Explaining discrepant findings in cross-sectional and longitudinal analyses:
An application to U.S. homicide rates

Julie A. Phillips *

Department of Sociology and Institute for Health, Health Care Policy and Aging Research,
Rutgers University, USA

Available online 2 September 2005

Abstract

Cross-sectional studies often reach different conclusions regarding the association between key explanatory variables and outcomes than those of longitudinal approaches. This study considers possible explanations for discrepant findings using a decomposition approach with panel data on 404 U.S. counties for the period 1970–1999. The analysis establishes that there are important differences in the effects of independent variables on homicide rates across counties as opposed to within counties over time. Explanations offered for these discrepancies are that variables may have differing temporary (flow) and permanent (stock) influences on outcomes and possible omitted variable bias. The findings highlight the importance of distinguishing among possible stock and flow effects and are significant not only for the study of crime but also for other social phenomena.

© 2005 Elsevier Inc. All rights reserved.

Keywords: Panel data; Stocks and flows; Homicide; United States

* Corresponding author. Fax: +1 732 445 0974.
E-mail address: jphillips@sociology.rutgers.edu

This research was supported in part by a grant from the National Science Foundation (SES #0111542). I thank David Greenberg, Ken Land, Louise Russell, Paul Allison, Robert Kaufman, Leslie McCall, Jane Miller, and the anonymous reviewers for helpful comments and advice on earlier drafts. Xiaohui Xin and JiWoong Bo provided excellent research assistance.

1. Introduction

A core objective within various fields of sociology is to understand why substantial variation in social behavior exists across time and place. A common assumption has been that the association between explanatory variables and the outcome of interest is the same across these two dimensions, but research often yields inconsistent findings. Cross-sectional studies reach different conclusions regarding several key relationships than those of longitudinal approaches (e.g., Clark, 1992; Fenwick and Barresi, 1981; Kaufman, 1993; Marvell and Moody, 1996; Mouw, 2002). Although researchers acknowledge such discrepancies, they have yet to explain them in any systematic way.

This study pools annual data on homicide rates for 404 U.S. counties over a 30-year period to unravel some of the paradoxes presented from past cross-sectional and longitudinal studies, making the following contributions to the literature. Using panel data and a decomposition approach, the analysis establishes that there are important differences in the effects of key explanatory variables on homicide rates across counties as opposed to within counties over time. These varying effects may be attributed in part to the fact that the permanent influence of a variable on an outcome can be distinct from the temporary one. Cross-sectional studies, which measure stocks or levels of variables, tend to capture permanent influences while longitudinal studies, which use flows or changes in variables, typically measure temporary effects. Omitted variable bias may also offer some explanation for the discrepant results. The findings highlight the importance of distinguishing among possible stock and flow effects and are significant not only for the study of crime but also for other social phenomena.

2. Background

Until recently, the vast majority of studies examining variation in rates of social behavior have adopted either a cross-sectional approach, focusing on variation across place, or a longitudinal approach, emphasizing variation over time (e.g., Blau and Blau, 1982; LaFree and Drass, 1996; Land et al., 1990). As noted above, although findings from these studies are fairly consistent when matched within the same methodological approach, a comparison of results across methodological approach reveals a number of inconsistencies. For example, within the criminological literature, Marvell and Moody’s (1991) review of the relationship between age structure and crime rates showed that although the majority of longitudinal studies suggest a moderate to strong age–homicide relationship, most cross-sectional studies find little or no support for a strong age–homicide association. A review of 63 studies examining the unemployment–crime relationship (Chiricos, 1987) also documented a number of discrepant findings. Murray and Erickson (1987) noted that findings within the juvenile justice and deterrence literature vary across the two methodological approaches. Beck (1980) reviewed the literature on the effect of unionization on black–white inequality, finding that cross-sectional analyses tend to support the
worker-solidarity position, but that his longitudinal work promotes the white-protectionist argument. Indeed, Mairesse (1990) cited such differences as one of the oldest questions in econometrics and observed that the dissimilarities in findings are often attributed to differences in units of analysis, the definition and measurement of variables, or other features of the sample.

More recently, researchers have employed econometric techniques, pooling data on cross-sections and time periods, to study social issues (e.g., Lott, 1998; Marvell and Moody, 1996). The advantages of such panel data are well known: panel data sets offer two dimensions of variation, cross-sectional and temporal, and thus more efficient estimation is possible. The fixed effects model, for which panel data are needed, also controls for certain types of omitted variable bias. For example, if there are unmeasured characteristics of the cross-sectional units (e.g., cities, individuals) that are stable over time or of the time periods that have a uniform effect on all cross-sectional units, fixed effects estimation (including dummy variables for all units and time periods) corrects this problem, thus eliminating a potential source of bias (Kennedy, 2003).

The standard assumption in the vast majority of studies employing panel data, which nearly always apply either a fixed effects or a random effects model specification, is one of a single effect of each explanatory variable on the outcome of interest (Kaufman, 1993; Mairesse, 1990; see also Neuhaus and Kalbfleisch, 1998). However, as noted above, there are two sources of variation—that across units and that within units over time. For this reason, statisticians suggest estimation of a ‘decomposition model,’ which provides separate estimates for the overall effect of a covariate on the dependent variable between units (the between-unit estimator) and for the annual effects of a covariate on the dependent variable within a particular unit (the within-unit estimator) (e.g., Judge et al., 1985; Kaufman, 1993; Neuhaus and Kalbfleisch, 1998; Wooldridge, 2002). The between-unit differences are calculated as the cross-section or unit mean of a particular characteristic over the entire period while the within-unit differences are defined as annual cross-section-year deviations from the overall cross-section mean.

Despite this recommendation, few studies adopt this general type of model specification. There are several exceptions, and these studies reveal that the two components often differ. For example, Kaufman (1993), noting the discrepant cross-sectional and longitudinal findings regarding the effect of unionization on wages, pooled data on industries over time to demonstrate a number of different effects across industry and time of unionization and economic conditions on race–gender group industrial earnings.1 Neuhaus and Kalbfleisch (1998) also found statistically significant differences in between- and within-person effects in their examination of mother’s age and infant birth weight. There were far greater differences in average

---

1 Kaufman (1993) applies a slightly different type of ‘decomposition model’ than the one described to distinguish between-unit effects from within-unit effects. In particular, the between-unit component is defined as the difference between the value of an explanatory variable for a given unit at a given point in time and the mean of the explanatory variable taken across all units at that point in time. The within-unit component in the Kaufman analysis is defined as described in the text above.
infant birth weight between women for each one-year difference in age than for a given woman for each year that she ages.

3. Possible explanations for discrepant findings

Explanations for the disparate findings become more difficult to offer when panel designs are applied. Unlike distinct cross-sectional and time-series analyses, for example, the units of analysis and variable measurement are the same. Reviewed below are several possible reasons for the incongruent findings, which relate to limitations in model specification and to actual differences in the effects of certain factors across time and place.

3.1. Different stock and flow covariate effects

Past discrepancies in findings from cross-sectional and longitudinal studies may be attributable to actual differences in how factors affect variation in behavior across place as opposed to time. That is, the lasting (or stock) effect of a variable, typically captured with cross-sectional studies, may be distinct from the transitory (or flow) impact on the outcome, usually measured by time-series analyses (Kennedy, 2003; Levitt, 2001; Mairesse, 1990; Norstrom, 1987). Indeed, economists have made such distinctions in demand analyses (Hsiao, 2003). Changes over time typically constitute temporary shifts, and adjustments to the shifts do not necessarily happen immediately. As Hsiao (2003) notes, “an incompletely adjusted response will typically have a lower coefficient than the fully adjusted one” (p. 286). These distinctions are analogous to those made within the literature on poverty and well-being; many studies demonstrate that long-term—cumulative or persistent—poverty (e.g., average income during the past 10-year period) is more strongly correlated with adverse health and education outcomes than short term or recent measures of poverty (e.g., income during the present year) (Korenman and Miller, 1997; Teachman et al., 1997). Thus, differing stock and flow effects offer one possible explanation for the inconsistent findings across methodological approaches.

Several studies have explored whether there are differing permanent and transitory associations between crime rates and certain explanatory variables, most notably unemployment rates. With regard to unemployment, strain theory suggests that unemployment may raise the motivation to commit crime, but routine activity theory suggests that unemployment reduces opportunities for crime due to increased guardianship. Cantor and Land (1985, 2001) and Land et al. (1995) have argued and found empirical support for the fact that the opportunity effects of unemployment rates on crime levels are likely to be relatively immediate whereas the motivational aspects are typically postponed. An increase in unemployment moves adults from the workplace to the household and curtails recreational activities immediately, instantaneously reducing the opportunity for crime. However, due to various support mechanisms such as unemployment benefits and financial savings, the motivational impact of unemployment on crime emerges over a longer period of time.
In addition, periods of sustained economic hardship may affect the behavior of people who are not themselves unemployed.

Regarding the association between imprisonment and crime, Liedka et al. (2004) argue that it is important to make distinctions between stock and flow influences. Assuming the deterrence model of incarceration, the authors argue that on the one hand, a potential offender may determine the cost of crime commission based on the number of offenders sent to prison in the previous year (a flow measure). Alternatively, s/he may make this determination using the number of community members who have been sent to prison over the years (a stock measure). Using both violent and property crime rates as dependent variables, the study concluded that any crime-control effects of incarceration arise from the accumulation of prisoners, not the intake of new prisoners in any given year.

3.2. *Omitted variable bias*

Methodologists argue that, under the basic regression assumption that the explanatory factors are not correlated with the error term, the between-unit and within-unit estimates should be consistent and unbiased (Allison, 2005; Mairesse, 1990). To the extent that this assumption is violated, estimates may be biased, explaining the discrepant findings across time and place. Most attention in this regard has focused on the possibility of unreliable between-unit estimates, because the within-unit estimates are unbiased to the extent that stable characteristics of cross-sectional units are controlled. For this reason, researchers typically argue in favor of the within-unit estimator, viewing it as more conservative and therefore with greater confidence (Mairesse, 1990).

Yet to the extent that there are omitted characteristics that vary over time, the within-unit estimators will also be biased. Indeed, some contend that these omitted time-varying factors will affect the within estimators far more than the between estimators, since the error term is averaged in the between regression and practically eliminated when the time series is sufficiently large (Kennedy, 2003; Mairesse, 1990). In the case of the within estimator, however, measurement error bias may be exacerbated since transformations wipe out the cross-section intercept effect. That is, all the variation used in estimation is variation within units, which is heavily contaminated by measurement error (Kennedy, 2003). The effect is typically to lead to downward bias in the within estimator (Mairesse, 1990).

Hsiao (2003) also makes the case that the within-unit coefficient can be underestimated due to omitted variable bias, noting that the covariations of the omitted variable and the included explanatory variables in a cross-sectional regression may be different from those in a time-series regression. For example, imagine an omitted variable correlated with an included explanatory variable. The cross-sectional parameter may capture the joint effect of the two variables, but the time-series estimate may well be smaller than the cross-sectional estimates because of negligible correlation between the included and omitted variable (see Hsiao, 2003, p. 286). This is certainly a plausible explanation for the findings reported by Neuhaus and Kalbfleisch (1998). The much bigger effect of increasing mean age on birth weight between women may...
be attributable to the fact that some women in the sample had their first birth at young ages; the between-coefficient may be capturing some of this effect. These observations suggest that the effects of omitted variables can be markedly different in time series and cross-sections, and that different spatial and temporal effects may simply be statistical artifacts.

4. Study objectives

This study aims to shed light on these issues, using homicide as an example. To my knowledge, no prior criminological research has compared in detail, using panel data, the ways in which key explanatory variables may be associated with homicide rates across time and place. Using county-level data for the period 1970–1999, I first determine whether there are indeed differences in the between-county and within-county estimates of the independent variables on homicide rates. Different effects are anticipated given the results from several review studies (see above) indicating that variables (e.g., age composition) may have varying effects across time and place. The analysis then explores, through the introduction of various lagged and differenced terms intended to measure different facets of stock effects, the ways in which possible explanations for such discrepant findings may be applicable in the case of homicide. Finally, I generalize the ways in which these approaches can be applied to other social issues.

5. Data

County-level data for the period 1970–1999 are used in this study. Counties are advantageous for the purposes of this research since they represent fairly small geographic areas (compared to states), annual data for a number of indicators are available, and the boundaries of large counties have not changed over time, unlike those of Metropolitan Statistical Areas (MSAs) or central cities. However, keep in mind that some argue that findings surrounding various theories of homicide should not be affected by the unit of analysis selected (Parker et al., 1999). The analysis focuses on homicide since information on this crime is the most accurate and complete of available data, unlike that for property crimes, which is plagued by problems of quality and measurement (Cohen and Land, 1987). Also, homicide is the crime least subject to changes in reporting over time, an important consideration given the time-series nature of this analysis.

5.1. Dependent variable

The dependent variable is defined as the homicide victimization rate. Data for the numerator, homicide victims (excluding homicides due to legal intervention) for the period 1970–1999, are obtained from the National Center for Health Statistics (NCHS) (U.S. Department of Health and Human Services, 1968–1999).
the NCHS does not provide geographical information for deaths occurring in counties with populations of less than 100,000, the sample of counties is restricted to those with a population of at least 100,000 throughout the entire period (approximately 13.1% of all U.S. counties). The denominator for the homicide victimization rate is the total mid-year population of each county in each year, acquired from the Census Bureau (U.S. Department of Commerce, 1970–1999). The Bureau provides population counts by age (5-year intervals), sex, and race for each census year (1970, 1980, 1990, and 2000) and inter-censal estimates of population size for these demographic groups. The crude homicide rates are logged to remove skewed data patterns.

It is noteworthy that this study uses information on homicide victims by county of occurrence rather than data on homicide perpetrators, which is available from the FBI’s Supplementary Homicide Reports (SHR). This study focuses on homicide victims for several reasons. First, the quality of SHR data is questionable since not all jurisdictions provide reports for each month, and for some incidents, the perpetrator is unknown. In addition, the efficacy of police departments to identify a perpetrator and make an arrest is likely to vary across counties and time, and thus the use of information on perpetrators could introduce a potential source of bias into the analysis. Second, a longer time series (1970–1999) can be examined with victimization rates compared to offending rates (1976–1999). Still, other analyses indicate that results are similar whether homicide victimization or offending rates are used (Phillips, 2004).

5.2. Independent variables

Strain (blocked opportunity) theory posits that places or time periods with relatively high levels of absolute and relative deprivation will exhibit high crime rates (Blau and Blau, 1982; Merton, 1938). Applied to violent crime commission such as homicide, such strain leads to feelings of frustration that are ultimately manifested in violent behavior (Dollard et al., 1939; Felson, 1992). Two variables that measure economic conditions across counties and year, per capita income and unemployment rates, are incorporated into the analysis. These particular measures are selected since county-level information on them is available for virtually all years under study. Data on per capita income for each county from 1970 to 1999 are gathered from

---

2 While the 1980–1989 and the 1990–1999 series produced by the Census Bureau are consistent with one another, they are not consistent with the 1970–1980 estimates. To obtain a consistent data series over the entire period, the original 1980 counts for total population by race and sex in the 1970–1980 series were replaced with the 1980 counts from the 1980 to 1989 series, and the total population counts for the years 1971–1979 were adjusted with the following formula: \( P_t = Q_t \times \left[ \frac{P_{10}}{Q_{10}} \right]^{t/10} \), where \( P_{10}/Q_{10} \) is the ratio of the new total population count for 1980 divided by the original 1980 total population, and \( Q_t \) is the original total population count for year 1970 + t (t goes from one to nine). New counts for each age group for the 1971–1979 period were estimated by assuming the same age distribution as that of the original 1970–1980 series estimated by the Census Bureau.

3 About 2.7% of cases had a homicide rate of 0. A small value (1) was added to the homicide rate so that the log transformation could be performed (see Hamilton, 1992, p. 17).
the Regional Economic Information System (REIS) (U.S. Department of Commerce, 1969–2000). The per capita income figures are converted to 1982–1984 constant dollars, using the regional Consumer Price Indices (CPI) available from the BLS website (U.S. Department of Commerce, 1967–1999). Information on unemployment levels, measured as the percentage of the civilian labor force that is unemployed, at the county level are provided by the Bureau of Labor Statistics (BLS) and are available for the years 1970 and annually from 1976 to 1999. Linear interpolation techniques assuming constant growth are applied to estimate unemployment rates for missing years (1971–1975).

Social control theory emphasizes the role of social bonds in explaining why people do not commit crime (Hirschi, 1969; Sampson and Laub, 1993). Social connections prevent people from engaging in criminal activity, and sociologists often argue that places or time periods with weak family structures should experience higher levels of violence (e.g., Greenberg, 2001; Land et al., 1990; Sampson, 1987). A measure of the percentage of the population that is divorced is therefore included for each county and year. Information on divorce levels for each census year (1970, 1980, 1990, and 2000) is available from the Census Bureau. Linear interpolation techniques, assuming constant growth over the period, are applied to interpolate the missing values for intercensal years.

Demographic factors are also highly correlated with criminal involvement—homicide victims and offenders are predominantly young, male, and minority members, and many note that in places or time periods with more crime-prone demographic features (that is, more males, more youth, and more minority members), homicide rates should be elevated. Age structure is captured by two variables indicating the relative size of the population that is young: The percentage aged 15–24 years and the percentage aged 25–34 years. Both homicide victims and offenders are heavily concentrated in the 15- to 34-year age group (note that the age distribution of victims peaks later than that for homicide offenders), and therefore, age composition affects homicide rates through the production of both victims and offenders (Cohen and Land, 1987). The sex and race composition of each county is controlled with variables measuring the percentage of the population that is male and black, respectively. These measures are all derived from Census data.

Finally, controls for year and county population size are included in all analyses. Period effects in homicide levels are controlled by including dummy variables indicating each year. I control for the population size of each county in each year. Not only do larger populations present a greater number of potential targets for criminals, but also, sociologists have long recognized the positive relationship between a place’s size and some forms of social disorganization, such as criminal activity (Wirth, 1938). The measure of county population size, obtained from the Census Bureau, is logged since its association with homicide rates is nonlinear (data not shown).

5.3. Descriptive statistics

Table 1 displays descriptive statistics for these variables, showing the mean values as well as overall, temporal and spatial variation in these characteristics.
Note that there is considerable overall variation across counties in characteristics, as indicated by the size of the standard deviations. In addition, there is ample variation in these characteristics within counties over time. For example, the percentage divorced varies quite substantially across counties and over time—a standard deviation of 1.57 and 2.09 percentage points across counties and time, respectively.

### 6. Methods

To analyze the association between these explanatory variables and homicide levels across counties and time, a cross-sectional time-series data set, containing repeated measurements on counties over time, is constructed. A model using such data can be expressed in the following general form (Bryk and Raudenbush, 1992; Johnston and DiNardo, 1997; Judge et al., 1985).

\[
y_{jt} = \alpha + x_{jt}\beta + v_j + e_{jt}.
\]

The dependent variable \(y_{jt}\) represents the logged Crude Homicide Rate (CHR) for county \(j\) in time \(t\). \(\alpha\) denotes the model intercept and \(\beta\) represents the estimated set of parameters for \(x_{jt}\), the matrix of explanatory variables for each county \(j\) and year \(t\). The primary difference between this model and the general linear model is in the treatment of the disturbance terms. The model includes a county-specific residual, \(v_j\), which varies across counties but not across time and allows for correlation among observations from the same county. \(e_{jt}\) is the model residual and captures random variation within counties over time.

Different models for clustered data make various assumptions to estimate model (1). For example, a fixed effects model treats \(v_j\), the between-county differences, as...
fixed and estimable and provides estimates of $\beta$ only for within-county effects. In contrast, a random effects model treats $v_j$ as independent and randomly distributed and provides estimates of $\beta$ that capture the combined effect of the between-county and within-county components (Bryk and Raudenbush, 1992; Judge et al., 1985). These standard approaches are inappropriate for the present research question since both model specifications essentially assume that between-county and within-county effects are the same. A fixed effects model ignores cross-sectional variation and obtains estimates only of within-unit effects while a random effects model provides an average effect of within-unit and between-unit variation. These approaches are appropriate when between-unit and within-unit effects are similar, but results are obviously misleading if this is not the case.

To bypass such assumptions and to identify differences in the way selected independent variables are associated with homicide rates over time within counties as opposed to across counties, a ‘decomposition model’ is estimated in this analysis. The decomposition model provides separate estimates for the effect of a covariate on the dependent variable between units (the between-unit estimator) and for the annual effects of a covariate on the dependent variable within a particular unit (the within-unit estimator) (Allison, 2005; Britt, 1997; Bryk and Raudenbush, 1992; Hsiao, 2003; Judge et al., 1985; Kaufman, 1993; Neuhaus and Kalbfleisch, 1998). The decomposition model can be expressed as follows:4

$$y_{jt} = \alpha + \beta X_j + \eta (x_{jt} - X_j) + v_j + \epsilon_{jt}. \tag{2}$$

The parameter $\beta$ measures the effect of the between-county differences, where these differences are represented by county means (all means are denoted by capital letters) for a particular characteristic over the entire period (a stock measure). The parameter $\eta$ captures the effect of within-county differences, annual county-year deviations from the overall county mean (a flow measure). Note that if Eq. (2) is true, it is also the case that

$$Y_j = \alpha + X_j \beta + v_j + \epsilon_{0(j)}. \tag{3}$$

Subtracting Eq. (3) from (2), it is also true that

4 In multilevel linear model parlance, a two-level hierarchical linear model is estimated in this analysis with county-year units (level-1 unit) clustered within counties (level-2 unit). When group mean centering is adopted (see Holman and Gavin, 1998; Kreft et al., 1995; Raudenbush, 1989 for more detail), the level 1 model is expressed in the following general form:

(i) $y_{jt} = \alpha_j + \beta_j (x_{jt} - X_j) + \epsilon_{jt}$.  

The level 2 equations are expressed as follows:

(ii) $\alpha_j = \eta_{00} + \eta_{0j} X_j + v_j$ and $\beta_j = \eta_{0j}$.

Combining these two levels, we obtain the following model, equivalent to the decomposition model described above:

(iii) $y_{jt} = \eta_{00} + \eta_{0j} X_j + \eta_{10} (x_{jt} - X_j) + v_j + \epsilon_{jt}$. 

\[ (y_{jt} - Y_j) = (x_{jt} - X_j)\eta + (e_{jt} - E_j). \] (4)

Eq. (4) is equivalent to the fixed effects model. All the models presented are estimated using maximum likelihood methods. 5

The decomposition model (Eq. (2)) incorporates a county-specific residual term, \( v_{jt} \), which is treated as a random variable as in the case of a random effects model. Thus, correlation among observations from the same county is allowed, but counties are assumed to be completely independent of one another. Given that some of the counties sampled are clustered within metropolitan areas and states, tests of random effects for metropolitan areas and states were conducted to determine whether this assumption is justified; the state-specific residual term is statistically significant, but the metropolitan area term is not. Thus, all analyses incorporate a state-specific residual term, permitting observations of counties from the same state to be correlated. Counties within the same state share criminal justice laws implemented at the state level that may affect homicide commission, such as gun control restrictions, and the state-specific residual controls for differences in such stable state characteristics over time.

The residual error term, \( e_{jt} \), allows for correlation within observations from the same county. An autoregressive error structure is applied, since preliminary analyses indicated this structure to be more appropriate than one in which the correlation of observations within counties is assumed to be the same (\( \chi^2 = 408.4 \)). Ideally, spatial autocorrelation in the residual error term should also be considered, since changes within a county in the homicide rate over time may produce changes in surrounding counties. 6 However, controls are not included since it is not possible to estimate in SAS or STATA spatial disturbance models with an autoregressive structure (personal communication with the SAS Institute and Stata Corporation; see also Worrall and Pratt, 2004). 7 The state random effect discussed above controls partially for spatial autocorrelation, but to the extent that there are omitted time-varying characteristics common to counties clustered in the same metropolitan area or state that are not captured by the time dummies, results should be interpreted with caution.

5 Tests for the stationarity of the various time-series included in this analysis were conducted in exploratory analyses. Using the Levin–Lin–Chu test with varying lag-lengths (from 1 to 8 years), the null hypothesis of non-stationarity was rejected in favor of a stationary process (Levin et al., 2002; Liedka et al., 2004), with the exception of three cases (the percentage divorced, the percentage aged 25–34, and per capita income). A graphical inspection of these three variables indicates that these series are trend-stationary. Since the models include a dummy variable for each year, thus controlling for time trends, the non-stationarity of these series is partialled out. Note that these tests should be treated with caution since the properties of these variables are being tested on a very short time-series, thus reducing the effectiveness of the tests.

6 Forty-five percent of the 404 counties belong to one of 63 metropolitan areas; the remaining 55% of counties are not adjacent to any other counties in the sample.

7 Worrall and Pratt (2004), who note that issues concerning spatial autocorrelation are typically ignored in time-series cross-sectional work, adopt an alternative model incorporating a spatial lag to control for spatial clustering in their analysis of crime across all counties in California, but find that results are virtually identical to those without controls for spatial autocorrelation. This may well be due to the fact that diffusion effects are most relevant to the areas immediately adjacent (i.e., within the first few miles) to a county’s border. Data limitations clearly preclude such an examination in this case.
7. Analytic strategy

I begin the analysis by estimating the decomposition model described above to identify the independent variables with effects that differ across place and time. I then proceed to explore explanations for the varying effects. The within-county model identifies flow or temporary effects; to determine whether there are distinct stock or lasting effects of variables within counties, I examine how differenced and lagged variables are associated with homicide rates within counties. Cantor and Land (1985) operationalized the motivational (permanent) impact of unemployment on crime as the difference between the national unemployment rate in any two contiguous years: that is, the longer-term effect of unemployment is represented by changes in the unemployment rate from year to year ($\Delta U_t = U_t - U_{t-1}$). Cantor and Land (1985) argued that: “If the former ($U_t$) is higher than the latter ($U_{t-1}$), and if this change in the level of unemployment results in a positive motivational impact on crime, then there should be upward fluctuations in crime rates. Similarly, economic recoveries and the associated declines in the unemployment rate should produce downward perturbations in crime rates (p. 322).”

Some have been critical of this specification of a long-term motivation effect of unemployment, arguing that a lagged variable (e.g., $X_{t-1}$) is a more appropriate measure of possible delayed effects of a variable (e.g., Greenberg, 2001; Pyle and Deadman, 1994). A lagged formulation implies that permanent effects are static but delayed. As noted by Greenberg (2001) and O’Brien (2001) among others, it is impossible to include all three combinations of variables ($X_t$, $X_{t-1}$, and $(X_t - X_{t-1})$) in the same model since the differenced term, $(X_t - X_{t-1})$, is a linear combination of the other two variables in the model. However, keep in mind that the model specification $Y_t = \alpha + \beta_1 X_t + \beta_2 (X_t - X_{t-1}) + \epsilon$ is equivalent to $Y_t = \alpha + \beta_3 X_t + \beta_4 (X_{t-1}) + \epsilon$, where $\beta_3 = \beta_1 + \beta_2$ and $\beta_4 = -\beta_2$. Thus, the effect on homicide rates of a lagged variable is exactly the opposite (although the same in magnitude) as that of the differenced term. In this analysis, I consider how both differenced and lagged variables within counties are associated with homicide rates over time and how they relate to cross-sectional (stock) associations.

8. Results

8.1. Spatial and temporal variation in homicide rates

Table 2 displays the results from the decomposition base model, which separates the effects of covariates on homicide rates into those between counties (cross-sectional variation) and those within counties over time (temporal variation). Consistent

---

8 Note that Cantor and Land (1985) examined the effects of unemployment rates and fluctuations therein on short-term fluctuations of crime rates around their long-term trends.
Table 2
Regression results of logged homicide rate on selected covariates, 404 U.S. Counties, 1970–1999

<table>
<thead>
<tr>
<th></th>
<th>Decomposition model</th>
<th></th>
<th></th>
<th>Random effects model</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Between-county</td>
<td></td>
<td>Within-county</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Coefficient</td>
<td>Standard error</td>
<td>Coefficient</td>
<td>Standard error</td>
</tr>
<tr>
<td>Percent divorced</td>
<td>0.142^{a,b}</td>
<td>0.013</td>
<td>0.005</td>
<td>0.007</td>
</tr>
<tr>
<td>Unemployment rate</td>
<td>0.066^{a,b}</td>
<td>0.010</td>
<td>−0.014^{a,b}</td>
<td>0.003</td>
</tr>
<tr>
<td>Per capita income (000)</td>
<td>−0.005</td>
<td>0.008</td>
<td>−0.015^{a,b}</td>
<td>0.004</td>
</tr>
<tr>
<td>Percent aged 15–24 years</td>
<td>0.011^{a}</td>
<td>0.006</td>
<td>0.017^{a}</td>
<td>0.006</td>
</tr>
<tr>
<td>Percent aged 25–34 years</td>
<td>−0.015^{b}</td>
<td>0.012</td>
<td>0.021^{a}</td>
<td>0.006</td>
</tr>
<tr>
<td>Percent male</td>
<td>−0.030^{a,b}</td>
<td>0.014</td>
<td>0.016</td>
<td>0.014</td>
</tr>
<tr>
<td>Percent black</td>
<td>0.035^{a,b}</td>
<td>0.002</td>
<td>0.018^{a}</td>
<td>0.003</td>
</tr>
<tr>
<td>Logged population size</td>
<td>0.198^{a,b}</td>
<td>0.021</td>
<td>−0.275^{a}</td>
<td>0.032</td>
</tr>
<tr>
<td>Region (West)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Midwest</td>
<td>−0.113</td>
<td>0.081</td>
<td></td>
<td>−0.323^{a}</td>
</tr>
<tr>
<td>Northeast</td>
<td>−0.228^{a}</td>
<td>0.090</td>
<td></td>
<td>−0.515^{a}</td>
</tr>
<tr>
<td>South</td>
<td>0.193^{a}</td>
<td>0.085</td>
<td></td>
<td>−0.039</td>
</tr>
<tr>
<td>Intercept</td>
<td>−0.882</td>
<td>0.757</td>
<td></td>
<td>0.527</td>
</tr>
<tr>
<td>Random effects</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>State</td>
<td>0.019^{a}</td>
<td>0.006</td>
<td></td>
<td>0.024^{a}</td>
</tr>
<tr>
<td>County</td>
<td>0.051^{a}</td>
<td>0.004</td>
<td></td>
<td>0.081^{a}</td>
</tr>
<tr>
<td>−2 Log Likelihood</td>
<td>9352.1</td>
<td></td>
<td>12.39^{a}</td>
<td>9731.2</td>
</tr>
</tbody>
</table>

Note. N = 12,076.

^{a} p < 0.05.

^{b} Statistically significant difference (p < 0.05) between the between- and within-county coefficient estimates.

^{#} p < 0.10.
with prior studies, the results clearly indicate that there are a number of statistically significant differences in the magnitude and sometimes direction of association of these covariates across counties as opposed to over time.

The majority of covariates are associated with spatial variation in homicide rates in the anticipated direction. Counties with greater levels of divorce and unemployment over the study period exhibit higher homicide levels—for example, for every one percentage-point increase in the average unemployment level of a county over the period, the homicide rate, on average, rises by 6.8% \((=e^{0.066} - 1)\).\(^9\) Age structure also affects violence levels in the expected way. Counties with a larger percentage of the population between the ages of 15 and 24 have higher average homicide rates, although those counties with relatively large proportions of their population in the older segments of the crime-prone age ranges (25–34 years) do not. Consistent with previous findings, larger counties and those with a relatively large black population are more likely to have higher homicide rates. After controlling for these social, economic, and demographic attributes of places, those counties located in the Northeast still exhibit significantly lower rates of homicide than do those in the South and West. Southern counties have the highest rates of homicide. Somewhat surprisingly, counties with a higher percentage of males have lower homicide rates on average. This finding, however, is in line with other studies that suggest that the positive effect of percent male on homicide levels is sometimes suppressed. A greater number of males in the population is also associated with more stable family structures, which in turn are associated with lower homicide rates (Messner and Sampson, 1991; Sampson, 1987).

Turning to the within-county model component, I find that all the explanatory variables, with the exception of percentage divorced and percentage male, are significantly associated with homicide rates within counties over time. For example, a one-point increase in the percentage aged 15–24 and 25–34 is associated with a 1.7 and 2.1% increase in the homicide rate, respectively. Although improvement in economic conditions, measured by increases in per capita income, is associated with declines in homicide rates, seemingly so too is an economic downturn, as measured by increases in unemployment rates. Rises in the relative size of the black population are associated with increases in homicide rates over time, but increases in population size are associated with declines in homicide rates over time.

Clearly, there are important differences in the associations between these covariates and homicide rates over time as opposed to place. Indeed, statistical tests reveal that all the between-county and within-county covariate estimates differ significantly from one another, with the exception of per capita income and the percentage aged 15–24.\(^{10}\) Some of these significant differences are simply a matter of degree—for

\(^9\) The percentage changes reported using the formula \(100(e^{\text{percentage change}} - 1)\) are approximate assuming an underlying unreported baseline homicide mortality rate of 1 per 100,000.

\(^{10}\) In exploratory analyses, weighted analyses (by the square root of a county’s total population) were conducted. The weighted findings are similar to the unweighted results presented here; in particular, statistically significant differences in the between- and within-county effects were found for all covariates except per capita income and percentage aged 15–24.
example, levels of divorce and the percentage of the population that is black are both positively associated with violence between and within counties, but the association, as gauged by coefficient size, appears greater between than within counties. In the case of percentage divorced, a one-standard deviation increase is associated with a 1.1% \( (=e^{0.005 \times 2.091} - 1) \) increase in homicide rates within counties over time (an effect not significantly different from zero), but with a 25% \( (=e^{0.142 \times 1.572} - 1) \) increase in homicide rates across counties.

These patterns are consistent with conjectures made by Hsiao (2003) and Mairesse (1990) that the between-county estimate can often be larger than the within-county estimate. They are also compatible with the notion of cross-sectional variation picking up lasting or stock effects of covariates on homicide. For example, the instability and lack of social control brought about by a high prevalence of divorce within a population takes time to occur and would have a delayed effect, particularly to the extent that it is the children of broken households who become most susceptible to criminal activity. Given the mechanism through which we expect family structure to affect homicide rates, fluctuations in divorce levels from one year to the next would necessarily have a weaker impact.

However, other variables exhibit a stronger relationship with homicide rates over time rather than place. The variable measuring the percentage aged 25–34 exhibits no association with homicide rates in the between-county model, but higher levels of the percentage young over time within a county are associated with higher homicide rates. Recent research demonstrates that the temporal relationship between age structure and homicide rates varies according to time period and other county characteristics (Phillips, 2004). It may be that the between-county effect of age composition is weaker, since the effects of age structure are averaged out over all years and socioeconomic conditions.

Other covariates exhibit diametrically opposite patterns of association with homicide rates in the between-county and within-county models. Higher unemployment is associated with greater levels of homicide across counties, yet within counties over time higher levels of unemployment correspond to lower homicide rates. A one-standard deviation increase in unemployment is associated with a 2.7% \( (=e^{-0.014 \times 1.937} - 1) \) decline in homicide rates within counties over time but with an increase of 12.6% \( (=e^{0.066 \times 1.793} - 1) \) between counties. Although larger places exhibit higher homicide rates, increases in population size over time within a county are associated with declines in homicide rates. The patterns observed with these factors also appear consistent with the notion that there can be different flow and stock effects of covariates, captured by time-series and cross-section models, respectively. As mentioned earlier, Cantor and Land (1985) suggest that the immediate effects of unemployment, which typically reduce criminal opportunities, may differ from the persistent consequences, which are motivational. In the case of population size, it is entirely plausible that the between-county estimator captures the social disorganization and anomie effect that larger places may promote, an effect that would take time to occur. On the other hand, fluctuations from year to year in population size (measured by the within-county estimator) may instead proxy the general desirability and economic well-being of a place.
Counties that experience increases in population over the study period (partly due to migration) may be enjoying economic growth and greater prosperity, factors we expect would drive down crime rates.

Finally, note that these important spatial and temporal differences in the association between key explanatory factors and homicide rates are averaged out in the random effects model, presented in Table 2 for comparison. Clearly, conclusions about covariate effects on homicide rates drawn from either the random effects or fixed effects approach can be misleading. I now explore some possible explanations for the differences just described between the cross-sectional and time-series components, focusing on a few key variables that exhibited the biggest disparities—unemployment rates, population size, percent male, and percent divorced.

8.2. Explaining discrepancies in between- and within-county effects

To test the notion that the above-described patterns may be attributable to differing permanent and transitory effects, results are presented from decomposition models that incorporate into the within-county specification various differenced terms, namely \((X_{t(j)} - X_{(t(j)-L)})\), where \(L\) varies in value from 1 to 7 (years). These differenced terms are intended to measure possible lasting (stock) effects of the explanatory factors.\(^{11}\) Within the context of the decomposition model, the coefficient for the differenced term can be interpreted as the effect of longer-term trends (the current year value relative to that during the previous year or up to 7 years previous) of the covariate on homicide. The opposite sign of the coefficient on the differenced term reveals the effect of the lagged factor \((X_{(t-L)})\) on homicide; a lagged term measures a possible static but delayed effect of the explanatory factors.

Within all the decomposition models with differenced or lagged terms, all explanatory variables are controlled, but their coefficients are not presented in tables since the associations with homicide do not change substantially from those shown in Table 2. Keep in mind that the construction of differenced/lagged terms necessitates the estimation of models based on shorter time periods. For example, a model with a differenced or lagged term of 1 year is based on the years 1971–1999 whereas a model with a differenced or lagged term of 7 years is based on the period 1977–1999.

Table 3 reports the results of the between-county and within-county effects of unemployment on homicide rates. Consistent with the results discussed earlier, counties with higher average levels of unemployment over the study period exhibit significantly higher rates of homicide. Within counties over time, however, higher unemployment is associated with lower homicide. Yet the differenced terms, incorporated into the within-county model to capture the relationship between the level of unemployment relative to a prior period (from 1 year up to 7 years prior) and

\(^{11}\) The coefficient for this differenced term captures the effect on homicide rates of differences in levels over time within counties \([X_{t(j)} - X_{(t(j)-L)}] - (X_{t(j)} - X_{(t(j)-L)})\].
homicide rates, are always positively associated with homicide rates (but not significantly so relative to periods of 7 years prior). Increases/decreases in unemployment rates relative to previous years that are greater than the average increase/decrease over the period are associated with rises/declines in homicide rates within counties. For example, an increase of one percentage-point in the unemployment rate relative to the previous year is associated with a 1.8% rise ($e^{0.018} - 1$) in the homicide rate within a county. Thus, this positive long-term trend effect within counties of unemployment is consistent with the positive cross-sectional (stock) association between unemployment and homicide. However, the coefficients for the within-county differenced terms are still much smaller than the between-county coefficients for unemployment. Indeed, I find that the three coefficients all differ significantly from one another in all model specifications (see Table 3). This may be due to the ways in which potential omitted variable bias affects the between- and within-county estimates differently. For example, if poverty levels are more highly correlated with unemployment rates across counties than within counties over time, the between-county estimator of unemployment may be picking up more of the poverty effect than the within-county estimator.

Researchers have offered substantive reasons to expect permanent and temporary differences, and thus variation across place versus time, in the effects of unemployment on homicide rates. However, as mentioned above, the negative between-county

---

Table 3
Results from decomposition model estimating permanent and transitory effects of unemployment rate on homicide

<table>
<thead>
<tr>
<th>Length of lag (L)</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Between-county effect</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(1) Unemployment rate</td>
<td>0.067</td>
<td>0.067</td>
<td>0.068</td>
<td>0.068</td>
<td>0.069</td>
<td>0.068</td>
<td>0.067</td>
</tr>
<tr>
<td><strong>Within-county effect</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(2) Unemployment rate</td>
<td>-0.019</td>
<td>-0.020</td>
<td>-0.021</td>
<td>-0.023</td>
<td>-0.022</td>
<td>-0.021</td>
<td>-0.019</td>
</tr>
<tr>
<td>(3) Differenced unemployment rate ($X_{t0} - X_{t0-L}$)</td>
<td>0.018</td>
<td>0.015</td>
<td>0.011</td>
<td>0.010</td>
<td>0.008</td>
<td>0.006</td>
<td>0.004</td>
</tr>
</tbody>
</table>

Significant differences in coefficients ($F$ statistic)

<p>| | | | | | | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) vs (2)</td>
<td>65.34</td>
<td>67.32</td>
<td>70.98</td>
<td>75.21</td>
<td>75.71</td>
<td>71.62</td>
<td>66.51</td>
</tr>
<tr>
<td>(1) vs (3)</td>
<td>20.15</td>
<td>24.19</td>
<td>29.69</td>
<td>32.29</td>
<td>36.20</td>
<td>37.76</td>
<td>39.48</td>
</tr>
<tr>
<td>(2) vs (3)</td>
<td>39.94</td>
<td>38.03</td>
<td>32.19</td>
<td>30.49</td>
<td>23.60</td>
<td>17.02</td>
<td>11.35</td>
</tr>
</tbody>
</table>

$-2$ Log Likelihood 8928.4 8513 7851.2 7547.1 7219.1 6951.3 6591.8

Note. Bold denotes $p < 0.05$. All other covariates controlled. The lag length refers only to the differenced term in the within-county model.

---

12 The differenced terms for unemployment with lags of more than 1 year may also pick up the impact of positive autocorrelation of the unemployment rate series for the counties.
effect of percent male and within-county effect of population size may well be due to omitted variable bias. Still, it is interesting that this omitted variable bias affects only one type of estimate—namely, the between-estimator in the case of percent male and the within-estimator in the case of population size. It is possible that these differences are related to the speed with which an effect takes place, similar to the immediate opportunity versus delayed motivation effect of unemployment. In the case of the within-county effect of population size, improvement in economic conditions, which may lead to increased migration into an area and population growth, has a fairly immediate impact on crime rates. On the other hand, the impact of social disorganization associated with growth may be delayed—increased anonymity among residents would develop gradually. In large cities, greater criminal opportunities and anonymity are already present, and population growth is unlikely to have a significant effect on these factors in the short run.

To determine whether the longer-term trend of population growth or decline within a county over time has a different association with homicide rates than short-term fluctuations in population size, a differenced term (a given year’s population size relative to that in the previous year up to 7 years prior) is added to the base model (Table 4). Although the change in population size relative to the previous 1 or 2 years is still negatively associated with homicide rates, the analysis reveals that changes in population size relative to 3 years prior up to 7 years prior are positively associated with homicide rates. In fact, the population size relative to that 7 years prior has a positive association with homicide rates that is similar in size to the between-county population size coefficient and is statistically significant at the 5% level. Moreover, while the between-county coefficient and the within-county

<table>
<thead>
<tr>
<th>Length of lag (L)</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Between-county effect</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(1) Population size</td>
<td>0.200</td>
<td>0.204</td>
<td>0.206</td>
<td>0.209</td>
<td>0.213</td>
<td>0.217</td>
<td>0.224</td>
</tr>
<tr>
<td><strong>Within-county effect</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(2) Population size</td>
<td>-0.292</td>
<td>-0.322</td>
<td>-0.337</td>
<td>-0.337</td>
<td>-0.356</td>
<td>-0.379</td>
<td>-0.402</td>
</tr>
<tr>
<td>(3) Differenced population size ($X_{t0} - X_{t-1-L}$)</td>
<td>-0.087</td>
<td>-0.019</td>
<td>0.071</td>
<td>0.092</td>
<td>0.092</td>
<td>0.095</td>
<td>0.175</td>
</tr>
<tr>
<td><strong>Significant differences in coefficients (F statistic)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(1) vs (2)</td>
<td>155.99</td>
<td>162.11</td>
<td>162.50</td>
<td>151.12</td>
<td>151.53</td>
<td>150.00</td>
<td>147.50</td>
</tr>
<tr>
<td>(1) vs (3)</td>
<td>4.25</td>
<td>4.26</td>
<td>2.15</td>
<td>1.98</td>
<td>2.47</td>
<td>2.71</td>
<td>0.45</td>
</tr>
<tr>
<td>(2) vs (3)</td>
<td>2.09</td>
<td>7.34</td>
<td>18.05</td>
<td>23.17</td>
<td>27.59</td>
<td>31.57</td>
<td>45.98</td>
</tr>
</tbody>
</table>

| −2 Log Likelihood | 8948.6 | 8533.7 | 7866.3 | 7560.4 | 7227.4 | 6955.7 | 6588.9 |

*Note.* Bold denotes p < 0.05. All other covariates controlled. The lag length refers only to the differenced term in the within-county model.
contemporaneous coefficient for population size are significantly different from one another in all models, there is no significant difference in the between-county parameter and the within-county differenced parameter for population size in any models (with the exception of models 1 and 2). The two specifications of population size in the within-county model, intended to pick up immediate versus cumulative effects of shifts in population size, also differ significantly from each other in almost all of the model specifications.

Regarding sex composition, the results from the base model indicated that variation in the percentage male within counties over time is not associated with variation in the homicide rates, but counties with a larger percentage of males in its population (the between-county estimator) exhibit lower homicide rates. A possible explanation for this pattern is that a higher share of males may indirectly capture the effect of more stable family structures on violence levels. Although we might expect contemporaneous fluctuations in the male population to have a fairly immediate impact on crime—e.g., an increase in the male population increases the number of potential criminal offenders and targets—the effect of more males on family structure would occur over the long run. In part, this conjecture is supported by the fact that when the divorce variable, one facet of family stability, is removed from the model (results not shown), the within-county coefficient for percentage male increases only slightly. However, the between-county coefficient for percentage male doubles in size, capturing some of the effect of family structure on homicide rates. In addition, analyses (Table 5) provide a little indication that there may be a distinct longer-term relationship between the percent male and homicide rate. The coefficients on the differenced measures of percent male (particularly that relative to the percent male 2 years prior)

### Table 5
Results from decomposition model estimating permanent and transitory effects of percentage male on homicide

<table>
<thead>
<tr>
<th>Length of lag (L)</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Between-county effect</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(1) Percentage male</td>
<td><strong>0.031</strong></td>
<td><strong>-0.033</strong></td>
<td><strong>-0.033</strong></td>
<td><strong>-0.033</strong></td>
<td><strong>-0.035</strong></td>
<td><strong>-0.036</strong></td>
<td><strong>-0.037</strong></td>
</tr>
<tr>
<td><strong>Within-county effect</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(2) Percentage male</td>
<td>0.025</td>
<td><strong>0.034</strong></td>
<td>0.028</td>
<td>0.032</td>
<td>0.025</td>
<td>0.028</td>
<td>0.021</td>
</tr>
<tr>
<td>(3) Differenced percentage male ((X_{tj} - X_{tj-1}))</td>
<td><strong>-0.023</strong></td>
<td><strong>-0.059</strong></td>
<td><strong>-0.026</strong></td>
<td><strong>-0.025</strong></td>
<td><strong>-0.010</strong></td>
<td><strong>-0.012</strong></td>
<td><strong>-0.003</strong></td>
</tr>
<tr>
<td><strong>Significant differences in coefficients (F statistic)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(1) vs (2)</td>
<td>7.57</td>
<td><strong>9.94</strong></td>
<td><strong>7.92</strong></td>
<td><strong>8.60</strong></td>
<td><strong>6.76</strong></td>
<td><strong>7.27</strong></td>
<td><strong>5.64</strong></td>
</tr>
<tr>
<td>(1) vs (3)</td>
<td>0.04</td>
<td>0.70</td>
<td>0.08</td>
<td>0.10</td>
<td>1.09</td>
<td>1.02</td>
<td>2.14</td>
</tr>
<tr>
<td>(2) vs (3)</td>
<td>1.33</td>
<td><strong>7.94</strong></td>
<td>3.34</td>
<td>3.92</td>
<td>1.43</td>
<td>1.83</td>
<td>0.66</td>
</tr>
</tbody>
</table>

Note. **Bold** denotes \(p < 0.05\). All other covariates controlled. The lag length refers only to the differenced term in the within-county model.
are all negative and closer in size to the between-county estimate (there is no significant difference between these two sets of coefficients) than the within-county estimates. However, this evidence is weak, given that most of the differenced terms in the within-county models are not statistically significant.

Unlike the explanatory factors just described, in the case of the percentage divorced, it is lagged, rather than differenced, levels of divorce in the within-county model that are consistent with the between-county association between the percent divorced and homicide rates (Table 6). For example, with a lag length of 7 years, the results indicate that fluctuations in the contemporaneous level of divorce have essentially no association with homicide rates. Yet fluctuations in a 7-year lagged level of divorce have a positive association with homicide rates—an increase of one-percentage point in the 7-year lagged percentage divorced results in a 2.7% rise in the homicide rate. This result suggests that the larger positive effect of divorce on crime rates is not cumulative, but simply delayed. However, note that there is no statistically significant difference between the within-county coefficients for contemporaneous as opposed to lagged divorce levels in this model. Furthermore, there remains a statistically significant difference between the within-county coefficient for lagged divorce levels and the between-county coefficient for the percentage divorced.

By definition, the coefficient for the differenced levels of divorce in the within-county model (with $L = 7$) equals $-0.027$ (the same value as that for the lagged term, but with the opposite sign). With a differenced term, the value of the coefficient for the contemporaneous level of percentage divorced within counties is $0.029$ ($=0.002 + 0.027$). Both terms are statistically significant. Expressing the

<table>
<thead>
<tr>
<th>Length of lag (L)</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Between-county effect</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(1) Percentage divorced</td>
<td>0.140</td>
<td>0.139</td>
<td>0.136</td>
<td>0.135</td>
<td>0.133</td>
<td>0.132</td>
<td>0.130</td>
</tr>
<tr>
<td><strong>Within-county effect</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(2) Percentage divorced</td>
<td>0.017</td>
<td>0.014</td>
<td>0.002</td>
<td>0.002</td>
<td>0.004</td>
<td>0.009</td>
<td>0.002</td>
</tr>
<tr>
<td>(3) Lagged percentage divorced ($X_{t-1}$)</td>
<td>$-0.013$</td>
<td>$-0.007$</td>
<td>$0.008$</td>
<td>$0.014$</td>
<td>$0.016$</td>
<td>$0.017$</td>
<td><strong>0.027</strong></td>
</tr>
<tr>
<td><strong>Significant differences in coefficients (F statistic)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(1) vs (2)</td>
<td><strong>18.69</strong></td>
<td><strong>29.84</strong></td>
<td><strong>44.56</strong></td>
<td><strong>49.77</strong></td>
<td><strong>51.75</strong></td>
<td><strong>49.16</strong></td>
<td><strong>55.02</strong></td>
</tr>
<tr>
<td>(1) vs (3)</td>
<td><strong>28.38</strong></td>
<td><strong>39.97</strong></td>
<td><strong>39.36</strong></td>
<td><strong>40.06</strong></td>
<td><strong>41.11</strong></td>
<td><strong>41.41</strong></td>
<td><strong>33.66</strong></td>
</tr>
<tr>
<td>(2) vs (3)</td>
<td>0.35</td>
<td>0.33</td>
<td>0.05</td>
<td>0.22</td>
<td>0.28</td>
<td>0.14</td>
<td>1.49</td>
</tr>
<tr>
<td>$-2$ Log Likelihood</td>
<td>8948.7</td>
<td>8533.6</td>
<td>7866.6</td>
<td>7560.6</td>
<td>7227.3</td>
<td>6955.6</td>
<td>6590.1</td>
</tr>
</tbody>
</table>

*Note. Bold denotes $p < 0.05$. All other covariates controlled. The lag length refers only to the lagged term in the within-county model.*
model in these terms suggests that there is a positive contemporaneous effect of percentage divorced on homicide rates within counties, but that the lasting pattern (cumulative effect) of divorce levels is one of a negative association with homicide rates.

9. Conclusion

A decomposition model, used to isolate the effects of key covariates on homicide rates between counties (cross-sectional effects) and within counties over time (longitudinal effects), demonstrated that with only a couple of exceptions, estimated between- and within-county effects differed significantly for each of the explanatory factors incorporated into the base model. In other words, there were many disparities in the temporal and cross-sectional components of the model. These findings are consistent with past research not only in the criminology field but more broadly within sociology and economics.

The study explored several possible explanations for these differing effects, among them that there are contrasting stock and flow effects of these covariates on homicide rates, which are captured by cross-sectional and longitudinal variation, respectively. A differenced term, which measures how trends in covariate levels are associated with homicide rates, was incorporated into the within-county model to measure possible longer-term (cumulative) effects of some explanatory factors. In a number of cases, the association between homicide rates and differences in the level of a covariate relative to previous years appeared to correspond more closely to the cross-sectional association, suggesting that there may be distinct transitory and permanent effects of factors. For unemployment, short-term conditions are what matter, not surprising given that business cycles are of fairly short duration. For population size, longer-term trends (e.g., population size relative to 7 years prior) in the within-county model are more consistent with the between-county effects, also to be expected given that the social disorganization influence of large population size would take time to have effect. In the case of the percentage divorced, fluctuations in lagged levels of divorce in the within-county model more closely corresponded to the between-county (cross-sectional) effect of divorce. The positive association between lagged divorce levels and homicide rates within counties over time increased with greater lag lengths, which is consistent with the notion that the effect of divorce on violence levels is static but would be delayed.

I propose that these two specifications, namely differenced and lagged forms of variables, may represent different aspects of the permanent (stock) relationship. In the within-county model, the lagged term tells us how fluctuations in a factor’s level sometime in the past are associated with homicide rates. The differenced term reveals how fluctuations in trends or changes in the level of an explanatory factor over time affect homicide rates. As noted above, the effects of these two on the outcome are completely opposite to one another. Although the two model specifications produce statistically equivalent results, they clearly offer different
interpretations of the ways in which factors affect homicide rates in the short and long term, as noted earlier with the percentage divorced. Take as another example the case of unemployment. The appendix displays results when lagged terms are incorporated as a possible measure of the stock effect of unemployment within counties; they tell a different story about the ways in which unemployment is associated with homicide rates within counties over time (for further discussion, see the symposium in the *Journal of Quantitative Criminology* 2001). In particular, the results suggest that there is no motivational effect to unemployment rates, since contemporaneous and lagged effects of unemployment are both typically inversely related to homicide rates. The models indicate that the opportunity effects are captured primarily by lagged unemployment rates when the lag is not long; however, the homicide rate does not appear responsive to lagged unemployment rates over longer periods (e.g., 5–7 years).

Which model specification do we believe? O’Brien (2001) argues that first and foremost, theory should determine the specification of a model. In the case where models are just identified, as here, O’Brien notes that “one can use the coefficient signs to evaluate the consistency of the results with the predictions based on each theory” (p. 366). My empirical findings indicate that in most cases, the differenced terms (capturing trends in the level of a particular explanatory variable) generally reflect expectations based on theory and more closely resemble the between-county coefficient (but not in the case of the percentage divorced). Understanding the conditions under which either the lagged or differenced term correctly picks up the lasting impact of a factor remains a question for future research. In part, the answer lies in determining the degree to which a particular specification corresponds to a theoretical expectation.

These findings notwithstanding, the contemporaneous effect on homicide rates of particular variables within counties continues to differ significantly from the effect between counties, whether or not cumulative or delayed measures of the factor have been incorporated into the within-county model. Given arguments that between and within estimates should be similar in the absence of omitted variables (e.g., Mairesse, 1990), it is important to recognize the potential role excluded factors may play in this regard. Some possible omitted variables have already been mentioned. In addition, note that region is a significant predictor of homicide rates across counties; if there are time-varying factors specific to region that are excluded from the within-county model, they may contribute to some of the observed discrepancies in the between-county and within-county associations. Within counties,

---

13 The models with lagged rather than differenced terms for population size indicate that population size has a negative contemporaneous association with homicide rates but generally no lagged effect. For percentage male, the contemporaneous and lagged values are generally positive but not statistically significant (with the exception of percentage male lagged by 2 years) (results not shown, but available upon request).

14 An alternative strategy for identifying stock or long-term effects of variables would be a distributed lag model, in which effects gradually die out or build up over the long term. Given the focus in the present analysis on the decomposition model and differences in between-unit and within-unit effects, such a model is not estimated here.
the time period dummies control for those omitted variables that have a uniform
effect on all counties over time (such as rising incarceration rates over this period).
However, such factors are not controlled in the between-county model of homicide
rates, introducing another potential source of difference between the two estimates.

A related limitation of this work is that the hypotheses about the ways in which
potential omitted variable bias may affect the between-county and within-county
estimates are tested only indirectly. If more nuanced measures of family structure
and economic well-being were included in the model, for example, we could deter-
mine directly how they affect the between-county and within-county estimates of
percentage male and population size, respectively. Despite the many advantages
to panel research designs, a drawback is the difficulty in obtaining detailed data
on a number of different variables for cross-sections over a long period of time.
I imposed fairly strict standards on the quality of data examined; with the excep-
tion of divorce levels, all variables are available annually for almost all the years
under study.

Finally, an additional caution should be mentioned about the possible effects of
interpolation procedures on the estimated time-series coefficients and on the differ-
ced terms for levels of divorce. Clearly, the interpolation will make these differenc-
es more smoothly related to each other than they would be if based on the actual
measured phenomenon. It is important to keep in mind the possible impact of mea-
surement error on the analysis.

Still, the study results provide considerable support for the notion that cross-
sectional studies tend to measure permanent effects while longitudinal work cap-
tures temporary associations. In some cases, explanatory factors may exhibit stock
and flow effects that correspond to different substantive hypotheses. In a perfect
world in which all relevant factors can be measured, some of these discrepancies
may disappear. However, when pertinent factors are omitted, it appears that
cross-sectional estimates tend to measure lasting or stock effects while time-series
estimates capture the impact of transitory fluctuations. The analysis thus demon-
strates that a common assumption—cross-sectional and temporal studies measure
the same concept—may be misguided. In future research, social scientists should
consider theoretical reasons for whether stock and flow effects are expected to be
equivalent and test them empirically by combining cross-sectional and time-series
data. Such considerations may be particularly useful in studies of income inequal-
ity, the effects of job contacts on wage outcomes, and residential mobility, a few
examples of research areas in which discrepancies in cross-sectional and longitudi-
nal studies are well documented.
## Appendix A

Results from decomposition model estimating permanent and transitory effects of unemployment rate on homicide

<table>
<thead>
<tr>
<th>Length of lag (L)</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Between-county effect</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(1) Unemployment rate</td>
<td>0.067</td>
<td>0.067</td>
<td>0.068</td>
<td>0.068</td>
<td>0.069</td>
<td>0.068</td>
<td>0.067</td>
</tr>
<tr>
<td>(2) Unemployment rate</td>
<td>-0.001</td>
<td>-0.005</td>
<td>-0.010</td>
<td>-0.012</td>
<td>-0.014</td>
<td>-0.015</td>
<td>-0.015</td>
</tr>
<tr>
<td>(3) Lagged unemployment rate ( (X_{t0-L}) )</td>
<td>-0.018</td>
<td>-0.015</td>
<td>-0.011</td>
<td>-0.010</td>
<td>-0.008</td>
<td>-0.006</td>
<td>-0.004</td>
</tr>
</tbody>
</table>

**Within-county effect**

<table>
<thead>
<tr>
<th>Significance differences in coefficients (F statistic)</th>
<th>1 vs (2)</th>
<th>1 vs (3)</th>
<th>2 vs (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) vs (2)</td>
<td>38.91</td>
<td>46.32</td>
<td>55.75</td>
</tr>
<tr>
<td>(1) vs (3)</td>
<td>59.71</td>
<td>59.36</td>
<td>57.86</td>
</tr>
<tr>
<td>(2) vs (3)</td>
<td>4.93</td>
<td>2.79</td>
<td>0.08</td>
</tr>
</tbody>
</table>

-2 Log Likelihood: 8928.4, 8513, 7851.2, 7547.1, 7219.1, 6951.3, 6591.8

*Note.* Bold denotes \( p < 0.05 \). All other covariates controlled. The lag length refers only to the lagged term in the within-county model.
References

Health Statistics, Hyattsville, MD [producer], 1993. Inter-university Consortium for Political and Social Research, Ann Arbor, MI [distributor].

