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Guest Editorial: Methodological Issues in Longitudinal Analyses of Criminal Violence

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Guest Editorial: Methodological Issues in Longitudinal Analyses of Criminal Violence

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This guest editorial introduces the Focus Section on Methodological Issues in Longitudinal Analyses of Criminal Violence. Longitudinal designs offer distinctive advantages for purposes of making causal inferences with observational data, but significant challenges must be confronted as well. This editorial highlights some of the more important methodological issues that arise, describes in general terms selected approaches for dealing with them, and indicates how the papers included in this focus section skilfully apply methodological techniques for longitudinal analyses to address substantively important issues pertaining to criminal violence.

1. Background

Much of the quantitative research conducted by criminologists is still based on data collected from regional units (such as districts or nation-states) at a single point in time. Assumed structural relationships between dependent and independent (predictor) variables are regularly specified in single-equation regression models, the parameters of which are estimated with Ordinary-Least-Squares (OLS) techniques. The impact of each of the predictor variables is indicated by its slope coefficient and its standard error. The quality of these estimates depends (particularly but not exclusively) on certain characteristics of the "errors", of the differences between the observed and the "expected" values predicted for each case on the basis of the values they have on the independent variables (impact factors). The most emphasized assumptions are that the errors should be normally distributed (or nearly so) around an expected value of zero with constant (homoscedastic) variance, and that they should not be correlated with each other or with any of the predictor variables. The last assumption is violated if impact factors not included in the model not only correlate with the dependent variable but also with one or more of the predictor variables actually included in the model (the problem of "omitted-variable bias").

Another assumption underlying causal inferences drawn from such models is often overlooked: At the time of measurement, the data should be in a state of equilibrium, in other words any more or less recent change in the predictor variable *X* should have completely unfolded its impact on the dependent variable *Y* at the time of measurement. If this is not the case and the speed of the dis- and re-equilibration process covaries with the level of *X* (for example, cases with higher *X*-values may adjust more quickly than cases with lower *X*-values), the estimated slope coefficient will be distorted (if there is no covariation with *X* only the intercept will be distorted).

Time-series data (in principle) allow the researcher to uncover the dynamics of the causal processes and discriminate between short-term change effects and long-term level effects. However, one might also be interested in the impact of variables which are quite stable over time (within the period of observation) but may vary considerably between individual or regional units of analysis. Furthermore, "process" effects may differ from "structural" effects attributed to the (seemingly) same variable. For example, in Germany during the last decades of the nineteenth century rapid social change (in terms of increasing urbanization and industrialization) was accompanied by, and apparently spawned, a strong increase in the rate of aggravated assault and battery across the roughly one thousand rural and urban districts. This can be explained in terms of Durkheimian assumptions concerning the anomic consequences inherent in rapid social change. This change, however, led to new social-structural arrangements and cultural features (the erosion of "collectivism"), which induced a *decrease* in violent crime. Cross-sectional comparisons between rural and urban districts in the German Empire reveal an interesting difference: already during this period of rapid change the average assault rate in large cities (though also rising) remained considerably below the rate in rural areas. In other words, the effects of "urbanization" differed from those of "urbanity" (cf. Thome 2010).[1]

In a similar vein Phillips (2006) differentiates (transitory) "flow" and (lasting) "stock" effects; the first are triggered by fluctuating changes noticeable over time, the second are more readily observed via time-stable variations across the units of analysis.[2] So, for example, a rise in unemployment may have an immediately negative effect on certain crime rates (like that of burglary), because more people stay at home and thereby decrease the opportunity for that type of crime. On the other hand, if people stay unemployed for longer periods of time they might become more motivated to commit criminal acts themselves.

Pooling cross-sectional and time-series data generally expands the possibilities for examining broader ranges of theoretically interesting impact factors and causal dynamics. This benefit, however, comes at the cost of increased data heterogeneity, making it more difficult to determine unbiased and efficient parameter estimates. It may become a rather challenging task to develop a model design which adequately balances the claims of substantive theory and the requirements of sound statistical analysis. While numerous models are to be found in the literature, none of them counts as "the best" under all circumstances, and quite often not even within the specific circumstances encountered in a well-defined research project. In a guest editorial we cannot present a detailed overview on such models; instead we will outline only some of the main alternatives that are considered or applied in the articles included in our focus section. Here and in the extant literature, much discussion is devoted to the respective merits and deficiencies of "fixed-effects" versus "random-effects" modelling strategies as they depend on characteristics of the data and on the kind of substantive hypotheses to be addressed. In the following paragraphs we describe some of the major alternatives and characteristic features that are involved in these modelling strategies.

Whereas in purely cross-sectional analyses we have a data set in which all the N cases (individuals, organizations, regional units etc.) are ordered row by row with their variable values given column by column, a Time-Series Cross-Sectional (TSCS)[3] data set consists of NxT cases, where each unit i ($i = 1, \ldots, N$) displays its T ($t = 1, \ldots, T$) time-specific values of all the variables measured successively row by row. The error assumptions underlying OLS regression analysis with purely cross-sectionally distributed data (T=1) are regularly violated by the pooled-data set to an extent that exceeds the limits set by the "robustness" assumption often applied in justifying OLS estimation techniques even in the case of "minor" departures from the regular error assumptions.

Nevertheless, for heuristic purposes one may start with a "completely pooled" model (as in the paper by Raffalovich and Chung included in this focus section) in which all the NxT cases are combined into one homogenous data matrix without making any structural distinctions with respect to

1 This observation was confirmed by extended regression analyses including additional indicators of the relative weight of collectivism vs. individualism. Instead of being overcome after a while, "anomie" might become "chronic" (Durkheim) or "institutionalized" (Messner and Rosenfeld 2013), in the sense of turning into a structural (besides a temporal) property of a social system. In the second half of the twentieth century the structural properties of individualism may also have been evolving towards strengthening its "disintegrative" (and therefore criminogenic) components over its "cooperative" components (Messner et al. 2008; Thome and Stahlschmidt 2013).

2 One may, however, encounter time-specific changes in the level of certain impact factors which affect all cross-sectional units in the same way, such as changes in prevention and incapacitation policies introduced by legislation in a centralized state and invariantly implemented across its regional units.

3 The TSCS label is often used to refer to pooled data for which $T > N$ or N not much larger than T. The label "Cross-Sectional Time-Series" analysis accordingly refers to a data set with N considerably larger than T (also referred to as "Panel Analysis"). But this terminology is not uniformly applied in this way.

cross-section or time dependencies. Thus, the measurements assigned to the *i*-th unit at time *t* count as measurements of one specific case drawn independently from the measurements of any *other* case constituted by the same unit at time $t \pm j$ or, equally, by another unit $i \pm n$ and time point *t*, and so forth. Under this assumption of time and cross-sectional independence one may also assume that the errors are not auto-correlated over time or space and that they have equal variance over all cases (homoscedasticity). On this basis an OLS regression model could be estimated in the same way as an OLS regression with purely cross-sectionally varied data. These assumptions, however, are empirically unrealistic. Even though there might be no "spatial" correlation across units (at the same or over different time points), the over-time measurements of any given unit will regularly be auto-correlated. It is also more realistic to assume that the error-variances and co-variances are not the same across all units (heteroscedasticity).

More realistic assumptions are introduced by the "Kmenta" pooling model (Kmenta 1986), which allows for unit-specific error-variances, errors correlated over time (auto-correlation), and "contemporaneous correlation" between errors of different units at the same time. The coefficients of such a model are to be determined by Estimated (Feasible) Generalized Least Squares (EGLS, FGLS) procedures. A major restriction of this model is the assumption that the vector of parameters to be estimated should be constant for all units at all points of time, including the intercept. The last component in this restriction (referring to the intercept), in particular, is often quite unrealistic. In many (probably most) cases, criminologists have to deal with (regional) units which across all time-specific measurements exhibit sizable and persistent level differences in the dependent variable (like assault or homicide rates), which cannot be explained by the predictor variables, because they are produced by "omitted" (unknown or unavailable) impact factors not included in the model. These level differences might be (and often are) correlated with the included predictor variables. In such cases, the base level (common intercept) and, more importantly, the slope coefficients estimated by EGLS according to the Kmenta or similar models would be largely distorted (Hsiao 1986; Stimson 1985, 919–21).

One approach to deal with this problem is the so-called "fixed-effects" modelling design. Here the time-invariant level differences not accounted for by the predictor variables are represented in the regression model by unit-specific intercepts.[4] They can be calculated as the slope coefficients of *N* dummy variables[5] D_{jt} additionally introduced into the regression equation (Least Squares Dummy Variable [LSDV] models). Each is coded with the value of "1" for each point of time for a specific unit $j = i$ and the value of "0" for all the other units $j \neq i$, where *i* runs from 1 (the first unit) to *N* (the last unit).[6] If *N* is large, it is recommended not to use dummy variables but to transform the dependent and all the independent variables by subtracting the observed values from their respective means calculated separately for each unit over all the time-specific measures available (for a detailed description see Alecke 1995, 11–15; for an application see the contribution by Entorf and Sieger in this focus section). With this transformation it becomes even more obvious that in *fixed-effects* modelling the estimation of the slope coefficients is based exclusively on the *within*-variation given for each unit over time. The *between*-variation across the units gets neutralized, levelled off, not used in the estimation of the slope coefficients (usually assumed not to vary over time and units).[7] This has the advantage of eliminating or

4 The model might be extended by the inclusion of time-specific intercepts (equal for all units) representing, for example, seasonal or business-cycle effects not captured by the predictor variables. See the contribution by Raffalovich and Chung, who elaborate such a model extension, including tests to check its appropriateness.

5 Or with *N*-1 dummies if one wants to have a reference unit (a "common" intercept) with zero values on all the dummy-variables included in the equation.

6 The equation can also be expanded by including lagged dependent variables on the right-hand side (thus presumably reducing serial correlation in the errors), and it can be modified by using first differences (for example, $\Delta X = X_t - X_{t-1}$) in order to deal with non-stationarity (cf. Beck and Katz 1996, 2011). However, adding lagged dependent variables may induce endogeneity bias and is particularly problematic with small T (cf. Nickell 1981).

7 LSDV models can not only be extended to include time-specific effects, but also transformed, for example, into Seemingly Unrelated Regression Equations (SURE models) which take into account slope coefficients that vary over units and allow for "contemporaneous correlation" between time-specific errors across individual units (cf. Alecke 1995, 24–28). Model designs and testing procedures that help to take into account such additional variants of structural effects are presented by Raffalovich and Chung in this focus section.

reducing the heterogeneity bias rooted in omitted variables that vary across units.[8] But this gain comes with a loss of estimation efficiency due to the reduction of variance in the explanatory variables included in the model. In addition, the impact of factors that do not vary over time cannot be estimated at all. To overcome these deficiencies Plümper and Troeger (2007) have proposed a three-stage "fixed effects vector decomposition" (FEVD) model which allows for retention of some of the between-unit variation in order to permit the estimation of effects attributable to time-invariant variables and a more efficient estimation of the effects attributed to "almost" time-invariant variables. This appears to be a rather attractive modelling strategy preserving the bias-reducing features of *fixed-effects* modelling while reducing the loss of efficiency by recovering some of the between-unit variance. However, the FEVD modelling strategy has received some rather critical comments as well (see Bell and Jones 2015; Breusch et al. 2011; and the replies in Plümper and Tröger 2011), and there are other versions of decomposition models which are either interpreted within the framework of *fixed-effects* or of so-called *random-effects* (RE) modelling (cf. Bell and Jones 2015); one of them is applied by Thames and McCall in their contribution in this focus-section. With regard to RE modelling (also referred to as Error Components [EC] modelling), we will not get into details here but point out at least some of its basic characteristics.

In the RE approach, unit- and/or time-specific effects which stem from sources outside the predictor variables actually included in the regression model are not "fixed" into unit and time-specific intercepts but treated as components of the error structure; that is, they are treated as random variables with mean zero and constant variance. The total error in the RE model thus has three components: "error systematic to space (cross-section), error systematic to time, and error systematic to both" (Sayrs 1989, 33). These three components have to be disentangled (under various assumptions) so that their systematic (but not fixed) effects can be combined into a single vector of slope coefficients. The partitioning of the error covariance matrix rests on the assumption that unit effects are captured as serial correlations that are constant at all lags over time. This in turn requires the restriction that the covariates X and the unit effects are uncorrelated and that there is neither spatial nor time-serial autocorrelation that would confound the constant serial correlation indicative of the unit effects (Stimson 1985, 924–25). There are several strategies to check for violations of assumptions and, if need be, to modify or expand the model in such a way as to allow for spatial and serial autocorrelation. So, for example, ARMA variations of the GLS model have been proposed to allow for serial autocorrelation (Stimson 1985, 925–29, 938–45; Sayrs 1989, 36–39). Whatever the specific characteristics of the applied models are, the components of the overall error matrix have to be identified in several steps, and the data (the values of the dependent and the independent variables) have to be transformed accordingly.

In a final step the required EGLS estimates are provided by an OLS regression performed on the transformed data. A weighting factor used in this final transformation reflects the relative size of the within- and between-error variances disentangled and estimated in previous steps. The final OLS estimates are thus a weighted average of the previously calculated within- and between-estimates. The larger the T, the more weight is given to the within estimates. In the limiting case of $T \to \infty$ the estimated regression coefficients of the fixed-effects LSDV model coincide with those of the EC model. Generally, the larger the T and the smaller the N, the more the advantages of FE modelling (minimizing bias) come to bear (Beck and Katz 1996, 4, fn. 7). On the other hand, with larger N and smaller T the efficiency gains achieved by EC *(random coefficient)* estimation become more paramount. But one should always keep in mind that the EC estimates (unlike the LSDV esti-

8 We know from ordinary cross-sectional regression analysis that the effect estimates are biased if omitted impact factors are correlated with included predictor variables. So it seems obvious that "to the extent that there are omitted characteristics that vary over time [in addition to those that vary only across units], the within-unit estimators will also be biased" (Phillips 2006, 952). Phillips and Greenberg (2008, 54, fn. 3) also note that if there are a small number of waves, "the fixed effects estimates are not necessarily unbiased no matter how many cases the researcher has. Random effects estimates, on the other hand, are consistent as the number of cases increases without limit, regardless oh how many observation times there are in the panel."

mates) are biased if the unit effects correlate with the predictor variables. The null-hypothesis of no correlation can be checked, for example, by the Hausman Test (Greene 1993, 479–80). The result of this test might confront the researcher with a difficult choice: either to maximize efficiency or to minimize bias. Much more discussion would be needed here, and we can only briefly draw attention to core issues. Bell and Jones, for example, note that "the Hausman test is not a test of FE versus RE; it is a test of the similarity of within and between effects" (Bell and Jones 2015, 144). They also "see the FE model as a constrained form of the RE model, meaning that the latter can encompass the former but not vice versa" (143). Beck and Katz (2007) strongly recommend considering the possibility of unit-to-unit variation in the model parameters, in other words the application of *random-coefficient* models (RCM) whenever a TSCS pooling format is given. And they present evidence from Monte-Carlo simulation studies demonstrating that in such cases Maximum-Likelihood estimation methods perform better than FGLS techniques.

2. Applications in the Focus Section

The initial paper by Thome ("Cointegration and Error Correction Modelling in Time-Series Analysis: A Brief Introduction") provides an introduction to cointegration and error-correction modelling in time-series analyses. The overarching substantive issues under investigation are how to distinguish between deterministic and stochastic trend-components, and how to avoid the associated dangers of spurious regression or spurious non-causality. The paper outlines some of the basic features and practical steps of cointegration modelling as a strategy for dealing with these issues, and illustrates this strategy with data on U.S. homicide rates and divorce rates, and with German data on sentencing and imprisonment.

In "Models for Pooled Time-Series Cross-Section Data," Raffalovich and Chung explain how modelling strategies for pooled data sets can also be conceptualized within the framework of *Multilevel/Hierarchical Linear Modelling* approaches. The authors use this analytic framework to develop a step-by-step testing strategy for identifying theoretically interpretable heterogeneities inherent in their pooled data set comprising $N = 40$ nations and $T = 56$ yearly measurements of homicide rates (dependent variable), divorce rates, and per-capita income (independent variables) between 1950 and 2005. They start with a "completely pooled" model implying that all countries over all time-points are identical in all unmeasured respects (perfect homogeneity given in the matrix of $NT = 2{,}240$ cases). They then test successively for country-specific, time-specific and time/country-specific effects, and finally for the possibility that the slope coefficients to be estimated for each of the predictor variables may vary across time and/or across countries. They apply log-likelihood ratio tests and FGLS estimation methods. Since the time-series are non-stationary they use first differences (yearly changes) for each variable. They also include the lagged dependent variable on the right-hand side of the regression equations. Raffalovich and Chung use this variable to control for time-dependencies but abstain from theoretical interpretations concerning the sign and magnitude of the respective coefficients.[9] They conclude with observations about how the models under consideration may help mitigate threats to validity that commonly arise in pooled time-series cross-section data analysis.

The general topic addressed by Thome – testing and modelling the over-time dynamics of structural relationships in a TSCS setting – is also the focal concern in Christoph Birkel's paper, "The Analysis of Non-Stationary Pooled Time Series Cross-Section Data".[10] If the time-series data for two or more variables exhibit trend components (non-stationarity) these variables will correlate even if they are causally unrelated. A common device to avoid such "spurious causality" (or "spurious regression") is to transform these time-series into their first (or higher-order) differences. This may however produce another problem: "spurious non-causality", where two trending series may be structurally related in the long run, but not in their

9 For a detailed discussion of the use of lagged dependent variables to model effect dynamics see Beck and Katz (2011).

10 Readers not familiar with the concepts of non-stationary, unit-root processes, cointegration and error-correction models are referred to the introductory paper by Helmut Thome, which has been included here to facilitate access to Birkel's contribution

short-term movements extracted by differencing. On the other hand, two variables may be structurally related in their short-term movements, but not with regard to their long-term level relationship (as exemplified by the series analysed by Raffalovich and Chung, and also by Thome). There are several testing and modelling strategies that help the researcher not to fall victim, one way or the other, to the spuriousness trap. There are various forms of unit-root tests to check for the presence of *stochastic* (instead of deterministic) trends (*integrated* processes) in a set of time-series data, and also to check for so-called "cointegration", in the sense of corresponding (causally related) stochastic trend movements across two or more time-series. If the hypothesis of cointegration has been confirmed we can estimate not only the long-term level relationship between a predictor and the dependent variable, but also the parameters identifying the time-path of the "re-equilibration" process leading to the final level change ("error-correction models"). Birkel gives a detailed overview on various testing and modelling strategies, whose applicability and adequacy in each case depend on the substantive questions to be pursued and on given characteristics of the pooled data set. These characteristics include: the size of the sample (the number of units and time-points), cross-section dependencies, level-shifts and structural breaks caused by external events, and the degree of homogeneity assumed for residual variances and covariances and for short- and long-run parameters (e.g., the short-run dynamics may differ across units, but the long-run effects might still be homogeneous). How this array of pertinent or less pertinent modelling and estimation strategies can be evaluated and put to use in practical research, and the often uncertain and risky decisions that have to be made in this context, are exemplified in Birkel's analysis of a pooled set of time-series data (year by year from 1971 to 2004) for the eleven West German federal states. Trending robbery rates are the dependent variable; the predictor variables include per-capita income, per-capita consumption, and clearance rates (as well as demographic control variables). Birkel concludes that the available methodological procedures perform reasonably well with sufficient sample size, but notes that this qualification can create difficulties in practical situations, and points to areas where future development is needed.

As we have already mentioned, conventional RE (error-composition) models derive common slope coefficients from weighted averages of within- and between-variance components. But the framework of RE modelling has also been used to construct "decomposition" models (in the literature also referred to as "hybrid" models) which disentangle within- and between-effect estimates, thus providing two sets of slope coefficients (Phillips 2006, Bell and Jones 2015). Such models help the researcher to gather empirical evidence that may support or refute substantive hypotheses regarding different modes of causal dynamics, such as those briefly indicated at the beginning of our editorial: temporary process effects versus lasting structural effects, flow versus stock effects. Such distinctions may also be conceptualized within the framework of multi-level analysis (Bell and Jones 2015): as context effects (possibly attributed to the regional units) versus individual effects resulting from the over-time variations within these contexts – or the other way round. Which of the two, cross-sectional units or time-points, should be assigned to the "higher" or "lower" level depends upon the specific hypotheses to be examined and the relative size of *N* and *T*. In "A Longitudinal Examination of the Effects of Social Support on Homicide Across European Regions", Thames and McCall apply such decomposition models to examine the impact that "social support" and other predictor variables (relative deprivation and unemployment plus demographic control variables) exert upon homicide rates. Their study examines these structural relationships across 197 Western European and (separately) 50 Eastern European regions at three time points: 2000, 2005, and 2009. The results of their analyses offer reasonably robust evidence in support of social support, thereby complementing and extending prior work based on cross-sectional data.

In criminological research predictor variables (like GNP per capita or unemployment rates) are usually treated as *exogenous* variables that impact some dependent variable (like assault or homicide rates). But various types of *endogeneity* may also be involved in such overall causal structures. In "Does the Magnitude of the Link between Unemployment and Crime Depend on the Crime Level? A Quantile Regression Approach", Entorf and Sieger consider, for example, the possibility that the effect of unemployment on various types of crime depends on the level of crime given in a regional environment. They refer to opportunity theory, which suggests that “those who become unemployed in a low-crime area have higher incentives to commit a crime than those in high-crime regions, because they would face less effective prevention of potential victims and lower competition from other criminals than those in high-crime areas”. On the other hand they note that the “stigma-based hypothesis … predicts low marginal effects … in low-crime areas, because here any potential detection bears a higher risk of stigma than in regions where criminal behaviour is more common”. They examine these opposing hypotheses by applying a “quantile regression” approach, rarely used so far in criminological research. This modelling strategy allows estimation of different sets of regression coefficients depending on pre-defined quantile (percentile) levels of the dependent variable. The authors base their study on a pooled data set with yearly measurements from 2005 to 2009 gathered from 301 rural districts and 111 urban municipalities in Germany. They apply the conventional mean regression approach and compare its results with the findings from quantile regressions specified for the 5-, 25-, 50-, 75-, and 95-percent quantiles of their dependent variables (burglary, car theft, assault rates). Their main focus is on the effect of unemployment rates, but they also include other variables (like household income and clearance rate) among their predictors. The results obtained by these different approaches confirm that “conventional mean regressions might produce misleading results”.

3. Outlook

The papers in this focus section underscore the promise of longitudinal analyses in research on criminal violence. Incorporating time into the design of studies can provide unique forms of leverage to facilitate inferences about causal processes. Moreover, the methodological foundations for longitudinal research have developed dramatically over recent decades, as reflected in the increasingly sophisticated approaches to statistical modelling. At the same time, debates are ongoing about the relative benefits and costs of various strategies, and there are often no easy solutions to some of the more difficult challenges. We hope that this focus section will stimulate further interest in longitudinal analyses of criminal violence and in the development of methodologies to advance such analyses.

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