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DETERRENCE, WORK AND CRIME: REVISITING THE ISSUES WITH BIRTH COHORT DATA

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Deterrence, Work and Crime: Revisiting the Issues with Birth Cohort Data

ABSTRACT

In this paper we analyze the criminal behavior of a cohort sample of young men over an eight year period using random effects probit and Tobit techniques. Our major advances relate to our careful conceptualization of general deterrence, and our data. As far as we are aware, this work represents the first time that a richly specified model of criminal activity has been estimated using panel data for a general population group.

We find very robust evidence for a general deterrent effect emanating from police resources. Our results regarding general deterrence are open to fewer questions than previous findings. We also find that working and going to school significantly decrease the probability of committing criminal acts and by virtually identical amounts. This similarity of effect when coupled with other findings suggests that crime does not serve mainly as a direct source of income and that incentive effects emanating from higher wages are not very strong. There is little empirical support for the "crime as work" model that has dominated economic thought regarding crime over the last two decades. More fruitful models of work and crime may result if work is conceived as having its primary effects either through preferences or through information.

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I. Introduction

In this paper we consider the criminal behavior of a cohort sample of young men over an eight year period. We are primarily interested in two issues: (1) the general deterrent effects of criminal justice resources and (2) the interaction between employment, schooling, and criminal choices.

Our work extends and we believe improves upon existing work in a number of ways. The vast majority of work on crime by economists uses aggregate data on crime rates not individual data that reflects individual choices. During the 1980s, there have been a small but increasing number of studies that use individual data to estimate economic models of crime. While these studies have improved our understanding of the criminal choice, they suffer from a number of difficulties some of which we are able to address. First, existing studies generally use data for "high-risk" individuals such as prison releasees. We use data for a cohort sample that is quite representative of the general population of young males in large urban areas. As a result, we provide evidence concerning factors associated with ever participating in criminal activity as well as evidence concerning the behavior of criminals. Second, existing studies generally use cross sectional data. We use longitudinal data. The longitudinal nature of our data allows us to separate the "pure" age effect from cohort effects on criminality. Further, the panel aspect of the data allows us to obtain more efficient estimates of the effect of work, schooling and socio-demographic factors on crime. Finally, existing individual-level studies have either ignored the deterrence issue or created individual specific deterrence variables that are: (1) unlikely to represent independent variation in the probability or severity of sanctions and (2) unlikely to

be uncorrelated with the random element in the crime equation. We exploit the longitudinal nature of our data to obtain a deterrence variable that is both conceptually superior to those used in the past and more policy relevant.

To summarize briefly our most interesting results, we find very robust evidence for a general deterrence effect emanating from police resources. We also find that both working and going to school significantly decrease the probability of committing criminal acts. Further, our empirical results indicate that the effect of schooling and work are virtually identical. This similarity of effect when coupled with some of our other findings and some previous work suggests that crime may not have as its only, or possibly even its primary, motivation monetary gain. The traditional economic model which sees crime mainly as a substitute for work (i.e., an income source) may be too simplistic. Recent results suggest that the similarity of effects for working and being in school may occur because such activities simply keep young males occupied, or, we believe more plausibly, because participation in these activities are related to preference (e.g., a lesser inclination for crime to begin with or lowered attraction for illegal activity as a result of working or going to school) or informational effects. We find that members of our cohort become less likely to commit criminal acts as they age. This is a "pure" age effect since we are dealing with a single cohort. A somewhat surprising finding is that young men who attended parochial high schools are significantly less likely to commit offenses as adults, ceteris parabis. The literature suggest that this finding may either come from some benefit due to active participation in religious activities (e.g., morality, networking), or to

the fact that parochial schools provide a more structured and disciplined learning environment. Such an environment both serves to better educate boys and to keep them out of trouble. Our data do not allow us to disentangle the various possible effects of a parochial education. However, the result is quite interesting and is consistent with a number of findings that have appeared lately in labor economics.

The organization of the remainder of the paper is as follows. In the next section we review the literature with particular attention to empirical work that uses individual data to estimate crime models. In section three, we present the conceptualization which structures our empirical work and in section four we describe the data we use to estimate our model. Section five contains a discussion of the way in which we measure our theoretical constructs and the estimation techniques we use. In section six we describe our empirical results, and the final section contains our conclusions.

II. The Literature

It is now almost twenty years since Becker (1968) published a pioneering article that led economists to reconsider the factors causing individuals to commit criminal acts. Work by economist since Becker's article can be conveniently divided into two decades of activity. Since our work is primarily empirical, we focus on the empirical literature in our review.

The First Decade

Ehrlich (1973, 1974) and a number of other economists extended Becker's model.[1] As this work progressed, the economic model of crime

became firmly embedded in labor economics. Most of the economic models were time allocation models in which criminal activity was one possible time use. Criminal activity was generally represented as being similar to employment in that it requires time and produces income. For convenience, we refer to such models as "crime as work" models.

Much of the empirical work during the first decade was based on aggregate cross sectional or time series data usually obtained from the FBI's Uniform Crime Reports (UCRs).[2] Most of the studies posit a set of simultaneous equations for the crime rate and some measure of sanctions (e.g., the fraction of cases "solved" or mean prison time served). Identification of the crime equation is achieved by excluding sociodemographic variables (e.g., population density), police resource variables, or lags of the crime rate from the crime equation.

First generation theoretical and empirical work dwindled in the late 1970s. Extensions of the theoretical model (e.g., enriching the vector of criminal justice system actions) provided needed directions for the specification of empirical models but few testable hypotheses. The empirical work that was done during this decade was seriously questioned by a National Academy of Sciences' panel (Blumstein, et al., 1978) and others (e.g. Brier and Fienberg, 1980). The panel could find little, if any, justification for the exclusion restrictions used to identify the crime equation in the simultaneous equation models. While the panel did not reject the simultaneous equation approach, they concluded that the previous research based on such models 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, 1979, 1980).

The Second Decade

During the second decade, empirical work by economist used either time series methods and aggregate, generally UCR, data (e.g., Cook and Zarkin, 1985; Ehrlich and Brower, 1987; Hashimoto, 1987; Phillips and Ray, 1982; Yamada, 1985) or qualitative and limited dependent variable techniques and individual data (e.g., Good, Pirog-Good and Sickles, 1986; Montmarguette and Nerlove, 1985: Myers, 1983; Viscusi, 1986a, 1986b; Schmidt and Witte, 1984; Witte, 1980,). Since the latter work relates most closely to our own we will concentrate our review on this literature. For completeness, we will also discuss briefly some relevant work by sociologists (e.g., Rossi, Berk and Lenihan, 1980; Thornberry and Christenson, 1984) and psychologists (e.g., Farrington, et al., 1986; Gottfredson, 1985).

In the first half of the 1980s a number of economists used data for prison releasees to estimate single equation models of criminal activity. For example, see Witte (1980) and Myers (1983). This work treated criminal justice system actions and labor market variables as exogenous. Deterrence variables were created using information on each individual's previous experience with the criminal justice system. For example, in Witte's work the probability of conviction is proxied by the fraction of prior arrests that resulted in conviction. This representation of 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.[3] Further, if there is autocorrelation in criminal behavior, these deterrence variables are not exogenous regressors.

There are, in addition, two other limitations to work that uses cross

sectional data for prison releasees. First, such work cannot reveal how criminal justice system actions or opportunities affect the decision as to whether or not to participate in criminal activity. Second, cross sectional data cannot yield information about the dynamics of criminal activity or about the effects of variables that vary mainly through time and not in the cross section (e.g., macroeconomic conditions).

Recent work using individual data has attempted to overcome some of the difficulties outlined above. Montmarquette and Nerlove (1985) and Thornberry and Christenson (1984) use data for general population groups. Farrington, et al. (1986), Good, Pirog-Good, and Sickles (1986), Gottfredson (1985) and Thornberry and Christenson (1984) use data that contain observations for at least two time periods. Only Farrington, et al. (1986) and Thornberry and Christenson (1984) have panels that extend over a number of years (four years in each instance). However, these authors do not use panel data techniques to estimate their models. Further, Farrington, et al. estimate their individual Poisson model for only 36 of the 399 youths in their sample.[4] Methods of estimation vary widely with some studies failing to take account of the qualitative or limited nature of measures of criminal activity (e.g., Gottfredson ,1985; Rossi Berk and Lenihan, 1980; Thornberry and Christenson ,1984).

Most of the recent studies either include no general deterrence variables (e.g., Good, Pirog-Good, and Sickles, Rossi, Berk and Lenihan, 1980; Thornberry and Christenson, 1984) or use measures that depend on the individual's past criminal choices (e.g.,Myers, 1983; Schmidt and Witte, 1984; Viscusi, 1986a, 1986b; Witte, 1980). Only Montmarquette and Nerlove's study of self reports of drug use, petty theft, and shoplifting

by juveniles in Canada uses deterrent variables that do not depend directly on the individual's past criminal behavior. One of the advantages of their data set is that it contains information on the probability of arrest for each of the three offense categories. The arrest probabilities are, however, self reports of the perceived probabilities of arrest. Some recent work that incorporate economic and psychological theories of decision making (e.g.,Akerlof and Dickens, 1982, and Dickens, 1986) would suggest that an individual's perception of the probability of arrest is not determined independently from the criminal decision. It is not clear whether the perceptions are ex ante causal factors or ex post rationalizations for crime choices. None of the studies consider the potential endogeneity of criminal justice actions.

A few empirical studies have considered the criminal and labor market decisions jointly. These studies are similar to the aggregate data studies of the previous decade in that they use a simultaneous equations method to determine the causal link. However, they differ from these studies in that it is the link between crime and employment not deterrence that is of primary interest. As in the earlier work based on aggregate data, the primary difficulty with this approach is in achieving identification of the crime and employment equations since the one-period "crime as work" model implies that the same variables appear in both equations.

Good, Pirog-Good and Sickles (1986) Rossi, Berk, and Lenihan (1980) and Schmidt and Witte (1984) identify their crime equations by assuming that variables related to job skills or job search affect the labor market variable but not criminal activity. Thornberry and Christenson (1984) obtain identification in their four year panel data study by treating the

crime rate and employment status for each year as separate and uncorrelated variables.

Our work seeks to extend existing economic work on crime in a number of ways. First, unlike much of the recent work, we explicitly consider both of the issues that have been central to economic work on crime: deterrence and the nature of the relationship between work and crime. In considering these two issues, we have stepped back and attempted to advance the state of the art. To more effectively estimate deterrence effects, we exploit the panel nature of our data and the fact that the data contains information on criminal justice resources as well as the usual individual-level information. To provide better information on the way in which crime and work are related, we carefully choose our period of study (the young adult year, 18-25) and explicitly recognize the need to consider schooling as well as work. Second, the nature of our data allows broader generalizations than have been possible with previous work. We estimate our model using data for a sample that can be considered quite representative of young, US males in large urban areas. Finally, we estimate our model using a technique which specifically accounts for both the qualitative and limited nature of the dependent variable and the panel nature of the data.

III. Conceptualization

In this section we present a simple model of criminal behavior. The purpose of the model is to explain our empirical specification and to help in comparing our work with that of others.

We assume a von Neumann-Morgenstern decision maker who chooses the level of criminal activity to maximize expected utility. Consistent with

the empirical literature, we assume that the number and type of offenses can be aggregated to an index measure and denote the index level of criminal activity as c. Also in accordance with empirical findings, we assume that the probability of apprehension and the extent of any sanctions depend on the level of criminal activity. In addition, the probability of apprehension and level of sanctions may depend on individual characteristics related to the person's ability to elude arrest or to mitigate the punishment and on exogenous factors related to the criminal justice system (e.g., the availability of resources and the legal code). The probability of apprehension and the level of sanctions if apprehended are denoted $P(c,\alpha^1,\beta)$ and $S(c,\alpha^1,\beta)$ respectively where α^1 is a vector of individual characteristics and β is a vector of exogenous factors related to the criminal justice system.[5]

For notational simplicity, we ignore the possibility of multiple arrests and assume that in any time period the individual is either arrested once or not at all.[6] In either of these two states, an individual's well-being depends on the rewards to legal and criminal activities and the level of sanctions. The utility with total income I, offense level c, and sanctions s is

$$U(I,R(c),s;\alpha^0)$$

where R(c) is the returns with criminal activity c, and α^0 denotes a vector of exogenous variables systematically related to preferences. It is often assumed that the benefits from crime and the sanctions can be monetized; the individual's utility then depends only on disposable income, income from all sources including crime and net of fines or other monetized

penalties. We do not find this traditional assumption appropriate since many offenses yield no direct monetary gain (e.g., assault, drug use) and most punishments involve restrictions on personal freedom (e.g., probation, imprisonment) not a monetary fine. Under our model both rewards and punishments may be nonpecuniary.

The individual chooses the level of criminal activity to maximize expected utility given by

$$EU = P\ U(I,R(c),S(c,\alpha^1,\beta);\alpha^0) + (1-P)\ U(I,R(c),0;\alpha^0).$$

The optimal level of criminal activity, c*, and the expected number of arrests, $P(c^*, \alpha^1,\beta)\ c^*$, depend on total income from legal activities, the preferences of the individual, and any factors that might cause the form of the probability, sanctions or criminal returns functions to shift.

Note that in this model the probability of arrest is endogenous and depends explicitly on the nature of the criminal act. This means that the probability of arrest can not be treated as a simple explanatory variable as it has been in recent individual-level work examining the deterrence issue. Under the model, one could either estimate an equation for arrest simultaneously with the crime equation or enter exogenous arguments from the arrest function in the crime equation and estimate the resulting semi-reduced form. We take the latter approach in our empirical work because of the very considerable difficulties involved in the former.[7]

This model differs from most economic models in two major ways. First, the model represents the individual as choosing an index of criminal activity rather than the time to allocate to crime. Much of the work of the first (e.g., Ehrlich, 1974) and second decades (e.g., Flinn, 1986)

adopted the crime as work assumption which sees the criminal choice as a time allocation decision. In recent years, some studies have suggested alternatives. They point out that many crimes require relatively little time (Crowley, 1981) and that in many instances crime and work are combined (e.g., Holzman, 1982; Phillips and Votey, 1985).

As a practical matter, empirical work based on a crime as work model and the criminal activity model presented above are quite similar. Since information on actual time allocations, wage rates and wealth are not generally available, studies based on crime as work models have estimated equations like the one described above for self reported offenses or arrests. We have simply presented a model which is more consistent with empirical practice and recognizes the fact that much crime is not very time intensive.

The second way in which the model differs from most of the previous literature is by allowing the probability of arrest to depend on the level of criminal activity. Despite considerable evidence to the contrary, the empirical work on individual criminal choices treats the probability of arrest as exogenous. We alter this assumption and are careful to include only exogenous factors related to the probability of arrest in our empirical model.

IV. The Data

Our primary data is for a ten percent random sample of males born in 1945 and residing in Philadelphia between their 10th and 18th birthdays. The birth cohort was identified and information collected from school records, draft registration records and the Juvenile Aid Division of the Philadelphia Police Department beginning in 1964. See Wolfgang, Figlio and

Sellin (1972) for a detailed discussion of this phase of data collection.

Beginning in 1970, researchers attempted to interview the 975 individuals in the ten percent random sample. The subjects were 25 or 26 when interviewed and were asked for detailed information on their activities since leaving high school including schooling, work and criminal activities (self reports). Interviews were carried out with 567 individuals (62 percent of the individuals in the ten percent sample).[8] Researchers have carefully considered the issues of response bias for the interview data.[9] Thornberry and Christenson conclude that the bias is "not sufficient to distort correlational analysis or to alter appropriate conclusions concerning statistical significance" (1984, p. 401). They do warn however that blacks were substantially less likely to be interviewed than whites.

In 1975, researchers carried out complete searches of FBI records for offenses committed by the ten percent sample. We added detailed information on the neighborhood of residence at the time each individual left high school.[10] In addition, for the years of the panel, we recorded information on police resources ,crime rates, and real wages in manufacturing in Philadelphia. We also completely restructured the data set so that we could exploit the longitudinal nature of information.

Our panel traces sample members' activities from the time they were 18 or 19 in 1964 until they were 25 or 26 in 1971.[11] These eight years are the time period in which there is generally the greatest mingling of work and criminal activity. Young adults are still in the high crime years and, yet are old enough to have begun working or training for work. Further, 18 is both a legal and psychological watershed. Legally, in Philadelphia,

individuals become subject to adult statutes and are handled by the adult criminal justice system after the age of 18. Criminal records are public after the age of 18. Psychologically, by the age of 18 individuals are beginning to pursue lives independent of their families.

V. The Empirical Model

Before we estimate our model, we must develop empirical measures for our theoretical constructs and select an estimation technique. We discuss measurement issues first and then describe the method we use to estimate our model.

Our two most difficult measurement issues relate to criminal activity and deterrence. One must make two decisions when measuring criminal activity. One must first select the source from which to obtain information on criminal offenses and then decide on a method of aggregating various types of offenses. Our data set contains both official records and self reports of criminal activity. The relative merits of these two sources have been discussed at length in the literature (e.g., Hindelang, Hirschi and Weis, 1981 and Weis, 1986). In his recent survey, Weis concludes: "In general there is surprising consistency in the descriptions of most important correlates of crime based on official crime records and self-reports of criminal involvement" (1986, p.4). Further, Thornberry and Farnworth (1982) report a substantial level of concordance between self reports and official records for the Philadelphia data. We use official arrest records as our source of information on criminal activity because the arrest records in the Philadelphia data contain detailed information regarding the timing of offenses and the self report information does not.

Next, one must decide how to represent such a heterogeneous activity

as crime. We measure criminal activity in two ways. Our first measure is a binary variable which indicates whether or not the individual committed an offense during the given time period. This measure, which is the type of measure generally used in the literature, implicitly treats all offense as identical. Our second measure of criminal activity is an index which uses Sellin and Wolfgang's (1964) offense seriousness scale to aggregate offenses. The index reflects both the seriousness and frequency of arrests.

The second measurement issue relates to deterrence. Recall that under our model, the probability of arrest depends on the type of criminal activity undertaken, on individual characteristics related to the ability to avoid arrest and on exogenous factors related to the criminal justice system. Simply using a probability of arrest as an explanatory variable as has been done in the existing literature is clearly unacceptable. Most of the observed variation in the probability of arrest will result from differences in crime type and crime frequency not from exogenous differences in the probability of arrest.[12] Further, the probability of arrest will be correlated with random element in the crime equation.

Under our model, individual characteristics related to the ability to avoid arrest and changes in criminal justice resources and policies may cause independent variation (i.e., variation that does not depend on the type and extent of criminal activity) in the probability of arrest. From a policy point of view, the effect of changes in the probability of arrest caused by changes in the criminal justice system are of far more interest than changes caused by differences in individual elusiveness. Since there were no major changes in police policies during the study period,[13] we use

the police budget in real dollars per index offense as our primary measure of general deterrence.[14] As was recently pointed out to us, Block, et al. (1981) use a similar approach in their study of antitrust enforcement.

It is, of course, possible that there are shifts in the probability of arrest that flow from differences in individual elusiveness. Unfortunately, we don't know precisely how to reflect elusiveness in our empirical model. Further, most variables likely to reflect differing abilities to elude arrest (e.g., intelligence or like minded friends) are also likely to be related to differences in the individuals' criminal choices. To reflect this confounding of effects, we interpret the coefficients on variables likely to be related to elusiveness as reflecting some mixture of preference and deterrence effects.

We are not able to measure the returns from legal activities directly since there are no income variables in our data set. However, we do have information on the amount of 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 further reflect both returns to work and schooling, we incorporate factors generally correlated with income (i.e., IQ and a binary indicating whether or not the individual received a high school degree). Recall that our data set contains information on the average wage in manufacturing in Philadelphia for each year of the sample. We had hoped to use this as a further measure of the return to legitimate time uses. However, the years represented in our panel were a period of sustained growth in real wages. As a result, the variable representing the year of the panel and the variable for the real wage rate in manufacturing are highly correlated (a correlation coefficient of .97). With this

correlation it is not possible to separate the year and real wage effects. As can be seen in Table 1 where we list all variables used in our empirical analysis, we include only the year variable in our model. However, when interpreting the coefficient on this variable, we recognize that it may reflect both age and real wage effects.

The variables we see as related to preferences are of three types: (1) variables that reflect family and community background (i.e., a binary equal to one if both parents were born in the US, an index measure of the occupational status of the household head when the sample member was in high school[15], a binary equal to one if the individual attended a parochial school, the number of addresses during primary and secondary school years, average income in the neighborhood of residence during high school years, a binary equal to one if the high school neighborhood was predominantly Italian); (2) variables reflecting personal characteristics (i.e., IQ, a binary equal to one if the individual is white[16]); (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 contact as a juvenile, the percent of juvenile police contacts resulting in formal criminal justice system processing, a binary for gang membership, a binary equal to one if the individual is married).

We estimate our model of criminal activity with the binary and index measures of criminal offenses by random effects probit and Tobit techniques respectively.[17] The two-factor random effects models are extensions of the usual probit and Tobit models. In the two-factor random effects probit and Tobit models, as in the linear two-factor random effects models, the disturbance terms are correlated across time for any individual but not

across individuals. The component of the disturbance term that is correlated across time for any individual is designed to reflect unmeasured, persistent individual effects. If the random disturbance terms are uncorrelated with the explanatory variables, the parameter estimates of the random effects probit and Tobit models are consistent and asymptotically efficient.[18]

Before selecting the random effects model, we considered the possibility of using a fixed effects model with a separate intercept term for each individual. With a linear model, differencing the data makes it possible to estimate at least some parameters without having to estimate the separate intercept terms. However, this approach exploits the linearity of the model. With nonlinear models like the probit and Tobit, estimation of a fixed-effects model requires that the individual specific intercept terms be estimated. This leads to the classical incidental parameter problem, and means that the parameter estimates from a nonlinear fixed effects model will not be consistent.

VI. Empirical Results

Tables 1 and 2 contain the empirical results for the random effects probit and Tobit models respectively. The first column of results is for a specification including only variables that are unaffected by an individual's criminal or time allocation decisions (e.g., police resources, and family background). The second column contains the 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., faction of the year employed) or previous years, possibly during the sample

period (e.g., high school graduation). We estimate several models 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.

Note first that the probit and Tobit models are significant in all specifications and the estimated coefficients, when significant, are of the same sign in all specifications. The estimated coefficients on the variables of primary interest are stable in sign and magnitude across specifications for a given estimation technique. In particular, note that the estimated coefficients on the variable for police resources are negative and significant in all specifications. Further, the magnitudes of the coefficients on the police resource variable are not statistically different across specifications for either estimation technique. 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 as we discuss below. The individual effects component of the random disturbance is a substantial part of the overall variance of the random element in the Tobit models, and is significant in both the probit and Tobit models.

From an economist's perspective the results for the police resource variable may be of most interest. Our findings provide robust (across specifications and estimation technique) support the general deterrence hypothesis. Further, the deterrence results we obtain are open to fewer questions than are previous results that use aggregate or individual data. Unlike the aggregate studies, our work is not subject to the type of

simultaneity bias common in studies that use crime rates as a dependent variable and the clearance rate as the independent variable. Unlike previous work using individual data, we recognize that general deterrence flows only from variation in the probability of arrest that is not related to the type of criminal activity and include a policy relevant variable related to such variation.[19]

Recall that under our model it is total returns to legal activity that will affect the criminal choice. We seek to reflect total income by incorporating variables related to time allocation and to the wage rate. See Table 1 or 2. We find greater time working, greater time in school and higher IQ to be significantly associated with lower probabilities of criminal activity. However, we find that receipt of a high school diploma has no significant effect on offending. If we consider only the results for the time allocated to work and IQ, we could interpret our findings as consistent with our model. However, when we consider the results for the high school degree binary and the time allocated to school as well interpretation becomes more complex.

The coefficient on the proportion of the time working and the proportion of time at school are not significantly different from one another. Yet, under our model or a crime as work model, we would expect the effect of these two variables to be quite different. Under our model, we expect time at work to affect the level of criminal activity primarily because it increases income. Specifically, our results for time at work could be interpreted as indicating that crime is an inferior good. Schooling does not enter either our model or most crime as work models explicitly. However, under such static models, our results for time in

school could be interpreted as reflecting preference effects. For example, more time in school might be associated with lower probabilities of criminal acts because those who choose to attend school are less inclined to participate in criminal activities or possibly because attending school lowers perceptions regarding the desirability of crime. Dynamic models of criminal activity provide other possible interpretations for our schooling results. For example Flinn's model (1986) implies that time in school affects crime because it lead to higher future legitimate income streams through human capital effects and possibly lower discount rates through dynamic preference effects.

To throw further light on the effect of school on criminality, consider the fact that we find attending a parochial junior or senior high school to be significantly related to lower levels of criminal activity as an adult in all specifications of our model. In a recent study, Viscusi (1986a) finds church attendance to be associated with lower levels of reported participation in criminal activity. Also, Freeman (1986) finds that church attendance is a significant determinant of who escapes inner-city poverty. These results may relate to our findings since the families of many parochial school children are actively involved in religious activities. The findings regarding religion would be consistent with an effect for parochial schooling operating through preference although informational effects (e.g.,networking) are also possible.

There may, however, be a more direct link between the type of education received in parochial schools and future decisions regarding criminal behavior. Studies have shown that schools that do the best job of reducing the expected level of delinquency also do the best job of

improving educational achievement, maintaining good attendance and reducing misbehavior.[20] Parochial schools are more likely to have these characteristics than are public schools, particularly public schools in central cities such as Philadelphia. Given that we find no significant effect for high school graduation on crime, it appears that this interpretation too is consistent with the primary effect of schooling running through preferences or informational effects. In this case the preference or informational effects would, of course, have to be dynamic, and could run directly through education rather than through participation in religious activities.

However one interprets the results for schooling, it seems unlikely that time in school and time at work would have similar effects with such different causal processes at work. Yet similar effects have been found by a number of authors (e.g., Farrington, et al., 1986; Gottfredson, 1985; Viscusi, 1986a). The similarity of results suggests that the effect of working on criminal activity does not stem primarily from immediate income or incentive effects as assumed by our model and crime as work models since schooling has no such effects. The fact that we find consistently insignificant effects for high school graduation (assumably associated with a higher wages on average) provides further evidence suggesting that the direct income effects of working are relatively unimportant.

There are at least two possible explanations for the similar effects of working and being at school. First, and most consistent with existing models, the effect of time in school and time working may result simply from the individual's time constraint. If an individual allocates more time to school or work, there is simply less time to allocate to other

activities, e.g., crime and leisure. If there is only this time constraint effect and no income effect from working than the effect of time in school and time working would be quite similar. However, note that the policy implications of a simple time constraint effect are quite different than the implications of a crime as work model. It is not higher incomes and better jobs that will keep young men out of trouble, but rather its is keeping them busy doing something legal that is important. Keeping them in school will have the same effect as putting them to work. Viscusi (1986a) makes this point.

However, this explanation seems a bit simplistic to us and does not square well with existing work suggesting that crime is not a very time intensive activity and that it is often combined with work. We believe that a more plausible explanation for the similarity of effects has to do with the preference or informational effects. Work like schooling may either reflect individual preferences or shape them in ways that are not conducive to criminal activity. Alternatively, either working or being in school may provide information conducive to legal activities or not conducive to illegal ones.

The coefficient on the year variable is negative in all specifications of the model and significant in all but one specification (Model 2 of Table 2). Recall that year may be reflecting either a pure age effect or a real wage effect since real wages were increasing throughout our study period.[21] The fact that we find no significant effect for receipt of a high school degree leads us to believe that year has a significant effect on criminal activity primarily because of aging. Thus, like other studies we find that older adults are less likely to participate in crime, ceteris parabis.[22]

However, since we are using panel data we can unambiguously interpret our findings as reflecting a pure age rather than a cohort effect. Separation of the age and cohort effect on crime was not possible in cross sectional studies.

As noted earlier, the coefficients on race, socioeconomic status of parent's occupation, number of addresses during the high school years, average income of the family neighborhood, and ethnic background of the family neighborhood (i.e., the Italian binary) are not consistently significant across the specifications of the probit and Tobit models. When significant, the coefficients of each variable are always of the same sign and support previous empirical findings or common hypothesis about individual or family characteristics and participation in criminal activity. See Blumstein, et al. (1986) or Wilson and Herrnstein (1985). As might be expected, these variables are correlated and the reason for the changes in significance across specification is the collinearity that results from this correlation. By omitting one or more of these variables we could obtain what might appear to be much cleaner results. However, such results would be a somewhat misleading representation of the information in our data. Since these factors tend to occur in the same families, it is not clear which has the more important role in reducing criminal behavior.

The limited evidence we obtain that whites are arrested less often than other racial groups is consistent with the common finding that blacks have far higher crime participation rates than white (e.g., Blumstein, et al., 1986). In our sample, though, this finding could be partly attributable to the practices of the criminal justice system. A study that

uses the same data set we do (Collins, 1985) indicates that blacks were treated more harshly than whites by the Philadelphia criminal justice system during the 1960s and in particular that they were more likely to be arrested given whatever crime they committed. Since we are using arrest as our measure of criminal activity, our findings reflect these differences in police arrest practices as well as differences in criminal behavior across racial groups.

As in other studies that use official crime data, we find that the probability of committing a criminal act (as measured by arrest records) is higher for young men whose household head during high school had a relatively low status occupation.[23] In light of this finding, it may seem surprising that young men who grew up in higher income neighborhoods are more likely to be arrested in their early adult years. There is some question though about what an ecological variable such as average neighborhood income measures. A number of economic studies based on aggregate data include such a variable.[24] In these studies, researchers interpret average community income as measuring the "opportunities for crime" and expect, and, indeed, often find this variable to be positively related to the crime rate. It is possible that many of the young men in our sample continue to frequent the neighborhood they grew up in after graduating from high school. In this case, the average income variable may be capturing "criminal opportunities".

Our results for other family background variables (growing up in an Italian neighborhood and the number of addresses during the primary and secondary school years) are not generally significant. However, when they are significant, they have the expected sign, i.e., growing up in an

Italian neighborhood or moving around a great deal as a child is associated with higher levels of criminal activity. We find no evidence of any difference in the arrest rates between boys whose parents are and are not born in the U.S.

For the variables that are related to the boys' juvenile criminal record and enter only the second and third specifications, we find that boys who had more police contacts as juveniles, whose first arrest was for a serious crime against persons and who were gang members as juveniles show higher crime rates as young adults. There is a great deal of support in the literature for the finding that a more extensive and serious previous record is associated with higher crime rates. There has been less study of the effects of gang membership of criminality but our results do not contradict previous findings or hypotheses.[25] Our finding for gang membership and juvenile record is not inconsistent with our interpretation of schooling and work effects as largely due to preference or informational effects. Just as schooling and work lead to preferences that are not supportive of criminal activity and to information about legal endeavors so gang membership may lead to criminal preferences or to information about illegal activities. A more substantial and serious record can, of course, be seen as a revealing a strong preferences for crime, and certainly helps to establish a network of criminal contacts.

Our finding for gangs may also relate to lower probabilities of arrest and ,thus, may in part be a deterrent effect. Juvenile gang membership may allow acquisition of skills related to avoiding arrest.

The coefficients on the percent of juvenile police contacts resulting in formal criminal justice system processing are consistently negative but

never significant. Whatever specific deterrent effect may flow through this variable is certainly not very strong.

As is clear from the above discussion, the results for our probit and Tobit model are quite similar and provide convincing support for the robustness of the effects we uncover. However, we are better able to explain the binary measure of criminal activity than our index measure. There are fewer significant coefficients in the Tobit models and the magnitude of the "t-ratios" for significant coefficients declines in a number of instances.

Since the Tobit technique models both the probability of offending and the seriousness of offending for those who do commit criminal acts, it is clear that we are better able to explain whether or not someone commits an offense than the seriousness of any offenses. This is disappointing since we had anticipated that one of the strengths of our work would be our ability to treat offending as a heterogeneous activity. We do not know why we are unable to explain the seriousness of offending as well as the probability. It is possible that although our measure of seriousness is one of the oldest and most widely used in criminology, it is not good enough. A more likely explanation, though, is that police charge at arrest has a much larger random element than does arrest.

It is also possible that the Tobit model is not an appropriate functional form for estimating the seriousness of offending. This would be true, for example, if the factors determining the seriousness of offending and the probability were different. Schmidt and Witte (1988) have recently experimented with models that allow the probability and the timing of offenses to be affected by explanatory variables in different ways. It is

interesting that they were able to explain the probability of offending to a much greater degree than its timing.

VII. Conclusions

Our most comforting finding relates to the general deterrence effect of police resources. In contrast to previous studies with individual data, we are able to use a general deterrence measure that does not depend on an individual's past criminal choices and experiences with the criminal justice system. We find a negative effect of real police expenditures on the probability and seriousness of arrests. Further, the effect is significant and of the same magnitude across specifications of the model. It may be that the sensitivity of the deterrence findings in previous work results partly from the use of variables that reflect differences in prior criminal record rather than ceteris paribus difference in the probability of apprehension or severity of punishment. With deterrence variables that depend on an individual's past experiences, the effects of actual differences in deterrence factors are confounded with differences in the legal codes and enforcement for different types of crimes.

Some of our other findings are more unsettling and when combined with the results of other research suggest that it may be necessary to change the way we conceive of the relationship between work and crime. The crime as work model has dominated economic analysis of crime during the last two decades. However, empirical practice has often not been consistent with this model and empirical results have not been terribly supportive of it. In our conceptualization, we sought to present a model that was more consistent with empirical practice and which more carefully dealt with the deterrence issue. Our empirical results suggest that we have indeed dealt

more effectively with the deterrence issue. However, it appears that we have not succeeded in developing a model that effectively deals with the work and crime issue.

Empirical work has provided little consistent support for a significant relationship between wages, unemployment or income and crime.[26] It does not appear that crime serves mainly as a direct source of income nor does it appear that incentive effects emanating from higher wages are very strong. Time spent working does appear to lead to lower levels of criminal activity, but time spent working has effects that are virtually identical to time in school for both juveniles and young adults. The effect of working or going to school might occur either because of the time constraint, or because of preference or informational effects. However, substantial effects operating through the time constraint do not seem likely since crime is not in general a very time consuming activity, and unemployment does not appear to be significantly related to crime. Further support for the importance of preference or informational effects flows from findings indicating that participating in legally oriented activities (e.g., religious activities) serves to lessen criminal activity and participating in illegally oriented ones (i.e., juvenile gangs) serves to stimulate it.

If we are to understand the way in which work and crime affect one another we must move beyond simple models adopted from labor economics or consumer theory. There is mounting empirical support for models of crime that either explicitly consider preference formation or that incorporate imperfect information.

NOTES

1. For a survey of this literature see Heineke (1978) or Schmidt and Witte (1984).

2. See Blumstein, et al.(1978), Cook (1980), Long and Witte (1981), or Freeman (1983) for surveys.

3. While variables such as the ratio of past arrest to past offenses cannot be considered measures of general deterrence, they may be worthwhile as measures of specific deterrence. The interpretation of the coefficients is of course quite different with the variables used as measures of specific rather than general deterrence.

4. They estimate individual specific Poisson parameters for periods of employment and unemployment for the 36 individuals who committed at least one offense and who spent at least one fourth of the year employed and at least one fourth of the year unemployed.

5. See Tauchen, Witte, and Long (1985) or Blumstein, et al. (1983) for discussions of the general determinants of criminal justice system actions. For the cohort of our sample, Collins (1985) considers the factors affecting the actions of the Philadelphia criminal justice system and finds that the seriousness of offense and prior records were the two most important determinants. In addition, he finds that race had an effect early in the adjudication process and that blacks were more likely to be arrested than whites for similar offenses.

6. The notation with multiple arrests is messy and complicated. The implications of the model for structuring the empirical work are the same as for the above model.

7. Pursuit of the former approach would require information on differences in arrest probabilities for various criminal offenses. Data on the probability of arrest for only the crimes committed recently is not sufficient since there is considerable evidence that individuals involved in crime do not specialize. For a recent survey see Blumstein et al. (1986). Such data is not available to us.

8. Missing data for some variables and the fact that information for 1971 was available for only part of our sample resulted in our using information for only 246 individuals in our analysis.

9. For example, see Thornberry and Christenson (1984) or Wolfgang, Thornberry and Figlio (1985).

10. This information came from the City of Philadelphia Community Renewal Program (1963) and the 1960 Census.

11. One might question the relevance of using data for the 1964-1971 time period. However, in a number of ways Philadelphia of the 1960s and early 1970s was quite like the US as a whole today. The violent crime rate in Philadelphia during our study period was quite like the violent crime rate in the US today. Further, both employment structure and family structure in Philadelphia during the study period was quite reflective of current nationwide patterns.

12. See Poterba (1987) for an excellent discussion of this issue in another context.

13. We discussed this issue at some length with Neil Weiner of the Center for Studies in Criminology and Criminal Law, University of Pennsylvania.

14. We believe that this variable is a very good measure of real police resources per offense and, thus, the relative probability of arrest in Philadelphia during the 8 year period we are studying. As noted earlier, the policies of the police did not change markedly during the period. Also, it appear that reporting practices were quite similar throughout the period. Finally, the mix of offenses changed little during the years covered by our data. For instance, the ratio of serious person offenses to total offenses was 23 to 26 percent during the 8 years. We had hoped to include deterrence variables related to the court system as well as the police system. We were unable to do so because resource data were not available and there were no major changes in the criminal code or court practices.

15. The index was developed by Reiss (1963).

16. The three Hispanics in our sample were classified as nonwhite.'

17. The index measure of criminal activity during the year is censored at zero and has a substantial pile-up at the censoring point (approximately 70 percent of our sample had no arrests during the years of the panel).

18. See Chamberlain (1984) or Hsiao (1986) for surveys of the literature on panel data.

19. As a further check on the stability of the coefficient on the police resource variable, we also estimated a specification that included only the police resource and year variables. With this approach we avoid any potential bias from correlation between the time invariant individual effects component of the disturbance term and the included individual

characteristics. The results do not change; the coefficients on both the police and year variables are negative, significant, and not significantly different from the coefficients in the other specifications.

20. These insights are drawn from Wilson and Herrnstein (1985) who provide a good survey of the effect of schools on criminality.

21. The negative coefficient on the year variable does not reflect crime trend factors since crime rates in Philadelphia were increasing throughout the study period. However, it is possible that this variable is picking up other trend factors.

22. See Blumstein, et al. (1986) and Wilson and Herrnstein (1985) for recent surveys.

23. Studies using crime data from self reports do not find this relationship. One explanation for the difference in findings between studies that use official and self report crime data is that boys from lower socioeconomic groups do not commit more crimes than other boys but their crimes are more serious crimes and that they are more likely to be arrested than others. Our results for the Tobit estimation in which we use a crime variable that reflects both frequency and seriousness of the criminal activity supports this claim. The variable for the occupational status of the family head of household is more significant in the Tobit model than in the probit model.

24. See Long and Witte (1981) or Freeman (1983) for a review of these studies.

25. Blumstein et al. (1986) conclude that involvement with delinquent friends is a significant risk factor for participation in delinquency. As in our work, Viscussi (1986) finds that gang membership is associated with

higher crime rates. However, Good, Pirog-Good and Sickles (1986) find no significant relationship between participation in crime and gang membership.

26. See Long and Witte (1981) and Freeman (1983) for reviews of the literature.

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Table 1

Results for the Probability of Offending
(Asympototic "t-ratios" in parentheses)

INDEPENDENT VARIABLE	MODEL 1	MODEL 2	MODEL 3
General Deterrence			
Real Police Resources Per Index Offense	-0.7036** (-2.05)	-0.6762* (-1.93)	-0.7109** (-1.96)
Total Legal Income			
IQ	-0.9217** (-2.31)	-1.7845*** (-2.87)	-1.9232*** (-4.03)
Fraction of Years Individual Was Employed			-0.3166*** (-2.32)
Fraction of Years Individual Was in School			-0.4271** (-2.28)
A Binary Equal to 1 if Received a High School Degree			-4.3344*** (-0.39)
Age/Returns to Legal Activity			
Year	-3.0080** (-2.06)	-3.1459** (-2.09)	-3.7219** (-2.28)
Family Background			
A Binary Equal to 1 if Parents US Born	-2.2744 (0.16)	12.7996 (0.64)	1.6143 (0.09)
Occupational Status of Household Head During High School	-0.3811* (-1.69)	-0.2756 (-1.01)	-0.3073 (-1.25)
Number of Addresses During Primary & Secondary School	1.2283 (0.59)	4.4748* (1.73)	5.4010** (2.22)
A Binary Equal to 1 if Attended Parochial High Schools	-45.5937** (-3.22)	-38.0703** (-2.65)	-29.6339 (-1.96)
Average Income in Neighborhood During High School ($1000)	-0.0633 (0.11)	1.6523 (2.59)	1.7972*** (2.82)
A Binary Equal to 1 if High School Neighborhood Was Predominantly Italian	20.5390* (1.73)	16.9506 (1.47)	8.6699 (0.74)

Table 1 (Continued)

INDEPENDENT VARIABLE	MODEL 1	MODEL 2	MODEL 3
Personal Characteristics			
A Binary Equal to 1 if White	-6.7084 (-0.52)	-21.533* (-1.69)	-34.6118*** (-2.81)
Past Activities			
A Binary Equal to 1 if First Arrest was a Serious Personal Crime		103.8508*** (4.60)	154.0178*** (5.23)
A Binary Equal to 1 if First Arrest was a Less Serious Personal Crime		-0.4859 (-0.02)	23.3528 (1.38)
A Binary Equal to 1 if First Arrest was a Property Offense		26.1157 (1.40)	28.2282 (1.93)
Number of Times in Police Custody as a Juvenile		8.5939** (2.52)	7.8902*** (3.03)
Percent of Juvenile Police Contacts Resulting in Some Formal Criminal Justice Processing		-0.1764 (-0.89)	-0.1525 (-0.83)
A Binary Equal to 1 if A Gang Member as a Juvenile		58.5823*** (4.44)	44.8760*** (4.76)
A Binary Equal to 1 if Married			0.9840 (0.11)
Estimated Individual Effects	1.1035 (7.14)	1.1598 (5.23)	1.2709 (5.63)
Log Likelihood	-288.51	-256.71	-245.89
N	1968	1968	1968

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

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Table 1 (Continued)

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Log Likelihood	-288.51	-256.71	-245.89
N	1968	1968	1968

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

Table 2

Results for the Seriousness of Offenses
(Asympototic "t-ratios" in parentheses)

INDEPENDENT VARIABLE	MODEL 1	MODEL 2	MODEL 3
General Deterrence			
Real Police Resources Per Index Offense	-0.1002** (-2.11)	-0.1014* (-1.74)	-0.0948** (-1.97)
Total Legal Income			
IQ	-0.1220** (-2.27)	-0.2584* (-1.77)	-0.2312** (-3.21)
Fraction of Years Individual Was Employed			-0.0330* (-1.90)
Fraction of Years Individual Was in School			-0.0397 (-1.41)
A Binary Equal to 1 if Received a High School Degree			1.5621 (0.89)
Age/Returns to Legal Activity			
Year	-0.3824* (-1.91)	-0.3984 (-1.44)	-0.4419** (-2.38)
Family Background			
A Binary Equal to 1 if Parents US Born	-0.3849 (-0.19)	-1.2828 (0.34)	2.8885 (1.27)
Occupational Status of Household Head During High School	-0.0609* (-1.87)	-0.0532 (-0.91)	-0.1009** (-2.39)
Number of Addresses During Primary & Secondary School	0.0645 (0.23)	-0.0540 (0.09)	-0.1020 (-.30)
A Binary Equal to 1 if Attended Parochial High Schools	-6.2157*** (-3.22)	-8.7969** (-2.25)	-6.4113*** (-4.50)
Average Income in Neighborhood During High School ($1000)	0.0225 (0.32)	0.0009 (0.57)	0.0021*** (2.62)
A Binary Equal to 1 if High School Neighborhood Was Predominantly Italian	2.1949 (1.25)	-0.0154 (-0.001)	2.5862* (1.73)

Table 2 (Continued)

INDEPENDENT VARIABLE	MODEL 1	MODEL 2	MODEL 3
Personal Characteristics			
A Binary Equal to 1 if White	-1.7519 (-1.00)	-4.9498 (-1.48)	-4.7158*** (-2.93)
Past Activities			
A Binary Equal to 1 if First Arrest was a Serious Personal Crime		8.6143 (1.58)	6.9317*** (3.42)
A Binary Equal to 1 if First Arrest was a Less Serious Personal Crime		3.0182 (0.62)	-2.3693 (-0.87)
A Binary Equal to 1 if First Arrest was a Property Offense		-0.4452 (-0.14)	1.1417 (0.60)
Number of Times in Police Custody as a Juvenile		0.9127** (2.12)	2.1033*** (3.11)
Percent of Juvenile Police Contacts Resulting in Some Formal Criminal Justice Processing		-0.0449 (-1.19)	-0.0408 (-1.13)
A Binary Equal to 1 if A Gang Member as a Juvenile		4.3500** (2.23)	9.1428*** (5.81)
A Binary Equal to 1 if Married			-0.2360 (0.20)
Estimated Individual Effects	0.1553 (7.38)	0.2098 (3.11)	0.2211 (5.78)
Random Error	0.1482 (4.90)	0.1401 (2.51)	0.1400 (4.46)
Log Likelihood	-6.78	11.64	13.84
N	1968	1968	1968

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