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ESTIMATING THE ECONOMIC MODEL OF CRIME WITH PANEL DATA

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Abstract—Previous attempts at estimating the economic model of crime with aggregate data relied heavily on cross-section econometric techniques, and therefore do not control for unobserved heterogeneity. This is even true of studies which estimated simultaneous equations models. Using a new panel dataset of North Carolina counties, we exploit both single and simultaneous equations panel data estimators to address two sources of endogeneity: unobserved heterogeneity and conventional simultaneity. Our results suggest that both labor market and criminal justice strategies are important in deterring crime, but that the effectiveness of law enforcement incentives has been greatly overstated.

I. Introduction

More than two decades have passed since Becker published his seminal work on the economics of crime (Becker (1968)). Since then, a large empirical literature has developed around the estimation and testing of the economic model of crime. Almost all of the contributions to this literature have used aggregate data, usually at the state or national level. Ideally, the economic model of crime should be estimated with individual level data since the model purports to describe the behavior of individuals. However, the expense and difficulty of creating a random sample of the population large enough to include representative information about individual criminal activity has been, and continues to be, an obstacle to individual level analysis. The few exceptions in the literature that have used individual data are fundamentally recidivism studies.

In the absence of empirical work at the individual level, interest in tests of the economic model of crime with aggregate data continues (see Craig (1987), Avio (1988) and Trumbull (1989)). While estimation with aggregate data has been criticized, results from such estimation have influenced public policy. For example, the conclusion of Ehrlich (1975) that capital punishment has a strong deterrent effect found its way into the proceedings of the Supreme Court during its series of decisions in the 1970s concerning the constitutionality of capital punishment (see Blumstein et al. (1978)).

The consensus of the empirical literature is that a strong deterrent effect of punishment (certainty and severity) exists. This consensus is reflected in most Law and Economics textbooks.

> "Estimates of the magnitude of the deterrent effect vary, but it appears that an increase in law enforcement activity that increases either the probability of punishment or the severity of punishment by 1 percent is on the average associated with a reduction in the number of offenses some-

Received for publication May 3, 1991. Revision accepted for publication April 30, 1993.
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We wish to thank Steve Craig, Tom Orsagh and three anonymous referees for their helpful comments. Support from the Regional Research Institute at WVU is gratefully acknowledged. Expert research assistance was provided by Sudha Subrahmanyam.

where between 0.3 and 1.1 percent. Further empirical investigation is necessary in order to gain a more accurate estimate of the magnitude of this deterrent effect coefficient, *though the true value of the coefficient is probably closer to* 1 *than to* 0.3." (Hirsch (1988) p. 271, italics ours).

In this paper, we present empirical evidence that the ability of the criminal justice system to deter crime is much *weaker* than previous results indicate.

Our deterrent effects estimates are obtained from a new *panel* dataset in which the unit of observation is the county. Since our data are county level, we are able to achieve a relatively low level of aggregation. The availability of panel data allows us to control for unobservable county-specific characteristics that may be correlated with the criminal justice variables in the model. In general, failure to condition on these unobservables will result in inconsistent estimates of the coefficients of these variables. Previous empirical work using cross-section data neglect this type of "endogeneity." This is even true of studies that estimated simultaneous equations models. In these studies, researchers were focused on conventional sources of endogeneity (simultaneity), such as those arising from the dependency of the probability of arrest or the size of the police force on the crime rate.

We apply both single and simultaneous equations panel data estimators to the economic model of crime, thereby addressing both sources of endogeneity. This is the first contribution to the economics of crime literature to exploit panel data in this way.[1] The results of our empirical investigation indicate that unobserved county heterogeneity is statistically important in our sample. In every case where county effects are controlled for, we obtain estimated deterrent effects that are substantially *smaller* than those obtained when county heterogeneity is ignored.

II. Review of Previous Work

The results of some of the more prominent empirical contributions to the criminal deterrence literature using aggregate, cross-section data are summarized in table 1. For each study noted, table 1 indicates the estimation procedure, the crime on which the study was based, and the estimated elasticities of the probability of arrest (P_A), the probability of conviction (usually conditional on arrest) (P_C), the probability of imprisonment (usually conditional on conviction) (P_P), and the severity of punishment (S). About one-half of the reported regressions were estimated simply by ordinary least squares (OLS). The other half were estimated either by two or three stage least squares (2SLS or 3SLS), reflecting attempts at modelling simultaneity between the criminal justice variables, particularly P_A, and the crime rate.[2]

The economic model of crime predicts that the estimated coefficients of P_A, P_C, P_P, and S will be negative since an increase in the probability or severity of punishment increases the expected cost, or decreases the expected utility, of crime. Furthermore, under certain assumptions the economic model of crime implies an ordering of deterrent effects (excluding S); the greatest impact on crime coming from P_A, followed by P_C and P_P. The estimated elasticities reported in table 1 are generally consistent with the predictions of the theoretical model. In all cases the estimated elasticities are negative, and where more than one criminal justice variable is included, the results satisfy *a priori* expectations. Finally, note that the estimated arrest elasticities tend to confirm Hirsch's assertion, with several exceeding one in absolute value.

A fundamental flaw in each of the studies is an inability to control for unobserved heterogeneity in the unit of observation. The use of 2SLS and 3SLS in these studies does not treat this problem. Neglected heterogeneity also may be correlated with the instrumental variables used to compute the 2SLS and 3SLS estimates. With panel data we can account for unobservable county characteristics by conditioning on county effects in estimation. As a result, we are able to treat both sources of "endogeneity," conventional simultaneity *and* neglected heterogeneity.

As an example of how the other source of "endogeneity"—correlation between the explanatory variables and omitted county attributes—might arise, consider two *identical* jurisdictions or counties, except that the police in jurisdiction 1 record half the crimes reported to them and the police in jurisdiction 2 record all crimes reported. Jurisdiction 1 will appear to have a lower crime rate and higher probability of arrest than jurisdiction 2. If this pattern of under-reporting is repeated in the sample, then the estimated deterrent effect of raising the probability of arrest will be overstated. Nagin (1978) and others have suggested that differences in the rate at which police record the crimes reported to them can result in an estimated

[1] Wolpin (1980) and Craig and Heikkila (1989) also used panel data, but not for the purpose of determining the statistical consequences of ignoring unobserved heterogeneity.

[2] Not represented in table 1 are empirical studies using individual level data. While these studies typically are based on samples of prison releases, they avoid problems emanating from the endogeneity of law enforcement. Good examples of this individual level work are Witte (1980), Myers (1983) and Grogger (1991). Witte found evidence of criminal justice deterrent effects, but little evidence of labor market effects, while Myers' results support the opposite conclusion; Grogger found evidence of both.

TABLE 1.—SUMMARY OF PREVIOUS CROSS-SECTION RESULTS

Study (Data)	Estimation Procedure	Crime Type	P_A	P_C	P_P	S
Ehrlich (1973)	OLS	All, 1960			−0.526[a]	−0.585[a]
(U.S. states)	2SLS				−0.991[a]	−1.123[a]
Sjoquist (1973)	OLS	Robbery,	−0.342[a]			−0.212
(U.S. cities)		Burglary & Larceny				
Carr-Hill &	2SLS	All, 1961	−0.66[a]			−0.28[a]
Stern (1973)		All, 1966	−0.59[a]			−0.17[a]
(U.K. police districts)						
Orsagh (1973)	OLS	Felonies		−0.26[a]		
(CA counties)	2SLS			−1.8[a]		
Phillips &	OLS	Felonies	−0.622[a]			−0.347[a]
Votey (1975)	2SLS/3 eq		−0.610[a]			−0.342[a]
(CA counties)	2SLS/4 eq		−0.701[a]			−0.376[a]
Mathieson &	OLS	Robbery	−1.06[a]			
Passell (1976)		Murder	−0.743[a]			
(NYC precincts)	2SLS	Robbery	−2.95[a]			
		Murder	−1.96[a]			
Craig (1987)	3SLS	Felonies	−0.57[a]			
(Baltimore police beats)						
Trumbull (1989)	OLS	All	−0.217[a]	−0.451[a]	−0.325[a]	−0.149[a]
(NC counties)						

[a] Statistically significant at the 5% level.

deterrent effect that is simply an artifact of the (reported) data. By exploiting the longitudinal nature of our sample, we can capture jurisdictional differences in crime reporting without data on actual crimes.

III. Model and Alternative Estimators

The basic assumption of the economic model of crime is that expected utility maximizing individuals participate in the criminal sector in response to the benefits and costs of illegal activities (see Becker (1968), Ehrlich (1973), Block and Heineke (1975), and Schmidt and Witte (1984)). This suggests an individual's participation depends on the relative monetary return to illegal activities and the degree to which the criminal justice system is able to affect the probabilities of apprehension and punishment. Using panel data on the counties of North Carolina, we specify the following crime equation:

$$R_{it} = X_{it}'\beta + P_{it}'\gamma + \alpha_i + \epsilon_{it}, \quad i = 1, \ldots, N;\ t = 1, \ldots, T, \quad (1)$$

where R_{it} is the crime rate, X_{it}' contains variables which control for the relative return to legal opportunities, and P_{it}' contains a set of deterrent variables which proxy for P_A, P_C, P_P and S. The α_i are fixed effects which reflect unobservable county-specific characteristics that may be correlated with (X_{it}', P_{it}').[3] The ϵ_{it} are typical disturbance terms, assumed to be iid with a zero mean and constant variance σ_ϵ^2.[4]

Since we wish to contrast cross-section and panel data estimators for our model, we define the "between" and "within" transformations of (1):

$$R_{i.} = X_{i.}'\beta + P_{i.}'\gamma + \alpha_i + \epsilon_{i.} \quad (2)$$

and

$$\tilde{R}_{it} = \tilde{X}_{it}'\beta + \tilde{P}_{it}'\gamma + \tilde{\epsilon}_{it}. \quad (3)$$

In the former, the data are expressed in county means (for example, $R_{i.} = T^{-1}\Sigma_t R_{it}$), while in the latter the data are in deviations from means (so that $\tilde{R}_{it} = R_{it} - R_{i.}$). Note that (3) does not depend on the county effects.

Basing estimation on (2) leads to standard cross-section estimators which neglect unobserved county heterogeneity. Thus, if unobserved characteristics are correlated with (X_{it}', P_{it}'), such procedures will produce inconsistent estimates. This is true for OLS and simultaneous equations estimators. The problem with simultaneous equations estimators like 2SLS is that the α_i

[3] In estimation, we also include time effects to capture variations in the crime rate common to all counties. For convenience, we omit them from the formal presentation of our model.

[4] Following Bhargava, Franzini and Narendranathan (1982), we tested for serial correlation in the ϵ_{it}s. We could not reject the null hypothesis of no serial correlation.

also appear in the reduced form, rendering the instrument set invalid.

However, by using (3) as a basis for estimation, both sources of endogeneity may be addressed. First, if the only problem is correlation between (X_{it}', P_{it}') and unobserved heterogeneity, then consistent estimation is possible by simply performing least squares on (3). This produces the so-called within estimator, which can be viewed as an instrumental variables estimator with instruments (deviations from means) that are orthogonal to the effects by construction. Conventional simultaneity can be accounted for by using 2SLS to estimate (3), where all variables have been subjected to the within transformation (Cornwell, Schmidt and Wyhowski (1992)).

IV. Empirical Results

Empirical measures of our crime rate and deterrent variables are constructed from several sources. The crime rate, R, is the ratio of FBI index crimes to county population, both taken from the FBI's Uniform Crime Reports, county level arrest and offense data. The probability of arrest, P_A, is proxied by the ratio of arrests to offenses, again from the arrest and offense files. We assume there is a direct correlation between this ratio and individuals' perceptions of the probability of arrest. Similar assumptions are made concerning individuals' perceptions of the probabilities of conviction and prison. We proxy these probabilities, P_C and P_P, by the ratio of convictions to arrests and proportion of total convictions resulting in prison sentences, respectively. The number of convictions was taken from the prison and probation files of the North Carolina Department of Correction. Finally, sanction severity, S, is measured by the average prison sentence length in days.

The variables in X are intended to control for the relative return to legal activities, as well as other observable county characteristics that may be correlated with the crime rate. Opportunities in the legal sector are captured by the average weekly wage in the county by industry. The industry categories for which we observe wages are: construction (*WCON*); transportation, utilities and communications (*WTUC*); wholesale and retail trade (*WTRD*); finance, insurance and real estate (*WFIR*); services (*WSER*); manufacturing (*WMFG*); and federal, state and local government (*WFED*, *WSTA* and *WLOC*). The wage data were provided by the North Carolina Employment Security Commission. Participation in the legal sector may differ across urban and rural environments. These differences are accounted for by a dummy variable (*URBAN*) for counties that are included in SMSAs and have populations over 50,000, as well as population density (*DENSITY*), which is county population divided by county land area, the latter obtained from Census data. Regional or cultural factors that may affect the crime rate are controlled for through dummies for western and central counties (*WEST* and *CENTRAL*). Since crime rates tend to vary with county demographic characteristics, we include the proportion of county population that is male and between the ages of 15 and 24 (*PERCENT YOUNG MALE*), along with the proportion that is minority or nonwhite (*PERCENT MINORITY*). Both of these variables were constructed from Census data.

The number of police per capita (*POLICE*) is included in the control vector X as a measure of a county's ability to detect crime. Previous empirical work suggests that the greater the number of police, the greater the number of reported crimes. As we explain below, this result may be due to a dependency of the size of the police force on the crime rate. We obtained our measure of *POLICE* from the FBI's police agency employee counts.

Table 2 reports summary statistics for the variables used in our empirical model. The results from estimation are presented in table 3. In each case, we adopt a log-linear specification so that our estimated coefficients are interpretable as elasticities. First, consider the "between" estimates, which are calculated by applying OLS to (2). Focusing on the coefficients of the variables in P_{it}, their estimates tend to corroborate previous empirical work that has concentrated on cross-section estimation of the economic model of crime with aggregate data. With the exception of the estimated coefficient of P_P, the elements of $\hat{\gamma}$ have the correct (negative) signs. However, only the estimated coefficients of P_A and P_C are statistically significant. The estimated arrest and conviction elasticities are, respectively, -0.65 and -0.53.

The between estimator is consistent only if (X_{it}', P_{it}') is orthogonal to both α_i and ϵ_{it}. The within estimator is a simple solution to the violation of the orthogonality condition that (X_{it}', P_{it}') is uncorrelated with unobserved heterogeneity. The second column of table 3 provides the within coefficient estimates.[5] Again, focusing on the estimated deterrent effects, the difference in the within and between estimates is striking. Conditioning on the county effects causes the (absolute value of the) estimated deterrent elasticities associated with P_A and P_C to *decrease* by approximately 45%. The estimated coefficient of P_P has the correct sign and is statistically significant. In addition, the estimated deterrent effects are ordered according to the prediction of restricted versions of the economic model of crime. Finally, the within estimate of the deterrent effect of S is small and statistically insignificant, possibly reflecting

[5] Since the region and urban dummies and percentage minority variable do not vary over time in our sample, they are eliminated by the within transformation.

TABLE 2.—MEANS AND STANDARD DEVIATIONS ($N = 90$ and $T = 7$)

	Mean	Standard Deviation
CRIME RATE	0.0316	0.0181
P_A	0.309	0.171
P_C	0.689	1.690
P_P	0.426	0.087
S	8.955	2.658
POLICE	0.00192	0.00273
DENSITY	1.386	1.440
PERCENT YOUNG MALE	0.089	0.024
WCON	245.67	121.98
WTUC	406.10	266.51
WTRD	192.82	88.41
WFIR	272.06	55.78
WSER	224.67	104.87
WMFG	285.17	82.36
WFED	403.90	63.07
WSTA	296.91	53.43
WLOC	257.98	41.36
WEST	0.233	0.423
CENTRAL	0.378	0.485
URBAN	0.089	0.285
PERCENT MINORITY	0.257	0.169

the fact that North Carolina has a policy of determinate sentencing. An alternative interpretation is that increasing the severity of punishment is not a very effective means of deterring crime.

Given the dramatic differences in our within and between estimates, it is not surprising that the null hypothesis of no correlation between (X'_{it}, P'_{it}) is soundly rejected. A Wu-Hausman test of this null can be constructed around the within/between contrast. The value of the test-statistic, which is asymptotically distributed as χ^2_{16}, is 97.31. We conclude that heterogeneity is statistically important in our sample and reject estimators that do not condition on county effects.

Controlling for county effects in estimation addresses only one source of endogeneity. Conventional simultaneity may exist between R, P_A and *POLICE*. For example, while the standard Becker model predicts that the crime rate will fall as the probability of arrest rises, counties experiencing rising crime rates, holding police resources constant, would see probabilities of arrest fall. But, increases in crime may motivate a county to increase policing resources which, in turn, would increase the probability of arrest. Thus, we also allow for the possibility that P_A and *POLICE* may be correlated with ϵ.

To address simultaneity, as well as unobserved heterogeneity, we apply 2SLS to (3), the within-transformed model. Because both P_A and *POLICE* are treated as endogenous, identification requires at least two instruments. These instruments must be *exogenous* variables that are excluded from the crime equation, where exogenous means *uncorrelated with* ϵ *and the effects*. Hence, the instruments also will be expressed in terms of *deviations from means*. We use as instruments a mix of different offense types and per capita tax revenue. Offense mix is defined as the ratio of crimes involving "face-to-face" contact (such as robbery, assault and rape) to those that do not.[6]

The rationale for offense mix is as follows. Since arrest is facilitated by positive identification of the offender, P_A should be higher in counties with a higher relative incidence of "face-to-face" offenses. However, it is unlikely that the offense mix has much effect on the overall crime rate. Our use of per capita tax revenue is based on the argument that counties with residents who have greater preferences for law enforcement will express their preferences by voting for higher taxes to fund larger police forces. Such counties would have larger police forces for reasons not directly related to the crime rate. Our sample provides little evidence to reject our instrument set. When offense mix and per capita total revenues are included in (3), they do not add to the predictive power of the model. An F-test of the null hypothesis that their joint effect is zero leads to a test-statistic with a value of just 0.053.

The fixed effects 2SLS estimates are reported in the third column of table 3. Treating both sources of endogeneity yields estimated deterrent effects that are no longer statistically significant, although the point estimates are closer to the within than the between estimates. By comparison, high (especially manufacturing) wages appear to be very effective in deterring

[6] Offense mix was suggested by an anonymous referee. Our use of per capita tax revenue also is based on this referee's comments.

TABLE 3.—RESULTS FROM ESTIMATION
(standard errors in parentheses)

	Between	Within	2SLS (fixed effects)	2SLS (no fixed effects)
CONSTANT	−2.097			−3.719
	(2.822)			(8.189)
P_A	−0.648	−0.355	−0.455	−0.507
	(0.088)	(0.032)	(0.618)	(0.251)
P_C	−0.528	−0.282	−0.336	−0.530
	(0.067)	(0.021)	(0.371)	(0.110)
P_P	0.297	−0.173	−0.196	0.200
	(0.231)	(0.032)	(0.200)	(0.343)
S	−0.236	−0.00245	−0.0298	−0.218
	(0.174)	(0.02612)	(0.0300)	(0.185)
POLICE	0.364	0.413	0.504	0.419
	(0.060)	(0.027)	(0.617)	(0.218)
DENSITY	0.168	0.414	0.291	0.226
	(0.077)	(0.283)	(0.785)	(0.103)
PERCENT YOUNG MALE	−0.0951	0.627	0.888	−0.145
	(0.1576)	(0.364)	(0.139)	(0.336)
WCON	0.195	−0.0378	−0.0358	0.329
	(0.210)	(0.0391)	(0.0467)	(0.279)
WTUC	−0.196	0.0455	0.0398	−0.197
	(0.170)	(0.0190)	(0.0282)	(0.197)
WTRD	0.129	−0.0205	−0.0196	0.0293
	(0.278)	(0.0405)	(0.0426)	(0.3240)
WFIR	0.113	−0.00390	−0.00700	0.0506
	(0.220)	(0.02806)	(0.03270)	(0.3224)
WSER	−0.106	0.00888	0.00600	−0.127
	(0.163)	(0.01913)	(0.02536)	(0.176)
WMFG	−0.0249	−0.360	−0.406	−0.0493
	(0.1339)	(0.112)	(0.217)	(0.1672)
WFED	0.156	−0.309	−0.273	0.170
	(0.287)	(0.176)	(0.296)	(0.327)
WSTA	−0.284	0.0529	−0.0129	−0.181
	(0.256)	(0.114)	(0.2599)	(0.300)
WLOC	0.0103	0.182	0.136	0.0237
	(0.4635)	(0.118)	(0.165)	(0.5187)
WEST	−0.229			−0.198
	(0.108)			(0.117)
CENTRAL	−0.164			−0.173
	(0.064)			(0.067)
URBAN	−0.0346			−0.0874
	(0.1324)			(0.1508)
PERCENT MINORITY	0.148			0.174
	(0.049)			(0.057)
s.e.	0.216	0.137	0.141	0.224

crime. In both fixed effects 2SLS and within regressions, the estimated coefficient of *WMFG* is statistically significant and at least as large in absolute value as any of the deterrent variables' coefficient estimates. The other variable revealed to influence the crime rate statistically significantly is *PERCENT YOUNG MALE*, whose estimated coefficient is 0.888. The large, positive effect of *PERCENT YOUNG MALE* is consistent with the fact that young males commit most of the crime. Interestingly, the effects of *WMFG* and *PERCENT YOUNG MALE* are *not* statistically significant in regressions that do not account for unobserved heterogeneity.

One interpretation of our fixed effects 2SLS estimates is that the efficacy of labor market solutions to the problem of crime exceeds that of traditional criminal justice strategies (along the lines of Myers (1983)). However, a Wu-Hausman test of the contrast between the within and fixed effects 2SLS estimates cannot reject the null hypothesis that P_A and *POLICE* are uncorrelated with ϵ.[7] Therefore, on efficiency grounds we prefer the within estimates, and conclude that both labor market and law enforcement incentives matter (consistent with Grogger (1991)).

Although estimators that ignore unobserved heterogeneity are inconsistent, it is instructive to contrast our fixed effects 2SLS estimates with those obtained from

[7] The value of the test-statistic, which is asymptotically distributed as χ^2_2, is 0.031.

applying 2SLS to (2). The latter are presented in the last column of table 3, and are directly analogous to the 2SLS and 3SLS estimates listed in table 1. With the exception of P_A, the estimated deterrent effects are very similar to those produced by the between estimator. However, the difference in the between and conventional 2SLS estimates of the P_A coefficient may have little to do with simultaneity. Since the *county means* of the instruments are used in the cross-section application of 2SLS, they may be capturing some of the dependence of P_A on the effects. As the within results demonstrate clearly, controlling for heterogeneity in estimation serves to reduce substantially the estimated deterrent effect of P_A. In any case, the P_A coefficient estimate is still greater than 0.50 in absolute value. We conclude that the statistical consequences of neglecting unobserved heterogeneity in our sample are serious whether single or simultaneous equations estimators are used.

V. Conclusions

Previous attempts at estimating the economic model of crime with aggregate data relied heavily on standard cross-section econometric techniques. We show that the results of these attempts are suspect since standard estimation procedures cannot control for unobserved heterogeneity. This is even true of studies that estimated simultaneous equations models to account for dependencies between the probability of arrest and the size of the police force and the crime rate.

Using a new panel dataset of North Carolina counties, we exploit both single and simultaneous equations panel data estimators to address both sources of endogeneity: unobserved heterogeneity and conventional simultaneity. In general, our results lead us to conclude that both labor market and criminal justice strategies are important in deterring crime, but that the effectiveness of law enforcement incentives has been greatly overstated. Specifically, we find the deterrent effects of arrest and conviction probabilities to be much smaller than those obtained from cross-section estimation. Neglecting county heterogeneity biases upward deterrent effects estimates. Given the statistical consequences of unobserved heterogeneity, future estimation of the economic model of crime with aggregate data should no longer disregard this important source of specification error.

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[Footnotes]

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[2] **Estimating the Economic Model of Crime with Individual Data**
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