Although event history analysis provides a highly developed body of methods for studying the causes of events, there is little consensus on the best ways for studying the consequences of events. This article develops some methods for using multiwave panel data to estimate the effects of either naturally occurring events or planned interventions. It does this by synthesizing the literature on interrupted time series with econometric treatments of pooled time-series, cross-section data. The emphasis is on fixed-effects models and estimators because of their capacity to control for all stable differences across individuals, whether or not those differences are correlated with measured variables. In contrast to earlier treatments of the problem, the models allow for time-varying covariates and for events that can occur at different time periods for different individuals. For continuous dependent variables, the basic estimators are easily obtained with standard OLS regression programs. For dichotomous outcomes, logit models can be estimated by the method of conditional likelihood.

Using Panel Data to Estimate the Effects of Events

PAUL D. ALLISON
University of Pennsylvania

Social scientists often want to know the consequences of events. Does divorce affect the social adjustment of children (Allison and Furstenberg 1989)? Does having a child cause parents to become more conservative (Morgan and Waite 1987)? Does loss of a job increase the likelihood of depression (Turner, Kessler, and House 1991)? Does a change in CEO affect the performance of a firm? Does a premarital pregnancy lower educational aspirations? Does a promotion decrease or increase work effort? Despite the ubiquity and importance of such questions, little attention has been paid to optimal research designs and methods of statistical analysis. Although there is now a highly developed methodology for studying

AUTHOR'S NOTE: *For helpful suggestions, I am indebted to Clifford Clogg, Nicholas Christakis, Ron Kessler, Jerry Jacobs, and students in Sociology 613.*

SOCIOLOGICAL METHODS & RESEARCH, Vol. 23 No. 2, November 1994 174-199
© 1994 Sage Publications, Inc.

from the SAGE Social Science Collections. All Rights Reserved.

the *causes* of events (Tuma and Hannan 1984; Allison 1984; Blossfeld, Hamerle, and Mayer 1989; Yamaguchi 1991), there is little consensus on the best ways to study the consequences of events.

In this article I describe some relatively simple but effective methods for using panel data to estimate and test hypotheses about the effects of events. Although these methods have several precedents, they have not previously been employed by sociologists, and they have not been used in the ways I shall suggest. The most directly relevant literature focuses on time-series data on the consequences of planned interventions (Simonton 1977; Algina and Swaminathan 1979; Fox 1984), but that methodology suffers from serious limitations: All events must occur at the same time, events cannot be repeated, and other independent variables cannot be included. Drawing on econometric methods for panel data (Mundlak 1978; Hsiao 1986; Heckman and Hotz 1989), I show how these limitations can be removed, yielding a flexible and general approach to the study of effects of events. In contrast to previous approaches, I shall concentrate on fixed-effects estimators because of their powerful ability to remove selectivity biases. Before jumping into the statistics, however, let us first consider some ideas about the nature of events and their consequences.

THE NATURE OF EVENTS

We generally use the term event to refer to a discontinuity in the history of an individual person, organization, nation, or other institution. Events involve sharp changes from one distinct state to another or from one level of some variable to a substantially higher or lower level. Often the changes represent a movement from one equilibrium state to another quite different equilibrium state.

Naturally occurring events are typically complex, involving major, simultaneous shifts along many different dimensions. Consider how many aspects of a person's social and physical environment change with a residential move, a retirement, or a divorce. Because of this complexity, any given classification scheme is likely to have a great deal of residual heterogeneity. A job termination, for example, can be either voluntary or involuntary, with quite different consequences. Thus, to inquire as to the consequences of a given type of event is to search for what is common to all events within that type or for the predominant effect averaged over the observed events. Later we shall see how this within-category heterogeneity can be usefully decomposed by statistical analysis. It is worth noting here, however, that such heterogeneity can be eliminated or controlled by planned interventions in which variation on only a single dimension (or a small number of dimensions) is introduced by the researcher. Nevertheless, the same methods of analysis can be used for naturally occurring events and for planned interventions.

Although the complexity and heterogeneity of naturally occurring events increases the difficulty of causal inference, the fact that they can be precisely situated in time, with a clear before and after, is a great boon to such inference. It is this distinguishing characteristic that makes the analysis of events so attractive. Often, we can draw surprisingly reliable conclusions about whether an effect exists. Then the problem is to determine which features of the event produced the outcome or whether the effect occurred for all events or just for some subset.

Along this line, it is important to recognize that some quantitative concomitants of events are more properly regarded as intrinsic features of the events themselves rather than as consequences. For example, within the class of events labeled *promotions*, there is usually a sharp increase in salary. Although it may be of interest to estimate the distribution of such increases, the estimation problem is relatively trivial and hardly a question of causal inference. On the other hand, promotions may also produce an increase in self-esteem. Estimating that effect is rather more problematic because (a) any changes in self-esteem are likely to be much less dramatic than changes in salary; (b) there is likely to be a substantial amount of variation over time in measured self-esteem within the job; and (c) changes in self-esteem may not show up immediately after the promotion, and the changes may be transitory. If an effect of a promotion on self-esteem can be detected, it would then be appropriate to ask which intrinsic feature of the promotion produced that effect. Was it a change in salary, a change in authority, or some other characteristic of the promotion?

THE TIME PATH OF EVENT EFFECTS

Leaving aside some of these complications, let us assume for the moment that for a given outcome variable the effect of a particular type of event is uniform within that type. For example, we may assume that all deaths of husbands have the same effect on the level of depression of the surviving wife. We are primarily interested in the effect of an event at time t_0 on some quantitative variable $Y(t)$, where t can vary continuously. Later we shall examine dichotomous outcomes. Now let us consider what possible forms that effect might take. For simplicity, we begin by supposing that in the absence of the event, $Y(t)$ is constant over time. The simplest sort of effect is an additive, permanent change in the level of $Y(t)$, as shown in Figure 1A. Formally, we can say that $Y(t) = \mu$ for $t < t_0$ and $Y(t) = \mu + \delta$ for $t \geq t_0$. Thus δ can be regarded as the "effect" of the event. Of course, the constant effect may be superimposed on more complicated patterns. More generally, we can say that $Y(t) = \mu(t)$ for $t < t_0$ and $Y(t) = \mu(t) + \delta$ for $t \geq t_0$, where $\mu(t)$ is some baseline function of time. For example, $\mu(t)$ might be a linear function as shown in Figure 1B. If the baseline time path is linear, the effect of an event could include both a change in slope and a change in intercept, as in Figure 1C.

In 1A through 1C, the effect of the event is immediate and permanent (at least in some sense). In many cases, however, the effect of the event may be delayed or transitory. Figure 1D shows a plausible time path for events that produce a transitory perturbation in the outcome variable. In some cases, the onset of such transitory effects may be more gradual, as shown in Figure 1E. Finally, although the event may have some more or less permanent effect, it may take some time for the new level to be realized, as in Figure 1F.

These examples are far from exhaustive, and much more complicated paths are possible. But they do serve to sensitize us to some of the possibilities and to point out the need for a flexible methodology that allows for a variety of time paths. In any empirical study, it makes sense to begin by examining the simpler possibilities and then checking for some of the possible complications. In many cases, moreover, there will be insufficient data to estimate very complicated effects. The crucial point, however, is that the occurrence of an event disturbs or

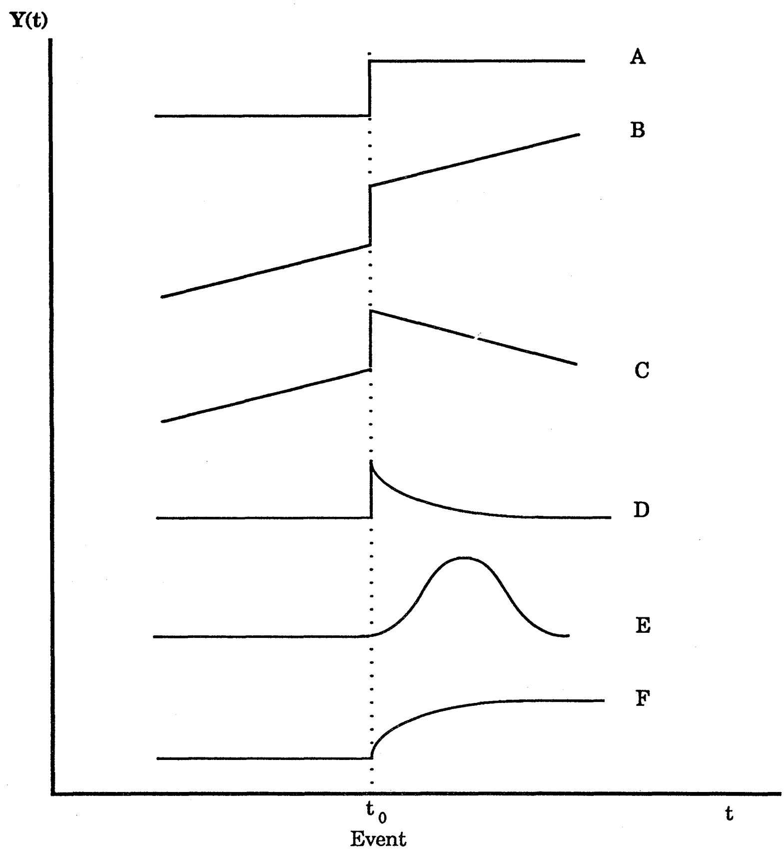

Figure 1: Possible Time Paths for the Effect of an Event on Y

modifies the time path of *Y*; the path differs in some way after the event, compared with what it would have been had the event not occurred.

TWO-PERIOD PANEL DATA

We turn now to models and methods for data analysis. To appreciate the logic of the proposed methodology, it is helpful to begin with the relatively simple situation in which the outcome variable is measured at only two points in time, Y_1 and Y_2, for a sample of independent individuals labeled $i = 1, \ldots, n$. Some of these individuals experience an event between the two measurements, others do not. Let $X_i = 1$ for those who had an event, and $X_i = 0$ for those who did not. With only two time points (and no knowledge of precisely when the event occurred between those two points), it is impossible to distinguish among the various possibilities in Figure 1. We might just as well assume, then, that the effect of an event is to add a constant to the score of each individual who experienced it. This is expressed in the following two-equation model:

$$\begin{aligned} Y_{i1} &= \mu_1 \qquad\quad + \gamma W_{i1} + \beta Z_i + \alpha_i + \varepsilon_{i1} \\ Y_{i2} &= \mu_2 + \delta X_i + \gamma W_{i2} + \beta Z_i + \alpha_i + \varepsilon_{i2}, \end{aligned} \tag{1}$$

for $i = 1, \ldots, n$. Here, Z is a vector of measured explanatory variables that are constant over time, W is a vector of measured explanatory variables that vary with time, and β and γ are vectors of coefficients. Our principal interest is in δ, which may be regarded as the effect of the event. The ε variables represent time-specific random disturbances that are assumed to be independent of the measured explanatory variables, of α_i, and of each other.[1] An important feature of this model is that Y_1 does not appear on the right-hand side of the equation for Y_2. For a justification of this omission, see Allison (1990).

The α_is represent unobserved differences across individuals that are constant over time and can be thought of as summarizing the effects on Y of all unmeasured, stable characteristics of individuals. For example, if the Z variables in Equation (1) were not measured for some particular set of data, we could define

$$\alpha_i^* = \beta Z_i + \alpha_i$$

and rewrite Equation (1) without the Zs. Although it is commonplace to treat such unobserved components of the model as random variables, they may also be regarded as fixed constants. Moreover, the choice between these two alternatives has important implications for how we estimate δ and the other parameters in the model. That is, we must decide whether to specify a *random-effects model* or a *fixed-effects model*.

For a random-effects model, it is typically assumed that α_i is uncorrelated with any of the measured predictor variables, and also with the ε_{ij}s. However, it is easily shown that the α_is induce a correlation between Y_{i1} and Y_{i2}, that is,

$$cov(Y_{i1}, Y_{i2} \mid W_{i1}, W_{i2}, Z_i, X_i) = var(\alpha_i).$$

This correlation arises because the two measurements have a common source of variation; the greater the variance across individuals, the greater the correlation between the two measurements of Y. Because the standard linear model assumes that all observations are uncorrelated, ordinary least squares (OLS) will be inefficient with biased estimates of standard errors. Nevertheless, efficient estimation of the two equations can be accomplished by generalized least squares (GLS) or maximum likelihood (under an assumption of bivariate normality for the Ys) (Hsiao 1986, chap. 3).

For the fixed-effects model, efficient estimation can be accomplished by a single application of OLS (Allison 1990). Specifically, if we subtract the first equation in (1) from the second, we obtain

$$Y_{i2} - Y_{i1} = (\mu_2 - \mu_1) + \delta X_i + \gamma(W_{i2} - W_{i1}) + (\varepsilon_{i2} - \varepsilon_{i1}). \tag{2}$$

To estimate Equation (2) we compute the change score for Y, and regress it on X and the change score for the W variables. When there are no W variables, the estimate for δ reduces to $(\overline{Y}_{E2} - \overline{Y}_{E1}) - (\overline{Y}_{N2} - \overline{Y}_{N1})$, where E refers to the group that experienced the event and N refers to the group that did not experience an event. Thus the estimate is the average change for the event group minus the average change for the nonevent group.

Which is the better approach? To answer this question, we must be careful to distinguish models from estimators. With that in mind, here are the relevant considerations:

(a) *In favor of GLS.* If the random-effects model is true, both the GLS method and the change-score method yield unbiased estimates of δ and γ. But GLS will be more efficient. Furthermore, in Equation (2) not only do the α_is, the constant *unobserved* differences, drop out of the equation but so do the Z_is, the constant *observed* variables.[2] Thus the change-score method is useless for estimating β, the effects of the Z_is.

(b) *In favor of change scores*. The fixed-effects model is much less restrictive. As already noted, the random-effects model typically assumes that α_i is uncorrelated with X_i and the other explanatory variables. In other words, those individuals who had events must have the same expected value of α_i as those who did not have events. This is a strong assumption that is unlikely to be satisfied unless events are assigned by a randomized experiment. And if it is not satisfied, the GLS (or maximum likelihood) estimate of δ may be severely biased. Suppose, for example, that the event of interest is an abortion, and Catholic women are much less likely to have abortions than other women. Unless religion is included in the model, the GLS estimate of δ could be confounded with preexisting differences between Catholics and those with other religious preferences. On the other hand, if we specify a fixed-effects model, we automatically control for all constant, unobserved differences between individuals, regardless of whether or not those differences are associated with the likelihood of event occurrence. Although it is possible to develop random-effects models that allow for correlations between α_i and the measured predictor variables, Mundlak (1978) has shown that if all such correlations are allowed, the GLS estimator reduces to the change-score estimator. Thus, in the less restrictive setting, there is really only one estimator, not two.[3]

Weighing the arguments in (a) and (b), it seems that the optimal choice depends on the research design. In randomized experiments, the possibility of correlations between the treatment and unmeasured individual characteristics is greatly reduced by random assignment. In that case, the random-effects model seems entirely appropriate, and it makes sense to choose the GLS estimator for the reasons given in (a). In nonexperimental studies, however, the potential biasing effects of "unmeasured selectivity" are so serious that some commentators question the very possibility of valid causal inference (Lieberson 1985). That danger leads me to conclude that the change-score estimator is nearly always preferable for estimating the effects of events with nonexperimental data.[4] In the remainder of this article, then, I shall focus largely on fixed-effects models and their associated estimators, although I shall also mention some models that combine both approaches.

There is another argument about fixed versus random effects that I believe is specious in this context. In the experimental design literature, a standard rule of thumb is that fixed-effects models are preferred when you only want to make inferences about the sample in hand, whereas random-effects models are preferred when you want to make inferences about some larger population. That reasoning may be appropriate when the objective is to estimate the fixed or random effects themselves. But here, the α_is are nuisance parameters that we only want to adjust or control for, not estimate. Hence the rule of thumb has little relevance.

SOME EXTENSIONS

With only two time periods, it is not possible to estimate some of the more complex effects of events such as those shown in Figure 1. But there are some important extensions even in the two-period case. First, instead of assuming that the event *adds* a constant to an individual's score, we can assume that the event multiplies a score by some constant. This is readily accomplished by replacing Y with the logarithm of Y in Equation (1), leading to a difference in logarithms of Y in Equation (2). Such a modification is especially attractive if the baseline $Y(t)$ is growing or decaying exponentially with time.

A second extension is to let Y be dichotomous (0,1) and to set the left-hand side of Equation (1) to be the logits for the probability that $Y = 1$ (suppressing the ε variables). In this case, a fixed-effects model is most appropriately estimated by the method conditional maximum likelihood. This is accomplished by (a) eliminating all observations with no change from time 1 to time 2 and (b) estimating the logit analog to Equation (2) with the dependent variable having a value of 1 for a change from 0 to 1 and a value of 0 for a change from 1 to 0.[5] For further details, see Chamberlain (1980) and the section on discrete outcomes below. See Allison and Long (1990) for the case where Y has a Poisson distribution.

MULTIWAVE PANEL DATA

We now consider the more general situation in which Y is observed at several points in time (possibly irregularly spaced) and events can occur at any point in time. Such data will (a) allow us to estimate and test more complicated effects of events, (b) give us more precise estimates of those effects, and (c) enable us to rule out some important alternative hypotheses. Our dependent variable is Y_{it} with $i = 1, \ldots,$ n and $t = 1, \ldots, T$. For now we assume that all T values of Y are observed for every individual, but the extension to missing values is straightforward.[6] The time index t could be calendar time, it could index the ages of the individuals, or it could represent some other appropriate time axis.

We begin again with the simple case of Figure 1A in which the effect of an event is to add immediately and permanently a constant to the baseline time path. We assume, for the moment, that no one experiences an event prior to $t = 1$ and that events are not repeatable. We know if an event occurred between any two time periods, but we do not know exactly when it occurred within the interval. Let $X_{it} = 1$ if an event occurred at any time prior to time t for person i, otherwise 0. The natural generalization of Equation (1) is then

$$Y_{it} = \mu_t + \delta X_{it} + \gamma W_{it} + \beta Z_i + \alpha_i + \varepsilon_{it} \quad t = 1, \ldots, T. \tag{3}$$

Notice that we allow for a different intercept μ_t for each point in time. In effect, this allows for an arbitrary baseline time path in Y. Later we shall consider restrictions on this time path that will sometimes be necessary or desirable. Again, for the reasons discussed earlier, we assume a fixed-effects model such that the α_is are a set of constants. (For the model to be identified, we must set one of the α_is equal to 0, or constrain their sum to be 0.) We also assume that the ε variables are independent of the measured covariates, the α_is, and each other.

How can this model be estimated? Because we have multiple values of Y for each individual, it is not obvious how the change-score method could be used. There are, however, two equivalent methods for estimating the model by OLS regression. One is to treat each time point for each individual as a distinct observation and specify a model that includes a dummy variable for each individual (less one). The observations are then pooled, and the model estimated by OLS. With individual-specific dummies, the Z variables (those that are constant over time) cannot be included because they are perfectly collinear with the α_is. To allow for an arbitrary function of time, dummy variables for each of the time periods (less one) are included in the model. Although this method is quite feasible, it becomes cumbersome and computationally burdensome when the sample is large with a concomitant large number of dummy variables.

An alternative approach avoids the necessity of dummy variables for all individuals by operating on a transformation of each of the variables in Equation (3). From each variable at each point in time, we subtract the individual-specific mean for that variable over time. Thus, for example,

$$Y_{it}^{*} = Y_{it} - \overline{Y}_i \text{ where } \overline{Y}_i = \frac{1}{T}\sum_{t=1}^{T} Y_{it}.$$

The same is done for X and all of the W variables. Then OLS regression is applied to the deviation scores for the pooled individual time periods. This method yields exactly the same coefficient estimates as the dummy variable method.[7] However, it is necessary to adjust the standard errors and degrees of freedom to allow for the fact that one is implicitly estimating a constant for each individual (Judge, Griffiths, Hill, and Lee 1980).[8] Regardless of which computational method is used, the resulting estimators are unbiased and fully efficient under the fixed-effects assumption. If there are only two time periods, either of these methods is equivalent to the change-score method described above.

As usual, if the ε disturbance terms have a multivariate normal distribution, the OLS estimator is also the maximum likelihood estimator. However, for fixed-effects models, one may distinguish between the conditional maximum likelihood estimator and the unconditional maximum likelihood estimator. The deviation-from-means method corresponds to the conditional estimator in which one conditions out the sufficient statistics (the individual-specific means) for the nuisance parameters (the α_is). The dummy variable method, on the other hand, corresponds to the unconditional estimator. As we have just seen, these estimators coincide in the case of a linear model. But for nonlinear models, that is not generally the case, as discussed below in the section on dichotomous outcomes (Chamberlain 1980).

At least two widely available statistical packages, SAS (SAS Institute, Inc. 1990) and LIMDEP (Greene 1992), will automatically calculate the regressions using the deviation scores and perform the necessary adjustments to the standard errors and degrees of freedom.

In both programs, the data are input as a separate record for each time point for each individual, with a variable containing a unique ID number for each individual. In the SAS procedure, GLM (used in the example reported below), the fixed-effects model, is implemented with the ABSORB command. A typical command file will look like this:

```
DATA;
   INFILE 'EVENT.DAT';
   INPUT ID Y X W T;
PROC GLM;
   CLASS T;
   ABSORB ID;
   MODEL Y = X W T;
```

The CLASS statement creates a set of dummy variables corresponding to the several values of the time variable T. The GLM procedure reports a sum of squares attributable to individuals and a test of the null hypothesis that all the α_is are equal to zero. But it does not report estimates for the α_is.

For the same problem, the LIMDEP commands (omitting data input) might read

```
REGRESS; PANEL;
LHS=Y; RHS=X, W;
STR=ID; PERIOD=T; FIXED $
```

LIMDEP does report estimates for the individual α_is, along with statistics that can be used to test the null hypothesis that all the α_is are zero. If the FIXED keyword is omitted, LIMDEP also computes the GLS estimator of the random-effects model and a Hausman (1978) chi-square test comparing the fit of the random- and fixed-effects models.

SPECIAL CASES AND EXTENSIONS

In Equation (3), Y_{it} was an arbitrary function of time. By forcing the baseline effect of time to be linear, however, we can examine the kind of effect represented by Figure 1B. The appropriate model is

$$Y_{it} = \mu_0 + \mu_1 t + \delta X_{it} + \alpha_i + \varepsilon_{it}, \tag{4}$$

where t is just time expressed as a quantitative variable, and W and Z have been suppressed for simplicity. Of course, if the time periods are unequally spaced, the coding of time should reflect the actual spacing.

A linear formulation is often desirable to facilitate interpretation. Moreover, if everyone in the sample has the event at the same point in time (which often happens with planned interventions), some restriction on the shape of $Y(t)$ is essential. That is because X_{it} is then linearly dependent with the set dummy variables for time.[9] But X is not linearly dependent with t expressed as a quantitative variable whenever there are at least three time periods. If there are four or more time periods, we can also have a quadratic baseline, as in

$$Y_{it} = \mu_0 + \mu_1 t + \mu_2 t^2 + \delta X_{it} + \alpha_i + \varepsilon_{it}. \tag{5}$$

Now we turn to models that allow for an event effect that is either not immediate or not permanent. By introducing a product term for t and X into Equation (4), we allow for changes in both slope and intercept, as in Figure 1C:

$$Y_{it} = \mu_0 + \mu_1 t + \delta_1 X_{it} + \delta_2 X_{it} t + \alpha_i + \varepsilon_{it}. \tag{6}$$

In this equation, δ_2 represents the change in the slope of the time path that follows the occurrence of an event, while δ_1 gives the change in intercepts. Further flexibility can be obtained by treating time as a set of dummies for the main effect, but as a quantitative variable for the interaction with X.

Equation (6) can also be used as an approximation to the functional form in Figure 1D. The main limitation to such an approximation is that 1D allows for a return to the baseline function, whereas Equation (6) implies that $Y(t)$ will either increase or decrease indefinitely. Within the range of the data, however, this may be an acceptable approximation. A somewhat better approximation to 1D (or to 1F) can be achieved by including t^2 and the product of X and t^2, as in

$$Y_{it} = \mu_0 + \mu_1 t + \mu_2 t^2 + \delta_1 X_{it} + \delta_2 X_{it} t + \delta_3 X_{it} t^2 + \alpha_i + \varepsilon_{it}. \tag{7}$$

This is still not an ideal approximation to 1D or 1F because it implies that eventually there will be a reversal: If the curve is decreasing, as in 1D, it will eventually begin to increase; if it is increasing, as in 1F, it will eventually start to decrease. On the other hand, that very fact makes it a decent approximation to Figure 1E.

A rather different approach to modeling increasing or decreasing effects is to code the occurrence of an event as a set of lagged dummy variables. For example, to represent Figure 1D, we could set

$X_{1it} = 1$ if an event occurred immediately prior to time t for individual i, otherwise 0;

$X_{2it} = 1$ if an event occurred immediately prior to time $t - 1$ for individual i, otherwise 0;

$X_{3it} = 1$ if an event occurred immediately prior to time $t - 2$ for individual i, otherwise 0.

Additional dummy variables with higher order lags could be created as needed. This particular coding would not be appropriate for Figure 1F, however, because the coding implies that all event effects disappear three periods after the event. A simple remedy is to eliminate the word "immediately" in the definition of X_{3it}.

AN EXAMPLE

For 249 scientists in four disciplines (mathematics, chemistry, physics, and biology) who were teaching in graduate departments in 1965, Allison and Long (1990) collected data on employer changes from one university to another. Annual counts of publications were obtained for each of the 5 years preceding the move, and each of the 5 years after the move. The question to be addressed is whether an employer change increases or reduces the rate of publication. A plausible hypothesis is that, other things equal, an employer change will temporarily decrease productivity because of the costs in adjusting to a new environment.

With $i = 1, \ldots, 249$ and $t = 1, \ldots, 10$, there are, in principle, a total of 2,490 person-years. However, article counts were missing for some scientists in the early or late years of the series. When these person-years are eliminated, the number of cases is reduced to 2,387. One of the virtues of this approach is that individuals do not have to be excluded entirely if they are missing some observations on the dependent variable.

The dependent variable, number of publications, was transformed by adding .5 to all scores and then taking the natural logarithm.[10] This transformation was chosen because it both reduced the skewness of the distribution of article counts and ensured that the model did not predict article counts less than zero. The first model to be estimated is Equation (4), with $X_{it} = 0$ for $t \leq 5$ and $X_{it} = 1$ for $t \geq 6$. Because X_{it} is linearly dependent with a set of dummy variables for time period, the time effect must be restricted. Here we examine linear and quadratic effects of time.

The model was estimated with PROC GLM in the SAS package, using the ABSORB command to handle the person-specific effects. Although no estimates are reported for these effects, as a set they are highly significant. Moreover, in all models reported, the person effects accounted for more than 57% of the variance in the article counts. The OLS parameter estimates are shown in Table 1, Model 1. The effect of an employer change is to reduce the rate of publication by about 12% (an effect that is statistically significant at the .05 level by a one-tailed test).[11] The linear increase with time is not significant, however.

In Model 2, we add a term for the square of time. (Note that in this and later models time is coded in deviations from the midpoint of 5.5). This term *is* statistically significant, indicating that publication counts first increase and then decrease with time, with the predicted peak occurring at about the second year after the employer change. But the effect of a move itself is virtually unchanged by allowing for a quadratic effect of time. Model 3 allows for an interaction between time and the occurrence of an employer change. The interaction just reaches statistical significance, and the overall effect can be described as follows: The rate of publication increases by about 3% per year, up to the time of the move. It immediately declines by about 11% following the move. Then it stays virtually constant for the remaining 5 years because the coefficients of t and X^*t essentially cancel.

These results suggest that there is some effect of an employer change, but they do not tell us whether that effect is a general consequence of such changes or whether there is some specific feature of the moves that is causing the reduction in publication counts. It is known, for example, that these moves were predominantly *downward* in the university prestige hierarchy. Might that account for the down-

TABLE 1: Estimates for Models Predicting Article Counts, 249 Scientists

	Model 1		*Model 2*		*Model 3*		*Model 4*	
Variable	β	*s.e.*	β	*s.e.*	β	*s.e.*	β	*s.e.*
X	–.125*	.055	–.126*	.055	–.127*	.055	–.088	.160
t	.012	.010	.013	.010	.033*	.014	.037	.047
t^2			–.004*	–.002			.0009	.009
$X*t$					–.040*	.020	–.048	.091
Prestige							.076*	.034

	Model 5		*Model 6*		*Model 7*		*Model 8*	
Variable	β	*s.e.*	β	*s.e.*	β	*s.e.*	β	*s.e.*
X	–.125*	.055	–.130*	.055	–.134*	.056	–.115	.066
Age	.012	.010	.013	.009	unrestricted		unrestricted	
Age^2			–.0015**	.0003				
Prestige							.078*	.034

* $p < .05$; ** $p < .01$.

ward trend in productivity? To examine this possibility, a measure of departmental prestige (Cartter 1966) was included as an independent variable in Model 4, along with both the squared term for time and the interaction between time and X. This prestige measure, which ranged in value from .53 to 4.97, was constant over time, except at the occurrence of an employer change when it jumped to a higher or lower level. As shown in Table 1, Model 4, the prestige variable has a significant positive effect on publication rate, and none of the other terms attains significance at the .05 level. Moreover, a joint test of X and $X*t$ is also nonsignificant. Thus we cannot reject the hypothesis that any effects of employer changes on publication rates are explained by changes in the prestige level of the employer.

One problem with the analysis so far is that the time variable has an origin that is somewhat arbitrary. It is anchored by the occurrence of an event—namely, an employer change—but there is no reason to expect that publication rates would be a function of this particular time axis. Moreover, as already noted, because the events occur to everyone at the midpoint of the time scale, the effect of time on publication rates must be restricted to a fairly simple functional form.

A more natural time scale would be professional age: The number of years between the receipt of the Ph.D. and the year of observation.

Because the sample was not a cohort, the employer changes occurred at a wide range of professional ages. The lower half of Table 1 shows the results of using professional age instead of time relative to the move. Model 5 constrains the age effect to be linear, with results identical to those for Model 1. This is not a coincidence. In a fixed-effects model, age at time 1 is automatically controlled, just like every other variable that does not change over time. Consequently, age at later times will be a perfect linear function of time relative to the events, and the two variables are linearly equivalent. This equivalence will not hold for nonlinear functions of age, as shown for Model 6, which introduces a squared term for age. Although the pattern is similar to that found for Model 2, the squared term attains a much higher level of significance in Model 6, even though the coefficient is smaller in magnitude. In Model 7, the effect of age is left unrestricted by introducing a dummy variable for each different professional age, a total of 44 dummy variables. This is conveniently handled in PROC GLM by simply specifying age as a CLASS variable. With this specification, the coefficient for an employer change actually increases slightly in magnitude, retaining its statistical significance. Finally, in Model 8, we introduce the prestige variable, finding that it depresses the coefficient for an employer change to the point where it just misses statistical significance—the p value is .06.

What about other possible explanatory variables, like academic discipline or prestige of the Ph.D. department? Because these are constant over time, their main effects are completely subsumed within the fixed-effects model. However, it would be possible to specify *interactions* between time-constant variables and any of the time-varying variables. Thus, for example, we could test whether the effect of an employer change was larger for chemists than mathematicians. Or we could see if disciplinary differences in publication rates increased with professional age.

REPEATED EVENTS

To this point I have assumed that at the beginning of the observation period no one has had an event and that each individual has no more than one event over the observation period. For many potential applications, however, repeated events are the norm. Suppose for example, that we wish to determine whether the birth of a child has an effect on marital adjustment scores. We observe couples over a 10-year period, with annual ratings of marital adjustment, and we record the birth dates of any children.

How we deal with this situation depends on how we think the effect of children operates. A very simple assumption is that the effect of a child is immediate, persistent, and uniform. Each additional child increases (or reduces) the marital adjustment score by the same amount. If this were the case, then the basic Equation (3) would still apply, except that X_{it} would now be the number of children born to couple i by time t, rather than simply a dummy variable for whether or not the event had occurred. The coefficient δ would then be the incremental (or decremental) effect of each additional child. Suppose, on the other hand, we believe that the effect of a child is immediate and persistent, but not uniform across children. Then, we need a separate dummy variable for each additional child. If we believe that the effect is immediate and uniform but not persistent, then we need only the dummy variables described above for transitory effect in the paragraph following Equation (7). The only difference is that with repeated events it may be possible for more than one of the dummy variables to be 1 at the same time. Other possibilities should be fairly straightforward.

OTHER ESTIMATION PROCEDURES

Because the fixed-effects approach is such an effective method for controlling for individual differences, it should be the natural first choice among its competitors. As already noted, however, there are some definite disadvantages. First, the effects of variables that are constant over time cannot be estimated. Second, because so many parameters have to be adjusted for (at least implicitly), it may be considerably less efficient than some other methods. Third, the OLS method I have described may not adequately deal with autocorrelation among the repeated observations.

Although the inability to estimate the effects of time-constant variables may seem daunting to those who are accustomed to throwing everything into their models, I do not see it as a substantial drawback for those whose primary focus is on the consequences of an event. Why clutter up the model with variables that are not of direct interest? And even though these variables are not explicitly in the model, they are implicitly controlled for in a way that does not impose any particular functional form. As for the second disadvantage, my own (limited) experience with these methods is that the estimated standard errors for the fixed-effects models tend to be only slightly larger than those of alternative estimators. A small (or even moderate) loss of precision seems tolerable when it comes with potentially large reductions in bias.

The third disadvantage is the one that I wish to focus on here. Correlations across repeated measurements are often quite large, and that means that any estimation method that does not take such correlations into account is likely to yield standard error estimates that are biased downward and test statistics that are biased upward. Because the major component of those correlations is, in most cases, attributable to stable differences across individuals, the fixed-effects estimator will probably correct for much of the cross-time correlations. However, in some cases, there may be enough remaining autocorrelation to significantly bias the standard errors and test statistics. Hence it may be worth considering alternative or supplemental estimation methods.

This is particularly true when, as in the example described above, everyone in the sample experiences the event. In such cases, there is no danger that the event indicator is correlated with unmeasured, stable characteristics of the sample members. (However, there could still be potential bias in the estimation of other time-varying variables, like departmental prestige, that change at different rates for different individuals). Consequently, the bias-reducing properties of the fixed-effects estimator may not be particularly relevant.

In such cases, we can consider random-effects estimators that still retain the basic structure of Equation (3). For example, by specifying the α_is as random variables drawn from a single distribution, but retaining the assumption that the ε_{it}s are independent across individuals and across times, we can solve the first two disadvantages of the fixed-estimator: no time-constant explanatory variables and lower efficiency. This is easily accomplished in LIMDEP. But random effects models impose a fairly restrictive structure on the correlation matrix, one that does not allow, for example, correlations between adjacent time periods to be larger than those for more distant points.

For a less restrictive approach, we can suppress the α_i term, allow the ε_{it}s to be correlated across time, and do generalized least-squares or maximum likelihood estimation. Unfortunately, standard suggestions for how to do this for event analysis (Simonton 1977; Algina and Swaminathan 1979) do not allow for time-varying covariates other than those that are deterministic functions of time itself. The same is true for many programs (like PROC GLM) for doing repeated measures analysis. The best program I have seen for accomplishing this is the BMDP 5V program, which is designed for maximum likelihood estimation of repeated measures analysis of variance with time-varying covariates and missing data. If the number of time periods is small, the covariance matrix for the ε_{it}s can be left unrestricted. Alternatively, there are several standard structures that can be imposed on that covariance matrix, including a first-order auto-regressive process, banded autoregressive, and compound symmetry. Within this framework, it may be possible to use a fixed-effects estimator by including dummy variables for individuals, while still allowing for an autoregressive structure for the disturbance terms. LIMDEP also provides a computationally convenient approach to fixed-effects models with autoregressive disturbances.

DICHOTOMOUS OUTCOMES

Drawing on recent computational advances, it is possible to extend the fixed-effects model of Equation (3) to dichotomous outcomes. Retaining the basic data structure, let us now suppose that Y_{it} can take on values of 1 and 0 only. If the 1s are rare relative to the 0s (or vice versa), it may be desirable to use the discrete-time event history methods described by Allison (1982) and Yamaguchi (1991). On the other hand, when 1s and 0s are both common, a logit analog to Equation (3) may be more appropriate:

$$\log\left[\frac{Pr[Y_{it}=1]}{1-Pr[Y_{it}=1]}\right]=\mu_t+\delta X_{it}+\gamma W_{it}+\beta Z_i+\alpha_i.$$

Unlike the linear model, however, using maximum likelihood to estimate this model on the pooled individual time units, with dummy variables for the α_is, yields inconsistent estimates of the parameters. The reason is that the number of parameters to be estimated increases directly with the sample size, thereby violating a basic assumption of likelihood theory (the so-called incidental parameters problem) (Neyman and Scott 1948; Chamberlain 1980).

This problem can be solved by the method of conditional likelihood. In this method, one works with the conditional likelihood of the data given sufficient statistics for the nuisance parameters (the α_is). (For binomial data, the sufficient statistics are the sums of the Y_{it}s over t for each individual.) This conditional likelihood is then maximized like an ordinary likelihood function. The α_is drop out of the likelihood and are not directly estimated. For the two-period problem, the solution is easily obtained with conventional logit programs and was described above. For three or more time periods, the conditional estimator is computationally demanding and the choice of statistical package is limited. LIMDEP will do it for five or fewer periods with every individual observed for the same number of periods.

A more flexible program is the PHREG procedure in SAS, which will do conditional likelihood logit analysis for an unlimited number of periods and varying numbers of periods across individuals. Although this procedure was originally designed for event-history analysis using Cox's partial likelihood method, it is easily adapted for conditional likelihood logit analysis.[12] The basic setup is this: Individuals whose dependent variables are all 1s or all 0s can be eliminated from the sample because they contribute nothing to the likelihood. For the remainder of the sample, the data are structured as in the linear model procedure using GLM: Each person-time period is treated as a distinct observation that includes a unique ID number for each individual. The dependent variable Y should be recoded so that all the 0s become 2s. After these data transformations, a typical command file would look like

```
DATA;
  INFILE 'EVENT.DAT';
  INPUT ID Y X W T;
PROC PHREG;
  MODEL Y = X W T / TIES=DISCRETE;
  STRATA ID;
```

Unlike event history applications, no censoring indicator is needed.[13]

DISCUSSION

In this article I have described a comprehensive set of techniques for investigating the effects of events. My conceptualization of events and their consequences draws much from the literature on the analysis of time-series interventions, but the methods I have proposed go beyond that literature in three crucial respects:

1. An emphasis on fixed-effects as opposed to random effects models, for the purpose of eliminating selectivity bias.
2. The incorporation of time-varying explanatory variables, especially those that change only at the occurrence of an event.
3. The ability to study events that occur at different periods in time for different individuals.

All three of these developments are essential for the methodology to be applied to the study of naturally occurring events.

With the fixed-effects approach, there is no bias resulting from selectivity on stable, individual characteristics. Individuals who experience the event may well be different from those who do not, but as long as those differences are stable over time (or would be in the absence of the event), we can get unbiased estimates of the effects of the event. On the other hand, if people are selected to experience the event because of some transitory variation in the outcome variable, bias may occur. With respect to Equation (3), this would mean that X would be correlated with the disturbance term ε.

To understand how this might occur, consider the possible effect of a job loss on depression. We know that some people are consistently more likely to be depressed than others, but we also know that people have bouts of depression followed by periods of remission. Suppose that a job loss has no effect on depression, but having a bout of depression (a transitory change) increases the likelihood that a person is laid off or fired. If one applied the methods described here, there would be an apparent tendency for job loss to reduce depression. But this would be purely an artifact of what is often called regression to the mean. In this scenario, job loss is merely a marker for temporarily high values of depression, which then tend to decline at later periods in time.

In short, like any statistical method, the fixed-effects approach is not foolproof. Caution in interpretation is always appropriate, but compared with alternatives, it appears to be one of the most promising statistical methods for analyzing nonexperimental panel data.

NOTES

1. In the two-period case, it is actually permissible for ε_1 to be correlated with ε_2, but complications arise with three or more periods.

2. If the model contains interactions between Z variables and the X variable, these do not drop out of the equation.

3. Because the random-effects model can be regarded as a special case of the fixed-effects model, it is possible to test the restrictions using a Hausman (1978) chi-square test, which is implemented in the LIMDEP package (Greene 1992). This possibility suggests a strategy of always applying this test and (a) using the change-score estimator when the test rejects the random-effects model or (b) using the GLS estimator when the test fails to reject the random-effects model. Although this would not be an unreasonable strategy, it should be kept in mind that, like any two-step method, the reported standard errors for the GLS estimates are probably underestimates of the true standard errors.

4. There has been some concern that the change score estimator (as well as the generalization described in the next section) may magnify the biasing effects of measurement error in the *independent* variables. This is unlikely to be a major problem in the estimate of event effects unless there are misclassifications of those who did and did not experience the event. It could be a problem for estimating the coefficients of other time-varying variables. Nevertheless, Griliches and Hausman (1986) have shown that panel data contain information that makes it possible to correct for such measurement errors.

5. This estimator is not feasible when *all* cases have a value of 0 at time 1 and some cases change to a 1 at time 2. More generally, fixed-effects models for dichotomous outcomes are not

identified when the change can only go in one direction, and random-effects models are only very weakly identified (i.e., by imposing arbitrary and untestable constraints).

6. Under the fixed-effects assumption, Equation (3) is just a standard linear model, with nT independent observations. In that case, it is well known that if some observations have missing data on Y and the data are missing at random, then maximum likelihood estimates are obtained by simply deleting the observations with missing data and applying OLS (Little and Rubin 1987). For panel data, then, we need only delete data for the missing periods for each individual.

7. Some treatments of fixed-effects models (Judge, Hill, Griffiths, Lutkepohl, and Lee 1982; England, Farkas, Kilbourne, and Dou 1988) calculate the deviation scores from both individual means and time period means. This may be useful in econometric analyses where the number of time periods is often large relative to the number of individuals. But in sociological applications, where the number of time periods is typically less than 10, one might just as well include dummy variables for time periods in the specified model.

8. Each standard error must be multiplied by $[(nT - K)/(nT - n - K)]^{1/2}$, where K is the number of coefficients in the model including the intercept. When using the t distribution, the number of degrees of freedom is $nT - n - K$.

9. This can be shown as follows: Suppose that for all i, $X_{it} = 1$ whenever $t \geq r$, otherwise 0. That is, everyone experiences the event between $r - 1$ and r. For $j = 2, \ldots, T$, define $d_{ijt} = 1$ if $j = t$, otherwise 0. This is just the set of dummy variables corresponding to the T points in time. It follows that

$$X_{it} = \sum_{j=r}^{T} d_{ijt}.$$

10. A preferable approach would be to assume that Y_{it} has a Poisson distribution with parameter $\lambda_{it} = \exp\{\mu_t + \delta X_{it} + \alpha_i + \varepsilon_{it}\}$. This is quite feasible for the two-period case, as Allison and Long (1990) showed for these data. Specialized software is required for the multiperiod case, however; this was not attempted because results from the Poisson analysis and the log-linear OLS analysis were virtually identical in the two-period case.

11. When the dependent variable is logged, the transformation $100(e^{\beta} - 1)$ gives the percentage of change in the dependent variable for a 1-unit increase in the independent variable, where β is the estimated coefficient. For small values of β, $e^{\beta} - 1$ is approximately equal to β, which facilitates a quick interpretation of the coefficients.

12. To my knowledge, PHREG is the only commercial partial likelihood program that will do this because all others use only an approximate likelihood in the presence of ties.

13. This method can be very computationally intensive for large samples with many time periods. For exploratory analysis, specifying TIES=EFRON invokes an approximate method that can greatly reduce the computation time.

REFERENCES

Algina, James and Hariharan Swaminathan. 1979. "Alternatives to Simonton's Analyses of the Interrupted and Multiple Group Time-Series Designs." *Psychological Bulletin* 86:919-26.

Allison, Paul D. 1982. "Discrete-Time Methods for the Analysis of Event Histories." Pp. 61-98 in *Sociological Methodology 1982*, edited by Samuel Leinhardt. San Francisco: Jossey-Bass.

———. 1984. *Event History Analysis*. Beverly Hills, CA: Sage.

———. 1990. "Change Scores as Dependent Variables in Regression Analysis." Pp. 93-114 in *Sociological Methodology 1990*, edited by Clifford Clogg. Oxford: Basil Blackwell.

Allison, Paul D. and Frank F. Furstenberg, Jr. 1989. "How Marital Dissolution Affects Children: Variations by Age and Sex." *Developmental Psychology* 25:540-9.

Allison, Paul D. and J. Scott Long. 1990. "Departmental Effects on Scientific Productivity." *American Sociological Review* 55:469-78.

Blossfeld, Hans-Peter, Alfred Hamerle, and Karl Ulrich Mayer. 1989. *Event History Analysis*. Hillsdale, NJ: Lawrence Erlbaum.

Cartter, Allan M. 1966. *An Assessment of Quality in Graduate Education*. Washington, DC: American Council on Education.

Chamberlain, Gary. 1980. "Analysis of Covariance With Qualitative Data." *Review of Economic Studies* 48:225-38.

England, Paula, George Farkas, Barbara Stanek Kilbourne, and Thomas Dou. 1988. "Explaining Occupational Sex Segregation and Wages: Findings From a Model With Fixed Effects." *American Sociological Review* 53:544-58.

Fox, John. 1984. "Detecting Changes of Level and Slope in Repeated-Measures Data." *Sociological Methods and Research* 12:263-77.

Greene, William H. 1992. *LIMDEP: Users Manual and Reference Guide*. Bellport, NY: Econometric Software.

Griliches, Zvi and Jerry A. Hausman. 1986. "Errors in Variables in Panel Data." *Journal of Econometrics* 31:93-118.

Hausman, Jerry. 1978. "Specification Tests in Econometrics." *Econometrica* 46:1251-71.

Heckman, James J. and Joseph Hotz. 1989. "Choosing Among Alternative Nonexperimental Methods for Estimating the Impact of Social Programs: The Case of Manpower Training." *Journal of the American Statistical Association* 84:862-74.

Hsiao, Cheng. 1986. *Analysis of Panel Data*. New York: Cambridge University Press.

Judge, George G., William E. Griffiths, R. Carter Hill, and Tsoung-Chao Lee. 1980. *The Theory and Practice of Econometrics*. New York: Wiley.

Judge, George G., R. Carter Hill, William E. Griffiths, H. Lutkepohl, and Tsoung-Chao Lee. 1982. *An Introduction to the Theory and Practice of Econometrics*. New York: Wiley.

Lieberson, Stanley. 1985. *Making It Count*. Berkeley: University of California Press.

Little, Roderick J. A. and Donald B. Rubin. 1987. *Statistical Analysis With Missing Data*. New York: Wiley.

Morgan, S. Philip and Linda J. Waite. 1987. "Parenthood and the Attitudes of Young Adults." *American Sociological Review* 52:541-7.

Mundlak, Y. 1978. "On the Pooling of Time Series and Cross Section Data." *Econometrica* 46:69-85.

Neyman, J. and E. L. Scott. 1948. "Consistent Estimates Based on Partially Consistent Observations." *Econometrica* 16:1-32.

SAS Institute, Inc. 1990. *SAS/STAT Users Guide, Version 6*. 4th ed. Vol. 2. Cary, NC: SAS Institute.

Simonton, Dean Keith. 1977. "Cross-Sectional Time-Series Experiments: Some Suggested Statistical Analyses." *Psychological Bulletin* 84:489-502.

Tuma, Nancy Brandon and Michael T. Hannan. 1984. *Social Dynamics: Models and Methods.* Orlando, FL: Academic Press.

Turner, J. Blake, Ronald C. Kessler, and James S. House. 1991. "Factors Facilitating Adjustment to Unemployment: Implications for Intervention." *American Journal of Community Psychology* 19:521-41.

Yamaguchi, Kazuo. 1991. *Event History Analysis.* Newbury Park, CA: Sage.

Paul D. Allison is a professor of sociology at the University of Pennsylvania. His forthcoming article (with Nicholas Christakis) in Sociological Methodology 1994 *is titled "Logit Models for Sets of Ranked Items." In addition to his methodological work, he is interested in theories of altruistic behavior and models of cultural evolution.*