Deterrence and Public Policy: Trade-Offs in the Allocation of Police Resources

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I. Introduction

A large econometric literature has tested the implications of Becker's (1968) path-breaking article on the economics of crime. This literature typically focuses on a supply of crime-specific offenses, hypothesizing that the crime rate is related to the probability and severity of punishment for the crime, the expected benefits from the criminal activity, returns from alternative legal activities, and other socioeconomic factors. Many studies conclude that criminal behavior can be deterred by public sector law enforcement efforts.[1] Critics such as Cameron (1988) and Brier and Fienberg (1980) have exposed apparent inconsistencies between the theoretical analysis and the empirical findings, however. For example, they emphasize that many studies use various aggregate measures of police resources in equations intended to explain crime rates, only to find either no relationship or a significant positive relationship.[2] Such critics also note that whereas crime rates are generally negatively related to the probability of arrest using simultaneous equation estimators, the probability of arrest often does not seem to be significantly related to the level of police resources.[3] The failure of measures of police

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[1] For example, see Myers (1982, 1983), Phillips and Votey (1975), Sjoquist (1973), Hakim et al. (1984), Craig (1987), Trumbull (1989), Grogger (1991), Benson et al. (1992), Sollars et al. (1994), Cornwell and Trumbull (1994), and Levitt (in press, 1996a, 1996b).

[2] See for example, Swimmer (1974), Allison (1972), Thaler (1977), Hakim (1980), Buck et al. (1983); and see Cameron (1988) for other references. But see Levitt (1996) for discussion and counter evidence.

[3] For example, see Ehrlich (1972, 1973), Mathur (1978), and Carr-Hill and Stern (1973), Furlong and Mehay

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manpower or budget to negatively influence the crime rate directly, or indirectly through the probability of arrest, is unsettling and leads some critics, such as Cameron (1988, p. 308), to question the validity of the deterrence hypothesis. The following presentation makes three interrelated points to divert this criticism.

First, the econometrics of crime literature generally fails to recognize that these models are misspecified because they only account for a small part of total police activity. Police do more than simply arrest and deter criminals who commit reported (Index I) crimes—murder, forcible rape, aggravated assault, robbery, burglary, larceny, and motor vehicle theft—that provide the data for standard econometric studies. In fact, Index II crimes (all crimes except Index I offenses and minor traffic violations, including simple assault, narcotics, vandalism, vice, fraud, and major traffic violations) account for far more arrests than Index I crimes. Florida data from 1983 through 1987 are used below, for instance, and in 1987 the state had 117,029 and 511,568 Index I and Index II arrests, respectively. Given competing demands for scarce police resources, an increase in police budgets or other measures of aggregate resources does not necessarily increase the deterrence of reported crimes, not because policing is not a deterrent, but because policy makers do not necessarily allocate the new resources to deter those crimes [Benson et al. (1994)].

Second, although data are not available to directly examine the consequences of police resource allocation decisions, an indirect test is possible. The 1980s ''War on Drugs'' represents a substantial change in law enforcement policy that provides an opportunity to see whether changes in the allocation of police resources changes the effective deterrence of Index I crimes. The ratio of drug arrests (an Index II crime) to arrests for Index I crimes in the United States was maintained at about 0.25 from the end of the previous major offensive against drugs in 1970 until the mid 1980s, when it began to rise, reaching a ratio of 0.40 in 1989. To increase drug arrests, *ceteris paribus,* police have to shift some resources away from alternative uses, thereby reducing patrolling to prevent nondrug crimes and/or the ability to respond and make arrests after such crimes have been committed. Inasmuch as individuals contemplating crime are deterred by patrolling or view the risk of arrest as a deterrent, such changes will generate more crime [Cook (1979)]. Therefore, an examination of the changes in Index I crime resulting from the policy shift toward greater emphasis on drug crime provides a test of the deterrence hypothesis.[4]

Third, the focus on changing crime rates in the face of changing criminal justice policy departs from the usual econometrics of crime literature, which employs cross-section or time-series socioeconomic and criminal justice system data to explain crime

(1981), Wahlroos (1981), and Cameron (1988). Many econometric models have assumed that the crime rate depends on the probability of arrest, that the probability of arrest depends upon the aggregate level of police resources or manpower, and that the level of police resources demanded by citizens depends on the crime rate, so that these models have employed simultaneous equation-estimating techniques with at least two and generally three or more equations. Some recent empirical studies of the economics of crime have broken with the simultaneous equation-estimating technique, however. In particular, Layson (1985) and Trumbull (1989) both test for exogeneity using Hausman's (1978) test, confirmed it, and used OLS estimators. Benson et al. (1994) offer theoretical reasons to expect this exogeneity.

[4] Cross-section simultaneous-equation and recursive models of property crime suggest that greater levels of police resources directed toward the control of drug crime are associated with fewer resources for the deterrence of property crime, and therefore with higher property crime rates [Benson et al. (1992); Benson and Rasmussen (1991); Sollars et al. (1994)]. The question explored here is a time-series version of this cross-section result: Do changes in drug enforcement policy produce changes in reported crime? The 1980s drug war involved a substantial policy shift that allows this question to be answered.

rates using ordinary least squares (OLS) or simultaneous equation estimation procedures. By using panel data and considering changes in independent and dependent variables, the resulting estimates have the benefits associated with standard longitudinal fixed-effect models.[5] As a consequence, the bias in the estimated coefficients attributable to the omission of unobservables fixed over time may be eliminated.

The relevance of all three of the points emphasized above is born out by the empirical results presented below. First, the allocation of police resources clearly matters. In fact, the input mix makes a difference. After controlling for the reallocation of policing resources in Florida from Index I crime control to Index II drug enforcement over the 1984–1989 period, increases in police labor seem to have a deterrent effect on Index I crime whereas increased spending on capital apparently reduces deterrence. This suggests that police in Florida are ''overcapitalized,'' supporting policy advocates who contend that community-policing techniques (removing police from their patrol cars and putting them back on the beat) are more effective than patrol and waiting-to-respond methods. Second, and perhaps more significantly, Florida's ''war on drugs'' apparently has increased Index I crimes: In contrast to the ''drugs-cause-crimes'' arguments, it seems that drug enforcement can ''cause'' Index I crime because fewer police resources are available to control these violent and property crimes. Furthermore, as Levitt (in press) has explained, evidence of such trade-offs provides strong support for the deterrence hypothesis. These trade-offs between drug and nondrug crime control are also consistent with findings in Benson and Rasmussen (1991), Benson et al. (1992), and Sollars et al. (1994), where OLS and/or simultaneous equations models typical of the economics of crime literature are employed, but they are found here by using more appropriate panel techniques. Indeed, the third point of emphasis is that strong support is provided for using panel estimation techniques, and particularly fixed-effect models, to test the implication of the economic theory of crime, as in Cornwell and Trumbull (1994) and Levitt (in press). OLS estimates clearly can produce biased results.

The presentation of these findings is organized as follows. Section II develops a theoretical model of Index I crime deterrence, recognizing that police resources are scarce and allocated among competing uses to generate a reduced-form equation that is then tested empirically. The econometric implications of this approach, the data, and the hypotheses are discussed in Section III, Section IV details the empirical results, and conclusions appear in Section V.

II. The Production of Deterrence

Assume that the police are in the business of producing crime deterrence. By allocating capital and labor to control the various crime types, police influence the level of such crimes. Of course, there are also inputs to this production process that are not controlled by police, such as the opportunity costs of criminals, the probability of conviction given arrest, and the severity of punishment given conviction, as well as community

[5] See for example, Chamberlain (1982, 1983) and Hsiao (1986). Cornwell and Trumbull (1994) show that panel data provide better estimates of deterrence effects than usual cross-section econometric techniques. They do not address the police resource allocation issue that is the focus of this study, however. Levitt (in press), on the other hand, also uses panel data and considers the incentives of criminals to substitute among crime categories in the face of differences in arrest rates, one of the consequences of the allocation of police resources discussed here.

characteristics such as the age and income distribution of the population and degree of urbanization. More specifically, assume that

$$C_{Ijt} = C_{Ijt}\,(L_{Ijt}, K_{Ijt}, \mathbf{W}_{jt}, \mathbf{M}_{jt}) \tag{1}$$

$$C_{IIjt} = C_{IIjt}\,(L_{IIjt}, K_{IIjt}, \mathbf{X}_{jt}) \tag{2}$$

$$C_{Djt} = C_{Djt}\,(L_{Djt}, K_{Djt}, \mathbf{Y}_{jt}) \tag{3}$$

for each jurisdiction j during time period t, where C denotes the level of crime in each category, with subscripts I for Index I crimes, D for drug crimes, and II for nondrug Index II crimes. The police labor and capital allocated to the control of each subscripted crime type in each jurisdiction during the time period are represented by L and K, respectively, $\mathbf{W}$ is a vector of non-policing factors that influence the level of Index I crimes (i.e., the probability of conviction and the severity of punishment for Index I crimes, the opportunity costs of potential criminals, and the community characteristics) in a jurisdiction, $\mathbf{X}$ denotes a vector of jurisdiction-specific factors beyond police control that influence the level of nondrug Index II crimes, $\mathbf{Y}$ represents a vector of nonpolice factors that influence a jurisdiction's drug crimes, and $\mathbf{M}$ is a vector of those aspects of drug markets in a jurisdiction that affects Index I crimes.[6]

Inclusion of $\mathbf{M}_{jt}$ reflects the fact that there are a number of competing hypotheses that imply significant relationships between Index I crimes and various aspects of drug markets. First, the ''drugs-cause-crime'' argument implies that as the number of drug users increase there should be more potential nondrug criminals at large and the number of Index I offenses should increase.[7] An alternative hypothesis is that as the risk of arrest increases for those who commit drug offenses to earn income (dealers, producers, smugglers, etc.), holding the policing efforts against Index I offenses constant, there may be a substitution effect, because criminals seeking income find Index I property crimes *relatively* more attractive than drug distribution activities. It is relative incentives that matter at the margin, after all, not absolute incentives, and in a relative sense nondrug crime becomes less risky as drug arrests rise, *ceteris paribus*.[8] A related and perhaps more fundamental factor arises because the *ceteris paribus* does not hold: Criminal justice resources are scarce, so when an increasing portion of them are reallocated to the control of drug crime, the expected costs of committing Index I

[6] Some factors are likely to appear in all of the $\mathbf{W}_{jt}$, $\mathbf{X}_{jt}$, and $\mathbf{Y}_{jt}$ vectors, of course. For example, empirical studies of the economics of crime frequently find that the age distribution within a jurisdiction, urbanization, and income proxies like unemployment, average wages, and the portion of the population that is black are significantly related to crime rates, but these same factors are also likely to influence Index II crimes, including drug crimes. However, to keep the mathematical analysis as manageable as possible, the variables in each of these vectors will be treated as distinct single variables in the theoretical analysis. The $\mathbf{M}_{jt}$ vector will be treated in a similar fashion. This set of assumptions may leave out some theoretically relevant relationships. For instance, an $\mathbf{M}$ vector might reasonably appear in equation (2), because drug market activity may influence the propensity to engage in prostitution, for example [Johnson et al. (1995), p. 281]. Similarly, perhaps cross-effects between Index I and Index II crimes exist. However, such effects seem to be much smaller than the drug-to-Index I relationship, and the ultimate reduced-form equation derived below is not impacted significantly anyway, so, to keep the analysis manageable, we have chosen to ignore these potential relationships.

[7] The academic literature does not support the drugs-cause-crime assumption that drug consumption must be or predominantly is financed by property crime. Chaiken and Chaiken (1990) review the literature on drugs and predatory crime and report that there is no simple general relationship between high rates of drug use and high crime rates. Also see Rasmussen and Benson (1994).

[8] See Levitt (in press) regarding evidence of such substitution effects among other crimes.

crimes may fall. The substitution effect suggested above is enhanced, but in addition, potential criminals who are not active in the drug market will find Index I crime relatively more attractive and will substitute such crime for alternative uses of their time (e.g., legal activities).[9]

An empirical estimation of the relationships in equations (1) through (3) is not possible because there are several unreported variables. In particular, whereas Uniform Crime Reports provide estimates of $C_{\mathrm{I}jt}$, they do not have values for $C_{\mathrm{II}jt}$ or C_{Djt}. Furthermore, many $\mathbf{M}_{jt}$, $\mathbf{W}_{jt}$, $\mathbf{X}_{jt}$, and $\mathbf{Y}_{jt}$ factors are not quantifiable. Finally, data on total police labor and total capital (capital expenditures) are available, but the actual allocation of these inputs is impossible to determine. After all, whereas some police resources are intentionally focused on particular crimes (e.g., homicide divisions, vice, drug squads, etc.), some police resources also are involved in activities such as general patrol and/or response that may deter all types of crimes: Their actual allocation in making arrests is not determined until a crime is observed or reported. This may seem to imply that the ultimate allocation of these general patrol/response resources is determined by the supply of offenses rather than the decisions of police. In reality, however, police prioritize crimes even for general response resources, as demonstrated by substantial differences in average response times for different types of crimes [Rasmussen and Benson (1994), p. 34]. Furthermore, by allocating such resources geographically, police decision makers can change the relative focus of general patrol and response. If resources are reallocated to be concentrated in areas that are characterized by high levels of vice activity, for instance, they will deter vice activities more effectively, but simultaneously reduce deterrence for crimes that are relatively more prevalent in other areas.

Clearly, the missing variables problem is severe. However, it can be mitigated to a substantial degree by considering changes in resource allocation rather than their levels (thereby eliminating the need to control for fixed effects) and then by manipulating the model to provide a reduced form equation.

Relative factor productivity in the production of deterrence may differ across crime types. Therefore, assume that police allocate resources so that

$$1 + A = (\delta C_{Dj}/\delta L_{Dj})/(\delta C_{Ij}/\delta L_{Ij}) = (\delta C_{Dj}/\delta K_{Dj})/(\delta C_{Ij}/\delta K_{Ij}) \tag{4}$$

$$1 + B = (\delta C_{IIj}/\delta L_{IIj})/(\delta C_{Ij}/\delta L_{Ij}) = (\delta C_{IIj}/\delta K_{IIj})/(\delta C_{Ij}/\delta K_{Ij}) \tag{5}$$

where A and B are constants.[10] This is consistent with a number of different underlying behavioral models for police decision makers.[11] The minimization of total crimes for a given level of expenditures and a Niskanen (1968)-type budget maximization both can yield $(P_{Lj}/P_{Kj}) = (\delta C_{Dj}/\delta L_{Dj})/(\delta C_{Dj}/\delta K_{Dj}) = (\delta C_{\mathrm{I}j}/\delta L_{\mathrm{I}j})/(\delta C_{\mathrm{I}j}/\delta K_{\mathrm{I}j}) = (\delta C_{\mathrm{II}j}/\delta L_{\mathrm{II}j})/(\delta C_{\mathrm{II}j}/\delta K_{\mathrm{II}j})$, for example, where P_{Lj} is the price of police labor and P_{Kj} is the price of police capital; cross-multiplying the marginal deterrent values provides equations (4)

[9] Indeed, the probability of arrest for Index I property crimes is negatively related to the portion of policing effort directed at drugs in both cross-section simultaneous-equation and recursive models [Benson et al. (1992); Benson and Rasmussen (1991); Sollars et al. (1994)].

[10] These assumptions are suggested by Ram (1986) in the context of an effort to estimate a very different set of production relationships. Note that the time subscript is not included because the changes in labor and capital occur over time (i.e., between time $t - 1$ and time t).

[11] All of the empirical studies of Index I crimes (or any subset of Index I crimes) that use some measure of total police resources in the analysis must implicitly assume relationships similar to those in equations (4) and (5).

and (5) directly.[12] Alternatively, weights or priorities can be assigned to various crime categories to reflect expected costs of crime for the median voter or the expected political costs for the bureaucratic decision maker and then resources might be allocated in a fashion that minimizes the weighted costs of crimes.[13] In this case, A and B reflect the resulting weights.

To focus on changes rather than levels, take the total differentials of equations (1), (2), and (3) (note that total investment in police capital is $dK_j = dK_{Ij} + dK_{IIj} + dK_{Dj}$, and the total change in police labor is $dL_j = dL_{Ij} + dL_{IIj} + dL_{Dj}$) and substitute the resulting equations into the total change in crime $[dC_j = dC_{Ij} + dC_{IIj} + dC_{Dj}]$ to produce

$$\begin{aligned} dC_j = {} & (\delta C_{Ij}/\delta L_{Ij})dL_{Ij} + (\delta C_{Ij}/\delta K_{Ij})dK_{Ij} + (\delta C_{Ij}/\delta \mathbf{W}_j)d\mathbf{W}_j + (\delta C_{Ij}/\delta \mathbf{M}_j)d\mathbf{M}_j \\ & + (\delta C_{IIj}/\delta L_{IIj})dL_{IIj} + (\delta C_{IIj}/\delta K_{IIj})dK_{IIj} + (\delta C_{IIj}/\delta \mathbf{X}_j)d\mathbf{X}_j \\ & + (\delta C_{Dj}/\delta L_{Dj})dL_{Dj} + (\delta C_{Dj}/\delta K_{Dj})dK_{Dj} + (\delta C_{Dj}/\delta \mathbf{Y}_j)d\mathbf{Y}_j \end{aligned} \tag{6}$$

Now, drawing upon equations (4), (5), and (6) to make a series of algebraic manipulations and substitutions, an equation for dC_{Ij} can be derived[14]:

$$\begin{aligned} dC_{Ij} = {} & (\delta C_{Ij}/\delta L_{Ij})dL_j + (\delta C_{Ij}/\delta K_{Ij})dK_j + (\delta C_{Ij}/\delta \mathbf{W}_j)d\mathbf{W}_j + (\delta C_{Ij}/\delta \mathbf{M}_j)d\mathbf{M}_j \\ & + [1/(1 + B)](\delta C_{IIj}/\delta \mathbf{X}_j)d\mathbf{X}_j + [1/(1 + A)](\delta C_{Dj}/\delta \mathbf{Y}_j)d\mathbf{Y}_j \\ & - \{[1/(1 + B)]dC_{IIj} + [1/(1 + A)]dC_{Dj}\} \end{aligned} \tag{7}$$

Given sufficient data, an econometric estimation of this equation can be performed.

III. Econometric Properties of the Theoretical Model

The relationship in equation (7) is derived theoretically, but focusing on changes rather than levels also has important econometric properties. In particular, all econometric models suffer from omitted variable bias, and this problem is particularly severe in the economics of crime literature using cross-section jurisdictional-specific data in levels. Furthermore, some of these unobservables that vary over policing jurisdictions may be fixed through time. This suggests that the error term in a levels regression has two components: the jurisdiction specific component, ϵ_j, which is fixed over time, and a random error term, $\acute{e}_{jt}$, with zero mean and constant variance. If the omitted fixed effects are correlated with any of the regressors, then the estimates of the relevant coefficients are biased. This may be particularly troubling in a model intended to test deterrence hypotheses, because many unmeasurable community characteristics seem to either complement or hinder policing efforts. The missing variable problem can be mitigated to a substantial degree, however, by using panel data in a difference-form model that tests the above-derived theoretical model of *changes* in crime as a function

[12] It is contended in Benson et al. (1995) and Mast et al. (1997) that the allocation of police resources can best be understood in the context of the theory of bureaucracy that has evolved from Niskanen's (1968) work [e.g., Breton and Wintrobe (1982)].

[13] Levitt (1996b) also points to the political process as an important determinant of total police resource availability, using election cycles in a model of crime deterrence.

[14] Space considerations preclude the inclusion of the several steps involved in the algebraic derivation of equation (7). A ''Reviewer's Appendix'' detailing the derivation was prepared for referees, however, and is available from the authors upon request.

of the *changes* in explanatory variables.[15] For example, consider cross-section equations for two time periods, t and $t - 1$,

$$C_{Ijt} = \sum \alpha_i Z_{jit} + \epsilon_j + \acute{e}_{jt}, \tag{8}$$

$$C_{Ijt-1} = \sum \alpha_i Z_{jit-1} + \epsilon_j + \acute{e}_{jt-1}, \tag{9}$$

where α_i includes a constant and $i - 1$ coefficients corresponding to the variables in Z, and Z_k consists of $Z_1 = 1$ for the constant and the $i - 1$ variables suggested in the context of the discussion of equation (7). Subtracting equation (9) from equation (10), yields:

$$\Delta C_{Ij} = \sum \alpha_i \Delta Z_{ji} + \Delta \acute{e}_j \tag{10}$$

where Δ represents changes between time periods $[t - (t - 1)]$. This equation no longer involves ϵ_j. Thus, a primary econometric advantage of difference-form analysis is that the jurisdiction-specific fixed effects arising from unobservables fall out, thereby increasing the potential for unbiased estimates of the coefficients in the deterrence model. Furthermore, factors that change in the same way across all locations should be captured in the constant term and should not bias the coefficients of other variables. This does not solve all of the data problems, of course. It simply comes closer than many other researchers in the economics of crime have.[16]

One particular data issue deserves mention: There are no data available for the ΔC_{Dj} and ΔC_{IIj} variables in equation (7). This data limitation means that the estimation of equation (8) must involve an explicit assumption that C_{IIj} and C_{Dj} do not change significantly from one year to the next (i.e., $\Delta C_{IIj} = \Delta C_{Dj} = 0$) at the county level in Florida over the 1983–1987 period (see the discussion of data below). Note, however, that *an even more troubling assumption must be implicit in all cross-section or time-series econometric studies of crime.* In contrast to implicit assumptions in this literature, we are *not* assuming that the *levels* of C_{IIj} and C_{Dj} are not important but simply that year-to-year changes in C_{IIj} and C_{Dj} in jurisdictions (j) are not significant during the periods analyzed below.

There is little evidence regarding changes in the incidence of nondrug Index II crime. National data suggest that the level of drug crime *per capita* may not have been changing much over the period, however. In fact, estimates regarding national trends in drug use are conflicting. An apparent reduction in the drug-consuming population is indicated by such sources as The National Institute on Drug Abuse (NIDA) *National Household Survey on Drug Abuse* conducted in 1982, 1985, and 1988, for example. The estimated number of users of both marijuana (including hashish) and cocaine were rising through much of the 1970s, but the NIDA surveys indicate that the numbers of both began to decline in the 1979–1982 period and that the decline continued through the 1980s. But conclusions based on the NIDA surveys alone are biased because the

[15] With this missing-variables problem in mind, tests for fixed effects in the data employed below were performed. These tests, briefly discussed below and available from the authors upon request, support the expectation of unobservable fixed effects that bias several coefficients.

[16] See note 5 regarding other recent uses of panel data in economics of crime models.

surveys focus exclusively on ''households.'' They do not account for use among other groups who are not picked up in a household survey, some of which may actually involve large numbers of drug users (e.g., people in prison and the homeless). Other surveys suffer from similar biases (e.g., high school surveys do not account for dropouts). Thus, a more accurate picture of total drug market activity would have to account for other sources of information on drug use (e.g., hospital admissions data, surveys of prison populations, data on drug treatment populations, etc.).

Moore's (1990) analysis of the level of use of various drugs, which pieces together information from a number of different sources, suggests that marijuana use may have declined through the 1980s but that use of heroin and cocaine was increasing. Of course, any estimates of user populations or use level have to be treated with considerable caution, because they are clearly based on limited information about markets that have strong incentives to avoid detection. Nonetheless, the conflicting implications of these different indicators of drug market activity suggest that overall drug market activities may not be changing very dramatically from year to year. In addition, Cave and Reuter (1988) and Reuter et al. (1988) provide strong theoretical reasons for expecting that the supply of drug sellers is very elastic if not perfectly elastic, so if demand-side activities do not change significantly from year to year, the supply side may not either. They explain that the large, well-established suppliers are not likely to be threatened by law enforcement activities (there are significant gains from experience in this market, as those who survive learn how to avoid detection and lower their costs), and that there is a fairly constant stream of entrants to replace the smaller inexperienced suppliers who are arrested. Thus, the assumption necessitated by the data that $\Delta C_{Dj} = 0$ may be valid.

On the other hand, if the deterrence hypothesis holds, the increase in drug arrests and convictions during the 1980s should have had an impact (unless the entry rate exceeded these increases so that the probabilities did not change). Furthermore, potentially important changes in drug market activity did occur during our study period. Crack cocaine was introduced into some Florida markets in November of 1985 [Allen (1987), p. 4], for instance, apparently as a substitute for marijuana, which was being effectively interdicted [Thornton (1991); Nadelmann (1993), p. 45; Rasmussen and Benson (1994), pp. 141–146]. Many policy makers allege that crack induces more criminal behavior than marijuana. In fact, however, the most reliable evidence to date refutes this ''crack-causes-crime'' thesis. The ''Careers in Crack Project,'' funded by the National Institute of Justice, shows that ''the advent of crack did not seem to have substantially increased offenders' rates of committing most forms of nondrug criminality'' with the exception of prostitution [Johnson et al. (1995), p. 281]. These researchers point out that the crack trade was sufficiently profitable so that users found crack dealing itself to be more attractive than engaging in property crime.[17]

In the end, it is impossible to say whether $\Delta C_{Dj} = 0$ or not, but there is nothing that

[17] In this regard, an anonymous referee raised the possibility that the results presented below could be a product of a spurious correlation because crack-induced crime is concentrated in larger cities. If crack caused more Index I crime, then its increasing availability after 1985 could simultaneously lead to higher crime rates and greater drug arrests. However, evidence supplied by Johnson et al. (1995) and many others who have examined the Careers in Crack Project data undermines this spurious correlation argument. Indeed, the findings from the Careers in Crack Project are also consistent with the notion that the crack scare itself was fueled by media attention, and it has been argued that law enforcement interests, not public safety concerns, were behind this wave of publicity [Johnson et al. (1995); Rasmussen and Benson (1994), pp. 142–146; Benson et al. (1995)].

can be done about it. Even if ΔC_{Dj} is not equal to zero, however, such an assumption clearly should not be as troubling as the implicit assumption in cross-section studies that *levels* of C_{Dj} do not matter, as Cornwell and Trumbull (1994) also have noted. Furthermore, if changes are uniform across Florida (e.g., if there really are increasing propensities to commit nondrug crimes resulting from the introduction of crack), they should be captured by the year dummies employed below for the periods after 1985. In addition, if there are changes not controlled for in the incidence of drug and nondrug Index II crimes *per capita,* they may be closely associated with changes in population and various population characteristics, which are controlled for below; thus, the inability to control for these variables may bias the estimates of these coefficients without affecting the signs on key policy variables of interest. Whether this is the case is purely conjecture, of course, given the lack of data to explore the hypothesized relationships within the confines of the theoretical model.[18]

Data

Cross-section data for Florida's 67 counties were obtained for 1983, 1984, 1985, 1986, and 1987. This period was chosen in part because Florida adopted determinant sentencing in 1983, and the resulting sentencing guidelines remained fixed until being revised in 1988. The guidelines were implemented fully during 1984 [Justice Research Associates (1988)]. Thus, 1983–1984 was chosen as the beginning period of the analysis. In addition to the fact that the sentencing guidelines changed in 1988, the final period was chosen because: (1) Significant changes in the severity of punishment occurred toward the end of 1987 when an early release program was implemented, an exogenous change that could impact all deterrence estimates[19]; and (2) changes in Florida crime data reporting and recording in 1988 make that year's data unreliable according to Florida Department of Law Enforcement statisticians.

In 1980, 7.4% of all arrests in Florida were for drug offenses while 31.8% were for Index I crimes. The new offensive in the war against drugs seems to have started in 1984.[20] Indeed, drug arrests as a percentage of all arrests in Florida had fallen to 7.1% by 1983, but they increased to 7.6% of the total in 1984. This trend continued so that by 1987 drug arrests accounted for 10% of total arrests while Index I arrests had fallen to 25.9% (drug arrests peaked at 13% of total arrests in 1989). In fact, drug arrests increased 115% between 1980 and 1987 (from 32,029 to 68,747), while Index I arrests as a whole increased by only 29.2% (138,548 to 179,029).[21]

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[18] Recall the referee's argument in note 17, for instance. In that context, the referee suggested using the percentage of changes in some explanatory variables, rather than absolute changes, but such an *ad hoc* empirical respecification is inconsistent with the theoretical model. The remodeling required did not seem appropriate, given the findings in Johnson et al. (1995) discussed above.

[19] See note 21 for details. Also see Levitt (1996a) for evidence of the impact of imprisonment on crime.

[20] See Benson et al. (1995) for an examination of the exogenous change in state and local polices' incentives that induced local police to increase drug enforcement efforts across the nation, a change created by the asset-seizure section of the Comprehensive Crime Act of 1984.

[21] Changing drug enforcement was reflected in prison admissions. There were 1620 Florida prison admissions for drug offenses during the 1983–1984 fiscal year (FY), accounting for 12.9% of total admissions. By FY 1986–1987 this figure had risen to 22.9% of total admissions (5274). These trends continued, with drug arrests rising to 13% of total arrests in 1989, resulting in 15,802 drug admissions for FY 1989–1990, or 36.4% of total admissions. Thus, prison admissions for drugs rose by 875.4% between FY 1983–1984 and FY 1989–1990, while nondrug admissions rose by only 158.2% (from 10,896 to 27,585). Our data stops with 1987 in part because the increased flow into the prison system forced the implementation of a program to ''facilitate the transition from prison to civilian life'' in fiscal year

Data on crime rates, police labor, and drug arrests were obtained from the Florida Department of Law Enforcement; data on police capital expenditures were provided by the Comptroller, Bureau of Local Government Finance, Florida Department of Banking and Finance; conviction data were found in documents provided by the Florida Supreme Court; unemployment and wage data, as well as data on population characteristics, including age and race distributions, were provided by the Bureau of Economic and Business Research, University of Florida.

Hypotheses

The change in reported Index I crime in county j, $\Delta CRIME\text{-}I_j$ ($CRIME\text{-}I_{jt} - CRIME\text{-}I_{jt-1}$, with t denoting the years 1987, 1986, 1985, and 1984) is the dependent variable in the following regression[22]:

$$\begin{aligned}\Delta CRIME\text{-}I_j = {} & \alpha_1 + \alpha_2 \Delta POL\text{-}CAP_j + \alpha_3 \Delta POL\text{-}LAB_j + \alpha_4 \Delta PRB\text{-}CON\text{-}I_j \\ & + \alpha_5 \Delta PRB\text{-}CON\text{-}II_j + \alpha_6 \Delta PRB\text{-}CON\text{-}D_j + \alpha_7 \Delta DRUG\text{-}ARR_j \\ & + \alpha_8 \Delta \% BLACK_j + \alpha_9 \Delta INCOME_j + \alpha_{10} \Delta \% AGE15\text{–}24_j \\ & + \alpha_{11} \Delta UNEMPLOY_j + \alpha_{12} \Delta POP + \alpha_{13} 84\text{–}85 DUM \\ & + \alpha_{14} 85\text{–}86 DUM + \alpha_{15} 86\text{–}87 DUM + e_j \end{aligned} \tag{11}$$

The variables are defined in Table 1 and are discussed below. Summary statistics for each year's data in levels appear in Table 2, and Table 3 provides summary statistics for the differenced data.[23]

As a preliminary step in the analysis, given the expectation that an inability to control for county-level fixed effects may bias coefficients in a cross-section test of the deterrence model, tests for both fixed effects and random effects were performed. The results are not reported in detail here (but are available from the authors upon request). They indicate that both fixed- and random-effects models are preferred to the

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1986–1987 which lowered sentences to be served in FY 1987–1988 by 37 days for eligible inmates. Furthermore, overcrowding led to an Administrative Gain Time Program in 1987, resulting in a 122-day reduction in sentence for almost all prisoners scheduled for release in FY 1987–1988. Before the latest war against drugs, prisoners typically served about 50% of their sentences. Indeed, this continued to hold during the early years of the drug war campaign, so, for example, in January 1987 inmates were serving an average of 52.8% of their sentences. However, as drug admissions continued to increase, the actual portion of sentence served began to fall, reaching 33% in December 1989. If expected punishment serves as a deterrent, these state-level changes could significantly reduce the ability of local criminal justice systems to control crime.

[22] Many crimes are not reported by victims, a measurement problem that forces researchers to use reported crime rates as a proxy for the actual crime rate. This measurement problem receives considerable attention and criticism in the economics of crime literature [e.g., see Brier and Fienberg (1980)]. However, it is not nearly so germane here. This is an investigation of changes in reported crime rates, and these changes should reflect changes in actual crime rates as long as the propensity to report crime remains constant over the study period (i.e., as long as it is a fixed effect). The measurement error is clearly of less consequence in this context, and this provides yet another reason to employ a longitudinal difference-form model to test for deterrence relationships.

[23] Note that ''levels'' of police capital are not reported. Rather, annual expenditures reflect changes in this variable, and the statistics reported in Table 2 are for these changes. Also note that in the raw data 8 observations out of the 1005 representing various probabilities of conviction exceeded 1.0. This occurs when using county-level data, as the lag in convictions in a small county can mean that more convictions actually occur in 1-year than arrests. For the results reported here these eight observations were ''corrected'' by interpolation or extrapolation. However, to see whether this affected the estimates, the regressions also were run with the uncorrected data. These corrections have no significant impacts on any of the estimates.

TABLE 1. Definition of variables

Variable	*Definition*
$CRIME\text{-}I_{jt}$	Index I crime rate in county *j* during time period *t*.
$\Delta CRIME\text{-}I_j$	Change in $CRIME\text{-}I_j$ from time period *t*-1 to *t*.
$POL\text{-}LAB_{jt}$	Number of sworn officers in county *j* during time period *t*.
$\Delta POL\text{-}LAB_j$	Change in $POL\text{-}LAB_j$ from time period *t*-1 to *t*.
$POL\text{-}CAP_{jt}$	Police capital expenditures in county *j*.
$\Delta POL\text{-}CAP_j$	Change in $POL\text{-}CAP_j$ from time period *t*-1 to *t*.
$PRB\text{-}CON\text{-}I_{jt}$	Index I convictions divided by Index I arrests in county *j* during time period *t*.
$\Delta PRB\text{-}CON\text{-}I_j$	Change in $PRB\text{-}CON\text{-}I_j$ from time period *t*-1 to *t*.
$PRB\text{-}CON\text{-}II_{jt}$	Index II convictions divided by Index II arrests in county *j* during time period *t*.
$\Delta PRB\text{-}CON\text{-}II_j$	Change in $PRB\text{-}CON\text{-}II_j$ from time period *t*-1 to *t*.
$PRB\text{-}CON\text{-}D_{jt}$	Drug crime convictions divided by drug crime arrests in county *j* during time period *t*.
$\Delta PRB\text{-}CON\text{-}D_j$	Change in $PRB\text{-}CON\text{-}D_j$ from time period *t*-1 to *t*.
$DRUG\text{-}ARR_{jt}$	Arrests for drug crimes in county *j* during time period *t*.
$\Delta DRUG\text{-}ARR_j$	Change in $DRUG\text{-}ARR_j$ from time period *t*-1 to *t*.
$\%BLACK_{jt}$	Percentage of population that is black in county *j* during time period *t*.
$\Delta\%BLACK_j$	Change in $\%BLACK_j$ from time period *t*-1 to *t*.
$INCOME_{jt}$	Average wage and salary income in county *j* during time period *t*.
$\Delta INCOME_j$	Change in $INCOME_j$ from time period *t*-1 to *t*.
$\%AGE15\text{–}24_{jt}$	Percentage of the population aged 15 to 24 in county *j* during time period *t*.
$\Delta\%AGE15\text{–}24_j$	Change in $\%AGE15\text{–}24_j$ from time period *t*-1 to *t*.
$UNEMPLOY_{jt}$	Unemployment rate in county *j* during time period *t*.
$\Delta UNEMPLOY_j$	Change in $UNEMPLOY_j$ from time period *t*-1 to *t*.
POP_{jt}	Population in county *j* during time period *t*.
ΔPOP_j	Change in POP_j from time period *t*-1 to *t*.
84–85 DUM	Intercept dummy for the 1984–1985 data period.
85–86 DUM	Intercept dummy for the 1985–1986 data period.
86–87 DUM	Intercept dummy for the 1986–1987 data period.

OLS specification in levels, and that a fixed-effects model is preferred to a random effects model. The results also suggest that key coefficients are affected by the failure to control for fixed effects. For instance, the variables controlling for the portion of the population that is black and for police capital both become significant in the fixed-effects model, suggesting that they are indeed correlated with missing variables in the OLS model. Therefore, the difference-form model in equation (11) is appropriate, both because it represents a test of theoretical equation (7) and because it controls for fixed effects. As a test for robustness and for the potential that 1-year changes might be too short, a regression with changes over the entire 1983–1987 period is also presented (therefore $\Delta CRIME\text{-}I_j$ is calculated as $[CRIME\text{-}I_{jt} - CRIME\text{-}I_{jt-4}]$, with *t* denoting 1987, and the year-to-year dummies are not included).

The change in the number of sworn officers serves as a measure of the change in police labor, $\Delta POL\text{-}LAB_j$. Total police capital expenditures during a year are employed as a measure of the change in capital, $\Delta POL\text{-}CAP_j$. Annual police capital expenditures, for which we have data, are a flow variable, and there are no published measures of the stock of police capital. Thus, this data limitation provides another justification for using a longitudinal-change model rather than a cross-section model. Theoretically, the coefficients on the police resource variables represent the marginal products of police

TABLE 2. Summary statistics by year for data from 67 Florida counties

Year (±)	Variable	Mean	Std. Dev.	Minimum	Maximum
1983	$CRIME\text{-}I_j^{a}$	10.810	24.680	0.0210	167.0
	$POL\text{-}LAB_j$	317.03	685.15	5.0	4,566
	$\Delta POL\text{-}CAP_j$	3,457,400	6,920,300	6,606	38,330,000
	$PRB\text{-}CON\text{-}I_j$	0.11981	0.095247	0.02712	0.6364
	$PRB\text{-}CON\text{-}II_j$	0.12410	0.069373	0.02104	0.3651
	$PRB\text{-}CON\text{-}D_j$	0.33177	0.15434	0.03125	0.6833
	$DRUG\text{-}ARR_j$	566.93	1,310.2	7.0	8,141
	$\%BLACK_j$	15.226	10.283	2.447	59.32
	$INCOME_j$	13,253	1,879.6	9,671	17,260
	$\%AGE15\text{–}24_j$	15.976	3.3054	9.421	30.23
	$UNEMPLOY_j$	9.6813	2.8052	4.984	17.26
	POP_j	158,090	285,650	4,167	1,739,000
1984	$CRIME\text{-}I_j^{a}$	11.183	25.877	0.0270	177.8
	$POL\text{-}LAB_j$	324.45	697.12	6.0	4,603
	$\Delta POL\text{-}CAP_j$	3,409,600	6,993,500	4,739	36,930,000
	$PRB\text{-}CON\text{-}I_j$	0.11338	0.087229	0.02564	0.5660
	$PRB\text{-}CON\text{-}II_j$	0.11639	0.066301	0.009174	0.3538
	$PRB\text{-}CON\text{-}D_j$	0.34939	0.15279	0.06557	0.9130
	$DRUG\text{-}ARR_j$	631.25	1,462.0	3.0	9,244
	$\%BLACK_j$	15.086	10.265	2.441	59.00
	$INCOME_j$	13,846	1,947.1	10,520	18,060
	$\%AGE15\text{–}24_j$	15.713	3.3222	9.321	29.98
	$UNEMPLOY_j$	7.3043	2.4790	3.717	14.73
	POP_j	163,140	290,220	4,356	1,744,000
1985	$CRIME\text{-}I_j^{a}$	12.850	29.427	0.0380	199.1
	$POL\text{-}LAB_j$	334.91	708.43	4.0	4,633
	$\Delta POL\text{-}CAP_j$	4,310,100	9,387,600	21,440	50,090,000
	$PRB\text{-}CON\text{-}I_j$	0.13174	0.10871	0.03364	0.7963
	$PRB\text{-}CON\text{-}II_j$	0.12899	0.066473	0.02652	0.3816
	$PRB\text{-}CON\text{-}D_j$	0.37730	0.16390	0.07407	0.8462
	$DRUG\text{-}ARR_j$	659.22	1,491.9	9.0	9,184
	$\%BLACK_j$	14.970	10.248	2.439	58.69
	$INCOME_j$	15,151	2,902.5	10,079	22,100
	$\%AGE15\text{–}24_j$	15.454	3.3480	9.236	29.72
	$UNEMPLOY_j$	6.8598	2.3191	3.382	13.11
	POP_j	168,340	295,510	4,499	1,758,000
1986	$CRIME\text{-}I_j^{a}$	14.334	32.053	0.0390	213.1
	$POL\text{-}LAB_j$	350.04	718.13	5.0	4,622
	$\Delta POL\text{-}CAP_j$	5,022,200	11,060,000	5,974	62,360,000
	$PRB\text{-}CON\text{-}I_j$	0.37001	0.16212	0.1346	0.7798
	$PRB\text{-}CON\text{-}II_j$	0.051688	0.18161	0.003546	0.2241
	$PRB\text{-}CON\text{-}D_j$	0.35638	0.13803	0.1293	0.7120
	$DRUG\text{-}ARR_j$	846.57	2,046.2	3.0	13,620
	$\%BLACK_j$	15.045	10.264	2.360	58.44
	$INCOME_j$	14,938	2,169.3	11,360	19,710
	$\%AGE15\text{–}24_j$	15.037	3.3043	8.899	28.97
	$UNEMPLOY_j$	6.4087	2.0058	2.941	11.84
	POP_j	174,000	301,940	4,567	1,776,000

(Continued)

TABLE 2. Continued

Year (±)	*Variable*	*Mean*	*Std. Dev.*	*Minimum*	*Maximum*
1987	$CRIME\text{-}I_j^a$	15.243	34.001	0.200	225.7
	$POL\text{-}LAB_j$	370.99	742.12	5.0	4,747
	$\Delta POL\text{-}CAP_j$	8,651,100	22,635,000	12,220	146,300,000
	$PRB\text{-}CON\text{-}I_j$	0.37916	0.13943	0.1633	0.7731
	$PRB\text{-}CON\text{-}II_j$	0.042934	0.022783	0.008993	0.1066
	$PRB\text{-}CON\text{-}D_j$	0.44078	0.17070	0.1028	0.9130
	$DRUG\text{-}ARR_j$	1,026.1	2,274.9	8.0	14,080
	$\%BLACK_j$	15.151	10.282	2.161	58.17
	$INCOME_j$	15,705	2,229.0	11,930	20,710
	$\%AGE15\text{–}24_j$	14.623	3.2652	8.567	28.21
	$UNEMPLOY_j$	6.2303	1.9463	2.779	11.07
	POP_j	179,760	308,980	4,974	1,802,000

[a] Per 1000 population.

labor and capital in deterring Index I crime. A general deterrence model that fails to recognize the incentives of police or the allocation of resources would hypothesize a negative coefficient on these variables, but this need not be the case [Benson et al. (1994)]. After all, the level of reported crime is an important statistic used in bargaining for police budgets. Thus, as Milakovich and Weis (1975, p. 10) note, police have a ''vested interest'' in having relatively high crime rates: If crime rates are low, support for more police and larger budgets declines and ''like all bureaucracies, criminal justice agencies can hardly be expected to implement policies that would diminish their importance.'' In this regard, the econometrics of crime literature indicates that higher Index I crime rates clearly are correlated with more police resources in ''demand for policing'' equations, supporting the assumption that taxpayers/voters demand more police services if reported crime rates are high. Thus, if police have sufficient discretion (a hypothesis supported by empirical evidence[24]), and if they respond to incentives (and evidence suggests that they do[25]), additional funding and even additional police resources need not lead to any decrease in reported Index I crime rates.[26] Models of

[24] See, for example, Stumpf (1988, pp. 327–332), Benson et al. (1995), and Rasmussen and Benson (1994, pp. 32–37, 132–141).

[25] See for example, Seidman and Couzens (1974), Benson et al. (1995), Rasmussen and Benson (1994, pp. 132–141), and Mast et al. (1997).

[26] Like many other bureaus, police forces face a large number of competing demands, and therefore, they have a number of different outputs, some of which are easily measurable and some of which are not. When this is the case, a bureaucrat has incentives to produce the measurable output in quantities that correspond to the monitor's desires, while exploiting the uncertainty associated with unmeasurable outputs to gain discretionary budget [Lindsay (1976)]. Index II vice and narcotics functions of police seem to be particularly relevant in this regard. As Blumberg (1970, pp. 184–185) emphasizes, arrest statistics are often relatively easily expanded by pursuing vice and narcotics criminals, so ''we have spent much of our limited resources . . . [to arrest] addicts, alcoholics, prostitutes, homosexuals, gamblers, and other petty offenders, simply because they are readily available and produce the desired statistical data that indicate 'production'.'' Thus, police have incentives to allocate some resources to the control of non-Index I crimes, like vice and narcotics, for which crime statistics are not kept, thereby holding Index I crime rates up while increasing arrest statistics, another ''output measure'' that is important in the budget bargaining process [Sherman (1983)]. Indeed, under certain circumstances, police may even have incentives to reallocate existing resources to increase their efforts against non-Index I crimes. This occurred during the period under consideration as a result of the change in asset-seizure laws discussed in Benson et al. (1995) and note 31.

TABLE 3. Summary statistics for differenced data

Variable	*Mean*	*Std. dev.*	*Minimum*	*Maximum*
Year-to-year differences for 268 observations				
$\Delta CRIME\text{-}I_j$	1,108.4	2,780.4	−2,065	21,270
$\Delta POL\text{-}LAB_j$	13.489	27.133	−65.00	192.0
$\Delta POL\text{-}CAP_j$	5,348,300	13,955,000	4,739	146,300,000
$\Delta PRB\text{-}CON\text{-}I_j$	0.069906	0.20278	−0.8623	0.9091
$\Delta PRB\text{-}CON\text{-}II_j$	−0.020291	0.054190	−0.2606	0.2478
$\Delta PRB\text{-}CON\text{-}D_j$	0.027430	0.24619	−1.429	0.6887
$\Delta DRUG\text{-}ARR_j$	114.79	378.28	−789.0	4,435
$\Delta \%BLACK_j$	−0.018769	0.29583	−1.315	1.411
$\Delta INCOME_j$	612.90	1,657.1	−9,445	10,280
$\Delta \%AGE15\text{–}24_j$	−0.33820	0.19123	−0.8462	0.2564
$\Delta UNEMPLOY_j$	−0.86275	1.3096	−6.260	2.824
ΔPOP_j	5,416.6	7,520.0	−1,607	38,860
1983–1987 differences for 67 observations				
$\Delta CRIME\text{-}I_j$	4,433.5	9,559.8	−254.0	58,680
$\Delta POL\text{-}LAB_j$	53.599	79.723	−5.0	437.0
$\Delta POL\text{-}CAP_j$(sum)	21,393,000	48,998,000	44,380	278,800,000
$\Delta PRB\text{-}CON\text{-}I_j$	0.27264	0.16759	−0.1529	0.8544
$\Delta PRB\text{-}CON\text{-}II_j$	−0.081165	0.062112	−0.3433	0.007833
$\Delta PRB\text{-}CON\text{-}D_j$	0.10263	0.26832	−0.8036	0.8460
$\Delta DRUG\text{-}ARR_j$	459.15	1,025.2	−50.00	5,935
$\Delta \%BLACK_j$	−0.075037	0.91521	−3.245	2.861
$\Delta INCOME_j$	2,451.6	747.74	692.0	4,266
$\Delta \%AGE15\text{–}24_j$	−1.3528	0.57111	−2.337	0.2157
$\Delta UNEMPLOY_j$	−3.4510	1.4970	−7.765	−0.9067
ΔPOP_j	21,670	29,510	−1,374	137,000

bureaucratic behavior suggest strong incentives to expand a bureaucracy beyond its most efficient size, after all, perhaps even into a range where additional resources reduce total productivity, given the ''fixed inputs'' that police do not control (e.g., punishment resources and community characteristics).

The function of police in the minds of most citizens is to ''fight crime.'' But how can voters, taxpayers, and/or elected representatives tell if police are doing a good job? The number of arrests is a natural measure of ''effectiveness,'' and this, along with reported levels of Index I crime, tends to be the primary ''statistics'' that police focus on in lobbying for expanded budgets [Sherman (1983), p. 156]. Thus, incentives to watch or patrol to prevent crimes are relatively weak, and incentives to wait until crimes are committed to make arrests are relatively strong. It is not surprising, therefore, that after an extensive review of research on police performance, Sherman concludes (1983, p. 149): ''Instead of *watching to prevent crime,* motorized police patrol [is] a process of merely *waiting to respond* to crime.'' Indeed, Sherman's review suggests that about half of an officer's time is spent simply waiting for something to happen; although police officials claim that this time is spent in preventative patrolling, systematic observation indicates that such time is largely occupied with conversations with other officers, personal errands, and sitting in parked cars on side streets [Sherman (1983) p. 151]. In fact, there is a growing body of evidence that implies that as policing has become more capital intensive it has become less effective at crime deterrence. Experiments in ''community policing,'' taking police officers out of patrol cars and out of the ''emergency response system'' so they can patrol neighborhoods on foot, creates more effective proactive crime prevention [Skolnick and Bayley (1988)], in part by giving the officer better information about the problems and people of the neighborhood [Trojanowicz and Moore (1988)]. Thus, it is not surprising that measures of police manpower are more successful in producing the anticipated deterrence relationships in econometrics of crime models than measures of police capital or police budgets.[27] The focus on emergency response to make arrests, rather than on proactive crime prevention, implies that additional police resources, and particularly additional policing capital, will not necessarily reduce reported crime rates directly. Capital (e.g., cars, radios, etc.) may be relatively effective at producing arrests but relatively ineffective at crime deterrence. As Sherman (1983, p. 149) laments: ''In general, as the level of *crime prevention watching* has declined, the level of crime has risen.''[28]

Police are not the only source of deterrence of Index I crime. There are several potential variables in the $\mathbf{W}_j$ vector to be considered. The severity of expected punishment may be important, for example. County-level data on the severity of punishment handed down by Florida judges is simply not available. This should not be a significant problem, however, for two reasons. First, in this difference-form model it is the year-to-year change in severity that must be assumed to be zero. Punishment could vary from jurisdiction to jurisdiction (Florida's 67 counties are divided into 20 judicial circuits), and therefore punishment levels could affect crime levels, but there is no obvious reason to expect that different judges will *change* their sentencing practices in different ways from year to year. Second, the level of punishment for Index I (and felony Index II crimes such as drug crime) is largely determined by Florida's sentencing guidelines. These guidelines effectively restricted the sentencing discretion of judges (and prison capacity constraints determine the portion of sentence served). As noted above, these state-level sentencing constraints were implemented during our first period (1983–1984) and remained in place through our final period (1986–1987).[29] Counties may

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[27] Most studies just use measures of police labor, suggesting that measures of capital or budget were either not tried or did not perform well. However, experimentation with the data used below, and with other data [e.g., see Sollars et al. (1994)], in standard simultaneous equation models, supported the deterrence hypothesis for some years using police manpower but not when police budgets or capital measures were employed.

[28] The econometrics of crime literature contains a considerable amount of support for the hypothesis that a higher probability of arrest for a particular crime reduces the *level* of that crime (deters that crime). Levitt (in press) reinforces this conclusion after discriminating between deterrence, incapacitation, and measurement error as potential explanations for this statistical relationship. The argument made here is not inconsistent with this empirical result, but, rather, it suggests that waiting to arrest may not be the most effective way to deter crime, and furthermore, that *too much* capital may be employed relative to the level that would maximize crime deterrence for a particular level of fixed inputs to crime deterrence not controlled by police (e.g., community characteristics). Indeed, this suggests that the focus in many simultaneous-equation models on the production of arrests as the only deterrent is both inappropriate and misleading.

[29] Sentencing guidelines were introduced in 1983 to reduce disparities in sentences among similar criminals and ''substantial progress was made toward this goal as a result of sentencing guidelines'' [Florida Legislature (1991)]. That is, cross-county differences were reduced after 1983, and the guidelines' effects remained unchanged until 1988 when courts were given more discretion to depart from the guidelines, more minimum mandatory sentences were imposed, and habitual-offender laws were more easily applied to repeat offenders. Furthermore, a review of limited Department of Corrections data for the years 1984 through 1987 also suggests that the geographic distribution of admissions did

differ in their conviction rates, however. Therefore, the change in the probability of conviction for Index I crimes given arrest is included, as proxied by total Index I convictions divided by total arrests, $\Delta PRB\text{-}CON\text{-}I_j$. The coefficients on $\Delta PRB\text{-}CON\text{-}I_j$ should be negative, assuming that this is a good proxy for the probability of conviction, that higher probabilities of conviction serve as a deterrent, that marginal deterrent effects are negative, and that this variable changes sufficiently from year to year to provide sufficient variation in the data.

Five variables are included that might actually be in the $\mathbf{W}_j$, $\mathbf{X}_j$, and $\mathbf{Y}_j$ vectors, in that they may be related to Index I, Index, II, and drug crime: (1) the change in the percentage of the population that is black, $\Delta\%BLACK_j$; (2) the change in average wage and salary income, $\Delta INCOME_j$; (3) the change in the percentage of the population between the ages of 15 and 24, $\Delta\%AGE15\text{–}24_j$; (4) the change in the unemployment rate, $\Delta UNEMPLOY_j$; and (5) the change in population, ΔPOP_j. These are variables that frequently seem to be significant in cross-section economics of crime studies.[30]

The economics of crime literature indicates that improved opportunity costs for potential criminals, perhaps reflected by a fall in unemployment, or a rise in potential income, should reduce Index I crime (as well as Index II and perhaps drug crime). In addition, labor market studies consistently find that blacks earn less than whites with similar attributes, so the percentage of the population that is black complements the average earnings and unemployment data by providing an estimate of changes in the portion of the population that is likely to have below-average earnings; a positive relationship with Index I crime (and with Index II and perhaps drug crime) might therefore be expected. Alternative hypotheses to the one noted above arise for income, however. For example, an increase in the value of potential targets for Index I property crimes, perhaps as a result of higher average income for the population, should lead to more Index I property crime. Thus, the relationship between changes in average income and Index I crime is unclear, and the same is true of other crimes (e.g., depending on whether drugs are normal goods or inferior goods).

Age may affect criminal behavior in many ways. Earnings typically rise with age, implying that the opportunity cost of Index I crime rises as one gets older. Juvenile offenders are also subject to a different set of punishments than adult offenders, and the punishments tend to be less severe than those adults face. Furthermore, punishment by imprisonment may be a relative condition rather than an absolute one: A 3-year sentence for a 20-year-old is a much smaller portion of life expectancy than a 3-year sentence for a 60-year-old. Young people may also be relatively myopic and/or relatively less risk averse than older people. All of these factors suggest that young people are more apt to commit Index I crimes (as well as Index II and drug crimes).

Finally, Florida has experienced rapid population growth, and growth is often found to be associated with increasing levels of Index I crime, so a positive coefficient could be expected on the ΔPOP_j variable. This positive relationship can arise for a combination of several reasons. First, and perhaps most importantly, population change pro-

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not change markedly over this period: That is, counties sending a disproportionate number of offenders to prison in 1984 exhibited the same behavior in 1987 [Florida Department of Corrections (1984, 1987)]. Thus, although sentencing practices may vary somewhat across counties despite the guidelines, they do not seem to be *changing* within counties for the period studied here. That is, they are likely to be fixed effects.

[30] Of course, the signs of coefficients from the $\mathbf{X}_j$ and $\mathbf{Y}_j$ vectors depend on $(1 + A)$ and $(1 + B)$, which cannot be signed *a priori* as noted below, as well as the partial derivatives of Index II and drug crimes with respect to each of the variables [see equation (7)], so this could alter the signs anticipated for the variables in $\mathbf{W}_j$.

vides a scaling factor in these regressions: Some portion of new residents in a jurisdiction are likely to be criminals. In addition, rapid population growth means weaker community ties and reduced probabilities of cooperation by neighbors and witnesses in crime prevention and reporting. Furthermore, potential criminals are less likely to be noticed because of the large numbers of new residents. Rapid growth may also imply both relatively large numbers of potential uninformed or unprotected targets for crime and relatively large numbers of temporarily unemployed who find crime a relatively attractive avenue for income generation.

In addition to the five variables listed above, which may be in the $\mathbf{W}_j$, $\mathbf{X}_j$, and $\mathbf{Y}_j$ vectors, the probability of conviction given arrest for drug crimes and for nondrug Index II crimes from $\mathbf{X}_j$ and from $\mathbf{Y}_j$ are included, proxied by total convictions divided by total arrests. The signs of these coefficients cannot be predicted *a priori* because they depend on $1 + A$ and $1 + B$ from equations (4) and (5).

Intercept dummies are included for the 1984–1985, 1985–1986, and 1986–1987 data periods to control for statewide effects that might influence the growth in crime during each period in the year-to-year model but not in the 1983–1987 model. No *a priori* hypotheses are made regarding the signs of these coefficients, however.

The Drug War

As noted above, *changes* in the level of drug market activity and in the effort by police to control drug markets could be related to Index I crime. Drug market variables in the $\mathbf{M}_j$ vector are not measurable at the county level, but by focusing on changes rather than levels it may be that they reasonably can be ignored as fixed effects. In particular, as explained earlier, conflicting information on the national trends in drug use at least suggest that county-level, year-to-year changes may not be dramatic. Some drug prices apparently did change over this period. Estimates indicate that the price of cocaine in Miami fell within a range of \$28,000 to \$37,000 per kilogram in 1985, for instance, but the range was down to \$12,000 to \$15,000 by 1987 (*Narcotics Control Digest,* April 12, 1989, pp. 5–6). Price data is not available for counties, however. Nonetheless, it is reasonable to expect that there should not be a great deal of cross-county variation in these price *changes*. Price levels may vary across counties due to differences in access to suppliers (e.g., coastal counties could have relatively low prices because suppliers do not have to transport imported drugs within the country, and/or urban markets might be relatively more competitive than rural markets), but the drug market seems to be sufficiently well integrated across counties so that arbitrage will occur if price differences become pronounced, so factors that cause price changes in one area will lead to similar changes in other areas. Thus, cross-county variation in drug price changes should not be significant, and statewide changes should be picked up in the intercept or annual dummy coefficients.

Police increased their drug enforcement efforts during the 1983–1987 in Florida. Although the actual allocation of police resources cannot be observed, drug arrests reflect the consequences of the allocation decisions and are employed as a variable from the $\mathbf{M}_j$ vector. The change in drug arrests, $\Delta DRUG\text{-}ARR_j$, appears in the estimating equation.[31] A significant increase in the risk of drug dealing implied by drug arrests

[31] The empirical model implicitly assumes that drug arrests are exogenously determined. It could be contended that these arrests should be endogenous and that a $\Delta DRUG\text{-}ARR_j$ equation should simultaneously estimated. Certainly, rhetoric about drugs causing crime suggests that increased drug enforcement efforts might be a response to increasing

could make other income-generating crimes relatively more attractive, *ceteris paribus*. Thus, a positive sign on this coefficient might be predicted. This prediction is reinforced to the extent that the war on drugs involved shifts in the allocation of police resources rather than additional resources [including changes in *A* and *B* in equations (4) and (5)]. Under these circumstances, the drug arrest variable also serves as a proxy for a change in the relative allocation of police labor and capital between the two periods (i.e., picking up a shift in the production of Index I crime deterrence function). Because police resources are scarce, such a reallocation would mean that Index I crimes are more attractive both in the relative sense suggested above (that is, with all else being equal) and in an absolute sense because fewer police resources are available to deter such crimes, again suggesting a positive sign. The alternative hypothesis, that drugs cause crime, means that increased drug arrests could remove potential Index I criminals from society, implying a negative coefficient on the drug arrest variable.

IV. Empirical Results

Table 4 contains the regression results for the year-to-year changes in crime, as well as the second regression examining changes over the entire data period. Note that the adjusted R^2 of 0.49 for the regression considering annual changes is quite reasonable for longitudinal-change models of this type. Furthermore, despite the tendency for fixed-effects models to bias coefficients toward zero, several coefficients are significant at customary test levels. The results of the two equations are generally consistent, even

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Index I crime rates (as well as the level of police labor and capital and several of the other variables included in this model). However, theoretical and empirical evidence in Benson et al. (1995) and Mast et al. (1997) denies this expectation. Changes in drug arrests at the local level during the 1980s drug war have been determined by exogenous changes in the incentives facing police bureaucrats arising from the 1984 Federal asset-seizure law [Benson et al. (1995)]. In addition, because the resulting federally ''adopted'' seizures are only partially turned back to the local police (the Federal authorities extract a 10–20% handling charge), police in states whose own laws allow them to retain seized assets are able to obtain even greater benefits from seizures than police who must involve federal authorities in the process. Therefore, Mast et al. (1997) tested the hypothesis that in states whose laws allow the police to retain seizures police make relatively greater efforts against drugs than in states whose laws take such proceeds away from the police. Their econometric model (including tests for exogeneity of several key variables) used a unique data set that allowed for the control of other potential explanations of this cross-sectional variation. The Drug Use Forecasting (DUF) program has produced estimates of the level of drug use among criminal populations in 24 cities. By using these data, Mast et al. (1997) were able to control for the level of drug use perceived by police in these cities to see whether variation in drug market activity explained the differences in drug arrests. Not surprisingly, the level of drug use is a determinant of drug arrests. Mast et al. (1997) also controlled for the level of property crime, reflecting the possibility that police see drug enforcement as a general crime control technique and that drug arrests are therefore determined by crime (various socioeconomic factors were also controlled for). Drug arrests were not significantly related to property crime rates, however, suggesting that drug arrests may indeed be exogenous. Furthermore, Mast et al. (1997) found that a state law that allows the police to keep any portion of seized assets was associated with significantly more emphasis on drug arrests in DUF cities. Indeed, the laws have a large and important impact on the allocation of police resources: The existence of a confiscation law that is favorable to the police raises the drug arrests/total arrests ratio between 15% and 20%, depending on the model specification. Allowing police to profit from the confiscation of assets from alleged drug offenders apparently provides a powerful exogenous incentive to law enforcement agencies, which, as expected, changes agency behavior.

Despite the expectation of exogeneity, Hausman (1978) tests for exogeneity were performed. Alternative instrumental variables for $\Delta DRUG\text{-}ARR_j$ were estimated in various regressions using different specifications that included all the other predetermined variables in the model, and these instrumental variables were inserted into the model. An instrumental variable was estimated using a 1-year lag of the Index I crime rate, for instance, reflecting the idea that increased drug enforcement might be induced by rising crime rates. Another specification used the lagged values of all of the explanatory variables. The *t*-ratio for these instrumental variable coefficients suggests that they were always insignificant (ranging from −0.158 to −0.778), so the expectation that $\Delta DRUG\text{-}ARR_j$ is exogenous is supported.

TABLE 4. Differenced-form OLS regressions

	Year-to-year differences		*1983–1987 Differences*	
Variable	*Coefficient*	*t-ratio*	*Coefficient*	*t-ratio*
Intercept	−196.323	−0.426	597.300	0.185
$\Delta POL\text{-}LAB_j$	−6.93665	−1.051	−60.8841	−3.366*
$\Delta POL\text{-}CAP_j$	0.0004709	3.098*	0.000718956	2.187**
$\Delta PRB\text{-}CON\text{-}I_j$	−462.904	−0.584	−841.434	−0.236
$\Delta PRB\text{-}CON\text{-}II_j$	−1,992.19	−0.646	−9994.90	−1.034
$\Delta PRB\text{-}CON\text{-}D_j$	332.197	0.477	51.6611	0.024
$\Delta DRUG\text{-}ARR_j$	0.740441	1.760***	4.62594	3.629*
$\Delta \% BLACK_j$	1,419.16	2.967*	900.326	1.296
$\Delta INCOME_j$	−0.0315009	−0.440	−0.216329	−0.225
$\Delta \% AGE15\text{–}24_j$	5.78692	0.008	500.440	0.476
$\Delta UNEMPLOY_j$	72.0610	0.552	−100.959	−0.261
ΔPOP_j	0.154891	5.772*	0.175331	3.878*
84–85 DUM	1,176.38	2.635*		
85–86 DUM	438.529	0.801		
86–87 DUM	−342.842	−0.708		
Adjusted R^2	0.49		0.82	
F-statistic	17.24 (14, 253)		22.39 (11, 55)	

* Significant at 0.01% test level.
** Significant at 0.05% test level.
*** Significant at 0.10% test level.

though there may be problems with multicollinearity between the $\Delta DRUG\text{-}ARR_j$ and $\Delta POL\text{-}CAP_j$ variables in the 1983–1987 equation where $\Delta POL\text{-}CAP_j$ was constructed by summing the capital expenditures for 1984, 1985, 1986, and 1987. The correlation coefficient between this constructed variable and $\Delta DRUG\text{-}ARR_j$ was 0.89. In a regression using the log transformation of $\Delta POL\text{-}CAP_j$ as a correction for possible multicollinearity, however, the coefficient on the logged police capital variable remained significantly positive, as did the coefficient on $\Delta DRUG\text{-}ARR_j$, and none of the other coefficients changed significantly.

The coefficients for the police policy variables, $\Delta POL\text{-}LAB_j$, $\Delta POL\text{-}CAP_j$, and $\Delta DRUG\text{-}ARR_j$, are of particular interest here. Therefore, before discussing these coefficient estimates, let us simply note that the 1984–1985 dummy variable also is significantly positive, implying a relatively large increase in Index I crime compared to 1983–1984, whereas the 1985–1986 and 1986–1987 increases in Index I crimes are not significantly different from the 1983–1984 increase after controlling for the other factors in the model. Furthermore, the coefficients for $\Delta \% BLACK_j$ and for ΔPOP_j are significant with the anticipated signs, whereas the coefficients for $\Delta PRB\text{-}CON\text{-}I_j$, $\Delta INCOME_j$, $\Delta AGE15\text{–}24_j$, and $\Delta UNEMPLOY_j$ cannot be distinguished from zero. Some of these ''nonresults,'' particularly for $\Delta AGE15\text{–}24_j$, may be surprising inasmuch as a positive correlation between a young population and crime is generally found in empirical studies. Furthermore, the lack of a deterrent effect for $\Delta PRB\text{-}CON\text{-}I_j$ seems troubling. One problem with fixed-effects models is that coefficients tend to be biased toward zero, of course, so less weight can be placed on such nonresults than on the findings of significant coefficients. Our age variable is significant in an OLS regression in levels, for instance, but apparently there is not sufficient change in the data to generate enough variation for significance in the change-form regressions. Indeed, another problem in using a county-level change-form model is that there may be insufficient variation in some variables (e.g., changes in the probability of conviction and in the age distribution) to find significant relationships.

Although the coefficient on the change in police labor, $\Delta POL\text{-}LAB_j$, is not significantly different from zero at customary test levels in the year-to-year model, it is significant and negative in the 1983–1987 model. Thus, police labor may have the anticipated marginal deterrent impact. On the other hand, changes in police capital, $\Delta POL\text{-}CAP_j$, are positively and significantly related to changes in reported crime. The coefficient estimate suggests that an annual \$10,000 increase in police capital is associated with four to five additional Index I crimes. This is consistent with the arguments made above regarding the possibility that capital intensive policing tends to be reactive rather than proactive in preventing crime. Thus, at the margin, taking a police officer out of a \$20,000 patrol car and putting him on a neighborhood patrol [e.g., the ''community-policing'' approach discussed by Skolnick and Bayley (1988) and Trojanowicz and Moore (1988)] might prevent 8 to 10 Index I crimes each year.

The increased law enforcement emphasis on drug arrests has significantly altered the incentives facing the crime-prone population, but not in ways that are typically suggested by drug-control advocates. The coefficient on $\Delta DRUG\text{-}ARR_j$ should be negative given the ''drugs-cause-crime'' hypothesis that increased drug arrests also will reduce the Index I crime population. Therefore, that hypothesis is not supported (it is either not valid or its effects are dominated by the effects of the other hypotheses). Over this period, substantial portions of the increases in police resources were allocated to drug enforcement rather than to Index I crime control. Indeed, some existing police resources previously used to combat Index I crimes were apparently also reallocated, reducing the ability of police to deter Index I crime. The significant positive coefficient on the change in drug arrests variable is consistent with both this interpretation and with the crime substitution hypothesis. Drug enforcement policy apparently has drawn resources away from Index I crime control, has caused some drug criminals to shift into nondrug crime, or both. The coefficient on $\Delta DRUG\text{-}ARR_j$ implies that allocating the resources necessary to make one more drug arrest a year results in about 0.7 more Index I crimes that year. These results, therefore, support the deterrence hypothesis: Policing does influence the incentives to commit crime.

V. Conclusions

Both the econometrics of crime literature and its critics generally ignore the incentives of police bureaucrats, the tremendous range of activities that police perform beyond the control of Index I (reported) crime, and the discretion that police have in allocating resources. When these factors are considered it becomes clear that the typical production function assumption that an increase in aggregate police resources necessarily leads to a reduction in Index I crime (or an increase in the probability of arrest for Index I criminals, which in turn leads to a reduction in Index I crime) is inappropriate. This is important because some critics reject the deterrence hypothesis simply because empirical tests suggest that crime rates are not negatively related to the level of aggregate police resources.

The allocation of police resources is not considered in the econometrics of crime literature, in part because data are not available to control for resource allocation in cross-section studies. However, an empirical test of the hypothesis that the allocation of police resources matters is performed here using panel data to examine *changes* in Index I crime as a function of a policy *change:* the reallocation of police resources toward the control of drug crime during the 1980s ''war on drugs.'' This approach has the added advantage of controlling for fixed effects that can bias the estimated coefficients in a cross-section study. The difference-form estimation employed here provides strong support for the deterrence hypothesis.

This analysis also reveals some unintended consequences of the drug war. When viewed in light of Becker's (1968) economic theory of crime and its deterrence hypothesis, it is not surprising that Index I crimes increased in Florida during the drug war period (1984–1989). In fact, Index I crime rates were falling in Florida in the early 1980s (from 8387.8 crimes reported per 100,000 population in 1980 to 6837.9 in 1983) as the relative effort against drugs fell (from 7.5% to 7.1% of total arrests), but they rose steadily after 1983 as drug enforcement efforts increased, reaching 8479.9 in 1987 and 8755.9 in 1989 when drug arrests reached 10 and 13% of total arrests, respectively. That is, from 1983 (the year before the offensive against drugs began to accelerate) to 1989, the Index I crime rate in Florida rose by 28% (25% over the period of our data, from 1983 to 1987) as drug arrests relative to total arrests rose by 83% (41% over our data period). The predicted benefits of a drug war, including reduced Index I crimes, clearly have not materialized [Zimring and Hawkins (1992); Rasmussen and Benson (1994)], and the opportunity costs of the drug war have been very high, as they include the consequences of increasing Index I crime.[32]

The emphasis on drug enforcement in Florida temporarily waned after 1989. For instance, drug arrests relative to total arrests fell back to 10% in 1992, and Index I crimes fell from their 1989 peak to 8289.0.[33] Given the reality of scarce police resources, getting ''tough'' on drug crime meant getting soft on Index I crime, and getting softer on drug crime after 1989 apparently allowed police to get tougher on Index I crime. Drug control efforts seem to be on the rise again, however, so *ceteris paribus,* crime rates can be expected to climb.[34]

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[32] The war on drugs may not have been a failure from the perspective of police, however: Both arrests as a measure of output and crime rates as a measure of the ''need'' for more police resources were higher. Police were then in a position to lobby for more resources. Beyond that, as explained in note 31, drug enforcement became a major source of asset forfeitures, and, under a federal statute in effect since 1984 (similar state laws also apply in many but not all states), asset seizures are returned to the police, significantly increasing police departments' discretionary budgets [Benson et al. (1995); Mast et al. (1997)].

[33] See Rasmussen and Benson (1994, pp. 146–150) for an explanation of this policy shift in the context of a bureaucratic resource allocation model. This explanation includes the fact that many states' asset-seizure laws have been revised to allow police to seize and keep more assets associated with nondrug crime, the growing perception that the drug war is not doing what it was advertised to do, and the public outcry against the rising rates of Index I crime and crowded prisons associated with the drug war.

[34] The *ceteris paribus* does not hold, however. For instance, Florida has undertaken a massive prison-building campaign since 1993 and has altered various minimum mandatory sentences to alleviate prison crowding and dramatically increase the portion of sentences served.

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