Online Appendix for

Partners in crime? Corruption as a criminal network

September 3, 2020

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A Additional theoretical result: cost of corruption as expected loss

In this extension, I consider a different formulation for the cost of corruption. While the main specification assumes that ϵ is a sunk cost that accomplices pay upfront, this extension assumes instead that agents pay the cost ϵ if and only if they get caught, which occurs with probability $1 - p(a, w, q)$. With equal-sharing, the payoff function in equation 1 and valuation functions become:

$$u(c, q, \epsilon) \equiv v(a_c, w_{cg}, q, \epsilon) \equiv \frac{p(a_c, w_{cg}, q)}{a_c} - [1 - p(a_c, w_{cg}, q)]\epsilon \tag{A1}$$

if $i \in c$, and 0 otherwise.

I show in the next subsection that all the results from the main specification; that is, lemmas 1.1, propositions 1.1 to 1.5 and corollary 1.3.1 are left virtually unchanged, under qualitatively similar assumptions. Similar to the monopoly rule, the only noticeable difference is that the set of coalitions that are optimal to the seed, $\mathcal{C}^*(g, s, q)$, now varies with ϵ. Assumptions 1.1 and 1.2 are replaced by qualitatively similar assumptions. The new assumption 1.2 still requires the ratio of partial derivatives of p with respect to q for any two coalitions to be bounded above by some quantity greater than 1. Additionally, I assume that $\epsilon \in (0, 1)$. Assumptions 1.1 and 1.2 become, respectively:

Assumption A1 (Assumption 1.1 under extension with cost as expected loss)**.** If $v(a_1, w_1, q, \epsilon) = v(a_2, w_2, q, \epsilon)$ for some $a_1 \leq a_2$, $w_1, w_2 \in \{0, ..., N\}$, $q \in (0, 1)$, $\epsilon > 0$, then $\frac{\partial p(a_2, w_2, q, \epsilon)}{\partial q} \Big/ \frac{\partial p(a_1, w_1, q, \epsilon)}{\partial q} \neq \frac{a_1 a_2 + a_2}{a_1 a_2 + a_1}$ for any $q \in (0, 1)$.

Assumption A2 (Assumption 1.2 under extension with cost as expected loss)**.** If $v(a_1, w_1, q, \epsilon) = v(a_2, w_2, q, \epsilon)$ for some $a_1 \leq a_2$, $w_1, w_2 \in \{0, ..., N\}$, $q \in (0, 1)$, $\epsilon > 0$, then $\frac{\partial p(a_2, w_2, q, \epsilon)}{\partial q} \Big/ \frac{\partial p(a_1, w_1, q, \epsilon)}{\partial q} < \frac{a_1 a_2 + a_2}{a_1 a_2 + a_1}$ for any $q \in (0, 1)$.

A.1 Results

Many of the proofs and propositions in this subsection are identical to results in the extension with monopoly rule. I detail the relevant changes here, and refer to the relevant results in Appendix B.3.

As in the extension under monopoly, Lemma 1.1 now must accommodate the fact that the set of optimal coalitions varies with ϵ. Its formulation and proof become as in lemma B8 in the extension under monopoly.

Proposition 1.1 is left unchanged. Its proof changes slightly.

Proof of proposition 1.1. The proof proceeds as the proof of proposition 1.1 in the main specification of the model (Appendix A), with the exception that equation A1 now becomes:

$$\nabla_x u_2 - u_1 = \left[\frac{\partial p_2}{\partial q}\left(\epsilon + \frac{1}{a_{c_2}}\right) - \frac{\partial p_1}{\partial q}\left(\epsilon + \frac{1}{a_{c_1}}\right)\right] x_q + (p_2 - p_1)x_\epsilon \tag{A2}$$

where $x = (x_\epsilon, x_q)$ is a unit-length vector. As in the main specification, I show that equation A2 has a finite number of solutions. This equation has an infinite number of solutions if and only if both coefficients on x_q and x_ϵ are zero.

I show that coefficient on x_q is non-zero. We have $\frac{\partial p_2}{\partial q}\left(\epsilon + \frac{1}{a_{c_2}}\right) - \frac{\partial p_1}{\partial q}\left(\epsilon + \frac{1}{a_{c_1}}\right) = 0 \iff \frac{\partial p_2}{\partial q} \Big/ \frac{\partial p_1}{\partial q} = \left(\epsilon + \frac{1}{a_{c_1}}\right) \Big/ \left(\epsilon + \frac{1}{a_{c_2}}\right)$. Define $h(\epsilon) \equiv \left(\epsilon + \frac{1}{a_{c_1}}\right) \Big/ \left(\epsilon + \frac{1}{a_{c_2}}\right)$. We have $h'(\epsilon) \propto$

$\frac{\epsilon}{a_{c_1}} - \frac{\epsilon}{a_{c_2}} \leq 0$, since $a_{c_1} \leq a_{c_2}$. Since $\epsilon \in (0,1)$, this implies $h(\epsilon) \leq h(1) = \frac{a_{c_1}a_{c_2}+a_{c_2}}{a_{c_1}a_{c_2}+a_{c_1}}$. Using assumption A1, $\frac{\partial p_2}{\partial q} - \frac{\partial p_1}{\partial q} < h(1) \leq h(\epsilon)$. As such, $\frac{\partial p_2}{\partial q}\left(\epsilon + \frac{1}{a_{c_2}}\right) - \frac{\partial p_1}{\partial q}\left(\epsilon + \frac{1}{a_{c_1}}\right) \neq 0$.

The rest of the proof proceeds as the proof of proposition 1.1 in the main specification of the model (Appendix A).

□

As in the extension under monopoly, propositions 1.3 and 1.2 change slightly to accommodate the fact that the set of optimal coalitions now varies with ϵ. Their formulation and proof become as in propositions B7 and B6 in the extension under monopoly.

Finally, propositions 1.4 and 1.5 and corollary 1.3.1 are left unchanged, and prove similarly to the main model.

B Additional computational results

This section reports power calculations for the models reported in Table 1 of the paper. For all models and all parameters except clustering, sample size is sufficiently large to pick up effects of magnitudes above 10^{-3}.

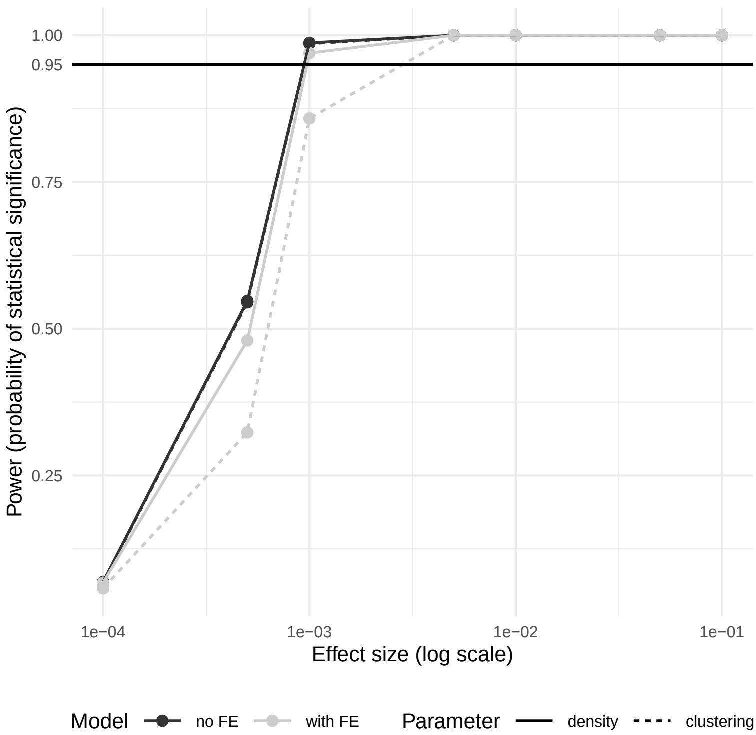

Figure B1: **Power calculations for models in Table 1 of the paper.** Power analysis conducted using 10,000 simulations per model/parameter-value, keeping the other parameter at the value observed in Table 1. Each simulation draws error terms for the models reported in Table 1, and considers a significance threshold of 5% with effect sizes reported on the x-axis. For all models and all parameters except clustering, sample size is sufficiently large to pick up effects of magnitudes above 10^{-3}.

C Additional experimental results

This section provides additional experimental results. I first give additional details about the experimental parameters and predicted equilibrium outcomes (section C.1). the protocol, and show that respondents displayed satisfactory levels of comprehension (section C.2), then report the models used to estimate the main results (section C.3). I report post-hoc power calculations, and show that the design was sufficiently well-powered (section C.4). I then test for pooling or learning effects, and find little evidence for either (section C.5). Accounting for group- and respondent-level effects, and comparing between employees and students, I also show that subject characteristics had little impact on the results (section C.6). Finally, I provide details about recruitment, prompts, and other experimental material (sections C.7 to C.10).

C.1 Equilibrium outcomes

I solve the game using backward induction in all treatment conditions, and for all three division rules. Note that the experiment rescaled the bribe to 12 credits, and the probability of success by a factor of .83. Figure C2 shows equilibra in the unscaled model. Graphical inspection shows that the comparative statics derived from propositions 1.2 to 1.5 hold in these particular cases, for all division rules. In line with proposition 1.2, as capacity increases, corruption decreases by selecting on grand corruption. In line with propositions 1.3 and 1.4, minimal coalitions are corrupt, and adding exposing ties decreases corruption. There is one difference in our three division rules: in the exposing tie treatment, removing the irrelevant tie decreases corruption under bargaining. This because is under bargaining, additional ties may help circumvent costly brokers (proposition 2.3). This, however, occurs outside the parameter range picked in the experiment.

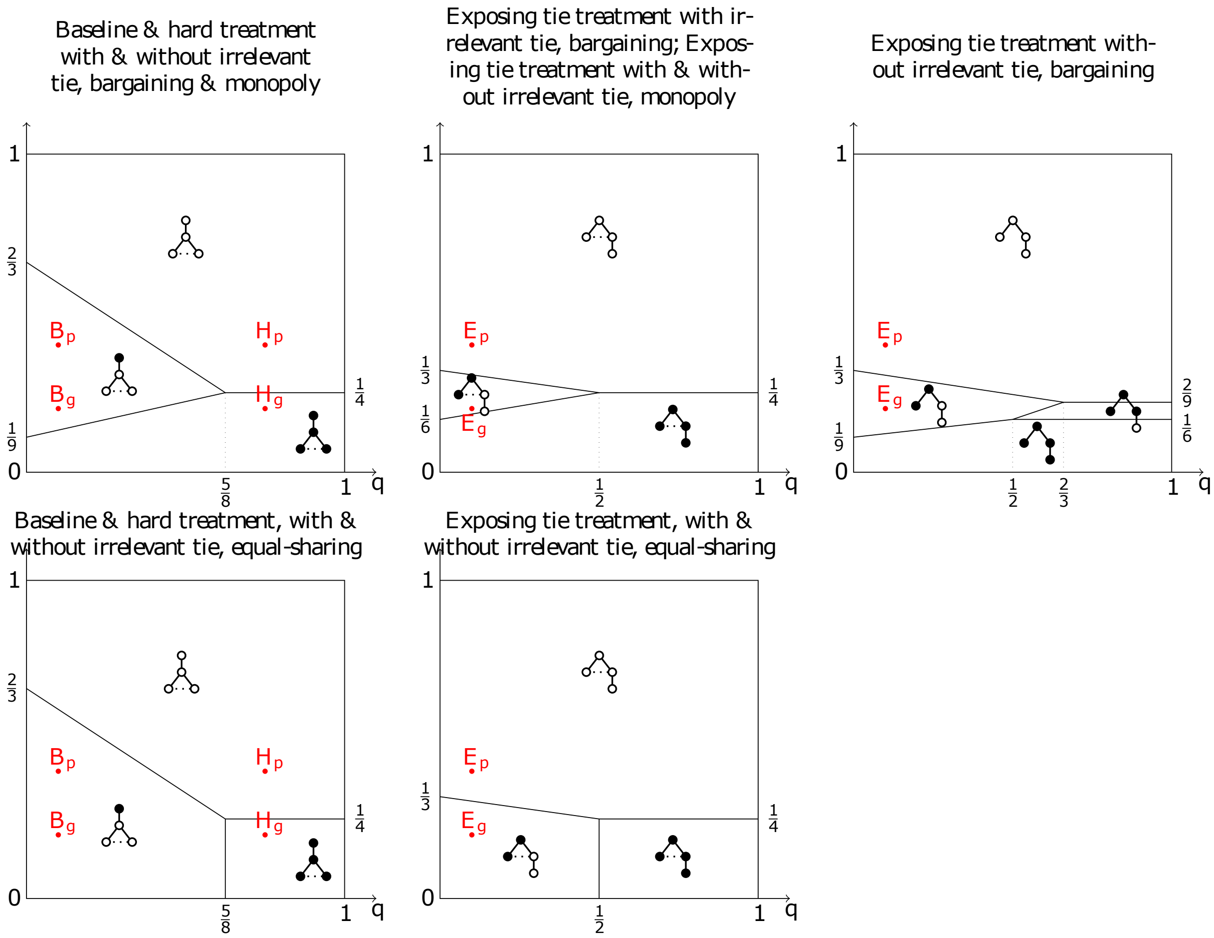

Figure C2: **Equilibrium outcomes in the unscaled model in all treatments.** The figures represent the equilibrium coalition in each region of the (ϵ, q) space. Points B, H, E correspond to the parameter values in the baseline, hard and exposing tie treatments respectively. The subscript indicates grand (g, i.e. $\epsilon = 2$) and petty corruption (p, i.e. $\epsilon = 4$).

C.2 Details about the protocol

I held 17 sessions of 16 respondents each in Mohammedia, Morocco, a mid-income country with median levels of corruption. This led to a sample of 272 subjects, and 808 games. I used a convenience sample that maximized sample size given existing budget constraints, and comprised of one quarter undergraduate students, and three quarters employees of the service industry (see next subsection for power analysis). Subjects were compensated, with the average payment amounting to daily minimum wage ($2.6 average gain and $5 show-up fee).

Figure C3 details the experimental protocol of a session. Before a session, I randomly decided whether it would be played with petty or grand corruption. Subjects entered the lab, and took a short pre-experiment survey. Subjects were then randomly assigned to groups of four. Each group had an enumerator that conducted the session. The enumerator read the instructions aloud, and conducted 12 repetitions of the game. The rent amounted to about $1 and was symbolized by 12 red cards. The cost ϵ, called the "salary" in the experiment, was represented by 2 or 4 blue cards held by the subjects. For each round, the network was drawn on a board that was placed on the table. The probability of success p associated to each coalition was communicated on a paper handout placed on the table.

In order to mimic the interpersonal interactions that arise in organizations, the game used face-to-face interactions. However, to implement take-it-or-leave-it, sequential offers, the enumerator would mediate communication around offers, asking subject i if she wished to offer

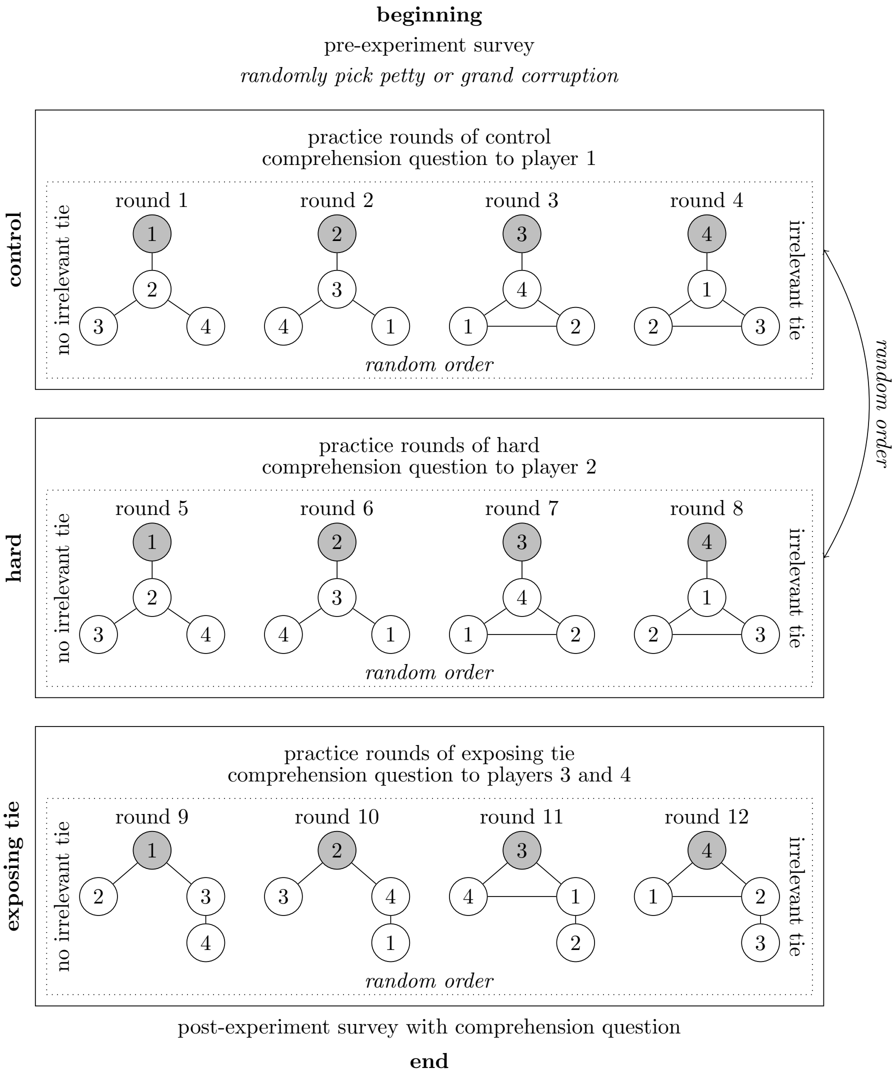

Figure C3: **Experimental protocol for group** $\{1,2,3,4\}$**.** The Figure reads from top to bottom. Grey nodes represent the seed. Each full rectangle represents a part of the experiment of 4 repetitions, and corresponds to a treatment. Within each block, the order of the 4 games was randomly permuted. The first two blocks were randomly permuted. Each block contains two treatments with the irrelevant tie, and two treatments without.

some amount to j, then asking j whether she accepted i's transfer, and if so, have j give up her salary. Cheap talk was otherwise allowed. The outcome was drawn by rolling a hundred-sided dice. To prevent framing effects from biasing the results, the experiment used a neutral framing.

The twelve repetitions were divided in three parts of four repetitions each, corresponding to the baseline and the two treatments. Within each block, each subject got to be the seed once, and to occupy each of the other network positions once according to the ordering in Figure C3. Within each block, two repetitions include the irrelevant tie, and two do not. The ordering was designed such that the same two subjects were always assigned to be the seed with the irrelevant tie, while the other two never did. After playing twelve repetitions, subjects took a post-experiment survey. They were paid their earnings, which amounted to about daily minimum wage ($2.6 average gain and $5 show-up fee).

Since subjects played the game several times, the design incorporates several features to test for potential learning and pooling effects – that is, whether subjects converge to or diverge from equilibrium predictions over time (learning), and whether they tie their behavior in a game to behavior in another game (pooling). Learning and pooling effects pose a tradeoff. On the one hand, the game is cognitively taxing, and playing it repeatedly gives room for convergence to some equilibrium (which may be different from the one predicted by the theory). On the other hand, repeating the game might bias the results by (1) incentivizing subjects to pool across games, and (2) getting subjects to learn other players' idiosyncratic strategies over time, making results diverge from the prediction in later repetitions. To discourage adverse learning and pooling effects, enumerators did not tell respondents how many repetitions of the game they would play, and did not allow them to keep track of their gains. In order to compare earlier and later games and test for potential learning and pooling effects, I randomized the order of the games within block, and randomly permuted the first two blocks (baseline and hard). I kept the exposing tie block last, because it was more cognitively demanding. I show in section C.5 that learning effects are insignificant and mixed, and that there are no pooling effects.

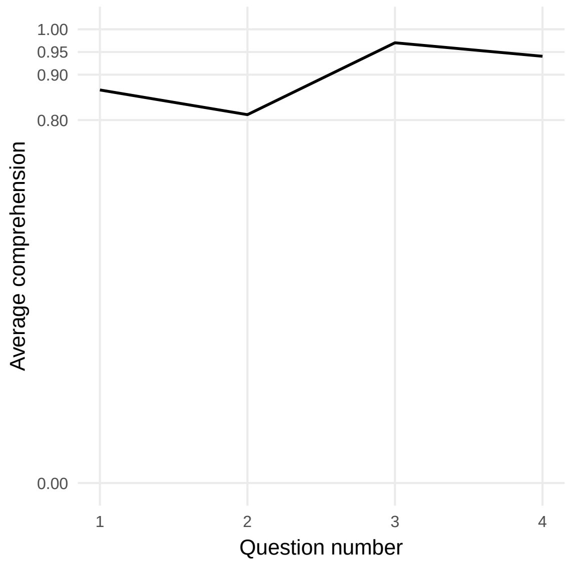

Figure C4: **Average comprehension over time.** Questions 1, 2, and 3 correspond to the questions asked before the beginning of the first, second and third blocks of the experiment respectively. Question 4 was asked in the post-experiment survey.

For comprehension, subjects played practice repetitions before each block until the enumerator was confident that at least two out of four understood the rules. In practice, the enumerator usually gave one to two practice repetitions, and never more than three. I measured understanding before each block and at the end of the experiment through comprehension quizzes. For all but the last question, the enumerator would first record the subject's answer, then correct her publicly so all could learn from her mistake. During a session, mean comprehension was above 80 percent, and reached 94 percent by the end of a session (Figure C4).

C.3 Main experimental results

	Dependent variable:			
	Baseline	Accept Irrelevant	GAM	N accomplices Size
	(1)	(2)	(3)	(4)
Hard, Grand	-0.089^{**}	-0.089^{**}		1.023^{***}
	(0.044)	(0.044)		(0.143)
Exposing, Grand	0.056^{**}	0.056^{**}		0.612^{***}
	(0.025)	(0.025)		(0.114)
Baseline, Petty	-0.081	-0.081		
	(0.064)	(0.064)		
Hard, Petty	-0.444^{***}	-0.444^{***}		
	(0.090)	(0.090)		
Exposing, Petty	-0.306^{***}	-0.306^{***}		
	(0.088)	(0.088)		
With irrelevant tie		-0.052^{**}		
		(0.022)		
History			-0.433^{***}	
			(0.133)	
Constant	0.906^{***}	0.932^{***}	2.888^{***}	1.563^{***}
	(0.024)	(0.023)	(0.397)	(0.101)
Observations	808	808	732	508
R^2	0.158	0.163		0.153
Log Likelihood			-324.154	
Note:			*p<0.1; **p<0.05; ***p<0.01	

Table C1: **Models used in the main text.** Clustered standard errors at the group-level in parentheses. Models 1, 2, 4 use use OLS, and all their variables are binary. Model 3 is a logistic generalized additive model (see footnote 22 in the main text for details about estimation). The variable history ranges from 2 to 4. Models 1 and 2 are used to construct Figure 7. Model 3 is used to construct Figure 11, panel b. Model 4 is used to construct Figure 8.

Treatment	p-value
baseline, grand	1.00
hard, grand	1.00
exposing, grand	1.00
baseline, petty	1.00
hard, petty	1.00
exposing, petty	1.00

Table C2: **Fisher exact tests for differences in the distribution of realized coalitions with and without the irrelevant tie.** The p-value column reports the p-value of these tests. Adding the irrelevant tie never significantly alters the distribution of realized coalitions.

C.4 Power analysis

This section conducts post-hoc power analysis to show that the design was sufficiently well-powered to guard against the risk of false discovery. The experiment relies on a fairly complex design, such that:

- Subjects are clustered in groups of 4 participants.
- Each group plays 12 iterations of the game, with 4 iterations of each main treatment (Baseline, Hard, Exposing tie).
- Within each main treatments, 2 conditions include the irrelevant tie, and 2 do not include it.
- Two thirds of groups play under grand corruption, and one third plays under petty corruption.

With these constraints, an experiment requires at least 3 groups, giving a minimum sample size $3 \times 4 = 12$ participants, and $3 \times 12 = 36$ games. Note that the design has the added difficulty that hypotheses on the size of the coalition can only be tested if the seed takes the rent. As such, I conduct power analyses using simulations, with sample sizes ranging from 36 groups (144 subjects) to 108 groups (432 subjects), and the conventional significance threshold of 5%. I evaluate the power underlying the main tests conducted in the paper (bottom panels of figures 7 and 8). Because sample size was determined so as to maximize the number of subjects within existing budget constraints, I did not conduct power analysis ex-ante. As a result, I report a post-hoc power analysis exercise and evaluate the statistical power associated with effects of the magnitude observed in the experiment. I report below the models used to conduct the main tests in the paper, and report in Table C3 the hypotheses that these models evaluate as well as the observed effect size.

$$\text{accept} = \beta_0 + \beta_1 \text{Hard, Grand} + \beta_2 \text{Exposing, Grand} + \beta_3 \text{Baseline, Petty} + \beta_4 \text{Hard, Petty} + \beta_5 \text{Exposing, Petty} + \epsilon \quad \text{(C3)}$$

$$N = \gamma_0 + \gamma_1 \text{Hard, Grand} + \gamma_2 \text{Exposing, Grand} + \epsilon \quad \text{(C4)}$$

$$\text{accept} = \delta_0 + \delta_1 \text{Hard, Grand} + \delta_2 \text{Exposing, Grand} + \delta_3 \text{Baseline, Petty} + \delta_4 \text{Hard, Petty} + \delta_5 \text{Exposing, Petty} + \delta_6 \text{With irrelevant tie} + \epsilon \quad \text{(C5)}$$

Hypothesis no.	Null hypothesis	Expected result	Observed effect size
H1	$\beta_1 = 0$	Fail to reject	-0.09
H2	$\beta_2 = 0$	Fail to reject	0.06
H3	$\beta_3 = 0$	Fail to reject	-0.08
H4	$\beta_4 - \beta_2 = 0$	Reject	-0.36
H5	$\beta_5 - \beta_3 = 0$	Reject	-0.36
H6	$\gamma_1 = 0$	Reject	1.02
H7	$\gamma_2 = 0$	Reject	0.61
H8	$\delta_6 = 0$	Fail to reject	-0.05

Table C3: **Hypotheses for power analysis.** Variables for null hypotheses are defined in equations C3, C4, and C5. The last column reports the effect size observed in the experiment.

Figure C5 shows that the design is sufficiently well-powered. Even the smallest sample size (144 subjects) is sufficient to pick up effects as large as the ones observed for the hypotheses where it is expected to reject the null. Conversely, the effect sizes observed for effects where it is expected to fail to reject the null are so small that even doubling the sample size would not allow detecting those effects at the conventional significance threshold of 5%.

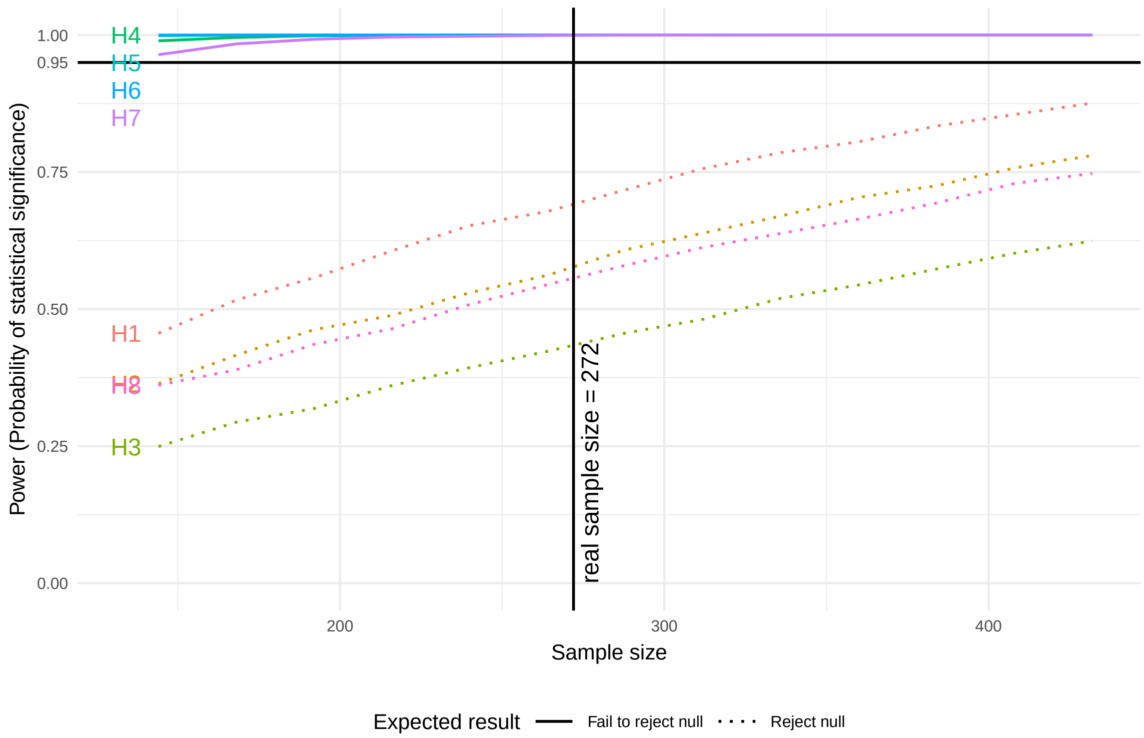

Figure C5: **Post-hoc power analysis.** Power analysis conducted using 10,000 simulations per sample size. Each simulation draws error terms for the models reported in equations C3 to C5, taking as many draws as required by the sample size under consideration, and considers a significance threshold of 5% with effect sizes matching the ones observed during the experiment. The design is sufficiently well-powered. Even the smallest sample sizes are sufficient to pick up effects of that magnitude for hypotheses where it is expected to reject the null. Conversely, even doubling the sample size would not allow detect effects as small as the ones observed for hypotheses where it is expected to fail to reject the null. See Table C3 for details about the hypotheses.

C.5 Learning and pooling effects

This section tests for potential learning and pooling effects. *Learning* effects refer to whether subjects converge to or diverge from equilibrium predictions over time, while *pooling* effects are the act of tying behavior in a game to behavior in another game. Learning and pooling effects are challenging because they pose a tradeoff. On the one hand, the game is cognitively taxing, and playing it repeatedly gives room for convergence to some equilibrium, which may be different from the one predicted by the theory. On the other hand, repeating the game might bias the results by (1) incentivizing subjects to pool across games, and (2) getting subjects to learn other players' idiosyncratic strategies over time, making results diverge from the prediction in later repetitions.

The experimental design detailed in section section C.2 incorporates several features to discourage adverse learning and pooling effects, and measure their magnitude. To minimize these effects, enumerators did not tell respondents how many repetitions of the game they would play, and did not allow them to keep track of their gains. To evaluate these effects, I randomized the order of the games. The experiment was divided into three parts of four repetitions each, corresponding to the main treatment conditions (baseline, hard, and exposing tie). I randomized the order of the games within part, and randomly permuted the first two parts (baseline and hard). I kept the exposing tie part last, because it was more cognitively

		did	
		share	not share
should	share	.07	*.00*
	not share	*.51*	.42

(a) Sender's decision, early rounds

		did	
		accept	reject
should	accept	.16	*.00*
	reject	*.55*	.29

(b) Recipient's decision, early rounds

		did	
		share	not share
should	share	.12	*.01*
	not share	*.56*	.31

(c) Sender's decision, late rounds

		did	
		accept	reject
should	accept	.24	*.01*
	reject	*.56*	.19

(d) Recipient's decision, late rounds

Table C4: **Learning effects for distribution of errors relative to the bargaining rule.** This table reproduces Table 4 separately for early and late rounds. Numbers denote observed frequencies. Subjects over-share and over-accept: errors (italicized cells) are overwhelmingly false positives. Trends are comparable in early and late round (Fisher exact tests not significant, table C6).

demanding.

Learning effects might go two ways. On the one hand, learning might have the expected effect: subjects may converge to the equilibrium strategy over time. Learning could also have an unexpected effect: subjects might learn about each others' types, and further diverge from equilibrium strategy. Suppose that a group contains a subject who never accepts the rent. Over time, other subjects may progressively learn about this and adjust their strategies accordingly, hence deviating from equilibrium strategy over time.

Comparing games that were played early and late in the first two blocks, I show that learning effects are insignificant and mixed. In early or late repetitions, results never vary significantly. Over time, some results converge to the equilibrium prediction, while others diverge. I use the first two blocks of the experiment to estimate variation in the effect of increasing monitoring over time between the early and the late block using a difference in difference strategy. I estimate similarly variation in the the effect of adding non-exposing ties. Figure C6 shows that both in early and late repetitions, results go in the expected direction. Effect size never varies significantly. The effect of monitoring on size gets marginally closer to the prediction, while its effect on coalition size gets further away from it. The effect of adding non-exposing ties is minuscule. Finally, I compare the distribution of errors, measured as deviations from predictions under bargaining (see section 3.2.2), in early and late blocks. Table C4 shows that errors follow a very similar distribution in early and late repetitions. I test for differences in the distribution of errors in offering and accepting behaviors between early and late repetitions within treatment using Fisher exact tests (Table C6). Both tests fail to reject the null that errors are distributed similarly in early and late treatments.

Pooling effects mean that participants may tie their behavior in one game to behavior in another game. This is problematic because the model analyzes a one shot game, and because pooling may explain why participants engaged in greedy bargaining, with offers that leave recipients with negative surplus, yet end up being accepted (Section 3.2.2). When pooling, subjects tacitly agree on reciprocal exploitation. Recipient i accepts to be exploited by offerer j in some round of the game because she knows that she will exploit j when she will get to be an offerer in a later round. Pooling effects should imply an end-game effect; that is differences in behavior in the very last repetitions of the game. Specifically, recipients in the last repetitions would be less inclined to accept greedy bargaining because there is no further opportunity to reciprocate.

Comparing the first two repetitions of the last part (exposing tie) to the last two repetitions of that part, I show no evidence for pooling effects. In particular, I look at the distribution of

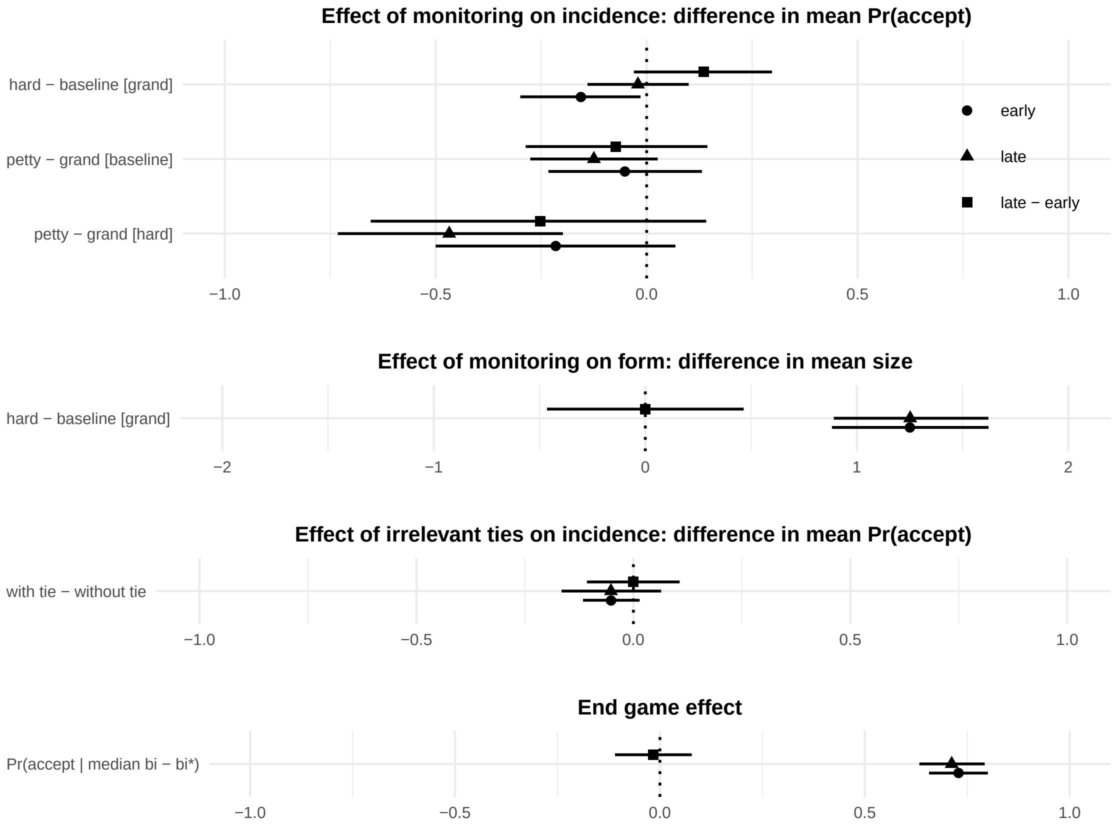

Figure C6: **Learning effects.** Bars are semi-parametric bootstrapped 95 percent confidence interval clustered at the group level using 10,000 replicates. The top three panel report estimates for early and late iterations of the first two blocks. The bottom panel reports estimates for early and late games within the last blocks. There is little evidence for learning and pooling effects: behavior never differ significantly between the late and early blocks. The models used to construct this Figure are reported in tables C7 and C8.

offers as deviation from predictions under bargaining. Table C5 shows that these distributions are very similar in early and late repetitions. I test for differences in the distribution of errors in offering and accepting behaviors between early and late repetitions within treatment using Fisher exact tests (Table C6). Both tests fail to reject the null that errors are distributed similarly in early and late treatments. Figure C6 also shows that facing an equally greedy offer (the median offer, which is greedy by about 1 credit), recipients are equally likely to accept that offer in early and in late repetitions.

		did	
		share	not share
should	share	.00	*.00*
	not share	*.54*	.46

(a) Sender's decision, early rounds

		did	
		accept	reject
should	accept	.30	*.04*
	reject	*.46*	.21

(b) Recipient's decision, early rounds

		did	
		share	not share
should	share	.00	*.00*
	not share	*.35*	.65

(c) Sender's decision, late rounds

		did	
		accept	reject
should	accept	.29	*.02*
	reject	*.45*	.24

(d) Recipient's decision, late rounds

Table C5: **Pooling effects for distribution of errors relative to the bargaining rule.** This table reproduces table 4 Numbers denote observed frequencies. Subjects over-share and over-accept: errors (italicized cells) are overwhelmingly false positives. Trends are comparable in early and late round (Fisher exact tests not significant, table C6).

Effect	Decision	p-value
Learning	Sender	1.00
Learning	Recipient	1.00
Pooling	Sender	1.00
Pooling	Recipient	1.00

Table C6: **Fisher exact tests for differences in the distribution of errors relative to the bargaining rule in early and late rounds.** The rows on learning effect compare the distributions reported in Table C4. The rows on pooling compare the distributions reported in Table C5. The distributions are never significantly different between early and late rounds.

C.6 Individual-level characteristics

This section shows that individual-level characteristics have little effect on the main results. I first show that group-, and individual-level heterogeneity have little influence, and that group-level heterogeneity is larger than individual-level heterogeneity. I re-estimate our quantities of interest, using linear mixed models with individual-level random effects, group-level effects, and both. Figure C7 shows that the quantities of interest are virtually unchanged. Table C.6 shows that random effect specifications fit the data marginally better than a specification without pooling, suggesting that there is little heterogeneity across groups, or across groups. Furthermore, individual-level effects add virtually no predictive power. This shows that individual-level effects are very small compared to group-level effects, and further justifies our decision to cluster errors at the group level.

Second, I show that although they have very different characteristics, students and employees have very similar behavior. Table 3 in the main paper showed that employees are poorer, less educated, more rural, less altruistic, and more extroverted than students. Yet, their behavior is very similar in the lab. I reestimate the quantities of interest separately for students and employees (Figure C8): the predictions for students are more noisy because of the smaller sample size, but they largely overlap with that of employees.

	Dependent variable:		
	Accept		N accomplices
	Baseline	Irrelevant	Size
	(1)	(2)	(3)
Hard, Grand	-0.156^{**}	-0.156^{**}	1.250^{***}
	(0.073)	(0.073)	(0.190)
Baseline, Petty	-0.052	-0.052	
	(0.093)	(0.093)	
Hard, Petty	-0.375^{***}	-0.375^{***}	
	(0.134)	(0.134)	
With irrelevant tie		-0.051	
		(0.034)	
Late	0.000	0.000	0.460^{**}
	(0.049)	(0.053)	(0.188)
Late × (Hard, Grand)	0.135	0.135	-0.455
	(0.106)	(0.106)	(0.334)
Late × (Baseline, Petty)	-0.073	-0.073	
	(0.122)	(0.122)	
Late × (Hard, Petty)	-0.115	-0.115	
	(0.179)	(0.180)	
Late × With irrelevant tie		-0.000	
		(0.045)	
Constant	0.906^{***}	0.932^{***}	1.333^{***}
	(0.033)	(0.032)	(0.091)
Observations	544	544	331
R^2	0.144	0.148	0.216
Note:		*p<0.1; **p<0.05; ***p<0.01	

Table C7: **Learning effects for main hypotheses.** Clustered standard errors at the group-level in parentheses. Analysis is subsetted to the first two blocks of the experiment. All models use OLS, and all variables are binary. Late is a binary variable equal to 1 if that block was played second in the experiment, and equal to 0 if it was played first. Most learning effects are not statistically different from zero. These models are used to construct Figure C6.

	Dependent variable:
	Accept
$b_i - b_i^*$	0.053^{***}
	(0.015)
Late	0.017
	(0.046)
$(b_i - b_i^*)\times$ Late	0.017
	(0.018)
Constant	0.834^{***}
	(0.028)
Observations	339
R^2	0.134
Note:	*p<0.1; **p<0.05; ***p<0.01

Table C8: **Pooling effects for main hypotheses.** Clustered standard errors at the group-level in parentheses. Analysis is subsetted to the last block of the experiment. The model uses OLS. Late is a dummy variable equal to 1 for the last two games, and 0 otherwise. There is no pooling effect: the effect of deviations from the equilibrium offer does not vary between early and late games. This model is used to construct Figure C6.

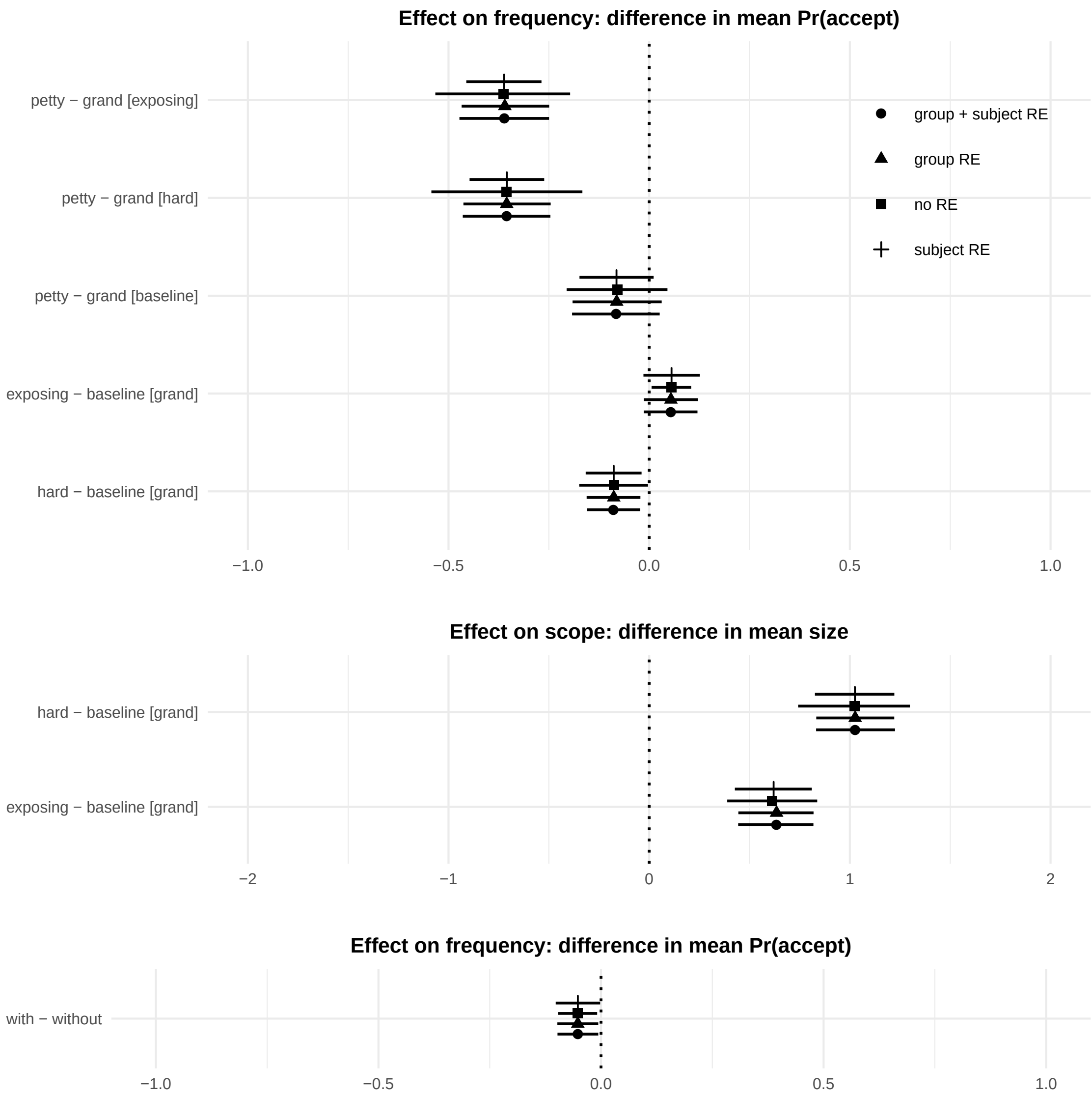

Figure C7: **Random effect specifications.** The specifications without random effects is estimated using a Gaussian GLM with errors clustered at the group-level; RE specifications use linear mixed models. Bars are semi-parametric bootstrapped 95 percent confidence interval clustered at the group level using 10,000 replicates. The main quantities of interest are robust to adding random effects. The models used to construct this Figure are reported in Table C.6

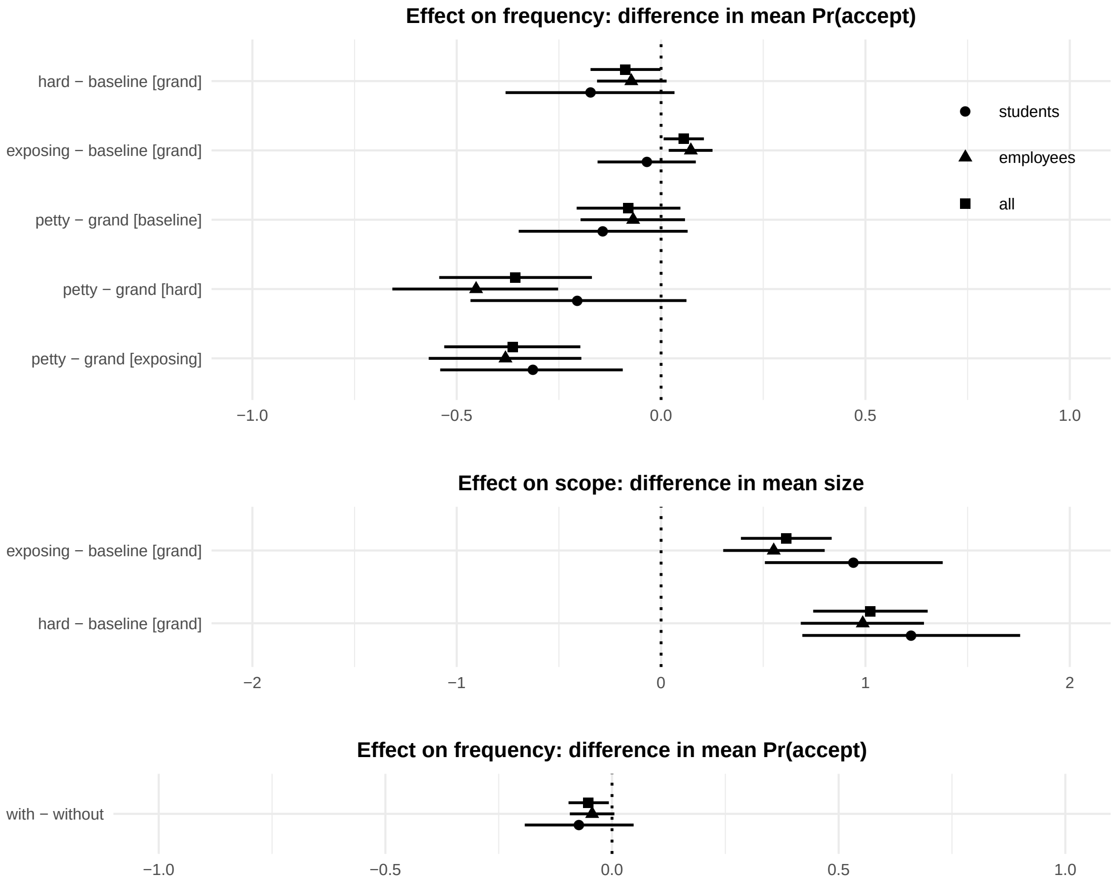

Figure C8: **Students vs. employees.** "All" reports the estimates from the main specification (Table C1). The specifications for students and employees are estimated using OLS with bootstrap errors clustered at the group-level. Bars are semi-parametric bootstrapped 95 percent confidence interval clustered at the group level using 10,000 replicates. Students and employees have similar behavior. The models used to construct this Figure are reported in Table C10.

	Dependent variable:											
	Accept								N accomplices			
	Baseline	Baseline	Baseline	Baseline	Irrelevant	Irrelevant	Irrelevant	Irrelevant	Size	Size	Size	Size
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Hard, Grand	-0.089^{**}	-0.089^{**}	-0.089^{***}	-0.089^{***}	-0.089^{**}	-0.089^{**}	-0.089^{***}	-0.089^{***}	1.023^{***}	1.026^{***}	1.026^{***}	1.026^{***}
	(0.044)	(0.036)	(0.034)	(0.034)	(0.044)	(0.036)	(0.034)	(0.034)	(0.143)	(0.101)	(0.099)	(0.099)
Exposing, Grand	0.056^{**}	0.056	0.055	0.055	0.056^{**}	0.056	0.055	0.055	0.612^{***}	0.620^{***}	0.634^{***}	0.634^{***}
	(0.025)	(0.036)	(0.035)	(0.035)	(0.025)	(0.036)	(0.035)	(0.035)	(0.114)	(0.098)	(0.096)	(0.096)
Baseline, Petty	-0.081	-0.081^{*}	-0.081	-0.081	-0.081	-0.081^{*}	-0.081	-0.081				
	(0.064)	(0.048)	(0.056)	(0.056)	(0.064)	(0.047)	(0.056)	(0.056)				
Hard, Petty	-0.444^{***}	-0.444^{***}	-0.444^{***}	-0.444^{***}	-0.444^{***}	-0.444^{***}	-0.444^{***}	-0.444^{***}				
	(0.090)	(0.048)	(0.056)	(0.056)	(0.090)	(0.047)	(0.056)	(0.056)				
Exposing, Petty	-0.306^{***}	-0.306^{***}	-0.306^{***}	-0.306^{***}	-0.306^{***}	-0.306^{***}	-0.306^{***}	-0.306^{***}				
	(0.088)	(0.048)	(0.056)	(0.056)	(0.088)	(0.047)	(0.056)	(0.056)				
With irrelevant tie					-0.052^{**}	-0.052^{**}	-0.052^{**}	-0.052^{**}				
					(0.022)	(0.026)	(0.023)	(0.023)				
Constant	0.906^{***}	0.906^{***}	0.906^{***}	0.906^{***}	0.932^{***}	0.932^{***}	0.932^{***}	0.932^{***}	1.563^{***}	1.554^{***}	1.546^{***}	1.546^{***}
	(0.024)	(0.026)	(0.030)	(0.030)	(0.023)	(0.029)	(0.032)	(0.032)	(0.101)	(0.074)	(0.090)	(0.090)
Indiv. RE		✓		✓		✓		✓		✓		✓
Group RE			✓	✓			✓	✓			✓	✓
Observations	808	808	808	808	808	808	808	808	508	508	508	508
Akaike Inf. Crit.	637.177	668.463	630.348	632.348	634.855	671.901	633.124	635.124	1,427.636	1,432.239	1,401.788	1,403.788
Note:												*p<0.1; **p<0.05; ***p<0.01

Table C9: **Random effect specifications.** Clustered standard errors at the group-level in parentheses. The specifications without random effects is estimated using a Gaussian GLM with errors clustered at the group-level; RE specifications use linear mixed models. All variables are binary. Models have vritually identical point estimates. Random effects have little impact on model fit (AIC), but group effects reduce it more than individual effects. These models are used to construct Figure C7.

	Dependent variable:					
	Accept				N accomplices	
	Baseline	Baseline	Irrelevant	Irrelevant	Size	Size
	(1)	(2)	(3)	(4)	(5)	(6)
Hard, Grand	-0.172	-0.074^{*}	-0.172	-0.074^{*}	1.224^{***}	0.986^{***}
	(0.107)	(0.043)	(0.107)	(0.043)	(0.274)	(0.154)
Exposing, Grand	-0.034	0.072^{***}	-0.034	0.072^{***}	0.942^{***}	0.551^{***}
	(0.061)	(0.027)	(0.061)	(0.027)	(0.223)	(0.125)
Baseline, Petty	-0.142	-0.070	-0.148	-0.068		
	(0.105)	(0.065)	(0.106)	(0.065)		
Hard, Petty	-0.377^{***}	-0.526^{***}	-0.383^{***}	-0.525^{***}		
	(0.103)	(0.097)	(0.104)	(0.098)		
Exposing, Petty	-0.348^{***}	-0.309^{***}	-0.354^{***}	-0.307^{***}		
	(0.110)	(0.099)	(0.111)	(0.099)		
With irrelevant tie			-0.075	-0.044^{*}		
			(0.061)	(0.025)		
Constant	0.966^{***}	0.896^{***}	1.007^{***}	0.917^{***}	1.429^{***}	1.589^{***}
	(0.034)	(0.029)	(0.054)	(0.026)	(0.133)	(0.115)
Sample	Students	Employees	Students	Employees		
Observations	189	619	189	619	78	430
R^2	0.119	0.182	0.127	0.185	0.251	0.140
Note:						$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table C10: **Students vs. employees.** Standard errors are in parentheses, and errors are clustered at the group level. All models use OLS, and all variables are binary. Effects for students and employees are comparable. These models are used to construct Figure C8.

C.7 Location

The experiment was held in Mohammedia, Morocco from September 9-21, 2015. Working with our local partner, Mhammed Abderebbi of "MEDA Solutions" firm, we rented an apartment in Mohammedia appropriate for our lab. The apartment featured a large salon that we converted into a waiting room, and two bedrooms that we converted into a survey room and an experiment room. The survey room contained a bed, a couch, and a table, thus allowing three surveys to take place simultaneously with relative privacy. The experiment room contained two circular tables, each with five chairs for the enumerator and four subjects to play the game.

Due to unforeseen security threats (local youth demanding to participate in the experiment), we temporarily relocated sessions on September 16, 17, and 21 to our partner's office, which similarly contained a waiting, survey, and experiment room.

C.8 Enumerators

Our partner selected two male and two female enumerators, three of them students from the Hassan II University in Mohammedia and one from our partner's company. Having uploaded our pre-experiment survey, experiment survey, and post-experiment survey to Qualtrics, we trained our enumerators to administer the Qualtrics surveys on handheld tablets. We trained all four enumerators to administer the pre- and post-experiment surveys, and trained three of them (one male, and two females) to administer the experiment as well. Enumerators received 200 dirhams per day.

Training was held on Tuesday, September 8, 2015 and lasted half a day. It consisted in having the enumerators administer the pre- and post-experiment surveys to each other, under the author's supervision. Similarly, they administered the diffusion game to each other, under the author's supervision.

C.9 Subjects

Recruiters solicited subjects from public squares in Mohammedia, presenting them with flyers with the address, time, and following description:

Invitation to participate in a study session

The company MEDA Solutions has the honor of inviting you to a study session that will last about an hour. The topic is one's financial behavior.

Day: XXX

Time: XXX

Address: Lotissement de la gare. Villa Mounia, no. 82. El Alia, Mohammedia

Phone number: 0668775219

Note: this invitation is personal and cannot be transferred to anyone else. You will not be allowed to participate without this invitation.

In recruiting subjects, we explicitly blocked on occupation, asking recruiters to recruit employees of the service industry, and, if necessary, completing with university students. Recruiters were told to select a diverse range of ages and occupations. Recruiters mentioned that all participants would receive 50 dirhams for their time plus any gains they won in the behavioral game.

C.10 Prompts and material

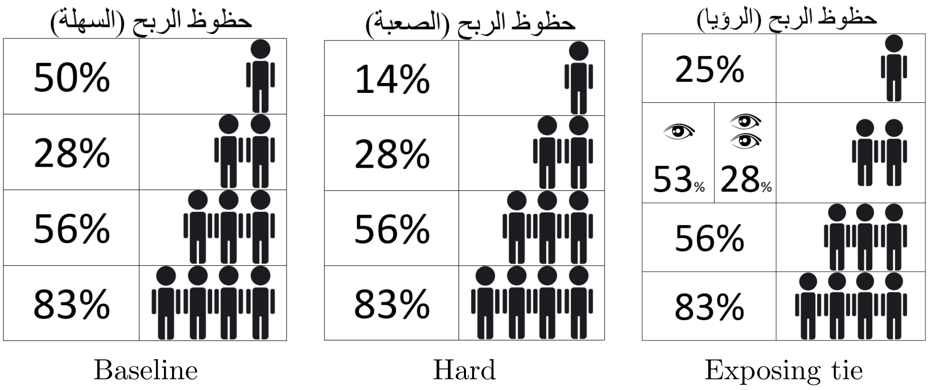

Table C11: **Document displaying the probability of success in each treatment condition, for coalitions of 1 to 4 accomplices.** In the exposing tie condition, the one-eyed cell denotes the coalition including the seed and the more isolated node, while the two-eyed cell denotes the coalition including the seed and the more exposed node.

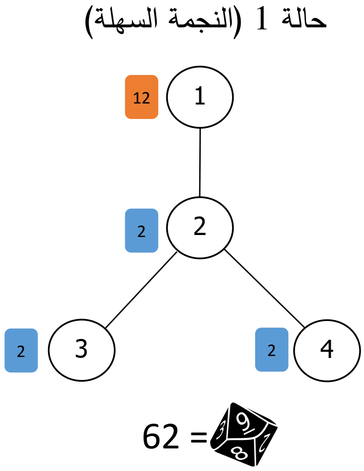

Figure C9: **Example comprehension question under grand corruption.** Red and blue rectangles correspond to the rent and salaries, respectively. Number 62 represents the outcome of the die. The question asked was: “How much as player 1 won?” [Answer: 0]

Prompt of the first block (control or hard)

You are about to participate in an experiment on behavior in uncertain situations. The experiment looks like a game in which you will have to make several decisions that may make you win money. We will count the money in credits. One credit is worth a bit less than one dirham.

The experiment is very short. We will repeat it several times. Sometimes, we will change a few details. It is very important that you remain silent during the experiment. You will be able to talk only when I will allow you.

During the experiment, each of you will have a salary of **2** [4] credits, represented by the **2** [4] blue cards. You will have to decide between winning your salary with certainty, or taking a risk to maybe win a higher amount. You will be assigned to positions on a network [draw the star network on the board]. If two people are connected, they are "neighbors," which allows them to communicate.

I will pick one of you and offer him 12 credits, represented by the 12 red cards. This person will have to decide between taking this sum and giving up her salary, or refusing this sum and keeping her salary. If he refuses it, the experiment is over, and you will all win your salary. If he takes it, I will offer him to share this amount with her neighbors. He will announce how much he wishes to offer to each. I will then allow the neighbors to accept or refuse. If they refuse, they keep their salary. If they accept, they give up their salary. The neighbors that have accepted will then be able to share the amount they have at hand with their neighbors that do not have pending offers and have not given up their salary. The experiment is over when no further offer can be made.

In the end, the ones that have held on to their salary win it. The ones that have given up their salary form a team. I will throw a dice. If the score is below some threshold, team members win their credits. Otherwise, they lose them. The threshold is written on this document [show the document]. It depends on the amount of team members.

Prompt of the second block (hard or control)

I will now change the probabilities of victory a bit. Note that now, it is **more difficult** [**easier**] for a player on his own to win.

Prompt of the third block (exposing tie)

I will now change the network you are playing on [draw the line network on the board]. I will also change the probabilities of victory. They now not only depend on the amount of people in the team, but also on the about of neighbors of the team that have held on to their salaries. Now, sharing with the left hand side player only is better than sharing with the right hand side player only because the latter has one extra neighbor.

D Additional supportive evidence: a comparison of corruption cases in the US and India

This section presents preliminary evidence supporting an implication of proposition 1.2: as monitoring improves, corruption has a broader scope. Using cross-country comparisons and a comparison of 110 cases of corruption in India and the US. I show that controlling for the scale of corruption, instances of corruption in the US involve more accomplices than in India.

I collected data on corruption cases in the US and India by searching for the words "arrest" and "corruption," "fraud," "bribery," "embezzlement," or "graft" (as well as their variants, such as "arrested" or "corrupt") in the National Desk of the New York Times (NYT) and the Times of India (TOI) using Factiva. I then went through each article to identify the ones actually covering corruption cases. For each selected article, I collected the amount stolen and the number of accomplices. While the latter measures the scope of corruption, I normalize the amount stolen by Gross National Income (GNI) per capita to obtain a measure of the scale of corruption indicating its profitability relative to average income. In the NYT data, I covered the 2000-2014 time period and ended up with 55 cases. For TOI, I started at December 31, 2014 and stopped collecting data when I obtained a sample of the same size.

I compare the US and India because the former has stronger institutions than the latter. I picked the NYT and the TOI because they are both major national dailies in two large democracies with a vivid free press. This lends confidence that both newspapers will cover corruption cases to a similar extent. I ran the above query because using a large vocabulary for corruption would select many articles while looking for the word "arrest" would select the first article on the case to appear in the newspaper, which would usually be the most detailed.

Using newspaper data on corruption is not uncommon (see, for instance, Glaeser and Goldin 2008). This data has several pitfalls. Most importantly, different newspapers may select differently on the types of cases they cover. The fact that both newspapers are national, generalist dailies should alleviate this concern. Furthermore, corruption being more widespread in India than in the US should push the TOI to select against petty corruption, which would be less interesting to its readers. As such, selection would only dampen the finding that petty corruption is more prevalent in India. In any case, the stylized facts below should only be taken as tentative evidence.

Table D12 provides a few descriptive statistics, and table D13 shows the finding: controlling for scale, corruption has a broader scope in the US than in India.

	India	USA
Median amount stolen, fraction GNI p/c	0.31	1.7
Mean N accomplices	3.02	10.79
Percent cases with strong ties	0.192	0.209
First case	2014-11-04	2000-03-18
Last case	2014-12-31	2014-10-21
N	55	55

Table D12: Descriptive statistics on corruption in India and the US.

	Dependent variable:	
	N accomplices	
	Poisson	*negative binomial*
	(1)	(2)
log(amount)	0.197***	0.237***
	(0.031)	(0.051)
USA	1.102***	1.182***
	(0.199)	(0.304)
Constant	−0.380**	−0.488**
	(0.176)	(0.234)
Observations	71	71
Akaike Inf. Crit.	253.305	216.467
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table D13: **Count regressions for number of accomplices.** Amount is measured as a fraction of GNI p/c. Controlling for the amount stolen, corruption in the US involves more accomplices than in India.

References

Glaeser, Edward L and Claudia Goldin. 2008. Corruption and Reform: Introduction. In *Corruption and Reform: Lessons from America's Economic History*, ed. Edward L Glaeser and Claudia Goldin. Chicago, IL.: University Of Chicago Press pp. 3–22.