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**Measurement Error and the Effect of Inequality
on Experienced versus Reported Crime**

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Abstract

This paper analyzes measurement errors in crime data to see how they impact econometric estimates, particularly of the key relationship between inequality and crime. Criminal victimization surveys of 140,000 respondents in 37 industrial, transition and developing countries are used. Comparing the crimes experienced by these respondents with those reported to the police, non-random and mean-reverting measurement errors are apparent. Some time-varying factors may also affect the propensity of victims to report crimes to the police, undermining the use of country-specific fixed effects as a means of dealing with measurement errors in official crime data. These measurement errors substantially attenuate both cross-sectional and panel estimates of the effect of inequality on crime.

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crime
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I. Introduction

Why do some countries have more crime than others? In a series of recent papers, Fajnzlber, Lederman and Loayza (2002, 2002a) and Lederman, Loayza, and Menéndez (2002) [hereafter FLL and LLM] suggest that high inequality, low growth and a lack of social capital all cause higher rates of violent crime in a panel of 39 countries.¹ These particular culprits are not new, and echo findings from many studies of crime in the United States.² But the unusual feature of this new research is that it uses cross-country data on official crime rates. Such an approach ignores criminologists' concerns about the reliability of cross-country analysis (Marenin, 1997). Doubts occur because of differences in the propensity to report and record crime in different countries:

“Police figures of recorded crimes cannot be taken at face value as measures of crime... [N]ot all crimes are reported or noticed by the police and not all reported crime is recorded... [Also] legal definitions of crime vary across countries. For these reasons, police figures cannot be used for comparative purposes. There is general consensus among criminologists about this.” (Van Dijk, 2000: 26)

These reporting problems show up in the discrepancy between victimization surveys and official crime statistics. For example, correlations between official rates for four types of crime and the corresponding victimization rates average only 0.22 across 26 countries in Europe, ranging from a correlation of zero for burglary to 0.36 for robbery (Gruszczynska and Gruszczynska, 2005).

Three strategies are used by FLL and LLM to deal with these data problems. First, they focus only on homicide and robbery rates, which they expect to be more reliably measured than less serious crimes. Second, panel data are used to control for unobserved, country-specific effects. If the factors determining the under-reporting of crime are relatively stable over time,

¹ The exact number of countries depends on the time period and the type of crime, with fewer observations available for robberies than homicides. LLM (2002) only use a cross-section, with some regressions based on only 25 countries.

² See, for example, Blau and Blau (1982), Patterson (1991), and Doyle, Ahmed and Horn (1999).

these country fixed effects can minimize any econometric bias resulting from measurement errors in official crime rates. Third, they use instrumental variables to take account of the possible simultaneous relationship between crime and key explanatory factors, such as the output growth rate and the level of inequality and social capital. These instrumental variable methods can also help to mitigate the effects of certain types of measurement error.

Despite these safeguards, incorrect inferences could still be drawn from cross-country research on official crime rates, especially if the nature of measurement error differs from its assumed behavior. Therefore, the objective of this paper is to examine measurement errors in crime data to see what impact these errors have on econometric estimates, particularly of the relationship between inequality and crime. The analysis is based on the International Crime Victim Survey (ICVS), covering 37 industrial, transition and developing countries. Victimization surveys are designed to measure ordinary people's experience of crime, rather than the statistician's view of crime as it is reported to and recorded by the police. While victimization surveys have been available for some time, most economic studies have chosen not to use them, partly because it is believed that such data are available for only a few countries (Bourguignon, 2000). This concern seems misplaced because the number of countries used here is close to the *maximum* sample size in the cross-country analyses of LLM. Moreover, lying behind these victimization estimates for the 37 countries are the survey responses of over 140,000 individuals, which are a potentially rich source of information on the nature of reporting errors.

The only other paper in the economics literature that considers the effect of reporting errors on cross-country analyses of the determinants of crime is Soares (2004). However, several limitations in that paper are addressed here. First it only uses a cross-section of country-level average victimization rates rather than exploring both the individual survey responses and the panel aspects of the ICVS data, as is done here. Second, it constructs a "reporting rate" by

comparing these victimization rates with official crime rates,³ which is problematic because this “reporting rate” also reflects under-recording by police and the justice system, differences in time periods for the two data sources, and definitional differences in the types of crimes covered by the two data sources.⁴ There is no need for these extraneous influences to be introduced into an analysis of reporting rates because the ICVS directly asks crime victims whether they reported the crime to the police, and it is these data that are used here. Moreover, while the framework used by Soares (2004) allows measurement errors in crime data to be correlated with explanatory variables it does not appear to allow correlation with true values, as could occur with mean-reverting error. Specifically, Soares regresses average reporting rates for each country on GNP per capita, yielding an elasticity of 0.6-1.0 depending on the type of crime.⁵ Predictions from this regression provide a (cross-sectional) estimate of the reporting error, which is then used to create an adjusted series of official crime rates for a panel of countries.

Despite the limitations in Soares (2004), similar conclusions are reached to those reported below, with measurement errors shown to greatly reduce the estimated effect of inequality on crime. Thus it is quite possible that the conclusions of FLL and LLM that inequality causes violent crime would be even stronger if appropriate treatments were used for the measurement error in official crime data. This finding is important because a number of criticisms of FLL and LLM claim that they have overstated the effect of inequality on crime. For example, Neumayer (2005) uses a wider sample of 59 countries and finds no effect of inequality on crime until the

³ Specifically, from the United Nations Survey of Crime Trends and Operations of Criminal Justice Systems (UNCS).

⁴ For example, the UNCS has data on rapes but the ICVS reports on “sexual incidents” which are defined more broadly as follows: “People sometimes grab, touch or assault others for sexual reasons in a really offensive way. This can happen either at home or elsewhere, for instance in a pub, the street, at school, on public transport, in cinemas, on the beach or at one's workplace.” This victimization is likely to be different than that for crimes officially recorded as rape, even in the absence of measurement error.

⁵ Soares (2004a) elaborates on this result, finding that reporting rates rise with longer periods of democratic stability and with reductions in perceived corruption. Because these factors vary strongly with GNP per capita, Soares (2004a) argues that it is not income *per se* that changes reporting rates but rather institutional development.

sample is artificially restricted to the same countries as used by FLL (2002, 2002a), when a significantly positive effect of inequality appears.⁶ Neumayer (2003) claims that the estimation methods of FLL (2002, 2002a) are unsuitable for their small samples and shows that inequality appears as a statistically significant cause of homicide only when country fixed effects are excluded from the panel data model he applies to 90 countries. However, neither of these critical studies addresses the measurement error problems in official crime data so the conclusions may not hold.

This focus on measurement errors is also required for evaluating the broader research strategy of extending econometric studies of crime to developing countries. These are the countries where crime is most costly, despite the skew in the literature towards the U.S.⁷ For example, Bourguignon (2000) estimates that the social cost of crime in Latin America is 7.5 percent of GDP, which is almost twice the level in the U.S. A better understanding of the determinants of crime in developing countries may also help to evaluate the claim that crime is one pathway for the link between inequality and low growth (Thorbecke and Charumilind, 2002). But these research gains may not be realized if official crime data from developing countries prove to be so error-ridden that they produce misleading inferences. Hence the need for the current paper.

In Section II, a framework for studying measurement error in reported (official) crime rates is explained. Section III describes the victimization surveys and basic features of the data. Section IV provides simple empirical tests of the measurement error framework. Section V

⁶ This assessment of sample selection biases is possible because Neumayer uses Interpol data which are available for a wider group of countries than the United Nations Crime Surveys data used by FLL (2002, 2002a).

⁷ Demombynes and Özler (2005) are one exception. They show that inequality within police precincts in South Africa has a positive and significant correlation with property crimes but no association with violent crimes.

contains cross-sectional and panel estimates of regression models relating reported and experienced crime to income inequality. Section VI concludes.

II. A Framework for Dealing with Measurement Error in Crime Data

Consider a cross-country relationship linking crime, y to a set of economic and social variables, x :

$$y_j = \beta'x_j + \varepsilon_j \quad (1)$$

To focus only on measurement error, assume that there is no simultaneity between crime and the variables in x , so that $\text{cov}(x, \varepsilon) = 0$ and also assume that there is no measurement error in x . Instead of observing the true level of crime, economists only have access to an error-ridden measure. Suppose that data on crime rates is subject to reporting error of the form:

$$y_j^* = y_j + v_j \quad (2)$$

where y_j^* is the official (or reported) crime rate, y_j is the true (or experienced) crime rate in the j^{th} country, and v_j is a measurement error in official crime rates. The impact of this error on the empirical estimates of each x_j variable's impact on crime, $\hat{\beta}$ depends on the particular nature of the measurement error. The standard textbook assumptions (case 1) are that the measurement error has mean zero and is uncorrelated with anything else, such that $E(v_j) = 0$ and $\text{Cov}(y_j, v_j) = \text{Cov}(X_j, v_j) = \text{Cov}(\varepsilon_j, v_j) = 0$. In this case a regression model of crime based on the error-ridden crime measure will produce unbiased coefficient estimates but the greater error variance may affect the precision of those estimates. However, these assumptions usually reflect convenience rather than conviction and there is accumulating empirical evidence that measurement errors do not follow these assumed patterns (Bound *et al.* 2001).

One obvious way in which the standard textbook case is unlikely to apply is that official crime data are usually considered to be understated rather than overstated. This may reflect both political and policing concerns, as well as the fact that it is usually illegal to make false crime reports in contrast to the lack of penalty for not reporting (minor) crimes. Therefore, a more realistic assumption may be that the errors have a non-zero (negative) mean, but are still uncorrelated with everything else (case 2). In this case, $E(v_j) < 0$ and $Cov(y_j, v_j) = Cov(X_j, v_j) = Cov(\varepsilon_j, v_j) = 0$. Hence, measurement errors are picked up in the intercept term, leaving the coefficients on all slope variables unbiased. In the case of panel data, these assumptions allow fixed effects models like those used by FLL, where separate country intercepts capture the effect of measurement errors which vary across countries but not over time.

Although uncorrelated measurement errors are convenient to deal with, evidence from validation studies in other fields of economics (e.g. labor) suggests that measurement errors are often negatively related to true values, causing mean reversion (Bound *et al.* 2001). In the case of errors correlated with true values (case 3), the use of the error-ridden dependent variable is likely to cause a proportionate bias in each regression coefficient. This can be seen from a generalized version of the measurement error model in equation (2):

$$y_j^* = \theta + \lambda y_j + u_j, \quad (3)$$

where $\lambda = 1 + \gamma = 1 + Cov(v_j, y_j) / Var(y_j)$ and u_j is a pure random error. This measurement error model is flexible in terms of mean bias ($E(y_j^*) = \theta + \lambda E(y_j) \begin{matrix} > \\ < \end{matrix} E(y_j)$) and bias in the estimated variance: $Var(y_j^*) = \lambda^2 Var(y_j) + Var(u_j) \begin{matrix} > \\ < \end{matrix} Var(y_j)$. Classical measurement error is a special case of equation (3) where $\lambda = 1$ and $\theta = 0$. Thus, with correlated errors (as long as measured crime

rates are still positively correlated with true crime rates), the measurement error follows a mean-reverting pattern ($0 < \lambda < 1$). This mean reversion in crime rates will tend to make estimated regression coefficients too small in magnitude, which is contrary to the textbook case where errors in the dependent variable cause no bias in slope coefficients. Thus it could be that the effect of inequality on crime is understated in a cross-section, as in the following equation:

$$y_j^* = \lambda\beta'x_j + \theta + u_j + \lambda\varepsilon_j \quad (4)$$

In other words, the estimated effect of inequality (and other x_j variables) on crime is rescaled by a mean-reverting pattern ($0 < \lambda < 1$) towards zero.

In addition to these cases of measurement errors in a cross-section, the effect in a panel depends on whether the errors are time-varying. If they are, fixed effects methods such as including country-specific intercepts or differencing the data may not remove measurement error bias. Some literature suggests that measurement errors in official crime data are likely to be time-varying, contrary to the assumption used by FLL to justify panel data methods as a way of dealing with measurement error. MacDonald (2000, 2001, 2002) shows that in the British Crime Survey (BCS), victims of property crime are significantly less likely to report the incident if they are unemployed. Because the unemployment rate varies over time, MacDonald concludes that it is likely that reporting rates fall during economic downturns. Consistent with this claim, in 1983 only 39 percent of the crimes experienced by victims in the BCS were reported to the police, at a time when the unemployment rate in the U.K. was 12.4 percent. But in 1991 the reporting rate had risen to 49.4 percent, while the unemployment rate had fallen to 8.8 percent.

III. Data

The International Crime Victim Survey (ICVS) is the most comprehensive program of standardized sample surveys of householders' experience with crime. Starting in 1989 it has gathered up to five rounds of data from up to 60 industrial, transition and developing countries. Subsequent rounds of the survey were in 1992, 1996, 2000 and 2005. The data for this paper come from 37 countries (listed in Appendix A) who were included in at least two rounds from 1992, 1996 and 2000. In total, these countries and survey rounds provide over 140,000 observations.

In most of the developing and transition countries, the ICVS draws representative samples of 1000 households from the largest metropolitan area, although in some cases from several cities. The small rural samples in these countries are not used here because they are not representative. The samples in industrial countries covered a wider range of locations. In all countries, one adult respondent was randomly chosen per household so weights are needed so that individuals in larger households are not underrepresented. In the developing and transition countries, face-to-face interviews were carried out, while telephone interviewing was used in the industrial countries. The response rate is high, averaging 95 percent for the developing countries in the 1996 round, 81 percent for the transition countries and 67 percent, in the industrial countries.

The ICVS covers 12 categories of crime, and for each of these the respondents were asked about their victimization in the previous five years and in the previous year. For the most recent victimization episode they were asked whether they or anybody else reported the crime to the police. Six of the categories such as theft of a motorcycle and theft from a garage are ignored here because they are experienced by very few people. The remaining categories cover both

violent crimes, such as assault and robbery, as well as more minor crimes such as theft from a car.

A descriptive summary of the ICVS data in Table 1 shows that, overall, victimization rates are highest in the developing countries. Amongst the more serious crimes, 7.2 percent of respondents in developing countries had been assaulted or threatened with assault at least once in the previous year, 6.2 percent were robbed, and 7.0 percent had their homes burgled. Amongst those who had use of a car, 4.6 percent had their car stolen and 21 percent had a theft from their car. While average victimization rates for most crimes in developing countries are at least twice as high as those in the industrial countries, for the transition countries victimization rates are closer to, and sometimes less than, those reported in the industrial countries.

The other data, which are used to explain the patterns of crime, come from several sources. The inequality data are from the World Income Inequality Database, constructed by the UN's World Institute for Development Economics Research (WIDER). This database includes 5,063 data points, with many (2,593) coming from the Deininger and Squire (1996) inequality database used in previous international research on inequality and crime.⁸ Data on GDP per capita relative to the U.S. (at Purchasing Power Parity exchange rates) comes from the Penn World Table. We also use data on ethnolinguistic fractionalization, coming from Roeder (2001). Higher values of this variable may be associated with less reporting of crime because there is evidence that communities with high levels of ethnic and cultural diversity have lower levels of interpersonal trust (Alesina and La Ferrara, 2000). While a wider set of explanatory variables are sometimes included in regression models from previous studies, the specification outlined here is

⁸ The Gini coefficients used here are averages for the three year period for the corresponding ICVS round. For example, Gini coefficients for 1995/1997 are matched to the 1996 ICVS round, where this averaging is designed to reduce the impact of measurement error. Following Deininger and Squire (1996) and FLL (2002a), any Gini estimates that were based on expenditure data were adjusted upwards by 6.6 percentage points to make them comparable to those for income data.

reasonably typical, especially because the main aim is to see how measurement errors in crime data affect the estimated coefficients rather than to debate the merits of one specification over another.

IV. Simple Tests of the Measurement Error Framework

The experience of crime is very different to the picture that comes from what is reported to the police. In both the developing and transition countries in the sample, car theft and burglary are the only types of crime where a majority of incidents are reported to the police. For all the other types of crime, it is more normal for victims not to report incidents. Similarly, in the industrial countries a majority of attempted burglaries and assault are not reported to the police.

The evidence on reported crime contradicts the assumption of FLL (2002) that robbery is more reliably measured than less serious crimes (and hence is a good proxy for overall crime). According to Table 1 the reporting rate for robbery is the second lowest out of the six categories of crime considered. Quite minor crimes, such as attempted burglary and theft from cars are all more likely to be reported than are robberies. Only for car theft, whose reporting rate is above 90 percent, would police statistics be considered a reliable measure of the true incidence of crime.⁹

Furthermore, the level of under-reporting is variable enough across countries to distort conclusions drawn from cross-country comparisons of reported crimes. For example, if the percentage of victimized respondents is recalculated, using only the crimes reported to the police, the risk of assault appears twice as high in rich countries than in developing countries when if

⁹ These results mirror those reported by Demombynes and Özler (2005) for South Africa, where robbery has the lowest reporting rate at 42 percent and car theft has the highest reporting rate at 95 percent. Even the reporting rate for homicides in South Africa is only 84 percent, contradicting the assumption of FLL that homicide would be the most reliably measured crime.

fact the victimization rates are almost the same. Reporting errors also obscure the fact that the robbery rate in developing countries is four times higher than in industrial countries.

The descriptive statistics also allow a basic diagnosis of the nature of the reporting errors. Comparing the mean and standard deviation of the reported crime rates to that of the experienced crime rates indicates that the measurement errors do not follow the textbook model (case 1). The mean of y_j^* is much smaller than that of y_j so the measurement error cannot have zero mean. For example, robbery, which is considered by FLL (2002) to be a crime that should not have large reporting errors, has a mean reported rate ($y_j^* = 0.0118$) only one-third that of the actual robbery rate ($y_j = 0.0384$).

More importantly, the standard deviation of y_j^* is smaller than that of y_j , which rules out case 2, where measurement errors have a non-zero mean but are uncorrelated with anything else. Recall that in the equation (3) framework: $\text{var}(y_j^*) = \lambda^2 \text{var}(y_j) + \text{var}(u_j)$. Because of the addition of the variance of the pure random error, u_j in the measurement error model, $\text{var}(y_j^*)$ can only be less than $\text{var}(y_j)$ if $0 < \lambda < 1$. The coefficient λ from the regression of reported crime rates on true crime rates can also be expressed in terms of a regression of reporting errors on true crime rates because $\lambda = 1 + \gamma$ and $\gamma = \text{Cov}(v_j, y_j) / \text{Var}(y_j)$. So the finding that $0 < \lambda < 1$ also implies that $\gamma < 0$. In other words, reporting errors appear to be negatively correlated with true crime rates. As long as the errors are not so large that measured crime rates lose any positive correlation with true values, the measurement errors will cause a mean-reverting pattern ($0 < \lambda < 1$) in official crime rates.

To get more direct evidence on the nature of the measurement error, equation (3) was estimated by regressing the crimes reported to the police, y_j^* on the crimes actually experienced, y_j . Individual respondent reports are used, giving between 80,000 and 140,000 observations, depending on the type of crime.¹⁰ Two estimation methods are used, a “between effects” regression, which mimics a regression on cross-country averages, and a “fixed effects” specification which relies on the fact that there are repeated observations for the same country in the three waves of ICVS data used here.

The relationship between experienced and reported robbery rates (Figure 1a), and the change over time in those rates (Figure 1b), provides some intuition for interpreting these regression estimates of equation (3). Figure 1a shows that in a cross-sectional regression on country-level averages, the reported robbery rate rises much more slowly than the actual robbery rate, with a slope of 0.23 rather than the 1.0 assumed by random measurement error. If this under-reporting reflects some fixed tendency of countries (as assumed by FLL) the relationship between changes over time in actual and reported robbery rates should lie along the 45 degree line. But Figure 1b shows smaller changes in reported robbery rates than in actual robbery rates, with a slope of 0.18 rather than the 1.0 assumed by fixed effects methods of dealing with measurement error. This time-varying measurement error affects the developing and transition countries only. For the industrial countries we cannot reject the hypothesis of the slope equaling 1.0. Thus, fixed effect methods may be appropriate for dealing with under-reporting of official crime rates in industrial countries but are likely to be less successful in transition and developing countries.

When all the different types of crime are considered the same results hold (Table 2). The hypothesis that $\lambda=1$ is strongly rejected in each case. The degree of mean reversion in the cross-

¹⁰ Questions about crimes involving a car are not asked if the respondent does not own a car, causing the number of observations to be reduced to about 80,000 for these types of crimes.

section is particularly strong for robbery, assault and theft from cars, all of which have $\lambda < 0.3$. According to equation (4), this degree of mean reversion will cause a substantial attenuation in regression coefficients, so it is likely that the estimated effect of inequality on crime is understated in a cross-section. Direct evidence on this bias is reported below.

Biased regression coefficients are also likely in panel studies because according to the fixed effects results in the right-hand columns of Table 2, measurement errors for particular crimes vary over time within countries. The change over time in reported crime exhibits mean reversion implying that there is a negative correlation between the actual level of crime and the measurement error in crime reports. This again leads to a rejection of the null hypothesis, $H_0: \lambda_{FE} = 1$. Robbery and assault exhibit the greatest degree of mean reversion.¹¹

Microeconomic Evidence on the Determinants of Reporting Rates

The evidence reported above suggests that measurement errors in official crime rates have a non-zero (negative) mean and are negatively correlated with true crime rates. An alternative but closely related model is that measurement errors are correlated with other determinants, which in turn are correlated with crime rates. To explore this interpretation, a multivariate model of the decision to report crimes to the police is estimated. The aim of the model is to identify the factors associated with under-reporting to better understand possible pathways through which time varying errors may occur. A similar model has recently been estimated with British Crime Survey data by MacDonald (2002), who finds that time-varying factors such as employment status affect the decision of victims to report burglary to the police. In the model used here, the probability of whether a victim reports the most recent crime is assumed to be a function of their

¹¹ The results in Table 2 for robbery differ from those shown in Figure 1b because the samples are different. The figure just uses the most recent two rounds of ICVS data for each country to measure the change in actual and reported robbery rates whereas the regression uses the change between each of up to three rounds of data.

personal characteristics, the characteristics of the local community in which they live, and a set of national level variables. An alternative specification also includes their subjective attitudes about the police and crime.

Several personal characteristics affect the decision of victims to report crimes to the police (Table 3). Older victims and victims with more years of schooling are more likely to report all of the crimes except car thefts. Respondents who are not employed are less likely to report both car theft and burglary. To the extent that employment rates change over the business cycle, these micro-level determinants of reporting may introduce a time-varying bias, as also found by MacDonald (2002). Fixed effects methods would not be expected to fully ameliorate this bias.

The nature of a respondent's dwelling and community affects reporting behavior, with reporting generally less likely in big cities, less likely for those living in shanty dwellings and more likely for those living in high status areas and in an insured dwelling. Conditional on the micro-level determinants, reporting rates are lower in more unequal and poorer countries. Reporting is also less likely the higher is the linguistic and ethnic diversity. When variables that measure respondents' attitudes are included, it appears that thefts from a car, attempted burglary and assault are all more likely to be reported, if the respondent believes that the police do a good job.¹² To the extent that perceptions about how well police are doing their job vary with actual crime rates, this attitudinal influence could be a pathway for mean-reverting errors.

V. Effect of Reporting Errors on Estimates of the Inequality-Crime Relationship

The above analysis of how the crimes experienced by ICVS respondents differ from those reported to the police suggests that there are non-random and mean-reverting measurement errors. There is also evidence that time-varying factors affect the propensity of victims to report

¹² Results with these additional controls are available from the authors.

some crimes, further undermining the use of country fixed effects as a means of dealing with measurement errors in official crime data. To directly assess how important the bias from these reporting errors is, cross-country regressions of crime rates on inequality are estimated in this section.

Table 4 contains the results of three different comparison exercises, where in each case either the experienced crime rate y_j or the reported crime rate y_j^* are regressed on the Gini coefficient.¹³ The first comparison is just a simple cross-country regression of crime rates on inequality, the second adds two covariates – the country’s income level (relative to the U.S.) and the ethno-linguistic fractionalization index, and the third comparison adds 36 country fixed effects to exploit the panel nature of the data.¹⁴ The first two comparisons may help to assess how measurement error affects the results of Lederman, Loayza, and Menéndez (2002), which are based on cross-sectional regressions on crime data from between 25 and 39 countries. The third comparison may help to assess the impact of measurement error on the results of Fajnzlber, Lederman and Loayza (2002, 2002a) who use cross-country panel data for between 27 to 49 countries (with between 85 and 136 observations).

Simple cross-country regressions show that higher inequality is associated with more crime being experienced, for all six types of crime. All of the coefficients on the inequality measures are highly statistically significant. To interpret the size of the coefficients, consider a 10-point rise in the Gini coefficient (equivalent to moving from, say, the United Kingdom to

¹³ The experienced crime rate is taken to be the proportion of the population who had been victimized one or more times in the year prior to the survey so the effects of multiple victimization is not taken into account.

¹⁴ The data are individual-level so it would be possible to estimate microeconomic models. But simply assigning macro-level variables like the Gini to each individual may be incorrect because the resulting *multi-level* model exaggerates the degrees of freedom for macro-level variables and ignores the correlation between errors for individuals in the same country (unless clustered standard errors are calculated). A standard treatment for multi-level models in this context would be to run the micro-level regression for each country and then use the resulting coefficient estimates as dependent variables in a cross-country analysis (Snijders and Bosker, 1999). But with only macro-level variables in the model, this is equivalent to collapsing the data to cross-country aggregates, as is done here.

Russia or from Uganda to South Africa). This rise in inequality would increase the robbery rate by almost three percentage points (about two-thirds of the mean value) and the burglary rate by almost two percentage points (over one-third of the mean value).

But the results are rather different when reported crime rates are used as the dependent variable. Only three types of reported crime have a statistically significant relationship with the Gini coefficient (theft from a car, attempted burglary and robbery). Moreover, the regression coefficients are considerably attenuated, being from one-half to one-tenth of the value of the corresponding regression coefficients when experienced crime is the dependent variable. The results of the hypothesis tests in column (3) show that these differences between the two sets of regression coefficients are all highly significant.

Once covariates measuring relative GDP and ethno-linguistic fractionalization are added to the regression model there is a slight weakening in the contrast between the results for experienced and reported crime. The null hypothesis of no difference between the two sets of coefficients is rejected at less significant levels, especially for burglary ($p < 0.09$). Also, there is no longer a statistically significant effect of inequality on either experienced or reported theft from cars. However, the attenuation of the regression coefficients when using reported crime rates rather than experienced crime is unchanged, with the estimated effect of inequality being from one-half to one-tenth the size of the effect on experienced crime.

Introducing country-specific fixed effects into the regressions does not remove the bias due to measurement error. All of the regression coefficients are smaller when reported crime is used, ranging from three-quarters of the size to one-tenth of the size of the corresponding coefficients for experienced crime. These differences are statistically significant for three important types of property crime (car theft, burglary and robbery).

It is instructive to compare the size of the bias with and without fixed effects. Without fixed effects the coefficients on inequality in the regressions for reported crime averaged, in absolute terms, 0.13 points less than the coefficients when experienced crime was used. Introducing fixed effects reduces this gap only slightly, to 0.10 points. In other words, country fixed effects do not appear to remove the bias in regression coefficients caused by measurement error in reported crime data, contrary to the assumptions of FLL. This lack of effect appears to be because the nature of the measurement error in reported crime differs from the assumed behaviour (uncorrelated errors) that is needed for the efficacy of fixed effects.

VI. Conclusions

According to the results presented here, cross-country econometric research that relies on official crime statistics is likely to be misleading. Biased results are likely even when researchers are as diligent as Fajnzlber, Lederman and Loayza (2002, 2002a) in restricting attention to crimes that are expected to be reliably measured and in using panel data to control for unobserved, country-specific effects, such as variations in reporting rates. These treatments are not especially effective because even for crimes like robbery and homicide there is considerable under-reporting, and the reporting errors are not time-invariant.

The analyses reported here suggest that mean-reversion is the most plausible model of measurement error in official crime data. Mean-reverting measurement errors are not very amenable to standard treatments like fixed effects and instrumental variables. Other methods such as reversion regression and bias bounds (Black et al, 2000) may have greater efficacy, which is a topic for further research. In the absence of these appropriate treatments, the measurement errors in reported crime data are likely to greatly reduce the estimated effect of inequality on crime.

Appendix A

Countries in the Sample

Industrial countries: Australia,² Canada, England, Finland,¹ France, Netherlands, Northern Ireland,¹ Scotland,¹ Sweden, Switzerland,¹ and USA.

Transition countries: Albania,¹ Belarus,¹ Bulgaria,¹ Croatia,¹ Czech Republic,³ Estonia, Georgia, Hungary,¹ Latvia,¹ Lithuania,¹ Poland, Rumania,¹ Russia, Slovakia,³ Slovenia, and Ukraine.¹

Developing countries: Argentina, Botswana,¹ Brazil,³ Colombia,¹ Costa Rica,³ India,³ Indonesia,³ Philippines, South Africa, and Uganda.

Note:

¹ Countries absent from the 1992 round of the ICVS.

² Countries absent from the 1996 round of the ICVS.

³ Countries absent from the 2000 round of the ICVS.

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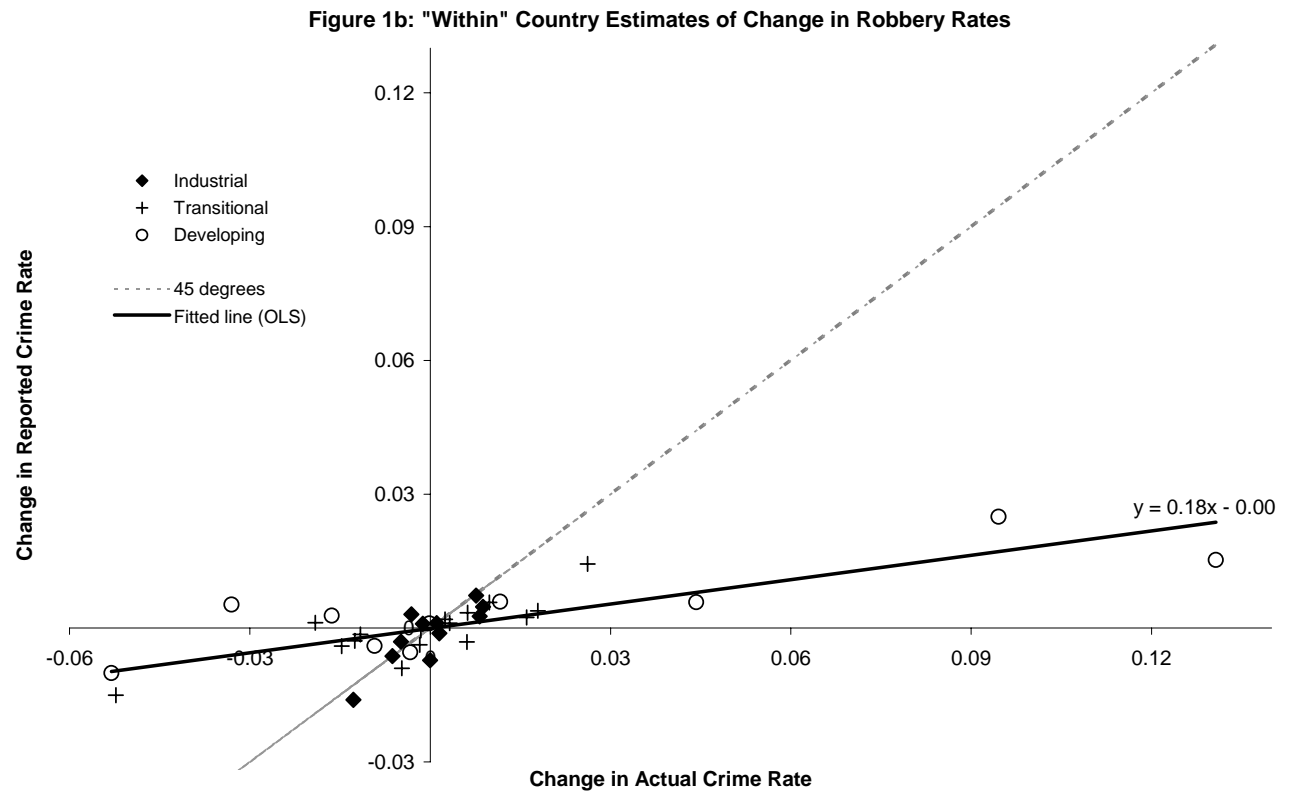
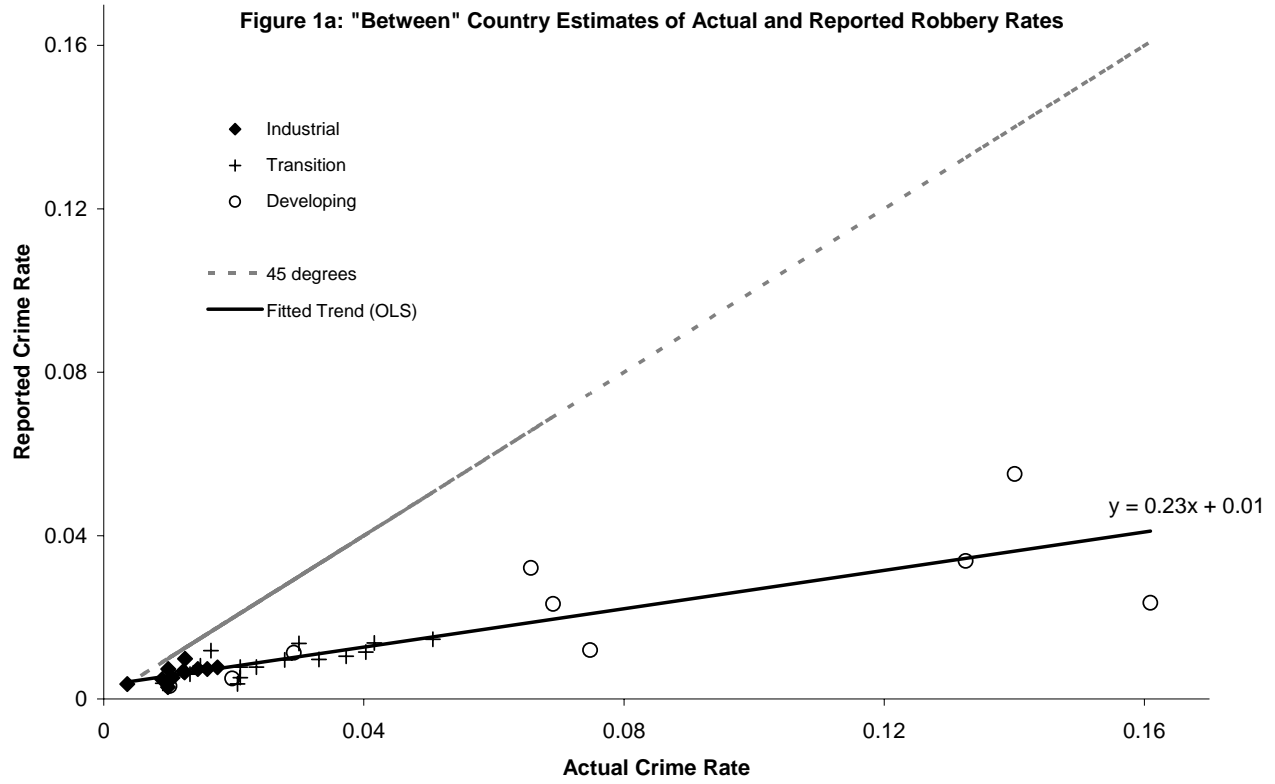


Table 1: Victimization Rates and Reported Crime Rates for Various Crimes

	Victimization Rate ^a				Reported Crime Rate and Reporting Rate ^b							
	Industrial	Transition	Developing	All	Industrial	R ^c	Transition	R ^c	Developing	R ^c	All	R ^c
Car theft	.0263 (.1763)	.0242 (.1720)	.0469 (.2380)	.0304 (.1918)	.0253 (.1731)	.96	.0222 (.1636)	.91	.0434 (.2301)	.92	.0283 (.1848)	.93
Theft from car	.1205 (.4444)	.2154 (.6280)	.2101 (.5919)	.1893 (.5777)	.0791 (.3694)	.65	.0970 (.4347)	.45	.0866 (.4026)	.41	.0897 (.4106)	.47
Burglary	.0356 (.2404)	.0394 (.2515)	.0705 (.3569)	.0490 (.2894)	.0293 (.2110)	.82	.0259 (.2032)	.65	.0389 (.2646)	.55	.0309 (.2269)	.63
Attempted burglary	.0367 (.2342)	.0389 (.2523)	.0614 (.3318)	.0460 (.2784)	.0177 (.1661)	.48	.0129 (.1505)	.33	.0282 (.2339)	.45	.0189 (.1851)	.41
Robbery	.0168 (.1694)	.0306 (.2339)	.0619 (.3391)	.0384 (.2687)	.0093 (.1279)	.55	.0096 (.1239)	.31	.0164 (.1712)	.26	.0118 (.1421)	.30
Assault & threat	.0840 (.4501)	.0570 (.3703)	.0729 (.3991)	.0674 (.3963)	.0317 (.2956)	.37	.0144 (.1940)	.25	.0159 (.1828)	.21	.0182 (.2140)	.27

Source: Author's calculations from International Crime Victims Survey data. All figures are weighted using the country weights calculated by the survey. The sample sizes depend on the crime type, varying from 140,352 to 140,487, except for car-related thefts (82,704 to 83,163) because questions on these thefts were only asked of car owners.

^a The proportion of respondents who experienced at least one episode of the particular type of crime in the year prior to the survey.

^b Refers to the most recent victimization episode and excludes any 'don't know' responses.

^c Reporting rate.

Table 2: Regression Estimates of Equation (3), the Relationship Between Actual and Reported Crimes

	<u>Between Effects Model</u>		<u>Fixed Effects Model</u>	
	$\hat{\lambda}$ (S.E.)	Test for Correlated Errors $H_0: \lambda = 1$ $p = .022$	$\hat{\lambda}$ (S.E.)	Test for Time- Varying Errors $H_0: \lambda = 1$ $p = .000$
Car Theft	.9428 (.023)	$p = .022$.9191 (.0009)	$p = .000$
Theft from car	.2994 (.0567)	$p = .000$.4805 (.0018)	$p = .000$
Burglary	.5437 (.0437)	$p = .000$.6025 (.0013)	$p = .000$
Attempted Burglary	.4856 (.0445)	$p = .000$.4277 (.0013)	$p = .000$
Robbery	.2227 (.0221)	$p = .000$.3596 (.0013)	$p = .000$
Assault and Threat	.2544 (.0541)	$p = .000$.3407 (.0012)	$p = .000$

Source: Author's calculations from International Crime Victims Survey data. All figures are weighted using the country weights calculated by the survey. The sample sizes depend on the crime type, varying from 140,352 to 140,487, except for car-related thefts (82,704 to 83,163).

Table 3: Determinants of Whether Victims Report Crimes to the Police

	<u>Car theft</u>	<u>Theft from car</u>	<u>Burglary</u>	<u>Attempt Burglary</u>	<u>Robbery</u>	<u>Assault/threat</u>
<i>Personal characteristics</i>						
Age at time of survey [‡]	0.000 (0.92)	0.002 (4.25)**	0.002 (3.61)**	0.002 (4.24)**	0.004 (8.06)**	0.003 (8.42)**
Male respondent	-0.014 (1.42)	-0.003 (0.25)	-0.018 (1.37)	0.018 (1.32)	-0.010 (0.66)	-0.016 (1.41)
Years of schooling [‡]	0.000 (0.36)	0.003 (1.68)+	0.009 (5.40)**	0.004 (2.05)*	0.009 (4.86)**	0.004 (2.94)**
Not employed	-0.021 (1.93)+	0.004 (0.35)	-0.024 (1.80)+	-0.003 (0.22)	-0.016 (1.10)	-0.004 (0.35)
<i>Dwelling and community characteristics</i>						
Shanty dwelling (interviewer obs)	0.023 (0.65)	0.038 (0.52)	-0.036 (1.00)	-0.094 (2.35)*	-0.068 (1.70)+	-0.037 (1.23)
House insured	0.004 (0.30)	0.083 (5.54)**	0.087 (4.41)**	0.063 (2.95)**	0.023 (0.92)	-0.004 (0.20)
City population > 1 million	0.009 (0.79)	-0.032 (2.38)*	0.007 (0.41)	-0.042 (2.63)**	0.014 (0.80)	-0.028 (2.04)*
High status area (interviewer obs)	0.006 (0.52)	0.039 (2.77)**	0.080 (4.42)**	0.017 (0.89)	0.041 (1.91)+	-0.007 (0.43)
<i>National variables</i>						
Gini coefficient [‡]	-0.004 (5.33)**	-0.008 (9.13)**	0.001 (1.05)	-0.000 (0.47)	-0.005 (4.11)**	0.001 (1.30)
Income relative to U.S. (PPP) [‡]	0.004 (5.43)**	0.008 (11.93)**	0.005 (6.34)**	0.001 (1.36)	0.007 (6.94)**	0.004 (5.43)**
Ethno-linguistic fractionalization index [‡]	0.000 (0.35)	-0.001 (2.49)*	-0.002 (4.50)**	-0.001 (1.76)+	0.001 (1.05)	-0.001 (1.63)
Pseudo- R^2	0.073	0.080	0.077	0.026	0.052	0.033
Wald test (slopes = 0) χ^2 (16 df)	107.3**	1032.7**	606.9**	177.7**	276.5**	259.2**
Predicted probability ($\hat{P} \bar{X}$)	0.933	0.456	0.635	0.328	0.318	0.252
Number of observations	3062	11333	7624	6344	5258	7691

Note: Coefficients are marginal effects from a probit model that includes fixed effects for each continent and are estimated on individually weighted data.

Variables with a ‡ are continuous and have marginal effects calculated for infinitesimal changes, other variables have marginal effects calculated for discrete changes from 0 to 1. The sample is individuals victimized in the previous year, which varies with the type of crime.

Table 4: Effect of Inequality on Experienced and Reported Crime

	Bivariate regressions ^a			Adding covariates ^b			Adding country fixed effects ^c		
	Experienced Crime $\hat{\beta}_1$	Reported Crime $\hat{\lambda}\beta_1$	Test of Difference ($H_0: \Delta=0$) ^d	Experienced Crime $\hat{\beta}_1$	Reported Crime $\hat{\lambda}\beta_1$	Test of Difference ($H_0: \Delta=0$) ^d	Experienced Crime $\hat{\beta}_1$	Reported Crime $\hat{\lambda}\beta_1$	Test of Difference ($H_0: \Delta=0$) ^d
Car theft	.1004** (.0352)	.0848* (.0351)	$p=.000$.0908* (.0421)	.0782+ (.0406)	$p=.024$.1635** (.0458)	.1185** (.0380)	$p=.037$
Theft from car	.2940** (.1314)	.0304 (.0871)	$p=.008$.2281 (.1517)	-.0261 (.1099)	$p=.045$.3342 (.3623)	.2533+ (.1318)	$p=.776$
Burglary	.1667** (.0592)	.0721 (.0448)	$p=.000$.1153** (.0592)	.0637 (.0670)	$p=.083$.2042** (.0493)	.1005** (.0275)	$p=.002$
Attempted burglary	.1618** (.0494)	.0676* (.0297)	$p=.002$.1246+ (.0692)	.0370* (.0297)	$p=.027$.1905** (.0657)	.1210** (.0755)	$p=.331$
Robbery	.2507** (.0662)	.0582** (.0145)	$p=.001$.2934** (.0662)	.0701** (.0145)	$p=.005$.2366** (.0760)	.0590** (.0222)	$p=.002$
Assault & threat	.1306** (.0514)	.0073 (.0159)	$p=.008$.2193** (.0514)	.0563** (.0159)	$p=.002$.1073 (.0802)	.0098 (.0262)	$p=.173$

Source: Author's calculations from International Crime Victims Survey data for 37 countries ($n=82$ survey year \times country observations). The experienced crime rate and reported crime rate are based on weighted aggregations from up to 140,000 respondents.

^a Values are regression coefficients. Robust standard errors in (). **=significant at 1% level, *=significant at 5% level, +=significant at 10% level.

^b The additional variables are GDP per capita relative to the U.S. (using purchasing power parity exchange rates) and the ethno-linguistic fractionalization index.

^c An additional 36 country-specific intercepts are added to the regression models.

^d Chi-squared test ($df=1$) for difference in coefficient values between the equations for experienced crime and reported crime, with p -values for the null hypothesis of no difference.