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Do Political Differences Inhibit Market Transactions? An Investigation in the Context of Online Lending

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Abstract. Do political differences, which are becoming increasingly acute among Americans, inhibit market transactions? We study this by examining whether the perceived political distance between investors and borrowers in an online lending market affects whom investors choose to fund. Using two complementary empirical approaches (a gravity model and a difference-in-differences analysis), we find a nuanced effect: Investors from comparatively conservative states consider political distance when making lending decisions, whereas investors from comparatively liberal states do not. Lending activity drops by as much as 11.6% when the investor's state is more conservative than the borrower's state. We also find that political distance between investors and borrowers reduces the likelihood that a borrower's listing will be funded, thereby limiting the ability of the market to fulfill its function. However, political distance does not predict loan performance, which is consistent with another finding: The relationship of political distance to lending activity is not significant for experienced investors. It may be that investors stop considering political distance after they learn from experience that it does not predict loan performance. We find evidence for two mechanisms underlying our results: (1) a preference-based mechanism, in which investors from conservative states have a general preference for borrowers from conservative states, and (2) a rationality-based mechanism in which investors from conservative states use political ideology as a signal of creditworthiness (rightly or wrongly). Our results contribute to the literatures on online frictions and political (in)tolerance and have implications for the design of online lending (and other) markets.

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Keywords: political difference • political distance • online lending • difference-in-differences • gravity model

Introduction

The United States is becoming increasingly polarized politically. In many cases, those with opposing political ideologies cannot agree on basic facts (Alesina et al. 2020). This has several negative effects, including a downturn in civil discourse and an increase in political conflict (Chen and Rohla 2016). It may also have negative implications for markets. We pose the following research question. Do political differences inhibit market transactions? We study this in the context of online lending. We use data from the first peer-to-peer online lending market in the United States: Prosper.com. This market matches borrowers seeking loans to investors willing to lend to them. We investigate whether political differences inhibit market transactions by examining whether the perceived political ideology of borrowers affects whether investors lend to them.

Because online markets eliminate transportation costs for digital goods and reduce search costs for all goods, they should theoretically experience few frictions and be highly efficient (Bakos 1991). However, frictions persist in online markets, including those stemming from geographic and cultural differences among participants (Burtch et al. 2014, Lin and Viswanathan 2016, Senney 2019). We contribute by investigating potential frictions due to perceived political differences. Prior research suggests that political differences may create friction. Political liberals and conservatives are often biased against each other and prefer their own "kind" (Chambers et al. 2013, Brandt and Crawford 2020). This bias may show up in online lending (and other) markets, with conservative investors preferring borrowers they perceive as conservative and liberal investors behaving analogously. If so, then the political distance between investors and borrowers would inhibit transactions. On the other hand, political differences might not matter. If investors' objective is to maximize their return on investment, then perceived differences between their political ideology and that of borrowers should be a secondary factor at most. It is also possible that the relationship between political distance and investor behavior is asymmetric across liberals and conservatives. For example, political distance might matter more for conservatives than for liberals. This would be consistent with the "prejudice gap" hypothesis from the psychology literature, which posits that conservatives are more intolerant of liberals than vice versa (Ganzach and Schul 2021). These different possibilities highlight the need for empirical investigation. Also, prior studies that examine the prejudice gap hypothesis (and the rival "ideological conflict" hypothesis, which posits that conservatives and liberals are similarly intolerant of each other) predominantly use survey-based measures of how individuals feel about others. By contrast, we use individuals' observed transactions to examine how political differences influence economic behavior.

We use two complementary empirical approaches. First, we use a gravity model to examine how differences in state-level political ideology relate to whether investors in state *j* lend to borrowers in state *k*. We find a nuanced relationship. Political distance has a negative and significant relationship to the lending decisions of investors from states with a comparatively conservative political ideology: investors from these states are less likely to lend to borrowers from comparatively liberal states. Lending activity drops by as much as 11.6% for investor states that are more conservative than borrower states. However, political distance has no significant relationship to the lending decisions of investors from states with a comparatively liberal political ideology. Second, we use a difference-in-differences (DID) approach to study how investors react to legalization of same-sex marriage in California. We examine how investors react to this signal of California's (liberal) political ideology, particularly how this varies based on the political distance between investor states and California. We find that investors react positively to this signal, with the exception of investors from states with much more conservative political ideologies than California's. The results from the two approaches suggest that the influence of political distance is asymmetric: it only appears to matter to investors from comparatively conservative states, which is consistent with the "prejudice gap" hypothesis. We also show that the decrease in lending activity due to political distance creates harm: borrowers from states that are more liberal than those of the investors active in the market are less likely to have their loans funded. A one-standard-deviation increase in political distance between these borrowers and the investor pool is associated with a 2.8% decrease in funding level. However, the political distance between investors and borrowers does not predict loan performance (only whether the loan is initially funded). This is consistent with another finding: The relationship between political distance and lending activity is not significant for experienced investors. It may be that investors stop considering political distance after they learn from experience that it does not predict loan performance. A possible explanation for this is that conservatives tend to be more uncertainty averse and tribal than liberals (Jost 2006, 2017). This may prompt investors from conservative states who are new to (and uncertain about) online lending to prefer borrowers from conservative states. However, as investors gain experience about which factors predict loan performance, their uncertainty about online lending dissipates, leading them to stop favoring their own "tribe."

To explore the mechanisms for our findings, we examine the relationship of political distance to lending activity for different subsets of our data. We find that the relationship is less negative for investors from conservative states when borrowers have low debt-to-income (DTI) ratios and/or plan to use the loans to consolidate existing debt. This suggests that these investors place more weight on political ideology when other, more traditional signals of creditworthiness (such as DTI) are unclear. This provides evidence that a rationality-based mechanism operates for these investors. However, the relationship of political distance to lending activity is often negative for investors from conservative states regardless of data subset (albeit more negative in some than others). This suggests that a preference-based mechanism also operates in which investors from conservative states have a general preference for borrowers from conservative states.

Our study contributes to research on frictions in online markets and to research on political differences and (in)tolerance. It also has practical implications. Given that many online markets are two-sided markets that rely on matching to facilitate transactions, understanding frictions that inhibit matching, and then mitigating them, is critical for the design and operation of these markets (Einav et al. 2016, Wei and Lin 2017). We find that perceived differences in political ideology inhibit online lending transactions. This is somewhat surprising, given that the market that we study, Prosper.com, did not provide information about participants' political ideology. This suggests that investors may be "hardwired" to infer (and act on) political beliefs even when clear political information is not provided. Thus, the friction appears to be more related to human nature than to a (flawed) market design choice. (By contrast, some online markets (including Prosper.com and Airbnb) have facilitated discrimination against racial minorities by showing photos of market participants, which is a market design choice (Pope and Sydnor 2011, Edelman et al. 2017).) However, market designers can mitigate the friction by educating investors on the factors that predict loan performance and prevalently displaying those factors. This could include borrowers' verified income and prior repayment history (for repeat borrowers). This will help investors avoid using irrelevant factors, such as perceived political ideology based on location. Market designers can also recommend borrowers that investors might otherwise disfavor, either automatically (using recommendation algorithms that do not consider borrowers' locations) or by purposefully highlighting successful repayment by borrowers from politically distant states (Younkin and Kuppuswamy 2018).

Background, Literature Review, and Motivation

Our study relates to research on political ideology and political distance and research on behavioral biases and market transactions. We review these areas and then discuss why political ideology and political distance might influence online investors' lending decisions.

Political Ideology and Political Distance

Political ideology is usually characterized along a continuum between liberalism and conservatism. Jost (2006) summarized the key differences between liberal and conservative ideologies as (1) attitudes toward inequality and (2) attitudes toward social change versus tradition. At the individual level, liberals and conservatives embrace different core beliefs and central values that manifest not only in political events but also in everyday behaviors (Feldman and Johnson 2014). For example, conservatives are more rigid, close-minded, organized, and uncertainty averse than are liberals (Jost 2017). Liberals and conservatives often have distinct preferences for media sources, nonprofit organizations, and commercial brands (Schoenmueller et al. 2022) and may interpret economic and social events differently even when faced with the same information (Alesina et al. 2020). At the state level, liberal states have policies that involve greater government regulation and welfare provision than do conservative states. Liberal states tend to have minimal restrictions on abortion, regulate guns more tightly, offer generous welfare benefits, and have progressive tax systems (Caughey and Warshaw 2016). Political distance reflects the difference in political ideology between two individuals, groups, states, countries, and so on.

Research has shown that political ideology and political distance influence interactions between individuals, groups, and countries. For example, Twitter users are more likely to connect and communicate with others who have similar political ideologies (Barbera et al. 2015, Boutyline and Willer 2017). Fund managers are more likely to allocate assets to firms managed by people who share their political affiliations, which is mainly due to in-group favoritism rather than possible offline connections or familiarity (Wintoki and Xi 2020). Job seekers request lower wages from employers who share their political ideology (McConnell et al. 2018). Similar political ideology between top management and independent directors is negatively associated with performance, likely because this alignment creates high empathy and leads to weak monitoring (Lee et al. 2014). Political distance also creates frictions in international trade and foreign direct investment (Morrow et al. 1998, Siegel et al. 2013). Countries with dissimilar political systems trade less than countries with similar systems (Decker and Lim 2009, Dajud 2013). Possible explanations are that political distance increases the cost of negotiating trade agreements and/or that consumers prefer products from politically similar countries (Dajud 2013).

Much of the research above implicitly assumes that the effect of political distance is symmetric: that conservatives (be they individuals, groups, countries, etc.) tend to eschew liberals and vice versa. A stream of research, much of it in psychology, has examined whether the effect is asymmetric (see Brandt and Crawford 2020 for a review). The "prejudice gap" or "traditional" hypothesis posits asymmetry: specifically, that conservatives are more prejudiced against (or less tolerant of) liberals than vice versa, in part because conservatives are more close-minded and uncertainty averse on average (as noted previously). By contrast, the "ideological conflict" or "worldview conflict" hypothesis posits symmetry: Both conservatives and liberals are similarly intolerant of each other. The prejudice gap hypothesis has enjoyed historical favor, perhaps because many studies have examined prejudice against disadvantaged social groups that lean liberal, such as racial minorities and the lesbian, gay, bisexual, and transgender community. Those studies show that conservatives express prejudice against these groups, whereas liberals do not, thereby supporting the prejudice gap hypothesis. Studies that examine social groups that lean both liberal *and* conservative have shown that conservatives and liberals are prejudiced toward each other, thereby supporting the ideological conflict hypothesis (Chambers et al. 2013, Crawford et al. 2017). Although many studies supporting the ideological conflict hypothesis are relatively recent, other recent research supports the prejudice gap hypothesis by showing that, although both liberals and conservatives display prejudice/intolerance toward each other, conservatives are *more* intolerant (Ganzach and Schul 2021). There also appear to be differences not only in who conservatives and liberals are intolerant of but also in who they tolerate. For example, although liberals are equally likely to condemn sexual harassment by prominent liberals and conservatives, conservatives are less likely to condemn sexual harassment by prominent conservatives (Linden and Panagopoulos 2019).

Existing studies predominantly measure prejudice/intolerance by asking respondents about their attitudes toward others; they are not based on actual behavior. They also focus on the impact of political differences on general attitudes and social issues, as opposed to economic activity. We fill these gaps by investigating how political distance relates to individuals' actual economic transactions. The only other study (to our knowledge) that examines how political distance relates to individuals' economic transactions considers the role of several socioeconomic and cultural differences (including political distance) in eBay transactions (Elfenbein et al. 2022). A key distinction of our paper is that we consider (and find evidence of) an asymmetric relationship between liberals and conservatives, with political distance being relevant to investors from conservative states but not to those from liberal states.

Behavioral Biases and Online Transactions

If political distance influences lending decisions, it may reflect behavioral bias. Online market participants have displayed several types of behavioral bias. For example, African Americans are discriminated against in online e-commerce markets, online accommodation markets, and online lending markets (Pope and Sydnor 2011, Edelman et al. 2017, Cui et al. 2020, Mejia and Parker 2021), and males are less preferred than females in crowdfunding markets and online labor markets (Greenberg and Mollick 2017, Chan and Wang 2018). These biases can inhibit online transactions and prevent the formation of matches that would otherwise benefit both parties.

Behavioral bias can operate unconditionally or conditionally. Unconditional bias occurs when members of a group (defined by race, gender, political party, etc.) are universally discriminated against, even by those in the same group. Conditional bias occurs when members of a group are discriminated against, but only by members of a different group. This includes in-group bias in which people are biased against others outside of their group, which may be defined by race, gender, geography, political ideology, and so on. For example, home bias occurs when traders in geographically distributed markets prefer to trade with those who are geographically nearby. Research on home bias has shown that institutional investors prefer same-state private equity, employers prefer same-country workers, and individual investors prefer same-state borrowers (Hochberg and Rauh 2013, Lin and Viswanathan 2016, Liang et al. 2021). As another example, research on cultural bias has shown that lenders tend not to lend money to borrowers in countries with different cultural values (Burtch et al. 2014). We study political bias by examining whether online lending investors prefer borrowers with ideologies likely to be similar to theirs, including potential asymmetry between liberals and conservatives.

Why Political Ideology and Political Distance Might Influence Investors' Lending Decisions

Online lending investors decide which borrowers to fund based on information provided by online lending platforms (Iyer et al. 2016, Gao et al. 2022). This includes traditional credit information such as credit scores, income, and debt-to-income ratio, and "soft" information such as other investors' decisions, loan descriptions, borrowers' friendship networks, and borrowers' demographics including gender, race, and overall "appearance" (Pope and Sydnor 2011, Duarte et al. 2012, Zhang and Liu 2012, Lin et al. 2013, Harkness 2016, Greenberg and Mollick 2017, Hildebrand et al. 2017, Hong et al. 2018).

Investors may also infer information about borrowers that is not directly provided by the platform. One way that investors may do this is by using a borrower's state of residence, which is the only location information provided for each borrower listing on Prosper.com during the time period of our analysis.[1] For example, investors may infer that a borrower is likely to be conservative if the borrower lives in Alabama (consistently regarded as a conservative state) and likely to be liberal if the borrower lives in Massachusetts (consistently regarded as a liberal state). This is consistent with statistical discrimination theory (Phelps 1972), which posits that when a decision maker lacks information about an individual (in this case, a borrower's political ideology), the decision maker will rationally substitute group averages (in this case, the political ideology of the borrower's state). These inferences are likely because a state's political ideology is one of its most visible characteristics to outsiders due to media coverage of state and national elections (Jones 2020). For example, average Americans are more likely to know that Vermont is a relatively liberal state than to know that it has below average gross domestic product (GDP) per capita.

We examine whether investors consider a borrower's (inferred) political ideology when making lending decisions. This is plausible because most Americans "think, feel, and behave in ideologically meaningful and interpretable terms" (Jost 2006, p. 667), and investors may prefer to lend to borrowers likely to share their ideology (as discussed above). However, it is not obvious that political ideology will matter in our setting or similar economic settings. This is because the key information about borrowers (and their creditworthiness) is provided by the online lending platform, including both traditional and "soft" information. If investors are purely profit-driven and rational, then they should rely on this information, and not on perceived political ideology, when making lending decisions. It is also possible that the degree to which investors consider political ideology when making lending decisions will differ between liberal and conservative investors, as discussed previously. Thus, it is important to examine a potential asymmetry in how liberal and conservative investors respond to political distance.

Assuming that political ideology influences investors' lending decisions, there are two key theoretical mechanisms that might drive the relationship: the "rationality-based" mechanism and the "preference-based" mechanism. The rationality-based mechanism would operate as follows. Assume that investors believe that liberal borrowers are better at managing debt (which signals their ability to repay) and/or are more trustworthy (which signals their intention to repay) than are conservative borrowers. If so, then investors will prefer liberal borrowers because they assume that any given liberal borrower (whom they probably do not know) is likely to repay the loan. (This follows from statistical discrimination theory (Phelps 1972).) This would yield a preference for liberal borrowers from both liberal and conservative investors, that is, political distance would not matter in investors' decisions. (By the same logic, investors could believe that conservatives are better at managing debt and/or more trustworthy, leading to a preference for conservative borrowers.) It is also possible that an investor does not view a borrower's likely political ideology as an unconditional signal of creditworthiness, but rather views it conditionally based on the investor's political ideology. For example, liberal investors may view liberal borrowers as highly creditworthy (and conservative borrowers as not), with conservative investors feeling analogously. If investors have these beliefs, then they would tend to fund borrowers whose political ideology is likely to match their own. In this case, political distance *would* matter in investors' decisions.

The preference-based mechanism would operate differently. Liberal investors will still prefer liberal borrowers, and conservative investors will still prefer conservative borrowers. However, these preferences are not based on the belief that political ideology signals a borrower's creditworthiness. Instead, liberal investors would prefer to support liberal borrowers simply because they are similar to them, because they share their worldview, because they wish to support them, and so on (Hirshleifer 2015). The same logic may be true for conservative investors and conservative borrowers. This mechanism is consistent with taste-based discrimination (Younkin and Kuppuswamy 2018). Thus, both mechanisms could drive a political distance effect, but for different reasons. In our empirical analysis, we examine each.

Empirical Setting and Data

The online lending market that we analyze is Prosper.com, which is the first peer-to-peer online lending market in the United States. We use data from 2006 to 2011.[2] During the study period, borrowers seeking a loan create a listing on Prosper.com, which shows the requested loan amount and maximum acceptable interest rate along with the borrower's credit information (including credit score, debt-to-income ratio, etc.) and state of residence. Investors choose borrowers to whom to lend money, lending anywhere from \$25 to the entire loan amount. After a borrower's listing attracts enough funding, the loan is issued. Prosper.com used an auction system until the end of 2010 and a posted price system afterward. Under the auction system, investors could present bids (i.e., loan offers) to borrowers, including the amount and interest rate. The auction system ranked the bids by interest rate (lowest to highest) and used the top-ranking bids to fund the loan (any remaining lower-ranking bids were discarded). Under the posted price system, Prosper.com set the interest rate for each borrower: investors chose only how much of the loan to fund. In our analysis, we refer to a "bid" as an instance in which an investor decided to lend to a borrower, including bids that were ultimately discarded under the auction system (given that those represent investors' lending decisions). The data include investor and borrower information (including their states of residence, when they joined Prosper.com, and borrower credit data), listing information (including amount requested, loan category, and loan term), and bid information (including which investors bid on which listings).

We supplement the Prosper.com data set with data on state-level political ideology from Berry et al. (1998, 2010), who construct a political ideology score for each state annually from 1960 to 2017 (see https://rcfording.wordpress.com/state-ideology-data/ and the Online Appendix, Table A1). The score is "the mean position on a liberal-conservative continuum of the active electorate in a state" (Berry et al. 1998, p. 327). Political ideology ranges from 0 (conservative) to 100 (liberal). We also collect demographic and economic data from the U.S. Census, Bureau of Labor Statistics (BLS), and other public sources.

Empirical Strategy, Models, and Results

We use two complementary approaches to study how political ideology and political distance influence investors' lending decisions: (1) a state-dyad gravity model and (2) a DID analysis based on a quasi-natural experiment. Using both approaches increases our confidence in the findings.

Gravity Model Analysis

We use a gravity model to examine whether political distance relates to whether investors in state j lend to borrowers in state k. The unit of analysis in gravity models is a location dyad; the dependent variable is typically a measure of transaction volume between the locations. Our dependent variable is $Bids_{jk}$, which is the number of bids from investors in state j to borrowers in state k from 2006 to 2011. (We also use $Amount_{jk}$, which is the amount of money lent by investors in state j to borrowers in state k, as an alternative dependent variable.) The main independent variables in gravity models are measures of the mass/size of the two locations and measures of distance between them. As our "mass" variables, we use the logs of the number of investors in state j ($Investors_j$) and the number of listings from borrowers in state k ($Listings_k$). As our "distance" variables, we use the logs of geographic, economic, and political distance ($GeographicDistance_{jk}$, $EconomicDistance_{jk}$, $PoliticalDistance_{jk}$). Geographic distance is the great circle distance between the investor and borrower state capitals. Economic and political distance are based on states' real GDP per capita and political ideology scores. Because these vary by year, we computed the average value for each state over the study period (2006–2011).[3] Economic distance and political distance are the absolute differences of these averages for the investor and borrower states. We sometimes use political difference ($PoliticalDifference_{jk}$), which is the average political ideology of the investor state minus that of the borrower state, instead of $PoliticalDistance_{jk}$; this helps us investigate a potential asymmetric relationship. We control for the quality of listings in the borrowers' state by including logged state-level averages (averaged across the study period) for borrowers' credit score, debt-to-income ratio, and estimated monthly payment from the Prosper.com listing data. We control for the potential lending power of investors by including the logged average of investor state median household income from the Census data. We control for the Prosper.com experience of investors by (1) measuring the number of months between when an investor joined Prosper.com and the end of 2011, (2) averaging these values across investors for each state, and (3) taking the log. In our focal analysis, we exclude same-state dyads, that is, those in which the distance measures equal zero. Table 1 provides summary statistics for the raw (i.e., nonlogged) data.

Gravity Model Results. Following prior research (Santos Silva and Tenreyro 2006, Burtch et al. 2014), we use Poisson pseudo-maximum likelihood (PPML) estimation. Results appear in Table 2. Column 1 shows the results of the main specification. Column 2 shows the results after including same-state dyads in the regression and marking them with a dummy variable ($SameState_{jk}$). Columns 3 and 4 are analogous to columns 1 and 2 but with $Amount_{jk}$ as the dependent variable. Across specifications, the coefficient for $PoliticalDistance_{jk}$ is negative and significant: A 1% increase in $PoliticalDistance_{jk}$ is associated with a 0.017% decrease in $Bids_{jk}$. A one-standard-deviation increase in $PoliticalDistance_{jk}$ over the mean (which is a 73% increase) is associated with an approximately 1.2% decrease in $Bids_{jk}$.

We next assess whether the influence of political distance is asymmetric, that is, whether political distance matters more (or less) for investors from comparatively conservative states versus comparatively liberal states. We do this in two ways. First, we create two versions of $PoliticalDistance_{jk}$, one in which the investor state in the dyad is more liberal and one in which the investor state is more conservative. The "investor state more liberal" version takes the value of $PoliticalDistance_{jk}$ when the investor state is more liberal and zero otherwise. The "investor state more conservative" version is analogous. Second, we use $PoliticalDifference_{jk}$ in

Table 1. Descriptive Statistics for Variables Used in the Gravity Model (Raw Values, That Is, Non-logged)

Variables	Observations	Mean	Median	Standard deviation	Minimum	Maximum
Dependent variable						
Bid Count from Investor State to Borrower State	2,450	3,526	763	10,266	0	153,411
Bid Amount from Investor State to Borrower State	2,450	275,981	54,469	878,837	0	13,366,890
Key independent variables						
Political Distance	2,450	17.09	14.63	12.53	0.03	61.68
Political Difference	2,450	0	0	21.19	−61.68	61.68
Other independent variables						
Investors	2,450	1,287	700	1,896	78	12,077
Listings	2,450	8,198	4,466	9,910	158	55,970
Geographic Distance (miles)	2,450	1,211	993	887	41	5,109
Economic Distance ($)	2,450	9,650	7,779	7,535	27	36,708
Average Credit Score	2,450	627.68	628.09	16.55	588.44	692.28
Average DTI Ratio	2,450	0.479	0.487	0.074	0.287	0.722
Average Monthly Payment ($)	2,450	264.03	258.71	29.23	209.88	354.74
Investor State Median Household Income ($)	2,450	50,086	47,972	8,029	36,596	68,619
Investors' Experience (months)	2,450	49.55	47.57	3.62	43.90	54.27

Table 2. Results of the Gravity Model

	Bid Count (ln)		Bid Amount (ln)	
	Excluding same-state pairs	Including same-state pairs	Excluding same-state pairs	Including same-state pairs
Political Distance (ln)	−0.017** (0.007)	−0.017** (0.007)	−0.021* (0.012)	−0.022* (0.012)
Investors (ln)	1.083*** (0.008)	1.081*** (0.008)	1.165*** (0.011)	1.159*** (0.011)
Listings (ln)	1.003*** (0.007)	1.001*** (0.006)	0.999*** (0.010)	0.994*** (0.010)
Average Credit Score (ln)	7.188*** (0.632)	7.315*** (0.628)	4.957*** (0.817)	5.054*** (0.821)
Average DTI Ratio (ln)	0.508*** (0.069)	0.518*** (0.068)	0.640*** (0.091)	0.655*** (0.090)
Average Monthly Payment (ln)	1.256*** (0.085)	1.255*** (0.083)	1.643*** (0.112)	1.626*** (0.110)
Median Household Income (ln)	0.531*** (0.055)	0.535*** (0.053)	0.700*** (0.067)	0.698*** (0.065)
Investors' Experience (ln)	−2.461*** (0.123)	−2.480*** (0.120)	−1.033*** (0.143)	−1.023*** (0.140)
Geographic Distance (ln)[a]	0.018** (0.008)	0.019** (0.008)	0.037*** (0.013)	0.040*** (0.013)
Economic Distance (ln)	−0.007 (0.005)	−0.007 (0.005)	−0.010 (0.007)	−0.010 (0.007)
Same State Dummy		0.049 (0.063)		0.244** (0.104)
No. of observations	2,450	2,500	2,450	2,500
R^2	0.98	0.99	0.97	0.98
Pseudo-log-likelihood	−183,499	−187,202	−20,616,285	−21,637,327

Notes. Standard errors in parentheses are clustered by state dyad. Results hold with standard errors two-way clustered by borrower state and investor state.

[a]The positive coefficient for Geographic Distance is because investors and borrowers are particularly active in west coast states (California, Oregon, Washington) for which geographic distance to other states is large. If we include a dummy variable for those states, the Geographic Distance coefficient becomes negative and insignificant ($\beta = -0.001$, standard error = 0.008). See the Online Appendix, Table A6, for details.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

the model rather than $PoliticalDistance_{jk}$. We represent $PoliticalDifference_{jk}$ via a set of dummy variables for different ranges, which are investor state: (1) much more liberal (investor state's ideology is 25–100 points higher than the borrower state's ideology, (2) more liberal (10, 25), (3) similar [−10, 10], (4) more conservative (−25, −10), and (5) much more conservative [−100, −25]. The "similar" group serves as the omitted baseline in these regressions.

As shown in columns 1 and 2 of Table 3, the $Political\ Distance_{jk}$ coefficient is only significant (and negative) when the investor state in the dyad is more conservative than the borrower state. Furthermore, the "investor state more conservative" and "investor state more liberal" coefficients are statistically different ($p < 0.01$). When the political ideology of the investor state is 60 points lower (i.e., more conservative) than that of the borrower state, the "investor state more conservative" coefficient in column 1 represents an 11.6% decrease in bids (i.e., $e^{(\ln(60) \times -0.030)} - 1$). The results using the $PoliticalDifference_{jk}$ dummy variables (columns 3 and 4) are similar. The coefficients for the "investor state more liberal" terms are insignificant, whereas those for the "investor state more conservative" terms are negative and significant. When the political ideology of the investor state is 25–100 points lower than that of the borrower state, the "investor state much more conservative" coefficient in column 3 represents an 11.7% decrease in bids ($e^{-0.124} - 1$). This indicates an asymmetric relationship: Political distance matters to investors from comparatively conservative states but not to those from comparatively liberal states.

DID Analysis

The gravity model allows us to examine behavior across all 50 states over the entire sample period (2006–2011). However, a common critique of gravity models is that they produce only correlational evidence. Accordingly, we supplement the gravity model with a DID analysis. The DID analysis also allows us to examine how investors react to political ideology and political distance (as discussed later), whereas the gravity model is specific to political distance.

Our strategy was to identify an event that shifted investors' perceptions of borrowers' political ideology in some states ("treated" states) but not others ("control" states) and assess whether this affected investors' lending decisions. We identified state-level legalization of same-sex marriage as a suitable event. First, it represents a signal of a state's relatively liberal political ideology, given that support for lesbian, gay, bisexual, and transgender rights is typically a liberal cause (Lewis and Gossett 2008).[4] Thus, same-sex marriage legalization should shift investors' perception of a state's political ideology (toward the liberal end of the continuum), but it should not shift investors' perception of the fundamentals of the state's economy (which could otherwise create a confound), at least not in the short run. Second, a state's legalization of same-sex marriage was (and remains) controversial and newsworthy, such that people across the United States are likely to notice (and therefore react to) the legalization event. Third, same-sex marriage was legalized in different states at different times (or not at all), thereby yielding the contrast necessary to explore its effect. There

Table 3. Results of the Gravity Model: Asymmetric Influence of Political Distance

	Bid Count (ln)			
	Excluding same-state pairs	Including same-state pairs	Excluding same-state pairs	Including same-state pairs
Political Distance: Investor State More Liberal (ln)	−0.008 (0.009)	−0.008 (0.009)		
Political Distance: Investor State More Conservative (ln)	−0.030*** (0.008)	−0.030*** (0.008)		
Political Difference in [25, 100]: Investor State Much More Liberal			0.011 (0.024)	0.008 (0.024)
Political Difference in (10, 25): Investor State More Liberal			0.018 (0.020)	0.017 (0.020)
Political Difference in [−10, 10]: Investor State Similar			Omitted baseline	
Political Difference in (−25, −10): Investor State More Conservative			−0.054* (0.029)	−0.054* (0.029)
Political Difference in [−100, −25]: Investor State Much More Conservative			−0.124*** (0.036)	−0.126*** (0.036)
Other gravity model controls[a]	√	√	√	√
No. of observations	2,450	2,500	2,450	2,500
R^2	0.98	0.99	0.98	0.99
Pseudo-log-likelihood	−181,204	−184,883	−181,618	−185,299

Note. Standard errors in parentheses are clustered by state dyad.
[a]We included all controls shown in Table 2.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

were three same-sex marriage legalization events during our study period: in California on May 15, 2008; in Connecticut on October 10, 2008; and in New York on June 24, 2011.[5] Due to data availability (discussed later), we focus our analysis on the California event.

We test whether borrowers in California, who were "treated" by legalization of same-sex marriage, received more (or fewer) bids after the legalization event than did borrowers in "control" states who were not treated. If so, then this would suggest that investors react to the ideological signal of same-sex marriage legalization.[6] We then explore the role of political distance by investigating potential differences in the effect based on whether investors are from comparatively liberal or conservative states.

We construct our analysis sample as follows. First, because each borrower listing was available on Prosper.com for seven days in May 2008, we collect all listings ($n = 484$) that were posted on May 12, 2008, which is three days before the California event. This allows us to examine bids placed three days before, on, and three days after the event. Of these 484 listings, 56 were for borrowers from California (the "treated" listings) and 428 were for borrowers from other states (the "control" listings).[7] Second, we limit our analysis to bids placed by "active" investors, which we define as any investor who placed at least one bid (on any listing) before and after the event during the seven-day period. This allows us to assess whether the event shifted the decisions of investors who were on Prosper.com looking for borrowers to fund. We also run our analysis using bids placed by all investors, not only "active" investors, and find similar results. We count the number of bids for each listing from active investors in each state per day. This yields a panel with listing-investor state-day as the unit of analysis. Specification (1) is the basic model.

$$BidsListing_{ijt} = \alpha + \beta\ Treated_{it} + Dyad_{ij} + Time_t + \varepsilon_{ijt} \tag{1}$$

$BidsListing_{ijt}$ is the number of bids for listing i from investors in a different state j on day t. (We also include investors from the same state as a robustness check and find virtually identical results.) We also use $BidAmountListing_{ijt}$, which is the dollar amount of the bids for listing i from investors in a different state j on day t, as an alternative dependent variable (see the Online Appendix, Table A2). $Treated_{it}$ is one for California listing-days on or after the event and zero otherwise. $Dyad_{ij}$ are listing-investor state dyad fixed effects, which capture all time-invariant factors (e.g., features of listings such as loan amount and borrower's credit score, features of investor states, and features of state-listing dyads). $Time_t$ are fixed effects for each day in the seven-day window; these control for unobserved daily shocks common to all listings. We ran the analysis on the full sample and on a matched sample. Using coarsened exact matching (CEM; Iacus et al. 2012), we matched treated and control listings on the bids received on each day before the legalization event and on the following listing features: loan amount requested, interest rate, whether the listing included an image, the percentage of the loan that was funded before the legalization event, the loan grade assigned by Prosper.com, and the borrower's debt-to-income ratio (each of which we collected from the Prosper.com listing data).[8] The

Table 4. Descriptive Statistics of the Sample Used in the DID Analysis

Full or matched sample	Full	Matched
$BidsListing_{ijt}$ (min, mean, max)	0, 0.041, 27	0, 0.024, 14
Investor State Political Ideology (min, mean, max)	28.40, 61.73, 91.90	28.40, 61.76, 91.90
Political Distance (min, mean, max)	0.02, 16.27, 63.50	0.02, 15.86, 63.50
Number of Listings (number of treated listings)	484 (56)	274 (45)
Number of Listing-Investor State Dyads[a]	23,716	13,426

[a]This is equal to the number of listings multiplied by the 49 other investor states. See text for details.

matching approach yielded 30 matched strata, each containing at least one treated and one control listing (including 45 of the 56 treated listings). A characteristic of matching procedures (including CEM) is that a stratum may contain unequal numbers of treated and control observations. To accommodate this, the CEM algorithm generates a weight for each observation, which we use in our regressions. Table 4 shows descriptive statistics for both the full and matched samples.

DID Model Results. We show the basic DID model results from Specification (1) in Table 5. We report results from both ordinary least squares (OLS) and Poisson estimations.[9] The treatment effect is positive and significant for both the full and matched samples. Using the matched sample OLS results, the estimated treatment effect of the same-sex marriage legalization event is 0.025. This indicates that treated listings received (on average) an additional 0.025 bids from investors in each state j on each day t on or after treatment. This represents a 129% increase over the average number of bids that treated listings in the matched sample received from investors in state j in day t *before* treatment (μ = 0.019). Another way to think about this is that treated listings would receive approximately 6.6 bids if they *were not* treated (i.e., 0.019 × 49 states × 7 days = 6.6). Treatment yields 4.9 additional bids (i.e., 0.025 × 49 states × 4 days on/after treatment = 4.9), which is a 74% increase. The Poisson results show a similar effect size (i.e., $e^{0.735}$ – 1 = 108.5%). To check the face validity of these estimates, the average number of daily bids from investors in each state on/after and before the legalization event is 0.047 (on/after) and 0.019 (before) for the treated listings and 0.007 and 0.005 for the control listings, respectively. This yields a "hand-calculated" DID estimate of 0.026 (i.e., (0.047 – 0.019) – (0.007 – 0.005) = 0.026).

The treated and control listings should follow parallel trends in bids received before the event. Otherwise, the coefficient for $Treated_{it}$ might pick up a pre-existing difference in the treated and control listings rather than measuring the treatment effect. Because we matched on bids received before the event, pretreatment trends should be parallel for the matched sample (see Abadie et al. 2015 for a similar approach). We confirmed this, along with examining the pretreatment trends in the full sample and how the effect evolves after treatment, via the leads/lags model shown in (2).

$$BidsListing_{ijt} = \alpha + \sum_{\tau=-3}^{-2} \beta_\tau Treated_{it+\tau} + \sum_{\tau=0}^{3} \beta_\tau Treated_{it+\tau} + Dyad_{ij} + Time_t + \varepsilon_{ijt} \quad (2)$$

Specification (2) mirrors (1) except that we replace $\beta\ Treated_{it}$ with $\sum_{\tau=-3}^{-2} \beta_\tau Treated_{it+\tau} + \sum_{\tau=0}^{3} \beta_\tau Treated_{it+\tau}$. $Treated_{it+\tau}$ is a dummy variable equal to one for treated observations if day t is τ days after the legalization event (or for $\tau < 0$, $-\tau$ days before the event). The β_τ coefficients measure the difference in the number of bids for treated and control listings on the days before, on, and

Table 5. Results of the DID Analysis

	OLS		Poisson	
	Full	Matched	Full	Matched
Treated	0.017*** (0.006)	0.025*** (0.007)	0.472*** (0.110)	0.735*** (0.110)
Time fixed effects	√	√	√	√
Listing-investor state dyad fixed effects	√	√	√	√
No. of observations (listing-investor state-days)	166,012	93,982	15,141	2,394
No. of groups (listing-investor states)[a]	23,716	13,426	2,163	342
Adjusted R^2	0.41	0.46		
Log likelihood			−8,260.72	−2,578.83

Notes. Standard errors in parentheses are clustered by listing-investor state. Alternatively, we clustered by borrower state-investor state; results are consistent (see the Online Appendix, Table A2).
[a]See text (Endnote 9) for why the sample size is smaller for the Poisson analysis.
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 6. Results of the DID Analysis, Including Lead and Lag Terms

	OLS		Poisson	
	Full	Matched	Full	Matched
Treated (t − 3)	−0.019*** (0.005)	0.002 (0.006)	−0.518*** (0.171)	0.067 (0.189)
Treated (t − 2)	−0.009* (0.005)	0.001 (0.006)	−0.335* (0.196)	0.037 (0.223)
Treated (t − 1)	Baseline			
Treated (t0)	0.005 (0.006)	0.014** (0.007)	0.122 (0.147)	0.447*** (0.164)
Treated (t + 1)	0.004 (0.008)	0.022** (0.009)	0.097 (0.160)	0.678*** (0.174)
Treated (t + 2)	0.017** (0.008)	0.026** (0.010)	0.436** (0.170)	0.694*** (0.176)
Treated (t + 3)	0.004 (0.008)	0.040*** (0.010)	0.095 (0.158)	1.314*** (0.172)
Time fixed effects	√	√	√	√
Listing-investor state dyad fixed effects	√	√	√	√
No. of observations (listing-investor state-days)	166,012	93,982	15,141	2,394
No. of groups (listing-investor states)[a]	23,716	13,426	2,163	342
Adjusted R^2	0.41	0.46		
Log likelihood			−8,251.28	−2,564.67

Note. Standard errors in parentheses are clustered by listing-investor state.
[a]See the Table 5 notes regarding the sample size for the Poisson analysis.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

after legalization, after conditioning on the dyad fixed effects. We use the −1 period as the baseline to avoid the "dummy variable trap." If treated and control listings have parallel pretreatment trends, then β_{-3} and β_{-2} will be insignificant. Table 6 and Figure 1 show that pretreatment trends are parallel in the matched sample. Accordingly, we focus our interpretation on the matched sample results. The effect is apparent on the day of the legalization event and is somewhat larger on the following days, perhaps as investors learn about and act on the news.

Role of Political Distance. We explore treatment effect heterogeneity to examine the role of political distance. If political distance matters, then the treatment effect should vary based on the political ideology of the investor state. For example, investors in liberal states may respond more positively to the signal of same-sex marriage legalization than may investors in conservative states. To test this, we classify investors into five groups based on the difference between their state's and California's political ideology.

We define these groups as follows, where CA = California's political ideology and σ = the standard deviation of political ideology across all 50 states: (1) much more liberal (investor state's political ideology is within $[CA + 1.5\sigma, 100]$), (2) more liberal $(CA + 0.5\sigma, CA + 1.5\sigma)$, (3) similar $[CA - 0.5\sigma, CA + 0.5\sigma]$, (4) more conservative $(CA - 1.5\sigma, CA - 0.5\sigma)$, and (5) much more conservative $[0, CA - 1.5\sigma]$. Using the matched sample, we rerun Specification (1) after interacting $Treated_{it}$ with dummy variables for four of the five groups; we use the "similar" group as the baseline, thereby avoiding the "dummy variable trap". Table 7 shows the results. The coefficient for $Treated_{it}$ is positive and significant, indicating that investors from states with a similar political ideology as California react positively to the signal of same-sex marriage legalization. The coefficients for the interaction terms measure the differential effect based on whether the investors' state is more liberal or more conservative. (Thus, the overall coefficient for each political distance group is the sum of the coefficient for $Treated_{it}$ and the coefficient for that group's interaction term.) The "Treated × Investor State Much More Conservative" coefficient is negative and significant. The coefficients for the other interaction terms are insignificant, with the exception of the "Treated × Investor State Much More Liberal" coefficient in the Poisson analysis, which is positive and significant. The insignificant coefficients for (almost all of) the interaction terms except for "Treated × Investor State Much More Conservative" indicate that investors react positively to the legalization event, except for investors from states that are much more conservative than California. This indicates that the effect of the legalization event does not vary based on the political distance between investor states and California,

Figure 1. Lead and Lag Coefficients from Table 6, with 90% Confidence Intervals (CIs)

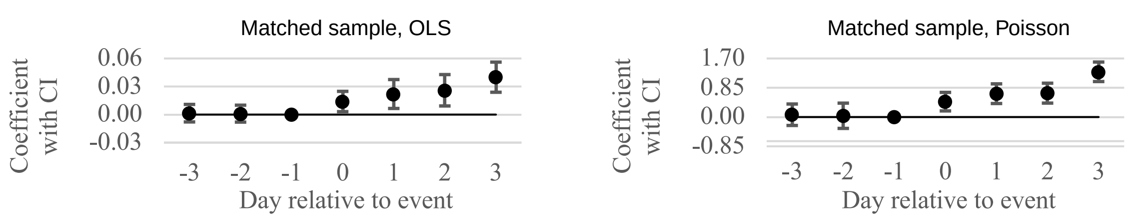

Table 7. Results of the DID Analysis Using the Matched Sample, Including Treatment Effect Heterogeneity Based on Political Distance

	OLS		Poisson	
	Focal measure	*Obama Advantage* measure	Focal measure	*Obama Advantage* measure
Treated × Investor State Much More Liberal	0.006	n/a	0.518**	n/a
	(0.032)		(0.262)	
Treated × Investor State More Liberal	0.002	0.025	0.044	0.164
	(0.018)	(0.025)	(0.252)	(0.242)
Treated	0.029***	0.024**	0.701***	0.667***
	(0.011)	(0.010)	(0.150)	(0.182)
Treated × Investor State More Conservative	−0.008	0.008	0.178	0.391
	(0.015)	(0.015)	(0.297)	(0.255)
Treated × Investor State Much More Conservative	−0.033***	−0.027**	−0.906***	−0.790**
	(0.012)	(0.010)	(0.347)	(0.331)
Time fixed effects	√	√	√	√
Listing-investor state dyad fixed effects	√	√	√	√
No. of observations (listing-investor state-days)	93,982	93,982	2,394	2,394
No. of groups (listing-investor states)[a]	13,426	13,426	342	342
Adjusted R^2	0.46	0.46		
Log likelihood			−2,575.29	−2,573.01

Notes. Standard errors in parentheses are clustered by listing-investor state. When using the Obama Advantage measure, no states are "much more liberal" than California, leading to n/a in the table.
[a]See the Table 5 notes regarding the sample size for the Poisson analysis.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

except for investors from much more conservative states. This is consistent with the results of the gravity model that political distance only matters to investors from conservative states.[10]

We assess whether the results are an artifact of how we define the groups in two ways. First, we use a linear interaction term (instead of the group interaction terms) and find similar results (see the Online Appendix, Table A3). Second, we shift states from the "more conservative" group to the "much more conservative" group. This allows us to assess whether the result is driven by the (potentially idiosyncratic) set of states in the "much more conservative" group: it is not (see the Online Appendix, Table A4).

For robustness against potential measurement error in our focal measure of political distance, we also use "Obama Advantage," which is the percentage of voters in state j who voted for Barack Obama (Democratic candidate) minus the percentage who voted for John McCain (Republican) in the 2008 presidential election (data from https://electionlab.mit.edu/data). A high value of Obama Advantage indicates a liberal leaning. As with our focal approach, we create investor groups (much more liberal, more liberal, etc.). Results (Table 7, columns 2 and 4) are consistent.

Extensions, Robustness Checks, and Potential Measurement Error

Gravity Model Extensions and Robustness Checks

Our working theory is that the distance between an investor's ideology and a borrower's ideology (as inferred from the borrower's state of residence) influences the investor's lending decision. However, it is possible that our political distance measure proxies for other types of social or economic distance. To examine this, we constructed other distance variables (many of them based on Census and BLS data) and controlled for them in the gravity model. We constructed educational attainment distance, which is the average (across the years of our sample) absolute difference in the percentage of citizens who completed high school for each state dyad. We constructed similar distance variables based on the percentage of white citizens, percentage of male citizens, unemployment rate, and median household income. We also controlled for listing type distance, which is based on the loan category published for each listing. We categorized each listing as "debt consolidation" (=1, e.g., the loan will be used to consolidate higher interest debt) or "debt expansion" (=0, e.g., the loan will be used to fund a vacation). We measured listing type distance as the absolute difference in the percentage of debt consolidation listings for each state dyad. This addresses the possibility that investors prefer borrowers from states with similar listing types. As earlier, we examined whether the influence of political distance is asymmetric by including interaction terms and political difference dummies that indicate whether the investor state is more liberal or more conservative than the borrower state. Results are shown in Table 8 and are similar to our focal results.

We conducted a falsification test to further reduce the possibility that our results are driven by a spurious correlation. Investors in our data are categorized on

Table 8. Results of the Gravity Model with Additional Demographic and Economic Distance Variables

	Baseline	More liberal/ conservative	Political distance categories
Political Distance (ln)	−0.018** (0.007)		
Political Distance: Investor State More Liberal (ln)		−0.010 (0.008)	
Political Distance: Investor State More Conservative (ln)		−0.030*** (0.008)	
Political Difference in [25, 100]: Investor State Much More Liberal			0.006 (0.023)
Political Difference in (10, 25): Investor State More Liberal			0.007 (0.019)
Political Difference in [−10, 10]: Investor State Similar			Omitted baseline
Political Difference in (−25, −10): Investor State More Conservative			−0.047* (0.028)
Political Difference in [−100, −25]: Investor State Much More Conservative			−0.106*** (0.037)
Other Gravity Model Controls[a]	√	√	√
PctWhiteCitizens Distance (ln)	−0.007 (0.007)	−0.006 (0.007)	−0.006 (0.007)
PctMaleCitizens Distance (ln)	−0.014* (0.008)	−0.013 (0.008)	−0.016* (0.008)
PctDebtConsolidationListings Distance (ln)	0.018*** (0.005)	0.017*** (0.005)	0.017*** (0.005)
PctHighSchoolAttainment Distance (ln)	−0.016* (0.009)	−0.015 (0.009)	−0.013 (0.009)
PctIncome Distance (ln)	−0.003 (0.005)	−0.002 (0.005)	−0.002 (0.005)
PctUnemployment Distance (ln)	−0.026* (0.015)	−0.019 (0.015)	−0.020 (0.015)
No. of observations	2,450	2,450	2,450
R^2	0.99	0.99	0.99
Pseudo-log-likelihood	−180,587	−178,882	−179,345

Note. Standard errors in parentheses are clustered by state dyad.
[a]We included all controls shown in Table 2.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Prosper.com as "regular" investors and "traders." During the study period, both "regular" investors ($n =$ 58,383) and "traders" ($n = 5{,}960$) could fund borrowers, but traders were also authorized to trade their notes (i.e., the portion of loans they funded and for which they receive payments) on a secondary market. We hypothesize that traders should be less likely to consider political distance than "regular" investors. This is because traders are more likely to be professional investors and financial advisors (including employees of financial institutions); as such, they are more likely to follow a structured selection process that does not include subjective, and potentially biased, criteria such as perceived political ideology. Traders are also more likely to use automated systems that do not consider a borrower's likely political ideology to select loans. (Similarly, Ganju et al. (2020) show that use of automated clinical decision support systems reduces racial biases in healthcare.) To test this, we decomposed $Bids_{jk}$ into bids placed by "regular" investors versus traders and reran the gravity model for each dependent variable. We adjusted independent variables (e.g., number of investors, investors' experience) to match each dependent variable. The results (Table 9) show that the $PoliticalDistance_{jk}$ coefficient is negative and significant for "regular" investors but not for traders, which supports our hypothesis and limits the possibility that our results are driven by a spurious relationship.

We implemented several other robustness checks. A common concern of gravity models is that all dyads are weighted equally, even if some dyads are more impactful than others. Thus, we reran the gravity model after (1) weighting each dyad by the total number of listings and (separately) investors and (2) excluding observations for states with the least (and most) activity. We also reran the gravity model: (1) using two-way

Table 9. Results of the Gravity Model: "Regular" Investors vs. Traders

	Bids: Regular Investors (ln)	*Bids: Traders (ln)*
Political Distance (ln)	−0.014* (0.007)	0.011 (0.009)
Other Gravity Model Controls[a]	√	√
# of Observations	2,450	2,450
R^2	0.98	0.99
Pseudo Log-likelihood	−135,018	−22,661

Notes. Standard errors in parentheses are clustered by state dyad. Results hold with standard errors two-way clustered by borrower state and investor state. Based on a seemingly unrelated estimation, the p value for the difference in the *Political Distance* coefficients for the "regular" investors and traders samples is 0.0073.
[a]We included all controls shown in Table 2 with one exception. Because *Median Household Income* is a state-level measure from Census data, we could not decompose it based on "regular" investors versus traders. We replaced it with the *Average Dollar Value of Bids* placed by "regular" investors and traders, which like *Median Household Income*, proxies for the lending power of investors.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table 10. Results of the DID Analysis Using National Sports Events

Treatment	2008 NFL final	2008 NHL final	2008 NBA final
Treated	−0.014 (0.011)	0.008 (0.008)	−0.004 (0.003)
Time fixed effects	√	√	√
Listing-investor state dyad fixed effects	√	√	√
No. of observations (listing-investor state-days)	67,116	79,611	67,116
No. of groups (listing-investor states)	9,588	11,373	9,588
Adjusted R^2	0.40	0.41	0.38

Notes. Standard errors in parentheses are clustered by listing-investor state. Results from OLS estimation.

clustered errors by investor state and borrower state, (2) with bid amount (instead of bid count) as the dependent variable, (3) after including a dummy variable for west coast states, given that investors and borrowers from these states are particularly active, and (4) using "Obama Advantage" to measure political distance. Results (Online Appendix, Tables A5 and A6) are similar to our focal results.

DID Model Extensions and Robustness Checks

An alternative explanation for our DID results is that investors increase their bids in California after the legalization event simply because the event makes California "top of mind" rather than shifting investors' perceptions of borrowers' political ideology. The treatment effect heterogeneity shown in Table 7 suggests that this is unlikely. If the effect is purely due to awareness, then we should *not* see investors from comparatively conservative states respond differently to legalization. We also test this "general awareness" rival explanation by testing the treatment effect of the occurrence of national sports events that are likely to increase awareness of a state without sending an ideological signal. We use three events: (1) the final game of the NFL (American football) playoffs between the New York Giants and the New England Patriots on February 3, 2008; (2) the final game of the NHL (hockey) playoffs between the Detroit Red Wings and the Pittsburgh Penguins on June 4, 2008, and (3) the final game of the NBA (basketball) playoffs between the Boston Celtics and the Los Angeles Lakers on June 17, 2008. If our findings are due to a general awareness effect, then these events should generate a positive and significant treatment effect for listings in the states whose teams were participating (i.e., New York and Massachusetts for the NFL playoffs, Michigan and Pennsylvania for the NHL playoffs, and Massachusetts and California for the NBA playoffs). We use the basic DID specification and report the results in Table 10. We confirm that the control and treated listings follow parallel pretreatment trends via the leads/lags specification. The effects of these sports events are not significant. This suggests that our main treatment effect is unlikely to be driven by a general awareness effect.

It is possible that another event occurred at the same time as California legalized same-sex marriage and that this (confounding) event could generate the effect that we see. However, this is unlikely. First, any confounding event would need to explain not only the average treatment effect but also why it differs for comparatively conservative states. Second, we searched for other key events that occurred on the legalization event date in California that might confound our results but found none. We also replicated the "regular" investors versus "traders" falsification test by decomposing $BidsListing_{ijt}$ and running the DID analysis for each, using the matched sample. (There are 3,263 "regular" investors and 464 traders in this analysis.) Results appear in columns 1–4 of Table 11 and show that the legalization event has a positive and significant effect on bids placed by regular investors but not on bids placed by traders. This helps rule out the possibility of a confounding event (and increases the evidence for causality), given that the legalization event should (and does) have a larger effect for regular investors.

Along similar lines, investors' reaction to the California same-sex marriage legalization should be larger for investor states that pay more attention to the event. We used Google Search Trends data to measure the attention paid by investors from different states. We used a dummy variable to indicate "high" versus "low" attention in the "same-sex marriage" topic from May 12, 2008, to May 18, 2008, for each state (with high/low determined by whether there was sufficient search interest from a state for Google to assign it a score). We interacted this dummy variable with the treatment indicator. Results (column 5 of Table 11) show that, as expected, the effect is larger for high-attention investor states.

We extended the DID analysis in six other ways. First, we reran the analysis after using two alternative matching strategies. For the first alternative strategy, we created dummy variables to categorize each listing as related to "debt consolidation" (=1) or "debt expansion" (=0) (based on the loan category published for each listing) and to indicate whether the borrower was affiliated with a Prosper.com group (which Prosper.com uses to create a sense of community). We added these variables as matching criteria (we did not match

Table 11. Results of the DID Analysis: "Regular" Investors vs. Traders and Based on Investor State Interest

	OLS		Poisson		OLS
	Bids: Regular Investors	*Bids: Traders*	*Bids: Regular Investors*	*Bids: Traders*	*Bids*
Treated	0.023***	0.001	0.780***	0.373	0.004
	(0.006)	(0.001)	(0.119)	(0.271)	(0.005)
Treated × *High Attention*					0.029***
					(0.009)
Time fixed effects	√	√	√	√	√
Investor state-listing dyad fixed effects	√	√	√	√	√
No. of observations (listing-investor state-days)	93,982	93,982	2,149	518	93,982
No. of groups (listing-investor states)	13,426	13,426	307	74	13,426
Adjusted R^2	0.43	0.14			0.46
Log likelihood			−2,357.04	−396.75	

Notes. Standard errors in parentheses are clustered by listing-investor state. Based on a seemingly unrelated estimation, the p value for the difference in the *Treated* coefficients for the "regular" investors and traders samples is 0.0006 for the OLS model. We are unable to conduct seemingly unrelated estimation for the Poisson model due to the relatively low number of observations.
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

on whether the listing had an image to maintain an adequate sample size). Results for the main effect and the effect broken down by political distance are shown in Table 12 (columns 1 and 2). For the second alternative strategy, in addition to matching on the listing features from our main matching strategy (loan amount requested, interest rate, etc.), we matched on investor state and on the number of bids listing i received from investors in state j on each day before the legalization event. (In our main strategy, we did not match on investor state, and we matched on the number of bids listing i received from investors in *all* states before the event.) This approach allows us to examine whether investors from the *same* state (say, New York) shifted their bids to (or from) California listings after the event. Results are shown in columns 3 and 4 of Table 12. Second, we reran the DID analysis using listings posted two, three, and four days before the California legalization event. This extends the main analysis, in which we used only listings posted three days before the

Table 12. Results of the DID Analysis: Extensions and Robustness Checks

	Focal sample		Focal sample		Listings two, three, and four days before event		Listings two weeks before and after event	
	Matched: Alternate strategy 1		Matched: Alternate strategy 2		Matched		Full	
Treated × *Investor State Much More Liberal*		0.008		0.006		−0.002		−0.008
		(0.034)		(0.032)		(0.013)		(0.019)
Treated × *Investor State More Liberal*		0.004		−0.001		0.000		0.025**
		(0.019)		(0.017)		(0.008)		(0.012)
Treated	0.042***	0.045***	0.022***	0.027**	0.010***	0.014***	0.037**	0.051**
	(0.008)	(0.012)	(0.007)	(0.011)	(0.003)	(0.005)	(0.017)	(0.023)
Treated × *Investor State More Conservative*		−0.007		−0.008		−0.006		−0.051**
		(0.016)		(0.015)		(0.007)		(0.020)
Treated × *Investor State Much More Conservative*		−0.030**		−0.033***		−0.022***		−0.039**
		(0.014)		(0.012)		(0.006)		(0.016)
Time fixed effects	√	√	√	√	√	√	√	√
Listing-investor state dyad fixed effects	√	√	√	√	√	√		
Borrower state fixed effects							√	√
Investor state fixed effects							√	√
Listing-level controls							√	√
No. of observations	95,011	95,011	92,351	92,351	223,293	223,293	572,222	572,222
No. of groups	13,573	13,573	13,193	13,193	31,899	31,899	572,222	572,222
Adjusted R^2	0.41	0.41	0.41	0.41	0.37	0.37	0.11	0.11

Notes. OLS results reported; Poisson results are similar. Columns 1–4 are based on alternative matching strategies. Columns 5 and 6 include listings posted two, three, and four days before the event. Standard errors are clustered by listing-investor state. Columns 7 and 8 are based on an alternative design for the DID model (listing-investor state level). Standard errors are three-way clustered by borrower and investor state and time period.
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

legalization event. We used the main matching strategy to create matched samples for listings created on each day, pooled the samples, and reran the DID analysis. Results are shown in columns 5 and 6 of Table 12. Third, we estimated a variation of the DID analysis by comparing seven-day (i.e., complete) results from California listings to those of control state listings, before and after the legalization event. For the before period, we gathered listings posted between 21 and 8 days before the legalization event. Stopping at eight days before the event ensures that none of the listings were "contaminated" by the event. For the after period, we gathered listings posted between 1 and 14 days after the event. The dependent variable for this analysis is the number of bids for listing i from investors in state j ($Bids\ Listing_{ij}$); as such, our unit of analysis is listing-investor state instead of listing-investor state-day. Results are shown in columns 7 and 8 of Table 12.

Fourth, we removed listing-investor state-day observations for California investors, in case California investors have an outsized influence on the results and/or react differently to California's legalization event than do investors in other states. Fifth, we reran the analysis using fully funded listings only. Sixth, we reran the OLS analysis after removing listing-investor state dyads for which there were zero bids throughout the seven-day listing period (which yields the same sample used in the Poisson estimation; see Endnote 9). These results are shown in the Online Appendix, Table A7, and are consistent with the main analysis.

Potential Measurement Error

Our use of state-level measures of political ideology could introduce measurement error, although our use of group measures to proxy for individual measures is common in studies examining the role of "distance" (Morrow et al. 1998, Decker and Lim 2009, Dajud 2013, Siegel et al. 2013, Sabzehzar et al. 2020). For example, in their study of cultural differences and online lending, Burtch et al. (2014) used a country-level measure of cultural values to approximate the cultural values of individuals in those countries, even though many countries are multicultural. We use a similar approach, although our measurement is more granular (state-level versus country-level). Despite the consistency of our approach with the literature, we explore how potential measurement error might influence our conclusions.

We first point out when potential measurement error is *not* a concern. The main effect uncovered in the DID analysis does not rely on state-level measures of political ideology. Instead, it shows how investors respond to a liberal signal issued by a borrower's state. Measurement error may be a concern in the gravity model and in the DID model when we explore treatment effect heterogeneity based on political distance. We address this in several ways. First, using state-level political ideology to approximate a borrower's individual political ideology likely reflects what investors do. This is because Prosper.com did not provide information on borrowers' political ideology during our study period, leaving investors to infer borrowers' ideology based on location. Second, we use different measures of political ideology (our focal measure and the Obama Advantage measure) and find similar results. This shows that our findings are not generated by measurement error specific to our focal measure. Third, we aggregate data to the state level for analysis, which can "wash out" measurement error across individuals (Cameron and Trivedi 2005, p. 899). In other words, although one investor might be more liberal than the average investor in a given state, another is likely to be more conservative. Aggregating to the state level allows us to approximate the behavior of an average investor from each state. Fourth, measurement error only leads to inconsistent estimates if the error term is correlated with the (potentially) mismeasured variable (Wooldridge 2002, p. 305). It is not clear that this is an issue for our models.

We also conduct two supplemental analyses to explore the possibility of measurement error. First, we identify those states for which state-level political ideology is most likely to match individual-level political ideology. We do this by calculating the Obama Advantage measure for each county in each state. (We use the Obama Advantage measure because our focal measure of political ideology is not available at the county level.) Second, we compute the standard deviation of the county-level Obama Advantage measure for each state. We assume that individual-level political ideology is most likely to match state-level ideology in the states with the lowest standard deviation. We rerun the gravity model after excluding the five investor and borrower states with the highest standard deviation (see the Online Appendix, Table A8). We rerun the DID model after excluding the five investor states with the highest standard deviation (see the Online Appendix, Table A9). (We only use the political ideology score for one borrower state in the DID analysis: California.) The findings are consistent across models. Second, we directly use county-level ideology instead of state-level ideology in our analysis. Nearly 15% of investors in our sample (optionally) self-report their city of residence, from which we determine their county of residence. This yielded 843 investor counties. For this subset of investors, we rerun the gravity model using investor-county/borrower-state dyads (instead of investor-state/borrower-state dyads). We create the political distance measures using the Obama Advantage measure. Results are shown in columns 1 and 2 of Table 13. Similarly, approximately 17% of borrowers (optionally) self-report their city of residence. We use this information to rerun the gravity model using investor-county/borrower-county dyads. Results are shown in columns 3 and 4 of Table 13.

Table 13. Gravity Model Results: Investor-County Analysis

	Investor-county/ borrower-state		Investor-county/ borrower-county	
Political Distance (ln)	−0.021* (0.012)		−0.015* (0.008)	
Political Distance: Investor State More Liberal (ln)		−0.007 (0.013)		−0.004 (0.009)
Political Distance: Investor State More Conservative (ln)		−0.041** (0.014)		−0.022** (0.009)
Other gravity model controls[a]	√	√	√	√
No. of observations	37,436	37,436	366,496	366,496
R^2	0.77	0.77	0.63	0.62
Pseudo-log-likelihood	−315,118	−314,363	−117,553	−117,525

Note. Standard errors in parentheses are clustered by state dyad.
[a]We included all controls shown in Table 2.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Results are consistent with our main analysis, including that the influence of political distance is stronger for comparatively conservative investor counties.

Relationship of Political Ideology and Distance to Lending Outcomes

The evidence indicates that (some) investors consider political ideology when making lending decisions. However, it is not clear how much this matters for online lending outcomes or whether it creates harm. To explore this, we conducted a listing-level analysis, using all listings from 2006 to 2011. We used Specification (3) to examine whether political distance affects whether a listing attracts bids and becomes a funded loan, which is a key outcome variable for borrowers and investors.

$$BidsListing_{it} = \alpha + \beta_1 PoliticalDistance\_InvestorPool_{it} + \gamma Controls_i + Time_t + \varepsilon_{it} \quad (3)$$

$BidsListing_{it}$ is the number of bids that listing i, posted on day t, received during its listing period. We also use $PctFunded_{it}$, which is the percentage of the requested loan amount that was funded, as an alternative dependent variable. $PoliticalDistance\_InvestorPool_{it}$ measures the average political distance between the borrower who posted listing i and the pool of active investors (i.e., who placed bids) on day t. To illustrate, consider two borrowers who posted on June 1, 2010: one from Texas (political ideology score = 38) and one from New York (score = 64). Assume there are 500 active investors on June 1, 2010: 300 from California (score = 58), 100 from Illinois (score = 56), and 100 from Connecticut (score = 68). $PoliticalDistance\_InvestorPool_{it}$ = 21.6 for the Texas borrower's listing ([|38 − 58| + |38 − 58| + |38 − 58| + |38 − 56| + |38 − 68|]/5) and = 6 for the New York borrower's listing. We explored asymmetry by using "Investor Pool More Liberal" and "Investor Pool More Conservative" versions of $PoliticalDistance\_InvestorPool_{it}$ analogous to that previously provided. $Controls_i$ include the loan amount requested, the borrower's debt-to-income ratio, and fixed effects for loan term, category, grade, and borrower's state.

The coefficient for $PoliticalDistance\_InvestorPool_{it}$ is negative and significant (Table 14, columns 1 and 3). This combines an insignificant relationship when the investor pool is more liberal than the borrower (on

Table 14. Results of the Listing-Level Analysis: Influence of Political Distance on Funding Success

	BidsListing	*BidsListing*	*Percent Funded*	*Percent Funded*
Political Distance: Investor Pool	−0.201*** (0.056)		−0.068** (0.028)	
Political Distance: Investor Pool More Liberal		−0.081 (0.066)		0.015 (0.035)
Political Distance: Investor Pool More Conservative		−0.227*** (0.072)		−0.097*** (0.036)
Amount Requested (in thousands)	0.865*** (0.036)	0.866*** (0.036)	−1.098*** (0.014)	−1.103*** (0.014)
DTI Ratio	−2.575*** (0.081)	−2.577*** (0.082)	−1.323*** (0.038)	−1.321*** (0.038)
Loan term fixed effects	√	√	√	√
Loan category fixed effects	√	√	√	√
Loan grade fixed effects	√	√	√	√
Borrower state fixed effects	√	√	√	√
Time fixed effects	√	√	√	√
No. of observations	365,604	357,281	365,604	357,281
Adjusted R^2	0.20	0.20	0.32	0.32

Notes. Standard errors in parentheses are clustered by listing date. OLS results reported; Poisson results for the *BidsListing* regressions yield similar results. The results are robust when (1) using Amount Listing or a dummy variable for whether a listing was funded as dependent variables; (2) including additional listing features as control variables; and (3) controlling for same-state investors.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table 15. Results of the Loan-Level Analysis: Relation Between Political Distance and Loan Performance

	Default rate	*IRR*	*CAGR*
Average Political Distance of Received Bids	0.0004 (0.0008)	−0.0006 (0.0004)	−0.0004 (0.0003)
Amount Funded (in thousands)	0.017*** (0.001)	−0.005*** (0.000)	−0.004*** (0.000)
DTI Ratio	0.021*** (0.003)	−0.007*** (0.002)	−0.006*** (0.001)
Bid Count	−0.0004*** (0.0000)	0.00005*** (0.00001)	0.00005*** (0.00001)
Same-State Bid Ratio	−0.228*** (0.030)	0.063*** (0.015)	0.056*** (0.012)
Loan term fixed effects	√	√	√
Loan category fixed effects	√	√	√
Loan grade fixed effects	√	√	√
Borrower state fixed effects	√	√	√
Time fixed effects	√	√	√
No. of observations	43,962	43,962	43,962
Adjusted R^2	0.11	0.08	0.07

Note. Standard errors in parentheses are clustered by listing date.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

average) with a negative and significant relationship when the investor pool is more conservative (see columns 2 and 4). Using the coefficients for "Investor Pool More Conservative," a one-standard-deviation increase ($\delta = 5.35$) is associated with a 5.4% decrease in $BidsListing_{it}$ ($\mu = 22.57$) and a 2.8% decrease in $Pct\ Funded_{it}$ ($\mu = 18.31$). This indicates that when the active investors are (on average) from more conservative states than the borrower, political distance reduces the attractiveness of a listing and the likelihood of its being funded, thereby creating harm to borrowers and potentially to investors (who do not get to invest) and to the platform. Results are consistent when using $Amount\ Listing_{it}$ and whether the listing was successfully funded as alternative dependent variables.

We also assessed whether political distance between a borrower and the borrower's investors predicts loan performance via a loan-level analysis. For each listing i that was successfully funded, we calculated the average political distance between the borrower and the borrower's investors ($AvgPoliticalDistance_i$). To illustrate, consider a listing from a Texas borrower (political ideology score = 38) funded by a New York investor (score = 64) and an Illinois investor (score = 56). $AvgPoliticalDistance_i = 22$ in this case ($[|38 - 64| + |38 - 56|]/2$). We regressed three measures of loan performance (whether it defaulted, its internal return rate (IRR), and its cumulative annual growth rate (CAGR), which are recorded in the listing data) on $AvgPoliticalDistance_i$ and control variables similar to those in (3) (see Table 15). The $AvgPoliticalDistance_i$ coefficients are insignificant. Thus, although political distance influences which loans are funded, it does not predict their performance thereafter. We explore this further below.

Exploration of Underlying Mechanisms and Investor Experience

As discussed earlier, our results may reflect investors' rationality or their preferences/tastes. We used the gravity model to investigate the rationality-based and preference-based mechanisms. We decomposed $Bids_{jk}$ into bids on listings with low and high DTI borrower ratios, splitting based on the median for all listings in the data (η = 0.24). We reran the gravity model for each of these two dependent variables (i.e., $Bids\_LowDTI_{jk}$ and $Bids\_HighDTI_{jk}$). We adjusted the independent variables (e.g., number of listings, average credit score) to match each dependent variable. We used the specification in which we decomposed $PoliticalDistance_{jk}$ based on whether the investor state was more liberal or more conservative. The intuition for this analysis is as follows. Assume that investors consider borrowers' likely political ideology to be a signal of their creditworthiness (rationality-based mechanism), but they use it only as a secondary factor if primary factors such as DTI ratio fail to convey a clear signal. (Similar behavior has been shown on eBay, where buyers use a seller's ethnicity as a proxy for quality but only when the seller's rating is low (Nunley et al. 2011.)) In this case, political distance should have little impact for the low DTI listings, because the relatively low DTI ratio of these borrowers provides a clear signal of creditworthiness.[11] Political distance should have more impact for the high DTI listings. This is because the higher DTI ratio may not provide a clear signal of creditworthiness, such that investors must use other information, such as political ideology. On the other hand, assume that investors do not consider political ideology as a signal of creditworthiness. Instead, they fund borrowers likely to have a similar political ideology purely due to preference (preference-based mechanism). In that case, political distance should have a similar impact for both high and low DTI listings. The results are shown in columns 1 and 2 of Table 16. The $PoliticalDistance_{jk}$ coefficient is not significant for investors from more liberal states for either the low DTI or the high DTI groups. By contrast, the coefficient is significant and negative for investors from more conservative

Table 16. Gravity Model Results by Debt-to-Income Ratio and Listing Category

	Low DTI	*High DTI*	*Debt Consolidation*	*Debt Expansion*
Political Distance: Investor State More Liberal (ln)	−0.006	−0.003	−0.009	−0.007
	(0.007)	(0.010)	(0.007)	(0.011)
Political Distance: Investor State More Conservative (ln)	−0.036***	−0.057***	−0.005	−0.040***
	(0.010)	(0.012)	(0.008)	(0.010)
Other gravity model controls[a]	√	√	√	√
No. of observations	2,450	2,450	2,450	2,450
R^2	0.98	0.98	0.98	0.98
Pseudo-log-likelihood	−109,179	−99,722	−64,885	−161,683

Notes. Standard errors in parentheses are clustered by state dyad. Values of control variables are adjusted according to different groups. Based on a seemingly unrelated estimation, the p value for the difference in the *Political Distance: Investor State More Conservative* coefficients is 0.02 for the Low DTI versus High DTI comparison and 0.0001 for the Debt Consolidation versus Debt Expansion comparison.

[a]We included all controls shown in Table 2.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

states for both groups. This disparity is consistent with our focal results. The results for investors from more conservative states suggest that the preference-based mechanism operates for these investors, given that political distance is negatively related to their lending decisions regardless of whether DTI is low or high. However, the coefficient is more negative (and significantly so (p = 0.02), based on a seemingly unrelated estimation) for the high DTI group. This suggests that the rationality-based mechanism is also at work, given that these investors place *more* weight on political distance when DTI is less likely to convey a clear signal about borrowers' creditworthiness.

We extended this line of inquiry by considering an additional signal of borrowers' creditworthiness: listing category. As previously stated, we coded listings as related to either debt consolidation or debt expansion. We posit that investors are likely to view a debt consolidation listing as a signal of greater creditworthiness than a debt expansion listing. This is because borrowers who consolidate their debt are more likely to repay than borrowers who take on additional debt (Wang and Overby 2022). We reran the gravity model with $Bids\_Consolidation_{jk}$ (the number of bids from investors in state j to borrowers in state k on debt consolidation listings) and $Bids\_Expansion_{jk}$ (analogous) as dependent variables. We adjusted the independent variables to match; for example, we changed $Listings_k$ to $Listings\_Consolidation_k$ (or $Listings\_Expansion_k$). Results appear in columns 3 and 4 of Table 16. The $Political Distance_{jk}$ coefficient for $Bids\_Consolidation_{jk}$ is insignificant, regardless of whether the investor state is more liberal or more conservative. The $PoliticalDistance_{jk}$ coefficient for $Bids\_Expansion_{jk}$ follows the same pattern as other analyses: It is insignificant for comparatively liberal investor states and negative and significant for comparatively conservative investor states. This provides additional support that the rationality-based mechanism operates for investors from relatively conservative states: They consider political ideology but only when the listing category (debt expansion) suggests potential credit risk.

We considered two other potential informational signals that might convey creditworthiness and thereby influence how much investors rely on political distance when making lending decisions: (1) whether the listing has a description (Gao et al. 2022) and (2) whether the listing contains an image (Pope and Sydnor 2011). As previously stated, we decomposed the $Bids_{jk}$ dependent variable into separate variables that reflect the presence of each signal and reran the gravity model for each. Results are shown in columns 1–4 of Table 17 and are broadly consistent with the Table 16 results. The $PoliticalDistance_{jk}$ coefficients are insignificant for more liberal investor states but negative and significant for more conservative investor states. This suggests that the preference-based mechanism operates for the latter group. However, the coefficients for more conservative investor states are more negative (and significantly so) when the signal (listing description or image) is missing. This suggests that the rationality-based mechanism also operates for investors from relatively conservative states, given that they place more weight on political distance when other informational signals (that might convey creditworthiness) are missing.

We also examined whether the role of political distance depends on investor experience. We split $Bids_{jk}$ into $Bids\_LowExperience_{jk}$ and $Bids\_HighExperience_{jk}$ based on whether the investors in state j had low or high experience (using the median for each state); see the gravity model description earlier for how we measured experience. We reran the gravity model for each dependent variable. As shown in columns 5 and 6 of Table 17, the $PoliticalDistance_{jk}$ coefficient is only significant for investors from comparatively conservative states with low experience. To explore this further, we split $Bids_{jk}$ into four groups based on experience quartiles for each state. As shown in the Online Appendix, Table A10, the "investor state more liberal" coefficients are generally

Table 17. Gravity Model Results: Description and Image Signals and by Investor Experience

	With Description	*Without Description*	*With Image*	*Without Image*	*Low Experience*	*High Experience*
Political Distance: Investor State More Liberal (ln)	−0.007	−0.007	−0.000	−0.008	−0.012	0.008
	(0.009)	(0.015)	(0.012)	(0.008)	(0.008)	(0.014)
Political Distance: Investor State More Conservative (ln)	−0.029***	−0.062***	−0.019*	−0.034***	−0.042***	−0.015
	(0.008)	(0.015)	(0.010)	(0.008)	(0.008)	(0.012)
Other gravity model controls[a]	√	√	√	√	√	√
No. of observations	2,450	2,450	2,450	2,450	2,450	2,450
R^2	0.98	0.95	0.98	0.99	0.99	0.98
Pseudo-log-likelihood	−177,424	−14,869	−111,795	−92,796	−80,272	−142,591

Notes. Standard errors in parentheses are clustered by state dyad. Values of control variables are adjusted according to different groups. Based on a seemingly unrelated estimation, the *p* value for the difference in the *Political Distance: Investor State More Conservative* coefficients is 0.03 for the With/Without Description comparison, 0.08 for the With/Without Image comparison, and 0.05 for the Low/High Experience comparison (but see the Online Appendix, Table A10).

[a]We included all controls shown in Table 2. In columns 5 and 6, we controlled for the *Average Dollar Value of Bids* placed by low and high experience investors instead of *Median Household Income* for the reason discussed in Table 9.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

insignificant, whereas the "investor state more conservative" coefficients are negative and significant, except for investors in the top quartile. This suggests that investors from conservative states stop considering political ideology as they gain experience, perhaps after they learn that the political distance between them and the borrower does not predict loan performance (as reported in Table 15).

Conclusion

Political differences are becoming increasingly stark in society, leading to a downturn in civil discourse. This paper shows that political differences also play an important role in how markets (specifically online lending markets) operate. We collect data from Prosper.com and apply multiple models and robustness checks to investigate how political ideology and political distance relate to investors' lending decisions. We find an asymmetric relationship: political distance matters for investors from relatively conservative states but not for those from relatively liberal states. We also show that borrowers are less likely to have their loan funded when they are from a state that is more liberal than those of the investors active when the borrower requests the loan. This causes harm both to the borrower (who does not get a loan) and to investors (who do not get to invest). However, the political distance between a borrower and the borrower's investors does not predict loan performance once the loan is funded. This relates to our finding that the relationship of political distance is not significant for investors with a high level of experience. It may be that after investors learn that political distance doesn't predict loan performance, they stop considering it in their lending decisions. This finding may also explain the asymmetric relationship for liberals versus conservatives. Conservatives are more uncertainty averse than liberals, causing them to stick to the familiar. It may be that as conservatives gain experience in the market, their uncertainty dissipates, and they fund a more diverse set of borrowers.

We extend our contribution by showing the likely mechanisms for our findings. We find evidence for a preference-based mechanism: investors from conservative states have a general preference for borrowers from conservative states. We also find evidence that a rationality-based mechanism operates, given that investors from conservative states have a *stronger* preference for borrowers from conservative states when signals of creditworthiness (such as DTI ratio) are unavailable and/or unclear.

Our study contributes to research on market efficiency. Despite the potential for online markets to improve efficiency, frictions persist (Burtch et al. 2014, Lin and Viswanathan 2016, Senney 2019, Liang et al. 2021). We contribute to this stream by showing that political differences create friction, albeit primarily for conservatives in our setting. This is important because determining whether and how frictions persist in online markets is necessary to either eliminate them or to acknowledge (and to develop workarounds for) the limitations they create. Our study also contributes to the literature in psychology and political science about political (in)tolerance between liberals and conservatives (Brandt and Crawford 2020). Understanding political (in)tolerance is important for maintaining a civil and productive society. The "prejudice gap" view holds that conservatives are more intolerant of liberals than vice versa, whereas the "ideological conflict" view holds that conservatives and liberals are similarly intolerant of each other (Crawford et al. 2017). These studies predominantly use survey-based measures of how individuals feel about others. We contribute by examining the relationship between political differences and individuals' *economic* transactions, finding evidence consistent with the "prejudice gap" view.

Our study has limitations, some of which we list here. First, we are unable to measure the political ideology of individual borrowers and investors, although our approach is consistent with the literature, and we conduct multiple analyses and robustness checks to limit the possibility that measurement error harms our conclusions. Second, as with most empirical studies, our results may not apply to time periods other than one that we study. It is possible that as more online lending decisions are made by automated algorithms, political distance will play a smaller role. Third, the results might not generalize beyond Prosper.com. Although we believe Prosper.com to be generally representative of online markets, political distance may play a smaller (or larger) in other markets, particularly if the design of those markets reveals more (or less) politically related information about market participants.

When considering the implications of our findings for market designers, it is important to note that Prosper.com provided no direct information about participants' political ideology during the study period. As such, there is no market design decision that created the friction that we document. This contrasts with other research that shows that design decisions, such as showing participants' names and photos, enable bias that creates frictions (Edelman et al. 2017, Mejia and Parker 2021). Despite there being no clear "flaw" to correct in our case, our results suggest steps that market designers can take. First, designers can educate investors on the factors that predict loan performance, which (notably) do not include a borrower's perceived political ideology based on state of residence. Our results suggest that investors learn the irrelevance of this factor anyway as they gain experience, but education may substitute for experience for new investors, thereby generating more matches. Second, designers can use a "group success intervention" (Younkin and Kuppuswamy 2018) that highlights successful repayment histories from borrowers that investors might otherwise disfavor. For example, a new investor from a conservative state (e.g., Oklahoma) might be shown that repayment statistics for borrowers from liberal states are similar to (or better than) those from conservative states. Third, designers can show the geographic distribution of the investors funding a borrower. This can highlight to new investors that other (likely more experienced) investors from their state are funding borrowers from politically distant states, which might generate a peer effect that mitigates the bias. Even if the bias cannot be eliminated, its practical effect may become muted as a larger (and more diverse) pool of investors enters the market (Greenberg and Mollick 2017). That is because if there are enough similar investors to fund a borrower's loan, then bias from dissimilar investors can be ignored. Although this did not appear to happen during our sample period (our results show that political distance reduces the likelihood that loans are funded), it might happen as online lending markets become more popular. Examining this is an opportunity for future research.

Endnotes

[1] Approximately 17% of listings include the borrower's city. We leverage this information later in the analysis.

[2] Prosper.com did not issue peer-to-peer loans during a "quiet period" from October 15, 2008, to July 13, 2009; our data do not cover this time period. The quiet period was required by the U.S. Securities and Exchange Commission (SEC) so that Prosper.com could register with the SEC and the states as a lender or loan broker.

[3] We also estimated separate gravity models for each year. Results are consistent across years and show no clear change in the relationship between political distance and investor behavior over time.

[4] Our focus on the same-sex marriage legalization event means that we are studying the effect of a signal of one aspect of liberal political ideology. Investors may react differently to other signals of liberal political ideology (e.g., reproductive rights).

[5] The state Supreme Courts for California and Connecticut ruled that same-sex couples had a right to marry on May 15, 2008, and October 10, 2008, respectively. The New York state legislature voted to legalize same-sex marriage on June 24, 2011.

[6] Some investors may have already viewed California as a relatively liberal state, such that the legalization event did not affect their belief of the (average) political ideology of California residents. However, the event likely *did* affect many investors' beliefs, perhaps by making those beliefs top-of-mind or by making investors believe that California was becoming more liberal.

[7] We used the same approach to construct analysis samples for the Connecticut and New York legalization events. However, this yielded only four treated listings for the Connecticut analysis and one for the New York analysis, rendering the results using these samples unreliable (although they are consistent with the results from the California analysis).

[8] In coarsened exact matching, each matching variable is first coarsened into broader bins. Treated and control observations are matched based on these bins. We used the following bins for the matching variables. *bids (t−3)*: [0,4] (4,85]; *bids (t−2)*: [0,4] (4,106]; *bids (t−1)*: [0,6] (6,15] (15,111]; *loan amount*: [0,5000] (5000,25000]; *interest rate*: [0.05,0.2565] (0.2565,0.36]; *image*: [0] [1]; % *funded before legalization event*: [0,0.116] (0.116,0.143] (0.143,2.18]; *loan grade*: [AA A B C D] [E F]; *DTI ratio*: [0.01,0.26] (0.26,2].

[9] Due to our inclusion of listing-investor state dyad fixed effects, the Poisson estimation drops listing-investor state dyads with the same number of bids for each of the seven days the listing is active (i.e., those with zero bids throughout). This is an artifact of the optimization algorithm used in the maximum likelihood estimation. This reduces the sample size used in the Poisson estimation, as shown in all tables that report OLS and Poisson results for the DID model.

[10] The "investor state more liberal" ("investor state more conservative") indicators in the DID and gravity models measure different things. In the DID model, they measure whether a state is more liberal (conservative) than California. In the gravity model, they measure whether a state is more liberal (conservative) than the other state in each two-state dyad. Despite this, both models show that political distance only influences the lending behavior of investors from comparatively conservative states.

[11] The coefficients for $DTIRatio_i$ in Table 15, which indicate that a higher DTI ratio predicts poorer loan performance, support this assumption.

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