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POVERTY, INFANT MORTALITY, AND HOMICIDE RATES IN CROSS-NATIONAL PERSPECTIVE: ASSESSMENTS OF CRITERION AND CONSTRUCT VALIDITY*

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Abstract

This paper assesses the extent to which the infant mortality rate might be treated as a “proxy” for poverty in research on cross-national variation in homicide rates. We have assembled a pooled, cross-sectional time-series dataset for 16 advanced nations over the 1993–2000 period that includes standard measures of infant mortality and homicide and also contains information on two commonly used “income-based” poverty measures: a measure intended to reflect “absolute” deprivation and a measure intended to reflect “relative” deprivation. With these data, we are able to assess the criterion validity of the infant mortality rate with reference to the two income-based poverty measures. We are also able to estimate the effects of the various indicators of disadvantage on homicide rates in regression models, thereby assessing construct validity. The results reveal that the infant mortality rate is more strongly correlated with “relative poverty” than with “absolute poverty,” although much unexplained variance remains. In the regression models, the measure of infant mortality and the relative poverty measure yield significant positive effects on homicide rates, while the absolute poverty measure does not exhibit any significant effects. Our analyses suggest that it would be premature to dismiss relative deprivation in cross-national research on homicide, and that disadvantage is best conceptualized and measured as a multidimensional construct.

Keywords

cross-national homicide; poverty; infant mortality; validity

I. INTRODUCTION

In a recent article in this journal, Pridemore (2008) has attempted to resolve a striking incongruence between two bodies of literature on social structure and homicide -- studies based on data for the U. S. and studies based on cross-national analyses. Reviews of U. S. studies consistently conclude that measures of poverty are among the most important and robust social structural determinants of homicide rates (Messner and Rosenfeld 1999; Pratt and Cullen 2005; Pridemore 2002; Sampson and Lauritsen 1994). In contrast, measures of poverty are conspicuously absent in cross-national studies, and the “main conclusion drawn from the cross-national literature is the importance of the inequality-homicide relationship” (Pridemore 2008: 135). Pridemore speculates that the failure to include measures of poverty in cross-national analyses probably reflects the lack of data – suitable poverty measures are not available for appreciable numbers of nations. Whatever the cause, the glaring omission

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of measures of poverty from cross-national analyses is highly unfortunate, Pridemore explains (2008: 134), not only because it results in incongruent findings across related literatures, but it also precludes confident conclusions about the determinants of cross-national variation in homicide rates because of the strong possibility of model misspecification.

Pridemore proposes that this deficiency in the literature can be rectified by using the infant mortality rate as a “proxy” for levels of poverty. He includes the infant mortality rate along with the most commonly used measure of income inequality (the Gini coefficient) and other relevant control variables in cross-sectional regression equations predicting homicide rates circa 2000 for a sample of 46 nations. The results of his analyses indicate that the infant mortality rate exhibits a significant positive effect on homicide rates, which is consistent with the findings on poverty and homicide for studies based on data for areal units within the U. S. Moreover, when the infant mortality rate is included in the model, the effect of income inequality becomes non-significant. Pridemore (2008: 146) cautions that the results of a single study do not allow for “sweeping conclusions,” but he proposes that his analyses clearly call for further studies on the role of poverty in explaining cross-national variation in homicide rates.¹

In this paper, we heed Pridemore’s call and delve further into the poverty/homicide relationship for research in which the units of interest are societies or nation states. We review Pridemore’s justification for using the infant mortality rate as a proxy for poverty and attempt to clarify some important conceptual and measurement distinctions that should inform the research on poverty and crime rates in criminology more generally. Our analysis then turns to empirical assessments of the interrelationships among alternative indicators of poverty using a pooled, cross-sectional time-series dataset for 15 European Union nations and the United States over the 1993–2000 period. This dataset includes standard infant mortality rates and homicide rates, but it also has information on two commonly used “income-based” measures which correspond to different conceptualizations of poverty. One measure is derived from an estimated income threshold for a minimum standard of living and is commonly conceptualized as an indicator of “absolute” deprivation. The other measure incorporates distributional considerations and is usually conceptualized as an indicator of “relative deprivation.” With these data on infant mortality, homicide, and varying conceptualizations and corresponding measures of poverty, we are able to address the following questions: (1) Insofar as the infant mortality rate is intended to serve as a proxy for an income-based poverty measure, does the infant mortality rate exhibit strong criterion validity when assessed with reference to the “absolute” and “relative” measures of poverty? (2) Which, if any, of the three candidate measures of poverty – infant mortality, absolute poverty or relative poverty – exhibits significant effects on homicide rates in regression equations using the pooled, cross-sectional time-series data?

II. THE RESEARCH CONTEXT: THEORY, CONCEPTUALIZATION, MEASUREMENT IN STUDIES OF THE POVERTY/HOMICIDE LINK

Pridemore locates his methodological excursus into the cross-national homicide literature within the broader context of research on the criminogenic effects of different types of deprivation. He correctly observes that there has been a longstanding debate among homicide researchers “over the predictive power of relative and absolute deprivation” (2008:

¹Paré (2006) has also conducted a cross-sectional analysis of national homicide rates to assess the relative effects of income inequality and poverty. His poverty measures are the Human Development Indices from the United Nations’ Human Development Report and infant mortality rates. He reports findings similar to those of Pridemore. For his sample of 63 nations, the poverty measures have significant effects on homicide rates, which account for the apparent effects of income inequality.

145). Prior to the 1980s, researchers studying areal units within the U. S. commonly conceptualized poverty as absolute deprivation and operationalized poverty with reference to the relative size of the population falling below the “official” poverty line (as determined by the U. S. Social Security Administration). Poverty thus understood refers to levels of resources that are inadequate for meeting the basic necessities of life (Messner and Rosenfeld 1999: 29). Criminologists have linked deprivation in this sense with the specific phenomenon of homicide by means of both motivational dynamics and processes of social control (Gartner, 1990: 93). With respect to motivational dynamics, Williams and Flewelling (1988: 423) explain that

“it is reasonable to assume that when people live under conditions of extreme scarcity, the struggle for survival is intensified. Such conditions are often accompanied by a host of agitating psychological manifestations, ranging from a deep sense of powerlessness and brutalization to anger, anxiety, and alienation. Such manifestations can provoke physical aggression in conflict situations.”

The deprivation associated with deficient resources has also been related to the ineffective operation of informal and formal social controls. As long recognized by the classical Chicago School theorists and as reaffirmed in more recent variants of the general social disorganization perspective, disadvantaged communities, especially those with “concentrated disadvantage,” confront difficulties mobilizing residents to exercise various forms of informal controls, and they are handicapped in securing external forms of formal (“public”) controls (Bursik and Grasmick, 1993; Sampson et al., 1997). In the absence of such effective controls, hostile impulses emerging in conflict situations are more likely to be expressed in violent ways. Moreover, violence can serve as a form of “self-help” in areas where formal means of social control are not available (Black, 1983).

In the early 1980s, studies by Messner (1982) and Blau and Blau (1982) emphasized the importance of differentiating between absolute and relative deprivation, and recommended that macro-level models of homicide rates include indicators of poverty and income *inequality*. This approach is based on the premise that assessments of well being are always relative to the conditions of others. Accordingly, “people are poor when they cannot live in ways which are ordinary for their own communities” (Messner, 1982: 104), i.e., when they are unequal. The relative deprivation associated with inequality has been linked with criminal violence through intervening mechanisms that are similar to those that have been invoked in arguments pertaining to absolute deprivation – the stimulation of motivations for violence and the weakening of social controls. As explained by Blau and Blau (1982: 119), inequality (especially ascriptive inequality) is likely to lead to “diffuse hostilities” that often engender aggression, which tends to be directed at convenient targets. Inequality can also undermine the legitimacy of the institutional order and weaken institutional controls, thereby “freeing” individuals to express hostile impulses in the form of criminal violence (see LaFree, 1998).

The studies by Messner and by Blau and Blau inspired a number of subsequent analyses based on varying areal units within the U. S. (cities, MSAs) that continued the exploration of the relative effects of measures of poverty and income inequality (e.g., Bailey, 1984; Blau and Golden, 1986; Messner, 1983; Williams, 1984). The results of these initial studies were often inconsistent. However, as evidence has accumulated over time, a fair degree of consensus has emerged in support of the conclusion that the association between poverty and homicide rates across territorial units in the U. S. is stronger and more robust than is the relationship between income inequality and homicide rates (Pridemore, 2002).²

The cross-national research on social structure and homicide has evolved in a different manner. Economic conditions in nations are usually represented by indicators of the overall

level of economic development (e.g., GDP/capita) and the size distribution of income (e.g., the Gini coefficient). In multivariate models with these economic measures and other socio-demographic covariates, income inequality has exhibited reasonably consistent positive effects on homicide rates (LaFree, 1999; Messner, 2003), although the effects of income inequality appear to be moderated by features of the social welfare system (Pratt and Godsey, 2003; Savolainen, 2000).³ More generous and expansive social welfare state regimes tend to moderate the criminogenic impact of income inequality. Interestingly, income-based measures of poverty have not been included among the standard predictors of cross-national variation in homicide rates; hence, the striking incongruence across literatures. It is in response to this incongruence that Pridemore proposes that the infant mortality rate can serve as a “proxy” for poverty in cross-national research.⁴

Upon close examination, it becomes apparent that Pridemore interprets “proxy” in two different ways implying different approaches to the study of poverty. The dictionary definition of “proxy” refers to an agent acting on behalf of another (Neufeldt, 1996: 1083). When applied in the present context the term implies that a suitable measure of poverty has been developed, but it cannot be used in certain circumstances, in this case because of data availability. The infant mortality rate becomes the effective “agent.” Moreover, by framing the discussion with reference to the incongruence between U. S.-based research and the cross-national research, Pridemore seems to imply that the infant mortality rate can serve as a proxy for the most commonly used measure in the U. S. research (the reference point for incongruence) – an income-based measure of poverty conceptualized as “absolute deprivation.” This interpretation of the meaning of “proxy” is also suggested by Pridemore’s (2008: 137) observation that infant mortality is “correlated with poverty.” An empirical correlation obviously implies two independent measures.⁵

Pridemore further argues, however, that the infant mortality rate is actually superior to income-based measures of poverty on both conceptual and methodological grounds. He notes that “traditional measures of poverty based on income are narrowly defined ...” (2008: 138) and observes that the use of infant mortality to measure poverty avoids arbitrary definitions of income that fail to reflect government benefits with economic value. He also points to the superiority of vital statistics registration systems, from which infant mortality rates are generated, in comparison with the surveys that are used to measure household income. These arguments imply that the infant mortality rate is useful for cross-national research on homicide not primarily because of limitations of data availability of income-based measures. Rather, the infant mortality rate is to be preferred on the grounds of both measurement and *validity* – it serves as a measure that better captures the meaning of poverty properly conceptualized. Poverty researchers have similarly observed that levels of infant mortality reflect access to the basic conditions necessary for a healthy life (e.g., clean water, air quality, sanitation, satisfactory diet) that are often difficult to measure among the poor in comparable ways across nations (Ross, 2006: 861).

²See Land et al. (1990) for a detailed review of the studies of homicide based on U. S. cities, states, and metropolitan areas using data for 1960, 1970, and 1980. Parker et al. (1999) update this review with studies published in the 1990s. Pratt and Cullen (2005) report the results of a meta-analysis of the quantitative research on crime more generally encompassing the 1960–99 period.

³The vast bulk of the cross-national homicide research has been based on cross-sectional designs and OLS estimation. An exception is the study by Neumayer (2003), which uses panel data and fixed-effects models. Neumayer claims that the widely observed positive effect of income inequality in prior work is likely to be spurious due to unmeasured time-invariant variables. As explained below, our analyses estimate fixed-effects models.

⁴Pridemore is correct in his assertion that income-based poverty measures are conspicuously absent in the cross-national homicide literature. However, measures of infant mortality rates have been included in prior analyses as components of indexes of socio-economic development (Messner 1989; Messner and Rosenfeld 1997; Savolainen 2000). Indicators of life expectancy, which tend to be very highly correlated with the infant mortality rate, have also been included in general development indexes (Antonaccio and Tittle 2007; Pratt and Godsey 2003).

⁵An empirical association between two measures can be distinguished analytically from the theorized relationship between an abstract concept and a proposed indicator. Following Costner (1985), the latter relationship is often referred to as an “epistemic correlation.”

This dualistic conceptualization and measurement of poverty advanced by Pridemore prompt different strategies for empirical assessments of the infant mortality rate in the study of cross-national variation in homicide rates. Insofar as the infant mortality rate is regarded as a proxy in the usual sense of the term for an income-based measure, it is instructive to examine the correlation between these two types of measures. This essentially constitutes an assessment of criterion validity with an income-based measure serving as a criterion variable (Neuman, 2003: 183). In contrast, by comparing the relative power of the infant mortality rate for explaining variation in homicide rates with that of income-based measures of poverty, it is possible to assess the construct validity of these two types of measures, assuming that the theoretical foundations for relating poverty and homicide are accepted. A valid measure of poverty should yield associations with other variables (in this case homicide rates) in a manner consistent with agreed upon theory (Frankfort-Nachmias and Nachmias, 1996: 168).

An additional conceptual distinction is highly relevant to the general study of social structure and homicide. As noted above, homicide researchers have distinguished between the notions of absolute and relative deprivation, and in the bulk of empirical work within the U. S., they have contrasted the effects of the official measure of poverty (percent of families below the poverty line) with those of measures of income inequality (summary measures of the overall size distribution of income such as the Gini coefficient). This approach stands in contrast to the work of poverty scholars, who have long distinguished between absolute and relative conceptualizations of *poverty*. Although any relative conceptualization of poverty obviously entails distributional considerations and thus is related to the concept of inequality, poverty researchers have concluded that “poverty and inequality are better understood as complementary but theoretically distinct concepts and measures” (Brady, 2003: 746).

Absolute measures of poverty have been constructed with a “basic budget” approach according to which researchers attempt to determine the threshold of income necessary to support a family at a basic level of subsistence by pricing out a basket of commodities. This method of measuring poverty was pioneered in the late 19th century in the United Kingdom by Booth and Rowntree, who examined the cost of a nutritionally adequate diet and determined whether a household had the means to procure it (Swanstrom et al., 2008: 297). Later, in the 1960s, Mollie Orshansky, an economist with the Social Security Administration, developed a similar method to determine poverty cutoffs in the United States. Orshansky identified the cost of an “economy level” food plan for different size families and multiplied the costs of the food plan by three, based on the 1955 Household Food Consumption Survey, which indicated that families of three or more spent about one third of their after-tax income on food in that year (Iceland, 2005: 201). According to this standard, a family is considered to be “poor” if its gross income is below the threshold based on family size and composition. Orshansky’s poverty thresholds were adopted as the official federal definition of poverty in 1969 and have been updated yearly only for inflation using the Consumer Price Index (CPI). Thus, the needs standard remains constant over time but is only adjusted to account for inflation. The intuitive appeal of the Orshansky poverty measure is that it reflects the basic premise accepted by some poverty researchers that “there is an irreducible absolutist core in the idea of poverty” (Sen, 1983: 159).

One of the main criticisms of the absolute approach is that what people judge to be poor varies across both time and place – the inherently “relative” dimension of deprivation discussed above. This becomes especially clear when standards of living in developed and developing regions of the world are compared. For example, while a car may be considered a luxury in some countries, in societies where most families own a car and where public transportation is poor, a car may be a necessity for transportation to and from work (Iceland,

2005: 203). Relative poverty measures explicitly address this criticism of the absolute approach by focusing on the distribution of incomes. This approach also takes into account the social relationships among people in that people whose resources fall considerably below those of others, even if they are physically able to survive, may not be able to participate adequately in social organizations and relationships (Iceland, 2005: 202).

Relative poverty measures are typically calculated by setting a threshold at a percentage of the national median household income. Those who fall below this distributional threshold are defined as poor. This approach was adopted by the European Union Member States in an effort to combat poverty and social exclusion. The Member States implemented the use of so-called “Laeken indicators,” which cover four dimensions of social exclusion -- financial poverty, employment, health and education, to monitor their progress in those areas (Notten and de Neubourg, 2007). The Laeken poverty measure reflects the economic dimension of social exclusion and is based on 60% of the national median adult equivalent income. When a household income falls below 60% of the national median, all the household members are classified as poor (Notten and de Neubourg, 2007).

Given this highly useful distinction between two different conceptualizations of poverty, it is possible to expand the empirical exploration of the utility of the infant mortality rate in the cross-national study of homicide rates. Specifically, indicators of both absolute and relative poverty can be used for purposes of assessing the criterion validity of the infant mortality rate. In addition, the relative power of the two types of income-based poverty measures and the infant mortality rate for explaining cross-national variation in homicide rates can be examined for purposes of assessing construct validity of the respective measures.

III. METHODS

A. Data Sources and Measures

To address our research questions, we have assembled a pooled, cross-sectional time-series dataset for 16 nations (the U. S. and 15 nations from the European Union) encompassing the eight years between 1993 and 2000. The specific sample has been determined by the availability of the two income-based poverty measures – a relative and an absolute measure.⁶ These poverty measures were computed by Notten and de Neubourg (2007) and have been distributed on the web under the auspices of the Maastricht Graduate School of Governance in the Netherlands.

The absolute measure of poverty follows the logic of the official measurement of poverty that is used in the U. S. As explained above, it is based on a threshold for an estimated minimum consumption basket with adjustments for cross-national variation in purchasing power. The relative measure of poverty follows the logic of the approach adopted in European Union Member States and is based on a poverty line “set at 60% of national median adult equivalent income” (Notten and de Neubourg, 2007), with adjustments for government transfers and the demographic composition of households. Both measures are expressed as “incidence” indicators, i.e., as the percent of individuals in poverty determined with respect to either the absolute or relative poverty threshold. The original data sources for the computation of the poverty measures are longitudinal national household surveys. The data source for the European Union members is the European Community Household Panel (ECHP). The income data for the U. S. are taken from the Cross-National Equivalent Files of the Panel Study of Income Dynamics (PSID).⁷ A few nations are missing the information

⁶The nations included are Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden, United Kingdom, and the United States.

necessary to compute the poverty measures in selected years. The measures of poverty are available for 119 nation-years during the 1993–2000 period.

Our measure of infant mortality is the standard one used in cross-national research, i.e., the number of infant deaths per 1,000 live births. The data for infant mortality rates are taken primarily from the United Nations (various years) *Demographic Yearbook*, supplemented with information received in personal correspondence.⁸ The homicide data have been taken from the cause of death statistics reported by the World Health Organization (WHO), which is widely regarded to be the most reliable source of information on homicide for cross-national comparison (LaFree and Tseloni, 2006: 32). WHO records homicides within the category of “external causes of morbidity and mortality” in its *International Classification of Diseases* (ICD). A death is classified as a homicide if it results “from injuries by another person with intent to injure or kill” (see <http://apps.who.int/classifications/apps/icd/icd10online/>; accessed July 1, 2009). We have recorded annual homicide rates, expressed per 100,000 population, for all nations in the sample for the time period under investigation.⁹

For purposes of assessing the construct validity of the infant mortality rate as a measure of poverty, it is important to control for other potential predictors of homicide rates to the extent possible. Model specification is complicated by the diverse array of theoretical arguments that have been put forth to explain cross-national variation in homicide (contrast, for example, Antonaccio and Tittle, 2007; Butchart and Engstrom, 2002; Neumayer, 2003; Savolainen, 2000) and by the limited degrees of freedom for the pooled, cross-sectional, time-series analysis. In the selection of control variables, we draw upon the work of Gartner (1990), who provides a useful framework for organizing the various hypothesized determinants of homicide. She proposes that these determinants can be grouped into four categories representing general contexts: the material, the integrative, the demographic, and the cultural. The material context refers to conditions that produce or reflect economic stress and affluence/deprivation. The integrative context refers to features of social ties that can exert social control. The demographic context refers to the contours of population structure. Finally, the cultural context encompasses norms supportive of violence, sometimes reflected in violent practices that are legitimated (e.g. capital punishment, warfare).

Our various measures (and proxy measure) of poverty capture aspects of the material context. As an additional indicator of this dimension, we include the commonly used measure of overall economic resources in a nation -- Gross Domestic Product (GDP) per capita in constant dollars.¹⁰ We have converted the GDP measure into natural logarithms to accommodate skewness in the distribution. The GDP data are available for download from the Penn World Table website (Heston, Summers, and Aten, 2006). To represent the “integrative context,” we use an indicator of family instability – the crude divorce rate (the number of divorces per 1,000 population). We include four features of population to reflect the demographic context: total population size, age structure, the sex ratio, and urbanism.

⁷Notten and de Nourbourg (2007) caution that “the task of applying both [poverty] approaches on each country/group of countries while ensuring comparability of results is not an easy, nor trivial one.” See their detailed exposition of the various adjustments to enhance such comparability. In an effort to assess the reliability of their procedures, we have compared their relative poverty measure with a comparable measure based on an alternative data source: the Luxembourg Income Study (LIS). The LIS is a cross-national data archive that contains income data from national surveys. The LIS reports the “Laeken” relative measure of poverty for selected years and countries (see <http://www.lisproject.org/>; accessed June 26, 2009). The correlation between Notten and de Nourbourg’s relative poverty measure and the LIS relative poverty measure for the 38 nation-years covered by both data sources is .951.

⁸We are very grateful to the UN *Demographic Yearbook* team at the United Nations Statistics Division for providing us with electronic files containing data on infant births and deaths to supplement figures retrieved on-line from the United Nations *Demographic Yearbook*.

⁹Gary La Free generously provided us with WHO homicide data.

¹⁰The measure of GDP per capita is the Laspeyres Index, which is a fixed-base index with 1996 serving as the reference year.

Total population size is expressed in natural logarithms; the sex ratio is measured as the number of males per 100 females; age structure is expressed as the percent of the population ages 15–24; and the measure of urbanism is the percent of the population in urban areas. The data on urbanism are taken from the World Bank (2008). The source for the other control variables is the United Nations *Demographic Yearbook*, supplemented with data from WHO in a few instances. The remaining context – the cultural context – has received the least attention in the literature given the difficulties in finding relevant information and devising plausible measures.¹¹ We do not attempt to capture the effects of cultural factors directly in our regression analyses, but the fixed-effects estimator (explained below) in effect controls for any time-invariant cultural differences across the nations in the sample.

B. Analytic Framework

Our approach to addressing the criterion validity of the infant mortality rate as a measure of poverty is straightforward. We treat the relative and absolute measures as criterion variables and compute the Pearson correlation coefficient between the infant mortality rate and each of these income-based poverty measures for the pooled dataset. A very strong positive correlation would indicate that the infant mortality rate can serve as a useful proxy for income-based measures of poverty when the latter are not available. Assessing construct validity through an analysis of cross-national variation in homicide rates is more complicated due to the longitudinal character of the data and the limited degrees of freedom.

With time-series data, it is necessary to address the issue of stationarity. Over long time spans, annual homicide rates for national samples tend to be non-stationary (Messner, Pearson-Nelson, Raffalovich, and Miner, 2008). Analyses with these data must therefore be conducted on the differenced variable (annual change). Over short time spans, however, such as the eight years in these data, there is insufficient information for meaningful inference about stationarity. To preserve degrees of freedom, we treat within-country homicide rates for the 1993–2000 period as stationary, and model any dynamic processes as auto-correlated error.¹² Clearly, when requisite data on poverty measures become available, our analyses need to be replicated over a much longer time period.

Time-series models of crime rates sometimes include lagged predictors along with predictors measured contemporaneously with the dependent variables. Ideally, theoretical knowledge about underlying processes should guide model specification. Criminological theories linking poverty with homicide, however, are too primitive to specify the precise nature of dynamic processes. Given that previous research has indicated lagged effects of economic conditions on crime rates (cf. Jacobs and Richardson 2008; Rosenfeld and Fornango, 2007), we explore both contemporaneous effects of the key independent variables and the effects for these variables with a one-year lag.

With so few within-country observations, model specification is severely constrained. We begin the regression analyses by estimating bivariate equations with each of the three contemporaneous poverty indicators and then estimate a model including all three of these indicators. Next, our analyses focus on lagged effects of these indicators. Subsequent models estimate the effect the poverty measures on homicide rates net of each of the control variables considered one at a time. To preserve degrees of freedom, we restrict all coefficients (other than the fixed-effects) to be constant across countries. All models include

¹¹A few studies have indicated a role for general value orientations, religious values, and values supportive of legitimate violence in the explanation of cross-national variation in homicide rates. For relevant citations, see Messner (2003: 705); see also Antonaccio and Tittle (2007) concerning religion.

¹²The estimated AR(1) coefficient shows no evidence of non-stationarity. Alternative estimation with dynamics modeled by the lagged dependent variable produced similar results.

country fixed-effects and are estimated using country weights and panel-corrected standard errors (Wooldridge, 2002). Coefficients therefore represent the weighted average over-time impact of within-country poverty measures on within-country homicide rates, net of the included control variable and net of any country-specific time-invariant unmeasured variables.

We recognize that there are some disadvantages associated with fixed-effects estimation. As Allison (2005) points out, fixed-effects models are less parsimonious than alternatives such as random-effects models or models that correct standard errors for correlated data. In addition, fixed-effects models eliminate all between-country variation in independent variables. Both features of these models result in larger standard errors than the alternatives, and thus less precise parameter estimates. We are nevertheless convinced that fixed-effects models are the most appropriate for addressing our research questions with these data given that these models control for time-invariant characteristics of nations that might be correlated with the explanatory variables (see Neumayer, 2003: 627).

IV. RESULTS

Before addressing our primary research questions, it is useful to describe relevant features of the sample. Table 1 reports univariate statistics for the variables in the analyses. The statistics reveal considerable variation in homicide rates, ranging from a low of .25 per 100,000 in Luxembourg (1993) to a high of 9.95 per 100,000 in the U. S. (1993). Given the outlier status of the U. S. on homicide rates (the lowest observed homicide rate in the U. S., 5.96, is almost 5 times the mean for the rest of the sample), we replicate the regression analyses with this case excluded.

Nations also exhibit appreciable variation in levels of poverty as reflected in the income-based measures, especially the absolute poverty measure. The highest level of absolute poverty is observed for Portugal in 1994: 42.2% of the population falls below the poverty line. At the other extreme, less than 1% (.4%) of the population in Luxembourg in 1994 is classified as poor according to the absolute criterion. Levels of relative poverty as reflected in the percentage of the population below 60% of the median income range from about 8% (Finland in 1995) to just over 25% (the U. S. in 1998). Infant mortality rates for the sample are clustered fairly closely around the mean value of 5.59 deaths per 1,000 live births (the minimum and maximum values are observed for Sweden in 1999 and Portugal in 1993, respectively).

In table 2, we report the correlations between the infant mortality rate and the two income-based poverty measures for the sample of 119 nation-years. Both correlations are significantly positive, as expected, but the correlation with the relative measure of poverty ($r = .744$) is noticeably stronger than the correlation with the absolute measure of poverty ($r = .539$).¹³ These results imply that the infant mortality rate performs much better as a proxy for poverty conceptualized as relative deprivation than conceptualized in the manner underlying the official poverty measurement in the U. S. (i.e., as absolute deprivation). However, although a correlation of .744 would be quite strong for a causal relationship between variables, it is not necessarily sufficient to indicate interchangeability of indicators. About 45 % of the variance in the relative measure of poverty is not explained by infant mortality rates ($1 - .744^2 = .446$).

¹³Examination of the full correlation matrix including the covariates considered in the regression analyses for the pooled sample reveals that the two poverty measures exhibit the strongest associations with infant mortality rates for the variables in the dataset (results not presented but available upon request).

Turning to the analyses of cross-national variation in homicide rates, we begin with an assessment of the contemporaneous effects of the candidate indicators of poverty. The first three models in table 3 report the results of bivariate regressions of homicide rates on each poverty indicator considered alone. Model IV then assesses partial effects for the equation with all three variables included. The results indicate that the only independent variable to yield significant contemporaneous effects on homicide rates is the infant mortality rate. High levels of infant mortality are associated with high homicide rates in the bivariate and multivariate regressions. In contrast, the coefficients for both income-based measures of poverty are always non-significant.

In table 4, we explore the possibility of lagged effects of the poverty measures on homicide rates, while including the contemporaneous measures. The results in Model I indicate that absolute poverty is once again not significantly related to homicide rates, while the results in Model III reveal that the infant mortality rate exhibits a positive and significant contemporaneous effect but no significant lagged effect. For the relative measure of poverty, in contrast, the coefficient for the lagged term is statistically significant. A high level of relative poverty in a given year is significantly associated with a high homicide rate in the following year. The results in Model IV show that the contemporaneous infant mortality rate and the lagged relative poverty measure retain their significant effects on homicide rates when the other variable is included in the regression equation.

In table 5, we assess the partial effects of infant mortality rates (contemporaneous) and relative poverty (lagged) on homicide rates with controls for other potential covariates. As explained above, only one control variable is included along with the primary independent variables in each of the equations given the limited degrees of freedom in the fixed-effects modeling. The findings are consistent with the previous analyses. The coefficients for the infant mortality rate (contemporaneous) and lagged relative poverty are significantly positive across all models. The only control variable with a significant coefficient for this sample of nations and years is the divorce rate. The negative sign of the divorce effect runs counter to prior research based on larger, more heterogeneous samples of nations (Gartner, 1990; Hunnicutt and LaFree, 2008; Messner et al., 2008). We have estimated comparable models with the absolute poverty measure in place of the infant mortality rate and the relative poverty measure to investigate the possibility of suppressor effects, but the coefficients for absolute poverty are non-significant in all equations (results not reported but available upon request).

To assess the robustness of the regression results, we have conducted several sensitivity analyses. We have re-estimated the equations in table 5 with the U. S. excluded given its outlier status on homicide. The results are highly similar to those reported above. There are also reasons to suspect that Finland might be a potentially influential case. Finland's homicide rate is relatively high, especially in comparison with other European nations, yet it has an advanced welfare state (Savolainen, 2000). Substantive conclusions of the regression analyses are unchanged when Finland is removed from the sample. We have also followed common practices in the cross-sectional homicide literature and computed composite indexes to represent collinear covariates. Principal components factor analysis of the covariates for the pooled dataset yields two distinct factors. The first factor can be readily interpreted as a general development dimension. Nations scoring high on this factor are the more economically developed (GDP/capita) and more urbanized (percent urban) nations, and those with high divorce rates and a relatively small young population (percent 15–24). The other factor reflects features of population structure. High values on this factor refer to nations with a relatively low total population and a comparatively high sex ratio. The effects of the infant mortality rate (contemporaneous) and relative poverty (lagged) once again prove to be robust. The coefficients for these variables are significantly positive when the

composite indexes are entered in the regression equation singly or in combination in place of the controls in table 5 (results not shown but available upon request).

Our analyses reported thus far have been limited to the period of the 1990s because comparable cross-national panel data on the income-based measures of poverty are available for this period. It is possible, however, to estimate a regression model with the infant mortality rate and the control variables for many the nations in the sample with data extending back to around 1950. For this sample, it is necessary to transform variables into first differences to induce stationarity.¹⁴ The large number of observations ($N = 539$ in the unbalanced design) allows for the estimation of a single model with all of the independent variables. The results indicate that the effect of infant mortality rates on homicide rates is positive but below conventional levels of statistical significance. This suggests that while our findings for infant mortality rates during the years under investigation are reasonably robust for the sample of advanced nations, they may not be generalizable to other time periods.

V. SUMMARY AND CONCLUSION

The purpose of our analysis has been to assess Pridemore's recent recommendation that the infant mortality rate serve as a "proxy" measure for poverty in cross-national analyses of homicide. We have argued that Pridemore uses "proxy" in two distinct senses: (1) to refer to a measure that can serve as an empirical substitute for conventional income-based measures; and (2) to refer to a measure that more faithfully captures the meaning of poverty properly conceptualized than do income-based measures. Given this point of departure, we have addressed two overarching questions. First, does the infant mortality rate exhibit strong criterion validity when assessed with reference to the two most commonly used income-based measures of poverty, a measure intended to capture "absolute deprivation" and a measure intended to reflect "relative deprivation"? Second, which of the three candidate indicators of disadvantage – infant mortality, poverty as absolute deprivation, and poverty as relative deprivation – exhibits effects on homicide rates that are consistent with theoretical expectations? A broad range of criminological theoretical arguments imply that poverty is conducive to lethal violence, and thus a valid poverty indicator should presumably be positively associated with national homicide rates. To address these questions, we have compiled a pooled, cross-sectional, time-series dataset for a sample of 16 advanced nations over the 1993–2000 period.

The results of our correlational analyses reveal that infant mortality rates exhibit a moderate positive association with an absolute income-based poverty measure but a noticeably stronger positive association with the relative income-based poverty measure. These findings are rather surprising given Pridemore's juxtaposition of the cross-national research on homicide with U. S. studies that have conceptualized and measured poverty as absolute deprivation. Our analyses suggest that to the extent that the infant mortality rate is a proxy in the sense of an "agent" or substitute for a poverty measure, it "stands in" for poverty understood as relative deprivation more so than for poverty as absolute deprivation for this sample of nations/years. However, we also observe that despite the strong correlation

¹⁴For the longer time span, both homicide and divorce rates are non-stationary, so our models with these data are estimated in differenced form, with additional lagged differences to accommodate more complex dynamics. The data for our primary analyses, which include the two income-based poverty measures, can thus be regarded as a narrow slice of the much larger, non-stationary time series. A slice of a non-stationary series is also non-stationary, but the narrow interval we have available (8 years) makes it impossible to estimate dynamic differenced models for this sample. Instead, as explained above, models are estimated as if the time slices were stationary. Indeed, panel unit-root tests reject the null of non-stationarity for the narrow window in our analyses including the income-based poverty measures.

between the infant mortality rate and relative poverty, almost half of the variance in one of these measures is not shared by the other.

Our regression analyses indicate that the absolute measure of poverty performs poorly as a predictor of variation in homicide rates. None of the estimated coefficients for this measure is statistically significant. In contrast, both the infant mortality rate and the relative poverty measure exhibit significant effects on homicide in the theoretically expected direction. High levels of infant mortality rates are associated with high homicide rates contemporaneously, whereas high levels of relative poverty predict high homicide rates with a one-year lag. These effects are sustained when both measures are included in the same equations, along with selected controls.

We recognize that our analyses are limited in several respects. While the pooled, cross-sectional time-series design allows us to generate a reasonable number of total observations for our sample of advanced nations, the available data encompass a short time-span (8 years). We are thus left with few degrees of freedom in the fixed-effects models, especially in models with the lagged measure of relative poverty. This has constrained our capacity to conduct multivariate analyses of the strategic independent variables with a comprehensive array of time-varying covariates. Generalizability is also a concern. The analyses are based on a relatively homogeneous sample of nations, and for these nations, the effect of the infant mortality rate on homicides rates is less impressive when the time period is extended back to around 1950.¹⁵ The extent to which the findings apply to distinct historical periods, and to the larger, more heterogeneous universe of nations, needs to be established in future research.

With these qualifications in mind, our findings have important methodological, conceptual, and theoretical implications. Why does the absolute measure of poverty perform so poorly in the regression analyses? One possible answer is suggested by the cross-national research on welfare state regimes and homicide rates. The advanced nations have established a range of social welfare policies intended to “decommodify” labor (Esping-Andersen, 1990), i.e., to make material well-being less dependent of the vicissitudes of the market. Moreover, prior research reveals that indicators of the extensiveness and generosity of welfare provisions are related to levels of homicide (Gartner, 1990; Messner and Rosenfeld, 1997), and that these indicators moderate the impact of economic conditions such as inequality on homicide rates (Pratt and Godsey, 2003; Savolainen, 2000). These findings underscore the fact that the correspondence between any particular level of individual or personal income and material well-being is contingent on the larger institutional context. Poverty researchers attempt to adjust for selected governmental transfers when computing poverty lines (e.g., Notten and de Neuborg, 2007), but insofar as these adjustments fail to incorporate the full panoply of social welfare provisions that effectively “decommodify” labor, a “basic budget” measure of poverty will not be strictly comparable across nations. The superior explanatory power of the relative poverty measure in comparison with the absolute measure in the homicide models suggests that the former is more sensitive to the institutional context and is accordingly a more valid indicator of the material conditions confronting the disadvantaged for the purposes of comparative analysis (see Iceland, 2005).¹⁶

¹⁵In an attempt to assess the generalizability of the results to a more diverse range of nations, we collected poverty data from the World Bank for supplemental analyses. The World Bank provides estimates of “headcount poverty” for selected developing nations based on national surveys that have been conducted at multiple time points. (See the World Bank online poverty analysis tool “PovcalNet,” at: <http://web.worldbank.org/WBSITE/EXTERNAL/EXTDEC/EXTRESEARCH/EXTPROGRAMS/EXTPOVRES/EXTPOVCALNET/0,,contentMDK:21867101~pagePK:64168427~piPK:64168435~theSitePK:5280443,00.html>; accessed June 25, 2009). Unfortunately, these data are not well suited for our purposes. The data are rather sparse, and observations are non-sequential and unevenly spaced within and across countries. As a result, it is not possible to estimate the kinds of regression models that are feasible with panel data. The World Bank data also do not include a measure of relative poverty comparable to that in our dataset.

Another important question raised by our results is why the infant mortality rate affects homicide rates independently of the effect of the relative poverty measure. Assuming that the relative measure of poverty is a reasonably valid indicator of levels of material well-being, these findings imply that the infant mortality rate captures social conditions relevant to homicide that extend beyond disadvantage in the strictly material sense. Especially in Europe, poverty researchers have increasingly expanded the conceptualization of disadvantage to encompass not only restricted access to material resources but also barriers to meaningful participation in society more generally. Such disadvantage is often discussed under the rubric of social exclusion, which has been conceptualized as a distinctively multidimensional and multilevel construct (Hills et al., 2002; Iceland, 2005; Sen, 2000).¹⁷ In addition to the capacity to consume goods and services (a central component of virtually any conceptualization of poverty), social exclusion encompasses the performance of productive economic roles (e.g., jobs), political engagement (e.g., involvement in collective decision-making), and integration in the local community (Burchardt et al., 2002b: 31). The focus of social exclusion is also not limited to attributes of individuals or households but extends to the community level and incorporates key insights pertaining to the geographic structuring of life chances.

This expanded conceptualization of deprivation associated with social exclusion can be related quite readily to the diverse array of intervening mechanisms that have been cited in the theoretical literature on poverty (both in the “absolute” and “relative” sense) and homicide. Social exclusion is likely to heighten the struggle for survival among those who confront extreme scarcity that is often associated with such exclusion. The socially excluded are likely to live in communities characterized by weak formal and informal controls. The marginalization accompanying social exclusion is likely to foster diffuse hostilities and to undermine the legitimacy of the institutional order. In short, it seems plausible to propose that the infant mortality rate yields independent explanatory power in our sample of advanced nations because it captures aspects of the adverse social conditions confronting excluded, marginalized populations that are not fully reflected in any of the income-based poverty measures.¹⁸

These interpretations considered in combination suggest an alternative account for the incongruence between homicide studies based on data for the U. S. and those based on data for cross-national samples. The U. S. exhibits the kind of institutional context in which an absolute measure of poverty serves as a reasonable indicator of material well-being, i.e., a nation with a comparatively underdeveloped welfare state.¹⁹ Accordingly, variation in homicide rates across areal units within the U. S. is reasonably well predicted by an absolute poverty measure, although such a measure performs poorly in our cross-national sample that includes nations with more generous and expansive social welfare policies. Moreover, the

¹⁶Our argument implies that the effect of an absolute measure of poverty will vary across nations reflecting the institutional context. This hypothesis could be tested in principle by estimating a random-effects model and including indicators of institutional context to explain any significant variation in the slope for absolute poverty across nations. However, because of data limitations, we have had to restrict within-nation effects to be the same across nations, which yields an average effect for the absolute measure of poverty of approximately zero.

¹⁷As Burchardt et al. observe (2002a: 2; see also Iceland 2005), the concept of social exclusion as used by European scholars is similar to notions of “ghettoization,” “marginalization,” and “underclass,” which have been linked with a range of social maladies in the U. S. literature (e.g., Wilson 1987). Burchardt et al. also note that the conceptualization of deprivation in a multidimensional and multilevel manner is not entirely new. In his classic research on poverty in the United Kingdom, Townsend (1979) anticipates many of the key elements of the contemporary concept of social exclusion. The distinctive extension associated with the concept of social exclusion is that “it allows the phenomenon of interest to extend beyond non-participation due to lack of material resources” (Burchardt et al. 2002a: 5).

¹⁸We are grateful to an anonymous referee for proposing this interpretation.

¹⁹Swanstrom et al. (2008) argue that relative measures of poverty are likely to be superior to absolute measures even for analyses within the U. S. Similar to European scholars, they emphasize the utility of adopting an expanded conceptualization of deprivation that focuses on social exclusion.

superior explanatory power of the infant mortality rate in comparison with the Gini coefficient in Pridemore's study does not necessarily imply that cross-national researchers have been misguided in their focus on the relative dimension of deprivation. Rather, the Gini coefficient, as a measure of the overall income distribution, may not fully reflect the social conditions of the theoretically strategic population – the socially excluded and marginalized.

We thus concur with Pridemore that cross-national homicide researchers should bring poverty back into the picture. The results of our analyses suggest, however, that it would be premature to dismiss considerations of the distribution of income. Instead, criminological researchers would be well advised to explore systematically the ways in which multiple facets of disadvantage, many associated with relative deprivation and social exclusion, operate in concert to generate conditions conducive to lethal violence under varying institutional contexts.

Another puzzle for criminological researchers is to solve is the distinctive lagged structures uncovered in our analyses, i.e., the one-year lagged effect of the relative poverty measure and the contemporaneous effect of the infant mortality rate. An anonymous reviewer has suggested that these findings might be interpreted by drawing on arguments advanced by Cantor and Land (1991) in their work on unemployment and crime. Cantor and Land (1991) propose that the effects of deteriorating economic conditions on criminal motivation are likely to require appreciable time intervals to emerge because workers typically have stocks of financial support to “cushion the blow” in the short run. Perhaps in an analogous manner, any effect of relative poverty on homicidal motivation might be delayed as objective economic conditions become manifested in perceptions of relative deprivation. The impact of the infant mortality rate, in contrast, might be more immediate reflecting the simple size of the socially excluded population.²⁰ These interpretation are of course speculative, but the results of our analyses suggest that another important task for future criminological theorizing is to delineate more fully and explicitly the *dynamic* processes that link poverty, and for that matter social structural covariates more generally, with homicide and other forms of criminal violence at the national level.

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²⁰In their analyses of unemployment, Cantor and Land (1991) contrast the “motivational” effect of unemployment with an “opportunity” effect reflecting overall system activity and guardianship, hypothesizing that the opportunity effect is likely to be contemporaneous. See Greenberg (2001) for a critique of Cantor and Land's approach and Cantor and Land (2001) for a response.

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Table 1

Descriptive Statistics

Variables	Mean	Standard Deviation	Minimum	Maximum	N
Homicide Rates	1.66	1.75	0.25	9.95	124
Absolute Poverty	13.21	10.52	0.40	42.20	119
Relative Poverty	15.82	4.48	8.10	25.40	119
Infant Mortality	5.59	1.16	3.37	8.73	128
Ln GDP/capita	9.99	0.27	9.39	10.78	128
Ln Population	16.54	1.50	12.89	19.46	128
Percent Ages 15–24	13.41	1.69	11.02	17.62	128
Males/Females	96.00	1.62	92.98	98.99	126
Divorce Rate	2.12	0.94	0.42	4.57	117
Percent Urban	73.70	12.07	49.82	97.10	128

Table 2

Correlations between Infant Mortality Rates and Income-Based Measures of Poverty

N = 119			
	Infant Mortality Rate	Absolute Poverty	Relative Poverty
Infant Mortality Rate	1.00	.539**	.744**
Absolute Poverty		1.00	.745**
Relative Poverty			1.00

** Correlation is significant at the 0.01 level (2-tailed test)

Table 3

Regressions of Homicide Rates on Contemporaneous Income-Based Poverty Measures and Infant Mortality Rates¹

Independent Variables	Model I (N = 97)	Model II (N = 97)	Model III (N = 108)	Model IV (N = 97)
Constant	1.277* (0.087)	1.235* (0.238)	0.996 (0.245)	0.150 (0.289)
Absolute Poverty	0.006 (0.007)	----	----	−0.003 (0.007)
Relative Poverty	----	0.008 (0.015)	----	0.018 (0.017)
Infant Mortality Rate	----	----	0.104* (0.049)	0.188* (0.043)
AR(1)	0.558* (0.093)	.545* (0.096)	.649* (0.087)	.327* (0.109)

*
p<.05

¹ All models include country-specific intercepts (fixed effects).

Table 4

Regressions of Homicide Rates on Contemporaneous and Lagged Income-Based Poverty Measures and Infant Mortality Rates¹

Independent Variables	Model I (N = 81)	Model II (N = 81)	Model III (N = 92)	Model IV (N = 82)
Constant	1.123* (0.113)	0.088 (0.276)	0.583 (0.371)	-0.240 (0.196)
Absolute Poverty	0.007 (0.009)	----	----	----
Absolute Poverty (-1)	0.007 (0.009)		----	----
Relative Poverty	----	0.017 (0.019)	----	----
Relative Poverty (-1)	----	0.062* (0.016)	----	0.052* (0.013)
Infant Mortality Rate	----	----	0.121* (0.051)	0.154* (0.043)
Infant Mortality Rate (-1)	----	----	0.060 (0.052)	----
AR(1)	0.385* (0.113)	0.174 (0.116)	0.525* (0.111)	0.139 (0.120)

* p < .05

¹ All models include country-specific intercepts (fixed effects).

Table 5

Regressions of Homicide Rates on Contemporaneous Infant Mortality Rates, Lagged Relative Poverty, and Control Variables^I

Independent Variables	Model I (N = 82)	Model II (N = 82)	Model III (N = 82)	Model IV (N = 82)	Model V (N = 71)	Model VI (N = 82)
Constant	-1.946 (3.534)	-36.310 (39.037)	-0.517 (0.466)	-5.939 (8.304)	1.012* (0.315)	2.185 (2.278)
Infant Mortality Rate	0.165* (0.049)	0.171* (0.045)	0.141* (0.053)	0.153* (0.043)	0.136* (0.043)	0.143* (0.045)
Relative Poverty (-1)	0.053* (0.014)	0.051* (0.013)	0.053* (0.014)	0.057* (0.015)	0.042* (0.012)	0.052* (0.014)
Ln GDP/capita	0.164 (0.338)	----	----	----	----	----
Ln Population	----	2.174 (2.353)	----	----	----	----
Pct. Ages 15-24	----	----	0.026 (0.042)	----	----	----
Males/Females	----	----	----	0.059 (0.085)	----	----
Divorce Rate	----	----	----	----	-0.447* (0.053)	----
Pct. Urban	----	----	----	----	----	-0.033 (0.031)
AR(1)	0.125 (0.121)	0.119 (0.124)	0.139 (0.120)	0.135 (0.118)	0.293* (0.091)	0.165 (0.120)

* p < .05

^I All models include country-specific intercepts (fixed effects).