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Inequality and Violent Crime: Evidence from Data on Robbery and Violent Theft*

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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 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. The results in the present article 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 for the link between inequality and violent property crime being 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 that 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 for 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 finds an outlet in violent crime.

And yet, 'the evidence in favour of that hypothesis is weak' (Bourguignon, Nuñez &

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Sanchez, 2003: abstract). Evidence from time-series analysis of the aggregate crime rate in the United States is rather inconclusive (Allen, 1996). Cross-sectional regressions across metropolitan areas or states within the USA, as well as sometimes across nations, often show a positive effect of inequality on violent crime – but this is not always the case (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 hereafter) (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 & Schargrodsky (2002: 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'. 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 seems to question FLL's findings. This article complements the latter study by looking at violent property crime.

Research Design

Dependent Variable

There are two main sources of cross-national data on robbery/violent theft: the United Nations and the International Criminal Police Organization (Interpol). Contrary to Interpol data, which are directly reported by police organizations, the United Nations Crime Surveys (UNCS) are answered by governments, even though they are probably derived from statistics gathered by police organizations as well. FLL (2002a,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 most 37 countries. Developed countries are overrepresented, as are Central and South American and 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 1 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 misreporting. Where data for a single year or a few years were substantially out of line with the values from prior or consecutive years, an observation was taken out. For some countries with several temporal breaks in a time-series, the whole series was set as missing. Appendix A 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 B 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, owing 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; 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 cutoff 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. An error of this sort 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 underreport crime more than rich countries. Underreporting 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).

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) instead as the source of data 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% 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 is not as available as 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 proportion 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 of 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.

Methodology

We take three-year averages of the dependent and all independent variables for the period 1980–97 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 1 million people; α is a constant; x' contains the explanatory variables; and 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

Table I. Descriptive Information on Variables

Variable	*Obs.*	*Mean*	*Std dev.*	*Min.*	*Max.*
ln (robbery rate per 1 million 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 per capita in US$1997)	206	9.01	0.99	6.67	10.30
Growth in GDP per capita	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

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 it 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 behaviour 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 future values. 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, and Table IV presents results with the ratio of the top to bottom income quintiles 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 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

[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).

Table II. Correlation Matrix of Variables After Fixed-Effects Transformation

	Robbery rate	Gini coefficient	Top to bottom quintile ratio	ln GDP per capita	ln GDP per capita squared	Economic growth	Unemployment rate	Female labour force participation	% urban	% male 15–64	Democracy
Gini coefficient	0.262										
Top to bottom quintile ratio	0.248	0.605									
ln GDP per capita	0.400	–0.042	–0.011								
ln GDP per capita 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 participation	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

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 stolen, 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 ages of 15 and 64 are statistically insignificant, however. What is very much contrary to expectation is the negative and significant coefficient of the urbanization 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 urbanization 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. Apart from 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 excludes the other insignificant variables from the model, this hardly affects the results. The Gini coefficient becomes very marginally significant at the 10% 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).

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 consistent only 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 quintiles 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

[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.

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)						0.930	
(lagged)						(0.030)***	
Gini coefficient	0.032	0.023	0.012	0.013	0.013	–0.004	0.027
	(0.015)**	(0.012)*	(0.010)	(0.010)	(0.009)	(0.005)	(0.009)***
ln GDP per capita	1.240	7.894	6.128	5.621	5.546	–0.292	5.598
	(0.394)***	(2.060)***	(1.804)***	(1.697)***	(1.603)***	(0.855)	(1.564)***
ln GDP per capita squared		–0.400	–0.327	–0.310	–0.321	0.016	–0.286
		(0.121)***	(0.104)***	(0.100)***	(0.095)***	(0.046)	(0.088)***
Economic growth	–0.013	–0.015	–0.006			–0.016	–0.010
	(0.012)	(0.010)	(0.008)			(0.007)**	(0.008)
Unemployment rate		0.052	0.030	0.025		0.029	0.061
		(0.019)***	(0.016)*	(0.015)		(0.009)**	(0.015)***
% urban	–0.001	–0.116	–0.017			0.002	0.016
	(0.031)	(0.032)***	(0.018)			(0.003)	(0.008)**
Female labour force		0.084	0.095	0.089	0.108	–0.004	0.039
participation		(0.027)***	(0.024)***	(0.021)***	(0.019)***	(0.003)	(0.012)***
% male 15–64		8.149	–2.529			2.759	–6.571
		(8.998)	(7.821)			(2.488)	(6.130)
Democracy		0.080	0.050	0.044	0.041	0.001	0.038
		(0.017)***	(0.016)***	(0.014)***	(0.012)***	(0.008)	(0.014)**
Human rights		0.379	0.255	0.254	0.235	–0.000	0.158
violation		(0.144)**	(0.118)**	(0.114)**	(0.099)**	(0.045)	(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.						103.03	
restrictions (p-value)						(0.224)	
Test 2nd order auto-						–0.75	
correlation (p-value)						(0.456)	
Hausman test chi^2							47.49
(p-value)							(0.0000)

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 $p < .1$; ** $p < .05$; *** $p < .01$.

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)						0.800	
(lagged)						(0.056)***	
Top to bottom income	0.240	0.115	0.065	0.085	0.080	0.014	0.106
ratio	(0.070)***	(0.066)*	(0.059)	(0.054)	(0.049)	(0.174)	(0.038)***
ln GDP per capita	1.613	8.879	5.820	6.302	6.199	–0.605	6.914
	(0.561)***	(3.184)***	(3.132)*	(2.835)**	(2.721)**	(1.020)	(2.237)***
ln GDP per capita squared		–0.427	–0.308	–0.348	–0.343	0.029	–0.360
		(0.185)**	(0.182)*	(0.169)**	(0.163)**	(0.056)	(0.129)***
Economic growth	–0.009	–0.016	–0.005			–0.030	–0.006
	(0.012)	(0.011)	(0.011)			(0.007)***	(0.010)
Unemployment rate		0.034	0.019			0.036	0.067
		(0.028)	(0.026)			(0.014)***	(0.021)***
% urban	–0.003	–0.218	–0.056			0.002	0.015
	(0.043)	(0.052)***	(0.035)			(0.003)	(0.010)
Female labour force		0.043	0.077	0.096	0.097	0.002	0.053
participation		(0.038)	(0.036)**	(0.030)***	(0.028)***	(0.004)	(0.014)**
% male 15–64		39.058	17.832			5.462	–9.598
		(16.497)**	(15.078)			(3.228)*	(8.043)
Democracy		0.091	0.061	0.050	0.052	0.007	0.030
		(0.023)***	(0.023)**	(0.020)**	(0.019)**	(0.011)	(0.018)*
Human rights		0.396	0.366	0.334	0.347	–0.037	0.103
violation		(0.181)**	(0.190)*	(0.184)*	(0.170)**	(0.058)	(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.						56.61	
restrictions (p-value)						(0.184)	
Test 2nd order auto-						–0.12	
correlation (p-value)						(0.905)	
Hausman test chi^2							28.99
(p-value)							(0.0012)

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 $p < .1$; ** $p < .05$; *** $p < .01$.

ratio of the top to bottom income quintiles remains insignificant, whether we restrict the sample to be the same as that 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 proportion 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.

Conclusion

No matter whether income inequality is measured by the Gini coefficient or by the ratio of the top to the bottom income quintiles, 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 is no longer 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 inequality spuriously picks up these effects, if they are not controlled for (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.

Appendix A. 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 B. 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, the 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, USA, Venezuela, Zambia.

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