# The Economic Effects of Political Polarization: Evidence from the Real Asset Market*

Ran Duchin, Abed El Karim Farroukh, Jarrad Harford, and Tarun Patel†

August 2024

**Abstract**

Politically divergent firms are considerably less likely to merge. This effect concentrates in years with high political polarization, and when firms plan to integrate their operations. The results hold after including industry-by-year and deal fixed effects, and controlling for geographical distance, product similarity, and nonpolitical differences in corporate culture or ESG policies. Following politically divergent mergers, employee turnover is higher, particularly of employees whose views misalign with the merger partner. Overall, the rise in political polarization changed the landscape of the real asset market in the U.S., with fewer mergers between politically divergent firms or firms from politically divergent states.

JEL Classification: G34, D72

Keywords: political polarization, voter registrations, campaign contributions, mergers and acquisitions, integration, employee retention.

* For helpful comments, we thank Elizabeth Kempf, Thomas Lambert, Petra Vokata, and Jason Zein (discussants). We also thank participants of the 11th Miami Behavioral Finance Conference (2021), Finance Down Under Conference (2022), NHH Corporate Finance Conference (2022), the American Finance Association Meetings (2023), the European Finance Association Meetings (2023), and seminar participants at Northeastern University, University of California Irvine, University of Connecticut, University of Colorado, University of Houston, University of Macau, University of Texas at Dallas, University of Texas at El Paso, George Mason University, American University of Beirut, Singapore Management University, National University of Singapore, National Central University, and West Virginia University.

† Contact information: Ran Duchin, e-mail: duchinr@bc.edu; Abed El Karim Farroukh, e-mail: farroukh@iu.edu; Jarrad Harford, e-mail: jarrad@uw.edu; Tarun Patel, e-mail: tdpatel@smu.edu.

# 1 Introduction

A defining feature of 21st-century American politics is the rise of political polarization, whose levels reached record highs in recent years. In 2019, 82 percentage points separated Republicans' (89%) and Democrats' (7%) average job approval ratings of President Trump – the largest degree of political polarization measured by Gallup until then.[3] By 2021, a new record of 84 percentage points separated Republicans' (8%) and Democrats' (92%) average job approval ratings of President Biden. This trend has not escaped the notice of political scientists, who documented a similar increase in the gap between Democrats and Republicans based on roll-call votes (McCarty, Poole, and Rosenthal (2016)), the political orientation of campaign contributors (Bonica (2013)), and the content of political speech and media coverage (DellaVigna and Kaplan (2007); Levendusky (2013); Martin and Yurukoglu (2017); Gentzkow, Shapiro, and Taddy (2019)).

Economic research attempts to understand the economic causes of political polarization. Mian, Sufi, and Trebbi (2014) document a transient increase in the polarization of congressional votes following financial crises. Voorheis, McCarty, and Shor (2015) investigate the link between rising inequality and political polarization. Autor, Dorn, Hanson, and Majlesi (2020) study the impact of import competition on polarization. Complementary to these studies, which explore the economic *determinants* of political polarization, this paper investigates its economic *consequences*. We focus on the real asset market, and explore the role of political divergence between acquirers and targets in mergers and acquisitions. We hypothesize that the rise in political polarization has made it more difficult for politically divergent firms to merge and integrate. This hypothesis is rooted in political science research showing that the rise of political polarization has led to a new type of division in the mass public coined "affective polarization," whereby Americans increasingly dislike and distrust those from the other party (e.g., Iyengar, Sood, and Lelkes (2012); Iyengar, Lelkes, Levendusky, Malhotra, and Westwood (2019)).

To test this hypothesis, we hand-collect data on the political views of corporate employees using two distinct approaches. The first approach matches the LinkedIn universe of corporate employees from Revelio Labs to state-by-state voter registration records. The data cover 6,090,261 employees, from 12,753 firms, over the period 1980-2019, whose LinkedIn profile matches a

[3] See Jeffrey M. Jones "Trump Third Year Sets New Standard for Party Polarization," *Gallup*, January 21, 2020. https://news.gallup.com/poll/283910/trump-third-year-sets-new-standard-party-polarization.aspx

unique voter registration record. We measure a firm's political attitude as the ratio of the number of employees registered as Democrats to the total number of employees registered as Democrats or Republicans. The second approach relies on the personal contributions of corporate employees to political campaigns from 1978-2019. These data include 1,555,766 contributions from 395,124 employees of 9,522 firms. We measure a firm's political attitude as the ratio of the number of employee contributions to Democratic campaigns to the total number of contributions to both Democratic and Republican campaigns over a rolling window of two presidential elections. Using the two distinct measures of firms' political attitudes, we construct two measures of the political divergence between any two firms, labeled *Political Divergence*, which equal the absolute value of the difference between their political attitudes. We estimate all the analyses in a pooled sample that combines both approaches, as well as in separate samples based on each approach.

We begin the empirical analyses by investigating aggregate time trends in the real asset market. Our main findings are that the rise in political polarization has had considerable consequences for the landscape of mergers and acquisitions in the U.S. In particular, the percentage of mergers and acquisitions between politically divergent companies significantly declined over time: before 2010, mergers between extremely divergent firms (top decile) comprised more than 11% of all deals. After 2010, they comprised 6% of all deals. By 2019, they comprised less than 3% of all deals. A similar trend emerges for the average *Political Divergence* between acquirers and targets, which declined by 26% between 1985 and 2019. This trend is also reflected in the geographical landscape of mergers and acquisitions in the U.S. Before 2010, roughly 18% of all interstate mergers occurred between firms from politically different states. After 2010, this number dropped to 10% of all deals, and by 2019 it dropped to 8%. Collectively, these findings are consistent with existing evidence that affective polarization exacerbates the impact of partisanship on behavior (e.g., Iyengar and Westwood (2015); McConnell, Margalit, Malhotra, and Levendusky (2018)), and with the recent findings of Fos, Kempf, Tsoutsoura (2024), who find that executive teams in U.S. firms have also become more partisan in recent years.

We analyze deal-level data to consider confounding factors and explore the mechanisms underlying these dramatic trends. We begin by investigating the effect of *Political Divergence* between firms on the likelihood of a merger. Following the method of Bena and Li (2014), we estimate the likelihood of mergers and acquisitions by generating synthetic (or pseudo) acquirers and targets for each merger in our sample of 2,228 mergers from 1985-2019. We implement this procedure using three different matching rules. First, we match each acquirer and target with random firms. Second, we match each acquirer and target with industry- and size-matched firms. Third, we match each acquirer and target with industry-, size-, and book-to-market-matched firms.

Across all approaches and matched samples, we find that greater *Political Divergence* corresponds to a lower likelihood of a future merger announcement. The estimates are economically meaningful and imply that an increase of one standard deviation in *Political Divergence* reduces the likelihood of a merger by 0.6 to 1.3 percentage points (or 6.4% to 14.6% relative to the sample-mean pseudo-likelihood of 9.2%). These estimates are statistically significant at the 1% level in all specifications. They hold after controlling for the geographic distance between the firms, product similarity (Hoberg and Phillips (2010, 2016)), acquirer- and target-specific characteristics, and industry-by-year and deal fixed effects.

The findings also hold after controlling for differences across other, nonpolitical dimensions of corporate culture – Innovation, Integrity, Quality, Respect, and Teamwork – adopted from Li, Mai, Shen, and Yan (2020), and after controlling for differences across Environmental, Social, and Governance (ESG) policies (Bereskin, Byun, Officer, and Oh (2018)).[4] The relation between *Political Divergence* and the likelihood of merger announcements remains equally important, both economically and statistically, after controlling for corporate cultural or ESG differences. Thus, *Political Divergence* appears unrelated to apolitical corporate culture or ESG differences, consistent with existing research in political science that documents distinct, untampered effects of political differences on group attitudes compared to other cultural or social divides (e.g., Himmelfarb and Lickteig (1982); Iyengar and Westwood (2015)).

We conjecture that the role of *Political Divergence* should be stronger when nationwide affective polarization is greater. We use a measure of affective polarization – the Partisan Conflict Index constructed by Azzimonti (2018) – to estimate the role of *Political Divergence* separately during periods of low and high polarization. We find that the relation between *Political Divergence* and merger likelihood is more pronounced when affective polarization is higher. During periods of low polarization, we estimate that a one standard deviation increase in *Political Divergence* decreases merger likelihood by only 0.4 percentage points. However, when polarization is high, the estimate more than triples to 1.3 percentage points (or 14.3% of the sample mean). The difference between the two coefficients is statistically significant at the 5% level. Taken together,

[4] We thank Kai Li, Feng Mai, Rui Shen, and Xinyan Yan for sharing their corporate culture data with us.

the estimates suggest that affective polarization strengthens the role of *Political Divergence* in merger formation.

In the next set of analyses, we explore the mechanisms underlying the relation between mergers and *Political Divergence*. First, we hypothesize that political differences and affective polarization can create considerable post-merger integration costs. These differences, however, are only relevant if the acquirer and target are planning to integrate their businesses. To test this hypothesis, we search the merging firms' SEC filings for words related to integration. We then re-estimate the analyses separately for firms that emphasize integration in their post-merger filings and those that do not. An increase of one standard deviation in *Political Divergence* corresponds to a decrease of 2.17% in merger likelihood (23.63% of the mean pseudo-likelihood) for firms that emphasize integration, compared to a decrease of 1.10% (11.96% of the mean) for firms that do not, and this relation is only statistically significant (at the 1% level) for firms emphasizing integration.

Second, we consider the distinct role of political divergence between top management teams and between rank-and-file employees. In the baseline analyses, we find that higher political divergence between both groups independently and jointly lowers the likelihood of a merger announcement. When considered jointly, an increase of one standard deviation in *Political Divergence* between top managers corresponds to a decrease of 0.9 percentage points (10.0% of the mean) in merger likelihood. For *Political Divergence* between rank-and-file employees, the estimate is 1.8 percentage points (19.4% of the mean). Moreover, for firms that emphasize integrating their workforces, only the political divergence between their rank-and-file employees significantly reduces the likelihood of merger announcement.

Third, we investigate post-merger employee turnover in both deal-level and employee-level specifications. At the deal-level, we expect higher turnover rates following mergers between politically divergent firms. Indeed, we find that an increase of one standard deviation in *Political Divergence* corresponds to an increase of 1.2 – 2.1 percentage points in employee turnover rates in the year following a merger. At the employee-level, we find that Democrat employees are more likely to leave if their firm merges with a Republican-leaning firm and vice versa.

Together, the findings are consistent with the hypothesis that affective polarization, that is, the dislike/distrust towards those from the other party (e.g., Iyengar, Sood, and Lelkes (2012); Iyengar, Lelkes, Levendusky, Malhotra, and Westwood (2019)), increases post-merger integration costs. Consequently, political divergence reduces the ex-ante likelihood of merger announcement, particularly when workforce integration is important, and leads to lower employee retention rates, particularly those with opposing views.

We alert the reader that we cannot fully rule out the existence of omitted variables in the analyses of *Political Divergence*. However, we argue that our collective evidence mitigates such concerns. Most importantly, the impact of alternative confounding factors that are unrelated to political attitudes would still need to vary in the time-series with aggregate levels of political polarization. Moreover, such factors would need to correlate with the importance of labor force integration, and to predict post-merger employee retention, through channels unrelated to political divergence and affective polarization.

In the final set of analyses, we investigate the relation between *Political Divergence* and merger outcomes. We begin by studying merger announcement returns. One possible scenario is that mergers of politically divergent firms occur when their managers incorrectly ignore or undervalue the costs of political integration. Assuming investors recognize these costs, announcement returns of more politically divergent mergers would be lower, all else equal. An alternative scenario is that managers correctly evaluate the costs of political integration. Under this view, equilibrium announcement returns, which capture the *net* expected value of mergers, would not be systematically related to *Political Divergence.* The findings suggest that announcement returns are unrelated to *Political Divergence.* The estimates are economically tiny and statistically insignificant at conventional levels. As such, the results are consistent with the view that managers of merging firms consider political integration costs in their merger decisions. Consistent with these findings, we also do not find a systematic relation between *Political Divergence* and merger withdrawals, ex-post operating performance, or future spinoffs.

Overall, our paper contributes to a growing body of research that studies the implications of political partisanship for economic behavior, including that of households (e.g., Makridis (2022); McGrath (2017); Mian, Sufi, and Khoshkhou (2018); Coibion, Gorodnichenko, and Weber (2020); Meeuwis, Parker, Schoar, and Simester (2022)), judges (e.g., Posner (2008); McKenzie (2012); Chen (2020)), investors (Cookson, Engelberg, and Mullins (2023)), entrepreneurs and innovators (Engelberg, Guzman, Lu, and Mullins (2023); Engelberg, Lu, Mullins, and Townsend (2023)), corporate executives (Fos, Kempf, and Tsoutsoura (2024)), and credit analysts (Kempf and Tsoutsoura (2021)). While these studies explore unilateral political views and economic decisions, we study bilateral corporate decisions in a setting where political partisanship is measured directly across the two interested counterparties (the acquirer and the target).

Our paper also contributes to a large body of research that studies the determinants and consequences of mergers. Some researchers focus on the value-maximizing attributes of mergers (e.g., Matsusaka (2001); Jovanovic and Braguinsky (2004)), while others study inefficiencies, possibly driven by agency conflicts (e.g., Baumol (1959); Jensen (1986, 1993); Stulz (1990)) or hubris (Roll (1986)). We add to this literature by showing that the political fit between acquirers and targets is an important determinant of merger formation, with implications for integration costs and employee retention.

This study also broadly relates to prior studies of the interaction between mergers and politics or regulation. Holburn and Bergh (2014) show that mergers in regulated industries are preceded by increases in election campaign contributions to influence regulatory merger approvals. Dinc and Erel (2013) provide evidence on the involvement of European governments in acquisitions to keep target companies domestically owned. Aktas, de Bodt, and Roll (2004), Carletti, Hartmann, and Ongena (2015), and Duso, Neven, and Röller (2007) study the stock market response to regulatory decisions or legislative actions. Contrary to prior work, which focuses on the role of governments and regulators in mergers, we study the role of political partisanship and polarization across the acquirer and the target themselves.

Lastly, our paper also contributes to understanding the role of culture and trust in mergers. Ahern, Daminelli, and Fracassi (2015) find that the volume of cross-border mergers is smaller when countries are more culturally distant. Li, Mai, Shen, and Yan (2020) generate machine-learning-based measures of corporate culture and show that it plays an important role in merger incidence. Bereskin, Byun, Officer, and Oh (2018) show that similarity in firms' corporate social responsibility is positively correlated with merger incidence and performance. Graham, Grennan, Harvey, and Rajgopal (2022) provide survey evidence that 46% of executives would walk away from a culturally misaligned target. More broadly, to the extent that political similarity fosters trust, our paper relates to the studies by Guiso, Sapienza, and Zingales (2009) and Bottazzi, Da Rin, and Hellmann (2008), which demonstrate the importance of trust in cross-border financial investments. Our results establish that, even within a country, trust of those with politically different views is a significant factor, whose importance has risen in parallel with polarization.

# 2 Data and Variables

*2.1 Announced Merger Deals*

We obtain information on all U.S. domestic merger and acquisition deals announced between 1985 and 2019 with a transaction value of at least $10 million from the Thomson Reuters Securities Data Company (SDC) Platinum database. We require that both the acquirer and the target be publicly listed, match with the Center of Research in Security Prices (CRSP) and Compustat databases, and have nonmissing SIC codes. Before matching with political affiliation measures, these sample screens yield a sample of 5,570 deals.

Using these data, we construct various variables that characterize acquirers, targets, and announced merger deals. Industries are defined using 2-digit SIC codes. The indicator variable *Diversifying* equals one if the acquirer's industry differs from the target's. The indicator variable *Hostile* equals one for deal attitudes other than "Friendly." The indicator variable *Withdrawn* equals one for deals with a "Withdrawn" status. We compute *Abnormal Announcement Return* as the value-weighted average of the acquirer and target CAPM cumulative abnormal return over the [-1,1] days relative to announcement date zero. We also calculate the following firm-level financial variables: *Book Assets*, *Book to Market*, *Sales Growth*, *Book Leverage*, *Return on Assets*, *Cash Ratio*, and *Past Return*. We define these (and all other) variables in Appendix A.

We augment these data with headquarter location data from SEC filings to calculate a measure of physical distance, labeled *HQ Distance,* between the acquirer and target. We also obtain product similarity scores from the Hoberg and Phillips Data Library to construct the indicator variable *Similar Products,* which equals one if firms have similar products. We obtain measures of corporate culture from Li, Mai, Shen, and Yan (2020) and Environmental, Social, Governance (ESG) ratings from RepRisk, and construct measures of corporate culture and ESG differences between firms. Using post-merger announcement 10-K filings data, we define an indicator variable, *Integration,* that equals one for deals with above median mentions of the term "integration" or related words. Lastly, we obtain data on spinoffs from the SDC New Issues database to examine if the combined firm has a spinoff in the three years after deal completion.

*2.2 Political Affiliation, Political Divergence, and Affective Polarization*

We measure the political affiliations of employees within firms using two separate methods. In the first method, we obtain state voter registration data through Freedom of Information Act requests. The data include political party registrations for voters in party primary elections. We match voters to LinkedIn profiles obtained through Revelio Labs by exact name and estimated birth year to obtain employment histories. Then, we match LinkedIn employer names to historical CRSP firm names. These data include 6,090,261 employees, from 12,753 firms, over the period 1980-2019, whose LinkedIn profile matches a unique voter registration record.

In the second method, we obtain information on individual contributions to political campaign committees from the Federal Election Commission (FEC) over the period 1978-2019. The FEC records self-reported donor information including the name of the donor, state, zip-code, and city where the donor resides, and the donor's employer name. We match these self-reported employer names to CRSP. These data include 1,555,766 contributions from 395,124 employees of 9,522 firms. We provide details on the methods used to match records in the Internet Appendix.

In the main sample, we calculate *Democratic Affiliation* as the average of (1) the number of a company's employees, identified through LinkedIn, that are registered as Democrats, divided by its total number of employees registered as Democrats or Republicans; and (2) the number of individual employees' political donations to Democrat committees divided by the total number of donations to both Democrat and Republican committees in the past two presidential election cycles. In cases where one of the two measures is missing, we use the other.[5] The variable *Democratic Affiliation* ranges from 0 to 1 and increases as the percentage of the firm's politically-involved employees that are affiliated with the Democratic party increases. After matching to the sample of merger announcements, we obtain the final sample of 2,228 announced deals with information about the *Democratic Affiliation* of both the acquirer and the target. In the Internet Appendix, we provide summary statistics for the overall sample of firms we match to CRSP and Compustat.

To measure the divergence in political attitudes across employees at the acquirer and the target, we calculate the absolute value of the difference between the acquirer's *Democratic Affiliation* and the target's *Democratic Affiliation.* This variable, *Political Divergence*, ranges from

[5] For robustness, we also re-estimate the analyses in each of the two samples – the one based on LinkedIn and voter registration records and the one based on employees' personal political donations – separately.

0 to 1 and increases as the misalignment between acquirer and target employees' political affiliations increases. The variable is not linearly determined by the acquirer and target *Democratic Affiliation* measures, allowing us to include them as control variables in our tests. Divergence of political opinions could hamper merger negotiations between executives, so we also construct measures isolating the political affiliations of CEOs, directors, and presidents (hereafter, top managers). We identify individuals using job titles in LinkedIn or by using names from BoardEx. We construct *Top Management Democratic Affiliation* as before using only the voter registrations and individual donations of top managers, and we construct *Rank-and-File Democratic Affiliation* analogously using all other employees.

To measure national affective polarization, we use the Partisan Conflict Index constructed by Azzimonti (2018) and obtained from the Federal Reserve Bank of Philadelphia website. The Partisan Conflict Index is computed monthly and measures the frequency of newspaper articles reporting political disagreement about government policy scaled by the total number of news articles in the same newspapers over the same month. We take the annual average and standardize it by subtracting the sample mean and then dividing by the sample standard deviation to generate *Standardized Partisan Conflict Index*. Finally, we define the variable *High PCI*, an indicator variable that equals one if *Standardized Partisan Conflict Index* is above zero. For robustness, we construct an alternative measure of national affective polarization using roll call votes from the U.S. House of Representatives. We describe the procedure in the Internet Appendix.

### *2.3 Employee Separation*

We use LinkedIn profiles to identify a sample of employees who worked at either the acquirer or target in the year prior to the merger announcement. Then, we determine which employees remain at the combined firm one year after merger announcement. To measure separation rates at the firm level, we define the variable *One Year Separation* as the percentage of the observed pre-announcement employees from either the acquirer or the target who are no longer employed at the combined firm one year after merger announcement. We measure separation from the announcement date rather than the completion date because employees might separate voluntarily once the deal is announced, and even if it fails to complete. This approach measures turnover and separation more accurately than changes in employee headcounts because headcounts can increase, decrease, or remain constant even when turnover is high.

To measure employee separation along party lines, we construct the variable *Disproportionate Democrat Separation* defined as the percentage of separating employees who are Democrat minus the percentage of pre-merger employees who are Democrat. The variable increases as Democrat employees separate from the combined firm at greater-than-random rates. The variable is opposite in sign but identical in magnitude if constructed using Republican employees instead. Thus, disproportionate Democrat separation is also disproportionate Republican *retention* and vice versa.

We use employee-level data to test whether employees are more likely to separate when their firm merges with a politically divergent workforce. Using the sample of employees registered with the Democrat or Republican party, we construct the indicator variable *Employee Separation*, which equals one if the employee separates from either the acquirer, target, or combined firm within one year of the merger announcement, and zero otherwise. In addition, we construct the indicator variable *Opposed*, which equals one if the employee's political party is opposite the majority party of the merger counterparty, and zero otherwise. For example, if a target employee is registered as Democrat, and the acquirer's *Democratic Affiliation* is less than 0.5, *Opposed* will equal one.

### *2.4 Summary Statistics from Announced Deals*

Table 1 presents summary statistics for acquirers (panel A), targets (panel B), deals (panel C), and employees (panel D) used in the analyses. Comparing panel A to panel B shows that the average acquirer and the average target have similar values of *Democratic Affiliation*. Acquirers have greater *Book Assets* and *Return on Assets;* lower *Book to Market*, *Cash Ratio*, and *Past Return*; and similar *Sales Growth* and *Book Leverage*. We control for these characteristics in our analyses. On average, we measure *Democratic Affiliation* using 1,391 observations of employees' voter registrations and individual donations in acquirers and using 133 observations in targets. In both panels, *Top Management Democratic Affiliation* is lower than *Rank and File Democratic Affiliation,* consistent with Fos, Kempf, and Tsoutsoura (2024), who find that executive teams have become more Republican.

Panel C of Table 1 presents summary statistics for the announced deals in each sample. The average *Political Divergence* of announced deals is 0.22. The average physical distance between the headquarters of acquirers and targets is 862 miles. Among announced deals, 29%

involve parties with similar products, and 37% involve parties with differing 2-digit SIC codes. We control for these predictors in our analysis. The average deal value is $4.62 billion. Around 12% of deals are hostile. Political divergence among top managers is greater than political divergence among other employees in the sample. The abnormal announcement return of deals in the sample is 2%. Averaged over deals, we estimate a 24% rate of employee separation[6]. We estimate an average *Disproportionate Democrat Separation* of 3%, with an interquartile range from -1% to 7%. In the sample, 16% of the deals are eventually withdrawn. After completion, 5% of combined firms have a spinoff within three years.

Panel D of Table 1 presents summary statistics for employees registered as Democrats or as Republicans who work for a merger participant in the year before the announcement. In 56% (44%) of observations, the employee registers with the Democrat (Republican) party. On average, 21% of employees separate within one year of a merger announcement.

# 3 Aggregate Evidence

We begin the empirical analyses by providing market-wide evidence on merger announcements between politically similar and dissimilar firms. Importantly, the measurement of *Political Divergence* depends on the underlying distribution of firms' political affiliations. To provide an extreme example, if all firms had the same value of *Democratic Affiliation* (e.g., zero), we would observe *Political Divergence* of zero in all mergers by definition. We address this issue using the procedure in Hoberg and Phillips (2010). In particular, we begin by constructing all possible pairs of firms with *Democratic Affiliation* data to generate a hypothetical distribution of *Political Divergence*. Then, in Figure 1, we compare the distribution of *Political Divergence* from realized announced deals against the hypothetical distribution from all firm pairs. Figure 1 shows that the mass of the distribution from announced deals has closer-to-zero *Political Divergence* compared to the hypothetical mass. A $\chi^2$ test comparing the distributions rejects the null hypothesis with 99% confidence ($\chi^2 = 124.8$, $p = 0.00$). This suggests that, in aggregate, merger announcements tend to occur between firms with more similar political affiliations than would be predicted by random pairing.

[6] According to the Society for Human Resource Management, the median employee turnover rate is 15%. See: https://www.shrm.org/topics-tools/research/shrm-benchmarking#accordion-a5599cb1d9-item-b5dbc3c3b3 and https://www.shrm.org/topics-tools/news/hr-magazine/5-ways-to-manage-high-turnover

In Table 2, we present the distribution of merger announcements across ranges of *Political Divergence* throughout the 9 presidential election cycles from 1988 to 2020. Rows correspond to election cycles and columns correspond to bins of *Political Divergence*. The main takeaways from Table 2 are twofold. First, cross-sectionally, within each election cycle, the percentage of mergers declines monotonically as the political divergence of the merging parties increases. Second, in the time-series, the percentage of mergers between politically aligned firms increases over time (column 2), whereas the percentage of mergers between politically divergent firms declines over time (columns 4 and 5). As we discuss later, aggregate levels of polarization increase dramatically around 2010. It is therefore illustrative to compare the first five election cycles (1988-2004) to the last four election cycles (2008-2020) in Table 2. The estimates show that the percent of deals in the lowest bin of *Political Divergence*, reported in column 2, averages 59.6% in the first five election cycles, but increases to an average of 67.5% in the last four election cycles. Conversely, the average percentage of deals in the higher ranges of *Political Divergence* declines from the first five election cycles to the last four election cycles.

In Table 2, we also formally test whether the distribution of *Political Divergence* in realized deal announcements differs from the hypothetical distribution derived from all firm pairs.[7] Specifically, for each election cycle, we calculate the $\chi^2$ goodness of fit test statistic against a hypothetical distribution of *Political Divergence* using all firm pairs in that cycle. In the bottom row, we conduct the test using all announced deals and all possible firm pairs throughout the sample period. The $\chi^2$ rejects the hypothetical distribution at the 5% level or better in all election cycles except the first election cycle ending in 1988. Furthermore, consistent with the increase in polarization over time, only one estimate rejects the hypothetical distribution at the 1% level in the first five election cycles. In contrast, the estimates reject the hypothetical distribution at the 1% level in three of the four election cycles ending in 2008 and onwards.

Overall, these findings suggest that politically divergent firms have become less likely to merge with each other over time. Next, we test the hypothesis that the above trend can be attributed to the rise in affective polarization in the United States over time. This hypothesis is consistent with prior research, which shows that affective polarization exacerbates the impact of partisanship on behavior (e.g., Iyengar and Westwood (2015); McConnell, Margalit, Malhotra, and Levendusky

[7] We provide the hypothetical distribution in the Internet Appendix.

(2018)), and with the recent findings of Fos, Kempf, and Tsoutsoura (2024) that show executive teams in U.S. firms have also become more partisan in recent years.

To explore this hypothesis, we use the Partisan Conflict Index constructed by Azzimonti (2018) and maintained at the Federal Reserve Bank of Philadelphia. We calculate an annual *Standardized Partisan Conflict Index* and plot it in Figure 2. The figure shows that the values of the index are considerably higher in the second half of the sample period, especially starting in 2010. This pattern is consistent with numerous studies showing that polarization and hostility across party lines have increased in the U.S. in recent years (e.g., McCarty, Poole, and Rosenthal (2016); Haidt and Hetherington (2012); Iyengar, Lelkes, Levendusky, Malhotra, and Westwood (2012); Lott and Hassett (2014); Iyengar and Westwood (2015); Gentzkow (2016); Boxell, Gentzkow, and Shapiro (2017); Autor, Dorn, Hanson, and Majlesi (2020)). We also note that political polarization appears lower during NBER recessions.

We begin these analyses in Panel A of Figure 3, which plots the yearly average of *Political Divergence* for the sample of announced deals alongside the average for hypothetical deals. The estimates show a decline in the average *Political Divergence* of announced deals that is not present for hypothetical deals. In the four years ending in 1989, the average *Political Divergence* for announced deals was 0.256 and the average for hypothetical deals was 0.286. By the end of the sample in 2019, the average *Political Divergence* between acquirers and targets declines by more than 19% to 0.207, and the 95% confidence interval no longer includes the hypothetical deal average of 0.271. This pattern suggests that firms have increasingly opted to merge with politically similar firms over time.

In Panel B of Figure 3, we focus on the top decile of politically divergent mergers and plot their relative prevalence (i.e., *%Top Decile*) before and after the sudden rise in polarization in 2010 shown in Figure 2. Absent time series changes, the top decile of *Political Divergence* would be around the 10% level in each time period. Instead, the value of *%Top Decile* is 10.74% before 2010, and only 6.72% after 2010, indicating that the occurrence of mergers between highly politically divergent firms has declined following the rise in polarization in 2010.

In Figure 4, we investigate the implications of this trend for the geographical landscape of mergers and acquisitions in the United States. Specifically, we explore whether the decline in the prevalence of merger announcements between politically divergent firms has led to a decline in mergers across firms from politically divergent states. We measure the political alignment of states using popular votes in the preceding presidential election. We sort states into terciles based on the fraction of popular votes cast for the Democrat nominee divided by the fraction of popular votes cast for either the Democrat or Republican nominee. We define two states as politically divergent if they are two terciles apart. We then classify all mergers each year into three categories: same state mergers, politically similar state mergers, and politically divergent state mergers.

Panel A of Figure 4 plots the annual percentage of interstate mergers occurring between companies in politically divergent states. The main finding in Panel A is that the prevalence of mergers between firms headquartered in politically divergent states has declined over time and has fallen sharply since 2010. In Panel B, we divide the sample period around 2010. Before 2010, 13.1% (18.1%) of all (interstate) mergers occurred between firms from politically different states. After 2010, this number dropped to 7.0% (9.9%). Taken together, the two panels of Figure 4 show that an increasing majority of mergers occur between firms from politically similar states or from the same state.

In Table 3, we formally explore the correlation between affective polarization and political divergence in mergers over time using aggregate time series fractional logistic regressions. We use fractional logistic regressions because all the dependent variables in Table 3 lie on the interval [0,1]. In column 1, we regress the annual average *Political Divergence* for announced deals on the *Standardized Partisan Conflict Index*. The estimates suggest that higher political polarization corresponds to lower average political divergence between acquirers and targets (coefficient = -0.077, z-stat = -3.51). A one standard deviation increase in the *Standardized Partisan Conflict Index* corresponds to a decrease of 0.013 in the yearly average political divergence of announced mergers, a 0.013 / 0.22 = 5.9% decrease relative to the overall average *Political Divergence*.

In column (2), we focus on the highest levels of political divergence. Specifically, column (2) provides estimates from regressions explaining the likelihood of mergers between firms in the top decile of political divergence. The results show that higher political polarization correlates with a lower incidence of the most politically divergent mergers. The estimates imply that a one standard deviation increase in the *Standardized Partisan Conflict Index* corresponds to a decrease of 1.5 percentage points in the proportion of high divergence deals, a decline of 15% relative to the sample mean of 10%. Finally, we regress deals between politically divergent states as a percentage of all deals (column 3) and interstate deals (column 4). The estimates show that greater affective polarization relates to a decline in deals between politically divergent states. A one standard deviation increase in polarization corresponds to a 2.2 (2.9) percentage point decline in deals between politically divergent states, or 19.6% (18.6%) relative to the mean for all deals (interstate deals).

Taken together, the results in this section provide aggregate evidence of a declining trend in mergers between politically divergent firms over time, coinciding with the rise of political and affective polarization in the United States. This trend is also reflected in the spatial distribution of mergers – the incidence of deals between firms from politically divergent states has also declined over time.

An important consideration in the analyses of political divergence is the existence of alternative explanations and omitted variables. The analyses thus far suggest that any alternative explanation, which is unrelated to political attitudes, would still need to vary in the time-series with aggregate levels of polarization. Nevertheless, comparing announced deals against all hypothetical deals does not control for other predictors of merger formation, such as industry affiliations, product similarity, or physical locations. We address this issue in the next section by providing estimates from deal-level specifications that allow us to control for confounding effects through matching and alternating combinations of control variables and fixed effects. Further, we provide micro evidence on the mechanisms underlying the aggregate time-series shifts documented in this section.

# 4 Deal-Level Evidence

In this section, we provide estimates from merger selection models following the method used by Bena and Li (2014). Specifically, we match each acquirer with up to five pseudo-targets, and each target with up to five pseudo-acquirers. This procedure generates up to ten pseudo deals for each announced merger deal. To compile a set of pseudo-acquirers and pseudo-targets for each deal, we first match each acquirer and target to firms in the same industry from the Compustat/CRSP merged database, using information from the year preceding the merger announcement. As such, this pool of potential merger partners captures merger clustering by industry and time (Mitchell and Mulherin (1996); Andrade, Mitchell, and Stafford (2001); Maksimovic, Phillips, and Yang (2013); Harford (2005)).

Next, we calculate propensity scores based on size (book assets) and book-to-market ratios for each firm in the above pool of same-industry pseudo merger partners. We match on book-to-market ratios because prior studies show that they capture important drivers of mergers, such as growth opportunities (Andrade, Mitchell, and Stafford (2001)), overvaluation (Shleifer and Vishny (2003), Rhodes-Kropf and Viswanathan (2004)), and asset complementarity (Rhodes-Kropf and Robinson (2008)). Finally, we select up to five pseudo-partners that most closely resemble the participant's actual partner in the announced deal to create firm-pair observations.[8]

In the resulting sample, we create an indicator variable, *Announced Deal*, equal to one for the firm-pair in the announced deal and equal to zero for the pseudo-deals. We estimate conditional logistic regressions in which the depending variable is *Announced Deal,* and the main explanatory variable is *Political Divergence.*[9] If differences in political affiliation impede a participant's selection of a potential partner, the coefficient estimate on the variable *Political Divergence* would be negative and statistically significant. Alternatively, if political diversity motivates partner selection, the coefficient estimate on *Political Divergence* would be positive and statistically significant, overturning the aggregate patterns shown in Section 3.

We include several control variables to address concerns about omitted variables correlated with *Political Divergence*. First, we include the *Democratic Affiliation* of each acquirer and target to capture direct partisan effects in overall merger participation. Second, we include *HQ Distance*, defined as the physical distance between the headquarters of the acquirer and the target, to control for location effects in merger formation. Third, we include the indicator variable *Similar Products*, which equals one if the firms have similar products according to Hoberg and Phillips (2010).[10] Following Bena and Li (2014), we also include control variables for size, growth, profitability, leverage, liquidity, past stock market performance, and diversification. Finally, we include industry-pair-by-year fixed effects and deal-level fixed effects, which group each announced deal with its associated pseudo-deals (hereafter, Deal fixed effects).

### *4.1 The Likelihood of Mergers*

In this subsection, we present baseline estimates from the merger selection models described above. We present the baseline estimates from the pooled sample in panel A of Table 4. In panel

---

[8] We provide estimates using alternative matching procedures in the Internet Appendix.
[9] We also provide estimates using linear regressions and weighted logistic regressions in the Internet Appendix.
[10] Even though the matching procedure ensures that pseudo-partners are from the same industry as the announced partner, we include the variable *Similar Products* because product similarity within an industry may still impact partner selection.

B, we use a subsample constructed exclusively using voter registrations, and in panel C, we use a subsample using only individual political donations. The columns in each panel differ with respect to the inclusion of control variables, industry-by-year fixed effects, and deal fixed effects. The last column of each panel excludes hostile takeovers to focus on the announcement of negotiated deals. While *Political Divergence* likely decreases the odds of announcing negotiated deals because it adversely affects the success of merger negotiations and post-merger integration, hostile deals are noncooperative and result from disagreement by definition. Hence, we expect the negative effect of *Political Divergence* on the likelihood of merger announcements to be stronger in the subset of negotiated deals.

Across all 12 regression specifications in Table 4, the coefficient on the main variable of interest, *Political Divergence*, is negative and statistically significant at the 1% level. The economic magnitudes of the effect of *Political Divergence* on the likelihood of merger announcements are nontrivial. We estimate that a one standard deviation increase in *Political Divergence* reduces the likelihood of merger announcement by 0.47 to 1.89 percentage points, a reduction of 5.1% to 20.5% relative to the sample-mean pseudo-likelihood of 9.2%.[11] Furthermore, we find that the coefficient estimate on *Political Divergence* is 8-21% larger in magnitude when we exclude hostile bids and only focus on announced merger agreements. This finding is consistent with the conjecture that hostile bids result from disagreements between acquirers and targets that may be exacerbated by, or reflective of, differences in political attitudes.

Taken together, the findings in this subsection show that political similarity across firms positively predicts merger announcements. These findings hold after controlling for the geographical distance and product similarity between firms. They also hold after including industry-by-year fixed effects and deal fixed effects, which absorb the influence of industry shocks and differences across merger deals, respectively. Furthermore, the findings are virtually unchanged across the different samples that use alternative approaches to measure political affiliations based on voter registration records, individual political campaign donations, or both. Lastly, in the Internet Appendix, we provide results from robustness tests that use different matching procedures and different regression functional forms.

[11] We estimate the marginal effect of *Political Divergence* using linear models because the inclusion of fixed effects can confound the interpretation of marginal effects in conditional logistic models. The linear model estimates are in the Internet Appendix.

*4.2 Affective Polarization*

In this subsection, we incorporate the trend of rising affective polarization in the United States to investigate whether it influences the relation between political attitudes and the likelihood of merger announcements. If *Political Divergence* affects merger formation and partner selection, the effect should be stronger during periods of high affective polarization. To test the influence of affective polarization on merger formation, we separately estimate the effects of political divergence between the acquirer and the target in subsamples corresponding to periods of low vs. high affective polarization. We divide the sample using values of the indicator variable *High PCI*, which is equal to one when the value of *Standardized Partisan Conflict Index* is above its mean, and zero otherwise. We also provide estimates from pooled regression specifications that interact the indicator variable *High PCI* with all the other covariates.

Table 5 reports the estimates from these tests. In columns (1) and (2), we separately estimate the effect of political divergence when *High PCI* is equal to zero and one, respectively. The coefficient estimate in column (1), where polarization is lower, is negative but not statistically significant. In contrast, the coefficient estimate on *Political Divergence* in column (2), where polarization is higher, is negative and statistically significant at the 1% level. The estimate in column (2) is more than three times the estimate in column (1). The estimates suggest that a one standard deviation increase in *Political Divergence* reduces the probability of a merger announcement by 1.28 percentage points (13.9% of the mean pseudo-probability) during periods of high polarization. By contrast, during periods of low polarization, the estimates suggest a marginal effect of only 0.38 percentage points (4.1% of the mean) for each standard deviation change in *Political Divergence*. Combined, these estimates suggest that the negative effect of political divergence on the likelihood of merger announcement is considerably stronger when polarization is high.

In column (3), we estimate the effects in a pooled regression that interact all the independent variables with *High PCI.* The coefficient estimate on the interaction term *Political Divergence* × *High PCI* is negative and statistically significant at the 5% level ($z$-statistic = -2.12). As such, the difference between the impact of political divergence on merger announcement during high vs. low polarization periods is not only economically significant, but also statistically significant. Importantly, the interaction terms between the other variables and *High PCI* are all statistically insignificant at conventional levels. These findings can be viewed as placebo tests that show that none of the effects of other, non-political observable attributes of firms, nor the differences between them, correlate with the time-series variation in polarization. Thus, they further mitigate concerns that political divergence and polarization correlate with omitted variables unrelated to politics.

Overall, the results in Table 5 support the hypothesis that greater affective polarization amplifies the role of political divergence in merger formation. The covariation of the effect's magnitude with political polarization conforms to our interpretation that the results reflect the effects of political attitudes rather than a correlated omitted variable unrelated to firms' political attitudes. In the Internet Appendix, we find similar results using an alternative measure of affective polarization obtained from roll call votes in the U.S. House of Representatives.

### *4.3 Nonpolitical Differences in Corporate Culture and ESG Policies*

Existing studies have shown that national cultural values and ESG practices play a role in merger formation and merger success (e.g., Ahern, Daminelli, and Fracassi (2015); Bereskin, Byun, Officer, and Oh (2018)). In this subsection, we aim to investigate the relation between political differences and differences across other aspects of corporate culture and ESG practices. Conceptually, existing research in political science documents distinct, untampered effects of political differences on group attitudes compared to other cultural or social divides (e.g., Himmelfarb and Lickteig (1982); Iyengar and Westwood (2015)). Moreover, the rise in political and affective polarization in the United States implies that political divergence exhibits a unique time trend not shared with other forms of corporate cultural differences.

We consider two sets of measures that aim to capture nonpolitical aspects of corporate culture and ESG practices. First, we use the five measures provided by Li, Mai, Shen, and Yan (2020, hereafter LMSY), namely *Innovation*, *Integrity*, *Quality*, *Respect*, and *Teamwork,* constructed using machine learning techniques. Second, we use Environmental, Social, and Governance (ESG) ratings from RepRisk. An important limitation of these analyses is that the LMSY and ESG measures are not available for all the deals in our sample. In particular, the five corporate culture measures from LMSY (2020) are available only from 2002 to 2018 for the subset of firm-years with electronically available transcripts. Similarly, the ESG ratings from RepRisk are available starting from 2007. Consequently, the sample size of the tests is significantly reduced compared to the baseline specification in Table 4.

Using the five LMSY corporate culture measures, we calculate an overall cultural distance between acquirers and targets, *Aggregate Cultural Distance*, as follows. For each of the five measures, we calculate a distance defined as the absolute value of the difference between the acquirer and the target. We then standardize the five distances by subtracting their respective means and dividing by their respective standard deviations. Lastly, we define the variable *Aggregate Cultural Distance* as the sum of the five distances.[12] For the ESG ratings, we start by transforming the Reputation Risk Rating (AAA to D) to a numerical scale from 1 to 10 in one-unit increments, where a higher number corresponds to a better rating. We then average the numerical score per firm-year to obtain an annual firm-level rating, and compute *ESG Distance* as the absolute value of the difference between acquirers' and targets' Reputation Risk Rating.

To facilitate a meaningful comparison, we standardize *Political Divergence*, *Aggregate Cultural Distance,* and *ESG Distance* by subtracting their respective sample means and dividing by their respective sample standard deviations. The correlation estimate between *Political Divergence* and *Aggregate Cultural Distance* is 0.023, and the correlation between *Political Divergence* and *ESG Distance* is 0.014. These correlations suggest that political differences are distinct from other corporate cultural differences, and they quell concerns about multicollinearity.

In Table 6, we provide estimates from regressions explaining the likelihood of merger announcement. In Panel A, we consider differences in the LMSY measures of corporate culture. Column (1) establishes a baseline coefficient estimate on *Political Divergence* in the smaller subsample of firms with available information on the LMSY measures. As before, the estimate is negative and statistically significant at conventional levels ($z$ = -2.54). In column (2), we estimate the baseline regression specification for *Aggregate Cultural Distance*. The coefficient estimate on *Aggregate Cultural Distance* is negative and statistically significant at the 1% level ($z$ = -2.62). In column (3), we include *Political Divergence* and *Aggregate Cultural Distance* simultaneously. The main takeaways are twofold. First, the coefficient estimate on *Political Divergence* remains negative and statistically significant at conventional levels ($z$ = -2.49). Second, the coefficient estimates on both *Political Divergence* and *Aggregate Cultural Distance* in column (3) are virtually identical to those in columns (1) and (2), respectively. Combined, these findings suggest that both political differences and other corporate cultural differences reduce the likelihood of a merger, and, more importantly, that these two effects are distinct from each other.

[12] We provide estimates using all five distances jointly in the Internet Appendix.

In Panel B of Table 6, we provide estimates that compare between the effects of *Political Divergence* and the effects of *ESG Distance*. As before, column (1) provides a subsample baseline coefficient estimate on *Political Divergence.* As before, it is negative and statistically significant at the 1% level ($z$ = -2.68). In column (2), we include *ESG Distance* but remove *Political Divergence*. There, the coefficient estimate on *ESG Distance* is negative and statistically significant at the 1% level ($z$ = -4.13), suggesting that differences in ESG ratings negatively predict the likelihood of merger announcement. In column (3), we include both *Political Divergence* and *ESG Distance*, and the coefficient estimates are negative and statistically significant at the 1% level for both covariates ($z$ = -2.75 and -4.14, respectively). The magnitudes of the coefficient estimates remain largely unchanged, suggesting that the effects of ESG differences on merger announcements are distinct from those of *Political Divergence*.

In Internet Appendix Table IA3, we also investigate whether political divergence exhibits a time trend, implied by the rise in polarization, which does not apply to nonpolitical corporate cultural differences. The estimates show that the time-series dynamics of political divergence are unique. The average political divergence between acquirers and targets declines as polarization rises, whereas nonpolitical corporate cultural differences do not vary with aggregate polarization.

Overall, the results in this subsection show that *Political Divergence* affects merger formation distinctly from standard measures of corporate cultural dissimilarities related to Innovation, Integrity, Quality, Respect, and Teamwork, or to differences in ESG practices.

### *4.4 Integration*

In this subsection, we study post-merger integration costs. We conjecture that the political divergence between acquirers and targets will be more important for merger formation when the acquirer and target are integrating their business operations. This hypothesis is motivated by ample evidence that political differences are barriers to cooperation. For example, McConnell, Margalit, Malhotra, and Levendusky (2018) show experimentally that partisanship hurts cooperation in everyday economic behavior of workers and consumers. Iyengar and Westwood (2015) show that political polarization exerts powerful effects on nonpolitical judgments and behaviors and leads to confrontation rather than cooperation.

We measure the importance of integration for each announced deal by searching for keywords in the acquirer's Securities and Exchange Commission (SEC) filings following merger announcement. Specifically, we read the closest form 10K/Q and the closest form DEF 14A filed within a year after announcement, and count the number of times the words "integrate" or "integration" appear in the documents.[13,14] We set the indicator variable *Integration* equal to zero for deals in which integration is mentioned less frequently than the median deal, and equal to one when integration is mentioned more frequently than the median. Because we obtain filings from the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) database, tests in this subsection start in 1995 and only include firms with a valid GVKEY-CIK link.

In columns (1) and (2) of Table 7, we separately estimate the relation between *Political Divergence* and the likelihood of merger formation in subsamples formed based on whether integration is mentioned in the acquirer's SEC filings more or less frequently than the median. In column (3), we estimate the effects in a pooled regression that interacts all the explanatory variables with the indicator variable *Integration.*

Column (1) corresponds to the subsample where acquirers in the realized deals mention integration in their SEC filings less frequently than the median acquirer (i.e., *Integration* = 0). The coefficient estimate on *Political Divergence* is positive and not statistically significant at conventional levels. In column (2), we repeat the test where the acquiring firms' SEC filings include above median references to integration. The coefficient estimate on *Political Divergence* is negative and statistically significant at the 10% level, indicating that greater political differences negatively influence the formation of deals when the merging firms plan to combine operations. The estimates suggest that a one standard deviation increase in *Political Divergence* reduces the probability of a merger announcement by 0.74 percentage points (8.0% of the mean pseudo-probability). Together, these estimates suggest that the negative effect of political divergence on the likelihood of merger announcement is significant only when the merging firms plan to combine operations.

In column (3), we estimate the effects in a pooled regression saturated with interaction terms. The coefficient estimate on the interaction term *Political Divergence* × *Integration* is negative (-0.817) and statistically significant at the 10% level (*z*-statistic = -1.88). This suggests

---

[13] An example where mentioning these terms is informative about the cost of integration is the acquisition of Asterias Biotherapeutics Inc by BioTime Inc. BioTime's 10-Q following the acquisition states: "If the merger is completed, BioTime expects to incur significant costs in connection with consummating the merger and integrating the operations of Asterias. BioTime may incur additional costs to maintain employee morale and to retain key employees."
[14] We exclude the acquisition of Rotech Medical Corp by Integrated Health Services Inc and all deals where Maxim Integrated Products Inc is the acquirer.

that the difference between the impact of political divergence on merger announcement when integration concerns are high vs. low is both economically and statistically significant. Furthermore, the interaction terms between the other variables and *Integration* are all statistically insignificant at conventional levels. These findings indicate that the effect of other, non-political differences between merging firms, such as the geographic distance between their headquarters or the dissimilarity between their products, does not correlate with their intention to integrate their operations. Thus, they further mitigate concerns about correlated omitted variables unrelated to politics. Altogether, the findings suggest that integration costs are a unique channel through which political divergence affects merger formation.

### *4.5 Top Management vs. Rank-and-File Employees*

In this subsection, we separately evaluate the role of political divergence between rank-and-file employees and the role of political divergence between top managers. On the one hand, political divergence at both the manager and the employee levels can negatively influence the likelihood of merger formation. Political differences between managers may hinder merger negotiations. Political differences between rank-and-file employees may increase the expected costs of integration and employee retention following mergers. If managers recognize these costs, they will be less likely to merge with firms whose rank-and-file employees hold different political views. On the other hand, managers are seasoned professionals who may ignore their political differences during merger negotiations. They may also ignore or underestimate the costs of integration between politically divergent workforces. Under this view, political divergence between managers or between rank-and-file employees will not affect the likelihood of mergers.

To test these views, we construct separate measures of political divergence between top managers and between rank-and-file employees. We identify the CEOs, presidents, and board members, and construct measures of *Top Management Democratic Affiliation* and *Top Management Political Divergence* analogously to our main measures, but only using data on the political affiliation of top management. We use all other employees to construct measures of *Rank-and-File Democratic Affiliation* and *Rank-and-File Political Divergence*.[15]

[15] We provide details on the procedures for identifying top management and for creating these measures for both the voter registration sample and the individual donation sample in their respective sections in the Internet Appendix.

We prepare deals and pseudo-deals as before, restricted to the intersection of firm-years where we observe top managers' and rank-and-file employees' political affiliations separately. We then re-estimate conditional logistic regressions explaining the likelihood of merger announcement using these new measures. We present the results in Panel A of Table 8. Columns (1) and (2) provide regression estimates when including the two measures separately, and column (3) provides estimates when including both measures simultaneously.

In column (1), the coefficient estimate on *Top Management Political Divergence* is negative and statistically significant at the 10% level. It implies that a one standard deviation increase in *Top Management Political Divergence* reduces the likelihood of merger announcement by 1.3 percentage points (14.2% of the mean). In column (2), the coefficient estimate on *Rank-and-File Political Divergence* is negative and statistically significant at the 5% level. It implies that a one standard deviation increase in *Rank-and-File Political Divergence* reduces the likelihood of merger announcement by 2.3 percentage points (24.3% of the mean). In column (3), which includes both measures of political divergence, the coefficient estimate on *Top Management Political Divergence* remains economically similar, but it is not statistically significant at conventional levels. On the other hand, the coefficient estimate on *Rank-and-File Political Divergence* remains both economically and statistically significant at the 5% level. Together, these estimates suggest that the political divergence of managers and of employees both negatively predict merger formation. However, the divergence between firms' rank-and-file employees has a considerably stronger effect than that of divergence between top managers.

In Panel B of Table 8, we directly test the conjecture that political divergence between rank-and-file employees operates through an integration costs channel. Specifically, in columns (1) and (2) we estimate separate conditional logistic regressions explaining the likelihood of merger announcement using *Top Management Political Divergence* and *Rank-and-File Political Divergence*, respectively, interacting all the independent variables with the indicator variable *Integration.* As a reminder, *Integration* equals zero for deals in which integration is mentioned less frequently than the median deal, and one when integration is mentioned more frequently than the median. The main variables of interest in both columns are the interaction terms between *Political Divergence* and *Integration,* which capture the effect of political divergence when the merging firms plan to integrate their operations. Columns (1) and (2) provide strikingly different results. Column (1) shows that political divergence between top managers does not affect the likelihood of merger formation when firms plan to integrate their operations, as the main and interaction coefficients approximately offset each other. Column (2), on the other hand, shows that political divergence between rank-and-file employees reduces the likelihood of merger formation when firms plan to integrate their operations, and only then.

Taken together, the main takeaways from Table 8 are twofold. First, while the political divergence between both top managers and rank-and-file employees matters for merger formation, the effect of that between rank-and-file employees is considerably stronger. Second, the political divergence between rank-and-file employees operates through an integration costs channel, implying that it is a fundamental barrier to realizing value from a merger.

### *4.6 Employee Retention*

In this subsection, we provide additional evidence on the role of political divergence in workforce integration following mergers. In particular, we provide estimates from both deal-level and employee-level analyses that evaluate the relation between political divergence and employee separation. We argue that political divergence between the acquirer and the target could motivate separation for several reasons. First, upon learning about the merger, employees at either the acquirer or the target might quit to avoid working for a politically misaligned employer or working with politically divergent peers. This view is highlighted in a recent Fortune article, which suggests that "Over a third of American workers would consider quitting if their CEO's politics don't align with their own."[16] Second, as noted by Lambrecht and Myers (2007), mergers often involve layoffs of redundant employees. Because political party registration is not a protected class in the U.S., managers may choose to retain employees with politically similar views.

To test these hypotheses, we collect employment histories from LinkedIn profiles to identify employees at the acquirer and the target in the year before a merger announcement, and follow their employment trajectory after the announcement. Using these data, Panel A of Table 9 provides estimates from deal-level analyses that investigate whether greater political divergence predicts higher rates of employee separation following merger announcements. In columns (1) and (2), we estimate regressions in which the dependent variable is *One Year Separation*, the fraction of employees who separate from the merging firms within one year of a merger announcement.

[16] https://fortune.com/2024/03/21/workers-consider-quitting-disagree-ceo-politics/

The estimates in columns (1) and (2) of Table 9 suggest that higher political divergence between the acquirer and the target corresponds to higher employee separation rates in the year following a merger. The coefficient estimates on *Political Divergence* are positive and statistically significant at the 5% levels ($t$ = 2.48 and 2.32, respectively). They suggest that an increase of one standard deviation in *Political Divergence* corresponds to a 1.2 percentage point increase (5.0% of the mean) in separation rates within one year of a merger announcement. The finding is consistent with the hypothesis that merging politically misaligned workforces results in lower retention and higher integration costs for the combined company.

In columns (3) and (4), we refine the analyses to explore whether employee separation occurs along party lines. Specifically, we conjecture that Democrats at the target will separate at disproportionately higher rates when the acquirer aligns more closely with the Republican party relative to the target, and vice versa. To test this conjecture, we construct a measure of *Disproportionate Democrat Separation,* which calculates the percentage of separating employees (from both the acquirer and the target) who are registered as Democrats relative to the overall percentage of employees (from both the acquirer and the target) who are registered as Democrats. Intuitively, this measure aims to identify the incremental separation rate of Democrat employees relative to the separation rate predicted by their overall share in the workforce.

In columns (3) and (4) of Table 9, we provide estimates from linear regressions explaining *Disproportionate Democrat Separation.* The key independent variable is *Target Less Acquirer Democratic Affiliation*, which captures the political affiliations of acquirers relative to their targets. This variable increases as the acquirer aligns more closely with the Republican party relative to the target. A positive and statistically significant coefficient on this variable suggests disproportionate separation along party lines in completed mergers.

The results in columns (3) and (4) support the conjecture that employee separation occurs along party lines. In particular, the coefficient estimates on the variable *Target Less Acquirer Democratic Affiliation* are positive and statistically significant at conventional levels. The estimates are also economically significant. A one standard deviation increase in *Target Less Acquirer Democratic Affiliation* corresponds to a 0.82-0.93 percentage point increase in *Disproportionate Democrat Separation*, a 27.3-30.9% increase relative to the mean rate of disproportionate separation of 2.6%. Interpreted broadly, these estimates suggest that Democrats separate from merging firms at greater rates when the acquirer is more closely aligned to the Republican party, and vice versa.

In Panel B of Table 9, we present estimates from employee-level regressions that allow us to consider the political affiliation of each employee, and control for employee demographics such as education, gender, race, ethnicity, tenure, and experience, which may play a role in employee separation decisions. The dependent variable in the regressions is *Employee Separation*, defined as an indicator variable equal to one if the employee separates in the year following the merger announcement and zero otherwise. We include the explanatory variable *Opposed*, defined as an indicator variable equal to one if the employee is Democrat and the merger partner is majority Republican or if the employee is Republican and the merger partner is majority Democrat. The key explanatory variable is the interaction of *Opposed* and *Political Divergence* which incorporates the magnitude of political differences between acquirer and target.

The results of Panel B show that an employee is more likely to separate from the company following announcements of mergers with companies whose employees tend to support the opposing party. The estimates are statistically significant at the 1% level, economically meaningful, and imply that an increase of one standard deviation in interaction term interaction *Opposed* × *Political Divergence* leads to an increase of 0.87 percentage points (4.13% of the mean) in the likelihood of employee separation in the year following the merger announcement.

Overall, the results in Table 9 provide deal-level and employee-level evidence that employees are more likely to separate from their firms if they merge with politically divergent firms. As such, these findings provide evidence of a real cost in mergers between politically divergent firms, namely, the cost of employee retention and replacement. As such, they provide direct evidence supporting the link between political divergence and integration costs in merger deals. To the extent that managers anticipate these costs when they consider merger proposals, these findings provide a novel channel explaining why merger announcements between politically divergent firms are relatively uncommon.

## 5 Announcement Returns, Withdrawals, Performance, and Spinoffs

In this section, we examine merger outcomes conditional on merger announcement or on merger completion. In particular, we investigate merger announcement returns and deal withdrawals, as well as post-merger operating performance and spinoffs.

*5.1 Announcement Returns and Deal Withdrawal*

We begin the analyses by investigating the combined returns of acquirers and targets surrounding merger announcements. On the one hand, mergers between politically divergent firms may occur when managers incorrectly ignore or undervalue the costs of integrating politically divergent companies. Assuming investors correctly evaluate these costs, announcement returns of more politically divergent mergers would be lower, all else equal. On the other hand, managers may correctly evaluate the costs of integrating politically divergent firms, and proceed with the merger only when they assess that the net present value of the merger is still positive. Under this view, merger announcement returns, which capture the net present value of mergers, would not be systematically related to *Political Divergence.*

We investigate these competing hypotheses by estimating regressions explaining abnormal merger announcement returns. Column (1) and (2) of Table 10 report the results. In column (1), the dependent variable is *Abnormal Announcement Return*, defined as the value weighted average of the acquirer and target CAPM excess return over the [-1,1] days around the merger announcement date 0, winsorized at the 1st and 99th percentiles. In column (2), the dependent variable is the indicator variable *Negative Announcement Return*, which equals one if *Abnormal Announcement Return* is negative. In both columns, the coefficient estimates on the key explanatory variable, *Political Divergence,* are negative yet statistically indistinguishable from zero at conventional levels. As such, the results are more consistent with the view that managers correctly evaluate the costs of integrating politically divergent firms, and choose to proceed with such mergers based on net present values that account for these costs.

In column (3), we study the likelihood of deal withdrawals following merger announcements. If managers ignore or underestimate the costs of integrating politically divergent firms before announcing a merger, and subsequently learn that they made a mistake (e.g., from the market's reaction), then the likelihood of withdrawals should be higher for more politically divergent mergers. The estimates in column (3), however, do not support this hypothesis. The coefficient estimate on *Political Divergence* is once again indistinguishable from zero at conventional levels, suggesting that political divergence does not predict merger withdrawals.

### *5.2 Post-Merger Operating Performance and Spinoffs*

In the final set of analyses, we evaluate the implications of political divergence for post-merger performance. We calculate return on assets, and following Hoberg and Phillips (2010), we calculate two measures of cash flow-based performance: operating cash flows scaled by book assets and annual sales growth. We average these measures over the three years following merger completion. As an additional measure of post-merger performance, we consider the likelihood of a spinoff in the three years following merger completion. Prior research has shown that firms spinoff underperforming assets to improve their financial positions (e.g., Prezas and Simonyan (2015); Lang, Poulsen, and Stulz (1995)).

We present the results of these analyses in Table 11. The first column provides estimates predicting future return on assets, and the next two columns provide estimates from linear regressions predicting future cash flows. Across the three columns, we find no relation between *Political Divergence* and future return on assets, operating cash flows or sales growth. In column (4), we study the relation between *Political Divergence* and the likelihood of post-merger spinoffs. Here too, we find no relation between *Political Divergence* and spinoffs. Taken together, the estimates in Table 11 are more consistent with the hypothesis that managers correctly assess the costs of political divergence in mergers, and choose to implement mergers based on their assessment of the merger's net present value, taking into account the costs of integrating politically divergent firms.

## 6 Conclusion

This study provides novel evidence that partisanship and political polarization influence the allocation of real assets in the United States. Our main findings are that politically divergent firms are substantially less likely merge, and the percentage of mergers between politically divergent firms has significantly declined over time with the rise of affective polarization in the U.S. This trend also appears in the geographical landscape of mergers and acquisitions in the U.S. The percentage of mergers between firms from politically different states has continuously shrunk over time, and such mergers have all but disappeared in the most recent sample years.

We provide deal-level analyses to account for confounding factors and correlated omitted variables, and to investigate the mechanisms underlying the aggregate trends. We find that differences in political attitudes between firms play an important role in merger decisions and outcomes, and that the nature of these decisions has changed with the rise of political and affective polarization. Specifically, we find that firms are more likely to announce mergers when they have similar political attitudes. These effects strengthen during periods of elevated political polarization, and when the target and the acquirer seek to integrate their business operations. We provide direct evidence at the deal- and employee-levels on the costs of integrating politically divergent firm, suggesting that employee separations are more common following politically divergent mergers.

Collectively, our findings provide some of the first evidence on the real economic effects of the rise in political polarization in the U.S. We document a structural shift in the real asset market for mergers and acquisitions in the U.S., with implications for the allocation of real assets in the economy. Our findings are consistent with numerous studies in political science showing that polarization and hostility across party lines have increased in the U.S. in recent years, and with a growing body of evidence that political polarization exerts powerful effects on nonpolitical behavior.

## References


Ahern, Kenneth, Daniele Daminelli, and Cesare Fracassi, 2015, Lost in translation? The effect of cultural values on mergers around the world, *Journal of Financial Economics* 117, 165–189.

Aktas, Nihat, Eric de Bolt, and Richard Roll, 2004, Market response to European regulation of business combinations, *Journal of Financial and Quantitative Analysis* 39, 731–757.

Andrade, Gregor, Mark Mitchell, and Erik Stafford, 2001, New evidence and perspectives on mergers, *Journal of Economic Perspectives* 15, 103–120.

Autor, David, David Dorn, Gordon Hanson, and Kaveh Majlesi, 2020, Importing political polarization? The electoral consequences of rising trade exposure, *American Economic Review* 110, 3139–3183.

Azzimonti, Marina, 2018, Partisan conflict and private investment, *Journal of Monetary Economics* 93, 114-131.

Baumol, William, 1959, *Business behavior, value and growth* (Macmillan, New York).

Bena, Jan, and Kai Li, 2014, Corporate innovations and mergers and acquisitions, *The Journal of Finance* 69, 1923–1960.

Bereskin, Fred, Seong Byun, Micah Officer, and Jong-Min Oh, 2018, The effect of cultural similarity on mergers and acquisitions: evidence from corporate social responsibility, *Journal of Financial and Quantitative Analysis* 53 (5), 1995-2039

Bonica, Adam, 2013, Ideology and Interests in the Political Marketplace, *American Journal of Political Science* 57, 294–311.

Bottazzi, Laura, Marco Da Rin, and Thomas Hellmann, 2008, Who are the active investors? Evidence from venture capital, *Journal of Financial Economics* 89, 488–512.

Boxell, Levi, Matthew Gentzkow, and Jesse Shapiro, 2017, Greater internet use is not associated with faster growth in political polarization among US demographic groups, *Proceedings of the National Academy of Sciences* 114, 10612–10617.

Carletti, Elena, Philipp Hartmann, and Steven Ongena, 2015, The economic impact of merger control legislation, *International Review of Law and Economics* 42, 88–104.

Chen, Daniel, 2020, Priming ideology: why presidential elections affect U.S. judges, *Available at SSRN 2816245*.

Coibion, Olivier, Gorodnichenko, Yuriy, and Michael Weber, 2020, Political Polarization and Expected Economic Outcomes, *NBER Working Paper 28044*.

Cookson, J Anthony, Joseph E Engelberg, and William Mullins, 2023, Echo Chambers, *The Review of Financial Studies* 36, 450–500.

DellaVigna, Stefano, and Ethan Kaplan, 2007, The Fox News Effect: Media Bias and Voting, *The Quarterly Journal of Economics* 122, 1187–1234.

Dinc, I. Serdar, and Isil Erel, 2013, Economic nationalism in mergers and acquisitions, *The Journal of Finance* 68, 2471–2514.

Duso, Tomaso, Damien Neven, and Lars-Hendrick Röller, 2007, The political economy of European merger control: evidence using stock market data, *The Journal of Law and Economics* 50, 455–489.

Dutz, Mark, 1989, Horizontal mergers in declining industries: Theory and evidence, *International Journal of Industrial Organization* 7, 1 l-33.

Engelberg, Joseph, Jorge Guzman, Runjing Lu, and William Mullins, 2023, Partisan Entrepreneurship, *SSRN Electronic Journal 3821106*.

Engelberg, Joseph, Runjing Lu, William Mullins, and Richard Townsend, 2023, Political Sentiment and Innovation: Evidence from Patenters, *SSRN Electronic Journal 4176649.*

Fos, Vyacheslav, Elisabeth Kempf, and Margarita Tsoutsoura, 2024, The political polarization of corporate America, *NBER Working Paper 30183*.

Gentzkow, Matthew, 2016, Polarization in 2016, Mimeo, Stanford University.

Gentzkow, Matthew, Jesse Shapiro, and Matt Taddy, 2019, Measuring Group Differences in High-Dimensional Choices: Method and Application to Congressional Speech, *Econometrica* 87, 1307–1340.

Graham, John R., Jillian Grennan, Campbell R. Harvey, and Shivaram Rajgopal, 2022, Corporate culture: Evidence from the field, *Journal of Financial Economics* 146, 552–593.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales, 2009, Cultural biases in economic exchange?, *The Quarterly Journal of Economics* 124, 1095–1131.

Haidt, Jonathan, and Marc Hetherington, 2012, Look how far we've come apart, campaign stops, The New York Times, September 17. https://campaignstops.blogs.nytimes.com/2012/09/17/look-how-far-weve-come-apart.

Harford, Jarrad, 2005, What drives merger waves? *Journal of Financial Economics* 77, 529–560.

Himmelfarb, Samuel, and Carl Lickteig, 1982, Social desirability and the randomized response technique, *Journal of Personality and Social Psychology* 43, 710-717.

Hoberg, Gerard, and Gordon Phillips, 2010, Product Market Synergies and Competition in Mergers and Acquisitions: A Text-Based Analysis, *The Review of Financial Studies* 23, 3773-3811.

Hoberg, Gerard, and Gordon Phillips, 2016, Text-Based Network Industries and Endogenous Product Differentiation, *Journal of Political Economics* 124, 1423-1465.

Holburn, Guy, and Richard Vanden Bergh, 2014, Integrated market and nonmarket strategies: political campaign contributions around merger and acquisition events in the energy sector, *Strategic Management Journal* 35, 450–460.

Iyengar, Shanto, Yphtach Lelkes, Matthew Levendusky, Neil Malhotra, and Sean Westwood, 2019, The Origins and Consequences of Affective Polarization in the United States, *Annual Review of Political Science* 22, 129–146.

Iyengar, Shanto, Gaurav Sood, and Yphtach Lelkes, 2012, Affect, not ideology: a social identity perspective on polarization, *Public Opinion Quarterly* 76, 405–431.

Iyengar, Shanto, and Sean Westwood, 2015, Fear and loathing across party lines: new evidence on group polarization, *American Journal of Political Science* 59, 690-707.

Jensen, Michael, 1986, Agency costs of free cash flow, corporate finance, and takeovers, The *American Economic Review* 76, 323–329.

Jensen, Michael, 1993, The modern industrial revolution, exit, and the failure of internal control systems, *The Journal of Finance* 48, 831–880.

Jovanovic, Boyan, and Serguey Braguinsky, 2004, Bidder discounts and target premia in takeovers, *American Economic Review* 94, 46–56.

Kempf, Elisabeth, and Margarita Tsoutsoura, 2021, Partisan professionals: evidence from credit rating analysts, *The Journal of Finance* 76, 2805–2856.

Lambrecht, Bart M., and Stewart C. Myers, 2007, A Theory of Takeovers and Disinvestment, *The Journal of Finance* 62, 809–845.

Lang, Larry, Annette Poulsen, and René Stulz, 1995, Asset sales, firm performance, and the agency costs of managerial discretion, *Journal of Financial Economics* 37, 3–37.

Levendusky, Matthew, 2013, Why Do Partisan Media Polarize Viewers?, *American Journal of Political Science* 57, 611–623.

Li, Kai, Feng Mai, Rui Shen, and Xinyan Yan, 2020, Measuring corporate culture using machine learning, *The Review of Financial Studies* 34, 3265–3315.

Lott, John, and Kevin Hassett, 2014, Is newspaper coverage of economic events politically biased?, *Public Choice* 160, 65–108.

Makridis, Christos, 2022, The effect of economic sentiment on consumption: evidence from social networks, *Available at SSRN 3092489*.

Maksimovic, Vojislav, Gordon Phillips, and Liu Yang, 2013, Private and public merger waves, *Journal of Finance* 68, 2177–2217.

Martin, Gregory, and Ali Yurukoglu, 2017, Bias in Cable News: Persuasion and Polarization, *American Economic Review* 107, 2565–2599.

Matsusaka, John., 2001, Corporate diversification, value maximization, and organizational capabilities, *The Journal of Business* 74, 409–431.

McCarty, Nolan, Keith Poole, and Howard Rosenthal, 2006, Polarized America: The Dance of Ideology and Unequal Riches (MIT Press, Cambridge, MA).

McConnell, Christopher, Yotam Margalit, Neil Malhotra, and Matthew Levendusky, 2018, The economic consequences of partisanship in a polarized era, *American Journal of Political Science* 62, 5-18.

McGrath, Mary, 2017, Economic behavior and the partisan perceptual screen, *Quarterly Journal of Political Science* 11, 363-383.

McKenzie, Mark, 2012, The influence of partisanship, ideology, and the law on redistricting decisions in the federal courts, *Political Research Quarterly* 65, 799–813.

Meeuwis, Maarten, Jonathan Parker, Antoinette Schoar, and Duncan Simester, 2022, Belief disagreement and portfolio choice, *Journal of Finance* 77, 3191–3247.

Mian, Atif, Amir Sufi, and Nasim Khoshkhou, 2018, Partisan bias, economic expectations, and household spending, *The Review of Economics and Statistics*, 1–46.

Mian, Atif, Amir Sufi, and Francesco Trebbi, 2014, Resolving Debt Overhang: Political Constraints in the Aftermath of Financial Crises, *American Economic Journal: Macroeconomics* 6, 1–28.

Mitchell, Mark, and J. Harold Mulherin, 1996, The impact of industry socks on takeover and restructuring activity, *Journal of Financial Economics* 41, 193–229.

Posner, Richard, 2008, *How Judges Think* (Harvard University Press, Cambridge, MA).

Rhodes-Kropf, Matthew, and David Robinson, 2008, The market for mergers and the boundaries of the firm, *Journal of Finance* 63, 1170–1211.

Rhodes-Kropf, Matthew, and S. Viswanathan, 2004, Market valuation and merger waves, *Journal of Finance* 59, 2685–2718.

Roll, Roll, 1986, The hubris hypothesis of corporate takeovers, *The Journal of Business* 59, 197–216.

Shleifer, Andrei, and Robert Vishny, 2003, Stock market driven acquisitions, *Journal of Financial Economics* 70, 295–311.

Stanig, Piero, 2013, Political polarization in retrospective economic evaluations during recessions and recoveries, *Electoral Studies* 32, 729-745.

Stulz, René, 1990, Managerial discretion and optimal financing policies, *Journal of Financial Economics* 26, 3–27.

Voorheis, John, Nolan McCarty, and Boris Shor, 2015, Unequal Incomes, Ideology and Gridlock: How Rising Inequality Increases Political Polarization, *SSRN Electronic Journal 2649215*.

**Appendix A**

**Variable Definitions**

| Variable | Definition |
|---|---|
| *Political Affiliation Measures* | |
| Democratic Affiliation | The average of (1) the fraction of employees identified in LinkedIn registered as Democrats over the total number of employees registered as Democrats or Republicans and (2) the fraction of individual employees' political donations to Democrat committees over the total number of donations to both Democrat and Republican committees in the past two presidential election cycles. In cases where one measure is missing, we use the other. |
| Top Management Democratic Affiliation | The same construction as *Democratic Affiliation* using only the voter registrations and political donations of CEOs, CFOs, and directors. |
| Rank-and-File Democratic Affiliation | The same construction as *Democratic Affiliation* excluding the voter registrations and political donations of CEOs, CFOs, and directors. |
| *Pairwise Measures of Political Divergence* | |
| Political Divergence | The absolute value of the difference between the acquirer's and the target's *Democratic Affiliation*. |
| Top Management Political Divergence | The absolute value of the difference between the acquirer's and the target's *Top Management Democratic Affiliation*. |
| Rank-and-File Political Divergence | The absolute value of the difference between the acquirer's and the target's *Rank-and-File Democratic Affiliation*. |
| Target less Acquirer Democratic Affiliation | The *Democratic Affiliation* of the target minus the *Democratic Affiliation* of the acquirer. |
| %Top Decile | We sort deals by *Political Divergence* and calculate the percentage of deals each year that are top decile deals (over the entire sample period). |
| % Divergent State | The percentage of deals where deal participants are headquartered in politically divergent states. We sort states into terciles using the percent of popular votes cast for the Democratic candidate in the preceding presidential election. We define an interstate deal as a deal across divergent states if the participants are headquartered in states that are two terciles apart. |
| *National Affective Polarization Measures* | |
| Standardized Partisan Conflict Index | An annual, standardized version of the *Partisan Conflict Index* constructed by Azzimonti (2018). It is computed monthly and measures the frequency of newspaper articles reporting political disagreement about government policy scaled by the total number of news articles. We take the annual average and standardize it by subtracting the sample mean and dividing by the sample standard deviation to generate *Standardized Partisan Conflict Index*. |
| PCI High | An indicator variable equal to one if *Standardized Partisan Conflict Index* is positive, and zero otherwise. |

*Firm Financial Variables (winsorized at the 1st and 99th percentiles)*

| | |
|---|---|
| Book Assets | Total Assets ($AT$), in $millions in the year before announcement year *t*, inflation adjusted to 2019. In regressions, we use the natural log of book assets. |
| Book to Market | Book equity divided by market equity in the year before announcement year *t*. Book equity is Book Assets minus Book Liabilities ($AT - LT$). Market equity is the equity market capitalization defined as ($PRCC\_C * CSHO$). |
| Book Leverage | Book liabilities divided by book assets ($LT/AT$) in the year before announcement year *t*. |
| Return on Assets | Net income divided by book assets (*NI/AT*) in the year before announcement year *t*. |
| Sales Growth | Year over year percentage growth in sales ($SALE_{t-1}$ / $SALE_{t-2}$ - *1*) in the year before the announcement year *t*. |
| Cash Ratio | Cash and cash equivalents divided by book assets ($CHE/AT$) in the year before announcement year *t*. |
| Past Return | Stock return compounded over the [-14, -3] months window before announcement month 0. |

*Deal Variables*

| | |
|---|---|
| Announced Deal | An indicator variable equal to one for the firm pair that announces a deal and equal to zero for the other firm pairs. |
| HQ Distance | The distance, in hundreds of miles, between the zip-codes of the acquirer's and the target's headquarters, winsorized at the 1st and 99th percentiles. |
| Similar Products | An indicator variable equal to one if the acquirer and the target have similar products from Hoberg and Phillips (2010, 2016), and zero otherwise. |
| Diversifying | An indicator variable equal to one if the acquirer and the target are classified under different 2-digit Standard Industrial Classification (SIC) codes, and zero otherwise. |
| Deal Value | Deal value at announcement, in $billions, inflation-adjusted to 2019. |
| Hostile | An indicator variable equal to one for non- "Friendly" deals, and zero otherwise. |
| Integration | An indicator variable equal to one for mergers where the DEF14A form or the post-merger 10K/Q filings mention "integrate" or "integration" more frequently than the median deal. |
| Abnormal Announcement Return | The value weighted average of acquirers' and targets' CAPM excess returns over the [-1,1] days window around announcement date 0, winsorized at the 1st and 99th percentiles. |
| Withdrawn | An indicator variable equal to one if the deal is withdrawn, and zero otherwise |
| Future Spinoff | For completed deals, an indicator variable equal to one if the combined firm has a spinoff in the three years following deal completion, and zero otherwise. |

| | |
|---|---|
| Future Operating Cash Flows | For completed deals, the average of operating cash flow divided by book assets ((OIBDP + DP) / AT) in years [1,4] relative to the announcement year 0, winsorized at the 1st and 99th percentiles. |
| Future Sales Growth | For completed deals, the average of sales growth ($SALE_t$ / $SALE_{t-1}$ - *1*) in years [1,4] relative to the announcement year 0, winsorized at the 1st and 99th percentiles. |
| *Culture and ESG Variables* | |
| Innovation, Integrity, Quality, Respect, and Teamwork | The five measures of culture from Li, Mai, Shen, and Yan (2020). For each measure, we compute *Cultural Distance* as the absolute value of the difference between the acquirer's and the target's value of that measure. |
| Aggregate Cultural Distance | The sum of the cultural distances calculated using each of the five measures of culture from Li, Mai, Shen, and Yan (2020). |
| ESG Rating | The industry-adjusted reputational risk score from RepRisk. |
| ESG Distance | The absolute value of the difference between the acquirer's and the target's *ESG Rating*. |
| *Employee Retention Variables* | |
| One Year Separation | The fraction of acquirer and target employees as of the year before announcement who separate from the merging firms within one year of announcement. |
| Percent Democrat Separators | The number of separating employees (from the acquirer or the target) who are registered Democrats divided by the total number of separating employees registered as Democrats or Republicans. |
| Percent Democrat Employees | The number of pre-announcement employees (from the acquirer or the target) who are registered with the Democrat party divided by the number of pre-announcement employees who are registered with either the Democrat or the Republican party. |
| Disproportionate Democrat Separation | *Percent Democrat Separators* minus *Percent Democrat Employees*. |
| Opposed | An indicator variable equal to one if the employee's party registration differs from the majority political affiliation of the merger counterparty, and zero otherwise. |
| Other Indicator Variables | Democrat, Republican, Associates, Bachelors, Masters, Doctorate, Female, Asian, Black, Hispanic, and Native are all indicator variables flagging the employee's party registration, highest reported degree, gender, and race. |
| Tenure (Experience) | The number of months since the employee joined the company (first reported job). |
| Number of Connections | The number of connected profiles on LinkedIn. |

## Figure 1

## The Distribution of Political Divergence in Announced vs. Hypothetical Deals

This figure plots the percentage of announced vs. hypothetical mergers across different ranges of *Political Divergence*. The left bars in orange present the distribution of the 2,228 announced mergers in the sample. The right bars in gray plot a hypothetical distribution of deals based on all firm pairs. To create the hypothetical distribution of deals, we calculate the average *Democratic Affiliation* of each firm. We then create all possible firm pairs, and calculate their *Political Divergence*. *Democratic Affiliation* is defined as the average of (1) the fraction of employees identified in LinkedIn registered as Democrats over the total number of employees registered as Democrats or Republicans and (2) the fraction of individual employees' political donations to Democrat committees over the total number of donations to both Democrat and Republican committees in the past two presidential election cycles. In cases where one measure is missing, we use the other. *Political Divergence* is defined as the absolute value of the difference between acquirers' and targets' *Democratic Affiliation*. All variable definitions are given in Appendix A. The Internet Appendix provides additional details on the construction of the hypothetical distribution and the different measures of political affiliation.

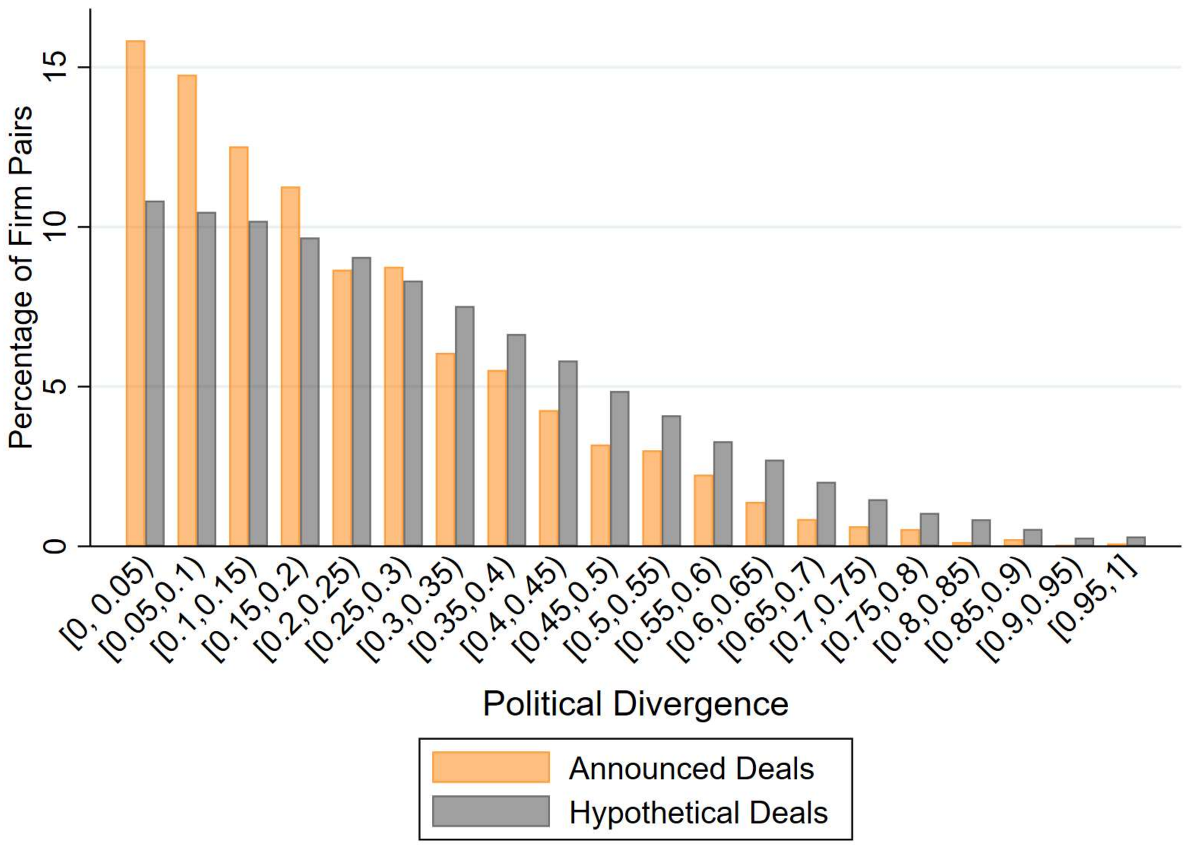
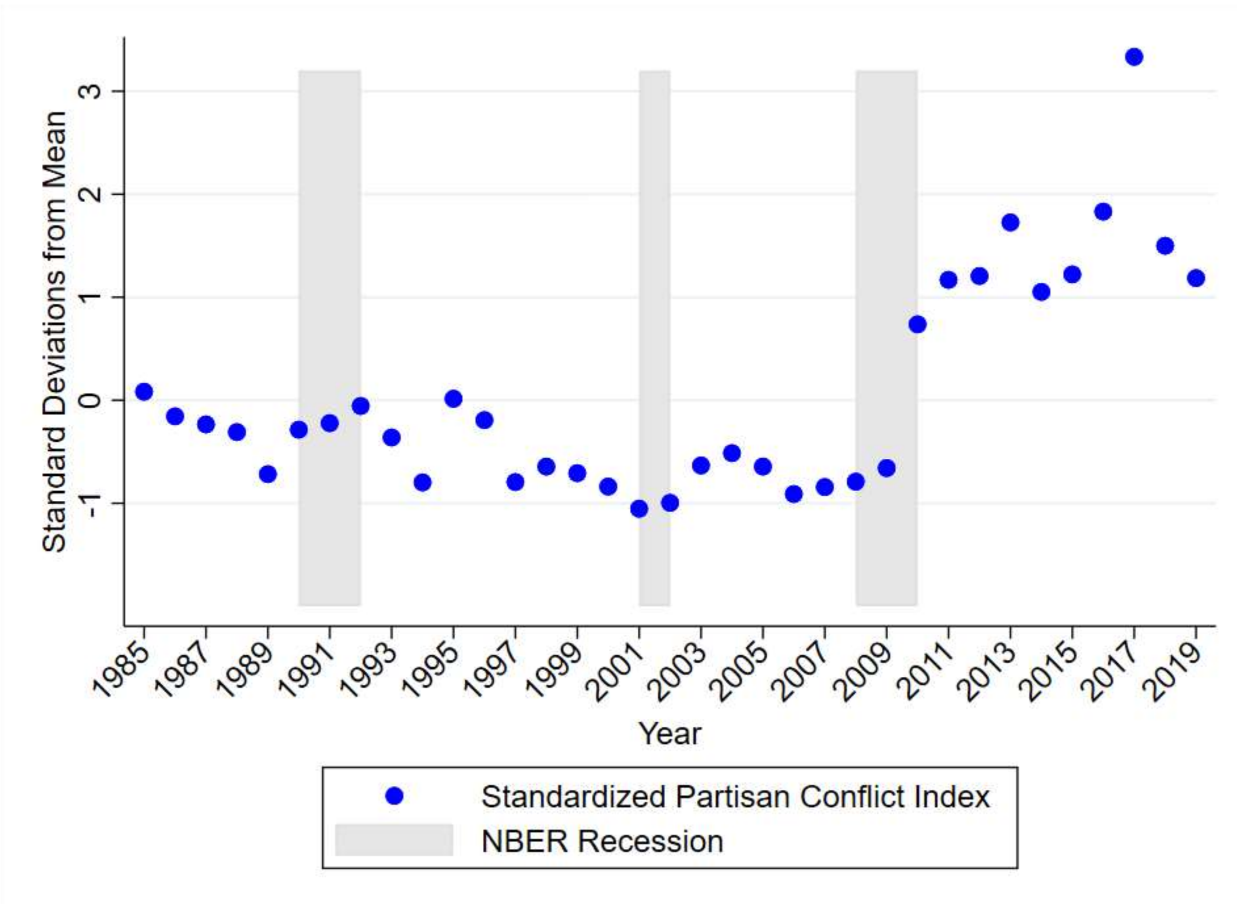


**Figure 2**

**Affective Polarization from 1985 – 2019**

This figure plots the evolution of political polarization from 1985 to 2019 using standardized annual averages of the Partisan Conflict Index from Azzimonti (2018), maintained by the Federal Reserve Bank of Philadelphia. We compute averages of the Partisan Conflict Index by calendar year and then subtract the time series mean and divide by the time series standard deviation to obtain the *Standardized Partisan Conflict Index*. Shaded areas are NBER recession periods. All variable definitions are given in Appendix A. We describe an alternative measure of affective polarization in the Internet Appendix.

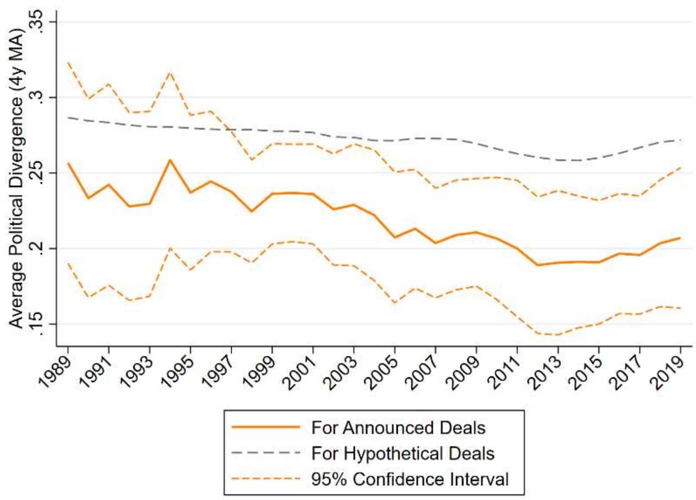


## Figure 3

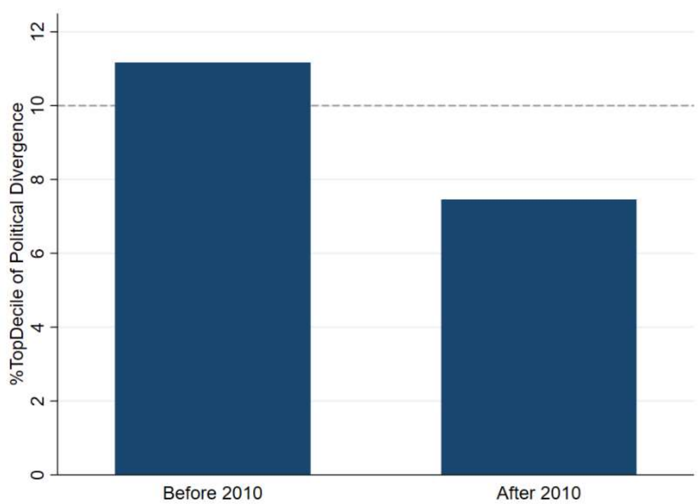
## The Dynamics of Political Divergence over Time

This figure describes the dynamics of *Political Divergence* over time. Panel A plots the four-year moving average of *Political Divergence* for all announced and hypothetical deals. Hypothetical deals comprise all possible firm pairs in each year. In Panel B, we sort announced deals on *Political Divergence* into deciles. We then calculate the percentage of deals that are in the top decile before 2010 and after 2010. *Political Divergence* is defined as the absolute value of the difference between acquirers' and targets' *Democratic Affiliation*. *Democratic Affiliation* is defined as the average of (1) the fraction of employees identified in LinkedIn registered as Democrats over the total number of employees registered as Democrats or Republicans and (2) the fraction of individual employees' political donations to Democrat committees over the total number of donations to both Democrat and Republican committees in the past two presidential election cycles. In cases where one measure is missing, we use the other. All variable definitions are given in Appendix A.

Panel A: Average Political Divergence



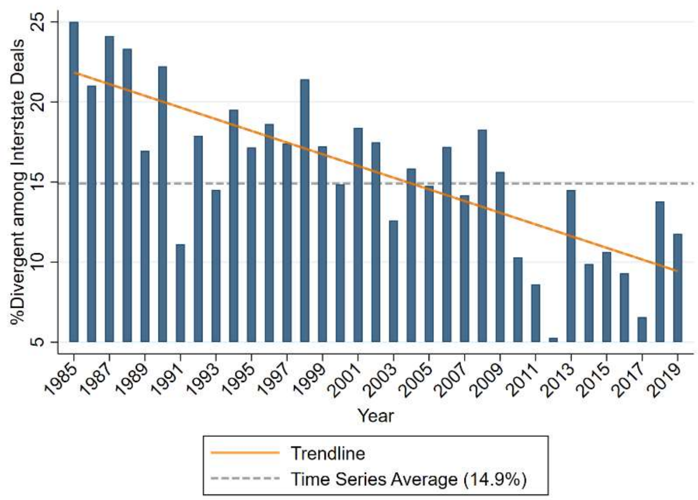

Panel B: %Top Decile before and after 2010



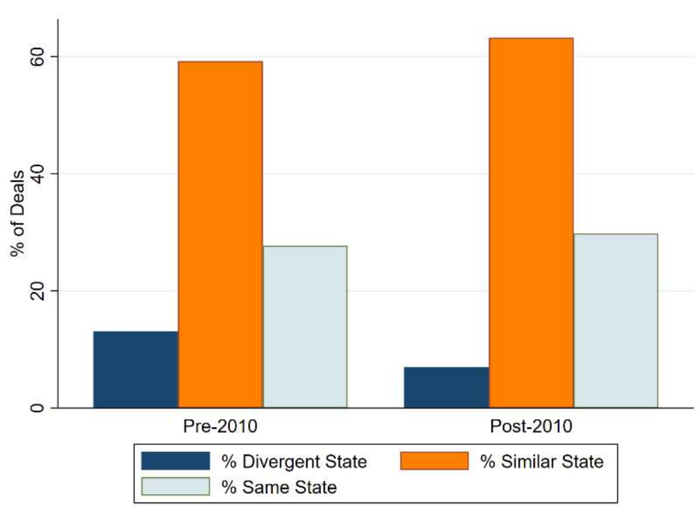
## Figure 4
## Mergers across Politically Divergent States

This figure describes the distribution of announced mergers across states. We sort states into terciles using the percent of popular votes cast for the Democratic candidate in the preceding presidential election. We then assign deal participants based on headquarter location. We define an interstate deal as a deal across divergent (similar) states if the participants are headquartered in states that are two (zero or one) terciles apart. Intrastate deals are defined as those between firms headquartered in the same state. All variable definitions are given in Appendix A.

Panel A: Percent of interstate deals across politically divergent states




Panel B: Distribution of deals before and after 2010



## Table 1

## Summary Statistics

This table presents acquirer-, target-, deal-, and employee-level descriptive statistics for announced deals. All variable definitions are given in Appendix A. The Internet Appendix provides additional summary statistics and details on the construction of the variables.

Panel A: Acquirers

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Variable | Mean | SD | 25th %ile | Median | 75th %ile | N |
| Democratic Affiliation | 0.48 | 0.2 | 0.34 | 0.48 | 0.63 | 2,228 |
| Book Assets ($bil) | 36.67 | 81.51 | 2 | 8.12 | 31.46 | 2,228 |
| Book to Market | 0.48 | 0.37 | 0.23 | 0.4 | 0.62 | 2,228 |
| Sales Growth | 0.15 | 0.34 | 0 | 0.07 | 0.2 | 2,174 |
| Book Leverage | 0.59 | 0.22 | 0.44 | 0.59 | 0.75 | 2,228 |
| Return on Assets | 0.04 | 0.09 | 0.01 | 0.04 | 0.08 | 2,227 |
| Cash Ratio | 0.15 | 0.17 | 0.03 | 0.08 | 0.21 | 2,228 |
| Past Return | 0.01 | 0.16 | -0.08 | 0 | 0.08 | 2,192 |
| Top Management Democratic Affiliation | 0.42 | 0.34 | 0.06 | 0.4 | 0.65 | 1,304 |
| Rank-and-File Democratic Affiliation | 0.49 | 0.21 | 0.34 | 0.48 | 0.63 | 2,228 |

Panel B: Targets

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Variable | Mean | SD | 25th %ile | Median | 75th %ile | N |
| Democratic Affiliation | 0.49 | 0.26 | 0.3 | 0.5 | 0.68 | 2,228 |
| Book Assets ($bil) | 7.4 | 21.68 | 0.27 | 1 | 4.2 | 2,228 |
| Book to Market | 0.59 | 0.52 | 0.27 | 0.48 | 0.79 | 2,228 |
| Sales Growth | 0.13 | 0.35 | -0.03 | 0.06 | 0.21 | 2,123 |
| Book Leverage | 0.56 | 0.25 | 0.35 | 0.58 | 0.76 | 2,228 |
| Return on Assets | -0.03 | 0.21 | -0.02 | 0.02 | 0.06 | 2,226 |
| Cash Ratio | 0.2 | 0.23 | 0.03 | 0.1 | 0.31 | 2,228 |
| Past Return | 0.03 | 0.22 | -0.1 | 0 | 0.12 | 2,159 |
| Top Management Democratic Affiliation | 0.41 | 0.41 | 0 | 0.33 | 0.85 | 796 |
| Rank-and-File Democratic Affiliation | 0.49 | 0.26 | 0.3 | 0.5 | 0.69 | 2,222 |

Panel C: Deals

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Variable | Mean | SD | 25th %ile | Median | 75th %ile | N |
| Political Divergence | 0.22 | 0.18 | 0.08 | 0.18 | 0.32 | 2,228 |
| HQ Distance (100s of miles) | 8.62 | 8.28 | 1.8 | 6.11 | 13.84 | 2,196 |
| Similar Products | 0.29 | 0.45 | 0 | 0 | 1 | 2,228 |
| Diversifying | 0.37 | 0.48 | 0 | 0 | 1 | 2,228 |
| Deal Value ($bil) | 4.62 | 13.01 | 0.26 | 0.97 | 3.47 | 2,228 |
| Hostile | 0.12 | 0.32 | 0 | 0 | 0 | 2,228 |
| Top Management Political Divergence | 0.38 | 0.29 | 0.13 | 0.35 | 0.57 | 653 |
| Rank-and-File Political Divergence | 0.22 | 0.18 | 0.08 | 0.18 | 0.33 | 2,222 |
| Aggregate Cultural Distance (standardized) | 0.00 | 1 | -0.67 | -0.23 | 0.40 | 582 |
| ESG Distance (standardized) | 0.00 | 1 | -0.87 | -0.20 | 0.48 | 276 |
| Integration | 0.50 | 0.50 | 0 | 0 | 1 | 1,175 |
| Abnormal Announcement Return | 0.02 | 0.07 | -0.01 | 0.01 | 0.05 | 2,116 |
| Negative Announcement Return | 0.37 | 0.48 | 0 | 0 | 1 | 2,116 |
| Withdrawn | 0.16 | 0.37 | 0 | 0 | 0 | 2,228 |
| One Year Separation | 0.24 | 0.15 | 0.15 | 0.21 | 0.3 | 1,396 |
| Disproportionate Democrat Separation | 0.03 | 0.12 | -0.01 | 0.03 | 0.07 | 1,389 |
| Target Less Acquirer Democratic Affiliation | 0.00 | 0.29 | -0.18 | 0.00 | 0.18 | 2,228 |
| Future Return on Assets | 0.02 | 0.09 | 0.01 | 0.03 | 0.06 | 1,896 |
| Future Operating Cash Flows | 0.11 | 0.07 | 0.07 | 0.11 | 0.16 | 1,836 |
| Future Sales Growth | 0.12 | 0.19 | 0.01 | 0.08 | 0.19 | 1,889 |
| Future Spinoff | 0.05 | 0.22 | 0 | 0 | 0 | 1,787 |

Panel D: Employees

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Variable | Mean | SD | 25th %ile | Median | 75th %ile | N |
| Employee Separation | 0.21 | 0.4 | 0 | 0 | 0 | 2,243,546 |
| Opposed | 0.47 | 0.5 | 0 | 0 | 1 | 2,243,546 |
| Democrat | 0.56 | 0.5 | 0 | 1 | 1 | 2,243,546 |
| Republican | 0.44 | 0.5 | 0 | 0 | 1 | 2,243,546 |
| Associates | 0.05 | 0.21 | 0 | 0 | 0 | 2,243,546 |
| Bachelors | 0.33 | 0.47 | 0 | 0 | 1 | 2,243,546 |
| Masters | 0.19 | 0.39 | 0 | 0 | 0 | 2,243,546 |
| Doctorate | 0.04 | 0.19 | 0 | 0 | 0 | 2,243,546 |
| Female | 0.46 | 0.5 | 0 | 0 | 1 | 2,243,546 |
| Asian | 0.06 | 0.24 | 0 | 0 | 0 | 2,243,001 |
| Black | 0.1 | 0.29 | 0 | 0 | 0 | 2,243,001 |
| Hispanic | 0.07 | 0.25 | 0 | 0 | 0 | 2,243,001 |
| Native | 0 | 0.02 | 0 | 0 | 0 | 2,243,001 |
| Tenure (months) | 81 | 89 | 18 | 48 | 112 | 2,243,546 |
| Experience (months) | 131 | 105 | 47 | 105 | 191 | 2,197,687 |
| Number of Connections | 244 | 1147 | 43 | 164 | 472 | 2,181,308 |

## Table 2

## The Frequency of Mergers by Political Divergence Over Time

This table shows the percentage of merger announcements across ranges of *Political Divergence* over election cycles. Each row corresponds to the four-year presidential election cycle ending that year. We present $\chi^2$ test statistics calculated using a hypothetical null distribution formed from all firm pairs in each election cycle and presented in the Internet Appendix. We evaluate *p*-values with 3 degrees of freedom. *Political Divergence* is defined as the absolute value of the difference between acquirers' and targets' *Democratic Affiliation*. *Democratic Affiliation* is defined as the average of (1) the fraction of employees identified in LinkedIn registered as Democrats over the total number of employees registered as Democrats or Republicans and (2) the fraction of individual employees' political donations to Democrat committees over the total number of donations to both Democrat and Republican committees in the past two presidential election cycles. In cases where one measure is missing, we use the other. All variable definitions are given in Appendix A.

| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|
| Cycle | Political Divergence | | | | | | |
| Ending | [0,0.25] | (0.25,0.50] | (0.5,0.75] | (0.75,1] | N | $\chi^2$ | *p*-value |
| 1988 | 55.1% | 32.4% | 9.6% | 2.9% | 136 | 3.85 | 0.28 |
| 1992 | 61.7% | 24.1% | 13.5% | 0.7% | 141 | 8.54 | 0.04 |
| 1996 | 59.2% | 30.5% | 8.8% | 1.5% | 262 | 9.04 | 0.03 |
| 2000 | 60.3% | 29.3% | 8.6% | 1.9% | 526 | 20.62 | 0.00 |
| 2004 | 61.7% | 28.0% | 10.3% | 0.0% | 261 | 12.11 | 0.01 |
| 2008 | 66.8% | 26.5% | 6.5% | 0.3% | 325 | 30.82 | 0.00 |
| 2012 | 71.2% | 21.5% | 6.8% | 0.5% | 205 | 23.72 | 0.00 |
| 2016 | 67.8% | 27.0% | 4.7% | 0.4% | 233 | 21.96 | 0.00 |
| 2020* | 64.0% | 29.5% | 5.8% | 0.7% | 139 | 10.14 | 0.02 |
| Overall | 63.06% | 27.78% | 8.12% | 1.03% | 2228 | 114.31 | 0.00 |

*The sample ends in 2019.

**Table 3**

**Average Political Divergence and Affective Polarization**

This table presents estimates from time series fractional logistic regressions explaining political divergence in announced deals. In column (1), the dependent variable is the average *Political Divergence* of announced deals in each year. In column (2), the dependent variable is the percentage of deals in each year that are in the top decile of *Political Divergence* over the entire sample, *%Top Decile*. In columns (3) and (4), the dependent variables are the percentages of mergers between firms headquartered in politically divergent states in each year. To calculate these variables, we sort states into terciles using the percent of popular votes cast for the Democratic candidate in the preceding presidential election. We then assign deal participants based on headquarter location. We define an interstate deal as a deal across divergent (similar) states if the participants are headquartered in states that are two (zero or one) terciles apart. Intrastate deals are defined as those between firms headquartered in the same state. In column (3), the denominator of *%Divergent State* is the count of all deals, and in column (4) the count excludes intrastate deals. All variable definitions are given in Appendix A. The Internet Appendix provides additional details on the construction of the variables. We report *z*-scores in parentheses. Significance: *p<10%, **p<5%, ***p<1%.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Variables | Average Political Divergence | %Top Decile | %Divergent State (all deals) | %Divergent State (interstate deals) |
| Standardized Partisan Conflict Index | -0.077*** | -0.186*** | -0.247*** | -0.252*** |
| | (-3.51) | (-2.83) | (-5.93) | (-5.97) |
| Constant | -1.258*** | -2.215*** | -2.077*** | -1.695*** |
| | (-43.66) | (-24.52) | (-36.43) | (-33.47) |
| Observations | 35 | 35 | 35 | 35 |
| Pseudo $R^2$ | 0.001 | 0.004 | 0.008 | 0.009 |

# Table 4
## Merger Partner Selection

This table presents estimates from conditional logistic regressions explaining merger partner selection. We follow Bena and Li (2014), and match each deal participant with up to five pseudo-partners in same industry as the actual partner. We select pseudo-partners of similar size and book-to-market as the actual partner. The dependent variable is *Announced Deal*, an indicator variable that is equal to one for the announced deal and zero for all the pseudo-deals. The variable *Political Divergence* is the absolute value of the difference between the acquirer's and target's *Democratic Affiliation.* In Panel A, we use voter registration and individual donation data to measure *Democratic Affiliation*. In Panel B, we use only voter registration data, and in Panel C, we use only individual donation data. In all panels, column (1) only controls for political affiliation measures. Column (2) adds other control variables and industry pair by year fixed effects. Column (3) adds deal fixed effects. Column (4) excludes hostile bids. All variables are defined in Appendix A. Additional details and alternative methods are described in the Internet Appendix. We report *z*-scores in parentheses. Significance: *p<10%, **p<5%, ***p<1%.

Panel A: Overall Sample

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Political Divergence | -0.812*** | -0.401*** | -0.432*** | -0.466*** |
| | (-6.40) | (-2.69) | (-2.94) | (-2.98) |
| Acquirer Democratic Affiliation | 0.039 | -0.055 | -0.074 | -0.078 |
| | (0.37) | (-0.58) | (-0.65) | (-0.64) |
| Target Democratic Affiliation | 0.091 | 0.163* | 0.222** | 0.239** |
| | (0.96) | (1.67) | (2.10) | (2.12) |
| HQ Distance | | -0.039*** | -0.040*** | -0.037*** |
| | | (-9.88) | (-11.06) | (-9.74) |
| Similar Products | | 0.528*** | 0.588*** | 0.438*** |
| | | (5.83) | (7.52) | (5.29) |
| ln(Acquirer Book Assets) | | 0.349*** | 0.589*** | 0.603*** |
| | | (29.71) | (31.90) | (30.43) |
| ln(Target Book Assets) | | 0.131*** | 0.227*** | 0.206*** |
| | | (8.73) | (14.46) | (12.55) |
| Acquirer Book to Market | | 0.067 | 0.247 | 0.306 |
| | | (1.00) | (1.25) | (1.33) |
| Target Book to Market | | 0.345*** | 0.732*** | 0.650*** |
| | | (6.86) | (5.79) | (4.74) |
| Acquirer Book Leverage | | 0.012 | -0.063 | -0.156 |
| | | (0.06) | (-0.27) | (-0.64) |
| Target Book Leverage | | -0.418*** | -0.628*** | -0.558*** |
| | | (-3.73) | (-4.79) | (-4.14) |
| Acquirer Return on Assets | | -0.212* | -0.321* | -0.462*** |
| | | (-1.68) | (-1.95) | (-2.60) |
| Target Return on Assets | | 0.286** | 0.455*** | 0.468*** |
| | | (2.28) | (3.13) | (3.05) |
| Acquirer Sales Growth | | 0.221*** | 0.286*** | 0.329*** |
| | | (4.21) | (4.47) | (4.71) |
| Target Sales Growth | | 0.062 | 0.111* | 0.105* |
| | | (1.21) | (1.90) | (1.76) |
| Acquirer Cash Ratio | | 0.270* | 0.574*** | 0.434** |
| | | (1.77) | (3.29) | (2.30) |
| Target Cash Ratio | | 0.515*** | 0.753*** | 0.861*** |
| | | (4.05) | (4.98) | (5.53) |
| Acquirer Past Return | | 0.049 | 0.121 | 0.045 |
| | | (0.49) | (0.74) | (0.26) |
| Target Past Return | | 0.170* | 0.157 | 0.131 |
| | | (1.77) | (1.26) | (1.01) |
| Diversifying | | 0.112*** | | |
| | | (2.64) | | |
| Industry Pair × Year FEs? | No | Yes | Yes | Yes |
| Deal FEs? | No | No | Yes | Yes |
| Excludes Hostile Bids? | No | No | No | Yes |
| Observations | 23,890 | 22,161 | 21,429 | 18,903 |
| Pseudo $R^2$ | 0.003 | 0.090 | 0.147 | 0.141 |

Panel B: Voter Registration Sample

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Political Divergence | -1.409*** | -0.904*** | -0.905*** | -0.985*** |
| | (-7.18) | (-4.24) | (-3.92) | (-4.00) |
| Acquirer Democratic Affiliation | 0.028 | 0.051 | -0.100 | 0.068 |
| | (0.16) | (0.30) | (-0.52) | (0.33) |
| Target Democratic Affiliation | 0.117 | 0.161 | 0.304* | 0.342* |
| | (0.80) | (0.93) | (1.73) | (1.83) |
| HQ Distance | | -0.033*** | -0.033*** | -0.030*** |
| | | (-8.27) | (-8.31) | (-7.07) |
| Similar Products | | 0.801*** | 0.862*** | 0.695*** |
| | | (8.42) | (9.28) | (6.97) |
| Additional Control Variables? | No | Yes | Yes | Yes |
| Industry Pair × Year FEs? | No | Yes | Yes | Yes |
| Deal FEs? | No | No | Yes | Yes |
| Excludes Hostile Bids? | No | No | No | Yes |
| Observations | 16,855 | 15,538 | 14,950 | 13,124 |
| Pseudo $R^2$ | 0.006 | 0.097 | 0.154 | 0.147 |

Panel C: Individual Donation Sample

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Political Divergence | -0.611*** | -0.334*** | -0.325*** | -0.396*** |
| | (-6.84) | (-3.52) | (-3.16) | (-3.56) |
| Acquirer Democratic Affiliation | 0.100 | -0.018 | -0.054 | -0.058 |
| | (1.34) | (-0.25) | (-0.70) | (-0.70) |
| Target Democratic Affiliation | 0.101 | 0.168*** | 0.156** | 0.175** |
| | (1.46) | (2.65) | (2.29) | (2.42) |
| HQ Distance | | -0.044*** | -0.046*** | -0.043*** |
| | | (-10.04) | (-10.90) | (-9.89) |
| Similar Products | | 0.494*** | 0.546*** | 0.393*** |
| | | (5.02) | (6.61) | (4.52) |
| Additional Control Variables? | No | Yes | Yes | Yes |
| Industry Pair × Year FEs? | No | Yes | Yes | Yes |
| Deal FEs? | No | No | Yes | Yes |
| Excludes Hostile Bids? | No | No | No | Yes |
| Observations | 20,796 | 19,568 | 19,042 | 16,790 |
| Pseudo $R^2$ | 0.004 | 0.090 | 0.139 | 0.133 |

## Table 5

## Merger Partner Selection and Affective Polarization

This table provides estimates from conditional logistic regressions explaining merger partner selection during periods of high vs. low levels of affective polarization. We follow Bena and Li (2014), and match each deal participant with up to five pseudo-partners in same industry as the actual partner. We select pseudo-partners of similar size and book-to-market as the actual partner. The dependent variable is *Announced Deal*, an indicator variable that is equal to one for the announced deal and zero for all the pseudo-deals. The variable *Political Divergence* is the absolute value of the difference between the acquirer's and target's *Democratic Affiliation. High PCI* is an indicator variable equal to one if *Standardized Partisan Conflict Index* is greater than zero, and zero otherwise. Columns (1) and (2) report estimates for periods where affective polarization is low and high, respectively. In column (3), we estimate the regression using the full sample, and add interactions of *High PCI* with the independent variables. Additional control variables are the same as in Table 4. All variable definitions are given in Appendix A. The Internet Appendix provides additional details on the construction of the variables and on alternative methods. We report *z*-scores in parentheses. Significance: *p<10%, **p<5%, ***p<1%.

| Variables | (1) High PCI = 0 | (2) High PCI = 1 | (3) Pooled sample |
|---|---|---|---|
| Political Divergence | -0.263 | -1.019*** | -0.263 |
| | (-1.57) | (-3.24) | (-1.57) |
| Political Divergence × High PCI | | | -0.757** |
| | | | (-2.12) |
| Acquirer Democratic Affiliation | -0.018 | -0.281 | -0.018 |
| | (-0.14) | (-1.25) | (-0.14) |
| Acquirer Democratic Affiliation × High PCI | | | -0.263 |
| | | | (-1.01) |
| Target Democratic Affiliation | 0.188 | 0.323 | 0.188 |
| | (1.52) | (1.53) | (1.52) |
| Target Democratic Affiliation × High PCI | | | 0.135 |
| | | | (0.55) |
| HQ Distance | -0.041*** | -0.037*** | -0.041*** |
| | (-9.52) | (-5.57) | (-9.52) |
| HQ Distance × High PCI | | | 0.003 |
| | | | (0.40) |
| Similar Products | 0.621*** | 0.510*** | 0.621*** |
| | (6.76) | (3.38) | (6.76) |
| Similar Products × High PCI | | | -0.111 |
| | | | (-0.63) |
| Additional Control Variables? | Yes | Yes | Yes |
| Industry Pair × Year FEs? | Yes | Yes | Yes |
| Deal FEs? | Yes | Yes | Yes |
| Observations | 15,601 | 5,828 | 21,429 |
| Pseudo $R^2$ | 0.163 | 0.118 | 0.150 |

**Table 6**

**Corporate Culture and Environmental, Social, and Governance (ESG) Policies**

This table provides estimates from conditional logistic regressions explaining merger partner selection, augmented with nonpolitical measures of corporate culture and ESG practices. We form the sample of deals and pseudo-deals as described in Table 4, but restrict the sample to firms with available measures of corporate culture from Li, Mai, Shen, and Yan (2020) in Panel A and available ESG ratings in Panel B. The dependent variable is *Announced Deal*, an indicator variable that is equal to one for the announced deal and zero for all the pseudo-deals. The variable *Political Divergence* is the absolute value of the difference between the acquirer's and target's *Democratic Affiliation. Aggregate Cultural Distance* is the sum of the cultural distances calculated using each of the five measures of culture from Li, Mai, Shen, and Yan (2020), which include Innovation, Integrity, Quality, Respect, and Teamwork. For each of these measures, we compute the corresponding cultural distance as the absolute value of the difference between the acquirer's and the target's value of that measure. *ESG Distance* is the absolute value of the difference between the acquirer's and the target's ESG Rating, defined as the industry-adjusted reputational risk score from RepRisk. Additional control variables are the same as those in Table 4. All variable definitions are given in Appendix A. The Internet Appendix provides robustness tests that consider each corporate culture measure separately. We report *z*-scores in parentheses. Significance: *p<10%, **p<5%, ***p<1%.

Panel A: Corporate Culture

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| Political Divergence | -0.137** | | -0.136** |
| | (-2.54) | | (-2.49) |
| Aggregate Cultural Distance | | -0.217*** | -0.212** |
| | | (-2.62) | (-2.54) |
| Acquirer Democratic Affiliation | -0.063 | | -0.071 |
| | (-0.25) | | (-0.27) |
| Target Democratic Affiliation | 0.128 | | 0.096 |
| | (0.57) | | (0.42) |
| HQ Distance | -0.042*** | -0.043*** | -0.042*** |
| | (-6.35) | (-6.48) | (-6.30) |
| Similar Products | 1.022*** | 1.001*** | 0.989*** |
| | (7.03) | (6.86) | (6.77) |
| Additional Control Variables? | Yes | Yes | Yes |
| Industry Pair × Year FEs? | Yes | Yes | Yes |
| Deal FEs? | Yes | Yes | Yes |
| Observations | 5,919 | 5,919 | 5,919 |
| Pseudo $R^2$ | 0.152 | 0.157 | 0.159 |

Panel B: ESG

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| Political Divergence | -0.206*** | | -0.218*** |
| | (-2.68) | | (-2.75) |
| ESG Distance | | -0.528*** | -0.531*** |
| | | (-4.13) | (-4.14) |
| Acquirer Democratic Affiliation | 0.090 | | 0.136 |
| | (0.24) | | (0.36) |
| Target Democratic Affiliation | 0.388 | | 0.381 |
| | (1.13) | | (1.09) |
| Acquirer ESG Rating | | -0.199** | -0.183* |
| | | (-2.08) | (-1.88) |
| Target ESG Rating | | 0.188** | 0.186** |
| | | (2.05) | (2.02) |
| HQ Distance | -0.039*** | -0.039*** | -0.038*** |
| | (-3.76) | (-3.73) | (-3.59) |
| Similar Products | 0.531** | 0.569*** | 0.521** |
| | (2.44) | (2.58) | (2.37) |
| Additional Control Variables? | Yes | Yes | Yes |
| Industry Pair × Year FEs? | Yes | Yes | Yes |
| Deal FEs? | Yes | Yes | Yes |
| Observations | 2,828 | 2,828 | 2,828 |
| Pseudo $R^2$ | 0.108 | 0.117 | 0.123 |

## Table 7
## Integration

This table provides estimates from conditional logistic regressions explaining merger partner selection, which consider the intent to integrate business operations. We follow Bena and Li (2014), and match each deal participant with up to five pseudo-partners in same industry as the actual partner. We select pseudo-partners of similar size and book-to-market as the actual partner. The dependent variable is *Announced Deal*, an indicator variable that is equal to one for the announced deal and zero for all the pseudo-deals. The variable *Political Divergence* is the absolute value of the difference between the acquirer's and target's *Democratic Affiliation.* The indicator variable *Integration* equals one for mergers where the DEF14A form or the post-merger 10K/Q filings mention the words "integrate" or "integration" more frequently than the median deal, and zero otherwise. Columns (1) and (2) report estimates for subsamples where *Integration* equals zero and one, respectively. In column (3), we estimate the regressions using the full sample, adding interactions of the indicator *Integration* with the independent variables. Additional control variables are the same as those in Table 4. All variable definitions are given in Appendix A. We report *z*-scores in parentheses. Significance: *p<10%, **p<5%, ***p<1%.

| | (1) | (2) | (3) |
|---|---|---|---|
| Variables | Integration = 0 | Integration = 1 | Pooled sample |
| Political Divergence | 0.195 | -0.622* | 0.195 |
| | (0.71) | (-1.84) | (0.71) |
| Political Divergence × Integration | | | -0.817* |
| | | | (-1.88) |
| Acquirer Democratic Affiliation | -0.389* | -0.263 | -0.389* |
| | (-1.68) | (-1.00) | (-1.68) |
| Acquirer Democratic Affiliation × Integration | | | 0.126 |
| | | | (0.36) |
| Target Democratic Affiliation | -0.124 | -0.058 | -0.124 |
| | (-0.62) | (-0.25) | (-0.62) |
| Target Democratic Affiliation × Integration | | | 0.066 |
| | | | (0.22) |
| HQ Distance | -0.046*** | -0.030*** | -0.046*** |
| | (-5.76) | (-4.64) | (-5.76) |
| HQ Distance × Integration | | | 0.016 |
| | | | (1.52) |
| Similar Products | 0.427*** | 0.672*** | 0.427*** |
| | (2.97) | (4.59) | (2.97) |
| Similar Products × Integration | | | 0.246 |
| | | | (1.20) |
| Additional Control Variables? | Yes | Yes | Yes |
| Industry Pair × Year FEs? | Yes | Yes | Yes |
| Deal FEs? | Yes | Yes | Yes |
| Observations | 5,819 | 5,923 | 11,742 |
| Pseudo $R^2$ | 0.161 | 0.186 | 0.174 |

**Table 8**

**Executives or Rank-and-File Employees**

This table provides estimates from conditional logistic regressions explaining merger partner selection. It separately considers the role of political divergence between top management teams and rank-and-file employees. The key independent variables are *Top Management Political Divergence,* defined as the absolute value of the difference between the acquirer's and the target's *Top Management Democratic Affiliation*, and *Rank-and-File Political Divergence,* defined analogously with respect to *Rank-and-File Democratic Affiliation*. *Top Management Democratic Affiliation* only uses the voter registrations and political donations of CEOs, CFOs, and directors. Conversely, *Rank-and-File Democratic Affiliation* excludes the voter registrations and political donations of CEOs, CFOs, and directors. Panel A provides baseline estimates. Panel B investigates the role of the intent to integrate business operations. Additional control variables are the same as those in Table 4. All variable definitions are given in Appendix A. We report *z*-scores in parentheses. Significance: *p<10%, **p<5%, ***p<1%.

Panel A: Top Management vs Rank-and-File Employees

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| Top Management Political Divergence | -0.289* | | -0.256 |
| | (-1.84) | | (-1.63) |
| Rank-and-File Political Divergence | | -0.690** | -0.642** |
| | | (-2.27) | (-2.11) |
| Acquirer Top Management Democratic Affiliation | -0.082 | | -0.103 |
| | (-0.66) | | (-0.78) |
| Target Top Management Democratic Affiliation | 0.033 | | 0.006 |
| | (0.30) | | (0.05) |
| Acquirer Rank-and-File Democratic Affiliation | | -0.033 | 0.056 |
| | | (-0.14) | (0.22) |
| Target Rank-and-File Democratic Affiliation | | 0.096 | 0.108 |
| | | (0.44) | (0.47) |
| HQ Distance | -0.038*** | -0.037*** | -0.037*** |
| | (-5.67) | (-5.52) | (-5.54) |
| Similar Products | 0.724*** | 0.723*** | 0.716*** |
| | (5.24) | (5.23) | (5.17) |
| Additional Control Variables? | Yes | Yes | Yes |
| Industry Pair × Year FEs? | Yes | Yes | Yes |
| Deal FEs? | Yes | Yes | Yes |
| Observations | 6,328 | 6,328 | 6,328 |
| Pseudo $R^2$ | 0.136 | 0.137 | 0.138 |

Panel B: Integration

| | Top Management Only | Rank-and-File Only |
|---|---|---|
| Variables | (1) | (2) |
| Top Management Political Divergence | -0.789*** | |
| | (-3.06) | |
| Top Management Political Divergence × Integration | 0.644* | |
| | (1.78) | |
| Rank-and-File Political Divergence | | 0.264 |
| | | (0.97) |
| Rank-and-File Political Divergence × Integration | | -0.745* |
| | | (-1.74) |
| Acquirer Democratic Affiliation | -0.422** | -0.385* |
| | (-2.03) | (-1.70) |
| Acquirer Democratic Affiliation × Integration | 0.329 | 0.252 |
| | (1.14) | (0.73) |
| Target Democratic Affiliation | 0.091 | -0.104 |
| | (0.50) | (-0.52) |
| Target Democratic Affiliation × Integration | 0.018 | -0.001 |
| | (0.07) | (-0.00) |
| HQ Distance | -0.050*** | -0.046*** |
| | (-4.23) | (-5.75) |
| HQ Distance × Integration | 0.016 | 0.016 |
| | (1.06) | (1.59) |
| Similar Products | 0.299 | 0.438*** |
| | (1.33) | (3.06) |
| Similar Products × Integration | 0.653** | 0.261 |
| | (2.14) | (1.28) |
| Additional Control Variables? | Yes | Yes |
| Industry Pair × Year FEs? | Yes | Yes |
| Deal FEs? | Yes | Yes |
| Observations | 5,171 | 11,691 |
| Pseudo $R^2$ | 0.159 | 0.174 |

## Table 9

## Employee Retention

This table presents estimates from linear regressions explaining employee separation after merger announcement. Panel A provides firm-level tests. In columns (1) and (2), we regress *One Year Separation*, the fraction of pre-announcement employees who separate from the firm within one year, on *Political Divergence*. In columns (3) and (4), the dependent variable is *Disproportionate Democrat Separation*, the percentage of separating employees who are registered as Democrats relative to the overall percentage of employees who are registered as Democrats. The independent variable *Target less Acquirer Democratic Affiliation* captures the political affiliation of the acquirer relative to the target. *Abnormal Announcement Return* is the value weighted average of the acquirer's and the target's CAPM excess return over the [-1,1] days window around the merger announcement date 0, winsorized at the 1st and 99th percentiles. We use indicator variables to control for deal attitudes and for consideration structures. Additional control variables are the same as in Table 4. Panel B provides employee-level tests. It provides estimates from linear regressions using the sample of pre-announcement employees registered as Democrats or as Republicans. The dependent variable is *Employee Separation*, an indicator variable equal to one if the employee separates within one year of the merger announcement. The independent variable *Opposed* is an indicator equal to one if the employee's party registration differs from the majority political affiliation of the merger counterparty. All variable definitions are given in Appendix A. We report *t*-statistics in parentheses. Significance: *p<10%, **p<5%, ***p<1%.

Panel A: Firm-Level Tests

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Variable | One Year Separation | | Disproportionate Democrat Separation | |
| Political Divergence | 0.069** | 0.065** | | |
| | (2.48) | (2.32) | | |
| Target less Acquirer Democratic Affiliation | | | 0.034** | 0.030* |
| | | | (2.08) | (1.78) |
| Acquirer Democratic Affiliation | 0.025 | -0.016 | | |
| | (0.75) | (-0.46) | | |
| Target Democratic Affiliation | 0.003 | -0.010 | | |
| | (0.16) | (-0.57) | | |
| HQ Distance | 0.000 | 0.000 | -0.001** | -0.001** |
| | (0.53) | (0.77) | (-2.12) | (-1.98) |
| Similar Products | -0.024*** | -0.024** | -0.001 | 0.003 |
| | (-2.60) | (-2.50) | (-0.07) | (0.30) |
| Abnormal Announcement Return | 0.017 | -0.051 | -0.043 | -0.067 |
| | (0.25) | (-0.66) | (-0.70) | (-1.00) |
| Additional Control Variables? | No | Yes | No | Yes |
| Consideration Structure Dummies? | Yes | Yes | Yes | Yes |
| Deal Attitude Dummies? | Yes | Yes | Yes | Yes |
| Industry Pair FEs? | Yes | Yes | Yes | Yes |
| Year FEs? | Yes | Yes | Yes | Yes |
| Observations | 1,722 | 1,596 | 1,685 | 1,562 |
| Adjusted $R^2$ | 0.190 | 0.229 | 0.039 | 0.037 |

Panel B: Employee-Level Tests

| | (1) | (2) |
|---|---|---|
| Variable | Employee Separation | |
| Opposed × Political Divergence | 0.062*** | 0.039*** |
| | (5.83) | (4.87) |
| Opposed | -0.016*** | -0.010*** |
| | (-6.25) | (-5.50) |
| Associates | | 0.033*** |
| | | (17.20) |
| Bachelors | | 0.053*** |
| | | (35.45) |
| Masters | | 0.066*** |
| | | (37.92) |
| Doctorate | | 0.052*** |
| | | (20.96) |
| Female | | 0.006*** |
| | | (5.40) |
| Asian | | 0.020*** |
| | | (9.49) |
| Black | | 0.004*** |
| | | (3.08) |
| Hispanic | | 0.008*** |
| | | (5.39) |
| Native | | 0.024 |
| | | (1.58) |
| ln(Tenure) | | -0.046*** |
| | | (-33.71) |
| ln(Experience) | | 0.007*** |
| | | (6.70) |
| ln(Number of Connections) | | 0.023*** |
| | | (40.87) |
| Industry Pair × Year FEs? | Yes | Yes |
| Deal FEs? | Yes | Yes |
| Observations | 2,243,546 | 2,135,597 |
| Adjusted $R^2$ | 0.043 | 0.084 |

**Table 10**

**Announcement Returns and Deal Withdrawals**

This table presents estimates from cross sectional regressions for the sample of announced deals. In column (1), we provide linear regression estimates explaining *Abnormal Announcement Return*, defined as the value weighted average of the acquirer's and the target's CAPM excess returns over the [-1,1] days window around the merger announcement date 0, winsorized at the 1st and 99th percentiles. In columns (2) and (3), we estimate logistic regressions predicting *Negative Announcement Return* and *Withdrawn*, respectively. The variable *Negative Announcement Return* is an indicator variable equal to one if the *Abnormal Announcement Return* is less than zero, and zero otherwise. The variable *Withdrawn* is an indicator variable equal to one if the deal is eventually withdrawn, and zero otherwise. We use indicator variables to control for deal attitudes and for consideration structures. Additional control variables are the same as those in Table 4. All variable definitions are given in Appendix A. We report *t*-statistics in parentheses in column (1), and *z*-scores in parentheses in columns (2) and (3). Significance: *p<10%, **p<5%, ***p<1%.

| | (1) | (2) | (3) |
|---|---|---|---|
| Variables | Abnormal Announcement Return | Negative Announcement Return | Withdrawn |
| Political Divergence | -0.015 | -0.003 | 0.298 |
| | (-1.61) | (-0.01) | (0.56) |
| Acquirer Democratic Affiliation | -0.003 | -0.312 | 0.349 |
| | (-0.31) | (-0.88) | (0.66) |
| Target Democratic Affiliation | 0.003 | -0.157 | 0.166 |
| | (0.49) | (-0.58) | (0.39) |
| HQ Distance | -0.000 | 0.019** | 0.002 |
| | (-1.39) | (2.44) | (0.14) |
| Similar Products | -0.005 | 0.333** | 1.327*** |
| | (-1.27) | (2.35) | (5.96) |
| Additional Control Variables? | Yes | Yes | Yes |
| Consideration Structure Dummies? | Yes | Yes | Yes |
| Deal Attitude Dummies? | Yes | Yes | Yes |
| Industry Pair FEs? | Yes | Yes | Yes |
| Year FEs? | Yes | Yes | Yes |
| Observations | 1,717 | 1,539 | 1,533 |
| Adjusted or Pseudo $R^2$ | 0.177 | 0.138 | 0.434 |

## Table 11

## Outcomes Following Merger Completion

This table presents estimates from cross sectional regressions explaining outcomes following deal completion. Columns (1)-(3) provide estimates from linear regressions explaining the three-year average of: (1) return on assets, (2) operating cash flows scaled by assets, and (3) annual sales growth. Column (4) provides estimates from a logistic regression explaining *Future Spinoff,* an indicator variable equal to one if the combined firm has a spinoff within the three years after merger completion. We use indicator variables to control for deal attitudes and for consideration structures. Additional control variables are the same as those in Table 4. All variable definitions are given in Appendix A. We report *z*-scores in parentheses. Significance: *p<10%, **p<5%, ***p<1%.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Variables | Future ROA | Future Operating Cash Flows | Future Sales Growth | Future Spinoff |
| Political Divergence | -0.009 | 0.008 | 0.002 | 0.372 |
| | (-0.58) | (0.83) | (0.05) | (0.38) |
| Acquirer Democratic Affiliation | -0.036** | -0.035*** | 0.038 | -2.644** |
| | (-2.32) | (-3.61) | (1.14) | (-2.05) |
| Target Democratic Affiliation | 0.008 | -0.009 | -0.009 | 1.271 |
| | (0.68) | (-1.19) | (-0.34) | (1.54) |
| HQ Distance | -0.000 | -0.000 | 0.001 | 0.008 |
| | (-0.21) | (-0.64) | (1.17) | (0.38) |
| Similar Products | 0.003 | 0.001 | 0.030** | -0.731 |
| | (0.45) | (0.22) | (2.23) | (-1.38) |
| Abnormal Announcement Return | -0.013 | 0.002 | 0.055 | -2.702 |
| | (-0.22) | (0.07) | (0.56) | (-0.71) |
| Additional Control Variables? | Yes | Yes | Yes | Yes |
| Consideration Structure Dummies? | Yes | Yes | Yes | Yes |
| Deal Attitude Dummies? | Yes | Yes | Yes | Yes |
| Industry Pair FEs? | Yes | Yes | Yes | Yes |
| Year FEs? | Yes | Yes | Yes | Yes |
| Observations | 1,127 | 1,092 | 1,124 | 645 |
| Adjusted or Pseudo $R^2$ | 0.355 | 0.586 | 0.245 | 0.339 |

# Internet Appendix
# The Economic Effects of Political Polarization: Evidence from the Real Asset Market

## IA1 Voter Registration Data and Matching Procedure

### *IA1.1 LinkedIn Data*

We obtain individual-level LinkedIn data from Revelio Labs,[17] a company that specializes in workforce intelligence. The data cover 768,943,741 users across 1,317,379,624 positions, globally. The data include names, geography, employment histories, education histories, and the number of LinkedIn connections for all LinkedIn users. Additionally, the data include the gender, race, and ethnicity of users predicted from the user's full name. We retain records where the employment position is located in a U.S. state. The resulting dataset covers 78,327,732 users to match with voter registration data.

### *IA1.2 Voter Registration Data*

We file disclosure requests for statewide voter registration data with each state's Department of State to obtain voter registration records. We file those requests in the year 2020 and obtain voter registration data for 30 states and the District of Columbia, comprising 68.5% of the overall 2020 U.S. population. We use voter registration records from 26 states: Alaska, Arkansas, California, Colorado, Connecticut, Delaware, Florida, Georgia, Idaho, Illinois, Kansas, Massachusetts, Maryland, North Carolina, Nebraska, New Jersey, Nevada, New York, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, Texas, Utah, and Wyoming. We obtain records from Michigan, Missouri, Minnesota, Vermont, and Washington, but these states do not provide information on primary election party affiliation.[18]

[17] https://www.data-dictionary.reveliolabs.com/index.html
[18] Minnesota Statutes 2021.091 states that data requester must be a resident of the state, and data "may only be used for purposes related to elections, political activities, or law enforcement."

We use each individual's most recently available (as of 2020) primary election party registration to classify them as Democrat or Republican.[19] Among 100 million registrants, we classify 58% as Democrats and 42% as Republicans. We use individuals' most recent registration because only 11 states provide historical data covering more than two years. The historical data represent less than 39% of the sample of classified registrants. We use this subsample to test the implicit assumption that individuals' political affiliations are persistent over time. We focus on registrants who participate in at least two primary elections[20] in the same state, and then we compute *Persistence,* an indicator variable equal to 1 if the individual registers for the same political party throughout their observed history in that state. Using this approach, 75.2% of the 32,717,920 registrants maintain the same party affiliation throughout their observed voting history. States provide historical records covering different lengths, so we also calculate the percentage registrants with *Persistence* = 1 by state. Across the 11 states, the mean of this percentage is 75.6% and the median is 81.28%. Overall, we find that primary elections registrants are unlikely to switch their party affiliations. Nonetheless, the limitation of the data motivates the choice to corroborate the voter registration approach by alternatively measuring political affiliations through campaign contributions. Campaign contributions provide greater historical and geographical coverage of individuals' party affiliations in the U.S. We describe methods using campaign contributions in Internet Appendix IA2, and we compare the two methods in Internet Appendix IA3.

### *IA1.3 Matching LinkedIn, Voter Registration Data, and Firms*

We join the LinkedIn data with the voter registration data by first name, last name, and state. To validate the matching, we distinguish among duplicate records where individuals share a name and work contemporaneously in the same state. First, we retain perfectly matched unique records and records where the assigned party affiliation is the same for all records. Where states provide registrants' year of birth, and where users provide an education history on LinkedIn, we use individuals' ages to cross validate records. To verify records in cases where individuals share a name and work contemporaneously in the same state, we iteratively narrow the age at the first

[19] Many states use primary elections to select candidates from each party to include on general election ballots. In most states, voters must register with a political party to participate in that party's primary election. Voters do not have to register with a party to vote in general elections, and their general election vote is private.
[20] Individuals might skip registering for and voting in elections.

education year until we obtain a unique record. The iterative procedure affords flexibility for cases where individuals enroll in college earlier or later than the US voting age of 18, and for cases where individuals report a secondary school in their education history. The procedure also eliminates records where the matched user is unlikely to be an eligible voter. We obtain 19,598,154 user records matched with party affiliations following the procedure.

Next, we join the records with public company data in CRSP and Compustat. We remove punctuation and standardize the spellings of words in the company name variables across the two databases, and then we match CRSP historical company names with LinkedIn employer names. The resulting dataset includes 6,090,261 unique employees of US public companies registered as either Democrats or Republicans.

## IA2 Individual Political Donation Data and Matching Procedure

### *IA2.1 Political Donation Data*

An alternative approach to measuring employees' political attitudes is through their political campaign contributions. We obtain information on individual contributions to political campaign committees from the Federal Election Commission (FEC). The FEC maintains transaction-level records of individual donations organized by election cycle. Donations made by an individual in a reporting period must be above a minimum value to be recorded in the file, and the minimum has changed over time: \$500 and above from 1975 to 1988, \$200 and above from 1989 to 2014, and above \$200 from 2015 onwards.[21] For each transaction, the FEC records the transaction amount, date, and ID of the committee receiving the donation, as well as information about the donor. The donor information includes, among other details, self-reported information on the name of the donor, state, zip-code, and city where the donor resides, and the donor's employer. We describe the procedure using the self-reported employer names to match individuals with firms in a later subsection.

We classify donations as Republican or Democrat based on the affiliated party declared by the Political Action Committee (PAC) receiving each donation. PACs registered with the FEC, however, often do not declare a party affiliation. In fact, only 35.4% of PAC-election cycle

[21] More information is available on the FEC's website: https://www.fec.gov/campaign-finance-data/contributions-individuals-file-description/.

observations correspond to PACs that declare an official party affiliation.[22] To overcome this issue, we first identify PACs with no declared party affiliation that are connected to a specific candidate who, in turn, declares a party affiliation. We assign these PACs the party affiliation of their connected candidate. This procedure populates an additional 9.2% of the PAC-election cycle observations in our sample with party affiliations.[23] PACs can also make donations to other committees. We use this information to classify the remaining PACs based on the recipients of their donations. Specifically, we assign a Democratic (Republican) affiliation to committees in a given election cycle when at least 80% of their donations go to committees declared Democratic (Republican). This procedure populates an additional 5.4% of the PAC-election cycle observations with party affiliations.[24]

*IA2.2 Matching Political Donations to Firms*

The FEC does not maintain a standardized method to record employer names. For example, the telecommunications company Verizon appears as "Verizon Communications Inc" in the Center of Research in Security Prices (CRSP) names file. However, it is reported in approximately 500 different ways in the FEC files. Examples include: "Verizon", "Verizon Comm", "Verizon Communications", "Verizon Communications Inc", "Verizon Communications, Inc", etc. Instead of matching directly on company names, we implement a fuzzy string matching procedure to merge FEC data to CRSP and Compustat.

We start from the FEC individual donations bulk data, available from 1979. We drop donations from individuals who are self-employed or are not employed.[25] Next, we drop any employer string that appears fewer than 5 times throughout the sample and then apply a series of edits to standardize the data. The edits include dropping all symbols such as hyphens, underscores, and question marks. To minimize false matches, we overwrite common terms such as

[22] An election cycle corresponds to the two-year House of Representatives election cycle. The FEC reports connected candidates for PACs every two years, and we use the same time frame to assign party affiliations.
[23] For example, in 2016, the committee "Secure Our Senate 2016" declared no party affiliation and was connected to Kamala Harris. We thus assign a party affiliation of "Democratic" to "Secure Our Senate 2016". From 2016 to 2018, 95% of the committee's donations went to committees declared Democratic, 5% went to committees with no declared party affiliation, and 0% went to committees declared Republican.
[24] To validate our method, we compare the donations of committees with declared party affiliations to donations of committees with assigned party affiliations. The assigned party affiliations have a higher correlation with partisan political donations than the officially declared party affiliations do.
[25] There are 38 million donations reporting one of the following employer strings: "Not Employed", "Retired", "None", "Self", and "Self Employed."

“communications”, “development”, “real estate”, “enterprise”, and “limited” with their respective abbreviations. These terms are common to many company names and can inflate the matching score, especially when the rest of the name is short. Finally, we replace numbers with their full spelling to increase the weight of numbers in the matching score. For example, “21ST CENTURY HOLDING CO” becomes “TWOONEST CENTURY HOLDING CO”. We apply the same set of edits to company historical names in CRSP.

After standardizing the data, we compute the bigram score between each employer string in the FEC files and each company name available in the CRSP names files after 1978. Bigram score decomposes each string into elements of two characters on a moving-window basis, and then calculates a similarity score as follows:

$$similscore = \frac{number\ of\ common\ bigrams}{\sqrt{number\ of\ bigrams\ in\ string\ 1 * number\ of\ bigrams\ in\ string\ 2}}.$$

*similscore* thus ranges from 0 to 1. For example, consider the two strings: “Verizon Inc” and “Verzon Inc”. Bigram decomposes each string into elements of two characters as follows:

“Verizon Inc”: “Ve”, “er”, “ri”, “iz”, “zo”, “on”, “n ”, “ I”, “In”, “nc”

“Verzon Inc”: “Ve”, “er”, “rz”, “zo”, “on”, “n ”, “ I”, “In”, “nc”

Hence, the similarity score between the above two strings is:

$$similscore = \frac{8}{\sqrt{10 * 9}} = 0.84.$$

We keep the best matched CRSP name for each FEC employer string, delete all matches with a bigram score less than 0.75, and manually check all matches with a score of 0.75 or higher. This yields 82,266 string matches that we manually check. Altogether, we match 6,576,640 donations out of 82,403,704 total donations with a valid employer name from the year 1979 to 2019. The match rate is low, but we only attempt to match employees with publicly traded firms with available CRSP data. Consequently, employees of private corporations, small businesses, non-profit organizations, and the government sector will not be matched. We then assign a political affiliation to each donation and end up with 1,555,766 donations corresponding to 9,522 unique firms, with an average of $6,957 in donations per firm each year, of which $3,732 is contributed to Democrat-affiliated committees and $3,631 to Republican-affiliated committees.[26]

[26] Earmarked contributions could be a concern with the donations data. The PAC receiving an earmarked contribution acts as a conduit and must forward the contribution to the candidate or the candidate’s authorized committee within ten days of receiving it. One concern arises because ActBlue, the PAC and fundraising platform that serves Democrat

## IA3 Additional Details on Political Affiliation Measures

### *IA3.1 Comparison of Political Affiliation Measures*

In this subsection, we detail and compare several variables to measure the political affiliations of firms. From the voter registration data, we construct *DemAffVR*, the number of employees in a company-year, identified through LinkedIn, that are registered as Democrats, divided by its total number of employees registered as Democrats or Republicans. From the political donation data, we define *DemAffN* as the number of individual employees' political donations to Democrat committees divided by the total number of donations to both Democrat and Republican committees in the past two presidential election cycles. Apart from voter registrations and political donations, we also use state popular votes in Presidential general elections. We assign companies to states based on historical headquarter locations. For each company and year, we construct *DemAffState* using the number of popular votes cast for the Democrat candidate divided by the number of votes cast for either the Democrat or Republican candidates in the preceding Presidential election.

While all of the measures of political affiliation use information from politically-involved individuals, each has its own advantages and disadvantages. For example, *DemAffVR* measures political affiliation using a larger number of individuals who participate in party elections, but the data covers a limited number of states, and we assign party affiliations using nonhistorical data. On the other hand, *DemAffN* uses the history of political donations and covers all states, but the number of donors is significantly less than the number of primary election registrations. Further, political donations might reflect the political affiliations of employees with greater discretionary income or greater wealth. While *DemAffState* incorporates the largest number of individuals and covers the largest number of firms, it does not use the actual employees in each firm. The mechanisms we propose relate to employees' political affiliations, so we blend the voter registration and political donation approaches in the majority of our tests. The variable *Democratic Affiliation,* the precursor to our main variable of interest *Political Divergence*, is the average of

politicians, preceded WinRed (its Republican counterpart) by several years. Prior to WinRed, Giv.GOP served as the Republican's response to ActBlue, but unlike WinRed and ActBlue, Giv.GOP did not report contributions to the FEC (it registered with the FEC and filed a Statement of Organization and a Termination Report, these filings are here: https://www.fec.gov/data/committee/C00703660/?tab=filings). By including earmarked contributions before WinRed, the percentage of donations towards Democrat committees might be biased upwards. We verify that the donation-based results are robust to excluding earmarked contributions.

*DemAffVR* and *DemAffN,* and where one is missing, we use the other. All of the measures range from 0 to 1, with a greater value representing closer affiliation to the Democrat party.

Table IA1 presents summary statistics for the political affiliation measures described above. Panel A provides summary statistics. The sample mean *DemAffVR* (0.514) is greater than the mean *DemAffN* (0.422), potentially because of differences in state coverage or because the latter reflects the political preferences of wealthier individuals. The mean *DemAffState* (0.529) is greater still, possibly representing states with a large number of firms like California or New York. Political donations are sparser than voter registrations; the average firm-year contains 56 political donations, 292 voter registrations, or hundreds of thousands of popular votes. The ranking of sparseness is inverse to the ranking of sample standard deviations. Finally, we note that *DemAffState* covers a larger number of firms than the other measures because its construction does not require matching to any individual employees. The various approaches to measuring political affiliations have clear advantages and disadvantages that motivate the construction of *Democratic Affiliation*.

Panel B of Table IA1 presents pairwise correlation coefficient estimates over the intersection of firm years where all measures are available (N = 50,699). While each measure captures different dimensions of political affiliations, they should nonetheless correlate positively with each other. As expected in column (1), the sample correlations between *Democratic Affiliation* and its inputs, *DemAffVR* and *DemAffN*, are positive and very large at 0.673 and 0.924, respectively. Despite being drawn from entirely different data sources, the correlation estimate between *DemAffVR* and *DemAffN* is positive and large at 0.339. Finally, *DemAffState* correlates positively with the other variables. The minimum *t*-statistic among all of the correlation estimates is 62.8, so all of the correlations are positive, large, and statistically significant at the 1% level. The estimates in Table IA1 highlight the advantages and disadvantages of each measure while illustrating their overall consistency with each other.

### *IA3.2 Political Affiliation by Industry and Geography*

We account for acquirer and target industries and geography in our methods. In this subsection, we investigate the extent to which political affiliations cluster by industry and by state. If a firm's industry largely determines its *Democratic Affiliation*, an empirical concern is that *Political Divergence* and merger formation have spurious negative correlation because mergers are more likely to occur within industries than they do across industries. A similar concern exists for states, though the majority of mergers in the sample occur across states rather than within states, as shown in Figure 4 of the main text.

To evaluate industries in our sample, we estimate regression models predicting *Democratic Affiliation* over our sample of 91,224 firm-years. We add varying levels of fixed effects and report the unadjusted $R^2$ value of each regression. To establish a baseline, we begin by adding year fixed effects, 35 in total, with a resulting $R^2$ estimate of 0.04. Next, we add 73 industry FEs at the 2-digit SIC code level, and the $R^2$ estimate increases to 0.15. We then redefine industry FEs to the narrower 4-digit SIC code level, increasing the number of industries to 782. The $R^2$ rises to 0.253. Next, we estimate including 4-digit industry by year FEs, totaling 13,576 explanatory variables, and the $R^2$ estimate rises to 0.313. We continue by saturating the regression model with state FEs and state by year FEs to consider geographical effects. We obtain a maximum unadjusted $R^2$ estimate of 0.399 from a model containing 15,662 industry by year and state by year FEs. The majority of variation in *Democratic Affiliation* among sample firm-years remains unexplained by time varying industry and state effects. By contrast, estimating a regression model with only the 8,323 firm FEs results in an $R^2$ estimate of 0.757. The estimates suggest that while industries and states do explain a significant portion of *Democratic Affiliation*, there is still significant variation in firms' political affiliations within industries and states.

## IA4 Additional Methods and Tests

*IA4.1 The Hypothetical Distribution of Political Divergence*

We hypothesize that merger participants tend to select merger partners with similar political affiliations. We define *Political Divergence* as the absolute value of the difference between the *Democratic Affiliation* measures of two firms. In Figure 1 of the main text, we follow Hoberg and Phillips (2010) and form pairs using all firms to construct a hypothetical mass for *Political Divergence*. The distribution of the *Political Divergence* measure among all firms depends on the underlying distribution of *Democratic Affiliation*. One concern is that the overall distribution of *Democratic Affiliation* among merging firms may be dissimilar to other firms in the sample. For example, merging firms might have a more centralized distribution of *Democrat Affiliation* than other firms. Such as case would yield a type I error because the average *Political Divergence* in announced mergers would be lower than the average for other firm pairs but not because merger participants choose politically similar partners.

To address this concern, in Figure IA1, we plot the distribution of *Democratic Affiliation* for all firms we match to CRSP and Compustat alongside the distribution for merging firms in the sample. The distributions are visually similar to each other. A test of whether the distributions of *Democratic Affiliation* differ across bins fails to reject the null hypothesis ($\chi^2$=18.27, $p$ = 0.50), suggesting that the distribution of *Democratic Affiliation* of merging firms is similar to that of all other firms.

A second concern is that the distribution of *Democratic Affiliation* changes over time with the shifting political landscape. At the extreme, if the Democrat (Republican) party obtains unanimous popular support nationwide, *Democratic Affiliation* would equal one (zero) for all firms. In these extreme cases, the measure of *Political Divergence* between any firm pair would equal zero. Observing a decline in *Political Divergence* between merging firms could be a consequence of greater political unity behind one party rather than a rise in selectiveness among potential merger partners.

Two observations alleviate concerns about the time series of *Democratic Affiliation*. First, dramatic popular majorities in nationwide elections are uncommon in our sample period. In all U.S. Presidential elections over the sample period 1985-2019, the percentage of popular votes cast for the winning[27] candidate, excluding third party votes, ranged from a minimum of 48.9% in 2016 to a maximum of 54.7% in 1996. Second, in Figure 3, we form all pairs of firms by year to obtain a hypothetical distribution of *Political Divergence* over time to account for the changing composition of firms and the shifting distribution of *Democratic Affiliation* over time. We use the yearly mean of *Political Divergence* from all firm pairs as the null hypothesis in Figure 3. We do not observe large, significant movements in the hypothetical mean of *Political Divergence* over the sample period, consistent with the near a 50% split of popular votes across the Democrat and Republican Presidential candidates.

Table IA2 presents the percentage of all firm pairs across ranges of *Political Divergence* over Presidential election cycles. We use the distribution represented in Table IA2 as the null hypothesis to estimate $\chi^2$ test statistics presented in Table 2. Notably, high *Political Divergence*

[27] Electoral college votes determine the winner of US Presidential elections, so a candidate may win the election despite having fewer popular votes than an opponent.

between firm pairs is uncommon; only 15% of firm pairs have *Political Divergence* greater than 0.5. Accordingly, the hypothetical distribution predicts 52% of announced mergers with *Political Divergence* in the range [0, 0.25], and 2% of announced mergers with *Political Divergence* in the range (0.75, 1]. By contrast, the observed percentages of mergers with *Political Divergence* in these ranges are 63% and 1%, respectively.

Taken altogether, the figures and estimates in this subsection alleviate concerns that the patterns of *Political Divergence* among merging firms relate to overall changes in *Democratic Affiliation* or the composition of firms.

### *IA4.2 Affective Polarization, Cultural Distances, and ESG Policies*

Existing political science research shows that political differences affect group attitudes distinctly from other cultural or social differences (e.g., Himmelfarb and Lickteig (1982); Iyengar and Westwood (2015)). In this subsection, we investigate the time series relation between nationwide affective polarization and nonpolitical differences, i.e., corporate culture and ESG practices, between merger partners. We hypothesize that greater affective polarization will relate to lesser political divergence in announced deals, but that greater affective polarization will have no relation to nonpolitical differences in announced deals.

We take the yearly average of each variable: *Political Divergence*, *Aggregate Cultural Distance,* and *ESG Distance*. Then, we standardize them by subtracting their respective time series means and dividing by their respective time series standard deviations to obtain *Standardized Political Divergence, Standardized Aggregate Cultural Distance,* and *Standardized ESG Distance*. We then regress these variables on the annual measure of affective polarization, *Standardized Partisan Conflict Index*.

Table IA3 presents estimates. In Table IA3 column (1), which predicts *Standardized Political Divergence*, the coefficient estimate is negative, economically large, and statistically significant at the 5% level (estimate = -0.399; *t*-statistic = -2.61). The estimate suggests that a one standard deviation increase in affective polarization decreases the average *Political Divergence* of deals in a year by 0.40 standard deviation. The result echoes the findings of Table 3 that the political divergence of announcing merger partners declines as affective polarization increases over the sample period. In Table IA3 column (2), predicting *Standardized Aggregate Cultural Distance,* the coefficient estimate is positive and not statistically significant. Column (3) predicts differences in ESG policies, and the coefficient estimate is negative, but it is small in magnitude and not statistically significant (estimate = -0.111; *t*-statistic = -0.44). The estimates in columns (2) and (3) are inconsistent with the view that affective polarization correlates with the nonpolitical cultural differences between merger partners. The estimates show that the rise in political polarization in the United States correlates with the time trend exhibited by political divergence in announced mergers, but that this correlation is not shared with other forms of corporate cultural differences. Taken together, the findings in Table IA3 conform to the hypothesis that affective polarization influences the effects of political divergence on merger formation distinctly from other, nonpolitical aspects of culture.

*IA4.3 Robustness Tests of Merger Partner Selection*

In the main text, we estimate merger partner selection following Bena and Li (2014) by matching each deal participant with up to five pseudo-partners matched on industry, size, and book-to-market. The dependent variable is *Announced Deal*, equal to one for the firm-pair in the announced deal and equal to zero for the pseudo-deals, and the explanatory variable of interest is *Political Divergence*. In this subsection, we provide estimates using four alternative approaches. First, we slacken matching to select on industry and size. Second, we match participants to random pseudo-partners, instead. Third, we return to matching with pseudo-partners based on the industry, size, and book-to-market ratio, but we estimate linear regressions instead. Fourth, we estimate weighted logistic regressions that penalize deals with fewer employees and donations used to estimate *Democratic Affiliation*.

Table IA4 presents estimates from these alternative merger selection models. Panel A presents estimates when matching on industry and size, and Panel B presents estimates when matching randomly. Panel C presents estimates from linear models, and Panel D presents estimates from weighted logistic models. In all panels, column (1) controls for political affiliation measures, but no other characteristics; column (2) adds additional control variables and industry pair by year fixed effects; column (3) adds deal fixed effects, and column (4) excludes hostile bids.

The coefficient estimate of *Political Divergence* is negative and statistically significant at the 1% level in 14 of the 16 specifications in Table IA4. In the last two columns of Panel D, the estimates are negative and statistically significant at the 10% level. The estimates are consistent with the main findings suggesting that greater *Political Divergence* between potential merger partners decreases the likelihood of announcing a merger.

### *IA4.4 Measuring Affective Polarization using Congressional Voting*

We construct an alternative measure of nationwide affective polarization using congressional voting records in the U.S. House of Representatives. The House votes on bills, resolutions, motions, nominations, and other significant matters by taking yea-or-nay roll-call votes (hereafter, roll-calls). We obtain outcomes from all roll-calls in each year. For each roll-call, we calculate *RepYes*, the proportion of "yea" votes cast by Republican representatives as a proportion of all Republican votes, and *DemYes*, the proportion of "yea" votes cast by Democrat representatives as a proportion of all Democrat votes. We exclude votes from third party, independent, absent, and abstaining representatives. For each roll-call, we calculate *Partisan Disagreement* = |*RepYes* – *DemYes*|. The variable *Partisan Disagreement* increases (decreases) when political parties cast votes in the opposite (same) direction. In each year, we average *Partisan Disagreement* over all roll-calls to obtain the variable *House Partisanship Index*. We standardize the variable by subtracting its sample mean and dividing by its sample standard deviation to facilitate a comparison of the measures of affective polarization.

Figure IA2 plots *Standardized House Partisanship Index* alongside *Standardized Partisan Conflict Index* and NBER recessions over the sample period. The congressional voting measure has greater variation over the time series. Overall, the two measures of affective polarization comove, though there are periods of divergence such as the early 1990s and the middle 2000s. Importantly, we observe a significant increase in both measures of affective polarization after 2010. Over the sample period, the two measures of affective polarization have a large and statistically significant correlation of 0.59, (*t*-statistic = 4.20). Overall, the two approaches to measuring affective polarization correspond with each other.

For robustness, we test whether affective polarization influences the effects of *Political Divergence* on merger formation using the congressional voting measure. We define *High HPI*, an indicator variable equal to 1 if the standardized measure is greater than its sample mean. We repeat the regressions presented in Table 5 of the main test using this alternative measure.

Table IA5 presents estimates. The pattern is strikingly similar to Table 5 in the main text. In column (1), the coefficient estimate on *Political Divergence* for low polarization times is negative but not statistically significant. In contrast, the same estimate in column (2), where polarization is higher, is negative and statistically significant at the 1% level. Again, the estimate in column (2) is more than three times the estimate in column (1). In column (3), we estimate a pooled regression that interacts all the independent variables with *High HPI*. The coefficient estimate on the interaction term *Political Divergence*×*High PCI* represents the difference between the estimated effect of political divergence on merger formation during high vs. low polarization periods. The estimate is negative and statistically significant at the 10% level (*z*-statistic = -1.76). Consistent with the patterns in Table 5, the estimates on the interaction terms of the other control variables are not statistically significant at conventional levels. Overall, the estimates in Table IA5 using the alternative measure of affective polarization corroborate the findings in the main text.

*IA4.5 Separated Corporate Culture Measures*

To test whether *Political Divergence* influences merger formation distinctly from nonpolitical aspects of corporate culture, we combine five measures provided by LMSY (2020), namely *Innovation*, *Integrity*, *Quality*, *Respect*, and *Teamwork*. In this subsection, we provide robustness tests where we consider these measures separately. For each culture measure, we form cultural distance variables equal to the absolute value of the difference between potential acquirers' and targets' measures. We standardize *Political Divergence* and each cultural distance variable by subtracting their respective sample means and dividing by their respective sample standard deviations.

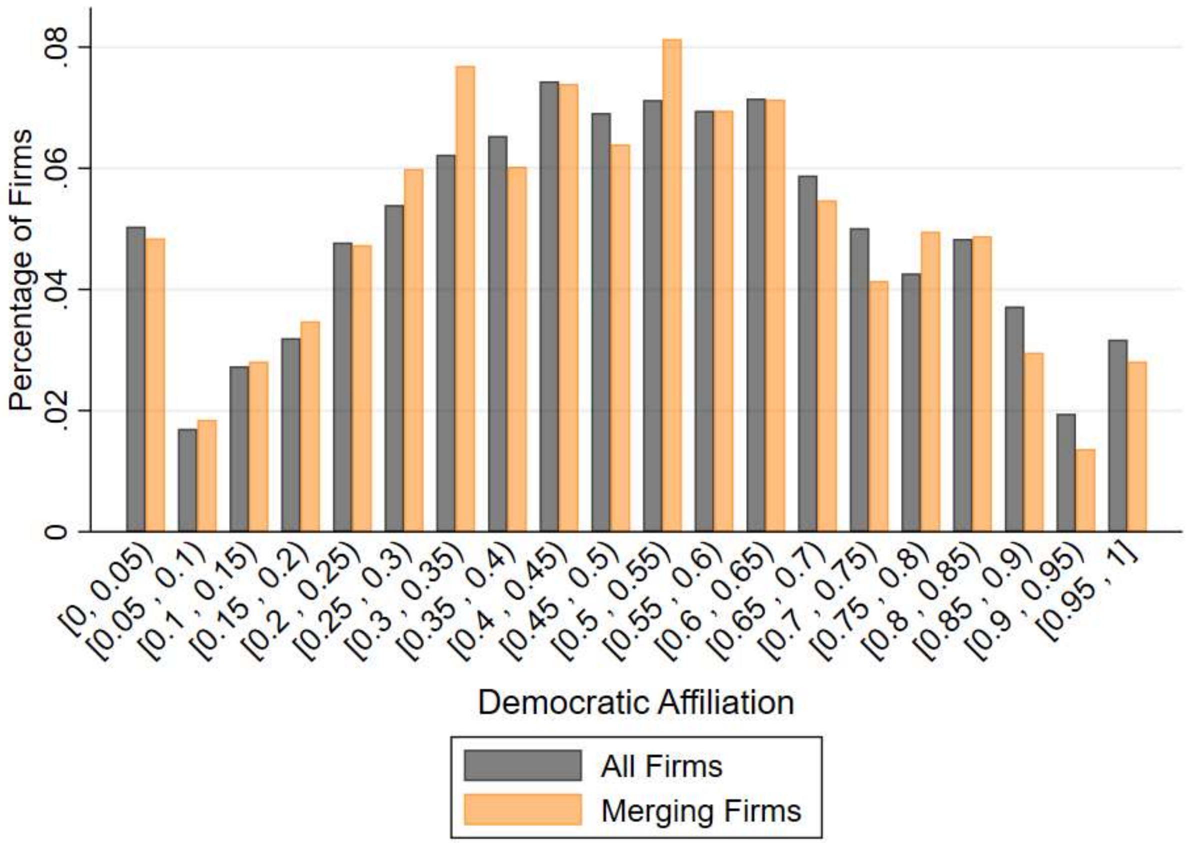
In Table IA6, we provide estimates repeating the procedure in Table 6 of the main text, but with these separate measures of corporate culture. Column (1) regresses on *Political Divergence* to establish a baseline. As before, the estimate is negative and statistically significant at conventional levels (*z* = -2.54). In column (2), we estimate the baseline for the cultural distance measures. The coefficient estimates on *Innovation Distance* and *Quality Distance* are negative and statistically significant (*z* = -1.91 and *z* = -2.25), respectively. The estimates suggest that differences in those cultural measures negatively predict merger formation. The coefficient estimate on *Teamwork Distance* is positive and statistically significant at the 5% level (*z* = 2.26), suggesting the opposite effect along that dimension. In column (3), we add back *Political Divergence*. As with tests using *Aggregate Cultural Distance* in Table 6, the coefficient estimate on *Political Divergence* remains negative and statistically significant at conventional levels (*z* = -

2.56). Second, the coefficient estimates among *Political Divergence* and the cultural distance measures in column (3) are nearly identical to those in columns (1) and (2). The findings suggest that both political differences reduce the likelihood of a merger distinctly from other, nonpolitical corporate cultural differences.

## Figure IA1

## Distribution of Democratic Affiliation across Firms

This figure plots the cross-sectional distribution of *Democratic Affiliation* for firms in our sample. We compute the time series average of *Democratic Affiliation* for each of the 8,418 firms in the sample over the period 1985-2019 to create the cross section for all firms, represented in the left, gray bars. We compute the value of *Democratic Affiliation* in the year before the merger announcement to create the cross section for merging firms.



## Figure IA2
## Alternative Measures of Affective Polarization from 1985 – 2019

This figure plots the evolution of political polarization in the U.S. from 1985 to 2019 using standardized annual averages of the Partisan Conflict Index from Azzimonti (2018) maintained by the Federal Reserve Bank of Philadelphia and *House Partisanship Index* constructed based on voting at the U.S. House of Representatives. We construct the *House Partisanship Index* using outcomes on yea-or-nay voting in the U.S. House of Representatives. Specifically, for each vote, we define *Partisan Disagreement* as follows:

$$Partisan\ Disagreement_{v,t} = |RepYes_{v,t} - DemYes_{v,t}|$$

where, $RepYes_{v,t}$ ($DemYes_{v,t}$) is the proportion of yea votes cast by Republican (Democratic) representatives as a proportion of all Republican (Democratic) votes cast on vote *v* in year *t*. We then compute the *House Partisanship Index* as the average *Partisan Disagreement* for all votes in the U.S. House of Representatives in calendar year *t*. We standardize both variables by subtracting their respective sample means and dividing by their respective standard deviations. Shaded areas are NBER recession periods.

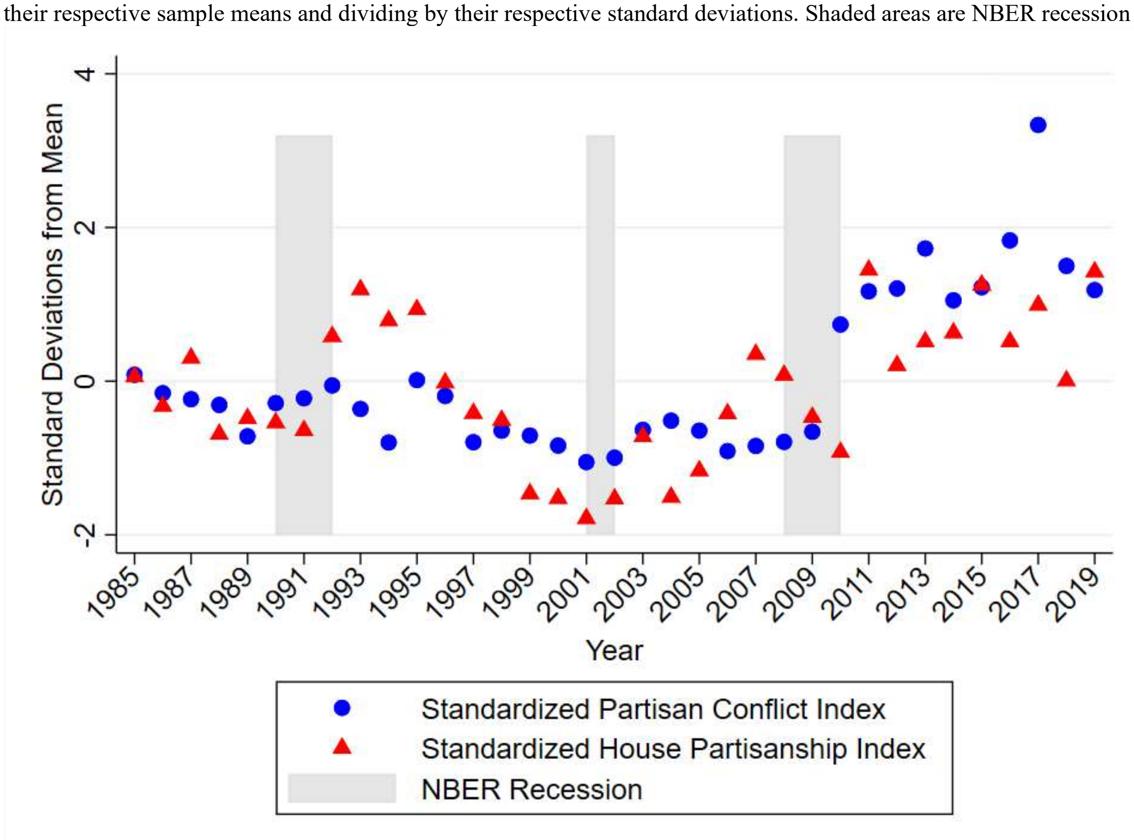


**Table IA1**

**Summary Statistics of Political Affiliation**

This table presents summary statistics of political affiliation for the sample of firm-years matched to CRSP and Compustat. Using voter registrations and LinkedIn profiles, *DemAffVR* is the number of employees in a firm-year that are registered as Democrats divided by the total number of employees registered as Democrats or Republicans. Using political donations, *DemAffN* is the number of employees' political donations made to Democrat committees divided by the total number of donations to both Democrat and Republican committees in the past two presidential election cycles. *Democratic Affiliation* is the average of *DemAffVR* and *DemAffN*, or where one is missing, it equals the other. After assigning firms to states based on headquarter locations, the variable *DemAffState* is the fraction of popular votes earned by the Democrat presidential candidate divided by popular votes, excluding third party candidates. Panel A provides summary statistics, and Panel B provides pairwise correlations estimates over the intersection of firm-years where all measures are available.

Panel A: Descriptive Statistics

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Mean | SD | 25th %ile | Median | 75th %ile | N |
| Democratic Affiliation | 0.473 | 0.244 | 0.286 | 0.466 | 0.656 | 91,224 |
| DemAffVR | 0.514 | 0.195 | 0.386 | 0.503 | 0.647 | 79,577 |
| DemAffN | 0.422 | 0.362 | 0.034 | 0.375 | 0.743 | 80,151 |
| DemAffState | 0.529 | 0.084 | 0.473 | 0.534 | 0.586 | 136,229 |

Panel B: Pairwise Correlations (N = 50,699)

| | (1) | (2) | (3) |
|---|---|---|---|
| | Democratic Affiliation | DemAffVR | DemAffN |
| DemAffVR | 0.673 | | |
| DemAffN | 0.924 | 0.339 | |
| DemAffState | 0.353 | 0.348 | 0.269 |

## Table IA2

## The Hypothetical Distribution of Political Divergence over Election Cycles

This table shows the percentage of all possible firm pairs across ranges of *Political Divergence* over U.S. presidential election cycles. Each row represents a presidential election cycle, defined as the four years leading up to a U.S. Presidential Election. We require firms be publicly listed with available political affiliation measures. All variable definitions are given in Appendix A.

| (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|
| Cycle | Political Divergence | | | | |
| Ending | [0,0.25] | (0.25,0.50] | (0.5,0.75] | (0.75,1] | N |
| 1988 | 49.1% | 32.1% | 14.6% | 4.2% | 3,532,520 |
| 1992 | 51.5% | 32.3% | 13.0% | 3.2% | 4,676,406 |
| 1996 | 51.3% | 32.6% | 13.1% | 3.0% | 10,423,212 |
| 2000 | 51.2% | 32.9% | 13.1% | 2.8% | 18,288,452 |
| 2004 | 52.7% | 33.0% | 12.2% | 2.1% | 15,323,310 |
| 2008 | 52.5% | 33.1% | 12.3% | 2.1% | 14,857,170 |
| 2012 | 54.4% | 33.0% | 11.1% | 1.5% | 11,843,922 |
| 2016 | 53.9% | 33.1% | 11.4% | 1.6% | 11,617,872 |
| 2020* | 52.8% | 32.4% | 12.4% | 2.3% | 10,052,070 |
| Overall | 52.4% | 32.8% | 12.4% | 2.3% | 100,614,934 |

**Table IA3**

**Affective Polarization and Cultural/ESG Distance**

This table provides estimates from time series linear regressions using affective polarization to predict standardized yearly averages of: (1) *Political Divergence,* (2) *Aggregate Cultural Distance,* and (3) *ESG Distance* among announced deals. For each variable, we average by year and then standardize by subtracting the time series mean and then dividing by the time series standard deviation. We report *t*-statistics in parentheses. Significance: *p<10%, **p<5%, ***p<1%.

| | (1) | (2) | (3) |
|---|---|---|---|
| Variables | Standardized Political Divergence | Standardized Aggregate Cultural Distance | Standardized ESG Distance |
| Standardized Partisan Conflict Index | -0.399** | 0.247 | -0.111 |
| | (-2.61) | (1.32) | (-0.44) |
| Constant | 0.019 | -0.123 | 0.108 |
| | (0.12) | (-0.49) | (0.29) |
| Observations | 35 | 18 | 13 |
| $R^2$ | 0.171 | 0.098 | 0.017 |

## Table IA4

## Merger Partner Selection: Alternative Specifications

This table presents estimates from alternative regression specifications explaining merger partner selection. We follow Bena and Li (2014) and match each deal participant with up to five pseudo-partners in same industry as the actual partner. The dependent variable is *Announced Deal*, an indicator variable that is equal to one for the announced deal and zero for all the pseudo-deals. The variable *Political Divergence* is the absolute value of the difference between the acquirer's and target's *Democratic Affiliation.* We calculate *Democratic Affiliation* using voter registration data and individual donation data. In Panels A and B, we alter the procedure for matching deal participants. In Panel A, we match based on industry and size, and in Panel B, we match participants to random pseudo-partners. In Panels C and D, we match on industry, size, and book-to-market but alter the regression form. In Panel C, we estimate linear regressions and in Panel D, we estimate conditional logistic regressions weighing deal groups by the total number of voter registrations and donations used to estimate *Political Divergence*. In all panels, column (1) only controls for political affiliation measures. Column (2) adds additional control variables and industry pair by year fixed effects. Column (3) adds deal fixed effects. Column (4) excludes hostile bids. All other variables are defined in Appendix A. We report $z$-scores in parentheses. Significance: *p<10%, **p<5%, ***p<1%.

Panel A: Industry and Size Matching

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Political Divergence | -0.637*** | -0.447*** | -0.444*** | -0.520*** |
| | (-4.99) | (-3.17) | (-3.07) | (-3.39) |
| Acquirer Democratic Affiliation | 0.216** | 0.091 | 0.172 | 0.112 |
| | (1.97) | (0.96) | (1.55) | (0.94) |
| Target Democratic Affiliation | 0.151 | 0.275*** | 0.314*** | 0.325*** |
| | (1.61) | (2.99) | (3.07) | (2.98) |
| HQ Distance | | -0.042*** | -0.043*** | -0.041*** |
| | | (-11.08) | (-11.91) | (-10.71) |
| Similar Products | | 0.729*** | 0.725*** | 0.579*** |
| | | (8.32) | (9.22) | (6.91) |
| Additional Control Variables? | No | Yes | Yes | Yes |
| Industry Pair × Year FEs? | No | Yes | Yes | Yes |
| Deal FEs? | No | No | Yes | Yes |
| Excludes Hostile Bids? | No | No | No | Yes |
| Observations | 23,890 | 22,040 | 21,469 | 18,939 |
| Pseudo R-squared | 0.002 | 0.056 | 0.139 | 0.132 |

Panel B: Random Matching

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Political Divergence | -0.863*** | -0.398*** | -0.427*** | -0.455*** |
| | (-6.84) | (-2.70) | (-2.91) | (-2.91) |
| Acquirer Democratic Affiliation | 0.117 | 0.046 | 0.139 | 0.122 |
| | (1.11) | (0.48) | (1.25) | (1.01) |
| Target Democratic Affiliation | 0.121 | 0.235** | 0.277*** | 0.272** |
| | (1.28) | (2.24) | (2.61) | (2.41) |
| HQ Distance | | -0.040*** | -0.040*** | -0.037*** |
| | | (-10.27) | (-11.03) | (-9.72) |
| Similar Products | | 0.661*** | 0.696*** | 0.563*** |
| | | (7.07) | (8.86) | (6.74) |
| Additional Control Variables? | No | Yes | Yes | Yes |
| Industry Pair × Year FEs? | No | Yes | Yes | Yes |
| Deal FEs? | No | No | Yes | Yes |
| Excludes Hostile Bids? | No | No | No | Yes |
| Observations | 23,890 | 21,887 | 21,320 | 18,799 |
| Pseudo R-squared | 0.004 | 0.115 | 0.158 | 0.155 |

Panel C: Linear Regression Model

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Political Divergence | -0.064*** | -0.030*** | -0.032*** | -0.033*** |
| | (-6.42) | (-2.95) | (-3.02) | (-2.95) |
| Acquirer Democratic Affiliation | 0.004 | 0.003 | 0.004 | 0.003 |
| | (0.45) | (0.60) | (0.68) | (0.53) |
| Target Democratic Affiliation | 0.007 | 0.017** | 0.023** | 0.024** |
| | (0.97) | (2.04) | (2.38) | (2.36) |
| HQ Distance | | -0.003*** | -0.003*** | -0.003*** |
| | | (-10.99) | (-12.13) | (-10.57) |
| Similar Products | | 0.051*** | 0.057*** | 0.041*** |
| | | (5.61) | (7.05) | (4.94) |
| Additional Control Variables? | No | Yes | Yes | Yes |
| Industry Pair × Year FEs? | No | Yes | Yes | Yes |
| Deal FEs? | No | No | Yes | Yes |
| Excludes Hostile Bids? | No | No | No | Yes |
| Observations | 23,890 | 22,354 | 22,354 | 19,725 |
| Pseudo R-squared | 0.002 | 0.052 | 0.064 | 0.060 |

Panel D: Weighted Logistic Regression Model

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Political Divergence | -1.901*** | -1.137*** | -0.752* | -0.877* |
| | (-5.38) | (-2.83) | (-1.68) | (-1.88) |
| Acquirer Democratic Affiliation | 0.748*** | 0.154 | 0.772** | 0.768* |
| | (3.18) | (0.44) | (2.11) | (1.95) |
| Target Democratic Affiliation | -0.142 | 0.041 | 0.061 | 0.115 |
| | (-0.56) | (0.14) | (0.21) | (0.38) |
| HQ Distance | | -0.019*** | -0.020*** | -0.018** |
| | | (-2.64) | (-2.60) | (-2.21) |
| Similar Products | | -0.026 | -0.089 | -0.183 |
| | | (-0.17) | (-0.45) | (-0.88) |
| Additional Control Variables? | No | Yes | Yes | Yes |
| Industry Pair × Year FEs? | No | Yes | Yes | Yes |
| Deal FEs? | No | No | Yes | Yes |
| Excludes Hostile Bids? | No | No | No | Yes |
| Observations | 23,890 | 22,355 | 21,429 | 18,903 |
| Pseudo R-squared | 0.013 | 0.145 | 0.324 | 0.312 |

## Table IA5

## Merger Partner Selection: An Alternative Measure of Affective Polarization

This table presents estimates from conditional logistic regressions explaining merger partner during periods of high vs. low levels of affective polarization, measured by the *House Partisanship Index*. We construct the *House Partisanship Index* using outcomes on yea-or-nay voting in the U.S. House of Representatives. Specifically, for each vote, we define *Partisan Disagreement* as follows:

$$Partisan\ Disagreement_{v,t} = |RepYes_{v,t} - DemYes_{v,t}|$$

where, $RepYes_{v,t}$ ($DemYes_{v,t}$) is the proportion of yea votes cast by Republican (Democratic) representatives as a proportion of all Republican (Democratic) votes cast on vote $v$ in year $t$. We then compute the *House Partisanship Index* as the average *Partisan Disagreement* for all votes in the U.S. House of Representatives in calendar year $t$. *High HPI* is an indicator variable equal to one if *Standardized House Partisanship Index* is greater than zero, and zero otherwise. We construct the *Standardized House Partisanship Index* by subtracting the sample mean and dividing by the sample standard deviation. Columns (1) and (2) report estimates for periods where affective polarization is low and high, respectively. In column (3), we estimate the regression using the full sample but add interactions of *High HPI* with independent variables. Additional control variables are the same as in Table 4. All other variables are defined in Appendix A. We report *z*-scores in parentheses. Significance: *p<10%, **p<5%, ***p<1%.

| | (1) | (2) | (3) |
|---|---|---|---|
| Variables | High HPI = 0 | High HPI = 1 | Interaction |
| Political Divergence | -0.225 | -0.767*** | -0.225 |
| | (-1.22) | (-3.13) | (-1.22) |
| Political Divergence × High HPI | | | -0.542* |
| | | | (-1.76) |
| Acquirer Democratic Affiliation | -0.105 | -0.091 | -0.105 |
| | (-0.72) | (-0.50) | (-0.72) |
| Acquirer Democratic Affiliation × High HPI | | | 0.014 |
| | | | (0.06) |
| Target Democratic Affiliation | 0.203 | 0.274 | 0.203 |
| | (1.50) | (1.60) | (1.50) |
| Target Democratic Affiliation × High HPI | | | 0.070 |
| | | | (0.32) |
| HQ Distance | -0.042*** | -0.037*** | -0.042*** |
| | (-8.94) | (-6.54) | (-8.94) |
| HQ Distance × High HPI | | | 0.004 |
| | | | (0.57) |
| Similar Products | 0.687*** | 0.448*** | 0.687*** |
| | (6.85) | (3.54) | (6.86) |
| Similar Products × High HPI | | | -0.239 |
| | | | (-1.48) |
| Additional Controls? | Yes | Yes | Yes |
| Industry Pair × Year FEs? | Yes | Yes | Yes |
| Deal FEs? | Yes | Yes | Yes |
| Observations | 13,078 | 8,351 | 21,429 |
| Pseudo $R^2$ | 0.167 | 0.122 | 0.150 |

**Table IA6**

**Individual Measures of Corporate Culture**

This table provides estimates from conditional logistic regressions explaining merger partner selection, augmented with nonpolitical measures of corporate culture from LMSY (2020), which include *Innovation, Integrity, Quality, Respect,* and *Teamwork*. We form the sample of deals and pseudo-deals as described in Table 4, but restrict the sample to firms with available measures of corporate culture. For each culture measure, we form cultural distance variables equal to the absolute value of the difference between firms' values. We standardize *Political Divergence* and each cultural distance variable by subtracting their respective sample means and dividing by their respective sample standard deviations. Columns (1) and (2) provide baseline estimates for political divergence and nonpolitical distances, respectively. In column (3), we include political measures alongside nonpolitical measures. Additional control variables are the same as those in Table 4. All variables are defined in Appendix A. We report *z*-scores in parentheses. Significance: *p<10%, **p<5%, ***p<1%.

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| Political Divergence | -0.137** | | -0.139** |
| | (-2.54) | | (-2.56) |
| Innovation Distance | | -0.098* | -0.096* |
| | | (-1.91) | (-1.87) |
| Integrity Distance | | -0.035 | -0.037 |
| | | (-0.66) | (-0.69) |
| Quality Distance | | -0.136** | -0.137** |
| | | (-2.25) | (-2.23) |
| Respect Distance | | -0.055 | -0.054 |
| | | (-1.00) | (-0.99) |
| Teamwork Distance | | 0.119** | 0.123** |
| | | (2.26) | (2.29) |
| Acquirer Democratic Affiliation | -0.063 | | -0.076 |
| | (-0.25) | | (-0.30) |
| Target Democratic Affiliation | 0.128 | | 0.124 |
| | (0.57) | | (0.55) |
| HQ Distance | -0.042*** | -0.043*** | -0.042*** |
| | (-6.35) | (-6.46) | (-6.28) |
| Similar Products | 1.022*** | 1.023*** | 1.011*** |
| | (7.03) | (7.03) | (6.93) |
| Additional Control Variables? | Yes | Yes | Yes |
| Industry Pair × Year FEs? | Yes | Yes | Yes |
| Deal FEs? | Yes | Yes | Yes |
| Observations | 5,919 | 5,919 | 5,919 |
| Pseudo $R^2$ | 0.152 | 0.154 | 0.157 |