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Time Series Analysis of Crime Rates

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A methodological critique of Cantor and Land's (1985) approach to the time series analysis of the crime–unemployment relationship is developed. Error correction models for U.S. homicide and robbery rates for the years 1946–1997 are presented to illustrate procedures for analyzing nonstationary time series data. The critique is followed by a discussion of methodological problems in work by Devine *et al.* (1988), Smith *et al.* (1992), and Britt (1994, 1997) that builds on Cantor and Land's approach.

KEY WORDS: time series; crime; unemployment; age; divorce; cointegration.

1. INTRODUCTION

David Cantor and Kenneth Land's (1985) analysis of the relationship between annual unemployment rates and crime rates in the United States has served as a paradigm for subsequent criminological time series analyses. Though Hale and Sabbagh (1991) and Hale (1991) have raised questions about their approach, Cantor and Land (hereafter C-L) (1991) have defended their work vigorously (Land *et al.*, 1995). Here I raise further questions about the C-L procedures. I then update their data set, add additional variables to it, and carry out further analyses of homicide and robbery rates in the United States during the years 1946–1997. Finally, I discuss the work of several other researchers who have used the C-L approach.

2. THE CANTOR–LAND APPROACH

C-L (1985) argue that earlier research dealing with the impact of unemployment on crime rates has led to weak and inconsistent findings because it has failed to take into account two possible ways unemployment might

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influence crime. Although the unemployed are expected to have greater motivation to violate the law, they might also spend more time at home, preventing burglaries and reducing their vulnerability to robbery, assault, and homicide[2] (an opportunity effect). C-L note that the two possibilities need not be mutually exclusive: unemployment could reduce the opportunities to violate the law while, at the same time, increasing the motivation to do so. If both effects are instantaneous, a coefficient representing the net effect of unemployment on crime might be small and insignificant even though both effects are substantial.

A linear relationship between the rate at which individual i commits crimes (C_i), and that individual's lawful opportunities and motivation at time t can be represented in the form of a regression equation with residual e_i:

$$C_{it} = a + b_1(\text{opportunity}) + b_2(\text{motivation}_{it}) + e_{it} \tag{1}$$

If opportunity at time t and motivation at time t are both proportional to unemployment at time t (U_t), a regression of C_t against U_t will yield an estimate of the sum $b_1 + b_2$. Without additional information, there is no way to estimate the individual coefficients b_1 and b_2.

C-L break the underdetermination by suggesting that opportunity effects should be instantaneous, while motivational effects are likely to be lagged. This is because most workers will have savings and welfare benefits to sustain them for a time after they lose a job. C-L represent the motivational factor with a term involving the difference in the unemployment rate, $\Delta U_t = U_t - U_{t-1}$, arguing that people will compare their current employment status with what it was in the past. Thus one can measure the motivational and opportunity effects of unemployment by using the expression $b_1 U_t + b_2 \Delta U_t$ to predict the crime rate at time t. The coefficient b_1 should be negative, while b_2 should be positive.

Analyzing nationally aggregated annual data for the years 1946–1982, C-L find evidence for trends in the crime rates, which they eliminate by taking first or second differences. The equations they consider thus take the form of

$$\Delta C_t = a + b_1 U_t + b_2 \Delta U_t + e_t \tag{2}$$

or

$$\Delta^2 \mathrm{C}_t = a + b_1 U_t + b_2 \Delta U_t + e_t \tag{3}$$

C-L present results for both logged and unlogged crime rates. Values of adjusted R^2 for these models range from 0 (assault) to .1979 (larceny).

[2]A similar argument was presented by Cook and Harkin (1985).

Evidence of the predicted opportunity and motivational effects is found primarily for offenses involving illegal acquisition. In a subsequent analysis, Land *et al.* (1995) extend the time series to 1990, obtaining similar results. In Section 2, I point out several problems associated with the C-L procedures. Because there is something of a disconnect between the narrative in which C-L present their theoretical ideas and the equations they use to represent those ideas, I discuss both.

2.1. The Lag Structure of Unemployment

2.1.1. Distinguishing Between Opportunity and Motivation Effects

The validity of the C-L strategy for distinguishing motivational from opportunity effects rests on the accuracy of the proposition that most workers have savings and welfare benefits to sustain them for a time on losing a job. This may well be true of some workers, but it is surely not true of all. Evidence to this effect can be found in Conley's (1999) study of a cohort of subjects in the Panel Study of Income Dynamics. Among blacks with an income of $15,000 or less in 1992, the median net worth of the family was zero (i.e., no assets). In the entire black subsample, the median assets excluding housing equity were $2000. In 1998, the bottom 40% of households in the Federal Reserve Board's Survey of Consumer Finances had a mean net worth of $1100 and a mean annual income of $13,500. Among non-Hispanics, 14.8% of whites and 27.4% of African Americans had zero or negative net worth (Wolff, 2001). Clearly, many would face serious financial difficulties very quickly after losing a job.

In an unpublished study[3] using monthly crime and unemployment data, C-L (1987) found that changes in unemployment were positively related to changes in burglary and larceny just 1 month later. Longer-lagged effects were absent, apart from negative effects with a lag of 2 months. These results suggest that analyses of annual data may be insufficiently fine-grained to detect the motivational effects of unemployment and that lags as long as a year are too long to model them. If the various motivational effects of unemployment are felt very quickly, then it will be impossible to distinguish opportunity effects from motivational effects with annual data because they will both appear as contemporaneous effects.

If the motivational effect is indeed lagged, so that loss of a job in year $t-1$ increases the motivation to violate the law in year t, then one could represent the motivational effect with a term in U_{t-1}, and the underdetermination of coefficients would no longer be a problem. This is not the strategy C-L adopt, but it would appear to be a straightforward translation of

[3] I know its contents only through the summary by Land *et al.* (1991).

the ideas in their narrative into a statistical model. Because C-L found evidence that motivational effects have a time lag much shorter than a year, it is questionable whether they should be studied by introducing a term in unemployment lagged by as long as a year, though they could legitimately be modeled with much shorter lags (e.g., weekly or monthly). However, the procedure would still be substantively dubious.

To see this, consider three sets of individuals. Members of the first set lose their jobs at the end of year t and remain unemployed through the following year. Members of the second set are employed in year t but lose their jobs during year $t+1$. Members of the third set become unemployed at the start of year t but find jobs a year later and keep them. If the motivational effect of unemployment were to be expressed through an unemployment rate lagged by 1 year, members of all three groups would have low motivation to violate the law in year t because they would both be living on earnings until they lost their jobs (groups 1 and 2) or on savings and welfare (group 3). Groups 1 and 2 would have equally low motivation to violate the law in year $t+1$, even though members of the first group are unemployed for the entire year, while members of the second group are working part of the year. On the other hand, members of group 3 would have higher motivation to violate the law in year $t+1$, even though they are working throughout that year, because they were unemployed the previous year.

These implications seem implausible. A current job will provide income to meet the present needs, as well as a stream of income in the future that the job-holder may not want to jeopardize (however, see the discussion of low-wage jobs below). To be sure, someone who is currently unemployed and who was also unemployed in the previous year might have greater financial need than someone who had been employed for a long time prior to the current spell of unemployment. Such a person might be more likely to have debts or needs that cannot be met with current income. But that would also be true of some people in the year they become unemployed. Very likely a distributed lag dependence of motivation on unemployment is needed, or a nonlinear expression in which the coefficient expressing the effect of contemporaneous unemployment on motivation depends on earlier levels of unemployment.

Ignoring these complications, one would operationalize Eq. (1) by writing

$$C_t = a + b_1 U_t + b_2 U_{t-1} + e_t \tag{4}$$

Making use of the identity $\Delta U_t = U_t - U_{t-1}$, and grouping terms, we have

$$C_t = a + (b_1 + b_2) U_t - b_2 \Delta U_t + e \tag{5}$$

Examination of Eq. (5) shows that the coefficient of the contemporaneous unemployment term is the sum of the opportunity and motivation effects, while the coefficient of the differenced term enters with opposite sign from the coefficient of the lagged term in Eq. (4).

Because Eq. (5) follows logically from C-L's discussion of motivational effects, even if it is not their own mathematical formulation, the implications of this equation are worth considering. Ignore for a moment the fact that the left-hand member of Eq. (5) is a crime rate and not a difference in crime rate, and consider the Cochrane–Orcutt estimates that C-L (1991, p. 329) present for robbery and burglary in light of the present discussion. They estimate the contemporaneous coefficient for the effect of unemployment to be −8.3501 for robbery and −41.685 for burglary; the corresponding coefficients for the change in unemployment are 7.2727 and 36.613. They interpret the positive coefficients as consistent with motivational theory, but based on the reasoning just presented, both coefficients have the wrong sign. Moreover, the coefficients representing opportunity effects are not −8.3501 and −41.685 but $-8.3501 + 7.2727 = -1.0774$ and $-41.685 + 36.613 = -5.072$. Both coefficients are negative, consistent with opportunity theory, but they are much smaller in magnitude than the coefficients C-L identify with opportunity effects. Because I doubt that a lagged unemployment rate is the best way to capture the motivational effect of unemployment on crime rates, I do not want to stress this finding too much; I mention it to highlight a problem in the way C-L translate verbal expressions of their ideas into equations.

Now consider not the C-L narrative, but its mathematical representation. Instead of representing the motivational effect of unemployment by lagging unemployment, C-L represent it by a difference score, ΔU_t. Two points may be made about this procedure. The first is the implausibility of representing motivation with a difference score.[4] This procedure would imply that the motivation to commit crime is as strong among those who have been unemployed for a long time as among those who have been continuously employed for a long time. In each case, there is no change in unemployment status from 1 year to the next. C-L's suggestion that motivation arises through a comparison with one's previous employment status seems off the mark here. Whatever standard one uses for a mental comparison, unemployment leads to real needs among some people that may be expected to affect criminal motivation. Moreover, someone who is unemployed at time *t* but who finds a job the next year would, by virtue of the

[4]After completing my paper, I discovered that Pyle and Deadman (1994) have also expressed reservations about Cantor and Land's specification, commenting, "It is not apparent why motivation should be related to the change in unemployment rather than its level in a previous period" (p. 341).

improvement, be less motivated to commit a crime in year $t+1$ than someone who had always been employed. This implication, too, is implausible. It is also at variance with C-L's suggestion that unemployment is more criminogenic when it has lasted awhile than when it first occurs.

Next, suppose that the equation to be estimated is Eq. (2). If the motivational effects are represented by ΔU_t, then the equation represents change in unemployment as causing change in crime rates without a lag, contrary to the C-L narrative. A lagged model would use ΔU_{t-1} as a predictor, not ΔU_t. It follows that the mathematical representation of motivational effects is no more satisfactory than the narrative version.

To determine empirically whether the influence of change in unemployment might be lagged rather than contemporaneous, I updated C-L's data set and examined the cross-correlation function for ΔU_t and the differenced murder rate for the United States, for the years 1946–1996. There were suggestions that an increase in unemployment might reduce the murder rate with a lag of a year or 2, but none of the correlations was statistically significant at the .05 level, and in a regression of change in the murder rate on ΔU_{t-1} alone or in a model that also includes ΔU_{t-2}, the F statistic for the regression was not significant. Nor were any of the individual coefficients.

2.1.2. Motivation and the Duration of Unemployment

C-L were probably on the right track in suggesting that long-term unemployment may be a more powerful motivater of crime than short-term unemployment, as several studies have found that long-term unemployment increases involvement in crime, while short-term unemployment does not. To take account of this one needs a variable representing the duration of unemployment. Neither a difference score nor a lagged aggregate unemployment variable can substitute for such a measure. Conceptually they are quite distinct.

One can have a constant level of unemployment with no one's employment status changing between time 1 and time 2 or with such a high level of turnover in unemployment that no one is unemployed at both times. Where aggregate unemployment has increased between time 1 and time 2, the increase establishes a floor on the number of people who have been unemployed for just a short time, but the number could be substantially larger. If, for example, the unemployment rate in one year is 5% and that in the next year it is 6%, the number of people who lost jobs in the intervening year could be anywhere between 1 and 6%. For this reason, neither change in unemployment nor aggregate unemployment is a good proxy for

Table I. Cochrane–Orcutt Estimates of Coefficients and Summary Statistics for the Effects of Unemployment on Crime Rates, 1946–1996[a]

Dependent variable	Intercept	U_t	DUR_t	Adj. R^2	$\hat{\rho}$	DW
Dhomicide	1.325*	.049	−.120*	.310	.39	1.90
Drape	2.725*	−.022	−.151	.030	.41	1.88
Drobbery	41.988*	4.127	−5.009*	.439	.36	1.85
Dassault	24.029*	3.676*	−2.950*	.127	.32	2.02
Dburglary	221.788*	14.378	21.798*	.330	.40	1.69
Dauto	80.242*	−1.133	−4.989*	.300	.59	2.02

[a]All dependent variables are first-differenced crime rates, measured as numbers of crimes per 100,000 population. Intercepts and coefficients for U_t and DUR_t (duration of unemployment) are metric coefficients.
*$p < .10$.

duration of unemployment.[5] Were strength of motivation to be measured by the numbers of people who are unemployed, no matter for how long, this would not be an issue, but when long-term unemployment is assumed to increase motivation, then it is an issue.

Using the updated data set, I replicated C-L's regressions using the duration of unemployment, rather than change in unemployment, as a measure of motivation.[6] The results of such analyses for six index offenses (homicide, rape, robbery, assault, burglary, auto theft) are shown in Table I, which is constructed parallel to C-L's (1985) Table II.[7] None of the contemporaneous unemployment coefficients is both negative and statistically significant, as predicted by opportunity theory. In addition, all of the coefficients of DUR (duration of unemployment) are negative, five of them significantly. Motivation theory predicts that these coefficients should be positive. Thus, using these procedures there is no support for either motivational theory or opportunity theory. However, in the remaining part of this article I argue that the procedures themselves are flawed, so that the lack of support for either theory has little meaning.

[5]Empirically, the two variables are quite weakly related. Using data from the Executive Office of the President (1998) I computed the correlation between average number of weeks unemployed and the difference in levels of civilian unemployment using annual data for the United States for the period from 1950 to 1997. It was .17.

[6]Because the duration of unemployment does not provide information on how many people are experiencing unemployment of that duration, it is not an ideal measure of motivation. The product of the level of unemployment and the duration of unemployment would be a better measure. However, in our data set, the correlation between the product variable and the level of unemployment is .940; empirically it would be difficult to distinguish the two.

[7]As definitions of larceny–theft changed during the period of study, I elected to omit this offense from the analysis rather than adjust for the change, as C-L did.

Table II. Error Correction Model Estimates of Models for Homicide Rates, 1946–1997[a]

Independent variable	Model A	B	C	D	E	F	G
Constant	−.14	−.17	−.18	.68	.76	.41	.47
	(.39)	(.50)	(.54)	(1.97)	(1.82)	(.94)	(.92)
ΔDivorce	.29***	.34**	.32***	.28*	.29*	.29*	.29*
	(4.72)	(5.75)	(5.48)	(2.57)	(2.56)	(2.65)	(2.61)
Error	−.15	−.29**	−.26**	−.22*	−.22*	−.24*	−.25*
	(1.74)	(3.34)	(2.80)	(2.51)	(2.48)	(2.69)	(2.63)
PCTM1529	4.52*	5.16**	5.75**	1.90	1.39	3.38	3.00
	(2.45)	(2.94)	(3.22)	(1.28)	(.66)	(1.60)	(1.11)
U_t	−.16***	—	−.07	—	.02	—	.02
	(4.15)		(1.41)		(.36)		(.23)
U_{t-1}	—	−.18***	−.13*	—	—	−.07	−.07
		(4.92)	(2.67)			(.99)	(.94)
DUR	—	—	—	−.09**	−.09**	−.06	−.07
				(4.73)	(3.31)	(1.87)	(1.60)
R^2	.48	.53	.55	.56	.56	.57	.57
DW	1.65	1.70	1.65	1.51	1.49	1.52	1.52

[a]Coefficients are unstandardized regression coefficients; figures in parentheses are values of Student's *t*.
*$p < .05$.
**$p < .01$.
***$p < .001$.

2.2. The Treatment of Trends

During the years covered by the C-L and Land–Cantor–Russell studies, crime rates rose. Because trends in time series pose problems for statistical estimation, statisticians remove them before carrying out further analyses. C-L (1985) and Land *et al.* (1991) do this by taking first or second differences, a standard procedure. To show that this differencing poses a problem for the interpretation that Land and his collaborators give to their findings, assume for the sake of simplicity that the trend in crime rates is linear. If we add a term linear in t to Eq. (1), the terms involving opportunity and motivation represent the degree to which variation in these variables raises or lowers the crime rate above or below the trend line. Assuming that the opportunity effects are proportional to U_t and that the motivational effects are proportional to the duration of unemployment, estimation of this equation for the six offenses listed in Table I yields coefficients for the effect of unemployment that are consistently positive, and for the effect of duration of unemployment that are consistently negative, except for rape, where both estimates fail to achieve statistical significance. Because C-L (1985) found the best-fitting models for all six offenses we are considering to be ones in which the crime rates were differenced twice, I reestimated the equation after adding a quadratic term in year.[8] Except for burglary, none of the quadratic terms was statistically significant. All coefficients for the contemporaneous unemployment rate were positive and statistically significant at the .05 level; all those for the duration of unemployment were negative and statistically significant.[9]

These conclusions were reached by estimating regression equations containing deterministic linear or quadratic terms in time. Although this is a perfectly acceptable procedure, the trends can also be eliminated by taking first differences (or second differences if needed). If this is done, the intercept in the regression equation drops out, and the term in t becomes a constant. We are thus left with an equation in which a difference in the crime rates is predicted by a difference in the opportunity variable and a difference in the motivation variable. This is what is expected theoretically. Corresponding to a given level of opportunity and motivation there should be, in equilibrium, a corresponding level of illegality. A change in opportunity or in motivation should lead to a change in the volume of crime.

Contrast the procedure just outlined with the one that C-L adopt. They difference the crime rates in Eq. (1) once or twice to eliminate trends but carry out no differencing of the independent variables. This procedure is mathematically unacceptable. If one accepts Eq. (1) as a theoretical representation of the processes of interest, then operations that transform the left-hand member of Eq. (1) must also be performed on the right-hand member if the equality is to be preserved.

The equations on which Land and his collaborators base their conclusions lead to absurd implications because they difference only the crime rates, and not their predictors. Suppose that the trend being eliminated is linear, that it is Eq. (2) being estimated, and that there are no motivational effects. However, there are opportunity effects, so that b_1 is negative. Equation (2) says that a constant level of unemployment would lead to a steady drop in the crime rate. If the crime rates require second-differencing, Eq.

[8]To reduce multicollinearity between *year* and *year*^{2}, I subtracted 1972 from *year*, and used this centered variable and its square in the regression.

[9]For purposes of comparison with C-L (1985), I also estimated these models using U_t, ΔU_t, as well as the linear and quadratic terms in year as predictors. In this set of estimates, all the linear and quadratic terms in time were significant except for rape, where the quadratic term was not significant. Four of the six coefficients for U_t were negative, but only two of the six were statistically significant at the .10 level: the coefficient for rape was positive; the coefficient for auto theft was negative. Of the six coefficients for change in unemployment, only the coefficient for rape was negative (not significant); the remaining five were positive, with the coefficients for murder, robbery, and auto theft being statistically significant at the .10 level. Discrepancies from the results reported by C-L (1985) may be due to differences in the years covered.

(3) would be estimated. It would say, under the same conditions, that a constant rate of unemployment will lead to a deceleration in the increase of crime rates. In a world in which there are no opportunity effects, only motivational effects, Eqs. (2) and (3) also predict that a constant level of unemployment will lead to an increasing or decreasing crime rate [Eq. (2)] or an accelerating or decelerating crime rate [Eq. (3)], as long as the constant term in the regression, *a*, is different from zero. These predictions do not correspond to any reasonable notion of the way unemployment should affect crime rates.

In responding to criticism from Hale and Sabbagh, C-L (1991) offer a justification for differencing crime rates not given in their original paper: differencing could eliminate omitted variables responsible for the upward trend in crime rate, allowing them to concentrate on the effects of unemployment. Yet differencing undertaken for this reason must still be carried out for both left- and right-hand members of an equation if logical consistency is to be maintained and the equations are to retain their original meaning. In addition, Hale (1991) has pointed out that this procedure works only if the omitted variable has a constant trend but does not contribute at all to fluctuations around the trend line. Should an omitted variable have a random component as well as a deterministic trend, the differencing procedure will not eliminate it. Residuals for the differenced equation will have a negative first-order serial correlation, and estimates for the effects of unemployment will be biased if the random component is correlated with unemployment. Because virtually every imaginable social variable that might contribute to a trend in crime rates will have a random component, differencing cannot be considered a satisfactory way of eliminating omitted variables responsible for trends. One must, therefore, echo Hale's (1991) observation that "differencing is no substitute for modeling." If there are variables responsible for trends in crime, it is desirable that they be identified and introduced into one's model whenever possible.

2.3. Cointegration Issues

2.3.1. Nonstationarity and Unit Root Tests

When a time series is not stationary, classical statistical theory breaks down (Granger and Newbold, 1974; Phillips, 1986) and special procedures are needed. Consequently, one of the very first issues a researcher must confront when analyzing a time series is the question of whether it is stationary. "Unit root" tests allow one to determine whether a series is stationary, and if it is not, whether it is a random walk, a random walk with drift, or a random walk with drift and trend (Banerjee *et al.*, 1993; Holden and Perman, 1994; Harris, 1995, pp. 27–39; Charemza and Deadman, 1997, pp. 98–122; Greene, 1997, pp. 847–851; Johnston and DiNardo, 1997, pp. 223–228) . These tests consider whether the coefficient a in the equation $y_t = ay_{t-1} + \cdots + e_t$, $y_t = \mu + ay_{t-1} + \cdots + e_t$, or $y_t = \mu + ay_{t-1} + bt + \cdots + e_t$ is significantly different from 1. Equations in which the coefficient a is equal to 1 represent "unit root" processes.

When I carried out two such tests, the Augmented Dickey–Fuller test and the Phillips–Perron test, for the six crime rates (homicide, forcible rape, robbery, assault, burglary, auto theft) for the years 1946–1997, the tests failed to reject the null hypothesis of a unit root for each offense, consistent with each series being nonstationary.[10] When the tests were repeated on the differenced rates, the unit root hypothesis was rejected in each instance, suggesting that no further differencings were needed. The same tests for unemployment indicated that it was stationary, and required no differencing.

A question could be raised whether it is really possible for a crime rate to be a realization of a random walk process. The variance of a random walk time series increases without limit over time, and this seems implausible for crime. It is unrealistic to suppose, however, that the same generating process responsible for temporal changes in crime will continue unchanged forever. A random walk seems to fit these offenses over the half-century for which we have data. Changes in the causes of crime (including social control strategies) or in the strength of their effects could change the structure of the crime rate series in the future.

Before proceeding to discuss cointegration, it is worth reflecting on the theoretical importance of the finding that crime rates appear to be realizations of a unit root process. This means that there are no effective social processes tending to reduce crime rates if they grow too large or to increase them if they fall too low. Instead, they bounce about randomly, uninfluenced by the instantaneous level of crime, and without tending to return to an equilibrium level.

If a time series requires d differencings to achieve stationarity, it is said to be integrated of order d, denoted $I(d)$. Logical consistency requires that if crime rates are a realization of a unit root process, they must be explained by a unit root process. It is thus mathematically impossible for a crime rate that is $I(1)$ to be caused only by a function that is $I(0)$. If a crime rate is $I(1)$, at least one of the explanatory variables must also be $I(1)$, though additional predictor variables can be $I(0)$. This knowledge can help focus a search for explanatory variables.

If one estimates the effect of an $I(1)$ series X_t on an $I(1)$ series Y_t by regressing Y_t on X_t, one runs the risk of spurious regressions. Until recently,

[10]I treat a failure to reject the null hypothesis as equivalent to accepting the null hypothesis.

differencing both sides of the regression equation to eliminate the nonstationary, and then analyzing the stationary differences, was the recommended way to proceed. Yet this is not a satisfactory procedure because it removes all information about the long-run tendencies of the differenced variables. Often it is the long-run trends that are of much greater interest than short-time fluctuations; yet researchers following procedures needed to avoid spurious regressions focused all their attention on the short-run, ephemeral fluctuations, rather than the long-run tendencies. In criminology this has meant ignoring the causes of the large rise in crime rates between the early sixties and the early seventies, while focusing on year-to-year fluctuations in crime rates around the rise. Recent developments in statistical theory based on the concept of cointegration offer an appealing way to avoid this loss of information.

2.3.2. The Cointegration of Crime and Divorce

Cointegration theory is based on the insight that even if the series X_t and Y_t are individually nonstationary, a linear combination of the two series may be stationary. If there exists a constant β such that $Y_t - \beta X_t$ is $I(0)$, the two series are said to be cointegrated. Cointegrated series will tend to move together. If a disturbance leads to a short-run increase in the distance between them, an equilibrating force will tend to bring them back together again. The relationship between the two variables thus tends to maintain itself over the long term. Even though each variable drifts, they drift together and do not grow farther apart, as they would were they not cointegrated, and were random-walking independently of one another. Information about this long-term relationship is what is lost when the series are differenced (Banerjee *et al.*, 1993, pp. 136–140; Harris, 1995, pp. 52–75; Greene, 1997, pp. 851–859).

To consider cointegration for each of the six index offenses would extend the length of this paper unduly. Instead, I analyze just the homicide rate and the robbery rate in this light. Visual inspection of graphs for the homicide rate, the robbery rate, and the divorce rate strongly suggests that each crime rate is cointegrated with the divorce rate:[11] both crime rates move roughly in parallel with the divorce rate, although the parallel movement may have weakened some in recent years (see Figs. 1 and 2). Augmented Dickey–Fuller and Phillips–Perron tests confirm that the divorce rate is $I(1)$, and that when the homicide rate and the robbery rate are each

[11]I was encouraged to examine divorce by a graph showing the divorce rate and the robbery rate given by LaFree (1998, pp. 140–144). LaFree rests his argument on the relationship between various crime rates and other social indicators on the basis of general trends, without conducting any statistical analyses to test their relationships.

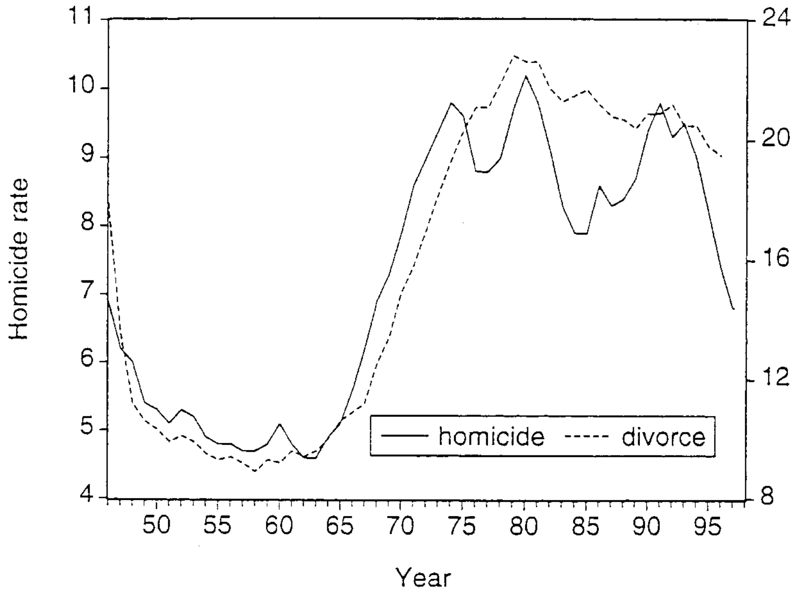

Fig. 1. Homicide rates and divorce rates.

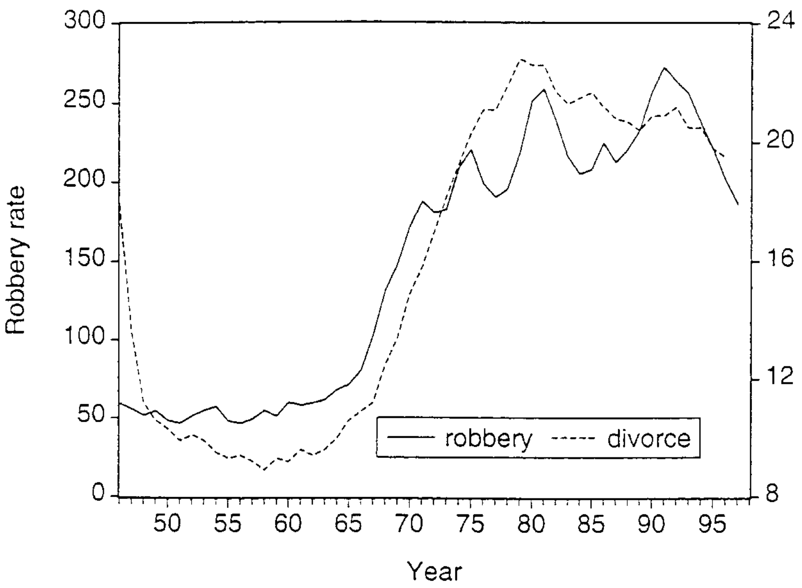

Fig. 2. Robbery rates and divorce rates.

regressed on the divorce rate, the residuals are $I(0)$, i.e., stationary. If we denote the divorce rate by D_t, the cointegrating equation is

$$C_t = \alpha + \beta D_t + v_t \tag{6}$$

A Johanssen test for cointegration is consistent with each pair of series being cointegrated.

It does not seem likely that this relationship is a direct, causal one or that people who are divorcing have exceptionally high divorce rates. More likely, divorce is an indicator of a strain in a fundamental social institution—the nuclear family. It is this strain that leads some individuals to kill, whether or not they themselves divorce. The divorce rate in the United States rose dramatically between 1960 and 1980, a time when gender relations and ideologies were undergoing major transformations, putting great strain on many families and leading some of them to divorce. The upsurge in homcides and robberies in those years may have been responses to strains related to this shift experienced by people who did *not* divorce.[12]

2.3.3. Error Correction Models

Fluctuations around equilibrium can be assessed statistically through a transformation of a dynamical equation. For illustrative purposes, suppose that the crime rate C is influenced by the lagged and contemporaneous divorce rates, a contemporaneous term in unemployment, and a lagged crime rate. We thus write

$$C_t = \gamma_0 + \gamma_1 C_{t-1} + \gamma_2 D_t + \gamma_3 D_{t-1} + \gamma_3 U_t + e_t \tag{7}$$

Writing $C_t = C_{t-1} + \Delta C_t$ and grouping terms, we can express this equation as

$$\Delta C_t = \gamma_0 \gamma_2 \Delta D_t + (\gamma_1 - 1)\left[C_{t-1} - \left(\frac{\gamma_2 + \gamma_3}{1 - \gamma_1}\right) D_{t-1}\right] + \gamma_4 U_t + e_t \tag{8}$$

By evaluating Eq. (8) at its equilibrium ($C_t = C_{t-1}$, $D_t = D_{t-1}$) and comparing with Eq. (6), we see that the ratio $(\gamma_2 + \gamma_3)/(1 - \gamma_1)$ can be identified with the coefficient β, and that the expression in brackets is the deviation

[12]Because unmarried women are more likely than married women to hold jobs (Nakosteen and Zimmer, 1989), one might wonder whether some or all of the divorce effects are related to women's greater labor force participation, a possibility that is given particular relevance to the present analysis by research indicating that women's employment is positively related to crime in Australia (Kapuscinski *et al.*, 1998). Although women's labor force participation and the divorce rate are positively correlated in the Unted States, they behave rather differently, and the women's labor force participation does not move together with the homicide rate or the robbery rate; the divorce rate does.

of the series from its equilibrium in the cointegration model. Equation (8) can thus be written more compactly as

$$\Delta C_t = \gamma_0 + \gamma_2 \Delta D_t + \varphi v_{t-1} + \gamma_4 U_t + e_t \tag{9}$$

where $\varphi = \gamma_1 - 1$ and v represents the quantity in brackets in Eq. (8). This "error correction" representation of Eq. (7) expresses short-run change in crime rates as an additive function of short-run change in the divorce rate, an error correction term that maintains the long-run tendencies of the model, and the exogenous variable U_t. Additional independent variables, such as a term in U_{t-1} or ΔU_t, can be added to the equation if that is appropriate, so long as they are $I(0)$.

Two points regarding Eq. (9) are noteworthy. First, if the coefficient φ is nonvanishing, the omission of the error correction term could lead to omitted variable bias. This term would be absent were one naively to write a simple regression equation in which a differenced divorce rate causes a differenced crime rate. Thus, differencing to deal with nonstationarity is not the recommended procedure when a pair of variables is cointegrated. Second, because all the variables in Eq. (9) are stationary, the equation can be estimated using OLS.

Equation (9) is the basis for the Engle–Granger two-step estimation procedure: one first estimates Eq. (6) to obtain the residuals, then uses them to estimate Eq. (9) (Engle and Granger, 1987). I carried out Engle–Granger estimations for various models involving, in addition to divorce, several stationary variables that might be expected to influence either criminal motivation or opportunity or both. For this purpose I chose the percentage of males between age 15 and age 29, the unemployment rate (various combinations of contemporaneous, lagged and differenced, but never all three at once, as only two of the three are linearly independent), and the duration of unemployment. This list of variables could, obviously, be extended, but with 51 observations, there are limits to what is practical. OLS estimates of these models are shown in Table II (homicide) and Table III (robbery).[13] F tests for those sets of models that are nested show that the most parsimonious and best-fitting models for both homicide and robbery are B (unemployment lagged by 1 year) and D (duration of unemployment). Adding additional measures of unemployment fails to yield a significant improvement in fit to these models.

The graphs in Figs. 3 and 4 show the actual changes in homicide rates and robbery rates alongside the rates predicted on the basis of model B.

[13]On the basis of LaFree's (1998, pp. 115–132) discussion of inflation, I also considered models in which percentage change in the consumer price index was used as a predictor, but it did not make a statistically significant contribution to the regressions. Estimates for these models are not listed in Tables II and III.

Table III. Error Correction Model Estimates of Models for Robbery Rates, 1946–1997[a]

Independent variable	Model						
	A	B	C	D	E	F	G
Constant	−.72	−2.33	−1.71	22.15	31.65*	−1.24	5.38
	(.95)	(.24)	(.17)	(1.98)	(2.38)	(.10)	(.34)
ΔDivorce	7.69**	7.21**	7.42**	2.61	2.85	3.95	4.00
	(3.00)	(3.62)	(3.58)	(.76)	(.83)	(1.23)	(1.24)
Error	−.13	−.11	−.12	−.02	−.02	−.12	−.11
	(1.51)	(1.70)	(1.73)	(.22)	(.22)	(1.43)	(1.34)
PCTM1529	105.41	174.30**	164.86**	90.13	32.40	204.97**	166.38*
	(1.52)	(3.34)	(2.87)	(1.74)	(.48)	(3.31)	(2.05)
U_t	−3.71*	—	.60	—	2.63	—	1.44
	(2.59)		(.42)		(1.30)		(.74)
U_{t-1}	—	−6.27***	−6.60***	—	—	−6.69**	−6.28**
		(5.97)	(4.99)			(2.91)	(2.64)
DUR	—	—	—	−3.10***	−3.97***	−.36	−1.00
				(4.74)	(4.29)	(.32)	(.71)
R^2	.34	.58	.58	.53	.54	.61	.61
DW	1.43	1.35	1.36	1.12	1.39	1.32	1.31

[a]Coefficients are unstandardized regression coefficients; figures in parentheses are values of Student's *t*.
*$p < .05$
**$p < .01$
***$p < .001$

The models do a fairly good job at predicting changes in both rates, while failing to predict the sharp drop in the rates that occurred after 1993. Significantly, the increases of the 1960s are explained without any criminal justice system variables in the model. There is little here to suggest, for example, that Supreme Court decisions entitling indigent defendants to attorneys (*Gideon* v. *Wainwright*, 1963; *Escobedo* v. *Illinois*, 1964) and requiring the police to issue warnings to suspects (*Miranda* v. *Arizona*, 1966), had anything to do with those increases. Nor is there an unexplained increase in crime associated with the suspension of executions between 1967 and 1976 while the constitutionality of capital punishment was being challenged in the courts. I do not mean to imply that law enforcement variables had no effect on crime whatsoever, only that they do not seem to have been responsible for major shifts in levels of crime in these years.

The value in Tables II and III show that short-run increases in the divorce rate tend to produce short-run increases in both homicide and robbery (though in model D for robbery, the coefficient for divorce is not statistically significant). As expected, the error correction terms are negative in all the models, indicating that divorce and crime tend to move together—

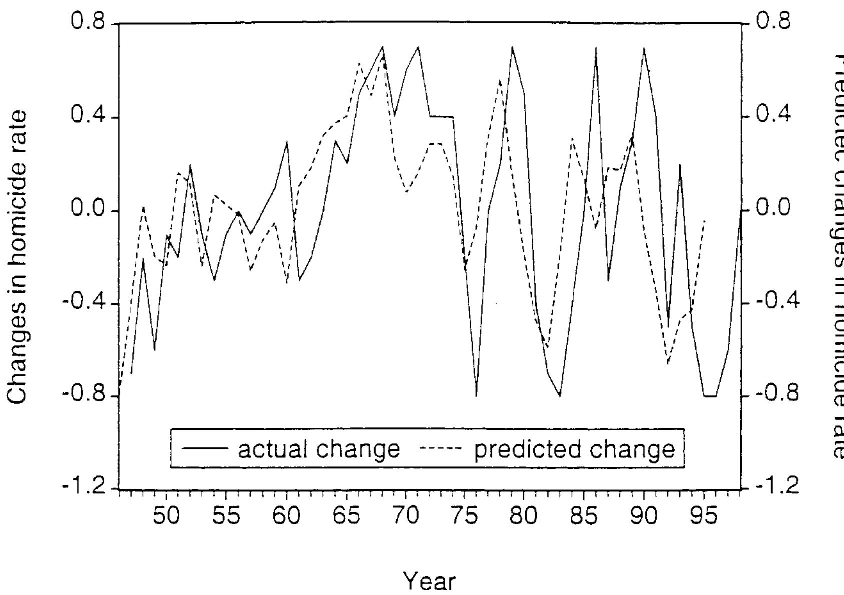

Fig. 3. Actual and predicted changes in homicide rates.

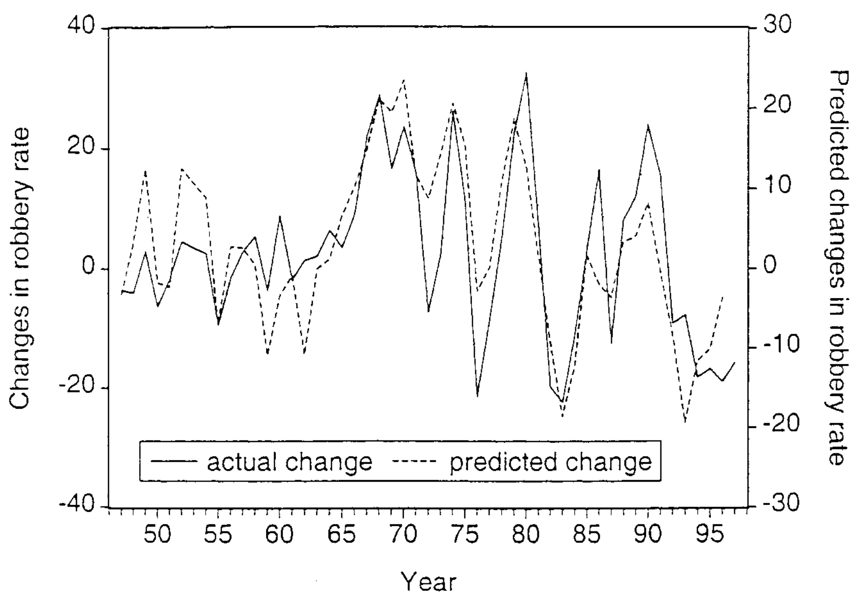

Fig. 4. Actual and predicted changes in robbery rates.

though in model D for robbery the coefficient is very small and not statistically significant.

The negative signs of the terms involving lagged unemployment and unemployment duration are opposite to what is expected of motivational effects, consistent with other studies (Ruhm, 1999; Raphael and Winter-Ebmer, 2001). Possibly the greater collective hardships of high or protracted unemployment strengthen social solidarity, reducing crime. Or it may be that unemployed people—especially those who have been unemployed for a long time—spend more of their time in public places, socializing in parks, and loitering and begging on the streets, discouraging crime by their presence. Consumption of alcohol rises in times of prosperity and drops when unemployment rises (Ruhm, 1995). Alcohol is a known disinhibitor and induces belligerence in some individuals. Unemployment might, then, curb violent crime by reducing the consumption of alcohol.

If the contemporaneous effect of unemployment measures the opportunity effect of unemployment, there is no unequivocal evidence for such an effect. Once the duration of unemployment or the lagged effect of unemployment is controlled, contemporaneous unemployment does not make a significant contribution.

How do these results compare with those of C-L (1985) and Land *et al.* (1995)? They found, in their first-differenced model incorporating both U_t and ΔU_t, that the instantaneous effect of unemployment was significantly negative, while the effect of change in unemployment was not. They also found a negative contemporaneous effect for robbery. If the Land *et al.* (1995) results are reparametrized in terms of U_t and U_{t-1} rather than U_t and ΔU_t to facilitate comparison of the estimates, their results are entirely consistent with mine: as discussed above, they find a contemporaneous effect that is positive, but very small and not statistically significant, and an appreciable negative effect of U_{t-1}.

For reasons that should be obvious from what has already been said, the results presented here are more credible and more informative than those presented by C-L (1985) and Land *et al.* (1995). The equation on which I base my analysis better represents a plausible theoretical model. The estimation procedure does not eliminate the drift in crime rates from the analysis but explains it. The resulting models reproduce the homicide and robbery rates reasonably well except that they do not predict the sharp drop in crime rates of the past few years.

That said, I do not want to exaggerate the strength of my findings. Because the correlations among unemployment, lagged unemployment, and duration of unemployment are high (between .75 and .80), it is difficult to assess their simultaneous effects with just 50 observations. The Durbin–

Watson statistics in Tables II and III lie in the "inconclusive" region, so the potential for omitted variable bia exists.[14]

3. USERS OF THE PARADIGM

The procedures developed by C-L for analyzing time series have been put to use by several researchers, notably Devine *et al.* (1988), Smith *et al.* (1992), and Britt (1994, 1997). Devine *et al.* use time series to study the impact of several variables on U.S. crime rates between 1948 and 1985. Following the standard recipe, Devine *et al.* difference the crime rates to eliminate what they consider trends. Probably the trends are actually manifestations of drift, but that is a distinction without a consequence here, as drift can be eliminated by differencing. In one set of models they introduce as independent variables change in the rate of male unemployment, the rate of inflation, the change in public relief, and the change in the imprisonment rate. Because all independent variables are measures of change, these models take the form of "change causes change" and do not raise problems of interpretation. In another set of models, Devine *et al.* add a static measure of opportunity and find that it makes a significant contribution to homicide, robbery, and some of the burglary models. However, as I have already argued, the use of a static measure of opportunity to explain change in the crime rate makes little sense.

Britt (1994) and Smith *et al.* (1992) apply the C-L paradigm to study the age-specific effects of unemployment on crime. This is a potentially valuable undertaking because recent crime rate changes have been highly age specific: increases in the murder rate in the years immediately after 1985 were restricted largely to people younger than age 25 (Blumstein and Rosenfeld, 1998), and recent decreases have been confined largely to the same age bracket (Butts, 2000). In these circumstances, an aggregate analysis that did not distinguish younger from older offenders could be quite misleading. Both studies take the number of arrests as a measure of the number of crimes committed, and regress it on the unemployment rate and the change in unemployment rate. Britt restricts his analysis to 16–19 year olds between 1958 and 1990 and considers all index crimes; Smith *et al.* consider four age categories—16–19, 20–24, 25–34, and 35–44—examining homicide, robbery and burglary. Each measures the crime rate by the arrest rate, and regresses it on the unemployment rate and the change in unemployment

[14]However, I did carry out Cochrane–Orcutt estimations of the error correction models that included first-order autocorrelations among the residuals, and the results were very similar to those presented in the text. Omitted variables often lead to serially correlated errors.

rate. Denoting arrests by A, and age and year by the subscripts i and t, this implies the estimation of an equation that takes the form

$$\Delta A_{it} = b_0 + b_1 U_{it} + b_2 \Delta U_{it} + e_{it} \tag{10}$$

Their findings are consistent: the contemporaneous effect of unemployment is negative, and the change effect positive, for all offenses. I comment on these studies below.

3.1. Britt's Analysis of the Age–Crime–Unemployment Relationship

In a further extension of this line of research, Britt (1997) analyzes age-specific U.S. arrest rates for the seven index offenses for the years 1958–1995 to test two propositions concerning the impact of unemployment on crime found in Greenberg's (1977, 1985) theoretical treatment of the age–crime relationship. Quoted verbatim from Britt's paper, the two propositions are as follows: (H1): "The unemployment–crime relationship will vary by age group, where youth and young adults are expected to show a greater motivational (positive) effect of unemployment on criminal behavior" and (H2) "The unemployment–crime relationship will vary over time, especially for youth and young adults, where the motivational (positive) effect of unemployment is expected to increase over time."

For several reasons, Britt's research does not provide a satisfactory test of Greenberg's ideas or these propositions. Here I summarize and then criticize Britt's research. My discussion deals with the following issues: (a) the extraction of propositions to be tested from Greenberg's narrative, (b) the translation of the discursive propositions into regression equations, (c) the operationalization of the variables in the regression equations, (d) the use of aggregate data to test propositions about individuals, and (e) the choice of a time frame for conducting the analysis.

3.1.1. The Extraction of Propositions from Greenberg's Narrative

Greenberg's (1977, 1985) model for the age dependency of crime draws on two major pillars of criminological theory to explain the age distribution of crime and the manner in which it has changed historically: strain theory and control theory. The model posits three types of institutional involvement or lack of involvement as sources of strain. It asserts that adolescent theft "occurs as a response to the disjunction between the desire to participate in social activities with peers and the absence of legitimate sources of funds needed to finance this participation" (p. 197). This is the first source of strain.[15] Greenberg argued that the intensity of this strain depends on age. Because adolescents lack the institutional affiliations that provide adults with alternative sources of self-esteem, participation in peer-focused social activities is expected to be more important to adolescents than to older adults.[16] The age stratification of American society has increased over time, so that this sort of strain should be greater now than in the past. These last two sentences form the basis of the two propositions Britt tests.

The model posits that a second source of strain—the denial of autonomy and the exposure to status degradation inflicted on students in school—is relevant primarily to the explanation of joy-riding, vandalism, acts of interpersonal violence, and seemingly irrational thefts in which the objects stolen are discarded or destroyed. Delinquency in response to this type of strain is also posited as contingent. It is primarily students who do not find compensating rewards and benefits from school (such as gratification from learning, extracurricular activities such as sports and clubs, socializing with peers, and future occupational rewards) who are expected to rebel against the restrictions and status degradations they encounter in school. Students who find school to be rewarding on balance, or who anticipate that it will enable them to achieve valued occupational goals, will tend to put up with its restrictions and status degradations. Any test of the theory must take these interaction effects into account. A third source of strain in the model is "masculine status anxiety," which intensifies at the end of the transition from adolescence to adulthood, when young males find that they cannot obtain jobs or discover that the jobs they can obtain pay badly and offer poor prospects for advancement. This source of strain is asserted to be relevant primarily to violent offenses. The control theory component of the argument focuses on the shift from juvenile court jurisdiction to the criminal court, which creates a substantially enhanced risk of serious punishment,

[15]Though not discussed by Greenberg (1977), the sale of illegal narcotics is another offense to which this reasoning might reasonably be expected to apply. Several studies have linked the sale of drugs by young people to their limited opportunities for earning income lawfully (Reuter *et al.*, 1990; Fagan, 1992; Myers, 1992; Hagedorn, 1994).

[16]This line of reasoning could be criticized on theoretical grounds for what it omits. Leisure-time social activities are usually considered discretionary. Someone—even a teenager—who cannot afford these activities might be unhappy but can survive: for most adolescents, paying for rent and food is not a serious problem, no matter what their employment status or personal finances may be, because someone else is paying for them. Homeless adolescents, who may have to steal to survive, would be exceptions (McCarthy and Hagan, 1992). In contrast, an adult who lacks financial resources may be unable to pay the rent or buy groceries. One might think that the strain induced by inability to pay for life necessities would be greater than the strain induced by inability to pay for leisure-time social activities. Greenberg's omission of this source of strain was intentional; a focus on life necessities would suggest that the peak age of involvement in theft should be in adulthood, not in adolescence, where it is in fact.

and on the informal sources of control associated with the institutions juveniles are able to establish or enter when they become adults: marriage and employment.

Several features of this argument are relevant to the present discussion. First, the relationship between strain arising from lack of income and employment is not simple, because employment is not the only source of legitimate income. Greenberg (1977, pp. 196, 198) remarks that as long as parents pay for their children's leisure-time social activities, children will not be strapped for cash even though they do not derive income from a job. It follows that if one wants to test Britt's propositions, one must assess variations in the extent to which parents subsidize their children. Parental ability and willingness to do this may well depend on their own employment circumstances.

When considering youths' aspirations toward conventional careers, the same problem arises. Fifteen-year-olds' aspirations may be influenced by the level of unemployment adults in their community experience, quite independently of their own employment statuses. If, for example, adolescents see that older adults in their communities are unable to find lawful jobs, they are likely to dismiss lawful employment as a possibility for themselves in the future. It follows that youthful involvement in crime may depend not only on their own employment, but on their parents' employment and income. Unfortunately, a model that incorporated the age-specific unemployment rates for both youths and individuals who belong to an older generation would be difficult to estimate with aggregate data because of severe multicollinearity: Britt notes that the correlation between the total annual unemployment rate and the unemployment rate for persons ages 16 to 19 between 1958 and 1990 is .98. This high correlation means that when Britt reports the effect of unemployment variables, we cannot be confident that the effects are those of the relevant age-specific unemployment rate, rather than those of the overall unemployment rate, or the unemployment rate for middle-aged workers who are parents of the 16- to 19-year-olds.

Second, the theory makes different predictions about the effects of unemployment for different kinds of crime. Unemployment is predicted to have the strongest effect on theft in adolescence but on interpersonal violence in young adulthood. For this reason, Britt's conclusion that the motivational effect of unemployment on homicide is greater at ages 18–24 (his Table III) than in earlier and later years is not at odds with Greenberg's theoretical model: quite the contrary (see further discussion below). Third, Greenberg's theory is explicitly multivariate. Estimating the effect of unemployment alone on illegality does not provide an appropriate test of a multivariate theory, unless the independent variables in the model are uncorrelated. The results reported in this paper suggest that the effect of unemployment on crime cannot be studied without taking into account the other factors that might influence crime rates.

In addition, a number of researchers have suggested that there is a relationship between adolescent employment and school experience. When high school students work long hours, their school performance may suffer, increasing their involvement in some kinds of illegal activities. Deficiencies in school performance may also lead some juveniles to take jobs. Because the Greenberg model posits irritating experiences in school to be criminogenic, these influences must be taken into account if the effect of unemployment is to be assessed. Regrettably, information about the relevant subjective variables is not to be found in nationally aggregated data sets. A better test of the model would use longitudinal data for individuals.

3.1.2. Formulating the Regression Equation

To test the two hypotheses reproduced above, Britt estimates equations in which differences in the age- and period-specific arrest rates are regressed on the age- and period-specific unemployment rates and on differences in these unemployment rates [see Eq. (10) above]. Each coefficient in the regression equation is expressed as the sum of a constant term and terms representing fixed effects for age and year. As written here, the subscripts for the regression coefficients arising from the fixed effects are omitted.

On carrying out the estimation, Britt finds no significant age dependence of the unemployment and differenced-unemployment coefficients for rape, larceny, and motor vehicle theft. For homicide and assault he finds the opportunity effect of unemployment, represented by b_1, to be negative for persons age 16 through 24 and positive for adults who are 25 or older. None of the motivational effects are significant for assault, but for homicide they are positive for ages 16–24, with the strongest effect at ages 18–19; at ages 25–34 they are negative but not significant.[17] Britt considers these homicide and assault findings to be "at odds with Greenberg's hypothesis that youth and young adults will have greater motivation to commit crimes in response to unemployment." Actually, Greenberg formulated no hypotheses about the age dependence of the opportunity effect; moreover, findings about the opportunity effect have no bearing whatsoever on the existence or nonexistence of a motivational effect, or about its strength.

[17]In fact, this is not exactly what Britt says about his findings. He says that the motivational effects for homicide have "a greater motivational effect on adults (25 years and older)." This statement is inconsistent with the figures in Britt's Table III. I strongly suspect that the reference to a motivational effect in this passage reflects an attention lapse while writing and base my remarks on the table, rather than the text.

Britt finds weak support for the motivational effect of unemployment on larceny being stronger for 16–17 year olds than for older people. The motivational effects of unemployment for rape, larceny, and motor vehicle theft did not vary significantly by age. For homicide, they were higher at ages 18–24 than at younger and older ages; for robbery and burglary they were higher at ages 20–24 than at earlier and older ages. Britt summarizes these findings by saying that they are generally consistent with the hypothesis that younger persons who are more likely to be excluded from the labor market will be more motivated to commit acts of theft. In contrast, the effect of unemployment on homicide and aggravated assault shows a greater motivational effect for older persons, suggestive of the possibility that adults may be faced with additional psychological pressures immediately upon being unemployed, which are not as pressing for younger individuals but lead to increased chances of homicide for adults. In fact, the homicide effects (strongest at ages 18–24) are exactly what Greenberg (1977) predicted as a consequence of masculine status anxiety.

Tests of the second hypothesis can be summarized more briefly: Britt finds no evidence that the impact of unemployment has been increasing over time. All these conclusions rest on the validity of Britt's translation of the discursive formulations of opportunity theory and motivational theory into the regression equation given in Eq. (10). In Section 2 I argue that this specification does not satisfactorily represent motivation theory; those arguments apply here as well. The equations that Britt and Smith *et al.* (1992) estimate, then, are not the equations that are appropriate for testing the theoretical ideas they wish to test. The coefficients they obtain are not measures of motivational effects or of opportunity effects.

This is not the only difficulty. It is implicit in Eq. (10) that opportunity effects are age specific. That is, the motivational contribution to the arrest rate in a given age bracket is assumed to be influenced only by unemployment levels of individuals in the same age bracket. This is as dubious theoretically as the notion that the motivational effects of unemployment are age specific (discussed earlier). Residences left unattended when a resident takes a job are easier to burglarize than residences where someone is cleaning and cooking during the day. People who are working at a job outside the home may have increased exposure to assault at the hands of strangers. These effects, however, are not expected to be age specific. Thus, if someone of a given age takes a job, leaving her apartment unattended while she is working, she is increasing her vulnerability to burglary and assault by perpetrators of all ages.

Of course, some opportunity effects of unemployment may be age specific. If teenagers are unemployed because they are attending school, they will be spending a sizable fraction of their waking hours in the company of others of roughly the same age, who are unemployed for the same reason. Their vulnerability to assault or theft at the hands of their peers may be influenced by the unemployment rate of their peers, but not so much by the unemployment rate of persons in other age brackets. Likewise, people who are working have opportunities to engage in employee theft that should be relatively uninfluenced by the unemployment rates of people in other age categories. But these possibilities do not refute my argument that for many kinds of crime, the unemployment rate in a given age bracket should affect opportunities of persons in all age brackets to commit crimes. These cross-age effects of unemployment must be taken into consideration in constructing a model. Failure to do so makes the interpretation of findings uncertain.

3.1.3. Operationalization of Variables

In testing a theory empirically, it is important that the variables be operationalized in such a way that they reasonably represent the variables in the theory. Serious discrepancies inevitably raise questions about the meaningfulness of the test. Britt justifies his use of arrests as a measure of crimes committed by citing studies that the age distribution of arrests is similar to the age distribution of offenses, However, the age categories used in some of these studies are crude (e.g., 21 and over), and so provide only a limited basis for assessing the accuracy with which the age distribution of arrests parallels that of offending behavior. In a study that Britt does not cite, Patterson and Arguer (1993) conclude on the basis of self-reports that the age distribution of arrests is not the same as the distribution of offenses.

The possibility that the probability of an arrest following the commission of a crime could be influenced by unemployment rates must also be taken seriously. Using data from the National Longitudinal Study of Youth, Glasser and Sarcerdote (1999) find that the effect of the local unemployment rate on whether the subject stole something worth less than \$50, shoplifted in the last year, or had income from crime in the last year was negative but not statistically significant. The effect on whether the subject was charged with a crime or ever convicted (also negative), however, was highly significant. This result suggests the possibility that local enforcement practices are in some way influenced by the level of unemployment in a community. If this is so, a regression of arrest rates on unemployment might actually be measuring changes in enforcement practices, not changes in motivation or opportunities.

The operationalization of unemployment in all these studies is also problematic. Without discussion, Britt, like C-L and Devine *et al.*, simply takes unemployment rates produced by the U.S. Department of Labor's

Bureau of Labor Statistics as valid representations of unemployment as a theoretical variable. A closer examination of the way unemployment rates are defined demonstrates that this equivalence cannot be taken for granted.

As summarized above, in Greenberg's model, theft is a response to the strain associated with the exclusion of juveniles from the world of full-time adult work. When they get older, this strain diminishes for those young adults who are able to find full-time jobs. This claim cannot be tested adequately with unemployment rates produced by the Bureau of Labor Statistics. The Bureau of Labor Statistics derives unemployment rates from responses to the Current Population Survey of the civilian noninstitutional population that is 16 years of age or older. Respondents are counted as unemployed if they did not hold a job during a particular calendar week (Sunday through Saturday of the week that includes the 12th day of the month), "were available for work, except for temporary illness, and had made specific efforts, such as contacting employers, to find employment sometime during the 4-week period ending with the reference week," or "were waiting to be recalled to a job from which they had been laid off" even if they were not looking for a job (U.S. Department of Labor, 1997, p. 5). These criteria exclude from the ranks of the unemployed discouraged individuals who have given up looking for a job or who never sought one because they thought it unlikely that a search would be successful. Housewives and students who are not working or looking for a job because they are in school are not classified as unemployed. Part-time workers who are underemployed because they want a full-time job but cannot find one are not counted. Youths who are less than 16 years old are not counted because in most states they would be barred by law from most jobs (Nixon, 1968; U.S. Department of Labor, 1997). Individuals who are working for very low wages are also not counted. Moreover, some youth may be employed part-time at jobs that could hardly qualify as part of "the world of adult work" (e.g., babysitting, mowing lawns). From the point of view of testing the Greenberg model, many of these individuals should be counted as unemployed because they will not earn enough from their jobs to meet their subjectively defined target level of expenditures. But they are not counted by the Bureau of Labor Statistics criteria.

Many criminals hold such jobs. In the RAND study of persons arrested in Washington, DC, for selling drugs (the great majority of them being young black males), roughly two-thirds were employed at the time of their arrest, but at low-wage jobs in which the median pay was $800 a month. Narcotics trafficking supplemented this income (Reuter *et al.*, 1990). Other studies have reached similar conclusions (Greenberg, 2001). Crutchfield and Pitchford (1997) found that young adults employed in the secondary sector, where wages were low and employment irregular, had higher levels of criminal involvement than those employed in the primary sector. Workers who expected their employment to be of long duration were less likely to engage in crime. To complicate matters further, some of those counted as unemployed, along with many who are considered outside the labor market, may be working "off the books," in the so-called informal economy, or at illegal occupations, such as prostitution or drug selling (Sassen-Koob, 1989; Reuter *et al.*, 1990; Fagan, 1992, 1994, 1997; Edin and Lein, 1997, pp. 145–146, 172–178). The number of state and federal prisoners serving sentences for drug offenses—277,859 in 1997—most of them for selling (Mumola, 1999, p. 2), suggests that the number of individuals earning money illegally may be quite large.

The lack of correspondence between a theoretically relevant conception of unemployment and the definition employed by the Bureau of Labor Statistics becomes evident when one computes the correlation between the proportion of the total civilian population that is employed and the civilian unemployment rate. One might think, naively, that, by definition, the employment rate and the unemployment rate would be negatively correlated. If one drops, the other should rise correspondingly. Yet the correlation between these variables between 1950 and 1997 is a *positive* .513. This is possible because someone who is not working will not be counted as unemployed if not considered to be in the labor market. When large numbers of people are newly entering the labor market (as women have been doing in the past few decades), it is possible for the employment rate and the unemployment rate to rise simultaneously (Cook and Harkin, 1985).

Were the undercount to be uniform across age categories and years, it would make no difference that officially defined unemployment failed to coincide with exclusion from the world of full-time adult work: the two variables would be exactly proportional. However, there is no reason to think that this is so (Bowen and Finegan, 1965; Tella, 1965; Dernberg and Strand, 1966). The proportions of individuals who are defined as not in the labor force because they are in school, or keeping house, or retired surely vary by age. To use the official unemployment statistics is, therefore, to risk serious systematic bias in the analysis.

Realizing that the employment–unemployment contrast might be inadequate, Phillips *et al.* (1972) analyzed age-specific U.S. arrest rates for property offenses for the years 1952–1967 using this contrast and, also, using the contrast "in the labor force–not in the labor force." Models based on the latter contrast had quite a bit more explanatory power. Phillips *et al.* note that the labor force measure may be less transitory, as it is based on past as well as current work status. This measure merits further exploration.

3.1.4. The Level of Analysis

All the authors under discussion here use nationally aggregated data in their analyses. When one simply wants to determine whether changes in certain variables characteristic of a nation affect other variables characteristic of that nation, this procedure is unexceptional. It is more problematic, however, when adopted as a means of testing theories about individuals.[18] Criminal motivation theory is a theory about individuals, and opportunity theory, though it involves features of a community, rests on arguments about how those features affect the behavior of potential offenders, making it easier or more difficult for them to violate the law.

It has been understood for decades that relationships found in aggregated data may not hold for individuals (Robinson, 1950; Langbein and Lichtman, 1978). Evidence that arrest rates are high when unemployment is high, for example, need not imply that the unemployed have a higher rate of arrest than the employed. Conversely, a relationship found for individuals need not appear in aggregate data. Teenagers and young adults have a higher likelihood of committing homicide than those who are younger or older, yet national homicide rates for nations are not always elevated when the percentage of the population in these age brackets is high (Gartner, 1990; Gartner and Parker, 1990; Marvel and Moody, 1991; Pampel and Gartner, 1995). Males are more likely than females to commit crimes of violence, but in American cities, the sex ratio does not significantly influence the violent crime rate (Messner and Sampson, 1991). Some analyses of crime rates in SMSAs have found that the percentage of young males in the population either has a *negative* effect on crime (Crutchfield *et al.*, 1982; Messner and Blau, 1987) or fails to make a significant contribution (DeFronzo, 1983), contrary to what is found in studies of individuals. Someone who failed to find an aggregate relationship between the percentage of young people and the crime rate, and inferred that among individuals age was unrelated to crime, would reach a mistaken conclusion.

One can see why this might happen by considering the propensity to steal C_{ij} of individual i in community j as a function of his or her own wealth W_{ij} and the mean wealth of the community. We allow for the possibility that the effect of an individual's wealth might depend on the mean wealth of the jurisdiction in which he or she lives. Writing a regression equation for these

[18]In a private communication, Ken Land mentioned to me that his papers were not intended to test theories about individuals, only to examine the relationships among macro variables. The discussion of motivation in his papers involves reasoning about individuals. Motivation in the Cantor and Land papers is conceptualized as a characteristic of individuals. To the extent that his findings are interpreted as bearing on motivation to commit crime, they are tests of ideas about individuals.

effects, letting a bar over a variable represent its mean in jurisdiction j , and letting u represent the residual, we have

$$C_{ij} = a + b_1 W_{ij} + b_2 \bar{W}_j + b_3 W_{ij} \bar{W}_j + u_{ij} \tag{11}$$

Summing over individuals in each jurisdiction and dividing by the number of individuals in each jurisdiction gives us

$$\bar{C}_j = a + b_1 \bar{W}_j + b_2 \bar{W}_j + b_3 W_j^2 + \bar{u}_j = a + (b_1 + b_2)\bar{W}_j + b_3 W_j^2 + \bar{u}_j \tag{12}$$

With aggregate data alone, the individual coefficients b_1 and b_2 are not identified (only their sum is), and the interaction term in Eq. (11) cannot be distinguished from a quadratic contextual variable. As a result of these limitations, aggregate data alone cannot distinguish between the effect of an individual's unemployment on his or her own criminal behavior and that of the unemployment rate in the community. Moreover, nonlinear terms, product terms, and ratios do not aggregate in any simple way. That is, if an equation for individuals involves terms such as X_{ij}^2, $X_{ij} Y_{ij}$, and X_{ij}/Y_{ij}, aggregation to the level of the jurisdiction will not, in general, lead to an equation with corresponding terms involving the powers, products, or ratios of the averaged variables in each jurisdiction (Greenberg and Kessler, 1981). This has immediate implications for analyses of crime rates and unemployment rates, each of which is defined as a ratio.

It follows that when one wants to test a theory formulated for individuals, it is preferable to obtain data for individuals, not just data for aggregates. Social scientists have sometimes avoided doing so because it is difficult, but also because they have been misled by Durkheim's (1951) fallacious argument that a rate characteristic of a jurisdiction is a collective phenomenon that cannot be explained by the individual characteristics of the people living in that jurisdiction. A rate is computed by adding up the contributions of individuals. Those contributions may, of course, be affected by characteristics of the aggregate. It is possible to work with individual data and yet incorporate contextual effects into one's model, as is done in Eq. (11). Thus, methodological individualism need not imply a rejection of the sociological axiom that the collective properties of the groups in which people live are consequential.

When contextual effects are considered, it will often be the case that the relevant context is something smaller than the nation. In studying the opportunity effects of unemployment, for example, it is likely to be the local unemployment rate that is relevant; few prospective criminals will be enabled to commit crimes by opportunities arising from changes in unemployment in distant regions. Nor need the collection of individual data imply that individual behaviors are statistically independent of one another. With appropriate data, one could take into account the influence that respondents (e.g., best friends or fellow gang members) have on one another's activities.

Whether an individual-level model explains variation among aggregate units, such as SMSAs, states, or nations, is a question separate from the validity of the model. A theory about individuals might be correct in the sense that every assertion it makes about the effects of variables in the theory is valid, but it might be incomplete, failing to incorporate additional variables or effects that are also important. It might, for this reason, fail to explain aggregate-level variation fully. When one wants to see whether an individual-level equation accounts for aggregate differences, one must first estimate the individual-level equation, use it to generate predictions for the dependent variable that can be aggregated, and then compare the predictions for the aggregate with the reality. This is not the same as estimating a regression equation with the aggregated variables.

Another disadvantage to the use of aggregate data in criminological analyses concerns the distinction between rates of participation (whether someone violates the law) and frequency of violation on the part of the violators (Blumstein *et al.*, 1986, p. 55, 1988). The number of crimes committed is the product of the number of violators and the mean frequency at which they violate the law. The two can vary independently. When analyzing aggregate crime rates it is possible to study only the product. With individual data it is possible, at least in principle, to study both.

A second issue is not inherent in the use of aggregate data but is frequently characteristic of it: information about the mechanisms by which independent variables bring about their outcome is often lacking. C-L noted that they did not have direct information about criminal motivation and opportunities. Their analyses, like those by Devine *et al.* and Britt, make assumptions about the effects of unemployment on individual behavior yet cite no evidence to support these assumptions. Is it true that unemployment increases the time people spend at home? Studies of individuals have the potential for collecting this sort of information.

3.1.5. The Time Span of the Study

To determine whether there is a trend in the effect of unemployment on crime, as hypothesized in H2, it would be necessary to examine the relevant time period. Greenberg's discussion does not make the ahistorical claim that there is a general tendency for unemployment to influence crime more and more strongly as one decade follows another until the end of time. He claims that specific, historically located developments have had that effect. In arguing that the exclusion of juveniles from the world of adult work is of greater importance now than in the past, Greenberg called attention to the particular importance of child labor laws and mandatory school attendance legislation adopted during the first few decades of the twentieth century, as well as post-World War II affluence, with its culture of consumerism, and marketing targeted to adolescents (Greenberger and Steinberg, 1986). All these developments were in place by 1958, the first year in Britt's time series. Outside the South, for example, high school enrollment rates and graduation rates rose rapidly between 1910 and 1940, then leveled off (Goldin, 1998; Goldin and Katz, 1998). To study the effect of these developments it would be necessary to start the time series much earlier.

To be sure, college attendance rose substantially during the years of Britt's study, but one would not expect this trend to have the same consequences for the crime rate as the earlier increase in high school attendance, some of which was required by state law. Many college students work or are supported by parents, scholarships, and loans. Almost all are in college because of its immediate and long-term rewards. Historically, this has not been as true of high school attendance.

In explaining the historical trend toward a greater concentration of criminality in the youthful years, Greenberg notes that exclusion of juveniles from the world of adult work had been going on gradually for a long period of time—perhaps a century—and therefore might plausibly be explained not merely as the result of child labor and mandatory school attendance laws but, rather, as a consequence of a capitalist economy's failure to generate sufficient employment to put youth and adults to work. Recent research allows us to flesh out this sketchy explanation. Compulsory school attendance legislation, combined with child labor laws, did increase school attendance at the start of the twentieth century (Margo and Finegan, 1996). Child labor dropped very substantially over a period of several decades, starting around 1870 or 1880 (Carter and Sutch, 1996), though only in part because of legislation or a weakening demand for labor. Silberman (1965) charts the declining labor force participation of teenagers in the first half of this century, noting that by far the largest component of the drop occurred for farm workers. The decline of the family farm, accompanied by migration to the city, eliminated their jobs. In addition, as parents' incomes rose, they increased their investment in their children's future by educating them for longer periods, in the process, deferring their entrance into the labor market.

Though in recent decades the American economy has endured large numbers of plant closings and downsizings (Sordius *et al.*, 1981), resulting in the loss of millions of jobs, unemployment rates have not trended upward because an even larger number of new jobs has been created. Many of the new jobs, however, are in the service sector, teach few skills, pay badly, are repetitive and boring, carry few or no benefits or opportunities for advancement, and offer only irregular employment (Bluestone and Harrison, 1982;

Kasarda, 1988, 1989, 1990, 1992; Burtless, 1990; Reubens *et al.*, 1981; Taylor, 1997). The proportion of students working at such jobs has increased in recent decades: in the 1950s, about 5% of high school students worked after school; in the 1990s, this figure increased to about 25%. Possibly in response to the increased supply of teenage workers, teen wages have fallen relative to adult wages. There is also evidence that employers have substituted young workers for older ones in response to the wage differential (Kalachek, 1969; Hills and Reubens, 1983; Greenberger and Steinberg, 1986, pp. 65–68; Rothman, 1992). According to one observer, the youths taking these jobs are largely "media-savvy teenagers hungry for designer clothes and cellular phones, or saving for the rising costs of college" (Thomas, 1998).

Black males are the one exception to the upward trend in high school students holding part-time jobs; for them the trend has been downward. Greenberg noted that the trend in labor force participation had been different for black and white youths: black teenage labor market participation dropped dramatically between 1950 and 1973, while for white teenagers it remained stable. After noting this difference, Greenberg did not comment further on it. This article provides an occasion for doing so. For the years 1958–1995 (the time spanned by Britt's data), black male labor force participation rates continued to decline for 16–19 year olds, while for white males they *rose*. This racial difference in employment trends calls for an explanation (Adams and Mangum, 1978) that goes beyond Greenberg's off-hand reference to the "disaccumulationist" phase of the capitalist mode of production. Developments such as the loss of manufacturing jobs to low-wage foreign countries, cuts in public sector employment, increases in the minimum wage, skill deficits among youth educated in center-city public schools, spatial mismatch between jobs and potential workers, and the high incomes now available to youths selling illegal narcotics must figure in such an explanation (Goodman and Dolan, 1979, pp. 170–171; Wilson, 1987; Hagedorn, 1988; Hughes, 1989; Fagan, 1992, 1997; Fernandez, 1992; Kasarda, 1992; Peterson and Vroman, 1992; Hagan, 1997; Taylor, 1997).

With employment trends differing for black and whites youths, it is essential that a statistical analysis examine crime trends separately for blacks and whites, lest important effects be washed out in the mix. That the increases in homicide rates in the late eighties were larger for black males than for white males (Blumstein and Rosenfeld, 1998) may be related to the racial differences in employment trends. Presumably it is because of their more limited legitimate labor market prospects that black males were disproportionately drawn into street-level crack distribution, where unregulated competition has been accompanied by high levels of violent crime

(Fagan and Chin, 1990; Blumstein, 1995; Cook and Laub, 1998; Grogger and Willis, 1998).

4. CONCLUSION

I have identified numerous difficulties in the attempts to study the impact of unemployment on crime rates by analyzing nationally aggregated data. The regression equations in these efforts do not adequately represent the theoretical ideas they are designed to test, and the variables in the theory are not adequately represented by those available in official unemployment statistics. Nationally aggregated data are less than ideal when estimating relationships posited by theory to hold for individuals. For this reason, it is doubtful that much confidence can be placed in the conclusions reached in the studies discussed here.

The issues raised in this discussion transcend the handful of studies I reviewed. Recent advances in the econometric analysis of nonstationary time series suggest that many—perhaps most—sociological analyses of crime rate time series (and, very likely, other kinds of rates as well) suffer from serious methodological deficiencies. The methods they have used fail to reveal long-run tendencies or rely on misspecified models and improper estimation procedures. The cointegration revolution provides a solution to these difficulties. Researchers making use of this solution will still have to grapple with the other issues raised here, such as the operationalization of variables, the translation of ideas into mathematical representations, and the limitations of aggregate data for purposes of testing theories about individuals. Some of these difficulties may be harder to solve than the purely statistical problems posed by nonstationarity.

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