Formalizing Inequality? Contract Formality and the Limits of Institutional Reform in Developing Markets*

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Abstract

Expanding access to formal contracting institutions is a central development policy prescription, yet its distributional consequences remain poorly understood. Formal contracts could counterintuitively *deepen* inequality by displacing powerful firms' influence: where such firms might otherwise exploit formal institutions, a formal contract levels that playing field, pushing them instead into informal channels where their leverage is less visible and harder to constrain. I test this using a survey experiment with 2,389 firms in Senegal's formal and informal economies, randomly assigning whether a hypothetical payment dispute involves a written or verbal contract. Formal contracts increase firms' use of courts and legal channels, but this effect is concentrated among registered, well-resourced firms. Inductive exploration shows that politically connected firms in particular respond differently. Rather than seeking formal enforcement, connected firms intensify their use of informal dispute resolution, leveraging political ties outside the legal system. These findings reveal an unintended consequence of institutional reform: formalization may reinforce the very inequalities it is designed to address by changing behavior in ways that entrench political advantage.

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1 Introduction

State institutions that protect property rights and allow for stable dispute resolution are widely considered critical for economic development. Because formal legal institutions are associated with increased firm activity and growth (North 1991; La Porta et al. 1998; Acemoglu, Johnson and Robinson 2001; Chemin 2009), policymakers have sought to expand access to formal contracting and encourage firms to formalize their practices (Williams and Round 2007; De Mel, McKenzie and Woodruff 2013; Bruhn and McKenzie 2014). Among other benefits, formal contracts are expected to clarify dispute terms, enable more complex transactions, and reduce forum shopping.[1]

Yet in much of the world, firms rely on informal contracts and social enforcement mechanisms even when formal institutions are available (Haley 1997; McMillan and Woodruff 1999*b*; Bhandari 2023). Firms may turn to local protection rackets, private security, or political patrons (De Soto 2000; Wank 2002; Sonin 2003; Frye 2004; Wilson 2010), or draw on dense social networks, reputation-based mechanisms, and relational contracts to structure their exchanges (Macneil 1973; Milgrom, North and Weingast 1990; Greif 1994; Baker, Gibbons and Murphy 2002; Brown, Falk and Fehr 2004; Macchiavello and Morjaria 2019).[2] These informal systems coexist with state-backed institutions and often structure everyday commerce.

Given this institutional pluralism, it remains unclear whether expanding access to formal contracts will improve equity or simply reinforce existing power dynamics. On the one hand, formal contracts might help level the playing field by curbing discretionary influence in dispute resolution. On the other hand, because political power transcends institutional boundaries, formalization may merely shift where and how that power is exercised.

In this paper, I theorize and test the possibility that expanding access to formal institutions can unintentionally deepen inequality.[3] While formal contracts can encourage all types of firms to use state legal enforcement mechanisms, I argue that this effect is driven by registered firms and firms with greater assets and revenue—firms with both the de jure standing and the de facto resources to

[1]Forum shopping refers to the practice of seeking the most favorable venue for dispute resolution.

[2]A firm's default strategy is often to avoid state-backed recourse entirely (Macaulay 1963; Pistor 1996).

[3]A note on terminology. The inequality at issue throughout this paper is not inequality in market outcomes—markets allocate unequally by design—but inequality in access to, and treatment by, the institutions that resolve commercial disputes: something closer to unequal rule of law. Where I refer to formalization "deepening inequality," I mean specifically that it widens differences between firms in their ability to obtain impartial resolution of a contract dispute.

use such institutions. [4] Following this logic, in exploratory analyses I explore whether politically connected firms respond to formal contracts differently as well.[5] If formal contracts limit connected firms' leverage within the legal system, they may instead turn to informal channels to exert their influence. The result, then, would be a displacement of political influence into less visible and potentially less accountable forms.

I test the impact of formal contracts on dispute resolution strategies using an original survey with 2,389 firms from both the formal and informal economies in Dakar, Senegal. Senegal offers a useful test case: its judicial institutions are relatively stable and formal contracts are legally enforceable, but these institutions are difficult to access and rule of law is selectively applied in favor of politically connected actors (Bhandari 2022; Kondylis and Stein 2023). Informal contracting remains widespread, with dense social networks structuring trade. I designed the survey based on in-depth interviews with firm owners in Dakar conducted over a year of fieldwork. The first wave was carried out in 2017-2018, followed by a second wave in 2022 among a randomized subset.[6] The survey included an embedded experiment in which respondents evaluated a hypothetical contract dispute under randomly assigned contract conditions—either a formal written contract or a verbal agreement. Respondents then rated their likelihood of pursuing various formal and informal dispute resolution strategies. This design allows for a test of how contract formality shapes firms' recourse preferences in a hybrid institutional environment.

Of course, contract formality is typically not exogenous to exchange, as firms choose their contracting mechanisms based on other characteristics of the exchange (Williamson 1975). The experiment deliberately circumvents this by stipulating the contract type, thus taking a partial-equilibrium approach: *if* a firm were to hold a formal contract, how would it then seek recourse?

[4] This paper engages with three distinct formal/informal distinctions: (i) *formal versus informal firms*, defined by whether a firm is registered with state authorities (e.g. De Mel, McKenzie and Woodruff 2013); (ii) *formal versus informal contracts*, referring to written legal agreements versus verbal ones; and (iii) *formal versus informal dispute resolution strategies*, referring to state-backed mechanisms (police, lawyers, courts) versus social or relational mechanisms (amicable resolution, social networks, religious networks, political connections). These dimensions are related but analytically distinct, and I use this terminology consistently throughout.

[5] For additional details on why this analysis is exploratory, see Appendix Section A.4 which describes deviations from the pre-analysis plan.

[6] The initial survey measured political connectivity coarsely, and it became clear when analyzing those data that connections were a theoretically decisive variable. I thus returned to the field in 2022 and administered a follow-up survey to a randomized subset of the original respondents, collecting detailed measures of familial political connections. Given this sequence, the analyses of political connections should be considered inductive exercises to be submitted for future testing.

Domestic and international interventions to boost formality have made formal contracts difficult to universally avoid, and this experiment helps provide evidence to understand how firms in transitional economies react to the introduction of formal institutions in trade.

The results show that when deals are based on formal contracts, firms are more likely to pursue state-backed enforcement—turning to police, lawyers, or courts—and less likely to rely on social enforcement mechanisms. But this aggregate effect masks important heterogeneity. The formalizing effect of contracts is especially pronounced for registered firms and firms with greater assets and revenue. Informal firms, while somewhat more likely to use formal channels under formal contracts, still face structural barriers to access. In the exploratory follow-up analyses, meanwhile, politically connected firms do not increase their use of formal enforcement when presented with formal contracts. Instead, they become significantly more likely to resolve disputes *informally*, including by turning to political intermediaries. This pattern is concentrated among firms in the formal economy. One reading of this empirical pattern is a displacement of influence: when formal contracts constrain favorable outcomes in the legal system, connected firms shift their recourse to informal venues where their leverage is more effective.[7] In this way, expanding access to formal contracts may unintentionally encourage forum shopping and reinforce inequalities in dispute resolution.

By providing evidence of the counterproductive impact of expanding access to formal institutions in developing economies, this paper makes several contributions. First, these findings highlight the limited reach of formal institutions in contexts where informal practices remain deeply embedded. While formal contracts, on average, increase firms' use of state-based dispute resolution, this aggregate pattern masks unequal effects. The benefits accrue disproportionately to registered firms and to firms with greater assets and revenue, while politically connected firms retain the ability to forum shop across institutional domains. Formalization, on this reading, does not fully eliminate informal influence so much as relocate part of it—though this displacement involves only the most privileged firms and coexists with an average decline in informal dispute resolution, including among the politically connected. These results underscore the limitations of formal institutions in achieving their intended goals where state and nonstate systems coexist. More broadly, the paper raises important questions about current policy approaches: large-scale efforts to formalize the private sector may inadvertently reinforce the very inequalities they aim to correct. As international

[7] I discuss alternative interpretations in the conclusion.

actors continue to pressure governments to adopt institutional reforms, careful attention to their distributive consequences is essential for promoting equitable and sustainable growth.

This paper also contributes to research on the demand for property rights protection (Pistor 1996; Hendley 1997, 1999; Hoff and Stiglitz 2002; Gans-Morse 2017). The mere presence or legal quality of state institutions does not ensure their use, particularly in societies where firms can access alternative means of enforcement. While much research has focused on the consequences of property rights, the determinants of property rights have been relatively understudied (Frye 2004). This paper demonstrates that variation in contract formality plays a significant role in shaping property security strategies. The results show that removing demand-side barriers to legal recourse does not address deeper issues of inequality in enforcement.

The paper also contributes to the study and measurement of "formality" in business and politics. Formality is often treated as a single attribute of a firm, but this paper considers three separable aspects that formality can entail: whether a firm is registered with the state, whether a transaction is structured by a written contract, and whether a dispute is taken to a state-backed forum. This study measures all three independently which makes it possible to ask how variation in one dimension produces variation in another. That question is unanswerable when the three are collapsed into a single index or inferred from one another, as they often are. This paper suggests that valuable insight can come from separating the dimensions empirically instead of assuming them away.

2 Theory

Before elaborating on the theoretical basis for pursuing formal or informal dispute resolution, I develop a tractable framework for understanding firms' objectives when it comes to disputes. This is because rather than making decisions by thinking through formal or informal enforcement in those terms, firms seek the resolution with the most profitable outcome possible. Consider a firm choosing among available recourse strategies j after a counterparty defaults. Its objective is to maximize the expected value of recovery net of the costs of pursuing it: roughly, $p_j R - c_j$, where p_j is the probability that strategy j produces a favorable outcome, R is the amount at stake, and c_j is the cost of using the forum (e.g., fees, time, paperwork, and so on). Simply, firms select the strategy with the highest expected net return.

Three implications follow, and they help to situate the rest of the theory and also help to make sense of the eventual empirical results. First, formality *per se* does not enter the objective function; it enters only through its effect on p_j and c_j. For example, a formal contract matters because it might raise p_j for state-backed forums—written evidence makes a court more likely to rule in the holder's favor—and not because firms place independent value on behaving formally. Second, firms differ in their cost vectors, and these differences can be structural (e.g., firm formality, assets, etc.). Unregistered firms, for example, may face a higher c_j for state forums because their legal standing is dubious and their access is more restricted; or perhaps firms with fewer assets face higher c_j because lawyers and court fees consume a larger share of their resources. A formal contract that raises p_j for everyone will therefore change behavior most among firms for whom c_j was not already prohibitive. Third, some firms may have a higher p_j in informal forums because they can bring political pressure to bear there, and for them a formal contract changes the relative attractiveness of the two arenas in a way it does not for anyone else. These politically connected firms, a logic I expand upon below, can counterintuitively behave more informally when made to use formal contracts.

2.1 Formal and informal dispute resolution

Efficient dispute resolution via state institutions is considered instrumental for economic development (North 1991; Olson 1993; Acemoglu, Johnson and Robinson 2001; Djankov et al. 2003). This can be a complex prospect in countries with weak rule of law, however, as firms often use both formal and informal institutions to resolve their disputes.

On the formal side, firms can engage legal institutions such as the police, lawyers, and courts when seeking recourse. While many firms use these formal dispute resolution strategies, even in developing countries, the existence of legal recourse options does not necessitate that firms will use them (Pistor, Wellons and Sachs 1999; Milhaupt and West 2000; Gans-Morse 2017).[8] Legal recourse options can be expensive and entail significant transaction costs (Williamson 1985; Kondylis and Stein 2023). This is especially true in places where legal institutions are excessively procedural, often inherited from former colonial powers (La Porta et al. 1998; Djankov et al. 2003).

[8]Similarly, weak rule of law does not automatically imply that firms will avoid formal institutions altogether (Whiting 2010).

Informally, firms have access to a diverse array of non-state dispute resolution options. As their initial strategy, firms typically try to resolve conflicts amicably, preferring to negotiate before bringing the matter to costly enforcement arenas (Macaulay 1963). Another strategy is to use shared social networks, particularly in contexts where these networks are dense and important for doing business (Akerlof 1970; Fafchamps 1996; Grimard 1997; Fafchamps and Minten 1999, 2001*b*, 2002; Miguel and Gugerty 2005; Habyarimana 2007; Golub and Hensen-Lewis 2012). This strategy relies on social enforcement that stems from mechanisms like in-group pressure, reputation costs, and shared sources of authority that agree to punish defectors (Klein and Leffler 1981; Greif 1989; Coate and Ravallion 1993; Fearon and Laitin 1996).[9] Ethnic and religious groups are examples of social networks that can underpin social enforcement, especially in much of Africa (Ebin 1993, 1995; Grimard 1997; Golub and Hensen-Lewis 2012). And in places where institutional access is a function of political proximity, firms can seek informal enforcement through their political connections (Wank 2002; Frye 2004; Gans-Morse 2017; Bhandari 2022). Indeed, political connections can be useful in both formal and informal dispute resolution.

How do firms decide which enforcement strategies—formal or informal—to use? The growing literature on legal pluralism and forum shopping offers some insights.[10] A frequent reason firms choose informal justice relates to state capacity: when rule of law is weak, reliance on non-state or customary institutions increases. Yet state capacity alone is an insufficient explanation, as there are places with strong state capacity where firms nonetheless seek informal resolution, and places with weak capacity where firms seek formal resolution (Gans-Morse 2017).[11] The primary reason that firms forum shop among state and non-state resolution venues is to increase their likelihood of a favorable outcome (Sandefur and Siddiqi 2013).

In this article, I focus on a factor that can determine firms' decisions to seek formal or informal dispute resolution but that has received comparatively little focus: the availability of formal

[9]While these studies suggest that repeated interactions can sustain stable trading equilibria (e.g. Baker, Gibbons and Murphy 2002; Brown, Falk and Fehr 2004; MacLeod 2007), reliance on social-based enforcement constrains economies to smaller sizes like flea markets (Fafchamps and Minten 2001*a*).

[10]See, for example, von Benda-Beckmann (1981); Meinzen-Dick and Pradhan (2002); Tamanaha (2008); Sandefur and Siddiqi (2013); Lazarev (2019); Acemoglu et al. (2020); Blair (2019); Blair, Karim and Morse (2019).

[11]Transaction costs can also explain why firms may choose informal enforcement over formal enforcement (Macaulay 1963; Williamson 1985), as can demand-side barriers to legal access related to operating in the informal economy (Hay and Shleifer 1998).

contracts. Especially given policymakers' push in recent years toward expanding the availability of formal contracts and mandating their use, it is important to establish whether and how formal contracts affect firms' willingness to seek formal or informal dispute resolution.

2.2 Formal contracts and forum shopping

Contracts specify exchange terms to mitigate risks that may otherwise prevent deals from occurring, and, importantly, clarify recourse options in the case of contract rupture (North 1981; Williamson 1985; Hart 1995). But in much of the developing world, contract type and formality can vary widely. At one end of the spectrum are formal, written contracts that use state contract templates that have undergone legal review. At the other end are verbal contracts, agreements made between two exchanging parties that are not specified in state-enforceable terms.[12] The type of contract chosen for a deal is often endogeneous to the deal itself (Williamson 1975). For example, in an interview I conducted in Senegal with the supply chain manager of a large industrial firm, he reported occasionally preferring informal contracts because it allowed the company to renegotiate terms as market prices fluctuate.[13] In contrast, when conducting business with a new partner that had not yet established a reputation in the marketplace, he preferred formal contracts to offset some of the risks (e.g. Johnson, McMillan and Woodruff 2002). The endogeneity of risk to contract choice has made studying the causal impact of contract choice difficult to date.

In this paper, I ask how formal contracts affect firms' dispute resolution strategies when a deal is ruptured. While existing theory suggests that strong contracting institutions increase the security of property rights (Baker, Gibbons and Murphy 1994; Djankov et al. 2003; MacLeod 2007), there are many places where both formal and informal contracting strategies coexist; the formality of contracts is an important yet understudied predictor for demand of rule of law. I argue that the type of contract used in a deal can affect the strategies available to firms. Given that formal contracts draw their power from state institutions, it follows that formal contracts will lead to greater use of state-backed enforcement methods such as courts in the aggregate.[14] Conversely, if a

[12]In between these levels are written agreements that either do not meet the requirements for state enforceability or that are incomplete. See Hart (1988); Hart and Moore (1988); Bernheim and Whinston (1998); Hart and Moore (1999); Tirole (1999) for discussion on the political economy of incomplete contracts.

[13]Author interview in Dakar, Senegal, July 4, 2016. This strategy was feasible given the large size of the firm as well as the near-monopoly it possessed within its sector. Firms that face greater competition or that seek protection from market fluctuations may prefer written contracts.

[14]An important scope condition for this theory is that formal institutions have sufficient capacity to

deal is based on an informal contract, it is more difficult to seek recourse via state institutions, and thus firms will use social dispute resolution strategies like ethnic or religious networks. Formal and informal methods of dispute resolution are not mutually exclusive, however (Akerlof 1970; Kranton 1996; Johnson, McMillan and Woodruff 2002; Sobel 2006; MacLeod 2007; Acemoglu et al. 2020). For example, simply because a firm hires a lawyer to work on a case does not preclude that firm from also making appeals to a religious intermediary to resolve the conflict. Similarly, political connections can be used to influence both state-based enforcement processes as well as enforcement via social networks.

There might also be important differences by economic sector and wealth. Informal and formal firms, for example, face different constraints when enforcing contracts. While formal firms can use both state and social enforcement methods, informal firms do not enjoy the same level of access to formal institutions.[15] Because of informal firms' dubious legal status, states may restrict use of formal enforcement institutions to firms that have registered with the state (Pistor 1996; Hay and Shleifer 1998). Thus, regardless of the type of contract used, I expect formal firms to be more likely to use formal dispute resolution strategies during contract disputes. By the same logic, formal contracts may differentially benefit firms with the greatest assets and revenue in developing economies, as these firms are most likely to be able to afford the costs associated with formal enforcement. Extending access to formal contracts may disproportionately lead registered and better-resourced firms to opt for state enforcement, without reducing their ability to rely on social enforcement strategies should they choose. Formal institutions might therefore enhance privileged firms' ability to forum shop, deepening inequality in dispute resolution.

2.3 Exploratory: Politically connected firms and displacement of enforcement

One informal advantage that expanding access to formal contracts can disrupt, but not necessarily eliminate, is the use of political connections. Whether via firm owners' or managers' direct

enforce contracts. Sufficient state capacity in routine commercial matters here more than regime type, as the capacity to adjudicate disputes is not unique to democracies. Just as many autocracies handle these relatively small-level disputes routinely, some democracies or hybrid regimes do not.

[15]Indeed, this may be a reason that some informal firms formalize in the first place. This selection problem is not a major concern in this paper as only 11% of the formal firms in the sample included this among their reasons for formalization. Furthermore, as the correlation matrix in Appendix Section A.7 shows, formal firms do not have substantially greater trust in courts than informal firms.

ties to state institutions (e.g. Markus 2015; Frye 2017; Betz and Pond 2023) or more subtle familial ties (e.g. Bhandari 2022), politically connected firms have repeatedly been shown to use their connections to their advantage. Among other benefits, connected firms and owners enjoy greater valuations and profitability (Fisman 2001; Faccio 2006), gain preferential access to bureaucrats (Szakonyi 2018), and benefit from the selective enforcement of regulations (Kubinec, Lee and Tomashevskiy 2021).

Interviews and respondents' accounts in open-ended survey questions suggest three channels through which political connections can shape commercial disputes in particular. The first is direct intercession: a connected party asks an official to intervene on its behalf. Respondents who reported resolving a past dispute through a connection described precisely these kinds of contacts, most commonly involving municipal officials and ministry staff. The second is anticipatory deterrence: the counterparty, knowing who the other side knows, concedes without any intervention actually occurring. This is largely undetectable, and, in equilibrium, gives connected parties an advantage that leaves no trace. The third operates through the state's own discretion, where officials exercise judgment in the connected party's favor (e.g., Lu, Pan and Zhang 2015). Whether due to information asymmetries or explicit biases, the result of this channel is the state siding with the connected party.

What these channels share is that their effectiveness in state-backed dispute resolution depends on the scope for political influence to alter the expected outcome. This is important for the question at hand because formal contracts may reduce this scope by documenting the terms of an exchange, thus limiting the discretion available to officials adjudicating a dispute. Political power imbalances should therefore matter especially when contractual terms are difficult to verify. Verbal agreements are less likely to be brought to courts in the first place because they are difficult to prove, but conditional on a dispute over an unwritten agreement reaching a state forum, the absence of documentation may make political influence particularly consequential, since the outcome depends more heavily on discretion than on written record.

This logic helps explain why domestic and international policymakers come to view formal contracting as leveling the playing field in economic exchange. By specifying contractual terms, formal contracts should limit the discretion of potentially biased state institutions and thereby diminish the disproportionate influence of politically connected firms. In this view, formal contracts can make formal enforcement both fairer and more predictable (Williamson 1985). This should be especially likely where firms take written contracts seriously and where state institutions face meaningful limits on their ability to disregard contractual terms in favor of connected parties.

Yet this reasoning leaves open how politically connected firms will respond once formal contracts become available. The answer depends on whether formal contracts actually constrain the political advantages that connected firms enjoy in state-backed enforcement. If they do not—because contracts remain weak, easily manipulated, or simply disregarded by formal institutional connections—then formal contracting may reinforce connected firms' preference for formal enforcement. In this line of reasoning, a written contract can become an additional instrument through which a politically powerful firm presses its claim before courts, police, or administrative officials.

If formal contracts do work as intended, however, the incentives point in the opposite direction. By reducing the value of political connections in formal forums, contracts may encourage connected firms to shift toward *informal* methods of dispute resolution where their political influence remains useful. These connections allow firms to pressure counterparties and mobilize intermediaries while bypassing state institutions altogether. The utility of political connections does not disappear with the introduction of formal contracts; instead, formal contracts may instead alter *where* those connections are most effectively deployed.

In the terms introduced earlier, the two possibilities differ in what a formal contract does to p_j (the probability of successful enforcement) for connected firms. Under the first, documentation adds to the leverage connections already provide in state forums, raising p_j there and pushing connected firms further toward formal resolution. Under the second, contracts limit the discretion that connections exploit, decreasing p_j by constraining what an adjudicator can plausibly decide. But because their advantage in informal arenas remains unchanged, connected firms might then seek resolution via informal forums. This second account predicts something distinctive and testable: the shift should appear only among firms that could plausibly have used formal institutions in the first place. Firms with no realistic access to courts (e.g., unregistered firms) have no formal advantage to lose, and so nothing to reallocate away from.

3 Context

I test these claims using data from a large-scale firm survey and embedded experiment in Dakar, the political and economic center of Senegal. I selected Senegal because of its mix of formal and informal enforcement institutions as well as recent changes that have made the experimental treatment—access to formal contracts—particularly salient.

3.1 Formal and informal contracting in Senegal

Senegal is a relatively stable democracy, but its institutions for rule of law lag behind.[16] It places poorly in the World Bank's ranking for ease of enforcing contracts (World Bank 2020), and its legal system is characterized by significant costs and high degrees of procedural formalism (Kondylis and Stein 2023). In this context of high transaction costs, businesses use a mix of formal and informal contracts as part of their regular business operations. Formal contracts typically take the form of written agreements that meet local standards to be executable in courts of law. While lawyers may review these contracts prior to signing, this is not necessary to make a contract legally binding. An informal contract in Senegal typically takes the form of a verbal agreement. Though verbal contracts are technically legally binding in Senegal, in practice legal authorities and firms see such agreements as carrying little weight before courts.[17] Verbal contracts are seen as particularly useful for politically connected firms, as the lack of written record of a deal facilitates the influence of state connections.[18]

A firm in Senegal is typically considered formal if it is registered with the state and possesses a registration number,[19] and informal if not. The Senegalese government in recent years has implemented policy reforms to lower the barriers of access to the formal economy as well as implemented

[16]Senegal's constitutional crisis in early 2024 highlights this institutional middle ground well. President Macky Sall unilaterally postponed presidential elections in a move widely perceived as an attempt to retain power amidst growing popular opposition. That he was able to do so suggests that Senegal's democracy is less stable than previously considered. However, in a show of institutional strength, the courts rejected Sall's claim, a decision with which he eventually complied. This case nicely reflects the present article's scope conditions—contexts in which rule of law exists and can be enforced, but where it is enforced selectively and often informally.

[17]Author interview at state procurement agency, Dakar, Senegal, July 20, 2016.

[18]Author interview in Dakar, Senegal, July 4, 2016. As the supply chain manager at a connected firm told me, there is greater leeway for their influence if terms are vague or unspecified.

[19]This number is called the *NINEA*, a term with which all firms are familiar.

tax reforms that might incentivize formalization, to mixed results (Gottlieb 2024). In practice, the distinction between formal and informal firms can be fuzzy; formal firms often negotiate their taxes informally, use informal contracts, and rely on social signals for deal security (Bhandari 2023). Still, visibility to the Senegalese state importantly distinguishes formal and informal firms.

International and domestic agencies alike in recent years have pushed to increase formalization in Senegal's private sector. Key drivers include the World Bank, the Organization for the Harmonization of Business Law in Africa (OHADA), the African Development Bank, and USAID, all of which have pushed Senegalese policymakers to adopt greater formalization of business practices.[20] The bulk of formalization programming in the Global South, including Senegal, has focused on firm *registration*: simplifying business registration, lowering its cost, and adjusting tax treatment to make entering the formal sector attractive (Bruhn and McKenzie 2014; De Mel, McKenzie and Woodruff 2013). Programs aimed specifically at getting firms to use written contracts are a smaller yet still important part of the agenda.[21] While the experiment in this paper manipulates contract formality, its results may also carry implications for formalization policy more broadly, as registration and contract formality both operate by moving transactions and disputes into the state's ambit.

For their part, firm owners are not universally opposed to such contract formalization. For example, both formal and informal firms in my sample stated that they wished they were able to use formal contracts more often than they currently do. This chimes with past work in Senegal that shows that even when firms in Senegal seek more formalized practices, informalities like political connections complicate their functioning (Bhandari 2023). Still, while the interplay of formal and informal institutions certainly characterizes the private sector in Senegal, firms appear, at least in theory, to be interested in a move toward formalizing their contractual behavior. Combined with the top-down push toward formality, it is important to examine how the expansion of formal institutions moderates firms' property security strategies.

[20]See, for example, the ILO's *Stratégie nationale intégrée de formalisation de l'économie informelle et plan d'action opérationnel de transformation du secteur informel*, the Government of Senegal's *Sénégal Émergent*, and the World Bank funded *Projet d'Amélioration du Climat des Investissements (PACI)*, among many others.

[21]For example, OHADA's Uniform Acts standardize commercial contracting across member states and supply model instruments intended for direct use by firms, and business-environment programs have distributed standardized contract templates and provided contract-drafting support as part of broader competitiveness work.

3.2 Contract dispute resolution in Senegal

Contract disputes are common among Senegalese firms, leading them to frequently seek resolution. Approximately one-third of my firm sample reported having experienced a past dispute, with both formal and informal firms reporting similar rates of dispute. The typical dispute stemmed from the failure of the other party to pay for goods or services rendered, or from the receipt of substandard products. Firms' strategies for resolving past conflicts varied widely, with informal firms relying largely on amicable solutions and social networks, and formal firms relying more on formal dispute resolution processes (police, lawyers, and courts). Still, neither formal nor informal enforcement is exclusive to a sector, as Table 1 makes clear. While there are wider divergences in the use of formal recourse by sector, informal recourse is common in both: approximately 76% of registered and 85% of unregistered firms used at least one informal strategy. Both formal and informal strategies are frequently combined rather than substituted: 16% of registered firms and 10% of unregistered firms used both formal and informal recourse, while only 15% and 3% respectively used formal recourse alone. The modal experience in both sectors is informal recourse only.

	Recourse used (% of firms)					
	Informal	Formal			*Any use of*	
Firm status	only	only	Both	Neither	Formal	Informal
	(1)	(2)	(3)	(4)	(5)	(6)
Registered (formal) firms	59.9	15.0	16.3	8.8	31.3	76.2
Unregistered (informal) firms	75.3	2.9	9.6	12.1	12.6	84.9

Notes: Among the 785 firms (546 registered, 239 unregistered) reporting a past contract dispute. Formal recourse comprises police, lawyers, and courts; informal recourse comprises amicable resolution, social networks, religious networks, and political connections. Categories in columns 1–4 are mutually exclusive and sum to 100%; columns 5 and 6 are not mutually exclusive.

Table 1: Recourse used in past disputes, by firm registration status

Formal dispute resolution in Senegal can take several forms. The most accessible state-based option, and the one firms use most frequently, is the police. Of firms in my sample that reported contract disputes, 18% sought help from the police, including 13% of informal firms and 21% of formal firms. Police involvement typically begins with filing a police report, after which further action is largely at the discretion of police staff. Although contract disputes are generally civil matters, police involvement is relevant in Senegal because filing a report provides an accessible and relatively low-cost first step toward formal recourse, particularly for smaller firms. Firms generally seek something short of adjudication from the police. A police report creates an official record of the claim, which can serve as documentation if the dispute later reaches court and signals the seriousness of the complaint. Police may also summon the counterparty for questioning, a step that carries no civil authority to compel payment but can nevertheless encourage settlement. In some cases, officers act as informal mediators, convening the parties and helping broker an agreement without a case proceeding further. Police do not adjudicate civil claims and cannot order payment; their leverage is primarily procedural and reputational. Their accessibility, low cost, and ability to generate pressure without court involvement help explain why they are the most commonly used state-based channel.

Firms seeking more extensive formal recourse can hire a lawyer or take a dispute to court. These options, however, are prohibitively expensive for most in Senegal (Kondylis and Stein 2023). In an effort to reduce long waiting times and administrative backlogs, Senegal introduced a commercial court designed to process business disputes more efficiently (PressAfrik 2018). Although reports suggest that the court has reduced waiting times and improved judicial efficiency (EuroNews 2019), concerns about the accessibility and performance of the judiciary remain prominent, and judicial reform has continued to feature on the national political agenda (RFI 2024). Along with broader efforts to promote formal contracting, these reforms make the consequences of contract formality for Senegalese firms particularly important to understand.

Informal dispute resolution also takes many forms in Senegal. As noted above, firms most often initially attempt to resolve conflicts amicably, negotiating directly with the counterparty in hopes of reaching an agreement before involving outside parties. When direct negotiation fails, social networks can provide another means of resolving disputes given the density of social networks and the importance of social standing in Senegal. Shared membership in business associations,[22] social organizations, and other social circles can reduce uncertainty about a potential trading partner's trustworthiness and provide third parties capable of intervening when a deal goes awry. Firm managers can appeal to individuals within a shared social network who may mediate the dispute or

[22] I show in Appendix A.6 that Senegalese firms, particularly informal ones, have relatively low membership in business associations, a notable difference from firms in neighboring countries (e.g. Grossman 2020).

use social pressure to encourage compliance. Religious networks are a particularly important source of informal enforcement in Senegal. The religious brotherhoods to which much of the Senegalese population belongs are important in structuring trade, and religious authorities such as *marabouts* can serve as trusted third parties in disputes (Cruise O'Brien 1971; Ebin 1993; Mbacké 2005; Minard 2009; Golub and Hensen-Lewis 2012).

Political connections provide a related but distinct form of informal enforcement. As elsewhere (Wank 2002; Gans-Morse 2017), firms can use personal political access and influence to pressure counterparties and encourage resolution. What makes this mechanism distinctive is that political influence can operate both outside and through state institutions: a connected firm might directly enlist a political intermediary, threaten to do so, or use political access to induce favorable treatment from police or other officials. In this sense, political connections constitute an informal enforcement mechanism even when their influence ultimately reaches a formal state actor, because enforcement depends on personal access rather than the impersonal application of formal rules. Indeed, even the threat of mobilizing such connections may be sufficient to encourage settlement.

Firm managers in my sample generally believed that politically connected firms enjoy substantial advantages in the Senegalese private sector. A majority viewed political connections as useful for business, most commonly citing privileged access to state contracts and an ability to disregard rules with relative impunity. These perceptions suggest that firms have reason to expect political access to matter not only in obtaining economic opportunities, but also when conflicts over economic exchange arise.

These enforcement mechanisms are particularly important because firms cannot always rely on reputation or repeated interaction to sustain exchange. While reputation can structure trade in some contexts (e.g. Greif 1989), reputational information is often limited, particularly for newer firms and trading partners with whom firms have little prior experience. Such information is inherently unavailable for newly entering firms and can be especially difficult to obtain for relatively faceless businesses, such as those based abroad or operating online (Dellarocas 2003; Elfenbein and Lerner 2003; MacLeod 2007). Repeated interaction is also an imperfect substitute for enforcement, particularly as firms become more specialized and one-shot transactions become more common.

Before turning to the research design and results, one feature of the Senegalese context is important to clarify: firm resources and political connections are empirically distinct. This contrasts with much of the literature on politically connected firms, which draws heavily on large listed companies (e.g., Fisman 2001; Faccio 2006). While political connections and firm resources often travel together in such settings, this is much less true in Senegal. The modal firm in Senegal's private sector is small, and the connections that matter for such firms are often personal and familial: a cousin in a ministry, a sibling in a municipal government, or a relative in the customs service (Bhandari 2022). Access of this kind often follows kinship and locality rather than commercial scale, and is thus not necessarily concentrated among the largest firms. Only 3.9% of firms in the follow-up sample report a current or former politician as an owner or board member—the ownership-based measure standard in much of the literature—while roughly a third report a family member employed as a state official. Consistent with this distinction, firm valuation does not predict familial political connections in the sample (Appendix Figure A2), and connected and unconnected firms do not differ appreciably in assets or size.

This pattern is unlikely to be unique to Senegal: in settings where small and medium firms dominate the private sector and the state is a major employer, personal access to officials does not necessarily track commercial scale. Like in the Senegalese context, this is an important point because "well-resourced" and "politically connected" can identify substantially different sets of firms. This distinction becomes critical in the results that follow.

4 Research design

To test the impact of contract formality on dispute resolution strategies, I fielded a large-scale survey of formal and informal firms in 2017-2018 in Greater Dakar. For supplementary data, particularly regarding firm managers' political connections, I conducted a follow-up survey with a randomized subset of respondents in 2022.[23] As noted in the introduction, this second wave was not anticipated in the pre-analysis plan. It was undertaken after analysis of the first wave of data convinced me that political connectivity was a theoretically decisive moderator and that the earlier instrument had measured it too coarsely to be informative. Analyses using the 2022 measures are therefore exploratory throughout, and I label them as such wherever they appear.[24]

[23]Separate data from this follow-up survey was used in *[ARTICLE CITATION REDACTED FOR ANONYMITY.]*

[24]See Appendix Section A.4 for the pre-analysis plan as well as details of any deviations.

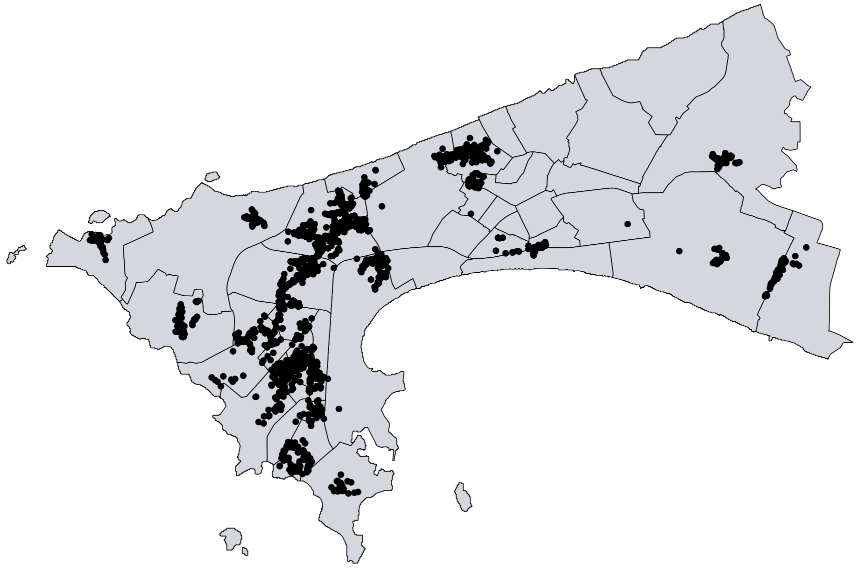


Figure 1: Location of sample firms in Dakar peninsula.

4.1 Sample

The survey was administered to 2,389 firms in both the formal and informal economies. I divided each district within Dakar into subdistricts, and enumerators followed a preselected random sampling step that varied by the density of the subdistrict in order to minimize spillovers. Figure 1 shows the location of sample firms in the Dakar peninsula.

Because the survey asked how firms would respond to contract breaches, it was important to interview respondents with direct responsibility for management and contracting decisions. The survey was therefore limited to firm owners and managers who made such decisions on behalf of their firms, rather than rank-and-file employees. In most cases, the respondent was the firm owner. At larger firms, when the owner was unavailable, the survey was completed by the director of administration and finance or the managing director. One respondent completed the survey per firm.

To both encourage participation and minimize social desirability bias, enumerators approached each business with a letter from a Senegalese research institution with which I have a longstanding affiliation. The letter specified to respondents that their data would be used solely for research purposes and that their responses would be anonymized. When asked at the end of the survey, almost all respondents (over 99%) confirmed that they believed the research team had no affiliation with the Senegalese government.

4.2 Survey content

Information about formal and informal firms in Senegal has been historically difficult to attain. There is limited reliability in the data the government has published on firms to date, as firms are hesitant to share information, especially with the state. The survey thus served an important descriptive purpose, particularly as it was administered to a sample of firm owners and managers that is otherwise difficult to reach. I included questions regarding formalization status, valuation, profits, number of employees, and firm behavior. To ascertain information about firm managers themselves, respondents were asked about their ethnicity, religion, education, membership in social and business organizations, confidence in state institutions, political affiliation, and past government involvement. Questions on firm behavior focused largely on contract enforcement and tax compliance, including the resolution of previous contract disputes as well as whether and to whom firms paid legal and illegal taxes.

I collected information about respondents' political connections with a randomized subset of the sample in 2022. Very few respondents are politically connected in the ways that prior research has tended to measure connectivity: few firms have current or former politicians as owners or board members (3.9%). Thus in addition to detailed questions about firm owners' political activity, I also included questions about their family's political connections, as these subtler connections have been shown to be a strong determinant of business practices in developing economies (Fafchamps and Minten 2001*a*; Fisman 2001; Bhandari 2023).[25] In this sample, the rare occurrence of connections through firm ownership or board membership mask the true political capital of companies operating in Senegal: of the 535 firms in the 2022 wave, 32.7% report at least one family member currently employed as a state official. Among the connected, the modal firm reports a single such relative

[25]Because this variable was measured after the original survey, a natural concern is whether connectivity between the two waves is stable. Unfortunately, the 2017-2018 instrument contained only two coarse political measures—whether the respondent belonged to a political party and whether the respondent had personally worked for the state—with familial connections going unmeasured. While the nature of party membership in Senegal is unstable by design (parties frequently emerge and die or get absorbed into others), it is encouraging that firm owners report similar levels of past state work in both sample waves: 3.6% in 2017-2018 and 3.9% in 2022. Although family connections are a relatively stable category in Senegal, this is additional reason to treat these analyses as exploratory.

(39% of connected firms), with 17% reporting two and the remainder reporting three or more. Given the doors that possessing even one connection can open, I define family political connections as having at least one family member employed as a state official.

The survey included an experimental element to test the effect of contract formality on firms' dispute resolution strategies. In the experiment, respondents stated their likelihood of using several formal and informal dispute resolution strategies when another business failed to make a payment that was stipulated in a randomized formal or informal contract. Payment breach was chosen for the survey experimental vignette because, as my data reflects, it is the most common reason for inter-firm disputes in Senegal. The wording of the survey experiment purposefully mirrored the survey conducted in Russia by Gans-Morse (2017), in order to facilitate comparisons to a separate context for generalizability. Respondents were asked the following:

> "Imagine that another business owes yours a significant sum of money as a result of services or products you provided, and that this business hasn't paid by the previously agreed upon deadline stipulated in...
>
> **Treatment**: [*...a legal, written contract signed by the two parties.*]
> **Control**: [*...a verbal agreement without a written contract.*]
>
> Which of the following strategies are you likely to use?"

Enumerators then asked respondents their likelihood of using the following dispute resolution strategies on a scale of 1 to 5:

> "How likely are you to..."
>
> - *Amicably:* "Resolve it amicably by discussing directly with the other company?"
> - *Social network:* "Contact someone in the other company's social network and ask them to intervene on your behalf?"
> - *Religious network:* "Take the problem to a religious authority (for example, a *marabout*)?"
> - *Political connection:* "Speak with someone—a connection—in government and ask them to intervene on your behalf?"
> - *Police:* "Take the problem to the local police?"
> - *Lawyer:* "Hire a lawyer to manage the case?"
> - *Courts:* "Use the court system?"

Responses to these questions constitute the main outcomes of interest. Respondents were not restricted in choosing one strategy over another—they could, and often did, say they were likely to use multiple strategies. Simple randomization was done within the tablet software to assign each respondent to the treatment or control group. Respondents were unaware they were participating in a survey experiment, and did not know that there were two versions of the question.[26]

4.3 Estimation

I estimate the following model to assess the impact of the formal contract treatment on firms' dispute resolution strategies:

$$y_i = \alpha + \beta_1 FormalContract_i + \gamma \mathbf{X_i} + \epsilon_i$$

where y_i is a firm manager's likelihood on a scale of 1 to 5 of using a given enforcement strategy,[27] $FormalContract_i$ is the treatment indicator, and $\mathbf{X_i}$ is a set of pre-registered covariates: age, gender, education, religion, ethnicity, and neighborhood.[28] In models estimating heterogeneous treatment effects by firm formality and by political connections, I interact the covariate of interest with the treatment indicator.

4.4 Diagnostics and threats to inference

To estimate balance across the experimental groups, I regress pre-treatment covariates on treatment assignment (see Appendix A.8).[29] While there was some missingness in outcomes from refusals to answer or "I don't know" responses, this was only the case for seven responses out of 16,723 total questions (2,389 respondents answering seven questions each). I impute mean values for these missing observations, and all results are robust to excluding them. Finally, though spillover could be considered a risk if sample firm owners discussed the survey to each other, data collection occurred on the same day within each subdistrict to minimize this risk. The sampling step also

[26] As confirmed with a question at the end of the survey, there were almost no spillovers: in less than 0.5% of cases, respondents had heard of the survey prior to taking it. The sampling step ensured sufficient distance between firms, and subdistricts were generally completed within the same day.

[27] Given the five-point outcome, I also estimate ordered logit models and present them in Appendix Section A.5.

[28] I estimate results with additional firm-level controls—registration status, number of employees, monthly revenue, firm valuation, firm age, business association membership, access to credit, and whether the firm declares revenue to tax authorities—in Appendix Section A.3.

[29] Of the 13 covariates tested, only one—respondent age—is significant at $p < 0.05$. An omnibus test regressing treatment assignment on all covariates jointly was also insignificant. Age is included in the pre-registered covariate set and is thus controlled for in all models.

	Enforcement strategy	Mean	St. Dev.	Median	Min	Max
Social enforcement outcomes	Amicably	4.412	0.743	5	1	5
	Social network	3.048	1.136	3	1	5
	Religious network	1.920	1.067	2	1	5
	Political connection	1.516	0.774	1	1	5
Formal enforcement outcomes	Police	3.266	1.050	3	1	5
	Lawyer	2.494	1.117	2	1	5
	Courts	2.672	1.270	3	1	5

Notes: This table presents a summary of the enforcement outcomes for the full sample of 2,389 firms. It includes each outcome's mean, standard deviation, median, minimum value, and maximum value.

Table 2: Summary table of main outcomes

ensured that immediately neighboring businesses would not be part of the sample. Data confirmed that almost no respondents had heard of the survey prior to taking it, and, as mentioned earlier, also confirmed that respondents believed the survey to be unaffiliated with the government.

5 Results

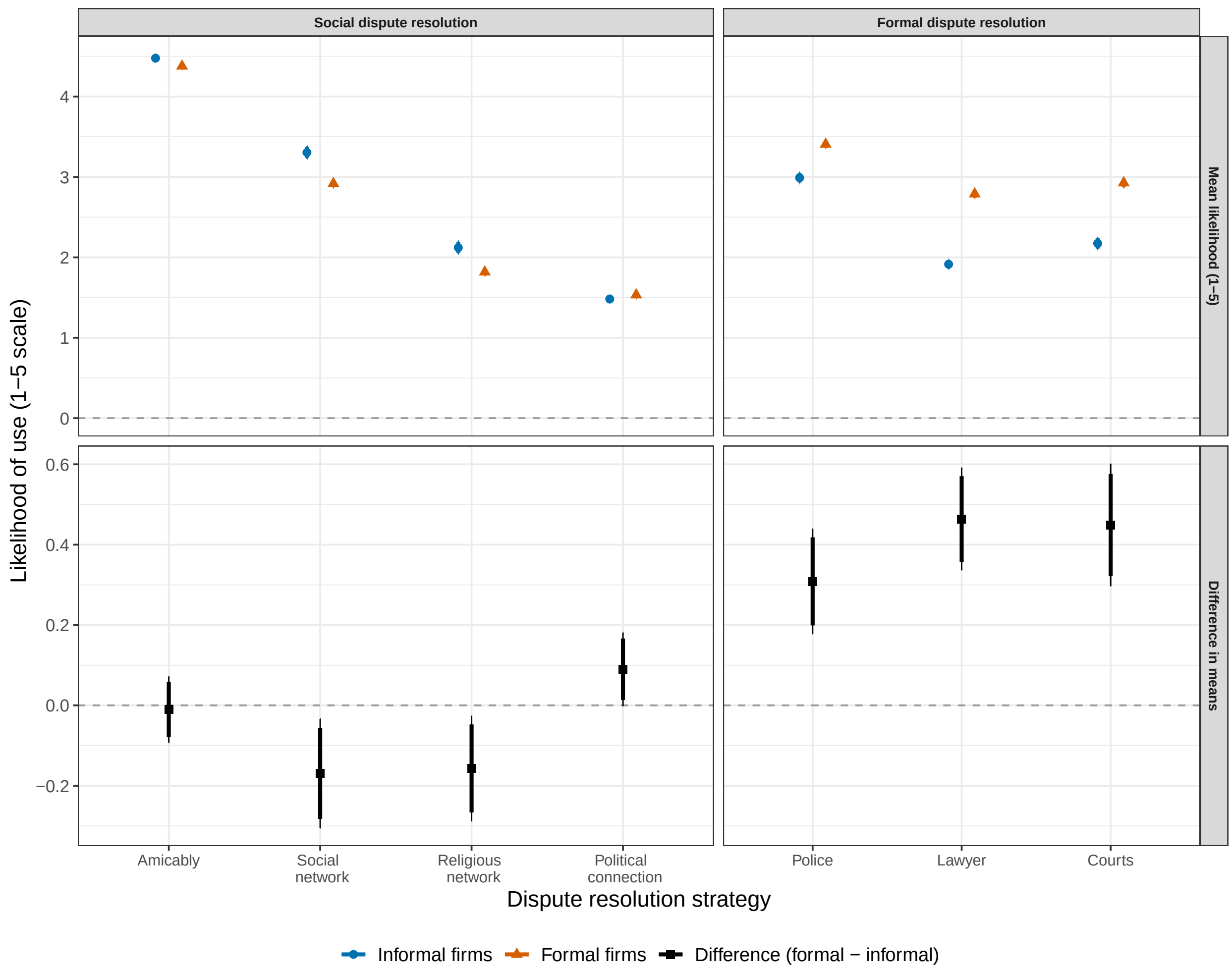
In this section, I begin by presenting descriptive findings that to date have been hard to come by in a context like Senegal, providing evidence of firms' enforcement behavior under selectively enforced rule of law. I then present the experimental results, including average treatment effects and heterogeneous treatment effects by firm formality and political connections. As a reminder, the latter set of heterogeneous analyses are presented as exploratory.[30]

5.1 Descriptive findings

I first examine descriptive trends in the outcome data to get a sense of the relative frequency of the various enforcement tactics firms use in Senegal. Table 2 presents summary statistics for firms' likelihood of pursuing the listed enforcement strategy. It demonstrates that by far the most common strategy is amicable resolution. Most firms will, by default, attempt to reach out to the defecting party before resorting to more costly options. Of the informal dispute resolution strategies, the use of social networks is the next most common, followed by religious networks, and finally political

[30]Note that formality and connections, while certainly correlated, are not measuring the same thing in Senegal. Connections are present on both sides of the registration divide; in my sample, 36.7% of formalized firm owners reported a familial political connection, while 27.4% of unregistered firm owners did.

connections. Although political connections are the least used informal enforcement mechanism, this is likely due to the limited number of firms who possess credible political connections relative to those with religious and other important social connections: 32.7% of the subsample reported familial political connections. In terms of formal enforcement, engaging the services of the police is the most popular option, followed by courts and then lawyers. This is reflective of the increasingly high transaction costs involved with hiring lawyers or bringing matters to court relative to filing a report with the police.



Notes: The top panel plots raw mean likelihood, on a scale from 1 to 5, of using the dispute resolution strategy on the horizontal axis, separately for registered (formal) and unregistered (informal) firms. The bottom panel plots the difference between those means (formal − informal). Thin lines are 95% confidence intervals and thick lines are 90% confidence intervals. Note that "formal" and "informal" here refer to firms' registration status, not to contract type, which is not varied in this figure. For the results in table form and the full list of controls, see Appendix Table A1.

Figure 2: Mean likelihood of dispute resolution strategy, by firm registration status

To examine how these strategies differ by firm formality, I calculate the outcomes for each subgroup and estimate the differences between them. Figure 2 presents the results and reflects the significant differences in strategies adopted by formal versus informal firms. Formal firms are significantly less likely than informal firms to resolve disputes via social and religious networks, and significantly more likely to use the police, lawyers, and courts. The only enforcement strategies where formal and informal firms do not diverge are in amicable resolution and the use of political connections. This latter result underscores the nature of political power and connections in Senegal, where firm assets and revenue are not significant predictors of political connectivity.[31]

5.2 Pre-registered experimental results

I next turn to the experimental results to assess the impact of formal contracts on firms' dispute resolution strategies. I start by presenting the average treatment effects for the full sample in Table 3. In line with the theorized expectations, firms are much more likely to seek formal enforcement when a deal is based on a formal contract as opposed to an informal one. The results also show that formal contracts substantially reduce dependence on social means of enforcement, suggesting that formal and informal dispute resolution tactics may indeed be substitutes in these types of disputes. However, the magnitudes of the coefficients for informal enforcement are lower than for formal enforcement—and the control group outcome means are high—suggesting that social dispute resolution strategies may still be viable and valuable when a formal contract is the basis of a broken deal. The only enforcement strategy that remains unchanged by a formal contract is the use of political connections to seek recourse. Using political connections to enforce deals remains an option whether deals are based on formal or informal contracts. This finding reinforces that informal means of dispute resolution do not disappear when formal institutions are introduced in developing economies.

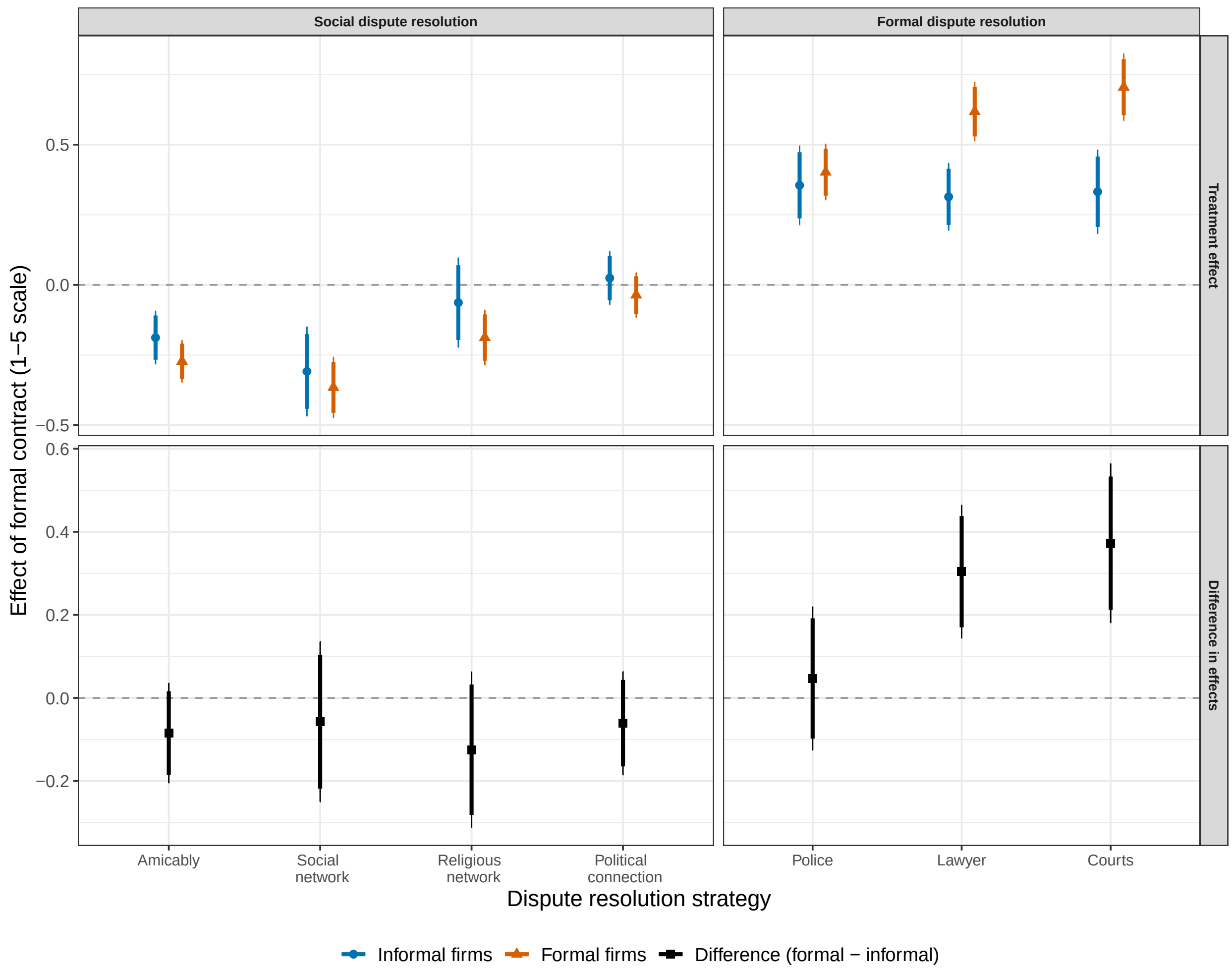
[31] See Appendix Figure A2 for correlational support of this claim. Note the distinction between two distinct quantities: firms in the two sectors are not detectably different in their use political connections as a resolution strategy (the result reported here), while registered firms are somewhat more likely to have a familial political connection in the first place.

	Outcome: Likelihood of using enforcement strategy						
	Social enforcement outcomes				*Formal enforcement outcomes*		
	Amicably	Social network	Religious network	Political connection	Police	Lawyer	Courts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Deal based on formal contract	-0.244^{***}	-0.347^{***}	-0.147^{***}	-0.015	0.389^{***}	0.520^{***}	0.583^{***}
	(0.030)	(0.046)	(0.044)	(0.032)	(0.042)	(0.043)	(0.050)
Control outcome mean	4.532	3.228	2.003	1.529	3.072	2.231	2.389
Control outcome std. dev.	0.687	1.128	1.134	0.783	1.039	1.019	1.173
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,361	2,361	2,361	2,361	2,361	2,361	2,361

Notes: Survey questions asked how likely respondents were to use each of the reported enforcement options, on a scale of 1 (never) to 5 (extremely likely). The question wording was: "Imagine that another company owes your firm a significant amount of money for services or products you provided, and the other company has missed the deadline to pay that was agreed upon in a [*control*: verbal agreement with no written contract][*treatment*: written, legal contract that you both signed.]" The pre-registered controls are age, gender, education, religion, ethnicity, and neighborhood. Appendix Section A.3 reports estimates without covariate adjustment and with an expanded set of firm-level covariates. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table 3: Average treatment effects: Main results of survey experiment

Given the differences between formal and informal firms, as well as the inherent differences in their available legal strategies at baseline, I next estimate treatment effects based on firm formality status. Figure 3 presents the treatment effects for formal and informal firms, as well as the differences between the two subgroups for each resolution strategy. The contract treatment only differentially affected firm types for the costly state enforcement outcomes: the use of lawyers and courts. That is, formal firms increased their use of the most cost-intensive formal enforcement methods when a formal contract was the basis of a deal. The estimates in Appendix Table A5 further show that firms with the greatest assets and largest workforces show substantially stronger responses to formal contracts, consistent with the view that formal legal institutions are most accessible to resource-rich firms. A one-category increase in firm valuation raises the treatment effect on lawyer use by 0.13 points, and on court use by 0.10 points while reducing the treatment effect on amicable resolution, social networks, and religious networks. Better-resourced firms thus substitute toward formal resolution more completely than others do.



Notes: The treatment is the random assignment of a written, legal contract rather than a verbal agreement as the basis of the disputed deal. The top panel plots the effect of that treatment on the likelihood of using each dispute resolution strategy, estimated separately for registered (formal) and unregistered (informal) firms. The pre-registered controls are age, gender, education, religion, ethnicity, and neighborhood. The bottom panel plots the difference between those two treatment effects (formal − informal). Thin lines are 95% confidence intervals and thick lines are 90% confidence intervals. As in Figure 2, "formal" and "informal" refer to firms' registration status, not to contract type. For the results in table form, see Appendix Table A2.

Figure 3: Effect of a formal contract on dispute resolution strategy, by firm registration status

These patterns are indicative of the distributional implications of contract formalization policies: they suggest that the biggest gains from increasing access to formal contracts go to firms that already have preferential access to legal institutions in the first place, thus potentially compounding existing inequalities. Still, as the decomposed results in the top panel of Figure 3 demonstrate, formal contracts significantly increase the willingness of informal firms to seek legal recourse, and somewhat reduce their dependence on informal property rights security strategies as well. While formal contracts may widen the enforcement gap between formal and informal firms—which is the distributional concern at play—the movement is in the direction reform is meant to produce for both groups.

5.3 Political connections: exploratory results

The above results might obscure the more insidious consequences of expanding access to formal contracts, however. While contracts can encourage formal enforcement and decrease social enforcement in dispute resolution, in this subsection I show how political connections might moderate the results in important ways. These results, while illuminating, are exploratory and differ from the analyses above in three respects: the moderator was not pre-registered; it was measured in the 2022 follow-up wave; and it is available only for the randomized subset of 535 firm owners who participated in that wave, rather than the full sample of 2,389. As such, these results warrant additional testing.

I present the results in Table 4. While politically connected firms are no more likely than unconnected firms to modify their formal dispute resolution strategies under formal contracts, they intensify their usage of *social* enforcement. Specifically, connected firms are more likely to seek amicable dispute resolution and enforcement by activating their political connections. The social-network and religious-network interactions point in the same direction but do not reach significance. Meanwhile, unconnected firm owners are pushed toward seeking formal dispute resolution strategies under formal contracts (the coefficients for "formal contract" in Models 1–7). Upon closer inspection, the pattern of connected firms' displacement toward informal resolution appears only within the subsample of registered firms, not those in the informal economy (see Appendix Section A.9). This helps to underscore a condition for displacement of political influence under formal contracts: a connected firm must have had a realistic prospect of exercising influence within formal institutions for a formal contract to compress that advantage and push it elsewhere. Unregistered firms have worse access to begin with, so a written contract changes little about where their connections are worth deploying.

These results are consistent with the paradoxical impact of introducing formal contracts: they can displace the activity of the most powerful firms toward informal enforcement methods. Of the two paths detailed in the theory above, it appears that legal contracts indeed seem to be

	Outcome: Likelihood of using enforcement strategy						
	Social enforcement outcomes				*Formal enforcement outcomes*		
	Amicably (1)	Social network (2)	Religious network (3)	Political connection (4)	Police (5)	Lawyer (6)	Courts (7)
Formal contract	-0.395^{***}	-0.336^{***}	-0.183	-0.070	0.303^{***}	0.559^{***}	0.605^{***}
	(0.086)	(0.126)	(0.119)	(0.081)	(0.115)	(0.115)	(0.131)
Political connections	-0.100	0.199	-0.118	-0.271^{***}	0.008	0.051	0.250
	(0.106)	(0.168)	(0.143)	(0.094)	(0.149)	(0.148)	(0.169)
Formal contract	0.323^{**}	0.225	0.246	0.340^{**}	-0.067	-0.135	-0.194
× political connections	(0.150)	(0.220)	(0.194)	(0.140)	(0.204)	(0.199)	(0.231)
Control outcome mean	4.589	3.189	2.011	1.53	3.065	2.216	2.389
Control outcome std. dev.	0.637	1.148	1.142	0.808	1.121	0.993	1.242
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	531	531	531	531	531	531	531

Notes: Survey questions asked how likely respondents were to use each of the reported enforcement options, on a scale of 1 (never) to 5 (extremely likely). The question wording was: "Imagine that another company owes your firm a significant amount of money for services or products you provided, and the other company has missed the deadline to pay that was agreed upon in a [*control*: verbal agreement with no written contract][*treatment*: written, legal contract that you both signed.]" Political connections is an indicator variable for whether respondent has a familial political connection. The pre-registered controls are age, gender, education, religion, ethnicity, and neighborhood. Appendix Section A.3 reports estimates without covariate adjustment and with an expanded set of firm-level covariates. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table 4: Treatment effects by familial political connections

constraining connected firms rather than spurring them to double down on formal enforcement. These results suggest that connected firms will not curry as much favor before formal institutions when formal contracts are the basis of a deal, but that they will still attempt to activate their political networks to resolve the dispute informally.

To address other competing explanations for dispute resolution in Senegal, in Appendix A.2 I show how membership in political parties, firm owners' trust in courts, and past exposure to corruption impact results. Of particular interest are the corruption findings: for firm owners with firsthand experience of state corruption, formal contracts produce a markedly stronger shift toward formal dispute resolution—and away from social enforcement. This may go against intuition, as one would think firms victimized by state officials might avoid state institutions. That they instead increase their dependence for formal resolution when armed with a written contract suggests that firms distinguish between the state as predator and the state as adjudicator, and that formal enforcement is valued as a check on precisely the sort of discretionary abuse these firms have experienced. A written contract appears to be what makes that check usable. This chimes with the connections results above. If formal contracts constrain the exercise of discretion, then those who suffer from discretion (the corruption-exposed) gain from formalization, while those who benefit from it (the connected) lose something and look elsewhere.

6 Conclusion

Efforts to formalize the private sectors of developing countries may have unintended consequences. Formalization initiatives do not occur in isolation; they must contend with competing informal political and economic influences. I show in this paper that one such formalization effort—expanding the use of formal contracts—can affect firms' dispute resolution strategies in unexpected ways. While formal contracts increase reliance on state institutions for enforcement and reduce informal dispute resolution strategies, this shift is primarily driven by registered firms and firms with greater assets and revenue that arguably need these benefits least. Moreover, rather than preventing forum shopping, expanding access to formal contracts may actually *encourage* it for the most powerful firms: in exploratory analyses, I show that politically connected firms intensify their use of informal enforcement under formal contracts. This has important distributional consequences that stand in contrast with policy agendas that tout the unchecked benefits of increasing access to formal law. Nevertheless, the patterns I uncover in this paper also coexist with movement toward formal resolution: summed across the whole sample, the net effect of formal contracts on formal dispute resolution is clearly positive. Overall, this study raises important questions about who captures the gains from formalization and where residual influence travels when one path is cut off.

What might this imply over a longer time horizon? One reading is more optimistic. The experiment in this paper captures firms' responses at a moment of transition, when formal contracts are being introduced into an environment whose norms were built around their absence. There are thus reasons to believe the patterns documented here are a feature of that transition rather than a stable equilibrium. For connected firms to reallocate their dispute resolution to informal channels, these informal forums must remain effective venues for exercising political influence. However, informal institutions' effectiveness cannot be assumed to be permanent. As more transactions come to be documented, as commercial courts process more cases and accumulate precedent, and as the share of disputes with a written record grows, the returns to informal pressure may erode. On this reading, the formal-to-informal displacement of enforcement is a transitional cost of formalization, and may be potentially self-limiting in the long run.

The less optimistic reading is that this displacement into informal institutions could instead be self-reinforcing. If connected firms learn that formal contracts erode their advantage in court, the rational response is not only to shift forums after a dispute arises but to avoid written contracts in the first place—which is precisely what my interview evidence from Senegal suggests some firms already do. Formalization would then produce sorting by firm influence, entrenching two parallel systems rather than converging on one. Which trajectory comes to pass may ultimately depend on whether firms can avoid contract formalization or the use of informal recourse options. I argue that this should thus be a growing area of policy focus. On the researcher end, panel data on firms' contracting choices as formalization policy is rolled out would be particularly illuminating for these policy debates. By construction, the partial-equilibrium research design used here cannot distinguish these trajectories because it holds contract choice fixed.

Beyond this equilibrium question, there are also lingering questions about mechanisms—especially around the impact of political connections—that should be submitted to future testing. First, connections may be endogenous to enforcement history, with firms that anticipate difficulty in court investing in political ties beforehand. Second, connected firms may simply face lower costs of informal resolution, in the sense of the objective function in Section 2, so that a formal contract shifts the relative price of forums without any loss of formal advantage being involved. Third, the connections results in this paper may reflect what connected respondents believe about how formal contracts constrain courts, which need not correspond to how courts actually behave. Distinguishing among these requires design features that this one lacks: measurement of connections over time, detailed information about the relative costs of enforcement methods, and data on the outcomes disputes actually produce rather than the forums firms say they would enter.

This paper offers two insights for generalizability to other contexts. The first is a measurement point: only by measuring firm registration, contract formality, and dispute resolution independently did the relationships in this paper become visible. Treating formality as a single firm attribute—as research and policy both routinely do—makes it difficult to observe that registered firms transact informally, that unregistered firms litigate, and that a change on one dimension can move firms in unexpected directions on another. Studies of firm behavior in transitional economies would benefit from disaggregating formality in this way as a matter of course, and the payoff is likely to be largest where the mixing of formal and informal practices is greatest. Second, this paper expands upon existing literature on formal versus informal property security strategies to advance this research agenda in new contexts. Many studies focus on post-Soviet states (e.g. Pistor 1996; Frye 2004; Gans-Morse 2017) or states with violence-based informal enforcement (e.g. De Soto 2000; Wilson 2010). By contrast, the results from this paper can help to understand informal enforcement in the many societies where violence or coercion are not common strategies in the private sector, and where social and religious networks are dense and important determinants of risk in exchange. These results also contrast with studies that juxtapose nonstate enforcement against very weak state enforcement institutions that citizens know to be ineffective (e.g. McMillan and Woodruff 1999*a*). Many states are similar to Senegal, where there are moderate levels of public support and trust in state institutions, but where informalities are pervasive in the private sector. The results from this paper contribute to our understanding of formal and informal institutions in such hybrid environments.

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A Appendix

Contents

A.1 Tables corresponding to figures in main body

	Outcome: Likelihood of using enforcement strategy						
	Social enforcement outcomes				*Formal enforcement outcomes*		
	Amicably (1)	Social network (2)	Religious network (3)	Political connection (4)	Police (5)	Lawyer (6)	Courts (7)
Formal firms	−0.010	−0.169**	−0.157**	0.090*	0.309***	0.464***	0.449***
	(0.042)	(0.069)	(0.067)	(0.047)	(0.067)	(0.065)	(0.078)
Informal firm mean	4.477	3.305	2.121	1.482	2.99	1.914	2.173
Informal firm std. dev.	0.688	1.152	1.159	0.693	1.021	0.856	1.11
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,336	2,336	2,336	2,336	2,336	2,336	2,336

Notes: Survey questions asked how likely respondents were to use each of the reported enforcement options, on a scale of 1 (never) to 5 (extremely likely). Registration status is not randomly assigned, so this comparison is observational; the reported coefficient is adjusted for the pre-registered covariates (age, gender, education, religion, ethnicity, neighborhood) plus an expanded set of firm-level covariates (number of employees, monthly revenue, firm valuation, firm age, business association membership, access to credit, and whether the firm declares revenue to tax authorities). * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table A1: Difference in enforcement likelihood between formal and informal firms

	Outcome: Likelihood of using enforcement strategy						
	Social enforcement outcomes				*Formal enforcement outcomes*		
	Amicably	Social network	Religious network	Political connection	Police	Lawyer	Courts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Formal contract used	−0.188***	−0.309***	−0.063	0.024	0.355***	0.314***	0.332***
	(0.048)	(0.081)	(0.081)	(0.048)	(0.072)	(0.061)	(0.077)
Formal firm	−0.018	−0.118	−0.154**	0.145***	0.445***	0.641***	0.564***
	(0.044)	(0.075)	(0.076)	(0.048)	(0.067)	(0.059)	(0.075)
Formal contract used × formal firm	−0.085	−0.057	−0.125	−0.061	0.047	0.304***	0.373***
	(0.061)	(0.098)	(0.096)	(0.063)	(0.088)	(0.082)	(0.098)
Control outcome mean	4.57	3.444	2.153	1.468	2.818	1.764	2.019
Control outcome std. dev.	0.627	1.148	1.229	0.677	0.989	0.736	1.03
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,361	2,361	2,361	2,361	2,361	2,361	2,361

Notes: Survey questions asked how likely respondents were to use each of the reported enforcement options, on a scale of 1 (never) to 5 (extremely likely). The question wording was: "Imagine that another company owes your firm a significant amount of money for services or products you provided, and the other company has missed the deadline to pay that was agreed upon in a [*control*: verbal agreement with no written contract][*treatment*: written, legal contract that you both signed.]" The pre-registered controls are age, gender, education, religion, ethnicity, and neighborhood. Appendix Section A.3 reports this model without covariate adjustment and with an expanded set of firm-level covariates (Panel B of Tables A6 and A7). * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table A2: Heterogeneous effects by firm formality

A.2 Additional heterogeneous treatment effects

To test other theoretically compelling factors for why firms may diverge in their enforcement of formal contracts, I estimate additional heterogeneous treatment effects by the following: party membership, trust in courts, past exposure to corruption, firm wealth, and firm size.

First, to extrapolate to a broader conceptualization of political networks than presented in the main text, I estimate heterogeneous effects based on firm managers' membership in a political party.[32] As Panel A of Table A3 shows, while party members use most types of enforcement mechanisms more than politically unaffiliated firms, they do not seem to respond to formal contracts differently than non-party members. The exception is using the police: party members are less likely than unaffiliated firms to go to the police when a formal contract is broken.

[32]Because political connections in clientelistic societies may matter more when one's connections are to the ruling party, I also estimate heterogeneous treatment effects by membership in the party of power and present the results in Appendix Table A4.

	Outcome: Likelihood of using enforcement strategy						
	Social enforcement outcomes				*Formal enforcement outcomes*		
	Amicably	Social network	Religious network	Political connection	Police	Lawyer	Courts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: By political party membership							
Formal contract	−0.241***	−0.362***	−0.179***	−0.031	0.440***	0.566***	0.597***
	(0.034)	(0.051)	(0.048)	(0.034)	(0.047)	(0.050)	(0.057)
Party member	−0.022	0.250***	0.312***	0.473***	0.135*	0.272***	0.263***
	(0.052)	(0.079)	(0.075)	(0.053)	(0.073)	(0.077)	(0.088)
Formal contract × party member	−0.015	−0.008	0.066	0.040	−0.233**	−0.149	−0.105
	(0.075)	(0.114)	(0.108)	(0.076)	(0.105)	(0.110)	(0.126)
Control outcome mean	4.57	3.444	2.153	1.468	2.818	1.764	2.019
Control outcome std. dev.	0.627	1.148	1.229	0.677	0.989	0.736	1.03
Panel B: By trust in courts							
Formal contract	−0.125**	−0.312***	−0.322***	−0.061	0.423***	0.486***	0.492***
	(0.054)	(0.082)	(0.078)	(0.057)	(0.076)	(0.079)	(0.089)
Trust in courts	0.084***	0.090***	−0.084***	−0.011	0.032	0.108***	0.176***
	(0.021)	(0.031)	(0.030)	(0.022)	(0.029)	(0.030)	(0.034)
Formal contract × Trust in courts	−0.078***	−0.037	0.100**	0.022	−0.020	0.029	0.049
	(0.029)	(0.044)	(0.042)	(0.031)	(0.041)	(0.043)	(0.048)
Control outcome mean	4.528	2.996	2.245	1.421	3.087	2.056	2.124
Control outcome std. dev.	0.771	1.198	1.237	0.733	1.067	0.992	1.09
Panel C: By exposure to corruption							
Formal contract	−0.162***	−0.303***	−0.165***	−0.042	0.290***	0.433***	0.482***
	(0.034)	(0.052)	(0.050)	(0.036)	(0.048)	(0.050)	(0.057)
Exposure to corruption	0.009	−0.139*	−0.151**	−0.210***	0.020	−0.090	0.075
	(0.050)	(0.077)	(0.073)	(0.053)	(0.070)	(0.074)	(0.085)
Formal contract × Exposure to corruption	−0.348***	−0.260**	−0.009	0.075	0.435***	0.431***	0.388***
	(0.071)	(0.108)	(0.103)	(0.075)	(0.099)	(0.105)	(0.119)
Control outcome mean	4.53	3.26	2.037	1.576	3.068	2.251	2.372
Control outcome std. dev.	0.697	1.076	1.086	0.773	1.057	1.013	1.216
Controls	No	No	No	No	No	No	No
Observations	2,389	2,389	2,389	2,389	2,389	2,389	2,389

Notes: Survey questions asked how likely respondents were to use each of the reported enforcement options, on a scale of 1 (never) to 5 (extremely likely). The question wording was: "Imagine that another company owes your firm a significant amount of money for services or products you provided, and the other company has missed the deadline to pay that was agreed upon in a [*control*: verbal agreement with no written contract][*treatment*: written, legal contract that you both signed.]" * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table A3: Heterogeneous treatment effects: party membership, trust in courts, exposure to corruption

Panel B of Table A3 examines effects by respondents' trust in courts. The results reflect that respondents with higher trust in courts were at baseline (i.e., under informal contracts) more likely to prefer lawyers and courts as formal enforcement solutions, and amicable solutions and mutual social networks as informal solutions. They were less likely to use religious networks, perhaps reflecting their distrust of competing legal authorities. The heterogeneous treatment effects show, however, that their trust in courts did not translate into higher use of lawyers or courts when formal contracts were the basis of exchange. High-trust respondents were less likely to use amicable solutions and more likely to seek out religious networks when a formal contract was the basis of exchange.

Finally, in Panel C I examine results by past exposure to corruption, which here is a dichotomous measure of whether firm managers have been forced to pay illegal taxes to government officials. I find that firm owners who have experienced illegal tax extraction by the state are *more* likely to use state institutions for enforcement than firms who have not experienced such corruption.[33] This may be due to respondents' exposure to the power of the state: those who have experienced illegal tax extraction might believe in the enforcing and punishing power of the state, and may thus be more likely to appeal to state institutions when they have a legitimate claim to use these institutions (e.g. when a deal is based on a formal contract). It may also simply be the case that larger firms—the types most likely to use formal dispute institutions—are also more subject to illegal taxation due to their size and visibility.

	Outcome: Likelihood of using enforcement strategy						
	Social enforcement outcomes				*Formal enforcement outcomes*		
	Amicably	Social network	Religious network	Political connection	Police	Lawyer	Courts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Formal contract	−0.240***	−0.359***	−0.170***	−0.020	0.401***	0.535***	0.580***
	(0.032)	(0.048)	(0.046)	(0.033)	(0.044)	(0.047)	(0.053)
Member of party in power	−0.057	0.202*	0.251**	0.570***	0.148	0.305***	0.445***
	(0.069)	(0.105)	(0.100)	(0.071)	(0.097)	(0.101)	(0.115)
Formal contract × Member of party in power	−0.051	−0.050	0.041	−0.011	−0.065	0.015	−0.017
	(0.100)	(0.153)	(0.145)	(0.104)	(0.141)	(0.148)	(0.168)
Control outcome mean	4.538	3.207	1.977	1.469	3.057	2.199	2.343
Control outcome std. dev.	0.686	1.14	1.124	0.729	1.046	1.021	1.156
Controls	No	No	No	No	No	No	No
Observations	2,389	2,389	2,389	2,389	2,389	2,389	2,389

Notes: Survey questions asked how likely respondents were to use each of the reported enforcement options, on a scale of 1 (never) to 5 (extremely likely). The question wording was: "Imagine that another company owes your firm a significant amount of money for services or products you provided, and the other company has missed the deadline to pay that was agreed upon in a [*control*: verbal agreement with no written contract][*treatment*: written, legal contract that you both signed.]" * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table A4: Heterogeneous effects by firm owners' membership in ruling party

[33] Le, Malesky and Pham (2020) find a similar result: businesses exposed to local corruption in Vietnam had a higher likelihood of tax registration.

The following table presents heterogeneous treatment effects by firm assets and size. These results underscore the results presented above: similar to the benefits received by formal firms, better-resourced firms seem to be reaping the most benefits of access to formal contracts. Firms with greater assets and revenue are significantly more likely to use costly formal dispute resolution strategies during contract disputes.

	Outcome: Likelihood of using enforcement strategy						
	Social enforcement outcomes				*Formal enforcement outcomes*		
	Amicably	Social network	Religious network	Political connection	Police	Lawyer	Courts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: By firm valuation							
Formal contract	0.221	0.099	0.196	−0.118	0.355**	−0.112	0.102
	(0.138)	(0.195)	(0.194)	(0.137)	(0.171)	(0.169)	(0.187)
Firm valuation	0.256***	0.247***	0.003	−0.042	0.095**	−0.037	−0.009
	(0.029)	(0.043)	(0.041)	(0.030)	(0.037)	(0.034)	(0.042)
Formal contract × firm valuation	−0.103***	−0.099**	−0.075*	0.021	0.007	0.133***	0.102**
	(0.029)	(0.040)	(0.040)	(0.030)	(0.036)	(0.036)	(0.040)
Constant	4.368***	2.625***	0.870**	0.674**	2.068***	2.147***	1.179***
	(0.266)	(0.464)	(0.424)	(0.302)	(0.485)	(0.386)	(0.456)
Panel B: By firm size							
Formal contract	−0.003	−0.079	0.088	0.118	0.349***	0.303***	0.443***
	(0.080)	(0.124)	(0.119)	(0.091)	(0.108)	(0.103)	(0.118)
Firm size	−0.036	0.122***	0.155***	0.197***	−0.030	0.048	0.112***
	(0.026)	(0.044)	(0.043)	(0.032)	(0.038)	(0.037)	(0.042)
Formal contract × firm size	−0.111***	−0.123**	−0.104**	−0.061	0.017	0.090**	0.059
	(0.034)	(0.050)	(0.048)	(0.041)	(0.041)	(0.041)	(0.047)
Constant	4.313***	2.525***	0.760*	0.440	2.099***	2.059***	1.082**
	(0.266)	(0.469)	(0.435)	(0.307)	(0.490)	(0.397)	(0.465)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,336	2,336	2,336	2,336	2,336	2,336	2,336

Notes: Survey questions asked how likely respondents were to use each of the reported enforcement options, on a scale of 1 (never) to 5 (extremely likely). The question wording was: "Imagine that another company owes your firm a significant amount of money for services or products you provided, and the other company has missed the deadline to pay that was agreed upon in a [*control*: verbal agreement with no written contract][*treatment*: written, legal contract that you both signed.]" Control variables include the pre-registered covariates (age, gender, education, religion, ethnicity, neighborhood), and the remaining expanded firm-level covariates (excluding the moderator itself). * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table A5: Heterogeneous treatment effects by firm assets and firm size

A.3 Robustness of the main results to alternative covariates

The main text reports estimates adjusted for the covariates specified in the pre-analysis plan: age, gender, education, religion, ethnicity, and neighborhood. This section reports the main results—average treatment effects, heterogeneous effects by firm formality, and heterogeneous effects by political connections—under two alternative specifications, so readers can see that no substantive conclusion depends on covariate choices. Table A6 reports estimates with no covariate adjustment at all. Table A7 adds an expanded set of firm-level covariates requested by reviewers beyond what the pre-analysis plan specified. This includes firm registration status, number of employees, monthly revenue, firm valuation, firm age, business association membership, access to credit, and whether the firm declares revenue to tax authorities. Across both alternatives, every estimate that is significant in the main text remains significant here, with one partial exception noted in the results section: the interaction between formal contracts and political connections on amicable resolution weakens from $p < 0.05$ under the pre-registered specification to $p < 0.10$ once the expanded firm-level covariates are added. All other conclusions, including the average treatment effects and the heterogeneous effects by firm registration status, assets, and size, are unchanged in sign, magnitude, and statistical significance.

A.3.1 No covariate adjustment

Table A6 below reproduces the main experimental results—Table 3 and Table 4—with no covariates.

	Outcome: Likelihood of using enforcement strategy						
	Social enforcement outcomes				*Formal enforcement outcomes*		
	Amicably	Social network	Religious network	Political connection	Police	Lawyer	Courts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: ATEs							
Formal contract	-0.244^{***}	-0.366^{***}	-0.169^{***}	-0.027	0.393^{***}	0.534^{***}	0.574^{***}
	(0.030)	(0.046)	(0.044)	(0.032)	(0.042)	(0.044)	(0.051)
Controls	No	No	No	No	No	No	No
Observations	2,389	2,389	2,389	2,389	2,389	2,389	2,389
Panel B: CATEs by firm formality							
Formal contract	-0.195^{***}	-0.293^{***}	-0.068	0.029	0.362^{***}	0.317^{***}	0.325^{***}
	(0.052)	(0.078)	(0.074)	(0.055)	(0.071)	(0.071)	(0.083)
Formal firm	-0.058	-0.332^{***}	-0.231^{***}	0.093^{**}	0.390^{***}	0.717^{***}	0.568^{***}
	(0.044)	(0.067)	(0.064)	(0.047)	(0.061)	(0.060)	(0.071)
Formal contract $\times$ formal firm	-0.071	-0.097	-0.143	-0.086	0.034	0.298^{***}	0.352^{***}
	(0.063)	(0.096)	(0.091)	(0.067)	(0.088)	(0.087)	(0.103)
Controls	No	No	No	No	No	No	No
Observations	2,389	2,389	2,389	2,389	2,389	2,389	2,389
Panel C: CATEs by political connections							
Formal contract	-0.361^{***}	-0.315^{**}	-0.165	-0.061	0.255^{**}	0.441^{***}	0.491^{***}
	(0.083)	(0.124)	(0.112)	(0.078)	(0.115)	(0.115)	(0.134)
Political connection	-0.138	0.140	-0.194	-0.231^{**}	-0.016	0.052	0.233
	(0.105)	(0.156)	(0.141)	(0.098)	(0.144)	(0.145)	(0.169)
Formal contract $\times$ political connection	0.307^{**}	0.147	0.176	0.289^{**}	-0.046	0.011	-0.048
	(0.146)	(0.217)	(0.196)	(0.136)	(0.201)	(0.202)	(0.235)
Controls	No	No	No	No	No	No	No
Observations	535	535	535	535	535	535	535

Notes: Survey questions asked how likely respondents were to use each of the reported enforcement options, on a scale of 1 (never) to 5 (extremely likely). * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table A6: Experimental results with no covariate adjustment

A.3.2 Expanded covariates

Table A7 below reproduces the same three models with the pre-registered covariates plus the expanded set of firm-level covariates described in the main body and the table notes.

	Outcome: Likelihood of using enforcement strategy						
	Social enforcement outcomes				*Formal enforcement outcomes*		
	Amicably (1)	Social network (2)	Religious network (3)	Political connection (4)	Police (5)	Lawyer (6)	Courts (7)
Panel A: ATEs							
Formal contract	−0.257***	−0.362***	−0.151***	−0.023	0.388***	0.509***	0.577***
	(0.029)	(0.045)	(0.043)	(0.031)	(0.042)	(0.041)	(0.047)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,336	2,336	2,336	2,336	2,336	2,336	2,336
Panel B: CATEs by firm formality							
Formal contract	−0.192***	−0.305***	−0.059	0.025	0.355***	0.302***	0.332***
	(0.046)	(0.080)	(0.082)	(0.048)	(0.072)	(0.061)	(0.077)
Formal firm	0.037	−0.128	−0.098	0.121**	0.288***	0.321***	0.280***
	(0.049)	(0.083)	(0.084)	(0.055)	(0.077)	(0.070)	(0.086)
Formal contract × formal firm	−0.098*	−0.087	−0.140	−0.073	0.050	0.315***	0.373***
	(0.059)	(0.096)	(0.096)	(0.062)	(0.088)	(0.081)	(0.097)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,336	2,336	2,336	2,336	2,336	2,336	2,336
Panel C: CATEs by political connections							
Formal contract	−0.356***	−0.266**	−0.142	−0.069	0.267**	0.506***	0.557***
	(0.082)	(0.123)	(0.119)	(0.082)	(0.119)	(0.112)	(0.130)
Political connection	−0.095	0.249	−0.065	−0.255***	−0.067	−0.054	0.158
	(0.099)	(0.163)	(0.145)	(0.096)	(0.145)	(0.143)	(0.163)
Formal contract × political connection	0.253*	0.098	0.178	0.316**	0.033	−0.031	−0.104
	(0.141)	(0.216)	(0.198)	(0.141)	(0.205)	(0.190)	(0.222)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	525	525	525	525	525	525	525

Notes: Survey questions asked how likely respondents were to use each of the reported enforcement options, on a scale of 1 (never) to 5 (extremely likely). Control variables include the pre-registered covariates (age, gender, education, religion, ethnicity, neighborhood) plus registration status, number of employees, monthly revenue, firm valuation, firm age, business association membership, access to credit, and whether the firm declares revenue to tax authorities. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table A7: Experimental results with expanded covariates

A.4 Pre-analysis plan

The below text constitutes the pre-analysis plan for this study, which is itself part of a broader pre-analysis plan for another project. The study was registered prior to data collection.

> The firm survey also includes a brief survey experiment, intended to measure the difference in strategies that formal and informal firms use when faced with the prospect of a deal gone astray. Those in the treatment group are told that the deal was based on a formal, written contract, while those in the control group are told that the deal was based on a verbal agreement.
>
> The specific wording of the question, inspired by Gans-Morse (2017), is as follows: "Imagine that another business owes yours a significant sum of money as a result of

services or products you provided, and that this business hasn't paid by the previously agreed upon deadline stipulated in [Treatment: **a legal written contract signed by the two parties**][Control: **a verbal agreement without a written contract**]. Which of the following strategies are you likely to use, on a scale from 1 to 5, where 1 is never and 5 is extremely likely?"

The outcomes are the respondent's likelihoods—on a scale of 1 to 5, where the options are 1) never, 2) unlikely, 3) maybe, 4) likely, and 5) extremely likely—of using the following strategies to resolve the problem:

1. Resolve it amicably by discussing directly with the other company
2. Contact someone in the other company's social network and ask them to intervene on your behalf
3. Speak with someone—a connection—in government and ask them to intervene on your behalf
4. Take the problem to the local police
5. Take the problem to a religious authority
6. Hire a lawyer to resolve the case
7. Use the court system

I will estimate results using both difference-in-means and OLS with and without covariate adjustment, with preference given to the covariate-adjusted results. In specifications with covariate adjustment, the following covariates will be used:

1. Age
2. Gender
3. Education
4. Religion
5. Ethnicity
6. Neighborhood of business

I will estimate heterogeneous treatment effects by the status of the firm [formal versus informal], as well as by wealth and size of the firm. I expect the informal approaches to solving the issue to be more likely when the dispute is based on a verbal contract, and I expect this to be particularly pronounced for respondents representing informal firms. I expect formal firms to be more likely overall to use formal methods for resolving the problem, particularly when based on formal contracts. I expect wealthier and larger firms to be more likely to use the formal means of enforcement relative to less wealthy and smaller firms.

Deviations from pre-analysis plan

The pre-analysis plan stipulated that estimates would be produced both with and without covariate adjustment, "with preference given to the covariate-adjusted results," and specified the adjustment set: age, gender, education, religion, ethnicity, and neighborhood. The appendix of this version also includes estimates adjusted for this pre-registered set together with additional firm-level covariates at the request of reviewers. Substantive conclusions are unchanged across all three specifications; where an estimate is sensitive to the choice, I note it in the text.

Importantly, the pre-analysis plan did not include detailed political connections as a covariate to be collected and tested. The 2017-2018 instrument measured political connectivity only through party membership and prior state employment, both of which proved too coarse to be informative.

Based on early feedback, and my own reading of the initial results, I conducted the 2022 survey wave to collect more refined political connection data, administered to a randomized subset of 535 of the original 2,389 respondents. I thus choose not to make theoretical predictions about the direction that political connections might operate, instead highlighting that it could go either way and leaving it to empirics to reveal the direction.

Three features of this deviation should be weighed by readers. First, the moderator was not pre-specified and no directional hypothesis was registered, so the analyses using it are exploratory and should be considered so. Second, the moderator was measured nearly five years after the outcomes, which means its stability across the interval is assumed rather than demonstrated. While I provide the evidence I can in the footnote discussing this point within the paper, there is no ideal test for confirmation of this. Third, the analyses are conducted on 22% of the original sample, with corresponding losses in precision. I have tried to label these analyses as exploratory wherever they appear, and I would encourage readers to weight them accordingly relative to the pre-registered results.

A.5 Ordered logit estimates

The experimental outcomes are on a five-point ordinal scale, so, as a robustness check, Table A8 reports results from ordered logit models. For the average treatment effects, the ordered logit estimates reproduce the linear results in every respect: all six substantive outcomes retain their sign and significance at $p < 0.01$, and the null effect on the use of political connections remains null.

The heterogeneous treatment effects by firm formality likewise hold up (Panel B of Table A8). Because coefficients on interaction terms in nonlinear models do not correspond to interaction effects (Ai and Norton 2003), I compute average marginal effects of the treatment on the expected response separately by subgroup and take the difference, with standard errors bootstrapped over 400 replications. As in the linear estimates in Appendix Table A2, the average marginal effect of the treatment is significantly larger for formal firms than informal firms on the two formal-enforcement outcomes—use of a lawyer and use of the courts—while the differences on the remaining five outcomes are small and not statistically significant, matching the sign and relative magnitude of the corresponding linear interaction estimates.

For the exploratory interaction with political connections, the differences retain the sign of the linear estimates but are not statistically significant ($p = 0.16$ and $p = 0.21$ for amicable resolution and using political connections, respectively). I read this as a limitation of the exploratory political connections analysis. With only 535 observations in the subsample, an outcome concentrated at the bottom of the scale (62% of responses to the political-connection item are “never” and a further 32% are “unlikely”), and a moderator that applies to only a third of the sample, the interaction is not as precisely estimated as desired. This is consistent with the framing adopted throughout: the connections results should be viewed as suggestive patterns warranting confirmatory testing on larger samples.

	Social dispute resolution				*Formal dispute resolution*		
	Amicably	Social network	Religious network	Political connection	Police	Lawyer	Courts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Average treatment effects (ordered logit coefficients), N = 2,389							
Formal contract	−0.694***	−0.619***	−0.229***	−0.073	0.734***	0.861***	0.815***
	(0.081)	(0.075)	(0.076)	(0.082)	(0.076)	(0.075)	(0.075)
Panel B: Average marginal effect of treatment on E[Y], by firm formality, N = 2,389							
Informal firms	−0.187	−0.319	−0.008	0.023	0.387	0.267	0.302
Formal firms	−0.275	−0.397	−0.174	−0.052	0.405	0.620	0.685
Difference	−0.088	−0.078	−0.166	−0.074	0.019	0.353	0.383
(bootstrap SE)	(0.060)	(0.095)	(0.094)	(0.061)	(0.091)	(0.083)	(0.102)
p-value	0.147	0.413	0.076	0.225	0.837	0.000	0.000
Panel C: Average marginal effect of treatment on E[Y], by connection status, N = 535							
Unconnected firms	−0.317	−0.315	−0.138	−0.008	0.299	0.403	0.495
Connected firms	−0.123	−0.275	0.007	0.144	0.217	0.484	0.448
Difference	0.194	0.040	0.145	0.152	−0.082	0.080	−0.047
(bootstrap SE)	(0.138)	(0.216)	(0.191)	(0.121)	(0.190)	(0.201)	(0.233)
p-value	0.161	0.853	0.448	0.207	0.666	0.689	0.842

Notes: Panel A reports ordered logit coefficients on the treatment indicator with standard errors in parentheses. Panel B reports average marginal effects of the treatment on the expected value of the response, computed separately for formal and informal firms from an ordered logit including the treatment, firm formality, and their interaction; standard errors for the difference are bootstrapped over 400 replications. Panel C reports the analogous estimates by political connection status. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table A8: Ordered logit estimates

A.6 Business associations

Membership in business associations was relatively low in the sample, especially for firms in the informal sector. This sets Senegalese firms apart from other contexts in sub-Saharan Africa where such membership is a critical factor in everyday business operations.

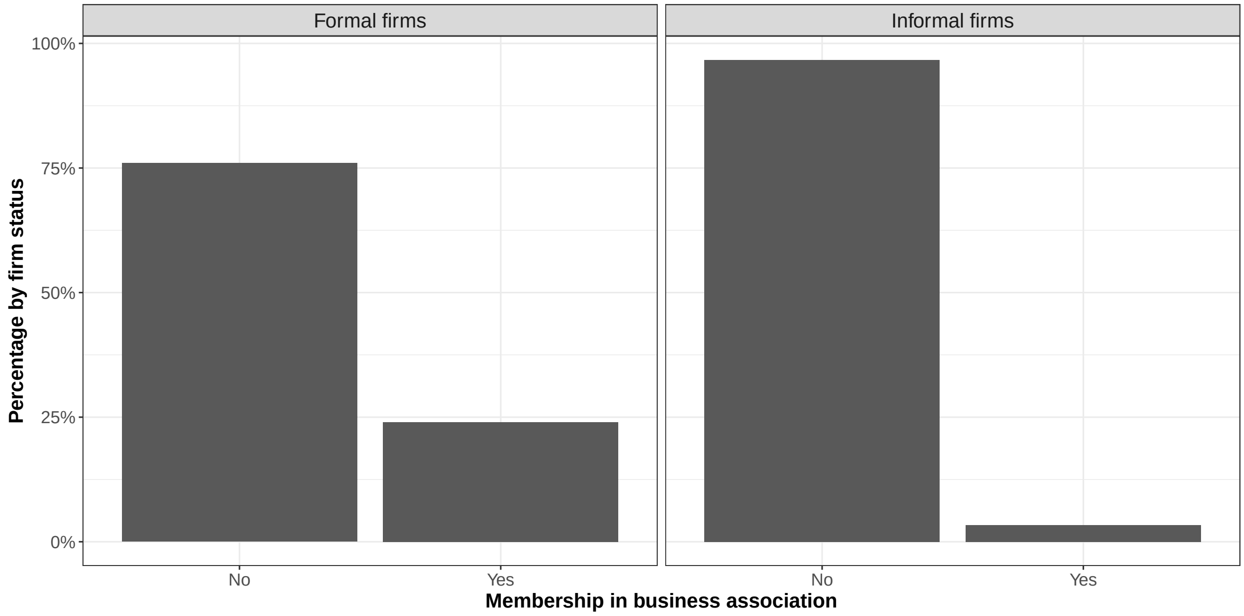

Figure A1: Membership in business association by firm status

A.7 Correlation table

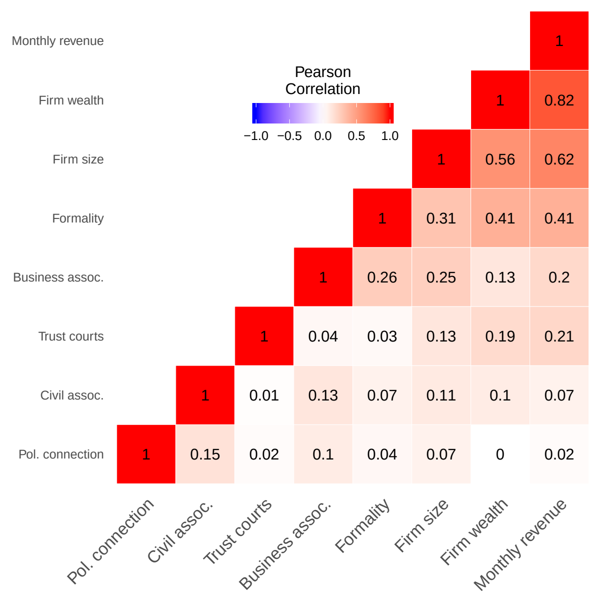

Figure A2: Correlation table

A.8 Balance

	Outcome:												
	Formal firm	Number employees	Monthly revenue	Valuation	Business assoc.	Access to credit	Declared revenue	Education	Age	Trust in courts	Worked for state	Member of party	Civil assoc.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Treatment	0.022	0.026	0.048	0.072	0.008	0.068*	0.028	0.416	0.769**	0.035	−0.006	−0.007	−0.037*
	(0.019)	(0.039)	(0.047)	(0.046)	(0.015)	(0.040)	(0.020)		(0.387)	(0.042)	(0.008)	(0.016)	(0.020)
Constant	0.651***	2.293***	3.688***	4.631***	0.167***	1.881***	0.465***	6.533	33.967***	1.516***	0.039***	0.205***	0.508***
	(0.014)	(0.027)	(0.033)	(0.032)	(0.011)	(0.028)	(0.014)		(0.271)	(0.030)	(0.005)	(0.012)	(0.014)
Observations	2,389	2,389	2,389	2,389	2,389	2,389	2,389	2,389	2,389	2,389	2,389	2,389	2,389

Notes: Covariates regressed on treatment indicator. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table A9: Balance

A.9 Moderating impact of political connections for formal versus informal firms

	Outcome: Likelihood of using enforcement strategy						
	Social enforcement outcomes				*Formal enforcement outcomes*		
	Amicably	Social network	Religious network	Political connection	Police	Lawyer	Courts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Formal contract	−0.379***	−0.519***	−0.116	−0.264**	0.222	0.495***	0.570***
	(0.115)	(0.161)	(0.133)	(0.110)	(0.160)	(0.158)	(0.182)
Political connection	−0.268**	−0.038	−0.266*	−0.308**	−0.146	−0.136	0.073
	(0.135)	(0.189)	(0.156)	(0.129)	(0.188)	(0.186)	(0.213)
Formal contract × political connection	0.369*	0.339	0.112	0.526***	0.0004	0.280	0.158
	(0.190)	(0.266)	(0.219)	(0.182)	(0.264)	(0.261)	(0.300)
Constant	4.577***	3.165***	1.866***	1.608***	3.309***	2.536***	2.691***
	(0.081)	(0.114)	(0.094)	(0.078)	(0.113)	(0.112)	(0.128)
Observations	305	305	305	305	305	305	305

Notes: Survey questions asked how likely respondents were to use each of the reported enforcement options, on a scale of 1 (never) to 5 (extremely likely). The question wording was: "Imagine that another company owes your firm a significant amount of money for services or products you provided, and the other company has missed the deadline to pay that was agreed upon in a [*control*: verbal agreement with no written contract][*treatment*: written, legal contract that you both signed.]" * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table A10: Moderating impact of political connections: formal firms

	Outcome: Likelihood of using enforcement strategy						
	Social enforcement outcomes				*Formal enforcement outcomes*		
	Amicably	Social network	Religious network	Political connection	Police	Lawyer	Courts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Formal contract	-0.336^{***}	-0.064	-0.208	0.177^{*}	0.268^{*}	0.339^{**}	0.361^{**}
	(0.119)	(0.188)	(0.184)	(0.107)	(0.156)	(0.140)	(0.179)
Political connection	0.138	0.525^{*}	0.089	-0.147	0.019	0.136	0.277
	(0.168)	(0.266)	(0.262)	(0.151)	(0.221)	(0.199)	(0.254)
Formal contract	0.151	-0.177	0.144	-0.001	-0.027	-0.339	-0.305
× political connection	(0.228)	(0.361)	(0.355)	(0.205)	(0.300)	(0.270)	(0.344)
Constant	4.602^{***}	3.216^{***}	2.170^{***}	1.443^{***}	2.795^{***}	1.864^{***}	2.057^{***}
	(0.082)	(0.129)	(0.127)	(0.073)	(0.107)	(0.096)	(0.123)
Observations	230	230	230	230	230	230	230

Notes: Survey questions asked how likely respondents were to use each of the reported enforcement options, on a scale of 1 (never) to 5 (extremely likely). The question wording was: "Imagine that another company owes your firm a significant amount of money for services or products you provided, and the other company has missed the deadline to pay that was agreed upon in a [*control*: verbal agreement with no written contract][*treatment*: written, legal contract that you both signed.]" * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table A11: Moderating impact of political connections: informal firms