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THE IMPACT OF DRUG ENFORCEMENT ON CRIME: AN INVESTIGATION OF THE OPPORTUNITY COST OF POLICE RESOURCES

BRUCE L. BENSON, IAN SEBASTIAN LEBURN, DAVID W. RASMUSSEN

The conventional wisdom among the law enforcement community is that drug use causes crime and that stringent enforcement of drug laws is an effective tool to combat property and violent crime. Previous research by some of these authors found that a sharp increase in drug enforcement in Florida during 1984-1989 resulted in a reallocation of police resources which reduced the effectiveness of property crime enforcement and increased the property crime rate. Some have suspected that this result is the product of the very large increase in drug enforcement during this time period and that under "normal" circumstances greater drug enforcement would not result in higher property crime. This paper rebuts that suspicion.

INTRODUCTION

Controlling illicit drug use has been a persistent policy issue in the United States, where law enforcement has been the primary orientation in the so-called "war on drugs." The relationship between drug enforcement and crimes against persons and property has been controversial. Drug enforcement can disrupt drug markets, leading to an increase in violent crime as drug dealers fight over turf and market share (Goldstein, 1989; Rasmussen, Benson, & Sollars, 1993). With respect to property crime, there is substantial evidence that enforcement can increase drug

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David W. Rasmussin, Ph.D., is the James H. Gapinski Professor of Economics and Director of the De Voe L. Moore Center at Florida State University. He has written in many areas of economics, including urban economics, the economics of crime, and illicit drug policy. **Bruce L. Benson**, Ph.D., is Distinguished Research Professor of Economics and holds the DeVoe L. Moore Professorship at Florida State University. He has published many articles in the fields of law and economics, the economics of crime, and illicit drug policy. He is co-author of the *Economic Anatomy of a Drug War: Criminal Justice in the Commons* (1994) and *The Enterprise of the Law* (1990). **Ian Sebastian Leburn** received his Master's degree in Economics from Florida State University in 1999.

prices and that higher prices suppress drug use (Caulkins & Reuter, 1998). There is also some evidence that higher prices can drive addicts to commit property crime to finance their habit (Silverman & Spruill, 1977), although the relationship between enforcement and drug prices is complicated by the fact that both drug users and suppliers can substitute among illicit drug activities (Rasmussen & Benson, 1994). Thus, the 1984-1989 period of rising enforcement in the U.S. was characterized by lower cocaine prices and higher marijuana prices, making it difficult to argue that the unambiguous effect of enforcement is to raise drug prices and increase crime.

An alternative and perhaps more cogent hypothesis that rising drug enforcement can increase property crime is based on the notion that the opportunity cost of drug enforcement is reduced efforts to combat these crimes (Benson, Kim, Rasmussen, & Zuehlke, 1992). When law enforcement resources are limited, rising drug enforcement must necessarily draw police resources away from other activities that represent the opportunity cost of these anti-drug efforts. Such a reallocation of police resources presumably lowers the probability of arrest for property crimes (or whatever alternative crime control effort has been reduced). This can influence the number of offenses in two ways. First, persons currently engaged in criminal activity will on average commit more crimes before being apprehended. Secondly, and perhaps less important, is the possibility that the lower probability of arrest will lead more people to commit offenses. Using data from Florida jurisdictions for 1987, Sollars, Benson, and Rasmussen (1994) found evidence that allocating *more* scarce police resources to drug offenses lowered enforcement against *property* crimes. During 1984-1989, a period that had a dramatic increase in drug enforcement, Benson et. al. (1992) found that rising drug enforcement was associated with an increase in property crime in Florida counties and Benson, Kim, and Rasmussen (1998), employing a different method of analysis using data for 1983-1987, reported that more drug arrests was correlated with an increase in Index I crimes (property and violent crimes) in aggregate.

Higher crime against property and/or persons does not necessarily follow from an increase in drug enforcement, however. One reason is that only a relatively small portion of all police resources are used to combat the property and violent crimes that constitute Index I reported offenses (murder, rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft, and arson) so that police officials could sacrifice some other activities to pursue the war of drugs (Benson, Kim, and Rasmussen, 1994). Furthermore, from a theoretical perspective, Caulkins, Dworak, Feichtinger, and Tragler (2000) argue that society is not in fact constrained from raising taxes or spending less on unrelated programs. Thus, they argue that Benson et al.'s (1992) contention that more Index I crime is the opportunity cost of rising drug enforcement is not theoretically correct because the police could fund these efforts by sacrificing other activities and/or by receiving higher budgets. Indeed, Benson and Rasmussen (1992) made a similar point when they found that when

police in Illinois increased drug enforcement during the 1984-1989 period, the result was a dramatic reduction in traffic control efforts and a sharp rise in traffic fatalities. While there is obviously an opportunity cost to drug enforcement efforts, it is not necessarily the cost experienced in Florida over the 1984-1989 period.

Caulkins et al. (2000) also suggest that the research using Florida data for 1984-1989 is not generally applicable because the there is no national evidence that expanding drug control efforts reduced the expected severity of sanctions for property offenses. This includes the probability of arrest that lies at the core Benson et al.'s (1992, 1998) argument. Caulkins et al. use of aggregate national data cannot convincingly deny the existence of a drug enforcement/crime trade-off, however, because they fail to control for other potential causal factors that changed over the period. They nevertheless raise a legitimate question as to the whether the 1984-1989 Florida experience is generally applicable. It is important to note, however, that the rapid rise in drug arrests in Florida was typical of the nation in the mid to late eighties. Drug arrests per 100,000 population in Florida rose 87.5 percent between 1984 and 1989, compared to 72.4 percent for the nation. Thus, a redeployment of criminal justice resources was likely to be common in many states. Indeed, such a reallocation is quite consistent with a theoretical analysis of police decision-making (Benson, Kim, & Rasmussen, 1994), and with changes in police incentives that occurred in 1984 (Benson, Rasmussen, & Sollars, 1995; Mast, Benson, & Rasmussen, 2000).

This paper revisits the empirical relationship between drug enforcement and Index I crimes but for a time period when aggregate drug enforcement measures and Index I arrest rates appear, on the surface, to deny the trade-off, as suggested in Caulkins, et al. (2000). Florida data from 1994-1997 for 67 counties are used here to re-estimate the empirical model is developed in Benson et al. (1998). Unlike the 1984-1989 period, aggregate drug arrests in the state actually fell by 8.0 percent during the period studied here, while arrests for Index I crimes also fell. Thus, the changes in the allocation of law enforcement resources are very different during this time period and, by controlling for other causal factors, this analysis may therefore provide additional empirical evidence about the potential trade-off between drug enforcement and Index I crime. The rest of this paper is organized as follows. The following section summarizes the empirical model developed by Benson, Rasmussen, and Kim (1998) and the data are examined in Section III. The empirical results are reported in Section IV and Section V presents the conclusions.

THE MODEL

Benson et al. (1998) assume that police are in the business of producing crime deterrence and allocate capital and labor to control various types of crime. The model recognizes that many determinants of this production process are not

controlled by the police, among them being the opportunity costs of criminals, the severity of punishment, and community factors. The econometric model is a difference-form analysis:

$$\Delta C_{Ik} = \alpha_i \Delta Z_{ki} + \Delta e_k$$

where Δ represents changes between time periods [t-(t-1)], C_I is the number of Index I crimes in the kth jurisdiction, and α_i includes a constant and i-1 coefficients corresponding to the independent variables in Z. A primary advantage of this approach is that it controls for jurisdiction-specific fixed effects arising from unobservable factors, thereby increasing the potential for unbiased estimates of the coefficients.[1] An alternative fixed-effects approach is to use levels rather than changes and add dummy variables for each jurisdiction. These alternatives are theoretically identical for a two period panel data set but since four years of data (three changes) are used here, both specifications are considered below.
The dependent variable is the change in reported Index I crime in county *k;* $\Delta CRIME\text{-}I_k$ ($CRIME\text{-}I_{kt}$ - $CRIME\text{-}I_{kt-1}$, with *t* denoting the years 1997, 1996, and 1995). The following regression replicates the Benson et al. model:

$$\begin{aligned}\Delta CRIME\text{-}I_k = {} & \alpha_1 + \alpha_2 \Delta POL\text{-}CAP_k + \alpha_3 \Delta POL\text{-}LAB_k + \alpha_4 \Delta PRB\text{-}CON\text{-}I_k \\ & + \alpha_5 \Delta PRB\text{-}CON\text{-}II_k + \alpha_6 \Delta PRB\text{-}CON\text{-}D_k + \alpha_7 \Delta DRUG\text{-}ARR_k \\ & + \alpha_8 \Delta\%BLACK_k + \alpha_9 \Delta INCOME_k + \alpha_{10} \Delta\%AGE15\text{-}24_k \\ & + \alpha_{11} \Delta UNEMPLOY_k + \alpha_{12} \Delta POP_k + \alpha_{13} 95\text{-}96DUM \\ & + \alpha_{14} 96\text{-}97DUM + \mu_k\end{aligned} \tag{2}$$

The variables in this model 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 shows summary statistics for the differenced data. Many crimes are not reported by victims, of course, so reported crime is used as a proxy for the actual crime rate. The consequences of this measurement error may be mitigated here, at least in the model that investigates changes in reported crime rates, since 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). Thus, this measurement problem indicates one of the main advantages of a longitudinal difference-form model to test deterrence relationships.

Since there exists the potential problem that one-year changes may present too little variation with regards to the county data in Florida during the study period, as noted above and observed in Table 2, a regression with changes over the entire 1994-1997 period is also presented (i.e., $\Delta CRIME\text{-}I_k$ is calculated as [$CRIME\text{-}I_{kt}$ - $CRIME\text{-}I_{kt-3}$] with *t* denoting 1997, and the year-to-year dummies are not included.

TABLE 1
DEFINITION OF VARIABLES

Variable	Definition
$CRIME\text{-}I_{kt}$	**Index I crime rate in county k during time period t.**
$\Delta CRIME\text{-}I_k$	**Change in $CRIME\text{-}I_k$ from time period t-1 to t.**
$POL\text{-}CAP_{kt}$	Police capital expenditures in county k during time period t.
$\Delta POL\text{-}CAP_k$	Change in $POL\text{-}CAP_k$ from time period t-1 to t.
$POL\text{-}LAB_{kt}$	Number of sworn officers in county k during time period t.
$\Delta POL\text{-}LAB_k$	Change in $POL\text{-}LAB_k$ from time period t-1 to t.
$PRB\text{-}CON\text{-}I_{kt}$	Index I convictions divided by Index I arrests in county k during time period t.
$\Delta PRB\text{-}CON\text{-}I_k$	Change in $PRB\text{-}CON\text{-}I_k$ from time period t-1 to t.
$PRB\text{-}CON\text{-}II_{kt}$	Index II convictions divided by Index II arrests in county k during time period t.
$\Delta PRB\text{-}CON\text{-}II_k$	Change in $PRB\text{-}CON\text{-}II_k$ from time period t-1 to t.
$PRB\text{-}CON\text{-}D_{kt}$	Drug crime convictions divided by Index II arrests in county k during time period t.
$\Delta PRB\text{-}CON\text{-}D_k$	Change in $PRB\text{-}CON\text{-}D_k$ from time period t-1 to t.
$DRUG\text{-}ARR_{kt}$	Arrests for drug crimes in county k during time period t.
$\Delta DRUG\text{-}ARR_k$	Change in $DRUG\text{-}ARR_k$ from time period t-1 to t.
$\%BLACK_{kt}$	Percentage of the population that is black in county k during time period t.
$\Delta\%BLACK_k$	Change in $\%BLACK_k$ from time period t-1 to t.
$INCOME_{kt}$	Average wage and salary income in county k during time period t.
$\Delta INCOME_k$	Change in $INCOME_k$ from time period t-1 to t.
$\%AGE15\text{-}24_{kt}$	Percentage of the population aged 15-24 in county k during time period t.
$\Delta\%AGE15\text{-}24_k$	Change in $AGE15\text{-}24_k$ from time period t-1 to t.
$UNEMPLOY_{kt}$	Unemployment rate in county k during time period t.
$\Delta UNEMPLOY_k$	Change in $UNEMPLOY_k$ from time period t-1 to t.
POP_{kt}	Population in county k during time period t.
ΔPOP_k	Change in POP_k from time period t-1 to t
95-96DUM	Intercept dummy for the 1995-1996 data period.
96-97DUM	Intercept dummy for the 1996-1997 data period.

* Dependent variable shown in bold.

The theoretical model of Benson et al. (1998) assumes that police use capital and labor to produce deterrence, thus requiring an estimate of the stock of police capital and labor resources used during each year. Unfortunately, there are no published measures of the stock of police capital, so total police capital expenditures during a given year are employed as a measure of the change in capital, $\Delta POL\text{-}CAP_k$. If the rate at which the capital stock depreciates among counties is constant over time, then annual police capital expenditures is likely to be a reasonable proxy for changes in the capital stock. The change in the number of sworn officers is the measure of the change in police labor, $\Delta POL\text{-}LAB_k$.

Standard deterrence models include the probability of conviction given arrest and three types of crime are included in this model: probability given arrest for drug crimes, for non-drug Index II crimes, and for Index I crimes.[2] Benson et al. show that the signs of these coefficients cannot be predicted *a priori*, given the derivation of the model. The variable of particular interest in this model is ΔDRUG-

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TABLE 2
SUMMARY STATISTICS BY YEAR FOR DATA FROM 67 FLORIDA COUNTIES

Year	Variable	Mean	Std. Dev.	Minimum	Maximum
1994	$CRIME\text{-}I_k$	16.879	37.854	0.0450	250.7
	$\Delta POL\text{-}CAP_k$	2,985,620	8,348,621	5,300	59,277,000
	$POL\text{-}LAB_k$	464.31	872.06	7.0	5,396
	$PRB\text{-}CON\text{-}I_k$	0.31692	0.185670	0.03152	0.8039
	$PRB\text{-}CON\text{-}II_k$	0.21128	0.145620	0.01259	0.8143
	$PRB\text{-}CON\text{-}D_k$	0.43880	0.17246	0.02277	0.8240
	$DRUG\text{-}ARR_k$	1,328.64	2,673.8	11.0	16,155
	$\%BLACK_k$	15.056	10.627	2.342	60.12
	$INCOME_k$	20,307	2,867.0	15,098	26,500
	$\%AGE15\text{-}24_k$	13.254	3.7120	8.112	26.82
	$UNEMPLOY_k$	6.6906	2.7489	3.080	16.65
	POP_k	198,865	334,916	5,826	1,990,445
1995	$CRIME\text{-}I_k$	16.099	36.795	0.0360	251.4
	$\Delta POL\text{-}CAP_k$	3,408,436	7,366,510	28,829	39,685,300
	$POL\text{-}LAB_k$	447.84	846.90	6.0	5,270
	$PRB\text{-}CON\text{-}I_k$	0.35367	0.204019	0.03148	0.8511
	$PRB\text{-}CON\text{-}II_k$	0.22717	0.159847	0.01297	0.7442
	$PRB\text{-}CON\text{-}D_k$	0.49118	0.21108	0.03098	0.9063
	$DRUG\text{-}ARR_k$	1,318.60	2,465.4	10.0	12,470
	$\%BLACK_k$	14,549	10.151	2.290	56.01
	$INCOME_k$	20,950	2,965.0	15,500	27,672
	$\%AGE15\text{-}24_k$	12.941	3.4254	7.923	25.33
	$UNEMPLOY_k$	5.6361	2.4785	2.240	14.99
	POP_k	211,277	349,758	6,518	2,014,817
1996	$CRIME\text{-}I_k$	16.114	34.768	0.0410	225.3
	$\Delta POL\text{-}CAP_k$	3,763,385	8,132,337	33,190	54,230,588
	$POL\text{-}LAB_k$	463.39	898.15	3.0	5,576
	$PRB\text{-}CON\text{-}I_k$	0.29794	.0113682	0.05385	0.7000
	$PRB\text{-}CON\text{-}II_k$	0.18717	0.092084	0.02689	0.4800
	$PRB\text{-}CON\text{-}D_k$	0.45058	0.20218	0.05332	0.8889
	$DRUG\text{-}ARR_k$	1,215.28	2,116.2	6.0	10,455
	$\%BLACK_k$	13.745	10.287	1.854	58.29
	$INCOME_k$	21,606	3,031.8	16,222	28,749
	$\%AGE15\text{-}24_k$	12.948	3.4162	7.911	25.31
	$UNEMPLOY_k$	5.4127	2.4722	2.670	13.76
	POP_k	215,098	355,274	7,012	2,043,316
1997	$CRIME\text{-}I_k$	16.026	34.708	0.0430	229.6
	$\Delta POL\text{-}CAP_k$	4,2220,799	8,653,492	28,762	55,600,282
	$POL\text{-}LAB_k$	474.73	927.40	3.0	5,795
	$PRB\text{-}CON\text{-}I_k$	0.33826	0.134709	0.00442	0.6461
	$PRB\text{-}CON\text{-}II_k$	0.21170	0.099192	0.00615	0.5263
	$PRB\text{-}CON\text{-}D_k$	0.49673	0.18561	0.01681	0.8997
	$DRUG\text{-}ARR_k$	1,200.70	2,184.2	12.0	11,029
	$\%BLACK_k$	13.649	9.760	2.058	52.27
	$INCOME_k$	22,286	3,122.1	16,467	29,867
	$\%AGE15\text{-}24_k$	5.1936	2,3580	2.330	12.83
	$UNEMPLOY_k$	5.1936	2.3580	2.330	12.83
	POP_k	219,596	361.372	7,002	2,070,573

TABLE 3
SUMMARY STATISTICS FOR DIFFERENCED DATA

Year-to-year differences for 201 observations

Variable	Mean	Std. Dev.	Minimum	Maximum
ΔCRIME-I_k	-284.4	2,734.2	-26,140	10,898
ΔPOL-CAP_k	3,797,540	8,034,588	28,762	55,600,282
ΔPOL-LAB_k	3.473	72.229	-386.00	477.0
ΔPRB-CON-I_k	0.007114	0.13447	-0.5137	0.6491
ΔPRB-CON-II_k	0.000141	0.100871	-0.5446	0.2453
ΔPRB-CON-D_k	0.019311	0.18525	-0.533	0.5843
ΔDRUG-ARR_k	-42.65	525.83	-4,173.0	2,313
Δ%$BLACK_k$	0.004691	0.01837	-0.148	0.096
$\Delta$$INCOME_k$	659.72	296.5	-610.0	1,506
Δ%$AGE15\text{-}24_k$	0.00099	0.01225	-0.1729	.0042
$\Delta$$UNEMPLOY_k$	-0.00499	0.0080	-0.035	0.046
$\Delta$$POP_k$	6,910.2	39,693.3	-482.0	560,284

1994-1997 differences for 67 observations

Variable	Mean	Std. Dev.	Minimum	Maximum
ΔCRIME-I_k	-853.2	4,089.0	-21,132.0	6,608
ΔPOL-CAP_k	14,378,240	31,138,972	364,023	208,793,170
ΔPOL-LAB_k	10.418	111.426	-394.0	487.0
ΔPRB-CON-I_k	.02134	.14734	-.4875	.2945
ΔPRB-CON-II_k	.000425	.142186	-.4732	.261811
ΔPRB-CON-D_k	.05794	.19234	-.4300	.4597
ΔDRUG-ARR_k	-127.94	752.5	-5126.00	1,566
Δ%$BLACK_k$	-.014110	.02283	-.1512	.0428
$\Delta$$INCOME_k$	1,979.1	803.25	250.8	4,042
Δ%$AGE15\text{-}24_k$	-.00299	.02113	-.172	.0068
$\Delta$$UNEMPLOY_k$	-.0150	.01418	-.0382	.05430
$\Delta$$POP_k$	20,731	70,736	-7.00	572,356

ARR, which is included to test the hypothesis that more Index I crime may occur if police resources are diverted from these crimes to drug enforcement.

Five variables are also included because they may be related to Index I, II, and drug crime: (1) the change in the percentage of the population that is black, Δ%$BLACK_k$; (2) the change in average wage and salary income, $\Delta INCOME_k$; (3) the change in the percentage of the population between the ages of 15 and 24, Δ%$AGE15\text{-}24_k$; (4) the change in the unemployment rate, $\Delta UNEMPLOY_k$; and (5) the change in population, ΔPOP_k. The economics of crime literature indicates that higher opportunity costs for potential criminals, due to lower unemployment rates or higher legal earnings, should reduce Index I crime, and perhaps even Index II and drug crime (Ihlanfeldt, 2001). Since blacks tend to have relatively low earnings compared to whites with similar attributes, the percent black variable is included as a complement to the income and employment variable. An alternative hypothesis arises with respect to income, however. For instance, 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 drug offenses (i.e., depending on whether the demand for drugs increases or decreases as income rises).[3]

Age may affect criminal behavior in a variety of ways, but both the economics and criminology literatures indicate that Index I crimes are expected to fall as the population ages. Finally, Florida has experienced rapid population growth, and growth is often found to be associated with increasing levels of Index I crime. Thus, a positive coefficient is expected on the ΔPOP_k variable. This positive relationship may arise for several reasons; among the possibilities is that rapid population growth may mean weaker community ties and reduced probabilities of cooperation by neighbors and witnesses in crime prevention and reporting.

Intercept dummies are included for the 1995-1996, and 1996-1997 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 1994-1997 model. No a priori hypothesis is made regarding the signs of these coefficients, however.

As noted above, the change form model has the desirable characteristic of controlling for fixed effects. A disadvantage of this model is that variables that could have important relationships may not appear to because they do not change very much from year to year. Indeed, fixed effects models tend to bias the coefficients toward zero for this and other reasons. That is why we also consider a model with a four-year change, but that too may not be sufficient to uncover relationships either. A theoretically equivalent (for a two year period) method of controlling for fixed effects is to use levels rather than changes in the variables and then add dummy variables for each county, however, and while this method also biases coefficients toward zero, the bias is less pronounced. Therefore this third specification is also considered. All of the variables listed above are included in the model, but in levels rather than changes and the hypothesized relationships remain the same. Year dummies are also included to control for any statewide changes that affect all jurisdictions.

DATA

Cross-section data for Florida's 67 counties were obtained for 1994, 1995, 1996, and 1997. This period was chosen in part because Florida restructured its sentencing guidelines in 1993-94 to prioritize prison bed space. The resulting 1994 sentencing guidelines eliminated "unearned gain-time", which had previously allowed sentences to be reduced by about one-third. The new guidelines also eliminated early release credits (Control Release) due to a reduction in prison admissions and a massive and accelerated prison building program. These guidelines were implemented fully during 1994 and on October 1, 1995 the legislature passed a "truth-in-sentencing" requiring inmates to serve a minimum of 85% of their court-imposed sentences

(Florida Department of Corrections,1998). Thus, 1994-1995 was chosen as the beginning period of this analysis. The final period was chosen for two reasons. First, dramatic changes in sentencing policy occurred toward the end of 1998 when the Criminal Punishment Code was passed by the legislature which allowed for the imprisonment, up to the statutory maximum, of any felony offender regardless of their Guidelines point level score, a change that could impact all deterrence estimates. Second, final crime data for 1998 were not available at the time that this empirical work was started.

During the 1994-97 period drug arrests in Florida declined from 88,911 to 81,846 (an 8.0% decline).[4] This suggests that police resources available for non-drug crime control were probably increased. Arrests for Index I crimes also fell over the period, however. In fact, they fell even more rapidly than drug arrests. In 1990, 10.2% of all arrests in Florida were for drug offenses while 27.3% were for Index I crimes. Drug arrests as a percentage of all arrests rose to 12.4% by 1994. This trend continued so that by 1997 drug arrests accounted for 14.5% of total arrests while Index I arrests had fallen to 26.8%. Therefore, drug arrests as a percentage of all arrests rose from 1990 to 1997, but this increase was moderate when compared to the "war-on-drugs" of the mid-to-late 1980's. In fact, as Benson et al. (1998) point out, drug arrests increased *115%* between 1980 and 1987 (from 32,029 to 68,747), while Index I arrests as a whole increased by 29.2% (138,548 to 179,029). In the 1990's, however, drug arrests increased only 6.1 % between 1990 and 1997 (from 77,174 to 81,846) and as noted above, they fell during our study period.

Data on crime rates, police labor, and drug arrests were obtained from the Florida Department of Law Enforcement;[5] data on police capital expenditures were provided by the Florida Department of Banking and Finance, Bureau of Accounting; conviction data were found in documents provided by the Florida Supreme Court, Office of the State Courts Administrator; 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, Florida Statistical Abstract. Summary statistics are presented in Tables 2 and 3.

EMPIRICAL RESULTS

A straightforward replication of the Benson et al. (1998) year to year difference model is unsatisfactory as it explains only 13 percent of the variation in the estimation of equation 1, compared to 49% in the original article. This regression is not significant when evaluated at the .01 level with an F-test. This is probably the result of a sharp decline in the magnitude of the dependent variable: the average annual change in crime in 1994-1997 is about one-fifth of that in the 1983-87 period. Benson et al. (1994) also ran a regression using 1983-1987 differences, but the replication of this result for the 1994-1997 is not much more successful: the F-test

indicates a significant regression but only one variable (ΔPOL-LAB) is statistically significant.

Table 4 reports result of a replication of Benson et al.'s 1983-1987 difference equation with one change of specification. In the replication of this regression ΔPOL-CAP$_k$ was constructed by summing the capital expenditures for 1994, 1995, 1996, and 1997. The correlation coefficient between this constructed variable and ΔDRUG-ARR$_k$ is 0.73. An F-test revealed this to be highly significant, so the regression reported in Table 4 employs log ΔPOL-CAP$_k$ as a correction for this multicollinearity.

Police capital spending is negatively correlated with crime in Table 4, while more sworn officers do not have a significant impact on crime. Benson et al. found ΔPOL-LAB had a deterrent effect but was only significant in the 1983-1987 differences regression. They report that the ΔPOL-CAP coefficient was positive and significant in both regressions. They interpret this result to be consistent with arguments in favor of community policing discussed by Skolnick and Bayley (1988) and Trojanowicz and Moore (1988) and that capital intensive enforcement policy tends to be reactive rather than proactive in preventing crime. The results reported in Table 4 do not support their earlier finding that the marginal productivity of police labor is higher than police capital, which may suggest that police departments in Florida have adjusted their resources to avoid the negative impact of over-capitalization. The increasingly widespread use of community policing techniques is consistent with this finding.

The variable of primary interest here, ΔDRUG-ARR, is significant and positive at the .01 level. This result suggests that the relationship between drug enforcement

TABLE 4
DETERMINANTS OF CHANGE IN CRIME FOR FLORIDA

Variable	Coefficient	T-ratio
Intercept	11834	1.71***
ΔLOG-POL-CAP$_k$	-861.5334	-1.81***
ΔPOL-LAB	-2.44956	-0.624
ΔPRB-CON-I$_k$	3252.5254	0.817
ΔPRB-CON-II$_k$	512.5675	0.130
ΔPRB-CON-D$_k$	-552.452	-0.209
ΔDRUG-ARR$_k$	2.19821	3.259*
Δ%BLACK$_k$	3684.798	0.131
ΔINCOME$_k$	0.38635	0.732
Δ%AGE15-24$_k$	-192136	-1.86***
ΔUNEMPLOY$_k$	-47835	-1.550
ΔPOP$_k$	-0.0566	-1.69**
Adjusted R^2	0.42	
F-Statistic	5.37	

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

and crime in 1994-1997, a period of modestly falling drug enforcement, is not unlike that of 1983-87 when drug arrests were rising rapidly. Since the relationship between drug enforcement and crime is theoretically indeterminate due to countervailing influences, this evidence supports the argument of Benson et al. (1992), Sollars, Benson, and Rasmussen (1994), and Benson, Rasmussen and Kim (1998) that the opportunity cost of police resources is the dominant factor in this trade-off. It is necessary to note the caveat that this result is not derived from an exact replication of the earlier study, but the relative homogeneity of the 1994-1997 period makes this evidence credible.[6]

The probability of conviction given arrest for Index I, Index II and drug offenses (PRB-CON-I_k, PRB-CON-II_k, and PRB-CON-D_k, respectively) cannot be distinguished from zero in Table 4. This is probably the result of two factors. First, all counties are affected by the same sentencing guidelines and variations in these laws are likely to affect all counties simultaneously, so the affect of such changes will be picked up in the time dummy variables. Furthermore, the years were selected to be relatively homogenous with respect to state sentencing policies. Second, fixed effects models tend to bias coefficients toward zero, so less weight can be placed on insignificant coefficients than on those that are statistically significant.

Three variables are intended to measure economic conditions and the opportunity cost of committing crime: INCOME, UNEMPLOYMENT, and %BLACK. As in Benson et al., all three of these variables are insignificant, no doubt because these variables are not changing much over such a short period of time. An increase in the proportion of the population between the ages of 15 and 24 is expected to increase crime, but this coefficient is negative and marginally significant at the .10 level in Table 4. Finally, ΔPOP also has a marginally significant negative coefficient that is contrary to our expectation.[7]

The regression in Table 4 is as close a replication of the Benson et al. model that is possible given the data for the 1994-1997 period. As suggested above, we also explored an alternative strategy to model changes in crime during the relatively homogenous 1994-1997 period. This is a fixed-effects model that directly controls for unobservable factors that can affect crime in jurisdictions but are relatively constant over time. To control for fixed effects we estimate an OLS regression in levels that also includes dummy variables for the years 1995, 1996, and 1997 as well as for 66 of the 67 Florida counties. Thus, the dependent variable is the number of Index I crimes reported each year over the period. The coefficients on the county dummies capture the fixed effects that are imperfectly controlled for in the difference model. In addition, all the independent variables are logged so the coefficients show the percentage change in crime that can results from a one percent change in the independent variable. The results (without displaying the 66 county dummies) are presented the Table 5.

Neither police capital or labor are significantly different from zero in this model.[8] The three variables measuring the probability of conviction given arrest for the three types of crime are also insignificant, probably for reasons noted above. The control variables are consistent with expectations. Income has a positive but insignificant impact on Index I crime while %BLACK is negative and insignificant. More unemployment, relatively more people between the ages 15 and 24, and a larger population significantly increase crime, although the latter two variables are only marginally significant at approximately the .10 level.[9] Of particular interest here is that DRUG-ARR is positive and significant at the .05 level, thus supporting the notion that increasing drug arrests, on the margin, siphon police resources away from combating crimes against persons and property.[10]

TABLE 5
A FIXED EFFECT MODEL OF THE DETERMINANTS OF CHANGE IN CRIME FOR FLORIDA COUNTIES, 1994-1997

Variable	Coefficient	T-ratio
Intercept	-20.5678	-1.34
ΔLOG-POL-CAP$_k$	0.003203	0.125
LOG-POL-LAB$_k$	-0.110964	-1.18
LOG-PRB-CON-I$_k$	-0.061804	-0.697
LOG-PRB-CON-II$_k$	-0.023813	-0.248
LOGPRB-CON-D$_k$	0.057191	0.712
LOG-DRUG-ARR$_k$	0.180184	1.96***
LOG-%BLACK$_k$	-0.128902	-0.459
LOG-INCOME$_k$	1.512901	1.24
LOG-%AGE15-24$_k$	1.522095	1.78***
LOG-UNEMPLOY$_k$	0.396396	2.30**
LOG-POP$_k$	1.461272	1.61***
95DUM	-0.041141	-0.608
96DUM	-0.025333	-0.254
97DUM	-0.038965	-0.285
Adjusted R^2	0.98	
F-Statistic	176.294	

+ Logged Least-squares dummy-variable regression, 66 county dummies not listed

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

CONCLUSIONS

Drug enforcement and crime have been linked in a variety of ways. Some advocates of the heavy drug enforcement argue that drug use is criminogenic, thus implying that crime will fall if the full price of drug use is increased by a higher probability of arrest and more severe punishments. Drug enforcement obviously increases drug prices, which, if illicit drugs are normal goods, will result in less use. Caulkins and Reuter (1998) report the drug use is responsive to prices. Thus, to the extent that drug use causes crime, higher enforcement would be associated with a decline in crime.

The widely recognized downside of rising prices, however, is the possibility that addicts may turn to crime to support their habit. There is relatively little empirical evidence that higher prices in turn compel people to commit more index crime, as the most commonly cited source is Silverman and Spruill (1977). Enforcement on the supply side is not likely to be uniform across all types of drugs and thus higher enforcement is likely to change the relative prices of various illicit substances. If drug users respond to relative prices, as commonly postulated by economists studying drug markets (Caulkins & Reuter, 1998; Rasmussen & Benson, 1994), then drug users facing a higher price for the drug of choice have a myriad of options which include lowering use so spending stays within currently available income, quitting altogether, perhaps shifting to a substitute drug that is now relatively cheaper, or maintaining current drug use and financing it through criminal activity. Even if they choose the latter option, there is some question whether Index I crime will necessarily rise because they may engage in Index II activities such as selling drugs or engaging in prostitution. The crack epidemic was thought to have bred criminal activity, but Johnson, Golub, & Fagan (1995, p. 281) refute this, finding 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.

Benson et al. (1992, 1998) and Sollars et al. (1994) argue that limited police budgets mean that the opportunity cost of resources used for drug enforcement is other police activity that is forgone. Caulkins et al. (2000) legitimately question the generality of this result, suggesting that Florida's experience in the mid-to-late 1980s is not necessarily typical of other times and places. This paper provides additional empirical support for the notion that the opportunity cost of higher drug enforcement is increased Index I crime found by Benson et al. (1992) and Sollars et al. (1994) by considering a very different time period for Florida. Other studies also provide evidence that is consistent with the trade-off hypothesis. Mendes (2000) replicates the Sollars et al. model using data for jurisdictions in Portugal and shows that the probability of arrest for property crimes falls with an increase in drug arrests. Furthermore, Corman and Mocan (2000), in a time-series analysis of crime in New York City, conclude that despite a positive relationship between heavy

drug use and robbery and burglary, this relationship is weaker than the positive effect of drug arrests on crime.

That police resources are constrained is a reasonable assumption given the other claims on state and local resources (Rasmussen & Benson, 1994), so the trade-offs between drug enforcement and Index I crimes reported above are not surprising. The implications for drug policy are also reinforced by the findings in Caulkins et al.'s (2000) innovative attempt to develop a dynamic optimal control model to explore the relationship between drug enforcement and property crime. Their model explores the conditions under which drug enforcement, which raises drug prices, is a productive policy. Their model ignores the opportunity costs of the police resources used for this purpose, but with parameter values based on the U.S. cocaine epidemic they find that "as use grows toward its steady state, enforcement *intensity* (italics in original) should decline." Thus even ignoring the opportunity costs of scarce resources, their model suggests that drug enforcement might be too high given plausible parameter values. The consistent evidence that drug enforcement may promote crime by redirecting police resources away from Index I crime is even more pertinent for efforts to curb drug use in light of Caulkins et al.'s questions about the potential ineffectiveness of enforcement in reducing drug use.

NOTES

1. For the derivation of this empirical model and a theoretical explanation of the econometric model's properties please refer to Benson et al. (1998, pp. 82-85).
2. To see why the probability of conviction for Index II and drug crime appear in the regression, see the theoretical derivations in Benson et al. (1998). The probability of arrest does not appear in this model because it is presumably a function of police capital and labor resources that are included in the model.
3. Rasmussen and Benson (1994) indicate that illicit drugs are normal goods, meaning that people who consume such goods tend to demand more as their income rises.
4. All arrests related to any illegal substance by state and local authorities are included in this measure.
5. Due to budget cuts, the way the Florida Department of Law Enforcement collected data changed for the years 1996 and 1997, but this change did not affect Index I crime rates. However, four Index II crime categories (Kidnap/abduction, extortion/blackmail, bribery, and intimidation) were not reported as separate categories, although they were probably captured in a new "miscellaneous" category. This should not present a real problem, however, for those categories were a small percentage of total Index II crimes.
6. An alternative interpretation of these results is that more drug use increases Index I crimes and drug arrests, thereby suggesting that the positive correlation

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between drug arrests and crime is spurious. We cannot directly test this hypothesis since data on drug use are not available. However, Benson et al. (1992) estimated a simultaneous equation model of the impact of drug arrests on crime in which they were able to estimate drug use by using a recapture methodology using data for 1986 and 1987. Those results are consistent with our findings: even after controlling for drug use, reallocating police resources to make more drug arrests appears to increase Index I crime. In addition, Resignato (2000) controls for drug use by using the cities sampled in the "Drug Use Forecasting" (DUF) surveys. He finds that drug enforcement measured by drug arrests as a portion of total arrests is positively and significantly related to violent crime rates in all specifications but that drug use, measured by the DUF estimates, is not significantly related to violence in general. Drug use is significant in one murder specification but the drug enforcement effect still holds in that specification.

7 As is shown below, this anomalous result appears to be the consequence of this specification that does not adequately control for many unobservable factors that may affect crime rates. Indeed, when these unobservable characteristics are controlled in the alternative fixed effects model below, these coefficients have the expected positive sign.

8 This result is not surprising, given the findings in the large empirical literature on deterrence. See Benson, Kim and Rasmussen (1994) for a discussion of the relationship between police resources and crime rates.

9 DRUG-ARR is consistently positive and significant in a variety of specifications using the fixed effects model. Control variables, particularly %AGE15-24 and UNEMPLOY, vary considerably in alternative fixed effect specifications.

10 The coefficient indicates that a one percent increase in drug arrests is associated a .18 percent increase in Index I crime. This coefficient is quite consistent with the results in Benson et al. (1992) where a control for drug use was included in a somewhat different model that employed data from the 1980s.

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