Political Alignment in Entrepreneurial Teams: Homophily in Venture Formation and Associations with Startup Success

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Abstract. We examine political affiliation's role in venture team formation and success. Using data from Crunchbase and L2 on 1,125 US-based startups, we investigate political homophily in team assembly and its association with startup outcomes. Our analysis reveals strong political homogeneity in founding teams: teams with similar political views form more frequently than diverse teams, even after controlling for founders' gender, age, location, and industry. This political homophily relates to venture performance. Startups with politically heterogeneous founding teams are more likely to shut down. Across additional performance measures (capital funding, employee size, Crunchbase rankings), we observe directionally consistent associations with worse outcomes, though these secondary findings vary in robustness. These findings highlight the dual role of founders' political affiliations: their relationship with team composition and startup performance.

Managerial Summary Our study examines political diversity's association with startup team formation and venture success. Analyzing data from 1,125 U.S.-based startups, we discovered a strong tendency for teams to form based on political similarity. Individuals with similar political views prefer starting companies together, even when accounting for age, gender, location, and industry. Startups with politically diverse founding teams are more likely to shut down. These teams also tend to receive less funding, have fewer employees, and exhibit worse Crunchbase rankings, although these patterns are less consistent than the survival effect. For entrepreneurs and investors, these findings highlight the need to balance team cohesion against diverse perspectives. Business practitioners should be aware of these dynamics when forming teams or developing management strategies, particularly in a politically charged environment.

INTRODUCTION

The United States is witnessing polarization's pervasive effects not only in public and legislative domains but also in everyday and professional life (Azzimonti, 2018; voteview.com, 2023). Political identities increasingly shape even ostensibly nonpolitical daily behaviors and lifestyle choices (DellaPosta et al., 2015; Talaifar et al., 2025), creating intense mutual disdain, known as affective polarization, between Democrats and Republicans, each viewing the other as morally and intellectually inferior (Iyengar et al., 2012; Lelkes, 2016; Pew Research Center, 2022; American National Election Studies, 2023). This discord and reluctance to engage across political divides raise important questions about its impact on organizational dynamics, with emerging research demonstrating that it causally impairs individual performance stemming from political heterogeneity even in non-collaborative settings (Sels & Kovács, 2025).

While ideological tensions within the entrepreneurial founding team often remain hidden from public view, the disruptive potential of political differences within organizations is evident in several prominent cases. For example, Coinbase's CEO announced a policy to minimize engagement on broader societal issues not directly related to the company's core mission, citing these as distractions that divide the workforce (Armstrong, 2020; Loizos, 2020). Basecamp founders instituted similar politics-discussion bans (Hansson, 2021; Fried, 2021). At Mozilla, co-founder Brendan Eich's 2008 donation to an anti-gay marriage bill in California led to a board member's resignation and ultimately Eich's own ousting (Rosenblath, 2014). These instances demonstrate the consequences of founders' ideological stances on organizational dynamics, suggesting that other political tensions might also relate to internal founding team interactions and decision-making even before or without becoming publicly visible.

Despite these real-world implications, political heterogeneity remains conspicuously underexplored in both the entrepreneurship literature and the broader research on team diversity.

While researchers have extensively examined team composition across dimensions such as age, gender, ethnicity, and professional backgrounds (Beckman, 2006; Eesley et al., 2014; Lazar et al., 2020; Ruef, 2010; Ruef et al., 2003), they have largely overlooked political ideology as a dimension of diversity. This oversight is striking given that political affiliations serve as proxies for underlying value systems and worldviews that shape interpersonal relationships (Mutz & Mondak, 2006) and professional collaborations (Swigart et al., 2020).

The broader literature on team diversity presents a complex picture regarding its effects on team performance (Ertug et al., 2022). Heterogeneity in attributes like cultural background, experiences, and perspectives can function as a cognitive asset, leading to more innovative solutions and higher-quality decisions (Ancona & Caldwell, 1992; Cox & Blake, 1991; Shi et al., 2019). Research on information processing suggests that diverse teams can access a broader range of knowledge and engage in more thorough problem analysis (Van Knippenberg et al., 2004; Williams & O'Reilly, 1998). However, diversity can also introduce interpersonal friction, communication overhead, and conflict, thereby impairing team processes and performance (e.g., Jehn et al., 1999; Pelled, 1996; Williams & O'Reilly, 1998). Political ideology often reflects deeply held values (Feldman, 2003; Graham et al., 2009; Schwartz et al., 2010), making it a consequential dimension of team diversity that has been surprisingly absent from most diversity research.

The entrepreneurial context offers an important setting to examine these dynamics. Startups operate under conditions of inherent uncertainty, lean structures, and the need for rapid, cohesive decision-making, where alignment of values and trust among founding members matter for venture creation and growth. Given the importance of cohesion and swift alignment for navigating early-stage venture challenges, the process costs associated with political heterogeneity may outweigh its cognitive benefits in a high-pressure, resource-constrained environment.

This paper analyzes two questions about political heterogeneity in venture founding teams. First, we ask: Is the political affiliation of founders associated with the composition of founding teams? Building on findings that venture founding teams tend toward homogeneity in age, ethnicity, and gender (e.g., Ruef 2010), we hypothesize that founding teams will also exhibit greater political homogeneity than expected from random matching, even after controlling for homophily along age, ethnicity, gender, and founding experience. Second: Does the political composition of founding teams benefit or hinder company performance? Given the complex demands of the startup context, we hypothesize that the costs of political heterogeneity are higher than its cognitive advantages, adversely affecting venture success.

Using data from Crunchbase on startup founding teams and voter registration data from L2, we identified the political affiliations of 2,292 U.S. founders. Using permutation tests, we find that politically homogeneous teams appear substantially more often in our data than would be expected under random matching, whereas politically heterogeneous teams appear substantially less often than this random baseline. In terms of startup performance, we found that, on average, politically heterogeneous startups are more likely to close down, even after controlling for funding amount. We also find that startups tend to raise less capital, have a smaller employee size, and are ranked worse on an organizational ranking, although these associations are less consistent.

The structure of the paper is as follows: First, we review theory and background literature on homophily in entrepreneurial team formation and the potential consequences of political heterogeneity. Next, we describe our research setting and data sources. We then present results testing our hypotheses on political homophily in team formation and its associations with venture performance. We conclude by discussing the study's contributions, limitations, and implications.

THEORY AND BACKGROUND LITERATURE

Homophily as the antecedent of entrepreneurial founding team formation

Homophily, the tendency of "birds of a feather to flock together" (Lazarsfeld & Merton, 1954), is well-documented (for reviews, see Kossinets & Watts, 2009; McPherson et al., 2001; Reagans, 2013). Homophily is present across dimensions such as gender, race, age, religion, education, hobbies, values, and occupation (Marsden, 1988; McPherson et al., 2001). Research has established homophily across occupational networks (Ibarra, 1992), educational backgrounds (Fischer, 1982), and racial identities (Verbrugge, 1977). Recent studies, such as Kovács and Kleinbaum (2020), demonstrate language-style homophily in MBA classrooms and social media networks. Existing research also points to homophily along deeper dimensions, such as value homophily, which refers to people's tendency to affiliate with others who share their values (Dahlander & McFarland, 2013; McPherson et al., 2001). As we elaborate later, value homophily is especially relevant to our arguments because political homophily is highly related to value homophily.

Homophily drives entrepreneurial founding team composition as well. For example, Eisenhardt and Schoonhoven (1990) demonstrated that similarity in past affiliations, such as education and previous collaborations, often leads to joint new venture formation. Ruef (2010), analyzing results of a US-based national survey, finds that business partnerships are highly homogeneous in terms of gender, occupation, age, and ethnicity, especially among minorities and immigrants facing employment prejudice (Aldrich & Waldinger, 1990; Portes & Sensenbrenner, 1993). In their analysis of Danish startup founders, Kaiser and Müller (2015) find that homophily of qualifications also plays a significant role.

Several processes explain the prevalence of homophily in entrepreneurial settings. Social science literature differentiates between two main drivers of homophily: choice homophily and induced homophily (Ibarra, 1992; McPherson & Smith-Lovin, 1987). Both types of processes exist in entrepreneurial settings as well. Choice homophily refers to in-group bias toward associating with similar individuals. This may be proactive, as in the case of prejudicial attitudes (Allport, 1954), or reactive, as when entrepreneurs, rejected by out-groups, rely on in-groups for business partners. To protect emerging ideas, nascent ventures require trust. This trust develops more readily among individuals with similar demographic characteristics (Greif, 2006). Entrepreneurs often believe that others with similar status characteristics will think and behave similarly even if this is a misperception (McPherson et al., 2001). This assumption of interpersonal understanding and trust among status-similar co-workers may contribute to the greater generation of jointly produced goods (e.g., innovations) in homophilous entrepreneurial groups. Startups involve significant collective decision-making. To avoid friction, entrepreneurs affiliate with like-minded individuals. Given that political identity can extend to distinct lifestyle patterns and everyday behaviors (DellaPosta et al., 2015; McPherson et al., 2001; Talaifar et al., 2025), choosing politically similar partners might be an implicit strategy to ensure broader value congruence or interpersonal compatibility, thereby reducing potential friction unrelated to the core business. Additionally, recruiting colleagues with similar characteristics ensures loyalty and maintains power, which matters in emerging organizations that depend on trust. These arguments suggest that individuals in startups who match dominant profiles of the startup tend to receive greater trust (Ruef, 2010).

Induced homophily refers to bias resulting from exposure to certain opportunities, even with random partner choice. It is influenced by the distribution of entrepreneurs in industries, areas, and networks. For example, if an industry is dominated by men, then even with random matching, we would likely see mostly gender-homophilous founding teams. Similarly, if an industry is dominated by a given political view, such as Republicans in the oil refinery industry in Texas, this leads to the formation of politically similar teams, even with random matching. Our empirical analyses will identify evidence for both choice and induced homophily.

Homophily occurs based on both visible social identities, such as gender or age, and less visible traits, such as similarities in values, beliefs, tastes, or other psychological dispositions (Lazarsfeld & Merton, 1954). While the entrepreneurial group literature mostly focuses on visible traits such as gender, ethnicity, and age (Ruef, 2010; Ruef et al., 2003), ample evidence in the general literature shows that values, tastes, and other psychological dispositions are also important (see e.g., Huston & Levinger, 1978; Lizardo, 2006; Vaisey & Lizardo, 2010). We expect these factors to operate in the formation of new venture founding teams as well.

In this article, we specifically focus on political values. Adults tend to associate with others who hold similar political orientations (Huckfeldt & Sprague, 1995; Knoke, 1990; Verbrugge, 1977). People are more likely to marry others with similar political views (Iyengar & Konitzer, 2017). Political views also matter within organizations. Wang and Elnahas (2023) describe how politically homogeneous executives can influence CSR policies. Maldonado-Bautista, Klein and Artz (2023) discuss the relationship between entrepreneurs' political values and their ventures' stakeholder orientation, as well as financiers' political ideologies. Ideological and political discussions within professional environments (Mutz & Mondak, 2006) affect social interactions (Swigart et al., 2020). In many organizations, employees are encouraged to bring their "authentic selves" to work (Inam, 2018; Opie & Freeman, 2017; Swigart et al., 2020), resulting in more common political discussions. This openness to political views may enhance the tendency towards political homogeneity, as political differences are more likely to emerge and lead to homophily. Political affiliation can become a salient social identity (Tajfel & Turner, 1979), fostering in-group cohesion and leading individuals to seek co-partisans when forming high-stakes collaborations like founding teams. Given that Democrats view Republicans (and vice versa) as more closedminded, unintelligent, immoral, lazy, and unpatriotic than members of their own party (Pew Research Center, 2022), individuals will likely prefer to start ventures with politically similar others.

Political homophily in founding teams likely reflects both induced and choice mechanisms. Induced homophily arises from the geographic and industrial concentration of partisans: if Democrats cluster in coastal tech hubs, politically homogeneous teams can form even through random local matching. Choice homophily reflects active partner selection based on political identity and the value alignment it signals. While these mechanisms are difficult to fully disentangle empirically, given that location and industry choices may themselves reflect political values, both likely contribute to the patterns we observe. Our empirical approach allows us to quantify their relative magnitudes.

Overall, we make the following prediction:

Hypothesis 1. Ventures with politically heterogeneous founding teams will be less common than expected from a random assignment of entrepreneurial founders. Conversely, ventures with politically homogeneous founding teams will be more common than expected from a random allocation of entrepreneurial founders.

The consequences of politically heterogeneous founding team compositions

Whether politically homogeneous or heterogeneous founding teams are more beneficial for new ventures taps into a broader debate regarding the consequences of team diversity (Ertug et al., 2022; Harrison & Klein, 2007; Zhou & Rosini, 2015; Reagans, 2013). Theory and empirical evidence present a "double-edged sword" perspective: heterogeneity can bring significant cognitive advantages but also considerable interpersonal and process-related challenges (Van

Knippenberg et al., 2004; Williams & O'Reilly, 1998). First, we review general arguments in this literature and then we situate the discussion in the entrepreneurial founding team setting.

Potential Benefits of Heterogeneity: The Cognitive Resource Argument

From one theoretical vantage, termed the information processing or cognitive resource perspective (Cox & Blake, 1991; Williams & O'Reilly, 1998), team heterogeneity is viewed as an asset. Diverse teams, by virtue of varied backgrounds, experiences, and perspectives, possess a broader pool of knowledge, skills, and problem-solving approaches (Van Knippenberg et al., 2004). This enhanced cognitive repertoire has been linked to increased creativity, innovative solutions, and higher-quality decisions, as differing viewpoints encourage thorough information search, critical evaluation of assumptions, and wider consideration of alternatives (Bantel & Jackson, 1989; Watson et al., 1993). Sundermeier and Mahlert (2023) argue for the advantages of diversity in navigating complex markets, and Brixy, Brunow, and D'Ambrosio (2020) demonstrate that ethnically diverse teams benefit innovations in German startups. Laboratory studies also suggest value in demographic diversity for task groups (see Williams & O'Reilly, 1998, for a review and critique). Research focusing on political diversity suggests that exposure to politically dissimilar views can foster greater awareness of alternatives and enhance political tolerance (Mutz & Mondak, 2006). This suggests that political heterogeneity in founding teams could strengthen innovation capabilities and market responsiveness through improved information processing.

Potential Costs and Challenges of Heterogeneity: The Social Categorization and Conflict Argument

Conversely, theories such as social identity theory (Tajfel & Turner, 1979), social categorization theory (Turner et al., 1987), and the similarity-attraction paradigm (Byrne, 1971) highlight potential downsides of heterogeneity. These perspectives suggest that differences, particularly in salient identities or deep-seated values associated with political affiliation, can trigger in-group/out-group dynamics, reduce interpersonal attraction, and lead to process losses (Williams & O'Reilly, 1998).

Specifically, political heterogeneity may increase the likelihood of task, relationship, and process conflicts (Jehn, 1995; see also Jehn et al., 1999; Pelled, 1996). Relationship conflict is detrimental to team cohesion, trust, communication effectiveness, and overall performance (De Dreu & Weingart, 2003; Jehn, 1995). In environments of heightened societal polarization, these challenges can be amplified, transforming differing viewpoints into sources of animosity rather than constructive debate (Iyengar et al., 2012; McConnell et al., 2018). Such dynamics can manifest as tangible performance costs even at the individual level: Sels & Kovács (2025) provide causal evidence that professional golfers experience performance declines when randomly assigned to play alongside politically dissimilar peers, an effect attributed to heightened anxiety and reduced psychological safety, especially when societal polarization is high.

The practical impact of these challenges is underscored by two exploratory interviews[1] we conducted with entrepreneurs. These discussions revealed that navigating significant political differences can be highly taxing, with one founder describing frequent debates as "tiring" and "energy-draining," making it "hard to focus" despite their cordial nature. Another founder recounted feeling "uncomfortable" expressing certain "leftist ideas" in the presence of a highly religious/conservative co-founder, suggesting that heterogeneity might suppress rather than encourage the sharing of diverse perspectives. Furthermore, value disagreements rooted in political outlooks such as concerns over a partner's tax non-compliance were cited by an interviewee as contributing to their decision to exit their venture, illustrating how deep-seated differences can

[1] We aimed at conducting more interviews but even though we tried hard and advertised through our LinkedIn networks and on entrepreneurship mailing lists, people were reluctant to talk to us about political conflicts in their ventures – this just illustrates how important politics is in the current polarized environment.

impact team stability. Consistent with these qualitative insights, survey data indicates that "politics, the election, and ideological issues" are major drivers of discord among founders (Kung Group, 2020), and that political disagreements can hamper communication, trust (Polarization Research Lab, 2024), and team performance in other professional settings (Evans et al., 2020). Studies have also shown that skill heterogeneity can hamper the growth of private companies (Ensley et al., 1998), and heterogeneity in age, tenure, and status can increase top management team turnover (Wiersema & Bird, 1993).

Synthesizing Perspectives in the Entrepreneurial Context

Thus, the theoretical literature presents competing arguments regarding the performance implications of team heterogeneity. One plausible outcome, supported by evidence that demographic diversity alone does not enhance entrepreneurial team effectiveness once commitment and decision-making processes are considered (Chowdhury, 2005), is no discernible net effect on overall venture performance. Although such findings involve non-political diversity, they suggest surface-level heterogeneity does not automatically improve outcomes. The broader empirical record is similarly mixed: some studies link diversity to innovation and growth, while others find higher turnover or failure. Meta-analytic evidence indicates that task-related diversity modestly improves performance, whereas demographic differences show little consistent impact (Horwitz & Horwitz, 2007). Another meta-analysis reveals only a negligible overall relationship between entrepreneurial team heterogeneity and new-venture performance (Jin et al., 2017). Large-sample research on entrepreneurial dyads further illustrates that demographic and skill diversity can enhance employment growth but lower survival, depending on partner traits (Coad & Timmermans, 2014). Ultimately, the impact likely depends on whether constructive debate and innovation or conflict and impaired cohesion dominate within a given organizational context.

Therefore, the unique pressures of entrepreneurial startups become critical when predicting the effects of political diversity.

Given these inconclusive findings about demographic and skill-based diversity, it is particularly important to consider how political diversity might play out in the distinct context of entrepreneurial startups. While diversity brings both benefits and costs in general organizational settings, the unique conditions faced by startups are likely to amplify certain dynamics. Early-stage ventures typically operate under extreme uncertainty, severe resource constraints, and the necessity of rapid and cohesive decision-making to survive and grow. In such environments, the costs associated with managing ideological differences—such as prolonged debates, diminished trust, and value-based interpersonal conflicts—may become burdensome and harmful (Wasserman, 2012). Indeed, entrepreneurship research consistently highlights the critical role of founding team cohesion, emphasizing strong interpersonal bonds and a shared vision as key predictors of early-stage performance, including faster execution and greater adaptability (Ensley et al., 2002; Klotz et al., 2014). Cohesive founding teams are more likely to achieve early sales growth (Ensley et al., 2002) and overall better performance (Mullen & Copper, 1994). Internal unity and a shared strategic vision significantly enhance a startup's attractiveness to venture capital investors, who place a premium on cohesive teams (Franke et al., 2008). Thus, the unique pressures of startup contexts may tip the balance, making the management of political diversity challenging and costly.

Furthermore, political affiliations serve as proxies for deeper values (e.g., Feldman, 2003; Graham et al., 2009), and misalignment in these core values can be particularly damaging in contexts like startups, where a shared vision and mutual reliance among founders are important (Pearce & Ensley, 2004; Ensley & Pearce, 2001). Founder conflict – especially affective conflict rooted in personal or ideological disagreements – is known to negatively impact new venture revenues and profitability (Ensley, Pearson, & Amason, 2002) and is frequently cited as a leading cause of startup failure (Wasserman, 2012). Unlike established organizations, startups operate with minimal organizational slack (Bourgeois, 1981), and typically lack structured mechanisms for conflict resolution, making them particularly susceptible to disruption from internal disputes. In this environment, the time and energy required to manage political or value-based disagreements represent substantial opportunity costs, diverting critical resources away from essential growth-related tasks (Deeds, DeCarolis, & Coombs, 1999). Such internal friction may exacerbate the classic "liability of newness" (Stinchcombe, 1965) by making it difficult for startups to present a coherent and stable identity to potential investors, customers, and other key stakeholders, ultimately hampering their efforts to build legitimacy and attract necessary external resources (Yang & Aldrich, 2017; Zimmerman & Zeitz, 2002).

While established firms with greater resources and stability may leverage the cognitive benefits of diversity to foster innovation, startups often face immediate survival pressures that constrain their ability to manage internal conflicts and absorb associated costs. The early-stage context, characterized by resource scarcity and the critical need for rapid decision-making, thus amplifies the risks and challenges posed by political dissimilarity. Accordingly, despite recognizing the potential cognitive advantages of diverse perspectives, we argue that the unique context of startups is likely to tip the balance toward the negative consequences associated with political heterogeneity, making these costs more salient and impactful.

Taken together, we make the following prediction:

Hypothesis 2. Ventures with politically heterogeneous founding teams will exhibit lower performance compared to those with politically homogeneous founding teams.

DATA

In this study, we use data from Crunchbase, an online platform collecting entrepreneurial data. Crunchbase provides business information on private and public companies. This includes details on investments, funding, mergers, acquisitions, and key personnel, including founders and leadership. Although Crunchbase has a global reach, it predominantly focuses on North American startups. Crunchbase is a suitable dataset to test our theory because it provides large-scale coverage of new and existing ventures. The Crunchbase dataset has been utilized in existing entrepreneurship literature (e.g., McCarthy et al. 2023).

Our analysis concentrates on U.S.-based firms. We are specifically interested in the founding team's composition. Because we only have data on political affiliation from 2015 onward (more on this later), we analyze a subsample of U.S.-based firms with available founder data and founded between 2015 and 2023. This sample encompasses approximately 94,000 startups and 150,000 founders. Most companies in our data (60%) have a single founder. Since our aim is to analyze the effects of political heterogeneity versus homogeneity among cofounders, we focused our analysis on the subsample of firms with at least two founders, comprising 38,000 startups and approximately 94,000 founders.

We collected data on several variables from Crunchbase, including the founders' names, genders, and U.S. states of residence. We also collected data on the founders' previous founding experience, as studies suggest the positive effects of founding experience on funding and going public (Beckman et al., 2007). We gathered information on each company's operational status to construct a dummy variable, *closure*, indicating whether a startup has ceased operations or remains operational (including being acquired). For accuracy, we verified the active status of the company's website and conducted cross-referencing with PitchBook, which also provides comprehensive data on private and public entities. In rare cases where a company was listed as operating on

Crunchbase, but evidence indicated closure, we adjusted the firm's status accordingly. We constructed a dummy variable *acquired* similarly, indicating whether the startup has been sold to another entity or not (including operational and closed).

Our analysis includes three additional outcome variables: the logarithm of the total amount of funding received, the logarithm of the number of employees, and Crunchbase Rank. For companies lacking funding data on Crunchbase, we assigned a total funding of zero dollars. Crunchbase does not display an exact employee count, instead providing an estimated range (e.g., 1-10 or 501-1000)[2]. To approximate employee count, we calculated the logarithm of the midpoint number of employees as shown by Crunchbase (e.g., ln(5.5) or ln(750.5) in the previous examples). Crunchbase Rank is determined by Crunchbase's proprietary algorithm that takes into account the number of connections of a profile within the platform, the amount of community engagement, funding events, news articles, acquisitions, and more (https://www.similarweb.com/corp/reports/market-intelligence-crunchbase-rank/). Additionally, we incorporated information from Crunchbase, including the state in which the company is headquartered in the U.S., the type of entity (company, investor, or school[3]), the founding year, and the industry sector.

To assess the founders' political attitudes, we used political registration data from L2, a specialized data platform providing information on the voter registration of over 210 million U.S.

[2] The values we use in the analyses are the values that were shared with us by Crunchbase in 2023. The values in our data do not vary over time; therefore, we cannot create a panel data sample from Crunchbase. This is, admittedly, a data limitation.
[3] We examined the organizations labelled as schools and investors and we believe it is reasonable to include these organizations in the analysis, as they appear to operate as profit-oriented organizations. For example, an organization that is labelled as school is "Health Coach Institute" (https://www.crunchbase.com/organization/health-coach-institute) which describes itself as "Health Coach Institute is an online Health and Life Coaching Certification Program that offers students Coaching and Business Techniques as well as a multitude of platforms to connect, share, learn and grow with their founders and with each other."

citizens.[4] This dataset, available for a subscription fee, has been used in multiple publications. For instance, Spenkuch, Teso, and Xu (2023) explored the impact of ideological alignment on turnover and performance in the U.S. federal bureaucracy, highlighting the costs of ideological misalignment within public organizations. Engelberg *et al.* (2022) studied the probability of Republicans and Democrats becoming entrepreneurs during different presidential administrations and several other papers used L2 for analyses to examine the behavior of Democratic or Republican inventors (Engelberg et al., 2023; Fehder et al., 2023) or patent examiners (Raffiee et al., 2023).

To merge the Crunchbase and L2 datasets, we matched founders based on their names and U.S. state addresses.[5] We standardized names by removing punctuation, accents, middle names, and suffixes before conducting exact matching on first and last names. This standardization addresses typographical inconsistencies across data sources (e.g., “Chloë Stevens Sevigny” could appear as “Chloe S. Sevigny” or “Chloe Sevigny”), ensuring consistent matching while maintaining the precision of exact matching. We included only unambiguous matches, excluding instances with more than one match where false positives were likely. This approach enabled us to uniquely identify 30% of the founders in L2.

Removing middle names was especially important for accurate matching. In the Crunchbase dataset, fewer than 7% of founders have middle names or additional identifying information (e.g., suffixes such as Jr., Sr., or Roman numerals), which are typically essential for fuzzy or probabilistic matching techniques to yield valid matches. In contrast, more than 75% of records in the voter registration data contain such identifiers. This mismatch limits the potential effectiveness of fuzzy matching for the majority of founders, as name similarity alone (e.g., slight

[4] We use L2 data current as of July 2023. While political science research indicates that most people do not change their political affiliation throughout their lifetime, some do. Therefore, we also analyzed L2 data from 2015 to identify individuals with changing political attitudes.
[5] In cases lacking founder location information, we used the startup’s U.S. state location, as currently indicated by Crunchbase.

spelling differences) can lead to a high risk of false positives without additional disambiguating information. In addition to the exact matching, we implemented fuzzy matching for the subset of founders with richer identifying information such as middle names or suffixes. We manually verified each fuzzy match and retained only valid matches, adding 54 startups to the matched dataset.

We classified founding teams as politically homogeneous if all founders were registered with the same party and heterogeneous if registered with different parties, considering only Democrats and Republicans. We restricted our sample to firms in which we could identify the political views of *all* founders as either Democrats or Republicans. In addition, we excluded companies with only one founder, IPOs[6], and companies in states where L2 does not provide actual voter registration data but only estimations.[7] We also excluded 1,665 firms founded before 2015 to align with our L2 data available from 2015 onward. Additionally, we dropped 101 startups whose founders changed their political identity since 2015[8].

Given the incompleteness of Crunchbase's gender data, we use L2's data on the genders and birth years of founders.[9] Using the birth years, we calculated the founders' ages at the time of founding. To assess the race of the founders, we followed the approach outlined by Rosenman, Olivella, and Imai (2023), inferring individuals' race from their names. Therefore, we analyzed whether the family name is associated with an indication of race. Due to the inherent uncertainty

[6] We excluded IPOs as there were very few cases (12) in our final sample, making statistical tests infeasible.
[7] These excluded states are Alabama, Hawaii, Minnesota, Missouri, Montana, North Dakota, Vermont, and Wisconsin.
[8] Political science research indicates that most people do not change their political affiliation throughout their lifetime; only 6-9% do (Pew Research Center, 2020). Analyzing L2 data from 2015 shows that switching from being a Republican to a Democrat, or vice versa, is quite rare (8%) among the founders in our data. In additional robustness checks (Table A2, M13), we show that our results hold even if the companies with such political switchers are included.
[9] Even after using L2 data, we could not find gender information for a small subset of founders, we coded the gender heterogeneity of these founding teams as missing. We could not find age information for all founders as well.

in coding names for specific races and ethnicities, we opted for a binary white/non-white distinction[10], with the understanding that future research could expand on this approach.

At the startup level, we created binary variables for gender homogeneity (all male or all female) and racial homogeneity (all White or all non-White) in founding teams. This mirrors the approach we used to categorize political composition. Since age is a continuous variable, we captured age heterogeneity by calculating the standard deviation of the founders' ages (in years).

Table 1 presents summary statistics for the final sample of 1,125 startups founded after 2014. This sample includes 840 firms with homogeneous founding teams and 285 with heterogeneous teams, comprising 1,590 Democrats and 759 Republicans (counting founders only once rather than multiple times if they founded more than one startup leads to 1,545 Democrats and 747 Republicans). Of the 840 politically homogeneous founding teams, 613 teams are all-Democrat (comprising 1271 founders), and 227 teams are all-Republican (comprising 461 founders). In contrast, the 285 politically heterogeneous teams consist of 319 Democratic and 298 Republican founders.

Insert Table 1 about here

Sample representativeness

To assess the representativeness of our final sample, we conducted multiple tests. First, we compared its descriptive statistics with those startups founded after 2014 of the full Crunchbase dataset using simple t-tests. We found that the successfully matched subsample does not substantially differ from the unmatched ones in terms of closure, acquisition, total amount of funding, and number of employees.

[10] We coded a person as White if the most likely predicted ethnicity/race was White.

Second, we compare the gender, race/ethnicity, and age composition of our sample publicly available entrepreneurial datasets in the US: the EPOP database (available from NORC https://gssdataexplorer.norc.org) and the PSED2 database (available at https://www.icpsr.umich.edu/web/ICPSR/studies/37202). We found that our sample overrepresents firms with a higher proportion of male founders (75%) as compared to EPOP's 62% and PSED2's 61%. Regarding the proportion of white founders, the PSED2 provides similar numbers to ours (83% vs 82%), while EPOP's sample shows more race diversity (45%). The average age of founders is roughly similar across our data (45 years) and the EPOP (44 years) and PSED2 (47 years) datasets. To address the representativeness issue along gender and ethnicity, we conducted additional analyses in which we reweighted the observations to align with the nationally representative samples (see the results in the next section).

Third, regarding political composition, our sample skews more Democratic than population-level entrepreneurship patterns. In our sample, 68% of founders are Democrats and 32% are Republicans. Engelberg et al. (2022) find that 6% of Republicans versus 4% of Democrats become entrepreneurs nationally, suggesting Republicans should represent 60% of entrepreneurs (6/(6+4)). Our sample's Democratic skew likely reflects Crunchbase's focus on growth-oriented, venture-backed startups concentrated in coastal regions and tech hubs that lean Democratic. To address this representativeness concern, we conducted weighted analyses using the Engelberg et al. (2022) proportions, and our main findings remain robust.

Finally, we acknowledge that our sample is not representative of US-based entrepreneurs in terms of non-US citizens: this has to be so by the nature of the data because we only have political affiliation data on US citizens. This is a limitation of our study. Overall, extant research shows that about a quarter of the ventures in the US are founded by immigrants (Chodavadia et al., 2024).

RESULTS

Testing the existence of political homophily among venture founders (Hypothesis 1)

We test for political homophily using a multi-step analysis. First, as a simple baseline analogous to prior work measuring homophily, we compare the observed frequency of homogeneous teams to what would be expected from random matching as calculated by multiplying the marginal distribution of the relevant dimensions, an approach used in prior work (e.g., Ruef et al. 2003). Given that 67% of founders in our sample are Democrats and 33% are Republicans, random assignment would yield 55.8% politically homogeneous teams (0.67^2+0.33^2). In our data, however, we observe that 74.7% of two-founder firms are politically homogeneous, a difference of nearly 20 percentage points, providing initial evidence for Hypothesis 1.

While illustrative, this test assumes that founder characteristics are statistically independent and that their relationships are linear. To create a more robust baseline that accounts for correlations between traits like age, gender, industry, location, and political affiliation, and also allowing for non-linear relationships, our second set of tests use permutation tests. By shuffling the complete, observed profiles of founders, this non-parametric approach directly simulates team formation from the actual pool of available entrepreneurs, preserving the complex interdependencies among their characteristics and providing a more conservative and realistic test for homophily.

Permutation tests also allow us to disentangle the two primary drivers of homophily: sorting based on shared contexts (induced homophily) and sorting based on active preference (choice homophily). To distinguish between these, we conducted two sets of permutation tests (each with 1,000 repetitions). First, to model a baseline that accounts for induced homophily, we randomly reallocated founders within the same U.S. state, industry, gender, race, prior experience, and age quartile. This controlled permutation yielded a mean political heterogeneity of 0.30. Second, to establish a baseline for random matching without any constraints, we conducted an uncontrolled permutation across the entire dataset, which resulted in a mean heterogeneity of 0.46. As shown in Figure 1, the actual observed heterogeneity in our data is 0.25. Comparing these three values allows us to quantify different sources of homophily: the difference between the uncontrolled and controlled baselines (0.46 - 0.30 = 0.16) reflects induced homophily, while the remaining difference between the controlled baseline and our observed data (0.30 - 0.25 = 0.05) represents choice homophily. We acknowledge that this distinction is not clear-cut, as geographic and industry contexts may themselves reflect politically-motivated sorting decisions; we discuss the interpretation and implications of these findings in the Discussion section.

Benchmarking political homophily against other demographic dimensions. To contextualize the magnitude of political homophily, we conducted identical permutation-based analyses for all other demographic dimensions in our data: gender, age, race, and founding experience. Figure A1 in the Appendix presents these results. The standardized effect sizes reveal that political affiliation (Cohen's d = 14.59) and age (d = 14.79) exhibit the strongest homophily, with nearly identical magnitudes indicating substantial deviation from random matching. These are followed by founding experience (d = 11.14), race (d = 6.70), and gender (d = 3.83), all of which show significant homophily but with varying effect sizes.

These descriptive patterns indicate that political affiliation is an important dimension of sorting. However, an important limitation of this approach is that permutation-based Cohen's d values lack standard errors, which means we cannot formally test whether the effect sizes differ across dimensions. We therefore interpret these descriptive differences cautiously and avoid drawing rank-order conclusions from permutation tests alone. To address this limitation and to examine homophily patterns while controlling for multiple dimensions simultaneously, we turn to multivariate regression analysis.

Insert Figure 1 about here

Regression analysis of team formation. To examine how political homophily persists after controlling for other dimensions and to enable formal statistical testing, we use logistic regression to model the likelihood of team formation. We created a counterfactual sample by randomly assigning founders within states to founding teams and combined it with our actual data. The dependent variable in our regression, observed, is set to 1 for teams in our dataset and 0 for the randomly constructed counterfactuals. This method allows us to assess the extent to which various types of heterogeneity independently predict the formation of a real team over a synthetic one.

Table 2 presents the results. The models systematically add controls to distinguish the effect of political homophily from other sorting mechanisms. M1 provides a baseline, showing a strong negative association between political heterogeneity and team formation (coefficient = -0.709) when controlling only for company type and industry. In M2, political heterogeneity remains a strong, negative predictor of team formation even after controlling for heterogeneity in gender, age, race, and experience. M3 introduces U.S. state fixed effects to control for induced homophily arising from geographic sorting. Even in this specification, the coefficient for political heterogeneity is negative and significant (coefficient = -0.615), indicating the presence of choice homophily. This core finding is robust across further specifications that reweight the sample to match national entrepreneurship statistics (M4 and M5), dropping the company type "school" (M6), or using company fixed effects (M7). Because our sample is skewed toward Democrats more strongly than population-level patterns—Engelberg et al. (2022) find that 6% of Republicans versus 4% of Democrats become entrepreneurs nationally—we apply weights based on the share of Democratic and Republican founders following Engelberg et al. (2022). M9 combines political weights with demographic EPOP weights. Both models show consistent but slightly lower coefficients for political heterogeneity. We also create different synthetic datasets using varying randomization schemes: uncontrolled (M10), controlled by state and industry (M11), controlled by state, industry, gender, and age (M12), and fully controlled by all demographics (M13). M11-M13 show slightly lower coefficients for political heterogeneity as more controls are added, which is expected as these models progressively account for induced homophily.

Insert Table 2 about here

Table 2 reveals negative and statistically significant coefficients for all forms of heterogeneity examined: politics, gender, age, race, and founding experience, confirming a general preference for homogeneity across multiple characteristics in entrepreneurial team formation. Our analysis reveals that heterogeneity in prior founding experience is the strongest deterrent to team formation (coefficient = -0.682, M2), consistent with Ruef et al.'s (2003) findings on occupational homogeneity. Political heterogeneity shows a strong negative association with team formation (coefficient = -0.597), with a magnitude comparable to race heterogeneity (coefficient = -0.507). Age homophily also shows a strong effect (scaled effect ≈ -0.482), while gender homophily, though present, is less pronounced (coefficient = -0.414).

To formally assess whether political heterogeneity differs in magnitude from other dimensions, we conducted Wald tests comparing the coefficient for political heterogeneity to each of the other four dimensions (gender, age, race, and experience). These tests reveal no statistically significant differences in any of the four comparisons (all $p > 0.10$), indicating that the magnitude of political homophily is statistically indistinguishable from other demographic dimensions despite variation in point estimates. This finding establishes that political affiliation plays an important role in entrepreneurial team formation, operating at magnitudes comparable to other well-studied demographic factors.

Integrating the two analytical approaches. The consistency of findings across permutation tests and regression models strengthens confidence in our results while highlighting their complementary nature. The permutation-based analysis shows that political homophily is substantial (Cohen's d = 14.59), descriptively similar in magnitude to age homophily and descriptively larger than gender, race, or experience homophily, though we cannot formally test the statistical significance of these differences. The regression analysis confirms that political heterogeneity is strongly associated with lower team formation likelihood even after controlling for other demographic factors, and critically, Wald tests reveal no statistically significant differences between political homophily and other dimensions. While the two approaches serve complementary purposes—permutation tests measure total deviation from random expectation while regressions isolate independent associations—both demonstrate that political sorting is an important phenomenon in entrepreneurial team formation. These analyses confirm Hypothesis 1, establishing that entrepreneurs exhibit a robust tendency to form politically homogeneous teams that cannot be explained by geographic, industrial, or other demographic sorting alone.

Robustness checks for Hypothesis 1.

Table A1 in the Appendix provides a set of robustness checks for the logistic regression results on the likelihood of political homophily among startup founders. To assess whether our findings for political heterogeneity are confounded by other forms of diversity, M1–M5 include one heterogeneity variable at a time, while M6–M10 include four out of five heterogeneity variables, systematically dropping one in each specification. All models control for company types, industries (47 categories), and U.S. states (except for M11-M13). To verify that our results are not driven by specific regional dynamics, M11–M13 replicate the full model (from M3 in Table 2) separately for California, New York, and Texas without State dummies. To ensure our findings are not sensitive to the construction of the counterfactual sample, M14 and M15 extend the original analysis by using alternative matching ratios: 1:2 (M14) and 1:4 (M15), instead of the baseline 1:1 matching.

The negative association of political heterogeneity and team formation is robust across all these specifications, providing strong confirmation for Hypothesis 1.

Testing the effect of founding team composition on venture performance (Hypothesis 2)

To explore the relationship between *political heterogeneity* within the founding team of startup *i* and its *performance,* we estimate variations of the following regression model:

$$performance_i = \alpha + \beta\ political\ heterogeneity_i + \delta X_i + \varepsilon_i$$

Performance is measured in the main model by the probability of *closure*, and in alternative models, it is assessed by the (log) total amount of *funding*, the (log) number of *employees*, the probability of the startup being *acquired,* and CrunchBase *rank* (*CB rank*). The primary independent variable, *political heterogeneity* is a dummy variable that takes the value 1 if the founding team is politically heterogeneous, i.e., composed of at least one Democrat and one Republican, and 0 otherwise. The regression also includes control variables at the company level (X_i), such as the number of founders, a continuous version of the founding year[11], the founders' heterogeneity along gender, age at founding, race, and founding experience as well as fixed effects for the company type, industry sectors, and states. ε represents the idiosyncratic error term. Due to factors such as missing gender or age information, multicollinearity, fixed effects, or separation issues in categorical variables (e.g., when certain covariates perfectly predict the outcome or when there is no within-group variation), the sample size varies across analyses. While our full analytic sample includes 1,125 startups and our selection sample includes 2,250 startups (1,125 observed plus 1,125 randomized control), some models exclude observations based on these data or model-specific constraints.

[11] The results are robust to including year dummies instead.

We first present results only on the second stage (i.e., the association between political heterogeneity and performance), then we present a series of models which also take into account the selection effects documented in Hypothesis 1. In our primary analyses, we focus on the effect of team political heterogeneity on *closure*, i.e., whether the company is still in existence or has closed down.

Figure 2 descriptively displays the average likelihood of *closure* for founding teams that are either politically heterogeneous or homogeneous[12]. The figure shows that startups with politically heterogeneous founding teams have a higher likelihood of *closure*. Table 3 presents the results of regression analyses examining the association of *political heterogeneity* and the likelihood of *closure*, controlling for founding team heterogeneity in terms of gender, age at founding, race, and founding experience, as well as fixed effects for the company type, industries, and states, using robust standard errors. M1 is a linear probability (OLS) model, and the results indicate that startups with politically heterogeneous founding teams have a 7-percentage point higher likelihood of being closed. In M2 and M3, we employ logistic and probit regressions, respectively, acknowledging that the dependent variable *closure* is binary. The results show robust support for H2.

Insert Figure 2 and Table 3 about here

Appendix Table A2 presents additional robustness checks for the relationship between political heterogeneity among founders and the likelihood of startup closure. To address potential sample representativeness concerns, M1-M4 estimate the association of political heterogeneity and

[12] This figure is based on the raw averages, without any controls.

closure using weighted regressions based on national founder characteristics: M1 uses OLS with EPOP weights, M2 replicates M1 using weights from PSED2 data, and M3 as well as M4 apply logistic and probit models, respectively, using EPOP weights. M5-M13 test robustness to changes in sample restrictions, control variables, and fixed effects: M5 includes only companies labeled as "company" type (addressing concerns about non-company entities), M6 excludes startups in the top distribution of funding and employment (ensuring outliers do not drive results), M7 includes startups from states where political affiliation is inferred (relaxing data quality restrictions), M8 includes startups with incomplete founder political affiliation data (testing sensitivity to our sample construction), M9 omits all fixed effects (verifying that results hold without absorbing state/industry variation), M10 includes only political heterogeneity, excluding other founder heterogeneity variables (isolating the political effect), M11 replaces the continuous variable for number of founders with fixed effects (accounting for nonlinear team size effects), M12 replaces the continuous founding year variable with fixed effects (controlling for time-specific shocks), M13 adds teams as politically heterogeneous if at least one founder changed party affiliation between 2015 and 2023 (relaxing our political stability assumption). Robust standard errors are shown in parentheses.

Table A3 in the Appendix provides further robustness checks on the association of political heterogeneity with closure, applying additional controls and state-level subsamples to the main regression model from Table 3 (M1). M1-M5 introduce new control variables to address potential confounds and alternative explanations: the logarithm of total funding received (in USD) and the logarithm of the average number of employees (M1, controlling for venture scale and resources), the number of founders affiliated as Democrats (M2, accounting for partisan composition beyond heterogeneity), a binary indicator for whether at least one founder is a Democrat (M3, testing for partisan direction effects), a binary variable for whether founding partners are family members

(M4, ruling out kinship ties as a confounder), and a binary variable whether the founders were former business partners (M5, accounting for prior collaboration history). To verify regional generalizability, M6-M8 replicate the main model separately for startups located in California, New York, and Texas, respectively. The results remain robust in these specifications as well. In M9, we further matched our founder sample to Federal Election Commission (FEC) records and identified 374 founders (16% of our sample) who made political donations during the study period. We tested whether the negative association between political heterogeneity and closure is stronger in teams where a higher proportion of founders made political donations (continuous measure of team-level political engagement), reasoning that donation behavior might proxy for ideological intensity. While the main association remains unchanged, the interaction term is not statistically significant (coefficient = -0.086, SE = 0.095), suggesting that the associations of political heterogeneity with closure do not vary systematically with donation behavior.

The results from our main performance model (Table 3) and the subsequent robustness checks (Tables A2 and A3) reveal a consistent pattern for startup performance: only political heterogeneity emerges as a regular and statistically significant predictor of closure, while other forms of diversity are not. This finding is particularly striking in light of our earlier analysis of team formation (Table 2), where differences in experience, politics, age, race, and gender all significantly reduced the likelihood of a team forming. The present results suggest that, once a team is formed, only value conflicts rooted in political differences are persistently negatively associated with a startup's chances of survival. This pattern is consistent with prior evidence that demographics do not necessarily harm or help venture performance once deeper processes and commitment are accounted for (Chowdhury, 2005; Coad & Timmermans, 2014).

It is worth noting that survival is not necessarily equivalent to success. Ventures that persist without growth may represent inefficient deployment of founder human capital. However, in the context of venture-backed startups in our sample, closure typically represents failure to achieve product-market fit or investor expectations rather than successful lifestyle businesses chosen by founders.

Matching and two-stage models

Our finding that team formation is itself characterized by homophily introduces a potential selection bias when estimating the relationship between political heterogeneity and performance. To address this, we employ three propensity score-based estimation strategies. These models address the central identification challenge in our study: teams with political heterogeneity might differ systematically in other measurable characteristics (e.g., industry, founder experience, geographic location) that also affect performance. By estimating the probability (propensity score) that a team is politically heterogeneous based on these covariates, we can create more comparable groups through matching, weighting, or combined approaches.

First, Propensity Score Matching (PSM) addresses selection on observables by creating matched pairs of politically homogeneous and heterogeneous teams that are similar on all measured characteristics (gender, race, age, experience, industry, state, founding year, team size). We use nearest-neighbor matching with replacement, which allows each control observation to be matched to multiple treated observations when appropriate. The approach relies on the conditional independence assumption: after conditioning on observables, political composition is as good as random. PSM is particularly valuable when there is limited overlap in covariate distributions, as it restricts analysis to the region of common support where comparable teams exist.

Second, Inverse Probability of Treatment Weighting (IPTW) also addresses selection on observables but uses weighting rather than matching. Each observation is weighted by the inverse of its probability of having its observed political composition (homogeneous or heterogeneous). This reweights the sample to create balance on covariates between the two groups. Unlike PSM, IPTW retains all observations rather than discarding those without good matches, making it more efficient when overlap in covariate distributions is adequate. The approach assumes correct specification of the propensity score model and sufficient overlap in the covariate distributions.

Third, Doubly Robust Estimation (DRE) combines both outcome regression and propensity score weighting. This approach is "doubly robust" because it provides consistent estimates if either the outcome model or the propensity score model is correctly specified, though not necessarily both. This provides additional protection against model misspecification, which is particularly valuable given the complexity of entrepreneurial team formation and performance.

These three approaches provide complementary evidence. The consistency of results across all three strengthens confidence that our findings are not artifacts of any particular modeling choice or assumption. All three methods rely on the assumption of selection on observables – that is, once we condition on the rich set of covariates in our models (demographics, experience, industry, location, founding year), there are no remaining unobserved confounders that both determine political composition and directly affect performance. While we cannot test this assumption directly, the robustness of our findings across multiple specifications and their alignment with theoretical expectations and qualitative evidence provide support for our conclusions.

Table 4 presents results from these strategies. M1 applies Propensity Score Matching (PSM) using nearest-neighbor matching with replacement. M2 uses Inverse Probability Weighting (IPW) derived from a logistic regression predicting political heterogeneity. In M1 and M2, the propensity score is estimated using covariates including gender, race, age, founding experience heterogeneity, industry, state, company type, founding year, and number of founders. These covariates are used only in the first-stage prediction of political heterogeneity. M3 employs a Doubly Robust Estimator (DRE), combining outcome regression and inverse probability weighting. Here, the propensity scores include gender, race, age, founding experience heterogeneity, and industry is included in both the treatment and outcome models. All models control for key covariates including gender, race, age, founding experience heterogeneity, and industry fixed effects. Fixed effects for company types and U.S. states are included for M1 and M2. All models indicate that political heterogeneity of the founder teams is associated with a higher likelihood of closure (M2's coefficient indicates an 8.8 percentage point increase in the likelihood of closure, while M1's coefficient estimate is only marginally significant).

Insert Table 4 about here

In additional analysis, we also estimated a survival model.[13] A graphical representation of the yearly survival rate in Figure 3 indicates that the difference in closure rates increases over time. This is consistent with the proposition that differences associated with value diversity emerge over time (Ye, 2021).[14]

Insert Figure 3 about here

These findings, taken together, provide robust support for Hypothesis 2: politically heterogeneous founding teams are more likely to close down.

Additional heterogeneity tests of Hypothesis 2

Table A4 explores whether the negative associations of political heterogeneity and performance are contingent on the surrounding political environment. We hypothesized that the

[13] To analyze survival rates, we supplemented the missing data on the year a startup closed in Crunchbase with information sourced from PitchBook.
[14] We also conducted a Cox proportional hazards model (yearly level). The results of the analysis reveal that startups with politically heterogeneous founding teams have a 54% higher risk of closure compared to those with politically homogeneous teams.

association between political dissimilarity and closure might be stronger in contexts where political identity is more salient or where cross-partisan collaboration is less common.

First, we tested whether industry political leanings moderate the associations of political heterogeneity and performance. Some industries like technology are known to lean Democratic, while others like oil and gas lean Republican (Broockman et al., 2019), which could exacerbate value conflicts in heterogeneous teams. We used different AI models to measure the political leaning of industries and took the average score across all industries the startup operates in. The results show no significant interaction effects with industry partisanship. Both the selection models (M1-M5) and the closure models (M6-M10) confirm that our main findings are robust; the negative association between political heterogeneity and startup outcomes does not appear to vary significantly across different industry political contexts.

Second, we tested whether the political homogeneity of the state environment moderates team outcomes, using two different measures. In M11, we measured state-level political homogeneity using the standardized absolute difference between Democratic and Republican vote shares, averaged across the 2016 and 2020 presidential elections. In M12, we used an alternative standardized measure, the Herfindahl index. Higher values for both measures reflect more politically homogeneous states. The results reveal a significant positive interaction in both models: politically heterogeneous teams are associated with worse outcomes specifically in politically homogeneous states (coefficient = 0.090 in M11; coefficient = 0.132 in M12). This suggests that the challenges of managing political diversity within founding teams are amplified in environments where cross-partisan collaboration is less common and potentially more norm-violating.

Separately, we also explored whether the association of political heterogeneity with closure has intensified over time, given the documented rise in U.S. political polarization since the 2000s. To capture political polarization in the U.S., we use the Partisan Conflict Index developed by

Azzimonti (2018) (data available from the Federal Reserve Bank of Philadelphia at https://www.philadelphiafed.org/surveys-and-data/macroeconomic-data/partisan-conflict-index). In these additional analyses, we added an interaction term between political heterogeneity and the polarization in the startup's founding year to our main performance model. These analyses yielded no significant interaction effect. It is important to note that this null finding may be a function of our data limitations. Because our dataset provides only a single observation per organization, we cannot track performance changes over time. A conclusive test of this temporal hypothesis would ideally require panel data, which could better capture the effects of an increasingly polarized environment on team dynamics from year to year.

Alternative performance measures

While the ultimate performance measure of a venture is whether it survives, we can also explore the association of political heterogeneity and alternative performance measures such as the Crunchbase rank, whether the company was acquired, the employee count (logged), and the amount of funding in USD (logged) it received. We acknowledge upfront that these measures are cross-sectional (limited to one observation per organization) and interrelated (e.g., employee size is related to funding); therefore, we interpret the following relationships as associations, not as causal tests.

Table 5 shows the results by replicating the main regression specification from Table 3 (M1) for these alternative dependent variables. The results for these measures are more nuanced than the primary finding for venture closure. We find a statistically significant association between political heterogeneity and a higher (worse) Crunchbase Rank (M1). The association with funding is negative and becomes highly significant when analyzing only firms that received funding (M6), though it is only marginally significant when we include firms with zero funding (M5). The association with a lower employee count is weaker, showing marginal significance only when firms with zero employees are excluded (M4). Finally, we find no statistically significant relationship with the likelihood of being acquired (M2).

Table A5 further tests these associations using more robust Propensity Score-based Estimators. The results largely confirm the patterns observed in Table 5. We continue to find evidence that politically heterogeneous teams are associated with worse performance. The negative association with funding is statistically significant across all three estimation methods (PSM, IPW, and DRE). The negative association with employee count remains only marginally significant in most specifications. We also find consistent associations with Crunchbase Rank across all methods, though this proprietary measure should be interpreted cautiously. Consistent with our other models, the estimates for the likelihood of being acquired show no significant effect. Overall, while these secondary tests should be interpreted cautiously, their directional findings are in line with H2's prediction of negative performance outcomes.

Insert Table 5 about here

DISCUSSION

Our study examined the role of political affiliation in both the formation and subsequent performance of entrepreneurial founding teams. Using a novel dataset combining Crunchbase and voter registration records, we documented two primary findings. First, we found strong evidence of political homophily in team formation. This tendency persists even after controlling for demographic characteristics and geographic sorting, with political heterogeneity reducing the likelihood of team formation at a magnitude that is statistically indistinguishable from the magnitudes of age, race, gender, or experience heterogeneity. Second, we found that startups with politically heterogeneous founding teams exhibit a 7-percentage point higher likelihood of closure.

While our secondary analyses suggest these teams may also receive less funding, have fewer employees, and achieve lower organizational rankings, these associations were less robust than the primary survival outcome.

Theoretical Contributions and Implications

Our findings make several contributions to the entrepreneurship and organizational literature. First, we extend the entrepreneurial homophily literature by establishing political affiliation as a powerful sorting mechanism in team formation. While prior work has extensively documented homophily along demographic dimensions (e.g., Ruef et al., 2003), political ideology has remained a blind spot despite its increasing salience in American society. Our analysis in Table 2 shows that political alignment plays an important role in team formation, with an effect size on the same order as other demographic dimensions, suggesting that value-based sorting may be as important as surface-level demographic matching in entrepreneurial contexts. Our permutation tests allow us to distinguish between choice homophily (active preference for politically similar partners) and induced homophily (sorting due to contextual factors like geographic concentration). We find evidence for both mechanisms: politically homogeneous teams form substantially more often than would be expected under random matching, whereas politically heterogeneous teams form substantially less often. Even after controlling for geographic sorting within states, industries, and demographic characteristics, we observe significant choice homophily, indicating that founders actively seek politically similar partners beyond what structural factors would predict.

However, we acknowledge that the distinction between induced and choice homophily is inherently ambiguous in this context. Geographic and industry contexts are neither purely exogenous nor purely politically-motivated choices. Political values may influence where people live and which industries they enter, meaning contextual sorting likely contains both politically-motivated and non-political components that we cannot empirically disentangle with our observational data. This ambiguity does not undermine our core findings. Political homophily remains substantial regardless of its precise source, and our regression results confirm that political sorting persists even after stringent controls. Moreover, for our performance analyses, the mechanisms of conflict and coordination challenges operate whether heterogeneity arose from deliberate cross-partisan partnering or circumstantial mixing within politically diverse contexts. Practically, this suggests that interventions to increase political diversity in founding teams must address both individual partner selection decisions and the broader structural sorting into politically homogeneous industries and regions.

Second, we contribute to the team diversity literature. The broader literature presents a “double-edged sword” perspective on team diversity, recognizing both cognitive benefits and potential process costs (Van Knippenberg et al., 2004). Our findings suggest that in early-stage ventures, value-based diversity is associated with higher closure rates, consistent with process costs dominating cognitive benefits. This pattern has two plausible interpretations. One interpretation is that political heterogeneity may primarily affect survival rather than conditional performance, with coordination costs pushing marginal ventures toward closure while not materially affecting those that overcome initial challenges. Alternatively, the survival finding might reflect persistence rather than performance: politically homogeneous teams may persist longer because founders find the partnership more pleasant, potentially representing inefficient deployment of human capital if they continue with ventures that should rationally be abandoned. Without data on founder opportunity costs and outside options, we cannot fully adjudicate between these interpretations. The pattern is most consistent with political heterogeneity being associated with conditions linked to higher failure rates, aligning with Wasserman’s (2012) finding that 65% of startups fail due to co-founder conflict. However, future research with revenue, profitability, and founder satisfaction data could help distinguish between these mechanisms.

Third, we advance entrepreneurship theory by highlighting how the "liability of newness" (Stinchcombe, 1965) may amplify the challenges of managing ideological differences. As Yang and Aldrich (2017) argued, founding teams with strong social connections and common interests exhibit greater internal coherence, helping them project a consistent identity to external stakeholders and overcome initial legitimacy challenges. The lack of established routines, formal conflict resolution mechanisms, and organizational slack that characterizes early-stage ventures appears to make them vulnerable to the friction associated with value misalignment. Our exploratory interviews provided illustrations of these mechanisms, with founders describing political disagreements as "energy-draining" and contributing to exit decisions.

Fourth, our findings contribute to understanding the distinction between task conflict and affective conflict in entrepreneurial teams. While task conflict about strategic decisions can be productive if managed well, the deep-level value differences reflected in political affiliation may be prone to escalating into affective conflict—personal, emotion-laden disputes that undermine team functioning (Jehn, 1995), especially in resource-constrained startup environments where founders lack the time and mechanisms to process disagreements constructively. Our survival analysis (Figure 3) shows that closure rate differences between politically homogeneous and heterogeneous teams increase over time, suggesting that while resource constraints make startups vulnerable to political friction, the cumulative effects of unresolved value conflicts compound as ventures face successive decisions and stressors.

Implications for Practice

Our findings have important implications for multiple stakeholders in the entrepreneurial ecosystem. For entrepreneurs, our results highlight the importance of explicitly discussing values and worldviews during team formation. While political affiliation reflects deeper value systems (Graham et al., 2009), direct conversations about core values, decision-making principles, and long-term vision may help founders assess compatibility beyond political labels. This is important given that startup teams operate with low or no organizational slack and every hour spent managing internal conflict detracts from critical activities like product development and customer acquisition.

For investors and accelerators, our findings suggest that team assessment should extend beyond traditional metrics of complementary skills and prior experience to include evaluation of value alignment and the team's capacity to manage ideological differences. Given that cohesive teams are better able to react faster, are more flexible, have superior problem-solving techniques, and can work more efficiently (Smith et al., 1994), investors may benefit from explicitly evaluating team cohesion as a risk factor. However, this presents a delicate balance. While our results indicate that political homogeneity may enhance early-stage survival, excessive homogeneity could limit cognitive diversity and market insight, particularly for ventures targeting diverse customer bases, and this may hurt a venture's innovativeness and performance in the long term.

For entrepreneurship educators and ecosystem builders, our findings point to the need for developing interventions that help diverse teams build ideological bridging capacity, the ability to maintain cohesion and effective decision-making despite value differences. This might include training in distinguishing between task conflict and affective conflict, developing shared mental models despite different worldviews, and establishing clear decision-making protocols that prevent value disagreements from paralyzing the venture.

Boundary Conditions and Contextual Factors

It is important to recognize that our findings may not generalize to all entrepreneurial contexts. Several boundary conditions likely moderate the relationship between political heterogeneity and venture performance. First, as ventures mature and accumulate resources, they may develop greater capacity to manage internal diversity. The acquisition of organizational slack, surplus resources that buffer against shocks, may allow later-stage companies to invest in conflict resolution mechanisms and absorb the process costs of value diversity. Second, in ventures where political values directly relate to the business model (such as sustainable energy, social impact ventures, or defense contractors) political alignment might matter more. Conversely, in purely technical or B2B ventures, political differences might matter less. Third, our findings emerge from the contemporary United States during a period of heightened polarization. In less polarized societies or different time periods, political heterogeneity might carry different implications. Fourth, ventures targeting politically diverse customer bases might benefit from founder diversity that provides market insight, offsetting internal coordination costs. As Sorenson and Audia (2000) note, successful entrepreneurs often leverage their social connections to recruit employees and raise capital. Politically diverse teams might access broader networks despite internal challenges.

Limitations

Our study faces several important limitations that should guide interpretation of the results. First, despite employing multiple analytical approaches, our observational design cannot establish causality. While recent randomized evidence shows negative effects of political dissimilarity on individual performance (Sels & Kovács, 2025), translating these findings to team-level entrepreneurial outcomes requires caution.

Second, our measurement approach has inherent constraints. Political party affiliation, while publicly observable and objectively measured, provides only a coarse proxy for underlying values and ideologies and surface-level indicators may miss important within-category variation in underlying psychological characteristics (Harvey, 2013). We cannot capture within-party heterogeneity, the intensity of political beliefs, or the salience of politics to individual founders. We explored whether political donation behavior (available for 16% of our sample) could proxy for ideological intensity, but found no significant moderation of our main effects, potentially due to limited statistical power or because donation behavior does not cleanly capture intensity. Future research using larger samples or alternative measures of ideological intensity such as social media activity could test whether ideology strength moderates these associations.

Third, our primary performance measure is survival, which is an imperfect proxy for venture success. While closure generally indicates failure in the venture-backed startup context, our secondary performance measures show more variable patterns across specifications. We cannot determine whether politically homogeneous teams survive longer because they perform better or simply because founders are more willing to persist together despite mediocre performance. Distinguishing between these interpretations would require data on revenues, profitability, and founder exit decisions that are not available in our dataset.

Fourth, our sample faces several restrictions that limit generalizability. One notable limitation is that our sample does not include US-based ventures with non-US-citizen founders. Overall, extant research shows that about a quarter of the ventures in the US are founded by immigrants (Chodavadia et al, 2024) – some of them are not naturalized US citizens and thus are not in our sample. Additionally, our reliance on Crunchbase may oversample growth-oriented, funding-seeking ventures while underrepresenting lifestyle businesses or bootstrapped ventures.

Fifth, our sample overrepresents Democratic entrepreneurs compared to population-level patterns. Engelberg et al. (2022) find that 6% of Republicans versus 4% of Democrats become entrepreneurs nationally, yet our sample skews more heavily Democratic. This reflects Crunchbase's focus on growth-oriented, venture-backed startups concentrated in coastal regions and tech hubs that lean Democratic. Republicans may be better represented in other entrepreneurial sectors with less Crunchbase coverage. This Democratic skew has several implications for interpreting our findings. For one, our results may be most applicable to growth-oriented, venture-backed startups rather than the full spectrum of entrepreneurial ventures. Republicans may be overrepresented in other entrepreneurial sectors (e.g., small businesses, lifestyle ventures) not well-captured by Crunchbase. Additionally, our findings should be interpreted in the context of the specific entrepreneurial ecosystem we study: one that is geographically concentrated in Democratic-leaning regions and focused on sectors (technology, innovation) that may themselves have partisan associations. The effects of political heterogeneity might differ in contexts where Republicans are more numerous or where different industries and geographic regions predominate.

Sixth, our cross-sectional performance data prevents us from examining how team dynamics evolve over time. Research on entrepreneurial teams suggests that shared experiences can build trust and shared mental models even among initially diverse members (Carson et al., 2007). We cannot observe whether teams develop mechanisms to manage political differences, whether such differences become more or less salient as ventures face different challenges, or whether the associations we document strengthen or weaken across venture life stages. Moreover, our design cannot capture how the external political environment might interact with internal team dynamics. Engelberg et al. (2022) demonstrate that political regime changes affect which individuals enter entrepreneurship, with Republicans and Democrats showing different entrepreneurial entry rates depending on which party controls the presidency. Our data spanning 2015-2023 cannot distinguish whether political heterogeneity's effects vary across these regime changes or during periods of heightened polarization.

Seventh, we lack direct observation of the mechanisms linking political heterogeneity to venture outcomes. While the broader literature distinguishes between process conflict (disagreements about how work gets done) and relationship conflict (interpersonal tensions), we cannot determine which type of conflict political differences generate or how they escalate. While our interviews suggest how political differences manifest as interpersonal challenges, we cannot systematically document the pathways from political heterogeneity to venture failure.

Future Research Directions

Our findings open several promising avenues for future research. Researchers should pursue in-depth qualitative studies of politically diverse founding teams to examine the mechanisms linking ideological differences to venture outcomes. Interviews and observational qualitative studies could document whether political disagreements manifest primarily as task conflicts that escalate to affective conflicts, or whether they directly lead to relationship tensions. Here, we reiterate that collecting such qualitative data may be especially delicate: our own efforts to interview founders whose ventures faced internal political conflict was hampered by the fact that founders were reluctant to talk to us about ideological and political disagreements within their firms.

Future work should also develop more nuanced measures of ideological alignment beyond party affiliation. Multi-dimensional assessments of values, utilizing frameworks from moral foundations theory (Graham et al., 2009) or cultural cognition research, could provide richer understanding of which specific value differences matter most for entrepreneurial teams. Research might also examine whether experience with effectuation versus causation decision-making logics moderates teams' ability to navigate value differences (Sarasvathy, 2001).

Longitudinal research designs could address temporality questions: Do the effects of political diversity change as ventures mature? As ventures develop organizational routines and accumulate slack resources, do they become better able to absorb the process costs of ideological diversity? Under what conditions might the cognitive benefits of ideological diversity outweigh process costs? Panel data could examine whether political heterogeneity's effects vary by presidential administration (testing if effects differ when founders' co-partisans are in versus out of power), whether effects intensify as societal polarization increases, and whether teams develop mechanisms to manage political differences as ventures mature.

Research should also examine how political diversity interacts with other forms of diversity. For instance, do gender-diverse teams experience different effects from political diversity than all-male or all-female teams? How does the intersection of demographic and ideological diversity affect resource mobilization processes?

Research across different institutional and cultural contexts could establish boundary conditions for our findings. Do similar patterns emerge in less polarized societies? How do different national cultures' approaches to political disagreement moderate these effects? Such research could help separate the effects of value diversity per se from the specific contemporary US manifestation of political polarization.

Finally, intervention research could test strategies for helping ideologically diverse teams succeed. Building on insights from research on shared leadership and psychological safety in entrepreneurial teams, researchers could develop and test interventions that help teams establish "rules of engagement" for navigating value-based disagreements while maintaining the trust necessary for effective collaboration.

Broader Societal Implications

Our findings contribute to broader concerns about political polarization's effects on economic and social life. The tendency toward political homophily in team formation suggests that entrepreneurship may become another domain of partisan sorting, limiting cross-political understanding and collaboration. This sorting may have particular implications for access to entrepreneurial opportunities, as social networks play a crucial role in resource mobilization for new ventures (Sorenson & Stuart, 2001). Increased ideological homogeneity due to political sorting might reduce the breakthrough innovations that often emerge from combining diverse perspectives. If entrepreneurs select co-founders from similar ideological backgrounds, ventures may miss opportunities to recognize and serve diverse market needs or to approach problems from different angles. However, our results also suggest a tension: while diverse teams might generate more innovative ideas, homogeneous teams may be better positioned to execute on those ideas given the resource constraints and legitimacy challenges facing new ventures.

For social cohesion, the entrepreneurial sector has served as a domain where individuals from different backgrounds could collaborate around shared commercial goals. If political sorting reduces such collaboration, it may eliminate one more avenue for building bridges across ideological divides. Yet paradoxically, the high failure rates we document for politically diverse teams might reinforce stereotypes and further discourage cross-political collaboration.

For economic opportunity, our findings raise questions about access to entrepreneurial networks and resources for political minorities in different regions or industries. If team formation increasingly occurs along political lines, individuals may face reduced entrepreneurial opportunities based on their political beliefs and geographic location. This could exacerbate regional inequalities in entrepreneurial activity and economic dynamism.

CONCLUSION

This study provides the first large-scale evidence that political ideology shapes both entrepreneurial team formation and venture performance. In documenting strong political homophily and its association with increased venture failure, we highlight how contemporary political polarization extends into the entrepreneurial domain. Our findings align with broader research showing that 65% of startups fail due to founder conflict (Wasserman, 2012) and that cohesive teams with shared values are better equipped to overcome the liability of newness inherent in new ventures (Yang & Aldrich, 2017).

While our correlational evidence cannot establish causation, the consistency of associations across multiple specifications and their alignment with qualitative insights support the view that value alignment, as proxied by political affiliation, plays an important role in venture formation and success. These associations indicate that in resource-constrained entrepreneurial environments, the process costs of managing deep-level diversity may outweigh potential cognitive benefits.

Our findings should not be interpreted as an endorsement of political homogeneity in founding teams. Rather, they highlight the need for greater attention to how entrepreneurial teams can successfully navigate deep-level diversity. Future research should examine not just whether diverse teams fail more often, but under what conditions they succeed, potentially identifying best practices for maintaining the psychological safety and shared purpose necessary for effective collaboration despite ideological differences.

As the US and other societies grapple with political polarization, the entrepreneurial sector faces a choice: accept increasing ideological segregation with its potential costs for innovation and social cohesion, or develop new approaches to building successful ventures across political lines. The stakes extend beyond individual venture success to encompass broader questions about innovation, economic opportunity, and whether entrepreneurship can serve as a domain for productive collaboration across ideological divides.

We hope this research stimulates both scholarly investigation and practical experimentation to understand and address the challenges of ideological diversity in entrepreneurial teams. By clarifying the role of political ideology in shaping entrepreneurial outcomes, we hope to inform efforts to build a more inclusive and innovative entrepreneurial ecosystem, one that harnesses the benefits of diverse perspectives while providing the cohesion necessary for venture success.

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TABLES

TABLE 1
Summary Statistics for the Crunchbase data (organizational level, at latest observation)

Variables	**Obs.**	**Mean**	**SD**	**Min**	**Max**
Closure	1,125	0.123	0.328	0	1
Acquired	1,125	0.066	0.248	0	1
Funding (in USD)	1,125	11.1 mil	114 mil	0	3.6 bil
Logarithm of Funding (in USD)	1,125	6.992	7.465	0	22.0
Number of employees	1,125	30.68	136.51	0	3,001
Logarithm of number of employees	1,125	2.558	1.128	0	8.007
Political heterogeneity of founders (1: heterog., 0: homog.)	1,125	0.253	0.435	0	1
Political heterogeneous	285				
Political homogeneous	840				
Gender heterogeneity of founders	1,083	0.315	0.465	0	1
Age heterogeneity of founders in years	1,104+	6.216	6.884	0	39.6
Experience heterogeneity of founders	1,125+	0.229	0.421	0	1
Race heterogeneity of founders	1,125	0.208	0.406	0	1
Number of founders	1,125	2.088	0.302	2	4
Founding year	1,125			2015	2023
Company type	1,125			1	3
Company	1,071				
Investor	53				
School	1				

Notes. Dummies for the industries, not shown in the table, encompass: administrative services, advertising, agriculture and farming, apps, artificial intelligence, biotechnology, clothing and apparel, commerce and shopping, community and lifestyle, consumer electronics, consumer goods, content and publishing, data and analytics, design, education, energy, events, financial services, food and beverage, gaming, government and military, hardware, health care, information technology, internet services, lending and investments, manufacturing, media and entertainment, messaging and telecommunications, mobile, music and audio, natural resources, navigation and mapping, other, payments, platforms, privacy and security, professional services, real estate, sales and marketing, science and engineering, software, sports, sustainability, transportation, travel and tourism, as well as video. + The number of observations is slightly lower for these variables because for a small set of firms we did not have the gender and/or age information of all founders.

TABLE 2
Regression results for the existence of political homophily among startup founders

	M1	M2	M3	M4	M5	M6	M7	M8	M9	M10	M11	M12	M13
DV	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed
Type of model	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit
Political heterogeneity of founders	-0.709	-0.597	-0.615	-0.689	-0.652	-0.615	-0.612	-0.380	-0.406	-0.855	-0.397	-0.383	-0.322
	(0.092)	(0.102)	(0.104)	(0.131)	(0.107)	(0.104)	(0.110)	(0.110)	(0.133)	(0.104)	(0.103)	(0.102)	(0.102)
Gender heterogeneity of founders		-0.414	-0.420	-0.468	-0.444	-0.417	-0.451	-0.459	-0.495	-0.378	-0.262	-0.025	-0.013
		(0.098)	(0.099)	(0.125)	(0.101)	(0.099)	(0.108)	(0.107)	(0.125)	(0.100)	(0.099)	(0.099)	(0.099)
Age heterogeneity of founders		-0.070	-0.072	-0.075	-0.076	-0.072	-0.072	-0.073	-0.072	-0.073	-0.046	-0.005	-0.005
		(0.008)	(0.008)	(0.010)	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)
Race heterogeneity of founders		-0.507	-0.519	-0.751	-0.553	-0.515	-0.582	-0.596	-0.821	-0.450	-0.324	-0.269	0.004
		(0.107)	(0.109)	(0.123)	(0.112)	(0.109)	(0.120)	(0.119)	(0.127)	(0.111)	(0.110)	(0.108)	(0.112)
Experience heterogeneity of founders		-0.682	-0.703	-0.720	-0.709	-0.700	-0.776	-0.681	-0.750	-0.833	-0.578	-0.421	0.026
		(0.102)	(0.104)	(0.133)	(0.108)	(0.104)	(0.117)	(0.112)	(0.132)	(0.105)	(0.102)	(0.102)	(0.107)
Dummy Variables													
Company types (all)	Yes	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Company types (without School)	No	No	No	No	No	Yes	No	No	No	No	No	No	No
Industries (47 categories)	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
States	No	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Companies	No	No	No	No	No	No	Yes	No	No	No	No	No	No
Weights (EPOP)	No	No	No	Yes	No	No	No	No	Yes	No	No	No	No
Weights (PSED2)	No	No	No	No	Yes	No	No	No	No	No	No	No	No
Weights (Engelberg)	No	No	No	No	No	No	No	Yes	Yes	No	No	No	No
Constant	0.261	1.282	1.395	1.375	1.424	1.393		1.146	1.071	1.180	0.708	0.353	0.257
	(0.109)	(0.142)	(0.870)	(0.962)	(0.949)	(0.869)		(0.942)	(0.990)	(0.959)	(0.875)	(0.850)	(0.851)
N	2,250	2,131	2,131	2,131	2,131	2,129	2,024	2,131	2,131	2,130	2,131	2,133	2,132

Notes. The table presents an analysis utilizing the original dataset and a second dataset, where founders from the original dataset are randomly assigned to the startups. The dummy variable ‘observed’ is set to one for the original data and zero for the randomized data. The analysis involves running a logistic regression of ‘observed’ on group heterogeneity variables, including political (all models), gender, age, race, and experience composition, along with fixed effects for company type (M1-M13; M6 without type school), industry dummies (all models, except M7), and states (M3-M6, M8-M13). M4 and M5 reweight startups to reflect national founder characteristics based on EPOP (M4) or PSED2 (M5). M7 includes only company fixed effects (thereby omitting several observations). M8 applies weights based on the share of Democratic and Republican founders following Engelberg et al. (2022). M9 combines political weights with demographic EPOP weights. In M10-M13, we create different synthetic datasets: using uncontrolled randomization (M10, founders randomly assigned to teams nationwide) and controlled randomization (M11: founders randomly assigned within state and industry; M12: founders randomly assigned within state/industry/gender/age; M13: founders randomly assigned within state/industry/gender/age/race/experience cells). Robust standard errors are indicated in parentheses. Due to factors such as missing gender or age information, multicollinearity, fixed effects, or separation issues in categorical variables (e.g., when certain covariates perfectly predict the outcome or when there is no within-group variation), the sample size varies across analyses.

TABLE 3
Regression results of closure on politically heterogeneous founding teams

	M1	M2	M3
DV	Closure	Closure	Closure
Type of model	OLS	Logistic	Probit
Political heterogeneity of founders	0.072	0.679	0.372
	(0.028)	(0.233)	(0.123)
Number of founders	-0.004	-0.058	0.006
	(0.036)	(0.342)	(0.184)
Founding year	-0.015	-0.170	-0.094
	(0.005)	(0.051)	(0.026)
Gender heterogeneity of founders	0.013	0.225	0.122
	(0.022)	(0.227)	(0.118)
Age heterogeneity of founders	0.000	0.001	0.000
	(0.002)	(0.014)	(0.008)
Race heterogeneity of founders	-0.018	-0.147	-0.077
	(0.025)	(0.263)	(0.135)
Experience heterogeneity of founders	-0.009	-0.027	-0.006
	(0.025)	(0.251)	(0.129)
Fixed Effects			
Company types (all)	Yes	Yes	Yes
Industries (47 categories)	Yes	Yes	Yes
States	Yes	Yes	Yes
Constant	30.992	341.817	189.481
	(9.636)	(102.948)	(52.757)
N	1,063	960	960

Notes. The table presents regressions of startup performance on the political heterogeneity of founders, incorporating control variables for the startup, such as its founding year and the number of founders. Additional control variables account for the heterogeneity of the founding team in terms of gender, age, race, and experience, along with fixed effects for the company type, industries, and states. M1-M3 use the likelihood of closure as the DV and employ an OLS (M1), logistic (M2), and probit (M3) regression, respectively. M2 and M3 report a smaller sample size because in the logit and probit models observations drop due to collinearity and lacking within-group variation. Robust standard errors are presented in parentheses.

TABLE 4
The Association of Political Heterogeneity and Startup Closure: Propensity Score-Based Estimators

	M1	**M2**	**M3**
DV	Closure	Closure	Closure
Type of model	PSM	IPW	DRE
Political heterogeneity of founders	0.056	0.088	0.072
	(0.033)	(0.034)	(0.026)
Fixed Effects			
Company types (all)	Yes	Yes	No
Industries (47 categories)	Yes	Yes	Yes
States	Yes	Yes	No
Constant	0.119	0.101	0.124
	(0.024)	(0.011)	(0.034)
N	448	1,021	1,060

Notes. The table presents results from three propensity score-based estimation strategies examining the association between political heterogeneity among founding teams and startup closure. M1 applies Propensity Score Matching (PSM) using nearest-neighbor matching with replacement (sample size is smaller here because for many cases the algorithm didn't identify any good matches). M2 uses Inverse Probability Weighting (IPW) derived from a logistic regression predicting political heterogeneity. M3 employs a Doubly Robust Estimator (DRE), combining outcome regression and inverse probability weighting. All models control for key covariates including gender, race, age, founding experience heterogeneity, and industry fixed effects. Fixed effects for company types and U.S. states are included for M1 and M2. Robust standard errors are shown in parentheses.

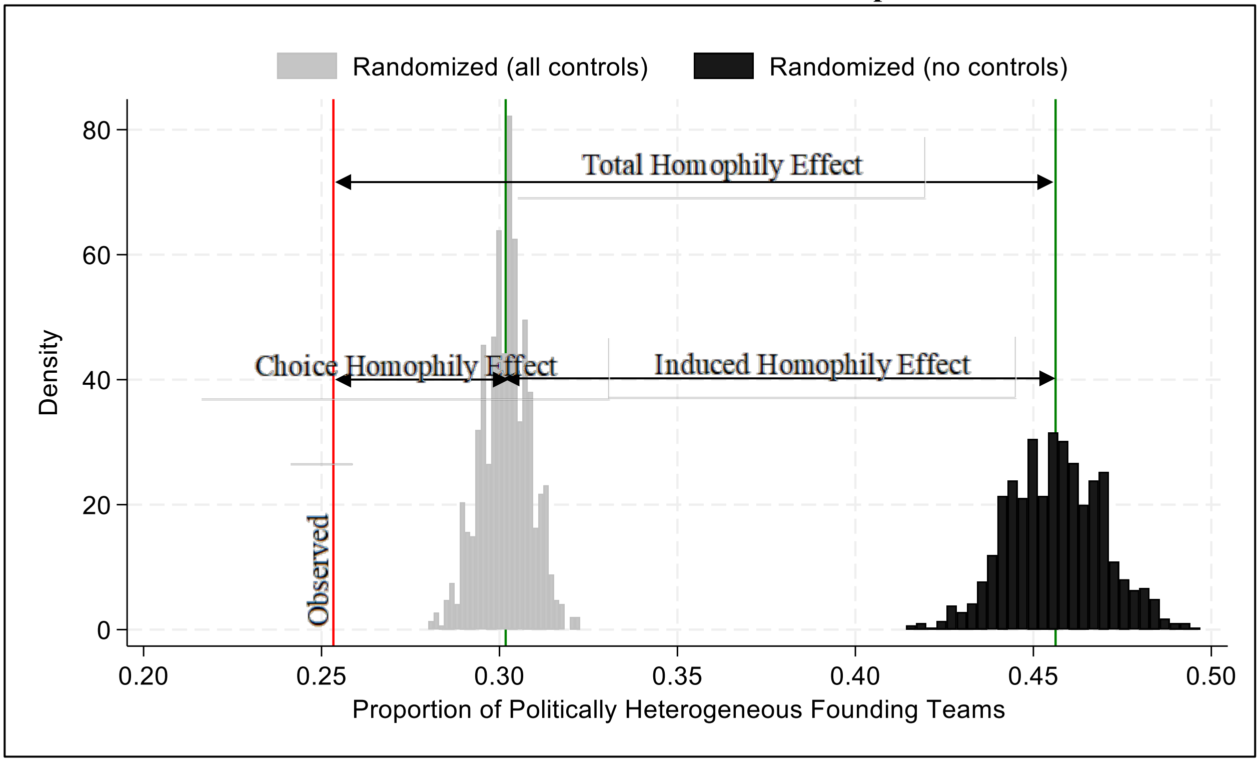
TABLE 5
The Association of Political Heterogeneity and Alternative Startup Performance Outcomes

	M1	**M2**	**M3**	**M4**	**M5**	**M6**
DV	Rank	Acquired	Employees	Employees	Funding	Funding
Type of model	OLS	Logistic	OLS	OLS	OLS	OLS
Political heterogeneity of founders	0.228	-0.148	-0.079	-0.112	-0.790	-0.632
	(0.109)	(0.329)	(0.081)	(0.076)	(0.518)	(0.217)
Number of founders	-0.301	0.265	0.131	0.074	1.000	0.328
	(0.169)	(0.484)	(0.111)	(0.107)	(0.772)	(0.249)
Founding year	-0.036	-0.443	-0.029	-0.036	-0.137	-0.062
	(0.023)	(0.078)	(0.017)	(0.016)	(0.110)	(0.047)
Gender heterogeneity of founders	0.124	-0.255	-0.065	-0.057	-0.554	-0.522
	(0.096)	(0.343)	(0.078)	(0.072)	(0.472)	(0.214)
Age heterogeneity of founders	0.005	0.001	-0.006	0.001	-0.001	0.011
	(0.007)	(0.021)	(0.006)	(0.005)	(0.033)	(0.014)
Race heterogeneity of founders	-0.189	-0.352	0.069	0.099	0.622	0.554
	(0.122)	(0.369)	(0.093)	(0.086)	(0.562)	(0.243)
Experience heterogeneity of founders	-0.242	0.156	-0.113	-0.086	1.056	0.924
	(0.111)	(0.319)	(0.082)	(0.076)	(0.528)	(0.218)
Fixed Effects						
Company types (all)	Yes	Yes	Yes	Yes	Yes	Yes
Industries (47 categories)	Yes	Yes	Yes	Yes	Yes	Yes
States	Yes	Yes	Yes	Yes	Yes	Yes
Constant	86.032	892.210	60.824	75.560	282.426	139.455
	(46.329)	(157.477)	(34.937)	(33.244)	(221.207)	(95.116)
N	1,063	753	1,063	1,022	1,063	509

Notes. The table replicates the main regression specification from Table 3 (M1) using alternative dependent variables related to startup performance. M1 examines the startup's Crunchbase Rank using an OLS regression. M2 estimates the likelihood of being acquired using a logistic regression model (especially losing sample size due to lacking variation in the outcome variable within fixed effect groups). M3-M6 use OLS regressions to model the logarithm of the average number of employees (M3 & M4) and the logarithm of total funding received (in USD) (M5 & M6), respectively. M4 and M6 exclude observations where the DV (Employees or Funding) is zero (leading to smaller sample sizes). All models include controls for the number of founders, founding year, and founder heterogeneity in gender, age, race, and experience. Fixed effects for company types, industries (47 categories), and U.S. states are included in all specifications. Robust standard errors are reported in parentheses.

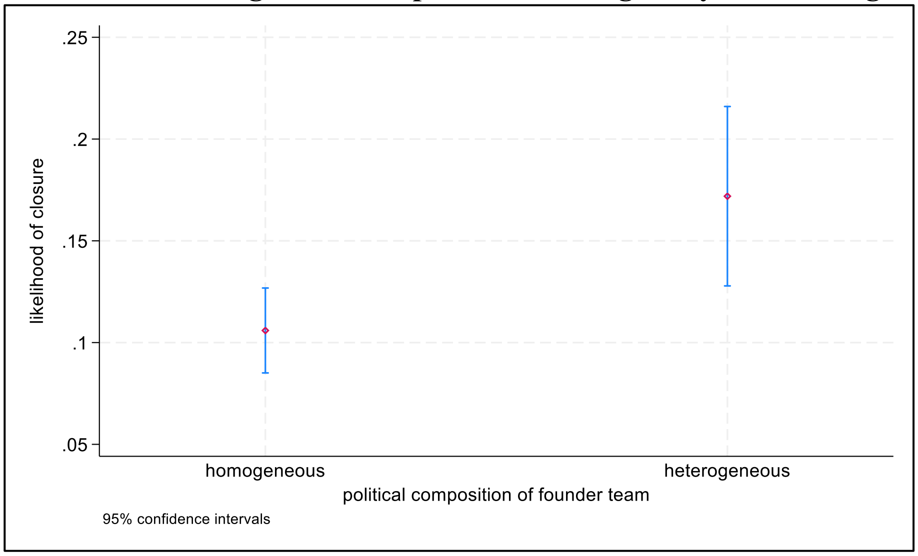
FIGURES

FIGURE 1
Randomization of founders into startups



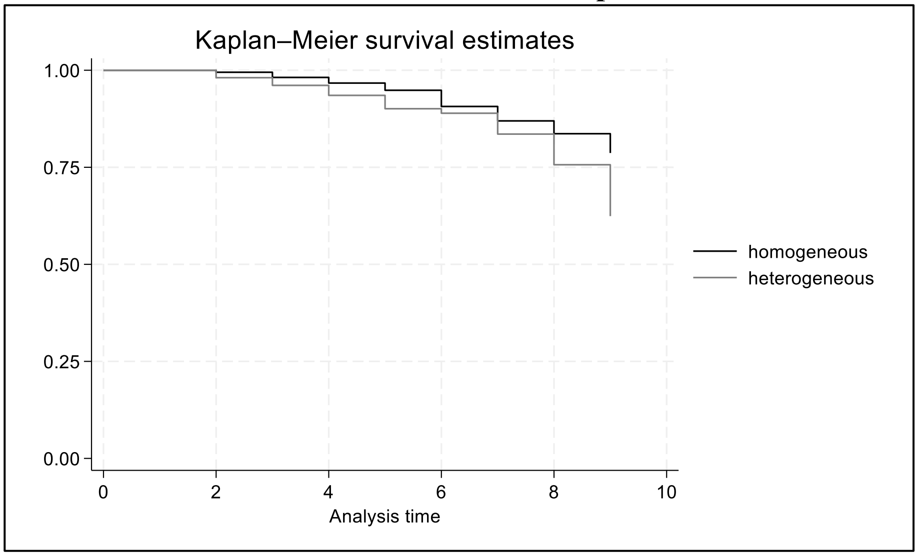
Notes. The figure displays the proportion of politically heterogeneous founding teams, comparing the average based on the actual distribution from Crunchbase data (indicated by the red line on the left) to the distributions generated by permutation tests. These tests involve 1,000 repeated random assignments of startup founders within each U.S. state, their main industry, gender, race, prior founding experience, and age quartile to companies (gray distribution in the middle), and 1,000 repeated random assignments of startup founders without any controls, as represented by the black histogram on the right side.

FIGURE 2
The likelihood of being closed for political heterogeneity of founding teams



Notes. The figure shows the likelihood of startups being closed for politically homogeneous and heterogeneous founding teams.

FIGURE 3
Survival rate of startups



Notes. The figure shows the survival rate for startups with politically homogeneous and politically heterogeneous founding teams in years.

APPENDIX
TABLE A1
Robustness Checks for Logistic Regression Results on the Likelihood of Political Homophily Among Startup Founders

	M1	M2	M3	M4	M5	M6	M7	M8	M9	M10	M11	M12	M13	M14	M15
DV	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed	Observed
Type of model	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit	Logit
Political heterogeneity of founders	-0.735						-0.576	-0.721	-0.636	-0.639	-0.511	-0.518	-1.206	-0.601	-0.647
	(0.094)						(0.102)	(0.099)	(0.103)	(0.102)	(0.218)	(0.360)	(0.363)	(0.090)	(0.083)
Gender heterogeneity of founders		-0.299				-0.386		-0.362	-0.410	-0.422	-0.550	-1.203	-0.237	-0.339	-0.303
		(0.093)				(0.098)		(0.095)	(0.099)	(0.098)	(0.201)	(0.315)	(0.413)	(0.084)	(0.076)
Age heterogeneity of founders			-0.078			-0.076	-0.071		-0.072	-0.074	-0.081	-0.090	-0.082	-0.077	-0.081
			(0.008)			(0.008)	(0.008)		(0.008)	(0.008)	(0.018)	(0.022)	(0.022)	(0.007)	(0.007)
Race heterogeneity of founders				-0.536		-0.546	-0.518	-0.505		-0.510	-0.693	-0.237	-0.829	-0.518	-0.510
				(0.100)		(0.108)	(0.106)	(0.105)		(0.108)	(0.208)	(0.332)	(0.441)	(0.094)	(0.087)
Experience heterogeneity of founders					-0.773	-0.723	-0.710	-0.746	-0.697		-0.793	-0.689	-0.946	-0.737	-0.764
					(0.096)	(0.103)	(0.102)	(0.101)	(0.104)		(0.202)	(0.319)	(0.371)	(0.091)	(0.083)
Dummy Variables															
Company types (all)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industries (47 categories)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
States	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	California	New York	Texas	Yes	Yes
Constant	0.515	0.083	0.718	0.005	0.267	1.008	1.279	0.870	1.409	1.217	1.438	1.999	1.672	0.745	0.169
	(0.878)	(0.883)	(0.811)	(0.838)	(0.773)	(0.807)	(0.802)	(0.905)	(0.867)	(0.908)	(0.292)	(0.532)	(0.531)	(0.732)	(0.656)
N	2,250	2,166	2,210	2,250	2,250	2,131	2,210	2,166	2,131	2,131	565	263	229	3,197	5,327

Notes. This table presents results from logistic regressions estimating the likelihood that a founding team is from the original (non-randomized) dataset (Observed = 1), as opposed to a counterfactual dataset where founders are randomly reassigned to startups (Observed = 0). The regressions examine the association between political, gender, age, race, and experience heterogeneity and the probability of observing political homophily in founding teams. M1–M5 include one heterogeneity variable at a time, while M6–M10 include four out of five heterogeneity variables, systematically dropping one in each specification. All models control for company types, industries (47 categories), and U.S. states (except for M11-M13). M11–M13 replicate the full model (from M3 in Table 2) separately for California, New York, and Texas without State dummies. M14 and M15 extend the original analysis by using alternative matching ratios for the counterfactual sample: 1:2 (M14) and 1:4 (M15), instead of the baseline 1:1 matching. Robust standard errors are reported in parentheses. Due to factors such as missing gender or age information, multicollinearity, fixed effects, or separation issues in categorical variables (e.g., when certain covariates perfectly predict the outcome or when there is no within-group variation), the sample size varies across analyses.

TABLE A2

Robustness Checks on the Association of Political Heterogeneity on Closure: Alternative Specifications, Samples, and Weights

	M1	**M2**	**M3**	**M4**	**M5**	**M6**	**M7**	**M8**	**M9**	**M10**	**M11**	**M12**	**M13**
DV	Closure	Closure	Closure	Closure	Closure	Closure	Closure	Closure	Closure	Closure	Closure	Closure	Closure
Type of model	OLS	OLS	Logistic	Probit	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Political heterogeneity of founders	0.101	0.074	0.913	0.517	0.067	0.073	0.069	0.069	0.072	0.064	0.072	0.074	0.076
	(0.035)	(0.029)	(0.295)	(0.151)	(0.028)	(0.027)	(0.026)	(0.027)	(0.026)	(0.026)	(0.028)	(0.028)	(0.024)
Number of founders	0.025	-0.004	0.141	0.135	-0.007	-0.003	-0.012	-0.008	-0.001	0.003		-0.004	-0.019
	(0.046)	(0.037)	(0.416)	(0.216)	(0.036)	(0.036)	(0.033)	(0.033)	(0.035)	(0.037)		(0.036)	(0.034)
Founding year	-0.018	-0.015	-0.204	-0.110	-0.016	-0.015	-0.015	-0.015	-0.016	-0.016	-0.015		-0.016
	(0.006)	(0.005)	(0.059)	(0.030)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)		(0.005)
Gender heterogeneity of founders	0.036	0.016	0.445	0.212	0.007	0.014	0.010	0.011	0.008		0.012	0.014	0.009
	(0.029)	(0.022)	(0.271)	(0.140)	(0.023)	(0.022)	(0.021)	(0.022)	(0.022)		(0.022)	(0.022)	(0.022)
Age heterogeneity of founders	0.002	0.000	0.022	0.011	0.000	0.000	0.000	0.000	0.001		0.000	0.000	-0.001
	(0.002)	(0.002)	(0.016)	(0.009)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)		(0.002)	(0.002)	(0.001)
Race heterogeneity of founders	-0.032	-0.023	-0.291	-0.199	-0.024	-0.023	-0.018	-0.024	-0.013		-0.018	-0.018	-0.003
	(0.027)	(0.025)	(0.287)	(0.145)	(0.025)	(0.025)	(0.024)	(0.024)	(0.024)		(0.025)	(0.025)	(0.024)
Experience heterogeneity of founders	-0.007	-0.010	0.043	0.009	-0.013	-0.009	-0.004	-0.010	0.001		-0.008	-0.009	0.002
	(0.032)	(0.025)	(0.311)	(0.158)	(0.026)	(0.025)	(0.024)	(0.024)	(0.024)		(0.025)	(0.025)	(0.024)
Fixed Effects													
Company type (all)	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Company type (only companies)	No	No	No	No	Yes	No	No	No	No	No	No	No	No
Industries (47 categories)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
States	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Number of founders	No	No	No	No	No	No	No	No	No	No	Yes	No	No
Founding year	No	No	No	No	No	No	No	No	No	No	No	Yes	No
Weights (EPOP)	Yes	No	Yes	Yes	No	No	No	No	No	No	No	No	No
Weights (PSED)	No	Yes	No	No	No	No	No	No	No	No	No	No	No
Without highest funding	No	No	No	No	No	Yes	No	No	No	No	No	No	No
Without largest number of employees	No	No	No	No	No	Yes	No	No	No	No	No	No	No
Constant	36.328	31.327	411.465	221.690	32.763	30.210	30.393	31.393	31.382	33.313	31.043	0.111	33.177
	(11.403)	(9.751)	(119.946)	(61.000)	(9.966)	(9.655)	(9.336)	(9.443)	(9.230)	(9.341)	(9.622)	(0.076)	(9.278)
N	1,063	1063	960	960	1,011	1,058	1,123	1,086	1,065	1,122	1,063	1,063	1,158

Notes. The table presents a series of robustness checks for the relationship between political heterogeneity among founders and the likelihood of startup closure. All models control for key founder characteristics, including gender, age, race, and experience heterogeneity (except M10), as well as number of founders and founding year. Fixed effects for company types, industries (47 categories), and U.S. states are included where specified. M1-M4 estimate the association of political heterogeneity using weighted regressions based on national founder characteristics: M1 uses OLS with EPOP weights, M2 replicates M1 using weights from PSED2 data, and M3 as well as M4 apply logistic and probit models, respectively, using EPOP weights. M5-M13 test robustness to changes in sample restrictions, control variables, and fixed effects: M5 includes only companies labeled as "company" type, M6 excludes startups in the top distribution of funding and employment, M7 includes startups from states where political affiliation is inferred, M8 includes startups with incomplete founder political affiliation data, M9 omits all fixed effects, M10 includes only political heterogeneity, excluding other founder heterogeneity variables, M11 replaces the continuous variable for number of founders with fixed effects, M12 replaces the continuous founding year variable with fixed effects, M13 adds teams as politically heterogeneous if at least one founder changed party affiliation between 2015 and 2023. Robust standard errors are shown in parentheses. Due to factors such as missing gender or age information, multicollinearity, fixed effects, or separation issues in categorical variables (e.g., when certain covariates perfectly predict the outcome or when there is no within-group variation), the sample size varies across analyses.

TABLE A3
Robustness Checks on the Association of Political Heterogeneity and Closure: Additional Controls and State-Level Subsamples

	M1	M2	M3	M4	M5	M6	M7	M8	M9
DV	Closure	Closure	Closure	Closure	Closure	Closure	Closure	Closure	Closure
Type of model	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Political heterogeneity of founders	0.066	0.077	0.067	0.069	0.072	0.116	0.147	0.117	0.083
	(0.027)	(0.028)	(0.029)	(0.028)	(0.028)	(0.064)	(0.078)	(0.102)	(0.031)
Number of founders	0.005	-0.014	-0.005	-0.003	-0.006	-0.080	0.202	-0.097	-0.002
	(0.036)	(0.039)	(0.036)	(0.036)	(0.037)	(0.068)	(0.137)	(0.172)	(0.036)
Founding year	-0.017	-0.015	-0.015	-0.015	-0.015	-0.033	0.001	-0.012	-0.016
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.011)	(0.018)	(0.016)	(0.005)
Gender heterogeneity of founders	0.008	0.012	0.012	0.020	0.014	0.101	-0.005	-0.040	0.014
	(0.022)	(0.022)	(0.022)	(0.023)	(0.022)	(0.051)	(0.071)	(0.118)	(0.022)
Age heterogeneity of founders	-0.000	0.000	0.000	0.000	0.000	0.001	0.001	-0.005	0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)	(0.006)	(0.002)
Race heterogeneity of founders	-0.013	-0.020	-0.020	-0.023	-0.018	-0.017	-0.063	0.085	-0.018
	(0.025)	(0.025)	(0.025)	(0.026)	(0.025)	(0.052)	(0.068)	(0.118)	(0.025)
Experience heterogeneity of founders	-0.009	-0.009	-0.009	-0.011	-0.006	-0.033	0.059	0.133	-0.008
	(0.024)	(0.025)	(0.025)	(0.025)	(0.025)	(0.052)	(0.063)	(0.075)	(0.025)
Total funding	-0.004								
	(0.002)								
Number of employees	-0.038								
	(0.010)								
Number of democrat founders		0.011							
		(0.014)							
Any Democrat founder			0.019						
			(0.029)						
Founding partners family member				-0.031					
				(0.027)					
Former founding partners					0.028				
					(0.041)				
Donation Share x Political heterogeneity of founders									-0.086
									(0.095)
Donation Share									0.003
									(0.039)
Fixed Effects									
Company types (all)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industries (47 categories)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
States	Yes	Yes	Yes	Yes	Yes	California	New York	Texas	Yes
Constant	34.426	31.359	31.269	31.091	31.199	67.590	-2.728	24.666	31.438
	(9.592)	(9.671)	(9.670)	(9.661)	(9.639)	(22.476)	(36.415)	(32.467)	(9.623)
N	1,063	1,063	1,063	1,063	1,063	280	129	114	1,063

Notes. This table extends the main regression model from Table 3 (M1), which estimates the effect of political heterogeneity among founders on startup closure, by including additional covariates and state-level subsamples. M1-M5 introduce new control variables: logarithm of total funding received (in USD) and logarithm of the average number of employees (M1), number of founders affiliated as Democrats (M2), binary indicator for whether at least one founder is a Democrat (M3), whether founding partners are family members (M4), and whether the founders were former business partners (M5). M6-M8 replicate the main model separately for startups located in California, New York, and Texas, respectively. M9 controls for the donation share of founders among the team members and also includes its interaction with the political heterogeneity of the startup. All models include controls for gender, age, race, and experience heterogeneity, number of founders, and founding year. Fixed effects for company types, industries (47 categories), and states (except state FEs for M6-M8). Robust standard errors are reported in parentheses. Due to factors such as missing gender or age information, multicollinearity, fixed effects, or separation issues in categorical variables (e.g., when certain covariates perfectly predict the outcome or when there is no within-group variation), the sample size varies across analyses.

TABLE A4
Associations of Political Heterogeneity with Founding and Closure Across Politically States, Industries, and State-Level Subsamples

	M1	M2	M3	M4	M5	M6	M7	M8	M9	M10	M11	M12
DV	Observed	Observed	Observed	Observed	Observed	Closure	Closure	Closure	Closure	Closure	Closure	Closure
Type of model	Logit	Logit	Logit	Logit	Logit	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Political heterogeneity of founders	-0.606	-0.527	-0.717	-0.216	-1.369	0.075	0.064	0.171	0.032	0.097	0.079	0.079
	(0.132)	(0.135)	(0.266)	(0.484)	(0.504)	(0.035)	(0.035)	(0.077)	(0.108)	(0.120)	(0.028)	(0.027)
Political heterogeneity of founders * Red State	-0.346					0.047						
	(0.266)					(0.071)						
Political heterogeneity of founders * Purple State	0.338					-0.058						
	(0.276)					(0.065)						
Political heterogeneity of founders * Red Industry		1.038					-0.003					
		(1.030)					(0.087)					
Political heterogeneity of founders * Purple Industry		-0.227	0.620	-0.717	0.317		0.019	-0.168	0.266	0.039		
		(0.211)	(0.454)	(0.702)	(0.741)		(0.053)	(0.111)	(0.167)	(0.193)		
Red Industry		-0.363			-0.843		-0.136		-0.401	-0.222		
		(0.622)			(1.518)		(0.071)		(0.247)	(0.147)		
Purple Industry		0.049	-0.033	-0.637	-0.052		-0.050	-0.074	-0.290	-0.101		
		(0.185)	(0.404)	(0.639)	(0.646)		(0.040)	(0.096)	(0.112)	(0.113)		
Political heterogeneity * std. political state homogeneity (abs. vote diff)											0.090	
											(0.032)	
Political heterogeneity * std. political state homogeneity (Herfindahl index)												0.132
												(0.023)
Number of founders						-0.006	-0.002	-0.076	0.209	-0.082	-0.005	-0.006
						(0.036)	(0.036)	(0.068)	(0.122)	(0.168)	(0.037)	(0.037)
Founding year						-0.015	-0.015	-0.035	-0.002	-0.013	-0.016	-0.016
						(0.005)	(0.005)	(0.011)	(0.020)	(0.016)	(0.005)	(0.005)
Gender heterogeneity of founders	-0.423	-0.417	-0.571	-1.274	-0.240	0.013	0.013	0.116	-0.038	-0.039	0.009	0.010
	(0.099)	(0.099)	(0.203)	(0.327)	(0.413)	(0.022)	(0.022)	(0.051)	(0.069)	(0.119)	(0.022)	(0.022)
Age heterogeneity of founders	-0.072	-0.072	-0.081	-0.093	-0.084	0.000	-0.000	0.001	-0.001	-0.005	0.000	0.000
	(0.008)	(0.008)	(0.018)	(0.022)	(0.023)	(0.002)	(0.002)	(0.004)	(0.005)	(0.006)	(0.002)	(0.002)
Race heterogeneity of founders	-0.519	-0.519	-0.713	-0.307	-0.844	-0.019	-0.018	-0.022	-0.085	0.082	-0.021	-0.023
	(0.109)	(0.109)	(0.210)	(0.339)	(0.451)	(0.025)	(0.025)	(0.054)	(0.067)	(0.120)	(0.025)	(0.025)
Experience heterogeneity of founders	-0.699	-0.700	-0.802	-0.706	-0.912	-0.008	-0.008	-0.036	0.071	0.140	-0.006	-0.007
	(0.104)	(0.105)	(0.202)	(0.325)	(0.376)	(0.025)	(0.025)	(0.052)	(0.060)	(0.075)	(0.025)	(0.025)
Fixed Effects												
Company type (all)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industries (47 categories)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
States	Yes	Yes	California	New York	Texas	Yes	Yes	California	New York	Texas	Yes	Yes
Constant	1.265	1.348	1.480	2.333	1.793	31.178	31.139	70.504	3.241	26.670	31.427	31.451
	(0.247)	(0.867)	(0.328)	(0.632)	(0.638)	(9.657)	(9.736)	(22.875)	(39.475)	(32.941)	(9.642)	(9.628)
N	2,131	2,131	565	260	229	1,063	1,063	280	129	114	1,063	1,063

Notes. This table extends the main models by incorporating interactions between political heterogeneity of founding teams and the political context of either the state or the industry (note that startups can belong to different industries). M1-M2 replicate the selection model from Table 2 (M3), estimating the likelihood that a founding team belongs to the original (non-randomized) dataset. These specifications interact political heterogeneity with the political leaning of the state (M1) or the industry (M2). M3-M5 repeat M2 but only for startups based in California (M3), New York (M4), or Texas (M5), respectively. M6-M7 replicate the closure model from Table 3 (M1), assessing whether political heterogeneity is associated with startup failure. These models include the same interaction terms with state (M3) and industry (M4) partisanship. M8-M10 rerun M7 but only for startups based in California (M8), New York (M9), or Texas (M10), respectively. M11 and M12 explore whether the likelihood of closure varies across states as a function of states' political heterogeneity, captured by the standardized absolute difference between Democratic and Republican vote shares (M11) and the standardized Herfindahl index of D–R vote shares (M12). All models include controls for gender, age, race, and experience heterogeneity, number of founders (only M6-M12), founding year (only M6-M12), and fixed effects for company types, industries (47 categories), and U.S. states. Robust standard errors are reported in parentheses.

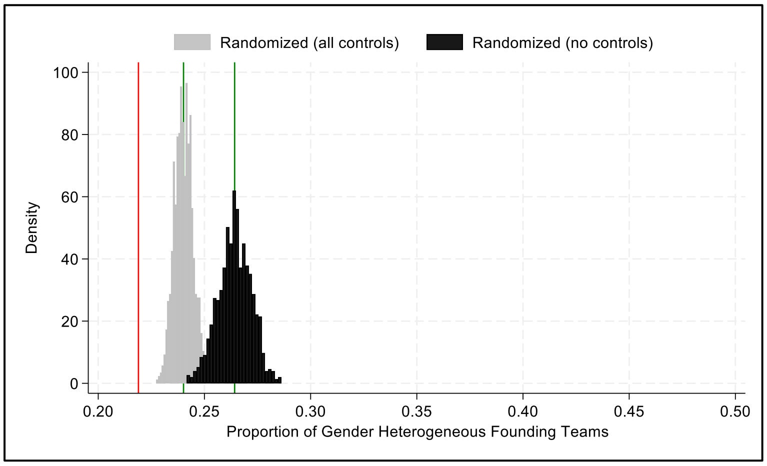
TABLE A5
The Association of Political Heterogeneity and Alternative Startup Performance Outcomes: Propensity Score-Based Estimators

	M1	**M2**	**M3**	**M4**	**M5**	**M6**	**M7**	**M8**	**M9**	**M10**	**M11**	**M12**
DV	Rank	Acquired	Employees	Funding	Rank	Acquired	Employees	Funding	Rank	Acquired	Employees	Funding
Type of model	PSM	PSM	PSM	PSM	IPW	IPW	IPW	IPW	DRE	DRE	DRE	DRE
Political heterogeneity of founders	0.065	0.026	-0.117	-0.515	0.258	-0.011	-0.157	-0.639	0.238	-0.005	-0.110	-0.587
	(0.142)	(0.021)	(0.093)	(0.286)	(0.124)	(0.017)	(0.084)	(0.304)	(0.111)	(0.019)	(0.077)	(0.222)
Fixed Effects												
Company types (all)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No
Industries (47 categories)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
States	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No
Constant	12.220	0.041	2.722	14.902	12.074	0.065	2.698	14.773	-1.311	-1.311	-1.404	-1.673
	(0.101)	(0.015)	(0.065)	(0.202)	(0.061)	(0.009)	(0.041)	(0.121)	(0.217)	(0.217)	(0.220)	(0.368)
N	448	448	440	202	1,021	1,021	983	495	1,060	1,060	1,020	513

Notes. The table presents results from three propensity score-based estimation strategies examining the association of political heterogeneity among founding teams with Crunchbase Rank (M1, M5, M9), the likelihood of being acquired (M2, M6, M10), the logarithm of the average number of employees excluding startups without employees (M3, M7, M11), and the logarithm of total funding received (in USD) excluding startups without funding (M4, M8, M12). M1-M4 apply Propensity Score Matching (PSM) using nearest-neighbor matching with replacement. M5-M8 use Inverse Probability Weighting (IPW) derived from a logistic regression predicting political heterogeneity. M9-M12 employ a Doubly Robust Estimator (DRE), combining outcome regression and inverse probability weighting. All models control for key covariates including gender, race, age, founding experience heterogeneity, and industry fixed effects. Fixed effects for company types and U.S. states are included for M1-M8. Robust standard errors are shown in parentheses.

FIGURE A1
Randomization of founders into startups

Gender



Uncontrolled permutation: 0.26

Controlled permutation: 0.24

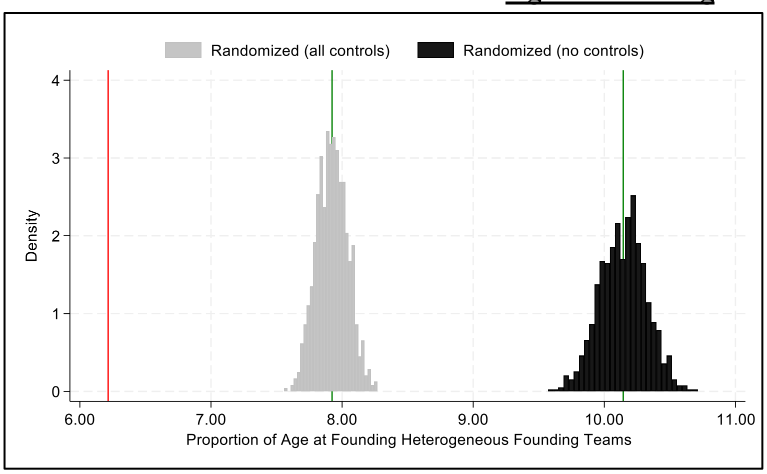
Observed data: 0.22

Induced homophily: 0.02

Choice homophily of 0.02

Cohen’s d: 3.83

Age at Founding



Uncontrolled permutation: 10.14

Controlled permutation: 7.92

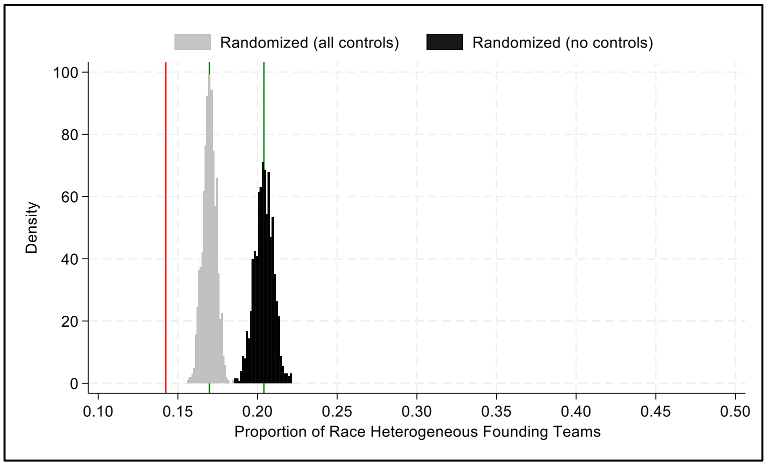
Observed data: 6.22

Induced homophily: 2.22

Choice homophily of 1.70

Cohen’s d: 14.79

Race



Uncontrolled permutation: 0.20

Controlled permutation: 0.17

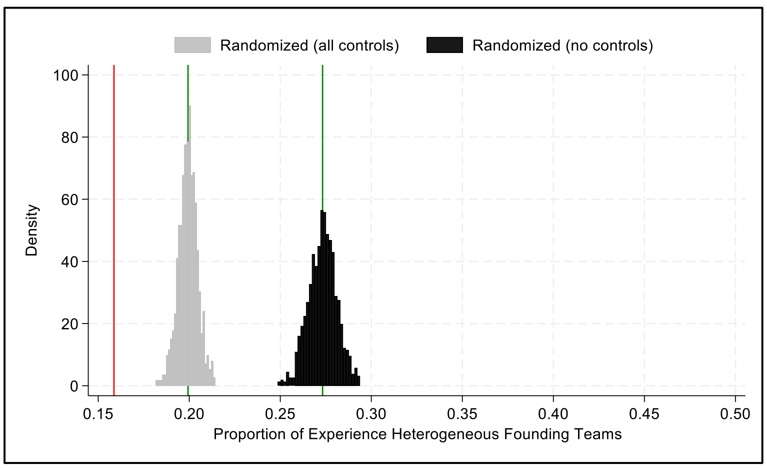
Observed data: 0.14

Induced homophily: 0.03

Choice homophily of 0.03

Cohen's d: 6.70

Experience



Uncontrolled permutation: 0.27

Controlled permutation: 0.20

Observed data: 0.16

Induced homophily: 0.07

Choice homophily of 0.04

Cohen's d: 11.14

Notes. The figure displays the proportion of gender, age, race, and experience heterogeneous founding teams, comparing the average based on the actual distribution from Crunchbase data (indicated by the red line on the left) to the distributions generated by permutation tests. These tests involve 1,000 repeated random assignments of startup founders within each U.S. state, their main industry, gender, race, prior founding experience, and age quartile (while excluding the focal demographic for each sub-analysis) to companies (gray distribution in the middle), and 1,000 repeated random assignments of startup founders without any controls, as represented by the black histogram on the right side.