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CORRUPTION PERCEPTIONS VS. CORRUPTION REALITY

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ABSTRACT

This paper examines the accuracy of beliefs about corruption, using data from Indonesian villages. Specifically, I compare villagers' stated beliefs about the likelihood of corruption in a road-building project in their village with a more objective measure of 'missing expenditures' in the project, which I construct by comparing the projects' official expenditure reports with an independent estimate of the prices and quantities of inputs used in construction. I find that villagers' beliefs do contain information about corruption in the road project, and that villagers are sophisticated enough to distinguish between corruption in the road project and other types of corruption in the village. The magnitude of their information, however, is small, in part because officials hide corruption where it is hardest for villagers to detect. This may limit the effectiveness of grass-roots monitoring of local officials. I also find evidence of systematic biases in corruption beliefs, particularly when examining the relationship between corruption and variables correlated with trust. For example, ethnically heterogeneous villages have higher perceived corruption levels but lower actual levels of missing expenditures. The findings illustrate the limitations of relying solely on corruption perceptions, whether in designing anti-corruption policies or in conducting empirical research on corruption.

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1 Introduction

Corruption is thought to be a significant problem in much of the developing world. Corruption not only imposes a tax on public services and private sector activity; it also creates potentially severe efficiency consequences as well (Kreuger 1974, Shleifer and Vishny 1993, Bertrand et. al 2006). Yet despite the importance of the problem, eliminating corruption has proved difficult in all but a few developing countries.

One potential reason why corruption is so persistent is that citizens may not have accurate information about corruption. After all, since corruption is illegal, regularly and directly observing corrupt activity is almost always impossible. If citizens' perceptions about corruption are accurate, then the democratic process and grass-roots monitoring can potentially provide incentives for politicians to limit corruption. If, on the other hand, citizens have little in the way of accurate information about corrupt activity – or even if citizens know about average levels of corruption but do not know who is corrupt and who is honest – then the political process may not provide sufficient incentives to restrain corruption.

The accuracy of corruption perceptions is also important because of their ubiquitous use by international institutions and academics to measure corrupt activity. For example, corruption perceptions form the basis of the much-cited cross-country Transparency International Corruption Index (Lambsdorff 2004) and World Bank Governance Indicators (Kaufmann et al. 2005), and are used extensively within countries as well to assess governance at the sub-national level. Perceptions have also been widely used in academic research on the determinants of corruption.[1] Measuring beliefs about corruption rather than corruption itself skirts the inherent difficulties involved in measuring corruption directly, but raises the question of how those being surveyed form their beliefs in the first place, and how accurate those beliefs actually are.

This paper examines the empirical relationship between beliefs about corruption and a more objective measure of corruption, in the context of a road-building program in rural Indonesia. To construct an objective measure of corruption, I assembled a team of engineers and surveyors who,

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[1] Prominent papers in this literature include Mauro (1995), Knack and Keefer (1995), LaPorta et al. (1999), and Treisman (2000). This literature is surveyed in detail in Rose-Ackerman (2004).

after the roads built by the project were completed, dug core samples in each road to estimate the quantity of materials used, surveyed local suppliers to estimate prices, and interviewed villagers to determine the wages paid on the project. From these data, I construct an independent estimate of the amount each road actually cost to build, and then compare this estimate to what the village reported it spent on the project on a line-item by line-item basis. The difference between what the village claimed the road cost to build and what the engineers estimated it actually cost to build forms my objective measure of corruption, which I label 'missing expenditures.' To obtain data on villagers' beliefs about corruption, in the same set of villages I also conducted a household survey, in which villagers were asked their beliefs about the likelihood of corruption in the road project.

Using these data, I find that villagers' beliefs about the likelihood of corruption in the road project do contain information about the level of missing expenditures in the project. Moreover, villagers are sophisticated enough in their beliefs to distinguish between general levels of corruption in the village and corruption in the particular road project I examine. However, the magnitude of this information is small: increasing missing expenditures by 10 percent is associated with just a 0.8 percent increase in the probability a villager believes that there is any corruption in the project.

One reason villagers' information about corruption may be limited is that officials have multiple methods of hiding corruption, and choose to hide corruption in the places it is hardest for villagers to detect. In particular, my analysis suggests that villagers are able to detect marked-up prices, but appear unable to detect inflated quantities of materials used in the road project. Consistent with this, the vast majority of corruption in the project occurs by inflating quantities, with almost no markup of prices on average. The inability of villagers to detect inflated quantities, combined with the fact that officials can substitute between hiding corruption as inflated prices or inflated quantities, suggests that officials may be strategic in how they hide corruption, and that effective monitoring requires specialist auditors who can detect multiple types of corruption.

The fact that the overall correlation between beliefs and missing expenditures is positive, however, is not sufficient to show that the two variables can be used interchangeably as measures of corruption. In particular, beliefs may be systematically biased. I first show that, even controlling for village fixed effects (and therefore controlling completely flexibly for the actual level of

corruption in the road) and benchmarking for how respondents answer the corruption question in other contexts, individual characteristics such as education and gender systematically predict respondents' perceptions of corruption in the road project. The fact that characteristics other than actual corruption systematically predict corruption perceptions suggests that people's perceptions may be biased.

Just because individual beliefs are biased does not necessarily mean that, in aggregate, corruption perceptions will give misleading results when investigating the determinants of corruption. To test for aggregate biases that would affect inference about the determinants of corruption, I examine the relationship between the two different measures of corruption and a host of village characteristics. Consistent with other studies, I find, for example, that increased ethnic heterogeneity is associated with higher levels of perceived corruption (e.g., Mauro 1995, LaPorta 1999), and that increased levels of participation in social activities is associated with lower levels of perceived corruption (e.g., Putnam 1993). But when I examine the relationship between these variables and the missing expenditures variable, I find very different results – ethnic heterogeneity is associated with *lower* levels of missing expenditures, and participation in social activities is not correlated with missing expenditures levels at all.

One explanation for these differences is that these village characteristics affect levels of interpersonal trust in the village. Low levels of trust may increase people's perceived levels of corruption, which may cause them to monitor more aggressively, in turn reducing the actual level of corruption. In fact, I find suggestive evidence that this is the case – villagers in more ethnically heterogeneous villages are less likely to report trusting their fellow villagers, and more likely to attend project monitoring meetings, than those in homogeneous villages. These results suggest that when examining the correlates of corruption, examining perceptions of corruption may lead to misleading conclusions, particularly when investigating factors related to trust. Instead, more objective methods of measuring corruption, such as the approach used here (or the related approaches used by Di Tella and Schargrodsky 2003, Reinikka and Svensson 2004, Fisman and Wei 2004, Yang 2004, Hsieh and Moretti forthcoming, and Olken 2006), may produce more reliable results.

This paper is related to several literatures in economics that seek to characterize the relationship

between beliefs and reality more generally. For example, there is a large literature examining the accuracy and potential biases in individuals' forecasts of their own future retirement decisions, mortality, and income.[2] In the public sphere, several authors have also found that perceptions are positively correlated with more objective measures of performance, in the very different contexts of international perceptions of bribery (Mocan 2004) and principals evaluating teachers (Jacob and Lefgren 2005). In the setting closest to that examined here, however, Duflo and Topalova (2004) document that women leaders in Indian villages deliver better public services than male leaders, yet score worse on measures of citizen satisfaction. Their results, consistent with the results presented here, suggest that there may be political market failures caused by inaccuracies in public perceptions about the performance of government officials.

The remainder of this paper is organized as follows. Section 2 discusses the empirical setting and the data used in the paper. Section 3 develops a theoretical framework that illustrates the relationship between beliefs about corruption and actual corruption and discusses how biases in perceptions might feed back to affect actual corruption levels. Section 4 presents the empirical results. Section 5 concludes.

2   Setting and Data

2.1   Empirical Setting

The data in this paper come from 477 villages in two of Indonesia's most populous provinces, East Java and Central Java. The villages in this study were selected because they were about to begin building small-scale road projects under the auspices of the Kecamatan (Subdistrict) Development Project, or KDP. KDP is a national government program, funded through a loan from the World Bank, which finances projects in approximately 15,000 villages throughout Indonesia each year. The data in this paper were collected between September 2003 and August 2004.

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[2]For example, Bernheim (1989) discusses systematic variability in individual accuracy in forcasting their retirement dates, Hurd and McGarry (1995) document that individuals with certain observable characteristics are systematically more likely to over or under-predict their own mortality, Dominitz and Manski (1997) document that individuals can forcast their expected income, and Bassett and Lumsdaine (1999, 2001) discuss how even controlling for observable characteristics, some individuals are likely to be over-optimistic accross a wide variety of beliefs whereas others are systematically over-pessimistic.

The roads I examine are built of a mixture of rock, sand, and gravel, range in length from 0.5 – 3 km, and may either run within the village or run from the village to the fields. A typical road project costs on the order of Rp. 80 million (US$8,800 at the then-current exchange rate). Under KDP, a village committee receives the funds from the central government, and then procures materials and hires labor directly, rather than using a contractor as an intermediary. The allocation to the village is lump-sum, so that the village is the residual claimant. In particular, surplus funds can be used, with the approval of a village meeting, for additional development projects, rather than having to be returned to the KDP program. These funds are often supplemented by voluntary contributions from village residents, primarily in the form of unpaid labor. A series of three village-level meetings are conducted to monitor the use of funds by the village committee implementing the project.

Corruption in the village projects can occur in several ways. First, village implementation teams, potentially working with the village head, may collude with suppliers to inflate either the prices or the quantities listed on the official receipts. Second, members of the implementation team may manipulate wage payments by inflating the wage rate or the number of workers paid by the project.

The villages in this study were part of a randomized experiment on reducing corruption, described in more detail in Olken (2005). Three experimental treatments were conducted in randomly selected subsets of villages: an increase in the probability of an external government audit of the project, an increase in the number of invitations distributed to the village meetings regularly held to oversee use of project funds, and the distribution of anonymous comment forms. All of the empirical specifications reported below include dummy variables for each of these experimental treatments to ensure that the effects reported here are not being driven by these experiments, though the results below are essentially similar if the experimental dummies are not included. (I discuss the effects of the experiments on beliefs about corruption in Section 4.4).

The data used here come from three surveys designed by the author: a household survey, containing data on household beliefs about corruption in the project; a field survey, used to measure missing expenditures in the road project; and a key-informant survey with the village head and

the head of each hamlet, used to measure village characteristics. In the subsequent subsections, I describe the two aspects of the data that are the focus of this study – the household survey on corruption perceptions and the field survey to measure missing expenditures in the road project. Additional details about the data collected can be found in Appendix B.

2.2   Beliefs about Corruption

Data on beliefs about corruption were obtained from a survey of a stratified random sample of adults in the village. The survey was conducted between February 2004 - April 2004, when construction of the road projects was between 80% - 100% complete. The sample includes 3,691 respondents.

The key corruption question I examine is the following: "Generally speaking, what is your opinion of the likelihood of diversions of money / *KKN* (corruption, collusion, and nepotism) involving [...]," where [...] is 1) the President of Indonesia (at the time, Megawati Sukarnoputri), 2) the staff of the subdistrict office (the administrative level above the village), 3) the village head, 4) the village parliament, and 5) the road project. KKN is the Indonesian acronym for corruption, collusion, and nepotism – the catch-all phrase for corruption in Indonesian. Respondents were given 5 possible choices in response – none, low, medium, high, and very high. The first four questions (from the President to the village parliament) were asked, in that order, in the middle of the 1.5 hour survey; the question about the road project was asked towards the end of the survey.

The tabulations of the responses to this question for corruption involving the road project and, by way of comparison, the President of Indonesia and the village head, are given in Table 1. Several things are worth noting about the responses. First, the more 'local' the subject being asked about, the less corruption respondents report – i.e., respondents report the highest corruption levels for the President, followed by the village head, followed by the road project.

Second, 8.9% of respondents do not answer the question about corruption in the road project, claiming either they do not know or they do not want to answer. In interviews it appeared that many people who refused to answer did so because they felt uncomfortable saying that there was corruption. Although respondents were assured that responses would remain anonymous, this reluctance to state opinions about corruption is common to many surveys of corruption. It is

particularly understandable in this context, given that free speech was restricted in Indonesia until the end of the Soeharto government in 1998, and that even now village heads still wield considerable local authority.

I therefore examine two versions of the corruption beliefs variable that deal with these non-responses in different ways. The first version is simply the five ordered categorical responses shown in Table 1, where "refused to answer" is treated as missing. I use ordered probit models to investigate the determinants of this categorical response variable. The disadvantage of this approach is that it disregards the potentially useful information contained in "refused to answer," namely that those who refuse to answer often believe there is corruption but are unwilling to say so. I therefore create a second version of the beliefs variable called "any likelihood of corruption" that groups all positive likelihood of corruption answers together with non-responses. This variable is equal to 1 if the respondent reports any positive probability of corruption (low, medium, high, or very high) or refused to answer the corruption question, and 0 otherwise.[3] I use probit models to investigate the determinants of this dummy variable. As will be discussed in more detail below, the two variables produce broadly similar results.

2.3   Missing Expenditures

The objective measure of corruption I use is "missing expenditures" in the road project. Missing expenditures are the difference in logs between what the village claimed it spent on the project and an independent estimate of what it actually spent. This measure is approximately equal to the percent of the expenditures on the road project that cannot be accounted for by the independent estimate of expenditures.

Obtaining data on what villages claim they spent is relatively straightforward. At the end of the project, all village implementation teams were required by KDP to file an accountability report with the project subdistrict office, in which they reported the prices, quantities, and total expenditure on each type of material and each type of labor (skilled, unskilled, and foreman) used

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[3]Alternatively, if I use a dummy variable for any positive perceptions of corruption, but drop missings rather than count them as a positive perception, the results are slightly weaker than the results presented. This is consistent with the idea that a non-response is associated with a positive perceived corruption probability.

in the project. The total amount reported must match the total amount allocated to the village. These reports were obtained from the village by the survey team.

Obtaining an independent estimate of what was actually spent was substantially more difficult, and involved three main activities—an engineering survey to determine quantities of materials used, a worker survey to determine wages paid by the project, and a supplier survey to determine prices for materials. In the engineering survey, an engineer and an assistant conducted a detailed physical assessment of all physical infrastructure built by the project in order to obtain an estimate of the quantity of main materials (rocks, sand, and gravel) used. In particular, to estimate the quantity of each of these materials used in the road, the engineers dug ten 40cm $\times$ 40cm core samples at randomly selected locations on the road and measured the quantities of each material in each core sample. By combining the measurements of the volume of each material per square meter of road with measurements of the total length and average width of the road, I can estimate the total quantity of materials used in the road. I also conducted calibration exercises to estimate a "loss ratio," i.e., the fraction of materials that are typically lost are lost as part of the normal construction process.[4]

To measure the quantity of labor, workers were asked which of the many activities involved in building the road were done with paid labor, voluntary labor, or some combination, what the daily wage and number of hours worked was, and to describe any piece rate arrangements that may have been part of the building of the project. To estimate the quantity of person-days actually paid out by the project, I combine information from the worker survey about the percentage of each task done with paid labor, information from the engineering survey about the quantity of each task, and assumptions of worker capacity derived both from the experience of field engineers and the experience from building the calibration roads.

To measure prices, a price survey was conducted in each subdistrict. Since there can be substantial differences in transportation costs within a subdistrict, surveyors obtained prices for each

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[4]For example, some amount of sand may blow away off the top of a truck, or may not be totally scooped out of the hole dug by the engineers conducting the core sample. I estimated the ratio between actual materials used and the amount of materials measured by the engineering survey by constructing four test roads, where the quantities of materials were measured both before and after construction. In calculating missing expenditures, I multiply the estimated actual quantities based on the core samples of the road by this loss ratio to generate the actual estimated level of expenditures on the road project.

material that included transportation costs to each survey village. The price survey included several types of suppliers—supply contractors, construction supply stores, truck drivers (who typically transport the materials used in the project), and workers at quarries—as well recent buyers of material (primarily workers at construction sites). For each type of material used by the project, between three and five independent prices were obtained; I use the median price from the survey for the analysis.

From these data – reported and actual quantities and prices for each of the major items used in the project – I construct the missing expenditures variable. Specifically, I define the missing expenditures variable to be the difference between the log of the reported amount and the log of the actual amount. As shown in Table 1, on average, after adjusting for the normal loss ratios derived from the calibration exercise, the mean of the missing expenditures variable is 0.24. Note, however, that while the *levels* of the missing expenditures variable depend on the loss ratios, the *differences* in missing expenditures across different villages do not.[5] As a result, I focus primarily on the differences in missing expenditures across villages rather than on the absolute level of missing expenditures.

3   Theoretical Framework

The empirical analysis will examine the relationship between the two measures of corruption just described – villagers' beliefs about the likelihood of corruption and a more objective measure of "missing expenditures." Before proceeding to the empirical analysis, this section develops a simple model of corruption beliefs in order to provide a theoretical framework that relates these two variables. The theoretical framework illustrates two key points. First, the beliefs about the likelihood of corruption reported in the household survey are an amalgam of respondents' general prior beliefs about the likelihood of corruption in the village and any specific information the respondent has learned about whether corruption actually occurred in this particular project. The

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[5]To see this, note that the loss ratio is a multiplicative constant for each component of the road. If there was only one type of material used the project, then since missing expenditures is expressed as the differences in logs, the loss ratio is simply an additive constant. With multiple components (e.g., rocks, sand, gravel, etc), the additive constant varies slightly from village, depending on the relative weights of the different components in different villages. These differences are small, however, so that changes in the loss ratios do not substantively affect the results.

efficacy of villager monitoring depends on the degree to which villagers have specific information about a particular project, rather than just a prior. The model shows that, in the presence of heterogeneity in the prior probability of corruption across villages, one needs to control for priors in order to determine whether respondents actually have specific information about corruption.

Second, villagers' beliefs are not only influenced by actual corruption levels; I show in the empirical work that they are also influenced by person-specific biases – i.e., for a given actual level of corruption, certain types of systematically people are more likely to believe that there is corruption than others. The model illustrates how an aggregate bias (i.e., a bias held by everyone in the village) can have a feedback effect on actual corruption levels. Empirically, these types of biases suggest that using perceptions to measure corruption can produce systematically misleading results.

3.1   Model Setup

I assume that there are two actors: a villager and a village head. The model takes place in three steps. First, the village head decides whether to try to steal or not. Second, the villager receives a noisy signal about whether or not the village head tried to steal. The signal is accurate with probability μ – i.e., if the village head tried to steal the villager receives the signal h with probability μ and the signal l with probability $1 - \mu$, where $\frac{1}{2} \leq \mu < 1$; if the village head does not try to steal, the probabilities are $1 - \mu$ and μ, respectively. The villager has a prior belief about the probability the village head tried to steal, which he updates based on the signal he receives. Third, the villager can choose whether or not to pay a fixed monitoring cost M to determine whether the village head tried to steal or not. Writing the payoffs in the form (village head, villager), the payoffs are:

	Monitor	Don't Monitor
Steal	$(-F, -M)$	$(c, -\alpha c)$
Don't Steal	$(0, -M)$	$(0, 0)$

where F is the fine imposed if the villager monitors and the village head was trying to steal, c is the amount stolen, and α is the social loss from theft. I assume that $\alpha > 1$, so that the socially

10

efficient outcome is (Don't Steal, Don't Monitor). I also assume that $\alpha c > M$, so that if the villager knew for sure that the village head was trying to steal the villager would monitor for sure, and that $F > c$, so that the fine imposed if the village head is caught stealing is greater than the benefits to the village head from corruption.[6]

3.2   Signal quality and the equilibrium level of corruption

I solve for a Perfect Bayesian Equilibrium (PBE). Define q as the equilibrium probability the village head steals, and define t (for theft) to be the realization of whether the village head tried to steal, so that $t = 1$ with probability q and $t = 0$ with probability $1 - q$. Define m_h as the villager's equilibrium probability of monitoring conditional on receiving the high signal, and m_l as the villager's equilibrium probability of monitoring conditional on receiving the low signal.

What does the villager infer from his signal? Define B to be the villager's posterior beliefs about the probability that corruption occurred (i.e., his belief that $t = 1$). Applying Bayes rule, along with the PBE assumption that the villager's priors must equal the true probability q, yields the following posterior beliefs B:

$$P\left(t = 1 \mid h\right) = \frac{q\mu}{q\mu + (1 - q)(1 - \mu)} \tag{1}$$

$$P\left(t = 1 \mid l\right) = \frac{q(1 - \mu)}{q(1 - \mu) + (1 - q)\mu} \tag{2}$$

If the signal contains information (i.e., if $\mu > \frac{1}{2}$), then $P\left(t = 1 \mid h\right) > P\left(t = 1 \mid l\right)$.

Under these assumptions, there is no equilibrium where the village head plays a pure strategy. To see this, note that if the village head always stole, the villagers would always monitor, since $\alpha c > M$, in which case the village head would deviate and not steal. Similarly, if the village head never stole, the villager would never monitor, in which case the village head would deviate and try to steal. In equilibrium, then, the village head must be indifferent between stealing and not-stealing.

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[6]This assures that if the villager were to monitor if he received the high signal, and were not to monitor if he received the low signal, then the village head will choose not to steal. I focus on this case to simplify the algebra and the presentation, but the qualitative results about the correlation of beliefs and reality presented below go through if we relax the assumption that $F > c$.

In fact, the unique Perfect Bayesian Equilibrium can be written as follows:

$$\left\{ q = \frac{1-\mu}{\frac{\alpha c}{M}\mu + (1-2\mu)}, m_h = \frac{c}{\mu(F+c)}, m_l = 0 \right\} \tag{3}$$

(See Appendix A for derivation and proofs.) Taking derivatives, one can show that $\frac{dq}{d\mu} < 0$, so that the equilibrium probability of corruption decreases as the quality of the signal improves, illustrating the potential for feedback between the accuracy of the signal and equilibrium corruption levels.

Since the village head must be indifferent in order to randomize, and since "don't steal" always yields a payoff of 0, the village head will always expect to receive 0 in equilibrium. This means that corruption doesn't actually increase the utility of the corrupt actor (the village head) in equilibrium above what it would be if corruption did not exist; rather, it simply reduces the utility of the villager. Since both corruption and monitoring decrease as the signal becomes more accurate, the total expected payoffs to the villager rise as the villager obtains better quality information. The villager therefore gains all of the benefits from increased transparency.

Many of the other comparative statics of the model are intuitive. For example, increasing the monitoring cost M increases corruption, since it reduces the willingness of the villager to monitor for a given level of corruption q. For analogous reasons, increasing the cost to the villager of unmonitored corruption – i.e., increasing αc – decreases corruption. Increasing the fine F does not reduce corruption; rather, increasing F reduces the monitoring probability m_h, as a higher fine means that the villager can keep the village head indifferent with less frequent monitoring.

3.3 Signal quality and the correlation between beliefs and reality

In the data, we do not observe the villagers' signals directly; instead, we observe villagers' stated posterior beliefs (B), and missing expenditures in the road, which corresponds to an objective measure of whether theft occurred (t). This section discusses what we can infer about the underlying signal quality μ – i.e., how much information villagers actually have – from the correlations of B and t in the data.

Specifically, there are two potential correlations we can observe in the data, the correlation con-

ditional on the villagers' priors q, i.e., $\text{Corr}\,(B,t\mid q)$, and the unconditional correlation, $\text{Corr}\,(B,t)$. Focusing first on the conditional correlation $\text{Corr}\,(B,t\mid q)$, using equations equations (1) and (2) one can show that:

$$\text{Corr}\,(B,t\mid q) = \frac{\sqrt{q}\,(q + 2\mu - 2q\mu - 1)}{\sqrt{(1-q)\,(2q\mu + 1 - \mu - q)\,(q + \mu - 2q\mu)}} \tag{4}$$

If there is heterogeneity in q across villages, then the $\text{Corr}\,(B,t\mid q)$ we estimate will be the expectation of (4) over the distribution of q in the data.

This conditional correlation $\text{Corr}\,(B,t\mid q)$ has several intuitive properties. First, one can show that if $\frac{1}{2} < \mu < 1$, then $\text{Corr}\,(B,t\mid q) > 0$, and if $\mu = \frac{1}{2}$, then $\text{Corr}\,(B,t\mid q) = 0$. Therefore, if the conditional correlation is positive, we can infer that the villager has information about the occurrence of corruption, i.e., that $\mu > \frac{1}{2}$. Second, the correlation between beliefs and actual corruption is increasing in the signal quality μ, i.e. $\frac{\partial\,\text{Corr}(B,t|q)}{\partial\mu} > 0$. Therefore, although we cannot recover the primitive μ directly from this correlation without knowing the other parameters of the model, we can infer that a stronger conditional correlation between beliefs and reality implies that the villagers receive a more accurate signal about corruption, i.e. there is a greater μ.

The same properties do not necessarily apply to the unconditional correlation $\text{Corr}\,(B,t)$, however, if there is heterogeneity across villages in the parameters that determine the equilibrium level of corruption in the village (q). To take a simple example, suppose $\mu = \frac{1}{2}$, so that the signal contains no information about the realization of corruption t. In this case, we know that $B = q$, i.e., within a given village, beliefs have no information and are equal to the prior probability of corruption q. Since beliefs are constant, $\text{Corr}\,(B,t\mid q) = 0$. But across villages of different parameters, beliefs do contain information – beliefs are equal to the prior probability of corruption in the village (q), and thus will be positively correlated with realized corruption levels. This means that in such a scenario $\text{Corr}\,(B,t) > 0$ even though $\mu = \frac{1}{2}$ and $\text{Corr}\,(B,t\mid q) = 0$. So it is possible to generate a positive overall correlation between beliefs and reality even if villagers receive no signal about the realization of corruption and are simply reporting their priors.

Thus, to separate out whether villagers have specific information about corruption – i.e., whether

$\mu > \frac{1}{2}$ and villagers have information about the realization of corruption rather than just its prior probability – it is important in the empirical work to control as best as possible for the villager's prior beliefs about the overall probability of corruption in his or her village. In the model, the only sources of heterogeneity in beliefs were due to differences in the underlying probability of corruption in the data; in reality, as will be discussed in Section 4.3 below, there may be other individual-level sources of heterogeneity in beliefs as well. For example, conditional on actual corruption levels, more highly educated villagers may be more likely to believe there is corruption than less highly educated villagers. In the empirical work below, I will use the respondent's beliefs about the probability of corruption involving the President, subdistrict officials, village head, and village parliament – i.e., general beliefs about corruption in the village but not specifically involving the road project or the ad-hoc committee elected to oversee it – as well as a host of other individual covariates such as age, gender, and education, to control as flexibly as possible for factors that would be related to the respondents' prior beliefs about the probability of corruption q.

3.4 Feedback between biases in beliefs and corruption levels

As just discussed, in practice, the model's assumption that individuals' beliefs are equal to the true probability q may not hold in equilibrium. Instead, beliefs may be biased. For example, if the villager and village head are from different ethnic groups, the villager might be prejudiced to believe that the village head is more likely to steal than he actually is. If these biases exist, they can feed back to affect real corruption levels.

To see this, consider the case where the villager is naïve and has beliefs $p = 0$ – i.e., he believes that the village head never steals. Biased beliefs violate the PBE equilibrium requirement that beliefs must be accurate in equilibrium; I will therefore assume a Bayesian Equilibrium (Fudenberg and Tirole, 1991) instead, which simply requires that all players choose optimally given their beliefs, but does not impose any restrictions on the form of those beliefs. However, I assume that the villager updates his beliefs according to Bayes rule based on the signal he receives and his (inaccurate) prior. In this case, if $p = 0$, the villager believes that the village head is always honest, ignores his signal, and never monitors, so $m_h = m_l = 0$ in equilibrium, i.e., there is no monitoring in equilibrium.

14

Since the villager never monitors, the village head always steals, so $q = 1$. Analogously, if the villager's prior belief is that the village head always steals, i.e. $p = 1$, then the villager always monitors and the village head never steals. Given this, factors about the village that create biases in beliefs – for example, factors that affect the general level of trust in the village – can have opposite effects on beliefs and actual corruption levels.

This simple example assumed that beliefs were constant, and unrelated to equilibrium levels of corruption. But similar arguments hold for smaller biases as well, and for biases that simply shift priors from the equilibrium level. Suppose, for example, that the villager's priors are equal to the true equilibrium probability of corruption q plus a small bias term ε, so that the prior $p = q + \varepsilon$. In this case, if ε is sufficiently small, the equilibrium will be as follows:

$$\left\{ q = \frac{1-\mu}{\frac{\alpha c}{M}\mu + (1-2\mu)} - \varepsilon, m_h = \frac{c}{\mu(F+c)}, m_l = 0 \right\} \tag{5}$$

This yields the same result as the more extreme example above – the greater the bias in perceptions ε, the lower the equilibrium level of corruption. Thus, a level bias ε, which does not affect the accuracy of the signal μ but which simply makes individuals more optimistic or pessimistic about corruption, can nevertheless also have a feedback effect to alter the equilibrium level of corruption. Of course, for this to be sustained over time, the bias must remain despite repeated signals - i.e., there must be some reason, such as ethnic prejudice, why beliefs do not adjust in equilibrium. Whether or not these biases exist will be examined in the empirical work below.

4 Empirical Results

4.1 Do individuals have information about corruption?

I begin by estimating whether beliefs contain any information about actual corruption levels – i.e., whether the unconditional correlation $\text{Corr}(B, t)$ is positive. Then, I investigate whether villagers have specific information about the road project - i.e., in the language of the model, whether $\mu > \frac{1}{2}$ or not – by estimating as best as possible the conditional correlation $\text{Corr}(B, t \mid q)$.

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To measure beliefs (B), I consider both versions of the corruption perceptions variable described above – the categorical response variable and a dummy variable for any positive probability of corruption in the road project (including missings as positive responses). To measure corruption objectively (t), I use the missing expenditures variable. To investigate whether $\text{Corr}(B, t) > 0$, I estimate an ordered probit model of the following form:

$$\mathbf{P}(B_{vh} = j) = \Phi\left(\theta_j - \beta t_v - X'_{vh}\gamma\right) - \Phi\left(\theta_{j-1} - \beta t_v - X'_{vh}\gamma\right) \tag{6}$$

where B is the respondent's answer to the question about perceptions of corruption in the road project, t is the estimate of missing expenditures in the road project, v represents a village, h represents a household, j is one of the J categorical answers to the beliefs question, θ_j is a cutoff point estimated by the model (with $\theta_0 = -\infty$ and $\theta_J = \infty$), X_{vh} are dummies for how the household was sampled, which version of the form the respondent received, and the experimental treatments, and Φ is the Normal CDF. The test of whether $\text{Corr}(B, t) > 0$ is a test of whether the coefficient $\beta > 0$. For the dummy variable version of the perceptions variable, I estimate the equivalent probit equation (i.e., with only one threshold level θ_j). Standard errors are adjusted for clustering at the subdistrict level, to take into account the fact that there are multiple respondents h in a single village v and that the missing expenditures variable may be correlated across villages in a given subdistrict.[7]

The results are presented in columns (1) and (4) of Table 2 for the categorical and dummy variables, respectively. Note that to facilitate interpretation, for the probit specification in column (4) I present marginal effects. Both results show a positive coefficient on the missing expenditures variable, though neither coefficient is statistically significant.

A respondent's beliefs about a particular type of corruption may be colored by the respondent's attitudes about corruption in general. The responses to the beliefs question may also differ if individuals perceive the levels of the scale (i.e., 'none,' low,' etc.) differently. To correct for these

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[7]There are 143 subdistricts in the sample. One subdistrict therefore includes an average of 3.3 villages, so clustering at the subdistrict is more conservative than clustering at the village level. Clustering at the village level reduces the standard errors from those presented in the table.

factors, I benchmark the respondent's attitudes about corruption in general by using the respondent's answer to the question about the likelihood that there is corruption involving the President of Indonesia. As discussed above, the phrasing of the corruption question is the same as the question about the road project, but in this case all respondents are evaluating the same individual – the President of Indonesia. Since the person being evaluated is the same for all respondents, the different answers to this question captures general differences in the way the respondents evaluate corruption and answer the perceptions question.[8] This is analogous to the approach taken by Bassett and Lumsdaine (1999), who use responses to a question about the probability of the weather being sunny tomorrow to benchmark the overall optimism or pessimism of the respondents when interpreting questions about the respondent's beliefs about future events.[9]

The results, controlling for dummies corresponding to the different possible answers to the question about how corrupt the President is, are presented in columns (2) and (5). Controlling for perceptions of how corrupt the President is substantially strengthens the results, increasing both the magnitudes and the statistical significance in both specifications.[10]

However, even controlling for the individuals beliefs about how corrupt the President is, it is possible that the correlation between missing expenditures in the road project and perceptions of corruption in the road project reflects only villagers' perceptions of the *average* levels of corruption in their village, rather than specific information about the road project *per se*. In the language of the model, we have examined the unconditional $\mathrm{Corr}\,(B, t)$ but not the conditional $\mathrm{Corr}\,(B, t \mid q)$.

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[8]One might be concerned that corruption perceptions of the President may also capture heterogeneity in overall attitudes towards the President of Indonesia rather than just benchmarking for how the respondent answers the corruption question. However, controlling for the respondent's overall approval of the President's job performance, rather than how corrupt they think the President is, has no effect on the correlation between perceptions of corruption in the road project and the missing expenditures variable. Conversely, controlling for any of the respondent's other answers to the corruption question – i.e., perceptions of subdistrict officials, village head, or village parliament – has a similar effect to controlling for the corruption of the President, although slightly smaller in magnitude. This suggests that the effect of controlling for perceptions of the President's corruption is due to it capturing differential interpretations of the corruption question, rather than individual opinions of the President.

[9]This benchmarking exercise is also related to the anchoring vignettes literature in political science, discussed by King et al. (2004). The advantage of the approach used here relative to benchmarking against a hypothetical vignette is that the approach here captures differences in the respondents' reluctantance to report corruption (due, for example, to fear of retaliation), which would not be captured in a hypothetical question.

[10]A natural question is why controlling for beliefs about the President changes the point estimates on the correlation, rather than just reduces the standard errors. Returning to the logic of the model in Section 3, if all people in a certain area believe there is more corruption, they may monitor more, reducing actual corruption levels. This will attenuate the raw correlation between beliefs and actual corruption unless one also controls for the overall average beliefs about corruption.

And, as discussed above, just because the unconditional $\text{Corr}(B, t) > 0$ does not mean that villagers have specific information about the project (i.e., $\text{Corr}(B, t) > 0$ does not imply that $\mu > \frac{1}{2}$).

To examine whether villagers have specific information about the road project *per se*, I estimate an alternative version of equation (6) that also controls as flexibly as possible for villagers' priors q:

$$\mathbf{P}(B_{vh} = j) = \Phi\left(\theta_j - \beta t_v - X'_{vh}\gamma - q'\delta\right) - \Phi\left(\theta_{j-1} - \beta t_v - X'_{vh}\gamma - q'\delta\right) \tag{7}$$

To capture as flexibly as possible the respondents' priors q, I include in q the respondents' answers to the corruption questions about subdistrict officials, the village head, and the village parliament (none of whom have any official role in the road project), as well as a variety of respondent-level control variables – age, gender, per-capita expenditure (predicted from assets), participation in social activities, and family relationships to government and project officials. (The role of these respondent-level variables in predicting perceptions will be discussed in more detail in Section 4.3 below.) As can be seen in columns (3) and (6) of Table 2, adding these many additional control variables reduces the standard errors but does not change the point estimates. This is despite the fact that, to take just one example, the correlation of respondents' perceptions of corruption involving the village head and corruption involving the road project is 0.4. Thus, despite the relatively high correlation of these perceptions of different types of corruption, the results suggest that villagers are actually able to distinguish between general levels of corruption in the village and corruption in the road project in particular.

To interpret the magnitudes of the estimated coefficients, consider the probit specification. The point estimate in column (6) suggests that a 10 percent increase in missing expenditures above the mean level – i.e. an increase of 0.024 from the mean level of 0.24 – would be associated with an increase in the probability the respondent reports any corruption in the project of 0.0030, or an increase of about 0.8 percent over the mean level of 0.36. Put another way, the "elasticity" of a respondent reporting any likelihood of corruption with respect to the missing expenditures variable is about 0.08. Calculating the marginal effects from the ordered probit specifications give results of similar magnitudes. While there is information about actual corruption levels in perceptions, the

magnitude of this information is weak.

An important question is whether this weak correlation is merely the result of measurement error in the missing expenditures measure, or actually reflects the fact that households have little information. Recall that to construct the missing expenditures measure, I used data from 10 core samples of each road, and between 3-5 price quotations for each type of materials used. To investigate the role of measurement error, for each road I randomly split these 10 core samples and 3-5 price quotations into two groups of 5 core samples and 1-3 price quotations each, and use these subsamples of measurements to construct two different estimates of missing expenditures for each village. I then repeat the regressions in Table 2 instrumenting for the measure of missing expenditure constructed using the first set of measurements with the measure of missing expenditure constructed using the second set of measurements. When I do this, I find that the implied "elasticity" increases only slightly, to about 0.085.[11] Thus, at least to the extent I can detect it here, measurement error alone does not seem to explain the low correlation between perceptions and reality.

4.2 Differential accuracy: prices vs. quantities

There are multiple methods village officials can use to hide corruption, and some of these methods may be easier for villagers to detect than others. In particular, village officials who steal a given amount have two options for how to account for this missing money in the accounts – they can either inflate the price paid for the materials procured, or they can inflate the quantities of the materials procured (or both). To examine how perceptions of corruption are formed, I re-estimate equation (6) with the missing expenditures variable separated into variables representing its constituent parts – "missing prices" and "missing quantities." Specifically, I define "missing prices" as the difference in logs between the prices reported by the village and the prices measured by the independent survey team, weighted by the quantities reported by the village; similarly, I define "missing quantities" as

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[11]In order to do this, I use an instrumental variables linear probability model. For comparison purposes, I first re-estimate equation (7) using a OLS linear probability model rather than the probit model used in the text, yielding an implied "elasticity" of 0.072 rather than the 0.08 from column (6) of Table 2. With the instrumental variables strategy described in the text, the "elasticity" increases to 0.085 from 0.072, suggesting that the impact of measurement error in the quantity and price samples is relatively slight. Of course, there may be other sources of measurement error, such as in reported expenditures, that are not captured in this methodology.

the difference in logs between the quantities reported by the village and the quantities measured by the independent survey team, weighted by the prices reported. Missing prices therefore captures markups in prices, while missing quantities captures markups in quantities.

The results are presented in Table 3. All specifications confirm that villagers' perceptions of corruption in the project are strongly positively correlated with price markups, and only very weakly (and statistically insignificantly) correlated with markups in quantities. The estimated magnitudes for missing prices are approximately double the magnitudes for missing expenditures overall. Market prices for commodities are commonly known to villagers, but quantities of commodities delivered are very difficult to estimate without careful measurement, even for trained engineers; therefore, it is not surprising that villagers are better at detecting marked-up prices than inflated quantities.

Given this result, it is interesting to compare the overall average levels of the missing prices and missing quantities variables. After all, if villagers can detect marked-up prices but cannot detect marked-up quantities, village officials would in general choose to hide their corruption by inflating quantities rather than marking up prices. As discussed above, one needs to interpret the levels of the missing expenditures variables with caution, because the levels of these variables depend on assumptions about the loss ratios and on the ability of surveyors to obtain exactly the same prices as the villages procuring the material for the project. Nevertheless, the levels of the missing prices and quantities variables are precisely what one would expect given the perceptions results: all of the missing expenditures are hidden by inflating quantities, not by inflating prices. Specifically, as shown in Table 1, the mean level of the missing quantities variable is 0.24, while the mean level of the missing prices variable is -0.014, very close to zero.[12] Thus, on average the vast majority of the missing expenditures appears to be occurring exactly where villagers cannot detect it. This raises the possibility that the relatively low correlation between beliefs and missing expenditures may in part reflect the strategic behavior of savvy corrupt officials who deliberately choose the types of corruption that are hardest to detect.[13] It also suggests that there may be limits in the degree to

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[12]Missing prices could be less than 0 if, for example, villages purchasing materials received bulk discounts on purchase prices that were not offered to the independent survey team.

[13]A natural question is how to reconcile the facts that 1) there appears to be no price-markups on average and 2) villagers are able to detect price-markups. The answer is that the fact that the average price-markup being 0 masks

which villagers can effectively monitor corruption, at least in the absence of external help detecting it.

4.3 Are individuals' beliefs systematically biased?

As discussed in Section 3.4, if beliefs are biased, these biases can alter the equilibrium level of corruption. If the extent of these biases vary across villages in systematic ways, they have the potential to make differences in beliefs about corruption a systematically unreliable measure of corruption differences and the determinants of corruption.

This section tests for whether certain types of individuals are systematically biased in their beliefs. To do this, I re-estimate a version of equation (7) that includes village fixed effects in addition to respondent-level variables. Since the actual level of corruption in the road project does not vary within the village – after all, there is only one road project in each village – if there are no individual biases, then once village fixed effects are included and once I benchmark for how respondents perceive the different possible answers to the corruption question, none of the individual characteristics in the regression should systematically predict corruption beliefs. If they do, then we know that those types of individuals described by the variable in question are systematically biased either towards finding or not finding corruption in the project.[14]

Given the incidental parameters problem, rather than estimate an ordered probit or probit model with a large number of dummy variables, I instead estimate a conditional logit model, which conditions out the village fixed effect. I therefore focus on the dummy variable version of the perceptions variable and estimate the conditional fixed effects logit equivalent of equation (7), with fixed effects for each village. In addition, to make the coefficients more easily interpretable, I also

the fact that some villages had higher-than-market prices, and others had lower-than-market prices. Villagers appear to detect these differences, and they are correlated with corruption perceptions. Perhaps the village officials in those villages where prices were marked-up did not realize that prices would be easier to detect than quantities.

[14] In interpreting these results, it is important to note that while I can estimate whether bias exists, I do not know which individuals are 'biased' and which are 'unbiased'. The reason is that the dependent variable, perceptions of corruption, does not have a numeric scale that we know should be comparable to the missing expenditures variable. Thus, unlike the literature evaluating subjective probabilities (e.g., Dominitz and Manski 1997, Hurd and McGarry 2002), I cannot say which individuals are right and which are wrong or whether the perceived level of corruption is "right on average"; rather, I can only say that conditional on the actual level of corruption, those with high education are more likely to report higher levels of corruption than those with low levels of education.

report the results from estimating an equivalent linear probability model with village fixed effects.[15]

The results are presented in Table 4. For each specification (conditional logit or linear probability model with fixed effects), I present two sets of results – one with no additional controls, and one controlling for perceptions about the President, to control for the fact that some respondents may have interpreted the multiple response categories differently from others.

Individual-level biases appear quite significant. Conditional on village fixed effects, better educated respondents and male respondents tend to report more corruption; those who participate in the types of social activity where the project was likely to be discussed, those who live near the project, and (naturally) those who are related to the head of the project all tend to report less corruption.[16] Taken together, these individual level biases are highly significant – the p-value from a joint test of these characteristics is less than 0.01 in all specifications.

Not only are these biases statistically significant, they are large in magnitude as well. For example, the results show that each year of education makes an individual between 0.7 and 0.9 percentage points more likely to report corruption in the project. This implies that, holding actual corruption levels constant, the "elasticity" of the probability of reporting any likelihood of corruption with respect to the respondent's education is between 0.21 and 0.27 — considerably larger than the impact of the actual missing expenditures variable discussed above.

The main conclusion from these results is that these individual biases are very substantial, especially when compared to the magnitude of the correlation between missing expenditures and beliefs about corruption found above. This suggests that the signal-to-noise ratio in reported beliefs is quite low, which may also help explain the low overall correlation between beliefs and missing expenditures.

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[15] I have also estimated a fixed-effects model on a linearized version of the categorical variable, where I assign a value of 0 to a response of 'none', 1 to a response of 'low', 2 to a response of 'medium', etc. The results are qualitatively similar to the results presented in Table 4.

[16] An interesting question is whether these individual characteristics lead to repsondents being more or less accurate at detecting corruption, not just biased. To examine this, I also interacted each of these individual characteristics with the missing expenditures variable. Across a wide range of specifications, I found no evidence of such interactions (results not reported).

4.4 Are aggregate biases substantial?

The previous section showed that certain types of individuals are systematically biased in their perceptions of corruption. For these biases to feed back to affect monitoring and, ultimately, corruption levels, these individual biases would have to be both large and correlated with village characteristics. If they are, then when examining the determinants of corruption – an exercise done quite frequently in the corruption literature (e.g., LaPorta 1999, Treisman 2000) – how corruption is measured may dramatically affect the results.

This section examines empirically whether aggregate biases are substantial enough to effect qualitative conclusions about the determinants of corruption. In doing this, it is important to note that I do not necessarily claim a causal interpretation of the relationship between these variables and corruption; rather, the main question of interest is the consistency of the partial correlations between these variables and corruption across the various ways of measuring corruption.

To examine whether perceptions can be reliably used as a proxy for missing expenditures, I estimate the following two regressions via OLS:

$$\widetilde{t}_v \quad = \quad \alpha_1 + Z_v' a_2 + \varepsilon_v \tag{8}$$

$$\widetilde{B}_{vh} \quad = \quad \beta_1 + Z_v' \beta_2 + + X_{vh}' \beta_3 + v_v' \tag{9}$$

and examine the the similarity or difference between the coefficients α_2 and β_2, which capture the impact of village characteristics Z on missing expenditures and perceived corruption, respectively. To obtain the most comparable possible coefficients across these very different measures, I normalize all of the corruption measures to have mean 0 and standard deviation 1, so that all coefficients can be interpreted in terms of standard deviation changes in the corruption measure. I denote the normalized versions of missing expenditures by $\widetilde{t}_v$ and the normalized version of beliefs by $\widetilde{B}_{vh}$.[17]

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[17] For the categorical-response perceptions variable, I impose a linear scale on the variable, and then normalize this linearized variable to have mean 0 and standard deviation 1. Although this imposes a linearized form on categorical response variable, as discussed in footnote 15 above, in other specifications OLS regressions using this linearized varaible produce qualitatively similar results to the ordered probit specifications, which suggests that the linear assumptions are not substantially affecting the results. I have also considered ordered probit and probit versions of equation (9), and they produce qualitatively similar results to those in Table 5 below. Similarly, for the binary dependent variable, I normalize the variable to have mean 0 and standard deviation 1.

That being said, I will focus primarily on those results for which the estimated coefficients α_2 and β_2 are of different sign, not just of different magnitude, so as not to rely too heavily on this normalization.[18]

The results are presented in Table 5. Column (1) presents the results when missing expenditures is the dependent variable, columns (2) and (3) present the result when the scaled linear version of perceptions is the dependent variable, and columns (4) and (5) present the results when the scaled dummy version of perceptions is the dependent variable. Columns (2) and (4) do not include the controls for corruption perceptions of the President, the village head, etc. or the respondent-level characteristics included in Table 4; columns (3) and (5) do.

The results suggest that for identifying the effects of basic demographic characteristics, such as population and education, the results from perceptions (columns 2-5) appear to give similar results to the more objective missing expenditures measure (column 1). But when considering characteristics related to trust – such as ethnic heterogeneity and social participation – examining the impact on corruption perceptions rather than actual corruption may lead to biased conclusions.

Of particular note are the estimates on ethnic heterogeneity. The cross-country corruption literature has found that heterogeneity is positively associated with corruption perceptions (e.g., Mauro 1995, LaPorta et al 1999). Following the standard approach in the literature, I construct as a measure of ethnic and religious heterogeneity the probability that two randomly drawn individuals are from different ethnic or religious groups, respectively.[19] Consistent with the literature, I find

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[18] An alternative, equivalent approach which does not rely on these normalizations is as follows. Suppose the true model of the world is:

$$t_v \quad = \quad \alpha_1 + Z_v'a_2 + \varepsilon_v \tag{10}$$
$$B_{vh} \quad = \quad \widetilde{\beta}_1 + Z_v'\widetilde{\beta}_2 + +X_{vh}'\widetilde{\beta}_3 + \widetilde{\beta}_4 t_v + \widetilde{v}_v' \tag{11}$$

The coefficient of interest is α_2, the effect of village characteristics Z on real corruption levels. In most circumstances, t is unobserved. If we assume that $\widetilde{\beta}_2 = 0$ (i.e., Z_v affects B_{vh} only through its effect on t_v), then the estimated coefficient β_2 in equation (9) in the text will be equal to α_2, the coefficient of interest. In the setting here, we also observe t, so we can estimate (11) directly and test whether $\widetilde{\beta}_2 = 0$ - i.e., we can test directly whether Z has no direct effect on perceptions other than through its effect on the percent missing. This is equivalent to testing whether $\alpha_2 = \beta_2$ in equations (8) and (9) in the text. Estimating (11) and testing whether $\widetilde{\beta}_2 = 0$ yields similar results to those presented in Table 5 and discussed below. In particular, the coefficients $\widetilde{\beta}_2$ in equation (11) are statistically significantly different from 0 for the mean village education level, ordinances from village parliament, social participation, and village ethnic fragmentation.

[19] Overall, the sample is relatively homogeneous – the probability that two individuals in the same village are from different ethnic groups is greater than 0.05 in only 9 percent of villages, and the probability that two individuals in the same village are from different religious groups is greater than 0.05 in only 10 percent of villages.

that ethnic heterogeneity is associated with greater perceived levels of corruption. Moving from a village with no ethnic heterogeneity to a village with the maximum ethnic heterogeneity in the sample (0.51) is associated with an increase of between 0.65 and 1 standard deviations in the perceived corruption measure, equivalent to an increase of about 50 percentage points in the probability of reporting positive corruption in the project. Moreover, controlling for the overall heterogeneity in the village, those respondents whose ethnic group differed from that of the village head were 12 percentage points more likely to report positive probability of corruption in the project. However, when I examine the relationship between ethnic heterogeneity and the missing expenditures measure, I get the opposite result – moving from a village with no ethnic heterogeneity to a village with the maximum ethnic heterogeneity in the sample is associated with a *decrease* in the percent missing variable of about 0.73 standard deviations.[20] The coefficients on religious fragmentation show a similar pattern – a large negative coefficient when missing expenditures is the dependent variable, and coefficients much closer to zero (and in some cases positive) when perceptions are the dependent variable – though the results on religious heterogeneity are not statistically significant.

One possible explanation for the difference in the coefficients on ethnic heterogeneity between perceptions and missing expenditures is that ethnic heterogeneity lowers the level of trust in the village. Greater suspicions could result in higher perceived levels of corruption, but could also result in greater monitoring and lower actual corruption, as suggested by the model in Section 3.

In fact, there is suggestive evidence consistent with this explanation. The household survey asked respondents a version of the World Values Survey trust question, in which respondents were asked about the degree to which they trust other residents of the village.[21] I find that the villagers in ethnically heterogeneous villages are less trusting of their fellow villagers than those in homogeneous villages; on average, 52% of residents in villages with ethnic heterogeneity less than 0.05 reported

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[20]It is worth noting that Glaeser and Saks (2006) also find that greater racial heterogeneity is associated with more corruption, even though they examine federal corruption convictions rather than perceptions as the measure of corruption. However, it is possible that federal corruption prosecutions could be confounded by the same types of biases, if racial heterogeneity leads to less trust and a greater propensity to demand federal government prosecutions.

[21]Specifically, they were asked: "In general, do you think that other residents of the village can be trusted, or you have to be careful in dealing with them?" The variable is coded 1 if the respondents say they can trust other residents of the village, and 0 if they say they have to be careful in dealing with them.

trusting their fellow villagers, whereas only 36% of residents in villages with ethnic heterogeneity greater than 0.05 reported trusting their fellow co-residents.[22]

Moreover, there is also evidence that ethnic heterogeneity is correlated with higher monitoring levels. As discussed above, the prime mechanism for local-level monitoring is the village accountability meeting, held three times during the course of the project. In high ethnic heterogeneity villages (defined similarly using the 0.05 threshold), the number of people who attended these accountability meetings was 22% higher than in villages with low ethnic heterogeneity.[23] This supports the story suggested by the model – lower levels of trust correlated with ethnic heterogeneity lead to more negative corruption perceptions, which in turn lead to higher levels of monitoring, lowering actual corruption levels.

A similar effect – though in the opposite direction – can be seen by looking at the social participation variables. I define the intensity of social participation as the average number of times an adult in the village participated in a social group of any kind in the past 3 months. This measure is obtained from a census of social groups obtained from the head of each hamlet. As can be seen in Table 5, increased participation in social groups in the village is associated with a decrease in perceived corruption levels. This is consistent with the results reported by Putnam (1993). But when we look at the actual corruption level, we find, if anything, that increased social participation is associated with higher measured corruption levels, though the point estimate is statistically insignificant. This suggests a similar story to the ethnic fragmentation story – more social participation may be associated with increased trust, which both decreases perceptions of corruption and creates an opportunity for corruption. Similar, though weaker, differences between the perceptions variable and missing expenditures appear when we consider a measure of whether there is a political opposition in the village that could potentially monitor the project – the degree of activeness of the village parliament, or BPD.[24]

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[22] This difference is statistically significant at the 5% level if no other village covariates are included. If I re-estimate a Probit version of equation (8) with this trust variable as the dependent variable and include other covariates, the coefficient attenuates slightly, and the p-value increases to 0.11.

[23] This differnce is statistically significant at the 1% level. The point estimates are virtually identical if the village-level characteristics included in Table 5 are included as well (except, obviously, ethnic heterogeneity). Using a linear ethnic heterogeneity measure, rather than the discrete cutoff for ethnic heterogeneity greater than 0.05, gives very similar results.

[24] To measure how active the BPD was, I examine the number of ordinances the BPD had issued in the previous

Finally, I examine how the experimental results reported in Olken (2005) would have differed had the perceptions-based measure been used to evaluate corruption instead of the missing expenditures measure. As described above, there were three experimental interventions in these villages – an audit treatment, in which villages were told in advance that they would be audited by the central government audit agency with probability 1, an invitations treatment, where hundreds of written invitations were passed out to villagers to attend accountability meetings, and an anonymous comment form treatment, where villagers were able to give comments about the project without fear of retaliation. As can be seen in column (1), the audit intervention was associated with a statistically significant reduction in missing expenditures of about 0.3 standard deviations, whereas the invitations and invitations plus comment forms treatments were associated with a very small, and statistically insignificant, reduction in the missing expenditures variable. By contrast, when examining the perceptions variable, the audit treatment has a much smaller (and in most specifications not statistically significant) effect, and the invitations and invitations plus comment form treatments are associated with increases in the perceptions of corruption, in some cases statistically significantly so. One reason for the difference in the audits results is that, as reported in Olken (2005), the audits primarily resulted in a reduction in missing quantities, whereas the results in Table 3 show that villagers are better at detecting missing prices. Also, one can also easily imagine that anonymous comment forms would increase people's beliefs about corruption by providing information about corruption, while in fact having the opposite effect on actual corruption levels.

5   Conclusion

This paper has examined the relationship between perceptions of corruption and a more objective measure of corruption, in the context of a road building program in rural Indonesia. After having developed a theoretical model of corruption perceptions, I showed empirically that villagers' perceptions of corruption do appear to be positively (though weakly) correlated with the more objective

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year. Though the BPD coefficients are not separately significant in either the missing expenditures or perceptions regressions, the difference between them appears to be statistically significant using the method discussed in footnote 18. I also consider another measure of transparency in the project – whether a written report of project expenses was provided at a village accountability meeting – though the results are not conclusive in either direction.

missing expenditures measure. Moreover, villagers appear to be able to distinguish between the overall probability of corruption in the village and corruption specific to the road project.

Despite this, the magnitude of the correlation between beliefs and missing expenditures is small. In part, this may be because, on average, almost all of the corruption in the project was hidden by inflating quantities, which are hard for villagers to detect, rather than marking up prices, which are easier for villagers to detect. This suggests an important feedback mechanism between transparency – which increases the ability of citizens to detect corruption – and corruption levels. It also suggests that, at least in this case, villagers do not currently possess enough capability to detect corruption to effectively monitor local officials, at least without additional external help.

I then examine the extent of biases in corruption perceptions. I show that there are significant individual-level biases in how respondents answer the corruption question. Moreover, I present evidence that for some village level characteristics, particularly those associated with levels of trust, such as ethnic heterogeneity and social participation, using perceptions to measure corruption can produce very different answers from the results obtained using a more objective measure of corruption. I present suggestive evidence in favor of the idea developed in the model that biases in individual's views about corruption can lead to increased monitoring behavior, which in turn reduces corruption. These results suggest that perceptions data should be used for empirical research on the determinants of corruption with considerable caution, and that there is little alternative to continuing to collect more objective measures of corruption, difficult though that may be.

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Appendix

A Proofs

Proof of PBE equilibrium (equation 3). The first step is to show that the equilibrium in (3) is a PBE. I will consider first the case where $\mu < 1$. First, consider the village head. Given the monitoring probabilities $\left\{m_h = \frac{c}{\mu(F+c)}, m_l = 0\right\}$, we need to show that the village head is indifferent between stealing and not stealing. If he does not attempt to steal, he receives a payoff of 0. If he attempts to steal, with probability $(1-\mu) + \mu(1-m_h)$ he is not monitored and receives payoff c and with probability μm_h he is monitored and receives payoff $-F$. The village head's payoff is therefore

$$c\left((1-\mu) + \mu\left(1 - \frac{c}{\mu(F+c)}\right)\right) - F\left(\mu\frac{c}{\mu(F+c)}\right) = 0 \tag{A-1}$$

So the village head is indifferent, and therefore does not deviate. Note that this is an equilibrium only if $0 \leq m_h = \frac{c}{\mu(F+c)} \leq 1$, so that m_h is a valid probability. This will be the case if $c \leq \mu(F+c)$, which is guaranteed by the assumptions that $F > c$ and $\mu \geq \frac{1}{2}$.

Next, to show that the villager is indifferent, consider first the case when the villager receives the high signal. If he monitors , he will receive payoff $-M$ for sure. If he does not monitor, he will receive payoff

$$
\begin{aligned}
-\alpha c \mathbf{P}(t=1|h) &= -\alpha c \frac{q\mu}{q\mu + (1-q)(1-\mu)} \tag{A-2}\\
&= -\alpha c \frac{\frac{1-\mu}{\frac{\alpha c}{M}\mu + (1-2\mu)}\mu}{\frac{1-\mu}{\frac{\alpha c}{M}\mu + (1-2\mu)}\mu + \left(1 - \frac{1-\mu}{\frac{\alpha c}{M}\mu + (1-2\mu)}\right)(1-\mu)}\\
&= -M
\end{aligned}
$$

The villager is therefore indifferent between monitors and not monitors if he receives the high signal. If he receives the low signal, rational beliefs ensure that his posterior belief about the probability of corruption is lower, and therefore if he was indifferent between monitoring and not monitoring if he received the high signal he will strictly prefer not to monitor if he receives the low signal. Note that the fact that the signal quality $\mu < 1$ implies that receiving both the high and low signals are on the equilibrium path, so beliefs must be rational. This shows that the equilibrium in (3) is in fact a PBE.

To show that this equilibrium is unique, first note that if we rewrite the indifference conditions (A-1) and (A-2) in terms of unknowns m_h and q, respectively, then the solutions for m_h and q are unique. This guarantees the uniqueness of the mixed-strategy equilibrium in (3) among possible mixed strategy equilibria. The argument for why there are no pure-strategy equilibria is given in the text, which shows the uniqueness of the equilibrium. ∎

Proof that equilibrium probability of monitoring is decreasing in μ. The equilibrium monitoring probability can be written as:

$$q(\mu m_h + (1-\mu) m_l) + (1-q)((1-\mu) m_h + \mu m_l)$$

Substituting in the equilibrium levels of q, m_h, and m_l from equation (3) and simplifying, this

expression is equal to:

$$\frac{\alpha\,(1-\mu)\,c^2}{(\alpha c\mu + M - 2\mu M)\,(F+c)}$$

The derivative of this expression with respect to μ is

$$\frac{-\alpha c^2\,(\alpha c - M)}{(\alpha c\mu + M - 2\mu M)^2\,(F+c)} \tag{A-3}$$

which is less than 0. $\blacksquare$

Proof of correlation between perceptions and reality (equation 4). By definition,

$$\mathrm{Corr}\,(B, t \mid q) = \frac{\mathbf{E}\,(Bt) - \mathbf{E}\,(B)\,\mathbf{E}\,(t)}{\sqrt{\mathrm{Var}(B)\,\mathrm{Var}\,(t)}}.$$

Since t is a Bernoulli with probability q, $\mathbf{E}\,(t) = q$ and $\mathrm{Var}\,(t) = q - q^2$. Since beliefs are correct on average, $\mathbf{E}\,(B) = q$. For $\mathbf{E}\,(Bt)$,

$$
\begin{aligned}
\mathbf{E}\,(Bt) &= q\,(\mu\mathbf{P}\,(t=1|h) + (1-\mu)\,\mathbf{P}\,(t=1|l)) \\
&= q\left(\mu\frac{q\mu}{q\mu + (1-q)\,(1-\mu)} + (1-\mu)\,\frac{q\,(1-\mu)}{q\,(1-\mu) + (1-q)\,\mu}\right) \\
&= q^2\frac{4q\mu^2 - 4q\mu - 1 + 3\mu + q - 3\mu^2}{(2q\mu + 1 - \mu - q)\,(-q + 2q\mu - \mu)}
\end{aligned}
$$

Finally,

$$
\begin{aligned}
\mathbf{E}\,(B^2) &= q\left(\mu\mathbf{P}\,(t=1|h)^2 + (1-\mu)\,\mathbf{P}\,(t=1|l)^2\right) + (1-q)\left(\mu\mathbf{P}\,(t=1|l)^2 + (1-\mu)\,\mathbf{P}\,(t=1|h)^2\right) \\
&= q\left(\mu\left(\frac{q\mu}{q\mu + (1-q)\,(1-\mu)}\right)^2 + (1-\mu)\left(\frac{q\,(1-\mu)}{q\,(1-\mu) + (1-q)\,\mu}\right)^2\right) \\
&\quad + (1-q)\left(\mu\left(\frac{q\,(1-\mu)}{q\,(1-\mu) + (1-q)\,\mu}\right)^2 + (1-\mu)\left(\frac{q\mu}{q\mu + (1-q)\,(1-\mu)}\right)^2\right) \\
&= q^2\frac{4q\mu^2 - 4q\mu - 1 + 3\mu + q - 3\mu^2}{(2q\mu + 1 - \mu - q)\,(-q + 2q\mu - \mu)}
\end{aligned}
$$

Combining terms,

$$
\begin{aligned}
\mathrm{Corr}\,(B, t \mid q) &= \frac{\mathbf{E}\,(Bt) - \mathbf{E}\,(B)\,\mathbf{E}\,(t)}{\sqrt{\left(\mathbf{E}\,(B^2) - \mathbf{E}\,(B)^2\right)\mathrm{Var}\,(t)}} \\[2mm]
&= \frac{q^2\frac{4q\mu^2 - 4q\mu - 1 + 3\mu + q - 3\mu^2}{(2q\mu + 1 - \mu - q)(-q + 2q\mu - \mu)} - q^2}{\sqrt{\left(q^2\frac{4q\mu^2 - 4q\mu - 1 + 3\mu + q - 3\mu^2}{(2q\mu + 1 - \mu - q)(-q + 2q\mu - \mu)} - q^2\right)(q - q^2)}} \\[2mm]
&= \frac{\sqrt{q}\,(q + 2\mu - 2q\mu - 1)}{\sqrt{(1 - q)\,(2q\mu + 1 - \mu - q)\,(q + \mu - 2q\mu)}}
\end{aligned}
$$

To show that equation (4) is well defined, recall from equation (3) that $q = \frac{1-\mu}{\frac{\alpha c}{M}\mu + (1 - 2\mu)}$ in equilibrium. Substituting and simplifying yields

$$\mathrm{Corr}\,(B, t \mid M, \alpha, c, \mu) = \frac{\sqrt{M}\,(M + 2\alpha c\mu - \alpha c - 2\mu M)}{\sqrt{\alpha c\,(\alpha c - M)\,(-2\mu M + M + \alpha c\mu^2)}} \tag{A-4}$$

We first need to check that the term under the radical in the denominator is positive. First, note that $\alpha c > M$ by assumption. To show $-2\mu M + M + \alpha c\mu^2 > 0$ it is sufficient to show that

$-2\mu M + M + M\mu^2 > 0$, i.e. that $1 - 2\mu + \mu^2 = (1 - \mu)^2 > 0$. To show that (A-4) is positive, we need to show that $(M + 2\alpha c\mu - \alpha c - 2\mu M) > 0$. This expression can be re-written as $(\alpha c - M)(2\mu - 1)$, which is non-negative if $\mu \geq \frac{1}{2}$ and strictly positive if $\mu > \frac{1}{2}$. $\blacksquare$

Proof that $\frac{\partial \operatorname{Corr}(B,s|q)}{\partial \mu} > 0$.

$$\frac{\partial \operatorname{Corr}(B, t \mid q)}{\partial \mu} = \frac{(1 - q)\sqrt{q}}{2\sqrt{((1 - q)(2q\mu + 1 - \mu - q)(q + \mu - 2q\mu))}(2q\mu + 1 - \mu - q)(q + \mu - 2q\mu)} \tag{A-5}$$

Note that $q + \mu - 2q\mu > 0$, since $\mu < 1$ and $q < 1$. Note also that $2q\mu + 1 - \mu - q = 1 - q(1 - \mu) - \mu(1 - q) > 0$, since $\mu \geq \frac{1}{2}$ and $q \geq \frac{1}{2}$ implies that $\mu(1 - q) \leq \frac{1}{2}$ and $q(1 - \mu) \leq \frac{1}{2}$, with the inequality strict whenever $\mu > \frac{1}{2}$. Therefore $\frac{\partial \operatorname{Corr}(B,t|q)}{\partial \mu} > 0$. $\blacksquare$

Proof of equilibrium with perception bias ε **(equation 5).** To show that equation (5) is an equilibrium, first note that the village head's problem does not change – since $\left\{ m_h = \frac{c}{\mu(F + c)}, m_l = 0 \right\}$, by the same analysis as above the village head is indifferent between attempting to steal and not attempting to steal. For the villager, if he receives the high signal he will be indifferent between monitoring and not monitoring if

$$\alpha c \frac{(q + \varepsilon)\mu}{(q + \varepsilon)\mu + (1 - q - \varepsilon)(1 - \mu)} = M \tag{A-6}$$

Solving for q yields the expression in (5).Since in the PBE, the villager strictly preferred not monitoring if he received the low signal, if ε is small enough, $q + \varepsilon$ is close enough to q that he will still strictly prefer not monitoring if he receives the low signal. This shows that (5) is an equilibrium. $\blacksquare$

B Data Details

The original data was collected in 608 total villages. The sample in this paper, however, is limited to the 477 villages where the missing expenditures variable, described above, could be constructed. The missing expenditures variable could not be calculated in some villages for one of four reasons: (1) surveyor error in locating the road, (2) the project consisted largely of a partial rehabilitation of an existing road, (3) agglomerated expenditures reports (i.e., the village expenditure report combined expenditures in the road project with other projects that could not be independently measured, such as a school), or (4) villages that had asphalted the road that refused to let the engineers break the asphalt to conduct the engineering survey.

The household survey was designed as a stratified random sample, containing between six and thirteen respondents per village, selected as follows. Two respondents were selected from the hamlets in which the road was located by first randomly selecting a hamlet, and then randomly selecting a neighborhood (RT) in that hamlet. A complete list of households in the RT was obtained from the neighborhood head, and two households were drawn randomly from that list. Individual respondents were drawn from the a list of all adults age 18 or over in the selected households. Two additional respondents were selected from the hamlets in which the road was not being built using the same procedure. As men in the village tend to participate much more in road construction activities, the randomization was designed such that, of the four respondents selected in this manner, three were men and one was a women. In villages receiving the Comment Form treatment, an additional four respondents were drawn using the same procedure, two from hamlets with the project and two from hamlets that did not contain the project. Two respondents

were drawn randomly from the attendance list of Village Meeting II, which was held before the randomization was announced, and is therefore exogenous with respect to the experiments. Finally, in some Comment Form villages an additional 3 respondents were added, randomly selected from the two neighborhoods above (the reasons for this will be discussed below). Each respondent received compensation of Rp. 10,000 ($1.20), equal to slightly more than half of the typical daily agricultural wage in the study area.

Given this sample selection, a natural question is whether the sample should be re-weighted to reflect the fact that different respondents had different probabilities of being sampled. As is apparent from the description of the sampling, women were systematically undersampled, and those who attended a pre-randomization village meeting were systematically oversampled. In all specifications, I control for how the respondent was sampled (i.e., whether the respondent was from a hamlet with or without the road project, whether the respondent was selected from the attendance list at Village Meeting II, and whether the household was one of the 3 additional households added in the Comment Form villages). The question is whether, given these control for level differences among these samples, one needs to reweight to account for treatment effect heterogeneity in the relationship between missing expenditures and perceptions. As discussed by Deaton (1995), weighting the sample makes the point estimates invariant to survey design, but reduces the effective power and, in the presence of treatment heterogeneity, does necessarily obtain consistent estimates for the true average population effect. Accordingly, the results presented below in the text are unweighted. Using sample weights in the regressions that account for the differential probability of sampling and which weight each village equally (i.e., so that comment form villages are not over-weighted in the regression) does not substantively change the results, but not surprisingly it does reduce the statistical significance of the missing expenditures variable in columns (2) and (3) of Table 2.

Beyond simply measuring perceptions, an additional goal of the survey was to measure how stated beliefs about corruption change when respondents know that their answers will be used for monitoring. To examine this, after all corruption questions except for questions involving corruption in the road project had been asked, a randomly selected subset of respondents in the Comment Form villages were told that their responses to the questions about corruption in the project would be used, anonymously, as part of the overall report on the comment forms presented at the accountability meeting. To simplify exposition, I will refer to this variant of the questionnaire as Form B, and to the normal questionnaire, in which all questions were explained to be anonymous and to be used for research purposes only, as Form A. Due to a training error, approximately 60% of enumerators appear to have given Form B surveys to all households in Comment Form villages. Therefore, in approximately half of all Comment Form villages, three additional households were surveyed, drawn randomly from the same neighborhoods as before, two of whom received Form A and one of whom received Form B. Although the Form B version of the form is similar to Form A (the coefficient on receiving a Form B form is always very close to 0 and statistically insignificant), I nevertheless include a dummy variable for which version of the form each household received in all specifications, as well as dummies for whether the household was sampled as part of this additional three households per village, in addition for dummies for which experimental treatment the village was assigned (i.e. comment forms, invitations, or audits). Although I include these dummies in all specifications in this paper, doing so does not substantially affect the results.

In addition to the corruption question, the household survey included a wide variety of other covariates, such as a household roster, education levels, participation in social activities and in the

road project, assets, and family relationships to various village officials. To estimate household expenditure of respondents, I used the 1999 SSD (Hundred Villages Survey), an Indonesian statistics bureau dataset, containing 3,193 rural Javanese households. The SSD asked both a detailed expenditure questionnaire and the same set of asset questions used in my household survey. In the SSD, I used OLS to estimate the relationship between log household expenditure and the following variables, all of which I observe in my survey: log household size, whether the household was headed by a woman, the percentage of household members consisting of children ages 0-3, 4-6, 7-9, 10-12, and 13-16, dummies for whether the household has a stove, refrigerator, radio, television, satellite dish, motorbike, car, and electricity, dummies for floor type, wall type, and ceiling type, the total amount of land held by the household, whether the household consumes meat at least once a week, whether each household member has at least two sets of clothes, whether the household uses modern medicine when a child is sick. I then used the estimated coefficients from the SSD to predict household expenditure in my survey. Combined, these 34 variables have an R-squared of 0.58 predicting log household expenditure in the SSD, which suggests that predicted expenditure is a reasonable approximation for actual expenditure, at least for the purposes used here.

Table 1: Summary Statistics

Perceived corruption involving:	Road Project	President	Village Head
None	64.1%	13.9%	47.1%
Low	21.1%	12.8%	18.0%
Medium	5.3%	22.9%	9.5%
High	0.4%	9.2%	1.8%
Very high	0.2%	3.4%	0.3%
Refused to answer	8.9%	37.7%	23.3%
Missing expenditures	0.236		
	(0.343)		
Missing prices	-0.014		
	(0.210)		
Missing quantities	0.243		
	(0.320)		

Notes: For perceived corruption, the figures given are percentage responses to the question "In general, what is your opinion of the likelihood of corruption / KKN (corruption, collusion, nepotism) involving […]?" where […] is the President of Indonesia (Megawati Sukarnoputri), the village head in the respondent's village, or the road project, as indicated in the columns. Standard deviations in parentheses Sample is limited to those villages where the missing expenditures variable is not missing. Total number of observations: 3,691. For missing expenditures, missing prices, and missing quantities, total number of observations: 477.

Table 2: Relationship Between Perceptions and Missing Expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
	Likelihood of Corruption in Road Project (Ordered Probit)			Any Likelihood of Corruption in Road Project (Dummy variable 0-1, Probit Marginal Effects)		
Missing expenditures	**0.186**	**0.280***	**0.307****	**0.097**	**0.119****	**0.123*****
	(0.175)	**(0.167)**	**(0.135)**	**(0.060)**	**(0.057)**	**(0.047)**
Corruption perceptions of:						
President	No	Yes	Yes	No	Yes	Yes
Subdistrict official	No	No	Yes	No	No	Yes
Village head	No	No	Yes	No	No	Yes
Village parliament	No	No	Yes	No	No	Yes
Respondent Covariates	No	No	Yes	No	No	Yes
Sample Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3314	3314	2931	3639	3639	3226
Mean dep. Var				0.36	0.36	0.35

Notes: Robust standard errors in parentheses, clustered at the subdistrict level. In columns (1) – (3), dependent variable is the categorical responses to the perceptions question, i.e., 'none', 'low', 'medium', 'high' and 'very high' (in that order). In columns (4) – (6), dependent variable is a dummy that takes value 0 if answer was 'none' and 1 if answer was 'low', 'medium', 'high', 'very high', or if the respondent refused to answer. Corruption perceptions of President, Subdistrict Official, Village head, and Village Parliament are dummies for respondent's perceived corruption levels of the respective officials. Respondent covariates are age, education, gender, predicted per-capita expenditure, participation in social activities, relationship to government and project officials. Sample controls are dummies for the three experimental interventions (audit, invitations, and invitations + comment forms), dummies for the different strata of respondents sampled, and a dummy for which version of the form the respondent received.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: Accuracy – Prices vs. Quantities

	(1)	(2)	(3)	(4)	(5)	(6)
	Likelihood of Corruption in Road Project (Ordered Probit)			Any Likelihood of Corruption in Road Project (Dummy variable 0-1, Probit Marginal Effects)		
Missing expenditures – prices	**0.433**	**0.627****	**0.669*****	**0.177***	**0.205****	**0.204****
	(0.277)	**(0.270)**	**(0.251)**	**(0.096)**	**(0.091)**	**(0.081)**
Missing expenditures – quantities	**0.057**	**0.118**	**0.112**	**0.049**	**0.069**	**0.070**
	(0.183)	**(0.177)**	**(0.155)**	**(0.062)**	**(0.060)**	**(0.053)**
Corruption perceptions of:						
President	No	Yes	Yes	No	Yes	Yes
Subdistrict official	No	No	Yes	No	No	Yes
Village head	No	No	Yes	No	No	Yes
Village parliament	No	No	Yes	No	No	Yes
Respondent Covariates	No	No	Yes	No	No	Yes
Sample Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3314	3314	2931	3639	3639	3226
Mean dep. Var				0.36	0.36	0.35

Notes: See Notes to Table 2.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Are beliefs systematically biased?

	(1)	(2)	(3)	(4)
	Any Likelihood of Corruption in Road Project (Conditional Logit)		Any Likelihood of Corruption in Road Project (Dummy variable 0-1, OLS with fixed effects)	
Education (years)	0.065***	0.051***	0.009***	0.007***
	(0.018)	(0.019)	(0.003)	(0.002)
Age	-0.003	-0.001	-0.000	-0.000
	(0.005)	(0.005)	(0.001)	(0.001)
Female	-0.183*	-0.160	-0.026*	-0.022
	(0.105)	(0.108)	(0.015)	(0.015)
Predicted per-capita consumption	0.217	0.148	0.028	0.021
	(0.193)	(0.199)	(0.025)	(0.025)
Participation in social activities	0.013**	0.011*	0.002**	0.002**
	(0.006)	(0.006)	(0.001)	(0.001)
Participation in social activities where road project likely discussed	-0.075***	-0.069***	-0.011***	-0.010***
	(0.021)	(0.021)	(0.003)	(0.003)
Lives in project hamlet	-0.781***	-0.764***	-0.110***	-0.106***
	(0.108)	(0.110)	(0.014)	(0.013)
Attended development meeting	-0.312***	-0.320***	-0.042***	-0.042***
	(0.110)	(0.112)	(0.014)	(0.014)
Family member of village government	0.043	0.021	0.003	0.001
	(0.112)	(0.116)	(0.016)	(0.016)
Family member of project leader	-0.399**	-0.402**	-0.051**	-0.051**
	(0.203)	(0.205)	(0.026)	(0.025)
Sample controls	Yes	Yes	Yes	Yes
President corruption perception	No	Yes	No	Yes
Observations	2675	2675	4084	4084
R-squared			0.47	0.48
Mean dep. var	0.40	0.40	0.34	0.34
Fixed effects	Village	Village	Village	Village
P-value of joint F-test	<0.01	<0.01	<0.01	<0.01

See Notes to Table 2. Village head corruption perception and president corruption refer to dummies for the respondent's response to the corruption question about village head and President of Indonesia, respectively, as in Table 2. Robust standard errors in parentheses. All specifications include village fixed effects. Note that the sample size is lower in the conditional logit specification since all villages where there is no variation in the dependent variable are automatically dropped from the conditional logit model.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Village Level Differences

	(1)	(2)	(3)	(4)	(5)
	Missing Expenditures	Likelihood of Corruption in Road Project (Linear scale, Std Dev 1)		Any Likelihood of Corruption in Road Project (Dummy variable, Std Dev 1)	
Demographics:					
Log population	0.262**	0.174***	0.128**	0.172***	0.118**
	(0.111)	(0.061)	(0.055)	(0.058)	(0.051)
Mean village education level (years)	-0.039	-0.050	-0.020	-0.047	-0.006
	(0.046)	(0.032)	(0.031)	(0.034)	(0.033)
Share of population poor	-0.334	-0.146	-0.124	-0.118	-0.065
	(0.252)	(0.164)	(0.133)	(0.159)	(0.140)
Social characteristics:					
Ethnic fragmentation	-1.443**	1.733***	1.303***	1.942***	1.477***
	(0.567)	(0.322)	(0.292)	(0.340)	(0.332)
Religious fragmentation	-1.359	0.069	-0.324	0.076	-0.309
	(1.088)	(0.732)	(0.704)	(0.720)	(0.700)
Intensity of social participation	0.024	-0.053*	-0.041	-0.072**	-0.062**
	(0.064)	(0.029)	(0.025)	(0.029)	(0.026)
Transparency:					
Meetings with written accountability report	-0.243	-0.017	-0.005	-0.072	-0.079
	(0.154)	(0.093)	(0.074)	(0.088)	(0.076)
Number of ordinances from village parliament	-0.019	0.008	0.015	0.012	0.015
	(0.017)	(0.012)	(0.009)	(0.011)	(0.010)
Experimental interventions:					
Audit treatment	-0.303**	-0.057	-0.126*	-0.033	-0.068
	(0.130)	(0.087)	(0.069)	(0.087)	(0.071)
Invitations treatment	-0.032	0.020	0.024	-0.012	0.019
	(0.106)	(0.077)	(0.068)	(0.069)	(0.064)
Invitations + comment treatment	-0.021	0.161*	0.120	0.107	0.095
	(0.094)	(0.088)	(0.073)	(0.087)	(0.077)
Corruption perceptions of:					
President	N/A	No	Yes	No	Yes
Subdistrict official	N/A	No	Yes	No	Yes
Village head	N/A	No	Yes	No	Yes
Village parliament	N/A	No	Yes	No	Yes
Respondent Covariates	N/A	No	Yes	No	Yes
Sample Controls	N/A	Yes	Yes	Yes	Yes
Observations	445	3074	2731	3384	3011
R-squared	0.06	0.06	0.22	0.06	0.16

Dependent variable in column (1) is missing expenditures; dependent variable in columns (2) and (3) is the linearized variable of corruption perceptions described in the text, and dependent variables in columns (4) and (5) are dummy variables of corruption perceptions. Note that all dependent variables have been rescaled to have mean 0 and standard deviation 1. Observations in columns (2) – (5) are weighted by the inverse of the number of observations in each village, to ensure that each village receives the same weight as in column (1). Estimation is by OLS, though as discussed in the text, estimation of columns (2) and (3) by ordered probit and columns (4) and (5) by probit produce qualitatively similar results. Sample controls are as defined in the notes to Table 2; household controls are all of the individual respondent-level variables considered in Table 4; village head and President Corruption perception refer to dummies for how the respondent answered the corruption questions about the village head and President of Indonesia, respectively, as included in column (3) of Table 2. Robust standard errors in parentheses, adjusted for clustering at subdistrict level.
* significant at 10%; ** significant at 5%; *** significant at 1%