Political Sorting and Information Frictions in Venture Capital[1]

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Abstract

I study whether partisan separation between investor and startup locations creates frictions in venture capital matching. Using a deal-anchored opportunity-set design and county-level presidential vote shares to measure political distance, I find that a one-standard-deviation increase in political distance lowers the probability of forming an investment match by 0.75 percentage points—roughly 8% of the baseline rate. Systematic mechanism tests reveal that political distance operates through soft information frictions: the penalty amplifies where qualitative assessments matter most—first-round investments, young startups, and unfamiliar geographies—and attenuates where information infrastructure is stronger, such as VC hubs and during the 2020–2023 pandemic period. There is little support for alternative explanations tied to political risk and narrative misalignment. Conditional on funding, higher-political-distance deals exhibit better outcomes, consistent with tighter screening. These findings document a hidden cost of political polarization that operates through information networks, with implications for spatial inequality in access to entrepreneurial capital.

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Key Words: Venture Capital, Political Distance, Soft Information, Matching Frictions, Entrepreneurial Finance

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All errors are my own.

1. Introduction

American communities are fracturing along partisan lines. The number of "super-landslide" counties—where one party wins with over 80% of the vote—has risen from fewer than 200 in 2004 to nearly 700 by 2020 (Sabato's Crystal Ball 2022). This partisan sorting into distinct local environments has created increasingly homogeneous political geographies, with profound implications for social cohesion, policy outcomes, and economic behavior. If local political environments shape the context through which business actors access information and evaluate opportunities, a natural question emerges: does partisan separation between investor and startup locations systematically affect which investor-startup pairs form?

Venture capital provides an ideal setting to investigate this question. VC investments inherently span diverse local political environments and are both information-intensive and relationship-dependent—precisely the type of transaction where frictions from divergent local contexts should be most salient. VCs must evaluate startups embedded in different political geographies, often relying on soft information and subjective assessments in environments where information is hard to verify and standardize. When transacting parties operate within politically distant communities, several potential frictions may arise. Most obviously, political distance may simply proxy for geographic separation. Alternatively, it may reflect exposure to heterogeneous policy regimes or divergent sector concentrations driven by local preferences. More subtly, political distance may capture differences in local information networks, shared reference frames, and access to validators—dimensions central to evaluating startups where hard information is scarce. If political segregation creates transaction frictions through any of these channels, we should observe systematic patterns in which investor-startup pairs successfully form.

This question matters for several reasons. First, venture capital plays a disproportionate role in funding innovation: VC-backed firms account for over 60% of U.S. public company R&D spending and employ millions of workers (Gornall and Strebulaev 2021). If political separation creates frictions in capital allocation, it could systematically disadvantage high-quality startups in politically distant locations, reducing both allocative efficiency and aggregate innovation. Second, the problem may be worsening and self-perpetuating. Political sorting has accelerated—the correlation between county partisanship and local characteristics has more than doubled since 2000 (Chen and Rodden 2013)—and any matching frictions will compound over time. Unlike traditional barriers that may erode with technology, political distance may be self-reinforcing: if VCs concentrate investments in politically similar areas, information networks and deal-flow channels will further calcify along partisan lines, creating a feedback loop that amplifies disparities in access to capital. Third, understanding the specific channel through which political distance operates is critical for policy design. If the friction reflects information costs, interventions that facilitate knowledge exchange across regions may help. If it reflects regulatory uncertainty, policy coordination may be needed. Identifying the mechanism matters for both welfare analysis and institutional design.

This paper builds on two strands of literature. A large body of work demonstrates that spatial proximity and social connections shape investment relationships. Geographic distance increases monitoring costs and information frictions, concentrating venture investments locally (Sorenson and Stuart 2001; Bernstein et al. 2017). Social proximity—through shared networks, education, or experience—facilitates the exchange of soft information critical to evaluating early-stage ventures (Bengtsson and Hsu 2015; Gompers et al. 2020; Ewens and Townsend 2020).

A separate literature documents that political preferences influence financial decisions. Partisan disagreement affects portfolio composition (Hong and Kostovetsky 2012), asset pricing (Meeuwis et al. 2022), and the interpretation of firm attributes (Bonaparte et al. 2017). Most closely related, Pan et al. (2025) show that county-level political differences are strong enough to affect mutual fund portfolio holdings through partisan disagreement over economic fundamentals.

I contribute by asking: do political differences matter for the formation of economic relationships in matching markets? While prior work focuses on portfolio allocation decisions by investors with established access to a choice set, venture capital investments involve bilateral matching where both parties must agree. This requires examining not only whether political distance influences VC preferences, but whether it creates frictions that prevent economically valuable matches from forming in the first place. Moreover, even if political distance matters for match formation, through what mechanism does it operate? Political distance may generate soft information frictions (differences in networks and reference frames), create systematic political risk (exposure to divergent policy regimes), reflect narrative and values misalignment (ideology-driven demand uncertainty), or simply proxy for geographic separation.

A simple descriptive contrast motivates the empirical analysis. Following Pan et al. (2025), I proxy each county's partisan environment with presidential vote shares and define political distance (PD) between a VC county and a startup county as the difference between their vote-share vectors across parties (Republican, Democratic, Other). This measure captures enduring differences in local preferences and beliefs that arise from partisan sorting at fine spatial scales. Nationally, the average county-to-county PD has trended upward since 2000, consistent with rising polarization. In contrast, among realized VC–startup matches, the average

PD has declined over time, indicating increasing concentration of deals among politically similar places. These facts are visualized in Figure 1. This divergence—the nation polarizes while deal making clusters—suggests that political distance increasingly binds at the matching margin, transmitting political geography into capital allocation through market mechanisms. The question is: through what friction?

To test whether partisan separation predicts which VC–startup pairs form, I use a deal-anchored opportunity-set design. For each realized first investment by VC i in startup j within an industry–year–stage cell, I form an opportunity set that holds market conditions fixed: the realized pair is matched to feasible alternatives on both sides—(i) up to five alternative VCs that invested in the same industry–year–stage (counterfactual investors for j, holding j fixed) and (ii) up to five alternative startups that raised in that market (counterfactual targets for i, holding i fixed). The final sample contains 1,264,271 matched pairs with a baseline investment incidence of 0.096. Each realized match and its counterfactuals inherit a common decision-set label, and I estimate models with decision-set fixed effects, so identification comes from within-set comparisons of pairs facing the same market.

Hypothesis 1 (Main Effect) predicts that, within a deal-anchored opportunity set, higher political distance between VC county and startup county is associated with lower investment incidence. Consistent with H1, the main finding is economically significant: a one-standard-deviation increase in political distance is associated with a 0.75 percentage-point decline in investment incidence—about eight percent of the baseline rate. This association is robust across specifications and plausibly causal: an instrumental-variables analysis exploiting historical ethnic composition distances from the 1900 Census yields larger magnitudes, consistent with a causal interpretation. Placebo tests that randomly reassign political distance within opportunity sets yield null results, confirming that the systematic relationship between county-level partisan separation and match formation is not spurious.

Why does political distance matter for investment matching? Political distance is a well-defined measure of divergence in local partisan environments, but the economic mechanism through which it affects VC–startup matching is not obvious. I systematically test three distinct channels, each with different welfare implications for how political polarization affects capital allocation.

First, political distance may operate through soft information frictions. Political alignment reflects broader cultural and social proximity (Iyengar and Westwood 2015; Mummolo and Nall 2017); when VC and startup counties are politically distant, investors may lack the local networks, shared reference frames, and tacit knowledge needed to verify soft information about founders, teams, and market positioning—dimensions central to early-stage screening when hard data are scarce (Gompers et al. 2020; Petersen 2004). This mechanism generates two complementary predictions. Hypothesis 2 (Opacity Amplification) predicts that if political distance raises the cost of verifying soft information, the penalty should be stronger in more opaque settings—first-round investments, young startups with thin track records, VC first entry into the startup's county where local networks are absent, and VCs with limited historical investment reach indicating weaker cross-regional information networks. Hypothesis 3 (Information-Infrastructure Attenuation) predicts that the political distance penalty should attenuate where information infrastructure is richer—VCs located in hub markets with denser networks and established intermediaries, and during the pandemic years (2020–2023) when remote diligence technology scaled rapidly and standardized data rooms became widespread. Empirically, the interactions align precisely with both predictions: the political distance penalty is significantly larger in opaque settings and attenuates sharply in VC hubs and during 2020–2023. The pandemic effect is particularly striking—the penalty declines from roughly 0.46 percentage points per 0.10 increase in political distance pre-pandemic to about 0.05 percentage points during 2020–2023, consistent with information-based frictions being reduced by technological advances in remote evaluation.

Second, political distance may create systematic political risk. Counties with different partisan compositions face divergent state-level policy regimes, regulatory environments, and differential access to federal resources. If cross-partisan investments expose VCs to higher policy uncertainty or startups to unpredictable regulatory treatment, the penalty should vary systematically with institutional features that affect risk exposure. Hypothesis 4 (Political-Risk Channel) predicts that the negative association between political distance and investment incidence should strengthen in presidential election years when nationwide policy uncertainty is elevated, attenuate for VC–startup pairs in the same state who share a common policy regime, and weaken when the startup's state government is co-partisan with the federal administration—reflecting more predictable policy environments and potentially greater access to federal support. Empirically, none of these predictions materialize: election-year interactions, same-state interactions, and state-federal alignment interactions are all small and statistically indistinguishable from zero, providing no support for a political-risk channel.

Third, political distance may generate narrative and values misalignment. Media consumption and brand preferences increasingly track political identity (Gentzkow and Shapiro 2010; Iyengar and Hahn 2009), and consumers use brands to signal identity and reward or punish firms based on perceived value congruence (Berger and Heath 2007; Sen and Bhattacharya 2001). If VCs believe that products or business models developed in politically distant regions will struggle to resonate with target markets—either because the founding team misreads consumer preferences shaped by different cultural values or because the brand carries unwanted partisan associations—investment decisions may reflect anticipated demand uncertainty. Hypothesis 5 (Narrative/Consumer-Exposure Channel) predicts that if political distance operates through this mechanism, the negative association should be stronger in clear-narrative industries—advertising and marketing, media, blockchain and cryptocurrency, entertainment production—where evaluation is especially frame-sensitive and stories drive investment decisions, and in consumer-facing (B2C) businesses where end-user ideology directly shapes product demand and brand loyalty. Empirically, I find no such patterns: interactions between political distance and narrative intensity or B2C exposure are small and statistically insignificant across specifications, providing no support for a narrative-driven mechanism.

Before testing these mechanisms, I first rule out that political distance is simply a proxy for geographic distance. A large literature links physical proximity to investment intensity via monitoring costs and local information advantages (French and Poterba 1991; Coval and Moskowitz 1999; Sorenson and Stuart 2001; Bernstein et al. 2017). Partisan sorting exhibits strong spatial patterns—conservative and liberal communities tend to cluster geographically—and political distance might merely reflect physical separation, with the true friction being monitoring costs that this literature has extensively documented. If so, the political distance penalty should vary strongly with geographic distance, intensifying at long ranges where monitoring is costly and disappearing at very short ranges where monitoring costs are minimal. Hypothesis 6 (Geographic-Proxy Test) predicts that if political distance merely proxies for physical distance, the association should vary significantly across geographic distance quintiles and be overturned at short ranges (≤100 miles; ≤500 miles) where physical proximity enables easy monitoring. Empirically, I find no such patterns: the political distance coefficient is stable across distance quintiles and remains negative and statistically significant even within the shortest distance bins. The interaction terms are small and statistically insignificant, confirming that political distance captures a dimension of local environment distinct from physical proximity.

Beyond these mechanism tests, I verify that the findings are robust to alternative measurement choices and sample restrictions. Replacing the continuous political distance measure with a binary same-party indicator yields consistent results. Jointly including political distance and its squared term shows that the linear specification adequately captures the effect. Dropping same-county pairs, excluding observations with VCs in California, or excluding startups in California leaves the coefficient magnitude close to baseline. Notably, excluding Democrat-favored sectors (healthcare, education, government services), Republican-favored sectors (energy, materials), or all politically sensitive sectors together leaves the political distance coefficient essentially unchanged, ruling out that the penalty reflects systematic sector composition differences across partisan geographies rather than a friction operating broadly across industries.

To provide additional support for the information-friction interpretation, I examine realized outcomes for funded investments. If political distance raises screening costs by making soft information harder to gather and verify, it should tighten the funding threshold: only stronger opportunities clear the bar, leading to better performance conditional on funding (Ewens and Townsend 2020; Kaplan and Strömberg 2004). Empirically, I find that higher-political-distance deals exhibit significantly higher IPO/M&A rates and lower write-off rates, conditional on receiving investment. A 0.10 increase in political distance is associated with roughly 0.27

percentage points higher IPO/M&A probability and 0.25 percentage points lower write-off probability, consistent with tighter screening when information is costly.

Taken together, the evidence points to soft information frictions as the mechanism through which political distance affects venture capital matching. The political distance penalty amplifies precisely where soft information is hardest to verify—in opaque settings involving first-time investments, young startups with minimal track records, and VCs entering unfamiliar geographies—and attenuates where information infrastructure is stronger—in VC hub markets with dense networks and established intermediaries, and during the pandemic period when remote diligence technology advanced significantly. Crucially, systematic tests with directional predictions reject three plausible alternative mechanisms: the effect does not vary with political risk exposure (election cycles, state policy regimes, federal alignment), does not concentrate in narrative-heavy or consumer-facing sectors where values might matter most, and does not operate through physical distance or monitoring costs. The superior performance of high-political-distance deals conditional on funding provides additional confirmation: soft information frictions raise the bar for investment, creating a selection effect where only the strongest opportunities secure funding.

This research makes three main contributions to our understanding of how political polarization shapes economic outcomes. First, I identify information-based matching frictions as a new economic cost of political polarization. Recent work traces economic effects of polarization through policy uncertainty (Baker et al. 2016; Julio and Yook 2012) and partisan disagreement over fundamentals (Meeuwis et al. 2022; Pastor and Veronesi 2020). I show that polarization creates frictions even absent direct policy effects: political distance reduces economically valuable matches from forming by raising the cost of verifying soft information. A one-standard-deviation increase in political distance lowers investment probability by 0.75 percentage points (8% of baseline), comparable to geographic distance effects (Sorenson and Stuart 2001; Bernstein et al. 2017). This friction operates not through disagreement over startup quality or regulatory exposure, but through the informational infrastructure of communities—the networks and validators that allow investors to assess opportunities when hard data are scarce. The findings reveal a hidden cost of polarization that operates through market mechanisms.

Second, I provide systematic evidence on the mechanism, establishing that political distance operates through soft information frictions rather than competing channels. Identifying why political distance matters is critical because different mechanisms—political risk (Julio and Yook 2012), values misalignment (Shiller 2019), or geographic separation (Sorenson and Stuart 2001)—have distinct welfare implications. Using a triangulation strategy combining amplification tests, attenuation tests, and outcome validation, I show the penalty amplifies where qualitative assessments matter most and attenuates where information infrastructure is stronger. Directed tests with explicit predictions reject political risk, values misalignment, and geographic proxying as alternative explanations, establishing not only that political distance creates frictions but how it does so.

Third, I document a striking temporal divergence revealing market-based balkanization of innovation networks. From 2000 to 2024, average county-to-county political distance rises nationally (consistent with partisan sorting documented by Chen and Rodden 2013; Brown and Enos 2021), yet average political distance in realized VC matches declines sharply. This "environmental polarization, transactional homophily" pattern reflects endogenous concentration of capital within politically familiar geographies. Unlike traditional barriers that erode with technology, information-based frictions may strengthen as communities become more homogeneous—what you know depends on who you know, and partisan sorting increasingly determines "who." These dynamics suggest current trends may fragment national innovation ecosystems into politically bounded sub-markets, amplifying regional disparities in entrepreneurial finance (Lerner and Nanda 2020; Nguyen et al. 2023) and imposing real economic costs beyond policy gridlock (Autor et al. 2020).

The remainder of the paper proceeds as follows. Section 2 describes the data, variable construction, and the deal-anchored opportunity-set design. Section 3 establishes that political distance significantly reduces investment incidence, using instrumental variables and ruling out that the effect proxies for geographic distance. Section 4 characterizes the underlying friction through systematic mechanism tests. Section 5 presents robustness checks. Section 6 concludes.

2. Data, Measures, and Empirical Strategy

2.1. Venture Capital Data

I use venture investment records from LSEG for 2000–2024, restricting the universe to U.S.-domiciled venture capital investors and U.S.-headquartered startups. The unit of observation is the first investment between a VC and a startup; all follow-on rounds are dropped so that the analysis centers on the initial screening and selection decision, before relationship history, VC–founder learning, reputation dynamics, or prior performance can shape subsequent financing.

County identifiers are assigned using Federal Information Processing Standards (FIPS) codes via the NBER Census County Names crosswalk (2010 release)[1]. The matching procedure uses state and county names as the primary key after normalizing suffixes and common variants

[1] https://www.nber.org/research/data/county-distance-database BROOMFIELD County (FIPS: 8014), Colorado was officially established in the year 2001. To ensure consistency and data completeness for cross-county distance measures, I impute its values for Education Distance, Income Distance, Population Distance, Industry Distance, and Religious Distance by taking the simple average of its four parent counties – ADAMS (FIPS: 8001), BOULDER (FIPS: 8013), JEFFERSON (FIPS: 8059), and WELD (FIPS: 8123).

(e.g., “Saint” or “St.”). When a state–county combination fails due to naming inconsistencies, I apply a county-name-only fallback only when that county name is unique nationwide within the dataset; otherwise, the observation is dropped to avoid ambiguous attribution.

To support the construction of deal-anchored opportunity sets and controls, I retain observations with non-missing VC founding year, startup founding year, and an industry classification. I keep pure venture financings and exclude non-VC transactions (e.g., buyouts, PIPEs, and other control-oriented deals) so that the sample reflects screening in the venture market rather than private equity or corporate transactions. Applying these filters yields a final sample of 35,937 startups and 9,332 VC firms, spanning 424 VC counties, 760 startup counties, and 9,988 unique VC–startup county pairs across all 50 states and the District of Columbia. This breadth of coverage provides the county-to-county variation needed to construct turnout-based distance measures and ensures that the matching analysis reflects initial investment decisions rather than the path dependence of follow-on financing.

For exit analysis, I link each startup to LSEG’s exit records by startup identifier to obtain ex post outcomes. I code two indicators—IPO/M&A and Write-off—and record the earliest announced exit or write-off date. Only deals whose first VC–startup investment occurs on or before the exit announcement are kept.

2.2. Dependent Variables

The main outcome at the formation margin is an investment event. For each deal-anchored opportunity set, I define a dyad-year indicator that equals one if VC i makes its first investment in startup j in year t, and zero for matched counterfactual pairs in the same industry–year–stage opportunity set.

To examine downstream consequences of PD, I track two exit outcomes for the subsample of realized investments. IPO/M&A is an indicator for whether the startup ultimately achieves a successful exit through an initial public offering or a merger or acquisition. Write-off is an indicator for failure defined either by an explicit write-off or—absent IPO/M&A—by no observed financing activity after 2019. This five-year dormancy threshold (as of December 31, 2024) is consistent with industry practice for identifying defunct ventures.

2.3. Key Independent Variables

Political environments are measured using county-level presidential election results from 2000-2024. Data come from the MIT Election Data and Science Lab (through 2020)[2] and supplementary sources for 2024[3]. Presidential elections provide the most comprehensive measure of local political orientations for several reasons: they achieve the highest voter turnout of any election type, engage citizens across all demographic groups, and focus on broad ideological questions.

Following Pan et al. (2025), I define PD between VC county i and startup county j in year t as:

$$\mathrm{PD}_{ij,t} = \left|\mathrm{Rep\%}_{it} - \mathrm{Rep\%}_{jt}\right| + \left|\mathrm{Dem\%}_{it} - \mathrm{Dem\%}_{jt}\right| + \left|\mathrm{Other\%}_{it} - \mathrm{Other\%}_{jt}\right| \in [0,2],$$

where Rep%, Dem%, and Other% represent the percentage of votes for Republican, Democratic, and other parties, respectively, and sum to one. To obtain an annual county series between presidential election years, I linearly interpolate vote counts for Republican, Democratic, and Other parties within each county (see, e.g., Hilary and Hui 2009), then calculate annual vote shares from the interpolated counts. This approach maintains internal consistency by

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[2] https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/VOQCHQ
[3] tonmcg's GitHub repository: https://github.com/tonmcg/US_County_Level_Election_Results_08-24/blob/master/2024_US_County_Level_Presidential_Results.csv

ensuring that vote shares sum to one in every county-year. This construction yields a uniform, annually indexed measure of local partisan composition from 2000 through 2024.

This continuous, composition-based measure has three advantages. First, it preserves intensity: a county with 51% Republican support is meaningfully different from one with 90%, which binary labels would treat identically. Second, the resulting fine-grained variation is essential for identifying how county-level differences affect cross-regional matching. Third, while county aggregation does not perfectly capture individual preferences, it better reflects the ambient political environment in which organizations are embedded and make decisions. The measure captures local political environments in which VCs and startups are embedded. When these environments are farther apart in political space, the resulting friction—whether through information networks, reference frames, or other local characteristics—may impede match formation.

The measure captures local political environments in which VCs and startups are embedded. Political alignment may reflect several dimensions relevant to investment matching: shared social networks and cultural reference frames that facilitate soft information transmission (Petersen 2004); similar policy regimes and regulatory environments (Julio and Yook 2012); or aligned values and narratives that shape product demand (Shiller 2019). When these environments are farther apart in political space, the resulting friction—whether through information networks, policy exposure, or demand uncertainty—may impede match formation. Section 4 tests among these channels.

For completeness, I also use the following key independent variables. Same Party is a binary indicator equal to one if the majority party (by vote share) is the same in the VC's county and the startup's county in year t, and zero otherwise.

PD (L2)[4] is the Euclidean distance between the two counties' presidential vote-share vectors, with components defined analogously to PD and summing to one in each county-year.

Ethnic Distance (1900) is the L1 (Manhattan) distance between the VC and startup counties' ethnic composition vectors constructed from 1900 Census county tabulations over major European origin groups (German, Irish, Italian, English, Scottish, Polish, Norwegian), proxying deep-rooted cultural differences orthogonal to contemporary partisanship.

2.4. Control Variables

To ensure that PD is not spuriously capturing broader socioeconomic or demographic variation, I include a set of dyad-level controls that proxy standard sources of county-to-county heterogeneity. These covariates span geography, socioeconomic composition, industrial structure, community institutions, and organizational life cycle—dimensions along which counties plausibly differ in ways that affect both entrepreneurial activity and the flow of venture capital.

I include Geographic Distance, measured as the great-circle distance between county centroids using the NBER U.S. County Distance Database[5], scaled by 1,000 so the unit is thousand miles.

To absorb the special case of co-location, I add Same County, a binary indicator equal to one when the VC and startup are in the same county.

I construct absolute county-to-county differences. Education Distance is the absolute difference between the counties in the share of residents aged 25 or older with a college degree or higher. Income Distance is the absolute difference between the counties in per-capita income,

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[4] $\text{PD (L2)}_{i,j} = \sqrt{\left(\text{Rep\%}_i - \text{Rep\%}_j\right)^2 + (\text{Dem\%}_i - \text{Dem\%}_j)^2 + (\text{Other\%}_i - \text{Other\%}_j)^2}$

[5] https://www.nber.org/research/data/county-distance-database

reported in thousands of dollars. Population Distance is the absolute difference between the counties in total population, reported in millions of persons. Each series is constructed from the 2000, 2010, and 2020 decennial Census and the ACS five-year tabulations, with linear interpolation to obtain annual county-year values. Industry Distance is the absolute distance between counties' BEA industry employment-share vectors, capturing differences in local production bases that can influence sectoral deal flow and investor specialization. Religious Distance is the absolute difference between the two counties' overall religious participation rates. Rates are taken from ARDA benchmark years (2000, 2010, 2020) and held piecewise constant by decade: 2000–2009 use the 2000 benchmark, 2010–2019 use 2010, and 2020–2024 use 2020.

In addition to these dyad-level measures, I control for organizational life-cycle variables: VC Experience, defined as log one plus years since founding, and Startup Age, defined analogously. These account for systematic differences in evaluation capacity and financing needs across VCs and startups. Formal definitions, sources, and construction details for all variables appear in Appendix A.

2.5. Interaction Variables

To probe mechanisms and heterogeneity, I interact PD with indicators ordered by spatial proximity, information opacity and organizational experience, information infrastructure, institutions, and market orientation/narrative clarity.

I begin with spatial proximity. ≤ 100 Miles and ≤ 500 Miles indicate whether the great-circle county-to-county distance falls within these short-range thresholds.

I capture opacity and experience with four markers. First Round flags initial financings, where information is thinnest. Following Babina et al. (2020), Young Startups identifies firms that are at most three years old. VC First Entry marks cases where the investor has not previously invested in the startup's county. Low Reach singles out investors whose cumulative geographic reach up to year t is at or below the cross-sectional median among VCs in that year.

To gauge information infrastructure, I use VC Hub and Pandemic Years. VC Hub identifies investors based in San Francisco County (CA), Suffolk County (MA), and the five counties of New York City, markets with dense deal flow and intermediaries (Nguyen et al. 2023). Pandemic Years covers 2020–2023, when remote diligence scaled and standardized processes diffused.

To separate soft-information frictions from national political risk and shared policy regimes, I interact with Election Years, Same State, and Startup State–Federal Alignment. Election Years marks presidential election years across the sample. Same State indicates a common state policy regime for investor and startup. Startup State–Federal Alignment records whether the startup's state government shares the partisan affiliation of the federal administration in the current presidential cycle.

Finally, I consider market orientation and narrative clarity. Clear-Narrative flags industries with clearer investment narratives. B2C & All identifies consumer-facing or mixed go-to-market settings, and B2C isolates purely consumer-facing settings.

2.6. Sample Construction

Testing how PD relates to investment requires credible counterfactuals. I therefore construct deal-anchored opportunity sets that compare each realized match with a set of close alternatives. For every first investment between VC i and startup j in industry s, year t, and stage r, I follow matched-sampling approaches used in the VC literature (Puri et al. 2024). Specifically, I draw two symmetric sets of candidates within the same industry–year–stage market (See Figure 2): (i) holding the startup fixed, up to five other VCs that invested in (s, t, r) but not in j; and (ii) holding the VC fixed, up to five other startups that received funding in (s, t, r) but not from i. When the eligible pool on a side exceeds five, candidates are sampled uniformly at random without replacement; when the pool is thinner, all eligible candidates are included. This yields roughly ten counterfactual pairs per realized deal (fewer when markets are thin).

All candidates inherit the realized deal's decision-set group, ensuring that comparisons are made inside the same opportunity set rather than across markets. In the final analytic sample, this construction produces on the order of 121,105 realized pairs and 1,143,166 counterfactual pairs, for a total of 1,264,271 matched dyads.

As a descriptive check, Figure 3 contrasts the distribution of PD for realized and counterfactual pairs drawn from the same opportunity sets. The two distributions have similar shapes, but realized matches are more concentrated at lower distances: the mean PD is 0.223 for realized investments versus 0.266 for counterfactuals. This pattern, evident before adding controls, provides initial evidence that PD influences investment selection.

2.7. Identification Strategy

Identification comes from within–opportunity-set comparisons under a common industry–year–stage context. For each realized first investment, I build a deal-anchored opportunity set inside the same (industry s, year t, stage r) market and attach all candidates—constructed as in Section 2.6—to the realized deal's decision-set group g. I then estimate models with group fixed effects, so any factors shared by candidates in the same set (the VC's available budget at that time, market conditions in (s, t, r), unobserved shocks common to the startup and its close competitors, etc.) are absorbed. The coefficient on PD is therefore identified purely from cross-candidate differences within the same set.

Formally, the baseline specification is

$$Investment\ Event_{ijtg} = \beta \cdot PD_{ijt} + \gamma \cdot X_{ijt} + \mu_g + \varepsilon_{ijtg},$$

where: $Investment\ Event_{ijtg}$ is an indicator equal to one if VC i invests in startup j in year t, and zero for the counterfactual pairs in the same group g. PD_{ijt} is the differences between county-level presidential vote-share vectors (Rep, Dem, Other) for the VC county i and the startup's county j in year t. X_{ijt} represents the control variables described in Section 2.4, including dyad-level socioeconomic and geographic differences and VC/startup life-cycle measures. μ_g denotes group (deal-anchored) fixed effects. ε_{ijtg} is the error term. Estimation is by linear probability model (LPM), with standard errors two-way clustered at the VC-county and startup-county levels, allowing arbitrary correlation over time and across pairs that share the same VC county or the same startup county. As a robustness check, I also estimate a conditional fixed-effects logit that conditions out μ_g.

Within each opportunity set g, political distance varies across candidate pairs because each set includes multiple VC counties (when comparing alternative investors for a given startup) and multiple startup counties (when comparing alternative targets for a given investor), generating the cross-candidate variation needed for identification.

Identification relies on within–decision-set contrasts rather than time-series shifts for a given county pair. The key as-if exogeneity assumption is that, conditional on X_{ijt} and μ_g, there are no remaining unobservables that are systematically correlated with PD_{ijt} and also affect selection. As a falsification check, I conduct within-set PD permutations (reported in the robustness section), which yield coefficients centered near zero, consistent with the identifying assumption.

3. Main Results

3.1. Descriptive Statistics

Table 1 reports summary statistics for the matched sample of 1,264,271 VC–startup pairs. Panel A shows that 9.6 percent of pairs result in a realized first investment. Exit outcomes are defined only for funded deals; among the 120,762 realized investments, 27.5 percent culminate in an IPO or acquisition and 22.8 percent are written off.

Panel B summarizes key independent variables. Political Distance (PD) averages 0.262 (0.220) with an interquartile range of 0.093 to 0.374. In 87.7 percent of pairs, the VC's county and the startup's county share the same plurality party. The squared term PD^2 has a mean of 0.055. Historical ethnic distance based on 1900 composition averages 0.336.

Panel C documents control variables. Mean county-to-county distance is 1,281 miles, while 7.7 percent of pairs are in the same county. Startups are on average 3.9 years old; the log age measure averages 1.329. VC firm experience averages 13.8 years (log measure 2.257). Education distance averages 0.098, industry distance 0.399, income distance 15.18, population distance 1.504, and religious distance 0.113, indicating substantial heterogeneity beyond geography.

Panel D reports interaction variables used in mechanism tests. Half of observations involve first-round financings, and 58.8 percent involve young startups (age ≤ 3). In 64.5 percent of pairs, the VC has no prior investment in the startup's county. The sample spans pandemic years (33.7%), election years (29.5%), and various proximity ranges, with 24.2 percent in the same state. Additional moderators include federal alignment, sector narratives, and business model orientation (see Table 1 for complete statistics).

3.2. Main Results

Table 2 summarizes estimates from the deal-anchored opportunity-set design. Column 1 reports a baseline linear probability model with group fixed effects. Column 2 adds VC and startup characteristics together with county-pair socioeconomic distance controls. Column 3 reports marginal effects from a conditional fixed-effects logit. Standard errors are two-way clustered at the VC-county and startup-county levels.

PD is negative and precisely estimated in every specification. In Column 2, the coefficient on PD equals −0.034 and is statistically significant at the one-percent level. Using the sample moments from Table 1, a one-standard-deviation increase in PD is associated with a 0.75 percentage-point lower investment probability. Relative to the baseline investment rate of 9.6 percent, this corresponds to a 7.8 percent decline. The estimate changes only slightly from Column 1 (−0.038) to Column 2 (−0.034), indicating that observable firm, geographic, and socioeconomic differences explain little of the relationship. The conditional fixed-effects logit in Column 3 yields a marginal effect of −0.434, also significant at the one-percent level, reinforcing that the finding does not hinge on linearity assumptions.

Other covariates move as expected. The indicator for Same County raises the probability of investment by 9.4 percentage points in Column 2 and remains positive in the logit. VC Firm Experience is positively associated with deal formation; the coefficient of 0.004 implies that a doubling of VC experience raises the investment probability by approximately 0.3 percentage points. The corresponding marginal effect in Column 3 equals 0.053 and is significant at the five-percent level. Geographic Distance carries a negative coefficient of −0.025 in Column 2 and a logit marginal effect of −0.334 in Column 3, highlighting the salience of spatial frictions even after conditioning on PD and rich fixed effects.

Taken together, the sign, magnitude, and stability of the political-distance estimates across models indicate a robust reduction in the likelihood of forming an investment tie when VC investors and startups are embedded in politically distant environments. The within-opportunity-set design and the modest attenuation with added controls suggest this relationship reflects information-related frictions rather than observable firm or geographic characteristics.

3.3. Instrumental Variables

To address endogeneity concerns beyond the rich controls and opportunity-set fixed effects, Table 3 implements an instrumental-variables design that leverages persistent historical settlement patterns. The instrument—Ethnic Distance 1900—is the absolute distance between county-level ethnic composition vectors from the 1900 Census across seven origin groups: German, Irish, Italian, English, Scottish, Polish, and Norwegian. The instrument satisfies two key conditions. First, relevance: historical ethnic composition strongly predicts contemporary political alignment, with first-stage F-statistic exceeding 15. Second, exclusion: 1900 settlement patterns are unlikely to directly affect contemporary VC-startup matching decisions after controlling for current economic conditions. The 125-year gap makes direct effects implausible—the economic structures, technologies, and information networks that govern today's venture capital markets bear little resemblance to those of the early 20th century. Moreover, conditional on our rich controls for present-day socioeconomic conditions (income, education, industry composition, population), any residual correlation between historical ethnic settlement and investment outcomes is most plausibly channeled through the durable political and cultural environments these patterns helped establish.

In Column 1, the first stage is strong and in the expected direction (F-statistic > 15, satisfying standard relevance thresholds). Regressing PD on Ethnic Distance 1900 yields a coefficient of 0.221 with a standard error of 0.054, significant at the 1 percent level. In Column 2, the second stage uses the predicted PD from the first stage; the coefficient on instrumented PD equals −0.456 with a standard error of 0.186 and is significant at the 5 percent level. Column 3 reports the reduced form, regressing Investment Event directly on Ethnic Distance 1900; the coefficient is significantly negative.

Taken together, the IV evidence indicates that exogenous historical variation linked to ethnic settlement patterns predicts contemporary PD and, through it, investment outcomes. The larger second-stage magnitude relative to OLS is consistent with a local-average-treatment interpretation and with attenuation in OLS from measurement error. In concert with the within–opportunity-set OLS estimates, the IV results reinforce the conclusion that PD constitutes a meaningful friction in VC match formation rather than a by-product of omitted contemporary covariates.

3.4. Political Distance Proxy Test

A large literature links geographic proximity to venture investment through monitoring costs and local information (Lerner 1995; Sorenson and Stuart 2001; Bernstein et al. 2017). If PD merely proxies for physical distance, the PD effect should weaken at very short ranges and strengthen with greater separation. To assess this interpretation, Table 4 interacts PD with (i) geographic-distance quintiles (Column 1) formed from the full-sample distribution (Q1 omitted) and (ii) short-range indicators—≤ 100 miles (Column 2) and ≤ 500 miles (Column 3).

The interactions are small and statistically insignificant across specifications. In particular, the PD effect does not attenuate at ≤ 100 miles or ≤ 500 miles, nor does it increase monotonically across distance quintiles. Distance variables themselves move as expected: short-range indicators are positive and precisely estimated, while higher distance quintiles are negative. The PD coefficient remains negative and precisely estimated when controls are included (Column 2), and modeling distance flexibly does not overturn the result.

Taken together, the evidence indicates that PD does not operate through physical distance. Instead, PD captures a dimension distinct from geographic separation.

4. Characterizing the Friction

Having established that PD reduces investment incidence within deal-anchored opportunity sets, I characterize the underlying channel. County vote shares may embed multiple dimensions of local divergence that could affect investment matching.

First, political alignment reflects broader cultural and social proximity (Iyengar and Westwood 2015; Mummolo and Nall 2017). When VC and startup counties are politically distant, investors may lack the local networks, shared reference frames, and tacit knowledge needed to verify soft information about founders, teams, and market positioning—dimensions central to early-stage screening (Gompers et al. 2020). This information-frictions channel predicts that the PD penalty should amplify where qualitative assessments matter most and attenuate where information infrastructure is stronger.

Second, PD may proxy for exposure to different policy regimes and regulatory uncertainty. If cross-partisan investments face higher political risk—through uncertain access to state support, unfamiliar regulations, or exposure to policy shocks—the PD penalty should intensify during election years, attenuate for same-state pairs sharing a policy regime, and weaken when the startup's state government aligns with the federal administration (Julio and Yook 2012; Pástor and Veronesi 2012).

Third, PD may capture misalignment in values and narratives that shape product demand. Consumption patterns and brand loyalty increasingly reflect political identity (Berger and Heath

2007; Micheletti 2003); if VCs avoid startups whose products may not resonate with politically distant consumer bases, the penalty should concentrate in narrative-heavy sectors and consumer-facing businesses (Shiller 2019).

I proceed in three steps. First, I test predictions of soft-information frictions along two dimensions—information opacity (early rounds, young startups, first entry, low reach) and information infrastructure (VC hubs, the COVID-19 remote-diligence period) (Section 4.1). Second, I test alternative mechanisms: systematic political risk and narrative/consumer exposure (Section 4.2). Third, I study realized outcomes for funded deals to assess whether tighter screening at higher PD is reflected in exits and write-offs (Section 4.3). The evidence is most consistent with an information-based channel.

4.1. Information Opacity and Infrastructure

If political distance operates through soft information frictions, the effect should vary systematically with information availability. A large literature establishes that venture capital screening relies heavily on qualitative, non-codified information when verifiable data are scarce (Kaplan and Strömberg 2004; Stein 2002; Gompers et al. 2020). Soft information—assessments of founder quality, team dynamics, and market positioning—is costly to transmit across distance and requires shared context for verification (Petersen 2004; Liberti and Mian 2009). Moreover, information infrastructure—dense local networks, established intermediaries, and standardized data—can reduce these costs (Sorenson and Stuart 2001; Hochberg et al. 2007; Agarwal and Hauswald 2010).

This framework generates two complementary predictions. First (Opacity Amplification). If PD reflects soft information frictions, the penalty should strengthen in settings where information is hardest to verify: (i) First Round financings with minimal accumulated disclosure and unproven business models; (ii) Young Startups (age ≤3) with thin track records and limited operating history; (iii) VC First Entry into a startup's county, where the VC lacks established local contacts and tacit knowledge; and (iv) Low Reach VCs with weaker cross-regional information networks. Second (Infrastructure Attenuation). The friction should weaken where information infrastructure is stronger: (i) VC Hub counties with dense ecosystems providing richer deal flow, better comparables, and more intermediaries; (ii) Pandemic Years (2020–2023) when remote diligence technology scaled and standardized data rooms became widespread.

Table 5 tests these predictions within the same deal-anchored opportunity-set (group) fixed effects. Each specification includes decision-set fixed effects, so identification comes from comparing pairs that face identical market conditions but differ in the relevant dimension. The results align precisely with the soft information hypothesis. In Column 1, the PD penalty for first-round investments is approximately twice as large as for later rounds—a 0.10 increase in PD is associated with 0.48 percentage points lower within-set investment probability in first rounds versus 0.20 percentage points in later rounds. For young startups (Column 2), thinner operating histories amplify the effect to 0.39 percentage points per 0.10 increase in PD. When a VC enters a county for the first time (Column 3), the PD penalty is materially larger—the total effect is about 0.25 percentage points per 0.10 increase in PD—consistent with limited local networks and unfamiliarity with the startup's ecosystem. The strongest amplification appears for low-reach investors (Column 4): the implied effect is 0.63 percentage points per 0.10 increase in PD, compared with roughly 0.13 percentage points for higher-reach peers who have developed broader information networks. For VCs headquartered in hub counties (Column 5), the interaction term (+0.052) almost exactly offsets the baseline PD effect (−0.052), reducing the net penalty to near zero. This pattern is consistent with hubs providing denser information networks, more standardized evaluation frameworks, and better access to local validators. During the pandemic years 2020–2023 (Column 6), the PD penalty declines dramatically—from roughly 0.46 percentage points per 0.10 increase pre-pandemic to about 0.05 percentage points during the pandemic. This sharp attenuation coincides with the rapid scaling of remote diligence technology, virtual roadshows, and standardized data rooms that reduced the importance of physical proximity and local presence for information gathering.

Taken together, these patterns provide strong evidence that political distance operates through soft information frictions. The effect amplifies precisely where qualitative assessments matter most and verifiable information is scarcest, and it attenuates where information infrastructure makes local knowledge more accessible.

4.2. Testing Alternative Mechanisms

Section 4.1 documents two signatures—amplification in opaque settings and attenuation when information frictions are lower—that point to a soft-information screening channel. To distinguish this interpretation from plausible alternatives, I test two competing mechanisms within the same deal-anchored opportunity-set design: systematic political risk and narrative/values exposure. Each mechanism delivers directional predictions for how the PD penalty should vary across settings; I take those predictions to the data.

Prior work shows that investment slows when policy uncertainty rises and recovers when policy regimes are unified or predictable (Julio and Yook 2012; Jens 2017; Baker et al. 2016; Pástor and Veronesi 2012). If political distance depresses matching because it raises exposure to policy risk (rather than because it raises screening costs), three predictions follow: (i) the PD penalty should intensify in presidential election years, when nationwide policy uncertainty is elevated; (ii) it should attenuate when VC and startup share the same state policy regime (same-state pairs); and (iii) it should attenuate when the startup's state government is co-partisan with the federal administration, reflecting more predictable access to federal support and alignment of rules. Table 6 shows that none of these predictions materialize. The interactions PD × Election Year, PD × Same State, and PD × State–Federal Alignment are small and statistically indistinguishable from zero, while the PD main effect remains negative and tightly estimated. These results provide no support for a systematic political-risk channel.

A second possibility is that PD operates through belief disagreement and identity-laden demand—i.e., the penalty is strongest where evaluation is shaped by ideology and "stories," not hard information. Media and online content consumption is sharply partisan (Iyengar and Hahn 2009; Gentzkow and Shapiro 2010); consumers use brands and cultural goods to signal identity (Berger and Heath 2007) and reward/punish firms based on ethical or political congruence (Sen and Bhattacharya 2001); field evidence shows willingness to pay for ethically framed products (Hainmueller et al. 2015); and "narrative economics" argues that contagious stories move markets (Shiller 2019), echoed by research on political consumerism (Micheletti 2003; Stolle and Micheletti 2013). If this narrative/values mechanism drives the PD penalty, two predictions follow: (i) the penalty should be stronger in "clean narrative" industries—advertising & marketing, online services, consumer publishing, broadcasting, entertainment production, blockchain & cryptocurrency, and crowd collaboration—where evaluation is especially frame-sensitive; and (ii) it should be stronger for B2C exposure, where end-users' ideology directly shapes demand and reputation. Table 7 estimates PD × Clean-Narrative, PD × B2C & All, and PD × B2C interactions. Across specifications, the interaction coefficients are near zero and statistically indistinguishable from zero; confidence intervals exclude amplifications of economically relevant size. Overall, the data do not support a narrative/values channel as the primary driver of the PD penalty.

Taken together, directed tests reject two natural alternatives—systematic political risk and narrative/values exposure—as first-order explanations of the PD effect. Combined with the Section 4.1 signatures, the evidence is most consistent with soft-information frictions at the screening stage: PD makes it harder to access and verify informal, local knowledge, thereby lowering match incidence even within the same market opportunity set.

4.3. Supporting Evidence from Realized Outcomes

The patterns in Sections 4.1–4.2—amplification in opaque settings and attenuation with information infrastructure, coupled with nulls for alternative channels—point to soft-information frictions at screening. This interpretation yields a testable implication for realized outcomes: when screening is costlier at high PD, the funding threshold tightens, so deals that do clear the bar should be positively selected (Ewens and Townsend 2020). Hence, conditional on investment, higher-PD matches should exhibit higher exit rates and lower failure rates, consistent with the idea that tighter screens improve realized performance (Kaplan and Strömberg 2004; Bottazzi et al. 2016; Bernstein et al. 2017).

I test this prediction on realized first investments between a VC and a startup, tracking exits through each startup's earliest IPO, acquisition, or write-off. The estimates in Table 8 show that PD is positively associated with IPO/M&A and negatively associated with write-offs, conditional on funding. A 0.10 increase in PD is associated with roughly +0.27 percentage points higher conditional IPO/M&A probability and −0.25 percentage points lower write-off probability. Specifications include VC-county and startup-county fixed effects, industry–year and stage fixed effects, with two-way clustering by industry–year and startup county.

These exit patterns provide additional support for the soft information friction interpretation. The finding that politically distant deals perform better conditional on funding is consistent with elevated screening costs that filter out weaker opportunities, reinforcing the evidence from opacity and infrastructure tests in Section 4.1.

5. Robustness and Diagnostic Test

Table 9 assesses the sensitivity of the main OLS estimates to measurement choices, sample restrictions, and a within–opportunity-set permutation placebo, holding constant the deal-anchored opportunity-set (group) fixed effects and the full control set.

In Panel A, I replace and augment the baseline construct and then break the signal by design. Replacing the continuous political distance with Same Party—an indicator equal to one when the VC and startup counties share the same plurality party—yields a positive coefficient of 0.016 (0.004), consistent with a higher propensity to invest in politically aligned locations. Jointly including PD (L1) and PD L2 leaves the PD coefficient negative and significant at −0.030 (0.017), while the L2 term is small and imprecise at −0.011 (0.023), indicating that the linear specification adequately captures the PD effect and that higher-order nonlinearities add little explanatory power. As a diagnostic, I randomly permute PD within each opportunity set while leaving all covariates and fixed effects unchanged; the placebo estimate is −0.001 (0.002), statistically indistinguishable from zero, confirming that the within-set correlation between PD and investment outcomes is not an artifact of the fixed-effects design itself.

In Panel B, I test whether the result is driven by ultra-local matches or California's outsized venture market. Dropping same-county pairs preserves the finding at −0.030 (0.010). Excluding observations with VCs in California yields −0.026 (0.013); excluding startups in

California yields −0.032 (0.011). Magnitudes remain close to baseline, indicating that neither within-county ties nor California exposure drives the estimate.

In Panel C, political preferences can map into sector tilts and differential regulatory exposure (Hong and Kostovetsky 2012). Survey evidence documents partisan differences—Democrats more favorable to healthcare, education, and government activity; Republicans more favorable to energy and materials (Gallup 2013; YouGov 2022). If the PD effect reflected sector-specific political preferences—with VCs and startups systematically avoiding cross-partisan matches in politically sensitive industries—the coefficient should attenuate when these sectors are excluded. It does not: removing Democrat-favored sectors yields −0.035 (0.013); removing Republican-favored sectors yields −0.034 (0.012); excluding all politically sensitive sectors together yields −0.035 (0.013). The magnitude is essentially stable across exclusions. The stability of the coefficient across exclusions indicates that PD operates broadly across industries rather than being concentrated in politically salient sectors.

Taken together, the checks indicate that the negative association between political distance and investment formation is not an artifact of local matching, California-specific dynamics, or the particular functional form used to measure political distance. The negative association becomes statistically indistinguishable from zero only in the within-set permutation placebo, where political distance is randomly reshuffled while preserving all other features of the data.

6. Conclusion

I show that partisan separation between investor and startup locations creates frictions in venture-capital match formation. Using a deal-anchored opportunity-set design on U.S. data from 2000–2024, a one-standard-deviation increase in PD lowers the probability of forming a first investment tie by 0.75 percentage points—roughly eight percent of the 9.6 percent baseline rate. An instrumental-variables design based on 1900 ethnic-composition distances yields larger magnitudes, consistent with a causal interpretation. Tests ruling out physical distance as a proxy indicate that the effect reflects a distinct dimension of local environment.

Mechanism evidence points to an information-based channel. The PD penalty amplifies where verification is hardest—first rounds, young firms, first county entry, and low-reach investors—and attenuates where information infrastructure is stronger—hub locations and during 2020–2023 pandemic period when remote diligence scaled rapidly. Directed tests provide no support for alternative explanations: the effect does not vary with election cycles, shared state policy regimes, or federal–state alignment, and it is not concentrated in narrative-heavy or consumer-facing domains. Conditional on funding, politically distant matches exhibit higher IPO/M&A rates and fewer write-offs, consistent with tighter screening when information is costly. The findings are robust to alternative measures of political distance, sample restrictions excluding California or same-county pairs, and exclusions of politically tilted sectors; a within-set permutation placebo yields null effects, confirming the design identifies true matching patterns rather than spurious correlation.

These results document an economic cost of political polarization operating through market matching. As Americans sort into politically homogeneous communities, cross-regional VC matching faces additional frictions that may widen geographic disparities in access to capital. The findings carry implications for both practice and policy. For investors, adopting standardized due-diligence protocols—data rooms, structured reference checks, and remote evaluation frameworks—can mitigate the information disadvantage in politically distant markets and expand feasible investment geography. For policymakers, the results highlight that efficient capital allocation depends not only on financial infrastructure but also on information networks that transcend partisan boundaries. Initiatives that facilitate knowledge exchange across regions—entrepreneur mobility programs, cross-regional accelerator networks, and neutral intermediary platforms—may help reduce political distance as a matching friction and narrow geographic disparities in startup funding.

Several questions remain for future work. First, while I identify soft information frictions as the primary channel, pinpointing the specific micro-mechanisms—network access, reference-frame divergence, or validator availability—would require data on VC-startup communication patterns and due-diligence processes. Second, understanding whether and how political distance affects post-investment value creation—board engagement, follow-on support, or exit facilitation—could further illuminate the costs of partisan sorting. Finally, extending the analysis to other matching markets where soft information matters—labor markets, supplier relationships, or scientific collaboration—would clarify the broader scope of political distance as an economic friction.

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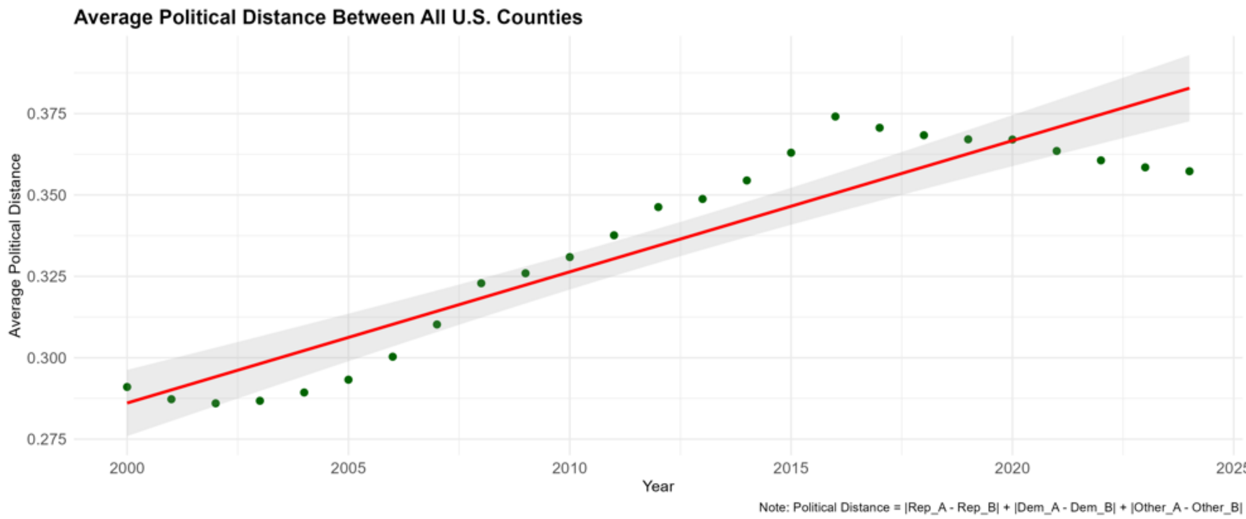
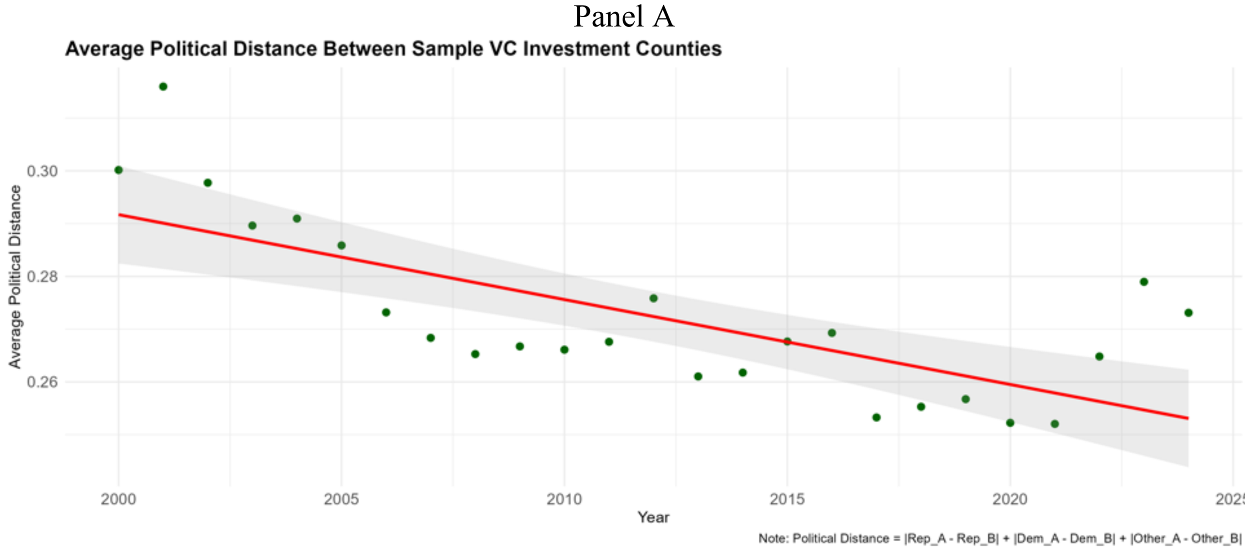
Figure 1. Political Distance Trends –

All Counties vs. Sample VC Investments

Average political distance (PD) over time for all U.S. county pairs (Panel A) and actual VC investment pairs in my sample (Panel B), 2000-2024. Panel A shows increasing political polarization nationally, while Panel B shows VC investments in my sample becoming concentrated in politically similar areas. $\mathrm{PD}_{i,j} = |\mathrm{Rep}\%_i - \mathrm{Rep}\%_j| + |\mathrm{Dem}\%_i - \mathrm{Dem}\%_j| + |\mathrm{Other}\%_i - \mathrm{Other}\%_j|$. Red lines show fitted trends with 95% confidence intervals (gray shaded areas).



Panel A



Panel B

Figure 2. Deal-Anchored Opportunity Set and Matched Counterfactuals

This schematic shows how I construct counterfactuals for each realized first investment by VC i in startup j within industry s, year t, and stage r. Two symmetric candidate sets are drawn within the same industry–year–stage market: (i) alternative VCs that invested in (s, t, r) but not in j; and (ii) alternative startups that raised in (s, t, r) but not from i. When more than five eligible candidates exist on a side, up to five are sampled uniformly at random without replacement; if fewer exist, all are included. The realized pair and its candidates inherit a common decision-set label, and estimation includes decision-set fixed effects so identification comes from within-set contrasts—in particular, differences in Political Distance across otherwise comparable dyads.

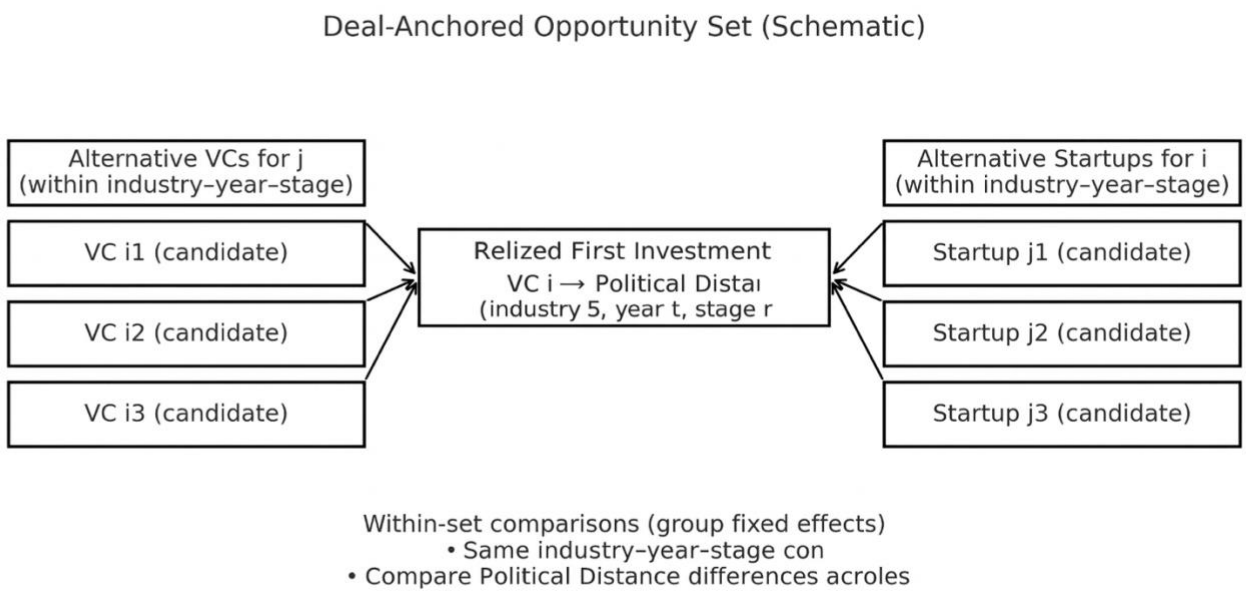


Figure 3. Distribution of Political Distance:

Actual Investments vs. Counterfactual Pairs

The distributions of political distance are broadly similar for actual investments and counterfactual pairs, with only a modest difference in mean values. This supports the plausibility of the dyadic sample construction and indicates that the matching approach yields comparable groups.

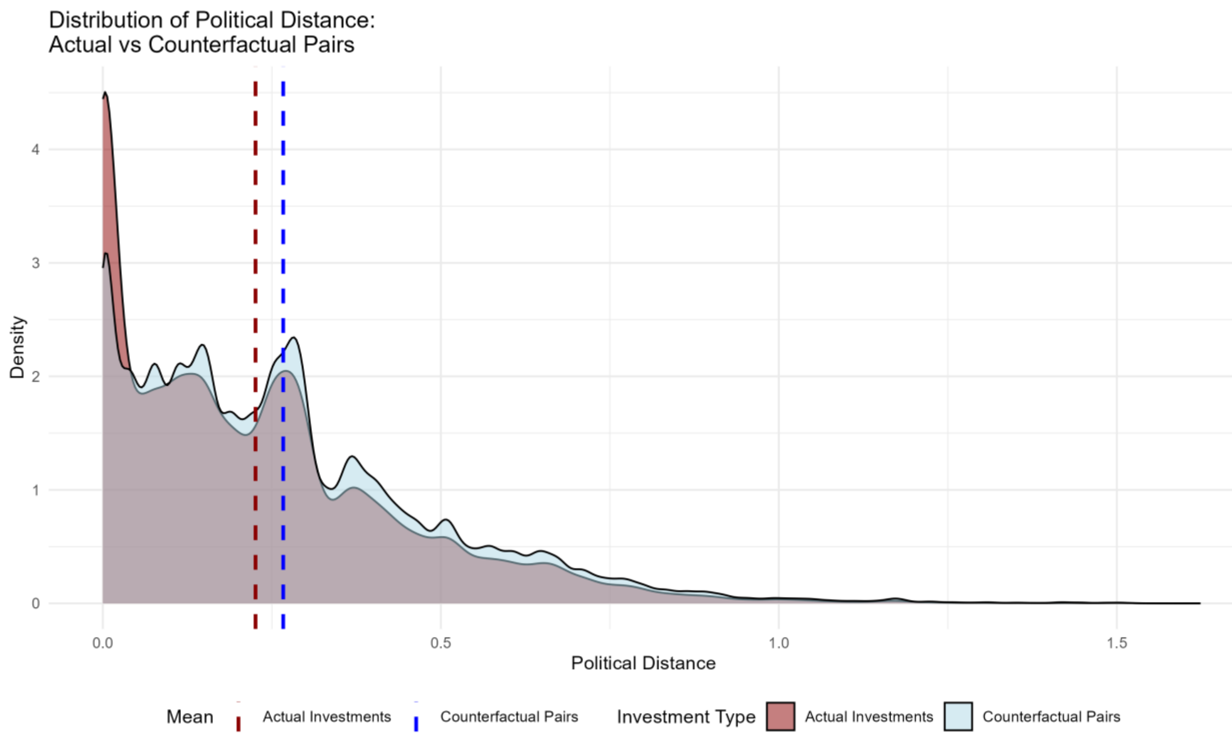


Table 1. Summary Statistics

This table presents summary statistics for the matched VC firm–startup dyadic sample covering 2000–2024. Panel A reports dependent variables measuring investment incidence and exit outcomes. Panel B reports key independent variables capturing political and historical ethnic distance. Panel C reports control variables related to geography, socioeconomic differences, and firm-level characteristics. Panel D reports interaction variables used in heterogeneity analyses, including round, startup and VC attributes, spatial proximity, and institutional environment indicators. All variables are defined in Appendix A.

Variable	N	Mean	SD	Q25	Median	Q75
Panel A. Dependent Variables						
Investment Event	1,264,271	0.096	0.294	0	0	0
IPO/M&A	120,762	0.275	0.447	0	0	1
Write-off	120,762	0.228	0.419	0	0	0
Panel B. Key Independent Variables						
Political Distance	1,264,271	0.262	0.22	0.093	0.222	0.374
Same Party	1,264,271	0.877	0.329	1	1	1
Political Distance L2	1,264,271	0.055	0.09	0.004	0.022	0.065
Ethnic Distance 1900	1,264,271	0.336	0.182	0.229	0.326	0.438
Panel C. Control Variables						
Same County	1,264,271	0.077	0.267	0	0	0
Startup Age (Years)	1,264,271	3.883	4.465	1	3	5
Startup Age	1,264,271	1.329	0.708	0.693	1.386	1.792
VC Firm Experience (Years)	1,264,271	13.846	15.941	4	9	18
VC Firm Experience	1,264,271	2.257	0.969	1.609	2.303	2.944
Geographic Distance	1,264,271	1.281	1.019	0.221	1.134	2.428
Education Distance	1,264,271	0.098	0.084	0.031	0.075	0.146
Income Distance	1,264,271	15.18	13.508	4.637	11.659	22.158
Population Distance	1,264,271	1.504	2.301	0.239	0.778	1.334
Industry Distance	1,264,271	0.399	0.182	0.311	0.398	0.486
Religious Distance	1,264,271	0.113	0.098	0.03	0.094	0.172
Panel D. Interaction Variables						
⩽ 100 Miles	1,264,271	0.199	0.399	0	0	0
⩽ 500 Miles	1,264,271	0.343	0.475	0	0	1
First Round	1,264,271	0.5	0.5	0	1	1
Young Startups	1,264,271	0.588	0.492	0	1	1
VC First Entry	1,264,271	0.645	0.478	0	1	1
Low Reach	1,264,271	0.335	0.472	0	0	1
VC Hub	1,264,271	0.324	0.468	0	0	1
Pandemic Years	1,264,271	0.337	0.473	0	0	1
Election Years	1,264,271	0.295	0.456	0	0	1
Same State	1,264,271	0.242	0.428	0	0	0
Startup State-Federal Alignment	1,264,271	0.551	0.497	0	1	1
Clear-Narrative	1,264,271	0.111	0.314	0	0	0
B2C & All	1,264,271	0.522	0.5	0	1	1
B2C	1,264,271	0.199	0.399	0	0	0

Table 2. Political Distance and VC Investment Decisions

This table examines the relationship between political distance and VC investment decisions. The sample consists of matched VC–startup pairs from 2000–2024. The dependent variable is Investment Event, which equals one if a VC makes an investment in a startup and zero otherwise. Political Distance is the L1 distance between county-level political preference vectors based on presidential election vote shares. Column (1) reports the baseline specification. Column (2) adds VC and startup characteristics as well as county-pair socioeconomic distance controls. Column (3) reports marginal effects from a conditional fixed-effects logit. All specifications include deal-anchored opportunity-set (group) fixed effects (within industry–year–stage, comparing the realized VC–startup pair with alternative VCs for the startup and alternative startups for the VC). Standard errors are clustered at the VC-county and startup-county levels and reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable		**Investment**	
	(1)	**(2)**	**(3)**
Political Distance	-0.038***	-0.034***	-0.434***
	(0.013)	(0.012)	(0.155)
Same County	0.142***	0.094**	0.657*
	(0.042)	(0.044)	(0.362)
Startup Age		-0.002	-0.022
		(0.003)	(0.039)
VC Firm Experience		0.004**	0.053**
		(0.002)	(0.021)
Geographic Distance		-0.025***	-0.334***
		(0.004)	(0.054)
Education Distance		0.016	0.175
		(0.049)	(0.614)
Income Distance		-0.000	-0.004
		(0.000)	(0.005)
Population Distance		-0.002	-0.018
		(0.002)	(0.025)
Industry Distance		-0.008	-0.104
		(0.025)	(0.309)
Religious Distance		-0.022	-0.277
		(0.020)	(0.259)
Group FE	YES	YES	YES
Observations	1,264,271	1,264,271	1,262,920
R^2	0.034	0.040	0.041

Table 3. Instrumental Variables

This table examines the relationship between political distance and VC investment decisions using an instrumental-variables approach. The instrument is Ethnic Distance 1900, the L1 distance between county-level ethnic composition vectors from the 1900 Census (German, Irish, Italian, English, Scottish, Polish, Norwegian). The sample consists of matched VC firm–startup pairs from 2000–2024. The dependent variable is Investment Event, which equals one if a VC firm makes an investment in a startup and zero otherwise. Column (1) reports the first stage, regressing Political Distance on the instrument; Column (2) reports the second stage (2SLS); Column (3) reports the reduced form, regressing Investment Event directly on the instrument. All specifications include deal-anchored opportunity-set (group) fixed effects. Standard errors are clustered at the VC-county and startup-county levels and reported in parentheses. The first-stage F-statistic exceeds 15. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

IV Analysis with Historical Ethnic Composition as Instrument			
Dependent Variable		**Investment**	
	First Stage	**Second Stage**	**Reduced Form**
	(1)	**(2)**	**(3)**
IV (Ethnic Distance 1900)	0.221***		-0.101***
	(0.054)		(0.028)
$\widehat{\text{Political Distance}}$		-0.456**	
		(0.186)	
Controls	YES	YES	YES
Group FE	YES	YES	YES
Observations	1,166,464	1,166,464	1,166,464
R2	0.499	-0.003	0.046
F-Test	> 15		

Table 4. Political Distance and Geography: Proxy Test

This table tests whether the political distance (PD) effect is merely a proxy for physical distance using matched VC–startup pairs from 2000–2024. The dependent variable is Investment Event, which equals one if a VC firm makes an investment in a startup and zero otherwise. Political Distance is the L1 distance between county-level political preference vectors based on presidential election vote shares. Column (1) interacts PD with geographic-distance quintiles formed from the full-sample distribution; Q1 (shortest quintile) is omitted, and Q2–Q5 indicate the 20–40th, 40–60th, 60–80th, and 80–100th percentiles, respectively. Column (2) interacts PD with ≤ 100 Miles, an indicator equal to one if the county-to-county distance is ≤ 100 miles. Column (3) interacts PD with ≤ 500 Miles, an indicator equal to one if the distance is ≤ 500 miles. Across specifications, interaction terms are small and statistically insignificant, indicating that the PD effect does not operate through physical distance. All specifications include deal-anchored opportunity-set (group) fixed effects and the full set of controls. Standard errors are two-way clustered at the VC-county and startup-county levels. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Dependent Variable		**Investment**	
	(1)	**(2)**	**(3)**
Political Distance × Geographic Distance Q2	-0.052 (0.074)		
Political Distance × Geographic Distance Q3	-0.043 (0.077)		
Political Distance × Geographic Distance Q4	-0.043 (0.077)		
Political Distance × Geographic Distance Q5	-0.079 (0.080)		
Political Distance × ⩽ 100 Miles		0.063 (0.077)	
Political Distance × ⩽ 500 Miles			-0.032 (0.023)
Political Distance	0.038 (0.076)	-0.024** (0.012)	-0.015 (0.012)
Geographic Distance Q2	-0.085*** (0.024)		
Geographic Distance Q3	-0.108*** (0.029)		
Geographic Distance Q4	-0.107*** (0.030)		
Geographic Distance Q5	-0.065* (0.034)		
⩽ 100 Miles		0.087*** (0.025)	
⩽ 500 Miles			0.080*** (0.014)
Controls	YES	YES	YES
Group FE	YES	YES	YES
Observations	1,264,271	1,264,271	1,264,271
R^2	0.048	0.047	0.043

Table 5. Mechanism—Information Opacity and Infrastructure

This table tests whether the political distance (PD) effect varies with information availability. The sample consists of matched VC firm–startup pairs from 2000–2024. The dependent variable is Investment Event, which equals one if a VC firm makes an investment in a startup and zero otherwise. Political Distance is the L1 distance between county-level political preference vectors based on presidential election vote shares. Column (1) interacts PD with First Round, which equals one when the financing round number is one. Column (2) interacts PD with Young Startups, which equals one when the startup's age is three years or less. Column (3) interacts PD with VC First Entry, which equals one when, prior to year t, the VC has not previously invested in the startup's county. Column (4) interacts PD with Low Reach, which equals one when the VC's cumulative geographic reach up to year t—measured as the maximum great-circle distance to any funded startup through year t—is at or below the cross-sectional median among VCs in year t. Column (5) interacts PD with VC Hub, which equals one when the VC is headquartered in the predefined hub counties (San Francisco, CA; Suffolk, MA; Bronx, NY; Kings, NY; New York, NY; Queens, NY; Richmond, NY). Column (6) interacts PD with Pandemic Years, which equals one when the investment year is 2020–2023. The interactions are negative and significant in settings where information is harder to verify (First Round, Young Startups, VC First Entry, Low Reach) and attenuate in settings with richer information infrastructure (VC Hub, Pandemic Years), consistent with information-based frictions. All specifications include deal-anchored opportunity-set (group) fixed effects. Standard errors are clustered at the VC-county and startup-county levels and reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	**Investment**					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Political Distance × First Round	-0.028*** (0.008)					
Political Distance × Young Startups		-0.010** (0.005)				
Political Distance × VC First Entry			-0.027** (0.012)			
Political Distance × Low Reach				-0.050*** (0.019)		
Political Distance × VC Hub					0.052* (0.030)	
Political Distance × Pandemic Years						0.041*** (0.010)
Political Distance	-0.020 (0.014)	-0.029** (0.013)	0.002 (0.018)	-0.013 (0.017)	-0.052** (0.021)	-0.046*** (0.014)
First Round	-0.001 (0.004)					
Young Startups		0.005*** (0.002)				
VC First Entry			-0.054*** (0.007)			
Low Reach				0.004 (0.008)		
VC Hub					-0.015 (0.018)	
Controls	YES	YES	YES	YES	YES	YES
Group FE	YES	YES	YES	YES	YES	YES
Observations	1,264,271	1,264,271	1,264,271	1,264,271	1,264,271	1,264,271
R^2	0.040	0.040	0.046	0.041	0.040	0.040

Table 6. Mechanism—Systematic Political Risk

This table tests whether the political distance (PD) effect varies with systematic political risk. The sample consists of matched VC firm–startup pairs from 2000–2024. The dependent variable is Investment Event, which equals one if a VC firm makes an investment in a startup and zero otherwise. Political Distance is the L1 distance between county-level political preference vectors based on presidential election vote shares. Column (1) interacts PD with Election Year, which equals one in presidential election years (2000, 2004, 2008, 2012, 2016, 2020, 2024). Column (2) interacts PD with Same State, which equals one when the VC and startup are in the same state. Column (3) interacts PD with Startup State–Federal Alignment, which equals one when, in year t, the startup's state government shares the federal administration's partisan affiliation. Interaction terms are small and statistically insignificant, while PD remains negative and significant, providing little support for a systematic political risk explanation. All specifications include deal-anchored opportunity-set (group) fixed effects. Standard errors are clustered at the VC-county and startup-county levels and reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable		**Investment**	
	(1)	**(2)**	**(3)**
Political Distance × Election Years	0.004		
	(0.003)		
Political Distance × Same State		-0.004	
		(0.063)	
Political Distance ×			-0.003
Startup State-Federal Alignment			(0.006)
Political Distance	-0.035***	-0.023**	-0.032**
	(0.012)	(0.011)	(0.014)
Same State		0.087***	
		(0.024)	
Startup State-Federal Alignment			0.000
			(0.004)
Controls	YES	YES	YES
Group FE	YES	YES	YES
Observations	1,264,271	1,264,271	1,264,271
R^2	0.040	0.045	0.040

Table 7. Mechanism—Narrative/Consumer Exposure

This table tests whether the political distance (PD) effect varies with narrative intensity and consumer exposure. The sample consists of matched VC firm–startup pairs from 2000–2024. The dependent variable is Investment Event, which equals one if a VC firm makes an investment in a startup and zero otherwise. Political Distance is the L1 distance between county-level political preference vectors based on presidential election vote shares. Column (1) interacts PD with Clear-Narrative, an indicator equal to one if the startup's primary industry falls in a pre-specified narrative-heavy set—Advertising & Marketing; Online Services; Consumer Publishing; Broadcasting; Entertainment Production; Blockchain & Cryptocurrency; Crowd Collaboration—and zero otherwise. Column (2) interacts PD with B2C & All, an indicator equal to one if the startup sells to consumers either exclusively (consumer-only) or jointly with businesses (mixed), and zero if it is enterprise-only. Column (3) interacts PD with B2C, an indicator equal to one if the startup is consumer-only, and zero otherwise (mixed or enterprise-only). Interaction terms are small and statistically insignificant, while PD remains negative and significant, providing little support for a narrative/values mechanism as the primary driver. All specifications include deal-anchored opportunity-set (group) fixed effects. Standard errors are clustered at the VC-county and startup-county levels and reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable		**Investment**	
	(1)	**(2)**	**(3)**
Political Distance × Clear-Narrative	0.003		
	(0.010)		
Political Distance × B2C & All		0.005	
		(0.007)	
Political Distance × B2C			0.003
			(0.008)
Political Distance	-0.034***	-0.036***	-0.035***
	(0.012)	(0.014)	(0.013)
B2C & All		0.001	
		(0.002)	
B2C			0.001
			(0.003)
Controls	YES	YES	YES
Group FE	YES	YES	YES
Observations	1,264,271	1,264,271	1,264,271
R^2	0.040	0.040	0.040

Table 8. Outcomes—Exit Performance

This table examines the relationship between political distance (PD) and realized outcomes for funded investments. The sample consists of VC–startup investments from 2000–2024. The dependent variables are IPO/M&A, which equals one if the startup exits via IPO or acquisition and zero otherwise, and Write-off, which equals one if the startup is written off or, in the absence of IPO/M&A, the startup's last observed investment year precedes 2020. Political Distance is the L1 distance between county-level political preference vectors based on presidential election vote shares. Political Distance is positively associated with IPO/M&A and negatively associated with Write-off, consistent with tighter screening at higher political distance. All specifications include VC-county fixed effects, startup-county fixed effects, industry–year fixed effects, and stage fixed effects. Standard errors are two-way clustered at the industry–year and startup-county levels and reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	**IPO/M&A**	**Write-off**
	(1)	**(2)**
Political Distance	0.027**	-0.025**
	(0.013)	(0.010)
Controls	YES	YES
VC County FE	YES	YES
Startup County FE	YES	YES
Industry-Year FE	YES	YES
Stage FE	YES	YES
Observations	120,762	120,762
R^2	0.346	0.317

Table 9. Robustness and Diagnostic Test

This table presents robustness tests for the main results from OLS regressions. The sample consists of matched VC firm–startup pairs from 2000–2024. The dependent variable is Investment Event, which equals one if a VC firm makes an investment in a startup and zero otherwise. Panel A replaces the baseline construct and reports a placebo: Column (1) replaces Political Distance with Same Party, an indicator that equals one if the VC and startup counties share the same plurality party; Column (2) jointly includes Political Distance (L1) and Political Distance L2; Column (3) reports a placebo in which the political-distance measure is randomly permuted within each deal-anchored opportunity set, leaving all covariates and fixed effects unchanged. Panel B applies sample restrictions: Column (1) drops same-county pairs; Column (2) excludes observations with VCs located in California; Column (3) excludes observations with startups located in California. Panel C examines sector composition by excluding politically tilted sectors: Column (1) excludes Democrat-favored sectors (healthcare; government activity; academic and educational services); Column (2) excludes Republican-favored sectors (energy; basic materials); Column (3) excludes all politically sensitive sectors together. All specifications include deal-anchored opportunity-set (group) fixed effects. Standard errors are clustered at the VC-county and startup-county levels and reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Measurement

Dependent Variable		**Investment**	
	(1)	**(2)**	**(3)**
Same Party	0.016***		
	(0.004)		
Political Distance		-0.030*	
		(0.017)	
Political Distance L2		-0.011	
		(0.023)	
Placebo Political Distance			-0.001
			(0.002)
Controls	YES	YES	YES
Group FE	YES	YES	YES
Observations	1,264,271	1,264,271	1,264,271
R^2	0.040	0.040	0.040

Panel B. Sample Restrictions

	(1)	**(2)**	**(3)**
Political Distance	-0.030***	-0.026**	-0.032***
	(0.010)	(0.013)	(0.011)
Controls	YES	YES	YES
Group FE	YES	YES	YES
Observations	1,166,883	721,380	737,807
R^2	0.045	0.087	0. 078

Panel C. Sector Composition

	Exclude Democrat-Favored Sectors	**Exclude Republican-Favored Sectors**	**Exclude All**
	(1)	**(2)**	**(3)**
Political Distance	-0.035***	-0.034***	-0.035***
	(0.013)	(0.012)	(0.013)
Controls	YES	YES	YES
Group FE	YES	YES	YES
Observations	1,007,616	1,254,327	997,672
R^2	0.043	0.038	0.040

Appendix A. Variable Definitions

Dependent Variables	
Investment Event	Binary indicator that equals one if VC i makes an investment in startup j in year t, and 0 otherwise.
IPO/M&A	Binary indicator that equals one if the startup achieves a successful exit through IPO or M&A, and 0 otherwise.
Write-off	Binary indicator that equals one if the startup is marked as written off or, in the absence of IPO/M&A, the startup's last observed investment year precedes 2020 (the bankrupt flag with a five-year observation window), and zero otherwise.
Key Independent Variables	
Political Distance	L1 distance between the VC county i and startup county j presidential vote-share vectors (Rep, Dem, Other). Calculated as $\lvert \mathrm{Rep\%}_i - \mathrm{Rep\%}_j \rvert + \lvert \mathrm{Dem\%}_i - \mathrm{Dem\%}_j \rvert + \lvert \mathrm{Other\%}_i - \mathrm{Other\%}_j \rvert$. Annual shares are obtained in election years; non-election years are filled by linear interpolation within county (components renormalized to sum to 1).
Same Party	Binary indicator that equals one if the preferred party (according to the majority vote share) of VC and startup counties are the same, and 0 otherwise.
Political Distance L2	The Euclidean (L2-norm) distance between the political preference vectors of VC and startup counties. Calculated as $\sqrt{(\mathrm{Rep\%}_i - \mathrm{Rep\%}_j)^2 + (\mathrm{Dem\%}_i - \mathrm{Dem\%}_j)^2 + (\mathrm{Other\%}_i - \mathrm{Other\%}_j)^2}$
Ethnic Distance 1900	L1-norm distance between ethnic composition vectors of VC firm and startup counties based on 1900 Census data. Calculated using major European ethnic groups (German, Irish, Italian, English, Scottish, Polish, Norwegian).
Control Variables	
Same County	Binary indicator that equals one if the VC and startup are in the same county, and 0 otherwise.
Startup Age (Years)	Number of years since the startup was founded, measured as the difference between the investment year and the startup's founding year.
Startup Age	Natural logarithm of one plus Startup Age Years.
VC Firm Experience (Years)	Number of years since the VC was founded, measured as the difference between the investment year and the VC's founding year.
VC Firm Experience	Natural logarithm of one plus VC Experience Years.
Geographic Distance	Great-circle distance between the VC's and the startup's counties divided by 1,000 for scaling (thousand miles). Source: NBER County Distance Database.
Education Distance	Absolute difference between the two counties' shares of residents aged 25 or older with a college degree or higher. Education shares are constructed from the decennial Census (2000, 2010, 2020) and ACS five-year tabulations, with linear interpolation used to obtain annual county-year values.
Income Distance	Absolute difference between the two counties' per-capita income, reported in thousand dollars. Income data are taken from the decennial Census (2000, 2010, 2020) and ACS five-year tabulations, and annualized via linear interpolation for non-census years.
Population Distance	Absolute difference between the two counties' total populations, divided by 1,000,000 for scaling (millions of persons). Population counts come from the decennial Census (2000, 2010, 2020) and ACS five-year tabulations, interpolated to annual frequency for non-census years.
Industry Distance	L1 (Manhattan) distance between the two counties' industry employment-share vectors. Each vector contains employment shares by Bureau of Economic Analysis (BEA) industry classifications.

(Continued)

Religious Distance	Absolute difference between the two counties' overall religious participation rates. Rates are derived from the Association of Religion Data Archives (ARDA) for benchmark years 2000, 2010, and 2020, and held piecewise constant by decade: 2000–2009 use the 2000 benchmark, 2010–2019 use 2010, and 2020–2024 use 2020.
Interaction Variables	
≤ 100 Miles	Binary indicator that equals one if the geographic distance between VC's and the startup's counties is less or equal to 100 miles, and zero otherwise.
≤ 500 Miles	Binary indicator that equals one if the geographic distance between VC's and the startup's counties is less or equal to 500 miles, and zero otherwise.
First Round	Binary indicator that equals one for round number equals to 1, and 0 otherwise.
Young Startups	Binary indicator that equals one if Startup Age (Years) ≤ 3 years, and 0 otherwise.
VC First Entry	Binary indicator that equals one if, prior to year t, VC i has never invested in county j, and 0 otherwise.
Low Reach	Binary indicator that equals one if the VC's cumulative geographic reach up to year t —measured as the cumulative maximum great-circle distance from the VC's county to any funded startup's county through year t—is at or below the cross-sectional median among VCs in year t, and zero otherwise.
VC Hub	Binary indicator that equals one if the VC is in one of the following counties: San Francisco CA (06075), Suffolk MA (25025), Bronx NY (36005), Kings NY (36047), New York NY (36061), Queens NY (36081), Richmond NY (36085), and zero otherwise. (Nguyen et al., 2023)
Pandemic Years	Binary indicator that equals one if the investment year is 2020, 2021, 2022, or 2023, and zero otherwise.
Election Years	Binary indicator that equals one if the investment year is in 2000, 2004, 2008, 2012, 2016, 2020, or 2024, and 0 otherwise.
Same State	Binary indicator that equals one if the VC firm and startup are located in the same state, and 0 otherwise.
Startup State-Federal Alignment	Binary indicator that equals one if the startup's state government shares the same partisan affiliation as the federal administration in current presidential election cycle, and 0 otherwise.
Geographic Distance Q (1-5)	Quintiles are defined from the full-sample distribution of the geographic distance: Q1 (shortest), Q2 (20–40th pct.), Q3 (40–60th pct.), Q4 (60–80th pct.), Q5 (80–100th pct.). (In regressions with quintile interactions, Q1 is the omitted category.)
Clear-Narrative	An indicator variable equal to one if the startup's primary industry falls within a pre-specified set of narrative-heavy sectors where investment evaluation is especially sensitive to framing and stories: Advertising & Marketing; Online Services; Consumer Publishing; Broadcasting; Entertainment Production; Blockchain & Cryptocurrency; and Crowd Collaboration; and 0 otherwise.
B2C & All	An indicator variable equal to one if the startup's business model involves selling to consumers, either exclusively (pure consumer-facing) or jointly with businesses (mixed B2B/B2C), and 0 otherwise.
B2C	An indicator variable equal to one if the startup is purely consumer-facing (sells exclusively to end consumers), and 0 otherwise.