Aggregation Bias in the Economic Model of Crime

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Abstract

This paper uses county-level panel data to test the appropriateness of the 'one size fits all' reduced-form regression approach commonly used when estimating the economic model of crime. Empirical results provide initial evidence that previous studies, which restrict deterrent effects to have identical impacts across crime types, may be presenting statistically biased results.

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1. Introduction

Since Becker's (1968) introduction of the theory of crime and punishment, a plethora of empirical tests have emerged in the literature. In a significant contribution, Cornwell and Trumbull (1994) illustrated that much of the existing empirical evidence contains biased and inconsistent parameter estimates from failing to properly control for unobserved heterogeneity. Cornwell and Trumbull (hereafter CT) addressed this issue by employing panel data models to control for heterogeneity in the unit of observation. As in previous studies, CT measured criminal activity with an overall crime index that aggregates across individual crime types. The purpose of this paper is to extend CT's study by examining the effects of measuring criminal activity with an aggregated index. We conjecture that it is inappropriate to pool crime types into a single decision model and that much of the existing empirical evidence suffers from aggregation bias. Our empirical results verify our premise, as parameter estimates from a disaggregated model differ significantly from the aggregate model. From a policy perspective our results are important as we show deterrence effects of various variables, such as probability of arrest, are quite heterogeneous across crime types.

2. Aggregation bias

The economic model of crime attempts to explain criminal activity. Empirical tests of the theory therefore require a measure of criminal activity as the dependent variable. Many previous studies use the crime index as an overall crime rate to measure the level of criminal activity. The index measure generally includes seven of the eight index crimes — murder, rape, robbery, assault, burglary, larceny and auto theft, with arson typically omitted from the analysis. The FBI separates the index measure into two broad crime types: violent (murder, rape, robbery and assault) and property (burglary, larceny, auto theft and arson). Further disaggregation yields a crime rate for each of the eight crime types. While previous studies have examined the deterrence hypothesis for individual crime types, to the authors' best knowledge the impacts of disaggregation have yet to be explored.[1]

Because sanctions vary systematically across crime types, estimated deterrent effects could differ substantially across offenses. Some offenses primarily call for incarceration as the sanction (murder and robbery), whereas others generally receive no confinement as punishment (burglary and larceny). Similarly, the probability of arrest also varies considerably across crime types, as clearance rates are much greater for violent crimes (0.78) than property crimes (0.22). Given that certainty measures for each crime type are constructed with data reported by jurisdictions, our contribution is parallel to CT's seminal paper, which addressed jurisdictional heterogeneity. But instead of controlling for only jurisdictional heterogeneity, we also allow parameter heterogeneity across crime types.

To empirically test the effects of aggregation, we follow CT and model the crime rate as a function of legal opportunities and a set of deterrent variables:

$$C_{it} = \alpha_i + \beta X_{it} + \varphi_t + \varepsilon_{it} \qquad i = 1, 2, \ldots, 90; t = 1, 2, \ldots, 7 \tag{1}$$

where C_{it} represents the natural logarithm of the crime rate in county i at time t, β is the unknown vector of K time-invariant slope coefficients, X_{it} is the vector of K exogenous factors for county i at time t; α_i are fixed effects which control for unobserved county factors that affect crime rates; φ_t are time effects that capture any relevant factors that are equivalent across North Carolina counties, such as macroeconomic effects, and ε_{it} is the contemporaneous error term.[2] Table 1 contains descriptive statistics for all variables. We should note that the regressors in X are identical to those used in CT, with one exception. Whereas CT aggregate clearance rates (probability of arrest) in an index form, we have disaggregated data on clearance rates for each crime type. For comparability purposes, we restrict the present analysis to include the same time period used by CT, 1981–88.[3]

A few noteworthy aspects of Eq. (1) warrant further discussion. First, we estimate Eq. (1) separately for both of the FBI major crime groups — violent and property; and also estimate the model individually for murder, rape, robbery, assault, burglary, larceny, and motor vehicle. This procedure enables us to compare our estimates from each model type with CT's parameter estimates.

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[1] Some studies that have estimated crime equations using disaggregated data include Swimmer (1974), Mathur (1978), Avio and Clark (1978), and Sjoquist (1973), amongst others.

[2] Following CT, we also use the natural logarithm of the variables in the X vector. We should note that some of the county-level crime rates are equal to 0. We added one to each crime rate before taking the logarithm to account for this nuance.

[3] CT generously provided their data for estimation purposes.

Table 1
Descriptive statistics

Variable	Mean	S.D.
Crime rate		
Index	0.0316	0.0181
Violent	0.0008	0.0017
Property	0.0090	0.0090
Murder	0.0001	0.0001
Rape	0.0002	0.0001
Burglary	0.0004	0.0005
Assault	0.0003	0.0003
Robbery	0.0093	0.0050
Larceny	0.0170	0.0110
Auto theft	0.0010	0.0009
Probability of arrest (P_a)		
Index	0.309	0.17
Violent	0.781	0.65
Property	0.221	0.77
Murder	0.987	0.45
Rape	0.741	0.99
Robbery	0.607	0.44
Assault	0.839	0.53
Burglary	0.226	0.36
Larceny	0.211	0.85
Auto theft	0.228	0.96
Probability of conviction (P_c)	0.689	1.690
Probability of imprisonment (P_p)	0.426	0.087
Sentence length (S)	8.955	2.658
Police	0.00192	0.00273
Density	1.386	1.440
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

Second, to estimate the violent and property crime models, we stack the crime data and estimate a seemingly unrelated regression (SUR) model assuming the response coefficients are homogenous within each of the two major crime types. Note that we do not assume the fixed/random effects are equivalent across crime types. Hence, we allow α_i to vary across crime types within each regression model. This allows a more direct test of the relationship between each different crime rate and the regressors. Third, a general concern in estimation of Eq. (1) is whether the effects α_i should be considered fixed or random. If they are assumed fixed, Eq. (1) is the within estimator and if they are assumed random, the random effects model, which accounts for between and within variation, results. The important consideration is whether the effects and the regressors are nonorthogonal. If they are, the random effects model returns inconsistent estimates. Alternatively, if the effects are orthogonal to the regressors, the random effects model should be used since the intercounty variation in crime rates is taken into account and, therefore, coefficient estimates are more efficient than the alternative within the model. Eq. (1) suggests a fixed effects formulation, in that the effects are most likely correlated with the deterrence variables. Indeed, for all regressions, a Hausman (1978) test rejects the random effects formulation in favor of the fixed effects model. Therefore, we direct our discussion of aggregation bias using the within estimates.[4]

3. Empirical results

Empirical results in Table 2 include CT's original within estimates in column 1 and our fixed effects estimates, which decrease in the level of aggregation from left to right. In columns 2 and 3 we aggregate crime types into violent and property crime groups and estimate Eq. (1) for the pooled data.

Table 2
Estimated crime functions of aggregate and individual crime measures with time and jurisdiction effects

	Index	Violent	Property	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto
P_a	−0.355	−0.284	−0.413	−0.327	−0.340	−0.167	−0.421	−0.557	−0.527	−0.313
	(0.032)	(0.022)	(0.016)	(0.054)	(0.045)	(0.038)	(0.043)	(0.031)	(0.029)	(0.026)
P_c	−0.282	−0.194	−0.214	−0.028	−0.111	−0.131	−0.546	−0.265	−0.249	−0.169
	(0.021)	(0.026)	(0.019)	(0.051)	(0.050)	(0.050)	(0.051)	(0.030)	(0.029)	(0.040)
P_p	−0.173	−0.115	−0.085	−0.100	−0.186	0.051	−0.229	−0.240	−0.132	0.044
	(0.032)	(0.048)	(0.036)	(0.094)	(0.093)	(0.093)	(0.092)	(0.054)	(0.054)	(0.074)
S	−0.00245	0.104	−0.007	0.119	0.119	0.096	0.0807	−0.036	0.016	0.001
	(0.0231)	(0.041)	(0.031)	(0.080)	(0.079)	(0.079)	(0.078)	(0.046)	(0.045)	(0.062)
Police	0.413	−0.200	−0.367	−0.157	−0.230	−0.281	−0.119	−0.393	−0.395	−0.250
	(0.027)	(0.039)	(0.030)	(0.077)	(0.076)	(0.076)	(0.075)	(0.045)	(0.044)	(0.060)
Density	0.414	−0.090	0.166	−0.410	−1.225	1.502	−0.334	0.412	0.300	−0.388
	(0.283)	(0.439)	(0.330)	(0.862)	(0.847)	(0.850)	(0.840)	(0.491)	(0.485)	(0.671)
Young	0.627	1.081	1.433	−0.662	3.621	−0.198	0.994	0.305	1.813	2.053
Male	(0.364)	(0.566)	(0.428)	(1.108)	(1.093)	(1.097)	(1.089)	(0.636)	(0.626)	(0.875)
N		2520	1890	630	630	630	630	630	630	630
F		64.61	150.35	4.05	11.08	23.46	23.06	55.01	74.85	28.27
(P-value)		(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
$\bar{R}^2$		0.906	0.959	0.352	0.642	0.800	0.797	0.906	0.929	0.829
ln L		−1215.65	−98.06	−279.67	−271.12	−273.09	−265.25	71.62	80.38	−124.15

Standard errors in parentheses unless otherwise noted; both jurisdiction and time effects were controlled for in the estimation leading to a two-way effects specification; wage variables provided in Table 1 were included in each of the models. Coefficient estimates are available upon request.

[4] Given that CT conclude that a model more intricate than a within estimator is unnecessary, we do not estimate models that account for simultaneity.

The results illustrate the impact of aggregation with some intuitively appealing parameter estimate variations across the disaggregated models. Specifically, the estimated deterrent effect of probability of arrest, P_a, is 45% greater for property crimes versus violent crimes, a difference that is significantly different from zero at the $P<0.05$ level. This differential is even more pronounced in the disaggregated crime types as the estimated effect of P_a is 55% greater for burglary and larceny than murder and rape. While the aggregation that remains in our data prohibits any specific interpretation, the deviation in P_a follows the characterization that murder and rape are crimes of passion, and sanctions are less influential in deterring these types of offenses.

Coefficient estimates of the remaining variables also vary considerably across models; however, the impact of aggregation on police is striking. Disaggregation appears to reverse the estimated relationship between police per capita and crime rates. Whereas CT's model carries the typical incorrect positive sign on police, our disaggregated models indicate the theoretically correct inverse relationship. Whether disaggregation actually curtails the simultaneity between police and crime rates may be questioned, but the results do indicate the importance of disaggregation in this setting.

To formally test for differences across aggregation levels, we employ a likelihood-ratio test. Considering the aggregation of violent crimes, we find that murder, rape, robbery, and assault should not be pooled in one regression ($\chi^2=252$) at the $P<0.01$ level. Rather the global test of parameter homogeneity suggests that the estimated parameters differ significantly across violent crime types. A similar result arises when considering the aggregation of property crimes. Parameter homogeneity across property crimes is also rejected soundly ($\chi^2=250$), implying that property crimes should not be pooled at the $P<0.01$ level. These results suggest that previous empirical estimates in the literature that impose isomorphic effects of deterrence and economic variables on reducing crime may be presenting erroneous estimates.

CT's original contribution concerned the role jurisdictional heterogeneity has in estimating deterrent elasticities with aggregate data. In a similar spirit, we re-estimate Eq. (1) excluding the fixed effects, α_i. Empirical estimates (available upon request) largely support the original findings in CT — ignoring jurisdictional heterogeneity will lead to an over-estimate of the deterrent effect. These results also provide additional evidence regarding the importance of aggregation because the deterrent effects are over-estimated to a larger extent in the 'loosely' reported regression models (burglary and robbery) relative to the tightly reported crimes such as murder. This is intuitively appealing because jurisdictional heterogeneity will be greater in loosely reported crimes than tightly reported offenses.

4. Concluding remarks

Numerous empirical and theoretical efforts on crime have been carried out since Becker's (1968) seminal paper over three decades ago. On the empirical side, use of aggregate crime data has been chastised, but the main criticisms have not hindered policymakers from using the important results (see, e.g. Blumstein et al., 1978). The value of our note is not only to make academics and public policymakers aware of the potential aggregation bias, but also give an indication of the degree of bias associated with aggregation.

Our major finding, that deterrent effects have heterogeneous impacts across crime types, does not serve to indict those who have used the isomorphic regression model to test the model of crime. Rather, it merely illustrates that past empirical results should be interpreted with caution given the potential of inconsistent and biased parameter estimates due to aggregation. Our result is particularly alarming given that one would expect if any cross-sections would have identical regression parameters it would be counties within one U.S. state, which are much more homogenous than samples of states and countries.

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