POLITICAL DETERMINANTS OF ECONOMIC EXCHANGE: EVIDENCE FROM A BUSINESS EXPERIMENT IN SENEGAL*

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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 because they introduce biases in expectations of judicial enforcement. Buyers avoid trade with politically connected sellers, and, in this context of unequal enforcement, formal contracts disproportionately protect politically connected buyers. To examine these features of connections and contracts, I created a sales business in Senegal and randomized whether employees signaled political connections and/or offered formal contracts during transactions. The results show that political connections decreased buyers' willingness to exchange. Formal contracts increased exchange, though primarily for connected buyers. These findings show that 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.

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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. For example, sellers may pocket payment and then deliver substandard goods, or may fail to deliver promised products altogether. 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 or its quality can be verified is a common feature of modern economies.

In countries with weak or selectively enforced rule of law, 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 certainly many benefits to possessing political connections (e.g. Szakonyi 2018), political inequalities between sellers and buyers may also 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 how they 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 politically connected and seek trade when sellers are less connected. In the context of these political inequalities, state-backed formal contracts may fail to mitigate risk for all types of buyers: if the successful enforcement of contracts is dependent on political access, they may only be useful to politically connected buyers. This theory implies that asymmetric political power shapes private-sector exchange and reinforces economic inequalities, and that political connections disrupt the function of formal institutions for contracting.

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 and legally registered a sales business, and hired employees to sell a mobile phone-credit service with purchase options that captured different types of risk 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. As outcomes, I measured whether respondents purchased any level of phone credit (where the risk was that the phone credit was not of the promised quality),[2] as well as whether respondents purchased a level with delayed delivery (which entailed the additional risk of potential non-delivery). To ensure that political connections were credible and consistent across employees, I partnered with three influential municipal councils in Dakar that agreed to hire and host my employees prior to the field experiment. During transactions, employees briefly mentioned their past employment at the councils to treated households as part of the extended introduction phase common to interactions in Senegal. To measure the political connections of *buyers*—as well as to parse the mechanisms by which the treatments operated—I implemented an endline survey among the sample several days after transactions occurred.

The results of the field experiment confirm that political connections can stifle exchange.

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

[2]Phone credit in Senegal can have varying levels of quality which can be unclear upon delivery. I describe this in greater detail in Section 3.

Driven by the risk of substandard quality of products, overall purchase rates declined when sellers signaled their political connections. Taking buyers' political connections into account shows that reduced levels of trade with politically connected sellers were driven by politically connected buyers. These results are robust to interacting treatment with covariates that are correlated with buyer political connections, as well as to buyer-seller co-ethnicity and co-religiosity, the primary competing explanations of non-state contract enforcement in markets like Senegal's. 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 a more difficult prospect when the opposing party is politically connected.

The results also show that formal contracts substantially increased propensities to trade, particularly with purchases involving delayed delivery. This finding is somewhat surprising in that it demonstrates that formal contracts can mitigate risk and boost confidence in exchange even in markets with weak norms of enforcement. But exploring this result more deeply reveals a less rosy picture: the positive effects of formal contracts were driven by buyers who were politically connected themselves. Offering formal contracts had little effect on unconnected buyers' confidence in exchange. This finding suggests that formal contracts may be useful primarily for already-privileged citizens in societies with selective rule of law. In these contexts, state institutions may counter-productively perpetuate inequalities in private-sector exchange.

Overall, these results show that political connections can 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 partial equilibrium effects for how individualized political connections can constrain private-sector growth, whereas existing work on political connections tends to emphasize other dimensions along which these connections can be profitable (e.g. Roberts 1990; Fisman 2001; Khwaja and Mian 2005; Faccio 2006). These studies condition on firms that already exist or on exchanges that 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 mischaracterize 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 via the design-based innovation of creating and operating a business to elucidate key aspects of the theoretical dynamic. This paper thus builds the evidence base for an important yet underexamined mechanism.

Second, I show that political connections influence trade even when controlling for established theories for social enforcement in sub-Saharan Africa like co-ethnicity and co-religiosity (Grimard 1997; Sanchez de la Sierra 2018). Political connections are non-ascriptive, vary dynamically over time, and critically shape even 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 myriad societies where ethnicity is not a salient political dimension (e.g. Posner 2004).

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.

2 Theory

Existing work has shown that the state can enforce secure exchange (e.g. North and Weingast 1989) or that enforcement equilibria can emerge in the absence of cooperative state institutions (e.g. Greif 1989).[3] However, in much of the world, particularly 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 setting can give rise to 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 altogether. In this section, I develop a theory of seller moral hazard, outlining the roles of both sellers' political connections and formal contracts as well as how buyers' connections might moderate their effects.

2.1 Seller political connections and formal contracts in exchange

Political connections are invaluable to firms when states 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 preferred 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,

[3] See Appendix A.17 for more discussion of the literature on commitment problems in exchange.

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

For individual buyers, the risks of trading with connected sellers often 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). Politically connected parties are also more likely to benefit from better access to and preferential enforcement from non-state institutions (Bhandari 2020). Thus, because of the moral hazard they produce, sellers' political connections may 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 (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.*

2.2 The moderating role of buyers' political connections

I argue that the advantages of political connections in navigating the enforcement system accrue not only to sellers with connections, but also to politically connected buyers. Because connected buyers can preferentially access enforcement institutions—cutting through the red tape that holds up the majority of citizens—and benefit from the bias of these institutions, 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.

Asymmetric buyer-seller political connections may similarly moderate perceptions of seller moral hazard and propensities to exchange. During transactions characterized by seller moral hazard, buyers can assess power differentials and make decisions to trade accordingly.[4] 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 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 choose not to exchange. When buyers and sellers are both connected, the playing field is relatively equal in terms of enforcement and buyers may choose to trade, though not as much as they would if sellers were unconnected. Table 1 summarizes these theoretical predictions.

Hypothesis 3. *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.*

In the context of these political power dynamics, do formal contracts differentially moderate the perception of seller moral hazard for connected and unconnected buyers? Formal contracts draw their power from the authority of the state. Politically connected citizens receive privileged access

[4]Sellers may possess incentives to hide their political connections, and in some cases this may mitigate the costs of being perceived as connected. However, in developing democracies where informal influences are rampant, buyers may already have preconceived notions of sellers' connectivity, or can make these assessments based on extended introductions.

		Buyer is politically connected	
		No	**Yes**
Seller is politically connected	**No**	Intermediate probability of purchase	High probability of purchase
	Yes	Low probability of purchase	Intermediate probability of purchase

Table 1: Theoretical predictions under asymmetric political connections

to and treatment from state institutions, and thus the power to have contracts enforced is 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 political connections. Thus, formal contracts are more likely to mitigate perceptions of seller moral hazard for connected buyers and should disproportionately motivate connected types to exchange. In this way, the use of formal institutions like formal contracts may unintentionally exacerbate inequality in environments with selectively enforced rule of law.

Hypothesis 4. *Formal contracts increase the likelihood of exchange for connected buyers more than unconnected buyers.*

3 Context

3.1 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 judi-

ciary's accessibility.

Despite these weaknesses, Senegalese citizens place a relatively high degree of trust in legal institutions, at least when politically connected parties are not involved.[5] 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.[6] If this fails, the affected party may involve the local police or courts. These means of enforcement become complicated by political connections, however.

3.2 Political connections in exchange

A commonly held view in Senegal is that political connections lead to preferential treatment at all levels of the state. With Senegal's often labyrinthine 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 unconnected citizens must contend, and knowing even a low-level bureaucrat can enhance the chances of gaining preferential access to institutions. Getting one's foot in the door can be among the most difficult steps in the enforcement process, and even non-direct connections help to overcome this constraint via shared governmental networks.[7] Political connections thus play a significant role in the business environment by determining access to means of enforcement. Indeed, citizens anticipate that enforcement will be biased toward the party with more political power even when non-state resolution mechanisms are used.

The consensus of my sample was that political connections decrease the probability of punishment for contract breach. Approximately 76% of respondents stated that connections enable trad-

[5] See Appendix Figure A3.

[6] For a discussion of social forms of enforcement in Senegal, see Appendix A.18.

[7] For example, when asked how he resolved his past contract dispute, a respondent 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 to file the correct paperwork.

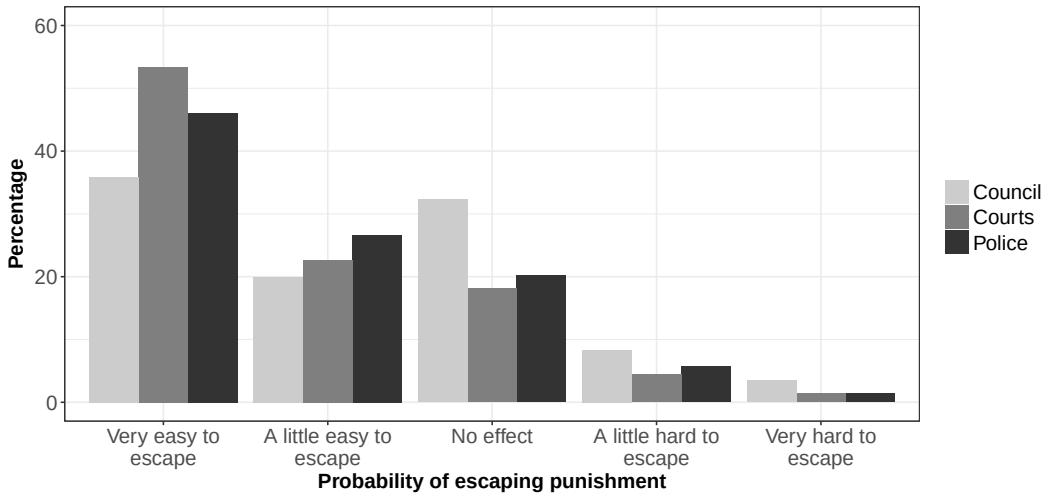


Figure 1: Connections lead to perceived impunity in contract disputes

ing partners to escape punishment when they break contracts, and only 27% reported confidence in local courts' and police's ability to impartially enforce a contract when politically connected people are involved. Figure 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 politically connected sellers.

3.3 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 in practice are typically 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.

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 direct-to-household sales by formal-sector businesses. For example, 82% of respondents in my sample reported participation in door-to-door sales campaigns in the past, some 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. Few people receive phone credit through wireless subscription services. Rather, phone credit is purchased as required, either from ambulatory traders or from neighborhood kiosks. There is significant demand for mobile credit, and prices are disproportionately high relative to income, particularly in middle-to-lower income neighborhoods. Buyers are thus keen on alternate methods for receiving phone credit, especially at competitive prices. Important for the research design of this project, phone credit in Senegal can have varying levels of quality. The major wireless companies typically provide bonus credit as an incentive for buyers to purchase, but this bonus credit is considered lower quality because its use is restricted. Thus, bonus phone credit that is just as good as "regular" credit is highly desirable, though the quality of this credit may not be immediately apparent upon purchase.

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. This paper thus takes a partial equilibrium approach to understand how connections and contracts affect propensities to exchange, holding other features of the seller and transaction constant. 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.

4.1 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.[8] After successfully formalizing the business, I received a unique business identification number called the NINEA, which is commonly understood in Senegal as proof that a business is formal.[9]

The business, called *Porte-à-Porte Sénégal* (Door-to-Door Senegal, or PAPS), offered discounted mobile phone credits. Mobile credit was chosen as the activity of interest for several reasons. First, this resembled common sales practices in Senegal, where ambulatory traders sell small items including discounted phone credit directly to households.[10] As mentioned above, 82% of respondents in my sample said they had purchased items in similar door-to-door sales campaigns in the past. Second, there is high demand in Senegal for discounted mobile phone credit, which helped to minimize the likelihood of floor effects. Finally, the varying quality of phone credit in Senegal, as well as the ability for phone credit to be delivered remotely at a later date, allowed for varying two dimensions of seller moral hazard: the risk of substandard product quality and the risk of nondelivery. There is already a common distrust of the telecommunications company in Senegal whose credit PAPS sold, which further spurred respondents to consider the risk of the deal.

[8]Despite the "one-stop" shop, registering the business required the acquisition of certain documents that are not centrally controlled. This required visits to a *chef de quartier* (neighborhood chief), police department, and government ministries.

[9]Appendix Figure A1 shows a copy of the business registration.

[10]Though 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.

4.2 Ethical considerations

This paper is arguably the first to manipulate how businesses deploy political connections, which I was able to accomplish by creating an original business. Though operating a business enables testing heretofore untestable development-related hypotheses in a realistic trading environment, it raises important ethical considerations. These concerns include conducting research in a manner appropriate to the local context, avoiding the displacement of economic activity, and minimizing the misuse of public resources.

To ensure that this project conformed to the local context, I sought and gained approval from the Ministry of Scientific Research in Senegal, the three district governments in which I operated the business, and a research center in Dakar with which I have a longstanding relationship. All approved the project, and the research institution confirmed that the project "does not go against cultural, social, or political norms in Senegal, and is in line with what is appropriate for research here." Because such transactions are commonplace for respondents in the sample districts, the IRB and Senegalese authorities agreed that respondents could be debriefed at the beginning of the endline survey, and although respondents were given the option to withdraw from the study at that point, none chose to do so. Furthermore, there was no deception in business practices, as all who paid for phone credit received the quality of credit they were promised on the date that they were promised.

To avoid displacing existing economic activity, I ensured the minimal number of transactions necessary to satisfy statistical power demands, and discussed the research plan with the three local governments where I conducted research activities. Sellers conducted transactions with only 486 respondents in each district from an average district population of around 165,000. Interviews with local businesses and purveyors of phone credit confirmed they were unconcerned by the prospect of displaced economic activity because of the limited reach and duration of the project. Finally, APIX understood that the business I registered was for research purposes, and confirmed that PAPS' creation was not displacing other registration activity or otherwise misusing public resources.

4.3 Sample selection and partner municipal councils

A key treatment arm in the experiment required sellers to signal their political connections to buyers. For ethical purposes as well as the interpretation of eventual results, I ensured employees possessed 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 most frequently, 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.

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

Because sellers had to work at the councils of the communes in which I implemented the experiment, I met with government administrators at communes that fit the following criteria: 1) densely populated communes inhabited primarily by middle-to-lower income 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 more affluent neighborhoods of Dakar); and 3) communes where household sales are commonplace enough such that door-to-door transactions would not be perceived as unusual. Of the five municipal governments I met with, three agreed to hire my employees and permitted research activities to occur: Golf Sud, Médina, and Pikine. I hired nine employees to work for my firm, and thus three employees worked at each council.

4.4 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 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 to household transactions in Senegal.[11] Rather than recreate a general trading equilibrium, this treatment aimed to induce buyers to consider the implications of sellers' political connections. To rule out the concern that the treatment may have appeared artificial or strange to buyers, the endline survey asked buyers about their skepticism; the evidence shows that suspicion was low and not affected by treatment (see Appendix Table A10).

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.[12] 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 by a Senegalese law firm, which deemed it to be executable in local courts of law. Sellers explained the contract during

[11] Appendix A.2 presents an outline of the transaction protocol that enumerators followed.
[12] Appendix A.3 includes a translation of this clause.

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

Table 2: Treatment groups

transactions, 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. While there is a risk that some buyers may have found this option to be unusual, Appendix Table A10 shows that, in line with expectations in Senegal, this treatment arm did not raise buyers' suspicions. 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 2 summarizes the components of the factorial design and shows the six treatment groups.

4.5 Data collection

There were two main stages of 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 as described in the next subsection and conducted door-to-door sales in the three sample communes. At the end of each

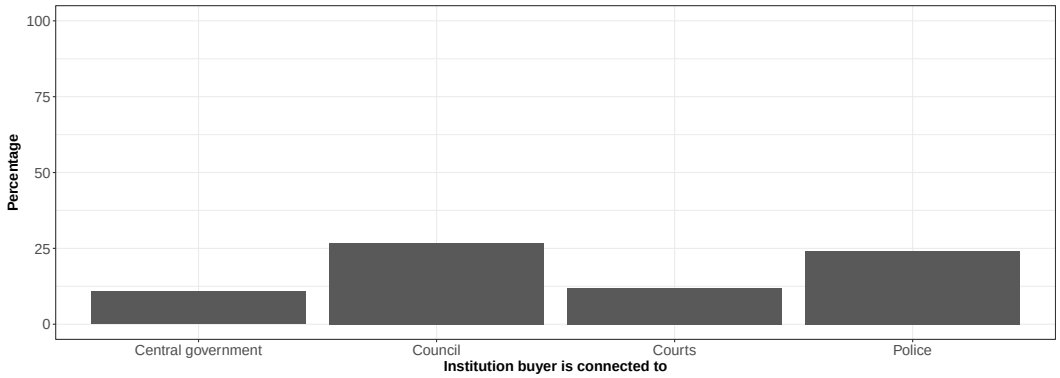


Figure 2: Percentage of respondents with connections

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 administered an endline survey to sample respondents. Endline surveys were conducted by different enumerators than those 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.[13]

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.[14] 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 to otherwise hermetic institutions. Figure 2 shows the percentage of the sample reporting political connections.

[13]Appendix Table A4 shows that treatment 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.

[14]Appendix Table A11 shows that buyers' to whom sellers signaled political connections did not overreport their own political connections.

4.6 Randomization

I implemented a block randomized design wherein six geographically sequential sample households constituted a block, with all six treatment groups represented in each 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 predetermined distance between households.[15] Enumerators offered the deal to only one person per household to avoid within-household spillovers.[16] With 486 sample households in each of the three sample communes, the total sample consisted of 1,458 buyers.[17]

4.7 Measurement of primary outcomes

Sellers offered phone credit at competitive, discounted prices to incentivize buyers to consider purchasing. These prices 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 since they fit market expectations 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 capture different types of risk. First, to receive the phone credit nearly instantly, buyers could pay 700 CFA and receive 1000 CFA worth of credit. The primary risk to buyers of this simple exchange was that the bonus credit delivered could be of lower quality than sellers had promised, especially as this is difficult to detect

[15] Only 1.6% of respondents reported telling someone farther away than a next-door neighbor about the deal, and, since transactions were conducted rapidly within blocks, the threat of spillover effects was low.

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

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

until sellers may have already left the area. 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.[18] This naturally required a greater amount of buyer trust in sellers, and attempted to mimic the typical hold-up problems in modern markets where nondelivery is a risk in addition to defective products. While at first glance this delayed delivery 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.[19] The third and final purchase option available to prospective buyers increased the risk by requiring a heftier sum (1000CFA) in order to receive the most phone credit (3000CFA), again with delayed delivery. The per capita daily income in the sample communes is approximately 1500 CFA (~3 USD), so these costs were significant to respondents. Table 3 summarizes the purchase options, and Figure 3 reports buyers' purchases.

I code the outcome in two ways, to distinguish between the different dimensions of risk presented by the purchase levels. First, to measure whether potential buyers were willing to engage in exchange at all, I create a binary indicator for whether respondents purchased any level of the deal. Second, to capture the higher-risk behavior and the willingness to take on the added risk of nondelivery, I also create a binary indicator for whether respondents purchased the phone credit with delay. I present results for both outcomes throughout.

[18]The 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.

[19]The 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. Indeed, as Appendix A.14 shows, respondents' overall skepticism of the deal itself was extremely low.

Purchase level	Cost	Credits received	When phone credit arrived	Type of risk
Declined deal	-	-	-	-
No delay	700 CFA	1000 CFA	Several minutes	Risk of substandard quality
Delay ($)	500 CFA	1500 CFA	In 3 days	Risk of substandard quality and nondelivery
Delay ($$$)	1000 CFA	3000 CFA	In 3 days	Risk of substandard quality and nondelivery

Table 3: Phone credit purchase options

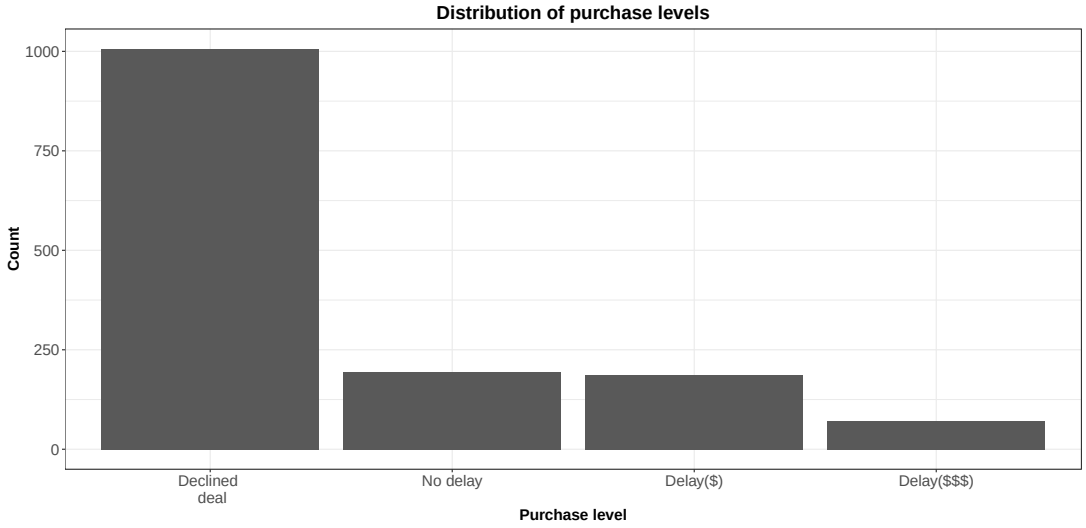


Figure 3: Distribution of buyers' purchases

4.8 Estimation

I estimate average treatment effects with the following OLS specification:[20]

$$\begin{aligned} y_i &= \alpha + \beta_1 connection_i + \beta_2 required\ contract_i + \beta_3 optional\ contract_i \\ &\quad + \beta_4 (connection_i \times required\ contract_i) + \beta_5 (connection_i \times optional\ contract_i) \\ &\quad + \gamma \mathbf{X}_i + \eta_b + \theta_e + \varepsilon_i \end{aligned} \tag{1}$$

where y_i is the indicator variable for purchasing at all or purchasing with delay, $\mathbf{X}_i$ is a matrix of covariates, η_b are randomization block fixed effects, and θ_e are enumerator fixed effects. To

[20]To take account of the non-linear nature of the purchase levels, I also present odds ratios from multinomial logistic regressions in Appendix A.19, and to take account of the binary outcome coding, I present probit models in Appendix A.23. The multinomial and probit results yield similar conclusions to those in the main body.

Table 4: Buyer belief of seller connections driven by connection signal

	Outcome: seller is connected
Connection signaled	0.188***
	(0.023)
Control group outcome mean	0.169
Control group outcome std. dev.	0.362
Outcome range	{0,1}
Fixed effects	Yes
Controls	Yes
Observations	1,458

Notes: The specification is estimated using OLS, and includes randomization block and enumerator fixed effects, as well as controls for gender, age, education, employment status, student status, and interactions 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.

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 pre-registered as one-sided. The models control for covariates that could affect acceptance of the deal, including age, education, employment status, whether the buyer was a student, and gender. To rule out competing theories of social enforcement, I also include interactive controls between treatments and 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).[21] Additional information about control variables can be found in the balance table presented in Appendix A.5 as well as the summary statistics table in Appendix A.6.

4.9 Randomization validation and manipulation check

As a heuristic for the randomization procedure's success, I estimate equation (1) using individual covariates to show that respondent-level traits do not predict treatment assignment. As shown in Appendix Table A2, there is balance over eight covariates across treatment groups. The two-sided joint F-test of the restriction that each treatment group is indistinguishable from the others was rejected in only one case.

[21]The seller team represented all of the major ethnic and religious groups of Senegal.

Important for the experiment was that the political connection signal successfully induced buyers to believe that sellers were connected. To that end, the endline survey included a question on whether buyers thought sellers were politically connected. Table 4 shows that treated respondents were 18.8 percentage points more likely to believe that sellers were politically connected, suggesting that the political connection signal was transmitted effectively.

5 Results

To test the 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 how political power asymmetries affect exchange, as well as how buyers' connections moderate the perceived utility of formal contracts.

5.1 The impact of political connections and formal contracts on exchange

What is the overall impact of signaling political connections and offering formal contracts on propensities to trade? Table 5 presents the average treatment effects (ATEs). These results first show that the impact of political connections was consistently negative (Hypothesis 1), though only significantly so for the outcome of purchasing at all. This suggests that sellers' political connections affected the perceived risk of substandard quality of products, though the multinomial results in Appendix Table A12 additionally indicate that connections decreased the likelihood of purchasing the more expensive delayed option as well. Importantly, these results do not take into account buyers' political connections, and might thus obscure important 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 5 also paint an interesting picture of the role of formal contracts. The reported estimates show that while offering formal contracts did not induce overall purchases, they did substantially boost willingness to accept the riskier delayed delivery (Hypothesis 2). Though

Table 5: Average treatment effects

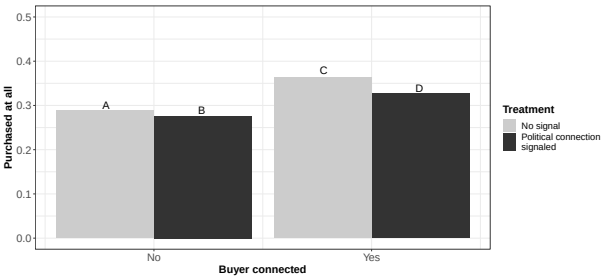
	Outcome: Purchased at all		**Outcome: Purchased with delay**	
	Unpooled	Pooled	Unpooled	Pooled
	(1)	(2)	(3)	(4)
Political connection signal	−0.053*	−0.044*	0.001	−0.013
	(0.042)	(0.031)	(0.036)	(0.027)
Required contract	0.047	0.048	0.104***	0.075**
	(0.043)	(0.037)	(0.037)	(0.032)
Optional contract	−0.003		0.059	
	(0.043)		(0.037)	
Political connection signal	0.045	0.035	−0.003	0.011
× required contract	(0.055)	(0.048)	(0.048)	(0.041)
Political connection signal	0.019		−0.029	
× optional contract	(0.055)		(0.047)	
Control group outcome mean	0.315	0.310	0.145	0.163
Control group outcome std. dev.	0.466	0.463	0.353	0.370
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 two outcomes are binary indicators for whether a buyer purchased any phone credit option or an option with delayed delivery. Specifications include randomization block and enumerator fixed effects, and controls for gender, age, education, employment status, student status, and interactions 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-registered one-sided tests.

this may appear at first blush to be an intuitive result, it is somewhat unexpected in Senegal's environment of weak norms of contract enforcement. That formal contracts were able to increase risky purchasing behavior by 10.4 percentage points is significant in this institutional context. This increase in exchange only occurred when the contract was a required part of the deal and not when buyers could opt for it, suggesting that formal contracts in these environments work best when sellers demonstrate self-constraint as an inherent part of the deal. Because the evidence suggests that the optional contract treatment arm was conceptually similar to not including a contract at all, I pool it with the control group to improve statistical power in the remainder of the paper. Models (2) and (4) in Table 5 report these pooled results. Results remain substantively similar throughout the paper with and without pooling the optional contract group.

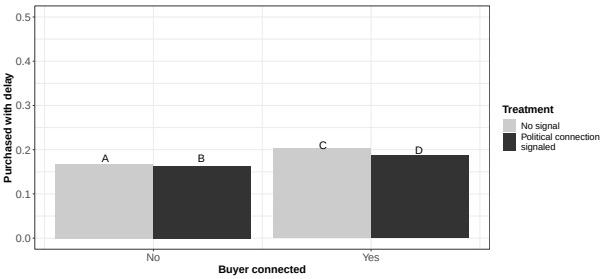
Figure 4: Effects of sellers' connections by buyers' connections

(a) Purchased at all



Panel A: Group means	**Mean**
A. Unconnected seller and unconnected buyer ($n = 346$)	0.289
B. Connected seller and unconnected buyer ($n = 362$)	0.276
C. Unconnected seller and connected buyer ($n = 360$)	0.364
D. Connected seller and connected buyer ($n = 354$)	0.328
Panel B: Difference tests	**Estimate (std. error)**
Effect of connection signal for connected buyers (D−C)	-0.059 (0.036)*
Effect of connection signal for unconnected buyers (B−A)	-0.007 (0.036)
Difference-in-differences (D−C)−(B−A)	-0.052 (0.050)

(b) Purchased with delay



Panel A: Group means	**Mean**
A. Unconnected seller and unconnected buyer ($n = 346$)	0.168
B. Connected seller and unconnected buyer ($n = 362$)	0.163
C. Unconnected seller and connected buyer ($n = 360$)	0.203
D. Connected seller and connected buyer ($n = 354$)	0.186
Panel B: Difference tests	**Estimate (std. error)**
Effect of connection signal for connected buyers (D−C)	-0.034 (0.031)
Effect of connection signal for unconnected buyers (B−A)	0.012 (0.031)
Difference-in-differences (D−C)−(B−A)	-0.046 (0.043)

Notes: Panels A presents group means for the four subgroups. Panels B presents differences estimated using OLS with linear restrictions. Otherwise, see Table 5.

Turning to the models' interactive terms, the results are inconclusive: though required formal contracts appear to mitigate some of the distrust that political connections induce, the estimates are too noisy to draw conclusions. 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 further parse the relationship between connections and contracts in Section 5.3 by taking buyers' political connections into account.

5.2 Imbalances in buyer-seller political connections affect exchange

As described above, asymmetric political power between buyers and sellers implies unequal contract enforcement privileges, and we should thus expect to observe differences in rates of exchange as a function of imbalances in the trading dyad. I therefore estimate the impact of the political connection treatment by buyers' political connections.

Figures 4a and 4b present decomposed results by buyers' and sellers' political connections

for the purchased at all and purchased with delay outcomes, respectively.[22] For each subfigure I present group means in Panel A, and in Panel B report covariate-adjusted differences from linear restrictions on Equation 1.

As these figures show, the expected result of stifled purchases when sellers were politically connected did not materialize across all buyer types. Rather, exchange was stifled only for the purchase at all outcome, and only for connected buyers, who preferred to exchange with less powerful sellers. The multinomial and probit results (Appendix A.19 and A.23, respectively) lend additional support for this finding. These trades represent the cases in which buyers are most powerful and least at-risk, in that connected buyers maintain disproportionate political power and recourse options if the deal were to go awry. Indeed, in the endline survey, when asked what they would have done had the terms of the exchange been violated, buyers with political connections were more likely than unconnected buyers to state that they would pursue recourse options, more likely to seek formal recourse options in hypothetical contract disputes, and more likely to believe in the preferential enforcement power of the state (see Appendix A.21).

Interestingly, there was no similar effect among unconnected buyers. This may be due to the generally low purchase rates, which suggest that these estimates are perhaps a lower bound for unconnected types. This may also be due to lack of political knowledge and experience: I show in Appendix A.20 that politically unconnected buyers were less likely to correctly update about sellers' political connections, and provide suggestive evidence that some unconnected buyers mistakenly believed that unconnected sellers were connected. These inferential issues may help to account for the lack of effect among unconnected buyers.

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 A.11, buyers' political connections are not strongly correlated with such variables, and the results throughout the paper are robust to including interactive treatment controls for these potential confounders (see Appendix A.22). This lack of correlation

[22] Appendix A.9 presents these results in table form.

fits the case of 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 income neighborhoods where I implemented the field experiment.

Overall, these results lend only partial support to Hypothesis 3: although connected buyers were less likely to exchange with connected sellers, there was no similar effect for unconnected buyers. Thus, at least among connected buyers, sellers' political connections stifled trade by enhancing the risk of receiving substandard quality goods and not the risk of nondelivery, though this latter estimate may be limited by the relatively low takeup of the offer. Although not in the scope of this paper's theory, I speculate that this might also be due to risk tolerances: political connections may affect the most risk-averse buyers, whereas among more risk-seeking buyers, political connections are not enough to deter purchasing the more lucrative delayed options.

5.3 Formal contracts primarily protect connected buyers

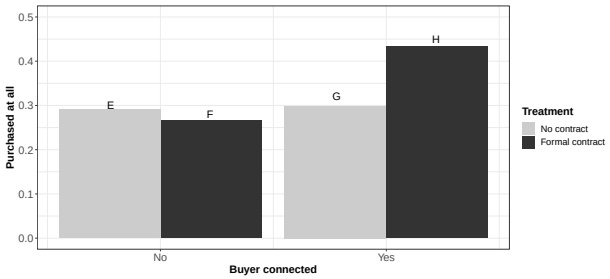
The ATE estimates in Section 5.1 showed that offering formal contracts increased the probability of the riskiest types of exchange. But how do formal contracts operate in the context of important political connections? In a world where the ability to enforce contracts is biased towards the politically 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 by buyers' political connections.

Figures 5a and 5b present these results, which reveal a stark pattern: the effect of formal contracts on propensities to trade is driven primarily by connected buyers, both for purchasing at all as well as purchasing with delay (Hypothesis 4).[23] Among unconnected buyers, formal contracts had no impact on the purchased at all outcome, and a small impact on the purchased with delay outcome. As with the previous models, these results are robust to interacting treatment with potential confounders as well as to controlling for predictors of social enforcement. These findings suggest that, while formal contracts may improve confidence in exchange, they do so for a

[23] I provide the corresponding model output in table form in Appendix A.9.

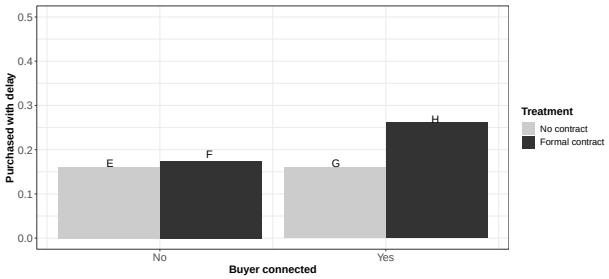
Figure 5: Effects of formal contracts by buyers' connections

(a) Purchased at all



Panel A: Group means	Mean
E. No contract and unconnected buyers ($n = 479$)	0.290
F. Formal contract and unconnected buyers ($n = 229$)	0.266
G. No contract and connected buyers ($n = 465$)	0.299
H. Formal contract and connected buyers ($n = 249$)	0.434
Panel B: Difference tests	**Estimate (std. error)**
Effect of formal contract for connected buyers (H−G)	0.117 (0.037)***
Effect of formal contract for unconnected buyers (F−E)	0.013 (0.038)
Difference-in-differences (H−G)−(F−E)	0.104 (0.052)**

(b) Purchased with delay



Panel A: Group means	Mean
E. No contract and unconnected buyers ($n = 479$)	0.161
F. Formal contract and unconnected buyers ($n = 229$)	0.175
G. No contract and connected buyers ($n = 465$)	0.159
H. Formal contract and connected buyers ($n = 249$)	0.261
Panel B: Difference tests	**Estimate (std. error)**
Effect of formal contract for connected buyers (H−G)	0.105 (0.032)***
Effect of formal contract for unconnected buyers (F−E)	0.055 (0.032)*
Difference-in-differences (H−G)−(F−E)	0.049 (0.045)

Notes: See Figure 4.

particular subset of the population: those who can be confident in their ability to sway enforcement in their favor during disputes (see Appendix A.21). Formal contracts may thus not enhance the recourse options or protect those who are otherwise powerless; they may be a viable enforcement solution only for those who are already most privileged in societies with selectively enforced rule of law.

5.4 Alternative hypotheses and robustness

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

As shared social identity has been shown to reduce transaction costs (Grimard 1997; Sanchez de la Sierra 2018), buyers with similar social networks to sellers—in Senegal proxied by shared ethnic group or religious network—may have experienced a greater sense of confidence and security in the deal compared to out-group members. This may have also moderated treatment; for example, while political connections may be off-putting to buyers in the aggregate, they could be perceived as valuable if the seller who has them belongs to the same in-group network. As the estimates in the preceding sections as well as in Appendix A.12 show, however, results are robust to interactive

Table 6: Quality measures from buyers and sellers

	Buyer's perception of...				Seller's perception of...					
	Seller's competence		Trustworthiness		# of questions asked		Buyer's politeness		Buyer's suspicion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Political connection signal	−0.023	−0.035	−0.004	−0.026	0.038	0.064*	0.050	0.040	0.063	0.076
	(0.032)	(0.038)	(0.050)	(0.059)	(0.028)	(0.033)	(0.043)	(0.050)	(0.069)	(0.081)
Formal contract	0.044	0.026	0.073	0.040	0.033	0.072*	−0.046	−0.061	0.006	0.027
	(0.034)	(0.045)	(0.052)	(0.070)	(0.030)	(0.039)	(0.045)	(0.060)	(0.072)	(0.096)
Political connection signal × formal contract		0.035		0.066		−0.077		0.029		−0.040
		(0.058)		(0.091)		(0.051)		(0.078)		(0.126)
Control group outcome mean	3.603	3.603	2.485	2.485	0.952	0.952	3.476	3.476	0.884	0.884
Control group 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: See Table 4.

controls between treatments and buyer/seller co-ethnicity and co-religiosity. I similarly rule out the possibility of findability mechanisms (e.g. Miguel and Gugerty 2005; Habyarimana et al. 2007), discussed in greater detail in Appendix A.13.

5.4.2 Addressing confounding interpretations of seller political connections

A potential concern is that by signaling political connections, sellers transmitted information about their quality or competence 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 completed a 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' competence and trustworthiness.[24] I regress these measures of perceived quality on the treatment indicators, and present the results in Table 6.

The findings show that treatment did not drive respondents' opinions of sellers' competence, 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

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

treatment groups. Examining the nature of these questions more closely, buyers typically asked logistical questions, such as where to sign and date, as well as some questions regarding the terms of the contract. Questions related to the political connection were typically about the nature of sellers' work at councils and whether they were still based there. The sum of evidence suggests that treatment effects were not driven by concerns over quality or competence.

6 Conclusion

In areas where rule of law is selectively enforced, political connections can produce moral hazard in exchange and complicate the function of formal enforcement institutions. Using evidence from a field experiment in a real 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 environments with weak 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. Counterintuitively, increasing the availability of formal contracts may intensify economic inequalities and market segmentation.

This project represents an initial, partial equilibrium approach to identify 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—including where reputational considerations significantly structure markets—and should strive toward testing the general equilibrium implications. Although this paper empirically distinguishes between core components of seller moral hazard—the risks of substandard quality products and of nondelivery—future research could more explicitly theorize and test the underpinnings of this distinction. Finally, while this field experiment highlighted the impact of political connections and formal contracts, more work is needed to identify the precise behavioral mechanisms behind this impact, including the psychological foundations of their effects.

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, people must use a mixture of formal and informal mechanisms to enforce their deals. This paper provides evidence for how informal networks of political influence in these places can impede the function of formal institutions in shaping private-sector economic development. These results also help to explain how legal institutions facilitating contract enforcement can coexist with rising inequality and lagging development.

As emerging markets continue to develop, problems stemming from unequal political influence in the private sector may grow more amplified as well. This can have distributive consequences 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, resulting in suppressed overall levels of trade and inefficiencies. Understanding how informal connections—political and otherwise—moderate

institutional access (e.g. Slough 2020) and interact with state institutions for enforcement will thus be particularly important for private-sector growth in the coming years.

In addition to its theory and findings, this project contributes a design-based technique to examine these important questions. If ethical standards are exhaustively met, creating a business offers political scientists unprecedented experimental control and realism, which has implications for the study of business and politics across a variety of settings.

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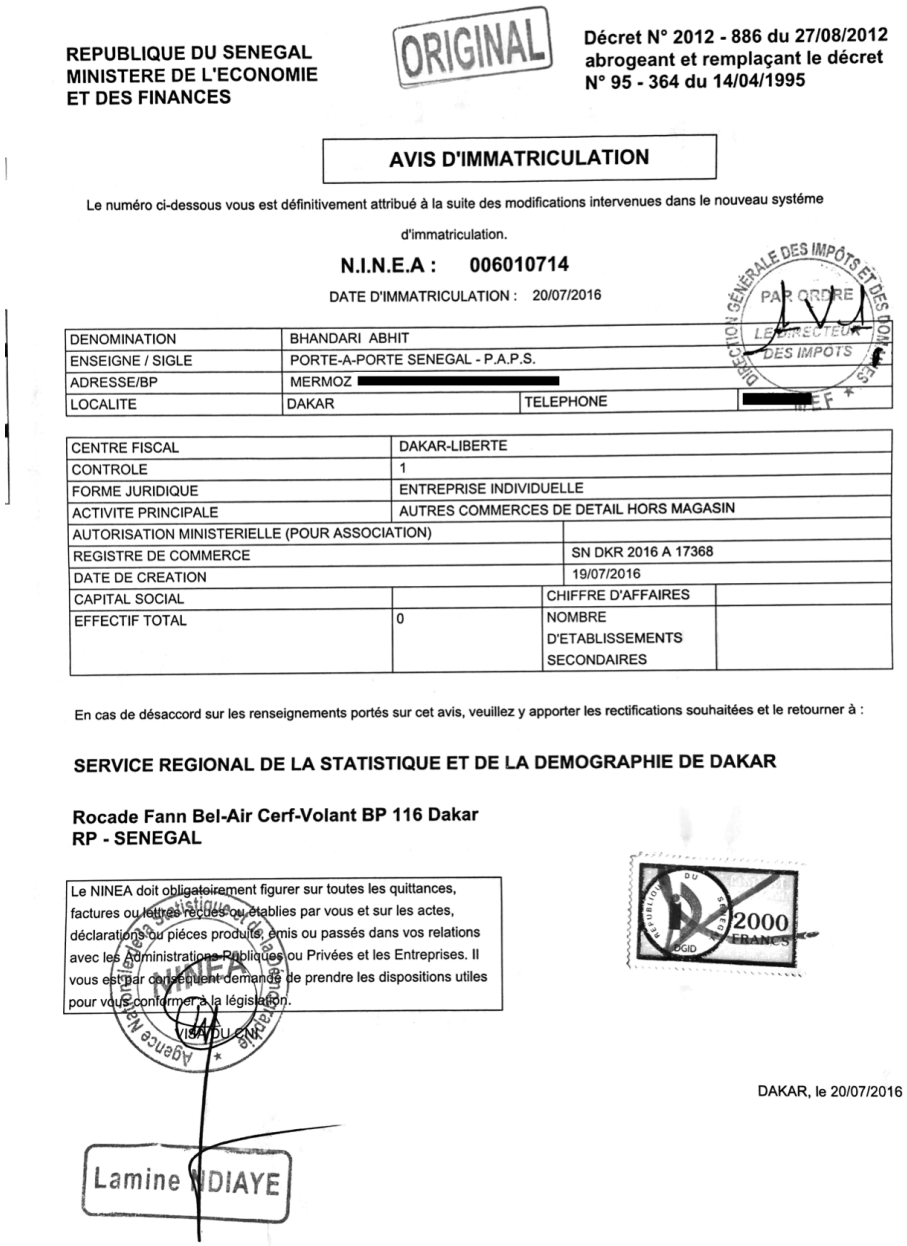
A Online Appendix

Contents

A.1 Business registration document



REPUBLIQUE DU SENEGAL
MINISTERE 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 systéme d'immatriculation.

N.I.N.E.A : 006010714

DATE D'IMMATRICULATION : 20/07/2016

DIRECTION GÉNÉRALE DES IMPÔTS ET DES DOMAINES
PAR ORDRE
LE DIRECTEUR DES IMPOTS

DENOMINATION	BHANDARI ABHIT		
ENSEIGNE / SIGLE	PORTE-A-PORTE SENEGAL - P.A.P.S.		
ADRESSE/BP	MERMOZ [redacted]		
LOCALITE	DAKAR	TELEPHONE	[redacted]

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.

Agence Nationale de la Statistique et de la Démographie
NINEA
VISA DU CNP
Lamine NDIAYE

2000 FRANCS
DGID

DAKAR, le 20/07/2016

Figure A1: PAPS business registration

A.2 Transaction protocol

The following is the translated protocol outline that sellers followed during transactions. While sellers 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. The tablets that sellers used were programmed to automatically generate the relevant protocol components depending on the household's predetermined treatment status.

> 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.3 Formal contract language

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.4 Main results when outcome is coded from 0 to 3

This section presents the main results using OLS with the primary outcome coded from 0 to 3, indicating the purchase level in Table 3. The results yield similar substantive conclusions to those presented in the body of the paper.

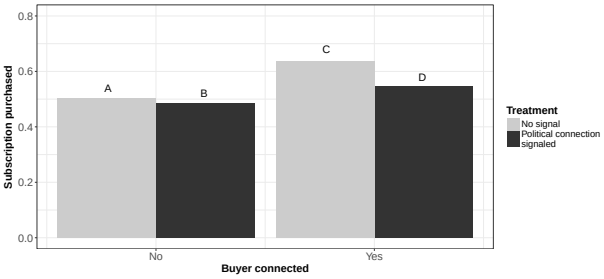
Table A1: Average treatment effects (outcome coded 0 to 3)

	Outcome: Purchase level			
	Unpooled		Pooled	
	(1)	(2)	(3)	(4)
Political connection signal	−0.075	−0.058	−0.083*	−0.058
	(0.081)	(0.052)	(0.061)	(0.052)
Required contract	0.165**	0.198***	0.139*	0.176***
	(0.084)	(0.063)	(0.072)	(0.054)
Optional contract	0.053	0.045		
	(0.085)	(0.064)		
Political connection signal × required contract	0.065		0.073	
	(0.109)		(0.094)	
Political connection signal × optional contract	−0.016			
	(0.108)			
Control group outcome mean	0.515	0.515	0.529	0.529
Control group 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, student status, and interactions 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.

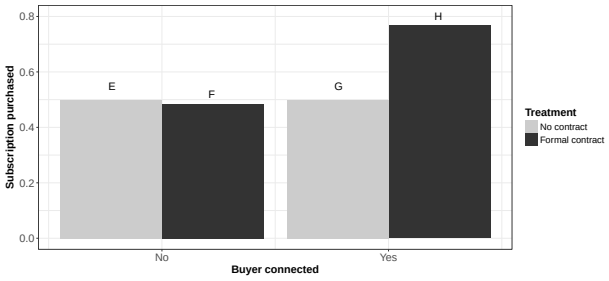
Figure A2: Treatment effects by buyers' connections (outcome coded 0 to 3)

(a) Political connection treatment



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)	-0.130 (0.071)**
Effect of connection signal for unconnected buyers (B−A)	0.009 (0.071)
Difference-in-differences (D−C)−(B−A)	-0.139 (0.098)

(b) Formal contract treatment



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)	0.267 (0.073)***
Effect of formal contract for unconnected buyers (F−E)	0.081 (0.075)
Difference-in-differences (H−G)−(F−E)	0.185 (0.102)*

Notes: Panel A presents group means and standard deviations of the four subgroups. Panel B presents differences estimated using OLS with linear restrictions. Difference tests include randomization block and enumerator fixed effects. Controls include gender, age, education, employment status, student status, and interactions 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.

A.5 Balance

Table A2: Covariate balance across treatment groups

Covariate	Observations	1. Control group	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	0.029	0.025	-0.025	0.045	-0.033	0.89
		(0.032)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	
Education	1458	2.473	0.058	0.029	0.066	0.091	−0.008	0.70
		(0.084)	(0.118)	(0.118)	(0.118)	(0.118)	(0.118)	
Age	1458	34.977	-0.180	0.891	-0.884	0.861	0.394	0.64
		(0.833)	(1.175)	(1.175)	(1.175)	(1.175)	(1.173)	
Employment	1458	0.682	0.028	-0.036	-0.037	0.012	-0.014	0.35
		(0.030)	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)	
Student	1458	0.089	0.037	0.034	0.049*	0.025	0.050*	0.12
		(0.021)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	
Trust council	1458	1.656	0.054	0.174	0.163	0.092	0.198*	0.05**
		(0.077)	(0.108)	(0.108)	(0.108)	(0.108)	(0.108)	
Trust courts	1458	1.961	0.088	0.116	−0.048	−0.048	−0.043	0.65
		(0.067)	(0.094)	(0.094)	(0.094)	(0.094)	(0.094)	
Use courts	1458	0.031	0.011	-0.005	-0.001	0.011	0.003	0.95
		(0.012)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	
Co-ethnic/co-religious to seller	1458	.251	-0.025	0.022	-0.033	-0.033	-0.032	0.60
		(0.027)	(0.038)	(0.038)	(0.038)	(0.038)	(0.038)	

Notes: Coefficients are estimated by regressing covariates on treatment groups using OLS. The final column reports the p-values from joint F-tests of the restriction that each treatment group is indistinguishable from the others. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

A.6 Summary statistics for control variables

Covariate	N	Mean	St. Dev.	Min	Median	Max
Age	1,458	35.158	12.923	15	32	83
Gender (1 = woman)	1,458	0.525	0.500	0	1	1
Currently employed	1,458	0.674	0.463	0	1	1
Currently a student	1,458	0.121	0.322	0	0	1
Highest level of education	1,458	2.512	1.301	1	2	5
Co-ethnic or co-religious to seller	1,458	0.234	0.418	0	0	1

Table A3: Summary statistics of control variables

A.7 Attrition not predicted by treatment

Table A4: Attrition as predicted by treatment arm

	Outcome: Attrited (1)
Political connection signal	−0.008
	(0.009)
Required contract	−0.015
	(0.011)
Optional contract	−0.002
	(0.011)
Control mean	0.046
Control std. dev.	0.209
Fixed effects	Yes
Controls	Yes
Observations	1,458

Notes: The specification is estimated using OLS. The outcome is whether a respondent attrited at endline. The specification includes randomization block and enumerator fixed effects, and controls for gender, age, education, employment status, student status, and interactions 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.

A.8 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 A5 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 A5: Contract choice as predicted by treatment

	Outcome: Chose contract (1)
Political connection signal	0.097
	(0.125)
Buyers connected	0.203
	(0.125)
Political connection signal	−0.198
× buyers connected	(0.182)
Control group outcome mean	.351
Control group std. dev.	.484
Fixed effects	No
Controls	Yes
Observations	140

Notes: The specification is estimated using OLS. The outcome is whether a respondent chose the formal contract. The specification includes controls for gender, age, education, employment status, and student status, and an interactive control between treatment and buyer/seller co-ethnicity/co-religiosity. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$ from two-sided tests.

A.9 Corresponding model output for Figures 4a, 4b, 5a, and 5b

Table A6: Heterogeneous treatment effects by buyers' political connections

	Political connection treatment		**Formal contract treatment**	
	Purchase at all	Purchase with delay	Purchase at all	Purchase with delay
	Figure 4a	*Figure 4b*	*Figure 5a*	*Figure 5b*
Panel A: Model output	(1)	(2)	(3)	(4)
Political connection signal	−0.007	0.012		
	(0.036)	(0.031)		
Formal contract			0.013	0.055*
			(0.038)	(0.033)
Buyer connected	0.064*	0.059*	0.001	0.018
	(0.037)	(0.032)	(0.032)	(0.027)
Political connection signal	−0.052	−0.046		
× buyer connected	(0.050)	(0.043)		
Formal contract			0.104**	0.049
× buyer connected			(0.052)	(0.045)
Panel B: Difference test				
Treatment effect among	−0.059*	−0.034	0.117***	0.105***
connected buyers	(0.036)	(0.031)	(0.037)	(0.032)
Control group outcome mean	0.289	0.168	0.290	0.161
Control group outcome std. dev.	0.454	0.374	0.454	0.368
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 two outcomes are binary indicators for whether a buyer purchased any phone credit option or an option with delayed delivery. Specifications include randomization block and enumerator fixed effects, and controls for gender, age, education, employment status, student status, and interactions 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.10 Trust in courts across Africa

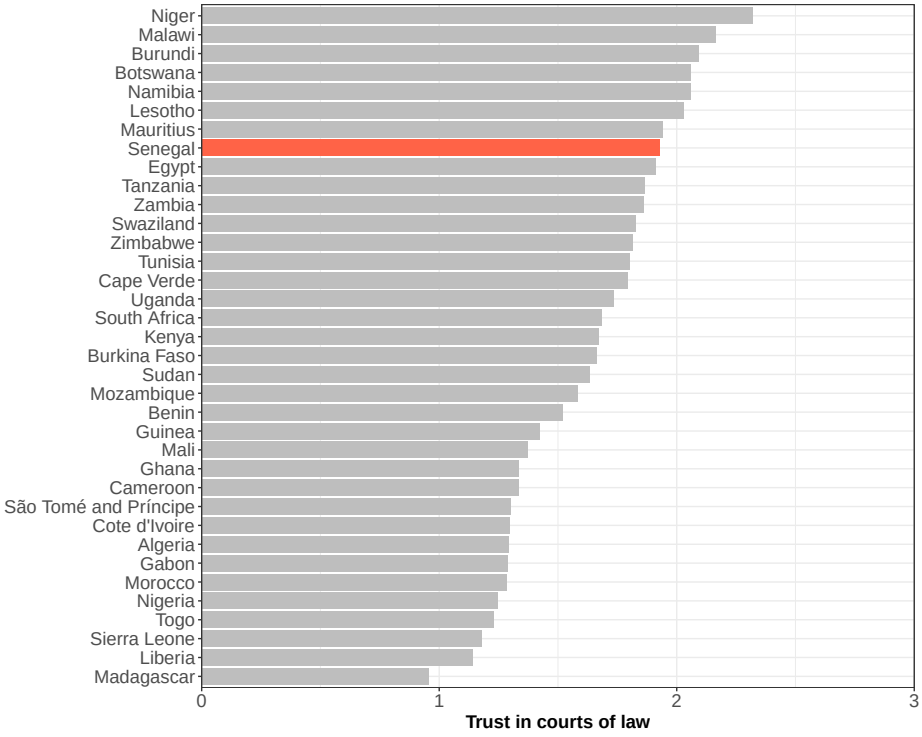


Figure A3: Trust in courts of law in Africa. Source: Afrobarometer 2016.

A.11 Correlates of buyers' political connections

Table A7: Correlation table

	Buyer connected	Gender	Education	Employed	Age	Student
Buyer connected	1	0.001	0.056	0.040	0.035	-0.023
Gender		1	-0.058	-0.203	0.041	-0.066
Education			1	-0.299	-0.253	0.480
Employed				1	0.143	-0.534
Age					1	-0.339
Student						1

A.12 Heterogeneous effects by in-group

Table A8: Heterogeneous effects by co-ethnicity and shared religious network

	Outcome:	
	Purchased at all	**Purchased with delay**
	(1)	(2)
Political connection signal	−0.044*	−0.013
	(0.031)	(0.027)
Formal contract	0.048	0.075**
	(0.037)	(0.032)
Buyer/seller co-ethnic/co-religious	−0.024	−0.023
	(0.048)	(0.042)
Political connection signal	0.035	0.011
× formal contract	(0.048)	(0.041)
Political connection signal	0.055	0.007
× buyer/seller co-ethnic/co-religious	(0.058)	(0.049)
Formal contract	−0.023	−0.082
× buyer/seller co-ethnic/co-religious	(0.061)	(0.052)
Control group outcome mean	0.297	0.154
Control group outcome std. dev.	0.457	0.362
Fixed effects	Yes	Yes
Controls	Yes	Yes
Observations	1,458	1,458

Notes: Each specification is estimated using OLS. The two outcomes are binary indicators for whether a buyer purchased any phone credit option or an option with delayed delivery. Specifications include randomization block and enumerator fixed effects, and controls for gender, age, education, employment status, student status, and interactions 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-registered one-sided tests.

A.13 Heterogeneous effects by district match

Findability mechanisms may improve perceptions of social enforcement (Besley, Coate and Loury 1993; Grimard 1997; Sanchez de la Sierra 2018). 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 A9 show, district match did not drive the results.

Table A9: Findability mechanism: heterogeneous effects by buyer/seller district match

	Outcome:	
	Purchased at all	**Purchased with delay**
	(1)	(2)
Political connection signal	−0.056*	0.0004
	(0.037)	(0.032)
Formal contract	0.070	0.064*
	(0.044)	(0.038)
District match	0.417	−0.179
	(0.336)	(0.289)
Political connection signal	0.020	0.001
× formal contract	(0.059)	(0.051)
Political connection signal	0.035	−0.040
× district match	(0.059)	(0.050)
Formal contract	−0.064	0.032
× district match	(0.072)	(0.062)
Political connection signal	0.043	0.031
× formal contract × district match	(0.102)	(0.087)
Control group outcome mean	0.314	0.171
Control group outcome std. dev.	0.465	0.377
Fixed effects	Yes	Yes
Controls	Yes	Yes
Observations	1,458	1,458

Notes: Each specification is estimated using OLS. The two outcomes are binary indicators for whether a buyer purchased any phone credit option or an option with delayed delivery. District match is a binary variable indicating when the seller's connection was from the buyer's home district. Specifications include randomization block and enumerator fixed effects, and controls for gender, age, education, employment status, student status, and interactions 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-registered one-sided tests.

A.14 Treatments did not raise buyers' suspicions

Table A10: Buyer suspicion as predicted by treatment

	Outcome: Buyer's suspicion	
	(1)	(2)
Political connection signal	0.045	
	(0.062)	
Optional contract		0.062
		(0.066)
Control group outcome mean	0.75	0.75
Control group outcome std. dev.	1.21	1.23
Fixed effects	Yes	Yes
Controls	Yes	Yes
Observations	1,458	1,458

Notes: Each specification is estimated using OLS. The outcome is the reported buyer's suspicion level on a scale from 0 to 4. Specifications include 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.15 No overreporting of connections based on political connections

Table A11: Buyers' political connections as predicted by political connection treatment

	Outcome: Buyer is politically connected
	(1)
Political connection treatment	−0.010
	(0.024)
Control group outcome mean	0.510
Control group outcome std. dev.	0.493
Fixed effects	Yes
Controls	Yes
Observations	1,458

Notes: The specification is estimated using OLS. The outcome is a binary indicator for whether the buyer reported any political connection. 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.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 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. These changes do not significantly affect the conclusions of the paper.

3. Due to space constraints, some of the secondary tests mentioned in the pre-analysis plan are not presented in the main body.

4. Based on reviewer comments, the coding of the outcome variable is different than in the pre-analysis plan. I present the pre-specified models in Appendix A.4. Both outcome codings yield similar substantive conclusions.

A.17 Solutions to commitment problems in trade

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 et al. 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; Jackson and Xing 2019). This paper concerns contexts that fall in between: areas where states do have the capacity to enforce and where institutions are generally cooperative, but where states preferentially rule in favor of privileged parties.

A.18 Social enforcement mechanisms in Senegal

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). Religious networks are particularly important, with the majority of citizens belonging to Islamic brotherhoods characterized by tight-knit social structures with common sources of authority (Cruise O'Brien 1971; Villalon 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. However, social networks are less cohesive in Dakar than in rural Senegal, and social networks may not substitute for formal contracts as much as they might in rural areas (Koter 2013).[25] These social mechanisms are also less relevant to large-scale trade and markets where relational contracting is rare or impossible, the increasingly common type of trade that this field experiment replicated. Nevertheless, in the results throughout, I control for shared in-group affiliation to ensure it does not drive results.

A.19 Multinomial logistic regression results

The following table presents the odds ratios from multinomial logistic regressions where the base outcome is not purchasing at all. Odds ratios can thus be interpreted as the odds of choosing the indicated purchase option relative to not purchasing at all. Odds ratios below 1 can be interpreted as a negative effect, and above 1 as a positive effect. As the overall treatment effects show, the political connection signal decreased both the immediate and the costly delayed purchase options, suggesting it affected both the risk of substandard quality of product as well as the the risk of nondelivery of goods. Formal contracts, by contrast, increased exchange primarily by inducing sales of the first delayed purchase option.

The conditional treatment effects for the political connection treatment, presented in models 4-6, do not show differential treatment effects by buyers' connections, but Panel B shows that politically connected buyers were less likely to purchase the immediate option when sellers were politically connected. The conditional effects for the formal contract treatment, presented in models 7-9, show that politically connected buyers were more likely than unconnected buyers to purchase both the immediate option and the costly delayed option, and Panel B shows that politically connected buyers were more likely to engage in exchanges with delayed delivery when they were offered formal contracts.

[25] Similarly, 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.

Table A12: Multinomial logit odds ratios

	Overall effects			Conditional: political connection treatment			Conditional: formal contract treatment		
Panel A: Model output	Immediate (1)	Delay ($) (2)	Delay ($$$) (3)	Immediate (4)	Delay ($) (5)	Delay ($$$) (6)	Immediate (7)	Delay ($) (8)	Delay ($$$) (9)
Political connection signal	0.633**	0.988	0.335**	0.791	1.081	0.787			
	(0.268)	(0.267)	(0.517)	(0.331)	(0.303)	(0.549)			
Formal contract	0.811	1.902**	1.645				0.641	1.550	1.337
	(0.322)	(0.305)	(0.525)				(0.360)	(0.323)	(0.588)
Buyer connected				1.268	1.793*	1.945	0.845	1.311	0.776
				(0.330)	(0.308)	(0.566)	(0.284)	(0.275)	(0.515)
Political connection signal × formal contract	1.597	1.060	3.096						
	(0.417)	(0.387)	(0.706)						
Political connection signal × buyer connected				0.862	0.787	0.462			
				(0.438)	(0.407)	(0.720)			
Formal contract × buyer connected							2.401*	1.520	4.138*
							(0.472)	(0.427)	(0.777)
Panel B: Difference test									
Treatment effect among connected buyers				0.502*	0.661	0.785	1.924	2.348**	30.470***
				(0.379)	(0.387)	(0.740)	(0.406)	(0.410)	(1.003)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	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

Notes: This table presents the odds ratios calculated by exponentiating coefficients from multinomial logistic regressions. The reference outcome is not purchasing. The table presents average treatment effects (ATEs) as well as conditional average treatment effects (CATEs) for each of the primary treatment arms. Specifications include randomization block and enumerator fixed effects, and controls for gender, age, education, employment status, student status, and interactions between treatments and buyer/seller co-ethnicity/co-religiosity. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

A.20 Variation in perceived seller connectivity by buyer type

The following table presents heterogeneous effects of beliefs about sellers' connections by buyers' connections. The positive and statistically significant interaction term indicates that, in the treatment group, politically connected buyers were more likely to correctly infer sellers were politically connected relative to unconnected buyers. Though statistically insignificant, the negative coefficient in front of the constituent term for buyers' connectivity suggests that, in the control group, unconnected buyers may have been more likely to mistakenly believe that sellers were connected.

Table A13: Belief in sellers' political connections by buyers' connections

	Outcome: Belief that seller is connected
	(1)
Buyer connected	−0.043
	(0.033)
Political connection signal	0.147***
	(0.032)
Buyer connected	0.082*
× political connection signal	(0.044)
Control group outcome mean	0.152
Control group outcome std. dev.	0.352
Fixed effects	Yes
Controls	Yes
Observations	1,458

Notes: The specification is estimated using OLS. The outcome is a binary indicator for whether buyers believed sellers were politically connected. The specification includes randomization block and enumerator fixed effects, and controls for gender, age, education, employment status, student status, and interactions 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.

A.21 Connected buyers more likely to seek recourse

Table A14: Connected buyers and recourse

	Outcomes:						
	Would have sought recourse	Would use formal enforcement	Have used courts in past dispute	Have used police in past dispute	Lawyers can resolve broken deal	Police can resolve broken deal	Courts can resolve broken deal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Buyer connected	0.144***	0.069***	0.013	0.030**	0.174***	0.192***	0.107*
	(0.026)	(0.024)	(0.009)	(0.015)	(0.060)	(0.064)	(0.061)
Control group outcome mean	0.418	0.280	0.026	0.075	2.369	2.325	2.579
Control group outcome std. dev.	0.494	0.449	0.158	0.263	1.2	1.239	1.19
Outcome variable range	{0,1}	{0,1}	{0,1}	{0,1}	{0,1,...,4}	{0,1,...,4}	{0,1,...,4}
Fixed effects	No	No	No	No	No	No	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,458	1,458	1,458	1,458	1,458	1,458	1,458

Notes: Each specifications is estimated using OLS. Outcomes are: (1) whether buyers would have sought recourse had the deal been broken, (2) whether buyers would seek formal enforcement options in the case of a hypothetical dispute, (3-4) whether they have used courts and police in past contract disputes, respectively, and (5-7) the extent to which they believe lawyers, police, and courts, respectively, could resolve hypothetical contract disputes. 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.22 Interactive controls

I use interactive controls between treatment and education as well as treatment and employment (proxies for wealth in Senegal) to help alleviate concerns that buyers' incomes might be confounding the main heterogeneous treatment effects of the paper. I compare current estimates to models with an interaction between the treatment variables and demeaned covariates. I demean the covariates such that the interactions of interest—between treatments and buyers' political connections—are evaluated at mean 0 of the potentially confounding covariates. This ensures that the treatment $\times$ covariate interactions do not affect the lower-order treatment variables. The similar interaction coefficients of interest across models in the table below indicate that estimates are not confounded by these interactions. I conclude that education and employment—which serve as good proxies for income in this context—are unlikely to be confounding interpretations of heterogeneous treatment effects by buyers' political connections.

Table A15: Interactive control model comparisons

	Outcome: Purchased at all							
	Education				**Employment status**			
	Current specification		Interactive specification		Current specification		Interactive specification	
Panel A: Model output	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Political connection signal	−0.008		−0.008		−0.007		−0.007	
	(0.036)		(0.036)		(0.036)		(0.036)	
Formal contract		0.013		0.010		0.013		0.013
		(0.038)		(0.038)		(0.038)		(0.038)
Buyer connected	0.063*	0.001	0.063*	−0.00	0.064*	0.001	0.063*	0.001
	(0.037)	(0.032)	(0.037)	(0.032)	(0.037)	(0.032)	(0.037)	(0.032)
Political connection signal	−0.050		−0.050		−0.052		−0.051	
$\times$ buyer connected	(0.050)		(0.050)		(0.050)		(0.050)	
Formal contract		0.104**		0.110**		0.104**		0.104**
$\times$ buyer connected		(0.052)		(0.052)		(0.052)		(0.052)
Panel B: Difference test								
Treatment effect among	-0.059*	0.117***	-0.059*	0.120***	-0.059*	0.117***	-0.059*	0.117***
connected buyers	(0.036)	(0.037)	(0.036)	(0.037)	(0.036)	(0.037)	(0.036)	(0.037)
Control group outcome mean	0.289	0.290	0.289	0.290	0.289	0.290	0.289	0.290
Control group outcome std. dev.	0.454	0.454	0.454	0.454	0.454	0.454	0.454	0.454
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	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

Notes: Each specification is estimated using OLS. The outcome is a binary indicator for whether a buyer purchased any phone credit option. Current specifications indicate the primary specifications used in the paper. Interactive specifications include an interactive control between treatments and the demeaned covariate of interest (education or employment, as indicated). Specifications include randomization block and enumerator fixed effects, and controls for gender, age, education, employment status, student status, and interactions 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.

A.23 Probit results

Table A16: Probit results

	ATEs		CATEs - political connection signal		CATEs - formal contract	
Panel A: Model output	Purchased at all (1)	Purchased with delay (2)	Purchased at all (3)	Purchased with delay (4)	Purchased at all (5)	Purchased with delay (6)
Political connection signal	−0.150*	−0.054	0.004	0.070		
	(0.113)	(0.134)	(0.132)	(0.153)		
Formal contract	0.186	0.395**			0.004	0.267
	(0.133)	(0.154)			(0.141)	(0.163)
Buyer connected			0.273**	0.340**	−0.028	0.095
			(0.134)	(0.155)	(0.117)	(0.138)
Political connection signal × formal contract	0.121	0.054				
	(0.172)	(0.196)				
Political connection signal × buyer connected			−0.239	−0.241		
			(0.179)	(0.206)		
Formal contract × buyer connected					0.471**	0.275
					(0.191)	(0.216)
Panel B: Difference test						
Treatment effect among connected buyers			-0.235**	-0.171	0.475***	0.543***
			(0.131)	(0.151)	(0.138)	(0.156)
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 probit. The two outcomes are binary indicators for whether a buyer purchased any phone credit option or an option with delayed delivery. Specifications include randomization block and enumerator fixed effects, and controls for gender, age, education, employment status, student status, and interactions 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-registered one-sided tests.

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