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Crime, Deterrence and Punishment Revisited*

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Abstract

Despite an abundance of empirical evidence on crime spanning over forty years, there exists no consensus on the impact of the criminal justice system on crime activity. We construct a new panel data set that contains all relevant variables prescribed by economic theory. Our identification strategy allows for simultaneity and controls for omitted variables and measurement error. We deviate from the majority of the literature in that we specify a dynamic model, which captures the essential feature of habit formation and persistence in aggregate behavior. Our results show that the criminal justice system exerts a large influence on crime activity. Increasing the risk of apprehension and conviction is more influential in reducing crime than raising the expected severity of punishment.

Key words: Crime, deterrence, simultaneity, omitted variable bias, measurement error, panel data, GMM.

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1 Introduction

Crime, originating from the root of Latin cernō ("I decide, I give judgment"), is the behavior judged by the State to be in violation of the prevailing norms that underpin the moral code of society. Where informal social controls are not sufficient to deter such behavior, the State may intervene to punish or reform those responsible through the criminal justice system. The precise sanctions imposed depend on the type of crime and the prevailing cultural norms of the society. For offences deemed to be serious, criminal justice systems have historically imprisoned those responsible, in the hope that a combination of deterrence and incapacitation may lower the crime rate. According to an estimate, about 10 million people in the world are institutionalized for punishment, almost half of which are held in America, China and the U.K. (Walmsley, 2009). Over the past thirty years, the American prison population has more than quadrupled. Such massive increases in the U.S. prison population may be explained almost entirely by an increase in punitiveness rather than an increase in crime rates (see e.g. Raphael and Stoll, 2009), leading some to label this extraordinary measure one of the largest scale policy experiments of the century. Other countries such as the U.K. and Australia have also experienced rising prison populations.

How effective is the criminal justice system in deterring crime? To what extent do changes in the expected punishment influence the motivation of individuals to engage in illegal pursuits? How much wrong-doing does each additional prisoner avert? In order to address these questions in a constructive way it is important to recognize that changes in the aggregate crime rate stem from individual behavior. Policies such as increased sentence lengths may lower the crime rate through two possible channels; deterrence and incapacitation. It is well accepted in the literature that for a particular policy to be effective it cannot operate on incapacitation effects alone (Durlauf and Nagin, 2010). In turn, for a policy to deter criminal behavior it must be designed with an understanding of what causes individuals to engage in criminal activity.

During the early part of the twentieth century criminal behavior was viewed as a type of social illness. For example, the strain theory of Merton (1938) suggests that crime is a behavioral response to social inequality. The seminal papers by Becker (1968) and Ehrlich (1975) changed this view, postulating that individuals engage in such activity simply because the subjective expected benefit exceeds the expected cost of doing so. Criminals, therefore, do not differ from the rest of society in their basic motivation but in their appraisal of benefits and costs. On this view a rational criminal behaves in a calculated manner, considering the benefit of the illegal act together with the risk of apprehension and conviction as well as the likelihood and severity of potential punishment, which are a function of three separate stages of processing through the criminal justice system pertaining to the roles of police, courts and prison system respectively. The idea of a rational criminal forges an important link with the deterrence hypothesis that underpins the criminal justice system − the notion that the crime rate can be reduced by raising the expected cost of criminal activity.

Since the seminal work of Becker (1968) and Ehrlich (1975), a large empirical literature has developed, seeking to inform public policy by collecting data on various populations and building econometric models that describe criminal behavior of individuals. The public concern about crime is well justified given the pernicious effects that it has on economic activity, as well as on the quality of one's life in terms of a reduced sense of personal and proprietary security. However, despite the rich history of econometric modeling spanning over forty years, there is arguably no consensus on whether there is a strong deterrent effect of law enforcement policies on crime activity. Empirical studies provide mixed evidence that are insufficient to draw clear conclusions, see Table 1 for an overview of recent studies.

The present study revisits the economics of crime and punishment and provides a case study for New South Wales (NSW), Australia. Our empirical strategy takes into account various important methodological issues arising in existing crime studies. First, identification strategies employed in the literature can be problematic because deterrence variables are often assumed to be strictly exogenous without testing such specification, or if otherwise, the instrumental variables used may be weak and/or invalid. This study makes use of dynamic panel data analysis, which provides natural instruments with respect to sufficiently lagged values of the endogenous regressors. These instruments are more likely to be valid in panels because the multi-dimensionality of panel data allows one to capture richer sources of unobserved heterogeneity relative to time series and cross-sectional data alone. Estimation is implemented using the well established Generalized Method of Moments (GMM). The validity of the instruments used is examined empirically using tests for weak identification and overidentifying restrictions.

Second, our study specifies a complete econometric model of crime, making use of a wide range of relevant deterrence, economic and demographic variables;

this is rarely the case with existing empirical studies, as shown in Table 1. We therefore avoid omitted variables bias as discussed in e.g. Mustard (2003), who concludes that previous estimates of arrest-rate effects are understated due to omitted conviction rates.

Third, while the economic model of crime purports to represent individual behavior, most data involve some form of aggregation – often, measurement takes place at the country or state level. In practice this could yield results that are inconsistent with economic theory. For example, Levitt (2001) argues that relying on national time series data can be particularly problematic since averaging across all of the locales removes useful variation, which may potentially result in misleading inferences. In the present study we are able to achieve a relatively low level of aggregation since the unit of observation is the Local Government Area (LGA) level in NSW.

Finally, we deviate from the majority of the literature in that we specify a dynamic model of crime, which captures the essential feature of habit formation and costs of adjustment in aggregate behavior. This is important because it permits distinguishing between the effect of law enforcement policies in the short- and the long-run, and deriving equilibrium conditions as well as other meaningful dynamic quantities such as mean and median lag length of the effects.[1]

The sensitivity of our results is analyzed extensively. First, we examine different moment conditions, depending on whether the probability of arrest is treated as endogenous or (weakly) exogenous, and we test for the validity of each specification. Second, we apply the methodology of Griliches and Hausman (1986) in order to test for measurement error in the data. Third, we estimate the crime model using a range of estimators other than GMM. Finally, we examine the effect of omitted variables in our model. The conclusions of our analysis appear to be robust.

The remainder of this paper is as follows. Section 2 discusses the empirical evidence pertaining to the economic model of crime and reviews the various specification problems. Section 3 presents the econometric specification employed in this study and discusses the identification strategy. Section 4 reports the empirical results. A final section concludes.

[1] For a recent overview on the dynamic panel data literature, see Bun and Sarafidis (2015).

2 Evidence on Crime Deterrence

Following the seminal papers by Becker (1968) and Ehrlich (1975), expected utility derived from criminal activity is a function of the probability of arrest (P_A), the probability of conviction given arrest ($P_{C|A}$), the probability of imprisonment conditional on conviction ($P_{P|C}$) and the expected prison sentence length (S). These variables are the standard deterrence variables that appear in the literature and are the focus of our analysis. Therefore, the expected utility from criminal or non-legal (NL) activity can be written as

$$\begin{aligned} E(U^{NL}) &= (1-P_A)U^{NL}(Y) + P_A(1-P_{C|A})U^{NL}(Y-C) \\ &\quad + P_A P_{C|A} P_{P|C} U^{NL}(Y-C-S) \\ &\quad + P_A P_{C|A}(1-P_{P|C})U^{NL}(Y-C-S'), \end{aligned} \tag{1}$$

where Y denotes the income owing from the criminal act, material or otherwise; C represents the costs that are incurred upon being charged with a crime but not necessarily punished (for example, social stigmatisation and diminished employment prospects); S is the cost of imprisonment; and S' denotes the cost when an alternative to imprisonment is used as punishment.

In words, the first term on the right hand side represents the full benefit of criminal activity in the case that one is not caught, which occurs with probability $(1-P_A)$; the second term represents the benefit from criminal activity in the event that one is arrested but not convicted of the crime (deflated by C, which denotes the costs) occurring with probability $P_A(1-P_{C|A})$[2]; the third term represents the benefit from criminal activity in the event that one gets caught, convicted and therefore is punished (deflated by $C+S$), occurring with probability $P_A P_{C|A} P_{P|C}$; and the fourth term captures all cases where the criminal is caught and found guilty, as with the previous term, but where an alternative to imprisonment is used. This occurs with probability $P_A P_{C|A}(1-P_{P|C})$, and the benefit from criminal activity is deflated by $C+S'$. It is typically assumed that imprisonment is the most severe punishment possible for any given crime $-$ that is, $S > S'$.

The main implication from the theoretical model for individual behavior towards crime is that increases in P_A, $P_{C|A}$ or $P_{P|C}$ decrease the expected utility derived from criminal activity. Thus, a potential criminal behaves in a calculated

[2] Although it is likely that the collateral costs of criminal charges are greater if the individual is actually convicted of crime, assuming that the full extent of these costs are incurred immediately upon arrest greatly simplifies the exposition of the analysis.

manner, taking into account the risk of apprehension and conviction as well as the likelihood and severity of punishment for a given level of benefit of the criminal act. Therefore, the crime rate can be reduced by increasing the expected cost of criminal activity.

Unfortunately, empirical analysis of the effect of law enforcement policies on criminal activity is inherently problematic due to the nature of crime data available. In particular, data collected from individuals are self-reported and are doubtlessly affected by significant measurement error. Moreover, the time and cost involved in surveying a representative population can be prohibitively large. As a result, empirical studies of crime typically use some form of aggregate data, which describe crime in locales (for example local areas, states or countries) and are based on official records rather than self-reported information.

However, aggregate data are also not without problems, leading some to suggest that the use of individual and aggregate data may be regarded as two complementary approaches (Trumbull, 1989). In particular, aggregate data may inherently introduce a form of bias by aggregating over individuals, since the economic model of crime purports to describe illegal behavior of individual agents. Furthermore, the use of aggregate data introduces a problem of simultaneity that makes the causal effect of law enforcement policies on crime more difficult to identify. For example, an exogenous upward shift in crime rate may overwhelm police resources, given that police resources are fixed in the short term, causing the probability of arrest to decrease. And even if reverse causality were not present in the data, the empirical probability of arrest (when defined as number of arrests divided by the number of crime incidents) is endogenous in the crime equation by construction, since the numerator of the dependent variable (number of crime incidents) is the denominator in the probability of arrest. This artificially induces a negative correlation between the two variables (Nagin, 1978) − a phenomenon that is known as "ratio bias" in the literature (see e.g. Dills et al., 2008). Closely related to this ratio bias is measurement error in both the crime rate and probability of arrest. Typically reported instead of the actual number of crimes has been used leading to a potential measurement error issue in both variables (Levitt, 1998).

Despite the potential for simultaneity when the relation between crime, policing and justice is modeled using aggregate data, the majority of earlier studies that span the period up to 1980 do not control for endogeneity, which casts doubt on their results (Blumstein et al., 1978). It is well known that in the presence of endogeneity, least-squares based estimates of the economic model of crime are contaminated by the reverse effect that crime may exhibit on law enforcement policies, and hence are biased and inconsistent. Dills et al. (2008) use aggregate data to demonstrate that raw correlations between crime rates and deterrence variables are frequently weak or even perverse due to the problem of simultaneity, and note that any identification strategy would need to be powerful enough to partial out the effect of deterrence on the crime rate and provide a result consistent with economic theory.

A further problem that may arise in empirical studies that use aggregate data is the potential for omitted variable bias in the estimated parameters. In particular, it is hardly ever the case that a complete model is specified that includes all deterrence variables prescribed by economic theory. This is likely to be due to lack of data or the fact that certain experimental designs intended to combat endogeneity preclude the possibility of examining all deterrence variables of interest. Whatever the appropriate explanation is, the evidence on crime deterrence has come to conform broadly to several distinct sub-literatures, in which the effect of the probability of arrest, the probability of conviction, the probability of imprisonment and the length of average sentence are rarely examined together.

Table 1 about here

Table 1 summarizes the empirical results for some of the most widely cited contributions to the crime deterrence literature using aggregate data. For each of the studies noted, the table reports the sampling population, the unit of observation, the structure of the data followed by the sample size[3], the method used to estimate the model, the type of crime analyzed and finally the actual results. Clearly, there is a paucity of studies that estimate a fully specified economic model of crime, with notable exceptions being the papers by Pyle (1984), Trumbull (1989) and Cornwell and Trumbull (1994). However, in both Pyle (1984) and Trumbull (1989) all deterrence variables are treated as exogenous and therefore least-squares based methods are used to obtain estimates of the parameters. Trumbull (1989) justifies this choice claiming that simultaneity is not a salient feature of the existing dataset, based on the results of a Wu-Hausman specification test. Cornwell and Trumbull (1994) treat the probability of arrest as endogenous but all remaining variables as exogenous. The authors fail to find a statistically significant relationship between the deterrence variables and crime using a 2SLS

[3]For panel data models the cross-sectional dimension, N, is given first, followed by the time dimension, T.

procedure.[4] Nevertheless, they produce inferences based on least-squares, arriving at a conclusion similar to Trumbull (1989) in that, as it is argued, the probability of arrest is exogenous.

The remaining studies restrict their attention to a particular variable of interest. Failing to include all deterrence variables fosters a disconnect between economic theory and empirical analysis. In order for a criminal to be punished, the person must be arrested and found guilty first; omitting the probability of arrest and conviction clearly ignores a fundamental aspect of the criminal decision. For example, Mustard (2003) shows that arrest rates are likely to be negatively correlated with the probability of conviction and sentence length since arrest rates are often substitutes for conviction rates and sentences. As a result Mustard (2003) concludes that previous estimates of the marginal effect of the probability of arrest may understate the true effect of the arrest rate by as much as fifty percent. Furthermore, omitted variables may invalidate estimation based on instrumental variables, as instruments may not be orthogonal to the deterrence variables omitted from the regression.

3 Econometric specification

3.1 Empirical Model

The dependent variable in our empirical analysis is the crime rate, which is the number of crime offences committed in a given local government area (LGA) i at time t (labeled crm_{it}) divided by population (pop_{it}). The rate of crime is not the same as the binary "crime - no crime" decision an individual faces, but it is arguably the closest substitute one can observe at the aggregate level. The economic model of crime postulates that criminals are rational individuals who assess the risk of apprehension and conviction as well as the likelihood of punishment prior to committing an offence, and ultimately evaluate the expected benefit and cost associated with an illegal activity. Therefore, the crime rate is modeled as a function of the empirical probability of arrest, the probability of conviction given arrest and the probability of imprisonment given conviction. This leads to the

[4] Bun (2015) shows that this is due to weak instruments.

following estimated empirical model:

$$\begin{aligned}
\ln\left(\frac{crm_{it}}{pop_{it}}\right) &= \alpha\ln\left(\frac{crm_{it-1}}{pop_{it-1}}\right)+\beta_1\ln\left(\frac{arr_{it}}{crm_{it}}\right)+\beta_2\ln\left(\frac{conv_{it}}{arr_{it}}\right) \\
&+\beta_3\ln\left(\frac{impr_{it}}{conv_{it}}\right)+\beta_4\ln avsen_{it}+\beta_5\ln income_{it} \\
&+\beta_6\ln unemp_{it}+\eta_i+\lambda_t+\varepsilon_{it}, \qquad (2)
\end{aligned}$$

for $t=1,...,T$ time periods and $i=1,...,N$ regions and with $(arr_{it}, conv_{it}, impr_{it})$ the number of arrests, convictions and imprisonments respectively. The inclusion of sentence length $(avsen_{it})$, income $(income_{it})$ and unemployment $(unemp_{it})$ in the equation captures the expected gains from the illegal and legal sectors. Precise definitions of all variables used in our regression analysis are provided in Table 2.

Table 2 about here

Using short-hand notation the empirical model can be rewritten as:

$$\begin{aligned}
\ln crmr_{it} &= \alpha\ln crmr_{i,t-1}+\beta_1\ln prbarr_{it}+\beta_2\ln prbconv_{it} \\
&+\beta_3\ln prbimpr_{it}+\beta_4\ln avsen_{it}+\beta_5\ln income_{it} \\
&+\beta_6\ln unemp_{it}+\eta_i+\lambda_t+\varepsilon_{it}. \qquad (3)
\end{aligned}$$

The error term in (3) allows for regional-level effects (η_i) that may be correlated with the regressors as well as time effects (λ_t) that capture common variations in crime across regions.

We remark that we deviate from the literature in a significant way in that we also allow for persistence in the level of crime due to habit formation and costs of adjustment, thus specifying a dynamic model of crime. In contrast, common practice in the literature employs a static model, where the entire effect of law enforcement policies is assumed to be realized immediately within the same time period. The specification (3) implies a dynamic effect of the deterrence variables on the crime rate, for which the speed of adjustment is determined by the coefficient of the lagged value of the dependent variable.

Many of the models used in the literature (see e.g. Table 1) are restricted versions of (3). For example, many studies do include the probability of arrest, but exclude the probabilities of conviction and imprisonment and sentence lengths. Mustard (2003) shows, however, that arrest rate effects are severely underestimated omitting convictions rates and sentence lengths from the analysis.

3.2 Identification Strategy

We estimate model (3) by the Generalized Method of Moments (GMM) developed originally by Hansen (1982) and adapted for estimation of dynamic panel data models by Arellano and Bond (1991), Arellano and Bover (1995) and Blundell and Bond (1998), among others. The GMM approach has the advantage that, compared to maximum likelihood, it requires much weaker assumptions about the initial conditions of the data generating process, and avoids full specification of the serial correlation and heteroskedasticity properties of the error, or indeed any other distributional assumptions. Moreover, GMM is a natural choice when multiple explanatory variables are endogenous. For the reasons discussed in Section 2, we treat the probability of arrest as an endogenous regressor. The lagged crime rate, which models the short-run dynamics, is an additional endogenous regressor.[5]

We combine information from the original model in levels (3) with the first-differenced model:

$$\begin{aligned}\Delta \ln crmr_{it} &= \alpha \Delta \ln crmr_{i,t-1} + \beta_1 \Delta \ln prbarr_{it} + \beta_2 \Delta \ln prbconv_{it} \\ &\quad + \beta_3 \Delta \ln prbimpr_{it} + \beta_4 \Delta \ln avsen_{it} + \beta_5 \Delta \ln income_{it} \\ &\quad + \beta_6 \Delta \ln unemp_{it} + \Delta \lambda_t + \Delta \varepsilon_{it}, \qquad (4)\end{aligned}$$

resulting in System GMM estimation (Blundell and Bond, 1998). Since dynamic panels are often largely overidentified, an important practical issue is how many moment conditions to use. It is well documented that numerous instruments can overfit endogenous variables in finite samples, resulting in a trade off between bias and efficiency. There is substantial theoretical work on the overfitting bias of GMM coefficient estimators in panel data models (Ziliak, 1997; Alvarez and Arellano, 2003; Bun and Kiviet, 2006). Furthermore, with many moment conditions the power of (mis)specification tests deteriorates rapidly (Bowsher, 2002). Roodman (2009) compares two popular approaches for limiting the number of instruments: (i) the use of (up to) certain lags instead of all available lags and (ii) combining instruments into smaller sets. There is a clear bias reduction especially under the second approach. Therefore, we follow these recommendations and only use the two nearest lagged instruments. Furthermore, we collapse them resulting in the

[5] We treat all remaining explanatory variables in model (3) as strictly exogenous regressors.

following four moment conditions for the model in first differences:

$$E\left[\sum_{t=3}^{T}\ln prbarr_{i,t-2}\Delta\varepsilon_{it}\right] = 0, \qquad E\left[\sum_{t=4}^{T}\ln prbarr_{i,t-3}\Delta\varepsilon_{it}\right] = 0,$$
$$E\left[\sum_{t=3}^{T}\ln crmr_{i,t-2}\Delta\varepsilon_{it}\right] = 0, \qquad E\left[\sum_{t=4}^{T}\ln crmr_{i,t-3}\Delta\varepsilon_{it}\right] = 0. \tag{5}$$

In addition, we exploit the following two (collapsed) moment conditions for the original model in levels:

$$E\left[\sum_{t=3}^{T}\Delta\ln prbarr_{i,t-1}(\eta_i+\varepsilon_{it})\right] = 0, \qquad E\left[\sum_{t=3}^{T}\Delta\ln crmr_{i,t-1}(\eta_i+\varepsilon_{it})\right] = 0. \tag{6}$$

We note that another advantage of collapsed instruments is that the underlying time-specific moment conditions do not need to hold exactly for each time period, but only in sum.

Regarding the probability of arrest we will also estimate specifications under further exogeneity assumptions. In case of weak exogeneity we add

$$E\left[\sum_{t=3}^{T}\ln prbarr_{i,t-1}\Delta\varepsilon_{it}\right] = 0, \tag{7}$$

to the set of moment conditions for the first-differenced model, while replacing the levels moment condition (6) by:

$$E\left[\sum_{t=3}^{T}\Delta\ln prbarr_{it}(\eta_i+\varepsilon_{it})\right] = 0. \tag{8}$$

In case of strict exogeneity we further add the following three moment conditions for the first-differenced model:

$$E\left[\sum_{t=3}^{T}\ln prbarr_{i,t-j}\Delta\varepsilon_{it}\right] = 0, \quad j = 0, -1, -2, \tag{9}$$

while for the levels model we use the same moment condition as under weak exogeneity.

It is well known that identification can be weak when the panel data are persistent. We therefore check the identification strength of the exploited moment conditions in various ways. First, we estimate pure autoregressive models for the endogenous regressors and check whether autoregressive dynamics are reasonably far away from the unit root. Second, we use the Kleibergen-Paap (2006) rank statistic to test for underidentification in the first-differenced and levels IV models.

To check the validity of the estimated specification, we report the p-value of Hansen's (1982) J test of overidentifying restrictions and the p-value of Arellano and Bond's (1991) test of serial correlation of the disturbances up to second order. The former is used to determine empirically the validity of the overidentifying restrictions in the GMM model. The latter is useful because the validity of lagged instruments crucially depends on the absence of higher-order residual autocorrelation in the first-differenced model.

Long-run estimates are computed by dividing the short-run slope coefficients by one minus the estimated autoregressive parameter. Robust standard errors are reported in parentheses, which are valid under arbitrary forms of heteroskedasticity and serial correlation. Furthermore, we perform the correction proposed by Windmeijer (2005) for the finite-sample bias of the standard errors of the two-step GMM estimator.[6] The standard errors of the long run estimated parameters are subsequently obtained using the Delta method.

4 Data and Empirical results

4.1 Data

We construct a new dataset containing information on criminal activity and deterrence for all $N = 153$ local government areas in New South Wales, each one observed over a period of $T = 13$ years from 1995/96 to 2007/08. The Australian Standard Geographic Classification (ASGC) defines the LGA as the lowest level of aggregation following the census Collection District (CD) and Statistical Local Area (SLA).[7] Thus, the LGA represents a low level of aggregation compared to standard practice in the literature, where regressions using city-, state- and country-level data are common. To the best of our knowledge, this is the first panel model of crime that has been constructed for Australia. The raw data for crime offences and deterrence variables have been purchased from the NSW Bureau of Crime Statistics and Research. Income and population data have been obtained

[6] All GMM results have been obtained using David Roodman's *xtabond2* algorithm in Stata 13.

[7] Each CD contains on average about 225 households (2001 Census). There are about 37,000 CDs throughout Australia. The boundaries of an SLA are designed to be typically coterminous with Local Government Areas unless the LGA does not fit entirely into a Statistical Subdivision, or is not of a comparative nature to other LGA's. There are 193 SLAs in NSW.

from the Australian Bureau of Statistics (ABS) website, while the unemployment data have been purchased from the Small Area Labour Markets division of the Department of Education, Employment and Workplace Relations (DEEWR).

The NSW Bureau of Crime Statistics and Research provides two alternative definitions for average prison sentence; average non-parole period and average head sentence. We use the non-parole period in the analysis because this represents more closely the actual amount of time spent in confinement. The raw data for income and population are not readily comparable with the crime data because they are based on different ASGC standards, i.e. LGA boundaries are defined slightly differently by the NSW Bureau and the ABS. To this end, we mapped the data to a common ASGC standard (2006) using a series of concordance tables, in order to achieve consistency. Similarly, the unemployment data were first mapped to the same ASGC standard (2006) to account for name and boundary changes that occured in the LGAs over the sample period. The resulting SLA data were then aggregated to the LGA level to be directly comparable to the other data.

Table 3 about here

Table 3 reports descriptive statistics for the different categories of crime considered in our analysis. As expected, the mean value of the rate of violent crime is smaller than that of property crime and it exhibits a much smaller dispersion as well, which indicates that violent crime occurs less frequently and is more localized. The empirical probability of arrest is higher on average for violent crime, which is perhaps reflective of the fact that violent crime typically involves face-to-face contact increasing the probability of apprehension. The mean value of average sentence length is much larger than the value in the 90th percentile, which shows that there is a relatively small number of very big sentences in the sample.

To get an idea of the time series persistence in our data we estimate pure autoregressive models for the crime rate and the probability of arrest, i.e. the endogenous regressors in our empirical analysis. System GMM estimates of autoregressive coefficients are in the range 0.2-0.6, which shows moderate persistence only. Not surprisingly panel unit root tests (Harris and Tzavalis, 1999; Im et al. 2003) reject the null hypothesis of a unit root for both crime rates and probability of arrest and for both property and violent crime.

4.2 Baseline estimation results

We analyse property crime and violent crime separately, based on the econometric model presented in the previous section. System GMM estimates allowing for endogeneity of lagged crime and the probability of arrest are reported in Table 4. We use the 6 moment conditions as defined in (5) and (6).

Table 4 about here

The p-values from the various reported mis-specification tests show no evidence of lack of validity of the estimated specification. Furthermore, the Kleibergen-Paap (2006) rank tests indicate no underidentification, hence lagged crime and arrest rate instruments have predictive power for the endogenous regressors in both the model in first-differences and the original specification in levels.

The GMM estimates of the parameters are of the expected sign in the short- and the long-run. Thus, for property crime a one percent increase in the probability of arrest appears to decrease the expected value of the crime rate by 0.256 percent in the short-run and 0.548 percent in the long-run, ceteris paribus. Likewise, the elasticity of the probability of conviction is about -0.275 and -0.590 in the short- and long-run respectively. The fact that the estimated elasticities are larger in the long-run is well anticipated, since typically one needs time to adjust fully to changes in law enforcement policies, due to habitual behavior, imperfect knowledge and uncertainty. In particular, the value of the autoregressive parameter indicates that it takes about 3.5 years for 90% of the total impact of either one of the explanatory variables on crime to be realized, all else being constant.

The estimated coefficient of the probability of imprisonment is not significant, while average sentence length only seems to matter for violent crime. Furthermore, they both appear to exhibit a much smaller effect on crime compared to the probability of arrest and the probability of conviction. This shows that imprisoning more criminals, or imprisoning them for longer, is not as effective as increasing the risk of apprehension or conviction once arrested. In other words, criminal activity seems to be highly responsive to the prospect of arrest and conviction, but less responsive to the prospect or severity of imprisonment. This provides support to the idea that the consequences of being arrested and found guilty of a criminal offence include the indirect sanctions imposed by society and not just the punishment meted out by the criminal justice system. A convicted individual may no longer enjoy the same opportunities in the labor market or the same treatment by their peers, and so the opportunity cost of lost income and the cost to the individual of social stigmatization is implied in the event of conviction. Zimring and Hawkins (1973, p174) argue:

> "Official actions can set off societal reactions that may provide potential offenders with more reason to avoid conviction than the officially imposed unpleasantness of punishment".

The results suggest that the lost social standing resulting from a conviction may well outweigh the effects of prison sentence, let alone a fine or community service order.

Compared with earlier deterrence estimates the estimated arrest and conviction elasticities are large. The long-run or total effects reported in Table 4 are (in absolute value) in the range $0.50 - 0.90$. Often the measured elasticities for these deterrence variables are (in absolute value) smaller than 0.50, see Table 1.

4.3 Sensitivity analysis

As discussed previously, the arrest probability is often seen as an endogenous regressor in the empirical crime literature. We have therefore estimated our baseline crime model allowing for endogeneity of the probability of arrest. However, efficiency gains may result by imposing further exogeneity assumptions on this main deterrence regressor. We distinguish between two cases, i.e. weak exogeneity and strict exogeneity. Table 5 reports these results based on System GMM. In case of weak exogeneity we add (7) to the set of moment conditions for the first-differenced model, while replacing the levels moment condition (6) by (8). In case of strict exogeneity of the arrest probability we further add the moment conditions in (9).

Table 5 about here

In Table 5 we also show p-values from difference-in-Hansen statistics testing the validity of various subsets of moment conditions. We distinguish the moment conditions due to (1) exogeneity/endogeneity of the probability of arrest; (2) endogeneity of lagged crime; (3) mean stationarity. The p-values from the various reported mis-specification tests show no serious problem with the assumption of weak exogeneity of the probability of arrest. This assumption furthermore greatly improves the estimation results[8] in the sense that the arrest rate elasticity is estimated with much more precision. The further assumption of strict exogeneity lowers the p-value of the Hansen test somewhat for property crime, although it is still insignificant.

To further test the exogeneity of the arrest probability we apply the empirical test of Griliches and Hausman (1986) on the presence of measurement error. The idea is that longer differences, as opposed to first differences, are less vulnerable to measurement error. Therefore, in absence of measurement error the OLS estimator of the arrest rate elasticity in the differenced crime model should not show any systematic pattern across the difference length. Levitt (1998) applied this test to investigate the extent of measurement error and ratio bias in the crime-arrest rate relationship and finds no significant measurement error. The empirical results for the Australian crime data are in Table 6.

Table 6 about here

The results in Table 6 corroborate the findings of Levitt (1998) that there is little evidence of measurement error and ratio bias in the probability of arrest. We took first, second, third and fourth differences, and the arrest rate elasticity does not change substantially across specifications.

Imposing (weak) exogeneity of the arrest probability one can also use alternative inference methods. Fixed effects Maximum Likelihood procedures are consistent for a small number of time periods T, while the standard fixed effects OLS estimator is consistent when T is large enough. Here $T = 13$ is double digit, hence we may apply such methods with some confidence.

Table 7 about here

Table 7 reports estimation results using a range of ML and OLS fixed effects estimators. Pooled OLS is shown (for completeness only) in column (1). It is obvious that not accounting for region specific effects leads to severe underestimation of the effect of the judicial system on crime, whereas the autoregressive coefficient is biased upwards, as expected. Column (2) reports standard fixed effects OLS (LSDV or within) estimates, while for column (3) the Transformed ML estimator (Hsiao et al., 2002) has been used. In general, coefficient estimates from

[8] Outcomes from the Kleibergen-Paap (2006) rank test also improve substantially under the assumption of weak exogeneity.

these estimators are plausible in sign and magnitude, and both give similar deterrence estimates. Arrest and conviction rate elasticities are (in absolute value) somewhat smaller than the System GMM estimates of Table 4, while estimated standard errors suggest much higher precision. Finally, column (4) shows Mean Group estimates (Pesaran and Smith, 1995) allowing for cross-sectional parameter heterogeneity. The pattern of the coefficient estimates is mostly in line with the estimates of other estimators, which suggests that the assumption of common parameters across regions is not restrictive. The only notable difference is the estimated autoregressive parameter, which indicates less persistence especially for violent crime.

Finally, we analyze the sensitivity of our results to omitted deterrence variables. Mustard (2003) shows how excluding conviction rates and sentence length from the model leads to omitted variables bias. In particular, due to the negative correlation between these regressors and the probability of arrest an underestimate of the true effect of arrest rates on crime may result. Table 8 reports results from specifications including only the probability of arrest as a deterrence regressor. We show both System GMM and fixed effects OLS (LSDV) estimates.

Table 8 about here

The pattern of the OLS estimates corroborate the findings of Mustard (2003), i.e. omitting other relevant deterrence variables lowers the arrest rate elasticity considerably. The GMM estimates, however, do not show such a result. The reason is that the moment conditions used are still valid as the outcomes of the various misspecification tests suggest. In other words, the lagged instruments used are robust to omitted variables and, hence, similar arrest rate estimates result compared with the GMM estimates in Table 4.

5 Concluding Remarks

We estimate an econometric model for crime using a new panel data set containing information on illegal activity and deterrence variables for local government areas in New South Wales, Australia. We take into account various endogeneity concerns expressed previously in the literature. Our findings suggest that the criminal justice system can potentially exert a large impact on crime.

Our results show that increasing the risk of apprehension and conviction exhibits a much larger effect in reducing crime compared to raising the expected severity of punishment. This may have significant policy implications. For example, if it were estimated that the cost of keeping a prisoner incarcerated for a year was roughly equivalent to the cost of making a single additional arrest, then one could justify a redirection of resources from prisons to policing. This implies that imprisoning more criminals, or imprisoning them for longer, is not optimal from a policy perspective, assuming that the cost involved behind these activities is of similar magnitude.

In our analysis we address the impact of simultaneity, measurement error and omitted variables. The resulting dynamic panel data model of crime is estimated by the Generalized Method of Moments. We show that the detrimental effects of measurement error and ratio bias are largely absent in our data. Moreover, we don't find overwhelming evidence for simultaneity between arrest rates and crime. Furthermore, our identification strategy is robust to the exclusion of relevant deterrence variables, which typically tend to understate the effect of law enforcement policies.

There are several interesting issues that remain to be explored. In particular, given our analysis it would be useful to measure the effectiveness of different police activities in influencing the risk of apprehension and determining the empirical probability of arrest following an offence. Furthermore, from an economic point of view it is inviting to examine the costs and benefits associated with crime prevention. We intend to pursue both of these issues in future research.

Table 1: Empirical estimates of the elasticity of the crime rate with respect to policing and justice

Author	Year	Population	Unit of observation	Data (sample size)	Method	Crime type	Arrest	Conviction	Imprisonment	Sentence length
Panel A: Complete economic models of crime										
Cornwell and Trumbull	1994	North Carolina	County	Panel (90, 7)	OLS, 2SLS	Total	-0.455	-0.336	-0.196	-0.03
Trumbull	1989	North Carolina	County	Cross section (98)	OLS	Total	-0.217	-0.451	-0.325	-0.149
Pyle	1984	England and Wales	Police authorities	Cross section (41)	OLS	Robbery	-0.5	0.73	-0.48	-0.57
						Property	-0.32	0.4	-0.55	-0.85
Panel B: Arrest										
Klick and Tabarrok	2005	Washington D.C.	Police district	Panel (7,506)	OLS	Violent	-0.3			
						Burglary	-0.3			
Fajnzylber et al	2002	United Nations	Country	Panel (45, 5)	GMM	Robbery	0.08			.035
						Homicide	-0.09			-0.346
Corman and Mocan	2000	New York	City	Time series (108)	OLS	Murder	-0.336			
						Burglary	-0.355			
Bodman and Maultby	1997	Australia	State	Cross section (60)	2SLS	Robbery	-0.258			-0.621
						Burglary	-0.367			
Levitt	1997	United States	City	Panel (59, 23)	2SLS	Violent	-0.9*			
						Property	-0.24*			
Marvell and Moody	1996	United States	City	Panel (56, 22)	Granger	Total	-0.133			
						Homicide	-0.241			
						Burglary	-0.151			
Sampson and Cohen	1988	United States	City	Cross section (171)	2SLS	Robbery	-0.28			
						Burglary	-0.12			
Carr-Hill and Stern	1973	England and Wales	Police districts	Cross section (64)	FIML	Total	-0.59			-0.17
Panel C: Imprisonment										
Johnson and Raphael	2012	United States	State	Panel (51, 27)	2SLS	Violent			-0.11	
						Property			-0.21	
Liedka et al	2006	United States	State	Panel (51, 29)	Granger	Total			-0.245	
						Murder			-0.13	
						Burglary			-0.136	
Levitt	2002	United States	City	Panel (100, 21)	2SLS	Violent	-0.435		-0.171	
						Property	-0.501		-0.305	
Witt and Witte	2000	United States	Country	Time series (38)	VAR	Total			-0.55	
Levitt	1996	United States	State	Panel (51, 23)	2SLS	Violent			-0.261	
						Property			-0.379	
Marvell and Moody	1994	United States	State	Panel (49, 19)	Granger	Total			-0.159	
						Homicide			-0.065	
						Burglary			-0.253	
Ehrlich	1973	United States	State	Cross section (47)	2SLS	Total			-0.991	-1.123
Panel D: Conviction and other studies										
Haas	1980	New Jersey	Municipality	Cross section (181)	2SLS	Total		-0.02		
Withers	1984	Australia	State	Cross section (104)	OLS	Violent		0.29	0.09	
						Total		-0.62	-0.6	
						Property		-0.59	-0.56	
Sjoquist	1973	United States	Municipality	Cross section (53)	OLS	Theft	-0.342	-0.678		-0.212

* indicates author provided multiple estimates, in which case the median is reported.

Table 2: Definition of variables in the crime model

Variable	Definition
crmr	Number of criminal incidents divided by total population
prbarr	Number of arrests divided by criminal incidents
prbconv	Number of convictions divided by arrests
prbimpr	Number of imprisonments divided by convictions
avsen	Average non-parole period (months) imposed for prison sentences
income	Average wage and salary earner income
unemp	Unemployment rate (%)

Table 3: Descriptive statistics

Variable	Crime type	Mean	Stdev	10th Perc	90th Perc
Crime rate					
	Property	0.066	0.036	0.034	0.096
	Violent	0.034	0.024	0.016	0.049
Probability of arrest					
	Property	0.111	0.052	0.056	0.175
	Violent	0.344	0.128	0.198	0.505
Probability of conviction					
	Property	0.333	0.211	0.153	0.502
	Violent	0.340	0.140	0.200	0.500
Probability of imprisonment					
	Property	0.108	0.097	0.000	0.207
	Violent	0.088	0.072	0.000	0.171
Average sentence (days)					
	Property	5.3	4.3	0.0	9.2
	Violent	606.1	9672.3	0	23.6
Income ($ '000)		34.0	9.4	25.2	44.0
Unemployment (%)		7.1	5.1	3.0	12.4

Descriptive statistics computed for the variables used in regression analysis. $N = 153$ and $T = 13$, yielding a total of 1,989 observations.

Table 4: System GMM estimates of the crime model

	property		violent	
	Short-run	Long-run	Short-run	Long-run
Lagged crime rate	0.534		0.554	
	(0.078)		(0.075)	
Probability of arrest	-0.256	-0.548	-0.355	-0.797
	(0.093)	(0.272)	(0.103)	(0.300)
Probability of conviction	-0.275	-0.590	-0.391	-0.876
	(0.055)	(0.198)	(0.062)	(0.263)
Probability of imprisonment	-0.007	-0.014	0.016	0.035
	(0.010)	(0.022)	(0.012)	(0.030)
Average sentence	0.004	0.009	-0.007	-0.016
	(0.008)	(0.019)	(0.004)	(0.009)
Income	-0.173	-0.371	-0.658	-1.476
	(0.099)	(0.247)	(0.145)	(0.506)
Unemployment	0.111	0.238	0.039	0.087
	(0.032)	(0.102)	(0.026)	(0.068)
p-value overidentifying restrictions	0.293		0.558	
p-value serial correlation				
- lag 1	0.000		0.000	
- lag 2	0.740		0.356	
p-value rank test				
- first-differences	0.022		0.000	
- levels	0.000		0.001	

Note: Windmeijer (2005) standard errors reported in parentheses. Each regression includes LGA-specific effects and time-specific effects.

Table 5: System GMM estimates under weak and strict exogeneity

	property			violent		
	endo	weak	strict	endo	weak	strict
Lagged crime rate	0.534	0.537	0.621	0.554	0.515	0.545
	(0.078)	(0.066)	(0.052)	(0.075)	(0.085)	(0.078)
Probability of arrest	-0.256	-0.267	-0.181	-0.355	-0.399	-0.355
	(0.093)	(0.063)	(0.041)	(0.103)	(0.080)	(0.052)
Probability of conviction	-0.275	-0.280	-0.225	-0.391	-0.426	-0.388
	(0.055)	(0.045)	(0.036)	(0.062)	(0.069)	(0.054)
Probability of imprisonment	-0.007	-0.006	-0.010	0.016	0.018	0.014
	(0.011)	(0.011)	(0.010)	(0.012)	(0.013)	(0.012)
Average sentence	0.004	0.004	0.002	-0.007	-0.007	-0.007
	(0.008)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)
Income	-0.173	-0.186	-0.130	-0.658	-0.723	-0.654
	(0.099)	(0.083)	(0.074)	(0.145)	(0.14)	(0.107)
Unemployment	0.111	0.110	0.078	0.039	0.048	0.039
	(0.032)	(0.029)	(0.024)	(0.026)	(0.029)	(0.027)
p-value overidentifying restrictions						
- probability of arrest	0.686	0.723	0.334	0.612	0.914	0.877
- lagged crime	0.176	0.147	0.241	0.403	0.591	0.353
- mean stationarity	0.471	0.366	0.285	0.380	0.608	0.404
- overall	0.293	0.366	0.174	0.558	0.830	0.834
p-value serial correlation						
- lag 1	0.000	0.000	0.000	0.000	0.000	0.000
- lag 2	0.740	0.749	0.736	0.356	0.291	0.370

Note: see Table 4.

Table 6: Difference OLS estimators of the arrest rate elasticity

	property	violent
first differences	-0.151	-0.186
	(0.028)	(0.029)
second differences	-0.133	-0.193
	(0.034)	(0.036)
third differences	-0.138	-0.200
	(0.036)	(0.041)
fourth differences	-0.134	-0.210
	0.044	(0.046)

Note: Cluster robust standard errors reported in parentheses.

Control variables and time-specific effects included.

Table 7: OLS and ML estimators of the crime model

	property				violent			
	POLS	LSDV	TML	MG	POLS	LSDV	TML	MG
Lagged crime rate	0.919	0.527	0.635	0.458	0.897	0.468	0.621	0.220
	(0.012)	(0.031)	(0.044)	(0.042)	(0.015)	(0.032)	(0.035)	(0.041)
Probability of arrest	-0.054	-0.181	-0.170	-0.121	-0.034	-0.234	-0.222	-0.235
	(0.014)	(0.025)	(0.028)	(0.036)	(0.017)	(0.026)	(0.025)	(0.041)
Probability of conviction	-0.100	-0.201	-0.200	-0.122	-0.149	-0.247	-0.211	-0.198
	(0.015)	(0.021)	(0.029)	(0.018)	(0.019)	(0.020)	(0.018)	(0.023)
Probability of imprisonment	-0.002	-0.019	-0.014	-0.003	0.004	-0.024	-0.010	-0.025
	(0.006)	(0.006)	(0.007)	(0.014)	(0.006)	(0.007)	(0.006)	(0.010)
Average sentence	-0.005	-0.013	-0.010	-0.038	-0.003	0.001	-0.000	0.002
	(0.008)	(0.008)	(0.008)	(0.017)	(0.003)	(0.003)	(0.002)	(0.006)
Income	-0.087	-0.535	-0.546	-0.427	-0.115	-0.194	-0.297	0.108
	(0.026)	(0.099)	(0.108)	(0.074)	(0.029)	(0.101)	(0.102)	(0.077)
Unemployment	0.020	0.016	0.004	-0.023	0.001	-0.008	-0.006	0.042
	(0.007)	(0.013)	(0.011)	(0.032)	(0.007)	(0.012)	(0.010)	(0.026)

Note: Pooled OLS (POLS), Least Squares Dummy Variables (LSDV), Transformed ML (TML), Mean Group (MG).

Table 8: Excluding conviction and sentencing data

	property		violent	
	GMM	LSDV	GMM	LSDV
Lagged crime rate	0.532	0.525	0.605	0.503
	(0.070)	(0.034)	(0.069)	(0.029)
Probability of arrest	-0.252	-0.048	-0.397	-0.067
	(0.098)	(0.018)	(0.130)	(0.023)
Income	-0.124	-0.259	-0.717	-0.206
	(0.124)	(0.168)	(0.175)	(0.186)
Unemployment	0.127	0.003	0.079	0.003
	(0.034)	(0.014)	(0.031)	(0.016)
p-value overidentifying restrictions	0.220		0.479	
p-value serial correlation				
- lag 1	0.000		0.000	
- lag 2	0.361		0.996	

Note: Probability of conviction, imprisonment and average sentence length omitted.

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