journal of peace RESEARCH

© 2003 Journal of Peace Research,
vol. 40, no. 6, 2003, pp. 619–640
Sage Publications (London, Thousand Oaks, CA and New Delhi) www.sagepublications.com
[0022-3433(200311)40:6; 619–640; 038284]

Good Policy Can Lower Violent Crime: Evidence from a Cross-National Panel of Homicide Rates, 1980–97*

ERIC NEUMAYER

Department of Geography and Environment, London School of Economics and Political Science

This article provides empirical evidence that good political governance and good economic policies can lower homicide rates. Therefore, violent crime is not simply determined by modernization, population characteristics, and cultural factors. This result follows from rigorous econometric testing based on a cross-national panel of homicide data from up to 117 countries over the period 1980–97. Contrary to most existing studies, which have applied ordinary least squares on data drawn from one time period only, this analysis uses a fixed-effects estimator with fully robust standard errors. A fixed-effects estimator elegantly controls for time-invariant determinants, such as cultural factors, and allows the pooling of homicide data from otherwise incompatible sources. This is complemented by random-effects estimation in sensitivity analysis. The results suggest that economic growth, higher income levels, respect for human rights, and the abolition of the death penalty are all associated with lower homicide rates. The same is true for democracy at high levels of democracy. The transition from autocracy to democracy is likely to be accompanied by a rising homicide rate, however, until full democracy has been reached. Results also indicate that policies aimed at improving equity have no effect on violent crime. In particular, there is evidence that the positive effect of income inequality on homicide rates found in many studies might be spurious. The results reported here are strikingly similar to those found for the causes of civil war.

Introduction

The existing empirical literature explaining variation in homicide rates suffers from two important shortcomings, one conceptual, the other methodological.[1] Conceptually, there is too little focus on how good policy can lower rates of violent crime, of which homicide is the most extreme and by far the most reliably reported form. The existing, mostly sociological, literature sees homicide rates mainly determined by structural determinants such as modernization, population characteristics, and country-specific cultural factors. Since these structural determinants are difficult, if not impossible, to change via policy, one might draw the misplaced conclusion that homicide rates cannot be lowered via good policies either. Instead, our study focuses on the effect of good policy on homicide rates.

The methodological shortcoming that characterizes many empirical studies of variation in homicide is that often only multivariate ordinary least squares (OLS) analysis

* E-mail 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 7 can be found at http://www.prio.no/jpr/datasets.asp.

[1]For an overview of this literature, see Neapolitan (1997a) and LaFree (1999).

on one-period data is used, which fails to exploit fully the potential of quantitative analysis. Few studies employ a fixed-effects estimator on panel data in order to control for unobserved country heterogeneity bias in the estimated coefficients. Such bias can arise if country characteristics are either impossible to quantify or unobservable and if the explanatory variables are correlated with these characteristics. Given that underlying time-invariant country characteristics that are difficult or impossible to measure are likely to be correlated with variables used for explaining variation in homicide rates, the use of a fixed-effects panel estimator gains great importance. In addition, usage of a fixed-effects estimator enables us to pool homicide data from two different sources that should not be pooled in any analysis that exploits between-country variation in the dependent variable, as OLS does.

We assess the impact of good policy on homicide in terms of political governance (democracy, respect for human rights, and absence of the death penalty), economy (economic growth and income levels), as well as equity (welfare expenditures, economic discrimination of minorities, and income inequality). We find that good policies in terms of political governance and the economy can lower homicide rates, but not good policies in terms of equity. Therefore, this article provides evidence on how policymakers can achieve a more pacified society.

There are striking similarities between our results and the ones found in recent explanations of the incidence of civil war. For example, Blomberg & Hess (2002) find that recessions raise the probability of civil war much as they raise the homicide rate in our analysis. Hegre et al. (2001) find that harsh autocracies and coherent democracies have few civil wars, while our results suggest that they have fewer homicides than intermediate regimes. More generally, Collier & Hoeffler's (2002) finding, that civil wars can be explained better by economic characteristics such as low average income levels rather than grievances such as income inequality, mirrors our result that low income levels are associated with high homicide rates whereas income inequality, economic discrimination of ethnic minorities, and social welfare expenditures are not related to the extent of homicide. This suggests that some of the same processes that drive violent crime are also responsible for the outbreak of violence in the form of civil war. Also, in many countries, people feel threatened by violent crime as much as by internal or external threats of military violence. This supports the call for a broad and encompassing conceptual framework for the analysis of security (Buzan, Wæver & de Wilde, 1998).

Explaining Variation in Violent Crime: Traditional Theories

The traditional, mostly sociological, approach to explaining variation in violent crime is based upon modernization, opportunity, and cultural theory.[2] As we will see, it leaves little space for good policy to lower rates of violent crime.

Modernization Theory

Modernization theory based on Durkheim (1964) is the most famous sociological theory explaining variation in homicide rates. The social and economic transformation a country goes through in the process of development and modernization is seen as having important consequences for violent crime rates. Traditional forms of status relations, role allocations, social organization, and control are disrupted and ultimately destroyed as masses of people leave their rural homes and flood into the anonymous urban

[2] Due to the applied focus of this article, the various theories can merely be sketched here. More detailed discussions can be found in Neuman & Berger (1988) and Neapolitan (1997a).

conglomerates (Shelley, 1981). The resulting alienation of people who are often faced with unemployment or under-employment and poverty, and the lack of social integration (so-called anomie) or social capital (as economists would call it), lead to increased crime rates, including violent crime rates.

Modernization theory predicts that strong economic growth raises homicide rates as it disrupts traditional modes of social organization and control. The same applies to higher levels of income, at least up to a certain point. It is only after a country has successfully developed, and has reached a new mode of socio-economic organization and non-traditional social forms of control, that crime rates can be expected to decline again – if not rates of property crime, then at least rates of violent crime. As Messner (1982) and, more forcefully, Huang (1995) argue, this is compatible with modernization theory based on Durkheim, in that moral individualism need not lead to higher rates of violent crimes as so-called organic solidarity will eventually substitute for traditional social control and constraints on individual behavior (mechanic solidarity).

Opportunity Theory

Opportunity theory is subsumed under the umbrella of modernization theory by Neapolitan (1997a). This is because the specific factors put forward towards an explanation of differences in violent crime rates by opportunity theory are all influenced by modernization and development. Nevertheless, in our view, opportunity theory should be regarded as a theory of violent crime in its own right, since its specific focus is different from modernization theory. As its name suggests, opportunity theory tries to understand variation in violent crime rates in terms of different opportunities or favorable conditions for committing crime. Opportunity refers to both the pool of potential offenders and victims. To start with the offenders, it is a well-established fact that within any one country young males are the major perpetrators of violent crime. Hence, the prediction is that a higher proportion of such males leads to higher rates of violent crime, including homicide. As to the victims, since in the majority of cases the victims of homicide are known or even intimately related to the offender, one of the main predictions of opportunity theory is that smaller household sizes will lead to lower homicide rates (Kick & LaFree, 1985). As a corollary, a high density of population with frequent interpersonal encounters might be associated with higher rates of violent crime.

Cultural Theory

According to cultural theories, modernization theory is at fault in putting all countries into one single straightjacket and ignoring the fundamental role that cultural differences play in determining variation in violent crime rates. There is no single process of modernization and development all countries go through. At the very least, these processes and their effect on violent crime rates are conditional on differences in cultural norms and values. Cultural differences, broadly defined, could impact upon variation in violent crime rates in a variety of ways. Neapolitan (1994, 1997b), for example, argues that the typically higher rates of violent crime in Latin American and sub-Saharan African countries can be explained by a culture of violence as the consequence of violent colonization, slavery, and the destruction of traditional religious institutions and other modes of social control. The violent history prolongs oppression and violence into the post-colonial societies as violence breeds violence in a vicious circle. Many have also argued that cultural, ethnic, or racial differences within a society might lead to higher violent crime rates (see, for example, Avison & Loring, 1986). A further popular argument is that the predominant

religion in a country might have an important cultural influence on violent crime (Groves, McCleary & Newman, 1985). Christianity, in particular Protestantism, is often seen as emphasizing individualism and the freedom of the individual over social integration and group cohesion. Other religions, in particular Islam, are regarded as giving priority to the community over the individual and as exerting heavy social control over individuals, which might deter individuals from committing socially undesirable acts of violent crime. Further examples of ways in which cultural factors can influence violent crime rates might be gun availability and alcohol consumption. Higher availability of guns due to differing cultural attitudes towards the right and the utility of owning a gun is seen as promoting violent crime, as some crimes are simply not undertaken if guns, one of the most common means of committing violent crime, are not readily available (Lester, 1991; Killian, 1993). Culturally contingent higher rates of alcohol and other drug consumption have also been related to higher rates of violent crime, since these crimes are often committed while the perpetrator is under the influence of alcohol or illegal drugs (Lester, 1995; Rossow, 2001). Another example would be violent crime as a consequence of gang warfare over the market for illegal drugs.

Competing Theories with a Focus on Policy

The conceptual problem with these most popular theories of variation in homicide rates is that they leave very little room for good policies to lower violent crime. Their correlates refer to factors that are either impossible or at least very difficult for policymakers to influence. However, there are several competing theories that have a much clearer focus on how good policy might lower homicide rates.

Political and Civilization Theory: The Importance of Governance

Political theories look specifically at the political organization within a country and the behavior of governments in explaining variation in violent crime rates. It is often argued that democracies that provide their citizens with substantial political rights and civil liberties might have lower rates of violent crime (Huang, 1995), whereas autocracies have a generally higher incidence of lethal conflict (Cooney, 1997). This is because the respect for these rights is probably reflective of and congruent with values of self-respect as well as respect for the rights of others within society. However, this argument ignores the fact that autocracies tend to suppress unwanted social behavior, including violent crime, and are often successful at doing so. Therefore, it might be that democracy is associated with lower violent crime rates only at high levels of democracy. A different argument can be made with respect to the behavior of the government itself. Where governments engage in unlawful violation of human rights, political executions, disappearances, and so forth, this is likely to set a bad example and encourage violent crime amongst its citizens. Even where the execution of state-ordered violence is backed up by law, as is, for example, the case with the death penalty, the consecutive disrespect for the sanctity of all human life might encourage violent crime within society. A similar argument is put forward by civilization theory, based on Elias (1982). This theory predicts that the process of civilization leads to a more humane penal system, which is partly made possible because self-respect, self-restraint, and respect for the sanctity of life make traditional forms of social control, external constraints, and enforcement of laws less important. The types and levels of sanctions imposed by society are an indication of the extent of its civilization, and a harsh

penalty system is predicted to be associated with a high level of violent crime (Heiland & Shelley, 1992).

Economic or Rational Choice Theory: The Importance of the Economy

Based on the path-breaking work by Becker (1968), the economic or rational choice theory of violent crime assumes that individuals make rational choices about whether to engage in (violent) crime. The *Homo oeconomicus* therefore weighs the benefits against the costs of committing violent crime and decides to commit the crime if the net present value benefit exceeds the net present value cost (see Piliavin et al., 1986; Cornwell & Trumbull, 1994). The prediction is that the prevalence of homicide depends, among other things, on the probability of arrest and conviction, and the severity of punishment. As the death penalty is the ultimate punishment, the prediction is that the usage of the death penalty will have a deterrent effect on violent crime and will therefore lower homicide rates, all other things being equal. However, in a more general interpretation of the economic theory, the opportunity costs also play an important role. As individuals gain better economic prospects, for example, they should commit fewer acts of violent crime, since such acts now become less attractive relative to other opportunities. The opposite is true for situations of economic stress or crisis. Thus, higher average income levels and strong economic growth should lower homicide rates.

Deprivation and Economic Theory: The Importance of Inequality

Some criminologists and sociologists see inequality as an important cause of crime (see, for example, Hagan & Peterson, 1995). The relative deprivation of groups of individuals creates frustration, anger, and lack of social integration, particularly if these individuals are spatially concentrated in ghettos or slums. Similarly, in Marxist theory violent crime is seen as a consequence of economic and social frustration caused by the inevitable inequalities of capitalism. This frustration unloads itself in misguided aggression against fellow sufferers instead of leading to political rebellion and revolutionary action (Neuman & Berger, 1988; Hagan & Peterson, 1995). Even economic theory predicts a positive effect of inequality on crime as higher inequality raises the potential net gains from crime (Fleisher, 1966; Ehrlich, 1973; Kelly, 2000; Soares, 2002; Fajnzylber, Lederman & Loayza, 2002a). This argument applies much more strongly to property crime than homicide, however.

Research Strategy

Explanatory Variables

The major predictions of modernization theory are that higher income levels first raise and eventually lower homicide rates, whereas rapid economic growth and higher rates of urbanization lead to higher rates of homicide. Therefore, we use gross domestic product (GDP) per capita in purchasing power parity, its growth rate, and the percentage of people living in urban areas as relevant variables of modernization theory. The urbanization data are from World Bank (2001). The income data are also taken from World Bank (2001) as the primary source and from Summers & Heston (1991) and WHO (2000) as supplementary sources and were converted into constant 1997 prices with the help of the United States GDP deflator.[3] It is important to use income data in purchasing power parity rather than conventional income data since the latter often substantially underestimate the power of incomes to purchase goods and services in low-income countries.

Also, modernization theory predicts that

[3] Missing cases were interpolated.

anomie, that is, the lack of social integration, leads to higher homicide rates. The problem with this prediction is that it is very difficult to operationalize. Some studies use divorce rates as a proxy; others use the female workforce participation rate. Divorce data are not available for many countries and were therefore not included in this study. A higher female labor force participation might lead to less parental care for youngsters and might disrupt the traditional role allocation between the sexes in many male-dominated societies. Both effects might lead to greater anomie in society. In addition, as Gartner (1990) argues, increased female participation in the workforce is likely to result in reduced guardianship, which renders women and children, particularly, more vulnerable to violent attacks. Thus, it can additionally be interpreted as a variable of opportunity theory. Data for this variable were taken from World Bank (2001).

To test opportunity theory, we include three variables. The percentage of males in the age group between 15 and 64 among the total population and population density are taken from World Bank (2001); the average size of households is from UN (1999, 2001). Cultural theory is difficult to test explicitly since cultural differences across countries are difficult to quantify. For example, data on the availability of firearms and alcohol consumption patterns are not available for more than a few countries. However, since cultural differences such as religious composition, ethnic heterogeneity, availability of firearms, and alcohol consumption patterns are changing only slowly over time, as a first approximation they can be interpreted as time-invariant. In this case, the application of a fixed-effects estimator elegantly controls for cultural theory, as we will see in the section on methodology.

To test for the impact of good policy in the area of equity, we include three variables. First, we need a variable measuring economic inequality or relative economic deprivation. Unfortunately, inequality data, whether in the form of a Gini coefficient or relative income shares, are not available for many countries. Due to the poor availability of income inequality data, we therefore first test our model without the income inequality variable, before we include this variable in further estimation. As our proxy for income inequality, we use the Gini coefficient, which measures the concentration of incomes between 0 (absolute equality) and 1 (maximum inequality), with data taken from UN-WIDER (2000). This database includes that of Deininger & Squire (1996), but in addition contains further observations. Similar to Fajnzylber, Lederman & Loayza (2002a,b), we follow Deininger & Squire's (1996: 582) suggestion to add 6.6 to Gini coefficients stemming from expenditure instead of income surveys. Also similar to Fajnzylber, Lederman & Loayza (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. Our results on income inequality do not depend on using the UN-WIDER source. Using only the data contained in Deininger & Squire (1996) instead makes no difference.

Previous studies indicate that general income inequality might not matter as much as economic discrimination of ethnic minorities (Avison & Loring, 1986; Neapolitan, 1997a; Messner & Rosenfeld, 1997). Similar to Messner (1989) and Messner & Rosenfeld (1997), therefore, we took the economic discrimination of ethnic minorities index from the Minorities at Risk dataset (CIDCM, 2002), which codes discrimination on a 0 to 4 scale according to the severity of discrimination. This index was multiplied by the population share of the minority discriminated against. In the case of several minorities, the sum of all population-weighted discrimination indices was taken.

Third, we include a variable that measures the extent to which the state engages in social welfare expenditures, in order to cushion the inequality-generating impacts of markets. We use the share of social security and welfare expenditures as a percentage of GDP taken from IMF (various years). As with income inequality, this variable is only included in separate model estimations, owing to poor data availability. Some scholars have constructed more complex variables for testing the effect of equity policies on homicide rates. For example, Messner & Rosenfeld (1997), extending earlier work by Esping-Andersen (1990), have constructed a so-called decommodification index that reflects the extent and quality of social rights and entitlements, indicating a society's willingness to shield its citizens from the detrimental impacts of negative economic shocks. However, such a variable is available for only a few countries, and we believe that our variable reflects policies oriented towards achieving equity within society similarly well with much better data availability.

For testing the impact of good political governance on homicide rates, we employ three variables. First, Freedom House (2001) provides two indices based on surveys among experts assessing the extent to which a country effectively respects political rights and civil liberties, both measured on a 1 (best) to 7 (worst) scale. A combined freedom index was constructed by adding the two indices and reversing the index, such that it ranges from 2 (least democratic) to 14 (most democratic). Using Freedom House data over a period of time is not unproblematic, since the scale with which countries are judged changes slightly over time and is not designed as a series. This is particularly problematic in the middle parts of the Freedom House scale. However, Freedom House data have slightly better availability in our sample than, for example, the Polity data (Gurr & Jaggers, 2000), which do not suffer from this problem. Therefore, we use Freedom House data in our main estimations and Polity data in sensitivity analysis.

Second, to measure human rights violations we employ data from the Purdue Political Terror Scales (PTS) (Gibney, 2002). One of the two PTS is based upon a codification of country information from Amnesty International's annual human rights reports on a scale from 1 (best) to 5 (worst). Analogously, the other scale is based upon information from the US Department of State's Country Reports on Human Rights Practices. The simple average of the two scales was used for the present study.[4]

Third, we use a dummy variable that indicates whether a country applied the death penalty for ordinary violent crimes, with information taken from Amnesty International (2002). Note that the prediction of economic theory is exactly the opposite to the prediction of political and civilization theories. Whereas political and civilization theories predict a positive effect of the usage of the death penalty on homicide rates, as the governmentally approved disrespect for the sanctity of all human life promotes violent crime, economic theory predicts a negative association as the death penalty functions as a strong deterrent for potential perpetrators. Additionally, we would have liked to include further variables characterizing the enforcement and penal system of a country, but lack of data prevents this.[5]

To test the impact of good economic policies on homicide rates, we can use the variables economic growth rates and the level of economic development, the sources of which have already been described above. Note that economic theory predicts opposite

[4] If one index was unavailable for a particular year, the other one available was taken over for the aggregate index.
[5] The number of police personnel used by Fajnzylber, Lederman & Loayza (2002a,b) is available for only a small sample of countries.

effects of these variables on homicide rates than modernization theory. Whereas modernization theory predicts a positive effect of rapid economic growth on homicides as it destroys traditional forms of social organization, economic theory predicts a negative effect as strong economic growth opens new economic opportunities and lowers unemployment. Whereas modernization theory sees higher levels of income associated with higher homicide rates (at least initially) as mechanic solidarity is eroded, higher income levels raise the opportunity costs of committing acts of violent crime from the perspective of economic theory and are therefore unambiguously associated with lower homicide rates. In principle, one would like to employ unemployment rates as well, particularly for low-skilled workers, to test the economic theory (Lee & Shihadeh, 1998). However, they are not available for many countries. Given the relatively strong correlation of unemployment rates with economic growth rates, we decided not to use unemployment rates in order to avoid a substantial decrease in sample size.

Dependent Variable: Homicide Rates

There are three main sources of cross-national data for homicide: the International Criminal Police Organization (Interpol), the United Nations (UN), and the World Health Organization (WHO). Interpol annually publishes its International Crime Statistics, in which national police organizations report the number of intentional homicides, defined as 'any act performed with the purpose of taking human life, in whatever circumstance'. The UN has irregularly collected data for and published its United Nations Crime Survey (UNCS, various years). It defines homicide as 'death purposely inflicted by another person, including infanticide'. Contrary to Interpol data, the UNCS is sent out to and answered by governments, even though the source of data is likely to be police organizations. As such they are governmentally approved data, which might improve their reliability, but also opens the way for willful manipulation of undesirably high crime rates. The WHO (various years) annually reports data on death due to 'homicide and injury purposely inflicted by other persons' as one of the causes of death contained in its annual World Health Statistics Annual. Information is based on death certificates issued by doctors.

The WHO data are widely regarded as the most reliable (Huang & Wellford, 1989; Neapolitan, 1997a; LaFree, 1999). Not least, this is because its definition of homicide, which is based upon the death of the victim, does not leave room for ambiguity with respect to whether a homicide was merely attempted. Data are available for practically all developed countries and many Latin American and Caribbean developing countries, but unfortunately for only few Asian and even fewer African countries. The next best source is UNCS data, but since in our sample all countries included in the UNCS are also included in the WHO data, we do not draw any homicide data from the UNCS source. Interpol data are available for a greater variety of countries from all continents. Since 1977, Interpol asks the reporting countries to indicate the percentage of cases that have been mere attempts at homicide. However, many countries do not follow this request, so for reasons of consistency we use the total number of homicides, which includes both completed and attempted cases.

We will use data from the WHO (various years) whenever they are available. For those countries for which only Interpol data are available, we include them as well (Interpol, various years). We can do so because we use a fixed-effects estimator (see the discussion in the next section). In the estimations below,

we decided to use 1980 as a cutoff point. The exact cutoff point is somewhat arbitrary, but Neapolitan (1997a) suggests that data from the 1970s and before are likely to be less reliable than later data.

All data entries for both sources were inspected for obvious mis-reporting. Where data for a single or a few years were considerably higher or lower than the values in the prior and consecutive years, the data entry was taken out. The same applies if there was only one single data entry available for a country. Where there were substantial temporal breaks in a time-series, the whole series was taken out. Data that seemed to be inflated by a country's experience of civil or inter-country war were also deleted. Following these rules led to the deletion of only a few data from the WHO source, but more data from the Interpol source, which confirms that the former source is the more reliable one. Appendix 1 contains data that have not passed the test of inspection.

Methodology: The Importance of Controlling for Fixed Effects

We average the dependent and all explanatory independent variables over three years for the period 1980 to 1997 to reduce the influence of uncommonly high or low values in any one year. Formally, we test the following model:

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

Time is indicated by t, countries are indicated by i, y is the homicide rate per 100,000 people, α is a constant, x' contains the explanatory variables and β is the corresponding vector of coefficients to be estimated. The γ variables are T–1 period specific dummy variables. Their inclusion lets each time period have its own intercept to allow for aggregate time effects such as secular changes in homicide rates that affect all countries. The a_i represents individual country effects. Their inclusion in the model to be tested ensures that unobserved country heterogeneity, that is, heterogeneity of countries that is not fully captured by the explanatory variables, is accounted for. The fixed-effects estimator subtracts from the equation to be estimated the over-time average of the equation for each country. Because of this so-called within transformation, the individual country effects a_i are wiped out and the coefficients are estimated based on the time variation within each cross-sectional unit only.

The usage of a fixed-effects estimator represents an important improvement over existing literature and has a number of advantages. As mentioned in the introduction, most of the existing studies testing the determinants of variation in violent crime rates use simple multivariate OLS estimations for data from one time period only; this practice suffers from a number of important shortcomings. The number of observations is usually rather small so that degrees of freedom are low, which often means that only a few control variables can be included. More important, there is no way to control for unobserved heterogeneity bias in the estimates. Any inference on the coefficients of explanatory variables will be biased if these variables are correlated with time-invariant unobserved country heterogeneity. Given the importance that the cultural theory of variation in homicide rates has gained in recent years, given the difficulties in controlling for it explicitly, and given the (near) time-invariance of cultural factors, usage of the fixed-effects estimator elegantly controls for country-specific cultural factors. It is not just cultural factors that can be controlled for in this way, however. Any (approximately) time-invariant factor will automatically be part of the country-specific effect and will therefore be controlled for. Lastly, as mentioned already, fixed-effects estimation allows us to pool homicide data from the WHO

and Interpol sources. The differences in definition and source of data imply that homicide rates from both sources for the same country are not equal and that homicide rates for different countries are not comparable if they stem from different sources. However, since we are merely exploiting the over-time variation in the data, we can pool data from both sources as the time-consistency of each source in isolation is not affected by their inconsistency across sources. To be sure of this, we will also demonstrate the validity of our pooling of homicide data from both sources empirically. More generally, if the error of measurement in the data is systematically related to the country, but does not change much over time, then this measurement error is also taken into account.[6]

A further possible extension to the model to be tested is to make it dynamic in adding the lagged dependent variable as an explanatory variable, as is done in Fajnzylber, Lederman & Loayza (2002a,b). Doing so creates statistical problems as the lagged dependent variable is automatically correlated with the error term and therefore needs to be instrumented for with further lags of the dependent variable. Instrumentation usually leads to (possibly substantial) loss in the efficiency of estimation (Wooldridge, 2002). Therefore, a lagged dependent variable should be included only if there are strong theoretical reasons for doing so and if the lagged dependent variable is found to be significant in tests. Fajnzylber, Lederman & Loayza (2002b: 1344) suggest a number of theoretical reasons why more violent crime in the past might create more violent crime now, a discussion of which is beyond the scope of this article. We will merely accept the possibility of such an effect and test for the statistical significance of the lagged dependent variable in our model. However, we fail to find evidence for it (see next section).

To our knowledge, the only studies that have used a fixed-effects estimator are Bennett (1991a,b), Pampel & Gartner (1995), and Fajnzylber, Lederman & Loayza (2002a,b). In Pampel & Gartner (1995), fixed effects seem to be controlled for in one model specification for a sample of 18 developed countries over the period 1951 to 1986. Clearly, this sample is far too small and non-representative. Bennett tests a fixed-effects model for 38 nations (Bennett, 1991a) and 52 nations (Bennett, 1991b) over a 25-year period. This is a somewhat more representative sample, if still much smaller than the one used here, but the major shortcoming of Bennett (1991a,b) is the lack of focus on the impact of good policies on lower homicide rates. Fajnzylber, Lederman & Loayza (2002a,b) test dynamic, fixed-effects models for 39 and 45 countries over the periods 1965–95 and 1970–94, respectively. Their main conclusion is that economic growth is negatively associated with homicide rates, whereas the opposite is true for income inequality. Bourguignon (2001: 30) cautions against too much confidence in Fajnzylber, Lederman & Loayza's (2002a,b) results, noting that 'the corresponding samples of observations are very limited so that the relevant effects may be estimated on the basis of a few observations'.

We use standard errors that are fully robust towards arbitrary heteroscedasticity and serial correlation in fixed-effects and general methods of moments estimation. The same is not true for random-effects estimation employed in sensitivity analysis, for which standard errors are very difficult to compute in an unbalanced panel. To test for a non-linear effect of income levels and democracy on homicide rates, we additionally use a squared term of these variables. Note that even though there are no strong

[6] On this aspect, Soares (2002) reports interesting evidence that poor countries tend to under-report crime more than rich countries.

Table I. Descriptive Information on Variables

Variable	*N*	*Mean*	*S.d.*	*Min.*	*Max.*
Homicide rate (per 100,000 people)	544	7.0	13.0	.04	108.0
Average household size	544	4.4	1.6	2.2	11.7
Population density (per sq. km)	544	156.2	520.7	1.8	5,910
% of population male aged 15–64	544	.30	.04	.22	.53
Female labor force participation	544	34.8	10.9	6	55.6
% urban	544	56.9	22.6	4.5	100
ln (GDP per capita in US$1997)	544	8.6	1.0	6.3	10.6
Growth in GDP per capita	544	.49	5.4	–27.8	24.6
Democracy (Freedom House)	544	8.5	4.2	2	14
Democracy (Polity)	531	1.9	7.6	–10	10
Death penalty dummy	544	.55	.50	0	1
Human rights violations	544	2.3	1.0	1	5
Economic discrimination of ethnic minorities	544	.22	.46	0	3.4
Gini coefficient	274	35.9	9.2	16.6	61.5
Social welfare expenditures (% of GDP)	380	8.0	6.7	0	24.8

theoretical arguments for a non-linear effect of any of the other explanatory variables on homicide rates, we have nevertheless pre-tested for such an effect without ever finding the respective squared term to be statistically significant. Table I provides summary descriptive information on the dependent and all explanatory variables. Table II provides a correlation matrix of variables after fixed-effects transformation, which suggests little reason to be concerned about multicollinearity.

Results

Column 1 of Table III presents results for our basic model specification where our homicide data draw from both the WHO and Interpol sources; the income inequality and social welfare expenditure variables are not included due to poor data availability. Appendix 2 lists all the countries with data available for at least two time periods included in this estimation, but the panel is unbalanced since not all countries have observations in all time periods.

Before we come to analyze the results in column 1, it is worthwhile comparing them with those contained in column 2, which tests the same model for homicide data taken exclusively from the WHO source. As can be seen, the results in columns 1 and 2 are very similar in terms of sign and statistical significance. Indeed, the only difference is that the share of the male population between the ages of 15 and 64 becomes insignificant in the smaller sample with WHO data only. This rather good consistency of results makes us confident that we are justified in pooling homicide data from the two sources. Next, in column 3 we test for a dynamic effect of homicide rates. Owing to the presence of the lagged dependent variable, this model is estimated with Arellano & Bond's (1991) one-step generalized method of moments (GMM) estimator.[7] The lagged dependent variable is insignificant, which suggests that we do not need to account for dynamic effects. As mentioned in the previous section,

[7] We assume strict exogeneity only for household size, population density, the urbanization rate, and the female labor force participation rate, whereas all other variables are merely assumed to be weakly exogenous or predetermined, which allows them to be affected by past and current homicide rates.

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

	Homicide rate	ln GDP p.c.	Economic growth	% urban	Female labor force part.	Household size	Population density	% male 15–64	Democracy (FH)	Democracy squared	Death penalty	Human rights violations
ln GDP p.c.	–0.0973											
Economic growth	–0.1813	0.2184										
% urban	0.1383	0.3108	0.0892									
Female labor force part.	0.1530	0.4283	0.1908	0.4543								
Household size	–0.1267	–0.0486	0.0951	–0.2444	–0.2621							
Population density	–0.0414	0.2161	0.0319	0.0490	0.1840	–0.1268						
% male 15–64	0.2138	0.2368	0.1119	0.5478	0.5034	–0.1547	–0.1015					
Democracy (FH)	0.2447	–0.1798	–0.1832	0.1857	–0.0272	–0.1813	–0.0443	0.1299				
Democracy squared	0.1977	–0.1453	–0.1247	0.1920	0.0009	–0.1503	–0.0266	0.1338	0.9688			
Death penalty	–0.0119	0.0666	–0.0092	–0.1332	–0.1709	0.1216	–0.0011	–0.1046	–0.3100	–0.3263		
Human rights violations	0.0102	0.1342	0.1571	0.0950	0.1077	0.1813	–0.0165	0.0278	–0.5141	–0.5041	0.1573	
Economic discrimination	0.0689	–0.0779	–0.0930	0.0797	0.1254	–0.1099	0.0926	0.0019	0.1431	0.1659	–0.1113	–0.0224

p.c.: per capita; part.: participation; FH: Freedom House.

this has the advantage of avoiding a loss of efficiency in estimation due to the usage of lagged variables as instruments. Fajnzylber, Lederman & Loayza (2002a,b) find very strong evidence for a dynamic effect instead. It is not quite clear where this contrast comes from. Our sample is more representative and employs more time-varying control variables. Under-specified models tend to find dynamic effects, since in the absence of comprehensive control variables, the lagged dependent variable is usually a good predictor. A further possibility is that they use Arellano & Bond's (1991) two-step estimator. The two-step estimator is known to underestimate standard errors, which is why Arellano & Bond (1991: 291) warn against using it for statistical inference in samples that are as small as the ones in Fajnzylber, Lederman & Loayza (2002a,b).

The results from column 1 of Table III provide only partial support for the opportunity theory. Neither average household size nor population density tests significantly. Only female labor force participation and the share of male population between the ages of 15 and 64 are positively associated with higher homicide rates, as predicted by opportunity theory. The result on female labor force participation is also in line with modernization theory, but the rate of urbanization is highly insignificant. Similarly unsupportive of modernization theory is the result that neither the economic growth rate nor the level of economic development tests in accordance with this theory. Note that the squared income term tested highly insignificantly and, therefore, the results are shown without it. The negative impact of both income and economic growth rates stands in contrast to modernization theory and confirms the importance of good economic policies for lowering homicide rates instead. Regarding good political governance, we find a clearly non-linear effect of democracy on homicide rates: democratization first raises and then lowers homicide rates. Human rights violations are associated with higher violent crime. So is the death penalty. On the other hand, the extent of economic discrimination of ethnic minorities, the only equity variable included so far, tests highly insignificant. The Gini coefficient is added to the model in column 4, but is also insignificant. The same is true for the social security and welfare expenditures variable, which is added in column 5. Results for our other variables are remarkably consistent across differing sample sizes. The negative impact on homicide rates of good economic policies and good political governance holds true in all model specifications. The only exception is that the death penalty dummy variable becomes insignificant in column 4. The share of male population between the ages of 15 and 64 loses significance in both of the restricted samples, while the significance of female labor force participation declines only in column 4.

Sensitivity Analysis

As mentioned above, the Polity data might be a more reliable indicator of democracy, particularly in a panel data context. Columns 1 and 2 of Table IV repeat the estimations of the respective columns in Table III using Polity data instead. The non-linear effect is less clearly statistically significant, but still existent. The other coefficients are hardly affected.

Another area of concern is that fixed-effects estimation, in spite of its many advantages as argued above, has also some disadvantages, which should be kept in mind as a caveat in interpreting the fixed-effects estimation results. In particular, fixed-effects estimation draws upon the temporal variation of the explanatory variables only, which implies that variables that vary little over time are estimated inefficiently. The problem is that some of our control variables vary

Table III. Estimation Results (Homicide Rates, 1980–97)

	1 *(FE)*	*2* *(FE)*	*3* *(GMM)*	*4* *(FE)*	*5* *(FE)*
Lagged homicide rate			.09		
			(.10)		
Average household size	–.36	–2.9	–1.8	–.06	–4.6*
	(1.67)	(3.8)	(1.8)	(1.4)	(2.8)
Population density (per sq. km)	.000	.001	.006*	.003	–.002
	(.002)	(.001)	(.003)	(.002)	(.002)
% of population male 15–64	90.7**	27.0	208.2*	50.9	–13.0
	(38.3)	(23.9)	(121.6)	(45.4)	(26.4)
Female labor force participation	.45**	.36*	.63	.29	.79***
	(.21)	(.21)	(.46)	(.32)	(.28)
% urban	–.02	.08	–.02	.21	.01
	(.07)	(.08)	(.16)	(.13)	(.07)
ln (GDP per capita in US$1997)	–3.3**	–4.6***	–7.0***	–5.4***	–8.2***
	(1.3)	(1.3)	(2.7)	(1.7)	(1.9)
Growth in GDP per capita	–.15***	–.08**	–.13***	–.16**	–.14**
	(.05)	(.04)	(.05)	(.06)	(.06)
Democracy (Freedom House)	1.8***	1.9***	2.2**	1.7**	2.2***
	(.40)	(.45)	(.92)	(.76)	(.57)
Democracy squared	–.08***	–.07***	–.10**	–.09**	–.10***
	(.02)	(.02)	(.05)	(.04)	(.03)
Death penalty dummy	2.2**	2.9**	5.2	.38	2.1**
	(1.1)	(1.2)	(3.3)	(1.02)	(1.0)
Human rights violations	1.5***	2.3***	1.1	1.6**	1.6**
	(.56)	(.84)	(1.1)	(.73)	(.64)
Discrimination of ethnic minorities	.05	2.7	2.0	2.9	–1.2
	(.95)	(1.7)	(1.6)	(1.9)	(1.1)
Gini coefficient				–.09	
				(.06)	
Social welfare expenditures					–.00
					(.08)
Number of observations	537	361	289	273	377
Number of countries	117	75	95	90	75
R^2 (within)	.2003	.3551	.3319	.3376	
F-value	4.92	6.52		2.55	3.64
Significance *F*-value	.0000	.0000		.0010	.0000
Wald chi^2			63.18		
p-value autocorrelation order 1			.2943		
p-value autocorrelation order 2			.1715		

Dependent variable is homicide rate (homicides per 100,000 people). Fixed-effects (FE) and generalized method of moments (GMM) estimation. Standard errors in parentheses (robust towards arbitrary heteroscedasticity and autocorrelation). Coefficients of constant and time dummies not reported.
* significant at $p < .1$; ** $p < .05$; *** $p < .01$.

only little over time, which renders it more difficult to test our theories. For example, it is not clear whether the correlates of modernization, economic, and deprivation theory vary sufficiently over a time period of 18 years to allow for reliable testing. Typically, variables such as the level of income, urbanization, inequality, and the like vary much more across countries than over time. The problem with fixed-effects estimation worsens if some of the over-time variation in some of the variables is associated with noise, that is, if the variation is spurious. This can be a problem particularly if the data for variables come from various sources, as is the case for our homicide and inequality variables, for example. Random-effects estimation draws upon both the cross-sectional and temporal variation in the data and, therefore, can overcome these problems. However, it depends on the assumption that the country effects are not correlated with the explanatory variables, something we argued above is unlikely to be the case. The random-effects assumption can be tested with the so-called Hausman test. We concentrate on the models that include the inequality and the social welfare expenditures variables, because both change little over time and because of our striking result that none of these matter. Columns 3 and 4 of Table IV estimate these models with a random-effects estimator. Most importantly, both income inequality and social welfare expenditures remain statistically insignificant. There is slightly more support for modernization theory as the urbanization rate becomes marginally significant with the expected positive sign. Both the income level and the economic growth rate remain supportive of the economic rather than modernization theory, however. Strangely, the average household size is statistically significant with opposite signs in columns 3 and 4. These results need to be treated with care, however, as the Hausman test clearly rejects the random effects assumption, which supports our preference for fixed-effects estimation in our main analysis.

That income inequality is not statistically significant in our analysis is a rather striking result, the more so as most prior studies have found it to be one of the most consistent determinants of variation in homicide rates (Gartner, 1995; Fajnzylber, Lederman & Loayza, 2002a,b). What explains this difference in results? Bourguignon (2001: 26) suspects that cross-country differences in income inequality are strongly correlated with time-invariant cultural factors. In this case, the many studies that employ OLS would find a spurious correlation. In our panel data, we can mimic OLS in estimating the model with the so-called between estimator, which estimates a model with all variables averaged over time for each country via OLS. Note that one should not use this estimator with panel data, as only fixed-effects estimation is consistent if the fixed effects are correlated with the explanatory variables and random-effects estimation is more efficient if they are not. We use the between estimator for illustrative purposes only. We find a positive effect of income inequality on homicide rates in column 5 of Table IV. We conclude that the positive effect of income inequality found in many studies that rely purely on cross-sectional information, is likely to be spurious. Fajnzylber, Lederman & Loayza (2002a,b) seem to be the only ones who find income inequality to be significant in a panel data, where they control for fixed effects via first differencing. This might be because in Fajnzylber, Lederman & Loayza (2002a) they employ Arellano & Bond's (1991) two-step estimator, which is known to underestimate standard errors in small samples such as theirs, as mentioned above. Alternatively, where they employ a simple first-differenced estimator instead, as in Fajnzylber, Lederman & Loayza (2002b), the sample of

Table IV. Sensitivity Analysis (Homicide Rates, 1980–97)

	1 *(FE)*	*2* *(FE)*	*3* *(RE)*	*4* *(RE)*	*5* *(Between)*
Average household size	–1.2	–3.7	2.6**	–1.6***	3.7**
	(1.9)	(3.9)	(.99)	(.63)	(1.4)
Population density (per sq. km)	–.000	.001	–.001	–.001	–.004
	(.001)	(.001)	(.002)	(.001)	(.003)
% of population male 15–64	87.4**	34.2	50.7	–9.6	–86.9
	(39.5)	(27.7)	(42.0)	(26.8)	(94.8)
Female labor force participation	.41**	.30	–.02	.24***	–.03
	(.21)	(.20)	(.12)	(.09)	(.18)
% urban	–.10	.03	.15*	.09*	–.02
	(.07)	(.08)	(.08)	(.06)	(.12)
ln (GDP per capita in US$1997)	–4.4***	–5.8***	–2.6*	–4.4***	12.2**
	(1.4)	(1.5)	(1.5)	(1.3)	(4.0)
Growth in GDP per capita	–.16***	–.09**	–.15**	–.10**	.17
	(2.84)	(.04)	(.06)	(.06)	(.42)
Democracy (Freedom House)			1.7***	2.3***	3.6
			(.58)	(1.1)	(3.3)
Democracy squared (Freedom House)			–.10**	–.10***	–.23
			(.04)	(.03)	(.19)
Democracy (Polity)	.23***	.34***			
	(.07)	(.08)			
Democracy squared (Polity)	–.02*	–.03***			
	(.01)	(.01)			
Death penalty dummy	1.8**	2.6***	1.2 2.0*	10.6***	
	(.83)	(.75)	(1.4)	(1.8)	(3.4)
Human rights violations	.94*	1.8**	1.4**	2.0***	1.8
	(.53)	(.81)	(.62)	(.46)	(2.26)
Discrimination of ethnic minorities	.05	2.8	1.5	.14	–10.8***
	(1.0)	(1.8)	(1.7)	(.94)	(3.7)
Gini coefficient			-.02		.43**
			(.06)		(.21)
Social welfare expenditures				-.10	
				(.09)	
Number of observations	529	355	274	380	274
Number of countries	111	76	91	78	91
R^2 (within)	.1734	.3484			
R^2 (overall)			.2012	.1430	
R^2 (between)					.4717
F-value / chi^2	3.55	6.10	74.5	116.5	3.57
Significance *F*-value/chi^2	.0000	.0000	.0000	.0000	.0001
Hausman test chi^2			156.8	210.8	
Significance Hausman test			.0000	.0000	

Dependent variable is homicide rate (homicides per 100,000 people). Fixed-effects (FE), random effects (RE), and between estimation. Standard errors in parentheses (robust in case of FE). Coefficients of constant and time dummies not reported.

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

countries is rather restricted and non-representative (39 countries).

Discussion and Concluding Observations

After controlling for cultural factors in employing a fixed-effects estimator and controlling for various factors suggested by modernization and opportunity theory, our analysis demonstrates how good policies can lower homicide rates. Our results suggest that pacifying the state might pay off in terms of pacifying society as well. Greater respect for human rights and the abolition of the death penalty can both be expected to lead to lower homicide rates. While to the best of our knowledge this study is the first one to test for the impact of respect of human rights on homicide rates, the result for the existence of the death penalty is consistent with earlier studies such as Archer, Gartner & Beittel (1983) and Gartner (1990). However, this result needs to be treated with great care. To start with, the variable becomes insignificant in one regression. More importantly, of all the variables we use, the existence of the death penalty is clearly the one most susceptible to being endogenous to the homicide rate. In other words, the positive association between the two variables might be because high homicide rates prompt policymakers to introduce the death penalty in order to fight violent crime. One would need strictly exogenous instrumental variables to deal convincingly with this potential endogeneity problem – something that is regularly impossible to find. Still, if we use lagged or even twice-lagged values of the variable instead of its contemporaneous value in order to reduce potential simultaneity bias in column 1 of Table III, we still find a positive impact of the death penalty on homicides (results not shown). This is compatible with Archer, Gartner & Beittel's (1983) finding that the abolition of the death penalty is often followed by decreasing homicide rates rather than caused by them. While we cannot be certain, therefore, we have some hope that the government's decision not to engage in legalized killing of people is followed by a consecutive decrease in homicides.

The effect of democratization on homicide rates is more complex than previous studies such as Braithwaite & Braithwaite (1980), Krahn, Hartnagel & Gartrell (1986), Messner (1989), or Huang (1995) would suggest, as none of these tested for a non-linear effect. Our study demonstrates a non-linear effect where democratization initially raises homicide rates as the iron grip of autocratic state control is loosened. This confirms casual evidence from Latin American and Eastern European countries as well as South Africa, where homicide rates in the often ongoing period of democratic transition are higher than they were under autocratic rule. Homicide rates are predicted to fall again only after a fully developed democracy has been reached. This result represents a warning to policymakers that they need to pay particular attention to violent crime in the period of democratic transition. Failure to do so might backlash against democracy as some people start to blame democratic transition for the rise in violent crime rates.

Our results also suggest that a good recipe for lowering homicide rates would be policies which achieve good economic outcomes. Economic growth is doubly beneficial. First, homicide rates fall with strong economic growth and rise in times of economic recession. Second, economic growth raises average income levels, and higher income levels are also associated with lower homicide rates.

In contrast, we find no evidence that equity matters: income inequality, economic discrimination of ethnic minorities, and

social security and welfare expenditures do not seem to matter. Combating violent crime in the form of homicides does not seem to be a matter of redistribution and economic fairness. To be sure, economics matters, but it does so in the form of higher job availability due to strong economic growth and greater payoff to work effort as represented by income levels. Our result with respect to social welfare expenditures apparently contradicts a finding by Savolainen (2000), who finds a negative effect on homicide rates. However, his sample is very small, being restricted to Western developed and a few Eastern European countries. If we artificially restrict our sample to the same range of countries, then we also find a negative and statistically significant impact (results not shown). This suggests that Savolainen's (2000) finding is not generally valid outside his very restricted sample.

In comparison, our results provide little evidence in favor of modernization theory. However, this is in accordance with most prior studies, which have similarly failed to support this theory (see Neapolitan, 1997a: 90–92, for an overview of studies). Only female labor force participation and the degree of urbanization are positively associated with the predictions of modernization theory, but the latter only in random-effects estimation. More cause for concern is that we also find only limited evidence in favor of opportunity theory. In particular, the share of male population between the ages of 15 and 64 has the predicted effect in only one model specification, while population density and average household size do not have the predicted effect in any model. However, note that most prior studies similarly fail to find an effect of population density on homicide rates (Neapolitan, 1997a). Also, our result on the share of male population does not become more supportive of opportunity theory in changing the relevant age group to 15 to 44 years or 15 to 24 years (results not shown). This result is compatible with time-series analysis from five nations over a period of 70 years in Gartner & Parker (1990: 352), who find that 'the proportion of young adult males in a population does not exert consistent effects on that population's homicides rates across time and place'. It is also compatible with most prior cross-national studies, which similarly failed to find a positive association between population density or the proportion of young males and homicide rates (see, for example, Avison & Loring, 1986; Messner, 1982, 1989; Neapolitan, 1994).

How strong are the effects of good policies with reference to the full sample model in column 1 of Table III? The abolition of the death penalty is estimated to lead to a lowering of the homicide rate by 2.2 in 100,000 people. As mentioned already, this result needs to be treated with care given the potential endogeneity of the death penalty. A one-point improvement in the 1 to 5 scale of respect for human rights is associated with a reduction of 1.5 homicides in 100,000 people. As mentioned, the effect of democracy on homicide rates is nonlinear. In columns 1 of Tables II and IV, the estimated threshold is at a Freedom House index of around 11.5 and a Polity value of 6.9. As the Freedom House index runs from 2 to 14 and the Polity measure from –10 to 10, this threshold implies that only full democratization will lower the homicide rate, whereas democratization in the period of transition to a full-fledged democracy is associated with higher homicide rates.[8] The immediate effect of economic growth rates is relatively small: a percentage point increase in the growth rate lowers the homicide rate by only .15 per 100,000. But economic growth also raises income levels and thereby exerts an indirect effect on homicide rates: a

[8] The threshold is estimated as $(-\delta/2\phi)$, where δ is the coefficient of the democracy variable and ϕ the coefficient of the squared term.

10% increase in the average income level is associated with a lowering of the homicide rate by .12 per 100,000. Clearly, the effect of good policies on homicide rates is not only statistically significant, but also significant in terms of the strength of the effect. Homicide rates are not simply determined by modernization, cultural factors, and population characteristics. Instead, good policies can substantially lower the number of people killed and thereby bring about a more pacified society.

Appendices

Appendix 1
Data excluded from samples:

- *WHO*: Azerbaijan, 1992–95; Guatemala, 1984; Iran, all years; Philippines, all years; Sri Lanka, all years; Tajikistan, 1992.
- *Interpol*: Bahrain, 1989; China, 1980 and 1981; Honduras, all years; Iraq, 1987; Lesotho, 1981 and 1997; Malaysia, 1993; Morocco, all years; Rwanda, all years; Sri Lanka, all years; Sudan, 1995; Suriname, 1996–97; Syria, 1980 and 1983; Turkey, 1980; Zimbabwe, 1994.

Appendix 2
Countries included in sample:
Albania, Argentina, Armenia, Australia, Austria, Azerbaijan, Bahrain, Bangladesh, Belarus, Belgium, Brazil, Bulgaria, Burundi, Cameroon, Canada, Chile, Colombia, Congo (Rep.), Costa Rica, Côte d'Ivoire, Croatia, (Cuba), Cyprus, Czech Republic, Denmark, Dominican Republic, Ecuador, Egypt, El Salvador, Estonia, Ethiopia, Finland, France, Gambia, Georgia, Germany, Ghana, Greece, Guinea, Guinea-Bissau, Guyana, Haiti, Hungary, (Iceland), India, Indonesia, Iraq, Ireland, Israel, Italy, Jamaica, Japan, Jordan, Kazakhstan, Kenya, Korea (Rep.), Kuwait, Kyrgyz Republic, Latvia, Lesotho, (Lebanon), Lithuania, Luxembourg, Madagascar, Malawi, Malaysia, Mauritania, Mexico, Moldova, Mozambique, Myanmar, Namibia, Nepal, Netherlands, New Zealand, Nicaragua, Niger, Nigeria, Norway, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Poland, Portugal, Romania, Russian Federation, Saudi Arabia, Senegal, Singapore, Slovak Republic, Somalia, (South Africa), Spain, Sudan, Suriname, Swaziland, Sweden, Switzerland, Syria, Tajikistan, Tanzania, (Togo), Trinidad and Tobago, Tunisia, Turkey, Turkmenistan, Uganda, Ukraine, United Arab Emirates, United Kingdom, United States, Uruguay, Uzbekistan, Venezuela, (Vietnam), Yemen, (Yugoslavia), Zambia, Zimbabwe.

Countries in parentheses are dropped from fixed-effects estimation as only one data point is available.

References

Amnesty International, 2002. *Abolitionist and Retentionist Countries*. London: Amnesty International.

Archer, Dane; Rosemary Gartner & Marc Beittel, 1983. 'Homicide and the Death Penalty: A Cross-National Test of a Deterrence Hypothesis', *Journal of Criminal Law & Criminology* 74(3): 991–1013.

Arellano, Manuel & Stephen Bond, 1991. 'Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations', *Review of Economic Studies* 58(2): 277–297.

Avison, William R. & Pamela L. Loring, 1986. 'Population Diversity and Cross-National Homicide: The Effects of Inequality and Heterogeneity', *Criminology* 24(1–2): 733–750.

Becker, Gary S., 1968. 'Crime and Punishment: An Economic Approach', *Journal of Political Economy* 76(2): 169–217.

Bennett, Richard R., 1991a. 'Routine Activities: A Cross-National Assessment of a Criminological Perspective', *Social Forces* 70(1): 147–163.

Bennett, Richard R., 1991b. 'Development and Crime: A Cross-National, Time-Series Analysis of Competing Models', *Sociological Quarterly* 32(3): 343–363.

Blomberg, S. Brock & Gregory D. Hess, 2002. 'The Temporal Link Between Conflict and Economic Activity', *Journal of Conflict Resolution* 46(1): 74–90.

Bourguignon, Francois, 2001. 'Crime as a Social Cost of Poverty and Inequality: A Review Focusing on Developing Countries', manuscript. Washington, DC: World Bank (http://poverty.worldbank.org/library/view/13480/).

Braithwaite, John & Valerie Braithwaite, 1980. 'The Effect of Income Inequality and Social Democracy on Homicide', *British Journal of Criminology* 20(1): 45–53.

Buzan, Barry; Ole Wæver & Jaap de Wilde, 1998. *Security – A New Framework for Analysis*. Boulder, CO: Lynne Rienner.

CIDCM, 2002. *Minorities at Risk Database*. College Park, MD: Center for International Development and Conflict Management, University of Maryland.

Collier, Paul & Anke Hoeffler, 2002. *Greed and Grievance in Civil War*, manuscript. Washington, DC: World Bank (http://www.worldbank.org/research/conflict/papers/greed.htm).

Cooney, Mark, 1997. 'From Warre to Tyranny: Lethal Conflict and the State', *American Sociological Review* 62(2): 316–338.

Cornwell, Christopher & William N. Trumbull, 1994. 'Estimating the Economic Model of Crime with Panel Data', *Review of Economics and Statistics* 76(2): 360–366.

Deininger, Klaus & Lyn Squire, 1996. 'A New Data Set Measuring Income Inequality', *World Bank Economic Review* 10(3): 565–591.

Durkheim, Emile, 1964. *The Division of Labor in Society*. New York: Free Press.

Ehrlich, Isaac, 1973. 'Participation in Illegitimate Activities: A Theoretical and Empirical Investigation', *Journal of Political Economy* 81(3): 521–565.

Elias, Norbert, 1982. *The Civilizing Process*. Oxford: Blackwell.

Esping-Andersen, Gosta, 1990. *The Three Worlds of Welfare Capitalism*. New York: Princeton University Press.

Fajnzylber, Pablo; Daniel Lederman & Norman Loayza, 2002a. 'What Causes Violent Crime?', *European Economic Review* 46(7): 1323–1356.

Fajnzylber, Pablo; Daniel Lederman & Norman Loayza, 2002b. 'Inequality and Violent Crime', *Journal of Law and Economics* 45(1): 1–40.

Fleisher, Belton, 1966. 'The Effect of Income on Delinquency', *American Economic Review* 56(1/2): 118–137.

Freedom House, 2001. *Annual Surveys of Freedom Country Ratings 1972–73 to 2000–01*. New York: Freedom House.

Gartner, Rosemary, 1990. 'The Victims of Homicide: A Temporal and Cross-National Comparison', *American Sociological Review* 55(1): 92–106.

Gartner, Rosemary, 1995. 'Methodological Issues in Cross-Cultural Large-Survey Research on Violence', in R. Barry Ruback & Neil Alan Weiner, eds, *Interpersonal Violent Behaviors – Social and Cultural Aspects*. New York: Springer (7–24).

Gartner, Rosemary & Robert Nash Parker, 1990. 'Cross-National Evidence on Homicide and the Age Structure of the Population', *Social Forces* 69(2): 351–371.

Gibney, Mark, 2002, *Political Terror Scales Dataset*. Asheville, NC: University of North Carolina.

Groves, W. Byron; Richard McCleary & Graeme R. Newman, 1985. 'Religion, Modernization, and World Crime', *Comparative Social Research* 8: 59–78.

Gurr, Ted Robert & Keith Jaggers, 2000. *Polity98 Project*. College Park, MD: University of Maryland.

Hagan, John & Ruth D. Peterson, eds, 1995. *Crime and Inequality*. Stanford, CA: Stanford University Press.

Hegre, Håvard; Tanja Ellingsen, Scott Gates & Nils Petter Gleditsch, 2001. 'Toward a Democratic Civil Peace? Democracy, Political Change, and Civil War 1816–1992', *American Political Science Review* 95(1): 33–48.

Heiland, Hans-Günther & Louise Shelley, 1992. 'Civilization, Modernization and the Development of Crime and Control', in Hans-Günther Heiland, Louise Shelley &

Hisao Katoh, eds, *Crime and Control in Comparative Perspectives*. Berlin: Walter de Gruyter (1–19).

Huang, W. S. Wilson, 1995. 'A Cross-National Analysis of the Effect of Moral Individualism on Murder Rates', *International Journal of Offender Therapy and Comparative Criminology* 39(1): 63–75.

Huang, W. S. Wilson & Charles F. Wellford, 1989. 'Assessing Indicators of Crime Among International Crime Data Series', *Criminal Justice Policy Review* 3(1): 28–47.

IMF, various years. *Government Financial Statistics Yearbook*. Washington, DC: International Monetary Fund.

Interpol, various years. *International Crime Statistics*. Saint-Cloud: International Criminal Police Organization.

Kelly, Morgan, 2000. 'Inequality and Crime', *Review of Economics and Statistics* 82(4): 530–539.

Kick, Edward L. & Gary D. LaFree, 1985. 'Development and the Social Context of Murder and Theft', *Comparative Social Research* 8: 37–58.

Killian, Martin, 1993. 'Gun Ownership, Suicide and Homicide: An International Perspective', in *Understanding Crime: Experiences of Crime and Crime Control*, Acts of the International Conference, Rome, 18–20 November 1992. Rome: United Nations Interregional Crime and Justice Research Institute (289–302).

Krahn, Harvey; Timothy Hartnagel & John Gartrell, 1986. 'Income Inequality and Homicide Rates: Cross-National Data and Criminological Theories', *Criminology* 24(2): 269–295.

LaFree, Gary, 1999. 'A Summary and Review of Cross-National Comparative Studies of Homicide', in M. Dwayne Smith & Margaret A. Zahn, eds, *Homicide – A Sourcebook of Social Research*. London: Sage (125–145).

Lee, Matthew R. & Edward S. Shihadeh, 1998. 'Labor Market Structure, Unemployment, and Crime: The Indirect Effect of Low-Skill Jobs on Homicide and Theft in 26 Countries', *Sociology of Crime, Law, and Deviance* 1: 49–64.

Lester, David, 1991. 'Crime as Opportunity – A Test of the Hypothesis with European Homicide Rates', *British Journal of Criminology* 31(2): 186–188.

Lester, David, 1995. 'The Association Between Alcohol Consumption and Suicide and Homicide Rates: A Study of 13 Nations', *Alcohol & Alcoholism* 30(4): 465–468.

Messner, Steven F., 1982. 'Societal Development, Social Equality, and Homicide: A Cross-National Test of a Durkheimian Model', *Social Forces* 61(1): 225–240.

Messner, Steven F., 1989. 'Economic Discrimination and Societal Homicide Rates: Further Evidence on the Cost of Inequality', *American Sociological Review* 54(4): 597–611.

Messner, Steven F. & Richard Rosenfeld, 1997. 'Political Restraint of the Market and Levels of Criminal Homicide: A Cross-National Application of Institutional-Anomie Theory', *Social Forces* 75(4): 1393–1416.

Neapolitan, Jerome L., 1994. 'Cross-National Variation in Homicides: The Case of Latin America', *International Criminal Justice Review* 4: 4–22.

Neapolitan, Jerome L., 1997a. *Cross-National Crime – A Research Review and Sourcebook*. Westport, CT: Greenwood.

Neapolitan, Jerome L., 1997b. 'Homicides in Developing Nations: Results of Research Using a Large and Representative Sample', *International Journal of Offender Therapy and Comparative Criminology* 41(4): 358–374.

Neuman, W. Lawrence & Ronald J. Berger, 1988. 'Competing Perspectives on Cross-National Crime: An Evaluation of Theory and Evidence', *Sociological Quarterly* 29(2): 281–313.

Pampel, Fred C. & Rosemary Gartner, 1995. 'Age Structure, Socio-Political Institutions, and National Homicide Rates', *European Sociological Review* 11(3): 243–260.

Piliavin, Irving; Craig Thornton, Rosemary Gartner & Ross L. Matsueda, 1986. 'Crime, Deterrence, and Rational Choice', *American Sociological Review* 51(1): 101–119.

Rossow, Ingeborg, 2001. 'Alcohol and Homicide: A Cross-Cultural Comparison of the Relationship in 14 European Countries', *Addiction* 96(3): S77–S92.

Savolainen, Jukka, 2000. 'Inequality, Welfare State, and Homicide: Further Support for the

Institutional Anomie Theory', *Criminology* 38(4): 1021–1039.

Shelley, Louise, 1981. *Crime and Modernization*. Carbondale, IL: Southern Illinois University Press.

Soares, Rodrigo Reis, 2002. Development, Crime, and Punishment: Accounting for the International Differences in Crime Rates, manuscript. Chicago, IL: University of Chicago (http://www.wam.umd.edu/~soares/academic.htm).

Summers, R. & A. Heston, 1991. 'The Penn World Table (Mark 5): An Expanded Set of International Comparisons, 1950–1988', *Quarterly Journal of Economics* 106(2): 1–41.

UN, 1999. *Women's Indicators and Statistics on CD-Rom*. New York: United Nations.

UN, 2001. *Compendium of Human Settlement Statistics 2001*. New York: United Nations.

UNCS, various years. *United Nations Crime Surveys*. Vienna: United Nations Center for International Crime Prevention.

UN-WIDER, 2000. *World Income Inequality Database*. Helsinki: United Nations University–World Institute for Development Economics Research.

WHO, 2000. *Estimates of Income Per Capita for the World Health Report 2000*. Geneva: World Health Organization.

WHO, various years. *World Health Statistics Annual*. Geneva: World Health Organization.

Wooldridge, Jeffrey M., 2002. *Analysis of Cross-Sectional and Panel Data*. Cambridge, MA: MIT Press.

World Bank, 2001. *World Development Indicators on CD-Rom*. Washington, DC: World Bank.

ERIC NEUMAYER, b. 1970, PhD in Development Studies (London School of Economics, 1998); Senior Lecturer in Environment and Development, London School of Economics (1997–). Most recent book: *The Pattern of Aid Giving – The Impact of Good Governance on Development Assistance* (Routledge, 2003).