How robust is institutionalized corruption?
A field experiment on extortion along West African highways*

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Abstract

Are dramatic policy interventions necessary to disrupt forms of corruption sustained through mutually reinforcing expectations, or can small shocks to uncertainty destabilize corrupt equilibria? I attempt to answer this question by randomly introducing the presence of a highly unexpected foreigner into interactions in which officials typically extort citizens for bribes according to a stable going rate. Specifically, I rode with truck drivers along 1,500km of highways in Burkina Faso, Ghana, Togo and Benin. At some of the 123 checkpoints manned by customs, police and gendarmerie, I openly observed attempts to extort bribes from drivers, at others I hid from sight. In contradiction to the expectations of well-informed experts, I find statistically significant evidence against large treatment effects. The findings suggest established patterns of corrupt behavior can be resilient to small shocks to uncertainty, and that experts may overestimate the effectiveness of novel anti-corruption measures.

Introduction

In many contexts in which bureaucrats extort bribes, payers and demanders of bribes act in highly regularized ways (Fisman and Miguel, 2007). One example of such behavior is the maintenance of a "going rate", whereby bribes for a given good or service are paid according to a pre-determined price schedule that is common knowledge (Olken and Barron, 2009; Svensson, 2003). Going rates constitute corrupt equilibria, sustained through mutually-reinforcing beliefs held by the parties. Ryvkin and Serra (2012) show theoretically that uncertainty about a partner's corruptibility can undermine the maintenance of such equilibria. Similarly, Herrera, Lejane, and Rodriguez (2007) provide observational evidence that bribe frequency decreases when firms are more uncertain about the "going rate" and have weaker expectations that the bribe will ensure delivery of the good or service. Could small shocks that increase uncertainty about the benefits and costs of bribery therefore disrupt corrupt equilibria?

In this paper I try to answer this question by randomly introducing the highly unexpected presence of a foreigner into a bribery setting characterized by very regulated patterns of behavior: the extortion of truck drivers by customs and police officials at highway checkpoints. In July of 2014, I traveled with cargo-truck drivers on over 1,500 km of highway in Ghana, Togo, Benin and Burkina Faso, and randomly assigned some of the 123 checkpoints we encountered to be openly observed and monitored by me. At the others I hid out of sight in the sleeping compartment. Contrary to the predictions of a well-informed group of local experts surveyed prior to the intervention, I find evidence that the presence of a foreigner in the bribe setting did not disrupt typical patterns of behavior. I find no evidence for effect heterogeneity, and can reject with 95% confidence any positive or negative average treatment effect greater than 10 cents on a one dollar bribe. The findings suggest that certain corrupt equilibria can be quite resilient in the face of uncertainty, and that this fact

may be overlooked by experts who overestimate the effectiveness of novel anti-corruption interventions.

1 Corrupt Equilibria and Foreigner Presence

In July 2014, a truck driver picks up a container of goods at the port of Tema, Ghana. To bring the goods to the capital of Burkina Faso, he travels two days along the sole highway that runs from the south to the north of Ghana. En route, he bribes police officers at roughly 50 different checkpoints, often comprising little more than a patrol car and a makeshift roadblock. At one such checkpoint the driver has encountered many times, a police officer waits. The officer has already stopped 30 trucks that day, carefully avoiding the tankers that ship oil for the state-owned petroleum company. The officer forms expectations about the bribe he will receive based on three features the driver reveals as he approaches: where the truck is registered (foreigners pay more), what it is carrying, and its direction of travel (imports go north, exports go south). The driver has already paid several bribes in the morning and therefore knows the going rate. When the officer demands a 2 USD fee for some invented infraction, the driver withdraws it quickly from the allowance provided by the transport company, pays, and proceeds immediately on his way.

Each time a bureaucrat extorts a truck driver, he must make a decision: maintain the status quo by extorting at the going rate, or depart from it by either extorting nothing or by bargaining with the driver to extract more. The stable patterns of price discrimination to which these highly routinized micro interactions give rise suggest that the going rate is a popular option (Bromley and Foltz, 2011; Arifari, 2006). Given the time-sensitive nature of logistics and the bureaucrat's incentive to speed up throughput when bribes are paid in a queue (Lui, 1985), a going rate may provide an important shortcut that minimizes transaction costs for both parties. Olken and Barron (2009) observe similar patterns in bribes extorted from truck drivers in Indonesia, as does Svensson (2003) in bribes paid by a cross section of

firms in Uganda. Such tarification systems constitute endogenous institutions (Greif, 2006; Greif and Kingston, 2011; Aoki, 2007): they are equilibria that emerge at the macro level through a process of "accumulation by precedent" at the micro level (Young, 1996). Do such equilibria constitute corruption traps that only a "big push" can disrupt (Abbink and Serra, 2012; Klasnja, Little, and Tucker, 2016)?

In this study, I attempt to manipulate the bureaucrat's uncertainty about the payoffs from bribes by randomly introducing highly unexpected[1] foreigner presence into a subset of driver-official interactions. Foreigner presence could conceivably alter payoff expectations in two ways. First, it might increase the anticipation of being detected and punished. Second, foreigner presence may signal a larger pie, since foreigners typically possess relatively more wealth. The bureaucrat cannot be certain about what foreigner presence implies in terms of the consequences of deciding to maintain or depart from the going rate.

How this uncertainty will impact behavior depends on the bureaucrat's decision-making procedure. Many studies rely on expected utility theory with high certainty to model extortionary decision-making (Becker and Stigler, 1974; Rose-Ackerman, 1975; Shleifer and Vishny, 1993; Olken and Pande, 2011): as the expected penalty and probability of being caught increase, the bureaucrat extorts a lower bribe. Models that incorporate uncertainty predict a negative relationship with bribe-taking (Ryvkin and Serra, 2012; Herrera, Lejane, and Rodriguez, 2007). However behavioral evidence often does not cohere with the predictions of expected utility theory about decision-making under uncertainty (Kahneman and Tversky, 1979). In an influential and oft-replicated study, Samuelson and Zeckhauser (1988) find that individuals have a strong bias towards preserving the current state of affairs when faced with decisions whose outcomes are uncertain.

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[1]It was clear from the reactions of those in the truck yards at border towns and in villages that my presence in the trucks was anomalous. Passengers in passing cars would sometimes point to me as they passed by. It is not the case, for example, that tourists and backpackers frequently travel with the drivers of cargo trucks.

Prior to the experiment, I conducted a survey to elicit expectations about treatment effects from a sample of 20 well-informed experts and participant informants who work in the trucking sector in West Africa. Respondents included the owners of logistics companies, former truck drivers, former checkpoint operators, and field reporters who frequently record such bribes in the field using a similar methodology (riding with drivers). Figure 1 contains the question wording and distribution of responses to two questions in the survey. Experts strongly believed that the treatment would reduce the amount paid by the driver, consistent with an expected utility notion of the bureaucrat's decision-making process. Expert opinion was more divided on the question of delay times, with some experts expecting longer and others expecting shorter waits at checkpoints.
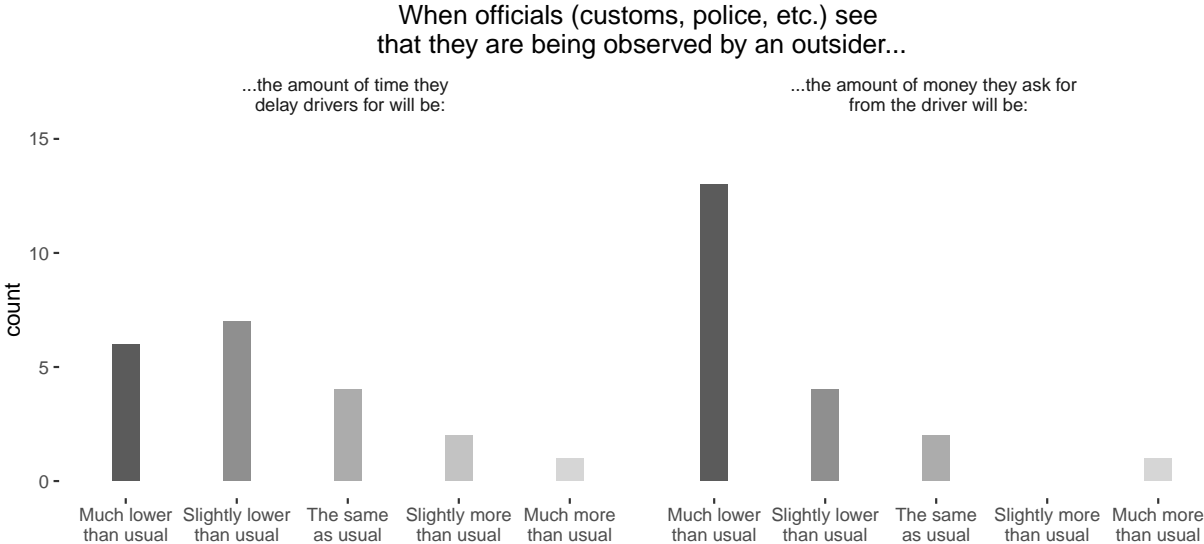

Figure 1: Expectations about treatment effects among experts and participant informants in the West African logistics sector (N = 20).

2 Experimental Design

To test the effects of uncertainty on corrupt equilibria, I conducted a field experiment in participation with truck drivers, in which I observed the extortionary behavior of officials at some checkpoints and not at others along highways in Ghana, Burkina Faso, Togo and

Benin. The experiment took place over a two-week period during July of 2014, and covered over 1,500km of road.

This study comprises an 'ethnographic' field experiment insofar as I collect data by participating in the social process I seek to understand. There are three treatment arms: *control*, in which I remain out of sight from the official behind the driver's seat; *NGO observation*, in which I observe officials while wearing the t-shirt of Borderless Alliance, a well-recognized anti-corruption group that monitors West African highways; and *neutral observation*, in which I observe officials while wearing a neutral t-shirt. The first trip did not include the NGO treatment: I randomized only control and neutral observation using a schedule of treatments created using a binomial random number generator in R. The remaining trips randomized the three conditions in blocks of four, such that every four checkpoints would have exactly two units assigned to control, one to neutral and one to NGO observation. This method of block randomization avoids long stretches of checkpoints assigned to control, which can create imbalances. Because assignment probabilities differ between the first and subsequent trips, all analyses weight observations by the inverse of the probability they are assigned to the observed treatment condition, following the method proposed in Gerber and Green (2012).

I do not condition outcome measurement on being stopped at a checkpoint, as this could induce post-treatment bias if the probability of being stopped is also a function of treatment. Thus, the data records every checkpoint the truck passes through, and measures a bribe and a delay of 0 if the truck is not stopped at all. A small number of checkpoints appear to have exhibited non-compliance, insofar as the official does not appear to have noticed me. As I do not have an objective measure of compliance, I calculate intent-to-treat effects only.

3 Estimation Strategy

As outlined in section 1, experts expected the treatment would reduce the amount of money that officials extort, whereas I give theoretical reasons to expect that they may not depart from the status quo at all, or may even extort more. Three kinds of estimands are particularly informative in adjudicating between these predictions.

First, the intent-to-treat effect on the average bribe paid conveys any systematic response by officials to monitoring. Let $Z \in \{0, 1, 2\}$ denote a random treatment variable where 0 is control, 1 is 'neutral' foreigner observation (no NGO t-shirt), and 2 is foreigner observation with the 'NGO' prime. The intent-to-treat estimands on mean bribes are

$$\tau_{\text{any}} = E[Y_i \mid Z > 0] - E[Y_i \mid Z = 0] \tag{1}$$

$$\tau_{\text{neutral}} = E[Y_i \mid Z = 1] - E[Y_i \mid Z = 0] \tag{2}$$

$$\tau_{\text{NGO}} = E[Y_i \mid Z = 2] - E[Y_i \mid Z = 0], \tag{3}$$

where Y_i denotes the potential outcomes of the official-driver interaction i. Second, if the treatment causes officials to depart from established norms of interaction, it may also systematically change the time spent in negotiations. Formally, I define the intent-to-treat effects on minutes of delay identically to those defined in equations 1 - 3. The third kind of informative estimand concerns the difference in variances between potential outcomes: if some officials extort more in response to treatment, while others extort less, then although there is a departure from the status quo of the going rate, the heterogeneous effects may cancel out. Since the hypothesis here specifically relates to an increase in variance, we can investigate the ratio in variances

$$\tau_{\sigma} = \frac{E[(Y_i - E[Y_i \mid Z > 0])^2 \mid Z > 0]}{E[(Y_i - E[Y_i \mid Z = 0])^2 \mid Z = 0]}, \tag{4}$$

where we expect $\tau_\sigma > 1$ if the treatment promotes heterogeneous departures from the status quo.

I make causal inferences using three kinds of Fisher randomization tests that rely on the known variation mechanism Z (Rosenbaum, 2002).[2] First, I estimate the intent-to-treat effects in equations 1 - 3 conditional on pre-treatment covariates using a least-squares regression of the form

$$y_i = \alpha + \hat{\tau} z_i + \mathbf{X}_i^\top \boldsymbol{\beta} + \epsilon_i, \tag{5}$$

where $z_i \in \{1, 0\}$ is an indicator for the treatment assignment (NGO, neutral, or any observation), and $\mathbf{X}_i$ is a vector of covariates including whether the checkpoint is manned by police, whether it is a mandated (official) checkpoint, its latitude and longitude, as well as indicators for trip fixed effects. Observations are weighted by the inverse of the probability of being assigned to the observed condition. I compute a randomization p-value for $\hat{\tau}$ under the sharp null of no effects for all units. Second, I test for the equality of variances by computing a p-value for the sharp null of no effect on the ratio of variances in treatment and control.

Finally, I compute confidence intervals for the ITT estimates in equations 1 - 3 using inverted hypothesis tests over a K-length vector $\boldsymbol{\theta}$ of hypothesized effects. I firstly obtain the predicted values $\hat{\mathbf{y}}$ from a restricted version of equation 5 that excludes the term $\hat{\tau} z_i$, so that the residuals are equal to sample variance and the treatment effect, $\mathbf{r} = \mathbf{y} - \hat{\mathbf{y}} = \tau \mathbf{z} + \boldsymbol{\epsilon}$. I then follow the method proposed in Bowers, Fredrickson, and Panagopoulos (2013), and construct the 'uniformity trial' under some θ_k using $\mathbf{r}_{0k} = \mathbf{r} - \theta_k \mathbf{z}$. Thus, if the true effect is constant and additive, there is some $\theta^* = \tau$ whereby $\mathbf{r}_0^* = \boldsymbol{\epsilon}$. Any two random draws from $\mathbf{r}_0^*$ will thus be random draws from the same distribution, $\boldsymbol{\epsilon}$. We can formally test the

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[2]A working paper gives this study a Bayesian treatment, with integrated models to incorporate expert expectations into the estimation strategy. These analyses are not reported here.

hypothesis that $\theta_k = \theta^*$ using the Kolmogorov-Smirnov test-statistic, which provides a non-parametric measure for the equality of two distributions. Specifically, I compute a p-value corresponding to each element of $\boldsymbol{\theta}$,

$$\phi_k = \Pr(\mathcal{KS}(\mathbf{r}_{0k}, \mathbf{Z}) > \mathcal{KS}(\mathbf{r}_{0k}, \mathbf{z})), \tag{6}$$

where $\mathcal{KS}()$ is the test statistic, and $\mathbf{Z}$ is the matrix of all permutations of the treatment assignment. If a hypothesized effect creates a much larger observed difference in the treatment and control distributions than we would expect under the uniformity trial, we reject θ_k with some level of confidence. This test has the attractive feature of showing the range of effects that could plausibly have generated the data, given sample variance and the assignment mechanism. The confidence interval consists of all effects for which we fail to reject the null.

4 Results

Table 1 presents estimates of the intent-to-treat on a log transformation of the amount paid in USD, so coefficients can be interpreted roughly as percent changes in the outcome. The first two columns present the estimated ITT effect of any kind of observation at all, without and with covariates. I estimate the treatment causes a roughly 5% decrease ($e^{-.048} - 1 = -.047$) in bribes, conditional on checkpoint covariates and trip fixed effects. This is substantively small: with a mean bribe of .77 USD in control, the treatment is estimated to have reduced bribes by less than five cents. Moreover, the effect is statistically insignificant: using the procedure described in section 3, I fail to reject a two-sided test of the sharp null of no effects for all units. Columns 3 and 4 use observations from all trips to estimate the effect of neutral observation, while columns 5 and 6 excludes trip one in which the NGO arm of the treatment was not randomized. Not only are the effects substantively small and statistically insignificant, but we do not observe the monotonic, negative relationship we may have expected between the neutral and NGO arms: rather than decreasing bribes

9

further by increasing expectations of punishment, the NGO treatment produces estimates that are closer to 0, if anything. In general, bribes are higher in the east (likely due to the particularly predatory situation in Cotonou, Benin), when they are extorted by police and when the checkpoint is a government-mandated one (i.e. a permanent structure).

The picture is much the same for the effects on minutes of delay time, presented on table 2. The estimates are substantively small: with an average delay of two and a half minutes in control, any kind of observation by an outsider is estimated to decrease wait times by only 13 seconds. No effect is statistically significant.

One explanation for the findings may be that the treatment produces cross-cutting heterogeneous effects: if some risk averse officials decrease bribes while other opportunistic officials increase them in response to outsider presence, we may estimate effects close to 0 on average. If there is effect heterogeneity of this kind, the variance in checkpoints that had any observation will be greater than that among those assigned to control. The left-hand panel of figure 2 shows that this is not the case: the estimated ratio of variances is .79, indicating lower variance in the treatment group. We fail to reject the null of equal variances, $\tau_\sigma = 1$, with 95% confidence.

Failure to reject the sharp null of no effects for all units or of equal variances between treatment and control does not constitute evidence in favor of either hypothesis: it may also be that the test is underpowered to detect effects. To understand what kinds of null hypotheses we *can* rule out given the power of the test, I conduct the uniformity trial tests described in section 3.

The second panel of figure 2 presents the results: with 95% confidence we can reject the null of an effect larger than 10% in absolute value. In other words, the experiment presents statistically significant evidence against large constant effects. Thus, I conclude that the experts' expectations that the experiment would show large negative effects on extortion were incorrect.

	Dependent variable:					
	log(Bribe in USD + 1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Any observation	−0.084	−0.049				
	(0.093)	(0.074)				
Neutral observation			−0.092	−0.073		
			(0.106)	(0.085)		
NGO observation					−0.0005	0.017
					(0.079)	(0.065)
Police checkpoint		0.204**		0.177		0.149*
		(0.093)		(0.111)		(0.089)
Mandated checkpoint		0.170**		0.188*		0.091
		(0.084)		(0.097)		(0.078)
Latitude		0.022		0.045		0.012
		(0.032)		(0.038)		(0.027)
Longitude		0.511***		0.452***		0.329***
		(0.094)		(0.103)		(0.123)
Trip 2		1.053***		0.792**		
		(0.321)		(0.354)		
Trip 3		0.536		0.305		−0.471***
		(0.327)		(0.367)		(0.093)
Trip 4		0.978***		0.765**		−0.212**
		(0.348)		(0.385)		(0.106)
Constant	0.342***	−0.451*	0.342***	−0.485	0.213***	0.531
	(0.066)	(0.271)	(0.075)	(0.323)	(0.056)	(0.365)
Mean control bribe	0.77	0.77	0.77	0.77	0.34	0.34
Observations	123	123	98	98	75	75
Adjusted R^2	−0.001	0.381	−0.002	0.346	−0.014	0.320

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

Table 1: Intent-to-treat effects on bribe paid at checkpoint.

All specifications use least-squares regression with observations weighted by the inverse of the probability they were assigned to their observed condition. All *p*-values calculated using randomization tests of the two-sided sharp null hypothesis of no effects for all units. Columns 5 and 6 exclude the first trip, for which no unit was assigned to the 'NGO' condition.

	Dependent variable:					
	log(Minutes delay + 1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Any observation	−0.104	−0.092				
	(0.154)	(0.125)				
Neutral observation			−0.125	−0.103		
			(0.172)	(0.140)		
NGO observation					−0.165	−0.159
					(0.175)	(0.126)
Police checkpoint		−0.382**		−0.340*		−0.491***
		(0.158)		(0.183)		(0.173)
Mandated checkpoint		0.226		0.210		0.341**
		(0.143)		(0.159)		(0.152)
Latitude		0.070		0.088		0.078
		(0.055)		(0.063)		(0.053)
Longitude		0.352**		0.331*		0.141
		(0.159)		(0.170)		(0.238)
Trip 2		0.732		0.615		
		(0.543)		(0.581)		
Trip 3		−0.171		−0.342		−0.796***
		(0.554)		(0.602)		(0.180)
Trip 4		0.243		0.154		−0.508**
		(0.589)		(0.632)		(0.205)
Constant	0.772***	0.291	0.772***	0.207	0.718***	0.723
	(0.109)	(0.459)	(0.122)	(0.531)	(0.124)	(0.707)
Mean control delay	2.36	2.36	2.36	2.36	1.88	1.88
Observations	123	123	98	98	75	75
Adjusted R^2	−0.004	0.346	−0.005	0.336	−0.001	0.485

Note: $^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 2: Intent-to-treat effects on time delayed at checkpoint.

All specifications use least-squares regression with observations weighted by the inverse of the probability they were assigned to their observed condition. All *p*-values calculated using randomization tests of the two-sided sharp null hypothesis of no effects for all units. Columns 5 and 6 exclude the first trip, for which no unit was assigned to the 'NGO' condition.
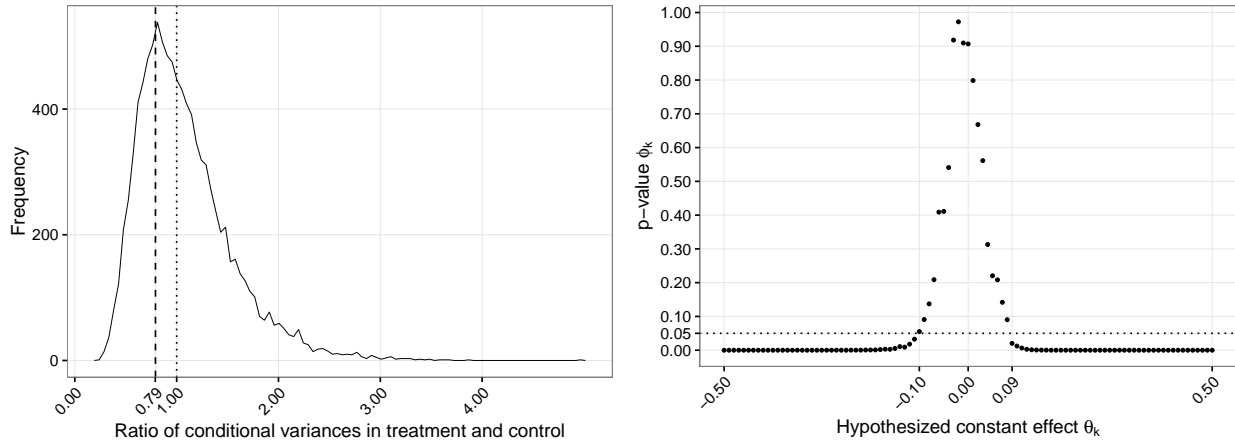
Figure 2: Fisher randomization tests for intent-to-treat effects on variance ratio and difference in distributions.

Both panels employ residuals from a regression of the outcome on pre-treatment covariates. The vertical dashed line on the left panel shows the observed ratio in variances (see eq. 4), while the solid line shows the distribution of variance ratios under the sharp null of equal variances. The right panel shows the p-value for the difference in treatment and control distributions under different hypothesized constant effects (see eq. 6), with the $\alpha = .05$ confidence level indicated through the horizontal dashed line.

5   Discussion

Despite strong expectations to the contrary, officials in Ghana, Benin, Togo and Burkina Faso did not depart from the going rate for bribes when a foreigner unexpectedly observed them extorting truck drivers for money. Although the sample size of the experiment is not large, it is well-powered enough to reject the hypothesis of constant effects larger than 10% in absolute value. There is no evidence to suggest that the small, possibly 0 effects represent the average of cross-cutting heterogeneous effects. Rather, my evidence suggests that foreigner presence does not cause officials to depart from corrupt equilibria by deviating from the going rate.

These findings are consistent with other studies that have found low-level monitoring is insufficient to reduce corruption (Olken, 2007). They contrast most obviously with Cilliers, Dube, and Siddiqi (2015), who find that a "white observer" produces strong behavioral

changes in lab-in-field games.

In terms of the theory and policy on so-called "corruption traps", the findings present two implications. First, they confirm the intuitive notion that nudges may not be enough to destabilize resilient, highly institutionalized equilibria, especially if such nudges produce uncertainty in payoffs. Second, and perhaps most importantly, they suggest that experts are prone to over-estimating the effectiveness of novel anti-corruption measures. The evidence presented in this paper refutes the expectations of a majority of experts who anticipated large effects.

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