Political Distance and Venture Capital Match Formation[1]

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Abstract

I study whether partisan separation between investor and startup locations shapes who gets funded in venture capital. Using a within-market, deal-anchored design on U.S. data from 2000–2024 and measuring political distance (PD) from county presidential vote shares, I find that a one-standard-deviation increase in PD lowers investment incidence by about 0.7–0.8 percentage points—roughly eight percent of the baseline rate. The association is robust across specifications and is corroborated by an instrumental-variables analysis. Mechanism tests are most consistent with a soft-information channel at screening: the penalty is larger when information is harder to verify and smaller where information is richer or more standardized. Alternative explanations tied to generic investor ability, systematic political risk, sectoral exposure, and pure geography receive little support. Conditional on funding, higher PD is associated with more IPOs or M&A and fewer write-offs, consistent with selection at a higher funding bar.

JEL Codes: G24, G34, D83, L26
Key Words: Venture Capital, Political Distance, Soft Information, Information Frictions

[1] I used ChatGPT for copy-editing assistance. All errors are my own.

1. Introduction

Venture capital (VC) activity remains heavily concentrated geographically, with approximately 60% of all VC deals in 2023 occurring in just five states—California, Massachusetts, New York, Texas, and Florida (NVCA 2024). Over the same period, American communities have become increasingly politically segregated, with the number of "super-landslide" counties—where one party wins with over 80% of the vote—rising from fewer than 200 in 2004 to nearly 700 by 2020 (Sabato's Crystal Ball 2022). This raises a natural question: as political polarization intensifies while venture capital clusters in specific hubs, does partisan separation between investor and startup locations systematically affect which investor-startup pairs form?

This question matters for several reasons. First, if political separation creates frictions in capital allocation, it could lead to systematic underinvestment in certain regions or sectors, reducing overall economic efficiency. Second, understanding these patterns has implications for regional development policy and the design of innovation ecosystems. Third, as political polarization continues to intensify, any effects on entrepreneurial finance may become increasingly consequential for the allocation of risk capital.

A large literature demonstrates that spatial proximity and social connections shape investment relationships. Portfolios tilt toward geographically close targets (French and Poterba 1991; Coval and Moskowitz 1999; Huberman 2001), local networks facilitate search and monitoring (Sorenson and Stuart 2001; Hochberg et al. 2007), and social proximity eases the exchange of soft information (Bengtsson and Hsu 2015; Bernstein et al. 2017; Gompers et al. 2020). Political preferences have also been shown to influence household financial decisions, affecting both portfolio composition and the interpretation of firm attributes (Hong and

Kostovetsky 2012). However, we know less about whether local partisan differences across counties leave a measurable footprint on cross-regional VC matching.

Building on Pan et al. (2025), who show that county-level political differences are strong enough to affect portfolio holdings through partisan disagreement, I examine whether similar forces operate in the venture capital matching process. County presidential vote shares capture enduring differences in local preferences and beliefs that arise from partisan sorting at fine spatial scales. When VC and startup counties are farther apart in this political space, their underlying priors and reference frames may differ substantially, potentially making consensus harder to reach during the screening process. In line with Pan et al. (2025), I proxy a 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). The appeal of this measure is practical as well as conceptual. It is observed uniformly over a long horizon, allows annual tracking of how separation maps into match formation, and does not rely on fragile, pre-assigned labels of "partisan sectors" or individuals that can shift over time. It also aligns with the geographic granularity at which VC headquarters and startup locations are observed, permitting coherent construction of a county-pair and year panel. Micro political labels for founders and VC partners would not offer comparable coverage. Voter-file access and formats vary across states and years; donation records observe only those who give and often do not map cleanly to party; identities and locations of decision makers are difficult to trace consistently over long windows. As a primary measure in a national, multi-decade setting, such labels are therefore partial and selective. County vote shares, by contrast, provide a scalable and stable basis for a place-based analysis of how separation relates to match formation.

A simple descriptive contrast further motivates the analysis. Nationally, the average county-to-county PD has trended upward since 2000, consistent with rising polarization; in contrast, among realized VC–startup matches in our data, the average PD has declined over time, indicating increasing concentration of deals among politically similar places. These facts are visualized in Figure 1.

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. The key regressor, PD e, follows Pan et al. (2025): I proxy each county's partisan environment with presidential vote shares and compute county-pair separation as the difference across the Republican, Democratic, and Other components.

Building on the data structure and on county-level political differences as revealed preferences (Pan et al. 2025), I expect separation to impede agreement at the formation margin. Accordingly, my first hypothesis (H1) is that, within a deal-anchored opportunity set, higher PD between VC and startup locations predicts a lower investment incidence for that dyad. To test H1, I estimate linear probability models that compare each realized match with its feasible alternatives within identical industry–year–stage opportunity sets, including decision-set fixed effects with standard errors two-way clustered at the VC-county and startup-county levels so identification comes from within-set contrasts. Consistent with this expectation, PD is negatively related to whether a VC–startup pair forms a deal, and the effect is economically meaningful: a one–standard-deviation increase in PD is associated with roughly a 0.7–0.8 percentage-point decline in investment incidence—about eight percent of the baseline rate. The association persists when co-located county pairs are excluded, reverses sign as expected when PD is replaced with a same-party indicator (pairs sharing a majority party invest more often), remains when an alternative political-distance metric is added alongside the baseline measure, and is not driven by the composition of California VCs or startups. Placebo exercises that randomly reassign PD within opportunity sets yield coefficients centered near zero, reinforcing that county-level partisan separation leaves a measurable footprint on match formation.

To identify what mechanism generates this relationship, I develop and test several competing explanations. Guided by evidence that county vote shares encode persistent local preferences (Pan et al. 2025) and that VC screening relies on qualitative, non-codified assessments (Kaplan and Strömberg 2004; Gompers et al. 2020), I posit a soft-information channel: partisan separation raises the cost of reaching agreement at the funding hurdle when information is hard to verify. This generates two complementary predictions. Hypothesis 2 (Opacity Amplification) predicts that the PD penalty is stronger in more opaque settings—first-round investments, young startups with less disclosure relative to mature startups, VC first entry into the startup's county, and VCs with relatively lower historical investment distance indicating weaker information-gathering ability. Hypothesis 3 (Information-Infrastructure Attenuation) predicts that the PD penalty attenuates where information is richer or more standardized—VCs located in hub markets with more information and during the pandemic years (2020–2023) when remote diligence scaled. Empirically, the interactions line up with both predictions: the PD penalty is larger in opaque settings and attenuates in VC hubs and during 2020–2023.

I systematically test four alternative mechanisms. A natural alternative mechanism centers on VC capability. Experienced and better‑connected VCs use syndication networks to bridge informational gaps across space (Sorenson and Stuart 2001; Hochberg et al. 2007), and greater specialization shifts evaluation toward repeatable playbooks and comparables, increasing the share of hard information in screening (Kaplan and Strömberg 2004; Gompers et al. 2020). If PD captures divergence in local priors and reference frames, then VCs with stronger generic ability should be less reliant on place‑specific cues and more able to reconcile differences in beliefs. Accordingly, Hypothesis 4 (Generic-Ability Attenuation) states that: within a deal-anchored opportunity set, the negative association between PD and investment incidence should be weaker for VCs who are either sector-level experts or industry-level specialists. Empirically, I do not find attenuation: interactions of PD with Expert and with Specialist are small and statistically indistinguishable from zero, offering no evidence that PD operates through a generic-ability mechanism.

Another possibility is national political risk: real investment slows when policy uncertainty rises, and clearer, unified policy regimes can ease frictions (Julio and Yook 2012; Jens 2017; Baker et al. 2016; Pástor and Veronesi 2012). If PD merely proxies such systematic political risk, then the PD penalty should intensify when nationwide uncertainty is elevated, weaken when VC and startup share a common policy regime, and weaken when the startup's state government is co-partisan with the federal administration, plausibly reflecting lower policy frictions and greater access to federal support. Hypothesis 5 (Systematic-Risk Channel) predicts that within a deal-anchored opportunity set, the negative association between PD and investment incidence should strengthen in election years and attenuate for VC–startup pairs in the same state and when the startup's state is aligned with the federal administration. Empirically, none of these predictions materialize, providing no support for a systematic-risk channel.

Sector composition is a further candidate mechanism: political preferences can map into sector tilts and regulatory exposure (Hong and Kostovetsky 2012). If PD operates because some industries are systematically politicized or differentially regulated, then removing such sectors should materially shrink the association. So, here is my Hypothesis 6 (Sectoral-Exposure Channel): the negative association between PD and investment incidence should weaken when I exclude Democrat-favored sectors, Republican-favored sectors, and all politically sensitive sectors. Empirically, the magnitude is essentially unchanged across all exclusions, offering no support for a sectoral-exposure mechanism.

Finally, PD might simply proxy geography. A large literature links physical proximity to investment intensity via monitoring costs and local information (French and Poterba 1991; Coval and Moskowitz 1999; Huberman 2001; Sorenson and Stuart 2001). If PD simply operates through a pure geographic-friction channel, then the PD penalty should vary strongly with distance—growing at long ranges and disappearing or reversing at very short ranges. So, Hypothesis 7 (Geographic-Friction Equivalence) is that within a deal-anchored opportunity set, the association between PD and investment incidence should vary strongly across Geographic Distance quintiles and be overturned at short ranges ($\leqslant$100 miles; $\leqslant$500 miles). Empirically, I do not find such patterns: the PD association does not hinge on physical distance.

Taken together, the evidence points to a soft information channel as the operative mechanism in match formation. The same soft-information logic delivers a prediction for realized outcomes. Tighter screening at the funding margin is associated with stronger realized performance among funded ventures (Ewens and Townsend 2020). If PD reflects soft-information frictions at match formation, then higher PD simply raises the bar to closing: only unusually strong opportunities clear the hurdle. So, here is my Hypothesis 8 (Selection at the Funding Hurdle): conditional on funding, higher PD is associated with higher IPO/M&A and lower Write-off rates. Empirically, I find higher IPO/M&A and lower write-offs at greater PD, further supporting the soft-information channel implied by H2/H3.

This research makes four main contributions. First, I provide systematic, deal-anchored evidence that PD between VC and startup locations shapes match formation. Using identical industry–year–stage decision sets to hold market opportunity fixed, I show that PD depresses the likelihood that a VC–startup dyad closes, extending frictions in entrepreneurial finance beyond geographic distance (Sorenson and Stuart 2001) and beyond social networks (Hochberg et al. 2007; Bengtsson and Hsu 2015). This moves recent political–finance insights from portfolio holdings (Hong and Kostovetsky 2012; Pan et al. 2025) to the formation of new investment relationships.

Second, I document a novel temporal divergence that quantifies polarization's market footprint: while PD among all U.S. county pairs rises over 2000–2024—consistent with geographic sorting and polarization (Bishop 2008; McDonald 2011; Sabato's Crystal Ball 2022)—the average PD among realized VC–startup matches declines. This "environmental polarization, transactional homophily" stylized fact links the literatures on VC spatial concentration (Samila and Sorenson 2011; Chen et al. 2010) and political sorting (Chen and Rodden 2013) and suggests that growing partisan separation binds at the matching margin, transmitting political polarization to capital allocation through market mechanisms rather than purely political channels.

Third, I show that the PD–matching relationship reflects selection rather than risk pricing: conditional on funding, startups from politically distant locations exhibit higher IPO/M&A and lower write-off rates. This aligns with evidence on tighter screening of underrepresented groups in VC (Ewens and Townsend 2020) and implies systematic underinvestment in high-quality opportunities when they are embedded in politically dissimilar environments.

Fourth, I surface an economic cost of political polarization that operates through capital allocation. As Americans sort into politically homogeneous communities, cross-regional VC matching faces additional frictions, potentially amplifying regional disparities in entrepreneurial finance and speaking to broader concerns about the economic consequences of polarization (Autor et al. 2020).

The remainder of the paper is organized as follows. Section 2 introduces the data, variable construction, and empirical strategy; Section 3 reports the baseline matching results together with robustness and diagnostics; Section 4 investigates mechanisms, rules out alternatives, and connects the estimates to exit selection; Section 5 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 (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.

[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).

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 decision 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 follow-on financing for at least five years by December 31, 2024 (i.e., the startup's last observed financing year is $\leqslant$2019). Both outcomes are coded at the deal level and are observed only for funded matches.

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 counties i and j in year t as:

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

[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

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). Let the sum of these interpolated counts be the county's total interpolated votes in year t. I then define annual vote shares as each party's interpolated votes divided by the interpolated total. 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 cognitive-friction effects in cross-county VC 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 also aligns with the proposed mechanism: when a VC's and a startup's home counties are farther apart in this political space, the mismatch between embedded interpretive frameworks increases screening frictions and lowers the likelihood of agreement.

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.

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

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, 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

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

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 a set of indicators that vary along information opacity, organizational experience, spatial context, and the institutional environment.

I capture opacity with four markers. First Round flags initial financings, where disclosure is thinnest and reliance on soft signals is greatest. Following Babina et al. (2020), Young Startups identifies firms that are at most three years old, a stage with limited track records. VC First Entry marks cases where the investor has not previously invested in the startup's county, reducing access to local networks. 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, indicating weaker information-gathering range.

VC Hub and Pandemic Years are used to gauge the role of information infrastructure. VC Hub identifies investors based in San Francisco, Suffolk County in Massachusetts, and the five counties of New York City, markets where deal flow, intermediaries, and comparable sets are dense (Nguyen et al. 2023); Pandemic Years covers 2020 through 2023, when remote diligence scaled and standardized processes diffused.

I test whether generic capability attenuates the effect using two specialization tags. Expert turns on when, prior to year t, the investor's first-time deals to date have been confined to a single economic sector. Specialist is analogous at the industry level within a sector. If specialization substitutes for place-based cues, the PD penalty should shrink for these investors.

To separate soft-information frictions from national political risk, I interact with Election Years, Same State, and Startup State–Federal Alignment. Election Years marks the seven presidential years across the sample. Same State indicates that the investor and startup operate under a common state policy regime. 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 allow PD to vary nonlinearly with geography. I partition great-circle distance into within-sample quintiles and interact with each, omitting the first quintile in regressions. I also include two short-range indicators that turn on when counties lie within 100 miles and within 500 miles, respectively, to test whether proximity eliminates or reverses the PD association.

2.6. Sample Construction

Testing how TG 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 decision 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.

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. Diagnostics—including within-set PD permutations—are reported in the robustness section and show coefficients centered near zero.

3. Main Results

3.1. Descriptive Statistics

Table 1 presents summary statistics for the matched sample of 1,264,271 VC–startup pairs. Panel A shows that 9.6 percent of pairs result in realized first investments. 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. PD averages 0.262 with a standard deviation of 0.220 and 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. 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. Education distance averages 0.098, and industry distance averages 0.399, indicating meaningful differences beyond geography.

Panel D reports interaction variables used in mechanism tests. First-round financings account for 50.0 percent of observations; 58.8 percent involve young startups (age $\leqslant$ 3). In 64.5 percent of pairs, the VC has no prior investment in the startup's county. Hub locations account for 32.4 percent, 33.7 percent fall in 2020–2023, and 34.3 percent lie within 500 miles.

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 lowers the investment probability by about 0.75 percentage points. 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, implying that each additional year of experience is linked to roughly a 0.4 percentage-point higher probability; 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 are consistent with an organizational cognition mechanism in which interpretive misalignment generates evaluation frictions that hinder match formation.

3.3. Robustness and Diagnostic Checks

Table 3 evaluates whether the political-distance result is sensitive to alternative samples and measurement choices while holding fixed the deal-anchored opportunity-set fixed effects and the full control set.

Column 1 excludes ultra-local matches; the result remains intact. In the cross-county sample, the coefficient on PD equals −0.030 with a standard error of 0.010 and is statistically significant, indicating that the negative relationship is not a mechanical artifact of within-county ties. Column 2 reports a coarser, binary notion of alignment. Replacing the continuous distance with an indicator for shared majority party between the VC and startup counties produces a positive and significant coefficient of 0.016, consistent with a higher propensity to invest in politically aligned places. Column 3 adds PD (L2) to the model and shows that the coefficient on PD remains significantly negative at −0.030, whereas the L2 coefficient is small at −0.011 and imprecisely estimated. This contrast suggests that once PD is accounted for, the incremental information in PD (L2) is limited within the same opportunity sets. In Column 4 and Column 5, VCs located in California and startups located in California are dropped, respectively, to assess whether the effect of PD is driven by California's outsized VC market; in both cases the PD coefficient remains negative and statistically significant. I also exclude VCs or startups located in California simultaneously; the result does not change (not reported in the table). Finally, Column 6 reports a placebo that breaks the PD signal: when the political-distance measure is randomly permuted within each deal-anchored opportunity set, the estimate is statistically indistinguishable from zero, confirming that the design is not picking up spurious correlation.

Taken together, these robustness tests and diagnostics show that the negative association between PD and investment formation is not an artifact of local matching, California-specific dynamics, or a particular functional form; rather, it reflects a stable pattern that disappears when the underlying signal is deliberately removed.

3.4. Instrumental Variables

To address endogeneity concerns beyond the rich controls and opportunity-set fixed effects, Table 4 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 plausibly affect contemporary VC investment only through their influence on political culture, given the fundamental economic transformations over the intervening century.

In Column 1, the first stage is strong and in the expected direction. 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.

4. Mechanisms Analysis

Having established that PD reduces the likelihood of investment within deal-anchored opportunity sets, I examine why this occurs. The tests proceed from an information-based mechanism to a set of rational alternatives. I first evaluate whether PD bites hardest when screening relies on non-verifiable ("soft") information and eases where information is richer or more standardized. This motivation follows work showing that county vote shares encode persistent local preferences (Pan et al. 2025) and that VC screening leans heavily on qualitative assessments, comparables, and references beyond audited, codified data (Kaplan and Strömberg 2004; Gompers et al. 2020). I then contrast these predictions with three alternatives—generic ability, systematic policy risk, and sectoral risk premia—as well as a pure geographic-friction explanation.

4.1. Homophily-in-Soft-Information Mechanism

Guided by evidence that county vote shares are persistent markers of slow-moving local preferences and routines (Pan et al. 2025) and that venture screening and contracting place substantial weight on non-codified information—references, tacit comparables, narrative assessments—when claims are hard to verify (Kaplan and Strömberg 2004; Gompers et al. 2020; Stein 2002; Liberti 2019), the first mechanism I test is a soft-information channel. The idea is that when a VC and a startup sit in counties that encode routines differently, translation costs at the funding margin rise—more back-and-forth to align materials, more redlining, and more bespoke protections before signing. The literature also specifies where soft information is most consequential and where thicker information infrastructures compress these costs. Thin disclosure and short operating histories push investors toward relationships and qualitative signals (Kaplan and Strömberg 2004; Liberti 2019); limited local ties and sparse networks make information acquisition and interpretation more expensive (Sorenson and Stuart 2001), while dense ecosystems and intermediary networks in hub markets reduce search and due-diligence frictions (Hochberg et al. 2007). Building on these insights, I test two comparative statics inside the same deal-anchored decision sets. Opacity is higher in (i) First Round financings, where accumulated disclosure is minimal; (ii) Young Startups, whose track records are thin; (iii) VC First Entry into a startup's county, which limits local ties and tacit knowledge; and (iv) Low Reach VCs, whose below-median historical geographic reach indicates narrower information-gathering breadth. If soft information is the margin, PD should be more punitive in these settings. Information infrastructure is thicker when VCs are headquartered in VC Hub counties—denser ecosystems with more intermediaries and richer comparables (Sorenson and Stuart 2001; Hochberg et al. 2007)—and during the Pandemic Years (2020–2023), when remote diligence and standardized data rooms scaled across markets. If infrastructure compresses reliance on soft information, the PD association should attenuate in these contexts.

Table 5 tests these implications within the same deal-anchored decision-set design. In the opacity settings, the PD slope steepens exactly where verification is hardest. In Column 1 for first rounds, the effect is about twice as large as in non–first rounds—on the order of 0.48 percentage points lower within-set investment probability for each 0.10 increase in PD, versus about 0.20 percentage points otherwise. For young startups in Column 2, thinner operating histories push the simple slope to about 0.39 percentage points per 0.10 increase. When a VC enters a county for the first time in Column 3, the PD effect is materially more negative—about 0.25 percentage points per 0.10 increase—consistent with limited local ties and tacit knowledge on first contact. The largest amplification appears for low-reach investors in Column 4: the implied simple slope is about 0.63 percentage points per 0.10 increase, compared with roughly 0.13 percentage points for higher-reach peers. By contrast, the information-infrastructure settings attenuate the association. In Column 5 for VC hubs, the interaction nearly offsets the baseline and the net PD effect is close to zero. In Column 6 during the pandemic years 2020–2023, the residual PD effect is small—about 0.05 percentage points per 0.10 increase—down from roughly 0.46 percentage points pre-pandemic, consistent with scaled remote diligence and more standardized data rooms.

Taken together, opacity makes the PD penalty larger, while thicker information infrastructures attenuate it. These patterns point to PD capturing a soft-information mismatch that raises the fixed costs of diligence and contracting for cross-county pairs, thereby lowering the within-set investment probability.

4.2. Alternative Mechanisms: Generic-Ability, Systematic Risk, Sectoral Risk Premium, and Pure Geographic Friction

A natural concern is that the political-distance result might be an artifact of other forces. I therefore examine four prominent alternatives within the same deal-anchored opportunity-set design, holding the full control vector and group fixed effects constant and clustering standard errors by VC and startup counties.

I begin with generic ability. Dense networks and accumulated know-how can reduce information asymmetry and ease coordination by supplying shared templates, third-party certification, and relational enforcement (Granovetter 1985; Uzzi 1999; Hochberg et al. 2007). If such capacity substitutes for the translation costs emphasized above, the PD penalty should be smaller for VCs with sharper focus and playbooks. Table 6 interacts PD with Expert and

Specialist indicators. The interactions are small and statistically indistinguishable from zero, while PD remains negative and precisely estimated. The absence of attenuation once hub embeddedness is accounted for suggests that general investor capacity does not explain the PD effect; what matters is the broader information infrastructure rather than investor specialization per se.

Next is systematic political risk. Investment tends to slow when nationwide policy uncertainty rises and to ease when regimes are unified or predictable (Julio and Yook 2012; Jens 2017; Baker et al. 2016; Pástor and Veronesi 2012). If PD were proxying this channel, the penalty should intensify in presidential election years and weaken when the VC and startup share a state policy regime or when the startup's state aligns with the federal administration. Table 7 shows none of these predictions materialize. Interactions of PD with Election Year, Same State, and Startup State–Federal Alignment are small and statistically insignificant. PD itself remains negative, and while Same State has a positive main effect consistent with familiar local advantages, it does not weaken the PD slope. This evidence does not support a systematic-risk interpretation.

A third possibility is a sectoral risk premium. Different political environments create distinct regulatory exposures across industries. Polling evidence shows Democrats view healthcare, education, and government activity more favorably, while Republicans are more supportive of energy and materials (Gallup 2013; YouGov 2022). Prior work also links political values to sector tilts in investment (Hong and Kostovetsky 2012). Under this view, VCs could rationally avoid certain sectors in politically distant regions because policy risk is higher there. Table 8 takes this mechanism head-on: I sequentially exclude Democrat-favored sectors (healthcare, government activity, academic and educational services), then Republican-favored sectors (energy, basic materials), and finally all politically sensitive sectors together. Across all exclusions, the PD coefficient remains negative, statistically reliable, and close to the baseline estimate, indicating that the result is not a by-product of sector composition or differential regulatory exposure.

Finally, PD could be pure geography by another name. Distance raises monitoring and travel costs and VC is famously local (Lerner 1995; Sorenson and Stuart 2001; Bernstein et al. 2016). If PD were just distance, its slope should flatten at short range and strengthen only with long separation. Table 9 interacts PD with geographic-distance quintiles and with indicators for distances at or below 100 miles and 500 miles. The interaction terms are uniformly small and statistically indistinguishable from zero. The usual distance controls behave as expected—short range is associated with higher deal incidence—but the PD–investment association does not vary with physical distance in a way consistent with a pure travel-and-monitoring story.

Taken together, the evidence provides little support for generic ability, systematic policy risk, sectoral composition, or pure geographic frictions as first-order explanations. Instead, the pattern is most consistent with a soft-information mechanism in which PD raises the fixed costs of diligence and contracting for mismatched pairs and lowers within-set investment probabilities.

4.3. Exit Implications: Selection at the Funding Hurdle

If PD raises screening costs through the information frictions documented above, only stronger opportunities should clear the initial funding hurdle. The resulting selection implies that, conditional on investment, pairs spanning larger political-distance mismatches should exhibit higher rates of successful exits without a corresponding increase in failures. This logic is consistent with evidence that investors tighten thresholds when monitoring and contracting are more demanding and that intensive screening and monitoring improve realized outcomes, including higher value creation and lower failure rates (Kaplan and Strömberg 2003; Bottazzi et al. 2016; Bernstein et al. 2016).

I test this implication on realized first investments between a VC and a startup, tracking exits through each startup's earliest IPO, acquisition, or write-off. Table 10 shows a positive association between PD and IPO or acquisition, and a negative association with write-offs. The coefficient on PD for IPO or M&A is positive and statistically significant at 0.027, which translates into roughly 0.27 percentage points higher conditional exit probability for a 0.10 increase in PD. For write-offs, the coefficient is −0.025 and statistically significant, implying about 0.25 percentage points fewer write-offs for a 0.10 increase in PD. All specifications include VC-county fixed effects, startup-county fixed effects, industry–year fixed effects, and stage fixed effects, with standard errors clustered by industry–year and startup county.

These exit patterns are consistent with tougher screening under information frictions. When documentation practices, comparables, and diligence heuristics are less transferable across the VC and the startup, the investment threshold is higher and only stronger opportunities clear it.

5. Conclusion

This paper shows that partisan separation between investor and startup locations depresses VC match formation within identical industry–year–stage decision sets. A one–standard-deviation increase in PD lowers investment incidence by roughly 0.7–0.8 percentage points, about eight percent of the baseline rate, and the result is robust to alternative measures, sample restrictions, and placebo assignments. An instrumental-variables design using 1900 ethnic composition distances points in the same direction.

Mechanism tests are most consistent with a soft-information channel at the screening stage. The penalty is larger when verification is hard—first rounds, young firms, the VC's first entry into a county, and for low-reach investors—and smaller where information is thicker or standardized—hub locations and during the 2020–2023 period. Competing explanations tied to generic investor ability, systematic political risk, sector composition, and pure geography receive little support. Conditional on funding, higher PD is associated with more IPOs and acquisitions and fewer write-offs, consistent with a higher bar at the funding hurdle.

The implication is practical and policy relevant. Standardizing diligence materials, using third-party verifiers, and building cross-jurisdiction reference networks can mitigate these frictions and expand viable cross-regional matches. As political sorting intensifies while venture activity remains concentrated, addressing these costs will matter for both capital allocation and regional innovation outcomes.

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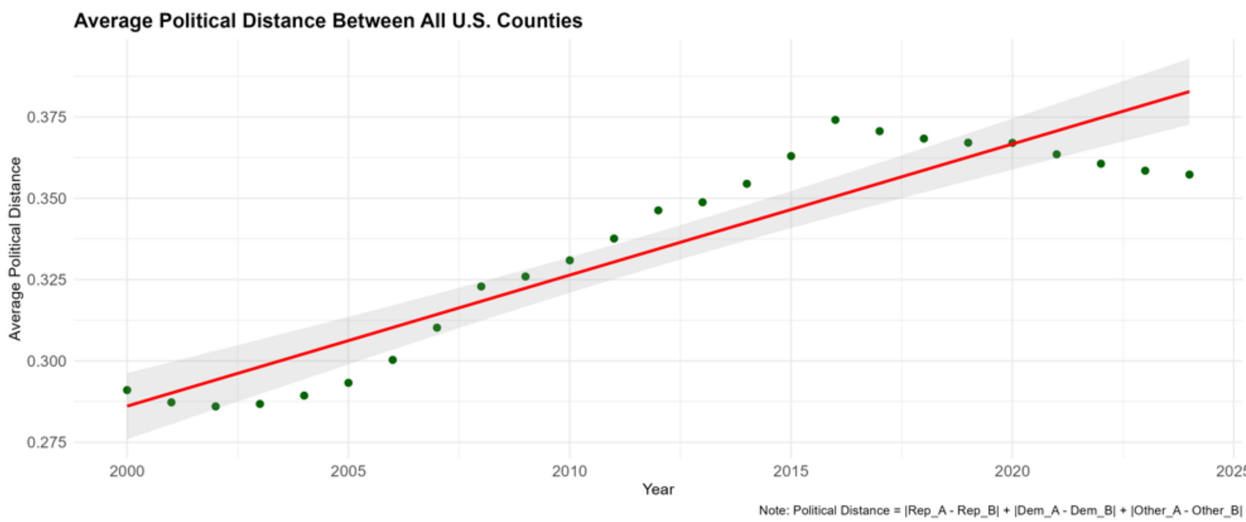
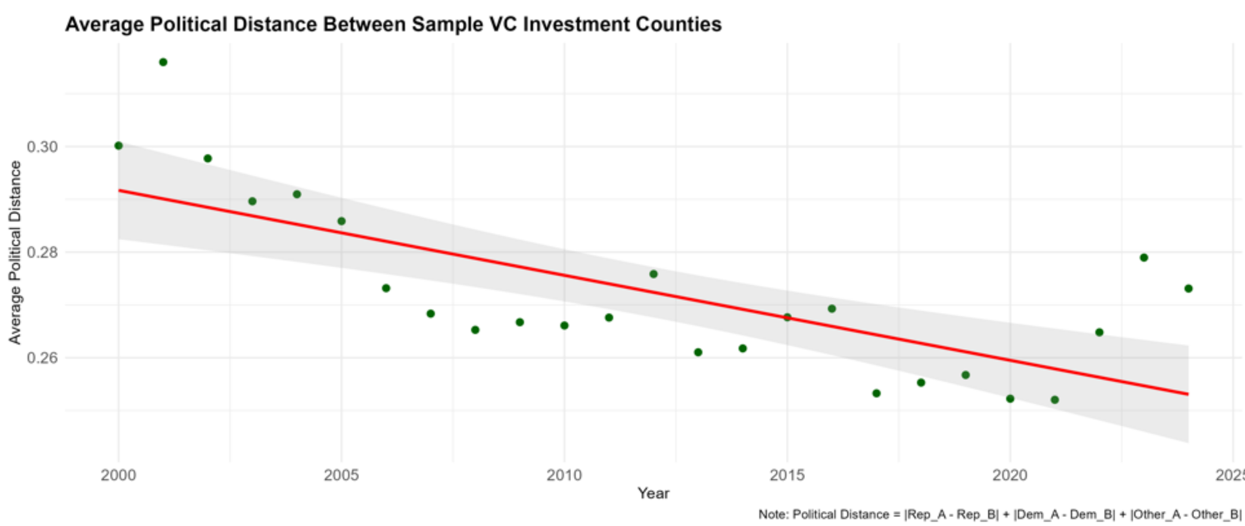
Figure 1. Political Distance Trends –

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. $\text{PD}_{i,j} = |\text{Rep}\%_i - \text{Rep}\%_j| + |\text{Dem}\%_i - \text{Dem}\%_j| + |\text{Other}\%_i - \text{Other}\%_j|$. 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.

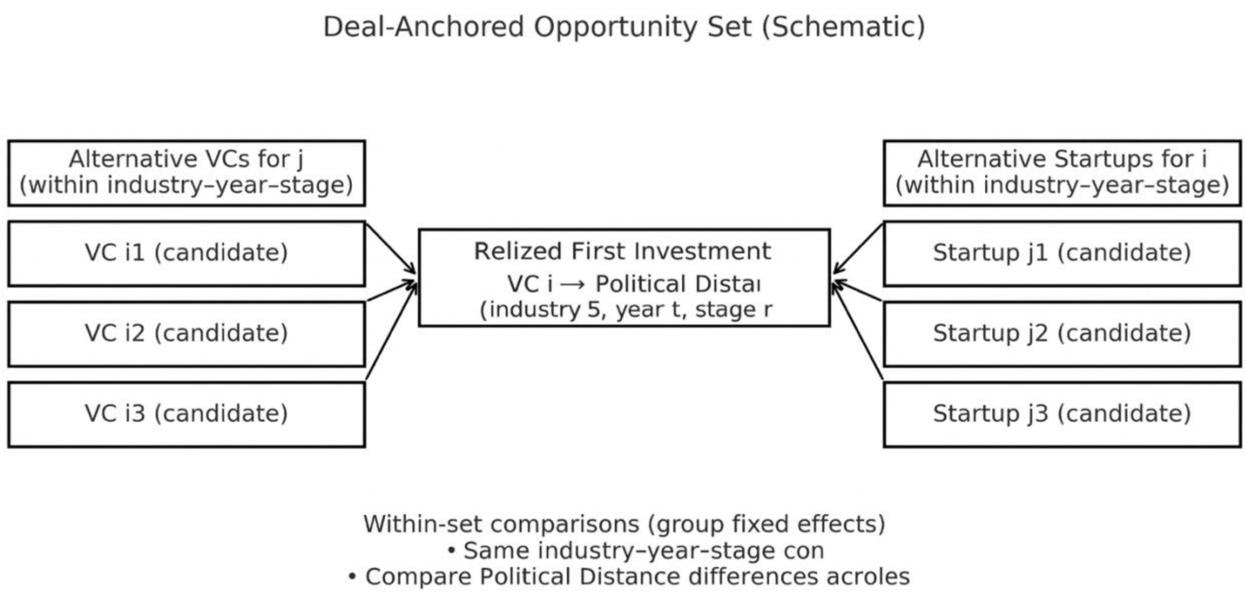


Figure 3. Distribution of Political Distance:

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.

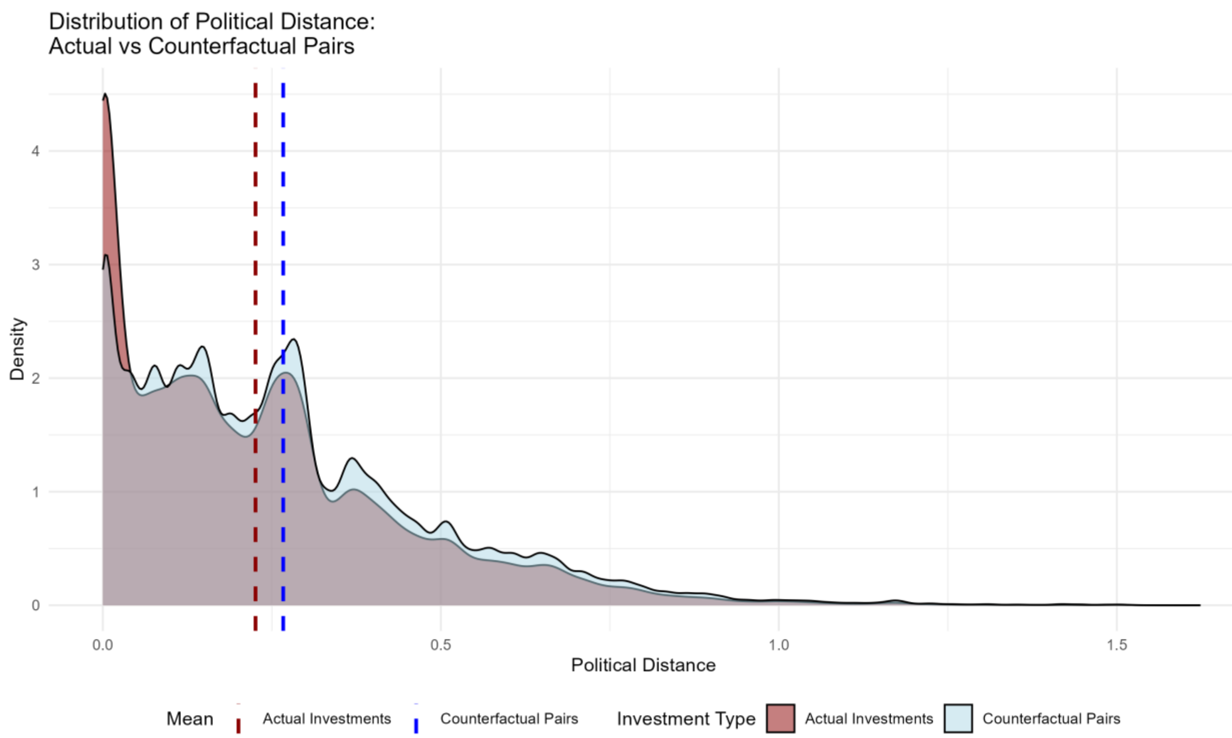


Table 1. Summary Statistics

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						
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
Expert	1,264,271	0.145	0.352	0	0	0
Specialist	1,264,271	0.072	0.259	0	0	0
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
$\leqslant$ 100 Miles	1,264,271	0.199	0.399	0	0	0
$\leqslant$ 500 Miles	1,264,271	0.343	0.475	0	0	1

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. Robustness Checks: Alternative Specifications

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. Column (1) drops same-county pairs (Same County = 0). Column (2) replaces Political Distance with Same Party, a binary indicator that equals one if the VC and startup counties share the same majority party. Column (3) jointly includes Political Distance (L1) and Political Distance L2. Column (4) excludes observations with VCs located in California. Column (5) excludes observations with startups located in California. Column (6) 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. 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	-0.030***		-0.030*	-0.026**	-0.032***	
	(0.010)		(0.017)	(0.013)	(0.011)	
Same Party		0.016***				
		(0.004)				
Political Distance L2			-0.011			
			(0.023)			
Placebo Political Distance						-0.001
						(0.002)
Controls	YES	YES	YES	YES	YES	YES
Group FE	YES	YES	YES	YES	YES	YES
Observations	1,166,883	1,264,271	1,264,271	721,380	737,807	1,264,271
R^2	0.045	0.040	0.040	0.087	0.078	0.040

Table 4. 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 5. Homophily-in-Soft-Information Mechanism

This table tests whether the political distance (PD) effect operates through a soft-information mechanism by examining whether PD strengthens when information is more opaque and weakens when VCs are located in information-rich hubs or during periods when remote due diligence is prevalent. 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 opaque-information settings (First Round, Young, VC First Entry, Low Reach) and attenuating or positive in information-rich hub locations and in periods of prevalent remote due diligence (VC Hub, Pandemic Years), supporting a soft-information channel. 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. Generic-Ability Mechanism

This table tests whether the political distance (PD) effect operates through a generic-ability mechanism by examining whether PD weakens for more specialized VCs. 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 Expert, a binary indicator that equals one if, prior to year t, the VC has invested in exactly one economic sector (constructed from LSEG deal histories). Column (2) interacts PD with Specialist, a binary indicator that equals one if, prior to year t, the VC has invested in exactly one industry within an economic sector (constructed from LSEG deal histories). PD remains negative and statistically significant, whereas the interactions of Political Distance with Expert and with Specialist are small and statistically insignificant, providing no evidence for a generic-ability mechanism. 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)**
Political Distance × Expert	0.007	
	(0.006)	
Political Distance × Specialist		0.009
		(0.008)
Political Distance	-0.035***	-0.035***
	(0.012)	(0.012)
Expert	-0.006**	
	(0.003)	
Specialist		-0.008**
		(0.003)
Controls	YES	YES
Group FE	YES	YES
Observations	1,264,271	1,264,271
R^2	0.040	0.040

Table 7. Systematic Risk Mechanism

This table tests whether the political distance (PD) effect operates through a systematic-risk channel by examining whether PD intensifies when nationwide policy uncertainty is heightened, weakens when the VC and startup share a common state policy regime with lower policy frictions, or weakens when the startup is in a state co-partisan with the federal administration, plausibly associated with greater access to federal support. 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 no evidence that PD operates through a systematic-risk channel. 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 Year	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 8. Sectoral Risk Premium Mechanism

This table tests whether the political distance (PD) effect operates through a sectoral risk premium by excluding politically sensitive sectors. 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) excludes Democrat-favored sectors (healthcare, government activity, and academic and educational services). Column (2) excludes Republican-favored sectors (energy and basic materials). Column (3) excludes all politically sensitive sectors simultaneously. PD remains negative and statistically significant with similar magnitude across all exclusions, providing no evidence that the PD association is driven by a sectoral risk premium. 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**	
	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

Table 9. Pure Geographic-Friction Mechanism

This table tests whether the political distance (PD) effect operates through a pure geographic-friction channel by examining whether its effect depends on physical distance. 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 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. Interaction terms are small and statistically insignificant across specifications, indicating that the PD–investment association does not vary with distance; this provides no support for a pure geographic-friction mechanism. 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 × 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	-0.024**	-0.015
	(0.076)	(0.012)	(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 10. Selection at the Funding Hurdle

This table tests whether the political distance (PD) effect is consistent with selection at the funding hurdle by examining ex post outcomes for realized 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 a higher funding bar under informational frictions. 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** **(1)**	**Write-off** **(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

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	
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) $\leqslant$ 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 (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 2000, 2001, 2002, or 2023, and zero otherwise.
Expert	Binary indicator that equals one if, prior to year t, the VC has invested in exactly one economic sector (i.e., the count of distinct sectors with first investment year < t equals one), and zero otherwise. Constructed from LSEG deal histories.
Specialist	Binary indicator that equals one if, prior to year t, the VC has invested in exactly one industry (within an economic sector) (i.e., the count of distinct industries with first investment year < t equals one), and zero otherwise. Constructed from LSEG deal histories.
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.)
$\leqslant$ 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.
$\leqslant$ 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.