This version: August 2004

Is Inequality really a Major Cause of Violent Crime?

Evidence From a Cross-National Panel of Robbery and Violent Theft Rates*

FINAL VERSION

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Abstract

This article argues that the link between income inequality and violent property crime might be spurious, complementing a similar argument in prior analysis by the author on the determinants of homicide. In contrast, Fajnzylber, Lederman & Loayza (1998; 2002a, b) provide seemingly strong and robust evidence that inequality causes a higher rate of both homicide and robbery/violent theft even after controlling for country-specific fixed effects. Our results suggest that inequality is not a statistically significant determinant, unless either country-specific effects are not controlled for or the sample is artificially restricted to a small number of countries. The reason why the link between inequality and violent property crime might be spurious is that income inequality is likely to be strongly correlated with country-specific fixed effects such as cultural differences. A high degree of inequality might be socially undesirable for any number of reasons, but that it causes violent crime is far from proven.

Introduction

In an analysis of the determinants of homicide rates in a cross-national panel, the present author has already argued that the apparent link between income inequality and homicide might be spurious (Neumayer, 2003). It is the objective of this short research note to complement this earlier argument in looking at robbery and violent theft. It demonstrates that income inequality is positively associated with robbery/violent theft only if either country-specific fixed effects are not controlled for or the sample is artificially restricted to a small number of countries.

Many economists have long since argued that income inequality is likely to be a cause of violent crime, particularly violent property crime. This is because greater inequality means a higher concentration of economic wealth in the hands of a few, which implies easier targets to potential criminals and raises the net gains of engaging in violent property crime (Fleisher, 1966; Ehrlich, 1973; Chiu & Madden, 1998; Kelly, 2000; Soares, 2002; for a dissenting view see Deutsch, Spiegel & Templeman 1992). From a different angle, deprivation theory, popular among many criminologists and sociologists, similarly regards economic inequality as a major source of violent crime (Hagan & Peterson, 1995). The relative deprivation of the poor is seen to cause frustration and anger that unloads itself in violent crime.

And yet, 'the evidence in favour of that hypothesis is weak' (Bourguignon, Nuñez & Sanchez 2003: abstract). Evidence from time-series analysis of the aggregate crime rate in the US is rather inconclusive (Allen, 1996). Cross-sectional regressions across metropolitan areas or states within the US as well as sometimes across nations often show a positive effect of inequality on violent crime, but not always (see Neapolitan (1997) and the many references cited therein). A major setback of simple cross-sectional analysis is that it cannot control for fixed effects. Freeman (1996) refers to an unpublished study in which the link between inequality and crime disappeared once fixed effects were controlled for. This is not surprising if, as Bourguignon (2001: 26) argues, unobserved factors are likely to simultaneously affect income inequality and crime. Given enormous variation in the rate of crime across space together with the fact that measurable characteristics can account for little of this variation, as Glaeser, Sacerdote & Scheinkman (1996) argue, income inequality might merely pick up the effect of unobservable factors such as cultural and other differences if fixed effects are not controlled for.

In the face of this weak evidence, Fajnzylber, Lederman & Loayza (FLL thereafter) (1998; 2002a, b) seemingly demonstrate an effect of income inequality on violent crime that is robust to controlling for country-specific fixed effects. The evidence provided by FLL thus seems much stronger and more robust than previous evidence. Di Tella, Galiani and Schargrodsky (2002, p. 4), for example, state with reference to FLL (2002a) that 'the main conclusion of the paper is that income inequality, measured by the Gini index, has a robust, significant and positive effect on the incidence of violent crimes'. Since publication, dozens of other authors have cited FLL's finding of a strong link between inequality and violent crime – see, for example, Buvinić & Morrison (2000), Gartner (2000), World Health Organization (2002), Alvarez (2002), Prillaman (2003), Saridakis (2003) and Glaeser, Scheinkman & Shleifer (2003). The World Bank's official World Development Report (2003: 155) refers to FLL (1998) when it claims that 'factors such as high levels of inequality continue to fuel homicides'. Professor Jan van Dijk (2003), the Chief of the Crime Reduction and Analysis Branch of the United Nations Office on Drugs and Crime, refers to FLL (2002b) when he states that 'World Bank studies on the comparative causes of violent crime show a strong correlation between incidents of violent crime and high levels of inequality'. With the exception of Bourguignon (2001) and Neumayer (2003) none of these authors seem to question FLL's findings. This article complements the latter study by looking at violent property crime.

Research Design

The dependent variable

There are two main sources of cross-national data on robbery/violent theft: the United Nations (UN) and the International Criminal Police Organization (Interpol). Contrary to Interpol data, which are directly reported by police organisations, the United Nations Crime Surveys (UNCS) are answered by governments, even though they are most likely derived from statistics gathered by police organisations as well. FLL (2002 a, b) base their analysis on data from the UNCS. One of the problems with this is that the coverage of countries is rather limited and non-representative, encompassing at maximum 37 countries. Developed countries are over-represented as are Central and South American as well as Asian countries among the non-developed ones. In contrast, the Interpol data are available for more and a wider variety of countries.

The dependent variable in this study is the number of robberies and violent thefts per one million inhabitants. Data have been collected from Interpol (various years) instead of UNCS (various years) to create a larger and more representative sample (up to 59 versus up to 37), as argued above. Following Neapolitan's suggestion (1997: 32ff.), each observation was checked for obvious mis-reporting. Where data for a single or a few years were substantially out of line with the values from prior or consecutive years, then an observation was taken out. For some countries with several temporal breaks in a time-series, the whole series was set to missing. Appendix 1 describes which observations failed to pass this test of inspection. Note, however, that the results reported below hardly change if these observations are not deleted from the sample (detailed results available upon request). The same is true if observations are deleted according to Belsley, Kuh & Welsch's (1980) DFITS criterion. Appendix 2 lists the countries included in the regression with the largest sample size. Note that not all countries report crime rates in all years and also due to the deletion of dubious data not all countries have observations over the entire period of study. With the exception of Peru, which falls victim to our data inspection process, Hong Kong, Mauritius and Trinidad and Tobago are the only countries included in FLL (2002b), but not in our sample, and the reason for this is non-availability of data for the explanatory variables for at least two periods of time.

As in Neumayer (2003) we decided to use 1980 as a cut-off point. Neapolitan (1997) suggests that data from before the 1980s are far less reliable than later data. Any remaining reporting error will be captured by the error term of our estimating equation. Such error renders our estimations less precise and raises the standard errors of the estimated coefficients. It does not, however, bias the estimates as long as the error is not systematically related to some of our explanatory variables. In this respect, the variable that is most problematic is the income level since Soares (2002) suggests that poor countries tend to under-report crime more than rich countries. Under-reporting might also be a problem in autocratic regimes. The coefficient size of the income and democracy variables can therefore be expected to be inflated by measurement error and the reported coefficients of all variables will be somewhat biased. In the absence of accurate information on the amount of bias, there is little scholars can do to avoid this problem, which equally affects FLL (2002a,b).

The explanatory variables

As our main variable of income inequality, we use the Gini coefficient measuring the concentration of incomes between the extremes of 0 (absolute equality) and 1 (maximum inequality). Data are taken from UN-WIDER (2000), which is more comprehensive in coverage than Deininger & Squire (1996). However, our results on income inequality do not depend on using the UN-WIDER source. Using only Deininger & Squire (1996) as the source of data instead makes no difference. Like FLL (2002a,b) we follow Deininger & Squire's (1996: 582) suggestion and add 6.6 to Gini coefficients derived from expenditure instead of income surveys. Also similar to FLL (2002a,b) we take the Gini coefficients of the highest quality first and averages of lower quality observations only where high quality ones are not available.

Some recent work argues, however, that the Gini coefficient might not be the most relevant measure of inequality with respect to crime. For example, empirical work by Bourguignon, Nuñez & Sanchez (2003) suggests that it is the relative income of the population with standards of living below 80 per cent of the mean that matters (see also Chiu & Madden, 1998). Unfortunately, in a cross-national setting the availability of detailed data on income distributions within countries is severely restricted. The only alternative indicator of income inequality we can employ is the ratio of the top to the bottom quintile of the income distribution, a measure also used by FLL (2002b). Data are taken from Deininger & Squire (1996). Unfortunately, this measure has less availability than the Gini measure.

As control variables, we include the gross domestic product (GDP) per capita in purchasing power parity and constant prices of 1997, its growth rate, the unemployment rate, the urbanization rate, the female labour force participation rate and the share of males in the age group 15 to 64. Additionally, we use the Polity measure of democracy (Gurr & Jaggers, 2000). Human rights violation is measured by the Purdue Political Terror Scales (Gibney, 2002). All these variables are suggested as potentially important determinants by the theoretical literature on violent crime – see Neuman & Berger (1988), Neapolitan (1997) and Neumayer (2003) for details. Of these variables, the female labour force participation rate is perhaps the one that is least intuitively plausible. Opportunity theory suggests that a higher female labour force participation rate leads to reduced guardianship for potential offenders, thus raising the rate of violent property crime. Unless otherwise stated, data are taken from UN (1999) and World Bank (2001). In accordance with most empirical studies we take the natural log of income per capita to render its distribution less skewed.

Table I provides summary descriptive variable information. Table II reports a correlation matrix of variables after fixed-effects transformation, which does not suggest that multicollinearity is likely to be a problem in our estimations. In addition, variance inflation factors were computed and pointed in the same direction.

< Insert Tables I and II about here >

The methodology

We take three year averages of the dependent and all independent variables for the period 1980 to 1997 to reduce the impact of atypically high or low rates in any one single year. Our model to be estimated is as follows:

$$\ln(y_{it}) = \alpha + \beta x'_{it} + (a_i + u_{it})$$

Time is indicated by t, countries by i, ln(y) is the logged rate of robbery and violent theft per one million people, α is a constant, x' contains the explanatory variables, β is the corresponding vector of coefficients to be estimated. The log-transformation of the dependent variable was undertaken to mitigate problems with heteroscedasticity of the error term. The a_i represent individual country effects capturing cultural and other (approximately) time-invariant factors. Their inclusion ensures that unobserved country heterogeneity is accounted for. The fixed-effects estimator is based on the time variation within each cross-sectional unit only.

In further estimations, we use a random-effects estimator, which draws upon both the cross-sectional and temporal variation in the data. It is more efficient than the fixed-effects estimator, but leads to consistent estimations only if the assumption is correct that the country effects are not correlated with the explanatory variables. In addition, like FLL (2002a, b) we also include a lagged dependent variable in separate estimations and estimate the model with a so-called systems generalized method of moments (GMM) estimator based on Arellano & Bond (1991) and Arellano & Bover (1995). A hysteresis effect might exist if, for example, criminals base their current year's behavior on past information (Sah, 1991). The GMM estimator also accounts for the possibility that the explanatory variables are partially endogenous. However, like FLL (2002a, b) we assume that all explanatory variables are at least weakly exogenous, that is the explanatory variables may be endogenous to past and current values of the dependent variables, but not to its future ones. The need to instrument the dependent variable with its lagged values in order to avoid correlation with the error term leads to the loss of one time period and often substantial losses in the efficiency of estimation (Wooldridge, 2002). FLL (2002a, b) use the two-step (rather than one-step) dynamic GMM estimator. The problem with this is that Arellano & Bond (1991: 291) themselves explicitly warn against using the two-step estimator as it is known to underestimate standard errors. For this reason we use the one-step estimator.

In pre-testing, we searched for non-linear effects of any of the explanatory variables. However, we found evidence for such effects only for the income variable. All variables therefore enter the regressions only in linear form with the exception of the income variable, for which both a linear and a squared term are entered.

Results

Table III presents our estimation results with the Gini coefficient, table IV presents results with the ratio of the top to bottom income quintile as the measure of income inequality. Column 1 reports results from fixed effects estimation, where the regression is constrained such that no countries and no control variables are included other than those also included in FLL (2002b).[1] The result on the Gini coefficient mirrors FLL's (2002a, b) finding as it is positive and statistically significant. In column 2 we add the squared term of the log of per capita income as well as additional control variables that are suggested by theory as determinants of violent crime, keeping the sample of countries still the same. It can be seen that the Gini coefficient remains positive and significant despite the fact that its significance is somewhat reduced. In other words, drawing data for violent crime from a different source (Interpol versus UNCS) and introducing more control variables does not change FLL's (2002a, b) main result that income inequality is associated with a higher violent crime rate. Per capita income has a non-linear effect on violent property crime. An increase in income leads to an increase in violent property crime over a range of income, but at a decreasing rate. The positive link over a range of income levels could be either because higher income raises the value of things to be robbed, rendering violent property crime more attractive, or because reporting of such crimes is higher in richer countries as argued by Soares (2002).[2] The female labour force participation rate, the unemployment rate, our measure of democracy and the measure of human rights violation are all positively associated with robbery/violent theft. All of these are in line with expectations. The economic growth rate and the share of males between the age of 15 and 64 are statistically insignificant, however. What is very much contrary to expectation is the negative and significant coefficient of the urbanisation rate. In column 3, we no longer artificially constrain the sample of countries. As a consequence, the sample size increases to 50 countries and the Gini coefficient becomes insignificant. The control variables test as before, with the exception of the urbanisation rate. This suggests that its strange and counter-intuitive statistically significant negative sign might have been caused by constraining the sample to a small and non-representative number of countries. It also suggests that the positive and significant coefficient of the Gini measure is likely to be due to the same effect. If we exclude the insignificant variables from the model, then the results are generally as before (column 4). If, in addition, we exclude the unemployment rate, which is now insignificant and whose inclusion constrains sample size, then the sample now covers 59 countries in column 5 with little effect on the results. In particular, the Gini coefficient remains insignificant. Column 6 reports results from the systems GMM estimator. Beside the lagged dependent variable, only the economic growth and the unemployment rate are statistically significant and their coefficients are negative and positive, respectively, as theory would predict. Importantly, the Gini coefficient remains insignificant. If one were to exclude the other insignificant variables from the model, this hardly affects the results. The Gini coefficient becomes very marginally significant at the 10 percent level now, but it has a negative sign, suggesting that if anything higher inequality is associated with a lower rate of violent robbery and theft (results not shown).

< Insert Table III about here >

Column 7 re-estimates the static model with a random effects estimator. Results are generally rather similar to the fixed effects model. Importantly, however, the Gini measure of income inequality assumes statistical significance together with the expected positive sign. Keep in mind though that the random-effects estimation results are only consistent if the explanatory variables are not correlated with the country-specific fixed effects. The Hausman test result rejects the assumption of no correlation and thus rejects the random effects assumption.

Table IV repeats the analysis of table III with the ratio of the top to bottom income quintile as the measure of inequality. Results are similar to those of table III. In particular, this alternative measure of income inequality is also highly significant in the regression with the constrained sample size and a constrained number of control variables (column 1). Adding further control variables reduces the statistical significance of the inequality measure, but does not render it insignificant (column 2). As with the Gini coefficient, the quintile ratio becomes insignificant if we no longer artificially restrict the sample size (column 3). Dropping the insignificant variables from the model does not change this result as the ratio of the top to bottom income quintile remains insignificant, whether we restrict the sample to be the same as in column 3 or not (columns 4 and 5). In systems GMM estimation the inequality measure remains insignificant (column 6). Compared to column 6 of table III, the share of males between the ages of 15 and 64 is also significant. As with the dynamic estimation in table III, these results do not change if the insignificant variables are dropped (results not shown). The coefficient of the inequality variable becomes significant in the more representative sample with a larger number of countries only in static random effects estimation (column 7). The Hausman test again rejects the hypothesis that the explanatory variables are not correlated with country-specific effects.

< Insert Table IV about here >

Conclusion

No matter whether income inequality is measured by the Gini coefficient or by the ratio of the top to the bottom income quintile, it is insignificant in fixed effects and dynamic estimation and significant only in random effects estimation, unless the sample of countries is constrained to contain no other countries than those included in FLL (2002b). Our results suggest that if we allow for a more representative sample and control for country-specific fixed effects, then income inequality no longer is a statistically significant determinant of violent crime. I conclude from the results reported above that the link between income inequality and violent crime is far less robust than FLL seem to suggest. The claim that income inequality is a major cause of violent crime is therefore questionable.

Of course, it could be that there is too much noise and too little real over-time variation in the income inequality data such that the within-country variation in inequality is not sufficient to render the coefficient statistically significantly different from zero. However, there is not much more variation in other variables either and still they turn out significant in accord with theoretical expectations in fixed effects estimation (e.g., the per capita income level and female labour force participation). An alternative explanation could be that country specific fixed effects simultaneously affect both inequality and crime such that without controlling for these effects inequality spuriously picks up these effects (Freeman, 1996). Without good instruments for inequality, which are extremely hard to come by, it is impossible to tell which is the case. Quite possibly, there are limits to identifying the effects of inequality on violent crime at the cross-national level and more micro-oriented studies such as Bourguignon, Nuñez & Sanchez (2003) are more promising in this regard.

Table I. Descriptive Information on Variables

Variable	Obs	Mean	Std. Dev.	Min	Max
ln (Robbery rate per 1,000,000 people)	206	5.58	1.45	0.21	7.91
Gini coefficient	206	34.76	7.87	16.63	60.60
Top to bottom income quintile ratio	132	6.82	3.44	2.76	23.88
ln (GDP p.c. in US$1997)	206	9.01	0.99	6.67	10.30
Growth in GDP p.c.	206	0.97	4.90	-17.81	14.49
Unemployment rate	183	7.57	4.36	0.73	21.20
% urban	206	63.94	21.20	11.20	100
Female labour force participation rate	206	37.05	9.97	7.47	55.63
% of population male aged 15-64	206	0.32	0.03	0.23	0.36
Democracy	206	15.91	5.98	0	20
Human rights violation	206	1.86	1.02	1	4.83

Table II. Correlation Matrix of Variables After Fixed Effects Transformation

	Robbery rate	Gini coefficient	Top to bottom quintile ratio	ln GDP p.c.	ln GDP p.c. squared	Economic Growth	Unemploy-ment rate	Female labour force part.	% urban	% male 15-64	Demo-cracy
Gini coefficient	0.262										
Top to bottom quintile ratio	0.248	0.605									
ln GDP p.c.	0.400	-0.042	-0.011								
ln GDP p.c. squared	0.374	-0.051	-0.006	0.991							
Economic Growth	0.093	-0.018	-0.152	0.335	0.346						
Unemployment rate	0.187	0.137	0.242	-0.113	-0.120	0.281					
Female labour force part.	0.318	-0.004	-0.057	0.602	0.575	0.200	-0.068				
% urban	0.493	0.086	0.141	0.645	0.671	0.274	0.135	0.483			
% male 15-64	0.513	0.001	-0.014	0.726	0.706	0.382	0.157	0.715	0.708		
Democracy	0.250	0.085	0.151	0.047	0.039	0.046	0.007	0.341	0.051	0.207	
Human rights violation	0.216	0.012	0.031	0.076	0.040	-0.172	-0.021	0.096	0.091	0.112	-0.348

Table III. Estimation Results for Gini Coefficient (1980-97)

	(1) FE	(2) FE	(3) FE	(4) FE	(5) FE	(6) GMM	(7) RE
ln (robbery/theft rate) (lagged)						0.930 (0.030)***	
Gini coefficient	0.032 (0.015)**	0.023 (0.012)*	0.012 (0.010)	0.013 (0.010)	0.013 (0.009)	-0.004 (0.005)	0.027 (0.009)***
ln GDP p.c.	1.240 (0.394)***	7.894 (2.060)***	6.128 (1.804)***	5.621 (1.697)***	5.546 (1.603)***	-0.292 (0.855)	5.598 (1.564)***
ln GDP p.c. squared		-0.400 (0.121)***	-0.327 (0.104)***	-0.310 (0.100)***	-0.321 (0.095)***	0.016 (0.046)	-0.286 (0.088)***
Economic Growth	-0.013 (0.012)	-0.015 (0.010)	-0.006 (0.008)			-0.016 (0.007)**	-0.010 (0.008)
Unemployment rate		0.052 (0.019)***	0.030 (0.016)*	0.025 (0.015)		0.029 (0.009)**	0.061 (0.015)***
% urban	-0.001 (0.031)	-0.116 (0.032)***	-0.017 (0.018)			0.002 (0.003)	0.016 (0.008)**
Female labour force participation		0.084 (0.027)***	0.095 (0.024)***	0.089 (0.021)***	0.108 (0.019)***	-0.004 (0.003)	0.039 (0.012)***
% male 15-64		8.149 (8.998)	-2.529 (7.821)			2.759 (2.488)	-6.571 (6.130)
Democracy		0.080 (0.017)***	0.050 (0.016)***	0.044 (0.014)***	0.041 (0.012)***	0.001 (0.008)	0.038 (0.014)**
Human rights violation		0.379 (0.144)**	0.255 (0.118)**	0.254 (0.114)**	0.235 (0.099)**	-0.000 (0.045)	0.158 (0.097)*
Observations	135	134	182	182	206	112	182
Number of countries	33	33	50	50	59	46	50
R^2	0.21	0.54	0.44	0.43	0.41		0.50
Sargan test over-ident. restrictions (p-value)						103.03 (0.224)	
Test 2nd order auto-correlation (p-value)						-0.75 (0.456)	
Hausman test chi^2 (p-value)							47.49 (0.0000)

Note: Dependent variable is ln (robbery and violent theft rate) in three year averages. Fixed effects (FE), systems Generalized Method of Moments (GMM) and random effects (RE) estimation. Standard errors in parentheses. Coefficients of constant not reported. * significant at p < .1; ** at p < .05; *** at p < .01.

Table IV. Estimation Results for Top to Bottom Income Quintile Ratio (1980-97)

	(1) FE	(2) FE	(3) FE	(4) FE	(5) FE	(6) GMM	(7) RE
ln (robbery/theft rate) (lagged)						0.800 (0.056)***	
Top to bottom income ratio	0.240 (0.070)***	0.115 (0.066)*	0.065 (0.059)	0.085 (0.054)	0.080 (0.049)	0.014 (0.174)	0.106 (0.038)***
ln GDP p.c.	1.613 (0.561)***	8.879 (3.184)***	5.820 (3.132)*	6.302 (2.835)**	6.199 (2.721)**	-0.605 (1.020)	6.914 (2.237)***
ln GDP p.c. squared		-0.427 (0.185)**	-0.308 (0.182)*	-0.348 (0.169)**	-0.343 (0.163)**	0.029 (0.056)	-0.360 (0.129)***
Economic Growth	-0.009 (0.012)	-0.016 (0.011)	-0.005 (0.011)			-0.030 (0.007)***	-0.006 (0.010)
Unemployment rate		0.034 (0.028)	0.019 (0.026)			0.036 (0.014)***	0.067 (0.021)***
% urban	-0.003 (0.043)	-0.218 (0.052)***	-0.056 (0.035)			0.002 (0.003)	0.015 (0.010)
Female labour force participation		0.043 (0.038)	0.077 (0.036)**	0.096 (0.030)***	0.097 (0.028)***	0.002 (0.004)	0.053 (0.014)**
% male 15-64		39.058 (16.497)**	17.832 (15.078)			5.462 (3.228)*	-9.598 (8.043)
Democracy		0.091 (0.023)***	0.061 (0.023)**	0.050 (0.020)**	0.052 (0.019)***	0.007 (0.011)	0.030 (0.018)*
Human rights violation		0.396 (0.181)**	0.366 (0.190)*	0.334 (0.184)*	0.347 (0.170)**	-0.037 (0.058)	0.103 (0.146)
Observations	88	88	112	112	119	61	112
Number of countries	30	30	40	40	43	34	40
R^2	0.33	0.62	0.47	0.43	0.44		0.66
Sargan test over-ident. restrictions (p-value)						56.61 (0.184)	
Test 2nd order auto-correlation (p-value)						-0.12 (0.905)	
Hausman test chi^2 (p-value)							28.99 (0.0012)

Note: Dependent variable is ln (robbery and violent theft rate) in three year averages. Fixed effects (FE), systems Generalized Method of Moments (GMM) and random effects (RE) estimation. Standard errors in parentheses. Coefficients of constant not reported. * significant at p < .1; ** at p < .05; *** at p < .01.

Appendix 1

Data excluded from sample:

Argentina (all years), Côte d'Ivoire (1997), Dominica (all years), Indonesia (1986), Lesotho (all years), Peru (all years), Philippines (1997), Tanzania (all years), Zimbabwe (all years).

Appendix 2

Countries included in sample (column 5 of table III):

Armenia, Australia, Austria, Bangladesh, Belgium, Bulgaria, Canada, Chile, China, Colombia, Costa Rica, Côte d'Ivoire, Denmark, Ecuador, Estonia, Finland, France, Georgia, Germany, Ghana, Greece, Guinea, Honduras, Hungary, India, Indonesia, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Latvia, Luxembourg, Malaysia, Netherlands, New Zealand, Norway, Pakistan, Philippines, Poland, Portugal, Romania, Russian Federation, Senegal, Singapore, Slovak Republic, South Korea, Spain, Sri Lanka, Sweden, Switzerland, Thailand, Uganda, Ukraine, United Kingdom, United States, Venezuela, Zambia.

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This version: August 2004

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ENDNOTES

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[*] Email address: e.neumayer@lse.ac.uk. Helpful comments from three anonymous referees and the associate editor, Han Dorussen, are gratefully acknowledged. The data used in this article as well as the routines used to generate the results with Stata 8 can be found at http://www.prio.no/jpr/datasets.htm.

[1] FLL (2002b) also include an educational attainment variable based on the average years of schooling of the population over 15 from a dataset constructed by Robert Barro and Jon-Wha Lee. This variable is not included here as it would further reduce sample size and is not consistently significant in FLL (2002b) either. It is unclear to this author why the inclusion of this variable does not further constrain the sample size reported by FLL (2002b).

[2] In this regression, the estimated turning point is at around US$19,000, after which further income increases are associated with a lower rate of violent crime. The estimated turning point differs somewhat from regression to regression, but is always above the mean income level with the exception of regression 5 of table III.