–2021 (departures are not defined for 2022 because it is the last year of our sample period). Standard errors are clustered at the firm level. The full set of coefficients is reported in Internet Appendix Table IA.X.

	Executive Departure					
	Full Sample		2008–2015		2016–2021	
	(1)	(2)	(3)	(4)	(5)	(6)
Misaligned	0.0353***	0.0283***	0.0267**	0.0163	0.0511***	0.0527**
	(0.0103)	(0.0105)	(0.0124)	(0.0124)	(0.0192)	(0.0209)
N	30,868	30,239	18,948	18,550	11,886	11,689
R^2	0.160	0.434	0.194	0.433	0.211	0.434
Controls Included	Yes	Yes	Yes	Yes	Yes	Yes
Role × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	No	Yes	No	Yes	No
Year FE	Yes	No	Yes	No	Yes	No
Firm × Year FE	No	Yes	No	Yes	No	Yes
Political Affiliation FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 7: Executive Insider Trading around Presidential Elections

This table reports the relationship between an executive's political affiliation and her likelihood of selling company stock around presidential elections, by estimating equation (7). The unit of observation is an executive × firm × month. Panel A presents the results for the 2016 and 2020 presidential elections, and Panel B for the 2008 and 2012 presidential elections. *Election* is an indicator equal to one during the election month (November), and zero otherwise. *PartyWins* equals one if the insider is Republican, and zero otherwise, in the 2016 election; it equals one if the insider is a Democrat, and zero otherwise, in the 2008, 2012, and 2020 elections, and it equals 0.5 for unaffiliated executives. *Unaffiliated* is an indicator equal to one for unaffiliated executives, and zero for partisan insiders. Standard errors, reported in parentheses, are double-clustered by executive and firm.

Panel A: 2016 and 2020 Presidential Elections

	Insider Sell		
	(1)	(2)	(3)
Election × PartyWins	-0.0449**	-0.0447**	-0.0455**
	(0.0187)	(0.0189)	(0.0188)
Election × Unaffiliated		0.0018	0.0009
		(0.0228)	(0.0225)
PartyWins	0.0148	0.0148	
	(0.0103)	(0.0103)	
N	19,236	19,236	19,704
R^2	0.570	0.570	0.696
Fixed Effects:			
Firm × Month FE	Yes	Yes	Yes
Political Affiliation FE	Yes	Yes	Yes
Executive × Election FE	No	No	Yes

Panel B: 2008 and 2012 Presidential Elections

	Insider Sell		
	(1)	(2)	(3)
Election × PartyWins	0.0019	-0.0005	0.0014
	(0.0143)	(0.0147)	(0.0148)
Election × Unaffiliated		0.0101	0.0074
		(0.0153)	(0.0153)
N	27,086	27,086	27,715
R^2	0.543	0.543	0.669
Fixed Effects:			
Firm × Month FE	Yes	Yes	Yes
Political Affiliation FE	Yes	Yes	Yes
Executive × Election FE	No	No	Yes

Table 8: Partisan Gap in Insider Trading and Executive Departures

This table reports the relationship between the partisan gap in executives' insider trading during the most recent presidential election and the likelihood of leaving the team. It repeats the analysis from Table 6, after adding an interaction between *Misaligned* and *Disagreement*. *Disagreement* is an indicator equal to one if the difference in the average propensity to sell company stock between executives whose party lost the election and those whose party won the most recent presidential election, measured within the same firm during the election month, is positive, and zero if it is negative, zero, or missing. *Missing Disagreement* is an indicator equal to one if *Disagreement* is missing, and zero otherwise. The sample is restricted to fiscal year-ends within 365 days following the 2016 and 2020 presidential elections. The unit of observation is an executive × firm × year. Standard errors, reported in parentheses, are clustered at the firm level. The full set of coefficients is reported in Internet Appendix Table IA.XIV.

	Executive Departure	
	(1)	(2)
Misaligned	-0.0498	-0.0544
	(0.0371)	(0.0392)
Disagreement	-0.1049**	
	(0.0514)	
Misaligned × Disagreement	0.1981***	0.2067***
	(0.0621)	(0.0635)
Missing Disagreement	-0.0748**	
	(0.0291)	
Misaligned × Missing Disagreement	0.0508	0.0431
	(0.0416)	(0.0430)
N	3,796	3,788
R^2	0.253	0.330
Controls Included	Yes	Yes
Role × Year FE	Yes	Yes
Firm FE	Yes	No
Year FE	Yes	No
Firm × Year FE	No	Yes
Political Affiliation FE	Yes	Yes

Appendix: Variable Definitions

Table A.1: Variable Descriptions

Variable	Description
Dependent variables	
Partisanship	The probability that two randomly drawn executives from the same firm are either both Republicans or both Democrats, as identified in the voter registration records. Computed following equation (1).
Excess partisan homogeneity (std.)	The firm-level partisanship measure minus the average partisanship measure for the same firm across 1,000 simulation runs, divided by the standard deviation of the simulated partisanship measure across 1,000 simulations runs.
SFirm	An indicator equal to one if two executives work at the same firm, and zero otherwise, multiplied by 10,000.
Democratic joiner	An indicator equal to one (zero) if the newly joining executive is Democratic (Republican), respectively.
Executive departure	An indicator equal to one in the last year an executive is reported among the top earners of a given firm in Execucomp, and zero otherwise.
Insider sell	An indicator equal to one if the insider sells company stock in a given calendar month, and zero otherwise.
Key independent variables	
SParty	An indicator equal to one if both executives have the same political party affiliation, and zero otherwise.
Misaligned	An indicator equal to one if the political affiliation of the executive does not match that of the majority in the team as of the prior year-end, and zero otherwise. If the team has no clear majority or the executive is not a partisan, the variable is set to missing.
Majority Democrat	An indicator equal to one if the number of Democratic executives in a given team exceeds the number of Republican executives by more than one, and zero if the number of Republican executives exceeds the number of Democratic executives by more than one.
PartyWins	An indicator equal to one if the party affiliation of the executive matches that of the newly elected U.S. President, and zero otherwise. If an executive is unaffiliated, the variable is set to 0.5.
Election	An indicator equal to one during a presidential election month, and zero otherwise.
Unaffiliated	An indicator equal to one for unaffiliated executives, and zero for partisan executives.
Disagreement	An indicator equal to one if the difference in the average propensity to sell company stock between executives whose party lost the election and those whose party won the most recent presidential election, measured within the same firm during the election month, is positive, and zero if it is negative, zero, or missing.
Missing Disagreement	An indicator equal to one if the difference in the average propensity to sell company stock between executives whose party lost the election and those whose party won the most recent presidential election, measured within the same firm during the election month, is missing, and zero otherwise.
Control variables	
SGender	An indicator equal to one if both executives have the same gender, and zero otherwise. Gender information is obtained from ExecuComp.

Continued on next page

Table A.1 – continued

Variable	Description
SRace	An indicator equal to one if both executives have the same race (white vs. non-white), and zero otherwise. Race is obtained by applying the API name-prism.com to executives' first and last names.
SAge	An indicator equal to one if the age gap between the two executives is at most five years, and zero otherwise. Age information is obtained from ExecuComp.
Tenure	Tenure of the executive in the firm, measured in years. Obtained from ExecuComp.
Tenure^2	Tenure of the executive in the firm, measured in years, squared. Obtained from ExecuComp.
White	An indicator equal to one if the executive is white, and zero otherwise. Information on race is obtained by applying the API name-prism.com to executives' first and last names.
Age	The executive's age as reported in ExecuComp.
Age $\geq$ 65	An indicator equal to one if the executive's age is greater or equal to 65 years, and zero otherwise. Age is obtained from ExecuComp.
Female	An indicator equal to one if the executive is female, and zero otherwise. Gender information is obtained from ExecuComp.
Log assets	Logarithm of total book assets. Obtained from Compustat Annual.
Tobin's Q	Ratio of the market to the book value of assets. Obtained from Compustat Annual.
Leverage	Ratio of long-term liabilities to the total book value of assets. Obtained from Compustat Annual.
Cash holdings	Cash plus receivables, normalized by total book assets. Obtained from Compustat Annual.
Cash flow	Earnings before extraordinary items plus depreciation, normalized by lagged total assets. Obtained from Compustat Annual.
Investment rate	Capital expenditures, normalized by lagged property, plant & equipment. Obtained from Compustat Annual.
Sales growth	Annual growth in total revenue. Obtained from Compustat Annual.