How Corruption Hits People When They Are Down *

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Abstract

Using cross–country and Peruvian data, I show that victims of misfortune, particularly crime victims, are much more likely than non–victims to bribe public officials. Misfortune increases victims' demand for public services, raising bribery indirectly, and also increases victims' propensity to bribe certain officials conditional on using them, possibly because victims are desperate, vulnerable, or demanding services particularly prone to corruption. The effect is strongest for bribery of the police, where the increase in bribery comes principally through increased use of the police. For the judiciary the effect is also strong, and for some misfortunes is composed equally of an increase in use and an increase in bribery conditional on use. The expense and disutility of bribing thus compound the misery brought by misfortune.

A combination of theoretical argument and empirical evidence has made a persuasive case that the high level of corruption in many developing countries reduces efficiency. For example, Mauro (1995) and later papers have shown that corruption reduces growth, and Wei (2000) has shown that corruption reduces foreign direct investment. Development specialists also fear that corruption reduces equity. In this context, the burden of corruption is usually considered inequitable or regressive if the poor pay a higher fraction of their income in bribes than the rich. Evidence on this point has only recently begun to emerge, and reveals different results for different countries: Hunt and Laszlo (2006) show that the burden of bribery is approximately constant by income in Peru, whereas Herrera et al. (2005) show that the burden is regressive in several African countries.

In this paper I explore a different dimension of equity. I document the degree to which people pay bribes in connection with misfortune or adverse events they experience, with the consequence that the expense and possible disutility of bribery compound the original problem. I do so by using the International Crime Victim Surveys (ICVS), with individual–level data from 37 transition, middle or low–income countries, and the Peruvian Household Survey (ENAHO), both of which contain information on bribery of public officials. The data sets are complementary, as the ICVS covers many countries and has detailed information on crime, while the Peruvian data set has a wealth of non–crime covariates and detailed information on bribery and especially use of public officials.

Misfortune or adverse events can lead an individual or household to bribe simply by increasing their need for public services. For example, victims of crime will want to report the crime to the police, an act that may require a bribe to ensure police cooperation. An illness, accident or assault may lead the victim to use public hospitals, which could involve a bribe to jump a queue or see a doctor. If a household member dies, his or her death must be registered, for which a bribe might be extorted. Burglary, robbery, fraud, job loss, fire, natural disasters, the death of an earner and the bankruptcy of a shop involve the loss of possessions or income, which may impoverish the affected individual or household and lead them to apply for unemployment insurance or welfare. The desertion of the household head can lead to legal issues concerning alimony or child custody, while the

creditors of a bankrupt shopkeeper may appeal to a judge. Involvement with the courts may be associated with bribery, for example of the opponent's lawyer.

Individuals or households with such problems may also be more likely to bribe than other users of the same public officials, however. They may be more vulnerable to extortion or more willing to bribe than other users. Alternatively, the heterogeneity of services provided by an institution such as the municipal government may be relevant: those who have just experienced an adverse event may use particularly corruption–prone services within a particular institution, such as the establishment of eligibility for welfare.

It is difficult to judge which misfortunes and problems are most likely to lead to increased bribery. The magnitude of the effect would depend not only on the severity of the problem, but also on the degree of corruption of the institution to which victims would have recourse. As the police force is a very corrupt institution in many countries, problems with which the police would be associated, such as crime, could be expected to lead to frequent bribery. The purpose of the paper, however, is to demonstrate the relevance of a wide range of adverse events for bribery, rather than to emphasize differences between adverse events.

I find that victims of misfortune, especially victims of crime, are substantially more likely to bribe than others, even after controlling for a wide range of individual and household characteristics. By contrast, victimization has no statistically significant effect on the amount paid in bribes, conditional on having paid a bribe. In the ICVS data, I find that crime victims are between 2.9 and 8.2 percentage points more likely to bribe an official than similar non–victims, depending on the crime, compared to an overall bribery rate of 12.5%. In Peru, a crime victim is 6.3 percentage points more likely to bribe than a similar non–victim, compared to an overall bribery rate of 4.9%. For victims of other misfortunes, the corresponding increase in the bribery rate generally lies between 2.2 percentage points for a job loss and 3.8 percentage points for the death of a household earner, although natural disasters have no effect and unspecified other misfortunes have an 8.9 percentage point effect. For victims of all misfortunes, the increase in bribery is highest for bribes paid to the police. While it is possible that those prone to misfortune

have unobservable characteristics making them more likely to bribe, the Peruvian results are almost all robust to adding household fixed effects to the subsample that is a panel: only the effect of serious illness appears to be overstated in the absence of fixed effects, while the effect of natural disasters may be understated.

The Peruvian data show that victims bribe more not only because they use more officials, but also because they bribe more than other users of the same officials. For the pairing of crime victims and the police, the combination with one of the highest bribery rates, the entire effect operates through greater use of the police. But for many pairings of misfortune and public official, the higher bribery compared to other users is at least as important as greater usage. This is true for victims of crime or shop bankruptcy in connection with the judiciary, and also for many victim types in connection with the municipal government.

In the case of crime victims and the police, it is evident that some of the higher usage must come from reporting the crime to the police. This appears not to be the only reason for increased usage, however, as the ICVS data indicate that crime victims who report their crime to the police do not have higher (or lower) bribery of the police than victims who do not report their crime. The Peruvian and ICVS results together suggest that crime victims who do not report their crime have an elevated police usage rate for reasons other than reporting. One possibility is that many unreported crimes are unreported because they are in fact perpetrated by the police. Another is that crime victims tend also to be perpetrators, and therefore come into contact with the police in their capacity as perpetrators, while being disinclined to report their own victimization to the police.

Individual micro–data have previously been used to show that richer households pay more frequent and larger bribes than poorer households, that people who know how to report corruption bribe less and that although bribers generally receive worse service, narrow benefits from bribing can be identified.[1] The results of the paper add to this limited knowledge of the process of bribery by individuals, and underline the extent to

[1]See Deininger and Mpuga (2005) for Uganda, Hunt (2006) for Peruvian hospitals, Hunt (2004) for the ICVS countries, Hunt and Laszlo (2006) for Peru and Thompson and Xavier (2002) for Kazakh hospitals.

which corruption lowers the quality of life by compounding other miseries. The results also reinforce other studies emphasizing the importance of combatting corruption in the police force, which is itself often set the task of reducing corruption in society.

1 Data

I use data from two sources: the International Crime Victim Surveys (ICVS) and the Peruvian household survey (ENAHO). In low–corruption countries, stigma may prevent respondents from answering honestly concerning bribery, so I drop high–income countries surveyed in the ICVS (see the Data Appendix for further discussion). In more corrupt countries, however, bribery is viewed as inevitable and the fault of the system, so stigma is low. Fear of prosecution (at least for small bribes, which are the majority of bribes) should also be low, as most anti–corruption campaigns target officials.

1.1 International Crime Victim Surveys

I use data spanning 1992–2001 on transition, middle and low–income countries from the International Crime Victim Surveys (ICVS), conducted for the United Nations Interregional Crime and Justice Research Institute. Interviews were conducted face–to–face with a randomly selected member of the household who reported his or her own individual victimization experience. About 60% of the observations are from countries making the transition from communism: Appendix Table 1 lists the full set of countries. Peru was not surveyed. In many countries the ICVS surveyed only particular neighborhoods, in the capital city. Neighborhoods were chosen based on economic status, rather than randomly, although the samples are random within neighborhoods.

The survey focuses on the details of respondents' experiences of criminal victimization in the previous calendar year, but also inquires about bribery in the previous calendar year. The question asked is: "In some countries, there is a problem of corruption among government or public officials. During 199x, has any government official, for instance a customs officer, a police officer or inspector in your country asked you, or expected you

to pay a bribe for his or her services?". Respondents who answer yes are then asked what type of government official was bribed (somewhat oddly, the first option is "government official"). The amount of the bribe is not asked. More importantly, the survey does not ask respondents if they used any public official in the past year: respondents who used but did not bribe an official cannot be distinguished from non–users. I use a sample of 55,019 individuals.

1.2 Peru's Encuesta Nacional de Hogares

The Encuesta Nacional de Hogares (ENAHO) is a household survey oversampling rural areas, conducted yearly by Peru's national statistical agency, the Instituto Nacional de Estadística e Información (INEI). I use the 2002 and 2003 surveys, which each contain over 18,000 households. In these years, the ENAHO included a module on governance in which one randomly chosen adult per household is asked numerous questions pertaining to the household's use of 21 different types of officials or institutions. If a particular type of official was used in the previous twelve months, respondents are asked a series of questions in connection with use and bribery of this official type in this time–frame, including whether the official asked for a bribe, gift, tip or "coima" (slang for bribe), whether the respondent felt obliged to make such a payment, made such a payment voluntarily, or refused to make such a payment, and the amount paid if she paid.

One quarter of the addresses surveyed in 2002 were also surveyed in 2003, and an indicator is provided to identify the cases where the household was the same in the two surveys. It is therefore possible to use a small panel of households. The mean characteristics of the panel households are virtually identical to the means of the full sample. The Data Appendix provides further information on the ENAHO data set.

2 Descriptive Statistics

Panel A column 1 of Table 1 shows that 46% of ICVS respondents reported having been a victim in the previous calendar year of one or more of the five main crime types: assault,

burglary, larceny, robbery and fraud. These crime victims account for 70% of all bribes (column 2), reflecting victims' bribery rate of 19.0%, compared to only 6.9% for non–victims (column 3). Panel B shows the equivalent for the Peruvian data. The Peruvian outcome of interest on which I focus is having bribed or refused to bribe, which I term a bribery episode. I include refusals in part because some may represent actual payments that respondents are reluctant to report, and in part because attempted bribery is also of interest. As shown in column 1, 20% of respondents reported that their household had been the victim of one of nine misfortunes in the previous twelve months. These victims accounted for 37% of bribery episodes (column 2). This overrepresentation reflects the 8.9% bribery (episode) rate for victim households compared with the 3.9% bribery (episode) rate for non–victim households (column 3).

Table 2 column 1 shows the share of individuals or households who were victims of the detailed misfortunes. In the ICVS (panel A), the most common crime by far is fraud, of which 29.4% of respondents were victim. The most common Peruvian misfortune (panel B) is natural disaster (such as drought etc.), of which 5.4% were victims. The least common misfortune is fire, affecting only 0.1%. A comparison of column 1 and column 2 in both panels shows that victims are generally two to three times overrepresented amongst bribers, with the exception of victims of natural disasters, who are not overrepresented. The bottom of column 1 shows that 13% of Peruvian households were victims who reported losing income as a result, 3.1% were victims reporting losing goods, and 2.6% were victims who reported losing both.

Columns 3–6 provide means of characteristics of households and individuals who were victims of the detailed misfortunes (and the means for those who were victim of no misfortune). Columns 3–5 of panel A shows that victims of crimes in the ICVS are more educated than non–victims, much more likely to be in a town with a population over one million, and slightly younger than non–victims. Column 6 shows that robbery was most frequently considered to be very serious by the victim (43% considered their robbery to be very serious, rather than fairly or not serious or unknown), while larceny was considered less serious than burglary and assault (fraud victims were not asked about the gravity of

their crime). Panel B, for Peru, indicates that respondents in crime victim households have similar education to non–victims and, oddly, that crime victims are only slightly more likely to be in cities with over 500,000 people than non–victims. The respondents are not younger than respondents in non–victim households, but the respondent is not necessarily the victim.

Respondents in job loss and shop bankruptcy households are considerably more educated than non–victims (panel B), and job losers are much more likely to live in large cities than non–victims. To have a shop bankruptcy one must be a shopkeeper, a relatively skilled job, and a job loss implies dependent employment, which is on average more skilled than self–employment. By contrast, victims of natural disaster are much less educated than non–victims and do not live in cities. As these farmers have such different observable characteristics from other victims, they may also have different unobservable characteristics. Column 6 of panel B shows that for most misfortunes, more than half of households reported a fall in their standard of living over the previous year (in response to a question asked not long after the misfortunes question), compared to only 23% of non–victim households. The least serious misfortunes in this regard are natural disaster, fire and crime, for which only 35–39% victims reported a fall in living standard.

Table 3 provides information on the types of official or institution bribed. In both data sets, the police receive the highest share of bribes, with 34–35% of the total. In Peru, at least, their large share of bribes is not the result of many users, since only 5.5% of households used the police in the previous twelve months (column 2 panel B), but is owing to the large share of users who bribe: 37.2% (column 3). For the judiciary the picture is similar, as only 4.1% of households used the judiciary, but the 16.6% of users who bribed brought the judiciary's share of bribes to 12% of the total. The municipal government and state schools, by contrast, have a large share of the bribes because of high usage rates. Together, these four official types account for 76% of all bribes in Peru.

Appendix Table 2 presents the means of all variables used in regressions with the ICVS data, while Appendix Tables 3 and 4 present the means of all variables used in regressions with the ENAHO data.

3 Empirical Methodology

3.1 ICVS

The basic regression for the ICVS is a probit for the probability of an individual i in country c paying a bribe in year t:

$$P(bribe_{ict}) = C_{ict}\beta_1 + X_{ict}\beta_2 + \delta_t + \gamma_c + \epsilon_{ict}. \quad (1)$$

C_{ict} contains dummies indicating whether the individual was a victim of five main crime types, and the coefficients β_1 are the coefficients of interest. All specifications include three year dummies (δ_t) and country dummies (γ_c). Since some neighborhoods are chosen based on city size and neighborhood affluence, I present only specifications that control for the respondent's income quartile and city size among the X controls.[2] All specifications also control for dummies for the size of the household (to adjust household income, to adjust for the under–representation of large households introduced by interviewing only one household member, and to take into account the number of people on whose behalf the respondent might potentially pay bribes). I adjust the standard errors to allow for correlation among observations in the same region of a country (to allow for serial correlation where more than one year of data on a region is present), and report marginal effects.

I then investigate how the determinants of bribes vary according to the recipient of the bribe by estimating multinomial logits with six categories: the first (omitted) for no bribe paid, and the remaining five for bribes paid to the five types of official.[3] I report odds ratios (exponentiated coefficients).

[2] I would like to control for the affluence of the neighborhood, but the regions I observe are generally considerably larger than the neighborhoods in question.

[3] In the multinomial logits the coefficients on the dummies of three low–bribery countries are ill-conditioned for some categories of official, so I group them with a neighboring country.

3.2 Peru's ENAHO

I estimate a similar probit for the probability that ENAHO household h in region r bribes an official type o in year t:

$$P(bribe\ episode_{hrot}) = M_{hrt}\beta_{3o} + X_{hrt}\beta_{4o} + t_t + \gamma_r + \eta_{hrot}. \tag{2}$$

M contains dummies for nine misfortunes of which the household might have been a victim, and its coefficients β_{3o} are the coefficients of interest. The 2003 survey year is represented by t_t, and γ_r represents 24 region dummies. Since sampling is stratified by city size, I only present specifications that include city size dummies among the X controls. I estimate five sets of probits for o representing any official, police, judiciary, municipal government and state schools. I adjust the standard errors to allow for correlation within districts (which are smaller than regions) and report marginal effects.

I also estimate a related probit where the seventeen officials other than police, judiciary, municipal government and schools are pooled, and the unit of observation is a household–official pair, rather than a household. In this case, I always present specifications controlling for official type dummies ν_o:

$$P(bribe\ episode_{hrot}) = M_{hrt}\beta_5 + X_{hrt}\beta_6 + t_t + \gamma_r + \nu_o + \zeta_{hrot}. \tag{3}$$

Despite the rich set of covariates for which I control, it is possible that in both (2) and (3) the coefficients on the crimes and other misfortunes are biased by the omission of variables. For example, if risk lovers are more likely to have shops, or if risk–loving shopkeepers are more likely to go bankrupt, and risk lovers always bribe more, the effect of shop bankruptcy on bribery will be biased upward by the omission of risk aversion. To the extent that risk aversion is a fixed effect, the bias caused by its omission may be removed by using the panel subsample of the data to estimate household fixed effects δ_h. The reduction in the sample size means that this is only meaningful for estimating the probability of bribing any official:

$$P(bribe\ episode_{hrot}) = M_{hrt}\beta_{7o} + X_{hrt}\beta_{8o} + t_t + \gamma_r + \delta_h + \phi_{hrot}. \tag{4}$$

I estimate this as a linear probability model. If the misfortune is so great as to change, say, a risk–lover into a risk–averse person, the bias caused by the omission of the original risk–aversion cannot be differenced out with fixed effects.[4]

While in principle the data permit a distinction between types of bribe and especially between bribes and refusals to bribe, in practice running multinomial logits instead of probits leads to large standard errors and insignificant marginal effects, so I do not pursue this.

It would be informative to present a decomposition of the unconditional bribery effect: if a household that is a victim of misfortune bribes more, this could be either because it simply uses more officials (which indirectly increases bribery), because it is more likely to bribe the officials it uses than other users, or some combination of the two. As a first step I rerun the regressions above changing the dependent variable to the probability of using an official, then rerun the bribery regressions above with the sample of households who used the official. Using the results of these regressions for a decomposition would be a simple matter were they linear. The probability of a bribery episode $P(B)$ is the product of the probability of using the official $P(U)$ and the probability of a bribery episode conditional on using the official $P(B|U)$:

$$P_j(B) = P_j(U)\ P_j(B|U), \tag{5}$$

where j represents V, for victims, or NV, for non–victims. The bribery gap between victims and non–victims is

$$P_V(B) - P_{NV}(B) = P_V(U)\ P_V(B|U) - P_{NV}(U)\ P_{NV}(B|U), \tag{6}$$

which can be rewritten as

$$P_V(U)\Delta P(B|U) + P_{NV}(B|U)\Delta P(U). \tag{7}$$

[4]Even if the household is the same in the two survey years, the respondent may not be. To the extent that the change in respondent changes the information conveyed, household fixed effects may not capture all relevant fixed effects. A matching algorithm would be needed to construct respondent fixed effects.

This decomposition is also valid conditional on X. However, it is not valid for non–linear regressions such as probits. Owing to the low probabilities often involved in the regressions, probit and linear probability estimates of coefficients differ, so I prefer to use probits and forgo an exact decomposition. However, I calculate $P(U)\Delta P(B|U,X)$ and $P(B|U)\Delta P(U|X)$, and call these the conditional bribery and usage components, even though they only approximately sum to $\Delta P(B|X)$. I calculate the share due to conditional bribery by calculating its share of the sum of the two components.

4 Results

4.1 ICVS

Table 4 reports the marginal effects, multiplied by 100, of criminal victimization on the unconditional probability of bribery in the ICVS. In column 1, the only other covariates are income quartile, household and city size dummies, while the remaining relevant individual and household covariates available in the ICVS are added in column 2. Whether the individual had been a victim of assault, burglary, larceny, robbery or consumer fraud in the previous year is strongly positively associated with the payment of bribes. The effects in column 1 range from 3.4 percentage points for bribery to 9.0 percentage points for fraud, compared to an overall bribery rate of 12.5%. The addition of the covariates in column 2 reduces the marginal effect of assault and larceny the most, and the magnitudes of the effects range from 2.9–3.1 percentage points for burglary and larceny to 8.2 percentage points for fraud. Although Table 2 indicated that victims consider robbery to be the most serious non–fraud crime, the effect of assault is as large as that for robbery.

One explanation for the victimization effects is that victims have to bribe the police when reporting the crime. This can be tested by adding to the specification interactions between the crimes and whether they were reported to the police, and checking whether the coefficients on the interactions are significantly positive. None of the interactions in column 3 has a significant marginal effect, and all except for fraud have a small point estimate. The interactions' marginal effects are also insignificant when the regressions are

run on a sample of victims only, when controls for whether the victim viewed the crime as very serious are added, and when larceny is broken down into its five component parts (these results are not reported).[5]

In column 4, I repeat the column 2 analysis using an expanded sample including surveys with inconsistent or missing information on the type of official bribed (which is not relevant for this table). The results are very similar in the two columns. In column 5, I repeat the specification of column 2 with the sample of Latin American countries, which may be more similar to Peru. The effect of assault appears larger in Latin America, but otherwise the results are similar. In unreported regressions, the effects of victimization in Latin America also do not vary according to whether the crime was reported or not.

In Table 5 I examine the effect of criminal victimization on the bribery of various types of official. The odds ratios in Table 5 are from a single multinomial logit, with an omitted category of no bribe. The full covariates are included. The top left odds ratio of 1.96, for example, means that a person who is the victim of assault is about twice as likely to bribe a government official as not to bribe (96% more likely), by comparison with a non–victim. The odds ratios are rather similar across columns, meaning that victimization causes similar percent changes in the probability of bribing various official types, rather than being higher for the police. This issue is discussed further below. The largest effects are for fraud, where the odds ratios are all above two, and the smallest effects are for larceny, where the odds ratios reflect bribery probabilities higher by only 31–59%.

In unreported multinomial logits, I have added interactions of the crimes with whether they were reported, to test whether crime victimization increases bribery through reporting. Of the 25 interactions, only four were significant, and one of these was significantly negative. I return to this puzzle later in the paper.

[5]Coefficients on interactions for reporting the crime to an authority other than the police are also insignificant. Controlling for these greatly reduces the sample size, however.

4.2 Peru's ENAHO

I begin by examining in some detail the effect of criminal victimization in the ENAHO in Tables 6–8, before turning to the effect of other misfortunes and adverse events.

4.2.1 Criminal Victimization

Each marginal effect in Table 6 (columns 1–4) is the marginal effect on criminal victimization from a different probit for the unconditional probability of a bribery episode. As in all subsequent tables, the marginal effects are multiplied by 100 and therefore represent percentage point effects.

In Table 6, as in Tables 7 and 8, I begin by controlling only for city size, year and other misfortunes (column 1). I then add many other characteristics of the household and the household respondent to the bribery questions (column 2), avoiding controlling for the value of household consumption, which is endogenous to misfortune, or variables that would proxy for consumption. In column 3 I control for dummies representing whether the household respondent reported that the household's living standard had risen or fallen over the previous twelve months, as well as for the equivalent questions for the community/town. It is possible that the misfortune dummies simply pick up the behavior of all households with a drop in their living standard and who might therefore qualify for welfare, for example. Finally, in column 4 I control for household consumption, the types of vehicles owned by the family (an important determinant of bribery to the police), the type of job held by the household respondent (self–employed in agriculture etc.) and dummies for whether the household received food aid in particular locations. The latter dummies include dummies for receiving food aid through the municipality or as a school breakfast or lunch. Because of the endogeneity of household consumption to misfortune, column 4 arguably overcontrols: consumption could fall in response to losses in income or wealth, or could rise temporarily as lost possessions are replaced.

Panel A shows the effect of criminal victimization on the probability of bribing (or refusing to bribe) any type of official. With minimal covariates (column 1) the effect

is 8.1 percentage points, while with maximum covariates (column 4) the effect is 6.3 percentage points, compared to an overall bribery episode probability of 4.9%. This large effect is similar to the effect of crime victimization found in the ICVS (between 2.9 and 8.2 percentage points in Table 4), where overall bribery is higher at 12.5%. The larger Peruvian effect relative to overall bribery is consistent with the possibility that the less detailed questioning of the ENAHO probably elicits reports of more serious crimes.

Panels B–F of Table 6 estimate the probability of a bribery episode for the four major official types and the seventeen pooled minor officials. One can see looking down the columns that the largest effects of criminal victimization are for the police, as would be expected, followed by the judiciary, municipal government, and schools. The effects for minor officials are almost an order of magnitude smaller. In column 4, the effect of criminal victimization on the police is 2.8 percentage points (panel B), a large effect compared to the 2.0% of households who bribe the police. The effect for the judiciary is 1.4 percentage points (panel C), also large compared to the 0.7% of households who bribe the judiciary.

The results of Table 6 may appear to contradict the ICVS results of Table 5, which showed that criminal victimization increased bribery by a similar magnitude for each official type. However, the multinomial logit of Table 5 gives results in terms of the percent increase in the probability, rather than the percentage point increases given in Table 6. The percent increases for the officials in Table 6 (column 4) are 140%, 200%, 58%, 40% and 47% for the police, judiciary, municipal government, schools and minor officials respectively. This transformation shows that the ICVS and ENAHO results are more similar than they appear, even if the ENAHO shows stronger evidence that the police and judiciary are more affected than other officials. The percentage point effects are more important for policy purposes, as they reflect how much overall bribery is increasing.

It is possible that criminal victimization is correlated with unobserved variables that cause individuals or households to bribe more. This may be tested by rerunning the regressions of Table 6 column 4 with the 4518 households for which two years of data are available, adding household fixed effects (I do not report these results). For the

probability of bribing any official, the probit marginal effect of criminal victimization is 2.1 percentage points (t-statistic of 2.4) without fixed effects; the linear probability equivalent is 3.3 percentage points (t-statistic of 2.1) and the linear fixed effects coefficient is 3.0 percentage points (t-statistic of 1.7). Thus, although adding fixed effects increases the standard error, the point estimate changes little. The results are similar if fixed effects are added to the specification of column 3 instead of column 4, suggesting that unobserved heterogeneity is not an important issue. The number of bribes for this subsample is small enough that little is significant when official types are examined separately.

I now turn to investigating how much of the unconditional bribery episode effects uncovered in Table 6 are the result of differences in usage of officials, and how much are the result of differences in bribery conditional on use of the official in question. The probits of Table 7 examine the probability of a bribery episode conditional on using an official. Panel A shows that for the police, there is no significant conditional bribery effect, and in fact, the point estimates are negative. For the judiciary (panel B), on the contrary, there is a very large conditional bribery effect that is not explained by covariates: in column 4, victims of crime are 15.3 percentage points more likely to bribe the judiciary than others users of the judiciary, compared to a bribery rate among users of 16.6%. There is a smaller significant effect of 2.1 percentage points in column 4 for the municipal government (panel C), compared to a bribery rate of 4.8%. The magnitude for schools and other officials are also large relative to their bribery rates.

In Table 8 I return to the sample of all households, and examine the association between criminal victimization and the probability of using an official, in order to gauge how much of the increase in unconditional bribery comes indirectly through increased use of officials. There is a significant and positive effect for all official types. The effect for the police is very large (panel A): victims of crime are 11.3 percentage points more likely to use the police than non–victims (column 4), compared to the 5.5% of households who use the police. The effects for the other three major official types are 4–5 percentage points (column 4), which is large compared to the judiciary usage rate (4.1%), but not compared with the usage rate for the municipal governments and schools (25.6% and 53.9%

respectively). The effect of 0.75 percentage points for the minor officials (column 4) is also small compared to their average usage rate of 9.7%.

The results of Tables 7 and 8 may be used to decompose the effect of misfortune on bribery into the indirect effect of higher usage of officials and the direct effect of higher bribery relative to other users. The direct effect of bribery represents -3% of the total effect for the police, 47% for the judiciary, 69% for the municipal government, and 87% for schools.

Hunt (2004), who used the ICVS data only, proposed that the reason for increased bribery of the police by crime victims regardless of whether they reported the crime was that crime victims lived in distrustful environments that also fostered bribery. However, as the Peruvian data show that the increased bribery comes wholly from a large increase in the usage of police, this interpretation seems unlikely to be correct.

4.2.2 Victims of All Misfortunes

In Table 9 I present the marginal effects of all nine misfortunes on the unconditional probability of a bribery episode. The marginal effects come from the regressions of column 4 in Table 6. The synthesis of the many numbers is that most misfortunes increase bribery, and bribes to many types of official are affected by many types of misfortune. The official type most affected is the police, and crime is the misfortune with the largest effect (apart from "other misfortunes"), despite its being among the less severe misfortunes in Table 2 in terms of effect on standard of living.

For the probability of a bribery episode in connection with any official, column 1 shows that the largest effects are the previously reported effect of criminal victimization, a 6.3 percentage point effect, and the effect of "other" misfortunes, a statistically indistinguishable 8.9 percentage point effect. The marginal effect of a fire is large at 4.9 percentage points, but the small number of fires leads to large standard errors and insignificance. Natural disasters have no significant effect, while the effects of other misfortunes lie in a range from 2.2 percentage points for a job loss to 3.8 percentage points for the death of an earner (within this range the effects are statistically indistinguishable).

Seven of nine misfortunes significantly increase the probability of bribing the police in column 2, with only the desertion of a household head and natural disasters insignificant. For the judiciary in column 3, the effects are somewhat smaller and four of nine are significant. For the municipal government in column 4, the effects are smaller still, and although again four of nine are significant, the pattern is different from that for the judiciary. For example, there is no effect of shop bankruptcy, unlike for the judiciary, whereas there is a significant effect of illness/accident, probably reflecting the use of municipal medical clinics. Effects for schools in column 5 are less significant, while the pooling of officials in column 6 leads to greater significance despite much smaller effects.

The fixed effects regressions on the panel subsample may again be used to shed light on the importance of unobserved heterogeneity for the probability of bribing any official (these results are not reported). Adding fixed effects to a linear probability regression reduces the coefficient for three misfortunes, increases it for five misfortunes, and changes it little for criminal victimization, suggesting there is no systematic upward bias. There is a hint in the case of natural disasters and illness/accident that adding fixed effects makes a qualitative difference, although the standard errors involved are large. For natural disasters, the point estimate rises from 0.5 to 2.5 percentage points, with the latter effect significant at the 11% level. The point estimate for illness or accident falls from 2.4 to -0.8, but the former effect is only significant at the 10% level.

I also check the robustness of the Table 9 marginal effects to reclassifying refusals to bribe as a non–bribe rather than a bribe: refusals are 22% of bribery episodes. For the bribery of any official, the new marginal effects are on average 94% of the column 1 effects. The results for the police, municipal government and judiciary are also robust to this change.

As with criminal victimization, I am interested in how much of the increase in bribery caused by misfortune is caused by greater use of officials, and how much is caused by increased bribery compared to other users. However, the latter effect (corresponding to the regressions of Table 7) is insignificant for most misfortunes and for schools. I therefore present results in Table 10 for selected misfortunes and officials with larger such effects.

All marginal effects in odd columns examining the probability of using an official are statistically significant except one. The largest effect (even considering the unreported misfortune effects) is for the pairing of criminal victimization and the police: a crime victim is 11.3 percentage points more likely to use the police, compared to an overall usage rate of 5.5%. Next largest are all the "other" misfortune effects, which range from 6.2 percentage points for the police to 9.0 percentage points for municipal government.[6]

Two misfortunes have a very large effect on the probability of bribing the judiciary conditional on using the judiciary (column 4). Criminal victimization raises the probability by 15.3 percentage points, as already reported, and a shop bankruptcy raises it by 12.3 percentage points, compared to the bribery rate of 16.6% among judiciary users. "Other" victimization raises conditional bribery of the police by 14.8 percentage points (though this is significant only at the 10% level, column 2), and several misfortunes raise conditional bribery of the municipal government by statistically significant amounts that are large compared with the conditional bribery rate of 4.8%: "other", illness/accident and crime (column 6).

In Table 11 I report the share of the overall bribery effect due to higher bribery of victims compared to other users (conditional bribery), for the misfortunes and officials of Table 10. The first row repeats numbers for criminal victimization cited in the text above. For the other four misfortunes, this component represents only a quarter of the effect on the police. For the judiciary the results are more varied. For job loss, illness/accident and "other", most of the effect comes through usage, while for shop bankruptcy and crime about half the effect is conditional bribery and half usage. For the municipal government, the shop bankruptcy effect comes mostly through usage, while for the other misfortunes the effects comes mostly through conditional bribery.

I have extended the analysis to seek effects of misfortune on the amount of a bribe

[6]Almost all misfortunes increase the usage of the police, judiciary and municipal government. The pairings for which the relationship is insignificant are fire–judiciary and sickness/accident–municipal government. Most misfortunes do not statistically significantly increase the usage of schools, while almost all increase the usage of the pooled other officials.

paid, conditional on a bribe being paid. However, all marginal effects are insignificant, owing to the limited sample size and associated large standard errors.

5 Conclusions

In this paper, I show that victims of misfortune and adverse events, particularly crime victims, are much more likely to bribe than non–victims. This holds even after conditioning on a variety of individual and household characteristics, as well as household fixed effects. The results hold with both cross–country and Peruvian micro–data. Victims of misfortune are more likely than non–victims to use public officials, particularly the police, which indirectly leads to more bribery. However, in many situations victims also bribe more than other users who are not victims. In such cases, victims may be more vulnerable or more desperate for service than other users, or they may have need of a more corrupt service (for example, establishing eligibility for benefits) than non–victims using the same institution. Whichever route leads the victim to the corrupt interaction with the official, the expense or disutility associated with the interaction compounds the original misfortune. People encounter corruption at the most difficult times of their lives, which is a form of inequity.

Misfortune especially spurs bribery of the police and judiciary. Of note among the detailed results is the frequent bribery of police by crime victims, caused by victims' very high use of police. The puzzle is that this high use or contact is apparently not principally the result of the reporting of crimes to the police, raising the possibility that the police commit crimes or that crime victims are also perpetrators of crime. The frequent bribery of the judiciary by crime victims and bankrupt shopkeepers is distinctive for the important role played by higher bribery compared to other users, as is the bribery of the municipal government by victims of several misfortunes.

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6 Data Appendix

6.1 International Crime Victim Surveys ICVS

The data are available at www.unicri.it/wwd/analysis/icvs/data.php.

I wish to drop countries with a sufficiently low bribery rate that stigma would impede honest answers to the survey. Low bribery countries are almost all rich countries, and I prefer to use GDP per capita as the criterion for inclusion rather than bribery, which will be the dependent variable. This means South Korea and Malta are excluded despite having higher bribery than two included countries. I include Slovenia, whose GDP per capita is similar to that of Malta and South Korea but whose bribery rate is lower, in order to include the complete set of European ex-communist countries. For most of the analysis I also drop 19 surveys (five of them for countries not otherwise represented in the sample) where the list of official types presented to respondents who bribed was longer than in the other surveys, or where the official type information was missing.

I recode victims of crimes occurring abroad as non–victims. In many of the surveys of cities, the variable called city size appeared to refer to the population of the neighborhood, not the city, or was missing. Using www.citypopulation.de and the region variable, I corrected these observations.

6.2 Peru's ENAHO

The data are available at www.inei.gob.pe/srienaho/English/Consulta_por_Encuesta.asp.

The 2002 survey was taken in October, November and December of 2002. The "2003" survey was taken from May 2003 to April 2004. One quarter of the 2003 households were also interviewed in 2002. I simply combine monetary values from surveys taken at different times with no adjustment for inflation or seasonality, which tests indicated was appropriate for household consumption. A noteworthy discrepancy between 2002 and 2003 is a leap in the share of households reporting in the bribery module that they had used a state hospital, apparently due to more complete reporting. Whenever I control for official type dummies, I therefore also permit an interaction of the state hospital dummy with a dummy for the survey year 2003. Household consumption, computed by the statistical agency, is based on the survey's 31 pages of questions on household expenditure and consumption. The bribery module was also included in the 2004 and 2005 surveys, but the bribery data have not been released with the rest of the data.

The twenty–one types of official listed in the survey are: municipal government, social security (providing social insurance other than pensions), state banks, judiciary, drinking water, telephone, electricity, state schools, arbitration, Ministry of Agriculture, Ministry of Industry and Tourism, tax/customs authority (SUNAT), state hospitals, national civil identification registry, Department of Migration, police, electoral office (ONPE), electoral court (JNE), development agency (FONCODES), food agency (PRONAA), and "other".

6.3 Are the bribery rates plausible?

While the bribery rates in some of the ICVS surveys are low – only 1.4% in Slovenia and 3.0–3.1% in Botswana and Hungary, for example – the high bribery rates in some countries suggest respondents were not afraid to answer the ICVS questions. The Peruvian ENAHO bribery (episode) rate of 4.9% is lower than the lowest ICVS Latin American bribery rate (of 5.3% for Argentina in 2001), raising the possibility that respondents may have been less eager to answer a government–sponsored survey. However, the ENAHO bribery episode rates for some official types are very high (37% for the police), indicating that, at least for some official types, respondents were not ashamed or afraid to acknowledge a bribery episode. The stigma associated with bribery in Peru is not large: Proética, a Peruvian anti–corruption group, found that when asked to define the Peruvian slang for bribe ("coima"), less than half their survey respondents gave answers with a negative connotation.[7] Furthermore, the Peruvian household survey does not attempt to force respondents to admit to having voluntarily paid a bribe, but allows them merely to acknowledge having paid a tip under duress. I therefore do not believe that reluctance to report is a major issue.

A 2004 Transparency International survey of 416 respondents in greater Lima found 14% of respondents had bribed in the previous twelve months, compared to 6.0 per cent among the 3758 Lima respondents in my 2002–2003 data.[8] However, the Transparency question did not restrict itself to bribes paid to public officials. Proética reports much higher bribery rates for the years 2002, 2003 and 2004 of 32%, 29% and 27%, respectively.[9] Proética's bribery rates, conditional on the use of particular officials, look very similar to those in my data, but their usage rates look implausibly high for a window of one year. This suggests that the Proética time frame, not reported in the documentation available to me, was in fact much longer than a year, even though yearly bribery rates are reported.

The share of households or individuals bribing and the number of bribes will be understated if clients commonly use agents to act as intermediaries between themselves and officials, and bribes paid by the agent are reported in the survey by the agent (or no-one), rather than the client. A 2003 survey by Proética gathered information on bribes and agents ("tramitadores") in Peru.[10] 52% of respondents who had bribed to obtain a driver's licence reported having paid the bribe to an agent, while the share was 15% or less for the other nine activities reported in the summary statistics.[11] The number of bribery episodes per household is also understated because each respondent can only report one bribery episode per year (per year per official, in the Peruvian case). Another issue is that corrupt officials may demand a fee that clients do not recognize as a bribe. Taking the various factors into consideration, it seems inevitable that my surveys somewhat underestimate bribery of public officials, but I do not believe the underestimation to be severe.

[7] Proética (2004).
[8] Transparency (2004).
[9] Proética (2004).
[10] Proética (2003).
[11] Bertrand et al. (2005) analyze the use of agents for obtaining drivers' licences in India.

Table 1: Bribery by victimization status

A. ICVS	(1) Share individuals in category	(2) Category's share of bribes	(3) Share of category bribing
Victim of any of five crimes	46%	70%	19.0%
Not victim	54%	30%	6.9%
All individuals	100%	100%	12.5%
B. ENAHO (Peru)	Share households in category	Category's share of bribery episodes	Share of category with bribery episode
Victim of any of nine misfortunes	20%	37%	8.9%
Not victim	80%	63%	3.9%
All households	100%	100%	4.9%

Notes: Based on 55,019 individuals in the ICVS and 35,964 households in the ENAHO. The five ICVS crimes are assault, burglary, larceny, robbery, and fraud. The nine household ENAHO misfortunes are criminal victimization, job loss, bankruptcy of a shop, death of an earner, serious illness or accident, desertion of household head, fire in house, shop or property, natural disaster and "other". Bribery episodes include both bribes and refusals to bribe.

Table 2: Criminal victimization and other misfortunes

A. ICVS	(1) All individuals	(2) Bribed	(3)	(4)	(5)	(6)
	Share in category		Means for individuals who are a victim of listed misfortune			
			Education (years)	In town above 1 million	Age (1=16-19, 12=70+)	Crime was very serious
Assault	0.041	0.080	11.4	0.46	4.7	0.34
Burglary	0.074	0.121	11.2	0.40	5.2	0.33
Larceny	0.173	0.277	12.0	0.44	5.2	0.28
Robbery	0.027	0.051	11.3	0.52	4.9	0.43
Fraud	0.294	0.508	12.2	0.48	5.5	--
No crime	0.539	0.300	10.5	0.34	6.2	--
Observations	55,019	6884	--	--	--	--
B. ENAHO (Peru)	All house-holds	Bribery episode	Means for households which are victim of listed misfortune			
	Share in category		Education (years)	In town above 500,000	Age (years)	Living standard fell
Crime	0.034	0.098	8.1	0.18	41	0.39
Job lost	0.042	0.085	9.8	0.35	38	0.55
Shop bankruptcy	0.013	0.034	9.3	0.19	39	0.58
Death of earner	0.010	0.019	7.3	0.11	43	0.51
Serious illness, accident	0.045	0.083	7.5	0.17	43	0.50
Desertion household head	0.007	0.012	8.4	0.17	34	0.55
Fire in house shop property	0.001	0.003	7.0	0.15	40	0.36
Natural disaster	0.054	0.055	5.4	0.01	44	0.35
Other misfortune	0.008	0.028	7.9	0.14	42	0.52
No misfortune	0.801	0.365	7.8	0.15	41	0.23
Lost income if had misfortune	0.131	0.227	--	--	--	--
Lost goods if had misfortune	0.031	0.060	--	--	--	--
Lost income and goods if had misfortune	0.026	0.055	--	--	--	--
Lost neither income nor goods if had misfortune	0.012	0.024	--	--	--	--
Observations	35,964	1757	--	--	--	--

Notes: Examples of crimes given in the ENAHO questionnaire are robbery and assault. Examples of natural disasters given in the ENAHO questionnaire are drought, storm, plague [of insects], flood. Household size is censored at 6 or more in the ICVS data. The means for whether the crime was considered very serious are computed over the non-missing values (about 15% are missing). The question was not asked of victims of fraud.

Table 3: Bribery and usage of different types of official

A. ICVS	(1) Share of bribes	(2) Share households using official type	(3) Share users bribing	(4) Number of bribes
Government official	22%	--	--	1491
Customs official	10%	--	--	711
Police officer	34%	--	--	2362
Inspector	11%	--	--	744
Other	23%	--	--	1576
Total	100%	--	--	6884
B. ENAHO (Peru)	Share of bribery episodes	Share households using official type	Share users with bribery episode	Number of bribery episodes
Police	35%	0.055	0.372	735
Judiciary	12%	0.041	0.166	244
Municipal government	21%	0.256	0.048	440
State schools	8%	0.539	0.008	162
Other officials	24%	0.097	0.009	524
Total	100%	--	--	2105

Note: In panel B for other officials, columns 2 and 3 give the unweighted average across the different official types. The other officials are in social security, state banks, water, telephone, electricity, arbitration, Ministry of Agriculture, Ministry of Industry and Tourism, SUNAT (customs and taxes), state hospitals, ID agency, Department of Migration, Election Office (ONPE), Election Court (JNE), development agency (FONCODES), nutritional welfare agency (PRONAA) and other.

Table 4: Effect of victimization on bribery in ICVS

	(1)	(2)	(3)	(4)	(5)
Assaulted	7.51	5.79	5.69	5.38	9.66
	(10.6)	(9.1)	(8.0)	(8.6)	(4.2)
Assaulted *	--	--	0.32	--	--
reported assault to police			(0.3)		
Burgled	3.36	2.93	2.92	2.94	3.34
	(7.0)	(6.5)	(5.5)	(7.0)	(2.3)
Burgled *	--	--	0.03	--	--
reported burglary to police			(0.1)		
Larceny victim	4.89	3.13	3.33	3.11	3.40
	(14.8)	(9.2)	(7.5)	(11.2)	(2.6)
Larceny victim *	--	--	-0.54	--	--
reported larceny to police			(-0.6)		
Robbed	6.14	5.14	4.94	5.14	6.26
	(6.0)	(5.5)	(4.9)	(6.5)	(2.5)
Robbed *	--	--	0.48	--	--
reported robbery to police			(0.5)		
Defrauded	9.03	8.16	8.08	8.72	8.68
	(15.1)	(15.1)	(14.9)	(17.0)	(7.7)
Defrauded *	--	--	1.67	--	--
reported fraud to police			(1.5)		
Income quartile, household, city size	Yes	Yes	Yes	Yes	Yes
Other covariates	No	Yes	Yes	Yes	Yes
Countries	All	All	All	Extended	Latin America
R-squared	0.13	0.17	0.17	0.18	0.11
Observations		55,019		78,383	7026

Notes: Marginal effects of probits, multiplied by 100. T-statistics are reported in parentheses, adjusted for correlation within regions of countries. All regressions include three year dummies, household size dummies, country dummies, and a missing income quartile dummy. The "other covariates" are respondent sex, age, education and labor force status, and household ownership of vehicles. The sample of extended countries also includes those countries whose survey offered respondents who bribed a longer list of official types; in the specification for this sample the covariates include a dummy indicating these countries.

Table 5: Effect of victimization on bribery of different types of official in ICVS

	(1) Gov't official	(2) Customs	(3) Police	(4) Inspector	(5) Other
Assaulted	1.96	1.95	1.66	1.62	1.64
	(5.9)	(4.9)	(5.8)	(3.4)	(4.7)
Burgled	1.49	1.44	1.38	1.15	1.34
	(4.6)	(3.2)	(3.5)	(1.1)	(3.7)
Victim of larceny	1.31	1.34	1.50	1.59	1.43
	(4.0)	(3.3)	(6.7)	(4.3)	(5.7)
Robbed	1.40	2.37	1.84	1.31	1.35
	(2.2)	(4.5)	(5.9)	(1.2)	(1.8)
Defrauded	2.49	2.21	2.15	2.50	2.54
	(10.7)	(8.8)	(10.1)	(7.4)	(10.4)

Notes: Multinomial logit odds ratios. 55,019 observations. R^2=0.17. T-statistics are reported in parentheses, adjusted for correlation within regions of countries. The omitted category is no bribe. The coefficients on Indonesia and Panama, for Inspector, and Brazil, for Other, are constrained to be zero. The country dummies for Botswana and South Africa, and Estonia, Slovenia and Hungary are combined. The unreported covariates are three year dummies; respondent sex, age, education and labor force status; household ownership of vehicles, size, income quartile, and city size.

Table 6: Effect of household criminal victimization on probability of a bribery episode in Peru's ENAHO

	Observations	Mean dependent variable	(1)	(2)	(3)	(4)
A. Any official	35,964	0.049	8.06	7.42	6.93	6.32
			(10.9)	(10.2)	(9.8)	(9.4)
R-squared			0.04	0.09	0.10	0.11
B. Police	35,964	0.020	4.72	3.35	3.22	2.81
			(9.9)	(9.2)	(8.9)	(8.5)
R-squared			0.06	0.14	0.14	0.16
C. Judiciary	35,964	0.007	2.32	1.76	1.61	1.41
			(7.7)	(7.7)	(7.3)	(7.2)
R-squared			0.06	0.11	0.11	0.14
D. Municipal government	35,964	0.012	1.20	0.92	0.83	0.71
			(3.8)	(3.9)	(3.6)	(3.3)
R-squared			0.03	0.08	0.09	0.10
E. Schools	35,964	0.005	0.46	0.26	0.22	0.20
			(2.6)	(2.3)	(2.1)	(2.1)
R-squared			0.02	0.10	0.11	0.12
F. Other officials	611,262	0.0009	0.066	0.050	0.045	0.037
(17 types pooled)			(4.5)	(4.3)	(4.1)	(3.9)
R-squared			0.07	0.11	0.11	0.12
Other household misfortunes			Yes	Yes	Yes	Yes
City size, year dummies			Yes	Yes	Yes	Yes
Official type dummies [panel F]			Yes	Yes	Yes	Yes
Main household, respondent characteristics			--	Yes	Yes	Yes
Change in household, town living standard dummies			--	--	Yes	Yes
Consumption, vehicle ownership, food aid, job type			--	--	--	Yes

Notes: Columns 1-4 contain marginal effects from probits, multiplied by 100. Each marginal effect is the marginal effect on the dummy for household criminal victimization from a different regression. T-statistics clustered by district are in parentheses. In panels A-E an observation is a household; in panel F it is a household-official pair. One region had no bribes to school officials and is combined with a neighboring region for the schools regressions containing region dummies. "Main household, respondent characteristics" are controls for age of respondent, its square, and respondent years of education; dummies for respondent sex, marital status, sex*marital status, and occupation in public administration; dummies for household region (24), size, acquisition of home through land invasion, presence of children aged 0-3, 4-7, 8-11, or 12-15; and a control for travel time to the district administrative center.

Table 7: Effect of household criminal victimization on probability of a bribery episode conditional on using official in Peru's ENAHO

	Observations	Mean dependent variable	(1)	(2)	(3)	(4)
A. Police	1977	0.372	-0.78	-0.20	-1.04	-1.91
			(-0.2)	(-0.1)	(-0.3)	(-0.5)
R-squared			0.02	0.09	0.09	0.11
B. Judiciary	1470	0.166	15.41	14.72	14.16	15.27
			(3.6)	(3.6)	(3.5)	(3.7)
R-squared			0.03	0.10	0.10	0.12
C. Municipal government	9201	0.048	2.82	2.52	2.31	2.08
			(2.7)	(2.7)	(2.5)	(2.3)
R-squared			0.03	0.08	0.09	0.09
D. Schools	19,367	0.008	0.75	0.48	0.41	0.37
			(2.3)	(2.1)	(1.8)	(1.8)
R-squared			0.03	0.09	0.10	0.12
E. Other officials	59,468	0.009	0.63	0.51	0.48	0.46
(17 types pooled)			(3.6)	(3.3)	(3.2)	(3.2)
R-squared			0.10	0.13	0.13	0.14
Other household misfortunes			Yes	Yes	Yes	Yes
City size, year dummies			Yes	Yes	Yes	Yes
Official type dummies [panel E]			Yes	Yes	Yes	Yes
Main household, respondent characteristics			--	Yes	Yes	Yes
Change in household, town living standard dummies			--	--	Yes	Yes
Consumption, job type, vehicle ownership, food aid			--	--	--	Yes

Notes: Columns 1-4 contain marginal effects from probits, multiplied by 100. Each marginal effect is the marginal effect on the dummy for household criminal victimization from a different regression. T-statistics clustered by district are in parentheses. In panels A-E an observation is a household; in panel E it is a household-official pair. One region had no bribes to school officials and is combined with a neighboring region for the schools regressions containing region dummies. "Main household, respondent characteristics" are controls for age of respondent, its square, and respondent years of education; dummies for respondent sex, marital status, sex*marital status, and occupation in public administration; dummies for household region (24), size, acquisition of home through land invasion, presence of children aged 0-3, 4-7, 8-11, or 12-15; and a control for travel time to the district administrative center.

Table 8: Effect of household criminal victimization on probability of using official in Peru's ENAHO

	Observations	Mean dependent variable	(1)	(2)	(3)	(4)
A. Police	35,964	0.055	14.31	12.55	12.04	11.30
			(18.2)	(17.1)	(16.7)	(16.2)
R-squared			0.07	0.11	0.11	0.13
B. Judiciary	35,964	0.041	6.04	5.32	5.04	4.50
			(8.8)	(8.5)	(8.3)	(7.8)
R-squared			0.04	0.07	0.07	0.09
C. Municipal government	35,964	0.256	7.61	6.54	6.30	5.02
			(5.7)	(5.0)	(4.8)	(3.9)
R-squared			0.02	0.07	0.07	0.09
D. Schools	35,964	0.539	3.10	4.45	4.29	4.03
			(2.1)	(2.7)	(2.6)	(2.5)
R-squared			0.02	0.31	0.31	0.32
E. Other officials	611,262	0.097	0.97	1.07	1.02	0.75
(17 types pooled)			(4.9)	(6.8)	(6.6)	(5.3)
R-squared			0.28	0.30	0.30	0.31
Other household misfortunes			Yes	Yes	Yes	Yes
City size, year dummies			Yes	Yes	Yes	Yes
Official type dummies [panel E]			Yes	Yes	Yes	Yes
Main household, respondent characteristics			--	Yes	Yes	Yes
Change in household, town living standard dummies			--	--	Yes	Yes
Consumption, job type, vehicle ownership, food aid			--	--	--	Yes

Notes: Columns 1-4 contain marginal effects from probits, multiplied by 100. Each marginal effect is the marginal effect on the dummy for household criminal victimization from a different regression. T-statistics clustered by district are in parentheses. In panels A-D an observation is a household; in panel E it is a household-official pair. "Main household, respondent characteristics" are controls for age of respondent, its square, and respondent years of education; dummies for respondent sex, marital status, sex*marital status, and occupation in public administration; dummies for household region (24), size, acquisition of home through land invasion, presence of children aged 0-3, 4-7, 8-11, or 12-15; and a control for travel time to the district administrative center.

Table 9: Effect of misfortunes on probability of a bribery episode in Peru's ENAHO

	(1) Any official	(2) Police	(3) Judiciary	(4) Municipal gov't	(5) Schools	(6) Other officials
Victim of crime	6.32	2.81	1.41	0.71	0.20	0.037
	(9.4)	(8.5)	(7.2)	(3.3)	(2.1)	(3.9)
Job lost	2.24	0.89	0.24	0.48	0.08	0.016
	(5.6)	(4.0)	(2.3)	(2.3)	(0.9)	(2.2)
Shop bankrupted	3.14	0.94	0.74	0.34	0.32	0.014
	(4.1)	(2.9)	(3.7)	(1.2)	(1.9)	(1.2)
Death of earner	3.84	1.41	0.04	0.79	0.34	0.050
	(3.9)	(2.6)	(0.1)	(1.9)	(1.7)	(2.6)
Illness, accident	2.85	1.03	0.11	0.66	0.13	0.039
	(6.1)	(3.9)	(1.1)	(3.2)	(1.5)	(4.7)
Desertion of household head	2.34	0.50	0.30	0.45	0.39	-0.004
	(2.2)	(1.0)	(1.2)	(1.1)	(1.8)	(-0.3)
Fire in house, shop, property	4.89	4.23	2.20	--	--	0.041
	(1.6)	(2.3)	(1.9)			(1.0)
Natural disaster	0.38	0.22	0.06	-0.01	0.07	0.020
	(0.8)	(0.8)	(0.5)	(-0.0)	(0.8)	(2.6)
Other misfortune	8.85	3.28	1.34	2.17	0.70	0.058
	(7.6)	(5.4)	(4.8)	(4.3)	(2.7)	(3.3)
City size, year dummies	Yes	Yes	Yes	Yes	Yes	Yes
All other covariates	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.11	0.16	0.14	0.10	0.12	0.16
Observations			35,964			611,262

Notes: Marginal effects from probits, multiplied by 100. T-statistics clustered by district are in parentheses. In columns 1-5 an observation is a household; in column 6 it is a household-official pair. There are too few bribes to municipal government and schools by victims of fire to include a dummy for this misfortune in the bribery regressions for municipal government and schools. "All other covariates" are controls for age of respondent, its square, and respondent years of education; dummies for respondent sex, marital status, sex*marital status, job type and occupation in public administration; dummies for household region, size, acquisition of home through land invasion, presence of children aged 0-3, 4-7, 8-11, or 12-15, change in standard of living (including town) and food aid locations; controls for travel time to the district administrative center and log household consumption; and for column 6, official type dummies.

Table 10: Effect of misfortunes on the probability of using official and the probability of a bribery episode conditional on using official in Peru's ENAHO

	(1)	(2)	(3)	(4)	(5)	(6)
	Police		Judiciary		Municipal gov't	
	Usage	Bribery episode if usage	Usage	Bribery episode if usage	Usage	Bribery episode if usage
Victim of crime	11.30	-1.91	4.50	15.27	5.02	2.08
	(16.2)	(-0.5)	(7.8)	(3.7)	(3.9)	(2.3)
Job lost	2.47	5.40	2.28	0.75	4.62	1.26
	(5.2)	(1.3)	(5.9)	(0.3)	(4.2)	(1.5)
Shop bankrupted	2.36	5.56	2.12	12.27	7.03	0.45
	(3.1)	(0.9)	(2.9)	(2.4)	(3.5)	(0.4)
Illness, accident	2.81	5.85	1.53	0.60	2.17	2.36
	(5.1)	(1.2)	(3.7)	(0.2)	(1.7)	(2.5)
Other misfortune	6.20	14.79	7.82	6.44	9.00	6.64
	(5.3)	(1.8)	(6.7)	(1.1)	(3.4)	(3.5)
City size, year dummies	Yes	Yes	Yes	Yes	Yes	Yes
All other covariates	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.13	0.11	0.09	0.12	0.09	0.09
Observations	35,964	1977	35,964	1470	35,964	9201

Notes: Marginal effects from probits, multiplied by 100. T-statistics clustered by district are in parentheses. An observation is a household. "All other covariates" are controls for age of respondent, its square, and respondent years of education; dummies for respondent sex, marital status, sex*marital status, job type and occupation in public administration; dummies for household region, size, acquisition of home through land invasion, presence of children aged 0-3, 4-7, 8-11, or 12-15, change in standard of living (including town) and food aid locations; and controls for travel time to the district administrative center and log household consumption.

Table 11: Share of misfortune's effect working through bribery conditional on use of official rather than through usage in Peru's ENAHO

	(1) Police	(2) Judiciary	(3) Municipal government	(4) Schools
Victim of crime	-3%	47%	69%	87%
Job lost	24%	8%	59%	--
Shop bankrupted	26%	59%	25%	--
Illness, accident	24%	9%	85%	--
Other misfortune	26%	17%	80%	--

Notes: The contribution of bribery conditional on usage is computed as the marginal effect of the misfortune on bribery conditional on use (Table 7 or 10) multiplied by the share of users using the official type. The contribution of usage is computed as the marginal effect of the misfortune on usage of the official (Table 8 or 9) multiplied by the share of users of that official type who have a bribery episode. Shares for schools are reported only for victims of crime, as the bribery conditional on use effects are estimated too imprecisely for the other misfortunes.

Appendix Table 1: Countries and survey years in ICVS sample

Baltic

Estonia (1995), Latvia (1996, 2000), Lithuania (1997).

Central and Eastern Europe

Czech Republic (1996), Hungary (1996), Poland (1992, 1996, 2000, 2000), Slovakia (1997).

Balkans

Albania (1996), Bulgaria (1997), Croatia (1997), Macedonia (1996), Romania (1996), Slovenia (1997), Yugoslavia (1996).

Former Soviet Union

Azerbaijan (2000), Belarus (1997), Georgia (1996, 2000), Kyrgyzstan (1996), Russia (1996), Ukraine (1997).

Latin America

Argentina (1996, 2001), Bolivia (1996), Brazil (1996), Colombia (1997, 2000), Costa Rica (1996), Panama (2000), Paraguay (1996).

Africa

Botswana (1997), Nigeria (1998), South Africa (1996), Uganda (1996), Zimbabwe (1996).

Asia

Cambodia (2001), India (1996), Indonesia (1996), Mongolia (1996, 2000), Philippines (1996, 2000).

Appendix Table 2: Means of variables in ICVS (Standard deviations are in parentheses)

	Full sample	Bribed
Top inc quartile	0.22	0.35
4th inc quartile	0.18	0.17
3rd inc quartile	0.23	0.20
Bottom inc quartile	0.26	0.17
Income missing	0.10	0.11
City <10,000	0.12	0.03
City 10-50,000	0.07	0.05
City 50-100,000	0.07	0.06
City 100-500,000	0.19	0.17
City 500-1,000,000	0.17	0.18
City 1,000,000+	0.39	0.51
Own one car	0.33	0.38
Own two cars	0.07	0.12
Own three or more cars	0.02	0.04
Own motorcycle or moped	0.13	0.21
Own bike	0.49	0.53
Age 16-19	0.06	0.06
Age 20-24	0.11	0.16
Age 25-29	0.11	0.16
Age 30-34	0.11	0.15
Age 35-39	0.11	0.14
Age 40-44	0.10	0.10
Age 45-49	0.09	0.09
Age 50-54	0.07	0.06
Age 55-59	0.06	0.04
Age 60-64	0.05	0.02
Age 65-69	0.05	0.01
Age 70+	0.07	0.01
Sex (female=1)	0.55	0.41
Education (years)	11.1 (4.1)	12.3 (4.1)
Working	0.47	0.57
Looking for work	0.10	0.11
Keeping house	0.12	0.09
Retired/disabled	0.19	0.06
Student	0.08	0.11
Other	0.05	0.06
Household size (censored at 6)	3.7 (1.6)	4.0 (1.5)
Assaulted * reported assault to police	0.010	0.018
Burgled * reported burglary to police	0.031	0.049
Victim of larceny * reported larceny to police	0.056	0.088
Robbed * reported robbery to police	0.008	0.016
Defrauded * reported fraud to police	0.010	0.021
Ex-communist country	0.61	0.50
Latin American country	0.13	0.18
Other developing country	0.26	0.32
Observations	55,019	6884

Appendix Table 3: Means of household variables in Peru's ENAHO
(Standard deviations are in parentheses)

	All households	Users of police	Users of judiciary	Users of municipal government	Households with bribery episode
Quarterly consumption -	3226	4880	4588	3902	4433
Nuevos Soles	(2941)	(4383)	(3787)	(3340)	(3612)
Travel time to district's	66	36	40	55	47
main town – minutes	(158)	(91)	(138)	(134)	(126)
Town >500,000	0.15	0.26	0.18	0.13	0.20
Town 100,000-500,000	0.22	0.26	0.29	0.23	0.27
Town 50,000-100,000	0.06	0.09	0.08	0.08	0.10
Town 20,000-50,000	0.08	0.10	0.11	0.10	0.10
Town 2000-20,000	0.08	0.08	0.08	0.09	0.09
Town 500-2000	0.05	0.04	0.05	0.06	0.08
Town about 200	0.27	0.12	0.15	0.23	0.17
Town about 100	0.10	0.05	0.06	0.08	0.06
Own bike	0.27	0.35	0.34	0.32	0.36
Own car, van	0.07	0.16	0.13	0.10	0.14
Own tricycle, truck, taxi	0.06	0.09	0.07	0.08	0.11
Own motorbike	0.03	0.05	0.05	0.04	0.06
Residence by invasion	0.05	0.05	0.04	0.04	0.06
Child aged 0-3 present	0.29	0.28	0.25	0.38	0.30
Child aged 4-7	0.33	0.33	0.31	0.38	0.34
Child aged 8-11 present	0.35	0.33	0.34	0.38	0.36
Child aged 12-15 present	0.33	0.32	0.35	0.35	0.33
Household size	4.4 (2.2)	4.4 (2.1)	4.4 (2.2)	4.8 (2.2)	4.5 (2.1)
Living standard rose	0.08	0.14	0.11	0.10	0.14
Living standard same	0.65	0.52	0.54	0.62	0.51
Living standard fell	0.28	0.34	0.35	0.27	0.34
Town living standrd rose	0.06	0.09	0.08	0.07	0.09
Town living standrd same	0.71	0.61	0.60	0.67	0.59
Town living standrd fell	0.24	0.31	0.32	0.25	0.33
Food aid in: school	0.18	0.16	0.15	0.21	0.18
communal room	0.07	0.05	0.06	0.08	0.06
community dining hall	0.05	0.05	0.04	0.05	0.05
home	0.01	0.01	0.01	0.01	0.01
municipal government	0.04	0.03	0.04	0.06	0.04
other house	0.17	0.14	0.12	0.18	0.15
health treatment	0.01	0.01	0.01	0.02	0.01
church, other	0.01	0.01	0.01	0.01	0.01
Observations	35,964	1977	1470	9201	1757

Note: One Nuevo Sol is worth about 0.3 US cents.

Appendix Table 4: Means of respondent variables in Peru's ENAHO
(Standard deviations in parentheses)

	All households	Users of police	Users of judiciary	Users of municipal government	Households with bribery episode
Male	0.48	0.54	0.49	0.49	0.55
Age	41	38	41	39	38
	(16)	(14)	(15)	(15)	(13)
Years education	7.8	10.3	9.9	8.9	9.8
	(4.8)	(4.4)	(4.7)	(4.8)	(4.6)
Married or cohabiting	0.64	0.64	0.56	0.68	0.65
Married/cohabiting*male	0.32	0.36	0.32	0.35	0.37
Not employed	0.21	0.20	0.22	0.20	0.19
Non-agricultural employer	0.02	0.05	0.03	0.03	0.04
Agricultural employer	0.03	0.02	0.04	0.03	0.03
Non-agricultural self-employed	0.19	0.24	0.22	0.19	0.25
Agricultural self-employed	0.16	0.07	0.09	0.14	0.08
White collar	0.12	0.21	0.20	0.15	0.18
Blue collar	0.12	0.12	0.09	0.10	0.11
Unpaid family worker	0.13	0.08	0.08	0.13	0.09
Domestic or other worker	0.02	0.02	0.02	0.01	0.02
In public administration	0.06	0.10	0.11	0.09	0.09
Observations	35,964	1977	1470	9201	1757