

Political Property Rights
Essays on Economic Opportunity Under Selective Rule of Law

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ABSTRACT

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Secure property rights are a major predictor of economic growth, yet property rights in much of the world are a function of political power. Those with political connections have privileged access to state institutions, benefit from preferential contract enforcement, and face fewer risks of expropriation in the private sector. This dissertation examines how consumers and firms navigate the complex interaction between formal and informal institutions in these environments of selectively enforced rule of law. I use original experimental data from Senegal, a state that epitomizes political property rights.

In Paper 1, I argue that political connections produce moral hazard in exchange and introduce biases in judicial enforcement. I present evidence from a field experiment in which I created and operated a sales company, randomizing political connections and formal contracts during transactions. The results show that asymmetric political connections decrease buyers' propensities to trade and that formal contracts only increase exchange among connected buyers. This work challenges conventional wisdom and extant literature on the value of political connections and formal contracts in the private sectors of developing countries.

Paper 2 examines how political connections and formal contracts, among other state and nonstate influences, affect the behavior of firms under selective rule of law. To illustrate the complicated decision calculus that firms face when social, formal, and political factors all motivate exchange, I implemented a conjoint experiment with 2,389 firm managers. The results show that firms avoid deals with partners

that have low-to-mid-level political connections, yet seek out deals with the most highly connected firms—despite believing they are more likely to breach contracts. These results demonstrate the countervailing effects of political connections and suggest why consumers and firms may react to them differently.

Finally, Paper 3 asks how firms enforce their property rights when deals go astray. I argue that contract formality can shape firms' property security strategies and demand for rule of law, and test this using evidence from a survey experiment administered to firms in both the formal and informal economies. I present descriptive evidence that enforcement strategies differ by firm formality status and political connections. The experimental findings show that while formal contracts increase the use of legal enforcement institutions, they also widen the enforcement gap between formal and informal firms.

Together, these papers present theory and evidence of politically determined economic behavior under selective rule of law. The results imply that political connections are a form of rent-seeking that can suppress overall trade and produce market inefficiencies. Under these conditions, state institutions may unintentionally exacerbate political and economic inequalities.

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For Charlotte. These are the “various tasks” I’ve been working on.

Introduction

“Virtually every commercial transaction has within itself an element of trust. . . . It can be plausibly argued that much of the economic backwardness in the world can be explained by the lack of mutual confidence.”

Arrow (1972)

The ability to exchange goods is fundamental to economic growth. Firms and consumers alike desire for their exchanges to be honored, but often cannot know if the other party will defect. This has been called the “fundamental problem of exchange:”

What determines the extent to which members of a society enter into technically feasible and mutually profitable exchange? The key to addressing this question is to consider its inverse: Why would decision-makers not enter into mutually profitable exchange? A decision-maker would not enter into an objectively mutually profitable exchange unless assured that the exchange can indeed make him better off and that the other party would act in a manner that will make him better off. . . . In other words, a necessary condition for exchange is that one can *ex-ante* commit to being able and willing to fulfill contractual obligations *ex-post*. (Greif 2000)

We know from a wealth of social science research that formal and informal institutions critically affect confidence in exchange (North 1991). But how do people

and businesses trade with confidence in places where these institutions work differently for different groups? How does the selective enforcement of rule of law affect private-sector development when both state and nonstate institutions substantially shape the private sector? What generates confidence in exchange in the absence of equal rule of law? The answers to these questions are essential for economic development that is efficient and equally distributed (Greif 2000).

Countless studies show that property rights—the ability to use, derive income from, or transfer an asset (Barzel 1997)—encourage economic development. In much of the world, however, property rights and their enforcement are dependent on external factors. This dissertation focuses on the political factors. I use the term “political property rights” to refer to systems in which property rights are a function of one’s proximity to political power. This characterizes the bulk of the world’s developing democracies, where property rights exist but are not universally distributed, and where formal and informal influences combine to determine whose property rights are secure—and whose are not.

This dissertation grew from a desire to examine fundamental questions of trust, state-building, and economy-building. What affects the trust that citizens place in fellow citizens, and how does political power shape this trust? Exchange is a useful setting for examining trust, as it serves as a costly behavioral signal of trust. Because trust in exchange is a building block for economies—and, by extension, states—it becomes a useful frame through which to view development at an almost primordial level. The determinants, structure, and enforcement of exchange hold implications not just for economic development, but also for how society functions without unraveling.

In this dissertation, I examine the impact of political property rights on private-sector behavior. Throughout, I highlight how state and nonstate influences structure the decisions that consumers and firms make when deciding to exchange. This,

in a sense, examines the consequences of a regime of political property rights, as who trades with whom reflects perceptions of property rights security. I also examine how people actually secure their property rights when the fundamental problem of exchange manifests in defection.

This dissertation makes use of data generated from fieldwork in Senegal, a stable country in an otherwise turbulent region of the world. I was initially drawn to Senegal as an undergraduate student conducting thesis research. At the time, I was living with a host family in Dakar whose home village was in Kédougou, deep in the interior of Senegal. There was some sort of business dispute the family was experiencing—I wasn't privy to the details—and they needed help getting it resolved. Still short on details, I tagged along with my host mother and brother to a lavish house in an expensive area of Dakar. I waited in the living room while they talked things over with someone who I later learned was a politician from Kédougou. After a half-hour or so, we left. "Good, that's sorted," my host brother said as we were leaving. Seeing my confused expression, he said: "This is how things get done." The logic I saw in operation there is one that gets confirmed and explicated in this dissertation: rule of law functions, by and large, but it helps to know powerful people.

The three papers of this dissertation rely on a combination of field and survey experiments that I conducted in Senegal between 2016 and 2018. This methodology allows me to make causal claims about the roles of political connections and contracts in exchange under selective rule of law, a literature that has traditionally relied on observational data that suffers from selection biases. The first paper presents a field experiment to examine the impact of political connections and formal contracts on *consumers'* propensities to exchange. The second and third papers draw from an original survey of 2,389 formal and informal firms in Dakar. Paper 2 examines the impact of political connections and contracts on *firms'* trade behav-

ior, with a conjoint experiment that simultaneously tests other formal and informal drivers of exchange. Paper 3 answers a question that arises from the first two papers: once an exchange has been entered, how do firms actually enforce contracts when they are broken? I summarize the papers in greater detail below.

Paper 1: The Political Determinants of Economic Exchange

Paper 1 asks whether political connections may have a countervailing effect on economic opportunity: because politically connected individuals benefit from preferential treatment from the state—particularly during contract disputes—others may resist trading with them. In this way, asymmetric political connections may affect perceptions of moral hazard and people’s propensities to exchange. And in contexts where informal political influences affect perceptions of property rights security, it remains unclear how state institutions for contract enforcement interact with such informalities.

Paper 1 measures the causal impact of political connections and formal contracts on consumers’ willingness to exchange in modern markets. I created and registered my own business in Dakar, and hired employees to conduct door-to-door sales of a phone credit service. These trades mimicked the fundamental problem of exchange mentioned above that required trust to overcome. I formed partnerships with three influential municipal councils that agreed to host my employees prior to the project’s implementation. Across 1,458 transactions, I randomized whether my employees signaled these political connections, as well as whether they offered formal contracts during transactions. I measured consumers’ purchases as the outcome, a costly behavioral signal of their perceived risk of exchange. I also implemented an endline survey to measure buyers’ political connections.

The results for this project show that, on balance, sellers’ political connections decreased consumers’ willingness to enter into exchange, though these estimates are fairly noisy. Asymmetries in political connections between my employees and

buyers reveal differential trading patterns reflective of politically driven moral hazard. Politically connected buyers were more willing to exchange with unconnected sellers than with connected ones. Average treatment effects for the formal contract treatment arm show a significant and substantial increase in consumers' willingness to trade, suggesting that formal contracts mitigate trade risks. However, examining the results by buyers' political connections reveals a less optimistic picture: this effect is driven by politically connected buyers, i.e. those who have preferential access to enforcement institutions. These results show that increasing access to formal institutions may in fact compound inequalities in the private sector. This theme—the potential for formal institutions to deepen existing inequalities—reappears throughout this dissertation.

Paper 2: Social, Formal, and Political Determinants of Trade

While Paper 1 examines how political connections and formal contracts affect consumers, Paper 2 examines how they affect firms. Firms might react to political connections differently than consumers in developing countries for a number of reasons. Whereas consumers seek political connections for their protective power, firms seek political connections for both defensive and offensive purposes. Political connections grant firms preferential access to credit, capital, and lucrative state contracts, and confer the ability to exert force over other firms. While politically connected firms can break contracts with relative impunity, Paper 2 asks if this risk may be worth the potential gains.

To answer this, and to measure how competing state and nonstate influences affect firms' decisions to enter into exchange, I implemented a conjoint experiment with a sample of 2,389 firms in Senegal. This sample is special in several regards. First, the size of the sample allows for testing underexplored hypotheses in an environment with selectively enforced rule of law. Second, the sample includes both formal and informal firms. Data on informal firms in places like Senegal has been

inherently difficult to collect, and this sample allows testing differences across the formal and informal sectors. Third, the survey sample consisted only of firm owners and managers. This sample thus represents those with actual decision-making power for their firms, and provides rich descriptive data that cannot be obtained by only surveying employees.

The results of this paper reflect the complicated decision calculus that firms face when taking on new business partners in environments of selective rule of law. I first show that firms place significant weight on ascriptive features of business partners, associating ethnicity and religious group affiliation with trustworthiness and risk of contract breach. Still, firm owners seek deals involving formal contracts and avoid deals based on informal contracts. The results for political connections reflect firms' complex risk calculations: firms avoid doing business with firms that have low- to mid-level political connections, but actively seek out deals with the most politically connected firms—despite believing highly connected firms are likely to break contracts. This suggests there is a point at which a firm's political connections pose seemingly no downside: not only do connected firms benefit from the bias of the state, but other firms actively seek their partnership due to their connections.

Paper 3: Firm Strategies, Weak Rule of Law

The first two papers examine the determinants of trade, in a sense proxying the extent to which firms and consumers perceive their property rights to be protected. The third and final paper asks how firms actually protect their property rights. How do firms resolve disputes when deals are broken, especially when deals are based on informal agreements? While Paper 2 of this dissertation shows that variation in contract formality can increase business owners' propensities to exchange, can contract formality also affect how business owners enforce their exchanges? In places where multiple dispute resolution methods exist, it is unclear when firms will choose to resolve their disputes with formal or informal methods.

I implemented a survey experiment with the same sample of 2,389 firms. The experiment presented firm managers with a scenario in which a trading partner renegeed on a deal that was based on a randomized formal or informal contract. Firm managers were asked their likelihoods of using various dispute resolution tactics, which I use to estimate how contract formality affects dispute resolution strategies. I also estimate effects by covariates of theoretical interest to this dissertation, including firm formality, trust in state institutions, and firms' political connections.

Descriptive results show that firms overwhelmingly prefer to enforce disputes amicably and that formal firms are more likely to use legal methods of contract enforcement. Politically connected firms are more likely to use both state and non-state methods of enforcement, reflecting the utility of political connections across the formal and informal sectors. The experimental results demonstrate that when deals are based on formal contracts, firm owners are more likely to use formal dispute methods. This effect is pronounced for formal firms. These results complement Paper 1 by suggesting that increasing access to the formal sector—in this case, to formal contracts—can widen existing inequalities.

Political Determinants of Economic Exchange: Evidence from a Business Experiment in Senegal

Abstract: Economic growth requires confidence in the state’s ability to enforce secure exchange. But when states selectively enforce rule of law, political considerations can moderate the trust that buyers have in sellers. I argue that political connections produce moral hazard in exchange and introduce biases in judicial enforcement. Buyers avoid trade with relatively powerful sellers, and, in this context of unequal enforcement, formal contracts accentuate power inequities by only protecting politically connected buyers. I created a legal business in Senegal to randomize whether salespeople signaled their political connections and offered formal contracts during transactions. The results show that relatively politically powerful sellers decreased—while formal contracts increased—buyers’ propensities to trade. However, formal contracts only boosted trade among connected buyers. These findings show how asymmetric political connections can impede daily trade and intensify economic inequalities in developing contexts, while simultaneously demonstrating the limits of state institutions for mitigating politically-driven

moral hazard.

1.1 Introduction

Confidence in basic forms of exchange is a fundamental building block for societies (Arrow 1972; North 1991). For an economy to function and grow, buyers must be confident that sellers will honor purchases and deliver the products promised to them. This is particularly true for modern markets where payment is due prior to product delivery, and where opportunities arise for seller moral hazard—pocketing payment and failing to deliver promised goods. Such seller moral hazard has become a salient problem for both firms and individuals in developing countries as emerging markets grow. Businesses cannot always rely on repeated trading relationships (e.g. Baker, Gibbons and Murphy 2002), and consumers similarly engage in one-shot exchanges with sellers who offer delivery contingent on payment. Agreeing to buy and at least partly pay an unfamiliar seller before a good is delivered is a common feature of modern economies.

In countries with weak or selectively enforced rule of law (e.g. Holland 2016), however, inequality in the application of rule of law can moderate buyers' confidence in sellers (North and Weingast 1989). These are places where *who* one knows can drastically affect business operations. Knowing someone in government can serve as a form of protection from punishment: a seller's political connectivity grants relative impunity in the case of failure to deliver promised goods. While there are many benefits of political connections (e.g. Szakonyi 2018), these inequalities may stifle trade by exacerbating perceptions of seller moral hazard. Furthermore, this context of informal influences may complicate how citizens view the utility of state contracting institutions. Given the ubiquity of legal inequalities in developing countries, understanding the factors that affect propensities to engage

in trade has significant implications for economic development.

In this paper, I propose a theory of seller moral hazard in exchange in societies with selective rule of law. Due to the preferential treatment that political connections confer in these societies, buyers believe that politically connected sellers can break contracts with relative impunity. As a result, buyers avoid trade when sellers are relatively more politically connected and seek trade when sellers are relatively less connected. In the context of these inequalities, state-backed formal contracts may fail to mitigate risk for all types of buyers: if contracts are more likely to be enforced in favor of the politically connected, they may only be useful to politically connected buyers. This theory implies that asymmetric political access shapes private-sector exchange, and that political connections disrupt the function of formal institutions.

To study the impacts of political connections and contracts on private-sector exchange, I designed a field experiment in the urban environment of Dakar, Senegal. Its mixture of semi-reliable state institutions and salient informal influences made Senegal a fitting setting in which to test this theory. The field experiment sought to replicate a natural trading environment with real financial stakes and seller moral hazard. To that end, I created a legal business, and hired employees to sell a mobile phone-credit service with delayed delivery to 1,458 households.¹ In a factorial design, I randomized whether, during transactions, my employees signaled their political connections and/or offered formal contracts. I measured purchase rates as the primary outcome. To ensure that political connections were credible and consistent across employees, I partnered with three influential municipal councils in Dakar that hired and hosted my employees prior to implementation. During transactions, employees briefly mentioned their work at the council to treatment households; this resembled common practices in Senegal, where

¹The business did not generate positive net profits; it was created solely for research purposes.

door-to-door sales are frequent and often involve extended introductions. To measure *buyers'* political connections—as well as to parse the mechanisms by which the treatments operated—I implemented an endline survey among the sample several days after transactions.

The results of the field experiment show that when sellers signaled their political connections, purchase rates declined. This finding demonstrates how political connections—even the low levels of connections examined here—stifle exchange. Taking into account buyers' political connections shows that political asymmetries between the buyer and seller also moderated rates of purchase. Exchange was at its highest when the buyer was most powerful: when the buyer was politically connected but the seller was not. Additional tests show that these results were not driven by other variables associated with political connections, nor were they driven by co-ethnicity or co-religiosity, the primary competing explanations of non-state contract enforcement in markets like Senegal. I also rule out the possibility that the political connection treatment operated by affecting the perceived competence or quality of sellers. Rather, sellers' political connections affect buyers' perceived recourse options: successfully resolving a contract dispute is an obstacle when the opposing party is politically connected.

The results further show that formal contracts substantially increased propensities to trade. This demonstrates that even in areas with weak rule of law, formal contracts can mitigate risk and boost confidence in exchange. But exploring this result more deeply reveals a less rosy picture: the positive effect of formal contracts was driven entirely by buyers who were politically connected themselves. Offering formal contracts had no effect on unconnected buyers' confidence in exchange. This finding suggests that formal contracts may be useful only for already-privileged citizens in societies with selective rule of law. In these contexts, state institutions may actually perpetuate inequalities in private-sector exchange.

Overall, these results show that political connections stifle private-sector exchange, and that formal contracts favor the powerful under weak rule of law. This paper thus makes several contributions. First, this project shows how individualized political connections can *constrain* private-sector growth. Work on political connections emphasizes the profitability of these connections (e.g. Roberts 1990; Fisman 2001; Khwaja and Mian 2005; Faccio 2006; Szakonyi 2018). These studies, however, condition on firms that already exist and trade that has already occurred. My findings, by contrast, provide evidence that political connections may prevent deals from occurring in the first place, implying that extant work may suffer from selection bias and thus overstate the value of political connections. Furthermore, there has been a dearth of evidence connecting *individualized* political connections—which serve different purposes than firm-level political connections and thus operate through different theoretical channels—to private-sector economic outcomes in modern, urban markets. I provide experimental evidence of this impact, carefully manipulating seller moral hazard to elucidate key aspects of the theoretical dynamic. This paper thus builds the evidence base for an important yet under-examined variable.

Second, I show that political connections influence trade even when controlling for more studied forms of social enforcement such as co-ethnicity and co-religiosity (e.g. Grimard 1997; Sanchez de la Sierra 2018). Political connections are non-ascriptive, vary dynamically over time, and affect demographically homogeneous societies. I thus argue that political connections merit study as a variable separate from other forms of social group enforcement that rely on mechanisms such as in-group pressure and reputation costs (Fearon and Laitin 1996; Habyarimana et al. 2007). My findings suggest that political connections operate through an alternate mechanism: legal system bias. Political connections may help to explain unequal development in the many places where ethnicity is not a salient political dimension.

Finally, I provide evidence for the impact of institutions on private-sector economic growth in states with weak rule of law. It is striking that contracts can increase confidence in exchange in Senegal, despite its reputation for weak contract enforcement. The results of this paper suggest that, even in trying environments, people do believe in the state to some degree. And while existing work suggests that institutions are important because they facilitate trade and improve growth prospects (e.g. North 1991; Acemoglu and Johnson 2005), I add nuance by pointing to important distributional implications that are likely to enhance inequalities. I show that formal contracts can accentuate power differentials, and may thus fail to protect non-connected citizens in societies where recourse options depend on political connections. These findings demonstrate that individual-level political connections can impede trade and limit the effectiveness of legal institutions for growth.

1.2 Theory

Existing research on solutions to commitment problems in trade fall into two broad categories: theories in which the state is the primary enforcement mechanism and theories in which it is not. State solutions for contract enforcement and the security of property rights depend on the state's commitment to constrain itself (e.g. North and Weingast 1989; Olson 1993), and, in many contexts, states possess neither this capacity nor incentive (North 1991; Firmin-Sellers 2007). Examined in a transaction-cost framework, writing contracts in these environments imposes costs that are prohibitively costly to overcome (e.g. Coase 1960; Williamson 1985). The other broad strain of work focuses on how enforcement emerges outside—or in the absence—of state institutions. In these studies, considerations such as reputation costs, ethnicity, relational contracts, and various informal constraints can result in

enforcement equilibria even when states are uncooperative (e.g. Greif 1989; Milgrom, North and Weingast 1990; North 1991; Greif 1993; Greif, Milgrom and Weingast 1994; Grimard 1997; Baker, Gibbons and Murphy 2002; Brown, Falk and Fehr 2004; Sanchez de la Sierra 2018).

In much of the world, particularly in developing democracies, states have the capacity to enforce contracts and institutions are generally cooperative, but state agents are biased in the application of rule of law toward certain parties (North 1990; Holland 2016). Those who possess connections to people in power receive preferential treatment, including in the business environment. This can exacerbate buyer moral hazard (Sanchez de la Sierra 2018) as well as seller moral hazard—pocketing payment and delivering substandard products or failing to deliver goods entirely.

Political connections and formal contracts in exchange

Political connections are invaluable to firms in states that selectively enforce the rule of law. Politically connected firms amass greater profit (Fisman 2001; Szakonyi 2018), achieve larger market valuations (Faccio 2006), and gain access to preferential state financing (Khwaja and Mian 2005). Dealing with politically connected firms can thus offer lucrative opportunities for potential business partners, including access to markets, better capital, and a launching pad for developing one's own political connections. However, the relevance and probability of realizing these advantages are different for individuals than for firms. While firms might value access to new markets, for example, this benefit is irrelevant to individuals engaging in one-shot exchanges with businesses. And even though individuals have incentives to develop their own political connections, they are unlikely to do so by trading with firm representatives they will never meet again. This is especially true of the types of trade that I focus on in this paper, which are increasingly common

in modern economies: one-shot exchanges involving seller moral hazard.

For individual buyers, the risks of trading with connected sellers outweigh the potential benefits. Buyers are hesitant to purchase from politically powerful sellers because connected sellers are able to break contracts with relative impunity: the state's selective application of the rule of law enables connected people to escape punishment more easily than non-connected people (Lu, Pan and Zhang 2015). In disputes with state-backed sellers, buyers expect the state—either in the form of courts or the more commonly used police and local mediators—to enforce in favor of politically connected sellers (Frye 2004). In the presence of seller moral hazard, we should thus expect sellers' political connections to stifle exchange.

Hypothesis 1 *Sellers' political connections decrease the likelihood of exchange.*

State-backed formal contracts could mitigate some of these moral hazard concerns. Contracts provide proof that a deal occurred, specify the responsibilities of the exchanging parties, and safeguard against hazardous exchanges (Williamson 1985). In large societies and economies, contracts can serve as third-party enforcement mechanisms that enable exchange to occur (Dixit 2003). Empirically, formal contracts have been shown to increase trade by improving agents' confidence in the trustworthiness and enforceability of exchange (Poppo and Zenger 2002; Sanchez de la Sierra 2018). Assuming some level of rule of law and function of enforcement institutions, we might thus expect contracts on the margins to boost confidence in trade. But in countries with weak rule of law, this effect is not a given, and varies depending on confidence in the formal institutions backing exchange (Poppo and Zenger 2002).

Hypothesis 2 *Formal contracts increase (do not have an effect on) the likelihood of exchange.*

In places where sellers' political connections intensify moral hazard in exchange, the interactive impact of formal contracts is also unclear. On one hand, the imposition of formal contracts could resolve the distrust that connections induce. On the other, problems of political connections may be so severe that formal contracts themselves are subject to manipulation by the politically connected. In these contexts, a contract may deepen moral hazard concerns by serving as an additional form of influence for the politically connected. Thus, based on institutional context, we should expect differential outcomes.

Hypothesis 3 *Formal contracts complement (substitute) the impact of politically connected sellers on exchange.*

Political asymmetries in trading dyads

I argue that the advantages of political connections accrue not only to sellers with connections, but also to politically connected *buyers*. Because connected buyers can access state enforcement institutions—cutting through the red tape that holds up the majority of citizens—and benefit from the bias of the state, connected buyers have powers that unconnected ones do not. A buyer's political connections might thus mitigate concerns of seller moral hazard and factor into the decision calculus to engage in trade.

In line with this logic, asymmetric levels of political connections between sellers and buyers may attenuate or heighten perceptions of risk. If political connections translate into favoritism from the state, we should expect unequal buyer-seller political connections to moderate rates of exchange. In the context of transactions characterized by seller moral hazard, buyers can assess power differentials and make decisions to trade accordingly. While this implies that sellers may possess incentives to hide their political connections, in developing democracies where informal influences are rampant in the private sector, buyers often already have an

		<i>Buyer is politically connected</i>	
		No	Yes
<i>Seller is politically connected</i>	No	Intermediate probability of purchase	High probability of purchase
	Yes	Low probability of purchase	Intermediate probability of purchase

Table 1.1: Theoretical predictions under asymmetric political connections

idea of sellers' connectivity, or can quickly make these assessments based on extended introductions.² (Im)balances in the connectivity level of the trading dyad can thus factor into the decision calculus of buyers by altering perceptions of seller moral hazard. Holding fixed the terms of a given deal, we should expect a lower likelihood of trade when sellers are more powerful than buyers. Correspondingly, buyers are more likely to enter into exchange when they have outsized influence relative to sellers. In situations where buyers are on similar enforcement playing fields, the predictions are less clear. When buyers and sellers are both unconnected, buyers may assume the worst about sellers' potential connections and thus not enter into a deal. When buyers and sellers are both connected, the playing field is relatively equal in terms of enforcement, and buyers may choose to trade, though perhaps not as much as they would if sellers were unconnected. Table 1.1 summarizes these theoretical predictions.

Hypothesis 4 *Buyers are more likely to exchange when they are politically connected and sellers are not, and less likely to exchange when sellers are politically connected and they are not.*

Given these asymmetric political power dynamics, how do formal contracts moderate the perception of seller moral hazard for connected and unconnected buyers? Formal contracts are vestiges of the states that grant them power; if po-

²In Senegal, for example, introductions are extensive in nature, particularly for door-to-door sales.

litically connected citizens have privileged access and treatment vis à vis state institutions, the power to have contracts enforced may be concentrated in the state-backed party. In the buyer-seller theoretical framework, politically connected buyers are more likely to have contracts enforced in their favor than unconnected buyers, holding constant the seller's level of political connections. Thus, connected buyers should place more value on formal contracts for protecting their claims and mitigating concerns of seller moral hazard.

Hypothesis 5 *Formal contracts increase the likelihood of exchange for connected buyers more than unconnected buyers.*

1.3 Context

Rule of law and methods of enforcement

Senegal is a multi-party democracy in West Africa. Despite its democratic tendencies, however, Senegal's rule of law institutions remain weak. The World Bank ranks Senegal at 140 of 190 economies in terms of overall ease of doing business, and 142 in enforcing contracts. Its judiciary is based on French civil law, generally considered inferior to common law systems for securing property rights and growth in Africa (Joireman 2001), and its legal institutions suffer from excessive procedural formalism, limited judicial independence, and high costs and waiting times (Kondylis and Stein 2018). This results in negative perceptions on the part of citizens of the judiciary's accessibility.

Despite these weaknesses, Senegalese citizens place a relatively high degree of trust in legal institutions.³ In my sample, 66.8% of respondents reported at least partial confidence in the courts, and around 60% stated that courts, lawyers, and

³Appendix Figure A.6 shows that Senegal ranks relatively highly within Africa for public trust in courts.

the police were likely to resolve hypothetical contract disputes successfully. Still, most citizens are unlikely to use high-level courts or lawyers to settle small-scale contract disputes—the type this project probes—due to the significant financial and time costs. Citizens typically first attempt to resolve petty disputes amicably, which involves contacting the defector (either directly or via shared social networks) and coming to an agreed-upon resolution. If this fails, involving the local police or small claims court is the next step. All of these means of enforcement become complicated by political connections, however.

Given the difficulties of legal enforcement, traders often make use of social heuristic devices to secure their deals. In-group networks in Senegal—formed around ethnic and especially religious cleavages—can lead to sustained trading equilibria due to enforcement mechanisms such as reputation costs and shared enforcement technologies (Fearon and Laitin 1996; Habyarimana et al. 2007). In Senegal, religious networks are particularly important, with many citizens belonging to Islamic brotherhoods characterized by tight-knit social structures with common sources of authority (Cruise O’Brien 1971; Villalón 1995; Beck 2008; Gottlieb 2017). This carries into the private sector, where people use ethnicity and religious affiliation as proxies for the probability of contract defection (Bhandari 2019*b*). However, in the urban environment of Dakar, social networks are less cohesive than in rural Senegal, and social networks may not substitute for formal contracts as much as they might in rural areas (Koter 2013).⁴ These social mechanisms are also less relevant to large-scale trade and markets where relational contracting is not an option, which is the common type of trade that my field experiment replicated. Nevertheless, in the results that follow, I control for shared in-group affiliation to ensure it does not drive results.

⁴Similarly, in their theoretical framework, Bohnet, Frey and Huck (2001) show that problems of trust are more pronounced in large group settings, a logic that extends to comparisons between villages and cities.

Political connections in exchange

A commonly held view in Senegal is that political connections lead to preferential treatment at all levels of the state. Especially with Senegal's dense and convoluted bureaucratic structures, knowing someone in power allows for quicker access, processing, and eventual success in matters involving the state. Connections reduce the massive amounts of red tape with which "ordinary" citizens must contend, and knowing even a low-level bureaucrat can enhance one's chances of gaining preferential access to institutions. Getting one's foot in the door is often the most difficult step of the enforcement process, but even non-direct connections help overcome this constraint via shared governmental networks.⁵ Political connections thus play a significant role in the business environment by determining access to means of enforcement. Even outside of court structures, citizens anticipate that resolutions will be biased toward the person with more political power, even when non-court dispute resolution mechanisms are used.

The consensus of my sample was that possessing some type of political connection decreases the probability of punishment for contract breach. Only 14% of respondents said that political connections do *not* help in court, while 76% stated that connections enable trading partners to escape punishment when they break contracts. Figure 1.1 shows the extent to which respondents believe that people with connections to councils, courts, and police are able to escape punishment during contract disputes. Overall, there is severe distrust in the enforcement process as it applies to sellers with any type of political connection.

⁵For example, when asked how he resolved his contract dispute, a respondent in my sample stated that his sister worked as a secretary at the local council, and was able to connect him to the local police chief, who helped him file the correct paperwork.

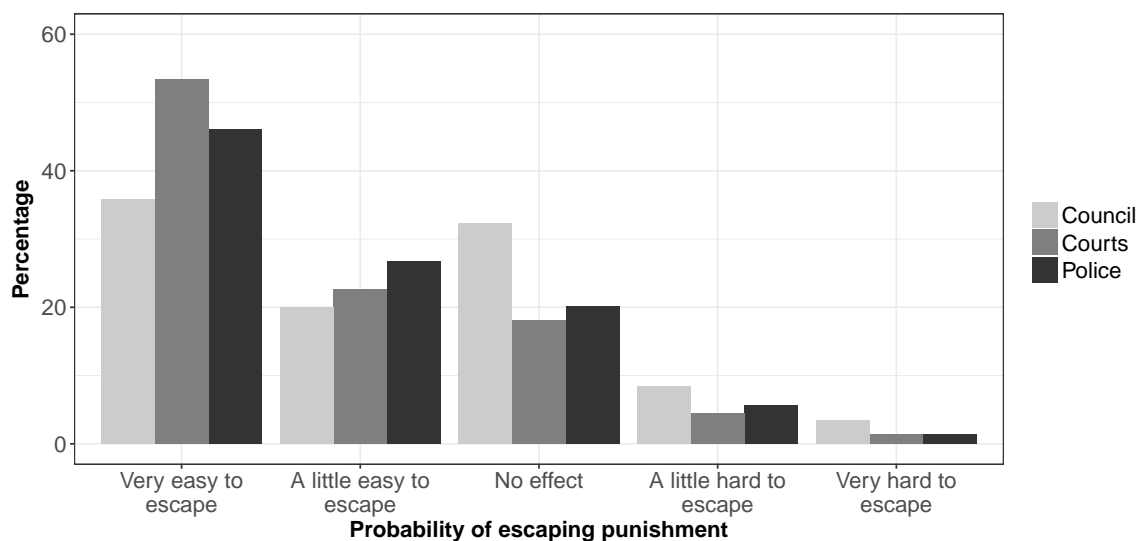


Figure 1.1: Perceived impunity of politically connected individuals in contract disputes

Contracts, transactions, and the phone credit market

A formal contract in Senegal typically takes the form of a written document that follows governmental standards to be executable by local courts of law. Informal contracts are those that do not meet this criteria, and are usually verbal agreements. In trade where delivery is made after payment, contracts serve as more than de facto receipts. Formal contracts include terms and conditions, delineate the contracting parties' responsibilities, and outline procedures in case of contract breach that make resolving disputes more streamlined. Though often a required part of deals, formal contracts in Senegal are sometimes offered optionally, because of the administrative and time costs as well as parties' differential valuations of contracts.

Transactions with delayed delivery to households are not uncommon in Dakar, particularly in densely populated neighborhoods. While typically this type of sale on credit at the household level has been done by informal traders, entrepreneurial growth in Senegal has led to an increase in formal-sector household sales. In the process, previously informal marketplaces have formalized, and citizens have en-

gaged in these types of exchanges at rapidly increasing rates. For example, over 80% of respondents in my sample reported participation in door-to-door sales campaigns in the past, many of which are run by major telecommunications companies in the country. The marketplace for phone credit in Senegal lends itself to door-to-door sales. Very few people receive phone credit through telecommunications subscription services. Rather, most people buy their phone credit as they require it, either from ambulatory traders, some of whom go door-to-door, or from neighborhood kiosks. There is significant demand for mobile credit, and prices are disproportionately high relative to income, particularly in middle-to-lower class neighborhoods. Buyers are thus keen on alternate methods for receiving phone credit, especially when it comes with competitive discounts.

1.4 Research design

I implemented a field experiment that allowed me to carefully manipulate seller moral hazard, in order to test the effects of formal contracts and political connections on exchange. To ensure a natural trading environment, I created and registered a legal, formal-sector business in Senegal, and hired employees to offer a phone credit service via door-to-door sales in sample municipal districts. In a factorial design, I randomized whether employees signaled their political connections, as well as whether they offered formal contracts as part of the deal. An endline survey was conducted several days after the transactions took place to measure buyers' political connections. The real economic environment and the panel structure of the data allows for the rare casual estimation of the effect of political connections and formal contracts on exchange based on political asymmetries in the trading dyad.

Business creation

In preparation for the experiment, I undertook the process of creating and registering a formal business in Senegal. I completed the process in 2016 at APIX, Senegal's primary agency for the promotion of investment and major works, which is also home to Senegal's *guichet unique* (one-stop shop) for formalizing a business. Despite the "one-stop" shop, the process took approximately one month from start to finish, as registering the business required the acquisition of certain documents that are not centrally controlled. This required visits to my local *chef de quartier* (neighborhood chief), police department, and the Ministry of Justice. The result of the process was the successful formalization of the business and the receipt of a unique business identification number called the NINEA, which is commonly understood in Senegal as proof that a business is formal.⁶

The business, called *Porte-à-Porte Sénégal* (Door-to-Door Senegal, or PAPS), offered mobile phone credits at a discount. Mobile credit was chosen as the activity of interest for three key reasons. First, this resembled common sales practices in Senegal, where ambulatory traders sell small items including phone credit directly to households.⁷ Indeed, 82% of respondents in my sample said they had purchased items in similar door-to-door sales campaigns in the past. Second, phone credit could be transferred to individuals via their mobile phone numbers, which ensured there was no deception in delivery of goods at a later date—all buyers received the credit that they purchased. Third, there is high demand in Senegal for mobile phone credit, which ensured sufficient take-up of sales and helped to avoid floor effects.

⁶Appendix Figure A.1 shows a copy of the business registration.

⁷Though the formalized method my company used to sell credit at a discount was perhaps novel to some buyers, it is not unusual during Senegal's entrepreneurial boom in which small businesses have formalized previously informal practices.

Sample selection and partner municipal councils

A key treatment arm in the experiment required sellers to signal their political connections to buyers. For both ethical and inferential reasons, I sought for my employees to have political connections that were credible and consistent across the team. To achieve this, I partnered with three influential municipal councils in Dakar, and arranged for my employees to work at these councils prior to data collection. These municipal units are the level of government with which the average citizen in Dakar interacts, and they have tremendous local influence across a range of political and economic dimensions. For the purposes of contract enforcement, being connected to the council enables access to officials at numerous state organizations via shared governmental networks; these connections open side doors to many enforcement institutions. Each of my employees performed a weeklong internship at a partner council. The typical internship consisted of rotating between the various divisions at the given council, gaining a sense of each division's activities, and meeting staff members throughout the council. A point was made to ensure that my employees knew the names of the people in charge of each division, in case questions about the internship were asked by well-connected buyers during transactions.

Of course, performing short internships with councils could result in a relatively weak type of political connection, so this design might serve as a hard test of the theory proposed above. Still, seemingly low-level political connections are important to daily life in Senegal, as they signal the types of networks and resources to which an individual has access, regardless of how small the connection may seem. Even casually knowing the right person can change one's dealings with bureaucratic structures entirely in the Senegalese context. Those without such connections do not have access to the same recourse options that connected individuals—especially those connected to powerful municipal councils—do in the event of con-

tract breach. As demonstrated in a manipulation check later in the paper, buyers considered my employees to legitimately possess political connections.

Sellers worked at the councils of communes in which the experiment was implemented. I thus followed a two-stage sample selection process for communes. First, each commune had to meet specific criteria, including: 1) densely populated communes that are inhabited primarily by lower-to-middle class workers for whom baseline take-up of discounted mobile credit would be sufficiently high; 2) communes where household access would be relatively straightforward (e.g. not obstructed by large gates, as is common in the wealthiest neighborhoods of Dakar); and 3) communes where household sales are commonplace such that door-to-door transactions would not be perceived as unusual. This first stage of sample selection yielded five ideal sample communes.

In the second stage of sample selection, I met with administrators from these five communes' municipal councils because implementation was conditional on councils' acceptance of the project and willingness to hire my employees as interns. Of the five communes that most closely met the project's criteria in the first stage, three municipal councils agreed to partnerships and allowed my employees to work there: Golf Sud, Médina, and Pikine (shown in Figure 1.2). I hired nine employees to work for my firm, and thus three employees worked at each council.

Treatment conditions

The experiment deployed a factorial design with three treatment arms to test the effects of political connections and formal contracts on economic exchange. In the first arm, sellers randomly signaled their municipal council political connections to buyers. They did so by briefly mentioning their former work experience at the beginning of transactions, during the lengthy introduction period that is common

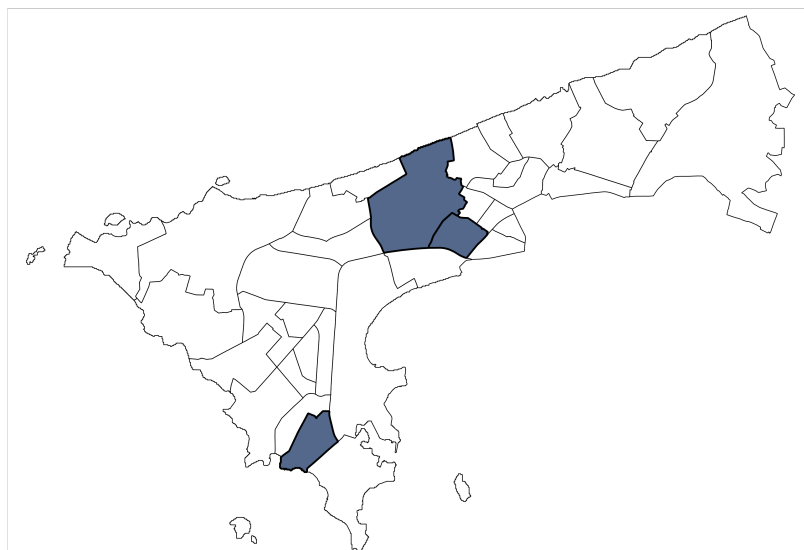


Figure 1.2: Map of the Dakar peninsula, with sample districts shaded

to household transactions in Senegal.⁸ Rather than recreate a general trading equilibrium, this treatment aimed to induce buyers to consider the implications of sellers' political connections. Because on first consideration this treatment may appear artificial or strange to buyers, in the endline survey I asked buyers about their suspicions and skepticism, and ultimately find no effect based on treatment status (see Appendix Table A.11).

In the second treatment arm, sellers included a formal contract as part of the deal. The contract contained key information about the terms of the deal, method of payment, and delivery. Critically, the contract also included a clause on the method of conflict resolution and procedures for recourse in the case of contract breach.⁹ If PAPS failed to deliver the quality or amount of mobile credit that buyers purchased, the contract stipulated that attempts would be made to resolve the dispute amicably before bringing the case before local courts. This mirrored the language of standard contracts in Senegal. Indeed, the contract was reviewed and approved

⁸Appendix Section A.1 presents the general protocol that enumerators followed.

⁹Appendix Section A.2 includes a translation of this clause.

		Contract availability					
		No contract		Contract (required)		Contract (optional)	
Signaled Connections	No	1. Pure control		2. Required contract		3. Optional contract	
	Yes	4. Connection		5. Connection + required contract		6. Connection + optional contract	

Table 1.2: Factorial treatment groups

by a Senegalese law firm, which deemed it to be executable in local courts of law. Sellers explained the contract as part of the transaction, and briefly mentioned that the contract contained information about recourse options. In this treatment arm, buyers and sellers were both required to sign two copies of the contract in order to execute the deal, as is standard in Senegal; the buyer kept one copy, and PAPS kept the other.

For the third and final treatment arm, sellers again offered formal contracts as part of the deal, but in this arm the formal contract was optional. To mimic the transaction costs of contracting, buyers receiving this treatment could elect to have a formal contract for a marginal additional cost. This is consistent with the costs of contracting in Senegal, where, at the end of some transactions, sellers offer a receipt or contract at a very small fee. Sellers explained this fee as an administrative requirement due to the costs of contracting in the formal sector, which was consistent with buyers' expectations. While there is a risk that some buyers may have found this option to be unusual, Appendix Table A.12 shows that, in line with expectations in Senegal, this treatment arm did not raise buyers' levels of suspicion. The two formal contract treatment arms attempted to capture variation in the extent to which sellers constrain themselves with contracts; in some cases, they fully constrain themselves by requiring a contract to be signed, and in others, the formal contract serves more as a non-binding signal. Table 1.2 summarizes the components of the factorial design and shows the six treatment groups.

Data collection

There were two main stages to data collection: 1) the transaction phase during which sellers sold the phone credit service, and 2) an endline panel survey several days after transactions took place. During the transaction stage, sellers followed the randomization scheme and conducted door-to-door sales in the three sample communes. At the end of each transaction, sellers completed a self-administered survey in which they noted the questions buyers asked during transactions, as well as answered subjective questions about buyers' politeness, confusion, and suspicion. In total, sellers conducted transactions with 1,458 respondents.

Three to five days following the transactions in each district, enumerators conducted an endline survey with the sample. Endline surveys were always done by different enumerators than who performed the original transactions, in order to minimize social desirability bias for questions about seller quality and competence, as well as to avoid awkwardness of being surveyed by someone previously associated with a business deal. Of the 1,458 buyers who participated in transactions, enumerators conducted the endline survey with 1,422 respondents.¹⁰

Critically, the endline survey included questions that measured buyers' political connections. Enumerators asked respondents about family, friends, and personal experience working at a variety of state institutions, including national government, councils, courts, and the police.¹¹ I code respondents as politically connected if they report a connection. This follows from the understanding that in Senegal, possessing any political connection can improve enforcement probability relative to unconnected citizens, as even low-level connections can help grant access

¹⁰Appendix Table A.2 shows that treatment arm does not predict differential rates of endline attrition. Covariates for missing respondents at endline were imputed using sample means; results throughout are robust to excluding these missing respondents.

¹¹Appendix Table A.13 shows that buyers' to whom sellers had signaled political connections do not over-report their own political connections.

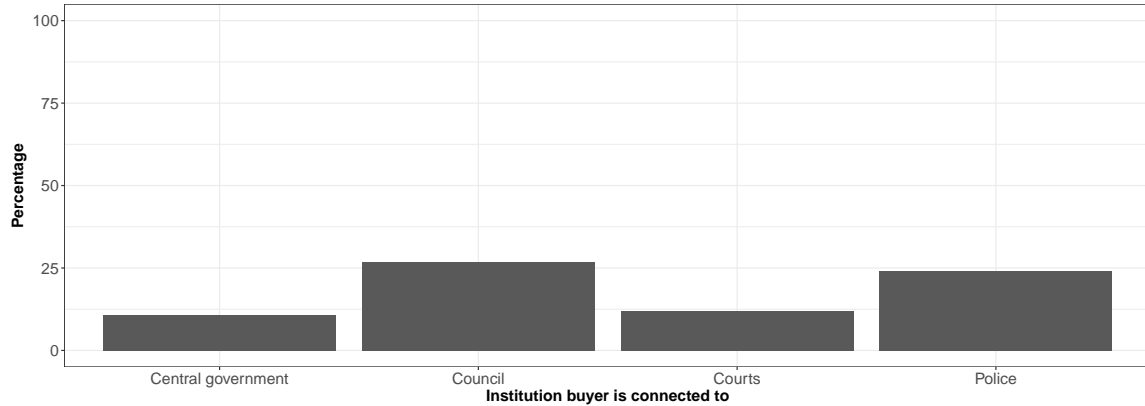


Figure 1.3: Percentage of respondents with connections

to otherwise hermetic institutions. Figure 1.3 shows the percentage of the sample reporting political connections. The endline survey also included questions about buyers' motivations for accepting or rejecting the deal, their perception of sellers' political connectivity, their past business transactions and use of formal contracts and courts, and basic descriptive covariate information including gender, education, ethnicity, and religion.

Randomization

I implemented a block randomized design whereby six geographically sequential sample households constituted a block, and all six treatment groups were represented in that block. Blocks were thus essentially micro-neighborhoods, similar in both observable and unobservable street-level variation. To minimize the risk of spillovers between buyers, enumerators ensured a distance of at least 50 meters between households.¹² Enumerators offered the deal to only one person per household, in order to avoid within-household spillovers.¹³ With 486 sample households

¹²Only 1.6% of respondents reported telling someone farther away than a next-door neighbor about the deal, which, coupled with the rapid succession of transactions, is highly unlikely to have caused spillover effects.

¹³The household limit was explained to respondents as an administrative constraint due to the initial roll-out phase of the business.

in each of the three sample communes, the total sample consisted of 1,458 buyers.¹⁴

Measurement of primary outcomes

The primary outcome at the transaction stage of the experiment was the level of phone credit that the buyer purchased, if at all. Sellers offered phone credit at competitive rates to incentivize acceptance of the deal, ensuring sufficient take-up to avoid floor effects. The rates PAPS offered were comparable to the discounts regularly promoted by the wireless company itself, with a key difference that PAPS' "bonus" credit was of higher quality: while the wireless company's bonus credit is not eligible for transfers, subscription purchases, or internet access, the bonus credit that PAPS offered was as good as regular mobile credit and thus highly desirable. The discounted rates did not raise buyers' suspicions, because they aligned with expectations for this market in Senegal; the only novel feature was the higher-quality bonus credit, a believable promotion in the competitive phone credit market.

Buyers could choose from three purchase option levels that were designed to be increasing in risk. First, to receive credit nearly instantly, buyers could pay 700 CFA and receive 1000 CFA worth of credit. At this level of purchase, the primary risk involved was that the bonus credit delivered was of lower quality than sellers had promised. Second, to receive a greater amount of credit (1500 CFA) at a cheaper price (500 CFA), buyers could opt for a second—and riskier—level, for which credit delivery would occur three days after the transaction took place.¹⁵ This level naturally required a greater amount of buyer trust in sellers, and attempted to mimic the typical hold-up problems in modern markets. While at first glance this delayed de-

¹⁴During an initial screening step, over 99% of respondents said they had a cell phone and were interested in cheap phone credit, and thus these logistical constraints are unlikely to affect the interpretation of results.

¹⁵The difference in cost between the first and second levels was decided after extensive piloting; the framing of "less money for more credit" was rhetorically useful for inducing respondents to seriously consider the risk of the second level.

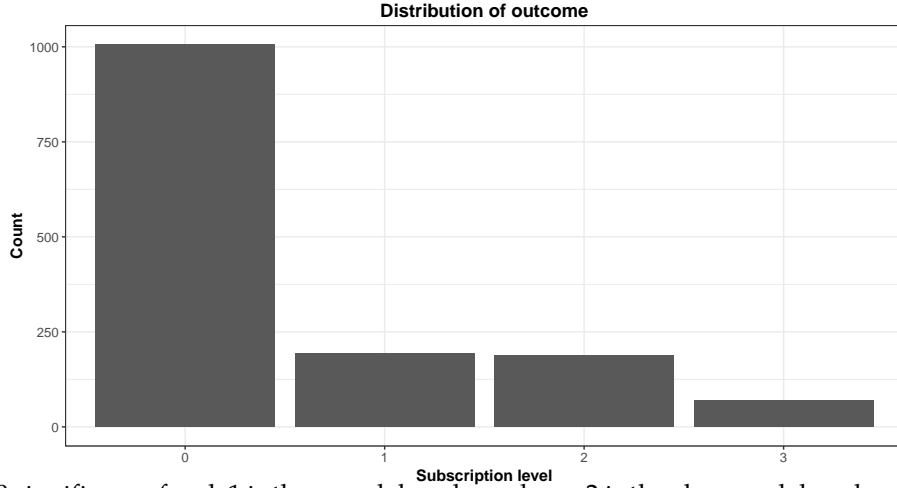
Categorization	Level	Cost	Credits received	When phone credit arrived
Declined deal	0	-	-	-
No delay	1	700 CFA	1000 CFA	A few minutes
Lower-risk delay	2	500 CFA	1500 CFA	In 3 days
Higher-risk delay	3	1000 CFA	3000 CFA	In 3 days

Table 1.3: Main outcome: Phone credit purchase levels

livery may have seemed odd to buyers, sellers explained that the delay was due to administrative processing requirements that were part of the business model which enabled these competitive rates. These types of terms were not new to most buyers, the majority of whom had participated in similar sales with delay in the past.¹⁶ The third and final purchase option available to prospective buyers attempted to further increase the risk; this level required the largest amount of money (1000CFA) in order to receive the most phone credit (3000CFA), again with a three-day delay. The per capita daily income in the sample communes is approximately 1500 CFA (~3 USD), so the costs of these options were non-marginal to respondents. I code the outcome using a four-point scale, as shown in Table 1.3, that corresponds to the level of purchase selected.¹⁷ Approximately 30% of the sample purchased some level of phone credit; Figure 1.4 shows the distribution in outcomes.

¹⁶The piloting prior to the experiment helped to ensure that the levels of the deal struck the correct balance of competitiveness and risk, as well as ensured that respondents were not taken aback by the nature of the delayed delivery.

¹⁷I present dichotomous codings for purchased at all and purchased with delay in Appendix Section A.9).



Note: 0 signifies a refusal, 1 is the non-delayed purchase, 2 is the cheaper delayed purchase, and 3 is the expensive delayed purchase.

Figure 1.4: Distribution of purchase levels from buyers.

Estimation

I estimate average treatment effects with the following fully saturated OLS specification:¹⁸

$$\begin{aligned}
 y_i = & \alpha + \beta_1 \text{connection}_i + \beta_2 \text{required contract}_i + \beta_3 \text{optional contract}_i \\
 & + \beta_4 (\text{connection}_i \times \text{required contract}_i) + \beta_5 (\text{connection}_i \times \text{optional contract}_i) \\
 & + \gamma \mathbf{X}_i + \eta_b + \theta_e + \epsilon_i
 \end{aligned} \tag{1.1}$$

where y_i is the purchase level chosen by respondents, \mathbf{X}_i is a matrix of covariates, η_b are randomization block fixed effects, and θ_e are enumerator fixed effects. To estimate the marginal effect of each treatment arm, I remove the interaction terms. To estimate heterogeneous effects, I interact the relevant covariate with the treatment terms. All tests in the paper are two-sided unless registered in the pre-analysis plan as one-sided.

¹⁸Results are robust to using an ordered probit model to account for the non-continuous dependent variable (see Appendix Section A.6).

Table 1.4: Buyer belief of seller connections driven by connection signal

	Outcome: seller is connected
Connection signaled	0.207*** (0.020)
Control mean	0.169
Control outcome std. dev.	0.378
Fixed effects	Yes
Controls	Yes
Observations	1,458
<i>Note:</i>	* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Notes: The outcome is a dichotomous coding of the question “Do you think the seller is connected to those in political power?” where the affirmative is coded as 1. The specification is estimated using OLS, and includes randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Randomization validation and manipulation check

As a heuristic for the randomization procedure’s success, I estimate equation (1.1) using individual covariates to show that respondent-level traits do not predict treatment assignment. As shown in Appendix Table A.1, there is balance on these covariates across treatment groups. The two-sided joint F -test of the restriction that each treatment group is indistinguishable from the others was rejected at the 10% level in only one case.

Important for inference is that the political connection signal successfully induced buyers to believe that sellers were connected. To that end, the endline survey included questions about buyers’ beliefs of sellers’ political connectivity. Table 1.4 shows that respondents in the connection signal group were 20.7 percentage points more likely to believe that sellers were politically connected, suggesting that the political connection signal was indeed transmitted effectively.

1.5 Results

To test the aggregate effect of political connections and formal contracts on exchange, I first estimate average treatment effects. I then take account of buyers' political connections to examine the role of political power asymmetries in exchange, and how they affect the utility of formal contracts.

The impact of political connections and formal contracts on exchange

What is the overall impact of signaling political connections and offering formal contracts—as required and optional parts of the deal—on propensities to trade? I estimate average treatment effects (ATEs) using both saturated and marginal-effect models. To rule out competing theories of social enforcement, I estimate all models with interactive controls between treatment groups and an indicator for buyer-seller co-ethnicity or co-religiosity, which in Senegal are the dominant informal social institutions for enforcement (Cruise O'Brien 1971; Koter 2013; Gottlieb 2017). In this experiment, approximately 23% of transactions occurred between buyers and sellers of the same ethnic or religious group.¹⁹ The models also control for covariates that could affect acceptance of the deal, including age, education level, employment status, whether the buyer was a student, and gender. Results remain substantively unchanged throughout when excluding these controls.

Table 1.5 shows that the impact of political connections was consistently negative. Although noisy, this offers suggestive support that sellers' political connections can stifle exchange by enhancing moral hazard. However, these results do not take into account buyers' political connections, and might thus obscure important

¹⁹The seller team represented all of the main ethnic and religious groups in Senegal.

Table 1.5: Average treatment effects

	Outcome: Purchase level			
	Unpooled		Pooled	
	(1)	(2)	(3)	(4)
Political connection signal	-0.074 (0.081)	-0.057 (0.052)	-0.082* (0.061)	-0.057 (0.051)
Required contract	0.165** (0.084)	0.197*** (0.063)	0.138* (0.072)	0.174*** (0.054)
Optional contract	0.053 (0.085)	0.045 (0.064)		
Political connection signal × required contract	0.064 (0.109)		0.072 (0.094)	
Political connection signal × optional contract	-0.016 (0.108)			
Control outcome mean	0.515	0.515	0.529	0.529
Control outcome std. dev.	0.871	0.871	0.895	0.895
Fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,458	1,458	1,458	1,458

Notes: Each specification is estimated using OLS. The outcome is the level of purchase chosen (0 to 3). Specifications include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status, and interactive controls between treatments and buyer/seller co-ethnicity/co-religiosity. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests and pre-specified one-sided tests.

variation that forms around asymmetric political power. In the next section, I incorporate buyers' levels of political connections to assess if asymmetries in political connections impacted confidence in exchange.

The results in Table 1.5 also paint an interesting picture of the role of formal contracts. The reported estimates show that offering a formal contract substantially boosted confidence in exchange in the aggregate. However, these results only obtained when the contract was a required part of the deal, not when buyers could opt for the contract. These effects are particularly pronounced in the higher-powered marginal effect estimation (model 2 in Table 1.5), where the required formal con-

tract treatment arm represented a 38.2% increase in the outcome over the control group. The substantial effect of the required contract treatment arm vis-à-vis the optional contract treatment arm suggests that formal contracts in these environments work best when the seller demonstrates self-constraint as an inherent part of the deal. Considering that all models control for predictors of social enforcement, these results suggest that formal contracts in Senegal have additional utility to buyers beyond social considerations. Taken as a whole, these results show that formal contracts can indeed boost trust in exchange, even in low-income countries with weak norms of enforcing property and contracting rights.

In the optional contract treatment group, approximately 40% of buyers paid the additional fee for the contract, though this was not driven by the political connection treatment (see Appendix Section A.7). Thus, while there does seem to be value placed on formal contracts such that a sizable portion of respondents were willing to pay extra to have them, this was not driven by concerns of sellers' political connections. As enumerators mentioned the optional contract toward the end of transactions, and as the results show that this did not move people to trade, the evidence suggests that this treatment arm was conceptually similar to not including a contract at all. Moving forward in the paper, I pool the optional contract treatment group with the control group to improve statistical power. Models (3) and (4) in Table 1.5 include these pooled results. Results remain substantively similar when pooling or dropping the optional contract group.

Turning to the models' interactive terms, the results are inconclusive: though formal contracts appear to mitigate some of the distrust that political connections induce, the estimates are too noisy to conclude complementary effects. This stands in contrast to previous work that has shown that social enforcement can substitute for formal enforcement, though as I have argued above, political connections affect exchange through different channels than social enforcement mechanisms. I parse

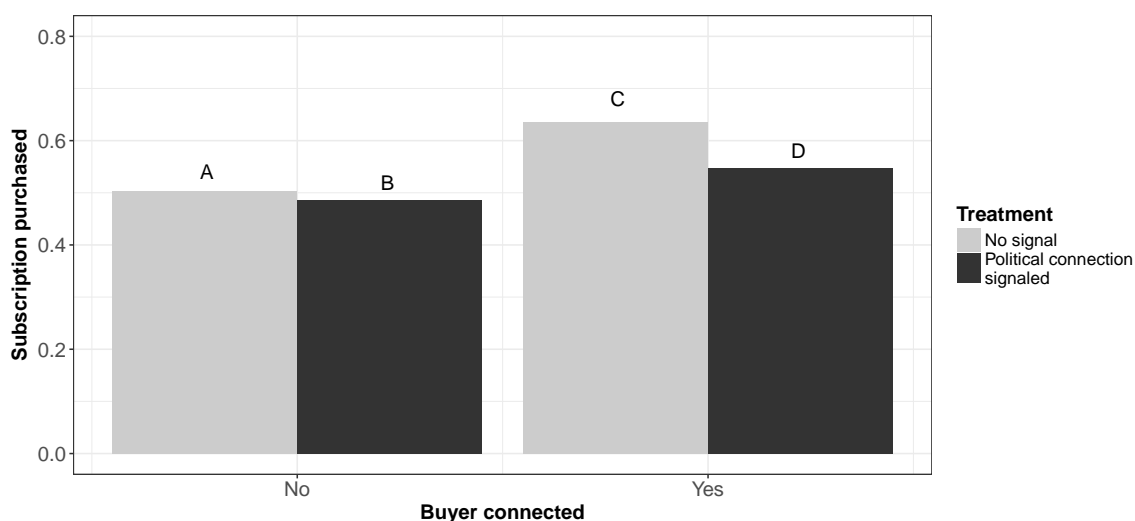
the relationship between connections and contracts more deeply in Section 1.5 by taking into account buyers' political connections.

Imbalances in buyer-seller political connections affect exchange

Asymmetric political power between buyers and sellers implies differential access to the state, and, as a consequence, unequal privileges for contract enforcement. The theory outlined in this paper suggests that we should thus expect to observe differences in rates of exchange as a function of imbalances in the trading dyad. It is therefore necessary to take buyers' levels of political connections into account. To test whether unequal political power dynamics between buyers and sellers affected rates of exchange, I estimate heterogeneous treatment effects by interacting the political connection treatment with buyers' political connections. Of course, buyers' political connections were not randomized as part of the experiment, and these connections may be indicative of other traits that are also associated with propensities to trade. However, as I show in Appendix Section A.11, buyers' political connections are not strongly correlated with education or employment, among other variables, and the results throughout the paper are robust to including interactive treatment controls for these potential confounders. This lack of correlation makes sense in Senegal, where possessing connections is not necessarily a signal of other forms of privilege such as wealth; this is especially true in the middle-to-lower class neighborhoods where I implemented the field experiment. Furthermore, by using block fixed effects—where blocks are essentially micro-neighborhoods in which there is little household variation in confounders such as income—I attempt to minimize the risk of confounding interpretations of heterogeneous treatment effects.

How do imbalances in political power affect trade? Figure 1.5 presents decomposed results by buyers' and sellers' political connections.²⁰ Below the figure are

²⁰Appendix Table A.5 presents these results in table form.



Panel A: Group means		Mean (std. dev.)
A. Unconnected seller and unconnected buyer ($n = 346$)		0.503 (0.879)
B. Connected seller and unconnected buyer ($n = 362$)		0.486 (0.875)
C. Unconnected seller and connected buyer ($n = 360$)		0.636 (0.958)
D. Connected seller and connected buyer ($n = 354$)		0.548 (0.871)
Panel B: Difference tests		Estimate (std. error)
Effect of connection signal for connected buyers (D–C)		-0.164 (0.083)**
Effect of connection signal for unconnected buyers (B–A)		-0.013 (0.084)
Most powerful buyer – least powerful seller (C–B)		0.127 (0.092)*
Connected buyer – unconnected buyer [C+D]–[A+B]		0.076 (0.053)*
Difference-in-differences [D–C]–[B–A]		-0.126 (0.099)

Notes: Panel A presents group means of the four subgroups along and their standard deviations. Panel B presents differences and standard errors estimated using OLS with linear restrictions. Difference tests include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status, and an interaction between treatment and buyer/seller co-ethnicity/co-religiosity. The outcome is the level of purchase chosen (0 to 3). * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests and pre-specified one-sided tests.

Figure 1.5: Outcomes by buyer and seller connection

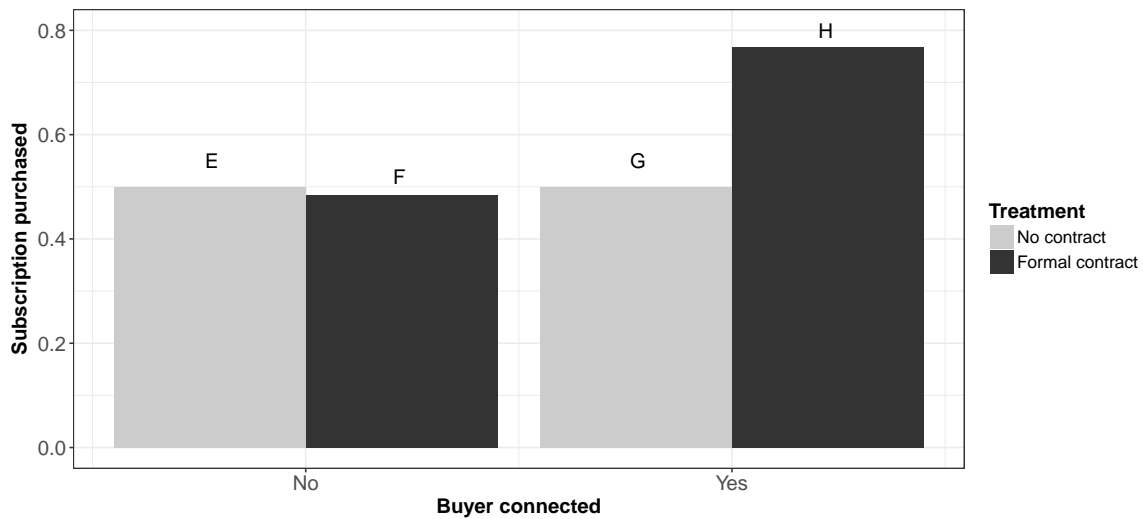
the means of each subgroup, as well as difference tests from linear restrictions on Equation 1.1. I present raw means and standard deviations in Panel A, and in Panel B present the covariate-adjusted differences and standard errors, using the same fixed effects and controls as earlier models. As Figure 1.5 shows, most likely to trade were connected buyers purchasing from unconnected sellers. These buyers had greater political power and recourse options if the deal went awry, and were

less likely to experience enforcement problems against the seemingly unconnected sellers. This subgroup was thus the least at-risk of buyers. When the seller and buyer were both connected, however, there was a sizable decrease in acceptance of the deal, in line with the expectation that options for recourse diminish when one's trading partner is politically connected. This drop-off was less pronounced among unconnected buyers. In line with expectations, the group least likely to exchange were unconnected buyers doing business with connected sellers; these buyers perceive the most risk because sellers' outsized political connections diminish the probability of successful enforcement should it be necessary.

Overall, non-connected buyers were less likely to purchase the deal than connected buyers. This could be due to uncertainty: the lack of the seller's *signal* of political connections does not mean the seller was unconnected. Those without connections are less likely to successfully distinguish connected parties from non-connected ones; the political symmetry or lack thereof is less certain than for connected buyers. The difference in the treatment effect between connected and unconnected buyers (the difference-in-differences) is relatively large though statistically insignificant. As with the ATE estimates, the heterogeneous results by buyers' connections include interactive treatment controls for co-ethnicity/co-religiosity and thus suggest that political connections are operating through alternate channels.

Formal contracts only protect connected buyers

The ATE estimates in Section 1.5 showed that offering a formal contract increased the probability of exchange overall. But how do formal contracts operate in the context of important political connections? In a world where the ability to enforce contracts is biased towards the politically connected, buyers may differentially value formal contracts based on their level of political connectivity. To test this claim, I estimate the impact of the formal contract treatment arm by buyers' political con-



Panel A: Group means		Mean (std. dev.)
E. No contract and unconnected buyers ($n = 479$)		0.499 (0.875)
F. Formal contract and unconnected buyers ($n = 229$)		0.485 (0.882)
G. No contract and connected buyers ($n = 465$)		0.499 (0.856)
H. Formal contract and connected buyers ($n = 249$)		0.767 (0.997)

Panel B: Difference tests		Estimate (std. error)
Effect of formal contract for connected buyers (H–G)		0.263 (0.085)***
Effect of formal contract for unconnected buyers (F–E)		0.068 (0.078)
Difference-in-differences [H–G]–[F–E]		0.183 (0.102)*

Notes: Panel A presents group means of the four subgroups along and their standard deviations. Panel B presents differences and standard errors estimated using OLS with linear restrictions. Difference tests include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status, and an interaction between treatment and buyer/seller co-ethnicity/co-religiosity. The outcome is the level of purchase chosen (0 to 3). * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests and pre-specified one-sided tests.

Figure 1.6: The effect of contracts given buyers' connections

nections.

Figure 1.6 presents these results, which reveal an interesting pattern: the effect of formal contracts on propensity to trade was driven almost entirely by connected buyers.²¹ Among unconnected buyers, formal contracts had essentially no impact. These findings suggest that, while formal contracts may improve confidence in exchange, they do so only for a particular subset of the population: those who can be

²¹I provide the corresponding model output in table form in Appendix Table A.5.

confident in their ability to sway enforcement in their favor during disputes. Formal contracts thus do not serve to enhance the recourse options or protect those who are otherwise powerless; they may be a viable enforcement solution only for those who are already privileged.

Alternative hypotheses and robustness

No evidence of social enforcement via in-group bias or findability mechanisms

Shared social identity has been shown to reduce transaction costs, which could facilitate trade (Besley, Coate and Loury 1993; Grimard 1997; Sanchez de la Sierra 2018). There is therefore a possibility in the field experiment that social factors altered buyers' expectations of contract enforcement. Buyers with similar social networks to sellers—in Senegal proxied by shared ethnic group or religious affiliation—may have experienced a greater sense of confidence and security in the deal compared to out-group members. Furthermore, the treatment arms could have interacted with the social mechanisms in ways that affected perceived enforcement probabilities. For example, while political connections may be off-putting to buyers in the aggregate, they may be seen as valuable if the seller who has them belongs to the same in-group network. As the estimates in the preceding sections show, however, results are robust to interactive treatments with co-ethnicity and co-religiosity. The heterogeneous treatment effects in Appendix Table A.9 similarly show that ascriptive social enforcement mechanisms do not drive results.

Findability mechanisms may also improve perceptions of social enforcement (e.g. Besley 1995; Miguel and Gugerty 2005; Habyarimana et al. 2007). In the business environment, knowing where to find a trading partner might lower perceived risks of contract breach and thus increase willingness to trade. During transactions, sellers stated the specific council at which they had worked. I am thus able to test

the findability hypothesis by creating a district match variable when the council to which the seller was connected matched the buyer's home district. As the estimates in Appendix Table A.10 show, enumerators' enhanced findability did not have an impact on exchange.²²

Addressing confounding interpretations of political connections

A potential concern is that by signaling political connections, traders transmitted information about their quality rather than induced considerations about the probability of contract dispute and enforcement. Questions in both the transaction stage and the endline survey attempted to measure the validity of this concern. First, at the end of each transaction, sellers filled out a short survey in which they recorded whether buyers asked follow-up questions, as well as their subjective measures of buyers' levels of suspicion and politeness. Second, the endline survey asked buyers about their perceptions of sellers' quality and trustworthiness.²³ I regress these measures of perceived quality on the treatment indicators, and present the results in Table 1.6.

The findings show that treatment did not drive respondents' opinions of sellers' quality, nor did sellers sense a differential level of suspicion or politeness based on treatment status. However, buyers asked a higher number of follow-up questions in both the connection and contract treatment groups. Examining the nature of these questions more closely, the main types of questions asked about the contract were logistical, such as where to sign and date, as well as some questions regarding the terms of the contract. Questions related to the political connection arm were typically about the nature of sellers' work at councils and whether they were still

²²Interactions with an indicator for the buyers' political connections similarly do not yield significant results.

²³To reduce social desirability bias, the endline survey was conducted by different enumerators than the transaction phase.

Table 1.6: Quality measures from buyers and sellers

	Buyer's perception of...				Seller's perception of...					
	Seller's quality		Trustworthiness		# of questions asked		Buyer's politeness		Buyer's suspicion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Connection signal	-0.037 (0.027)	-0.050 (0.034)	-0.027 (0.043)	-0.051 (0.052)	0.051** (0.024)	0.077*** (0.030)	0.030 (0.037)	0.021 (0.045)	0.052 (0.059)	0.066 (0.072)
Contract	0.027 (0.029)	0.008 (0.041)	0.063 (0.045)	0.028 (0.064)	0.036 (0.026)	0.075** (0.036)	-0.022 (0.039)	-0.036 (0.055)	-0.029 (0.063)	-0.008 (0.089)
Connection signal × contract		0.039 (0.058)		0.070 (0.091)		-0.079 (0.051)		0.027 (0.078)		-0.041 (0.125)
Control outcome mean	3.603	3.603	2.485	2.485	0.952	0.952	3.476	3.476	0.884	0.884
Control outcome std. dev.	0.540	0.540	0.827	0.827	0.753	0.753	0.940	0.940	1.25	1.25
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,458	1,458	1,458	1,458	1,458	1,458	1,458	1,458	1,458	1,458

Notes: Each specification is estimated using OLS. Specifications include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

based there. The sum of evidence suggests that treatment effects were not driven by concerns over quality or competence.

1.6 Conclusion

In areas where rule of law is selectively enforced, political connections can produce moral hazard in exchange. Using evidence from a field experiment in a naturalistic trading environment, this article demonstrates that asymmetric political connections can affect basic forms of exchange. By showing that political connections can prevent exchange where it would otherwise occur, this study suggests that research that observes outcomes conditional on trade may be clouded by sample biases and excessive focus on the intensive rather than extensive margin. And while existing work focuses on ascriptive predictors of social enforcement such as co-ethnicity, I show that political connections can explain patterns of trade even when accounting for social enforcement. The findings of this paper suggest that low-level political connections of both sellers and buyers merit consideration for understanding patterns of private-sector growth in developing countries.

This paper also provides causal evidence that state-backed formal contracts can boost confidence in trade, even in an environment of weak norms of rule of law and contract enforcement. Upon closer inspection, however, these results also highlight fundamental inequalities in developing democracies with uneven rule of law: formal contracts do not protect all buyers equally. Rather, formal contracts primarily protect the claims of the politically powerful. This paper thus implies the limits of ad hoc legal solutions in the presence of broader political inequalities. Paradoxically, increasing the availability of formal contracts may intensify economic inequalities and market segmentation.

This project was an initial step in identifying the impact of political connections on daily types of economic exchange in modern developing markets. Future work would benefit from examining different types of markets and connections in order to form a unified theory across firms, individuals, and sectors. As bureaucratic organizations determine much of citizens' economic and political lives, better understanding the determinants of access to these institutions may help further illuminate inequalities in development. As emerging markets continue to develop, concerns over seller moral hazard are only likely to grow. Understanding how informal influences like political connections interact with state institutions for enforcement will thus be particularly important for private-sector growth in the coming years.

I argue that the theory and findings of this paper are likely to apply to contexts where enforcement institutions are weak and personal connections moderate access to the state. Indeed, these conditions characterize the bulk of the world's developing democracies. In societies where a state apparatus exists for enforcing property rights and contracts, and where business occurs at such a scale that social enforcement mechanisms alone are not viable, agents must use a mixture of formal and informal mechanisms for enforcing deals. This paper provides evidence for how informal networks of political influence in these places can impede the func-

tion of formal institutions in shaping private-sector economic development.

The role of asymmetric political connections in the private sector has distributive implications for ordinary citizens. When only the politically connected can contract with confidence, and when those without connections are averse to exchanges with moral hazard, distinct economic networks can develop around differently privileged groups. This results in suppressed overall levels of trade and inefficiencies in private markets. As marketplaces characterized by seller moral hazard continue to grow, the scale of this problem will grow as well. Significant opportunities for private-sector growth may either never come to pass or may further contribute to economic inequality. Breaking the connection between political connections and preferential enforcement will be essential for unlocking more efficient private-sector growth.

Social, Formal, and Political Determinants of Trade Under Weak Rule of Law: Experimental Evidence from Senegalese Firms

Abstract: When contracting institutions are weak or exploitable, firms in developing countries rely on a mixture of social and formal heuristics to select business partners. What is the relative importance of social and formal determinants of trade, and how do political considerations factor in firms' risk calculus? Politically connected partners can be at once risky and useful to firms: they can break contracts with relative impunity, but they also open access to lucrative markets. I implement a survey with a conjoint experiment among 2,389 formal and informal firms in Senegal. The results demonstrate the surprisingly large influence of formal predictors of exchange even in an overwhelmingly informal business environment, and also establish the countervailing effects of political connections on trade. This evidence suggests that firms in developing countries must contend with an intricate political calculus to ensure growth, thus complicating economic policy intended to

develop the private sector.

2.1 Introduction

Markets are embedded in social structures (Polanyi 1944; Granovetter 1985). An economy is never fully autonomous, but is subordinated to society and its organization, including politics, religion, and social relations. This is particularly true of developing countries, where, due to weak institutions for rule of law, informal influences and social relations permeate the core of state institutions. When conducting business in such contexts, firms rely on social heuristics to choose business partners in order to ensure smooth private-sector operations (North 1991). But such social influences coexist with state institutions, weak as they may be, and formal considerations like state-backed contracts also affect how firms conduct private-sector trade (Williamson 1985; North 1990; Poppo and Zenger 2002).

In the midst of these competing influences, markets can also be subordinated to the informal influences of asymmetric political power. When informalities pervade all corners of the marketplace, connections to the politically powerful can critically influence the enforcement of property rights and contracts: they lead to preferential treatment from the state, including the ability to break contracts with relative impunity, and thus ultimately affect economic behavior. There are numerous studies highlighting the value of political connections for firms in developing countries (e.g. Fisman 2001; Khwaja and Mian 2005; Faccio 2006), but the full equilibrium is less understood: How do firms' political connections affect how *other* firms perceive them, and does this in turn affect the likelihood of exchange? What role do political connections play in moderating the confidence that businesses have in their potential partners? These questions are important to address in order to mitigate trade losses stemming from politically induced risk. Concerns of this type are in-

creasingly salient as emerging markets develop, pitting traditional, often-informal forms of doing business against formal, state-related considerations.

This paper seeks to understand how this confluence of formal and social influences shapes modern markets in developing countries. Building on existing theory, I argue that when the state's institutions for rule of law are weak or selectively enforced, both social and formal factors can critically affect firms' decisions to conduct trade. Social mechanisms such as shared ethnic or religious networks lend greater security to deals by increasing the perceived likelihood of contract enforcement (Grimard 1997; Keefer and Knack 2002), particularly for firms in the informal economy that cannot rely on state institutions. At the same time, formal considerations like state-backed contracts similarly inspire confidence in trade by decreasing the perceived probability of contract breach (North 1990). This is especially likely to be the case for firms in the formal economy that can actually access the state's contract enforcement institutions.

This paper also puts forth a theory of the role of political connections in trade. Given that political influence enables the biased subversion of state institutions in contract disputes, I argue that the political connections of potential business partners pose significant risks. If a potential trading partner is able to break a contract with relative impunity, a firm is less likely to engage in exchange with this partner, unless the advantages of doing business with politically influential firms outweighs the risks of defection. These concerns are particularly relevant in exchanges with previously unknown business partners as well as in one-shot deals, both of which are increasingly common in modern, large-scale economies for which sustaining equilibrium forces like self-enforcing institutions and reputation costs are immaterial. In this context, political connections are a form of rent-seeking, in that they are economically inefficient and the benefits obtained for connected firms are purely distributional in nature. Political connections, I thus argue, can suppress overall

levels of trade when states selectively enforce rule of law.

To test these claims, I conducted a survey with a conjoint experiment among 2,389 firms in Senegal, a country in West Africa where social influences and formal state structures compete in a rapidly modernizing business environment. My sample included firms from both the formal and informal economies, in order to gain theoretical leverage from the different strategy sets and enforcement recourse options available by sector. From each firm, I sampled the employee who is most responsible for the firm's deals and contracts (most often firm owners themselves), and therefore actually holds decision-making powers when considering new business opportunities. The conjoint experimental framework enabled simultaneous testing of the theory's multiple observable implications, by presenting respondents with two hypothetical deals with randomized social, formal, and political profile attributes. Respondents selected which deal they were more likely to accept, as well as which deal they believed more likely to result in contract breach.

The results show that social, formal, and political considerations can all motivate firms' decisions to engage in business. Ethnic group and religious affiliation affected respondents' likelihood of trade overall, while co-religiosity—not co-ethnicity—increased the likelihood of deal acceptance. Formal considerations also motivated trade, even in this context of valuable social networks: respondents were much more likely to conduct business with firms when the deal included a formal, written contract. Political connections, meanwhile, both in the form of party affiliation and personal connections to those in power, decreased the likelihood of exchange. This was the case for all but the highest type of political connection: when potential partners were personally connected to the president, respondents were more likely to select the deal. This suggests that there exists a threshold at which dealing with the political connected becomes an asset.

Results also show that respondents' perceptions of contract breach inversely

correlated with their decisions to do business; that is, firm owners chose deals with partners they believed less likely to break contracts. Again an exception, however, were business partners who had the strongest type of personal political connection. Although these highly connected partners were perceived as significant risks of contract breach, respondents nevertheless sought deals with them due to the potential rewards: access to lucrative state markets and contracts. Thus, in some cases, it seems the potential advantages of dealing with the politically connected outweigh the risks of broken deals. This effect was driven by formal firms, not informal ones, in line with the fact that these potential rewards require formal status in Senegal. Examining differences between formal and informal firms more closely, the motivating factors to engage in trade align with expectations of available recourse options by sector. Because informal firms are unable to access formal institutions for enforcement, they place greater weight on social networks and less on formal factors when choosing business partners, relative to firms in the formal economy.

This paper makes several contributions to the literatures on political connections, economic development, and the formal and informal institutions underpinning property rights and contract enforcement. First, I develop a more complete picture of the political economy of political connections in economic exchange. While political connections confer enormous benefits to firms in developing countries, this paper demonstrates that there may be unintended consequences in the form of stifled exchange. The results also suggest, however, that there is a tipping point at which connections become so powerful that they dominate the private sector—the potential advantages of working with such powerful-yet-risky firms outweigh the costs of potential defection. This paper thus makes the case that firms in countries with weak rule of law must deal with a complicated political calculus to ensure their growth. Second, the results of this paper show that despite weak

rule of law, firms still seek out deals based on formal, state-backed contracts. Even in societies where markets are irrevocably embedded in social structures, the findings in this article suggest that formal institutions can still offset social and political risks. Finally, this project reached an important sample of both formal and informal firms at a substantial scale. This enables the causal examination of differing motivations and mechanisms of trade by the formal versus informal sector, with firm owners and managers who are actually responsible for their firms' business decisions.

2.2 Theory

What influences firms' decisions to take on new business partners in weak contracting environments? Above all, firms are most likely to conduct trade when they believe their deals to be secure. When the risk that a partner will break a contract is perceived to be high, firms are unlikely to make significant investments (e.g. Li and Resnick 2003, 185). Institutions—both formal and informal—that protect against defection can solve commitment problems in exchange, thereby facilitating trade and contributing to economic growth (North and Weingast 1989; North 1991; Acemoglu and Johnson 2005). Institutional solutions to the commitment problems in exchange fall into two broad categories: (1) those in which secure exchange emerges from social institutions that do not depend on a central state, and (2) those in which the state serves as the primary enforcement authority. In the following subsections, I examine the factors affecting risk perceptions and trade propensities for firms in developing countries, and use them to structure the paper's empirical design.

Social mechanisms for secure exchange

Turning first toward social mechanisms, a “dense social network of informal constraints” can lower transaction costs, boost confidence in exchange, and ensure a sustained enforcement equilibrium (North 1991, 99). Greif (1989, 1993) uses the example of 11th century Maghribi traders to show that a lasting enforcement equilibrium in trading markets emerged via a reputational mechanism based on honoring contracts and punishing defectors. Other historical evidence from medieval Europe shows how merchant guilds enabled secure exchange prior to the emergence of the state via similar reputation and sanctioning mechanisms (e.g. Milgrom, North and Weingast 1990; Greif, Milgrom and Weingast 1994). But there are many commercial markets where such self-enforcing institutions are weak or non-existent, which may be in part due to the competing presence of an existing state. And when the state does not adequately protect property rights, these self-sustaining mechanisms often constrain growth to the scale of “flea market economies” (Fafchamps and Minten 2001a). Unlike the historical examples described above, much of the modern world is characterized by hybrid democracies in which some state apparatus to enforce contracts exists, even if its institutions selectively favor certain citizens (Diamond 2002).

Repeated interactions also play a vital role for secure exchange via social mechanisms. Folk theorem results based on repeated play show how relationships over the long term lead to stable equilibria (e.g. Fudenberg and Maskin 2009), even in the absence of third-party enforcement. Related work on incomplete contracts demonstrates how contract enforcement can arise via relational contracts based on repeated interactions (e.g. Baker, Gibbons and Murphy 2002; Brown, Falk and Fehr 2004). However, modern markets increasingly involve one-shot exchanges—with partners for whom reputational information is scarce or too costly to accrue—in

which a sustained equilibrium based on repeated interaction is not an option by definition.

Another form of social enforcement that has received much attention, particularly in Africa, stems from shared identity, often based on ascriptive features such as ethnicity or religion. In-group enforcement can operate via several channels, including through risk-sharing mechanisms, shared tastes and preferences, shared enforcement technologies, and common behavioral patterns (Grimard 1997; Habyarimana et al. 2007). Co-ethnics may also interact more frequently and be better placed to identify each other's type, which increases opportunities for sanctioning in cases of defection (Fearon and Laitin 1996). Combined, these mechanisms can enable secure exchange among in-group members both by decreasing the probability of defection and by making punishment after defection more probable (Besley, Coate and Loury 1993; Miguel and Gugerty 2005). Experimental evidence from the DRC confirms this, showing that in environments with weak rule of law, co-ethnicity smooths trade and decreases contract defection (Sanchez de la Sierra 2018). This type of in-group enforcement is likely to extend beyond ethnicity to other salient, identity-based cleavages such as religion. I hypothesize that members of shared social groups are less likely to fear defection without recourse, and are thus more likely to exchange with one another.

Formal mechanisms for secure exchange

Formal solutions to commitment problems in exchange involve the state as the third-party enforcement mechanism. The state protects property rights and enforces contracts for private-sector exchange (Barzel 1997; Acemoglu and Johnson 2005). Assuming a certain threshold of state strength, contracts reduce the transaction costs of trade and also allow for riskier exchanges to occur (North 1981; Williamson 1985). There are two broad mechanisms by which formal contracts

might boost confidence in trade. First, legal explanations are the most common argument for the utility of formal contracts: contracts establish proof an exchange occurred, explicitly set the terms of a deal, and clarify recourse options in the case of breach (e.g. Williamson 1985; Hart 1995). Second, there may also be a signaling effect of formal contracts: by virtue of offering a formal contract as part of a deal, a business partner signals something positive about their type.¹ Empirically, evidence confirms that even in environments with weak contracting institutions, formal contracts can boost levels of trade (Li, Poppo and Zhou 2010; Sanchez de la Sierra 2018; Bhandari 2019a). I thus hypothesize that state-backed contracts increase the likelihood of exchange, while their relative importance vis-à-vis social considerations remains an empirical question.

How political connections shape exchange

In market contexts where the social and the formal intermix, how do a potential business partner's political connections influence willingness to trade? I argue that political connections impact fundamental considerations of risk and deal security, and thus affect firms' decisions to engage in exchange. However, existing theory does not give clear predictions for the direction in which a potential partner's political connections should impact trade.

On the one hand, firms may be hesitant to conduct trade with politically connected businesses. In many developing contexts, personal connections to people in power result in privileged access to and treatment from state institutions (e.g. Hicken 2011; Holland 2016; Post 2018). These connections can be invaluable in states that have limited capacity to serve the whole population, or in states where

¹It is possible that formal contracts have a countervailing effect, however, if politically connected firms are able to use formal contracts as an additional channel of political influence, given the inherent dependence of such contracts on the state. However, existing empirical evidence shows that formal contracts are unlikely to negatively affect trade, even when the potential trading partner is politically powerful (Bhandari 2019a).

administrative procedures are prohibitively costly (either in money or in effort). As a result, politically connected firms enjoy significant advantages in private-sector exchange. During contract disputes, they benefit from the bias of the state when they break contracts and from the punishment capability of the state when they seek to enforce contracts (Lu, Pan and Zhang 2015). The result is that politically connected firms are able to break contracts with relative impunity. Non-connected firms thus have incentives to avoid conducting business with politically connected firms: why do business with firms that can break contracts without consequences? These disproportionate advantages should induce perceptions of risk of contract breach, and stifle trade with politically connected firms.

On the other hand, doing business with politically connected firms may confer significant advantages. Politically connected firms in developing countries have access to lucrative state contracts, have privileged access to capital, and benefit from a host of other political and economic advantages in the private sector (Fisman 2001; Khwaja and Mian 2005; Faccio 2006; Szakonyi 2018). These advantages are increasing in the level of connectivity, with presidential connections being the most lucrative, particularly in hyper-presidential regimes characterized by disproportionately powerful executive branches. For the firms that have them, these high types of connections increase economic opportunity within markets and judicial might within contracting institutions. At the expense of deal security, working with such firms could open access to these lucrative opportunities and potential rewards. Furthermore, developing partnerships with politically connected firms could be a launching pad for developing valuable political connections for one's own firm. Thus, political connections can serve contradictory roles—at once a trade risk and potential boon.

I argue that when the advantages are high enough, firms will conduct trade with politically connected businesses *even if* they think such businesses are most likely

to break contracts. Under what conditions will advantages outweigh risks? The higher the level of political connection, the greater the potential reward. Conducting business with firms whose owners are extremely well connected may appeal to businesses who hope to access these rewards, despite these partners' ability to break contracts with relative impunity. When partners' political connections are less powerful, firms are more likely to see the risks rather than the potential advantages of doing business; the risk of defection, even if lower than that of more politically powerful firms, outweighs the marginal potential benefits of dealing with these firms. I thus expect that firm owners will avoid deals with weakly connected firms and seek deals with the most politically powerful ones, despite believing that politically connected firms are more likely to break contracts.

The impact of firm formality

In developing countries where the informal economy dominates, the above claims can critically depend on the formality status of firms. Firms in the informal sector differ from those in the formal sector in several key aspects. First, informal firms do not have access to the same type of enforcement institutions as do formal firms, which may factor into their risk calculations when considering new deals and potential business partners. Due to legal requirements, informal businesses are often unable to use state institutions, including police and courts, to enforce their contracts. In the absence of formal means of enforcement, informal firms rely more on social heuristic devices when considering the risk of a given trade. Recourse via shared social networks in the case of contract disputes offers some protective insurance against risky deals for informal firms. I thus expect social factors such as ethnicity and religion to be more valuable to informal firms than formal firms. Formal considerations such as written contracts may still increase confidence in trade, however. For example, there is the signaling value of contracts as described

above, and formal contracts may also enable clearer *social* enforcement by providing written evidence of a trade with concrete terms. Thus, I expect informal firms to react positively to formal contracts in deals, though not as much as formal firms that actually have the means to enforce contracts using the institutions they were designed for.

Second, firms in the informal sector, vis-à-vis formal-sector firms, may view the risk of dealing with politically connected firms differently. The primary potential benefits of doing business with politically connected firms are access to lucrative state markets and preferential capital. But informal firms are unlikely to benefit from these rewards; state contracts typically set formalization as a requirement for firms' involvement, and formal lenders often restrict their capital to firms in the formal sector. Thus, informal firms are less likely to value high levels of political connections in their business partners. In fact, political connections in general could be a larger risk to informal firms, because firms in the informal sector are directly violating the law by their very existence. Especially during recent crackdowns on informal firms spurred by international finance institutions, these risks are particularly pronounced. This puts them at an extreme disadvantage during contract disputes with politically connected firms, especially firms that are connected to local officials who exert control over informal businesses and possess the authority to remove them. I thus hypothesize that firms in the informal sector are more likely than formal firms to resist deals with politically connected partners.

2.3 Context

For its stable yet biased institutional environment, its rapidly modernizing markets, and the nature of social networks in the country, Senegal is well suited for testing the competing roles of formal, social, and political determinants of modern,

private-sector exchange. Given the relative strength of its rule of law institutions compared to much of sub-Saharan Africa, it constitutes a relatively hard test of the influence of political factors in the business environment.

Contracting institutions and political connections

Senegal represents a stable, multi-party democratic environment in an otherwise turbulent region. Nevertheless, there are persistent issues in Senegal's legal institutions, which have been plagued by recurrent problems of low capacity and executive overreach (Thomas and Sissokho 2005; Bingham 2009; Kondylis and Stein 2018). The World Bank ranks Senegal poorly in terms of contract enforcement, and its legal institutions are based in French civil law, which in Africa is typically associated with high degrees of formalism, low efficiency, and weak rule of law (Joireman 2001; Djankov et al. 2003). As a result, enforcing contracts in Senegal can be prohibitively difficult.

Political connections help circumnavigate the high financial and time costs of accessing legal institutions and enforcing contracts in Senegal. Knowing a well-placed person within government helps to avoid the red tape associated with contract enforcement by enabling firms to get their feet in the door of relevant bureaucratic institutions. This in turn becomes a useful tool in the business environment. The survey data from this project reflects this reality. Approximately 60% of firm owners said that political connections are useful for business, and 53% believe that it is easier to break a contract if you are well connected. Only 21% of respondents report having complete confidence in the courts, with 29% reporting partial confidence. While low, Appendix Figure A.6 shows that Senegalese citizens have higher confidence in courts compared to citizens of most other African countries, where the influence of political connections in the business environment is likely to be even higher.

Business environment and disputes

Informal influences epitomize the business environment in Senegal. The Government of Senegal estimates that as much as 97% of the country's economic units are informal, and that the informal sector accounts for half of job-seekers and 31.7% of all GDP activity. Even for formal firms, informalities often dictate business operations. Approximately 83% of the formal firms in my sample report using informal, verbal contracts as part of their regular business dealings, with 31% identifying them as the contract type they use most often—despite only 5% stating that this is their preference.

Yet markets in Senegal are rapidly modernizing. After decades of low growth, the Government of Senegal implemented broad reforms in 2013 to develop its private sector. Driven by a simplified formalization processes and a technological boom that has inspired a new generation of entrepreneurs, there has been a surge in firm formalization in recent years. In many markets, firms are increasingly conducting exchange with newly entering businesses—businesses that have not yet established reputations, nor developed repeated trading relationships with existing firms. One-shot exchanges have increased in frequency as well, as specialized firms have grown in number as the economy has modernized. The result is that firms must increasingly rely on mechanisms outside of reputational considerations and repeated interactions to ensure secure operations.

Classic hold-up problems typify many deals that Senegalese firms make. While the precise nature of the modal deal differs by industry and sector, a typical deal involves purchasing goods or services without full knowledge of whether products are of promised quality—or whether they will be delivered at all.² Conversely,

²Depending on the sector, payments can be made at least in part upfront or after goods or services are provided. Moral hazard problems exist even when payment is made after delivery, however, as the quality of goods and services is often not apparent until much later.

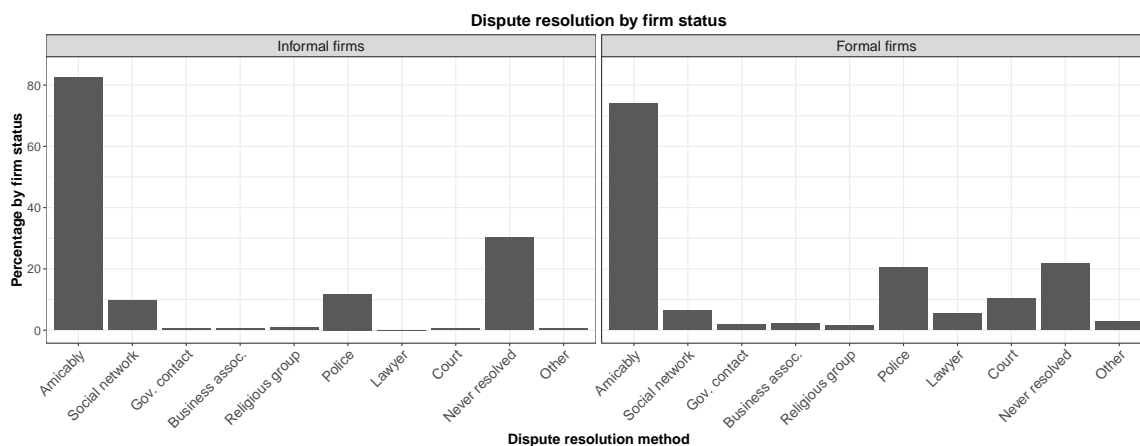
firms often provide goods and services and never receive the agreed-upon payments from their business partners. Disputes among firms are common in this context. Of this project's sample, 35% of formal firms and 30% percent of informal firms reported involvement in contract disputes. The most common causes were lack of payment by another company and the provision of substandard services or products by another company, at 74% and 19%, respectively. Figure 2.1 shows how sample firms, grouped by their formality status, resolved their contract disputes when faced with these holdup problems. Given the difficulty and expense of more serious forms of enforcement, most firms reported solving contract disputes amicably. However, informal firms relied more heavily than formal firms on social means of enforcement, including attempts to resolve disputes amicably or via the defector's social network. Although informal firms used the police to resolve some disputes—still at a rate lower than formal firms—virtually none used other means of formal enforcement such as lawyers or courts.³ Informal firms were also more likely to never resolve their contract disputes. Uncertain contract enforcement in a fraught business environment makes Senegal an apt context for examining the impact of formal and social determinants of exchange.

Social institutions in Senegal

Scholars of Senegal have focused extensively on identity-based social networks and their role in structuring daily life in the country. Senegal is an ethnically diverse country, and ranks near the median in sub-Saharan Africa according to most ethnolinguistic fractionalization rankings (e.g. Roeder 2001; Alesina et al. 2003; Fearon 2003). The dominant ethnic group are the Wolof, comprising some 43% of the population, followed by the Fula at 24%,⁴ Serer at 15%, and Diola and Mandingue

³This is true even for the wealthiest informal firms in the sample, whose profits rival those of the largest formal firms.

⁴Fula, Peul, and Toucouleur are considered members of the same ethnic group.



Notes: Of firms that have experienced a contract dispute (N=785), this figure plots the percentage that reported ever using the dispute resolution method indicated on the horizontal axis.

Figure 2.1: Methods of contract dispute resolution for Senegalese firms

at 4% (Bass and Sow 2006). However, ethnicity is not as politically salient in Senegal relative to other African countries (Posner 2004), and state institutions, particularly in Dakar, are not organized by ethnicity (Koter 2013). Even in rural Senegal, the importance of ethnic dynamics has recently been challenged (Wilfahrt 2018).

Religious affiliation is a more politically and economically salient identity in Senegal. More than 90% of the population identifies as Muslim, and the majority belong to Sufi brotherhoods that structure daily life in the country (Mbacké 2005). The largest brotherhoods, in descending order of membership, are the Tidjane, the Mouride, the Qadiriyya,⁵ and the Layenne. The Mouride brotherhood in particular is deeply entrenched in the state and society, and for many serves as the dominant source of local authority (Cruise O'Brien 1971; Villalón 1995; Gottlieb 2017). There is a high degree of deference to Mouride religious leaders (*marabouts*), who serve as strong enforcers of social order (Beck 2008). This deference to authority and high degree of centralization diffuses into the private sector; many Mouride entrepreneurs are linked in informal business networks in Senegal and throughout

⁵The Qadiriyya are located mostly in Eastern Senegal, and are less prevalent in Dakar, where I implemented this project. The conjoint design thus excluded this religious group. Indeed, only 21 of the 2,389 respondents reported membership in the Qadiriyya brotherhood.

the world (Ebin 1993; Golub and Hensen-Lewis 2012). In this trading environment, breaking a contract with another member of one's religious brotherhood carries large social costs, and thus co-religiosity, perhaps even more than co-ethnicity, serves as a meaningful predictor of secure trade.

2.4 Research design

To test the impact of social, formal, and political influences on private-sector exchange, I designed and implemented a survey with a conjoint experiment in Senegal in early 2018.

Sample

I conducted the survey in nearly all districts of Dakar, the capital of Senegal where the vast majority of economic activity in the country is concentrated, and its surrounding suburbs. I targeted firms in both the formal and informal economies. These are populations that are difficult to reach: large firms in the formal sector are often difficult to access, and informal-sector firm owners face incentives to keep low profiles and not participate in surveys. To ensure access and reduce perceptions of state affiliation, enumerators approached businesses with a letter of research approval from a well-known local research institution. I subdivided each district into sub-neighborhood zones, and enumerators followed a pre-determined sampling step that varied by sub-neighborhood. The sampling step was chosen to ensure sufficient distance between firms to minimize spillovers.

At each firm, enumerators requested to speak with the firm owner or the employee who had decision-making power for their firm's contracts and business deals.⁶ I intentionally over-sampled formal firms, for which there is wider variance

⁶When not the owner, this was typically the managing director, or the director of administration

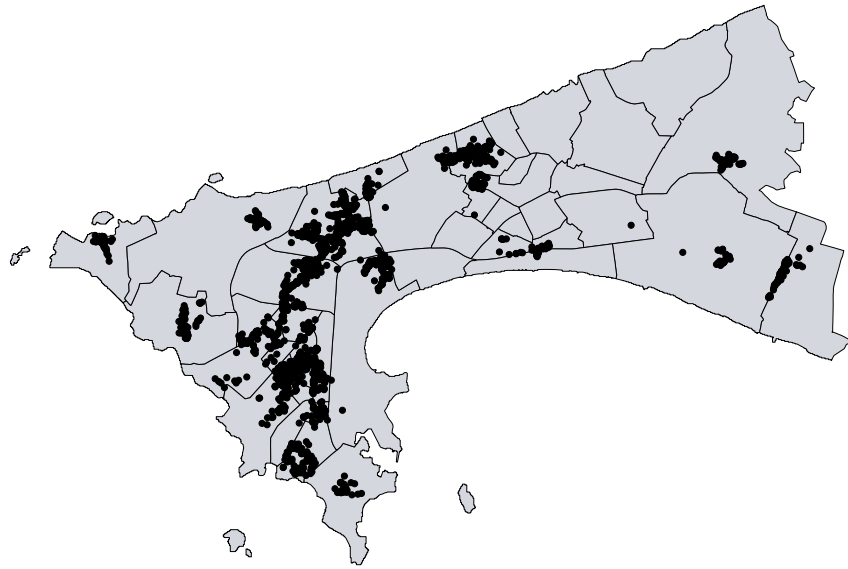


Figure 2.2: Dakar peninsula with district borders, and locations of sample firms in both industry and scale of operations compared to informal firms. The final sample totaled 2,389 businesses, with 1,582 formal firms and 807 informal firms. Figure 2.2 shows the location of sample firms in Dakar and its suburbs.

The distributions of firm wealth and size by formality status are presented in Figure 2.3. Sample firms in the formal economy were wealthier and larger overall than informal firms. Though the majority of economic units in Senegal are informal, they tend to be much smaller in scale—many are individually operated—relative to formal firms. This in part due to a threshold at which, due to scale, firms become more visible to the state and are compelled to formalize. Indeed, 46% of formal firms in the sample began as informal businesses. Appendix Table B.1 presents additional summary statistics for the sample, which generally skews young, educated, and male.

and finance.

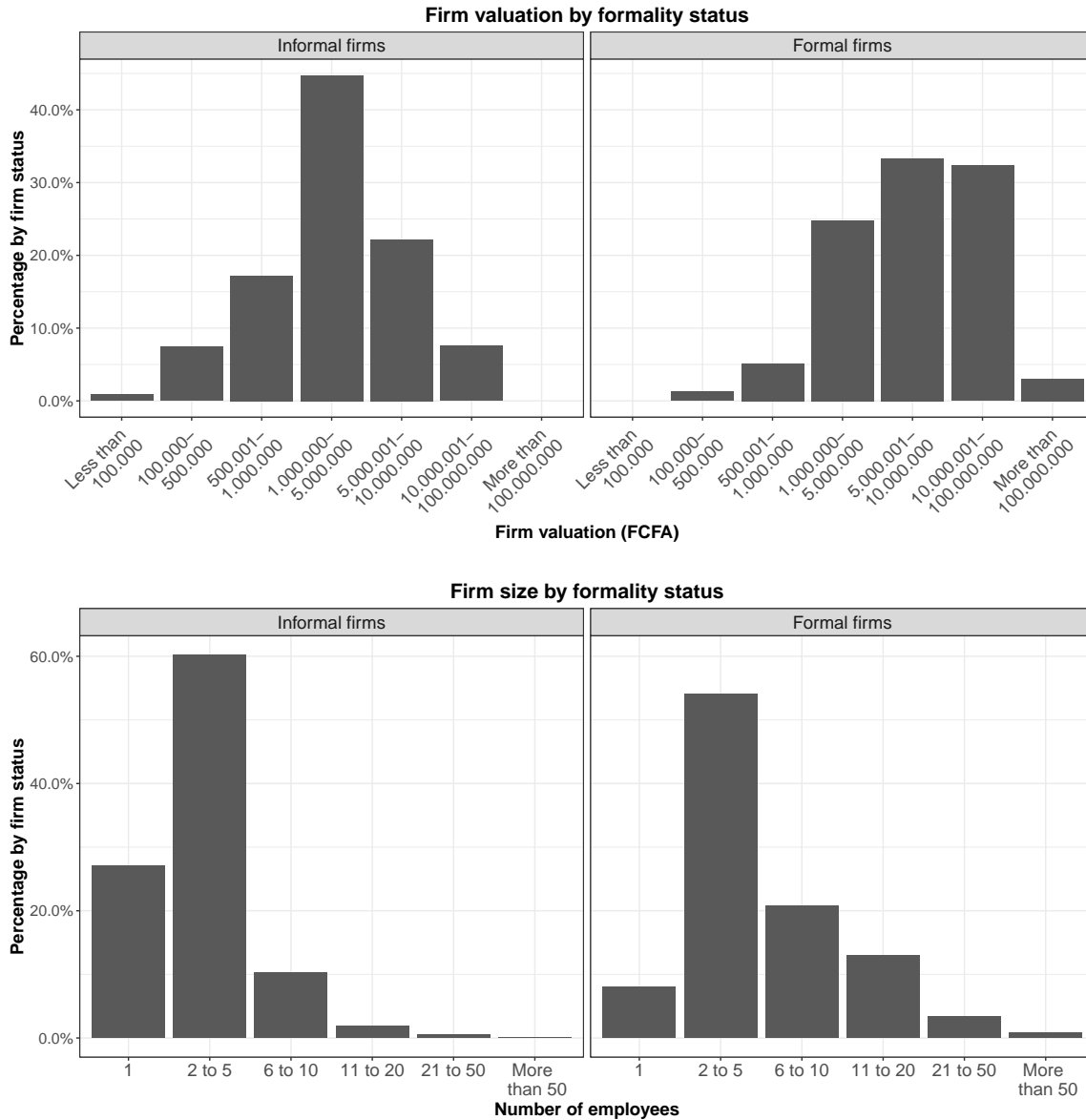


Figure 2.3: Distributions of firm size and valuation by formality status

Conjoint experimental design

Among this sample of respondents who control their firms' business deals, I implemented a choice-based conjoint experiment. Conjoint analysis offers several advantages for this project. First, it enables the non-parametric estimation of multiple treatment components simultaneously and is thus well suited to test this paper's multi-faceted theory (Hainmueller, Hopkins and Yamamoto 2014). Second, as op-

posed to survey experiments that randomize a single dimension, conjoint experiments enable the manipulation of multiple mechanisms, and thus more realistically mimic actual decision-making environments. Finally, conjoint experiments mitigate social desirability concerns as they give respondents plausible deniability via the multiple ways to justify their choices. The conjoint design tests the claims outlined in Section 2.2, and answers how the varied formal, social, and political aspects of trade affect respondents' decisions to conduct business with new partners.

Respondents were presented with two hypothetical profiles of business deals. Each profile consisted of six attributes: the religion of the firm manager, the ethnicity of the firm manager, the type of contract to be used in the deal, the personal political connections of the firm manager, the political party of the firm manager, and the size of the business. Based on extensive qualitative interviews with formal and informal firms, I selected these attributes to capture the most important formal, social, and political determinants of business in Senegal, as well as to avoid conflicting interpretations of variables. For example, the conjoint design included the size of the business to prevent respondents from falsely assuming that a politically connected firm must be large and wealthy. The full list of attributes and their associated values are listed in Table 2.1. The order of appearance of these attributes was randomized, and each value within an attribute had an equal probability of assignment.⁷

When administering the conjoint experiment, enumerators read instructions aloud—encouraging respondents to keep in mind their modal type of deal—and then handed respondents the enumeration tablets.⁸ The tablets presented respondents with two side-by-side profiles of potential business deals, each with ran-

⁷There were no restricted combinations of profile attributes. While some combinations are less common than others (e.g. a firm manager who is friends with an MP but has no political affiliation), none are impossible in both theory and practice.

⁸For illiterate respondents, enumerators read the profiles aloud, and turned away when respondents clicked on the tablet to make their choices.

Attribute	Randomized traits
Religion of firm manager	Tidjane, Mouride, Layenne, Muslim (no brotherhood)*, Christian
Ethnicity of firm manager	Wolof*, Serer, Peul, Mandingue, Diola
Contract to be used	Formal written contract, Verbal agreement (no written contract)*
Personal political connections of firm manager	Friend of local mayor, friend of MP, friend of president, no personal political connections*
Political party of firm manager	Ruling party member, opposition party member, no political affiliation*
Size of business	Large business, medium business*, small business

Notes: Asterisks indicate the pre-specified reference traits used for estimating treatment effects.

Table 2.1: Attributes and their trait values

domized attribute values. Appendix Figure B.1 shows an example of how the profiles appeared to respondents. For each profile pairing, respondents answered two questions that serve as the primary outcomes of the conjoint experiment: (1) “Which deal are you more likely to accept?” (I refer to this as the *accept* outcome) and (2) “Which deal is more likely to end in contract breach?” (the *breach* outcome). As a forced choice between two potential business partners would be incongruous with the real-world decision-making process, respondents also had the ability to select “both firms” or “neither firm” in order to approximate reality (Hainmueller, Hangartner and Yamamoto 2015).⁹ Each respondent performed four rounds of choice tasks, and then returned the tablet to the enumerator. Enumerators administered the survey’s questions about formal and informal contracts, past legal disputes, and political affiliation after the conjoint experiment, to minimize priming effects in the conjoint analysis.

Estimation

The principal quantity of interest in this project is the average marginal component effect (AMCE), which is the marginal effect of an individual treatment component

⁹Respondents could also choose “I prefer not to respond” or “I don’t know.”

in Table 2.1 averaged over the joint distribution of all other attributes (Hainmueller, Hopkins and Yamamoto 2014). Under a certain set of assumptions, which are met here (see Section 2.4), AMCEs can be non-parametrically estimated and are unbiased. I estimate these treatment effects by linear regression. The choice outcome is regressed on a vector of indicator variables for treatment components, excluding baseline attribute levels. Treatment coefficients can thus be interpreted as the probability that the deal is chosen when it contains that particular attribute trait, relative to the baseline trait.¹⁰ To analyze how these causal effects interact with background characteristics (i.e. formality status of the respondent’s firm, ethnicity and religion of the respondent), I estimate heterogeneous treatment effects by interacting the treatment groups with the relevant covariate of interest.

In all analyses, standard errors are clustered at the respondent level. As there were 2,389 respondents, each of whom performed four choice tasks that contained two profiles, there are a total of 19,112 observations. However, due to some cases of refusal and “don’t know” responses, there are 18,794 observations for the *accept* outcome and 17,596 observations for the *breach* outcome. I address these missing observations in the following subsection.

Diagnostics and threats to inference

I first rule out the presence of carryover effects by estimating treatment effects separately for each of the four rounds of the experiment, as suggested by Hainmueller, Hopkins and Yamamoto (2014). As Appendix B.5 shows, effects are similar across all rounds, and F-tests are unable to reject the null of equality. Second, I ensure that there are no profile-order effects, by confirming that effects are similar regardless of the profile position (left or right) in a given task round (Appendix B.5). Third, I demonstrate in Appendix Table B.3 that randomization was successful by verify-

¹⁰The full specification is presented in Appendix Section B.4.

ing balance across the sample and across a variety of background characteristics of firms and respondents. Finally, I test and confirm that treatment effects were consistent regardless of the randomized vertical position of attributes within profiles (Appendix B.5), thus ruling out primacy effects.

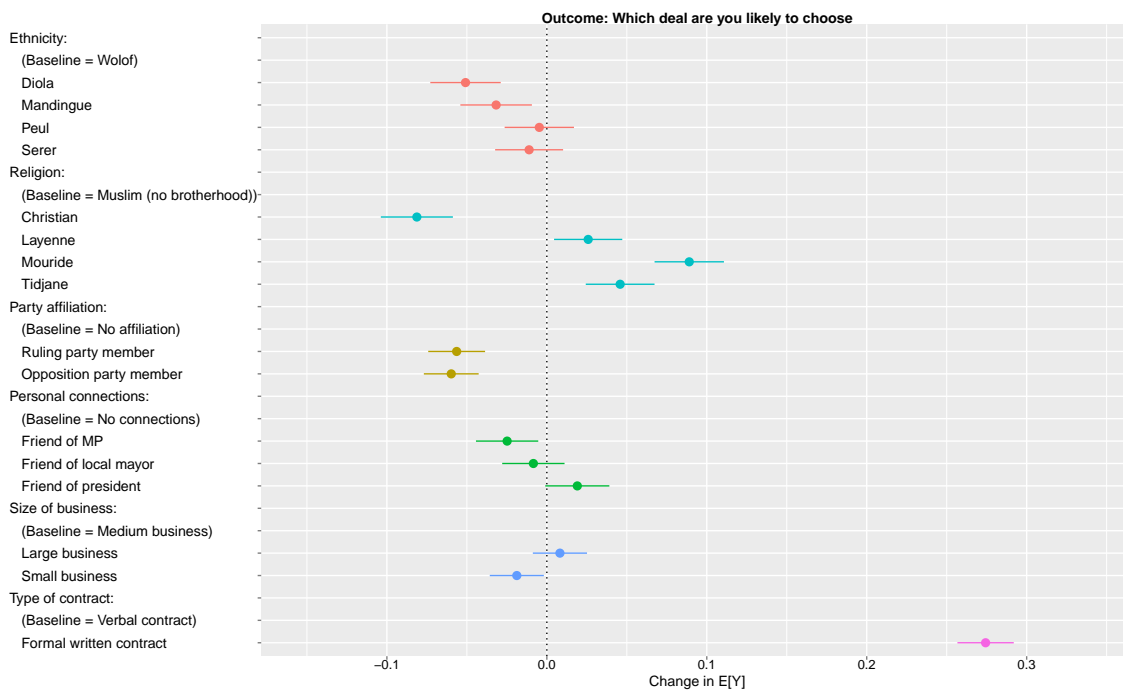
Attrition, caused by refusals to respond and “I don’t know” responses in the conjoint experiment, poses a potential threat to inference. However, this missingness is rare—only 1.66% of total observations for the primary outcome of interest (*accept*)—and I also verify that missingness was not driven by treatment assignment (Appendix B.5). Furthermore, including these observations with interpolated means does not change the substance or significance of results throughout.

2.5 Results

Estimating results for the entire experimental sample—both formal and informal businesses—is important for considering how firms in Senegal are, on the whole, moved by varied social, formal, and political forces. What is the relative importance of these salient factors when choosing business partners? I estimate treatment effects for the *accept* outcome, and present these full-sample results in Figure 2.4. The baseline traits are listed at the top of each attribute grouping in parentheses, with the other traits’ AMCEs and their 95% confidence intervals below. I examine and extend these results in the following subsections.

The importance of social features for trade

Focusing first on ethnicity, the top attribute grouping in Figure 2.4, the results reflect the importance of ascriptive ethnic features in the private sector in Senegal. Relative to the baseline ethnic group of Wolof, which is the largest ethnic group in Senegal, respondents were significantly less likely to accept a business deal when



Notes: The outcome is based on the question: “Which deal are you more likely to accept?” The change in probability of a deal being chosen, relative to the baseline attribute trait, is on the horizontal axis. The corresponding table results are in Appendix Table B.6.

Figure 2.4: Main result: Influences of firms’ likelihood to exchange

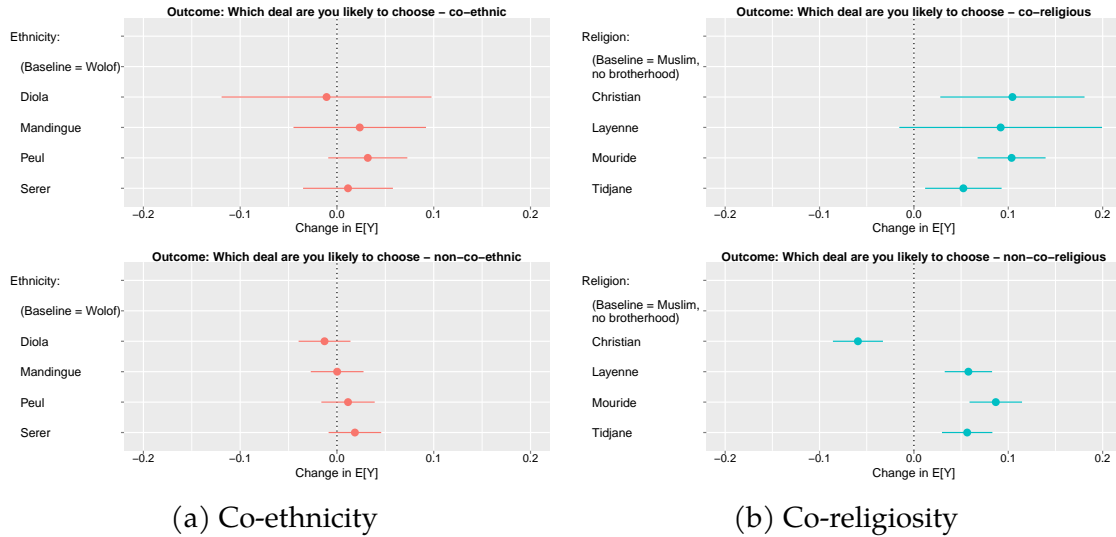
the opposing firm manager was Diola or Mandingue. Overlapping ethnic and religious cleavages may explain the Diola result: Diola are disproportionately Christian relative to other ethnicities in Senegal, which may reduce the ability of the median firm owner in the sample—a member of a Muslim religious brotherhood—to seek recourse via social sanctioning.

Indeed, in line with the politicization of religion in Senegal, religion appears to be the more salient identity-based factor for determining the likelihood of trade. In contrast to the results for ethnicity, all religious identities significantly affected the likelihood of trade, and these effects were also larger in magnitude. Respondents avoided deals with Christian firm managers and sought deals with firm managers affiliated with Muslim brotherhoods. Perhaps reflecting the density, structure, and

authority of Mouride networks in particular, as well as their reputation for secure exchange, deals were much more likely to be chosen when the hypothetical firm manager belonged to the Mouride brotherhood. Of all ascriptive identity features included in the experiment, Mouride membership moved respondents the most, increasing the probability of deal acceptance by 0.09. Membership in the Layenne or Tidjane brotherhoods also increased the likelihood of deal acceptance, though at around half the magnitude. Overall, these results confirm that informal features can shape how business occurs in places where such features are correlated with perceptions of enforcement.

The results in Figure 2.4 represent the sample's *overall* perception of various ascriptive groups in Senegal, rather than specific mechanisms of in-group enforcement. I re-estimate the results with stratified data by in-group and out-group status, i.e. whether the hypothetical business partner in the conjoint experiment was of the same ethnicity or religion as the respondent. In total, 17.6% of deals occurred with co-ethnics, and 20.4% of deals with co-religious firms managers. Figure 2.5 shows the conditional treatment effects. The coefficients can be interpreted as the change in probability that the respondent chose to trade with someone from the same ethnic or religious group as their own, holding all other traits constant.

There were no significant effects along ethnic lines. This may be partly due to the weakness of ethnic factors in the urban environment of Dakar (Koter 2013), or due to the lack of explicit ethnic networks for conducting business. Religious networks are stronger, however, as the results in Figure 2.5 reflect. Respondents who were co-religious to a hypothetical business partner were much more likely to select that partner. Interestingly, among non-co-religious respondents, decisions to trade were still affected by religious factors—respondents avoided trade with Christians, though sought trade with members of the three primary Muslim brotherhoods. This reflects the trading discrimination against certain identity groups that is com-



Notes: The outcomes are based on the question: “Which deal are you more likely to accept?” Changes in probability of a deal being chosen, relative to the baseline attribute trait, are on the horizontal axes. The corresponding table results are in Appendix B.6.

Figure 2.5: Results conditional on co-ethnicity and co-religiosity

mon to many developing contexts (e.g. Michelitch 2015), and also reflects the majority Muslim population and sample; only 3.5% of respondents were not Muslim. Overall, these results suggest that in-group membership boosts the likelihood of trade, which highlights the importance of social mechanisms underpinning trade when rule of law is weak.

The value of formal protections in an informal business environment

While informal features heavily moderated firm owners’ choices, formal factors also played a surprisingly large role. As Figure 2.4 shows, the largest result came not from ascriptive features, but from the type of contract used in the deal. When a formal, state-backed contract was part of the deal instead of a verbal contract—how the majority of trade is conducted in Senegal—the probability that a respondent chose the deal increased by a staggering 0.27. Firm managers had much greater

confidence in deals that used state-backed contracts. This result is somewhat surprising given the rampant inefficiencies that plague Senegal's legal institutions. I attempt to distinguish the mechanisms supporting this result later in the paper by testing differences across the formal and informal sectors, using the fact—supported by Figure 2.1—that informal firms have a smaller set of recourse options available to them relative to formal firms.

The size of the hypothetical firm was primarily included to control for alternate interpretations of other attribute traits (e.g. conflating political connections for size/wealth). Still, firm size affected business owners' calculus, though at a smaller magnitude than identity-based features and formal contracts. Relative to medium-sized businesses, respondents resisted trade with small firms. This might be due to the lack of accountability for small businesses, which can slip through the cracks in terms of enforcement or are unconstrained by the institutional structures that larger businesses develop over time. It could also be the case that respondents reacted to the lack of potential rewards that comes from trading with smaller, non-lucrative businesses.

The “value” of political connections

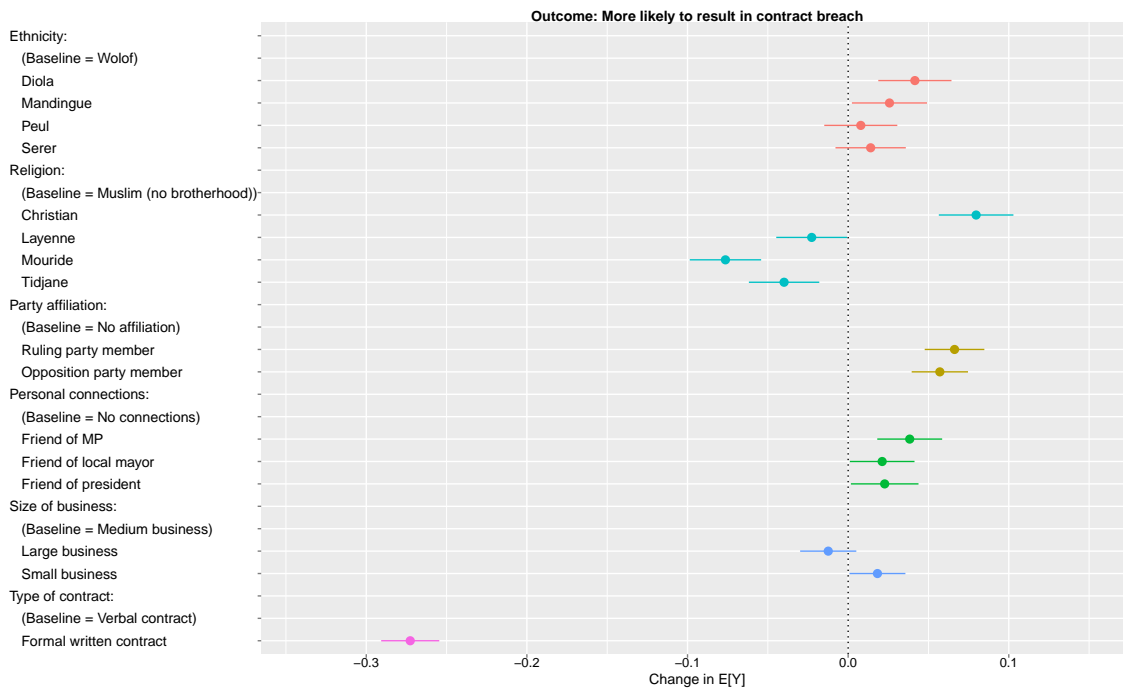
Figure 2.4 also shows that political variables can factor in firm owners' decisions to do business under weak rule of law. Respondents avoided deals with politically affiliated firms: potential firm managers associated with either the ruling party or opposition parties decreased the likelihood that a deal would be selected by a probability of approximately 0.06, relative to firm managers who did not have a political affiliation. In Appendix B.7, I show that these results are not driven by a general distaste for politics, but rather, as I argue in this section, weakened perceptions of enforcement.

Beyond direct political affiliations, personal connections to those in political

power also affected firm managers' decisions. In the full sample, there was a small but statistically significant decline in trade probability when the opposing firm manager was connected to an MP. In contrast, there was no detectable effect when the opposing firm manager was connected to a local mayor, and a *positive* effect when connected to the president. This suggests that the effects of personal political connections are not universally negative, and confirms the hypothesis that firm owners gain value from doing business with the most politically influential business partners. Why are firms entering these potentially risky deals, or could it be the case that firm owners do not view deals with highly politically connected partners as risky in the first place?

To rule out the possibility that respondents viewed highly connected partners as "good types", I examine respondents' answers to the second outcome question: "Which deal is more likely to end in contract breach?" Because firms tend to avoid deals that they believe are more likely to result in contract breach, we should expect to observe a mirror image of the results in Figure 2.4, *except* for cases where the advantages of risky deals outweigh the negatives. Figure 2.6 presents the treatment effects for the contract rupture outcome, and plots how the various traits influenced respondents' beliefs that the deal was likely to end in contract breach. These findings show that, in general, the traits that businesses valued when selecting a deal (Figure 2.4) were indeed inversely correlated with the traits that businesses associated with higher risk of contract breach (Figure 2.6). This intuitive result suggests that firms chose to do business with partners they thought were likely to uphold their side of the bargain.

There was an important exception to this trend of inverse correlation, however, for when the opposing business partner had the highest type of personal political connection (i.e. to the president). Respondents preferred deals with these highly connected trading partners *despite* believing they were more likely to renege on con-



Notes: The outcome is based on the question: “Which deal is more likely to end in contract breach?” The change in probability of a deal being chosen, relative to the baseline attribute trait, is on the horizontal axis. The corresponding table results are in Appendix Table B.7.

Figure 2.6: Perceptions of likelihood of contract breach

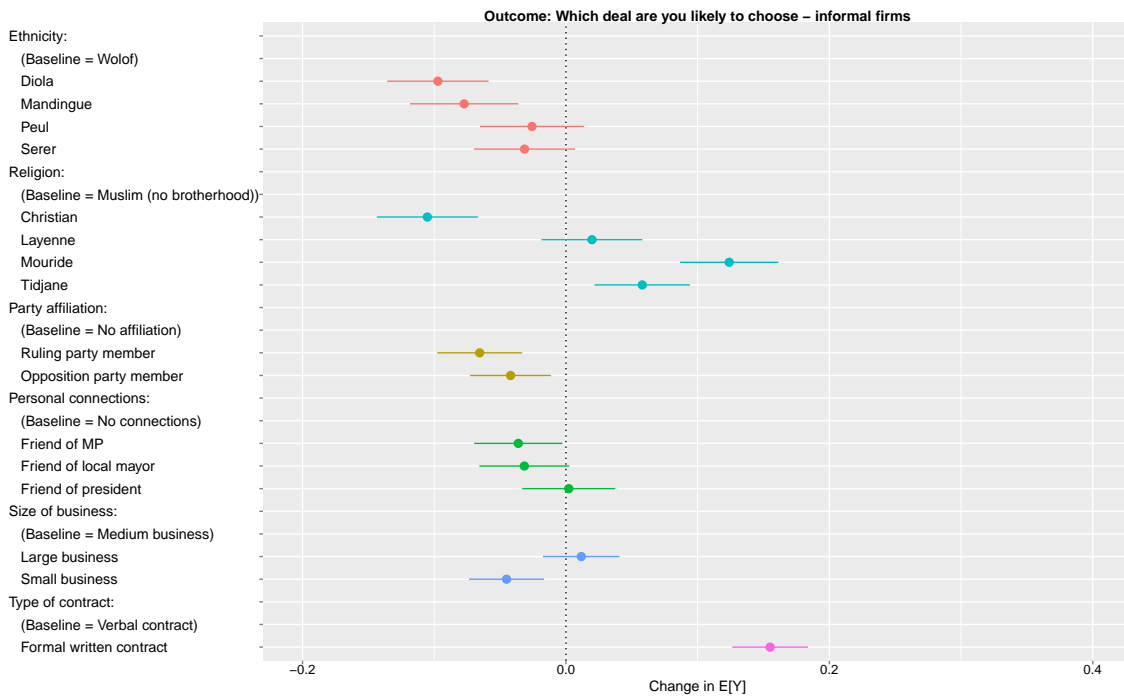
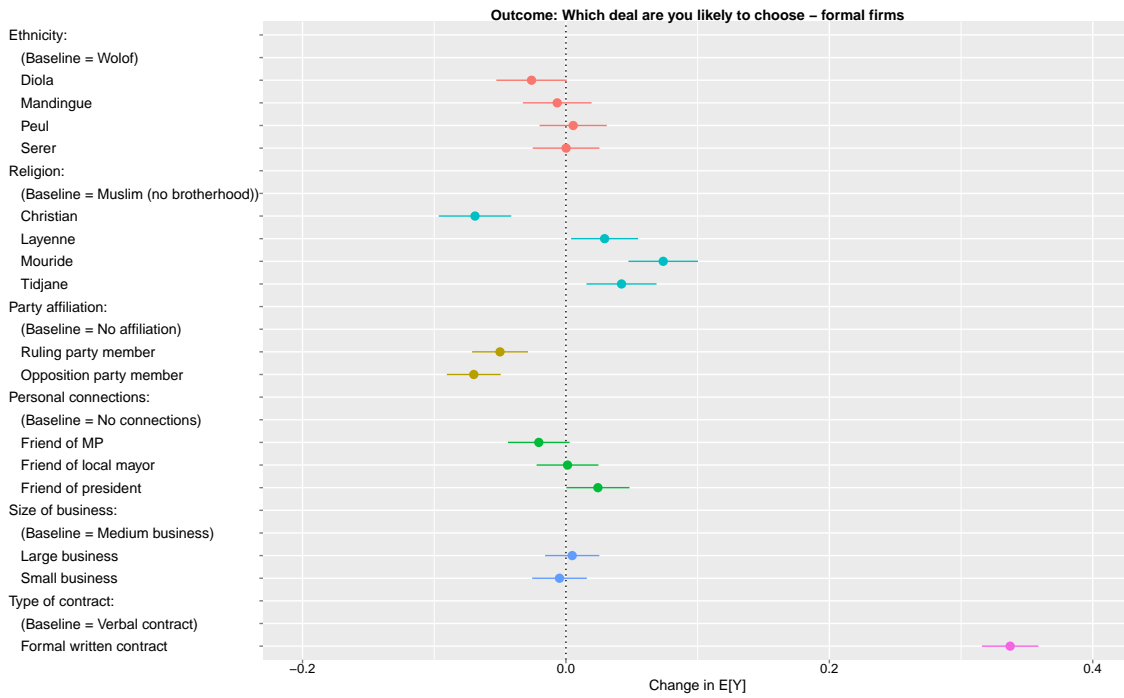
tracts. These are cases where the potential rewards of doing business outweigh the higher risks of defection. In contexts where rule of law is selectively enforced and politically connected firms receive outsized benefits, firms face incentives to engage in the rent-seeking system rather than disassociate from it. From the perspective of the most highly politically connected firms, it seems that there are no downsides to possessing these political connections; they are able to both benefit from the bias of the state when it comes to enforcement (or the lack thereof), *and* they are met with neither resistance nor reluctance from potential business partners. In line with presidentially connected firms elsewhere in the developing world (e.g. Fisman 2001), these highly connected businesses reap immense value from their connections and seemingly face few repercussions.

To add nuance to how firms view politically connected partners, I included open-ended questions in the survey that asked why respondents solicit or avoid business with politically connected firms. The most common explanations for conducting business with politically connected firms were financial in nature, including access to lucrative state contracts—the most common response—and better access to state sources of financing. The reasons for avoiding these firms were related to enforcement and instability: politically connected firms benefit from impunity, greater ability to get out of contracts, and favoritism from powerful institutions. Interestingly, respondents also reported that politically connected firms are especially dangerous when the political connections “run out”; in these cases, the political advantages that previously sustained firms expire, which can cause major disruptions to business operations. Along with the results of the conjoint experiment, the evidence suggests that firms must contend with a complicated political calculus when conducting their affairs.

Firm formality drives decision-making

Given that legal recourse options differ across the formal and informal sectors (see Figure 2.1), how does the formality status of firms moderate the effects of social, formal, and political factors underpinning trade? The theory proposed in this paper suggests that formal firms are more likely to value formal heuristics for the likelihood of contract enforcement, while informal firms disproportionately value social heuristics. Furthermore, informal firms are more likely to resist politically connected partners, particularly when these partners have local influence. To test these claims, I stratify the sample by formality status and re-estimate results.

Figure 2.7 presents conditional treatment effects by firm formality status. As hypothesized, social heuristics were a larger influence for informal firms compared to formal firms. While ethnicity motivated trade for informal firm owners, it had no



Notes: The outcome is based on the question: “Which deal are you more likely to accept?” The corresponding table results are in Appendix Table B.8.

Figure 2.7: Results conditional on firm formality status (formal vs. informal firms)

effect for formal firm owners. In contrast, both formal and informal firm managers made decisions to exchange based on religious affiliation, and in generally similar patterns, although the magnitudes of these effects were larger for informal firms. These results not only confirm that informal firms rely more on social heuristic devices to ensure smooth trade, but also that, in business environments where informalities and institutional weaknesses are common and social cues are meaningful predictors of secure trade, formal firms place value on these social influences as well.

Turning to formal motivations of trade, informal firms were half as likely to trade due to formal contracts relative to firms in the formal sector. This is in line with expectations over enforcement recourse options, as formal firms are more likely to possess the ability to access institutions for state-backed contract enforcement. Still, the fact that informal firms were significantly and substantially moved to choose deals that involved formal contracts suggests that contracts can be of use even in informal economies where state institutions for enforcing contracts are unavailable.

Formality status also moderated the risk of doing business with politically connected firms. While managers in both the formal and informal sectors avoided politically affiliated partners, they differed in the extent to which they sought deals with politically connected firms. Formal firms were unperturbed by partners' connections to local mayors. In contrast, national-level political connections significantly affected formal firms' decisions to trade—but in opposing directions. Formal firms avoided trade with partners connected to MPs (low-level connections) and sought deals with those connected to the president (high-level connections). Thus, formal firms seem willing to take on the risk of dealing with politically connected firms only when the potential benefits as a function of those connections are sufficiently high. For informal firm respondents, weaker types of political

connections negatively affected their propensities to trade while connections to the president had no effect. This aligns with the reality that lower-level connections are more applicable to the social enforcement mechanisms available to informal firms, and also that local politicians—particularly mayors—exert considerable control over the operations and existence of informal firms.

2.6 Conclusion

This paper examined the relative importance of social, formal, and political factors for private-sector exchange in contexts with selectively enforced rule of law. The findings demonstrate that social and in-group identity features affect propensities to trade, and that state-backed contracts substantially boost confidence in exchange. The results also show that political connections increase the risks of exchange, stifling trade for all but the most highly connected partners. The risks of trade—and how firms respond to them—differ by formality status. Informal firms avoid business partners who have political connections, while formal firms face incentives to seek out highly connected partners.

In an environment where political connections translate to imbalanced influence in both the marketplace and the institutions girding secure exchange, these results suggest that firms rationally resist conducting trade with politically connected parties. Thus, an unintended consequence of the value of political connections in weak institutional environments is an overall loss of trade. These findings suggest that existing policies for addressing the economic or institutional constraints to private growth should be expanded to take account of political risk. Particularly as one-shot deals, newly proliferating firms, and low-information marketplaces increasingly characterize emerging markets, considering underlying political inequalities and their effects on private-sector development is essential for

ensuring efficient and equitable growth. The results of this project also suggest, promisingly, that formal contracts can increase confidence in exchange and enable deals to occur that may not otherwise, even in markets where informal influences abound. Increasing access to formal contracts—while breaking the link between political connections and enforcement—may thus help spur private-sector development even when the underlying state institutions lack capacity or independence.

While the competing influences of social, formal, and political factors affect with whom firms conduct business, future research is still needed to understand how these influences manifest in eventual enforcement outcomes. How firm owners resolve disputes has important implications for the function of the private sector amidst informal influences, and may help to understand why informal firms remain informal despite the risks. Additional research is likewise needed to test whether these factors driving trade lead to broader distributive consequences in poor societies. That certain ethnicities or religions are more favored in trade might lead to negative distributive outcomes overall for excluded groups. Understanding how private-sector exchange may shape broader societal inequalities is important for ensuring equal growth in rapidly shifting economies. Finally, while the purpose of this paper was to establish the relationship between the influences of trade and ultimate trade decisions, we still know little about the precise mechanisms by which these influences operate. Future work should more directly dissect these mechanisms.

Given that Senegal ranks squarely among African, East Asian, and Latin American countries in terms of its contract enforcement institutions—and given that many of these countries are characterized by a similar hybrid democratic environment as Senegal—I expect the theory and results presented in this project to carry beyond Senegal. Indeed, these effects may be considerably stronger in contexts that are institutionally weaker than Senegal, where political and informal influences in

the private sector are even more rampant. The results presented in this paper may help to illuminate private-sector inefficiencies and stalled growth in other contexts where social and formal influences compete in the marketplace.

Firm Strategies, Weak Rule of Law: Contract Enforcement in Informal Environments

Abstract: How do firms secure their property rights when deals are broken in societies with weak rule of law? As domestic and international actors alike push toward the use of formal contracts, how these contracts affect enforcement strategies remains to be tested. To understand firms' recourse decisions, I administered a survey with an embedded experiment to 2,389 formal and informal firm managers in Senegal. Respondents stated their likelihood of using various legal and social enforcement strategies when a business partner reneged on a randomized formal or informal contract. The survey results provide needed descriptive evidence of formal and informal firms' divergent strategies for protecting property rights, and experimental evidence that formal contracts increase dependence on legal institutions—and decrease dependence on social institutions—for property rights security. These effects are strongest for formal firms, suggesting that the largest gains from increasing access to formal contracts accrue to firms that already receive preferential access to legal institutions.

3.1 Introduction

Longstanding research acknowledges that state-based property rights institutions are critical for economic development (e.g. North and Thomas 1973; North and Weingast 1989; North 1991; Olson 1993; Besley 1995; Kaufmann, Kraay and Zoido 1999; Johnson, McMillan and Woodruff 2002; Rodrik, Subramanian and Trebbi 2004). Formal legal institutions can spur economic activity (Porta et al. 1998; Acemoglu, Johnson and Robinson 2001; Chemin 2009*a,b*), and states and international organizations alike attempt to increase their availability and quality. Firms use state-backed institutions such as formal contracts to structure exchange, drawing on the power of the state for third-party enforcement (Hart 1995). But in developing countries, these formal legal institutions coexist with informal institutions for contract enforcement, and firms often eschew legal solutions in favor of informal ones when resolving disputes (Haley 1997; McMillan and Woodruff 1999*b*). Firms turn toward local protection rackets, private security companies, and informal government support and political connections (De Soto 2000; Wank 2002; Sonin 2003; Wilson 2010), or rely on dense social networks, reputation-based mechanisms, and relational contracts to structure their exchange (Macneil 1973; Milgrom, North and Weingast 1990; Greif 1994; Baker, Gibbons and Murphy 2002; Brown, Falk and Fehr 2004; Habyarimana 2007; Macchiavello and Morjaria 2019). Indeed, firms' default strategies may be to avoid state-backed recourse entirely (Macaulay 1963; Pistor 1996). How do firms decide when to enforce contracts formally or informally? Despite the rich literature on property rights institutions, there is less evidence of the demand side of property rights.¹

Though property rights and contracting are sometimes conflated or treated as

¹See Gans-Morse (2017) for an important exception. Still, the non-state enforcement strategies in post-Soviet Russia trend toward violence and protection rackets, which differs from the many areas of the world where these are not common strategies.

mutually exclusive, they are often endogenous in nature. Contracting institutions can affect strategies to secure property rights, and, as Frye (2004) argues, the impact of legal institutions on the security of property remains understudied.² When a mixture of formal and informal institutions structure trade, it is not obvious how contracts affect the strategies that firms ultimately pursue to secure their property. Especially as states make strides toward increasing the formality of the private sector, analyzing the impact of contract formality on property security strategies is important for strengthening policy intended to develop the private sector. Though contract formality is not an exogenous factor to trade (Williamson 1975), domestic and international interventions have made formal contracts difficult to universally avoid, even for firms in the informal economy. How do formal contracts affect the enforcement strategies that firms use to protect their property rights?

The answer has implications for core theoretical issues in state- and economy-building. Trust in exchange is required to overcome fundamental credible commitment problems, and the ability to conduct trade without fear of expropriation or contract rupture is important for efficient economic development (Arrow 1972). The use of legal institutions is also important for “state-in-society” conceptions of state-building, in which the state develops its legitimacy as more citizens buy into its institutions (Migdal 2001). Moreover, as countries continue to urbanize at rapid rates—especially in sub-Saharan Africa—the societal factors that structure trade in smaller markets become less effective (Fafchamps and Minten 2001a; Dixit 2003). Understanding the determinants of third-party enforcement strategies is important for development in larger-scale economies where reputations are not always known and where repeated interactions are not always possible.

²Property rights are alternatively defined as constraints on government or elite expropriation (Acemoglu and Johnson 2005), or a broader conception as the ability to use, obtain income from, or exchange ownership of an asset (Barzel 1997). *Property security strategies* are broader yet, referring to “firms’ efforts to resolve conflicts related to acquiring assets, protecting property, and enforcing contracts” (Gans-Morse 2017). Property security strategies are the primary focus of this paper.

This paper first seeks to provide much needed description of firms' property rights security strategies in a context with selectively enforced rule of law. What enforcement strategies are available to firms, and how often are they used? Are there key differences across the formal and informal sectors, or perhaps, in line with the rest of this dissertation, based on political connections?

Second, I seek to understand how variation in contract formality affects property security strategies. When deals are based on formal contracts—rather than the informal agreements that characterize the bulk of trade in developing countries—to which institutions do firms turn to resolve their disputes? In environments where formal and informal contract enforcement institutions coexist and compete, I argue that formal contracts should move firms toward using state-backed institutions to resolve disputes and that this effect should be particularly pronounced for formal firms. It is unclear, however, whether formal contracts should decrease dependence on social means of enforcement, given that social institutions may still be effective for resolving disputes. I also explore the role of factors beyond firm formality that may moderate the utility of formal contracts. I argue that those with greater trust in courts and with less exposure to corrupt governmental practices are more likely to use formal enforcement mechanisms when their deals are based on formal contracts. Firms' political connections can also affect their likelihood of using certain property security strategies. Given that politically connected firms have preferential access to state institutions, I argue that connected firms are more likely to use formal enforcement strategies when formal contracts are the basis of trade.

To examine the determinants of property rights security, I administered a survey in Senegal that reached 2,389 firms in the formal and informal sectors. Senegal is an apt location to set this study because it fits the criterion of stable rule of law where formal contracts are enforceable, yet also where legal institutions can be biased and difficult to access. It is also a society where informalities characterize much of the

private sector, and dense social networks and informal contracting structure trade. The survey included questions about firms' enforcement behavior, past disputes and resolution methods, and other important descriptive information that to date has been difficult to collect in the country, particularly from informal firms. To test the impact of contract formality on firms' property security strategies, the survey included an embedded experiment that randomized whether the basis of a broken agreement was a formal or an informal contract, and asked respondents their likelihood of using various formal and informal enforcement strategies to resolve the dispute.

The descriptive results show that firms in both the formal and informal sectors overwhelmingly prefer to resolve disputes amicably where possible. Perhaps unsurprisingly, informal firms are more likely to use social enforcement mechanisms, and formal firms are more likely to use formal enforcement mechanisms. Still, formal firms use social enforcement mechanisms often, epitomizing the informality of the Senegalese private sector. The descriptive results further show that politically connected firms are not just more likely to use formal enforcement, but also to use social enforcement, suggesting that politically connected firms may have preferential access to a multitude of institutions.

The results of the survey experiment confirm that when formal contracts are the basis of a ruptured deal, firms are more likely to seek recourse via legal institutions. Furthermore, formal institutions appear to substitute for informal ones, as the use of social enforcement mechanisms declines as the use of legal enforcement increases. Examining the treatment effects by sector, formal firms are significantly more likely to seek formal enforcement methods when the basis of a deal is a formal contract, suggesting that increasing access to formal contracts may unintentionally increase disparities in institutional access. Interestingly, politically connected firms are *less* likely to seek out the police when a broken deal is based on a formal contract,

and no more likely than unconnected firms to use other formal enforcement methods. Also surprising, firm managers' trust in courts does not moderate treatment effects. Finally, firm managers who have been exposed to governmental corruption in the form of illegal tax extraction are *more* likely to turn to state-backed property security strategies when using formal contracts. This counterintuitive result suggests that firms may associate the state's extractive ability with greater enforcement power.

With its descriptive and experimental findings, this paper makes several contributions. First, at a basic level, this paper provides description of firms' political and economic behavior in a context where such data has been historically difficult to obtain. This evidence helps to clarify how firms in the formal and especially informal sector secure their property rights amidst competing state and nonstate influences. Second, by examining the determinants of property rights security, this paper contributes to the literature on demand for rule of law in weak institutional environments (Pistor 1996; Hendley 1997, 1999; Hoff and Stiglitz 2002; Gans-Morse 2017). The existence and quality of state institutions are not sufficient to explain their usage, particularly in societies where legal institutions coexist with informal ones. This paper explains how variation in contract formality can affect this demand for rule of law. Finally, building on the earlier chapters of this dissertation, I provide additional evidence that increasing access to the formal sector may have the unintended consequence of deepening private-sector inequalities. Removing demand-side barriers to legal recourse does not address more deeply rooted problems of inequality in enforcement.

The remainder of the paper is structured as follows. In the next section, I provide theoretical background on the determinants of property security strategies and present the hypotheses to be tested in this paper. Then, I discuss Senegal's contracting environment and why it makes a suitable testing ground for this theory. I

next describe the survey and experimental design, and also describe the sample in detail. I then present the descriptive and experimental results, and finally conclude by discussing the broader implications of this paper for policy and future research.

3.2 Theory

Formal and informal enforcement of property rights

Property rights are considered instrumental for economic development (North 1991; Olson 1993; Acemoglu, Johnson and Robinson 2001; Djankov et al. 2003), but the enforcement of property rights in developing countries can be a complex prospect (Firmin-Sellers 2007). Firms in these contexts use both formal and informal institutions to secure their property rights. Formally, firms engage legal institutions such as the police, lawyers, and courts when seeking recourse. Still, the existence and availability of legal recourse options do not imply that firms will actually use them (Pistor, Wellons and Sachs 1999; Milhaupt and West 2000; Gans-Morse 2017), in the same way that weak rule of law does not imply that firms will avoid formal institutions altogether (Whiting 2010). Legal recourse options can be expensive and entail significant transaction costs, further preventing their use (Williamson 1985).

Firms have access to a diverse array of options for informal enforcement of property rights. As a first option, firms typically try to resolve conflicts amicably, preferring to negotiate before bringing the matter to enforcement arenas, including the state (Macaulay 1963). Another strategy comes in the form of shared social networks, particularly where such 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 authority sources that

punish defectors (Klein and Leffler 1981; Greif 1989; Coate and Ravallion 1993; Fearon and Laitin 1996).³ Ethnic, religious, and other ascriptive networks can underpin social enforcement, including in Senegal where this project is set (Ebin 1993, 1995; Grimard 1997; Golub and Hensen-Lewis 2012). Finally, 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 2019a).

How do firms decide which property rights security strategies—formal or informal—to use? The growing literature on legal pluralism and forum shopping offers some insights.⁴ A common reason why 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 of weak capacity where firms seek formal resolution (Gans-Morse 2017). Transaction cost economics can also help to partially 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). Firms may also forum shop among state and non-state resolution tactics to increase their likelihood of a favorable outcome (Sandefur and Siddiqi 2013). While acknowledging that these factors may be relevant, in this paper, I examine an additional factor that can structure firms' decisions to seek formal or informal property rights security: contract formality.

³While 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 could constrain economies to smaller sizes (Fafchamps and Minten 2001a).

⁴See von Benda-Beckmann (1981); Meinen-Dick and Pradhan (2002); Tamanaha (2008); Sandefur and Siddiqi (2013); Lazarev (2018); Acemoglu et al. (2019); Blair (2019); Blair, Karim and Morse (2019).

The role of contract formality

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 and/or go through legal review. At the other end are verbal contracts, agreements made between two exchanging parties that are not specified in state-enforceable terms.⁵ The type of contract chosen for a deal is often endogenous to the deal itself (Williamson 1975). For example, a firm may decide to use an informal contract because they are in a position of power and plan to renegotiate terms as market prices fluctuate.⁶ On the other hand, if a firm conducts business with a new partner that has not yet established a reputation in the marketplace, a formal contract can offset some of the risks (Johnson, McMillan and Woodruff 2002).

In this paper, I ask how the formality of a contract affects the strategies firms use to secure their property rights 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), the formality of contracts is an important predictor for demand of rule of law that has not received much focus. 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

⁵In 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.

⁶Interview with supply changer manager of large distribution center in Senegal, July 4, 2016.

state-backed enforcement methods.⁷ Conversely, if a deal is based on an informal contract, it is more difficult—or, firms may perceive it to be more difficult—to seek recourse via state institutions, and thus firms will use social enforcement strategies.

Hypothesis 1a *Firms are more likely to resolve their disputes with formal (informal) enforcement strategies when deals are based on formal (informal) contracts.*

Formal and informal methods of dispute resolution are not mutually exclusive. 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. Existing literature differs on the extent to which formal and informal enforcement mechanisms are substitutes or complements (Akerlof 1970; Kranton 1996; Johnson, McMillan and Woodruff 2002; Sobel 2006; MacLeod 2007; Acemoglu et al. 2019). Given these theoretical ambiguities, this remains an empirical question to be tested in the paper.

Hypothesis 1b *Firms' use of formal strategies when deals are based on formal contracts will correspond with a decrease in (have no effect on) their use of social enforcement strategies.*

The role of firm formality

Informal and formal firms face different constraints when it comes to enforcing contracts. While formal firms can use both formal and informal contract enforcement methods, informal firms in many institutional environments do not enjoy the same level of access to formal institutions.⁸ Informal firms are often, by their very

⁷An important scope condition for this theory is that formal institutions have sufficient capacity to enforce contracts. Thus, this theory does not apply to stateless societies.

⁸Indeed, this may be a reason that some informal firms formalize in the first place (Bhandari and Gottlieb 2017). 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

existence, illegal, and states can restrict use of formal enforcement institutions to firms that have registered with the state (Mnookin and Kornhauser 1979; Pistor 1996; Hay and Shleifer 1998). Thus, regardless of the type of contract used, at baseline, formal firms are more likely than informal firms to use formal enforcement strategies during contract disputes.

Hypothesis 2a *Formal firms are more likely overall to use formal enforcement strategies when resolving contract disputes.*

Do firms across the formal and informal sectors react differentially when a formal or informal contract is part of a deal? That is, are formal firms more likely than informal firms to seek formal recourse when deals are based on formal contracts, and are informal firms more likely to seek informal enforcement when deals are based on informal contracts? Given that formal enforcement strategies already privilege formal firms (Hypothesis 2a) and that informal firms are often shut out of formal enforcement institutions altogether, and given that I expect formal contracts to compel firms overall toward formal enforcement (Hypothesis 1a), it follows that formal firms will drive this effect. This implies that greater access to formal contracts could potentially further segment already unequal institutional access. It remains unclear, however, whether informal contracts should differentially drive informal firms toward informal enforcement. While formal firms have disproportionate access to formal institutions for enforcement, the same is not necessarily true for informal firms with informal institutions. Thus, when informal contracts structure trade, I do not expect informal firms to differentially use social enforcement mechanisms relative to formal firms.

Hypothesis 2b *When a formal contract is the basis of the trade, formal firms are more likely to seek formal enforcement strategy.*

correlation matrix in Appendix Section C.5 shows, formal firms do not have substantially greater trust in courts than informal firms.

Moderators of contract utility

Several other factors can affect the likelihood that firms seek legal recourse when deals are based on formal contracts. First, work in developing contexts shows that exposure to corruption can moderate firms' behavior (Alm, Martinez-Vazquez and McClellan 2016; Acemoglu et al. 2019; Le, Malesky and Pham 2019). Firms coerced into paying illegal taxes to state officials may be less willing to use state institutions to resolve their contract disputes because they believe these institutions are biased against them.

Hypothesis 3a *Exposure to corruption decreases the likelihood of seeking formal enforcement when resolving disputes based on formal contracts.*

Second, supply-side theories argue that the quality of rule of law can explain some of the variation in property rights protection (e.g. Djankov et al. 2003). Following this logic, I argue that firm managers' perceptions of the quality of rule of law may also affect willingness to use legal institutions to secure property rights. When those who trust legal institutions such as courts use formal contracts in their deals, they should be more likely than those who distrust courts to seek legal recourse when resolving business disputes.

Hypothesis 3b *Confidence in courts increases the likelihood of seeking formal enforcement when resolving disputes based on formal contracts.*

Finally, political connections have been explored extensively in earlier chapters and have been shown to impact firm behavior. If politically connected firms believe that the state is more likely to enforce in their favor, I expect politically connected firms to use legal enforcement strategies to resolve conflicts when formal contracts are the basis of trade. The extent of this effect may be limited, however, if firms

believe that legal institutions are likely to rule in their favor even if the broken deal was based upon an informal contract.

Hypothesis 3c *Political connections increase the likelihood of seeking formal enforcement when resolving disputes based on formal contracts.*

3.3 Context

I test these claims with data from a survey with an embedded experiment in Senegal. Senegal is an apt place to test this theory 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—especially topical.

Formal and informal contracting in Senegal

Senegal is a stable democracy in a turbulent part of the world, but its institutions for rule of law lag behind. It places poorly in the World Bank's ranking for ease of enforcing contracts (World Bank 2018), and its legal system is characterized by significant costs of access and high degrees of procedural formalism (Kondylis and Stein 2018). 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 such contracts can be reviewed by lawyers 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. Using data from my sample, approximately 85% of firms reported using formal contracts, and 88% reported using informal contracts, as part of their regular business operations.

Altogether, 85% of firms reported using multiple types of contracts, typifying the mix of formal and informal practices in Senegal's private sector. I break these descriptive findings down by sector formality in Figure 3.1, which shows that 82% of formal firms use informal contracts and 68% of informal firms use formal contracts during their regular business operations. Almost no informal firms use lawyers to draft and review their contracts, compared to 25% of formal firms.

When asked about their modal contract, Senegalese firms fall more in line with expectations for their sector, though still with a healthy mix of the formal and informal. Figure 3.2 shows that 69% of formal firms use formal contracts most often, while the remainder use informal, verbal agreements most often as part of their regular operations. By contrast, 78% of informal firms use informal contracts and 22% use formal contracts most often. In terms of whether they *prefer* this status quo, I asked respondents what type of contracts they preferred to use when conducting their regular operations. Figure 3.3 shows that formal and informal firms alike wished they were able to use formal contracts more often than they currently do. These summary statistics chime with the results of the conjoint experiment presented in Chapter 2 (see Figure 2.4), which also shows that firms in Senegal prefer using formal contracts. Thus, 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.

Contract dispute resolution in Senegal

Contract disputes are common among Senegalese firms. Approximately 33% of the sample reported a past dispute, and Figure 3.4 shows that formal firms experience more disputes than informal ones. The typical dispute in Senegal stems from the failure to pay for—or receipt of substandard—goods or services. As Figure 2.1 in Chapter 2 shows, firms' strategies for resolving past conflicts varied widely, with

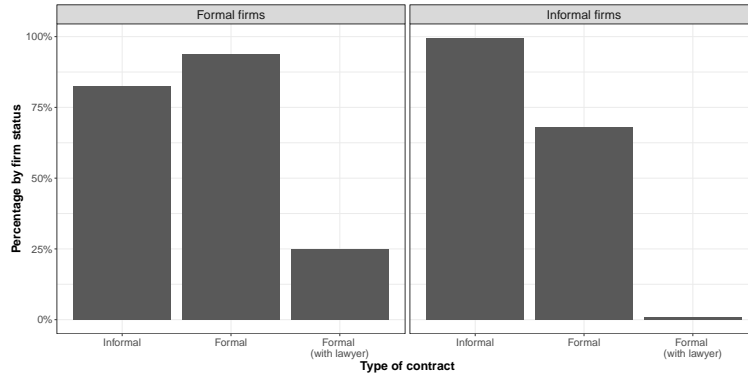


Figure 3.1: Types of contract formal and informal firms regularly use

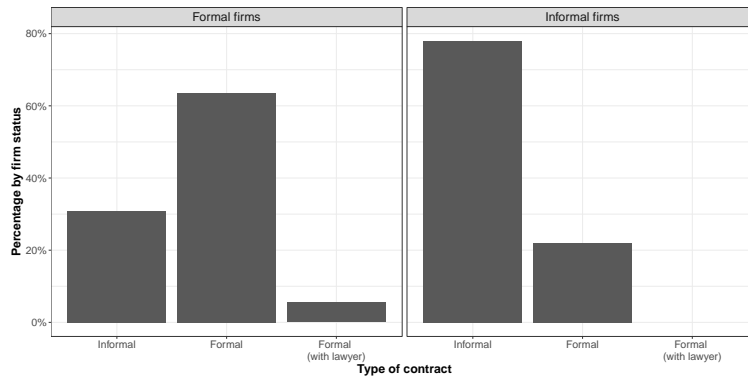


Figure 3.2: Type of contract formal and informal firms use most often

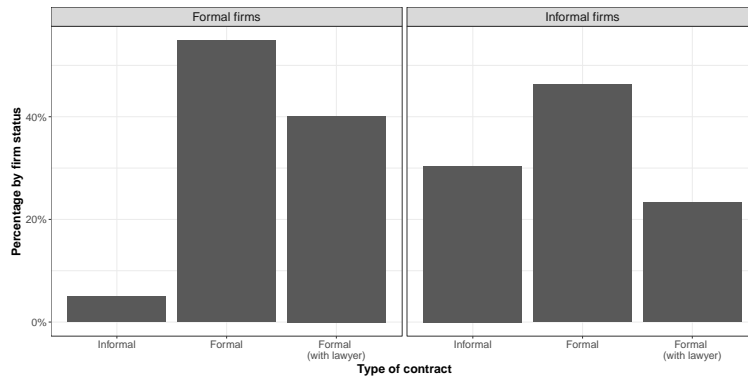


Figure 3.3: Type of contract formal and informal firms would prefer to use

informal firms relying on amicable solutions and social networks more than formal firms that relied more on formal dispute resolution processes (police, lawyers, and courts). Still, informal firms do use the police, and formal firms use social networks and amicable solutions, suggesting that both formal and informal approaches are available to formal and informal firms in Senegal.

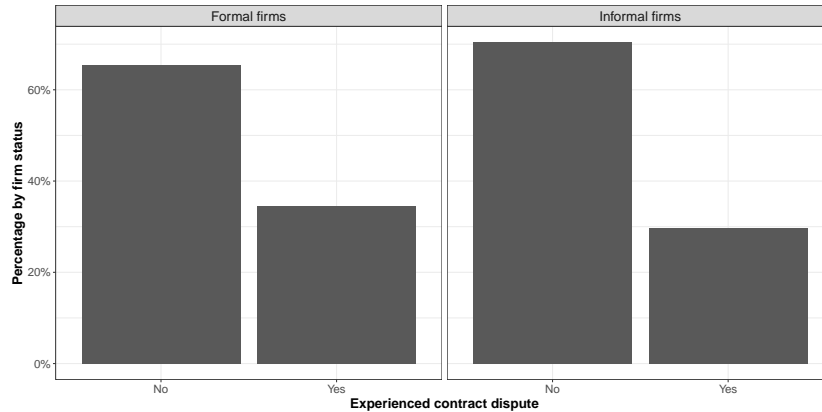


Figure 3.4: Experienced contract dispute in the past

Formal dispute resolution in Senegal takes several forms. The formal option with the fewest transaction costs is the police. Of the firms in my sample that reported experiencing contract disputes, 18% stated that they sought help from the police, including 13% of informal firms and 21% of formal firms. Using the police as a dispute resolution strategy typically involves filing a police report; whether police investigate further is at the discretion of police staff. Formal enforcement can also take the form of hiring a lawyer and/or taking the case to court. In Senegal, these options are prohibitively expensive for most (Kondylis and Stein 2018). However, in an attempt to mitigate long waiting times and administrative backlogs, Senegal recently introduced a new commercial court to more efficiently process and resolve cases (PressAfrik 2018). Thus far, there are positive reports about decreased waiting times and increased judicial efficiency (EuroNews 2019), though data is not yet available to statistically assess these claims. These new efforts, combined with recent pushes in Senegal to formalize the informal sector—as well as to further formalize the formal sector—make it a timely moment to examine the impact of contract formality on Senegal’s private sector.

Informal dispute resolution can also take several forms in Senegal. As noted above, firms most often attempt to resolve their conflicts amicably, attempting to reach agreement prior to involving external parties. Social networks can also play

a role in structuring trade and resolving disputes when they arise, given the density of social networks and the importance of social standing in Senegal. Membership in shared business associations, social organizations, and social circles are used as proxies for trustworthiness, and knowing someone in a business partner's social network can relieve some of the risk involved in a deal. When deals go awry, firm managers can make appeals to someone in the shared social network who helps to resolve the dispute. By a similar logic, religious networks are important for social enforcement in Senegal. The religious brotherhoods to which much of the country belongs are particularly critical in structuring trade, and, as the dominant non-state authority in their members' lives, often serve as third-party enforcement (Cruise O'Brien 1971; Ebin 1993; Mbacké 2005; Minard 2009; Golub and Hensen-Lewis 2012). Finally, as in other parts of the world (Wank 2002; Gans-Morse 2017), seeking enforcement via political connections is an informal strategy firms may use to enforce their property security. Politically connected firms benefit from the preference and bias of the state, and, as such, political connections are useful for placing pressure upon the contract-breaking party to hasten resolution.⁹

Reputation and repeated interactions can play a large role in structuring informal trade in many contexts (e.g. Greif 1989), including Senegal. That said, reputational information is not always available in Senegal. The explosion of small- and medium-sized businesses in recent years due to governmental policy changes has outpaced the availability of reputational information. This type of information is also inherently unavailable for newly entering firms, and is difficult to obtain for faceless businesses such as those based abroad or online (Dellarocas 2003; Elfenbein and Lerner 2003; MacLeod 2007). Repeated interactions are also not always a possibility for securing stable trading relationships, particularly as firms become more specialized and the frequency of one-off trades increases.

⁹Even the threat of using political connections may hasten resolution.

3.4 Research design

To test the empirical implications of the theory presented above, I fielded a survey in late 2017 and early 2018 in Dakar, the capital of Senegal where the majority of the country's economic activity is concentrated.

Survey overview

The first purpose of the survey was to collect information that, to date, has been difficult to find in Senegal due to limited data availability. It thus served important descriptive purposes. To that end, the survey included a slew of questions about firms' characteristics and behavior. This included questions about formalization status, valuation, profits, number of employees, and other firm-level covariates. 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 questions on resolving previous contract disputes as well as whether and to whom firms paid taxes. This data helps to paint a clearer picture of the private sector in Senegal, across the formal and informal economies.

Survey experiment

The survey also included an experimental element intended to test the effect of contract formality on firms' enforcement strategies. Respondents were asked how likely they were to use different types of formal and informal dispute resolution strategies when another business failed to make a payment, thereby breaking an agreement that was based on a randomized formal or informal contract. This specific scenario was chosen because it is the most common basis for inter-firm dis-

putes in Senegal. The wording of the survey experiment closely mirrored a survey 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 likelihoods of using the following dispute resolution strategies on a scale of 1 to 5:¹⁰

“How likely are you to...”

- “Resolve it amicably by discussing directly with the other company?”
- “Contact someone in the other company’s social network and ask them to intervene on your behalf?”
- “Speak with someone—a connection—in government and ask them to intervene on your behalf?”
- “Take the problem to the local police?”
- “Take the problem to a religious authority?”
- “Hire a lawyer to settle the case?”
- “Use the court system?”

Responses to these questions, ranging from 1 to 5, constitute the main experimental 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

¹⁰Enumerators read the response choices out loud, which were “never,” “unlikely,” “maybe,” “likely,” and “extremely likely.”

they were participating in a survey experiment, and had no way of knowing that there were two versions of the question.¹¹

Sample

This paper uses the same sample of firm managers as Chapter 2, which can be consulted for details about the sampling strategy. I use this section to provide greater detail about the sample, with particular focus on covariates of theoretical interest that will be used later in the paper to estimate heterogeneous treatment effects.¹²

As the earlier chapters of this dissertation demonstrate, political connections can affect individuals' and firms' behavior. Firm managers in my sample generally thought that politically connected firms receive outsized advantages in the Senegalese private sector. Approximately 60% stated that they thought political connections were useful for business, with the most common explanation being that connected firms receive privileged access to state contracts and can disobey rules and regulations with impunity. In terms of direct political participation, Figure 3.5 shows that 21% of formal and 18% of informal firm managers belong to a political party. Although imperfect, I use this as a proxy for political connectedness, as those in political parties are much more likely to know people in positions of political power than those who are not.

¹¹Indeed, the treatment text was relatively subtle in nature given the overall length of the question, and may have thus been missed by some respondents. The estimates presented in this paper might therefore represent lower bounds.

¹²Although not presented in the main body because it was not of pressing theoretical interest, I show in Appendix Section C.3 that Senegalese firms differ significantly from neighboring countries in terms of business association membership (Grossman 2020). Figure C.1 shows that while approximately 24% of formal firms belong to business associations, only around 3% of informal firms do. While other studies focus on the importance of firm coordination for enforcement (Hendley 1999; Gans-Morse 2017), this mechanism may not be as relevant within Senegal's private sector.

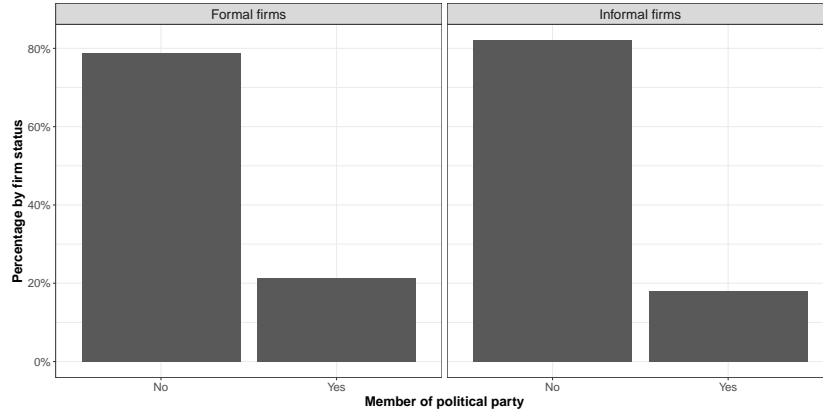


Figure 3.5: Membership in political party by firm status

As those who trust courts are more likely to use them, I also estimate heterogeneous treatment effects by confidence in courts. My sample reflects a moderate amount of confidence in courts in the abstract. Figure 3.6 shows that 53% of formal firm managers and 48% of informal firm managers have at least partial or total confidence in Senegal’s courts.

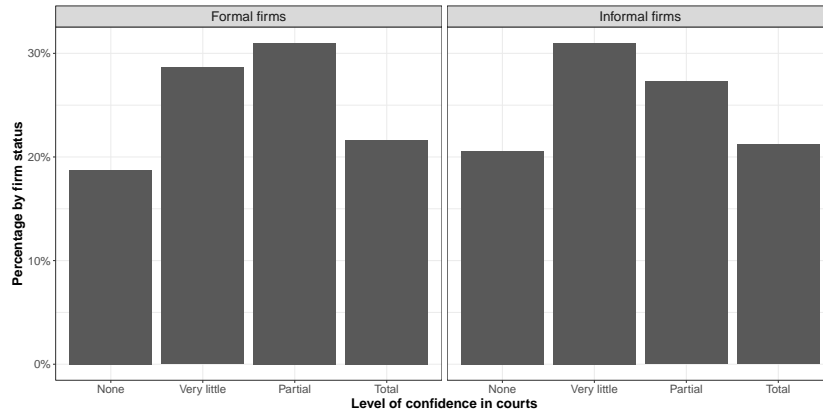


Figure 3.6: Confidence in courts by firm formality

I also estimate treatment effects based on my sample’s exposure to corrupt practices in the form of illegal tax extraction. In general, my sample was non-tax compliant. Only 69% of formal firm managers and 6% of informal firm managers stated that they declared their revenues to the relevant tax authorities within the past

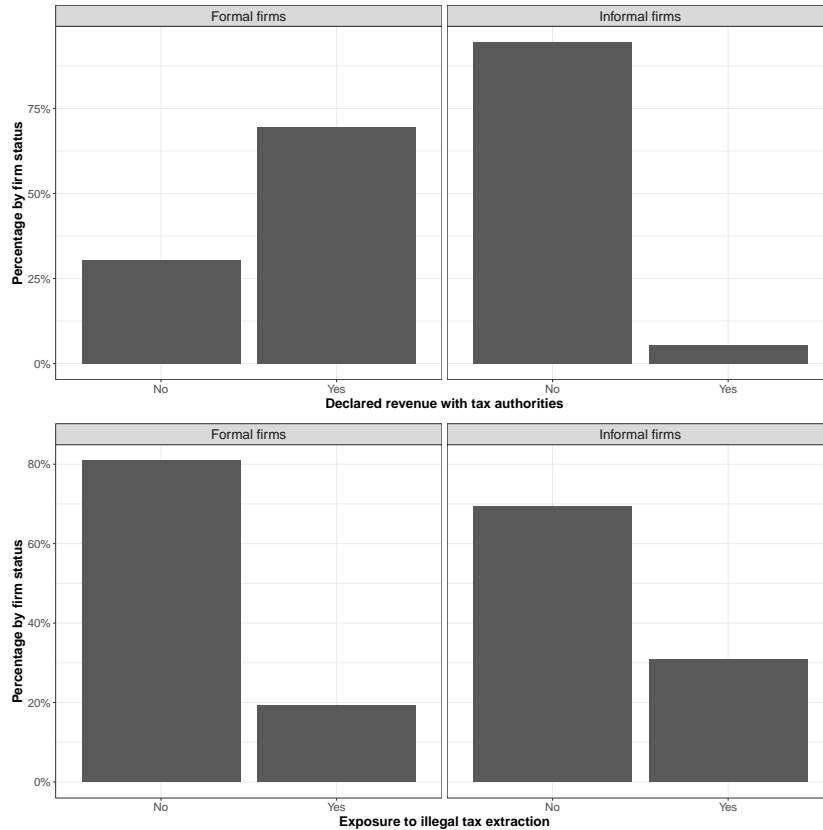


Figure 3.7: Declaration of tax revenue and exposure to illegal tax extraction

year.¹³ Respondents were also asked to whom they paid taxes or fees, if at all. I code respondents as exposed to corruption if they reported having to pay illegal taxes to government officials. As Figure 3.7 shows, 19% of formal firm managers and 30% of informal firm managers reported illegal tax extraction.

Estimation

I use OLS to estimate the impact of the formal contract treatment on firms' property rights security strategies. I estimate the following specification:

$$y_i = \alpha + \beta_1 \text{FormalContract}_i + \gamma \mathbf{X}_i + \epsilon_i$$

¹³Their willingness to admit this behavior suggests that the enumeration team was successful in convincing respondents that the research was non-government affiliated.

where y_i is a firm manager’s likelihood on a scale of 1 to 5 of using a given enforcement strategy, $FormalContract_i$ is the treatment indicator, and \mathbf{X}_i is a matrix of pre-treatment covariates. In models estimating heterogeneous treatment effects, I interact covariate X of interest with the treatment indicator.

Diagnostics

To estimate balance across the experimental groups, I regress pre-treatment covariates on treatment assignment. The results, shown below, reflect balance in the sample.

	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 (0.019)	0.026 (0.039)	0.048 (0.047)	0.072 (0.046)	0.008 (0.015)	0.068* (0.040)	0.028 (0.020)	0.416	0.769** (0.387)	0.035 (0.042)	-0.006 (0.008)	-0.007 (0.016)	-0.037* (0.020)
Constant	0.651*** (0.014)	2.293*** (0.027)	3.688*** (0.033)	4.631*** (0.032)	0.167*** (0.011)	1.881*** (0.028)	0.465*** (0.014)	6.533	33.967*** (0.271)	1.516*** (0.030)	0.039*** (0.005)	0.205*** (0.012)	0.508*** (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 3.1: Balance

There was some missing data in the form of respondents refusing to answer questions about their enforcement strategies or responding with “I don’t know.” However, this was only the case for seven responses to the 16,723 total questions (2,389 respondents answering seven questions each) and does not pose a threat to inference. I impute mean values for these missing observations, and all results are robust to excluding them.

3.5 Results

In this section I present descriptive results, average treatment effects from the survey experiment, and heterogeneous treatment effects using interactions with the covariates of interest described above.

Enforcement strategy	Mean	St. Dev.	Median	Min	Max
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
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: N=2,389.

Table 3.2: Summary table of main outcomes

Descriptive results

I first examine descriptive trends in the outcome data, averaging across the full experimental sample, to get a sense of the relative frequency of the various enforcement tactics firms use in Senegal. Table 3.2 presents summary statistics for firms' likelihood of pursuing the listed enforcement strategy. It demonstrates that by far the most common strategy is amicable resolution.¹⁴ Most firms will, by default, attempt to reach out to the defecting party before resorting to more costly options. Of the informal enforcement strategies for securing property rights, the use of social networks is the next most common, followed by religious networks, and finally political connections. Although political connections are the least used informal enforcement mechanism, this is likely due to the limited amount of firms who possess credible political connections relative to those with religious and other types of social 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 relatively high transaction costs involved with hiring lawyers or bringing matters to court.

¹⁴A key distinction between this paper and Gans-Morse (2017) is that the latter does not include this enforcement option. Past research suggests, however, that amicable resolution is firms' default strategy (Macaulay 1963).

	Outcome: Likelihood of using enforcement strategy						
	<i>Social enforcement outcomes</i>				<i>Formal enforcement outcomes</i>		
	Amicably (1)	Social network (2)	Religious network (3)	Political connection (4)	Police (5)	Lawyer (6)	Courts (7)
Formal firms	-0.099*** (0.032)	-0.388*** (0.049)	-0.304*** (0.046)	0.051 (0.033)	0.416*** (0.045)	0.874*** (0.045)	0.753*** (0.053)
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	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.” * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table 3.3: Difference in enforcement likelihood between formal and informal firms

To examine whether these strategies differ by firm formality, I regress the outcomes on an indicator variable for formal firm status. Table 3.3 presents the results and reflects the vast differences in strategies adopted by formal versus informal firms, in support of Hypothesis 2a. Formal firms are significantly less likely than informal firms to resolve disputes amicably and via social and religious networks, and significantly more likely to use the police, lawyers, and courts. The only enforcement strategy where formal and informal firms do not diverge is in the use of political connections for securing property rights. In line with earlier chapters of this dissertation, this may be due to the nature of political power and connections in Senegal, in which wealth and income are not significant predictors of political connectivity.¹⁵ Indeed, as Appendix Table C.6 shows, firm wealth does not predict political connections in the sample.

To investigate this finding more deeply, I next present descriptive results of the main outcome by the political connections of firms. Table 3.4 reveals an interest-

¹⁵Indeed, this is an argument why political connections, independent of the variables that scholars may argue political connections proxy for, are deserving of study.

	Outcome: Likelihood of using enforcement strategy						
	<i>Social enforcement outcomes</i>				<i>Formal enforcement outcomes</i>		
	Amicably (1)	Social network (2)	Religious network (3)	Political connection (4)	Police (5)	Lawyer (6)	Courts (7)
Connected firms	-0.027 (0.038)	0.251*** (0.058)	0.346*** (0.054)	0.493*** (0.038)	0.017 (0.053)	0.192*** (0.057)	0.204*** (0.065)
Unconnected firm mean	4.417	2.997	1.85	1.416	3.262	2.455	2.63
Unconnected firm std. dev.	0.748	1.154	1.028	0.67	1.063	1.12	1.271
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.” * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table 3.4: Difference in enforcement likelihood between connected and unconnected firms

ing descriptive pattern: politically connected firms are not just more likely to use legal methods of dispute resolution—conventionally thought to be how political connections are channeled—but are also more likely to use *informal* dispute resolution tactics. This suggests that, overall, politically connected firms are more likely to believe in the security of their property rights—or to believe they have stronger property rights in the first place—and to rely on third-party enforcement to enforce their claims. Indeed, politically connected firms are no more or less likely to resolve their disputes amicably than unconnected firms. Interestingly, there are no differences in seeking enforcement via the police for politically connected versus unconnected firms, perhaps suggesting that access to the police in Senegal is not necessarily a function of political power.

Experimental results

I next turn to the experimental results, first testing whether the inclusion of formal contracts affects strategies for securing property rights across the full sample,

and then estimating heterogeneous treatment effects to test whether certain groups are more likely than others to respond to the formal contract treatment. I start by presenting the average treatment effects for the full sample in Table 3.5. In line with Hypothesis 1a, firms are much more likely to seek formal enforcement methods 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 (Hypothesis 1b), suggesting that formal and informal dispute resolution tactics may indeed be substitutes in these types of disputes. Still, the magnitudes for the coefficients for informal enforcement are lower than for formal enforcement, suggesting that social enforcement strategies may still be viable and sought-after when a formal contract is the basis of a broken deal. The only enforcement strategy likelihood that remains unchanged by a formal contract is the use of political connections to seek recourse. Following the descriptive results above, this might be because firms with political connections use them to seek recourse for all types of deals gone wrong, regardless of whether the dispute is based on a formal or an informal contract.

Given the inherent differences between formal and informal firms, as well as the differences in their available strategies at baseline, I next estimate treatment effects based on firm formality status. As Table 3.6 shows, the formal contract treatment only differentially affected formal firms, and only for formal enforcement outcomes (lawyers and courts). That is, formal firms increased their use of the most transaction-cost-intensive formal enforcement methods when a formal contract was the basis of a deal. This suggests 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. That said, as the top row of Table 3.6 indicates, relative to informal contracts among informal firms, formal contracts significantly increase the willingness of informal firms to seek legal recourse as well, and mitigates their

	Outcome: Likelihood of using enforcement strategy						
	<i>Social enforcement outcomes</i>				<i>Formal enforcement outcomes</i>		
	Amicably (1)	Social network (2)	Religious network (3)	Political connection (4)	Police (5)	Lawyer (6)	Courts (7)
Deal based on formal contract	-0.244*** (0.030)	-0.366*** (0.046)	-0.169*** (0.044)	-0.027 (0.032)	0.393*** (0.042)	0.534*** (0.044)	0.574*** (0.051)
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	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 3.5: Average treatment effects: Main results of survey experiment

dependence on informal property rights security strategies. So, while formal contracts may widen the enforcement gap between formal and informal firms, they still improve the baseline for informal firms.

To test other compelling reasons firms may diverge in their enforcement of formal contracts, I estimate additional heterogeneous treatment effects. First, to build upon the other work in this dissertation and extrapolate beyond the descriptive results presented above, I estimate heterogeneous effects based on firms’ political connections, here proxied by membership in a political party.¹⁶ As Panel A of Table 3.7 shows, while connected firms use most types of enforcement mechanisms more than unconnected firms, they do not seem to respond to formal contracts differently than unconnected firms. The exception is using the police. Interestingly, connected firms are *less* likely than unconnected firms to go to the police when a

¹⁶Political connections may only matter when one’s connections are to the ruling party. Thus, I also estimate heterogeneous treatment effects by membership in the party of power and present the results in Appendix Section C.1. The results, however, are similar to those presented here.

	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 used	-0.195*** (0.052)	-0.293*** (0.078)	-0.068 (0.074)	0.029 (0.055)	0.362*** (0.071)	0.317*** (0.071)	0.325*** (0.083)
Formal firm	-0.058 (0.044)	-0.332*** (0.067)	-0.231*** (0.064)	0.093** (0.047)	0.390*** (0.061)	0.717*** (0.060)	0.568*** (0.071)
Formal contract used × formal firm	-0.071 (0.063)	-0.097 (0.096)	-0.143 (0.091)	-0.086 (0.067)	0.034 (0.088)	0.298*** (0.087)	0.352*** (0.103)
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	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 3.6: Heterogeneous effects by firm formality

formal contract is broken.

Panel B of Table 3.7 examines effects by respondents’ trust in courts. The coefficients for the trust variable reflect that respondents with higher trust in courts were more likely to prefer lawyers and courts as formal enforcement solutions, and amicable solutions and mutual social networks as informal solutions. Interestingly, 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 counterintuitively more likely to seek out religious networks when a formal contract was used in exchange. These heterogeneous effects cut against the expectations of Hypothesis 3b, as I expected those

	Outcome: Likelihood of using enforcement strategy						
	<i>Social enforcement outcomes</i>				<i>Formal enforcement outcomes</i>		
	Amicably (1)	Social network (2)	Religious network (3)	Political connection (4)	Police (5)	Lawyer (6)	Courts (7)
Panel A: By political connections							
Formal contract used	-0.241*** (0.034)	-0.362*** (0.051)	-0.179*** (0.048)	-0.031 (0.034)	0.440*** (0.047)	0.566*** (0.050)	0.597*** (0.057)
Connected firms	-0.022 (0.052)	0.250*** (0.079)	0.312*** (0.075)	0.473*** (0.053)	0.135* (0.073)	0.272*** (0.077)	0.263*** (0.088)
Formal contract used × Connected firms	-0.015 (0.075)	-0.008 (0.114)	0.066 (0.108)	0.040 (0.076)	-0.233** (0.105)	-0.149 (0.110)	-0.105 (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 used	-0.125** (0.054)	-0.312*** (0.082)	-0.322*** (0.078)	-0.061 (0.057)	0.423*** (0.076)	0.486*** (0.079)	0.492*** (0.089)
Trust in courts	0.084*** (0.021)	0.090*** (0.031)	-0.084*** (0.030)	-0.011 (0.022)	0.032 (0.029)	0.108*** (0.030)	0.176*** (0.034)
Formal contract used × Trust in courts	-0.078*** (0.029)	-0.037 (0.044)	0.100** (0.042)	0.022 (0.031)	-0.020 (0.041)	0.029 (0.043)	0.049 (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 used	-0.162*** (0.034)	-0.303*** (0.052)	-0.165*** (0.050)	-0.042 (0.036)	0.290*** (0.048)	0.433*** (0.050)	0.482*** (0.057)
Exposure to corruption	0.009 (0.050)	-0.139* (0.077)	-0.151** (0.073)	-0.210*** (0.053)	0.020 (0.070)	-0.090 (0.074)	0.075 (0.085)
Formal contract used × Exposure to corruption	-0.348*** (0.071)	-0.260** (0.108)	-0.009 (0.103)	0.075 (0.075)	0.435*** (0.099)	0.431*** (0.105)	0.388*** (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: See Table 3.6.

Table 3.7: Heterogeneous treatment effects

who most trusted courts to differentially make appeals to legal institutions during disputes based on formal contracts.

Finally, 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. Contrary to Hypothesis 3a, I find that firms 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.¹⁷ While I am unable to specify the precise mechanism by which this effect occurs, I speculate that it 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, i.e. when a deal is based on a formal contract.

Ruling out alternative explanations

The most probable confounding variables for the results presented above are firms' wealth and size, which has already been established in Figure 2.3 in Chapter 2 to be slightly correlated with firms' formality status. I thus reestimate the main findings with controls for these variables. The results are presented in Appendix Section C.2 and show that results remain generally unchanged both substantively and statistically with these controls.

3.6 Conclusion

Contract formality can drastically affect the strategies that firms use to secure their property rights. Using a survey experiment with a large sample of formal and

¹⁷A similarly counterintuitive result comes from Le, Malesky and Pham (2019), who find that businesses exposed to local corruption in Vietnam had a higher likelihood of tax registration.

informal firms in Senegal, I find that formal contracts increase firms' reliance on state institutions for enforcement and decrease reliance on informal enforcement strategies. I also find evidence that firms' formality status predicts their enforcement strategies, and that formal contracts differentially move formal firms toward state-backed methods for securing their property rights.

These results are potentially promising for interventions that aim to increase access to the formal sector in developing countries: formal contracts lead to greater use of state institutions. However, an unintended consequence of increasing access to formal contracts appears to be a widening of the enforcement gap between formal and informal firms. This contrasts with other research showing more universal benefits of increasing access to formal law (Sandefur and Siddiqi 2013). Still, these results may have positive implications for the state-building process. The continued and persistent use of state institutions reinforces their legitimacy and effectiveness (Pistor 1996; Migdal 2001). Understanding the factors that motivate firms to use these state institutions over enforcing property rights "off the books" may thus help to ultimately strengthen these property rights institutions. While state-building is not an exclusively demand-driven process, states' efforts to lower the barriers of access to legal institutions may encourage an equilibrium of better quality of rule of law.

The descriptive results in this paper help to illuminate firms' enforcement strategies under imperfect rule of law, while underscoring the complexities of navigating the (in)formalities of the private sector in such environments. These results may also help to understand formal and informal property rights security in other contexts. Many studies on formal and informal property security strategies 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 most citizens think to be useless (e.g. McMillan and Woodruff 1999a). However, 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 may thus travel to these contexts.

This paper shows that formal contracts can increase firms' reliance on state-backed property security strategies. Increased use of state enforcement institutions, however, does not solve more systemic issues of biased legal enforcement. Future research would thus be well served to investigate not just the use of enforcement institutions, but the outcomes these institutions deliver to disputing parties. And while this paper contributes to the evidence base explaining firms' use of formal and informal property security strategies, a more unified theory is needed to explain variation in such strategies across contexts. Finally, the inconclusive heterogeneous treatment effects for political connections suggest additional research is needed to probe the mechanisms by which connections and contracts interact. Do politically connected firms prefer informal contracts because the vagueness allows them to more easily benefit from the bias of state and nonstate institutions, or do they prefer formal contracts that allow them to make direct appeals to the state institutions that privilege them? Such questions pose important implications for policy intended to strengthen formal institutions and private sectors under selective rule of law.

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Appendices to Chapter 1

A.1 Transaction protocol

The following is the translated protocol outline that enumerators followed during transactions. While enumerators adhered to the essential substantive elements, the surrounding language was extensively practiced and modified in Wolof in order to appear as natural as possible, as well as to adapt to buyers' interjections.

Hello, I'm _____ (introductions continue at length).

[Political connection treatment]: I recently finished working for the [Golf Sud / Pikine / Medina] Municipal Council, where I worked closely with political staff and developed solid relationships. I had a great experience, and got to know the people in charge.

[All groups:] I'm now part of an exciting new business called Porte-à-Porte Sénégal, which has been registered for almost a year. We're offering a new and exclusive deal because we want to develop our client base. We offer phone credit subscriptions at a discounted rate, and offer savings to our customers. Our subscription offers are detailed on this sheet. [Talk through the options.] For maximal savings, you pay us now, and we deliver the phone credit to you in three days.

[Required contract treatment:] Because we're a formal business, we can only do this deal if you sign this contract, which was approved by a lawyer. The conditions are detailed in the contract, including what happens in case of a contract breach. [Show them the contract and answer any questions.]

[**Optional contract treatment:**] We are a formal business. If you accept the deal, we can also offer you this contract which was approved by a lawyer, for a small additional fee of 50CFA. You have the choice to accept or decline the contract. The conditions are detailed in the contract, including what happens in case of a contract breach. [Show them the contract and answer any questions.]

[**All groups:**] Would you like to take advantage of this deal? What subscription level would you like?

A.2 Formal contract

The formal contract included a clause on conflict resolution in the case of contract breach. The English translation of this clause is below:

“The parties expressly agree that any dispute or controversy arising out of or in connection with this Agreement, including its interpretation, performance, or breach, occurring during or as a result of its execution, shall be settled amicably, and, in the event of persistent disagreement, brought before the competent courts of Senegal.”

A.3 Business registration document

REPUBLIQUE DU SENEGAL
MINISTRE DE L'ECONOMIE
ET DES FINANCES

ORIGINAL

Décret N° 2012 - 886 du 27/08/2012
abrogeant et remplaçant le décret
N° 95 - 364 du 14/04/1995

AVIS D'IMMATRICULATION

Le numéro ci-dessous vous est définitivement attribué à la suite des modifications intervenues dans le nouveau système
d'immatriculation.

N.I.N.E.A : 006010714

DATE D'IMMATRICULATION : 20/07/2016

DENOMINATION	[REDACTED]		
ENSEIGNE / SIGLE	PORTE-A-PORTE SENEGAL - P.A.P.S.		
ADRESSE/BP	MERMOZ CITÉ SOTRAC VILLA N°126 /		
LOCALITE	DAKAR	TELEPHONE	772499202



CENTRE FISCAL	DAKAR-LIBERTE		
CONTROLE	1		
FORME JURIDIQUE	ENTREPRISE INDIVIDUELLE		
ACTIVITE PRINCIPALE	AUTRES COMMERCES DE DETAIL HORS MAGASIN		
AUTORISATION MINISTERIELLE (POUR ASSOCIATION)			
REGISTRE DE COMMERCE	SN DKR 2016 A 17368		
DATE DE CREATION	19/07/2016		
CAPITAL SOCIAL		CHIFFRE D'AFFAIRES	
EFFECTIF TOTAL	0	NOMBRE D'ETABLISSEMENTS SECONDAIRES	

En cas de désaccord sur les renseignements portés sur cet avis, veuillez y apporter les rectifications souhaitées et le retourner à :

SERVICE REGIONAL DE LA STATISTIQUE ET DE LA DEMOGRAPHIE DE DAKAR

Rocade Fann Bel-Air Cerf-Volant BP 116 Dakar
RP - SENEGAL

Le NINEA doit obligatoirement figurer sur toutes les quittances, factures ou lettres reçues ou établies par vous et sur les actes, déclarations ou pièces produits, émis ou passés dans vos relations avec les Administrations Publiques ou Privées et les Entreprises. Il vous est par conséquent demandé de prendre les dispositions utiles pour vous conformer à la législation.



DAKAR, le 20/07/2016

Lamine NDIAYE

Figure A.1: Business registration from APIX

A.4 Balance

Table A.1: Covariate balance across treatment groups

Covariate	Observations	1. Control mean	2. Required contract	3. Optional contract	4. Connection signal	5. Connection + required contract	6. Connection + optional contract	F-test two-sided p-value
Gender	1458	0.519 (0.032)	+0.029 (0.045)	+0.025 (0.045)	-0.025 (0.045)	+0.045 (0.045)	-0.033 (0.045)	0.89
Education	1458	3.684 (0.111)	+0.104 (0.155)	+0.073 (0.155)	+0.126 (0.155)	+0.121 (0.155)	+0.179 (0.155)	0.50
Age	1458	35.331 (0.867)	-0.54 (1.214)	+0.556 (1.216)	-1.256 (1.214)	+0.519 (1.214)	+0.047 (1.215)	0.79
Employment	1458	0.683 (0.031)	+0.029 (0.043)	-0.037 (0.043)	-0.038 (0.043)	+0.012 (0.043)	-0.015 (0.043)	0.35
Student	1458	0.087 (0.022)	+0.039 (0.03)	+0.035 (0.03)	+0.051* (0.03)	+0.026 (0.03)	+0.052* (0.03)	0.12
Trust council	1458	1.649 (0.081)	+0.059 (0.113)	+0.184 (0.114)	+0.171 (0.113)	+0.098 (0.114)	+0.209* (0.114)	0.04**
Trust courts	1458	1.96 (0.071)	+0.091 (0.098)	+0.123 (0.099)	-0.048 (0.098)	-0.049 (0.099)	-0.045 (0.099)	0.67
Use courts	1458	0.031 (0.012)	+0.011 (0.017)	-0.005 (0.017)	-0.001 (0.017)	+0.012 (0.017)	+0.003 (0.017)	0.95

Notes: Coefficients are estimated by regressing covariates on treatment group using OLS. Standard deviations are listed for the control group, and standard errors are listed for the treatment groups. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

A.5 Attrition not predicted by treatment

Table A.2: Attrition as predicted by treatment arm

	Attrited (1)
Political connection signal	-0.005 (0.008)
Formal contract (required)	-0.013 (0.009)
Formal contract (optional)	-0.002 (0.009)
Control mean	0.046
Control std. dev.	0.209
Fixed effects	Yes
Controls	Yes
Observations	1,458

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Notes: The specification is estimated using OLS. The outcome is whether a respondent is recorded as attrited. The specification includes randomization block and enumerator fixed effects, and controls for gender, age, education, employment status, and student status. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

A.6 Ordered probit

Table A.3: Ordered probit results

	Outcome: Purchase level					
	ATEs		Heterogeneous effects by buyer connections			
	Unpooled (1)	Pooled (2)	(3)	(4)	Pooled (5)	(6)
Political connection signal	-0.147 (0.137)	-0.147* (0.103)	0.014 (0.142)	0.031 (0.120)	0.027 (0.120)	
Required contract	0.243* (0.139)	0.211* (0.119)	0.044 (0.176)	0.072 (0.129)		0.070 (0.129)
Optional contract	0.062 (0.143)					
Buyer connected			0.162 (0.147)	0.122 (0.134)	0.264** (0.120)	-0.016 (0.105)
Political connection signal × required contract	0.136 (0.000)	0.136 (0.153)	0.051 (0.240)			
Political connection signal × optional contract	0.001 (0.182)					
Political connection signal × buyer connected			-0.342* (0.201)	-0.263* (0.162)	-0.255 (0.161)	
Required contract × buyer connected			0.286 (0.242)	0.381** (0.170)		0.380** (0.170)
Political connection signal × required contract × buyer connected			0.191 (0.342)			
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,458	1,458	1,458	1,458	1,458	1,458

Notes: Each specification is estimated using ordered probit. The outcome is the level of purchase chosen (0 to 3). Specifications include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status, and interactive controls between treatments and buyer/seller co-ethnicity/co-religiosity. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests and pre-specified one-sided tests.

A.7 Contract choice not motivated by political connections

The optional treatment group did not increase trade relative to the control group. Among the buyers in the optional contract group who opted for the contract, however, did sellers' political connections influence their decisions? And did buyers' own political connections factor into the contract choice decisions? Table A.4 shows that among this non-experimental subsample, choosing the contract was not significantly associated with the political connection treatment nor one's own political connectivity.

Table A.4: Contract choice as predicted by treatment

	Outcome: Chose contract (1)
Political connection signal	0.154 (0.286)
Buyer connected	0.197 (0.394)
Political connection signal × buyer connected	−0.707 (0.482)
Observations	137
Fixed effects	Yes
Controls	Yes

Notes: The specification is estimated using OLS. The outcome is whether a respondent chose the formal contract. The specification includes randomization block and enumerator fixed effects, and controls for gender, age, education, employment status, and student status. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

A.8 Table form of heterogeneous effects by buyers' connections

Table A.5: Heterogeneous effects by buyers' connections

	Outcome: Purchase level			
	Both treatments (1)	(2)	Connections only (3)	Contracts only (4)
Political connection signal	0.008 (0.085)	0.013 (0.071)	0.009 (0.071)	
Required contract	0.072 (0.103)	0.082 (0.075)		0.081 (0.075)
Buyer connected	0.108 (0.088)	0.088 (0.080)	0.146** (0.073)	0.014 (0.063)
Political connection signal × required contract	0.017 (0.141)			
Political connection signal × buyer connected	-0.186* (0.121)	-0.145* (0.098)	-0.137* (0.098)	
Required contract × buyer connected	0.130 (0.145)	0.186* (0.102)		0.184* (0.102)
Political connection signal × required contract × buyer connected	0.112 (0.207)			
Control outcome mean	0.573	0.583	0.553	0.560
Control outcome std. dev.	0.871	0.871	0.871	0.871
Fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,458	1,458	1,458	1,458

Notes: Each specification is estimated using OLS. The outcome is the level of purchase chosen (0 to 3). Specifications include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status, and interactive controls between treatments and buyer/seller co-ethnicity/co-religiosity. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests and pre-specified one-sided tests.

A.9 Dichotomous coding of outcome

ATE estimates

Table A.6: Average treatment effects with dichotomous outcome coding

	Unpooled		Pooled	
	(1)	(2)	(3)	(4)
OUTCOME: PURCHASED AT ALL				
Political connection signal	-0.053*	-0.032	-0.043*	-0.032
	(0.042)	(0.026)	(0.031)	(0.026)
Required contract	0.046	0.068**	0.048	0.065**
	(0.043)	(0.032)	(0.037)	(0.028)
Optional contract	-0.004	0.007		
	(0.043)	(0.032)		
Political connection signal × required contract	0.044		0.034	
	(0.055)		(0.048)	
Political connection signal × optional contract	0.020			
	(0.055)			
OUTCOME: PURCHASED WITH DELAY				
Political connection signal	0.001	-0.010	-0.014	-0.010
	(0.036)	(0.023)	(0.027)	(0.023)
Required contract	0.104***	0.102***	0.096***	0.102***
	(0.037)	(0.028)	(0.035)	(0.028)
Optional contract	0.058	0.044		
	(0.037)	(0.028)		
Political connection signal × required contract	-0.003		0.011	
	(0.048)		(0.041)	
Political connection signal × optional contract	-0.029			
	(0.047)			
Fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,458	1,458	1,458	1,458

Notes: Each specification is estimated using OLS. In the top panel the outcome is a dummy for whether a buyer purchased any level of mobile phone credit, and in the bottom panel a dummy for whether the purchase level involved delayed delivery. Specifications include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status, and interactive controls between treatments and buyer/seller co-ethnicity/co-religiosity. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests and pre-specified one-sided tests.

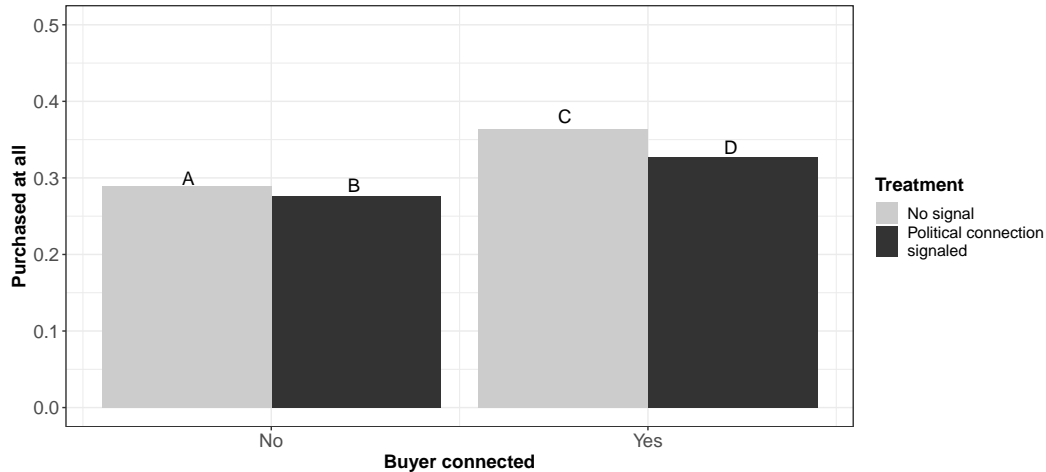
Heterogeneous effects

Table A.7: Heterogeneous effects with dichotomous outcome coding

	Both treatments		Connections only	Contracts only
	(1)	(2)	(3)	(4)
OUTCOME: PURCHASED AT ALL				
Political connection signal	-0.011 (0.044)	-0.006 (0.036)	-0.007 (0.036)	
Required contract	0.004 (0.052)	0.012 (0.038)		0.013 (0.038)
Buyer connected	0.036 (0.045)	0.029 (0.041)	0.063* (0.037)	0.001 (0.032)
Political connection signal ×required contract	0.015 (0.072)			
Political connection signal ×buyer connected	-0.068 (0.062)	-0.054 (0.050)	-0.051 (0.050)	
Required contract ×buyer connected	0.085 (0.074)	0.104** (0.052)		0.103** (0.052)
Political connection signal ×required contract×buyer connected	0.039 (0.106)			
OUTCOME: PURCHASED WITH DELAY				
Political connection signal	0.026 (0.037)	0.014 (0.031)	0.012 (0.031)	
Required contract	0.072 (0.045)	0.055* (0.033)		0.055* (0.033)
Buyer connected	0.059 (0.039)	0.043 (0.035)	0.058* (0.032)	0.018 (0.027)
Political connection signal ×required contract	-0.034 (0.062)			
Political connection signal ×buyer connected	-0.081* (0.053)	-0.049 (0.043)	-0.045 (0.043)	
Required contract ×buyer connected	0.004 (0.064)	0.049 (0.045)		0.049 (0.045)
Political connection signal ×required contract×buyer connected	0.090 (0.091)			
Fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	1,458	1,458	1,458	1,458

Notes: Each specification is estimated using OLS. In the top panel the outcome is a dummy for whether a buyer purchased any level of mobile phone credit, and in the bottom panel a dummy for whether the purchase level involved delayed delivery. Specifications include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status, and interactive controls between treatments and buyer/seller co-ethnicity/co-religiosity. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests and pre-specified one-sided tests.

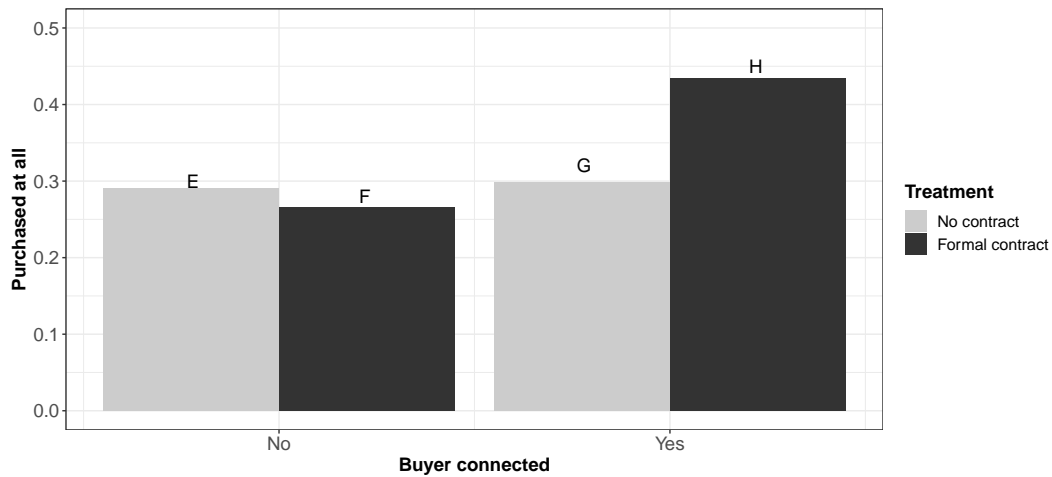
Figures for purchased at all



Panel A: Group means		Mean (std. dev.)
A. Unconnected seller and unconnected buyer ($n = 346$)		0.289 (0.454)
B. Connected seller and unconnected buyer ($n = 362$)		0.276 (0.448)
C. Unconnected seller and connected buyer ($n = 360$)		0.364 (0.482)
D. Connected seller and connected buyer ($n = 354$)		0.328 (0.470)
Panel B: Difference tests		Estimate (std. error)
Effect of connection signal for connected buyers (D–C)		-0.082 (0.042)**
Effect of connection signal for unconnected buyers (B–A)		-0.019 (0.042)
Most powerful buyer – least powerful seller (C–B)		0.045 (0.047)
Connected buyer – unconnected buyer [C+D]–[A+B]		0.038 (0.027)
Difference-in-differences [D–C]–[B–A]		-0.051 (0.049)

Notes: Panel A presents group means of the four subgroups along and their standard deviations. Panel B presents differences and standard errors estimated using OLS with linear restrictions. Difference tests include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status, and an interaction between treatment and buyer/seller co-ethnicity/co-religiosity. The outcome is the level of purchase chosen (0 to 3). * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests and pre-specified one-sided tests.

Figure A.2: Political connection treatment effects by buyer and seller connections - purchased at all

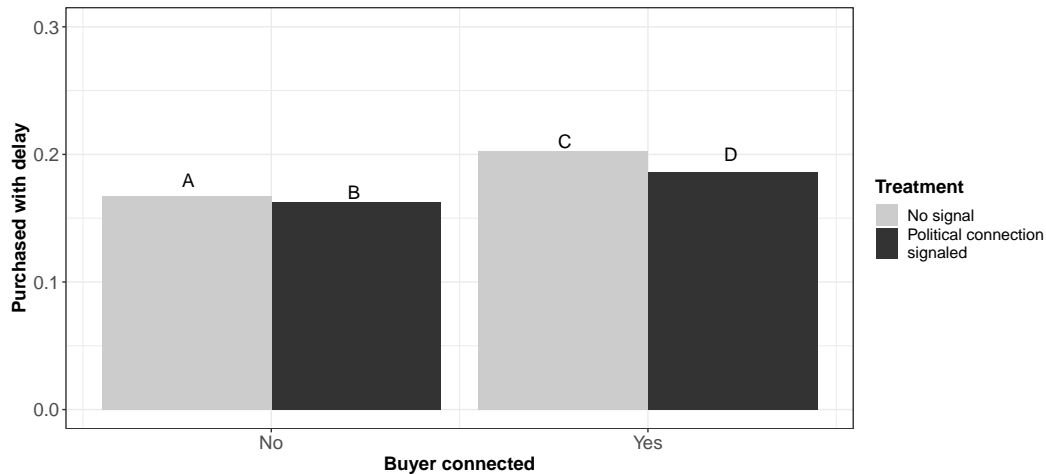


Panel A: Group means		Mean (std. dev.)
E. No contract and unconnected buyers ($n = 479$)		0.290 (0.454)
F. Formal contract and unconnected buyers ($n = 229$)		0.266 (0.443)
G. No contract and connected buyers ($n = 465$)		0.299 (0.458)
H. Formal contract and connected buyers ($n = 249$)		0.434 (0.497)
Panel B: Difference tests		Estimate (std. error)
Effect of formal contract for connected buyers (H–G)		0.114 (0.043)***
Effect of formal contract for unconnected buyers (F–E)		0.022 (0.039)
Difference-in-differences [H–G]–[F–E]		0.103 (0.052)**

Notes: Panel A presents group means of the four subgroups along and their standard deviations. Panel B presents differences and standard errors estimated using OLS with linear restrictions. Difference tests include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status, and an interaction between treatment and buyer/seller co-ethnicity/co-religiosity. The outcome is the level of purchase chosen (0 to 3). * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests and pre-specified one-sided tests.

Figure A.3: Formal contract treatment effects by buyers' connections - purchased at all

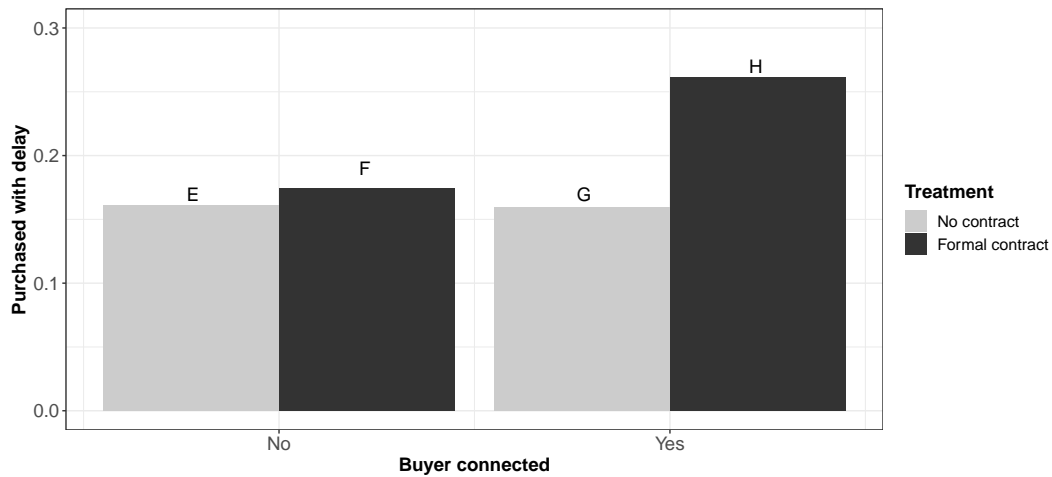
Figures for purchased with delay



Panel A: Group means		Mean (std. dev.)
A. Unconnected seller and unconnected buyer ($n = 346$)		0.168 (0.374)
B. Connected seller and unconnected buyer ($n = 362$)		0.163 (0.370)
C. Unconnected seller and connected buyer ($n = 360$)		0.203 (0.403)
D. Connected seller and connected buyer ($n = 354$)		0.186 (0.390)
Panel B: Difference tests		Estimate (std. error)
Effect of connection signal for connected buyers (D–C)		-0.045 (0.037)
Effect of connection signal for unconnected buyers (B–A)		0.014 (0.036)
Most powerful buyer – least powerful seller (C–B)		0.048 (0.040)
Connected buyer – unconnected buyer $[C+D]-[A+B]$		0.035 (0.023)
Difference-in-differences $[D-C]-[B-A]$		-0.045 (0.043)

Notes: Panel A presents group means of the four subgroups along and their standard deviations. Panel B presents differences and standard errors estimated using OLS with linear restrictions. Difference tests include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status, and an interaction between treatment and buyer/seller co-ethnicity/co-religiosity. The outcome is the level of purchase chosen (0 to 3). * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests and pre-specified one-sided tests.

Figure A.4: Political connection treatment effects by buyer and seller connections - purchased with delay



Panel A: Group means		Mean (std. dev.)
E. No contract and unconnected buyers ($n = 479$)		0.161 (0.368)
F. Formal contract and unconnected buyers ($n = 229$)		0.175 (0.381)
G. No contract and connected buyers ($n = 465$)		0.159 (0.366)
H. Formal contract and connected buyers ($n = 249$)		0.261 (0.440)
Panel B: Difference tests		Estimate (std. error)
Effect of formal contract for connected buyers (H–G)		0.097 (0.038)**
Effect of formal contract for unconnected buyers (F–E)		0.043 (0.033)
Difference-in-differences [H–G]–[F–E]		0.049 (0.045)

Notes: Panel A presents group means of the four subgroups along and their standard deviations. Panel B presents differences and standard errors estimated using OLS with linear restrictions. Difference tests include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status, and an interaction between treatment and buyer/seller co-ethnicity/co-religiosity. The outcome is the level of purchase chosen (0 to 3). * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests and pre-specified one-sided tests.

Figure A.5: Formal contract treatment effects by buyers' connections - purchased with delay

A.10 Afrobarometer: Trust in courts

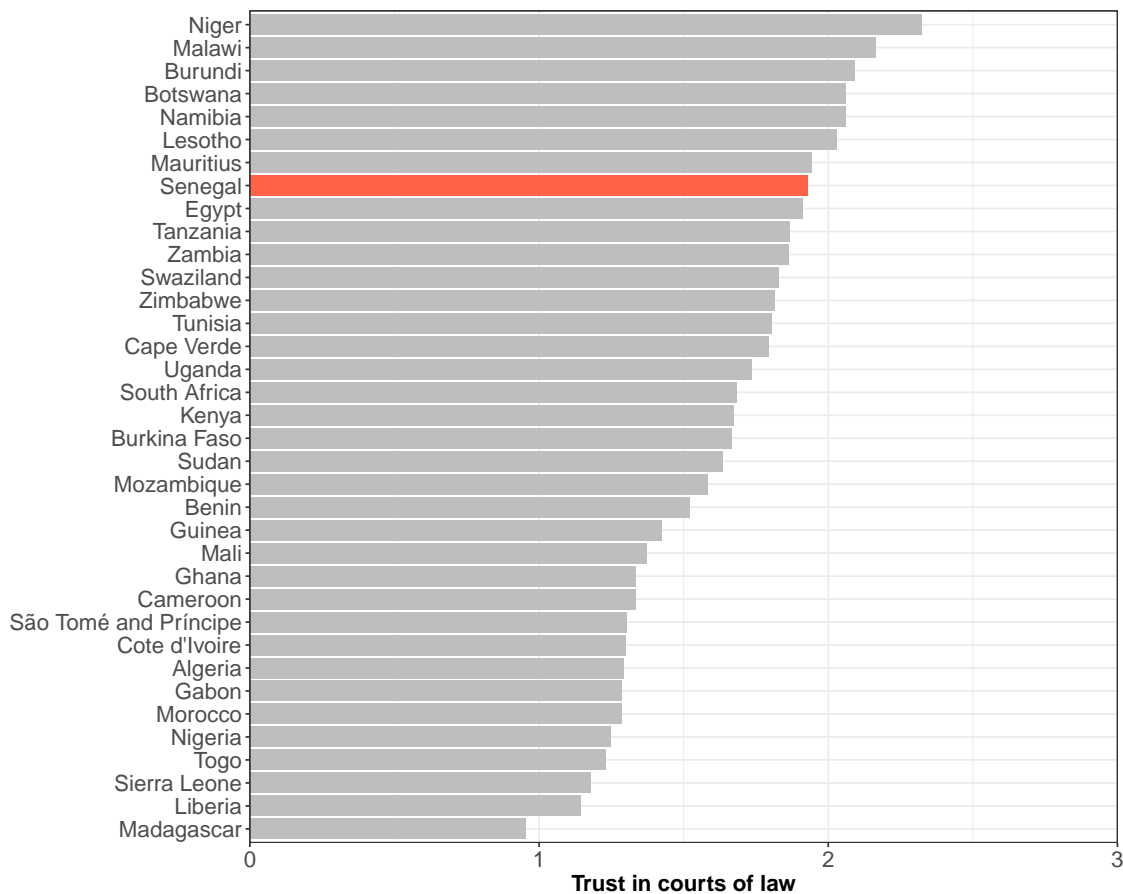


Figure A.6: Trust in courts. Source: Afrobarometer 2016.

A.11 Correlates of buyers' political connections

Table A.8: Correlation table for buyer political connections and covariates

	Buyer connected	Gender	Education	Employed	Age	Student
Buyer connected	1	0.001	0.056	0.040	0.032	-0.023
Gender		1	-0.058	-0.203	0.038	-0.066
Education			1	-0.299	-0.257	0.480
Employed				1	0.138	-0.534
Age					1	-0.340
Student						1

A.12 Heterogeneous effects by in-group

Table A.9: Heterogeneous effects by co-ethnicity and shared religious network

	Outcome: Purchase level	
	(1)	(2)
Political connection signal	-0.065 (0.064)	-0.057 (0.051)
Formal contract	0.162** (0.077)	0.174*** (0.051)
Co-ethnic/co-religious	0.008 (0.103)	-0.023 (0.095)
Political connection signal × formal contract	0.025 (0.109)	
Political connection signal × co-ethnic/co-religious	-0.011 (0.137)	0.054 (0.113)
Formal contract × co-ethnic	-0.244 (0.167)	-0.146 (0.119)
Political connection signal × formal contract × co-ethnic/co-religious	0.204 (0.237)	
Control outcome mean	0.497	0.497
Control outcome std. dev.	0.864	0.864
Fixed effects	Yes	Yes
Controls	Yes	Yes
Observations	1,458	1,458

Notes: Each specification is estimated using OLS. The outcome is the level of purchase chosen (0 to 3). Specifications include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

A.13 Heterogeneous effects by district match

Table A.10: District match and findability mechanisms

	Outcome: Purchase level			
	Subsetted data		Heterogeneous effects	
	(1)	(2)	(3)	(4)
Political connection signal			-0.072 (0.067)	-0.053 (0.054)
Contract		0.208** (0.084)	0.113 (0.082)	0.142** (0.057)
District match	1.351 (1.045)	1.357 (1.039)	0.907 (0.694)	0.900 (0.693)
Political connection signal × contract			0.058 (0.115)	
Political connection signal × district match			0.012 (0.115)	0.025 (0.093)
Contract × district match		0.005 (0.143)	-0.018 (0.141)	0.002 (0.099)
Political connection signal × contract × district match			0.040 (0.199)	
Control outcome mean	0.546	0.502	0.544	0.544
Control outcome std. dev.	0.905	0.878	0.910	0.910
Fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	729	729	1,458	1,458

Notes: District match is coded 1 when the seller's connection is from the buyer's home district. Each specification is estimated using OLS. The outcome is the level of purchase chosen (0 to 3). In Models 1 and 2, I subset the data to only the transactions in which sellers signaled their connections. Models 3 and 4 include the full sample and estimate heterogeneous effects by district match. Specifications include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

A.14 Treatments did not raise buyer's suspicions or skepticism

Political connection treatment

Table A.11: Buyer suspicion as predicted by political connection treatment

	Outcome: Buyer's suspicion (1)
Political connection signal	0.043 (0.062)
Control outcome mean	0.750
Control outcome std. dev.	1.214
Fixed effects	Yes
Controls	Yes
Observations	1,458
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01

Notes: The specification is estimated using OLS. The outcome is the level of the buyer's suspicion. The specification includes randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Optional contract treatment

Table A.12: Buyer suspicion as predicted by optional contract treatment

	Outcome: Buyer's suspicion (1)
Optional contract	0.060 (0.066)
Control outcome mean	0.754
Control outcome std. dev.	1.232
Fixed effects	Yes
Controls	Yes
Observations	1,458
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01

Notes: The specification is estimated using OLS. The outcome is the level of the buyer's suspicion. The specification includes randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

A.15 No overreporting of connections

The following table demonstrates that people to whom sellers signaled political connections did not overstate their own political connections.

Table A.13: Buyers' political connections as predicted by political connection treatment

	Outcome: Buyer is politically connected (1)
Political connection treatment	-0.011 (0.024)
Control outcome mean	0.510
Control outcome std. dev.	0.493
Fixed effects	Yes
Controls	Yes
Observations	1,458
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01

Notes: The specification is estimated using OLS. The outcome is whether the buyer reported any political connection. The specification includes randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, and student status. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

A.16 Deviations from pre-analysis plan

The analyses presented in this paper follow the pre-analysis plan, with the following minor exceptions:

1. The pre-analysis plan mistakenly omitted enumerator fixed effects, which I correct here. Enumerator fixed effects are important for ensuring that variation in enumerators does not drive results.
2. Due to the sensitive timing of the survey as well as the wishes of the municipal councils, the final survey did not include questions about respondents' political affiliation, which were thus not included as control variables. However, employment status and student status—predictors of financial status—were added as controls as they affect the likelihood of a buyer having enough money to purchase the deal. An interaction term between treatment and buyer/seller coethnicity was also added due to the potential confounding influence of shared social networks. The changes do not affect my conclusions, however.
3. Due to space constraints, some of the secondary tests mentioned in the pre-analysis plan were not presented in the main body.

Appendices to Chapter 2

B.1 Summary statistics of sample

Variable	Mean	Std. Dev.	Min	Median	Max	Other	Refuse	Don't know
Gender	0.220	0.414	0	0	1	0	0	0
Age	34.346	9.468	17	33	76	0	0	1
Education	1.569	1.067	0	2	3	20	0	0
Formal firm	0.662	0.473	0	1	1	0	0	0
Belong to business association	0.170	0.376	0	0	1	0	0	0
Meetings with other businesses	0.456	0.498	0	0	1	0	0	0
Access to credit	1.915	0.980	1	2	4	0	1	8
Declared revenue for taxes	0.478	0.500	0	0	1	0	3	1
Negotiated tax payment	0.345	0.476	0	0	1	0	3	1
Amount of tax paid	2.584	1.552	0	2	9	0	6	2
Confidence in courts	1.534	1.032	0	2	3	0	1	0
Experienced contract dispute	0.329	0.470	0	0	1	0	0	0
Frequency of business with formal firms	3.248	1.491	1	3	7	0	0	1
Previously worked for the state	0.036	0.186	0	0	1	0	0	0
Political connections useful	0.594	0.491	0	1	1	0	0	1
Political connections help break contracts	0.527	0.499	0	1	1	0	2	5
Member of political party	0.201	0.401	0	0	1	0	4	0
Member of civil association	0.490	0.500	0	0	1	0	0	0
Contacted politician in past	0.030	0.171	0	0	1	0	0	0

Notes: N=2,389 for all variables. The final three columns list the number of respondents who responded “other,” “refuse to respond,” and “I don’t know” to the survey item. The reported statistics elsewhere in the table do not include these respondents.

Table B.1: Sample summary statistics

B.2 Deviations from pre-analysis plan

There were no deviations from the pre-analysis plan (PAP) with regard to data collection. The analysis presented in the main body of the paper also adheres to the PAP, though is not fully comprehensive due to space constraints. However, all results indicated in the PAP as “primary results of the project” are included in the main body. The other deviations are summarized below:

- Section 3 of the PAP referred to an interaction with the formality status of the firm as an ACIE, though it is a conditional ACME. I correct this mistake in the paper.
- For certain subgroup analyses (e.g. in-group identity), I opted to present graphical representations in the main body and the table representations in the appendix, instead of the converse as the PAP indicated. This decision was made for ease of interpreting results.

B.3 Conjoint experiment appearance on tablet

Figure B.1 shows the conjoint experiment as it appeared to respondents on tablets.

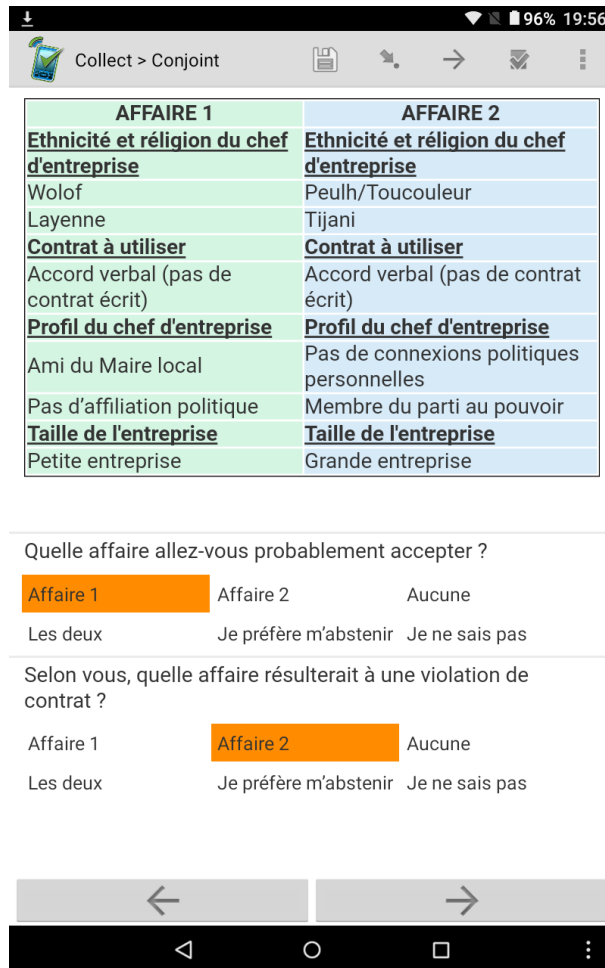


Figure B.1: Screenshot of conjoint experiment as it appeared to respondents

B.4 Full specification

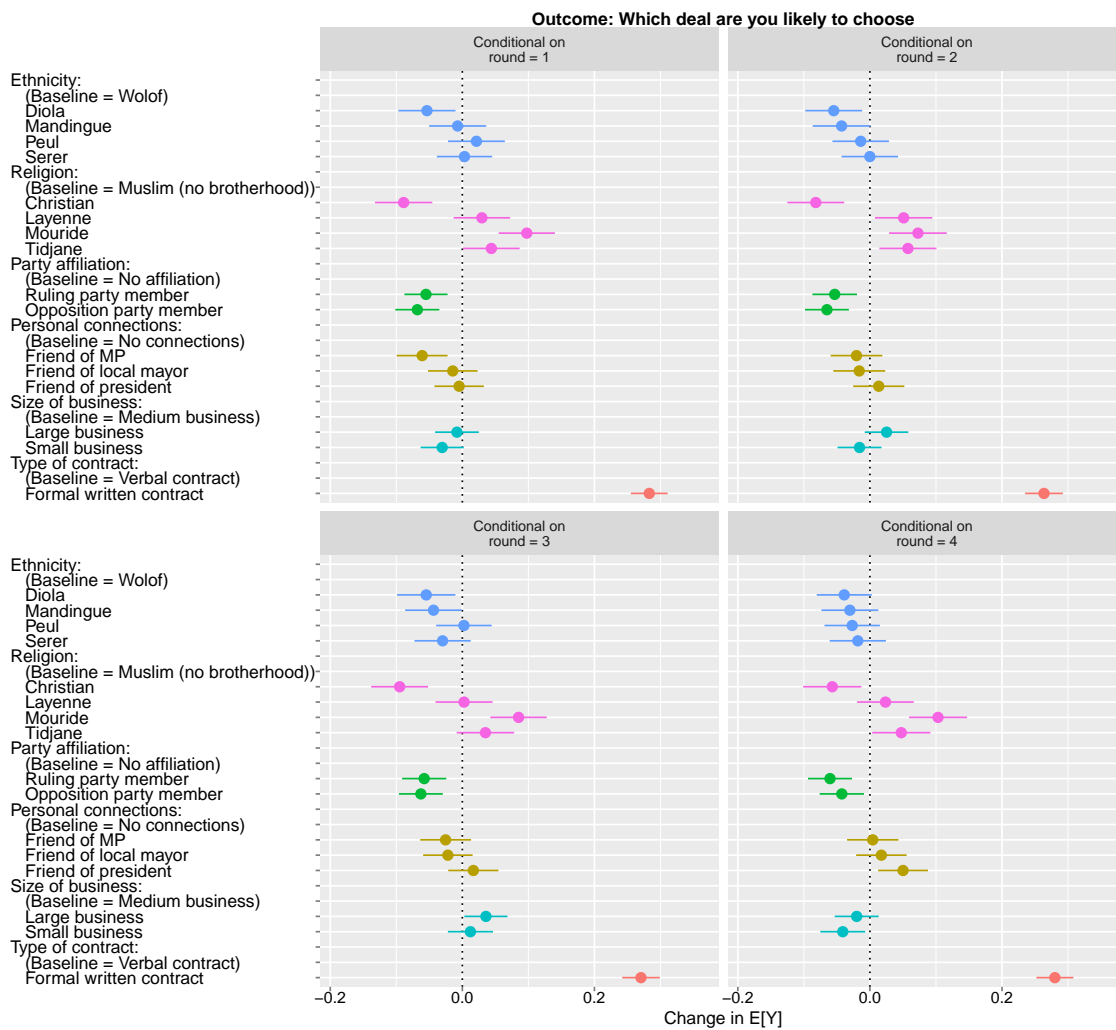
For respondent i for the j th profile in choice round k :

$$\begin{aligned} \text{deal_chosen}_{ijk} = & \theta_0 + \theta_1[\text{party}_{ijk} = \text{ruling}] + \theta_2[\text{party}_{ijk} = \text{opposition}] & (B.1) \\ & + \gamma_0 + \gamma_1[\text{friend}_{ijk} = \text{mayor}] + \gamma_2[\text{friend}_{ijk} = \text{MP}] \\ & + \gamma_3[\text{friend}_{ijk} = \text{president}] \\ & + \zeta_0 + \zeta_1[\text{ethnicity}_{ijk} = \text{Serer}] + \zeta_2[\text{ethnicity}_{ijk} = \text{Peul}] \\ & + \zeta_3[\text{ethnicity}_{ijk} = \text{Mandingue}] + \zeta_4[\text{ethnicity}_{ijk} = \text{Diola}] \\ & + \beta_0 + \beta_1[\text{religion}_{ijk} = \text{Tidjane}] + \beta_2[\text{religion}_{ijk} = \text{Layenne}] + \\ & \beta_3[\text{religion}_{ijk} = \text{Mouride}] + \beta_4[\text{religion}_{ijk} = \text{Christian}] \\ & + \alpha_0 + \alpha_1[\text{Size}_{ijk} = \text{large}] + \alpha_2[\text{Size}_{ijk} = \text{small}] \\ & + v_0 + v_1[\text{Contract}_{ijk} = \text{informal}] + v_2[\text{Contract}_{ijk} = \text{formal}] + \epsilon_i \end{aligned}$$

B.5 Diagnostic tests

No carryover effects

Figure B.2 presents the main results by choice task, and shows that responses did not substantially change by round. To formally test this claim, Table B.2 presents the p-values from an F-test of joint significance of the interaction terms in a regression that interacts attribute traits and choice task indicators. For all attributes, I fail to reject the null hypothesis that the effects are identical across rounds.



Notes: This figure shows the average marginal component effects (AMCEs) based on the round of the survey.

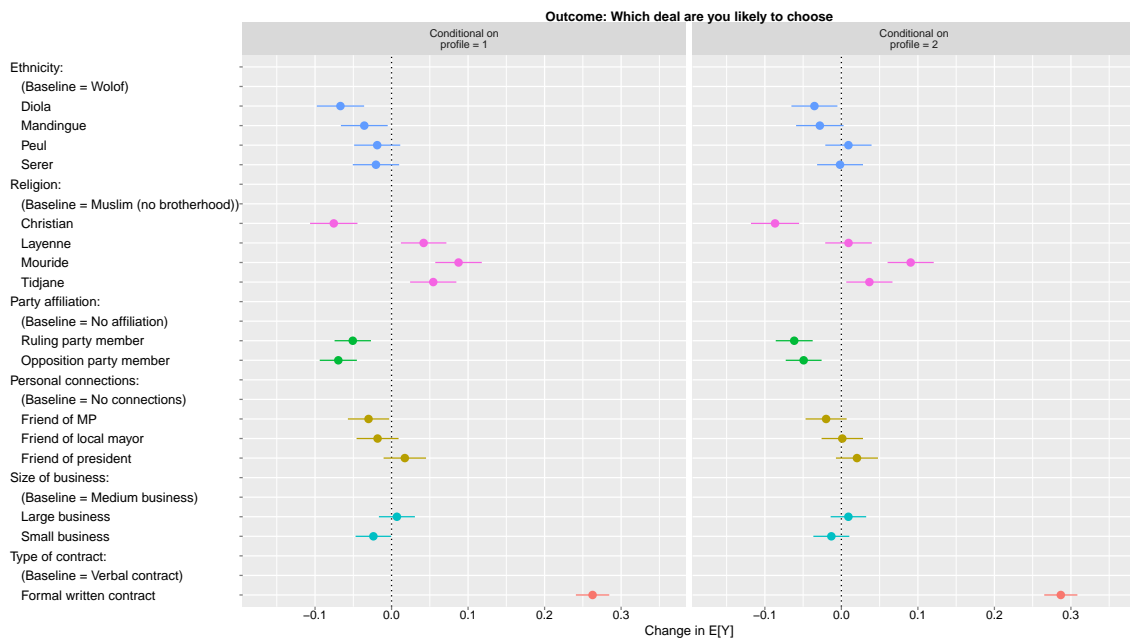
Figure B.2: Results by conjoint experiment round

Attribute	p-value from F-test
Religion of firm manager	0.8213
Ethnicity of firm manager	0.7123
Contract to be used	0.9634
Size of business	0.7901
Political party of firm manager	0.195
Personal political connections	0.389

Notes: Two-sided p-values from an F-test of joint significance of interaction terms between attribute traits and choice tasks indicators.

Table B.2: Attributes and F-test p-values

No profile order effects



Notes: This figure shows the average marginal component effects (AMCEs) based on the profile position (left or right).

Figure B.3: Results by profile order

Randomization validation and balance

Across profile attributes

As Table B.3 confirms, the randomization generation procedure on the enumeration tablets was executed properly.

Attribute	Trait	Profile presence (%)
Religion of firm manager	Tidjane	19.8
	Mouride	20.1
	Layenne	20.3
	Muslim (no brotherhood)	20.0
	Christian	19.9
Ethnicity of firm manager	Wolof	19.9
	Serer	20.2
	Peul	20.0
	Mandingue	20.0
	Diola	20.0
Contract to be used	Formal written contract	49.4
	Verbal agreement (no written contract)	50.6
Personal political connections	Friend of local mayor	24.9
	Friend of MP	24.8
	Friend of president	25.0
	No personal political connections	25.2
Political party of firm manager	Ruling party member	34.0
	Opposition party member	32.9
	No political affiliation	33.0
Size of business	Large business	33.5
	Medium business	33.3
	Small business	33.1

Notes: This table presents the percentage of profiles containing the given attribute trait. N=19,112.

Table B.3: Balance across the conjoint design's treatment groups

Across respondent characteristics

Treatment group	Covariate:									
	Confidence in court	Has worked for state	Gender	Formal firm status	Number of employees	Monthly revenue	Firm valuation	Formalized after start	Member of business assoc.	Experienced dispute
Diola	-0.005 (0.024)	0.012** (0.005)	-0.008 (0.010)	-0.016 (0.011)	-0.023 (0.022)	-0.012 (0.027)	-0.022 (0.026)	0.007 (0.010)	-0.007 (0.009)	-0.010 (0.011)
Mandingue	0.002 (0.024)	0.004 (0.004)	0.001 (0.010)	-0.009 (0.011)	-0.024 (0.023)	-0.020 (0.027)	-0.042* (0.025)	0.002 (0.009)	-0.012 (0.009)	-0.010 (0.011)
Peul	0.038 (0.023)	0.011** (0.004)	0.008 (0.009)	-0.009 (0.011)	-0.013 (0.022)	-0.009 (0.027)	-0.050** (0.025)	-0.007 (0.009)	-0.017* (0.009)	-0.008 (0.011)
Serer	-0.003 (0.024)	0.0003 (0.004)	-0.008 (0.009)	-0.021** (0.011)	-0.024 (0.022)	-0.045* (0.026)	-0.064*** (0.025)	-0.003 (0.009)	-0.023*** (0.009)	-0.020* (0.011)
Christian	0.001 (0.023)	0.002 (0.004)	-0.016* (0.009)	0.003 (0.011)	0.019 (0.022)	0.003 (0.027)	0.003 (0.026)	0.007 (0.009)	0.005 (0.009)	0.011 (0.011)
Layenne	0.037 (0.023)	0.005 (0.004)	-0.008 (0.010)	0.009 (0.011)	0.023 (0.021)	0.020 (0.026)	0.034 (0.025)	-0.001 (0.009)	-0.001 (0.008)	0.009 (0.011)
Mouride	0.010 (0.023)	0.010** (0.004)	-0.007 (0.009)	-0.007 (0.011)	0.025 (0.021)	0.015 (0.026)	0.015 (0.025)	-0.005 (0.009)	-0.010 (0.008)	0.018* (0.011)
Tijani	0.011 (0.023)	0.010** (0.004)	0.001 (0.009)	-0.002 (0.011)	0.014 (0.022)	-0.003 (0.026)	0.021 (0.025)	0.007 (0.009)	-0.006 (0.008)	0.021** (0.011)
Ruling party member	-0.009 (0.019)	0.001 (0.004)	0.002 (0.007)	0.006 (0.009)	-0.004 (0.017)	0.008 (0.021)	0.015 (0.020)	-0.003 (0.007)	-0.0001 (0.006)	0.006 (0.008)
Opposition party member	0.030 (0.019)	-0.001 (0.003)	0.001 (0.007)	-0.007 (0.009)	0.001 (0.017)	0.017 (0.021)	0.029 (0.020)	-0.001 (0.007)	-0.003 (0.007)	0.004 (0.008)
Friend of MP	0.036* (0.021)	-0.001 (0.004)	-0.006 (0.008)	0.008 (0.009)	0.022 (0.019)	0.052** (0.023)	0.036 (0.022)	-0.008 (0.008)	-0.003 (0.008)	-0.006 (0.009)
Friend of local mayor	0.015 (0.021)	-0.001 (0.004)	0.001 (0.008)	0.019* (0.010)	0.019 (0.019)	0.032 (0.024)	0.027 (0.024)	0.002 (0.008)	0.002 (0.008)	-0.010 (0.009)
Friend of president	-0.003 (0.022)	0.004 (0.004)	0.001 (0.009)	0.020** (0.010)	0.016 (0.020)	0.022 (0.023)	0.018 (0.022)	-0.003 (0.008)	0.007 (0.008)	-0.008 (0.010)
Large business	0.020 (0.018)	-0.002 (0.003)	-0.001 (0.007)	0.009 (0.008)	-0.006 (0.017)	0.015 (0.021)	0.010 (0.020)	-0.009 (0.007)	0.00002 (0.007)	0.014* (0.008)
Small business	0.013 (0.018)	0.003 (0.003)	0.003 (0.007)	0.001 (0.008)	-0.012 (0.016)	0.006 (0.020)	0.007 (0.019)	-0.004 (0.007)	0.0003 (0.006)	0.012 (0.008)
Formal written contract	-0.002 (0.015)	0.001 (0.003)	-0.006 (0.006)	-0.016** (0.007)	-0.008 (0.014)	-0.034** (0.017)	-0.028* (0.016)	0.001 (0.006)	-0.007 (0.005)	0.007 (0.007)
Two-sided p-value from F-test of joint significance	0.075*	0.086*	0.524	0.688	0.477	0.468	0.658	0.458	0.123	0.337

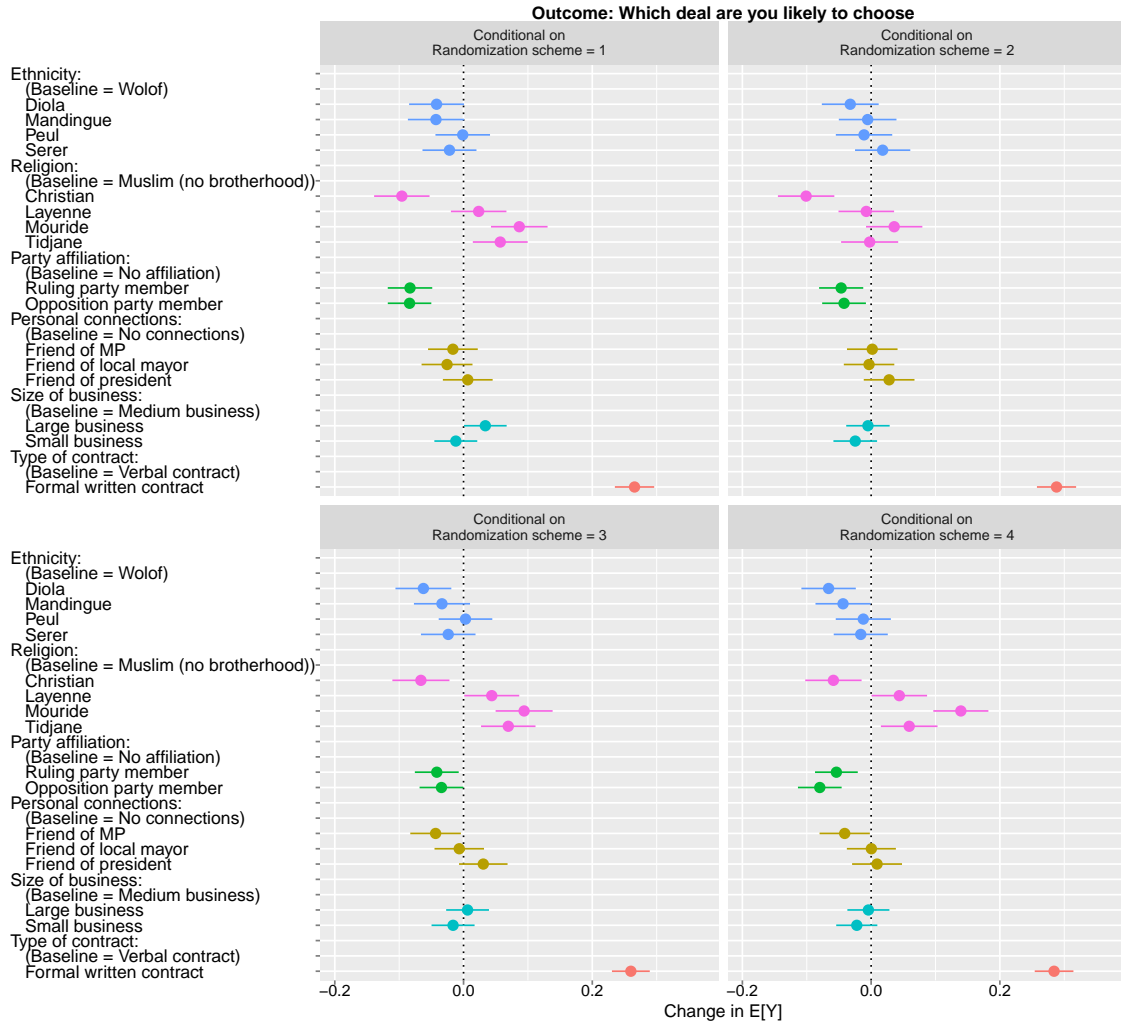
Note:

*p<0.1; **p<0.05; ***p<0.01

Notes: This table presents the results of regressing respondent characteristics on treatment group indicators, and also presents the two-sided p-values from F-tests of joint significance.

Table B.4: Balance across respondent characteristics

No attribute order effects



Notes: This figure presents the results by the four randomization schemes for attribute groupings. The specific attribute position orders are shown in Table B.5. For example, the bottom-left graph corresponds to the third randomization scheme, for which the size of the business appeared first on the profile, ethnicity and religion composed the second attribute grouping, the contract type appeared third, and the political connections of the firm manager appeared last.

Figure B.4: Results by randomized vertical order of attributes

	Contract type	Political profile	Business size	Ethnicity and religion
Randomization scheme = 1	1	2	3	4
Randomization scheme = 2	4	1	2	3
Randomization scheme = 3	3	4	1	2
Randomization scheme = 4	2	3	4	1

Table B.5: Position order of attributes per randomization scheme

Treatment does not predict attrition

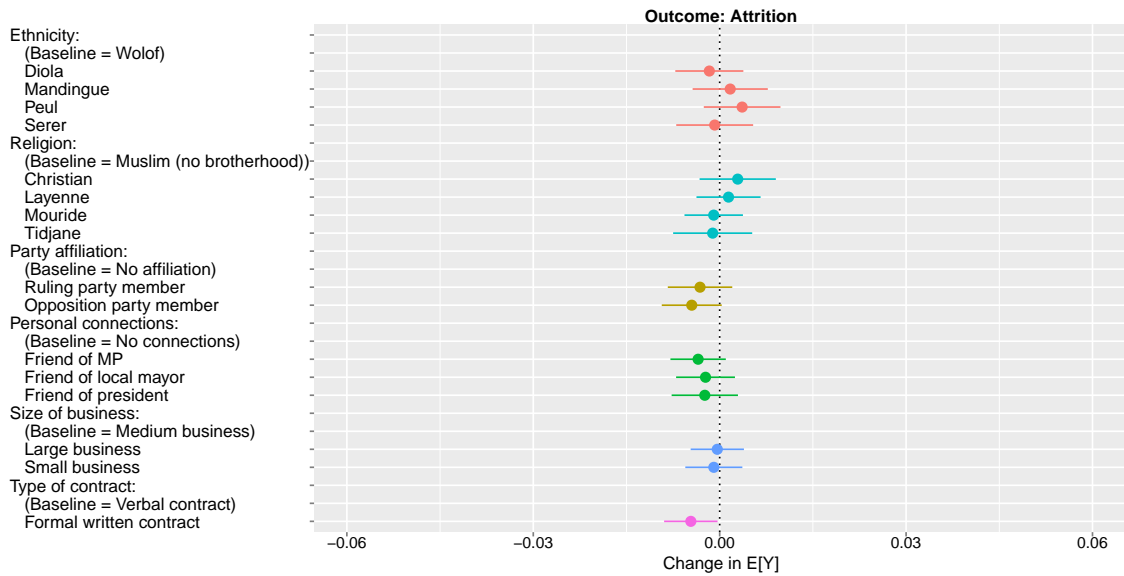


Figure B.5: Attrition as predicted by treatment

B.6 Corresponding tables for figure results

AMCE table results: full sample

Attribute	Trait	AMCE	Std..Err	Significance
Ethnicity	Diola	-0.051	0.011	***
Ethnicity	Mandingue	-0.032	0.011	**
Ethnicity	Peul	-0.005	0.011	
Ethnicity	Serer	-0.011	0.011	
Party affiliation	Ruling party member	-0.056	0.009	***
Party affiliation	Opposition party member	-0.060	0.009	***
Personal connections	Friend of MP	-0.025	0.010	*
Personal connections	Friend of local mayor	-0.008	0.010	
Personal connections	Friend of president	0.019	0.010	
Religion	Christian	-0.081	0.011	***
Religion	Layenne	0.026	0.011	*
Religion	Mouride	0.089	0.011	***
Religion	Tijani	0.046	0.011	***
Size of business	Large business	0.008	0.009	
Size of business	Small business	-0.019	0.009	*
Type of contract	Formal written contract	0.274	0.009	***

Note: *p<0.1; **p<0.05; ***p<0.01

Table B.6: AMCE results for prefer outcome

Attribute	Trait	AMCE	Std..Err	Significance
Ethnicity	Diola	0.042	0.012	***
Ethnicity	Mandingue	0.026	0.012	*
Ethnicity	Peul	0.008	0.012	
Ethnicity	Serer	0.014	0.011	
Party affiliation	Ruling party member	0.066	0.009	***
Party affiliation	Opposition party member	0.057	0.009	***
Personal connections	Friend of MP	0.038	0.010	***
Personal connections	Friend of local mayor	0.021	0.010	*
Personal connections	Friend of president	0.023	0.011	*
Religion	Christian	0.080	0.012	***
Religion	Layenne	-0.023	0.011	*
Religion	Mouride	-0.076	0.011	***
Religion	Tijani	-0.040	0.011	***
Size of business	Large business	-0.012	0.009	
Size of business	Small business	0.018	0.009	*
Type of contract	Formal written contract	-0.273	0.009	***

Note: *p<0.1; **p<0.05; ***p<0.01

Table B.7: AMCE results for breach outcome

Conditional AMCE table results: formal vs. informal firms

Variables	Conditional Estimate	Std. Error	Significance
Diola	-0.097	0.020	***
Mandingue	-0.077	0.021	***
Peul	-0.026	0.020	
Serer	-0.031	0.020	
Formal firm	-0.104	0.032	***
Ruling party member	-0.065	0.016	***
Opposition party member	-0.042	0.016	***
Friend of MP	-0.036	0.017	**
Friend of local mayor	-0.032	0.017	*
Friend of president	0.002	0.018	
Christian	-0.105	0.020	***
Layenne	0.020	0.020	
Mouride	0.124	0.019	***
Tijani	0.058	0.018	***
Large business	0.012	0.015	
Small business	-0.045	0.015	***
Formal written contract	0.155	0.015	***
Formal firm × Formal written contract	0.182	0.018	***
Formal firm × Friend of MP	0.016	0.021	
Formal firm × Friend of local mayor	0.033	0.021	
Formal firm × Friend of president	0.022	0.022	
Formal firm × Ruling party member	0.015	0.020	
Formal firm × Opposition party member	-0.028	0.019	
Formal firm × Large business	-0.007	0.018	
Formal firm × Small business	0.040	0.018	**
Formal firm × Diola	0.071	0.024	***
Formal firm × Mandingue	0.071	0.025	***
Formal firm × Peul	0.031	0.024	
Formal firm × Serer	0.032	0.023	
Formal firm × Christian	0.036	0.024	
Formal firm × Layenne	0.010	0.023	
Formal firm × Mouride	-0.050	0.023	**
Formal firm × Tijani	-0.016	0.023	

Note: *p<0.1; **p<0.05; ***p<0.01

Table B.8: AMCE results by firm formality

Conditional AMCE table results: co-ethnicity and co-religiosity

Co-ethnicity

Variables	Conditional Estimate	Std. Error	Significance
Coethnic	0.115	0.035	***
Diola	-0.014	0.013	
Mandingue	-0.001	0.013	
Peul	0.012	0.013	
Serer	0.018	0.013	
Coethnic × Diola	0.002	0.055	
Coethnic × Mandingue	0.035	0.038	
Coethnic × Peul	0.024	0.023	
Coethnic × Serer	-0.013	0.027	

Note: *p<0.1; **p<0.05; ***p<0.01

Table B.9: AMCE results by co-ethnicity

Co-religiosity

Variables	Conditional Estimate	Std. Error	Significance
Co-religious	0.104	0.032	***
Christian	-0.054	0.013	***
Layenne	0.057	0.012	***
Mouride	0.087	0.014	***
Tijani	0.056	0.013	***
Co-religious × Christian	0.138	0.039	***
Co-religious × Layenne	0.034	0.055	
Co-religious × Mouride	0.006	0.023	
Co-religious × Tijani	-0.004	0.024	

Note: *p<0.1; **p<0.05; ***p<0.01

Table B.10: AMCE results by co-religion

B.7 Ruling out general distaste for politics

As a check for whether a general distaste for politics drives the results presented in Figure 2.4, I subdivide the sample by respondents' political affiliations and re-estimate results. Members of political parties arguably do not have a distaste for politics, and as Figure B.6 shows, these subgroups still produce significant effects. This suggests that there are alternate mechanisms at play in the results discussed in Section 2.5.

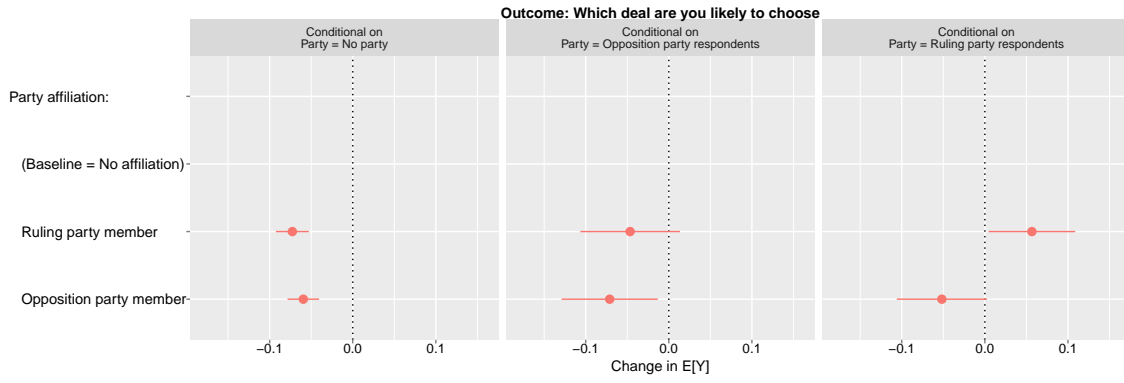


Figure B.6: Effects by respondents' political affiliations

APPENDIX **C**

Appendices to Chapter 3

C.1 Heterogeneous effects by member of political party in power

	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 used	-0.240*** (0.032)	-0.359*** (0.048)	-0.170*** (0.046)	-0.020 (0.033)	0.401*** (0.044)	0.535*** (0.047)	0.580*** (0.053)
Member of party in power	-0.057 (0.069)	0.202* (0.105)	0.251** (0.100)	0.570*** (0.071)	0.148 (0.097)	0.305*** (0.101)	0.445*** (0.115)
Formal contract used × Member of party in power	-0.051 (0.100)	-0.050 (0.153)	0.041 (0.145)	-0.011 (0.104)	-0.065 (0.141)	0.015 (0.148)	-0.017 (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 C.1: Heterogeneous effects by firm connections

C.2 Results with controls

This section presents results for models that include controls for valuation (wealth) and size of firm.

Descriptive results

	Outcome: Likelihood of using enforcement strategy						
	<i>Social enforcement outcomes</i>				<i>Formal enforcement outcomes</i>		
	Amicably (1)	Social network (2)	Religious network (3)	Political connection (4)	Police (5)	Lawyer (6)	Courts (7)
Formal firm	-0.187*** (0.034)	-0.450*** (0.053)	-0.320*** (0.050)	0.042 (0.037)	0.328*** (0.049)	0.671*** (0.048)	0.540*** (0.057)
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	2,389	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). Controls include firm wealth and size. 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." * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table C.2: Difference in enforcement likelihood between formal and informal firms (with controls)

	Outcome: Likelihood of using enforcement strategy						
	<i>Social enforcement outcomes</i>				<i>Formal enforcement outcomes</i>		
	Amicably (1)	Social network (2)	Religious network (3)	Political connection (4)	Police (5)	Lawyer (6)	Courts (7)
Connected firms	0.005 (0.037)	0.260*** (0.058)	0.334*** (0.054)	0.470*** (0.038)	0.022 (0.053)	0.166*** (0.054)	0.172*** (0.062)
Unconnected firm mean	4.417	2.997	1.85	1.416	3.262	2.455	2.63
Unconnected firm std. dev.	0.748	1.154	1.028	0.67	1.063	1.12	1.271
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
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). Controls include firm wealth and size. 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." * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table C.3: Difference in enforcement likelihood between connected and unconnected firms (with controls)

Experimental results

	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)
Deal based on formal contract	-0.252*** (0.029)	-0.366*** (0.046)	-0.164*** (0.043)	-0.025 (0.031)	0.382*** (0.042)	0.514*** (0.042)	0.556*** (0.049)
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	2,389	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). Controls include firm wealth and size. 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 C.4: Average treatment effects: Main results of survey experiment (with controls)

	Outcome: Likelihood of using enforcement strategy						
	<i>Social enforcement outcomes</i>				<i>Formal enforcement outcomes</i>		
	Amicably (1)	Social network (2)	Religious network (3)	Political connection (4)	Police (5)	Lawyer (6)	Courts (7)
Panel A: By political connections							
Formal contract used	-0.247*** (0.033)	-0.362*** (0.051)	-0.176*** (0.048)	-0.030 (0.034)	0.429*** (0.047)	0.543*** (0.047)	0.576*** (0.054)
Connected firms	0.012 (0.051)	0.260*** (0.079)	0.298*** (0.075)	0.447*** (0.053)	0.139* (0.072)	0.238*** (0.073)	0.223*** (0.084)
Formal contract used × Connected firms	-0.022 (0.073)	-0.011 (0.114)	0.068 (0.107)	0.047 (0.076)	-0.232** (0.104)	-0.136 (0.104)	-0.090 (0.121)
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 used	-0.153*** (0.052)	-0.315*** (0.082)	-0.308*** (0.078)	-0.043 (0.056)	0.406*** (0.075)	0.477*** (0.075)	0.490*** (0.086)
Trust in courts	0.063*** (0.020)	0.094*** (0.032)	-0.072** (0.030)	-0.007 (0.022)	-0.001 (0.029)	0.048* (0.029)	0.125*** (0.033)
Formal contract used × Trust in courts	-0.065** (0.028)	-0.034 (0.044)	0.094** (0.042)	0.012 (0.030)	-0.015 (0.040)	0.023 (0.041)	0.041 (0.047)
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 used	-0.175*** (0.033)	-0.304*** (0.052)	-0.158*** (0.049)	-0.034 (0.036)	0.279*** (0.047)	0.421*** (0.048)	0.473*** (0.055)
Exposure to corruption	0.020 (0.049)	-0.141* (0.077)	-0.159** (0.073)	-0.209*** (0.052)	0.045 (0.069)	-0.038 (0.070)	0.124 (0.081)
Formal contract used × Exposure to corruption	-0.325*** (0.069)	-0.254** (0.108)	-0.020 (0.103)	0.051 (0.074)	0.433*** (0.098)	0.392*** (0.099)	0.345*** (0.115)
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
Panel D: By firm formality							
Formal contract used	-0.198*** (0.050)	-0.295*** (0.078)	-0.068 (0.074)	0.030 (0.054)	0.359*** (0.071)	0.313*** (0.069)	0.320*** (0.082)
Formal firm	-0.146*** (0.045)	-0.397*** (0.070)	-0.249*** (0.067)	0.083* (0.049)	0.309*** (0.064)	0.522*** (0.062)	0.364*** (0.074)
Formal contract used × Formal firm	-0.079 (0.061)	-0.100 (0.096)	-0.141 (0.091)	-0.083 (0.066)	0.029 (0.087)	0.294*** (0.085)	0.348*** (0.101)
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	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,389	2,389	2,389	2,389	2,389	2,389	2,389

Notes: See Table C.4.

C.3 Business associations

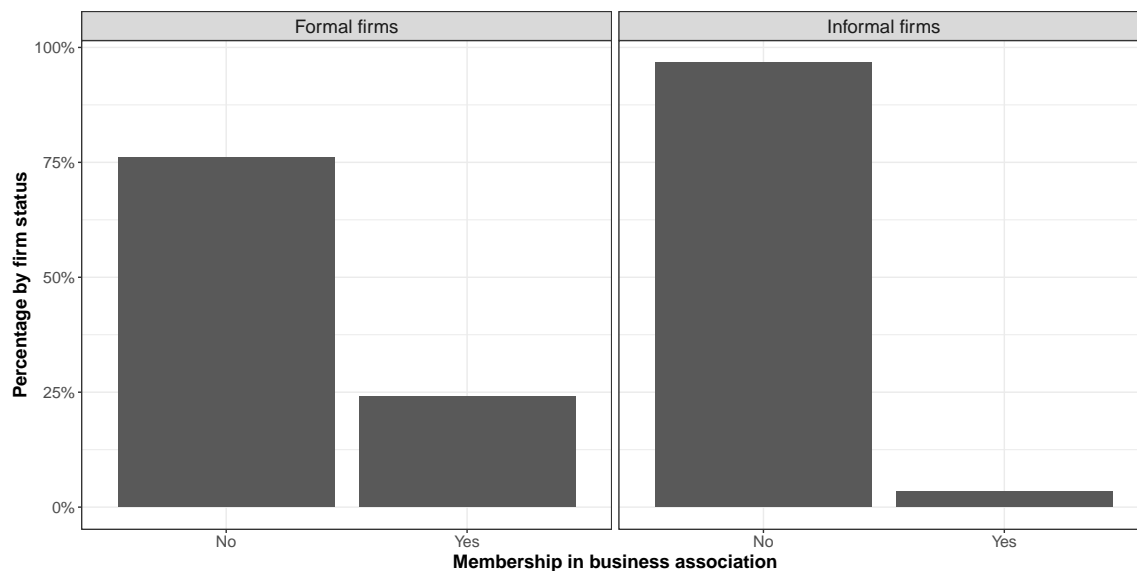


Figure C.1: Membership in business association by firm status

C.4 Firm wealth does not predict political connections

	Political connections (1)
Firm valuation	-0.002 (0.007)
Constant	0.209*** (0.035)
Observations	2,389

Notes: OLS results from regressing indicator for politically connected firms on indicator for firm wealth. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

Table C.6: Firm wealth does not predict political connections

C.5 Correlation table

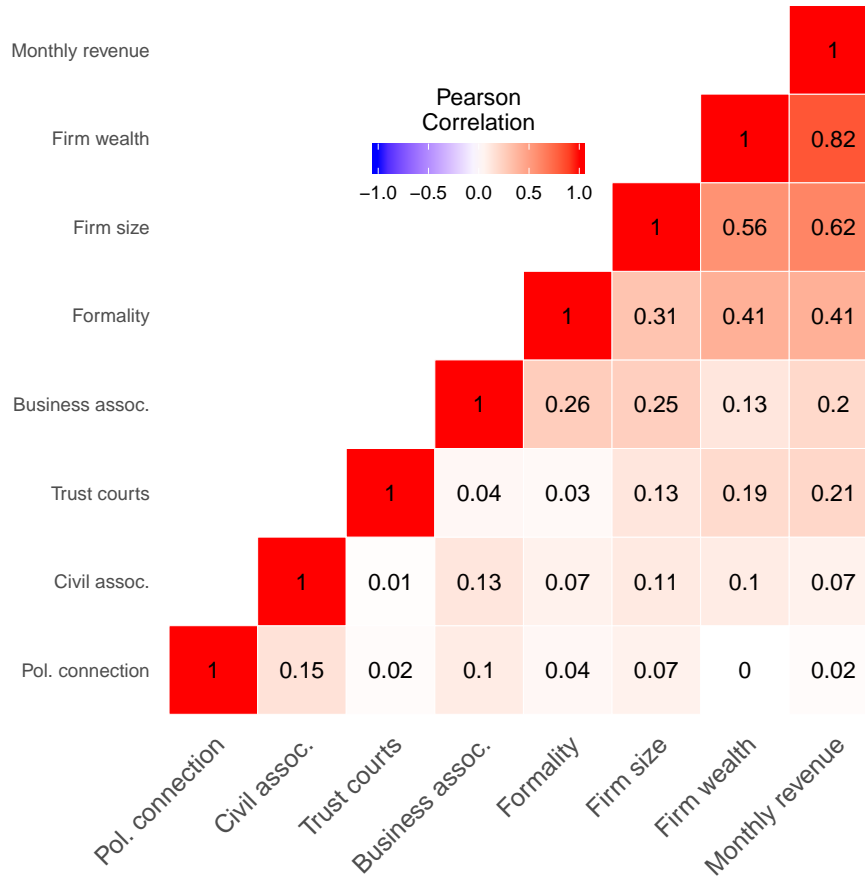


Figure C.2: Correlation table