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Distributional Effects of Corruption When Enforcement is Biased: Theory and Evidence from Bribery in Schools in Bangladesh

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In many models of corruption where enforcement is unbiased, the rich are more likely to pay bribes for their children's education, implying that corruption reduces educational inequality. We develop models of bribery that reflect the fact that in developing countries, anticorruption enforcement is not unbiased, and higher income of a household is associated with higher bargaining power and better quality of institutions. In models of biased enforcement, the rich are less likely to pay bribes, making bribery regressive. The OLS estimates of the effects of household income are likely to find spurious progressivity in the incidence of bribery in schools. We exploit temporary rainfall shocks to provide suggestive evidence on the ability-to-pay effect, while long-term rainfall differences capture the combined 'poor people' and 'poor area' effects. We find that the poor are more likely to pay bribes, and the amount paid does not depend on household income. The evidence rejects the ability-to-pay and related models based on unbiased enforcement, and is consistent with the 'refusal-to-pay model' of bargaining power where the rich decline to pay bribes. 'Free schooling' is free only for the rich, and corruption makes the playing field skewed against the poor.

Introduction

Over the last few decades, a large number of developing countries invested heavily in policies such as free universal primary and secondary schooling, stipends for girls, free books, and midday meals. The basic assumption is that such policies would lessen the burden on poor families for educating their children, and thus help to reduce educational inequality and improve economic mobility. The evidence, however, shows that inequality has increased in many developing countries, and educational mobility has not improved (World Bank 2006; Hertz *et al*. 2008; Emran and Shilpi 2015).

The goal of this paper is to understand whether corruption in schools constitutes part of the explanation for the lack of improvements in educational mobility despite public policies aimed at improving access for children from disadvantaged socioeconomic backgrounds. According to Transparency International Bangladesh, about 50% of households in Bangladesh report paying some form of bribe for children's education. Evidence from a seven-country study in Africa by the World Bank shows that 44% of parents had to pay illegal fees to send their children to school (World Bank 2010).[1] Our focus is on the question: who are the unfortunate half that end up paying bribes for their children's schooling? A canonical ability-to-pay model provides us with a sharp answer: the richer households are more likely to pay bribes, and they also pay more among the subset of bribe payers. If the ability-to-pay model is a valid description of the bribery in schools, then corruption helps to reduce educational inequality: only the rich pay for their children's schooling.[2] The available empirical evidence, in contrast, is conflicting: some shows that corruption is regressive, and other evidence suggests progressive incidence of corruption (see the discussion in Section I).

We make both theoretical and empirical contributions to this literature. The ability-to-pay and related screening models rely on an important assumption that law enforcement is impersonal and unbiased, thus a household's socioeconomic status is irrelevant for anticorruption enforcement. We develop two models where the legal enforcement system is not impersonal or unbiased, but works in favour of the rich to reflect the fact that higher permanent income (wealth) confers significant social and political influence in a developing country. The higher bargaining power of the richer households may allow them to avoid paying bribes altogether, making bribery regressive. The bargaining power that derives from higher permanent income of a household is modelled as a higher probability of punishment faced by an official when asking for bribes.

The models differ in terms of the information set of the official. In the first model, the standard (but heroic) assumption that the official observes all of the household characteristics relevant for extracting the full surplus is abandoned, but the information set is still rich enough so that the official observes income at the household level. The official infers the bargaining power from the observed income at the household level, and the bargaining power of a household works primarily as a deterrent against demand for bribes; richer households are less likely to face such demand for (and pay) bribes. In the second model, the information set is more limited; the official does not have income information to discriminate among households and demands the same amount of bribe from everyone. A household with high bargaining power can refuse to pay and still get a child admitted into school. The bargaining power of richer households thus leads to refusal to pay bribes in this limited information model (henceforth called the 'refusal-to-pay model'), and delivers the prediction that propensity to pay bribes is a *negative* function of household income, but among those who pay, the amount of bribe *does not* vary with household income. The distinguishing feature of 'bargaining power as deterrence' is that among the households paying bribes, the amount paid *increases* with a household's income. The theoretical analysis in this paper thus yields contrasting predictions regarding the effects of household income on bribery.

The OLS estimates of the effects of household income on propensity to bribe, and the amount paid conditional on bribing, are biased upwards due to genetic correlations between parents and children in cognitive ability and moral preference. The bias is reinforced by measurement error when the true effect is negative (regressive), as it also causes *positive* bias (towards zero) in the OLS estimates (Pischke 2007). Endogenous formation of moral preference acts as a multiplier of the upward bias caused by omitted heterogeneity. The OLS estimates are thus susceptible to finding spurious progressivity in the incidence of bribery in schools on account of both unobserved heterogeneity and measurement error when the true effect is regressive. This also implies that if we find a negative or insignificant effect of income, then the OLS estimates lead to correct conclusions regarding the distributional consequences of bribery in schools.[3] A related important insight is that it is incorrect to interpret the OLS estimate of the coefficient on household income as an ability-to-pay effect, as is common in the literature, because it also captures the bargaining power effect and the quality of institutions effect.

In order to provide suggestive evidence on the omitted variables bias in the OLS estimates, our empirical strategy relies on the observation that rainfall is an important exogenous determinant of rural income, but short-term rainfall shocks and long-term average rainfall variations across villages contain different identifying information. To test the ability-to-pay model, we focus on the effects of transitory rainfall shocks to income. If ability-to-pay is the primary mechanism at work, then positive transitory shocks to income would increase both the probability that a household pays bribes and the amount paid. Transitory rainfall shocks are unlikely to have any significant correlation with the genetic components of preference and ability, or with a household's bargaining power and the enforcement regime in a village, because they are determined by permanent income. One might, however, worry that transitory rainfall shocks may affect bribery through channels different from ability-to-pay. For example, school administrators may ask for money from parents to rebuild a school's infrastructure damaged by floods due to unusually heavy rainfall.[4] Since it is not possible to identify all potential channels through which transitory rainfall shocks might affect bribery, we provide estimates of the effects of income allowing for a direct effect of rainfall on bribery using the recent approach developed by Conley *et al.* (2012). This approach, however, does not provide point identification, and yields bounds on the causal effect of interest.

To estimate the effects of poverty on propensity to pay bribes and the amount paid, we exploit the variation in long-run average rainfall across villages, and its interaction with exogenous household characteristics. It is important to emphasize that we are not estimating the effects of permanent income in the standard sense, because variation in long-term average rainfall across villages is useful for identifying the combined 'poor people' and 'poor area' effects that are the focus of this paper. It captures the 'poor people' effect because it affects permanent income, for example, through agricultural productivity, crop choices and cropping intensity. Part of the 'poor people' effect may also be due to endogenous preference and ability formation, shaped by poverty as emphasized in the recent literature (Corbin and Heckman 2016; Currie and Almond 2011; Mullainathan and Shafir 2013). The long-term rainfall differences capture the 'poor area' effect because of the quality of institutions, including law enforcement, as they can affect the reach of formal legal apparatus, and may have shaped the informal (relational) arrangements in a village. The poor may be doubly vulnerable: they have lower bargaining power, and they also face weak enforcement against corruption. The upshot of the above discussion is that the effect of poverty on bargaining power of a household is an amalgam of different mechanisms. To address the possibility that long-term rainfall may have direct effects on bribery through some unspecified channels, we take advantage of the Conley *et al.* (2012) approach and provide bounds estimates.

The empirical evidence reported in this paper suggests that bribe-taking by officials in schools affects poor households disproportionately; poor parents are more likely to pay bribes for education of their children, and among the bribe payers, the poor pay as much as the rich. The results reject the unbiased enforcement models, including the ability-to-pay model. The evidence that the amount paid by a household does not depend on its income rejects the deterrence version of the bargaining power model, but supports the refusal-to-pay version. We provide suggestive evidence that the estimated effects are primarily driven by a 'pure bargaining power' effect that captures the notion that the poor are unable to inflict any costs on an official if they ask for bribes. While enforcement heterogeneity across villages seems to play a moderate role, we do not find any evidence that endogenous ability and preference formation are important in explaining the pattern of bribery.

The evidence and analysis presented in this paper have important policy implications. Free schooling in the presence of corruption results in a perverse outcome: 'free' schooling is free only for the richer households as they are not likely to pay bribes, while the poor still pay for their children's schooling. Without fundamental reform to improve law enforcement, free schooling and similar policies are not likely to be effective in reducing educational inequality.

The rest of the paper is organized as follows. Section I discusses the related literature and thus helps to put the contributions of this paper in perspective. Section II develops testable predictions from three models of bribe-taking by school officials, based on alternative assumptions regarding the nature of the enforcement regime and information set of the official. The empirical strategy to address the potential biases from household heterogeneity is discussed in Section III. Section IV provides a discussion of the data sources and variables. Section V, arranged in a number of subsections, reports the estimates from alternative econometric approaches: the OLS results, the results on the ability-to-pay effect using transitory rainfall shocks, and the corresponding results for the bargaining models based on long-term rainfall variation. The paper concludes in Section VI with a summary of the results and their implications for the broader debate about the role of public schooling and anticorruption measures to address inequality in educational opportunities.

I. Related Literature

The economics literature on corruption is substantial and has been the focus of innovative research in the last two decades. For recent surveys of the literature, see, for example, Olken and Pande (2011), Banerjee *et al.* (2012), Rose-Ackerman (2010) and Bardhan (1997).[5] The literature has, for good reasons, focused on the measurement of corruption, its effects on efficiency, and policies to combat corruption in different contexts. For recent contributions on measurement, see, for example, Olken (2009), Olken and Barron (2009), Olken and Pande (2009), Hsieh and Moretti (2006), and Niehaus and Sukhtankar (2013a,b). For contributions on costs of corruption, see, among others, Svensson (2003), Bertrand *et al.* (2007), Ferraz *et al.* (2012), Olken (2006, 2007, 2009), and on policies to combat corruption, see, for example, Muralidharan *et al.* (2016), Di Tella and Schargrodsky (2004), Niehaus and Sukhtankar (2013a), Olken (2007) and Banerjee *et al.* (2012).

The literature on the effects of corruption on households, and in particular on educational inequality, is, however, limited. In an interesting recent paper, Borcan *et al.* (2017) show that anticorruption efforts in schools in Romania increased the score gap between poor and non-poor students. The available evidence on the heterogeneity in the burden of corruption in other types of public services on households, however, leads to conflicting conclusions. Kaufmann *et al.* (1998, 2005) report bribes to be regressive at the intensive margin as the poor pay a higher share of their income as bribes. On the other hand, Hunt (2007) reports evidence suggesting that corruption in health care in Uganda is progressive at both the intensive and extensive margins. Hunt and Laszlo (2012) find that bribery is, in general, not regressive in Uganda and Peru. Most of the existing studies on the relationship between household income and the propensity to pay bribes, and the bribe amount paid, rely on OLS regressions, and do not analyse the biases due to unobserved heterogeneity and measurement error. Hunt and Laszlo (2012) take a first step and tackle biases due to measurement error using household assets such as telephone and quality of dwelling as instruments. While this approach reduces the attenuation bias due to measurement error, the likelihood of estimating spurious progressivity is, in fact, higher compared to the simple OLS regressions. This is because the estimates in this case are unambiguously biased upward (towards positive effect of income) due to correlations between parents and children in ability and preference, irrespective of whether the true effect is progressive, neutral or regressive. The empirical results of Hunt and Laszlo (2012) are not comparable to ours for two additional reasons. First, they use consumption expenditure as an indicator of permanent income, which suffers from simultaneity bias. Instead, we use household income. Second, our focus is on whether the poor are more likely to pay bribes for the *same service* (e.g. admission into school). In contrast, Hunt and Laszlo (2012) (also Mocan 2008) argue that bribery is progressive at the extensive margin because the rich utilize many more public services and thus are more likely to pay bribes. We believe that it is conceptually cleaner to focus on a given public service. For example, it makes little sense to say that the incidence of corruption is progressive because the rich pay bribes for passports but the poor do not (because the poor usually do not need passports).[6]

To the best of our knowledge, there is no work in the current economics literature that deals with the central issue of our analysis: the implications of biased enforcement for the incidence of corruption. Although the potential role of relative bargaining power of briber and bribee is well-recognized in the policy analysis of corruption (see, for example, Rose-Ackerman 1999), formal theoretical and empirical analysis has been scant. In an important and widely cited paper on misgovernance, Banerjee (2012) develops a model where a bureaucrat uses red tape to screen the ability to pay. If the bureaucrat is able to distinguish the ability to pay in a separating equilibrium, then we should observe rich households paying more bribes more frequently, thus delivering conclusions similar to the canonical ability-to-pay model. See below for a more extended discussion on this point.

In an interesting paper on corruption faced by firms, Svensson (2003) develops a 'refusal-to-pay model' where sectors differ in terms of sunk costs, and a firm's power to say no when faced with a bribe demand depends on the sunk costs (costs of exit) in the sector in which it operates. The source of the heterogeneity in power is thus not a biased enforcement regime as is the case in our analysis.

II. Models of Bribery in Schools

We develop alternative models of bribery for admission into school under different assumptions about the nature of the enforcement regime and the information set of the official.

The basic setup

The official has two sources of income: salary w received from employment in public schools, and bribes for admitting students to school. The households in village j are heterogeneous in terms of their economic status as measured by income y_i and bargaining power μ_i, where i is the household index. The probability of punishment for taking bribes from household i is $\delta_j(\mu_i)$, and we assume that the probability is increasing in the bargaining power of the household. The village index j captures the notion that enforcement quality may differ across villages. The bargaining power of household i depends on income and also a set of factors ϕ_i uncorrelated with income, that is, $\mu_i = \mu(y_i,\varphi_i)$.[7] Bargaining power μ_i is increasing in both its arguments. The assumption that μ_i is a positive function of household income captures the idea that the rich in a village have better bargaining power, given an enforcement regime $\delta_j(\cdot)$. The functions $\delta_j(\cdot)$ and $\mu(\cdot)$ are common knowledge. If caught and convicted of corruption, the school official loses her job, thus the payoff is zero in this case.

The income of household i is a function of its resource endowment E_i and ability of parents A_i^f. The households also vary in terms of their moral costs of corruption (measured in terms of utility loss) $M_i^f \in [M_L, M_H]$.

The income function is

$$(1) \qquad y_i = y(E_i, A_i^f, M_i^f), \quad \text{with } \frac{\partial y(\cdot)}{\partial E_i} > 0,\ \frac{\partial y(\cdot)}{\partial A_i^f} > 0,\ \frac{\partial y(\cdot)}{\partial M_i^f} < 0.$$

So household income is increasing in its endowment and parental ability, but is a negative function of moral cost M_i^f. A household with low moral cost can profit from corrupt deals and activities, for example, by getting a contract through bribing. The households are ordered according to income as $y_0 < y_1 < \ldots < \overline{y}$. Each household has one school-aged child. The quality of education received by a student i is $q(A_i)$, where $A_i \in [A_L, A_H]$ is the ability of the child. The human capital function $q(A_i)$ is strictly increasing and concave in ability.

In addition to possible bribes for schooling, a household spends its income on a consumption good c. The utility function takes the form

$$(2) \qquad V_i = R_q^i q(A_i) + u(c_i - B_i) - M_i^f,$$

where $u(\cdot)$ is assumed to be increasing and strictly concave, R_q^i is the returns to education, and $B_i \geq 0$ is the amount of the bribe. Admission into school ensures human capital $q(A_i)$, and the return to human capital may depend on the family connection, with the rich expected to get higher returns given their network in the labour market. However, we will ignore the heterogeneity in returns, and focus on the implications of ability to pay, because the higher expected returns for the rich will only strengthen the conclusions below. We thus set $R_q^i = 1$ for all households.

A model of bribe determination when an official has perfect information and the probability of punishment does not vary across households

We first consider a setup where legal enforcement systems are impersonal, and the common probability of punishment faced by the corrupt official across different households is $\widetilde{\delta}$. All of the existing analysis of which we are aware relies on the assumption of an unbiased enforcement regime. We also assume that the school official observes income and the type of a household in terms of ability and moral preference; that is, the information set of the official is $\Omega = (y, A^f, A, M^f, \widetilde{\delta})$.[8] The school official decides whether to ask for bribes from household i given the information set. If he decides to ask for a bribe, then the official makes a take-it-or-leave-it offer to the parents. The parents decide whether or not to accept the bribe demand. Then the official decides whether or not to admit the child into the school.

Consider a household's decision regarding whether to pay a bribe or not for school admission when the official makes a take-it-or-leave-it bribe demand. Given that the household cannot influence the probability of punishment, it is optimal for a household to pay a bribe to get admission for its child into the school if the bribe demand B_i satisfies

$$(3) \qquad q(A_i) + u(y_i - B_i) - M_i^f \geq u(y_i).$$

The main results that follow from the ability-to-pay model are summarized in Proposition 1.

Proposition 1 Assume that the admission official has full information and makes a take-it-or-leave-it bribe demand. In this case, the participation constraint (3) binds for each household that sends a child to school.

(a) Bribery is progressive at the extensive margin in the sense that there exists a threshold income $\widetilde{y}$ such that a household with income $y_i < \widetilde{y}(A_H, M_L)$ is not asked for any bribe for admission.
(b) There exists a threshold income $y^L(A_H,M_L)$ below which a household is unwilling to pay a positive (however small) bribe for admission.
(c) Among households with a child in school, the bribe amount is a positive function of income if the household utility function is strictly concave. In other words, the bribe is 'weakly progressive' at the intensive margin.
(d) Bribes are progressive at the intensive margin (i.e. the bribe as a share of income increases with the level of income) only if the utility function exhibits strong enough concavity.

Proof See the Online Appendix.

Variants of parts (a)–(c) of Proposition 1 have been discussed in the literature before, but part (d) is new, to the best of our knowledge. Part (d) shows that even with perfect information, the maximum bribe that an official can extract is not progressive in the standard sense if the curvature of the utility function is not strong enough. With an isoelastic utility function, it can be shown that the bribes are progressive in the standard sense only if the utility function has more curvature than a log function (see the Online Appendix).

Although the predictions that the rich are more likely to pay bribes, and pay higher amounts conditional on paying, are derived in the context of the simple stylized model above, similar conclusions arise in other models that share the unbiased enforcement assumption. It is instructive to consider the versatile model developed by Banerjee (1997) and extended in Banerjee *et al.* (2012). These authors consider a model where corruption is the result of misaligned incentives between the bureaucrat and the government, and many different types of corruption can be considered within a common framework. Although their focus is on allocational inefficiency and red tape, the model can be used to understand who has to pay bribes, and how much. The bureaucrat can use costly screening (testing) to find out the types of the agents who are differentiated by ability-to-pay and private benefit. The private benefit in their model corresponds to $R_q^i q(A_i)$ in equation (2), and ability-to-pay to y_i in our setup. If we assume that returns in the labour market are higher for rich households, then both private benefit and ability-to-pay are higher for richer households. The rich in our case correspond to the low type in their model, if the goal of free primary schooling is to provide education to the poorest. The price set by government is zero (free schooling). Then it is easy to see that an income-maximizing official will use costly screening only if she can charge more for admission for the children of rich parents. Income-maximization also implies that if there are a limited number of slots available, then they will screen in the children from rich families.[9] In fact, it is impossible to have the opposite conclusion that the poor are more likely to pay bribes in any model that is built on the following widely used set of assumptions: (1) the bureaucrat maximizes income; (2) the high type (poor in our case, assuming that social returns are higher) has less ability-to-pay; (3) the high type assigns higher private value; (4) the anticorruption enforcement does not depend on household characteristics. The intuition is as follows. Assume that the school official charges B_p and B_r to the poor and rich, respectively, with $B_p > B_r$. Also assume that the poor are more likely to pay bribes; they are asked for bribes with a higher probability. Assume that this mechanism maximizes the bribe income of the corrupt official. The poor can pay y_p and the rich y_r, with $y_p < y_r$. Then it is obvious that $y_p \geq B_p > B_r$. Clearly, the school official can increase her income by charging the same bribe B_p to the rich, and by increasing the probability of asking for bribes from the rich to equal that of the poor. In other words, there is a pooling equilibrium that yields more income without incurring any costs for screening, contradicting that the official is maximizing her bribe income.

Testable Prediction 1: Ability-to-pay model

If heterogeneity in ability-to-pay across households determines the incidence of corruption, then a transitory positive shock to income would increase both the propensity to pay bribes for children's schooling and the amount paid conditional on paying.

To test for the importance of the ability-to-pay effect, we need a source of exogenous variation in household income that is not correlated with bargaining power derived from higher permanent income (wealth). Thus a transitory rainfall shock to rural income would be an excellent source of identifying information in this context.

Heterogeneity in bargaining power in a model of biased enforcement

In this subsection, we develop two models that abandon the assumption that the legal enforcement regime is impersonal (the 'rule of law' assumption). We emphasize that 'bargaining power' is used as a portmanteau term that represents a household's economic, social and political influence, and the 'connections' that come with higher income and wealth in a developing country. It also represents confidence and negotiation ability that may be affected by impairment of cognitive and non-cognitive abilities and endogenous preference formation because of poverty. Another important point to keep in mind is that the poor are also victims of weak institutions because they live in a poor area where anticorruption enforcement may be lax. For expositional simplicity, we assume in this section that households do not vary in terms of ability or moral costs; the main conclusions do not depend on this simplification.

A model of bargaining power as a deterrent to bribe demand

This subsubsection is devoted to the case where the information set of the school official is not as rich as in the ability-to-pay model, but it is assumed that the official observes household-level income. The official has no independent information on cognitive ability or moral costs, and thus can try to infer them from the income information. Since the official observes the income of a household, the estimated probability of punishment is $\widehat{\delta}_i(y_i) = \delta(\mu(y_i))$. Note that once the official decides to ask for bribes from a household, it is optimal to extract full surplus from the household, because the probability of getting caught and punished does not depend on the bribe size. We assume that there are lower $(\widehat{y}_l)$ and upper $(\widehat{y}_h)$ thresholds of income such that $\widehat{\delta}(y_i) = 0$ for $y_i \leq \widehat{y}_l < \overline{y}$, and $\widehat{\delta}(y_i) = 1$ for $y_i \geq \widehat{y}_h < \overline{y}$. Thus we assume that the poorest of the households have no bargaining power, while the richest ones can punish the official for bribe-taking with probability 1.

It follows that there exists a threshold $y^m < \overline{y}$ such that the following equality holds (assuming that the official maximizes expected income):

$$(4) \qquad \left(1-\widehat{\delta}(y^m)\right)\left[B^*(y^m)+w\right]=w,$$

where $B^{*(ym)}$ is the optimal bribe function. If the bargaining power effect of income is strong enough in the sense that $\mathrm{d}\widehat{\delta}(y)/\mathrm{d}y$ is greater than a positive threshold, then the official does not ask for bribes from any household with income higher than y^m defined in equation (4) (for details, see the the proof below). The model thus predicts that when the bargaining power effect of income is strong enough, among all households with a child in school, only the relatively poor pay bribes, and the richer households ($y_i > y^m$) are not asked for bribes, even though they have higher ability to pay. Higher income and the resulting bargaining power thus work as a deterrent. The testable predictions from the bargaining power as *ex ante* deterrence are summarized as follows.

Testable Prediction 2: Bargaining power as deterrence model

Assume that the poorest households have no bargaining power, but bargaining power increases with income, and the richest households can punish the corrupt official with certainty. The official can observe household income. Consider the set of households with a child in school.

The probability that a household had to pay bribes for admission is a negative function of income if the bargaining power effect of income is strong enough,. Among those who pay bribes, the amount of bribes paid is a positive function of household income.

Proof. Given the assumptions that the poorest have no bargaining power and the richest can punish the teacher with certainty, it follows that there exists a threshold $y^M < \overline{y}$ such that the following equality holds (assuming that the teacher maximizes expected income):

$$(5) \qquad \left(1-\widehat{\delta}(y^M)\right)\left[B^*(y^M)+w\right]=w.$$

It is easy to check that the expected income from bribery $\left(1-\widehat{\delta}(y)\right)\left[B^*(y)+w\right]$ is a decreasing function of income if the bargaining power effect of income is strong enough in following sense:

$$(6) \qquad \widehat{\delta}'(y) > \frac{B^{*\prime}(y)\left(1-\widehat{\delta}(y)\right)}{B^*(y)+w}.$$

Thus equation (5) and inequality (6) imply together that when the bargaining effect of income is strong enough to satisfy inequality (6) for all $y_i > y^M$, the following inequality holds:

$$(7) \qquad \left(1-\widehat{\delta}(y_i)\right)\left[B^*(y_i)+w\right]<w.$$

When inequality (7) is satisfied, it is optimal for the teacher not to ask for bribes when facing a household with income $y_i > y^M$. $\square$

A model of bargaining power as refusal to pay bribes

The model developed in this subsubsection restricts the information set further and assumes that the school official cannot discriminate across households. The sequence of events unfolds in the following way.

The official sets a bribe demand $B(I_{jk})$, where I_{jk} is the indicator observed by the official for a group k in village j. We assume that the bribe $B(\cdot)$ is a positive function of I_{jk}. We do not spell out a complete model of how the exact amount B is determined, as it is not necessary for deriving the testable predictions. Our results are valid for any model where the official cannot bribe discriminate among households. In some cases, the information may be so coarse that the officials across villages charge effectively the same bribe rates. This can happen when the bribe amount is tied to the national cost of living index, for example.[10]

When a household goes for admission of its child into the school, the school official demands a payment of $B(I_{jk})$. The parents decide whether to pay or not. If they pay the bribe, then the child is admitted. If they decline, then the parents can deploy their bargaining power, for example, a call to the official from the local political leader or from the office of the education minister in the capital city (revealing higher bargaining power). The official estimates the probability that she will be punished for insisting on bribes and not admitting the child, following the revelation of the household's bargaining power. If the bargaining power is strong enough, then the school official admits the child even without the bribe. We assume that the estimated probability of punishment $\widehat{\delta}_i$ is a positive function of a household's income (or wealth), that is, $\widehat{\delta}_i = \delta(y_i)$ and $\mathrm{d}\widehat{\delta}_i(\cdot)/\mathrm{d}y_i > 0$. Note that the probability of punishment estimated by the official is a positive function of household income in the data even though the official does not know the household income, as long as the bargaining power revealed by a household's refusal to pay is correlated with household income.

The above model implies that households with income higher than a threshold refuse to pay the bribe, but still get their children admitted into the school. The threshold household income level (denoted as $y^r_{jk} < \overline{y}$) above which a household gets the child admitted after refusing to pay bribes is determined by the following equation (assuming that the official maximizes expected income):

$$\left(1 - \widehat{\delta}(y^r_{jk})\right)\left[B(I_{jk}) + w\right] = w.$$

Since the official cannot tailor the bribe amount to an individual household, the model predicts that bribes for school admission are regressive at both the extensive and intensive margins: the rich are less likely to pay bribes, and the poor pay more as a proportion of income among the households that pay bribes for children's admission. An important testable implication is that the amount of bribe paid does not depend on household income. The above discussion yields the following testable prediction about the distributional effects of bribes for school admission, which we take to data in a later section of the paper.

Testable Prediction 3: Refusal-to-pay model

Assume that the official does not observe individual household income, but observes wealth indicators at a group level. The household can deploy its bargaining power once the bribe demand is made. Bargaining power is a positive function of a household's income. Higher bargaining power leads to higher probability of punishment for the corrupt official. Consider the set of households with a child in school.

The higher household income reduces the probability that parents had to pay bribes for a child's admission into school, but among those who pay, the amount paid does not depend on a household's income.

III. Empirical Issues and Strategy

Our focus is on household income as an indicator of a household's economic status. An alternative, widely used in the existing literature, is household consumption expenditure. The choice of consumption expenditure by many researchers is motivated by the observation that it is less prone to measurement error compared to income. However, an important problem with consumption expenditure as an indicator of economic status in an analysis of bribery by households is that consumption and bribe payments are simultaneously determined, given income (see equations (1) and (3)). Simultaneity bias is thus a serious problem in addition to omitted heterogeneity and measurement error in the case of household consumption expenditure. We thus prefer income as the indicator of economic status of a household.

Consider the following triangular model for bribery for school admission at the extensive and intensive margins, where the first two equations refer to propensity to pay bribes and amount paid conditional on paying, respectively, and the third is a selection equation that captures heterogeneity in household income:

$$(8) \qquad P(D_{ij}=1)=\beta_0+\beta_1\delta_j+\beta_2 y_{ij}+\pi X_{ij}+\beta_A A_{ij}+\beta_M M^f_{ij}+\beta_p\mu_{ij}+\zeta_{ij},$$

$$(9) \qquad B_{ij}=\theta_0+\theta_1\delta_j+\theta_2 y_{ij}+\Gamma X_{ij}+\theta_3 I_{jk}+\theta_A A_{ij}+\theta_M M^f_{ij}+v_{ij},$$

$$(10) \qquad y_{ij}=\alpha_0+\Pi X_{ij}+\alpha_A A^f_{ij}+\alpha_M M^f_{ij}+\varsigma_{ij},$$

where i is the household index and j is the village index, D_{ij} is a binary variable that takes the value 1 when the household had to pay bribes for educational services, B_{ij} is the amount paid, δ_j is a measure of enforcement in village j, y_{ij} is the income of household i in village j, X_{ij} is a vector of control variables, μ_{ij} is household's bargaining power, I_{jk} is the indicator used by the official under the refusal-to-pay model, and ζ_{ij} and v_{ij} are the error terms. It is important to emphasize here that the bargaining power effect (β_p) in the above empirical model does not include the enforcement effect or the possible endogenous effects of poverty through ability and preference formation, as we include controls for them. Below, we develop the empirical model to include such effects. To avoid confusion, we call the effect represented by β_p the 'pure bargaining power effect'.

We expect that $\beta_p, \alpha_M, \beta_M, \theta_M < 0$. Since better enforcement in a village reduces the scope for corruption, we expect $\beta_1, \theta_1 < 0$. As discussed before, $\theta_2 = 0$ if the refusal-to-pay model holds in the data. Also note that household bargaining power does not affect the amount paid, a prediction shared by all three models discussed in Section II.[11] The ability-to-pay model implies that $\beta_2 > 0$ and $\theta_2 > 0$.

The empirical model in equations (8)–(10) makes precise the idea of higher ability to pay of a high-income household represented by the parameters β_2 and θ_2. Part of the difficulty in estimating the ability-to-pay effect arises from the fact that household income also captures the effects of household bargaining power and village enforcement regime. As discussed before, the enforcement regime in a village is likely to depend on its level of development, and the probability is higher that a high-income household chooses to live in a village with better law enforcement due to spatial sorting based on income and amenities. We can thus decompose enforcement into two components as $\delta_j = \lambda_1 y_{ij} + \lambda_2 \delta_j^{-y}$, that is, a part of the enforcement is correlated with household income, but it is also determined by a vector of other factors that are uncorrelated with income (denoted δ_j^{-y}). Similarly, it is useful to decompose the bargaining power of a household into two components: $\mu_{ij} = \omega_1 y_{ij} + \omega_2 \mu_{ij}^{-y}$, with a component correlated with household income and a second part orthogonal to income (denoted μ_{ij}^{-y}). Since poverty can affect cognitive and non-cognitive abilities and shape preference, we decompose the ability and moral costs into genetic and non-genetic components: $A_{ij} = \tau_0 A_{ij}^f + \tau_1 y_{ij}$ and $M_{ij}^f = \rho_0 M_{ij}^{gf} + \rho_1 y_{ij}$.[12]

Note that when cognitive and non-cognitive abilities of a child are affected adversely by poverty ($\tau_1 > 0$), the parents may expect low returns to investing in her education, suggesting that they will be less willing to pay bribes. In contrast, when the main impact of low ability is low confidence and negotiation ability, we expect that poor households will be more likely to pay bribes, *ceteris paribus*. Under the assumption that moral probity is a normal good, we expect $\rho_1 \geq 0$. To avoid confusion, we reiterate that this endogenous effect of income on preference is different to the effect of moral costs on the income of a household captured by $\alpha_M < 0$ in equation (10).

The empirical model can now be rewritten as

$$P(D_{ij} = 1) = \beta_0 + \beta_T y_{ij} + \pi X_{ij} + \varepsilon_{ij}, \tag{11}$$

$$B_{ij} = \theta_0 + \theta_T y_{ij} + \Gamma X_{ij} + \kappa_{ij}, \tag{12}$$

$$y_{ij} = \psi_0 + \Pi_1 X_{ij} + \upsilon_{ij}, \tag{13}$$

where we have

$$\beta_T = \beta_2 + \beta_1 \lambda_1 + \beta_p \omega_1 + \beta_A \tau_1 + \beta_M \rho_1,$$
$$\theta_T = \theta_2 + \theta_1 \lambda_1 + \theta_A \tau_1 + \theta_M \rho_1,$$

and the error terms are

$$\varepsilon_{ij} = (\beta_A \tau_0) A_{ij}^f + (\beta_M \rho_0) M_{ij}^{gf} + (\beta_p \omega_2) \mu_{ij}^{-y} + (\beta_1 \lambda_2) \delta_j^{-y} + \zeta_{ij},$$
$$\kappa_{ij} = \theta_3 I_{jk} + (\tau_0 \theta_A) A_{ij}^f + (\theta_M \rho_0) M_{ij}^{gf} + (\theta_1 \lambda_2) \delta_j^{-y} + v_{ij},$$
$$\upsilon_{ij} = \psi_A A_{ij}^f + \psi_M M_{ij}^{gf} + \varsigma_{ij}.$$

The parameters in equation (13) are the rescaled parameters from equation (10), for example, $\psi_0 = \alpha_0/(1-\alpha_M\rho_1)$. Note that $1-\alpha_M\rho_1 > 0$ because $\alpha_M < 0$ and $\rho_1 \geq 0$. The empirical model in equations (11)–(13) is useful in developing a credible empirical strategy to estimate the ability-to-pay effect and in discriminating among alternative models of bribery in school developed in Section II. The first important point that is often not adequately appreciated is that the effect of income on propensity to pay bribes (β_T) may capture a number of things, including ability to pay ($\beta_2 \geq 0$), a pure bargaining power effect ($\beta_p\omega_1 < 0$) and a quality of institutions effect ($\beta_1\lambda_1 < 0$). Thus the common practice of interpreting the coefficient of household income in a propensity-to-pay regression as an ability-to-pay effect is likely to be misleading. The second important point that is not adequately recognized in the current literature is that the nature of omitted variables bias depends on the information set of the official. We provide a fuller discussion below.

Information set of the official and the omitted variables bias

To understand the role played by the official's information set in stark terms, consider first the standard ability-to-pay model where this is observed by the official (or she has information to make estimates reasonably well). In this case, both the propensity to pay bribes and the amount paid conditional on paying are susceptible to bias due to omitted heterogeneity in ability and moral probity, as the official uses this information in her decision. In this case, the OLS estimate is clearly biased towards finding a positive coefficient on household income because it is difficult to control for ability and moral cost heterogeneity. This can be illustrated by the classic ability bias applied to the present context. The demand for children's education may be higher in richer (high-ability) households because of higher cognitive ability transmitted genetically from parents to children. Moral deficiency may help households to accumulate wealth through corruption, and they may also be less likely to object to paying bribes for schooling. The resulting bias in the estimated effect of household income on the probability of paying bribes (denoting the variance of the genetic component of moral cost by $\sigma^2_{M^{gf}}$) is given by

$$\mathrm{Cov}(\varepsilon_{ij}, \upsilon_{ij}) = \frac{\beta_A\tau_0\alpha_A}{1-\alpha_M\rho_1}\mathrm{Cov}(A_{ij}, A^f_{ij}) + \frac{\alpha_M\beta_M\rho_0}{1-\alpha_M\rho_1}\sigma^2_{M^{gf}} > 0.$$

The last inequality reflects that fact that we expect $\mathrm{Cov}(A_{ij}, A^f_{ij}) > 0$, with $\alpha_A, \beta_A, \tau_0, \rho_0, \rho_1 > 0$ and $\alpha_M, \beta_M < 0$.[13] Thus the source of positive bias in the OLS estimates of the income effects is the genetic correlations in ability and preference regarding corruption (moral costs). Evidence from OLS regressions showing that the rich are more likely to pay bribes thus should be treated with due caution. Note that the endogenous formation of moral preference captured by $\rho_1 > 0$ gives rise to a multiplier effect in the bias due to omitted genetic ability and preference correlation.

In the deterrence model of bargaining power, the official observes household income and estimates the ability to pay based on this information. If the official is aware that a higher-income household is likely to value children's education more because of the expected higher genetic endowment of ability as discussed above, then the official's optimal decision would reflect this positive correlation between income and ability, and the OLS estimate of the effects of income on propensity to pay bribes and the amount paid will be biased upwards. However, we would expect the bias to be less pronounced, in general, when compared to the bias implied by the ability-to-pay model where the official observes cognitive ability directly. Similar arguments hold for moral cost heterogeneity: the OLS estimates are biased if the official makes inferences about moral probity from the household's income information.

In the refusal-to-pay model, whether a household pays bribes once a bribe demand is made depends on its bargaining power. Ability heterogeneity can bias the OLS estimate of the effects of household income on propensity to pay for the following reason. How successful a parent is in deploying her bargaining power may depend on cognitive (and non-cognitive) ability, which would result in positive correlations among $\widehat{\delta}$, y_{ij} and parental unobserved ability A^f_{ij}. The available household surveys on corruption pay little attention to the information set of the official, and it is not possible to construct an adequate proxy for I_{jk}. Under the refusal-to-pay model, it is thus absorbed in the error term. *More important is the observation that in a refusal-to-pay model, the official cannot discriminate across a household's economic status, implying that* $\mathrm{Cov}(y_{ij}, I_{jk}) = 0$ *in a village*. If an official uses village-level indicators to set significantly different bribe rates across villages, then the estimates of the effect of household income on the amount paid should change substantially when we include village fixed effects in the OLS regression.

It is important to appreciate that the possibility of incorrectly concluding that bribes are progressive increases when we control for the heterogeneity in village enforcement regime, and this is true irrespective of the model under consideration. When a researcher uses village fixed effects in household-level bribe regressions to control for 'unobserved village heterogeneity', the parameter that is being estimated for propensity to pay bribes is $\beta_2 + \beta_p\omega_1 + \beta_A\tau_1 + \beta_M\rho_1$, not $\beta_2 + \beta_p\omega_1 + \beta_A\tau_1 + \beta_M\rho_1 + \lambda_1\beta_1$. It is thus easier to obtain a positive coefficient from OLS regressions with a relatively moderate amount of omitted variables bias due to genetic components of ability and moral cost, because $\lambda_1\beta_1 < 0$. An important implication of the above discussion is that when one finds a negative effect of household income on the propensity to pay bribes in OLS estimates from an empirical model with village fixed effects, it is sufficient evidence for a regressive causal effect of bribes. However, the degree of regressivity is underestimated due to unobserved heterogeneity in ability and preference.

An instrumental variables approach

Testing ability to pay: the effects of temporary shocks on household income To disentangle the role of ability to pay from bargaining power, we rely on transitory rainfall shocks because bargaining power and enforcement quality are determined by permanent income differences.

To the best of our knowledge, there is no evidence that short-term rainfall shock is correlated with genetic transmission of ability and preference from parents to children. There is evidence that a short-term rainfall shock when a child is *in utero* can have a negative effect on cognitive ability. This, however, does not compromise our identification strategy for the following reason. We calculate rainfall shock as the deviation of the rainfall in the immediate past year (2009) from its 10-year average (2000–9). Thus it is not relevant for the schoolchildren who are the focus of our analysis; they are not *in utero* at the time of the rainfall shock.[14] Note that under the null hypothesis that the ability-to-pay model is appropriate, $\theta_3 = 0$ and the variable I_{jk} is not a component of the error term κ_{ij} in equation (12).

Another concern with this approach is whether transitory rainfall shocks affect the demand for bribes by school teachers and administrators. If the school infrastructure is damaged by the flood caused by monsoon rain, then the school administrators may ask for financial help to do repairs. This is especially important when the schools are locally financed, as is the case in much of the USA. However, schools in Bangladesh are not locally financed; the central government and NGOs (donor funded) provide the funds and resources for repair and rehabilitation of school infrastructure. Most of teacher salaries, even in so-called private schools, are government financed and thus are not affected by local rainfall shocks.[15] Since it is impossible to identify and address all such potential effects of rainfall on bribery through complex and indirect mechanisms, we allow for some direct effect of rainfall on the outcomes of interest by implementing the Conley *et al.* (2012) bounds approach (for more details, see the subsubsection 'Relaxing the exclusion restrictions: Conley *et al.* bounds' below).

We define five dummies for five quintiles of rainfall deviations. The preliminary analysis indicates that too little (lowest quintile) or too much (upper two quintiles) rainfall relative to long-term trends are negatively correlated with household income. These three quintile dummies are then interacted with household head's age and religion to generate household-level variations. There is substantial experimental evidence that older people are more risk-averse, which implies that a rainfall shock may have different effects depending on the age of the household head. Religious minorities (Hindu and Buddhist) in Bangladesh have more dense social networks, which may help in risk and information sharing. We control for household head's age and religion to ensure that the instruments do not pick up any direct effect of these variables on propensity to pay bribes, and on the amount paid. The estimates of the effects of household income on propensity to pay and amount paid using the rainfall shocks will be significantly positive if ability to pay is important in bribing decisions of the households.

Estimating the effects of poverty and testing refusal-to-pay model versus deterrence to bribe demand model By design, the transitory rainfall shock gets rid of the effects of permanent income and thus purges the role played by enforcement and bargaining power effects of household income. To estimate the causal effect of permanent income, that is, $\beta_T = \beta_2 + \beta_p\omega_1 + \beta_1\lambda_1 + \beta_A\tau_1 + \beta_M\rho_1$, we exploit variations in long-run rainfall levels, as opposed to deviations from long-run trends used for rainfall shock in the previous subsubsection. The instrument is defined as a dummy that takes the value 1 if average rainfall in an area is above the 75th percentile of rainfall for the country during the last 10 years (2000–9), and 0 otherwise. This high rainfall dummy is then interacted with household head's age and religion to generate additional exogenous variations. Economic activities in the villages in Bangladesh have historically been dominated by agriculture and determined to a large extent by rainfall variations. High-rainfall areas are more flood prone in Bangladesh, submerging standing crops, and adversely affecting access to urban markets by washing away transport and communications infrastructure, which is likely to reduce income (Emran and Hou 2013). We thus expect lower permanent income in high-rainfall areas. Again, we use household-level variations by interacting the high-rainfall dummy with household head's age and religion.

The litmus test for discriminating between the deterrence versus refusal-to-pay models of bargaining power is the effects of long-term rainfall induced differences in permanent income on the amount of bribe paid conditional on paying; the effect should be positive if the deterrence model is valid, and it should be zero if the refusal-to-pay model is consistent with the evidence. Note that while ability and preference heterogeneity (genetic) can bias the estimated effect under both the deterrence and refusal-to-pay models, any potential bias arising from the unobservability of I_{jk} is relevant only under the refusal-to-pay model where $\theta_3 > 0$. Thus only if we are testing the null hypothesis of the refusal-to-pay model, the source of exogenous variation in household income should not be significantly correlated with the indicators used by the official for setting the bribes. Since we exploit household-level variations by interacting rainfall with household head's age and religion, it is highly unlikely that our instruments would be systematically correlated with the indicators used by corrupt officials in the refusal-to-pay model where the official possesses no household-level information in a village. As noted earlier, we can test the importance of village-level indicators in deciding the amount of bribes by comparing the estimates with and without the village fixed effect in OLS regressions. If the officials do not rely on village-level indicators in deciding the bribe amount, then the estimated effect of income will not change substantially when we include village fixed effects. The evidence below in fact shows that the effects of income on the amount of bribes remains virtually unchanged when village fixed effects are included. The official does not rely on village-level indicators including rainfall to set the bribe amount. To ensure that the long-term rainfall based instruments do not capture any direct effect of age and religion on bribery, we include household head's age and religion as controls in all of the IV regressions. Again, we relax the exact exclusion restriction imposed in the standard IV regressions, and present estimates of bounds on the causal effect using the Conley *et al.* (2012) approach (see the discussion in the final subsubsection of this section, below).

Sorting out the mechanisms The effects of permanent income on propensity to pay bribes using long-term rainfall variations provides us with an estimate of $\beta_T = \beta_1\lambda_1 + \beta_2 + \beta_p\omega_1 + \beta_A\tau_1 + \beta_M\rho_1$. The different components of the parameter β_T refer to different mechanisms through which a household's economic status can affect its vulnerability to corruption. We take advantage of rich data on bribe payments for non-educational public services to create indicators of village-level enforcement and household-level moral cost heterogeneity (see the next paragraph). This allows us to make progress on understanding the role of different components by using controls for enforcement in a village and heterogeneity in moral probity across households. However, it is not possible to control for ability differences across households that would have allowed us to provide an estimate of the 'pure bargaining power effect' as captured by $\beta_1\lambda_1$. This is because the most credible indicator of ability is the education of the household head, but it is also highly correlated with a household's permanent income, making it impossible to isolate the effects. Note that the estimate of the income coefficient when controlling for indicators of moral cost and enforcement heterogeneity is approximately $\beta_T \simeq \beta_A\tau_1 + \beta_2 + \beta_p\omega_1$. However, we get an estimate of β_2 from the IV estimate using short-term rainfall shocks as discussed above. We can thus net out the effects of the ability-to-pay mechanism, by using this estimate. This provides us with a lower bound estimate of the 'pure bargaining power effect' $\beta_p\omega_1 < 0$ because ability bias is positive, that is, $\beta_A\tau_1 > 0$.

The measures of village-level enforcement and household level-moral cost heterogeneity are constructed as follows. For moral costs, since the worry is that people with low moral costs of corruption enrich themselves through corrupt activities, we need indicators of corruption-generated income (wealth). The main sources of corrupt wealth in rural Bangladesh are credit default (by bribing officials at the banks), land administration (bribing to grab others' land), tax evasion and agricultural services (subsidies and government loans for agriculture). The survey used in the empirical analysis fortunately collected detailed information on a household's propensity to pay bribes for a range of services required for wealth accumulation by a morally deficient household. We construct a measure of household heterogeneity in corruption for income generation and wealth accumulation by aggregating the propensity to pay bribes for these activities. As an index of village-level enforcement, we calculate the propensity to pay bribes for non-educational services by *all other* households (i.e. we exclude the household under focus).

Relaxing the exclusion restrictions: Conley et al. bounds The empirical strategy discussed in the first two subsubsections of this subsection imposes exact exclusion restrictions on the rainfall instruments, implying that rainfall cannot have non-zero direct impact on bribery. The identification assumption thus rules out even arbitrarily small direct effects of the instrument on the outcomes of interest, which may be unrealistic in most applications. We take advantage of the approach developed by Conley *et al.* (2012) to relax this identifying assumption and provide estimated bounds on the causal effect of interest. To understand this approach, consider the following extension of the empirical model for the amount paid as bribes set up earlier in equations (12) and (13):

$$B_{ij} = \theta_0 + \theta_T y_{ij} + \Gamma X_{ij} + \gamma Z + \kappa_{ij}, \quad y_{ij} = \psi_0 + \Pi_1 X_{ij} + \gamma_1 Z + \upsilon_{ij}, \tag{14}$$

where Z is the instrument such as rainfall shock or long-run rainfall level. The standard IV approach is based on the assumption that $\gamma = 0$, but $\gamma_1 \neq 0$; Conley *et al.* (2012) instead assume $\gamma \neq 0$. They develop alternative methods to estimate the parameter of interest θ_T under the assumption that γ belongs to a narrow interval around zero, $\gamma \in [\gamma^-, \gamma^+]$. This method provides set identification and yields the lower and upper bounds on the estimated causal effect, given values of $\gamma^{\pm}$. The most conservative method only assumes the support for the parameter γ, but no distributional assumptions are used. In the absence of any prior information about the magnitude of γ, we follow Conley *et al.* (2012) and assume that $\gamma^{\pm} = \pm 0.01\widehat{\theta}_T, \pm 0.05\widehat{\theta}_T, \pm 0.10\widehat{\theta}_T$, where $\widehat{\theta}_T$ is the estimate of θ_T with $\gamma = 0$.

IV. Data

The data on corruption and bribe payments in acquiring educational services come from the National Household Survey on Corruption 2010 (the NHSC 2010) conducted by Transparency International Bangladesh. Using the Integrated Multi-Purpose Sample (IMPS) design developed by the Bangladesh Bureau of Statistics as the sample frame, the survey selected 300 primary sampling units (PSUs) from 16 strata. The IMPS design identified 1000 PSUs using the 2010 population census as the frame. The PSU borders are defined to be contiguous census enumeration blocks (usually about two blocks) and consists of 200 households. Note that with 200 households, a PSU would be a small geographical unit in the context of Bangladesh, where population density is very high. According to the 2011 population census (preliminary report), the per square kilometre population in Bangladesh is 964. The average household size in our sample is 5.84, which would imply that a PSU covers a somewhat larger area than one square kilometre. Thus a PSU can be treated as a small village in most cases.

From each PSU, 20 households were selected randomly, giving us a total sample of 6000 households. The sample used in our empirical study is smaller, however. We restrict the sample to those households that reported using educational services during the survey year to make sure that the households that face a zero probability of paying bribes for education are excluded, which reduces the sample size to 4876. Since incomes of households in metropolitan city corporations are not likely to be affected significantly by rainfall, we drop 851 households living in metropolitan areas. We also drop 257 households that reported having no school-age children (age 6–20 years), 2 households that failed to report the gender of the household head, and another 161 households that did not use any of the income generating non-educational public services. Our final sample thus consists of 3605 households.

The NHSC 2010 collected detailed information on 13 different types of services usage, and corruption faced by households in obtaining those services. In the case of education, an adult member of the household was asked detailed questions about facing bribery regarding different educational services. The bribe questions were organized in four main categories: (i) bribe payment for admission into school, (ii) bribe payment for receiving free books, (iii) bribe payment for receiving scholarships, and finally (iv) implicit bribe payment in the form of paying fees or donations without receipts. Using responses to these questions, we define an overall propensity to pay bribes for education services as a dummy that takes the value 1 if a household reported paying any of these four types of explicit or implicit bribes, and 0 otherwise. Since paying without receipts is common in Bangladesh, and many people may not view it as paying bribes, we define an alternative propensity-to-pay-bribe variable by excluding 'paying without receipt' as a bribe category. We also make a distinction between bribes paid for admission and all other types of bribe. Table A.1 in the Online Appendix reports the summary statistics for different bribes related to education. About 48% of the households reported to have paid bribes, including payments made without receipts. Among the subcategories, bribes for school admission are reported by 10%, for free books by 6%, and for drawing scholarship money by 4% of the households. All together, 18% of households paid bribes for admission, free books and scholarships. About 40% of households reported making a payment without receipt. In the empirical analysis we present results on both the overall propensity to pay bribes (including payment without receipt) and the subcategories as well. As is to be expected, the sample used for the analysis of the intensive margin (i.e. the amount of bribes paid) is smaller, about 1747 households, because about half of the households with children in school do not pay bribes. The amount of bribe paid includes payments made for any of the four different categories of bribe defined above. Among the households that reported a positive amount of bribe payment, on average a household paid about Tk 247 ('Tk' denotes the Bangladeshi taka) during the survey year. To get a better sense of the financial burden imposed on the poor, it is instructive to look at the average bribe paid as a proportion of household savings. The average bribe paid in schools is 9% of average annual household savings, while for the first and second quintiles it amounts to 83% and 28% of annual household savings, respectively. Bribes paid for schooling of children can thus be a substantial burden on the poorest households.

The NHSC 2010 collected information on household size, the number of school-age children, and household head's age, gender and religion. We use this information to define control variables for our regression analysis. The survey also collected information about a household's total monthly income and expenditure. Summary statistics for all of these variables are provided in Table A.1 of the Online Appendix.

For the instrumental variables analysis, we need rainfall information, which is not collected in the NHSC survey. The rainfall data are drawn from Bandyopadhyay and Skoufias (2012). The original data on rainfall come from the Climate Research Unit

(CRU) of the University of East Anglia. The CRU reported estimated monthly rainfall for most of the world by the half-degree resolution from 1902 to 2009. The CRU estimation combines weather station data with other information to arrive at the estimates. To estimate the thana (type of district) level rainfall from the CRU data, Bandyopadhyay and Skoufias (2012) use area-weighted averages.[16]

V. Empirical Evidence

We begin with preliminary evidence on the extent and pattern of bribery in schools. The first interesting point to note is that the average per capita income of the bribe payers (Tk 1930 per month) is much lower compared to the average per capita income of non-payers (Tk 2580 per month). This indicates that on average, the households that end up paying bribes for their children's education are relatively poor. To explore further the partial correlations in the data, we report a series of OLS regressions with alternative sets of controls.

All standard errors reported in this paper are clustered at PSU level if not reported otherwise.[17] Clustering at the PSU level is motivated by two factors. First, households living in a village face similar choices in terms of school access and quality. Second, the first stage of stratified random sampling used in the NHSC 2010 selected 300 PSUs from the IMPS frame of 1000 PSUs, as discussed above in the data section. All of the regressions also include regional dummies (six regions called 'divisions') to account for any spatial differences.[18] The household-level controls include household head's age and gender, a dummy for religion (1 if muslim, 0 otherwise), household size and number of school-age children.

OLS estimates

Table 1 provides the OLS estimates of the coefficients on per capita income in the regressions of propensity to pay bribes and the amount paid as bribe. The Probit estimates are similar and omitted for brevity. The results for propensity to pay bribes for different types of schooling services are presented in panels A–D, while panel E reports the estimates for the bribe payments. In addition to an overall indicator of bribing propensity that aggregates bribery for various educational services in panel A, we provide estimates for three disaggregated categories in panels B–D: bribing for admission, bribe payments with receipts, and payments without receipt.

For each indicator of bribery, we report estimates from four different specifications across the columns of Table 1. The simplest specification, in column (1), includes only a vector of household controls such as age, gender and religion of household head, along with regional fixed effects, but no controls for moral cost or village enforcement regime are included. The OLS estimate in column (1) shows that the effects of higher household income are consistently negative for propensity to pay bribes, across three categories and also in the aggregate. This suggests that the propensity to pay bribes for educational services may be lower for a higher-income household. The estimated effect of household income on the amount paid as a bribe is, however, positive and statistically significant at the 1% level. The evidence in column (1) thus seems to suggest that among the bribe payers, the richer households pay more, implying that bribes are progressive at the intensive margin, which we show later is an incorrect conclusion.

Column (2) of Table 1 adds a measure of a household's corruption-generated income to address the bias due to moral cost heterogeneity, and the estimate is a little smaller in

TABLE 1
HOUSEHOLD INCOME AND BRIBERY IN SCHOOLS (OLS ESTIMATES)

	(1)	(2)	(3)	(4)
Panel A: Propensity to pay bribe (aggregate)				
Per capita income	−0.0383***	−0.0363***	−0.0335***	−0.0169***
	(−8.224)	(−7.970)	(−7.147)	(−3.791)
Panel B: Propensity to pay bribe for admission				
Per capita income	−0.0121***	−0.0113***	−0.0103***	−0.00791***
	(−5.333)	(−5.001)	(−4.468)	(−2.848)
Panel C: Propensity to pay bribe with receipts				
Per capita income	−0.0248***	−0.0238***	−0.0224***	−0.0155***
	(−8.440)	(−8.229)	(−7.623)	(−4.342)
Panel D: Propensity to pay bribe-payment without receipt				
Per capita income	−0.0293***	−0.0281***	−0.0261***	−0.0107**
	(−6.592)	(−6.382)	(−5.827)	(−2.432)
Observations	3605	3605	3605	3605
Panel E: Total amount paid as bribe				
Per capita income	0.0786***	0.0783***	0.0762***	0.0796***
	(2.831)	(2.851)	(2.965)	(4.582)
Observations	1747	1747	1747	1747
Household-level controls	Yes	Yes	Yes	Yes
Moral cost	No	Yes	Yes	N/A
Village-level controls				
Enforcement in the village	No	No	Yes	N/A
Village fixed effects	No	No	No	Yes
Regional fixed effects	Yes	Yes	Yes	N/A

Notes
Standard errors are clustered at primary sampling unit (village) level. Each cell in the rows labelled 'Per capita income' represents a result from a separate regression. Robust t-statistics in parentheses.
*, **, *** indicate significant at 10%, 5%, 1%, respectively.

magnitude (compared to that in column (1)).[19] The specification in column (3) introduces a control for differences in enforcement regimes across villages: the average propensity to pay bribes for non-educational public services in a village (excluding household i). If the enforcement is weak in a village, then the incidence of bribery will be higher, *ceteris paribus*.[20] For all four measures of propensity to bribe, the effect of higher household income is negative, but numerically smaller in column (3) compared to the corresponding estimates in column (1), suggesting a less regressive effect, consistent with the theoretical insights and the empirical model that weak enforcement in low-income villages affects the poor adversely. The estimates in column (3) of panel E suggest that the amount paid increases with income, but the numerical magnitude of the effect does not vary significantly between columns (2) and (3), and the magnitude is somewhat larger in column (1). Column (4) reports estimates from a specification where we use village fixed effects instead of village-level controls for heterogeneity in enforcement regime. The main conclusions remain intact, although the numerical magnitudes of the estimated effect are significantly smaller.

It is important to emphasize that a negative coefficient on income estimated in the OLS regressions is sufficient to establish that bribes for education are regressive at the extensive margin, although OLS underestimates the degree of regressivity, especially when village *fixed effects are used*. As noted earlier, measurement error results in a *positive* bias in the OLS estimate when the true causal effect is negative (Pischke 2007). The positive bias due to measurement error in this case is reinforced by the omitted variables bias arising from positive correlations in ability and preference of children and parents. The OLS estimate is thus unambiguously biased upwards towards finding spurious progressive effects, and a negative OLS underestimates the degree of regressivity. It is important to appreciate that the OLS estimate of a progressive effect at the intensive margin can easily be spurious in this case; we may find a positive effect of income driven largely by the positive biases from both measurement error and omitted heterogeneity when the true causal effect is negative (regressive).

A comparison of columns (1) and (4) for panel E of Table 1 shows that the estimate of the effects on the amount paid does not change when we include village fixed effects. This implies that the amount paid does not vary across villages, suggesting that enforcement heterogeneity across villages is not an important factor in this context. Perhaps the more important implication relates to the refusal-to-pay model: the officials do not use village-level indicators including rainfall to set the bribe amount. This is important evidence in favour of rainfall-based identification under the null hypothesis that the refusal-to-pay model is valid.

The discussion on the biases in the OLS estimates above suggests that the regressive effects found in some existing studies based on OLS regressions are not likely to be off the mark, while the studies that find progressive effects in OLS should be interpreted with due caution.

Ability to pay: instrumental variables estimates using transitory rainfall shocks

The estimated effects of household income variation that arise from transitory rainfall shocks are reported in Table 2. Columns (1)–(4) refer to propensity to pay bribes. Starting with the estimates for aggregate propensity (adding up admission, other payments with and without receipts) in column (1), we report estimates for three disaggregated categories of educational services in columns (2)–(4). Column (5) reports the estimates for the amount of bribe paid.

To ensure that the IV estimates do not suffer from weak instrument bias, we follow a procedure suggested by Rajan and Subramanian (2008). We first use the transitory rainfall shock based instruments to predict household income from a 'zero stage' regression. Then predicted income is used as the single instrument in a just identified model.[21] The first-stage results for various specifications of the propensity-to-pay equation show that the transitory rainfall shock based instruments are strong enough to identify the effects of a transitory change in household income; the Angrist–Pischke *F*-statistic in each case is higher than the Stock–Yogo critical value of 9.08 for 10% maximum relative bias.

When estimating the effects of household income on the amount paid conditional on paying, the power of the instruments is a little lower but adequate in specifications with no controls for village-level enforcement heterogeneity. Thus the estimates for the causal effect of household income on amount paid conditional on paying do not suffer from weak instrument bias when no controls for village enforcement are used. However, the Angrist–Pischke *F*-statistic for the specification including an indicator of enforcement in the village is 7.73, and some readers may worry that these particular estimates may not be free of weak instrument bias. We thus check estimates from alternative estimators, such as Fuller-LIML and CUE-GMM, for the amount of bribe paid that are robust to weak

TABLE 2
TESTING THE ABILITY-TO-PAY EFFECT: ESTIMATES WITH RAINFALL SHOCK AS IV (2SLS)

	Propensity to pay bribe				Amount paid
	Aggregate (1)	Admission (2)	With receipt (3)	Without receipt (4)	(5)
Per capita income	−0.0401	−0.0777	−0.0675	−0.0176	0.0797
	(−0.447)	(−1.449)	(−1.204)	(−0.196)	(0.507)
First-stage F-statistic	11.60	11.60	11.60	11.60	9.075
Controls for moral cost heterogeneity					
Per capita income	−0.0321	−0.0754	−0.0642	−0.0125	0.0807
	(−0.364)	(−1.400)	(−1.144)	(−0.139)	(0.518)
First-stage F-statistic	11.68	11.68	11.68	11.68	9.109
Controls for village enforcement heterogeneity					
Per capita income	−0.00888	−0.0727	−0.0564	0.00482	0.0551
	(−0.0976)	(−1.284)	(−0.957)	(0.0507)	(0.298)
First-stage F-statistic	10.13	10.13	10.13	10.13	7.726
Household-level controls	Yes	Yes	Yes	Yes	Yes
Regional fixed effects	Yes	Yes	Yes	Yes	Yes

Notes
Standard errors are clustered at primary sampling unit (village) level. Each cell in the rows labelled 'Per capita income' represents a result from a separate regression. Robust t-statistics in parentheses.
None of the estimates is significant at the 10% level or lower.

instrument bias, in addition to the 2SLS estimates. As noted by Stock and Yogo (2005), if there is significant weak instrument bias, then the estimates from Fuller-LIML and CUE-GMM will differ substantially from the 2SLS estimates.

The estimates from three different specifications for propensity to pay bribes point to a robust conclusion: there is no evidence of a significant effect of transitory changes in household income—none of the estimates is statistically significant at the 10% level across specifications and different categories of bribe payments. The IV estimate for the amount paid as a bribe is reported in column (5) of Table 2; the estimate is consistently positive, but, again, none of the estimates is significant at the 10% level. For the specification that includes a control for enforcement heterogeneity across villages, the Fuller-LIML and CUE-GMM estimates are very close to the 2SLS estimate, thus allaying worries about weak instrument bias (not reported in Table 2). The estimates from Fuller-LIML and CUE-GMM are omitted for the sake of brevity and are available from the authors. The results, taken together, do not provide support for an important ability-to-pay effect in distributional consequences of bribery in schools.

The IV estimates in Table 2 impose the strict exclusion restriction (i.e. $\gamma = 0$ in equations (14)). We provide bounds estimates that allow for some direct effect of the instrument using the Conley *et al*. (2012) approach below in the subsection 'Set identification: bounds estimates' below.

Instrumental variables estimates of the effects of poverty ('being poor and living in a poor village')

In this subsection, we discuss the estimates using long-term rainfall variations across villages. The estimates represent the effect of a household's economic status β_T, and represent the combined 'poor people' and 'poor area' effects. The 2SLS estimates are reported in Table 3, and the presentation of the results follows that in Table 2. The conclusions do not depend on the exact time window (2000–9) used for defining the long-term rainfall instrument; see Table 4 for estimates with a different time window (2005–9). The results and conclusions are similar if we use the Rivers and Vuong (1988) two-step conditional maximum likelihood estimator for the propensity-to-pay estimates (reported in the Online Appendix).

The first striking observation that comes across from Table 3 is that household permanent income has a consistently negative, statistically significant and numerically substantial effect on the aggregate propensity to pay bribes for children's schooling. The negative OLS estimates in Table 2 and the negative IV estimates in Table 3 taken together provide strong evidence that bribery in schools is regressive at the extensive margin. As discussed in the theoretical section above, the evidence contradicts any model based on the following set of assumptions: (i) the returns to education are higher for the rich, and their ability to pay is higher; (ii) the school official maximizes her income; (iii) the official can use costly testing to screen in the rich (or screen out the poor); and (iv) the enforcement regime is unbiased: punishment for corruption does not depend on household characteristics.

The disaggregated estimates in columns (2)–(4) of Table 3 show that the effect observed in aggregate propensity to pay is driven by bribery for admission into schools and payments with receipts; the estimates are not statistically significant at the 10% level for payments without receipt. When compared to the corresponding OLS estimates in Table 1, the estimates, especially for propensity to pay bribes for admission and payments with receipt, are numerically much larger. For example, the estimate for admission is −0.15 using the long-term rainfall IV, while it is only −0.0084 according to the OLS estimate. This is consistent with the insight that a negative OLS estimate provides a lower bound on

TABLE 3
REFUSAL TO PAY VS. DETERRENCE MODELS: ESTIMATES WITH LONG-TERM RAINFALL AS IV

	Propensity to pay bribe				Amount paid
	Aggregate (1)	Admission (2)	With receipt (3)	Without receipt (4)	(5)
Per capita income	−0.139**	−0.149**	−0.143***	−0.0853	0.0334
	(−2.251)	(−2.527)	(−2.647)	(−1.387)	(0.388)
First-stage *F*-statistic	11.23	11.23	11.23	11.23	12.41
Controls for moral cost heterogeneity					
Per capita income	−0.133**	−0.148**	−0.141***	−0.0812	0.0319
	(−2.136)	(−2.487)	(−2.596)	(−1.289)	(0.366)
First-stage *F*-statistic	11.14	11.14	11.14	11.14	12.35
Controls for village enforcement heterogeneity					
Per capita income	−0.129**	−0.149**	−0.141**	−0.0779	0.0336
	(−2.058)	(−2.437)	(−2.551)	(−1.202)	(0.365)
First-stage *F*-statistic	11.22	11.22	11.22	11.22	12.18
Household level controls	Yes	Yes	Yes	Yes	Yes
Regional fixed effects	Yes	Yes	Yes	Yes	Yes

Notes
Standard errors are clustered at primary sampling unit (village) level. Each cell in the rows labelled 'Per capita income' represents a result from a separate regression. Robust *t*-statistics in parentheses.
*, **, *** indicate significant at 10%, 5%, 1%, respectively.

TABLE 4
REFUSAL TO PAY VS. DETERRENCE MODELS: ROBUSTNESS CHECK (ESTIMATES FROM 2005–9 RAINFALL IV)

	Propensity to pay bribe				Amount paid
	Aggregate (1)	Admission (2)	With receipt (3)	Without receipt (4)	(5)
Rainfall shock IV					
Per capita income	−0.0115	−0.0650	−0.0495	−0.00174	0.0641
	(−0.124)	(−1.190)	(−0.849)	(−0.0180)	(0.358)
First-stage F-statistic	10.16	10.16	10.16	10.16	8.497
Long-term rainfall IV					
Per capita income	−0.116*	−0.142**	−0.119**	−0.0770	0.0654
	(−1.716)	(−2.258)	(−2.196)	(−1.107)	(0.632)
First-stage F-statistic	10.19	10.19	10.19	10.19	9.576

the true regressive effect. This vindicates the worry that the OLS estimates are biased towards finding less regressive effects of bribery because both the measurement error and omitted heterogeneity bias the estimate upwards when the true effect of income is negative.

Perhaps the most striking result in Table 3 relates to the effects of household permanent income on the amounts paid as bribes. In contrast to a statistically significant positive effect found in all of the OLS estimates in Table 1, the long-term rainfall IV estimates show that the effect is not statistically significant at the 10% level, and this conclusion is robust across three different specifications. The estimates in Table 3 thus suggest strongly that among those who end up paying bribes, the poor pay the same amount for educational services as the rich do. This has important implications: (i) it rejects the model based on bargaining power as deterrence, and provides support for the refusal-to-pay model; (ii) it is not consistent with any model where the school teacher is able to use a screening device to reveal ability-to-pay in a separating equilibrium; and (iii) it implies that bribes are regressive at the intensive margin, because a constant amount irrespective of income implies that the poor pay higher as a share of their income.

Set identification: bounds estimates

The estimates in Tables 2 and 3 are based on the standard exclusion restriction that the instrument has exactly a zero coefficient in the equation for the outcome of interest. Table 5 presents interval estimates for the causal effect under the assumption that the instrument can have some direct impact. To check the sensitivity of the IV estimates, we estimate the Conley *et al*. (2012) bounds for a number of alternative values for $\gamma^{\pm}$. We report the estimated lower and upper bounds on the causal effect of interest; panel A in Table 5 reports the estimates for the rainfall shock instrument (test of the ability-to-pay model), and panel B contains the bounds for the effects of permanent income on bribery in schools for the long-term rainfall instrument.

The estimates in panel A of Table 5 are strikingly consistent; the lower bound is negative, but the upper bound is positive for all the regressions, implying that a zero causal effect is contained in the interval of estimates across the board. This substantially strengthens the conclusions based on the evidence in Table 2 that ability to pay does not seem to play an important role in bribery in schools in Bangladesh. The estimates in

TABLE 5
RELAXING THE EXCLUSION RESTRICTION: CONLEY *ET AL*. (2012) APPROACH

		Propensity to pay bribe				Amount paid
		Aggregate	Admission	With receipt	Without receipt	
		(1)	(2)	(3)	(4)	(5)
Panel A: Bounds on estimates using rainfall shock instrument						
$\gamma^{\pm} = \pm 0.01\hat{\beta}$	Lower bound	−0.205	−0.182	−0.175	−0.189	−0.226
	Upper bound	0.141	0.031	0.046	0.164	0.387
$\gamma^{\pm} = \pm 0.05\hat{\beta}$	Lower bound	−0.205	−0.186	−0.178	−0.189	−0.229
	Upper bound	0.141	0.034	0.049	0.164	0.390
$\gamma^{\pm} = \pm 0.10\hat{\beta}$	Lower bound	−0.206	−0.191	−0.181	−0.189	−0.233
	Upper bound	0.142	0.037	0.052	0.165	0.393
Panel B: Bounds on estimates using long-term rainfall instrument						
$\gamma^{\pm} = \pm 0.01\hat{\beta}$	Lower bound	−0.256	−0.266	−0.248	−0.205	−0.139
	Upper bound	−0.010	−0.031	−0.034	0.043	0.203
$\gamma^{\pm} = \pm 0.05\hat{\beta}$	Lower bound	−0.259	−0.270	−0.252	−0.207	−0.140
	Upper bound	−0.008	−0.029	−0.032	0.044	0.204
$\gamma^{\pm} = \pm 0.10\hat{\beta}$	Lower bound	−0.262	−0.276	−0.257	−0.209	−0.141
	Upper bound	−0.005	−0.026	−0.029	0.047	0.204

Notes
$\hat{\beta} = \widehat{\beta_T}$ for columns (1)–(4) (see equation (5)), and $\hat{\beta} = \widehat{\theta_T}$ for column (5) (see equation (6)).

panel B, in contrast, show that the intervals do not contain zero in columns (1)–(3), corresponding to the aggregate propensity to bribe, the propensity to bribe for admission, and the propensity to bribe for payments with receipt. The intervals in columns(4) and (5), however, contain zero, thus we cannot reject the null hypotheses that the propensity to pay without receipts and the amounts paid as bribes do not depend on the economic status of a household. The evidence from the bounds estimates thus show that the main conclusions from the IV estimates in Tables 2–4 are not fragile; they are robust to allowing for some direct effect of the instruments on the outcome of interest.

Understanding the mechanisms

The focus of our analysis is to understand the effects of poverty on vulnerability to corruption in the context of bribery in schools. As discussed in detail above, the effects captured by the parameter β_T include a variety of mechanisms, and the evidence in Tables 2 and 3 is useful in sorting out the relative roles played by some of the mechanisms. From Table 2, we find that the ability-to-pay effect is approximately zero, that is, $\beta_2 = 0$. A comparison of the estimates in the top two panels of Table 3 suggests that the role played by moral costs heterogeneity is limited; the estimated effect of permanent income on the aggregate bribe propensity declines in magnitude marginally from −0.139 to −0.133 when we control for moral cost heterogeneity in the regression, and the estimates of the effects on the propensity to pay bribes for admission remain virtually identical. This can be interpreted as suggestive evidence that $\beta_M \rho_1 \simeq 0$. In order to understand the importance of weak enforcement regimes in poor villages, it is instructive to compare the estimates in the top panel of Table 3 (without controls for enforcement heterogeneity) with those in the third panel (which includes controls for enforcement heterogeneity). The estimated income effect for aggregate bribing propensity declines in magnitude from −0.139 to −0.129, which implies that the effect of enforcement heterogeneity is larger than that of moral costs heterogeneity. But the effects in the case of bribery for admission is again zero, similar to the case of moral cost heterogeneity.

As discussed earlier, it is not possible to check the sensitivity of the estimates by including indicators of ability such as education, as they are highly correlated with permanent income and would wipe off much of the causal effect in which we are interested. Thus the estimated effect of permanent income in the third panel of Table 3 represents the combined effect of 'pure bargaining power' and any endogenous response of ability to poverty. However, note that we expect $\beta_A \tau_1 > 0$, because $\beta_A > 0$ (higher ability implies higher returns to education and more willingness to pay) and $\tau_1 > 0$ (higher ability implies higher income, which improves nutrition and health, leading to higher ability).[22] Thus the estimated negative effect of permanent income should be interpreted as a lower bound estimate of the 'pure bargaining power effect'.

Magnitudes of the effects

The parameter estimates in Tables 1–3 provide marginal effects of an increase in income by a unit (by Tk 1000 since income is expressed in thousand taka) but they are sensitive to the unit of measurement of income. To provide a sense of the magnitude of the effects, we express income in US dollar terms and compute elasticities at the mean values of income and relevant propensity to pay bribes. The average per capita monthly income in our sample is around \$28, and a 1% increase in income is thus equivalent to a 28 cent increase. According to the estimates in Table 3, the largest effect of an increase in income by 1% is found for propensity to pay a bribe for admission. When considering total causal effects of household income (i.e. the estimate of the parameter β_T using long-term rainfall as the IV), a 1% increase in income reduces the propensity to pay a bribe for admission by 3.32%, for payments with receipt by 1.80%, and for payments without receipt by 0.46%. The decline in aggregate propensity to pay a bribe is about 0.61%.

VI. CONCLUSIONS

This paper provides a theoretical and empirical analysis of distributional effects of corruption in schools in developing countries where law enforcement is not impersonal or unbiased. The analysis yields both substantive and methodological insights.

The existing theoretical models, based on the assumption of unbiased enforcement, deliver the prediction that bribery is progressive at both the intensive and extensive margins. We develop two alternative models based on biased enforcement where the rich have more bargaining power. In the 'deterrence model', the official has information on household income, and higher income deters bribe demands. The poor are more likely to pay bribes for educational services (i.e. bribery is regressive at the extensive margin), but the rich pay more bribes for the same service. In contrast, in the 'refusal-to-pay model', the official does not observe household-level income and uses some group-level indicator of economic status. The rich (and powerful) can refuse to pay bribes once asked, and still obtain educational services. The testable predictions of the 'refusal-to-pay model' are that bribery is regressive at both the intensive and extensive margins.

The analysis is useful in sorting out the biases in the OLS estimates, and provides insights and guidelines for understanding the distributional effects of corruption at the household level. The OLS estimates are biased upwards because of omitted ability and preference heterogeneity, and the likelihood of finding spurious progressive incidence of bribery is higher when village fixed effects are used with the OLS estimator and when endogenous preference formation is important for moral costs of corruption. A negative OLS estimate of the effects of household income is sufficient to establish regressivity, implying that the existing studies based on OLS can be treated as credible evidence when the estimated income coefficient is negative and statistically significant. In contrast, a positive OLS coefficient is consistent with all three possibilities: the true effect of income is positive, zero or negative. Thus the available OLS estimates showing progressive incidence of corruption should be interpreted with due caution.

Using household-level data from Bangladesh, we provide suggestive evidence on the incidence of bribery on households. The OLS and IV estimates using short-term rainfall shock as instrument show that the canonical ability-to-pay model is not supported by the evidence. We provide estimates of the effects of 'being poor and living in a poor village' on bribery in schools by exploiting long-term rainfall differences across villages. The results show that corruption in schools is regressive at both the extensive and intensive margins. We find that the amount paid does not vary significantly with household income, which rejects the ability-to-pay model, screening models with separating equilibrium under unbiased enforcement, and a version of the bargaining power model under biased enforcement that assumes that the official observes household-level income (or wealth). The evidence on both the intensive and extensive margins is consistent with the 'refusal-to-pay model' where the official does not observe household-level income (and cannot use screening to reveal that information), and the bargaining power of a household manifests itself as the 'power to say no' once a bribe demand is made. These conclusions do not depend on the validity of the exact exclusion restriction imposed in the standard IV estimates. Bounds estimates from the Conley *et al.* (2012) set identification approach show that these conclusions are robust to allowing for some direct effect of the instrument on the outcome of interest.

Recent evidence shows that intergenerational correlation in schooling, a standard measure of immobility in education, shows no improvements in a large number of developing countries over the last few decades (Hertz *et al.* 2008; Emran and Shilpi 2015). In fact, in the case of Bangladesh, Hertz *et al.* (2008) find that intergenerational educational mobility has worsened over the years. This widening inequality in educational opportunity may seem difficult to reconcile with the standard theory developed by Becker and Tomes (1979) and Solon (2004), according to which policies such as free schooling should improve educational mobility and reduce inequality. Our analysis points to corruption in schools as a potentially important factor behind the persistence of educational immobility and inequality. Even though schooling is supposed to be free (or highly subsidized) for the poor to make the 'playing field' level, the evidence presented in this paper suggests that the burden of bribery in schools falls disproportionately on the poor households, and skews the 'playing field' against them.

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NOTES

1. According to a *New York Times* report (Levin 2012), not only is bribery rife in school admissions in China, but even the front row seats in the classroom are up for sale!
2. The conclusion that the rich—i.e. households with higher ability to pay—are more likely to pay bribes and pay larger amounts also arises in models where a bureaucrat uses a screening device such as red tape to reveal the ability to pay in a separating equilibrium. In these models, it is not possible for high-income households to pay fewer bribes if the bureaucrat is maximizing her income. See the discussion below on Banerjee (1997) and Banerjee *et al*. (2012).
3. This is valid *a fortiori* when one uses village fixed effects with OLS.
4. As noted by an anonymous referee, teachers may demand more bribes to supplement their income if floods adversely affected it. Later in the paper, we discuss why these two channels (teachers/administrators demanding money (i) for school repair after flood, (ii) to supplement their own income) are unlikely to be major sources of bias in the specific context of Bangladesh.
5. The early contributions to corruption literature include Rose-Ackerman (1978), Klitgaard (1988) and Shleifer and Vishny (1993).
6. Hunt and Laszlo (2012) model highlights that bribe payments can affect the quality of public services, which is important in healthcare but not in the context of schools, as a teacher cannot tailor a class lesson for a child according to the bribe paid by the parents.
7. Since the focus of our analysis is on household income, for most of what follows in this section, we will ignore φ_i.
8. This information assumption is common in many models where the official is able to extract the full surplus from the household.
9. The assumption of limited slots may not be appropriate in our context, as rural schools never refuse to admit someone because of congestion in the classroom. The fact that half of the children who are admitted into school do not pay bribes for admission also contradicts the limited slots assumption.
10. In fact, there is evidence that bribe rates for some public services in some countries look like market prices, because the same bribe is demanded irrespective of socioeconomic background of the briber (Rose-Ackerman 1978, 2010).
11. This captures the idea that if you are not able to refuse a demand for bribes, then it is likely that you have little bargaining power to resist the official extracting the surplus.
12. A large literature on intergenerational mobility shows that parent's and children's income, education and occupation are positively correlated. On developing countries, see Hertz *et al*. (2008), Emran and Shilpi (2011), and Emran *et al*. (2018), among others.
13. The available evidence from economics and behavioural genetics shows that the correlation in cognitive ability of parents and children is about 0.30–0.40, providing a firm basis for $\mathrm{Cov}(A_i, A_i^f) > 0$. See Black *et al*. (2009) and Plomin and Spinath (2004).
14. Perhaps more important for our conclusions is the fact that lower cognitive ability due to negative rainfall shocks would imply that the demand for education is lower, and poor households should be *less* likely to pay bribes, contrary to the findings reported below.
15. It is also rare that a teacher is a farmer in rural Bangladesh during the study period. Also, if the teacher needs financial help (or loans), then they go to the richest households, which are either large landlords or business owners. This would make the income effect positive (progressive). Note that part of the identifying variations that we use is at the household level (the interaction of rainfall with household characteristics), thus school-level changes in the demand for bribes cannot account for the variations in a given school.
16. For example, if an upazila/thana covers two half-degree grid cells for which the CRU has rainfall estimates, then upzila/thana rainfall is estimated as the average rainfall of the two grid cells, where the weights are the

proportions of the area of the upazila/thana in each grid cell. For details, see Bandyopadhyay and Skoufias (2012).

17. A PSU is a geographic unit approximately equal to a 1 square kilometre in our dataset. All the conclusions in this paper remain valid if we cluster the standard errors at the thana level, which is a somewhat larger geographic unit than the PSU.
18. Note that although Rangpur became the seventh division at the beginning of 2010, the NHSC 2010 data are organized based on the six divisions before 2010. The results and conclusions reported below, however, do not depend on the inclusion or exclusion of regional fixed effects.
19. The coefficient of the propensity to bribe for income-generating activities is positive and statistically significant at the 1% level in all regressions for propensity to pay bribes. Note that this controls for both the genetic and non-genetic components of preference, thus the estimates are lower bounds.
20. The average propensity to pay bribes for non-educational services by other households in the village has positive and statistically significant coefficients in the regressions for payments without receipt and aggregate propensity to pay bribes.
21. As pointed out recently by Kolesar *et al.* (2015), this procedure requires that only the predicted income satisfies the exclusion restriction, which is a weaker assumption than exclusion restrictions imposed on the individual instruments separately.
22. Recent evidence shows that poverty affects the development of the brain of a child in a significant way (see Noble *et al.* 2015).

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SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article:

(1) Proof of Proposition 1
(2) Heavy Rainfall and Interactions Based Instruments: What Do They Represent?
(3) Primary and Secondary Education in Rural Bangladesh