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CRIMINAL DETERRENCE: REVISITING THE ISSUE WITH A BIRTH COHORT

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ABSTRACT

In this paper, we estimate the general deterrent effect of criminal justice resources on criminal behavior. Our panel data, which combine individual-level information on arrests and personal characteristics with aggregate measures of criminal justice resources, allow us to obtain deterrence measures that more closely reflect theoretical concepts and are of potential policy relevance. We find robust evidence of a general deterrent effect in our estimates of error components probit and Tobit models.

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Introduction

In this paper, we consider the criminal behavior of a cohort sample of young men over a seven year period. We are interested primarily in the general deterrent effect of criminal justice resources. The general deterrence hypothesis is premised on the idea that criminal justice system actions (e. g., arrests) affect the decisions of individuals who have had no contact with the system as well as individuals apprehended and punished by the system.

Although the basis for the deterrent effect is well-established, it has proved remarkedly difficult to determine its significance or magnitude. The vast majority of the empirical work on deterrence uses aggregate data on crime rates rather than data that more directly measure individual criminal behavior. Not until the 1980s did researchers begin to use individual data to estimate economic models of crime. While these studies have made valuable contributions, they suffer from a number of difficulties some of which we are able to address. Previous studies generally use data for "high-risk" individuals such as prison releasees. In addition, most of the data is for cross-sectional samples. With this type of data, it has proved very troublesome to obtain general deterrence measures that differ across individuals in a manner independent of their criminal decisions.

To estimate our model, we use data for a cohort sample that is representative of the general population of young males in large urban areas. These data allow us to examine the general deterrence effect for individuals with limited, if any, contact with the criminal justice system as well as more serious offenders. We exploit the longitudinal nature of the data and

the combination of individual and aggregate data to obtain general deterrence variables such as the police resources per offense. These measures of general deterrence are consistent with theoretical concepts and are of potential policy relevance.

To preview briefly our results, we find robust evidence for a general deterrent effect of criminal justice resources. The effect is significant across specifications of the model, measures of criminal justice resources, measures of criminal activity, and estimation methods. Our results suggest that the general deterrent effect may be strongest for individuals with limited previous contact with the criminal justice system. This finding may explain, in part, the difficulties uncovering general deterrent effects in studies of prison releasees. These results are for one data sample and should, as such, be viewed with caution.

The organization of the paper is as follows. In the next section, we review the empirical literature and discuss difficulties identifying general deterrence effects. In Section II, we describe the data and our empirical model. Section III contains our empirical results and the final section our conclusions.

I. The Literature

Becker (1968) established the framework for applying economic models of individual decision making to questions related to crime and public policy. The early empirical work by economists used aggregate data on crime rates. Most of the studies were simultaneous equations models of the crime rate and some measure of sanctions such as mean prison time served. Identification of the crime equation was achieved by excluding sociodemographic variables such as population density, police resource variables, or lags of the crime rate.

This empirical work was seriously questioned by a National Academy of Sciences' panel (Blumstein, Cohen, and Nagin, 1978) and others (e.g., Brier and Fienberg, 1980). The panel noted the aggregation bias inherent in using aggregate data on crime rates to test models of individual crime decisions and could find little, if any, justification for the exclusion restrictions used to identify the crime equation. While the panel did not reject the simultaneous equations approach, they concluded that the previous research based on this model warranted no definitive conclusions about the extent of any deterrence effects. Further, many researchers suggested that little would come from additional attempts to estimate deterrence models with this approach (e.g., Cook, 1980).

In 1980, researchers began using individual data, generally cross-sectional, to estimate economic models of crime. This work focused upon the effect of the individual's experiences with the criminal justice system and did not address the issue of general deterrence. For example, in Witte's (1980) work, the probability of conviction is proxied by the fraction of prior arrests that resulted in conviction. This method of measuring deterrence variables can be challenged since such variables may reflect differences in the types of crimes committed rather than any difference in the probability of arrest, ceteris paribus. Further, if there is autocorrelation in criminal behavior, these deterrence variables are not exogenous regressors.

This work does provide information on the relationship between work and crime and on the effect of punishment on the individual punished. This latter effect is referred to as the specific deterrent effect in the criminology literature. However, this work provided limited, if any, information concerning the general deterrent effect of the criminal justice system. See Tauchen, et al. (1988) or Trumbull (1989) for more detailed reviews.

II. Empirical Model

Our primary data are for a ten percent random sample of males born in 1945 and residing in Philadelphia between their 10th and 18th birthdays. We combine this individual information with data on the total number of offenses, police budgets, macroeconomic indicators and neighborhoods in Philadelphia. Information was collected from school records, draft registration records, the Philadelphia Police Department, the FBI, a compendium on city government finances, the Philadelphia Community Renewal Program, and interviews carried out in 1970-1971. Of the 975 individuals in the ten percent sample, 567 were interviewed. Researchers have carefully considered the issues of both response and nonresponse bias for the interview data and conclude that the bias is "not sufficient to distort correlational analysis or to alter appropriate conclusions concerning statistical significance" (Thornberry and Christenson, 1984, p. 401).

Using this data base, we create two panels, one a seven year panel that traces cohort members' activities from 1964 through 1970 and the other an eight year panel that ends in 1971. Since almost half of the interviews were conducted in 1970, the seven year panel contains approximately twice the number of observations as the eight year panel. The results reported in the paper are for the seven year panel. Results for the eight year panel and more details on the data are available in Tauchen, et al. (1988).

According to the standard economic model of crime, as described by Becker (1968) and developed by Ehrlich (1973) and others, an individual's criminal activity depends on total income from legal activities, the preferences of the individual, and exogenous factors that affect the probability of arrest and the sanctions imposed. The empirical measures of these

variables and the method of estimation are discussed below.

The two most difficult measurement issues relate to criminal activity and criminal justice system actions. We measure criminal activity in two ways. The first measure is a binary for whether or not the individual was arrested during the year. This measure, which is widely used in the literature, implicitly treats all offenses as identical. Members of our sample committed a wide variety of offenses[1] and at times committed more than a single offense during a year. Given this heterogeneity in behavior, we use a second measure of criminal activity that reflects both the seriousness and frequency of arrests. This measure uses a crime seriousness index developed by Sellin and Wolfgang (1964) to obtain a seriousness score for each arrest. The seriousness scores of all arrests during a year are summed to obtain an index for criminality during the year.

The data provide some insights on elements of the criminal career paradigm (Blumstein, et al., 1986). The data has information on "participation" (the proportion engaging in crime), "frequency" (the rate of criminal activities for those who are active), and "career length" (the length of time an offender is active). Of the individuals in the sample, 63 percent had neither a juvenile record nor an adult arrest. Nineteen percent had a juvenile record but no arrest as an adult; nine percent had a juvenile record and one or more adult arrests; and nine percent had no juvenile record but one or more adult arrests. Of the adult offenders, 56 had a single arrest, 22 percent two arrests, and 22 percent more than two arrests. The maximum number of arrests is six, a feat achieved by two individuals.

The second measurement issue relates to criminal justice system actions and general deterrence. In a standard model of criminal choice, the probability of arrest depends on the

level of criminal activity, on the individual's ability to avoid arrest and on exogenous factors related to the criminal justice system. Since the probability of arrest depends on an individual's criminal decisions, the individual's own experience with the criminal justice system cannot be used as an explanatory variable. Most of the observed variation in the probability of arrest will result from differences in crime seriousness and crime frequency not from exogenous differences in the probability of arrest. Cook (1979) has made a similar observation concerning the use of the clearance rate (i.e., fraction of crimes cleared by arrest or convictions) as a measure of deterrence. As he points out, the deterrent effect cannot be captured through the use of a variable such as the clearance rate which is affected by criminal behavior. Appropriate measures of criminal justice actions are the exogenous factors that cause independent variation (e.g., variation that does not depend on the type and extent of criminal activity) in the probability of arrest.[2] These variables include changes in criminal justice resources and policies, and differences in individual ability to avoid arrest. Since theory provides strongest support for the general deterrent effect of arrest and weaker support for such an effect flowing from conviction or imprisonment (Schmidt and Witte, 1984), we concentrate on the police system. Since there were no major changes in police policies during the study period, we use the police budget in real dollars per offense as our primary measure of general deterrence.[3] In addition, we consider the effect of police employment and total criminal justice system employment.

We are not able to measure the returns from legal activities directly since there are no income or wage variables in our data set. However, we have information on the time allocated to work and school during each year and we incorporate these variables as partial

measures of the returns to work and schooling. To reflect further returns to both work and schooling, we incorporate factors generally correlated with wages (i.e., IQ and a binary for whether or not the individual received a high school degree).

The variables related to preferences are of three types: (1) variables that reflect family and community backgrounds (i.e., a binary equal to one if both parents were born in the U.S., a measure of the occupational status of the household head when the boy was in high school, a binary equal to one if the boy attended primarily parochial schools, the number of addresses during the school years, average income in the neighborhood of residence during high school); (2) variables reflecting personal characteristics (i.e., IQ, a binary equal to one if the individual is white); (3) variables reflecting activities that occurred during the juvenile or young adult years (i.e., three variables indicating the type of charge at first arrest, the number of police contacts as a juvenile, the percent of juvenile police contacts resulting in formal criminal justice system processing, a binary equal to one if the individual is married). Finally, we include a variable for the year of the panel to reflect the aging of the cohort and other trend factors.

Most variables likely to reflect differing abilities to avoid arrest (e.g., intelligence or like-minded friends) are also likely to be related to differences in the individual's "taste" for crime. To reflect this confounding of effects, we interpret the coefficients on such variables as reflecting some mixture of preference and deterrence effects.

We estimate the models for the binary measure of criminal activity by an error components probit model. The underlying equation for the probit model is

$$c^*_{it} = x_{it}\beta + \eta_{it}$$

where c^*_{it} is an unobservable latent variable for the level of arrest, x_{it} is a vector of explanatory variables including the criminal justice variables, β is a vector of parameters to be estimated, and η_{it} is the random error term. The subscripts i and t index the individuals and time periods respectively. The observed binary for whether or not an individual was arrested is assumed to be determined as in the usual probit model,

$$c_{it} = \begin{cases} 1 \text{ if } c^*_{it} \geq 0 \\ \\ 0 \text{ otherwise.} \end{cases}$$

The error term in our probit model has three components,

$$\eta_{it} = \mu_i + \lambda_t + \varepsilon_{it}$$

where μ_i, λ_t and ε_{it} are normally distributed i.i.d. random variables. The first component of the disturbance term (μ_i) is correlated across time for any individual and is included to reflect unmeasured, persistent individual effects. The second component of the disturbance term is correlated across individuals for any given time period (λ_t) and is included to allow for the possibility that there is correlation in the random component of criminal behavior for members of our sample during any given year. As has been pointed out by Dickens and Katz (1987) and Moulton (1990), there may be correlation in the error terms for observations that have the same value for aggregate variables such as our criminal justice variables. The final component (ε_{it}) is the purely random portion of the disturbance. If the error is uncorrelated with the explanatory variables, the parameter estimates of the error component probit model are consistent and asymptotically efficient (Chamberlain, 1984).

III. Empirical Results

Table I contains empirical results for the binary measure of criminal activity. The first column is for a specification including only variables that are unaffected by an individual's criminal or time allocation decisions (e.g., police resources, family background and neighborhood characteristics). The second column contains results for a specification that also includes predetermined variables related to the juvenile criminal record. The last column is for a specification including variables related to activities that occurred in the current year (e.g., fraction of the year employed) or previous years, possibly during the sample period (e.g., high school graduation). We estimate three specification to check the robustness of results. We are particularly concerned about possible correlation between the variables included only in Models 2 and 3 and the error term and with the bias this can impose.

The significance tests reported in Table I are based on likelihood ratio statistics. These test statistics are distributed asymptotically χ^2 with one degree of freedom. For nonlinear models, such as ours, likelihood ratio tests are preferred to classical "t-tests" based on the information matrix because they are invariant to nonlinear reparameterizations (Gallant, 1987). For convenience, we provide the probabilities of obtaining the likelihood ratio statistics (p-values) under the null hypotheses that the parameters are zero.

The estimated variance of the individual effects component of the random disturbance (μ_i) is significant at any normal level and is of the same order of magnitude as the variance of the purely random component of the error term. The individual effects component might reflect such things as differences in attitudes towards the law. The estimated variance of the time component of the random disturbance (λ_t) is extremely small in magnitude and

statistically insignificant. We find no evidence of significant correlation in the error terms across individuals within the same year.

Given these findings, we estimate Tobit models for the index measure of criminality allowing for only two error components, the individual effect and the purely random effect. Since the implications of the Tobit models are similar to those of the probit model, the estimated coefficients are not reported. The results are available in Tauchen, et al. (1988).

The probit and Tobit models are significant in all specifications and the estimated coefficients, when significant, are of the same sign in all models. The estimated coefficients on the variables of primary interest are stable in sign and magnitude across specifications for a given estimation technique. In particular, the estimated coefficients on the variable for police resources are negative and significant in all specifications for both the seven and eight year panels. Further, the magnitudes of the estimated coefficients on the police resource variable are not significantly different across specifications.[4] The magnitude of significant coefficients for most other variables is also quite stable although the significance of coefficients on some family background (e.g. occupational status of household head) and personal characteristics are more variable due to collinearity.

The results for the police resource variable may be of most interest. We find that increased real police budget per offense is consistently (across panels, specifications, and estimation techniques) associated with decreases in both the binary and index measure of crime. We also estimated the binary model using alternative measures of criminal justice resources including police officers per offense, total criminal justice employees (police, courts, and local corrections) per offense, real police budget per young male and real police

budget per capita. With all measures and for all specifications of the model, the estimated coefficients on the criminal justice resource variable were negative and statistically significant at the .05 level or better.

The use of individual data on crime and aggregate, city-wide measures of general deterrence avoids some obvious sources of simultaneous equations bias inherent in general deterrence measures based on the individual's own criminal history. However, use of a disaggregated dependent variable does not preclude endogeneity of aggregate explanatory variables (Cushing and McGarvey, 1985). To explore the possibility that the police resource variable is endogenous, we applied endogeneity tests developed for probit models by Rivers and Vuong (1988). All tests support the null hypothesis that the police resource variable is exogenous. These finding are consistent with the recent work by other researchers. For example, in his study using aggregate data for individual states, Trumbull (1989) finds very strong evidence for the exogeneity of his police resource variable.

With the use of the aggregate deterrence variable, an additional concern might be that the police resource variable is correlated with some other factors, such as general economic conditions, that might affect the level of crime.[5] We estimated models including variables for the real manufacturing wage in Philadelphia, real per capita income, and the unemployment rate (created by assigning the white male unemployment rate to whites and the black male unemployment rate to nonwhites).[6] None of the estimated coefficients on these variables were significant (p-values greater than .2). The estimated coefficients on the police resource variable remained negative and significant. Although there is no way to rule out all possible "third" factors, we do not find evidence that factors commonly hypothesized to affect

the level of crime serve this role.

A final concern is that the coefficients on our criminal justice resource variables reflect at least in part the "incapacitation" of some members of our sample by imprisonment. Men who are incarcerated are not at risk of being arrested. To check on this possibility, we estimated our model excluding individuals incarcerated during the sample period. The estimated coefficients on the police resource variable remained negative and were somewhat larger in magnitude and significance. These results suggest that individuals who were not incarcerated during the sample period are more deterred by additional police resource than those who were incarcerated. This finding may explain why studies using prison releasee have difficulty identifying significant general deterrent effects.

To explore further the relative magnitude of the general deterrent effect for different groups, we estimated a model with an interaction term that was the product of the police resource variable and a binary for whether the young man had a juvenile record. The estimated coefficient on the interaction term is positive and marginally significant (p-value .12). The coefficient on the police resource variable remains negative and is now slightly larger in magnitude. This coefficient reflects the general deterrent effect on individuals with no police contact prior to our sample period. When coupled with the previous finding, this suggests that general deterrence may be strongest for individuals with limited, if any, previous contact with criminal justice system.

We find no evidence of a specific deterrent effect. The second and third specifications of the model include a variable for the percent of juvenile police contacts that result in formal criminal justice processing. The estimated coefficient on this variable is negative but not

significant, or even marginally significant, in any specifications of the model.

From a policy perspective, the magnitude as well as the existence of general deterrence is of interest. Our results imply that the elasticity for the expected number of years with no arrests with respect to police resources is .47. This elasticity indicates that increasing real police resources per offense by ten percent throughout the seven year period would have led to a 4.7 increase in the expected number of years with no arrests for individuals representative of our sample. For values representative of our data, the estimated percent of individuals with no arrests would have increased from 85.2 to 89.2 percent. While such a change is certainly desirable, an increase in the percent of young men remaining crime free of this magnitude can not be expected to solve the crime problem.

The magnitude of the general deterrent effect may be larger than implied by the coefficients on police resources we report. The coefficients on police resources in an arrest equation is the net result of two conflicting effects. The first is a negative general deterrent effect upon crime, and the second is a positive resource effect on the probability that a person who commits a crime is arrested. In order to obtain a negative coefficient on police resource variables, the deterrent effect of police resources on crime must be sufficient to outweigh the positive effect of police resources on the probability of arrest.

Recall that in the standard economic model of crime the returns to legal activity also affect the criminal choice. We seek to reflect these returns by incorporating variables related to time allocation and to the wage. The third specification of the model includes variables for the fraction of time working and in school, and both are associated with less crime. As do other researchers, we find that the estimated effect of working is larger than that of schooling

but the difference is not statistically significant (Farrington et al., 1986; Gottfredson, 1985; and Viscusi, 1986). The estimated effects of employment and schooling on crime may in part indicate the way in which labor market status affects the opportunity cost of crime. Also, the coefficients on these variables undoubtedly reflect differences in personal characteristics associated with working or being in school. As Viscusi (1986) points out in his study of black youths, the estimated coefficients on the employment and education variables may overstate the crime reduction that would result from greater legal-sector opportunities.

Although being in school is associated with fewer arrests, educational attainment has no effect. The insignificant coefficients for high school graduation do not appear to result from collinearity.[7] In addition, the significance of the high school graduation variable does not change if the work and schooling variables are omitted. The estimated coefficient on IQ is negative and significant in all specifications. This is consistent with the hypothesis that individuals with higher IQs have better legal labor market opportunities, but may also reflect the ability of high intelligence individuals to avoid arrest.

Individuals who attended parochial junior and senior high schools are significantly less likely to be arrested than those who attended public schools. The parochial school effect might arise because of the educational experience, omitted characteristics related to parochial school choice, creaming, or factors associated with religious commitment. In a study using the National Longitudinal Survey of Young Men and Women for a time period that overlaps with our sample, Kessler (1990) finds that private school (primarily parochial school) students are more likely to obtain managerial or professional positions but do not earn significantly higher wages than their public school counterparts. Studies with data on church attendance find that

it is a significant determinant of who escapes inner-city poverty (Freeman, 1986) and is associated with lower self-reported participation in crime (Viscusi, 1986). To check on the possibility that parochial school are creaming by expelling crime-prone students, we estimated a model with a binary for whether the boy switched from parochial to public schools. The coefficient on the binary was positive and significant indicating that individuals who switched, ceteris paribus, were more likely to be involved in criminal activity. However, there was essentially no change in the magnitude or significance of the coefficient on the parochial school binary. While no definitive conclusion regarding the source of the parochial school effect is possible, the literature and additional work with our data suggest that the effect may stem from non wage-benefits of white-collar employment, factors associated with church attendance, or omitted family characteristics associated with the parochial school choice.

We do not discuss results for other variables in detail since they generally support previous findings (see Blumstein, et al. (1986) for a survey) and are of less interest to economists. To summarize, we find that unmarried, nonwhite young men with serious and extensive juvenile records, who grew up in households the heads of which had low status jobs are more likely to be arrested, ceteris paribus. We find no evidence that growing up in immigrant families or low income neighborhoods affect subsequent arrest rates.

IV. Conclusions

This paper provides new evidence concerning the general deterrent effect of criminal justice resources. We exploit the longitudinal nature of our data to construct general

deterrence measures that reflect variation in the probability of arrest that is independent of the individual's criminal choices. The deterrence measures we use (e.g., police resources per offense or per capita) can reasonably be regarded as exogenous to the individual. Statistical tests provide further support for the exogeneity of police resources. Thus, our approach appears to be less subject to one of the biases that has been common in both individual and aggregate studies.

Our empirical results provide robust evidence of a general deterrence effect of criminal justice, particularly police, resources. Further, our result suggest that general deterrence may be strongest for individuals with limited previous contact with the criminal justice system. Future studies of general deterrence may benefit from having data that includes individuals with limited or no previous contact with the criminal justice system as well as offenders.

Our results should be viewed with caution since they are based on one data sample. In future work, it would be useful to have data for a number of urban areas along with more detailed information on the criminal justice system. Estimates of the general deterrent effect could be strengthened if the criminal justice system experienced exogenous policy changes during the study period.

Endnotes

1. Members of the sample had 147 arrests during the sample period. Eight percent of these arrests were for crimes with potential or actual violence (homicide, rape, assault, and robbery), 25 percent involved theft of property (burglary, larceny and motor vehicle theft) and the remaining arrests involved offenses such as drug sales and possession, and buying and receiving stolen property.

2. Block, Nold, and Sidak (1981) make this same observation with respect to antitrust enforcement and use an approach similar to ours in their study.

3. Specifically, our measure is real (in 1984 dollars) police resources per FBI index offense. The FBI provides aggregate information on the number of index offenses (homicide, rape, assault, robbery, burglary, larceny and motor vehicle theft) for metropolitan areas. We believe this measure is a good indicator of the relative risk of arrest in Philadelphia during the period we study. The policies and reporting practices of the police department and the mix of offenses changed little during the period. For example, the ratio of person offenses to total index offenses was 23 to 26 percent during the seven years.

4. Since this is a nonlinear model, we also compared the implied effect of a change in police resources on the probability of the crime patterns and found no appreciable differences across the three specifications of the model.

5. Other factors that might have caused changes in both police resources and individual offenses are riots and racial tensions. According to the Facts on File Yearbook, the most serious riots in Philadelphia during the years of our panel occurred in 1964 and 1967. Wikstrom (1974) classifies these riots as minor by the standards of the time. For 1964, our sample members had high arrest rates and in 1967 low arrest rates. Police resources were relatively low in 1964 and high in 1967. The riots seem unlikely to provide an explanation for both the pattern of individual offenses and the level of police resources.

Yet another possibility is that changes in court or correctional practices or in the legal code could have caused the general deterrence result we observe. Our conversations with researchers familiar with the Philadelphia criminal justice system and our reading of the literature indicate no major changes during this time period.

6. All but two of the nonwhites in the sample are black.

7. The correlation between high school graduation and IQ, fraction of time in school, fraction of time employed, and attendance at a parochial high school are .395, .264, -.187, and .278 respectively.

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TABLE I

RESULTS FOR THE PROBABILITY OF ARREST
(P-Values in Parenthese)

INDEPENDENT VARIABLES	MODEL 1	MODEL 2	MODEL 3
GENERAL DETERRENCE			
Real police budget per offense	.020*** (.01)	-.019*** (.01)	-.018*** (.01)
RETURNS TO LEGAL ACTIVITIES			
IQ	.011 (.11)	-.016** (.03)	-.015* (.07)
Percent of year employed	.....	.....	-.006** (.02)
Percent of year in school	.....	.....	-.009*** (.00)
Binary equal to 1 if had a high school degree			-.086 (.63)
AGE/TREND			
Year	.005 (.88)	-.003 (.93)	-.003 (.93)
FAMILY BACKGROUND			
Binary equal to 1 if parents US born	.314 (.34)		.145 (.67)
Occupational status of household head during high school	-.005 (.22)		-.008* (.06)

(.17)				
Number of addresses during primary and secondary school	.110*	-.041		-.031
	(.03)			(.32)
(.48)				
Binary equal to 1 if attended parochial high school		-.390*	-.596**	-.458*
		(.08)		(.02)
(.08)				
NEIGHBORHOOD CHARACTERISTICS				
Average income in neighborhood during high school ($1000)		.077		.114
		(.52)		(.34)
.137 (.19)				

TABLE I (Continued)

INDEPENDENT VARIABLES	MODEL 1	MODEL 2	MODEL 3
PERSONAL CHARACTERISTICS			
Binary equal to 1 if white		-.560**	-.602***
		(.02)	-.643*** (.00)
Binary equal to 1 if married			-.304* (.05)
PAST CRIMINAL ACTIVITIES			
Binary equal to 1 if first arrest is a serious personal crime			1.756*** 1.432*** (.00)
Binary equal to 1 if first arrest is a less serious personal crime			.555** (.03) .410* (.09)
Binary equal to 1 if first arrest is a property offense			.666*** .598*** (.00) (.02)
Number of times in police custody as a juvenile			.152*** .134*** (.00) (.00)
Percent of juvenile police contacts resulting in formal criminal justice processing			-.002 -.002 (.65) (.60)
CONSTANT	1.297		1.319 .923
	(.92)		(.53) (.67)
VARIANCE OF ESTIMATED INDIVIDUAL EFFECT			.957*** 1.093*** .937
	(.00)		(.00) (.00)
VARIANCE OF ESTIMATED		1.03 E-8	4.24 E.9 . . 7.72 E-7

TIME EFFECT		(1.00)	(1.00)
(1.00)			
LOG LIKELIHOOD		-935.21	-
914.08 -907.56			
N . . .	2856		2856
2856			

*Significant at the .10 level, two-tailed test.
**Significant at the .05 level, two-tailed test.
***Significant at the .01 level, two-tailed test.