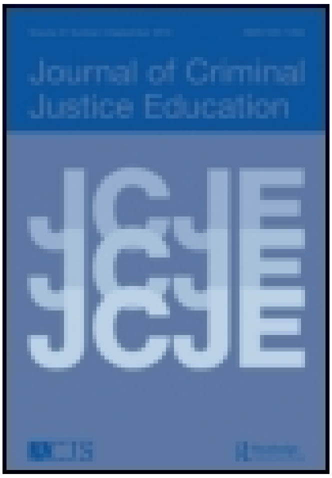

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A User-Friendly Introduction to Panel Data Modeling

John L. Worrall

رگرسیون کمترین توان های دوم عادی

This paper offers a brief user-friendly introduction to panel data modeling in criminal justice and criminology. The author assumes that the reader is familiar with ordinary least squares regression. Topics covered include the problems of pooling data over time and space, model selection, and estimation issues in a fixed-effects context. The paper concludes with a look at some extensions that help researchers model and/or account for simultaneity, heterogeneous year effects, unit-specific effects, and events.

Ordinary least squares regression is not equipped to deal with repeated observations on the same units of analysis. Problems arise when researchers pool their data across both time and space. One solution to such problems is the estimation of panel data models. "Panel data" refer, simply, to observations on same units (e.g., individuals, organizations, cities, states) collected at multiple points in time. Most panel data are compiled in yearly increments (see, e.g., Evans and Owens 2007; Marvell and Moody 1996; Zhao, Scheider, and Thurman 2002), which is likely due to availability, but there is no requirement that data should be observed in yearly increments.

It would be convenient if statisticians and econometricians used the same terminology across disciplines, especially in the panel data context. In political science, researchers often refer to "time series-cross section" (TSCS) models. In other disciplines, and even in criminology and criminal justice, "multiple time series" (MTS) refers to multiple observations on the same units. Some have argued that TSCS data contain many observations on relatively few units (Beck and Katz 1995); yet, researchers have used the language of TSCS—and MTS—when analyzing data that could just as easily be described as being of the panel variety (e.g., Kovandzic and Sloan 2002). No matter the choice of terminology, many of the same issues arise when the sample consists of the same units observed over more than one time period (Kristensen and Wawro 2003).

Despite the problems of pooling (discussed in detail shortly), there are significant advantages to it. By collecting data on the same units over time, researchers can increase their sample sizes by increasing the number of time

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periods. This makes it feasible, for example, to conduct state-level analysis. That there are 50 states makes statistical estimation practically impossible, but if, say, 10 years of data are collected, that sample of 50 increases to 500. Not only does the sample size increase but also researchers are afforded an opportunity to control for unobserved (and, perhaps, inconceivable) time-stable factors associated with each unit of analysis. In county-level research, for example, a case can easily be made that county A is fundamentally different from county B. Panel data models permit researchers to effectively control for such differences.

The Problems

Three problems arise when data are pooled over time and space. In the typical cross-sectional dataset, statistical estimation does not raise autocorrelation (or serial dependence) concerns. Indeed, autocorrelation is impossible if data are collected at one point in time. In the panel data context, however, because a time dimension is included, researchers are all but forced to address the possibility that data in time period A are correlated with data in time period B. This is normal and expected, especially in research on aggregate units. Large cities, for example, tend to stay large over a long period of time. The same holds at the individual-level. We know that people exhibit characteristics that are relatively constant over time. A tall adult stays relatively tall throughout his or her lifespan.

Ordinary least squares assumes that errors are homoskedastic, that is, constant across units. When this assumption is not met, the result, of course, is heteroskedasticity. When heteroskedasticity is present, researchers need to take steps to control for it. But when data are pooled across space *and* time, heteroskedasticity problems, if they exist, become compounded. When a time dimension is added to the data, the effects of heteroskedasticity increase in accordance with the number of time periods in the data (Stimson 1985, p. 919). The more time periods there are, the more problems heteroskedasticity can pose.

The third problem that arises in the panel data context is heterogeneity. This occurs when all units are affected by a "shock" during the same time period. In a state-level panel data analysis, an example of one such shock could be a presidential election. As a presidential election would affect all states to some extent, the errors across each state will be correlated due to the event each experienced. Such a tendency would be largely undetectable in a cross-sectional dataset. So, while panel data are advantageous from a sample size and "control" standpoint, there are clearly some issues that need to be addressed.

Model Selection

At the risk of simplification, there are two main choices for panel data modeling: fixed effects estimation and random effects estimation. There are *many* other modeling choices, and new models for panel data modeling continue to be developed, but most bear a fair degree of resemblance to these two main techniques. What is the difference between these? In order to answer this question, let us first consider the basic cross-sectional ordinary least squares (OLS) model. In matrix notation, it is:

$$y = \alpha + \beta x + \varepsilon \tag{1}$$

The variable *y* in Equation 1 is, of course, the dependent variable, variable α is the intercept, variable *x* is a vector of independent variables, variable β represents the regression coefficients, and variable ε is the error term. When the data are pooled over time and space, Equation 1 extends to something like this:

$$y_{i,t} = \alpha + \beta x_{i,t} + \varepsilon_{i,t} \tag{2}$$

Note the addition of the *i* and *t* subscripts in Equation 2. These denote units and time periods, respectively. Also, note that there is nothing different between Equations 1 and 2, other than the fact that Equation 2 shows that there are repeated observations on the same units. Finally, although Equation 2 is not a model that we want to estimate, it is sometimes called the "constant coefficients model." It assumes that the regression coefficients are constant across units and time periods. In fact, Equation 2 makes no attempt to explicitly model the repeated observations. Fixed effects and random effects models do just this and address some of the problems associated with estimating the constant coefficients model via OLS.

The fixed effects model, again in matrix notation, looks like this:

$$y_{i,t} = \alpha_i + \delta_t + \beta x_{i,t} + \varepsilon_{i,t} \tag{3}$$

Note that Equation 3 is basically the same as Equation 2, with several key exceptions. First, Equation 3 adds separate intercepts for each unit. This is denoted by α_i. Second, it adds δ_t, which denotes a dummy variable for each time period. We will look later at the logic for adding these unit and time dummies. For now, though, contrast Equation 3 with the random effects model, which typically looks like this:

$$y_{i,t} = \alpha + \beta x_{i,t} + u_i + w_t + \varepsilon_{i,t} \tag{4}$$

Also called an "error components model," Equation 4 is different from Equation 3 in that Equation 4 adds u_i and w_t, each of which denotes a separate error term, the former for the units analyzed and the latter for the time periods. Equation 4 assumes that the unobserved differences between units and time periods are random variables. In contrast, the fixed effects model assumes that they are fixed. In one sense, it is easier to wrap one's head around Equation 3 because the unit and time period effects are modeled explicitly.

Choosing between Random and Fixed Effects Models

What considerations go into choosing between the random and fixed effects models? There are three of them: (1) the size of *N* and *T*; (2) whether there is correlation between the error term and the unobserved variables; and (3) whether predictors vary over time. For those who do not feel comfortable in choosing between either approach on the basis of these considerations, there is a test that helps researchers decide between one approach and the other. It will be considered, too. First, though, let us look at each of the three main considerations in more depth.

Size of *N* and *T*

Fixed effects estimation in large samples can be unwieldy and sacrifice precious degrees of freedom. Consider a sample of 1000 cities observed in yearly increments for a full decade. To model the α_i in Equation 3, the researcher would need to add 999 additional parameters to the model, and this is just to model the city effects. Some are not comfortable in sacrificing degrees of freedom in this fashion and, as such, opt for random effects estimation.

Related to the problem of unwieldy models is the problem of complicated interpretation. The coefficients for the unit and time dummies are themselves rarely interesting from an interpretation standpoint, but for those who are interested, making sense of the relationships can be complicated. And there is another concern:

> [Fixed effects estimation…] removes any of the average unit-to-unit variation from the analysis. The introduction of fixed effects for each unit, for example, simply asks whether intraunit changes in some dependent variable are associated with intraunit changes in one or more independent variables. In other words, fixed effects estimation ignores the possibility that unit-to-unit variation sheds light on the relationship between *x* and *y*. (Worrall 2008a, p. 235)

Correlation between the Error Term and Unobserved Variables

Random effects estimation is more efficient, which would seem to make it a logical choice. However, a key assumption underlying the random effects approach is zero correlation between the error term and predictor variables. More specifically, the predictor variables should be correlated with the unobserved unit-specific error terms (the u_i in Equation 4).

A related assumption underlying the random effects approach is that the errors are constant across time (see the w_t in Equation 4). This assumption is difficult to meet in most real-world situations (Berk 2004, pp. 178–180). Why, for example, would some unmeasured event have a constant effect across all time periods observed?

Time Invariant Predictors

In choosing between random and fixed effects estimation, it is also important to consider whether predictor variables vary over time. If a predictor does not vary over time, it will be perfectly collinear with the unit dummies in a fixed effects setting. As an example of such a problematic predictor, one may wish to denote whether a particular jurisdiction is conservative or liberal with a "1" or a "0." This approach should always be avoided in fixed effects estimation.

In a similar vein, if one wishes to estimate the effect of an event that affected every unit at the same point in time (say, e.g., the 9/11 terrorist attacks), then such a variable will be perfectly collinear with time dummy variables (Allison 1994). It is still possible, in some situations, to estimate the effects of events in a fixed effects context, but we will turn attention to that later.

Finally, predictors that change slowly over time are also problematic. As Beck (2001, p. 285) observed, "Although we can estimate [a model] with slowly changing independent variables, the fixed effect will soak up most of the explanatory power of these slowly changing variables. Thus, if a variable … changes over time, but slowly, the fixed effects will make it hard for such variables to appear either substantively or statistically significant." Plumper and Troeger (2007) pioneered a fixed effects vector composition procedure to deal with this, so it is a problem that can be overcome (see Worrall 2008b, for an application).

The Hausman Test

There is also a statistical test for choosing between random and fixed effects estimation: the so-called Hausman test (Hausman 1978). It is a test of the null hypothesis that random effects would be consistent and efficient against the alternative hypothesis that random effects would be inconsistent. The key question underlying the test is whether there is significant correlation between the unobserved unit-specific random effects and the predictors. If there is no correlation, then it may be desirable to opt for the random effects approach.

The test statistic is as follows: $[(\beta_{FE} - \beta_{RE})/(s^2{}_{\beta FE} - s^2{}_{\beta RE})]$. The β_{FE} are the fixed effects model coefficients, the β_{RE} are the random effects model coefficients, and the $s^2{}_{\beta FE}$ and $s^2{}_{\beta RE}$ are the variances of the fixed and random effects model coefficients, respectively. The Hausman statistic has a chi-square distribution with as many degrees of freedom as there are predictors in the model. If the p-value is insignificant (i.e., greater than 0.05), this means it is probably safe to use random effects estimation. Fixed effects should be used, however, if the statistic is significant. The Hausman test offers somewhat an objective take on whether one should use random or fixed effects estimation. Yet, researchers should not put total faith in it. They should also consider the issues discussed in the preceding subsections.

Estimation Issues

In the interest of keeping it simple, the remainder of this paper will be limited to fixed effects modeling. Also, it is this author's observation that criminologists and criminal justice researchers tend to opt more frequently for fixed rather than random effects estimation (see, e.g., Worrall 2008b, p. 789). Finally, it is also worth focusing on fixed effects because of how unlikely it is for the random effects error terms to be uncorrelated with the model's predictors.

A researcher's work is by no means done once he or she selects fixed in lieu of random effects estimation. There are at least five key estimation issues that deserve consideration. They are: (1) heterogeneity; (2) dynamics; (3) panel heteroskedasticity; (4) stationarity; and (5) trends. More often than not, several of these issues present themselves in the same research context. Moreover, this list is partial and certainly does not cover *all* problems that a researcher could encounter.

Heterogeneity

In the panel data context, "heterogeneity" refers to differences in the units of analysis. If a researcher has a panel dataset on, say, 1,000 cities, all of different sizes, then common sense suggests that most, if not all, of the cities will differ from one another in key respects. Cherry (1999) offers this illustration:

> "Suppose there are two criminal justice departments. Department A follows strict accounting practices that report high percentages of crimes, while department B is more lenient and reports lower percentages. Noting that certainty of sanctions is typically measured by the clearance rate, this disparity causes a problem when the reported data from the two jurisdictions are analyzed. (p. 754)

The same issue presents itself in a cross-sectional context, but researchers, not having access to data collected across some span of time, cannot do much about it. With the use of unit-specific dummy variables in a fixed effects context, however, researchers can control for unobserved differences between each city. They can do the same with time periods. If data are collected in yearly increments, then it is common to include year dummies to capture time effects.

Why not just enter year and unit dummies as a default condition? Degrees of freedom are lost with either approach, so it may prove helpful to test whether unit and/or year dummies are even needed. One common approach, particularly in terms of unit dummies, which tend to gobble up many degrees of freedom, is to conduct a simple *F*-test. If the *F*-test for all the unit dummies is significant, they should be modeled. It bears mentioning, though, that it is not difficult to achieve significance with such a test. It may be less likely to obtain a significant statistic when conducting an *F*-test on the year dummies.

Researchers often *assume* that both unit and year dummies are necessary in fixed effects estimation. More often than not, it is advisable that they should be included. However, researchers should do their homework and make sure that either of them is necessary.

Dynamics

A more pressing issue in the panel data context, and particularly in terms of fixed effects estimation, is that of dynamics. Panel data are rarely independent across the time dimension, as one unit's value during the current time period is probably quite correlated with its value on the same variable in a previous time period (Hanushek and Jacson 1977; Maddala 1992). Worrall and Pratt (2004 offered this example:

> It is commonly understood that when public agencies do not spend their budgetary allotments for a particular year, their budgets can be reduced in subsequent years. Thus, the very nature of the public budgeting process ensures that an agency's budget for one year is highly associated with its budget for the previous year. (p. 89)

There are several approaches for detecting serial correlation in panel data, but one straightforward approach is to take the following steps: (1) estimate an OLS regression model; (2) regress the residuals on all of the independent variables and the lagged residual; and (3) check whether the coefficient on the lagged residual is significant. If the coefficient is significant, it is safe to conclude that serial correlation is present.

Assuming that there is autocorrelation in the data, what can be done? One approach is, simply, to include one or more lags of the dependent variable in the main model. This is an attractive approach when there are more time periods than units (Beck and Katz 1995). A number of criminologists have included lagged dependent variables in their panel data models, however, without much attention the number of units compared with the number of time periods (e.g., Kovandzic, Sloan, and Vieraitis 2002; Lilley and Boba 2008; Marvell and Moody 1996). When there are more units than time periods and researchers lag the dependent variable to control for autocorrelation, this can result in "Nickel bias" (Nickel 1981), which, in a short time series, can push coefficients toward zero.

Lagging the dependent variable also suffers from two additional problems. One is that it results in lost observations (i.e., it is not possible to lag the dependent variable in the first time period). Another more important problem is that lagging the dependent variable makes for a dynamic model. It then becomes necessary to make it clear that the results are "short-run" estimates. Adjustments to the standard errors must be made in order to arrive at long-run estimates (e.g., De Boef and Keele 2008).

Another approach to dealing with serial correlation is to estimate models that include first-order autoregressive disturbance terms. This is done by estimating

ρ from β in a residual regression of $u_{it} = \beta u_{i,t-1} + \eta_{it}$ (Hsiao 1986; Mundlak 1978). Most statistics packages allow for such estimation (e.g., Stata's-xtregar-command).

Still, other approaches for dealing with autocorrelation include estimation models with robust standard errors (e.g., by using Stata's-robust-feature) or estimating models that are robust to autocorrelation, especially if the form is unknown. The generalized method of moments is considered robust to autocorrelation of unknown form (for an overview, see Wooldridge 2001).

Panel Heteroskedasticity and Contemporaneous Correlation

In their seminal work, Beck and Katz (1995) highlight the problems associated with panel heteroskedasticity and contemporaneous correlation. The former is concerned with unit-to-unit variances in the errors. Franzese (2002) proposed a simple test for detecting this problem. It entails regressing the absolute values of the OLS residuals on the unit-specific dummy variables. A resulting significant *F*-test suggests the presence of panel heteroskedasticity. Very few criminologists and/or criminal justice researchers have made any effort to detect or correct for this problem.

Contemporaneous correlation is concerned with correlated errors between two or more units at the same time. This is different from the heterogeneity discussed earlier, as it is conceivable that an event affects a few (though not all) of the units at the same time. Tests for detecting contemporaneous correlation are particularly intuitive, so motivated readers may wish to explore the work of Breusch and Pagan (1980). Otherwise, for those who use Stata, the user-written—xttest2—may prove helpful.

Nonstationarity

Panel data must be stationary, meaning that the means, variances, and auto-covariances (at various lags) stay constant across time periods. The augmented Dickey–Fuller test can be used to test for nonstationarity. This test involves regressing the first-differenced dependent variable on the one-period lag of the dependent variable as well as lags of the first-differenced dependent variables (Enders 1995). A number of different, more sophisticated tests have been introduced in the recent years (e.g., Choi 2001; Hadri 2000; Im, Pesaran, and Shin 2003). Stata users now have access to the—xtunitroot—command to check whether panels are stationary. The assorted tests accommodate balanced as well as unbalanced panels, the latter being panels with either more time periods than units or more units than time periods. For more on dealing with nonstationarity, see Banerjee (1999).

Pre-existing Trends

The inclusion of unit- and time-specific dummies in a fixed effects context, while advantageous for reasons touched on earlier, simplifies reality. Some researchers have built on this approach by replacing the unit dummies with unit-specific trend variables, typically coded with a 1 to *T* for each unit. If a panel contains 10 years of data, then each unit would get its own variable coded 1 through 10 for each time period. Such trend variables help control for trends in each unit that depart from *annual* events captured by the time dummies (Black and Nagin 1998; Marvell and Moody 1996). Whether this approach is desirable or necessary hinges on several factors, including the data in question and the substantive question at hand.

Extensions

Continuing with our focus on fixed effects estimation, there are several extensions that researchers may wish to consider. The ones that we will consider here help researchers model and/or account for: (1) simultaneity; (2) heterogeneous year effects; (3) unit-specific effects; and (4) events. These extensions not only add a level of sophistication to any one study, but they also make it more feasible for researchers to deal with real-world problems.

Simultaneity

A number of panel data studies published in criminology and criminal justice journals have examined the effects on aggregate crime rates of various policies. For example, many have studied the effect of policing on crime rates (e.g., Marvell and Moody 1996; Worrall and Kovandzic 2007). Others have sought to determine whether state laws, such as three-strikes, affect crime rates at various levels (e.g., Kovandzic, Sloan, and Vieraitis 2004). A problem arises in both situations. While the policies in question may affect crime, crime most likely affects the policies. For example, state legislatures do not enact three-strikes laws in a vacuum. Concerns over crime are at least partially responsible for their passage. Researchers, then, need to account for this problem. For ease of exposition, we will call it the "simultaneity problem."

One solution to deal with the simultaneity problem—that works in both a cross-sectional and panel data context—is to estimate instrumental variables regressions. Briefly, this involves identifying a variable that is correlated with a problematic predictor (the one that is thought to be affected by the dependent variable) and that is uncorrelated with the error term in the main equation. At the risk of simplification, this new variable (the instrument) basically replaces the problematic predictor. A common problematic predictor in the literature is police presence, or "police levels," usually measured as officers per capita.

Researchers have identified a number of creative instruments for police levels, ranging from federal grants to (e.g., Evans and Owens 2007; Government Accountability Office 2005), mayoral elections (Levitt 1997) and firefighters hired (Levitt 2002).

Researchers who opt for the instrumental variables approach to dealing with simultaneity in their panel data models need to be cognizant of the so-called "local average treatment effects" (e.g., Angrist and Imbens 1995; Heckman, Urzua, and Vytlacil 2006; Heckman and Vytlacil 2005). Basically, a problematic predictor (in the above example, police levels) is affected by multiple factors, not just by the instruments selected. So, the findings will be "local" to the selected instruments.

Panel data also afford researchers with another technique to address the simultaneity problem, namely, the Granger causality test (Granger 1969; Pindyck and Rubinfeld 1991, pp. 216–219). The test, which involves two separate autoregressive analyses, has been used on a few occasions in criminology (e.g., Kovandzic et al. 2002; Marvell and Moody 1996). Consider, again, an analysis of the effect of police levels on crime. The first step is to regress crime on lags of itself and lags of police levels (and the controls). The number of lags on each depends on how many are significant. The first lags that become insignificant are dropped. If, after the proper number of lags has been selected, lags of police levels are significant, it is said that police levels "Granger cause" crime. The second test is similar, but in it, police levels serve as the dependent variable. Then lags of it and lags of crime (along with controls) appear on the right-hand side of the equation. If lagged crime is significant, then it "Granger cause" police. There are two problems with this approach, however. One is that observations are lost. Another is that the use of lags means it is impossible to determine whether policing affects crime or crime affects police in the current time period.

Heterogeneous Year Effects

Using panel data, Worrall (2008c) found that the Local Law Enforcement Block Grants led to significant reductions in serious crime. There were two possible problems with this finding. First, police forces may have already been expanding beforehand, making the relationship between grants and crime spurious. Second, crime may have already been on the decline, a trend that would not have been captured in the models. In response to this problem, Worrall (2008c) emulated an approach taken by the Government Accountability Office (2005) and Evans and Owens (2007) wherein heterogeneous year effects were calculated and then used to replace the year dummies. Worrall (2008c) described the procedure thusly:

> These effects were calculated by (1) regressing police levels in the pre-LLEBG period on a linear time trend for each unit; (2) doing the same for the aggregate crime rate; (3) organizing the coefficients from each regression into quartiles; and (4) interacting the resulting cells with year dummies. (p. 343)

Because there were 12 years of data in his analysis, this resulted in 192 separate year effects (four quartiles from the first regression × four quartiles from the second regression × 12 years of data). For more details on this procedure, see Evans and Owens (2007, p. 192) and Government Accountability Office (2005, p. 77).

Unit-Specific Effects

Black and Nagin (1998, p. 213) called attention to what they termed as a "geographic aggregation assumption," or the assumption that parameters are constant across all units analyzed. Reality, unfortunately, is rarely this simple. The typical panel data model of, say, the effect of a new law on crime will yield a single coefficient for the law variable. But it is naive to assume that the law would work in the same way in all areas examined. Indeed, Pesaran and Smith (1995) found that this can result in significant bias. What, then, can be done? Black and Nagin (1998) adopted the simple approach of interacting the unit dummies with the predictor variable of interest. In their case, the key predictor was a "right to carry" (RTC) variable (coded with 1 or 0 to denote the presence or absence of a RTC law in any given state).

Black and Nagin's (1998) work was important, as it resulted in one of the first major attacks on Lott and Mustard's (1997) controversial finding that RTC laws were inversely associated with crime. Needless to say, gun control advocates were not impressed with the findings. And, when Black and Nagin estimated unit-specific (or, in their case, state-specific) effects, they concluded:

> We strongly reject the Lott and Mustard model's assumption of a uniform impact across states … The estimates are disparate. Murders decline in Florida but increase in West Virginia. Assaults fall in Maine but increase in Pennsylvania. (pp. 213–214)

Modeling the Effects of Events

Campbell and Stanley (1967, pp. 55–57) went so far as to call panel designs as "excellent quasi-experimental design[s], perhaps the best of the more feasible designs." Their observation rings true, in part, because panel data lend themselves nicely to the study of events. When some units experience an event and others do not, it is akin to having treatment and control conditions in a classical experiment (minus the random assignment, of course). Panel designs have proven quite popular of late in criminology and criminal justice, particularly for their ability to model events.

An "event" can take on many manifestations, but, generally, the term refers to some "happening" or condition that affects all or several units and one or many points in time. In models of aggregate crime rates, the event is most often a policy change or a law. There are multiple options available for the modeling of each, but four approaches have proven quite popular for modeling events in a panel data context (see Allison 1994, for an overview).

The first and most basic approach is to create a 1,0 variable denoting the presence or absence of the event or condition. This approach is quite popular for examining the effects of state laws on state crime rates. The state law variable is coded with a "1" for those states that have the law in question, or "0" otherwise. But researchers need to be careful with this approach because if the event or condition affects all units at the same time, then the resulting variable will be perfectly collinear with time dummies (see earlier discussion on "Choosing between Random and Fixed Effects").

The 1,0 variable approach is limited in that it fails to reveal any details about the event/condition in question. It may also be impossible in a fixed effects context for reasons just mentioned. An alternative is to code the event/condition in levels—if the data are available. For example, in studies of the effect on crime of policing grants (e.g., Worrall and Kovandzic 2007; Zhao et al. 2002), this approach is quite feasible. The crude 1,0 variable is replaced with the value of the grant secured (usually the amount is divided by population and multiplied by some metric). If a jurisdiction does not get a grant, it gets coded with a "0." The advantage of this coding strategy, aside from overcoming the collinearity problem highlighted in the previous paragraph, is that it reveals additional details about what crime returns can be expected with each additional dollar of spending.

Researchers have also opted to measure events and conditions with trend variables. Although somewhat crude, this approach is at least creative. An example of it appears in Marvell and Moody's (2001) study of the "lethal effects" of three-strikes laws. They sought to measure the incapacitative effect of three-strikes laws, with a linear trend variable starting at the time when the law was enacted. In their words, the variable "assumes that, in the absence of the law, very few defendants would have escaped prison sentences, so the incapacitation effect grows over time" (p. 103). Researchers have had a difficult time separating deterrent from incapacitative effects, so Marvell and Moody deserve a certain measure of credit for trying (see also Kessler and Levitt 1999).

Linear time trends, 1,0 variables, and even coding events and conditions in levels may simplify reality. It is not a stretch to conceive of nonlinear effects. For example, funding may be helpful to a certain amount and then yields fewer returns as amounts increase. Alternatively, a law may reduce crime in the short term and then have little effect some years later. Various linear and nonlinear coding schemes are possible to address these and related complications. Readers would be well advised to consult Allison (1994) for a thorough treatment of the alternative coding schemes.

Conclusion

This paper has provided a brief and simple introduction to panel data modeling. The author has assumed throughout that the reader is familiar with OLS regression. For those who have yet to attempt panel data modeling, there are many additional—and more thorough—introductions to the technique that they may wish to consult (see, e.g., Baltagi 2008; Hsiao 1986; Wooldridge 2002). Otherwise, it is recommended, to recap, that researchers follow this process:

(1) Choose between random and fixed effects estimation on the basis of three considerations: (i) the size of *N* relative to *T*; (ii) whether there is a correlation between the error term and the unobserved variables; and (iii) whether the predictors vary over time. A Hausman test is also recommended.
(2) Model heterogeneity only if necessary.
(3) Be cognizant of dynamics and panel heteroskedasticity.
(4) Test for stationarity.
(5) Consider modeling pre-existing trends, if they are present.

Once these steps are taken, researchers can then venture into more sophisticated territory by correcting for simultaneity, if present, accounting for heterogeneous year effects, modeling unit-specific effects, and examining the effects of events. This author is partial to the fixed effects, once again, because of its frequent use in criminology and criminal justice—and also because many of the extensions introduced toward the latter part of this paper would not be possible in a random effects context.

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