

Changes in child welfare and subsequent crime rate trends A cross-national test of the lagged nurturance hypothesis

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Abstract

The General Evolutionary Ecological Paradigm for understanding criminal behavior [Criminology 32 (1994) 501.] predicts that social programs which improve the quality of such things as maternal and infant health care, parenting, and education will tend to reduce population-level crime rates 10–15 years later when children exposed to those conditions reach adolescence and early adulthood. This prediction—more simply referred to as the “lagged nurturance hypothesis”—is theoretically important because it integrates population-level perspectives on crime with the large body of microlevel research linking child development with delinquency. We test the lagged nurturance hypothesis using an extensive set of cross-national data. Despite inherent measurement and methodological difficulties associated with cross-national research that weaken statistical power, our findings support the lagged nurturance hypothesis. Implications for future research and public policy are discussed. © 2002 Elsevier Science Inc. All rights reserved.

1. Introduction

1.1. Lagged nurturance

Cohen and Machalek (1988) pioneered the evolutionary ecological approach in criminology. The cornerstone of their approach was the idea that well-understood theory and

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concepts from population biology and behavioral ecology could be used as an organizing scheme for understanding expropriative crime and our responses to it. The General Evolutionary Ecological Paradigm (GEEP) for understanding criminal behavior (Vila, 1994) extended the evolutionary ecological approach to apply to all criminal behaviors that involve force, fraud, or stealth. Subsequent development of this approach has proceeded along theoretical (Cohen & Machalek, 1994), experimental (Bueno de Mesquita & Cohen, 1995; Vila & Cohen, 1993), and applied (Cohen, Vila, & Machalek, 1995; Ekblom, 1999; Vila 1997a, 1997b) dimensions. The approach has been used to arbitrate fundamental theoretical debates in criminology (Cohen & Machalek, 1994; Cohen & Vila, 1996). And it has provided explicit falsifiable predictions that had not been derived from other theoretical approaches (see Cohen & Machalek, 1988, p. 497; Vila, 1994, pp. 341–343). Thus far, the logical adequacy of these predictions has been tested using game theoretic computer simulation models (Vila & Cohen, 1993) and mathematical analysis (Bueno de Mesquita & Cohen, 1995). Theoretical assertions regarding the importance of interaction effects between causal factors at different levels are consistent with subsequent findings by massive nationwide analyses regarding the causes of violent youth crime (Loeber & Farrington 1998, pp. 114–132).

Following a long tradition in behavioral ecology, the General Paradigm focuses attention at the macrolevel on strategic dynamics that influence responses to crime and decisions to use behaviors that have been socially defined as criminal. At the microlevel, the paradigm draws attention to the ways people develop over the course of their lives, placing special emphasis on the importance of experiences and environment early in life, especially those that affect child development and the transmission of biological traits and family management practices across generations. Thus, GEEP models link crime directly with a number of public health, educational, and childcare problems that often are considered outside the scope of crime control policy. It also identifies the time scales over which different kinds of crime control strategies should be applied and over which results should be anticipated.

The General Paradigm attempts to explain how people acquire a style of strategic behavior that disproportionately favors the use of force, fraud, or stealth to obtain valued resources, when and why they express this strategic style as crime, how individuals and groups respond to those crimes, and how all these phenomena interact as a dynamic, self-reinforcing system that evolves over time (see Fig. 1). To summarize the paradigm's explanation of criminal behavior and our responses to it, over the course of their lives, people acquire attributes such as knowledge, skills, attitudes, beliefs, and strategic styles via interactions among biological, sociocultural, and developmental factors (see Vila, 1994, p. 338). These attributes affect a person's ability to obtain resources at a particular point in time and how they value those resources. In turn, an individual's motivation to commit a crime in order to, for example, obtain a valued object, vent anger or frustration, or gain power over others is strongly influenced by other behavioral options open to them for obtaining these resources and the value they place on them. Another important factor affecting motivation is the presence of a tempting opportunity (Braithwaite, 1992; Clarke & Felson, 1993; Grasmick, Tittle, Bursik, & Arneklev, 1993; Katz, 1988). If motivation is sufficiently high and if an opportunity exists, a crime can occur (Cohen & Felson, 1979).

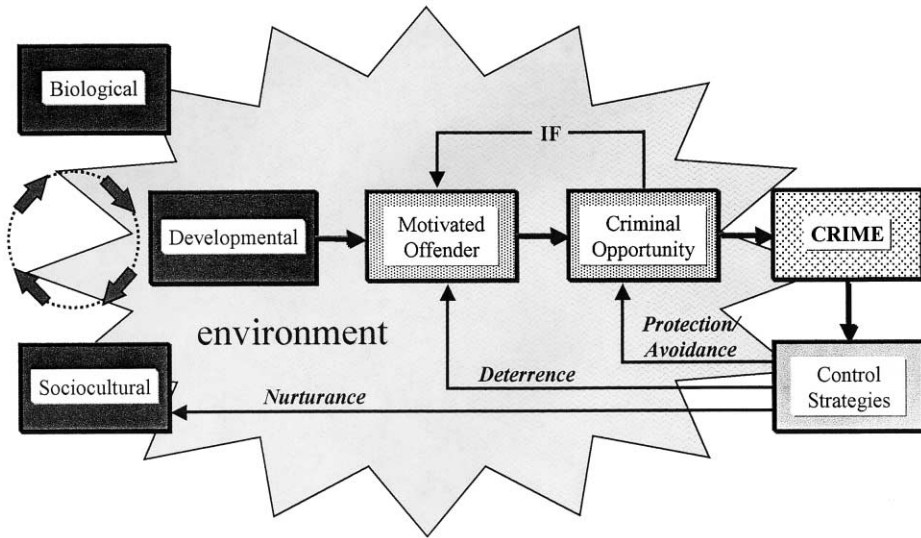


Fig. 1. The general paradigm for understanding criminal behavior and crime control.

In a transition from micro- to macrolevel, the paradigm then shifts focus to the responses of individuals and groups to the criminal behavior. These crime control responses can either: (a) attempt to diminish opportunities for crime by protecting people and property through such strategies as target hardening, environmental design, and changes in personal activity patterns or by incapacitating potential offenders; (b) modify proximate motivation for crime by changing the reward structure associated with an act; or (c) influence the probability that individuals will develop strategic styles that favor the use of force, fraud, or stealth by improving the developmental opportunities of young people. Respectively, the General Paradigm characterizes these responses as attempting to limit crime via protection/avoidance, deterrence, or nurturance.

From both a theoretical and policy standpoint, one of the General Paradigm's most important predictions is that "nurturant" factors which improve the ability of young people to develop human and social capital will tend to reduce crime rates over time by decreasing the pool of highly motivated offenders. Stated more directly, this "lagged nurturance hypothesis" predicts that "nurturant" factors at the aggregate level (maternal and infant health care, positive parenting, and early education, for example) will have an impact on crime rates when the children exposed to a given set of "nurturant" conditions reach their high-crime adolescent and young-adult years. Better-nurtured cohorts of children are expected to commit less crime. Two aspects of the lagged nurturance hypothesis are particularly important—the use of nurturance specifically for crime prevention, and the lagged nature of the expected effect on crime rates. From a policy standpoint, the hypothesis predicts that jurisdictions that do a better job of nurturing children (indicated by such things as elevated levels of maternal and infant health care, preschool enrollment, teen pregnancy prevention, parenting training, and lower levels of exposure to toxic metals) will have lower levels of crime 15–20 years

later—when the fetuses, infants, and children subject to those conditions, having developed more appropriate and desirable strategies for obtaining what they need, reach the age at which criminal behaviors become realistic strategies for them to use to get what they want.

Many social theories—and most empirical tests of theory—focus on *contemporary* relationships between crime and causal variables such as unemployment, inequality, or class conflict. Although the General Paradigm accounts for these proximate causal forces, it also emphasizes the importance of much longer-term dynamics. This has important policy implications. For example, our preoccupation with today's crime rates, crime fads, and sensational crimes tends to divert scarce public resources and attention toward criminal justice system components and away from programs affecting the health, education, and welfare of young people. If the hypothesized time-lagged effects exist between levels of child welfare and later crime rates, it is plausible that by diminishing resources available for nurturance, short-term imperatives for crime control actually may *increase* crime over the long-term (see Vila, 1997b, pp. 49–51 for a discussion of these dynamics).

One of the most straightforward and conservative ways to test the lagged nurturance hypothesis is to examine relationships between measures of change in child nurturance and appropriately lagged measures of change in crime rates across nations. This approach is methodologically daunting because of difficulties in compiling the broad range of crime and social data required for such a comparison at the relevant intervals, problems inherent in comparing international data, difficulties finding appropriate measures of child nurturance, and problems finding data for a large-enough and broad-enough sample to allow generalization from the results. If significant lagged associations exist between nurturance and crime at national levels despite these challenges, it would provide strong support for the lagged nurturance hypothesis and the policy proposals that flow logically from it.

We focus on *change* in nurturance and crime rather than absolute levels in the present study due to several methodological problems encountered in earlier analyses. The use of change scores is discussed in more detail later in the paper. We are aware of no other cross-national studies that have used this changed-based approach. Studies of lagged effects on crime have been limited to Archer and Gartner's (1984) analysis of the lagged effects of wars on postwar homicide rates and Gartner and Parker's (1990) analysis of age structure and homicide.

Below we briefly review previous cross-national research on crime and problems with earlier attempts to evaluate the lagged nurturance hypothesis. We then discuss the change-based methods employed here and report the results of a series of regression analyses, the stability of these results, and their implications for theory, future research, and policy.

1.2. Cross-national research

The bulk of studies evaluating theories of cross-national variation in crime rates examines the relationships between inequality and crime, especially homicide (Avison & Loring, 1986; Braithwaite & Braithwaite, 1980; Hansmann & Quigley, 1982; Krahn, Hartnagel, & Gartrell, 1986; Krohn, 1976; Messner, 1980, 1982, 1989; Unnithan & Whitt, 1992), economic development and crime (Bennett, 1991b; Bennett & Basiotis, 1991; Groves, McCleary, & Newman, 1985; Kick & LaFree, 1985; Krohn, 1978; Krohn & Wellford, 1977; LaFree &

Kick, 1986; Messner, 1980, 1982; Unnithan & Whitt, 1992; Wolf, 1971), or routine activities and crime (Bennett, 1991a; Kick & LaFree, 1985). The method typically employed in these studies consists of a cross-national analysis of contemporaneous crime and economic data. Crime rates are regressed on indicators of theoretical constructs and, in most cases, a series of covariates. Although these methods have been successful in discerning some consistencies in cross-national variation in crime (e.g., the positive association between income inequality and homicide rates and between level of economic development and rates of property crime), problems inherent in cross-national methods have prevented consistent findings from emerging when attempting to evaluate the lagged nurturance hypothesis.

The first such methodological problem is paucity of data. There are gaps in the data because international measures of crime, child nurturance, and covariates are collected by different agencies at inconsistent intervals, and an incomplete set of countries report on any one of them. Interpol data, for example, were reported by approximately 98 countries in 1980 and by approximately 136 countries in 1990. Of the 98 countries reporting any Interpol data in 1980, only 86 reported homicide data. Gross domestic product per capita, female labor force participation, and data relevant for the computation of the gini index of income inequality data—which are reported by the International Labour Office (1984, 1994), United Nations (1983/1993, 1991), and World Bank—were available for only 89, 56, and 49 countries, respectively. Similar attrition is also evident in the 1990 data set. As more gaps appear with the addition of each independent variable, sample size for multivariate comparisons quickly diminishes to unsatisfactory levels. Techniques such as mean substitution tend to aggravate error and statistical power problems.

A second problem is that collinearity between measures of child nurturance and measures of crime covariates has a destabilizing effect on parameter estimates. In many comparisons, the correlation between lagged measures of child nurturance and measures of other factors associated with crime is very high. For example, the correlation between infant mortality rates in 1970 and per capita energy consumption in 1990 is $-.7216$; the correlation between average life expectancy in 1970 and per capita gross domestic product in 1990 is $.69$, and the correlation between primary school pupil/teacher ratios in 1980 and television sets per 100 population in 1990 is $-.68$. Although such high correlations among independent variables in a multiple regression model do not constitute a strict violation of the assumption of the absence of perfect multicollinearity, “the standard errors for partial slope coefficient estimates for variables among the collinear set will be quite high, so that estimates of the effects of the independent variables will fluctuate considerably from sample to sample” (Berry, 1993, p. 27). Schroeder, Sjoquist, and Stephan (1986) suggest that it is very difficult to determine whether collinearity is a problem in a given comparison but “when two or more independent variables are correlated . . . statistical estimation techniques . . . are incapable of sorting out the independent effects of each on the dependent variable” (p. 72). In other words, multiple regression models that include indicators of child nurturance and highly correlated covariates are not very useful for estimating the exact contribution of child nurturance to the crime rate.

Partly as a result of these problems, a series of cross-national analyses conducted to test the lagged nurturance hypothesis using traditional multiple regression methods resulted in a series of intriguing, but inconsistent, findings. Savage and Vila (1995) found evidence for the

hypothesized negative relationship between child nurturance and a lagged, proxy measure of total crime termed “base crime.” Using a supplemented data set, Savage and Vila (1997) could not replicate that finding but, instead, found evidence for a negative relationship between child nurturance and *violent* crime. In a later analysis, Savage (1997) found fairly consistent evidence for a lagged negative relationship between child nurturance and *homicide*, but no relationship (in the multivariate comparisons) between other crime types and child nurturance. In short, although previous analyses using similar data to test relationships between child nurturance and lagged crime rates resulted in some findings consistent with the lagged nurturance hypothesis, none of the results was stable enough to establish the hypothesized relationship—or the lack thereof.

1.3. *Analyzing changes in crime instead of absolute crime rates*

We argue here that the types of multivariate regression analyses typically used to assess cross-national variation in crime are not conducive to an accurate assessment of the lagged nurturance hypothesis. We propose an alternative strategy—examining the impact of *change* in child nurturance on later *changes* in crime rates. Methodologically, this approach helps solve the problems mentioned above. First, it reduces error due to reporting and definitional incomparability. “Reporting” incomparability refers to inconsistency from country to country in the tendency to report crimes of equivalent type and severity. In cross-national analyses, this source of error is either random in nature (reducing statistical power) or systemic (covarying, perhaps, with level of economic development). In the present analysis, the use of change scores is expected to reduce random error due to reporting as it is expected that within-country variation over a 10-year period is much lower than between-country variability.

Similarly, “definitional” incomparability refers to the problem, in cross-national analyses of crime, whereby countries have different definitions of the crimes in question. For example, some countries define “homicide” to include “attempted homicide,” while others do not. That problem has been corrected directly here with an attempt-adjusted homicide rate. But definitions of other crimes, such as the group included in the “violent crime” measure—robbery, serious assault, rape, and homicide—can be expected to vary from country to country. Once again, in the present analysis, we are not looking at absolute levels of crime but at changes in reported crime levels over time. Again, it is expected that within-country changes in crime definition over the 10-year period will be less significant than between-country differences at any one point in time.

The reduction in error due to differences in reporting and definitional differences that we gain by using change scores instead of absolute levels cannot be estimated precisely. However, if we look at the overall sum of squared error for the dependent variables, we have some indications that random variance is reduced when we use change scores. For example, the sum of squared error for the common cases of homicide rates in 1990 and the change in homicide rates from 1980 to 1990 were 19,932 and 14,997, respectively, a decrease of approximately 25%. The sum of squared error for theft rates in 1990 was 394,727,815 compared to that of the change in theft rates from 1980 to 1990, 69,283,878 (a change of approximately 83%).

Second, the use of change scores reduces collinearity among variables included in the models to well within acceptable levels. For example, when change scores are used, the high correlations between absolute levels of energy consumption and infant mortality rates ($-.72$), per capita gross domestic product and average life expectancies ($.69$), and television sets per capita and pupil/teacher ratios ($-.68$) are reduced to $r=.25$, $r=-.42$, and $r=.07$, respectively.

2. Methods

Simple regression models using measures of change in levels of child nurturance at the national level (e.g., change in infant mortality rates) are used to estimate changes in crime rates from 1980 to 1990. As is customary in this type of research, the sample included all countries for which data were available for any given model. Eleven indicators of child nurturance are used, and the timing of each measure is based on its logical location in the lifespan of persons aged 20 in 1980 and 1990. For example, infant mortality rates from 1960 and 1970 are used as they correspond to the years that 20-year-olds in 1980 and 1990 were born. Four crime types are examined separately (homicide, violent crime, “base crime,” and theft); attempt-adjusted homicide rates are also analyzed. In order to eliminate “rival hypotheses,” a series of multiple regressions is also reported, which controls for constructs that may be related to both the dependent and independent variables.

2.1. Data

An attempt was made to create a dataset that was as comprehensive as possible. In addition to collecting Interpol data for all reporting countries for 1978–1982 and 1988–1992, an effort was made to collect data on *every* independent variable that has been used in previous quantitative studies of cross-national variation in crime along with a large number of indicators of our construct of interest for every nation for which data were available. The resulting dataset has several hundred variables, over 200 countries overall, and includes an extensive set of indicators of economic development, routine activities, population demographics, inequality, population heterogeneity, and crime (Interpol, 1978–1982, 1988–1992). These data are useful for testing a variety of hypotheses at the cross-national level over two different time periods using more recent and comprehensive data than previous studies. More detail about this dataset is available (see Savage, 1997).

2.2. Dependent variables

Crime data were drawn from Interpol crime reports for 1980 and 1990. When 1980 or 1990 data were not available, data submitted within 2 years (1978–1982 and 1988–1992 for the 1980 and 1990 comparisons, respectively) were substituted. In response to calls for a distinction between violent crime and theft in theoretical and empirical analyses (Bennett, 1991a; Kick & LaFree, 1985), data for murder, rape, robbery, serious assault, and theft were used to compile a set of dependent variables (theft, “base crime,” violent crime, homicide)

that represent a continuum from property to violent crime. Our measure of “violent crime” consists of homicide, rape, robbery, and serious assault; “base crime” consists of homicide, rape, serious assault, and theft (which includes robbery under the Interpol scheme). All dependent variables reflect the change in crime rate per 100,000 population from 1980 to 1990 and were computed simply by subtracting the 1980 rate from the 1990 rate. Each Interpol report entered in the dataset was subjected to a series of data checks to detect and correct obvious typographical and computational errors. Homicide rate data for 1980 were supplemented with rates reported to the United Nations.¹

Wilson (1997, p. 38) and others (see, e.g., Bennett & Shelley, 1985; Hansmann & Quigley, 1982; Krohn, 1976, 1978; Messner, 1982; Neapolitan, 1998) have noted criticisms of Interpol crime data, the major ones being incomparability of legal definitions and potential differences in tendencies to report crime. Bennett and Lynch (1990) suggest that like other international data sources, Interpol data are probably safe for analytic purposes despite known differences in legal definitions and reporting. Since legal definitions are unlikely to be systematically linked to our independent variable of interest—change in child nurturance—definitional differences are not a source of serious concern for these change-based analyses. Further, random variation would lead to increased random error in the dependent variable, increasing the likelihood of a type II error—thus creating a more conservative test of our hypotheses. Although it could be speculated that reporting is associated with levels of economic development (which in turn *is* associated with child nurturance), several studies suggest that levels of crime reporting in less developed nations are similar to those in developed nations (Bayley, 1969; Bennett & Wiegand, 1994; Ennis, 1967; Tanner, 1970).

From our perspective, two less discussed issues are more troubling: (1) possible bias in crime rates based on the ability of national criminal justice systems to record crimes and maintain records; and (2) the inclusion by countries of attempted homicides in the homicide data. Our use of change scores seems likely to reduce the seriousness of the first source of bias substantially when compared with traditional studies using absolute crime levels. However, it is conceivable that countries with improved child nurturance in the 1960s and 1970s also experienced increases in police and crime recording capacities during this time or the subsequent decade. If this is the case, we would expect two types of bias: (1) the expected negative effect of nurturance on crime will be missed because the increasing ability to record crime results in artificially increased crime rates; or (2) the expected negative effect will be overestimated because increased criminal justice capacity correlated with child nurturance resulted in deterrence, which is being mistaken for a child nurturance effect. It is not currently known if child nurturance is correlated with criminal justice capacity or if criminal justice capacity is a significant contributor to error in crime rate measures because there is no good measure of “crime recording capacity” available at the national level. Lacking evidence to the contrary, we assume that measures of general levels of change in economic development will also reflect changes in criminal justice resources and will at least partially control for this

¹ The correlation between Interpol and United Nations homicide data for 1980 was .95 for the 43 common cases.

problem. Further, by using a continuum of measures from violent crime to theft crime—homicide, violent crime, base crime, theft—that are expected to be increasingly linked to reporting tendencies and recording capacities, we hope to detect this bias if it does exist.

The second critical problem is that more than half of the countries reporting homicide rates to Interpol include some “attempted” homicides in their homicide data. Interpol provides a column where each country can report “percent attempts.” The average percentage of homicides reported, which were merely “attempted homicides” (among countries reporting attempts), was approximately 38% and ranged from 2% to 90%. Although probably not systematic, the inclusion of attempted homicides constitutes a large source of error and can be expected to weaken statistical power. In an effort to improve the measure, we calculated an attempt-adjusted homicide rate by subtracting the proportion of homicides noted as “attempts” as was done by Neapolitan (1998) and Savage (1997). However, the possibility still remains that some countries included “attempts” in their homicide reports but did not report that percentage. We therefore use both homicide and attempt-adjusted homicide rates in our analyses.

2.3. *Independent variables*

The lagged nurturance hypothesis predicts that nurturant factors, such as maternal and infant health care, parenting skills, and early education, which have been shown to affect the probability that *individuals* will become involved in high-rate offending later in life (e.g., Farrington, 1994; Franklin, 1786/1987; Gottfredson & Hirschi, 1990; Loeber & Farrington, 1998; Masters, Hone, & Doshi, 1999; McCord & Tremblay, 1992; Moffitt, 1993; Patterson, DeBaryshe, & Ramsey, 1989; Sampson & Laub, 1993; Wilson & Herrnstein, 1985), will also be negatively related to crime at the *aggregate* level when the cohorts of children they affect reach their high-crime adolescent and young-adult years. Independent measures were chosen to tap into constructs corresponding to early-life factors associated with the development of criminality (biological, sociocultural, and developmental factors) based on factors recommended by Vila (1994, 1997a, 1997b). Measures of change in “birth prospects” include change in average life expectancy at birth, birth rate, and infant mortality rate. These measures are expected to reflect nurturant conditions by measuring health of the birth cohort early in life (life expectancy, infant mortality) and competition for resources (birth rate). (Fig. 2 shows the timing of these and subsequent measures.) These indicators of health are intended to reflect “nurturance” at the societal level. They are expected to be associated with a variety of health-related biological factors associated with criminality, such as head injury, exposure to toxins, and ability to treat other sources of brain injury. Similarly, birth rate is expected to reflect per child resources for health, education, and parental attention. In countries with higher birth rates (births per 1000 population), such resources must be divided among a greater number of children, thus reflecting less “nurturance” for any individual child. Birth prospects indicators were collected from the United Nations’ *Demographic yearbook*, UNICEF’s (1987/1993/1994) *The state of the world’s children*, and the World Bank’s *World development reports*.

Four measures of change in education were used: primary school enrollment, primary school pupil–teacher ratios, percentage of the age cohort persisting in school to fourth grade,

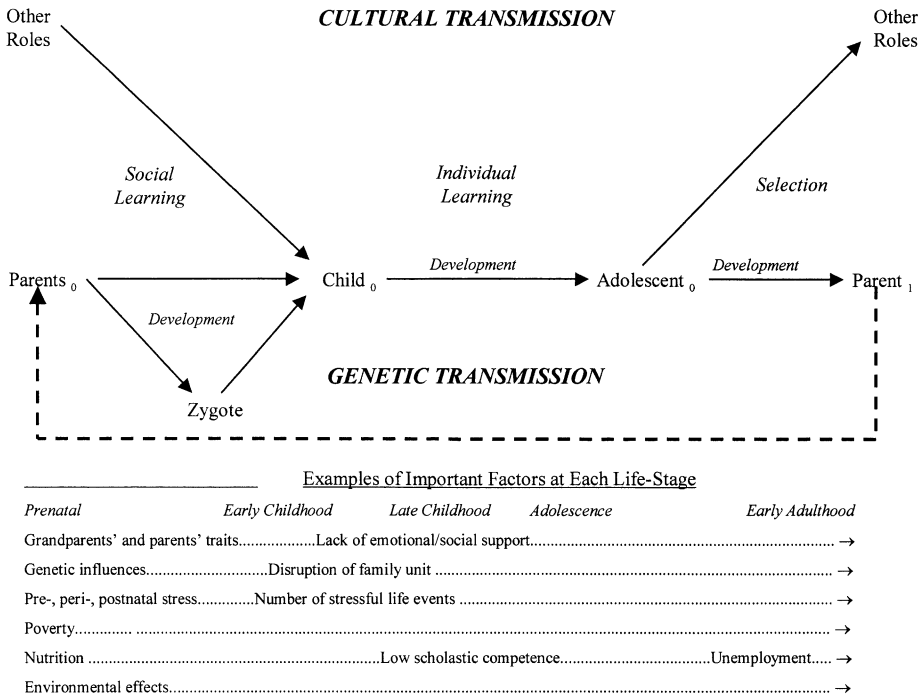


Fig. 2. Acquisition of behavior-influencing traits over the human life cycle (Vila, 1994, p. 332).

and secondary school enrollment. Increased participation of children in education is expected to be associated with improved “nurturance” because of the obvious beneficial impact of education on child development and also because children who are attending school are probably less likely to be working at adult jobs and thus are less likely to be exposed to unhealthy, hazardous, or exploitative conditions. Pupil–teacher ratios reflect the quality of education and level of attention children are receiving. Education indicators were collected from the World Bank’s *World development reports*, UNESCO’s (various years) *Statistical yearbooks*, and the World Bank’s (1995) *Social indicators of development*.

General measures of change in health include changes in caloric intake as a percentage of daily requirements, population per physician, population per nurse, and population per hospital bed. While caloric intake reflects nutrition and standard of living, population per physician, population per nurse, and population per hospital bed are more closely associated with availability of medical care and, perhaps, general health conditions. Although these measures are related to the population as a whole, measures of general nutrition and access to health care are especially important to children who, if not treated for health problems, may have impaired behavioral development. Health indicators were collected from the World Bank’s *World development reports* and *Social indicators of development*.

Age 20 was chosen as the approximate modal age at which offenders’ officially reported crimes would peak (see Gottfredson & Hirschi, 1990, p. 125; Hirschi & Gottfredson, 1983; Moffitt, 1993; Nagin & Land, 1993). Thus, measures of “birth prospects” were measured

when the 20-year-old cohorts of 1980 and 1990 were born—in 1960 and 1970, respectively—and educational measures such as primary school enrollments and primary school pupil–teacher ratios were measured when those cohorts were primary school-aged (1965–1970 and 1975–1980). Similarly, as Fig. 2 illustrates, measures for other indicators of child nurturance also were targeted to appropriate points in each cohort's life course. Note that two variables had irregular timing because of difficulty obtaining data. First, the measure of the percentage of the cohort persisting in school to Grade 4 was from 1975 and 1985 when the cohorts were 15 years old. Population per hospital bed data also were difficult to acquire and we settled for 1975 and 1985 data although we would have preferred that they be timed 5 years earlier.

To facilitate interpretation, all independent measures were coded to reflect “improvements” in the measure given. So, for example, an improvement in primary school education from 1980 to 1990 was computed by subtracting the 1980 enrollment from the 1990 enrollment since a higher value is “better.” Likewise, computing an “improvement” in infant mortality rate was done by subtracting the 1990 rate from the 1980 rate since, in this case, a lower value is “better” (the larger the decline from 1980 to 1990, the greater the “improvement”).

2.4. Covariates

Covariates have two important functions in multiple regression, besides providing a more complete model of the phenomenon under study. First, they reduce bias in parameter estimates stemming from confounding factors. By including a covariate measurement of a confounding factor in a multiple regression model, one can distinguish between the impact of the factor of interest on the dependent variable and the impact of the confound. Second, covariates increase statistical power for partial parameter estimates. Because the partial *t* statistic for an individual parameter is based on the percentage of the remaining variance that is explained when the parameter is added to the model (after taking into account the covariates), reducing the “remaining variance” increases the chances of a statistically significant result for subsequent parameters added to the model. Covariates chosen for use in the present analyses were those, based on previous cross-national research and associated theory, which measured constructs thought to represent “rival hypotheses” or confounding factors for the relationship between change in nurturance and change in crime rates. For example, improvements in child nurturance may be associated with increases in economic development and therefore, economic development might provide an alternate explanation for an association between nurturance and changes in crime levels. Other potential confounding factors suggested by previous cross-national research on crime are routine activities (improvements in nurturance may be associated with changes in routine activities associated with changes in crime levels), inequality (measures of nurturance may be related to changes in economic inequality which is thought to be related to homicide rates), and demographics (changes in nurturance may be related to demographics, such as the proportion of the population that is aged 15–24, which may affect crime rates). Below we briefly discuss each of these constructs and their measures.

2.5. Economic development

The work of Emile Durkheim has informed much of the cross-national literature (Durkheim, 1933) and motivated researchers to examine the impact of economic development and industrialization on crime rates. Because indicators of child nurturance are associated with levels of economic development, and levels of economic development in turn are associated with crime rates, it is most important to control for change in economic development in the present analyses. The measure of change in level of “economic development” used was change in average per capita annual energy consumption. This measure was obtained from the World Bank’s *World development reports*.

2.6. Routine activities

Changes in child nurturance may be associated with change in the sorts of routine activities associated with changes in crime. For example, countries that experience improvements in childhood conditions are probably the same countries whose economies are becoming stronger, bringing about increases in material goods that constitute potential targets for theft. These improvements also tend to be associated with declines in guardianship related to employment outside the home and decreased household size (Cohen & Felson, 1979; Kick & LaFree, 1985). We used two indicators believed to be related to routine activities constructs: change in average household size, and change in automobiles per 1000 population. As used by Kick and LaFree (1985), household size is expected to be related to homicide via increased contacts among intimates. We also use it for violent crime, acknowledging that a better indicator of “routine activities” associated with violent crime would be desirable. The per capita number of automobiles is expected to reflect “suitable targets” for theft crime. Measures of routine activities were obtained from the *UN compendium of political and social indicators* and the World almanac and book of facts (1996).

2.7. Inequality

A substantial proportion of the cross-national literature on crime is devoted to the effect of inequality on homicide rates (based largely on *Inequality and Heterogeneity* of Blau, 1977). Although it is clear that there probably is a significant positive association between inequality and homicide, thus far it is unclear whether inequality has a significant impact on other types of violent crime or on property crime rates. Because of the expected positive association between child nurturance and national wealth, changes in nurturance may be associated with changes in inequality. Change in the gini coefficient of income inequality 1980–1990 was used to control for this construct. Rather than use published sources of the gini coefficient, which are based on older data, we calculated inequality with the following equation (Kakwani, 1980, p. 69) using data published in the World Bank’s *World development reports* (World Bank, 1977–1996). Fig. 3 displays the equation used for the gini index of inequality.

In order to enhance sample size, these measures were supplemented using estimates based on gini values provided by He and Wells (1996) and others published in the *World development*

$$G = \frac{\Delta}{2\mu}$$

$$\Delta = \left[\frac{1}{n(n-1)} \right] \sum_{i=1}^n \sum_{j=1}^n |x_i - x_j|$$

Fig. 3. Formula used for the gini index of income inequality.

report (World Bank, 1993).² Additionally, estimates of the gini index based on simple regression slope estimates were used to fill in missing data when gini values were available for either 1980 or 1990 but not both. Although these procedures add some unreliability to the data and increase overall error, for the purposes of controlling for inequality, this is probably acceptable practice. Allowing estimates of gini increased our sample of potential cases from 54 to 98. The tradeoff between using only computed gini values and estimates is between increased reliability for a small number of cases or dramatically increasing sample size. For our purposes, we believe the dramatic increase in sample size more than offset the decline in reliability.

2.7.1. Demographics

Measures of the age structure and size of the population are often used as covariates in cross-national analyses of crime. We used change in percentage of the population aged 15–24, change in absolute population size, and the percentage change in population for 1980–1990 to control for demographic fluctuations.

2.8. Analysis

If the lagged nurturance hypothesis is correct, measured improvements in child nurturance should have significant negative associations with subsequent crime rate trends. First, each dependent variable representing change in crime rates in 1980–1990 was regressed on each measure of change in child nurturance. Although simple regression is normally misleading in analyses of crime rate levels, it is thought to be informative here because of greatly reduced collinearity with potentially spurious factors compared to using absolute crime levels.

Second, a series of multiple regression models, designed to eliminate the most likely “rival hypotheses,” was run to test the robustness of the significant simple regression findings. Because of serious problems with missing data in the multiple regression models, missing data were estimated by using regression slope-based estimates when values were available for at least one time period. Estimates were not made for the dependent variables. In spite of an extensive effort to estimate missing values for all of the independent variables used in the analysis, the sample sizes were not greatly increased. For example, using estimates increased

² It should be noted that due to poor sample size, a wide range of years was permitted. This should not overly compromise the measure since the correlation between gini scores over a 10-year period is high ($r=.82$).

the sample size for the change in homicide rate analysis of infant mortality rates by approximately six cases. In no case were more than seven countries made available for analysis due to the use of estimates.

Because our goal was theory testing rather than descriptive analysis, outliers were dealt with aggressively and consistently in order to identify emergent patterns affecting the bulk of countries for which data were available and avoid reporting results heavily influenced by a few unusual cases (see Appendix A).

3. Results

3.1. Simple regression analyses

The body of findings reported in Table 1 suggests that several measures of change in child nurturance are associated with subsequent time-lagged changes in crime rates. Using the strictest criterion, the Bonferoni-corrected significance test (where $p=.0045$ when 11 tests are done on the same data) (see Chatfield, 1988, pp. 52–53), improvements in primary school enrollments from 1965 to 1975 and in availability of hospital beds were associated with lower increases in homicide rates from 1980 to 1990.³ (Improvements in primary school enrollments were also significantly, negatively associated with trends in attempt-adjusted homicide.) Less strict criteria ($p<.05$ or $p<.01$) suggest that improvements in infant mortality rates and caloric intake may also have significant negative associations with subsequent homicide rate trends.

Findings also suggest that changes in violent crime rates are significantly negatively associated with improvements in infant mortality rates and average life expectancies. Using the less strict criteria, improvements in primary school enrollments and availability of hospital beds may also have a beneficial impact on violent crime rates.

Changes in base crime rates appear to be significantly negatively associated with improvements in average life expectancies ($p<.0045$) and perhaps associated with improvements in infant mortality rates ($p<.01$).

Finally, there was a significant negative relationship between changes in theft rates and improvements in average life expectancies ($p<.0045$); improvements in infant mortality rates were also significantly negatively related to changes in theft rates at the lower criterion ($p<.01$).

There were also a large variety of relationships which were marginally negatively related, though not statistically significant in nature; 39 of 55 relationships were in the hypothesized *negative* direction (when, by chance, we might expect 27 or 28 to be so), including all the relationships among infant mortality rates, average life expectancies, birth rates, primary

³ Note that a negative coefficient does not indicate “declines” in crime necessarily—only *lesser increases*. A proper interpretation of a negative coefficient, then, might read, “As improvements in child nurturance increase, increases in crime rates diminish.”

Table 1

Correlations and unstandardized simple regression coefficients—regressing cross-national change in crime rates 1980–1990 on lagged measures of child nurturance^a

Improvement in	Change in homicide rate	Change in attempt-adjusted homicide rate	Change in violent crime rate	Change in base crime rate	Change in theft rate
Infant mortality rates, 1960–1970	-.0537 (53) <i>b</i> = -0.010	-.182 (51) <i>b</i> = -0.025	-.498 [†] (37) <i>b</i> = -2.831	-.372** (43) <i>b</i> = -23.203	-.357** (51) <i>b</i> = -20.427
Average life expectancies, 1960–1970	-.155 (59) <i>b</i> = -0.143	-.104 (58) <i>b</i> = -0.086	-.497 [†] (39) <i>b</i> = -11.724	-.503 [†] (47) <i>b</i> = -135.969	-.492 [†] (56) <i>b</i> = -139.026
Birth rates, 1960–1970	-.003 (56) <i>b</i> = -0.002	.082 (55) <i>b</i> = 0.051	-.047 (36) <i>b</i> = -0.999	-.204 (42) <i>b</i> = -47.713	-.190 (53) <i>b</i> = -42.377
Primary school enrollment, 1965–1975	-.457 [†] (58) <i>b</i> = -0.094	-.391 [†] (57) <i>b</i> = -0.0757	-.429** (38) <i>b</i> = -2.145	-.231 (45) <i>b</i> = -13.527	-.194 (56) <i>b</i> = -10.802
Secondary school enrollment, 1978–1987	.092 (54) <i>b</i> = 0.027	.118 (54) <i>b</i> = 0.031	.393* (37) <i>b</i> = 2.633	.062 (43) <i>b</i> = 5.235	.111 (50) <i>b</i> = 10.115
Pupil–teacher ratios, 1970–1980	-.070 (51) <i>b</i> = -0.026	.020 (51) <i>b</i> = -0.007	.027 (36) <i>b</i> = 0.430	.124 (42) <i>b</i> = 16.745	.110 (51) <i>b</i> = 14.558
Percentage of children persisting until Grade 4	.348** (53) <i>b</i> = 0.110	.357 (52) <i>b</i> = 0.105	.161 (37) <i>b</i> = 1.192	.056 (43) <i>b</i> = 5.278	.047 (53) <i>b</i> = 4.322
Population per physician	-.011 (58) <i>b</i> = -0.000	.002 (57) <i>b</i> = 0.000	-.199 (40) <i>b</i> = -0.003	-.183 (45) <i>b</i> = -0.036	-.188 (55) <i>b</i> = -0.023
Population per nurse	-.204 (45) <i>b</i> = -0.001	-.215 (44) <i>b</i> = -0.001	-.120 (34) <i>b</i> = -0.013	-.047 (38) <i>b</i> = -0.047	.022 (44) <i>b</i> = 0.016
Caloric intake as percentage of daily requirement	-.120 (61) <i>b</i> = -0.036	.202 (59) <i>b</i> = -0.060	-.082 (39) <i>b</i> = -0.529	-.112 (46) <i>b</i> = -8.745	-.053 (55) <i>b</i> = -3.776
People per hospital bed	-.449 [†] (48) <i>b</i> = -0.016	.043 (51) <i>b</i> = -0.001	-.379* (36) <i>b</i> = -0.239	.056 (42) <i>b</i> = 0.349	-.036 (49) <i>b</i> = -0.126

^a Computed after eliminating outliers.

* *p* ≤ .05.

** *p* ≤ .01.

[†] *p* ≤ .0045, Bonferroni-corrected criterion of significance for 11 tests on same data.

school enrollments, population per physician, and caloric intake and the five crime rate types suggesting that the body of findings overall support a negative relationship between improvements in measures of “child nurturance” and subsequent crime rate trends of various types. This included 8 of 11 comparisons for homicide, 7 of 11 for violent crime, 6 of 11 for base crime, and 8 of 11 for theft, indicating that the effect is not limited to one crime type. This is consistent with the general paradigm (and other theories) that do not emphasize a distinction between root causes of expropriative versus violent behavior. This pattern is also consistent with the substantial body of research on offender versatility, which indicates that most offenders tend not to specialize (e.g., Blumstein, Cohen, Das, & Moitra, 1988; Bursik,

1980; Farrington, Snyder, & Finnegan, 1988; Hindelang, 1971; Hindelang, Hirschi, & Weis, 1981; Hirschi, 1969; Klein, 1984; Petersilia, 1980; Rojek & Erikson, 1982; Wolfgang, Figlio, & Sellin, 1972).

A few measures used as indicators of child nurturance were definitely not negatively related to later changes in crime. Secondary school enrollments, pupil–teacher ratios, and percentage of children persisting until Grade 4 showed positive, mostly nonsignificant associations with changes in crime rates. These findings were not completely unexpected, since secondary school enrollments may be too late in the life course to influence “criminality” as described by Vila (1994), and the Grade 4 data were taken later than desired, making them somewhat inaccurate for the sample. However, it was expected that pupil–teacher ratios would be a significant indicator as a measure of quality of education and amount of attention given to children, and the failure of that variable to predict changes in crime was surprising and contradicted the lagged nurturance hypothesis.

Logic dictates that violent crime would tend to be more predictable than property crime in cross-national research because murder, rape, robbery, and serious assault are less prone to definitional differences. Thus, measures of theft and base crime are expected to have more random error because of definitional, reporting, and recording differences. Although it is measured more reliably, homicide is a low base rate phenomenon that tends to be more sensitive to random fluctuation. Earlier, we suggested that using a continuum of measures from theft to homicide might help reveal whether recording error is important. The fact that we found less consistent and less dramatic findings for theft than for violent crime suggests that theft may indeed have more error variance. But the fact that theft and base crime findings contained a number of consistencies suggests that cross-national theft data may not be as overrun with error as many have feared—or at least that change scores are much less error-prone for cross-national analyses of theft-related crimes.

3.2. Eliminating rival hypotheses

Despite the support for the lagged nurturance hypothesis reported above, the possibility remains that changes in crime actually were due to changes in economic development, criminal opportunity, income inequality, or demographic factors, the impact of which may have been represented by the measures of child nurturance. For example, the findings that improvements in infant mortality rates were negatively related to subsequent homicide, violent crime, base crime, and theft crime trends could be linked to increases in level of economic development, for example, or changes in level of inequality or routine activities. In order to eliminate such rival hypotheses, we ran a series of multiple regression analyses, replicating those comparisons that were significant at the $\alpha=.01$ level, controlling for four constructs that are very commonly controlled in the cross-national literature: economic development (previously found to be associated with levels of theft), economic inequality (previously found to be associated with homicide), two measures of routine activities—household size (associated with homicide) and car ownership (introduced here as a measure of suitable targets for theft), and finally, percentage of the population aged 15–24, which is often controlled in such analyses in spite of the fact it is not likely to covary with other factors. Here, it is

conceptually logical that changes in “nurturance” would be associated with change in cohort size, which would later be reflected in the youth population (Easterlin, 1987).

Table 2 presents the multiple regression findings. Overall, associations between improvements in infant mortality rates and average life expectancies and changes in subsequent crime rates were quite robust, even controlling for the series of covariates and in spite of dramatically decreased statistical power due to greatly reduced sample sizes (the exception was the relationship between changes in homicide rates and infant mortality rates). Note that when a consistent set of cases is analyzed, the standardized β coefficients do not vary a great deal when changes in crime rates are regressed onto changes in infant mortality rates or average life expectancies with and without control factors in the model.

The multiple regression findings for primary school enrollments and hospital bed availability were not robust, although this failure to support the hypothesis may be due, in part, to the change in cases (there were 14 fewer cases in the analysis of homicide and primary school enrollments, for example).

We do not think it was a mere coincidence that the relationships between improvements in infant mortality rates and average life expectancies and crime trends were more robust than those among primary school enrollments, hospital bed availability, and crime. The measurement of infant mortality rates and average life expectancies do not have the known problems associated with the other two measures (primary school enrollments are measured by using an age group in the numerator that does not always match that used in the denominator for all countries, resulting in some inflated and some deflated estimates; the bed availability measure was taken in 1975 and 1985, which was, perhaps, not reflective of “early health care” or “nurturance” as we would like it to be).

3.3. *The “room for improvement” problem*

Early reviewers of this paper suggested that there might be a problem with “room for improvement.” For example, the level of the independent variable at time t_1 , such as primary school enrollment in 1965, may be important when assessing the impact of change since some countries already had very high primary school enrollments and had little “room for improvement.” Some countries could not improve a great deal on measures such as average life expectancy and infant mortality rates because their rates were already so good in 1960.

The room-for-improvement problem, in one light, is a problem of measurement. For example, average life expectancy is supposed to measure nurturance by indicating levels of health care, and changes in life expectancies should indicate improvements in health care. However, for countries whose life expectancies are high, changes in life expectancy may no longer be very precise in measuring improvements in health care.

This issue is also related to the use of change scores in the analysis, also mentioned as a problem by reviewers. Change scores are sometimes not indicative of the same phenomena the original indicators reflect. For example, in the present case it, could be argued that the meaning of zero change in average life expectancy is different for countries that had high average life expectancies in 1960 than it is for countries with low average life expectancies; in the former case, zero is not bad. However, because the distribution of change in infant

Table 2
Eliminating rival hypotheses: results of multiple regression analyses

Change in	Change in homicide rate		Change in violent crime rate		Change in base crime rate		Change in theft rate	
Infant mortality rates	-.072 (.655)	-.033 (.860)	-.463 (.0087) [†]	-.491 (.031) [†]	-.364 (.029)*	-.468 (.034)*	-.351 (.018) [†]	-.396 (.029)*
Energy consumption		-.034 (.861)		-.201 (.325)		.0714 (.719)		-.051 (.770)
Economic inequality		-.016 (.934)		.208 (.270)		-.007 (.962)		-.063 (.651)
Household size		.115 (.555)		-.224 (.253)				
Car ownership						.101 (.615)		.033 (.842)
Percentage of the population aged 15–24		.168 (.338)		.199 (.264)		.389 (.036)*		.384 (.014)
<i>R</i>	.072	.201	.463	.577	.364	.559	.351	.529
<i>F</i>	0.203	0.294	7.927	2.498	5.204	2.723	6.035	3.026
<i>p</i> of <i>F</i>	.655	.913	.0087	.058	.029	.038	.018	.021
<i>n</i>	41	41	31	31	36	36	45	45

Change in	Change in violent crime rate		Change in base crime rate		Change in theft rate	
Average life expectancies	-.561 (.002) [†]	-.564 (.019) [†]	-.504 (.002) [†]	-.534 (.011) [†]	-.494 (.001) [†]	-.551 (.003) [†]
Energy consumption		-.084 (.708)		.179 (.373)		.093 (.600)
Economic inequality		.154 (.442)		.034 (.822)		-.107 (.896)
Household size		-.112 (.570)				
Car ownership				.164 (.368)		.035 (.818)
Percentage of the population 15–24		.062 (.735)		.279 (.081)		.330 (.020)*

<i>R</i>	.561	.587	.504		.494	.611
<i>F</i>	12.376	2.412	11.249	3.817	13.552	4.537
<i>p</i> of <i>F</i>	.002	.0674	.002	.009	.001	.002
<i>n</i>	29	29	35	35	44	44

Change in	Change in attempt-adjusted homicide rates				Change in violent crime rates		Change in	Change in homicide rate	
	Change in homicide rates		Change in homicide rates		Change in violent crime rates			Change in homicide rate	
Primary school enrollments	-.138 (.372)	-.139 (.383)	-.119 (.461)	-.178 (.328)	-.186 (.325)	-.091 (.668)	Hospital bed availability	-.094 (.539)	-.086 (.626)
Energy consumption		-.121 (.451)		-.110 (.526)		-.339 (.115)	Energy consumption		-.057 (.723)
Economic inequality		-.218 (.151)		-.181 (.295)		.120 (.572)	Economic inequality		-.1897 (.264)
Household size		-.391 (.013)		-.266 (.125)		-.069 (.741)	Household size		-.2698 (.103)
Percentage of the population aged 15–24		-.073 (.628)		-.215 (.198)		.125 (.526)	Percentage of the population aged 15–24		-.019 (.904)
<i>R</i>	.138	.442	.119	.347	.186	.400	<i>R</i>	.094	.322
<i>F</i>	0.815	1.842	0.555	0.958	1.003	0.911	<i>F</i>	0.384	0.899
<i>p</i> of <i>F</i>	.372	.128	.461	.457	.325	.490	<i>p</i> of <i>F</i>	.539	.491
<i>n</i>	44	44	41	41	30	30	<i>n</i>	45	45

Standardized beta values and their associated *p* values.

* *p* ≤ .05.

† *p* ≤ .0045, Bonferoni-corrected criterion of significance for 11 tests on same data.

mortality rates and average life expectancies was relatively normal, we did not anticipate such problems when we originally analyzed the data.

The most likely scenario that we expected under the “room for improvement” scenario is that a certain number of cases would vary on the dependent variable but not on the independent variable, adding error variance to the analysis but no explanatory power. Thus, we would expect an increased chance of obtaining a Type II error, missing an effect that exists. In the present analysis, the expectation is that developed countries would have little “room for improvement” and would tend to group together with values near zero on the independent variable. There was no reason to expect that there would be any systematic change in crime rates among these countries. If an effect was discovered under such circumstances, we would expect that it may be due entirely to the less developed countries and an explanation would have to be made.

The easiest way to see if there is a “room for improvement” problem is to look at the distribution of the variables in question and the scatterplots to see if there are ceiling or floor effects. This was done for all the “robust” findings—namely, the relationships between infant mortality rates and violent, base and theft crime, and life expectancies and violent, base and theft crime. The results proved to be rather interesting and merit a detailed discussion.

While the scattergram for the relationships between improvements and nurturance variables was fairly linear in nature, Fig. 4 presents a rather stark example of a recurring theme among the scattergrams for base crime and theft crime and infant mortality rates and average life expectancies. The first thing we wish to point out is that the distribution of “change in infant mortality rates” is somewhat skewed, with the largest group of countries with values between 0 and 20. Secondly, and unexpectedly, a subset of the countries which had less “room for improvement” was also those which had the greatest crime increases from 1980 to 1990. In fact, none of the countries that experienced large improvements in infant

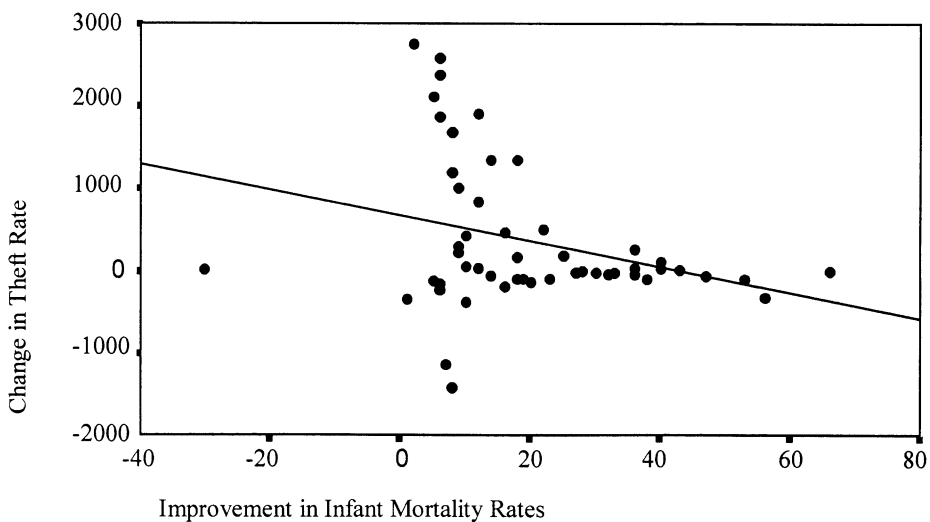


Fig. 4. Relationship between improvement in infant mortality rates and change in theft rate.

mortality rates also experienced marked increases in theft crime rates. This interpretation of the graph supports that of the lagged nurturance hypothesis. It also suggests that the ability of some nations to improve infant mortality rates may have been approaching an asymptote. But this nonlinearity was unexpected and other measures of nurturance, where the distribution is more normal, would be useful in a further evaluation of the lagged nurturance hypothesis.

If we dichotomize the countries and analyze two groups separately—those with improvements in infant mortality rates less than or equal to 20, and those with improvements in infant mortality rates greater than 20—we find that that both groups still have negative regression slopes, neither of which is statistically significant (though this may not be an appropriate test since the sample sizes are now quite small). This suggests that the effect in question does exist, but that the slope has been exaggerated by a truly dichotomous comparison. This begs the question: What is it about the countries that experienced huge increases in theft crime that is different?

Table 3 provides a comparison of countries that experienced increases in theft crime in 1980–1990 to those which did not. As we can see, among the countries that experienced the large increases in theft crime in 1980–1990, the majority are highly developed European and Northern European countries. Meanwhile, with a few exceptions such as Canada and the US, the group of countries which did not experience major increases in theft crime during this period represents many continents all over the world, and is less economically developed than those in the former list. This obvious disparity between the groups immediately suggests a potential confound—level of, or change in, economic development. However, when change in economic development was accounted for in the prior multiple regression analyses, it did not change the coefficient for improvements in infant mortality rates.

A follow-up analysis was conducted to subject the robust findings from the multiple regression analyses to further testing. Changes in base crime, theft crime, and violent crime were regressed onto changes in infant mortality rates and average life expectancies, controlling for the value of the independent variable at time t_1 (infant mortality rates 1960; average life expectancies 1960) and the average annual energy consumption in 1990 (an indicator of level of economic development).

The relationship between improvements in average life expectancies and subsequent crime rate trends was largely robust, although the relationship with violent crime was weakened by controlling for life expectancies in 1960 and energy consumption. The relationship between improvements in infant mortality rates and subsequent crime rate trends was not robust under these circumstances; the addition of these covariates caused the coefficient for change in infant mortality rates to become nonsignificant. This suggests that the relationship between improvements in infant mortality rates and changes in crime rates was largely due to a group of countries which happen to be economically developed, and did not, perhaps could not, improve a great deal on infant mortality rates, which also happened to experience great increases in theft and violent crime from 1980 to 1990.

But it is important to remember that, despite clear evidence of a “room for improvement problem” in the present dataset, there is still some support for the lagged nurturance hypothesis. Although the countries that experienced huge increases in crime rates from 1980 to 1990 can be characterized as highly developed in 1990, this does not provide a

Table 3

A comparison of countries grouped based on change in theft rate in 1980–1990

Dramatic increase in theft crime, 1980–1990			No dramatic increase in theft crime, 1980–1990		
Country	Change in theft rate, 1980–1990	Improvement in infant mortality rate, 1960–1970	Country	Change in theft rate, 1980–1990	Improvement in infant mortality rate, 1960–1970
Bermuda	3154.22		Canada	– 1414.78	8.00
Bahamas	2965.36		Israel	1129.31	7.00
Australia	2765.41		Monaco	– 906.01	
New Zealand	2602.38	6.00	Seychelles	– 609.67	
England and Wales	2522.54		Lesotho	– 366.47	10.00
Sweden	2396.23	6.00	Trinidad and Tobago	– 329.93	1.00
The Netherlands	2125.33	5.00	Central African Republic	– 314.50	56.00
Scotland	1974.45		US	– 217.50	6.00
Hungary	1914.79	12.00	Columbia	– 189.34	16.00
Norway	1873.19	6.00	Northern Ireland	– 179.11	
Malta	1849.56		Mauritius	– 171.19	
Denmark	1699.85	8.00	Switzerland	– 150.39	6.00
Luxembourg	1522.53		Tanzania	– 136.88	20.00
Spain	1361.54	18.00	Rwanda	– 101.88	5.00
Italy	1360.00	14.00	Sri Lanka	– 101.41	18.00
Finland	1203.25	8.00	Papua New Guinea	– 91.18	53.00
Barbados	1148.91		Sudan	– 86.81	19.00
France	1009.69	9.00	Coted'Ivoire	– 85.37	38.00
Austria	856.32	12.00	Hong Kong	– 83.90	23.00
Boswana	805.23		Malawi	– 57.92	14.00
Zimbabwe	516.89	22.00	Senegal	– 54.95	47.00
Singapore	462.46	16.00	Kuwait	– 42.76	
Greece	438.21	10.00	Libya	– 41.77	
West Germany	351.97		Indonesia	– 39.99	32.00
Ireland	297.21	9.00	Syrian Arab Republic	– 39.44	36.00
Chile	267.17	36.00	Venezuela	– 35.93	32.00
Brunei	257.11		Morocco	– 17.98	33.00
Costa Rica	221.93	9.00	South Korea	– 14.11	27.00
Portugal	191.74	25.00	Thailand	– 12.37	30.00
Japan	171.96	18.00	Iraq	– 2.88	
Cyprus	132.80		India	– 1.17	28.00
Ecuador	106.48	40.00			
Qatar	72.51				
Belgium	57.09	10.00			
Kenya	39.87	36.00			
Jordan	39.51				
Philippines	34.28	40.00			
Burundi	32.24	12.00			
Turkey	26.31	43.00			
Fiji	24.91				
Egypt	16.31	– 30.00			
Saudi Arabia	5.31	66.00			

satisfactory explanation for why those particular countries experienced increases in property crime. We have not eliminated the possibility that failure to improve childhood conditions played a role in those increases.

In summary, simple regression analyses and subsequent multiple regression analyses showed strong support for the lagged nurturance hypothesis when infant mortality rates and average life expectancies were used to measure “nurturance.” However, there is some indication that the findings for infant mortality rates may have been biased by level of economic development—a large subgroup of highly developed European countries had both small improvements in infant mortality rates from 1960 to 1970 (which was largely due to their very low values on this measure in 1960) and huge increases in theft crime from 1980 to 1990. It was argued that the fact, that the finding of a “bias” in the relationship—that the countries responsible for the effect all happened to be highly economically developed, does not effectively eliminate the possibility that failure to improve child nurturance was responsible for the effect (after all, those countries did, in fact, have little change in nurturance and large changes in crime rates as expected). Therefore, although there is adequate evidence, in the body of the findings, to reject the null hypothesis that changes in child nurturance will have no relationship to changes in crime rates a generation later, the interpretation of these findings is complicated by their nonlinear nature.

4. Exploring policy implications

4.1. Nurturance as crime control

The general paradigm for understanding criminal behavior (Vila, 1994) suggests that traditional strategies for crime control, which rely primarily on protection/avoidance and deterrence, always must be balanced with nurturant crime control strategies. The importance of using child development programs to attack the root causes of criminality is receiving substantial attention from researchers in many disciplines. Most agree that child development plays an important role in later criminal behavior (e.g., Farrington, 1994; Gottfredson & Hirschi, 1990; Loeber & Farrington, 1998; McCord & Tremblay, 1992; Moffitt, 1993; Patterson et al., 1989; Sampson & Laub, 1993; Wilson & Herrnstein, 1985). And evidence is mounting that early prevention efforts can be effective (Greenwood, Model, Rydell, & Chiesa, 1996; Tremblay & Craig, 1995; Yoshikawa, 1994). Our ability to understand—and potentially prevent—the development of criminality is increasing rapidly as results accumulate from research like the massive 10-year Project on Human Development in Chicago Neighborhoods (formerly the Program on Human Development and Criminal Behavior), which was designed with input from more than 200 experts in criminology, education, law, and psychology and supported by the US National Institute of Justice, Harvard School of Public Health, and the John D. and Catherine T. MacArthur Foundation (Earls & Reiss, 1994).

Still, however, many question the efficacy and feasibility of nurturant programs as crime control (see Vila, 1997a). In the present analysis, the indicators used were blunt and were not specifically measuring social programming. The measures were subject to a variety of errors,

sample sizes were low, and under these circumstances, a small effect would be expected to result in a Type II finding: missing an effect that actually exists. We think that the support we found for the lagged nurturance hypothesis indicates that nurturant programs may have measurable impacts on crime rates—so long as we look for them over the appropriate period of time.

5. Summary and conclusions

According to the data, there is a general tendency for countries that experienced larger improvements in measures of child nurturance from 1960 to 1970 (infant mortality rates and average life expectancies) to have experienced lesser increases in violent crime and property crime from 1980 to 1990. This supports the “lagged nurturance hypothesis,” that jurisdictions with better nurturance for small children will have lower crime rates when the children subject to those conditions reach their high-crime age. This effect, however, was partly due to the “room for improvement” problem; a subset of countries had very high average life expectancies and low infant mortality rates in 1960 and their “improvements” were therefore constrained. A group of these countries—European, highly economically developed countries—experienced very large increases in property crime from 1980 to 1990 and those sharp increases largely accounted for the relationships reported here between improvements in infant mortality rates and subsequent crime rate trends. Unexplained, however, is why those countries experienced such large increases in crime; we only know that they were characterized by small improvements in measures of nurturance.

The relationships between improvements in life expectancies and infant mortality rates and violent crime rates are somewhat more linear and straightforward but do not remain robust when we control for values at time t_1 and for level of economic development in 1990. Graphs do not suggest a dichotomy between countries having “crime waves” in the 1980s and those whose crime rates did not change much as was indicated by scattergrams of changes in base crime and theft crime rates.

It should be noted that the tests conducted here were very conservative because the dependent and independent variables had known errors, the sample sizes were low, and the changes in child nurturance were incidental, not planned to affect crime. Future tests of the lagged nurturance hypothesis should utilize smaller units of analysis and operationalize constructs with measures more closely associated with child nurturance policy. For example, an ideal test of the hypothesis would assess the lagged impact of city-level nurturant programs using measures such as Apgar scores,⁴ expenditures for maternal and infant health care, the availability and use of parenting training and child care, and educational expenditures for primary school.

The lagged nurturance hypothesis has important long-term implications. Short-term concerns about such things as gang violence, drug abuse, and street crime often divert

⁴ The Apgar score is an index used to evaluate the condition of a newborn infant based on ratings for five characteristics (color, heart rate, response to stimulation of the sole of the foot, muscle tone, and respiration) measured shortly after birth (Merriam-Webster’s collegiate dictionary, 1998).

resources and attention away from child development needs. If this dynamic is allowed to proceed unchecked, disproportionate spending on the criminal justice system may, over the long term, cause an increase in crime because it diminishes the nurturant activities that promote the development of self-control and behavioral strategies such as patience, creativity, and hard work over criminal strategies such as force, fraud, and stealth.

Spending too much on external control and too little on nurturance thus may trigger a self-destructive cycle in which poor nurturance increases the number of people who are likely to commit crimes; more crime stimulates spending for external controls and further diminishes funding for nurturant programs; and diminished funding for nurturant programs once again guarantees a pool of motivated offenders for the next generation.

Conversely, the lagged nurturance hypothesis also suggests that keeping adequate resources flowing toward child development programs is a social investment strategy that pays compound interest; over time, as more human potential is realized, the concentration of criminality in society and its attendant costs will tend to decline more and more rapidly. Both the public and policymakers need to be reminded frequently and vociferously of the importance of maintaining a balance between programs that address our fears and those that express our ideals and knowledge.

Appendix A. Outlier elimination procedures

It should be noted that a very aggressive policy was used to eliminate outliers. Initially, all dependent variables and indicators of child nurturance were screened for cases that appeared to diverge significantly from the main body of cases. When such cases were detected, they were eliminated from all analyses. In most cases, this process resulted in either no cases being eliminated or one or two cases being eliminated. For the change in homicide rates and change in attempt-adjusted homicide rates, however, five to six countries were eliminated as obviously different from most cases. When regression analyses were run, Cook's D was examined in each comparison and when the highest Cook's D exceeded 1, or if there was a jump in Cook's D such that one of the higher values was more than twice the preceding value, the case highest in Cook's D was eliminated until a set of consistent cases emerged. In most cases, this occurred; occasionally, more and more outliers appeared as cases were deleted, causing difficulty in choosing the set of cases to use. The justification for this policy regarding outliers is that, first, the data are not without serious reliability problems. When we see extreme changes in the value of a particular variable over a 10-year period, it is certainly possible that the measure is reflecting change in measurement, change in policy, or some other change and not necessarily actual change in the variable of interest. Because we are suspicious of such cases, we believe it is better to eliminate them than to allow them to unduly bias results. Moreover, because of small sample sizes, outliers can destabilize regression slope estimates—sometimes causing slopes to reverse in order for the model to accommodate the enormous squared error of one case. In some cases, the rigorous outlier elimination process strengthened findings supportive of our hypotheses, but the opposite occurred equally often.

Appendix B. Countries included in Table 3 robust comparisons

Country	Homicide and primary school enrollment	Homicide and hospital bed availability	Violent crime and infant mortality	Violent crime and average life expectancy	Violent crime and hospital bed availability	Theft and average life expectancy
Argentina	X					
Australia	X					X
Austria	X	X	X	X	X	X
Bahamas	OD	OD				
Bahrain		X				
Bangladesh	X	OI			OI	
Barbados		X			X	
Belgium	X	X	X	X	X	X
Brunei		X			X	
Burundi	X		X	X		X
Canada	X		X	X		X
Central African Republic	X	X	X	X	X	X
Chile	X	X	X	X	X	X
China		X			X	
Columbia	OD	OD	X	X	X	X
Costa Rica	X	X	X	X	X	X
Cyprus		X			X	
Czech Republic	X					
Denmark	X		X	X		X
Ecuador	X	X	X	X	X	X
Egypt	X	X	OI			X
Fiji		X			X	
Finland	X	X	X	X	X	X
France	X		X	X		X
Greece	X	X	X	X	X	X
Hong Kong	X	X	X	X	X	X

Hungary	X	X	X	X	X	X	X
India	X						
Indonesia	X	OI	X	X	X	OC	X
Iraq	X	X					X
Ireland	X	X	X	X	X	X	X
Israel	X	X	X	X	X	X	X
Italy	X						X
Jamaica	X	X					
Japan	X	X	X			X	
Jordan	X	X		X	X	X	X
Kenya	X	X	X	X	X	X	X
Kuwait	X	X					X
Lesotho	OD	OD	X	X			X
Libya		X		X	X	X	X
Luxembourg	OC	X					
Madagascar	X		OI				
Malawi	X		OD	OD	OD	OD	X
Morocco	X	X					X
Myanmar		X					X
Nepal	X	OC					
The Netherlands	X	X	X	X	X	X	X
New Zealand	X		X	X	OC		X
Norway	X	X					X
Panama	X	X					
Papua New Guinea	X	X	OC	X	X	X	X
Philippines	OD	OD					X
Poland	X	X					
Portugal	X		X	X	X		X
Qatar		X			X	X	
Rwanda	X	X					X

Saudi Arabia							X
Senegal	X						X
Seychelles		X					
Singapore	X	X	X	X	X	X	X
South Korea	X	OC	OD	OD	OD		X
Spain	X		X	X			X
Sri Lanka	X	X	X	X		X	X
Sudan	X	X					X
Swaziland	OD	OD					
Sweden	X	X	X	X	X	X	X
Switzerland							X
Syrian Arab Republic	X	X	X	X	X	X	X
Tanzania	X						X
Thailand	OD	OD	X	X	X	X	X
Trinidad and Tobago	X		X	X			X
Turkey	X	X	X	X		X	X
UK		X					
US		X	X	X	X	X	X
Venezuela	X		X	X			X
West Germany	OI				X		X
Zimbabwe	X	X	X	X	X	X	X

X=Included; OD=deleted as outlier based on dependent variable; OI=deleted as outlier based on independent variable; OC=deleted as outlier based on Cook's *D*.

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