# CERTAINTY VS. SEVERITY OF PUNISHMENT

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*Recent research has generated conflicting findings regarding the role of employment and earnings versus criminal justice sanctions in reducing crime. Further disagreement exists over the relative effectiveness of increased certainty versus increased severity of punishment as deterrents to crime. This paper uses a large data set containing criminal and labor market histories of a broad sample of young male arrestees to estimate an economic model of crime. Deterrence, incapacitation, and criminal human capital effects are measured, and the effects of employment and earnings on criminal activity are estimated. The results largely reconcile the conflicting findings from previous research.*

## I. INTRODUCTION

In recent research, analyses of individual-level data have led to conflicting conclusions concerning the economic model of crime. Two points of contention have arisen: first, whether increases in the severity of punishment exert a stronger deterrent effect than comparable increases in the certainty of punishment, and second, whether stronger criminal justice sanctions or better labor market performance more effectively reduces crime. Ann Witte [1980] concluded from her analysis of North Carolina prison releasees that certainty of punishment carried a greater deterrent effect than punishment severity, and that better labor market rewards had relatively little effect on criminal activity. In contrast, Samuel Myers's [1983] analysis of the post-release experience of ex-prisoners in federal and Maryland institutions led him to conclude that improved labor market measures led to lower levels of criminal activity than increased criminal sanctions. Further, while his results indicated that a decrease in crime was associated with increasing severity of the most recent sanction, he found an apparently perverse positive effect associated with increasing punishment certainty.

One is tempted to speculate that these results stem in some way from the characteristics of the released prisoners, who on average had extensive criminal histories prior to the observation period. Individuals with extensive prior imprisonment may simply be unable to find any but the most menial jobs, leading them to prefer criminal activities in spite of apparently high risks of punishment. Further, the use of such specialized samples of individuals brings into question the extent to which even corroborating findings could be generalized to inform policy.

Phillips and Votey [1987] have recently analyzed the effect of income and police contact measures on the proportion of income derived from illegitimate means. They look at a broader population, examining data from respondents to the 1980 wave of the National Longitudinal Survey Youth Cohort who admitted some contacts with police. While their results indicated

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that both police contacts and higher earnings were negatively associated with their outcome measure, the significance of their findings was sensitive to the exclusion of the roughly 15 percent of their sample members who reported the greatest number of police contacts.

Taken as a whole, these mixed results are troubling and offer little guidance as to the optimal mix of criminal justice and labor market measures to be used in reducing crime. In this paper, a variant of the economic model of crime is estimated from a sample of male arrestees in California. Their criminal records range from a single misdemeanor arrest to repeated felony convictions and imprisonments.[1]

The size of the sample and detailed information contained in the data allow one to examine a broader set of questions than previous authors could. In addition to measuring responses to increased certainty and severity of punishment, I estimate the incapacitative effect of prison, that is, of the amount of crime prevented during the time that the offender is isolated from society. Furthermore, I find evidence of a criminal human capital effect, a finding which has serious implications about the optimal mix of certainty versus severity of punishment as strategies to deter crime.

The model is estimated using a generalized Poisson regression model. In addition, a specification test for endogeneity of the regressors in such models is developed in section IV.

## II. THE DATA

The data analyzed in the next section are taken from a unique longitudinal data set established by merging official arrest records maintained by the California Department of Justice with official earnings records on file at the California Employment Development Department (EDD). Initially, men from two birth cohorts were sampled from the central arrest file, which contains complete adult arrest records of nearly all individuals whose first adult arrest in California occurred after 1972. This file contains extensive information on each arrest for a retainable offense[2], as well as information on prosecution and sentencing, and limited demographic items such as sex, race, and age. Arrests are matched to individuals on the basis of fingerprints, assuring the accuracy of the arrest records.

Earnings records for the period 1980 to 1986 are contained in EDD's central wage files, which are used to determine eligibility for unemployment insurance benefits. Earnings information is obtained from mandatory quarterly reports filed by employers covered by the unemployment insurance system; virtually all non-federal employers are required to file.

Random samples of men on the arrest file born in the years 1960 and 1962 were matched to their corresponding earnings records on the basis of Social Security number and last name. Procedures were used to assure the anonymity of all subjects, including third-party matching and the removal of all personal identifiers from the merged sample. About 60 percent of the original sample members were matched to earnings records. Of those

1. The broader representativeness of this sample compared to prison releasee samples is clear, particularly in light of a recent estimate by Tillman [1987] that roughly one-third of all men in California are arrested at least once between the ages of eighteen and thirty, for an offense of sufficient severity to have resulted in a jail term upon conviction. Furthermore, the inclusion criterion for this sample is similar to that for Phillips and Votey's [1987] "desister/persister" sample, except that whereas formal police contact was required for inclusion into the current sample, only informal contacts were required for inclusion in theirs.

2. Retainable offenses include all charges for which the offender could receive an incarcerative sentence if convicted. They therefore include such serious crimes as murder, rape, assault, burglary, and robbery, as well as lesser charges such as shoplifting, fraud, and receiving stolen goods. Traffic offenses and minor infractions such as vagrancy are not included.

who were not matched, roughly half lacked Social Security numbers, while earnings records were simply missing from the wage files for the rest. While the final merged sample members tended to have slightly more arrests on average than the original sample members, and there were proportionately fewer Hispanics, the departure from randomness does not seem too severe.

Forty-eight percent of the sample were arrested only once in the seven- to nine-year period between their eighteenth birthdays and the end of 1986, and 21 percent reported earnings well in excess of national per capita income. On the other hand, several individuals were arrested more than thirty times and were frequently incarcerated during that period.

The age-arrest profile of the sample, shown in Table I, is similar to others reported in the criminal justice literature, rising sharply during the teens and declining thereafter. Age- earnings profiles, also given in Table I, exhibit the same steep increase as is usually reported for this age group in the labor economics literature. These univariate relationships, then, would seem to indicate a role for labor market policies as a tool to reduce crime.

To generate the sample used in this analysis, all individuals were excluded for whom no arrest, court, or earnings activity was recorded in any of the years 1984 to 1986. This criterion, which resulted in the exclusion of 951 of the 14929 original sample members, is an admittedly coarse means to account for migration of the sample members, which is unobservable in the data. It should be noted, however, that the erroneous inclusion of individuals who had in fact left California (hence whose earnings and criminal activity are unobserved) would lead to a bias of the estimates reported below toward zero, weakening the reported results relative to the true magnitudes.

Table II presents summary statistics of the variables used in the study. The principal dependent variable, *NARR86*, is the number of times the individual was arrested in 1986. About 27 percent of the sample was arrested at least once in 1986. Conditional on arrest, the mean number of arrests was about 1.5. *NFARR86* and *NPARR86* give the number of felony and property crime arrests in 1986, respectively. Eighteen and 9 percent of the sample were charged with these types of crimes, respectively. Conditional means for both types of arrest were the same at 1.3.

As in all studies of crime, we are restricted to analyzing noisy indicators rather than a direct measure of criminal activity itself. Arrests, of course, are not a measure only of criminal activity, but also of the response of law enforcement agencies to prevent such behavior. In the discussion below, I will generally interpret the arrest measures as proxies for the individual's true level of criminal activity, although I do point out particular instances where alternative interpretations would lead one to draw differing substantive conclusions.

The variable *PCNV* is the proportion of adult arrests prior to 1986 which resulted in convictions, that is, the number of adult convictions divided by the number of adult arrests. This is the estimator of the individual's expected certainty of punishment. The next measure, *AVGSEN*, estimates the expected severity of punishments.[3] Two measures were initially examined, the average length of prison sentences served since age eighteen, in months, and the length of the most recent sentence. Since results were very similar, only equations containing the first measure are re-

3. In the criminology literature, these variables are said to provide measures of specific deterrence, that is, of the effect of a given individual's criminal justice history on his current behavior. An analysis of general deterrence, or the effect of community law enforcement efforts on individuals' behavior, is provided by Tauchen et al. [1988].

**TABLE I**
Arrests and Earnings, by Age

| Age | 18 | 19 | 20 | 21 | 22 | 23 | 24 | 25 | 26 |
|---|---|---|---|---|---|---|---|---|---|
| Arrests per 100 persons | 23 | 45 | 39 | 38 | 36 | 36 | 36 | 31 | 31 |
| Earnings per Capita (1980 dollars) | 2656 | 3694 | 4538 | 5147 | 5714 | 6458 | 7079 | 8235 | 8475 |

ported below. Six percent of the sample spent some time in prison, among whom the average sentence served was about thirteen months. The variable *PTIME86* gives the number of months the individual spent in prison during 1986 and will provide an estimate of the incapacitative effect of incarceration. *TOTTIME* measures the total amount of time the individual has spent in prison since age eighteen and can be considered an indicator of criminal human capital as discussed by Myers [1983] or Grogger [1989]. Among those with any prison time, the average time served was eighteen months.

The next three variables are measures of labor market opportunities. The first, *QEMP86*, is a measure of employment, giving the number of quarters in 1986 during which some positive earnings were reported. *INC86* is reported earnings for 1986, expressed in hundreds of 1980 dollars. It should be noted that only earnings from establishments which contribute to the unemployment insurance system are included. For the time period covered, this includes virtually all non-federal employers. Not included, however, are cash or any other under-the-table payments. *DURATION* is a measure of the duration of the current spell of unemployment. For all individuals who reported zero earnings in the first quarter of 1986, *DURATION* gives the number of quarters since the individual last had positive earnings or was released from prison, whichever was more recent.

## III. THE EMPIRICAL MODEL

I specify the conditional expectations of arrests as a (generally non-linear) function of the explanatory variables described above, as well as binary, mutually exclusive race- ethnicity indicators *BLACK* and *HISPANIC* and a binary variable *BORN60* indicating 1960 birth. The simplest models of criminal activity, such as those of Becker [1968] or Ehrlich [1973], would yield the intuitive predictions that conviction probabilities and expected sentences should enter the equation with negative signs, as should contemporaneous prison time for obvious reasons. Total prison time is expected to enter positively. Such simple theoretical models would also predict negative coefficients for the employment and earnings variables. Intuitively, one would expect the duration of the current jobless spell to enter the equation with positive sign, indicating the greater attractiveness of crime after longer periods of unemployment.

## IV. ESTIMATION

The non-negative integer nature of the dependent variable makes one of the so-called count regression methods the natural choice in estimating the above model, as discussed by Hausman, Hall, and Griliches [1984]. Based on the Poisson

**TABLE II**
Summary Statistics

| Variable | Mean | Variance | Standard Deviation | Min | Max |
|---|---|---|---|---|---|
| *NARR86* | 0.40 | 0.70 | | 0 | 13 |
| *NFARR86* | 0.23 | 0.34 | | 0 | 7 |
| *NNFARR86* | 0.16 | 0.25 | | 0 | 7 |
| *NPARR86* | 0.12 | 0.20 | | 0 | 8 |
| *NNPARR86* | 0.28 | 0.42 | | 0 | 9 |
| *PCNV* | 0.37 | | 0.40 | 0 | 1 |
| *AVGSEN* | 0.70 | | 3.61 | 0 | 59.2 |
| *TOTTIME* | 0.97 | | 5.07 | 0 | 77.4 |
| *PTIME86* | 0.33 | | 1.78 | 0 | 12 |
| *QEMP86* | 2.30 | | 1.63 | 0 | 4 |
| *INC86* | 55.62 | | 69.03 | 0 | 927.9 |
| *DURATION* | 2.25 | | 4.59 | 0 | 25 |
| *BLACK* | 0.16 | | | 0 | 1 |
| *HISPANIC* | 0.21 | | | 0 | 1 |
| *BORN60* | 0.37 | | | 0 | 1 |
| $n$ = 13978 | | | | | |

*NARR* = Number of 1986 arrests; *NFARR86* = number of 1986 felony arrests; *NNFARR86* = number of 1986 non-felony arrests; *NPARR86* = number of 1986 property arrests; *NNPARR86* = number of 1986 non-property arrests; *PCNV* = probability of conviction; *AVGSEN* = average prison sentence length; *TOTTIME* = total prison time; *PTIME86* = time in prison in 1986; *QEMP86* = quarters employed in 1986; *INC86* = 1986 income; *DURATION* = duration of current jobless spell; *BLACK* = 1 if black; *HISPANIC* = 1 if Hispanic; *BORN60* = 1 if born in 1960.

probability distribution, which is defined only over the set of non-negative integers, these regression models achieve for count variables what probit and logit do for binary data: they directly account for the important discreteness of the variable being modelled and restrict predicted values to lie in the permissible range.

The basic Poisson regression model can be written as

$$p(y_i) = \lambda_i^{\,y_i} e^{-\lambda_i} / y_i! \qquad y_i = 0,1,\ldots; \quad i = 1,\ldots,n,$$

where

$$\lambda_i = \exp(X_i\beta)$$

and $\beta$ is the vector of regression coefficients to be estimated. The regression function for the model is given as

$$\mathrm{E}(y_i) = \lambda_i = \mathrm{var}(y_i).$$

The exponential functional form for $\mathrm{E}(y_i)$ constrains predicted values to lie above zero, which is clearly a desirable property for a model of non-negative integers. The mean-variance equality imposed by the Poisson model has proven problematic in applied work, however, since most data analyzed exhibit overdispersion, or variance which exceeds the mean. Univariate statistics exhibited in Table II provide evidence of considerable overdispersion among the arrest measures to be analyzed here. While overdispersion does not affect the consistency of the Poisson estimator, it does cause the standard errors to be biased downward, inflating significance levels.

In these cases, the negative binomial model is often useful, since it is a model which generates overdispersion. It can be derived from the Poisson by assuming $\lambda_i$ itself to be a random variable distributed according to a gamma distribution. It is written as

$$p(y_i) = [\Gamma(y_i+1/\alpha)/\Gamma(y_i+1)\Gamma(1/\alpha)]$$

$$(\alpha\lambda_i)^{y_i}(1+\alpha\lambda_i)^{-(y_i+1/\alpha)}$$

$$y_i = 0,1,\ldots; \quad i = 1,\ldots,n$$

where $\lambda_i$ is as above, $\Gamma(\cdot)$ is a gamma function, and $\alpha > 0$ is a nuisance parameter estimated along with $\beta$. The regression function is the same as for the Poisson model, while the variance is given by

$$\mathrm{var}(y_i) = \lambda(1+\alpha\lambda_i)\,.$$

A specification test for the Poisson model can be obtained as a test of $H_0$: $\alpha = 0$. Rejection of the null hypothesis provides formal justification for the use of a model which permits overdispersion such as the negative binomial. Estimation is by the method of maximum likelihood and is simplified by the global concavity of both the Poisson and negative binomial log-likelihoods.

In spite of their obvious appeal for the problem at hand, one drawback of these count models is their inability to account for endogeneity of the regressors, which must be considered due to the inclusion in the model of contemporaneous values of employment and conventional income. I therefore devised a specification test based on Hausman's [1978] principle to test for the endogeneity of 1986 employment and earnings.

Assuming that the exponential functional form of the regression function is the correct specification, Gourieroux, Monfort, and Trognon [1984] have shown that its parameters can be consistently estimated under the null of exogenous employment and earnings by maximizing any (pseudo-) likelihood belonging to a linear exponential family. This category includes the normal, Poisson, and negative binomial probability models. Of course, if the model chosen is in fact the correct model, the estimator is efficient as well, achieving the Cramer-Rao lower bound. Given the non- negative integer and overdispersed nature of the data, the negative binomial (NB) model was chosen as the best candidate for the true model from among the linear exponential family.

In addition to an estimator which is consistent and efficient under the null hypothesis, Hausman's principle requires an estimator which is consistent under the alternative as well. Amemiya's [1985] nonlinear instrumental variables (NLIV) estimator meets this criterion. The instruments used included the second and third lagged values of employment and earnings, and interactions between the second lag of arrests and of employment and earnings. First lags were omitted due to apparent first-order autocorrelation in the data.

The test statistic is given by

$$H = (\hat{\beta}_{NLIV} - \hat{\beta}_{NB})' \; [\hat{V}_{NLIV} - \hat{V}_{NB}]^{-1} (\hat{\beta}_{NLIV} - \hat{\beta}_{NB})$$

where $\hat{V}_j$ denotes the estimated covariance matrix of $\hat{\beta}_j$. Only the coefficients of the 1986 employment and earnings variables were used in order to achieve the greatest

**TABLE III**
Estimates of Basic Model Parameters

| Variable | Negative Binomial Estimator | Non-Linear Instrumental Variables Estimator |
|---|---|---|
| Constant | -0.718 | -0.719 |
| | (-17.57) | (-1.70) |
| *PCNV* | -0.474 | -0.474 |
| | (-9.76) | (-4.84) |
| *AVGSEN* | -0.008 | -0.008 |
| | (-1.03) | (-1.16) |
| *PTIME86* | -0.067 | -0.067 |
| | (-5.76) | (-1.89) |
| *TOTIME* | 0.023 | 0.023 |
| | (4.18) | (4.97) |
| *QEMP86* | -0.001 | -0.003 |
| | (-0.03) | (-0.01) |
| *INC86* | -0.007 | -0.039 |
| | (-16.54) | (-1.26) |
| *DURATION* | 0.011 | 0.010 |
| | (3.08) | (0.48) |
| *BLACK* | 0.661 | 0.661 |
| | (16.55) | (8.99) |
| *HISPANIC* | 0.518 | 0.517 |
| | (13.31) | (6.03) |
| *BORN60* | -0.144 | -0.144 |
| | (-4.25) | (-2.16) |
| $\alpha$ | 0.966 | |
| | (20.70) | |
| Log L | -10985.03 | |

$n$ = 13978
H-statistic for exogeneity of *QEMP86, INC86* = 3.228.
Numbers in parentheses are asymptotic t-statistics.

*PCNV* = probability of conviction; *AVGSEN* = average prison sentence length; *TOTTIME* = total prison time; *PTIME86* = time in prison in 1986; *QEMP86* = quarters employed in 1986; *INC86* = 1986 income; *DURATION* = duration of current jobless spell; *BLACK* = 1 if black; *HISPANIC* = 1 if Hispanic; *BORN60* = 1 if born in 1960.

possible power. Under the null hypothesis, $H$ has an asymptotic $\chi^2_2$ distribution.

Table III presents the results of negative binomial and non- linear instrumental variables estimation and the Hausman test statistic for the exogeneity of employment and earnings. Note first that the coefficients of all other regressors are virtually identical across estimation method. The coefficients of the two suspect variables do vary somewhat, although by an amount insufficient to warrant rejection of the null hypothesis. The results discussed in the next section are therefore based on the negative binomial estimator. As a final note on the specification of the regression model, one observes that the size and significance of the nuisance parameter $\alpha$ strongly reject the simpler Poisson model in favor of the negative binomial.

## V. RESULTS

We turn now to a discussion of the basic estimation results presented in Table III. The coefficients of the negative binomial regression model can be interpreted as the average proportionate change in the dependent variable arising from a one-unit change in the corresponding explanatory variable, similar to the interpretation given coefficients in a log-wage equation. It remains, however, difficult to interpret the coefficient of the probability of conviction as the expected proportionate change in the number of arrests that would result if the probability of conviction were to change from zero to unity. However, we can use the information in Table I to calculate that the expected decrease in the number of arrests stemming from a one-standard deviation increase in the conviction rate is about 19 percent. In contrast, we see that an increase of one month to the expected prison sentence (insignificantly) reduces criminal activity, as proxied by arrests, by less than 1 percent. A one-standard deviation increase of over three months brings an expected reduction of only about 3 percent. This evidence suggests that increased certainty of punishment provides a much more effective deterrent than increased severity and calls into question the wisdom of relying on lengthier prison sentences as a means to decrease crime. A six percentage point increase in average conviction rates would deter as many arrests as a 3.6 month increase in average prison sentences. Given annual per-offender incarceration cost estimates of $15,000-30,000, it seems unlikely that imprisonment is the least-cost policy tool to achieve a given reduction in crime.

The coefficient of the contemporaneous prison time variable measures the incapacitative effect of prison. The point estimate indicates that each month in prison reduces the average individual's criminal activity by about 7 percent. This is slightly less than the 8.3 percent (= 1⁄12) reduction that would obtain if the effect of prison were to reduce arrests in exact proportion to the time spent isolated from society. The hypothesis that arrests are reduced in exact proportion to the time spent in prison cannot be rejected at any customary significance level, however.

The coefficient of the total prison time variable provides a measure of recidivism on the part of released prisoners. The estimated coefficient indicates that each additional month spent in prison increases average arrests by about 2 percent. The conclusions one draws from this result depend crucially on one's interpretation of the dependent variable. If arrests are a valid proxy for crimes committed, this effect may indicate an increase in criminal activity on the part of ex-prisoners, stemming either from greater criminal human capital or from negative labor market signals acquired while in prison. On the other hand, this result may simply be due to increased surveillance by police of individuals released from prison.

Under the former interpretation, the magnitude of this effect provides further impetus against reliance on a criminal justice policy based on lengthy but relatively infrequently imposed incarcerative sentences. Focusing only on the point estimates, note that increased average sentence length exerts a gross deterrent effect. However, the criminogenic effect of imprisonment is nearly three times as great as the deterrent effect. The net effect of a prison sentence apparently is to increase criminal activity on average once the offender is released into the general population.

The next three variables measure the responsiveness of my measure of criminal activity to employment and earnings. The point estimate of the coefficient of the employment variable is negative, but not at all significant. The income coefficient, however, is negative and highly significant, indicating that a $100 increase in earnings reduces arrests by just under 1 percent, on average. This effect is very large; a one-standard deviation rise in income is associated with an average decrease in arrests of 48 percent. The non-employment duration coefficient indicates that increasing the length of a current jobless spell by one calendar quarter increases arrests on average by about 2 percent. A one- standard deviation increase of 4.6 quarters results in an average increase of about 7 percent.

Finally, the demographic indicator variables indicate that blacks and Hispanics on average were arrested 66 and 52 percent more often than whites, respectively, and that members of the older cohort were arrested about 14 percent fewer times on average. These numbers therefore reflect race and age effects very commonly found in studies of crime.

These estimates provide evidence that both criminal justice sanctions and economic factors play a considerable role in determining the extent of an individual's criminal activity, at least as proxied by official arrest statistics. While these results are quite useful in resolving some of the conflicting evidence stemming from previous research, the size of the data set lets me examine these effects in yet more detail. In particular, it is of interest to determine whether different types of criminal activity are influenced differently by criminal justice and economic factors, and whether the responsiveness of criminal activity in general differs among different demographic groups. I therefore turn first to an analysis of felony and property arrests, followed by estimates of the general model disaggregated by race.

Table IV presents parameter estimates for models of felony arrests, non-felony arrests, and arrests for property and non-property crimes.[4] Consider first the felony and non-felony arrests. Comparing estimation results for these models with each other and with the estimates for all arrests presented in Table III, we see that increases in the probability of conviction seem to be more effective in deterring serious felony crimes than less serious crimes. Sanction severity, on the other hand, is about equally (in)effective in deterring both types of criminal activity. One also sees, by comparing the estimates of the coefficients of *PTIME*, that most of the crimes prevented by the incapacitative effects of prison are of the less serious non-felony type. The estimates of criminal human capital effects are roughly the same for both models.

The estimates of employment effects are quite different for these two types of crimes. An additional quarter of employment is associated with a 4 percent de-

4. Note that the conviction probability and average sentence variables are still computed from individuals' entire criminal history, rather than felony or property arrests, convictions, and sentences only. This was done for a number of reasons. First, data limitations preclude one from ascribing prison terms to the arrest or conviction from which they resulted. Next, when a felony conviction probability variable, constructed from felony arrests and felony convictions, was used in the felony arrest equations, qualitative results were similar to those shown, but the coefficient was less significant. This might indicate that felony conviction rates are an inefficient estimator of the individuals' expected probability of conviction, excluding important information known to the offender about the likelihood of conviction. When a property-crime conviction probability variable, constructed from property arrests and property convictions, was used in the equation for property arrests, it entered with a perverse positive sign. Again, this brings into question the validity of using only a part of the available information set in attempting to estimate individuals' expected conviction probabilities. Furthermore, mandatory prison terms for burglary were introduced in California in the early 1980s. This is likely to have led police to "overcharge" lesser offenses, and there is evidence that such mandatory sentencing schemes lead prosecutors to reduce conviction charges to avoid overly harsh sentences for many relatively minor crimes (see, e.g, Blumstein et al. [1983, 24-30]). In this case, restricting an estimator of conviction probabilities to property offenses may well lead to inconsistent and misleading results.

**TABLE IV**
Negative Binomial Estimates for Models of Felony and Property Arrests

| Variable | Felony Arrests | Non-Felony Arrests | Property Arrests | Non-Property Arrests |
|---|---|---|---|---|
| Constant | -1.194<br>(-23.39) | -1.695<br>(-26.71) | -1.88<br>(-24.68) | -1.110<br>(-23.11) |
| *PCNV* | -0.495<br>(-8.12) | -0.420<br>(-5.56) | -0.277<br>(-2.95) | -0.541<br>(-9.82) |
| *AVGSEN* | -0.009<br>(-1.01) | -0.008<br>(-0.60) | -0.023<br>(-1.51) | -0.003<br>(-0.30) |
| *PTIME86* | -0.038<br>(-2.85) | -0.134<br>(-5.35) | -0.025<br>(-1.36) | -0.092<br>(-5.55) |
| *TOTTIME* | 0.025<br>(3.94) | 0.020<br>(2.11) | 0.029<br>(3.10) | 0.022<br>(2.93) |
| *QEMP86* | -0.038<br>(-1.93) | 0.057<br>(2.41) | 0.072<br>(2.31) | -0.012<br>(-0.66) |
| *INC86* | -0.007<br>(-12.34) | -0.008<br>(-12.17) | -0.015<br>(-13.34) | -0.005<br>(-11.36) |
| *DURATION* | 0.015<br>(3.43) | 0.005<br>(0.91) | 0.010<br>(1.74) | 0.011<br>(2.78) |
| *BLACK* | 0.747<br>(15.18) | 0.508<br>(8.07) | 0.736<br>(10.19) | 0.620<br>(13.16) |
| *HISPANIC* | 0.321<br>(6.18) | 0.751<br>(13.15) | 0.220<br>(2.83) | 0.627<br>(14.29) |
| *BORN60* | -0.147<br>(-3.42) | -0.134<br>(-2.60) | -0.118<br>(-1.88) | -0.154<br>(-3.91) |
| $\alpha$ | 1.068<br>(13.50) | 1.867<br>(14.49) | 3.038<br>(13.83) | 1.013<br>(16.07) |
| Log | -7853.7 | -6250.4 | -5142.0 | -8891.4 |

$n$ = 13978
Numbers in parentheses are asymptotic t-statistics.

*PCNV* = probability of conviction; *AVGSEN* = average prison sentence length; *TOTTIME* = total prison time; *PTIME86* = time in prison in 1986; *QEMP86* = quarters employed in 1986; *INC86* = 1986 income; *DURATION* = duration of current jobless spell; *BLACK* = 1 if black; *HISPANIC* = 1 if Hispanic; *BORN60* = 1 if born in 1960.

crease in felonies, but a 6 percent increase in non-felonies. Both effects are at least marginally significant as well. This difference in employment effects explains the small and insignificant coefficient in the aggregate model; apparently, relatively minor criminal activity complements employment, while employment and serious crime are substitute activities. When these two crime types are pooled, one erroneously concludes that employment has no effect on criminal activity.

Income effects, in contrast, are roughly the same across crime types, while felony crimes are more sensitive to the duration of a spell of joblessness. The coefficients of *BLACK* and *HISPANIC* indicate that blacks are even more overrepresented in the serious crime category than is generally true, while more arrests of Hispanics are for non-felony offenses.

Turning now to the results for property and non-property crimes, we see that non-property arrests are much more responsive to higher conviction probabilities than property arrests, but that sanction severity matters much more for the property offenses than the non-property category. Incapacitative effects are larger for non-property offenses as well, while increases in criminal human capital lead to slightly greater increases in property offenses.

Again we see mixed results for the effect of employment. The results for property crimes indicate that property crimes and employment are complementary, as was seen for non-felony arrests above. Together, these findings suggest that relatively minor property crimes are undertaken to supplement income from conventional employment. Another possibility is that these positive relationships reflect employee theft from the workplace, although the data do not allow this hypothesis to be tested directly. Property arrests are seen to be roughly twice as responsive to changes in income as are arrests overall, and three times as responsive as non-property arrests. The coefficients of the demographic variables indicate that blacks are arrested much more often for property crimes than whites or Hispanics, and that Hispanics are much more likely to be arrested for a non-property crime than for a property offense. The age coefficients indicate a slightly slower "ageing-out" effect for property offenses than for other crimes.

The distribution of arrests is well known to differ widely by race; it is therefore of interest to examine whether the response of this measure of criminal activity to criminal justice and labor market factors varies according to race as well. Table V presents estimates for models of all arrests disaggregated by race. In general, the results for blacks accord most closely with the intuitive predictions of simple economic models of criminal activity, whites accord the least, and Hispanics lie in between.

The certainty of punishment measure is negative and significant across all three groups, but is strongest among whites, and weakest among Hispanics. In contrast, the severity of punishment measure is negative for both blacks and Hispanics, and significant for blacks, but is positive and marginally significant for whites. The incapacitative effects of prison are at best marginally significant among whites, but for blacks indicate a proportionate reduction in arrests in excess of the proportion of time excluded from the general population. The positive effect of past prison time on current arrests is strongest among blacks, and weakest among whites.

For whites and Hispanics, we again see evidence of complementarity between my measure of crime and conventional employment, although for Hispanics this effect is insignificant. Earnings effects are negative and significant for all groups, and slightly greater in absolute value among Hispanics than among whites and blacks. The effect of longer spells of joblessness is greatest among whites, but negative and marginally significant among Hispanics. Finally, negative age effects vary by race, being strongest among whites, and weakest among blacks.

## VI. CONCLUSIONS

This paper provides estimates of deterrent, incapacitative, and criminal human capital effects, as well as measures of the responsiveness of one indicator of criminal activity, arrests, to employment and earnings variables. The estimates were derived from a large sample of arrestees for whom matching earnings records could be located. Sample members' criminal histories range from slight to severe.

In contrast to several previous studies,

**TABLE V**
Negative Binomial Estimates of Model of All Arrests, by Race

| Variable | White | Black | Hispanic |
|---|---|---|---|
| Constant | -0.811<br>(-15.33) | 0.101<br>(1.42) | -0.159<br>(-1.82) |
| *PCNV* | -0.562<br>(-8.52) | -0.425<br>(-3.97) | -0.331<br>(-3.30) |
| *AVGSEN* | 0.017<br>(1.65) | -0.040<br>(-2.39) | -0.017<br>(-0.82) |
| *PTIME86* | -0.028<br>(-1.45) | -0.119<br>(-5.64) | -0.065<br>(-3.25) |
| *TOTTIME* | 0.015<br>(2.08) | 0.037<br>(3.54) | 0.025<br>(1.83) |
| *QEMP86* | 0.036<br>(1.60) | -0.114<br>(-3.41) | 0.026<br>(0.83) |
| *INC86* | -0.007<br>(-12.31) | -0.006<br>(-5.25) | -0.010<br>(-9.95) |
| *DURATION* | 0.022<br>(4.43) | 0.005<br>(0.79) | -0.016<br>(-1.69) |
| *BORN60* | -0.187<br>(-3.84) | -0.075<br>(-1.10) | -0.130<br>(-1.92) |
| $\alpha$ | 1.104<br>(14.16) | 0.789<br>(10.52) | 0.903<br>(9.80) |
| Log L | -5768.6 | -2468.5 | -2711.9 |
| $n$ | 8743 | 2271 | 2964 |

Numbers in parentheses are asymptotic t-statistics.

*PCNV* = probability of conviction; *AVGSEN* = average prison sentence length; *TOTTIME* = total prison time; *PTIME86* = time in prison in 1986; *QEMP86* = quarters employed in 1986; *INC86* = 1986 income; *DURATION* = duration of current jobless spell; *BLACK* = 1 if black; *HISPANIC* = 1 if Hispanic; *BORN60* = 1 if born in 1960.

I found consistent evidence indicating the importance of both criminal justice sanctions and labor market activity in determining the individual's level of criminal activity. The results point to large deterrent effects emanating from increased certainty of punishment, and much smaller, and generally insignificant effects, stemming from increased severity of sanction. Additionally, I found evidence of a sizeable criminogenic effect of imprisonment. These findings, I believe, call into question the economic rationality of a sanctioning strategy based on increasingly lengthy prison terms as a means of reducing crime.

Results on the effect of contemporaneous employment on the individual's criminal activities are mixed, but among blacks, at least, there is evidence that greater employment is associated with lower levels of arrest. Income is seen to have a strong negative effect, and longer spells of joblessness are also associated with increased criminal activity.

The results from this broad sample of arrestees contrast sharply with results from samples of prison releasees obtained in earlier research. This contrast, combined with the results presented here, offer some general suggestions for criminal justice policy. First, penal efforts should be directed toward less experienced, more deterrable offenders whenever possible, since the returns to deterrence appear to outweigh returns to incapacitation. Furthermore, among young offenders with little prior criminal history, non-incarcerative, employment- and earnings-enhancing sanctions may be effective means of reducing criminal activity. Programs such as intensive probation supervision and house arrest, with or without electronic monitoring to ensure compliance, might be expanded on an initially experimental basis to cover this type of offender, as discussed by Petersilia [1987]. Any sanctioning strategy which imposes its costs on the offender early in the criminal life cycle, and without damaging his prospects for employment in the future, is likely to reduce the costs to society of both the offender's future crimes and of future incarceration.

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