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The effect of arrest and imprisonment on crime

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Aim: *The aim of this study was to assess the extent to which the probability of arrest, the probability of imprisonment and imprisonment duration impact on property and violent crime rates in New South Wales, Australia.*

Method: *A dynamic panel data model with fixed Local Government Area and time effects was adopted to explore this, while adjusting for potential confounders of the relationship between arrest, imprisonment and crime. The first-differenced generalised method of moments was used to estimate the model parameters.*

Results: *One per cent increases in arrest rates for property and violent crime are estimated to produce 0.10 per cent and 0.19 per cent decreases in property and violent crime, respectively. If the one per cent increase in arrest rates is sustained, the long-run effect is estimated to be 0.14 and 0.30 per cent decreases for property and violent crime, respectively. The short-run elasticities for imprisonment probabilities were smaller, (-0.09 and -0.11), as were the long-run elasticities (-0.12 and -0.17), for property and violent crime, respectively. There was no evidence that increases in the length of imprisonment has any short or long-run impact on crime rates.*

Conclusion: *The criminal justice system plays a significant role in preventing crime. Some criminal justice variables, however, exert much stronger effects than others. Increasing arrest rates is likely to have the largest impact, followed by increasing the likelihood of receiving a prison sentence. Increasing the length of stay in prison beyond current levels does not appear to impact on the crime rate after accounting for increases in arrest and imprisonment likelihood. Policy makers should focus more attention on strategies that increase the risk of arrest and less on strategies that increase the severity of punishment.*

Keywords: *Arrest, imprisonment, crime rates, heroin use, economic factors, time series analysis.*

INTRODUCTION

Australia currently spends more than $11.5 billion annually on law and order (SCRGSP, 2011). In per capita terms, this amounts to $511.00 per person per annum. The bulk of this money goes to police ($8.2 billion) and prisons ($2.2 billion), with the remainder being spent on the administration of the criminal courts ($673 million) and community corrections ($383 million). Much of the money allocated to police is spent investigating crime and arresting and prosecuting suspected offenders. In the financial year 2009/10, 661,713 people appeared in Australian courts on criminal charges (Australian Bureau of Statistics (ABS), 2011a). More than 90 per cent of those charged with one or more criminal offences were convicted of one or more charges. In the financial year 2009/10, more than 60,000 of these people received a custodial sentence (ABS, 2011a).

Despite the large amounts of public money spent arresting, prosecuting and imprisoning offenders, little research has been conducted in Australia into the effectiveness of arrest and imprisonment in preventing and controlling crime. Not surprisingly, policy makers and academics have disagreed on the utility of the criminal justice system in controlling crime. Policy makers, the public and the media have been content to assume that any policy that increases the risk of arrest, the likelihood of imprisonment or the length of a prison term will of necessity result in a decrease in crime. Academics, on the other hand, have traditionally been rather sceptical. Russell Hogg and David Brown, for example, described 'the manipulation of criminal justice measures to combat crime [as] rather fanciful; a symbolic gesture rather than a serious policy response' (Hogg & Brown, 1998, p. 9). Over the last ten years, views on the effectiveness of the criminal justice system in controlling crime have become

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somewhat less polarised as growing evidence emerged that the criminal justice system does have an effect on crime (see, for example, Brown, 2010). Most of this evidence, however, emanates from North America. Its applicability to Australia remains unclear.

Kelaher and Sarafidis (2011) have recently carried out one of the most sophisticated attempts to date in Australia to examine the effect of the criminal justice system on crime. They examined the joint effects of arrest risk, conviction risk, imprisonment risk and sentence length on violent and non-violent crime using a panel of 153 New South Wales (NSW) Local Government Areas (LGAs) over the 13 year period from 1995/6 to 2007/8. They found evidence that both types of crime were inversely related to: the risk of arrest; the risk of conviction given arrest; the risk of imprisonment given conviction; and sentence length (non-violent crime only). These effects held up in the face of controls for other factors likely to influence crime and for the reciprocal effects of crime and criminal justice variables on each other. The study by Kelaher and Sarafidis (2011) nonetheless suffered from a number of methodological drawbacks, including mismeasurement of the crime variable; mismeasurement of the probability of arrest; and omitted variable bias (resulting from a failure to control for the effects of the heroin shortage on crime). This bulletin seeks to overcome these problems and improve on their estimates of the effects of the NSW criminal justice system on crime.

The specific aims of the bulletin are to examine the effect on property and violent crime of variations in (a) the likelihood of arrest, (b) the likelihood of imprisonment and (c) the average prison term. The study we discuss is one of only a handful to examine the joint effects of arrest and imprisonment on crime in Australia. We focus on property and violent crime because these forms of crime are better measured through police data than many other forms of crime (e.g. illegal drug use). We seek to determine whether any of (a) to (c) influence property and violent crime and, if so, which of (a) to (c) has the largest effect. In the next section of this bulletin we discuss some important theoretical issues. In the section after that we discuss four main methodological challenges that must be overcome to properly estimate the impact of the criminal justice system on crime. In the section entitled 'past research' we summarise existing evidence in relation to the effect of the justice system on crime. We then describe the methods employed in the current study, our findings, and their limitations and implications for policy.

THEORY

The classical view of the criminal justice system is that it functions as a deterrent to offending. The first formal expression of this view appeared in the writings of Cesare Beccaria in the C18th. According to Beccaria, people possess free will and are guided by reason and self-interest. If the pain obtained from punishment exceeds the pleasure obtained from crime, he argued, then people will choose not to commit crime (Vold, Bernard, & Snipes, 2002, p. 17). Although Beccaria had faith in the deterrent effect of punishment, he was not an unqualified supporter of harsh penalties. In fact he was critical of many of the sentencing practices of courts in his time, believing that the severity of punishment should be adjusted so that it only just offsets the rewards associated with criminal activity. More severe punishments, he believed, would be counter-productive (Vold et al., 2002, p. 17).

Beccaria's views about the deterrent effects of punishment fell into disfavour for a long time but were revived by Gary Becker (1968), who reformulated Beccaria's ideas in the language of economic theory. According to Becker (1968), people make choices that maximise their expected utility. The expected utility of a course of action is the likelihood that the course of action will deliver a set of benefits, multiplied by the value of those benefits minus any costs associated with procuring them. According to Becker (1968), the benefits and costs of crime are not purely monetary. They include, for example, the satisfaction that might flow from retribution, the stigma associated with arrest, conviction and imprisonment and the diminished prospects of employment associated with being convicted of a criminal offence.

The key deterrence variables in Becker's theory of crime are the likelihood of apprehension and the cost of being caught. Ehrlich (1973) later pointed out that to be punished for a crime it was necessary first to be convicted. Where the punishment is prison, moreover, he argued we are really looking at two factors: the probability of imprisonment and the length of the prison term. This led him to put forward a model according to which offenders separately assess the likelihood of arrest (P_A); the likelihood of conviction given arrest ($P_{C|A}$); the likelihood of imprisonment given conviction ($P_{I|C}$) and the average prison term (S), if imprisoned. According to Ehrlich's (1973) theory, crime can be reduced by increasing any of these factors. The theory carried specific implications about their ordering of their effects on crime; predicting that $P_A > P_{C|A} > P_{I|C}$.

METHODOLOGICAL ISSUES

In the years immediately following the publication of the Ehrlich's (1973) theory, a large number of studies were conducted into the effect of arrest and imprisonment on crime (e.g. Avio & Clark, 1976; Carr-Hill & Stern, 1973; Sjoquist, 1973; Tittle & Rowe, 1974). These early studies mostly found a strong negative association between the risk or frequency of arrest and crime. Many of them, however, were plagued by *omitted variable bias*, that is, they failed to control for other factors that might influence crime (Greenberg, 1977). If an omitted variable is correlated with one or more of the criminal justice variables included in the analysis they will end up acting as proxies for its effects, giving the impression that the criminal justice system is having an effect on crime when it is not.

In 1978 the US National Research Council (NRC) published an analysis of deterrence studies published up to that point (Blumstein, Cohen, & Nagin, 1978). The NRC review and its commissioned papers (especially Fisher & Nagin, 1978) identified two other major problems with the past research on the effect of the criminal justice system on crime. The first was *simultaneity*. Just as the criminal justice system influences crime, crime influences the criminal justice system. As with failure to deal with omitted variables, failure to deal with the problem of simultaneity can result in biased and inconsistent estimates of the effect of arrest, conviction and imprisonment on crime. If, for example, higher crime rates result in higher arrest rates, any suppression effect of arrest on crime will be hidden or obscured by the effect of crime on arrest. The same argument applies to the relationship between crime and imprisonment. To be able to detect the effect of the criminal justice system on crime, we must find some way of filtering out the effect of crime on the criminal justice system.

The other problem identified by Fisher and Nagin (1978) was *ratio bias*. When we try to explain variations in crime rates as a function of arrest rates, crime enters the numerator of the crime rate variable (crime divided by population) and the denominator of the arrest rate variable (viz. arrests divided by crimes). If there are errors in the measurement of these two variables they will show a strong negative relationship even if, in fact, arrest rates have no effect on crime (Levitt, 1997).[1] Consider two areas X and Y, for example, that have the same true crime rate C/P and the same arrest rate A/C. Suppose, however, that in X only 50 per cent of the crimes are reported to police while in area Y, 100 per cent of the crimes are reported to police. When we compare the two areas in terms their *measured* (i.e. recorded) crime and arrest rates we will find that Area X has a low crime rate (.5*C/P) and a high arrest rate (A/.5*C) while area Y has a high crime rate (C/P) and a low arrest rate (A/C). If this pattern is repeated across other areas, crime and arrest rates will be negatively correlated even if arrest actually has no effect on crime.

More recently, researchers have become concerned about another problem, known as *aggregation bias* (Levitt, 2001). Although we are interested in the effect of the criminal justice system on individual criminal behaviour, we cannot study the effect of changes in the risk of arrest, the likelihood of imprisonment or the length of imprisonment within an individual. Instead, we are forced to examine changes in these variables at an aggregate level (e.g. national trends in rates of crime and arrest). If we analyse the relationship between crime and criminal justice variables at too high a level of spatial aggregation (e.g. at a state or national level), we will fail to capture important local variation in criminal justice variables or crime or both. If this variation is correlated with the criminal justice variables being examined, it will lead to biased and inconsistent estimates of the effect of criminal justice variables on crime.

There are several ways of addressing these problems. The best defence against omitted variable bias is to identify and control for all the factors that might influence crime. When (as often happens) it is not clear what these factors are, another approach is to regress the crime rate variable on the criminal justice variables of interest as well as past (lagged) values of crime. This tactic has the advantage of ensuring that any extraneous factors influencing the crime rate are automatically controlled, even if they have not been identified and explicitly included in the regression equation.

Another defence against both omitted variable bias and simultaneity is to use a panel design. Panel designs (in the current context) involve repeated measurements of crime and criminal justice variables over time and across a set of geographic areas (panels). The focus of attention in panel studies is on changes in criminal justice variables and crime *within* each panel over time, not between panels. In effect, each area serves as its own control. This has the advantage of allowing us to ignore any factors that influence crime that are constant over time and specific to a particular area. If a panel design is used it is only necessary to include controls for time-varying factors that affect crime. Panel designs allow the researchers to sort out the temporal order of changes in criminal justice variables and crime and to remove the effects of shocks to a system (e.g. a general change in penalties) that are not directly measured (Levitt, 2001).

An alternative way of dealing with simultaneity is to find variables (known as instruments) that influence the independent variable (e.g. arrests) but are known to have no direct causal effect on the dependent variable (e.g. crime). These variables can be used in a two-stage least squares (2SLS) regression to 'identify' the effects of any criminal justice variables, that is, to separate the effects of crime on criminal justice variables from the effects of criminal justice variables on crime. Yet another approach (known as Granger analysis) is to restrict the analysis to the relationship between current values of the crime variable and past values of the criminal justice variables. This tactic exploits the fact that current values of the crime variable can only influence current or future values of the criminal justice variables. They cannot influence past values of criminal justice variables.

Two approaches can be used to cope with the ratio bias due to measurement errors. The first involves implementing an estimation method known as generalised method of moments (GMM). This method can provide consistent parameter estimates in dynamic panel data models if random transient measurement errors appear in the independent variables and lagged dependent variables. GMM can also deal with the ratio bias due to measurement errors in the dependent variable by using suitable lagged dependent variables as instruments (Bond, Hoeffler, & Temple, 2001). Alternatively, ratio bias can be minimized by using the first lag of arrest rate as the independent variable in lieu of the contemporaneous arrest rate (Levitt, 1998).

The simplest way to deal with aggregation bias is to conduct the analysis using units of spatial aggregation that are relatively homogenous with respect to the variables thought to influence crime (e.g. income, drug use, unemployment). In most cases, this will mean choosing units of spatial aggregation that are fairly low (e.g. Local Government Areas).

PAST RESEARCH

In this section we review research conducted into the effect of arrest, conviction and imprisonment on crime. The review is not intended to be comprehensive. Our aim is to illustrate the various approaches taken to address the problems mentioned above rather than to offer a comprehensive summary of the (voluminous) research conducted to date. Because much of the pre-1978 literature on the effect of arrest and imprisonment on crime was methodologically flawed, our review is limited to studies published since the NRC report. Moreover, because we are interested in the effects of the criminal justice system on crime regardless of whether the mechanism is deterrence or incapacitation, our review does not include simulation studies of incapacitation, or 'bottom-up' studies as Spelman (2000) calls them. We exclude these studies on the grounds that they only measure incapacitation effects whereas aggregate-level studies of the prison-crime relationship capture both incapacitation and deterrence effects.

Arrest and conviction

Wilson and Boland (1978) were among the first to have credibly addressed the problem of simultaneity in the context of a study on the effects of arrest. They examined the relationship between robberies and the robbery arrest ratio (robbery arrests divided by robberies) in 35 large American cities. They used police aggressiveness (as measured by the number of moving traffic violations issued) as an instrumental variable, arguing that it influenced the arrest ratio but had no direct effect on crime. Additional controls were included for the age/sex profile of each city, the percentage of the population in each city that was non-white, the unemployment rate in each city and the population density in each city.

After controlling for these factors, Wilson and Boland (1978) found a strong negative correlation between the robbery arrest ratio and rates of robbery. These findings were later confirmed by Sampson and Cohen (1988) using a larger number of cities and a different instrument (number of arrests per officer for driving under the influence of alcohol and disorderly behaviour). Unfortunately, the studies by Wilson and Boland (1978) and Sampson and Cohen (1988) were cross-sectional. Cross sectional designs are ill-suited to testing hypotheses about causal effects because it is impossible in such designs to sort out the temporal order of the events. Panel designs, as noted earlier, are much better suited to this purpose.

Greenberg and Kessler (1982) conducted one of the first panel analyses of the relationship between arrests and crime. They examined the relationship between clear-up rates and a variety of offences (murder, burglary, grand larceny, aggravated assault, robbery and auto theft) using a sample of 98 U.S. cities over the years 1964 to 1970. Their controls included population, population density, percentage of the population under the age of 18, percentage of labour force in manufacturing (an index of social status), the unemployment rate, median income, percentage of population with Spanish surnames (a proxy measure of blocked access to legitimate employment opportunities), percentage of female-headed households, percentage black residents and a dummy variable for whether the city was in the north or south of the United States. They found little evidence that clear-up rate (defined as offences 'cleared' to offences recorded by police) had any effect on any of the offences they examined. However as they themselves acknowledge, clear-up rates are not necessarily a good indicator of the risk of arrest since crimes can be cleared (i.e. offenders identified) without police being able to effect an arrest.[2]

Withers (1984) conducted the first (and one of the very few) Australian studies on the effect of the criminal justice system on crime. He used a panel dataset comprising data from the eight Australian States and Territories over the financial years 1963-64 to 1975-76. The principal dependent variables in his analysis were property and violent crime. The criminal justice measures were committals to court (a variable closely tied to the number of arrests) and the rate of imprisonment. His controls included State, income, male unemployment rate, proportion of young males, proportion completing school, proportion of time spent watching realistic violent scenes on television, proportion of European-born residents, proportion of Indigenous Australians and proportion of New Zealand born residents. Withers tested for simultaneity by comparing estimates of the effect of his criminal justice variables in an ordinary least squares (OLS) regression with the estimates obtained from a two-stage least squares (2SLS) regression. Finding no difference between these models, Withers (1984) proceeded on the basis of the OLS results, which suggested strong negative relationships between the deterrence variables (court committals and imprisonment) and crime.

If the effects of changes in arrest or punishment extend over less than a year, studies such as those by Withers (1984) and

Greenberg and Kessler (1982) may not pick up any effect. Chamlin (1988) examined the relationship between arrests and crime when the temporal unit of analysis was months rather than years. Apart from making it possible to examine the relationship between arrests and crime at lags of less than a year, the use of months as the unit of temporal aggregation helps deal with simultaneity since criminal justice responses to rising crime are likely to take place over years rather than over months. Rather than use conventional regression methods to explore the relationship between crime and arrests, Chamlin (1988) used autoregressive moving average (ARIMA) models. ARIMA models are designed to establish whether fluctuations in one time series (e.g. arrests) are correlated with later fluctuations in another time series (crime) when trends in the two series have been removed. The offences in his study were monthly counts of robbery, burglary, grand larceny and auto theft over the period January 1967 to November 1980 in two Oklahoma cities. Chamlin (1988) found a significant negative relationship between arrests and robbery one month later but no other significant effects.

D'Alessio and Stoltzenberg (1998) conducted a similar study but examined daily changes in crime rather than monthly changes. They applied a Granger analysis to estimate the lagged effect of arrests and crime. The primary data for their study consisted of trends in arrests and reported crimes in Orange County Florida over the 184 day period between July 1, 1991 and December 31, 1991. They found that the number of arrests on any given day exerted a significant but lagged effect on the number of recorded crimes. Corman and Mocan (2000) obtained similar results when they looked at the effect of arrests on crime using data on monthly arrests by the New York Police Department (NYPD) over the period January 1970 to December 1996. Rates of crime in New York (murder, assault, robbery, burglary and motor vehicle theft) varied considerably over this period. They found that the number of arrests exerted a significant lagged negative effect on robberies, burglaries and motor vehicle theft.

Other studies using different methods have found similar results. Cornwell and Trumbull (1994) conducted a 2SLS panel study of the effect of arrest probability (proxied by the arrest/reported crime ratio) on the index crime rate (viz, index crimes divided by county population) across a set of county panels in the U.S. state of North Carolina. They controlled for a wide variety of factors, including average weekly wage, level of urbanisation, percentage of the population who were young males, percentage of the population who were from an ethnic minority group, number of police per capita, likelihood of conviction, likelihood of imprisonment given conviction and sentence length. They found a significant negative relationship between the probability of arrest and the index crime rate but the effect was much smaller than that obtained in previous cross-sectional studies of the effect of arrest.

Bodman and Maultby (1997) updated and extended Withers (1984) analysis using annual data on four types of crime (robbery, burglary, motor vehicle theft and fraud) across four Australian States (Victoria, Queensland, South Australia, Western Australia and Tasmania) over the period 1982-1991. Their controls included labour force participation rate, percentage of the population aged 18-24, percentage of the population who were male in each year, percentage of the population tertiary trained, percentage of the population who identified as Aboriginal or Torres Strait Islander, percentage of the population who were born in a foreign country, population density and police expenditure per capita. Bodman and Maultby (1997) dealt with the problem of simultaneity by estimating three simultaneous equations; one linking crime to criminal justice variables plus controls, one linking crime clearance rates to crime, police numbers plus controls and a third linking the number of police per capita to the crime rate and police expenditure per capita. They found consistent evidence of a statistically significant negative relationship between crime and crime clearance rates across all crime types.

Only four studies appear to have examined whether the risk of conviction affects crime. Pyle (1984) examined the effects of conviction rate, imprisonment rate and sentence length in a regression analysis of recorded crime rates across 41 police force areas in England and Wales. Pyle (1984) found a strong negative relationship between conviction rate and crime in an analysis that controlled for the percentage of males aged 15-24, the unemployment rate and two measures of income: the rateable value of land per hectare and average adult male earnings. Pyle's analysis did not control for clear-up rate, a variable likely to be highly correlated with conviction rate. The effects attributed by Pyle (1984) to conviction rates might have been due to variation in the risk of apprehension.

Trumbull (1989), Cornwell and Trumbull (1994), and Kelaher and Sarafidis (2011) are the only studies published since the review by Blumstein et al. (1978) to have examined the joint effects on crime of the probability of conviction while controlling for the risk of arrest, the proportion of convicted offenders imprisoned and sentence length. Trumbull (1989) found a strong negative relationship between crime and both P_A and $P_{C|A}$, while controlling for a wide range of economic, social and police manpower variables. The effect disappeared, however, when the relationship between crime and P_A, $P_{C|A}$, $P_{I|C}$ and S was estimated using two stage least squares (2SLS). Cornwell and Trumbull (1994) in the panel study described earlier also found a significant negative relationship between crime and both P_A and $P_{C|A}$. Once again, however, the effects disappeared in their two stage least squares (2SLS) analysis. The authors of both studies gave reasons for believing that there was no problem of simultaneity between crime and criminal justice variables in their studies. However, as Kelaher and Sarafidis (2011) point out, even if reverse causality were not present in their data, the probability of arrest in their models is endogenous by construction because the numerator in the dependent variable (number of crime incidents) is the denominator in the probability of arrest.

As noted earlier, Kelaher and Sarafidis (2011) carried out one of the most sophisticated attempts to date to examine the effects of arrest and conviction on crime. They identified a number of weaknesses in past studies, including failure to include all relevant deterrence variables (i.e. P_A, $P_{C|A}$, $P_{I|C}$ and S), aggregation bias and failure to deal properly with the problem of simultaneity. To overcome these problems they constructed measures of crime, P_A, $P_{C|A}$, $P_{I|C}$, S, income and unemployment across a panel of 153 New South Wales Local Government Areas (LGAs) over the 13 year period from 1995/6 to 2007/8. They then analysed the effects of P_A, $P_{C|A}$, $P_{I|C}$, S on violent and non-violent crimes, while controlling for income and unemployment rate using the GMM procedure mentioned earlier. The Kelaher and Sarafidis (2011) model was dynamic, that is, it assumed that the current crime rate is affected by its past values and permitted estimates of the short and long run effect of changes to the deterrence variables. We will have occasion to review the results of their analysis of the effects of imprisonment and sentence length shortly. For now it suffices to note that P_A and $P_{C|A}$ were both found to have a significant negative short- and long-term relationship with violent and non-violent crimes.

Imprisonment

In his review of research on crime and imprisonment, Spelman (2000) identified four studies (Cappell & Sykes, 1991; Devine, Sheley, & Smith, 1988; Levitt, 1996; Marvell & Moody, 1994) which had overcome most of the methodological problems described earlier. The first (Devine et al., 1988) used annual national time series data on homicide, robbery and burglary in the United States between 1948 and 1985. The authors employed 2SLS to overcome problems of simultaneity between crime and imprisonment. Their estimates indicated that a one per cent increase in the imprisonment rate would produce an average 2.2 per cent reduction in crime. Spelman (2000) described this very large effect as 'deviant', suggesting that it might have been attributable to poor instrument selection. Devine et al. (1988) did not report the instruments they used but it is worth noting that their controls were fairly limited.

The remaining three studies produced much smaller and more tightly clustered estimates of the effect of prison on crime. Cappell and Sykes (1991) conducted a 2SLS analysis of national level data for the period 1933-1985; finding that prison had a strong negative association with their overall measure of crime (which combined rates of homicide, assault, rape, robbery, burglary and motor vehicle theft). The overall elasticity of crime with respect to prison in their study was -0.26. Rather than use 2SLS to deal with the problem of simultaneity, Marvell and Moody (1994) applied a Granger test to crime and imprisonment data pooled over 49 States across 19 years. Their results suggested an average elasticity of crime with respect to imprisonment of -0.16. Levitt's (1996) study is arguably the most convincing effort to deal with the problem of simultaneity. He used civil litigation to reduce prison overcrowding as an instrument to identify the effect of prison on crime and obtained an estimate of the elasticity of crime with respect to prison of -0.31.

One concern with studies examining the effect of imprisonment in isolation from other criminal justice variables is that the imprisonment rate variable may be capturing the effect of other aspects of the criminal justice system, such as the risk of arrest or the risk of conviction (von Hirsch, Bottoms, Burney, & Wikstrom, 1999). Since imprisonment rates are likely to be correlated with arrest and conviction rates, the effects that appear to be due to prison may in part or in whole be attributable to arrest and/or conviction. The use of prison population or imprisonment rate to measure the effects of imprisonment also makes it impossible to tell whether offenders are more responsive to changes in the risk of imprisonment or changes in the average length of sentence. It is of some interest to know, therefore, whether the deterrent effect of imprisonment on crime remains significant after controls are introduced for risk of apprehension and/or risk of conviction, and how imprisonment probability and sentence length compare in terms of their effect on crime.

Surprisingly few studies have examined the joint effects of P_A, $P_{C|A}$, $P_{I|C}$ and S on crime. The studies that have are Kelaher and Sarafidis (2011), Cornwell and Trumbull (1994), Trumbull (1989) and Pyle (1984). Trumbull (1989) and Pyle (1984) are both vulnerable to the criticisms raised in the NRC report (Blumstein et al., 1978), which leaves us with Cornwell and Trumbull (1994), and Kelaher and Sarafidis (2011).

In their OLS analysis, Cornwell and Trumbull (1994) found that P_A, $P_{C|A}$, $P_{I|C}$ were significant but not S. In the 2SLS analysis none of these factors (or S) exerted a significant effect on crime. Cornwell and Trumbull (1994) favoured their OLS regression results on the grounds that they found no evidence of simultaneity. As noted earlier, however, even if simultaneity were not present in the Cornwell and Trumbull (1994) data, the risk of arrest is endogenous by construction because crime frequency appears in the numerator of their dependent variable and the denominator of their variable measuring risk of arrest.

Given the care taken by Kelaher and Sarafidis (2011) to deal with this issue and the other methodological problems described earlier, their study and its results deserve particular attention. They found significant negative (deterrent) effects for P_A and

$P_{C|A}$ in their violent crime model, and for each of P_A, $P_{C|A}$, $P_{I|C}$ and S in their non-violent crime model. The ordering of these effects for non-violent crime matched that predicted by Ehrlich's (1973) theory of crime. The short-run elasticities for P_A and $P_{C|A}$ in the violent crime model were estimated to be -0.26 and -0.27, respectively. In the non-violent crime model, the short-run elasticities were estimated to be -0.92, -0.58, -0.18 and -0.21 for P_A, $P_{C|A}$, $P_{I|C}$ and S, respectively. As expected, their long run effects were stronger than short run effects. At face value, then, the Kelaher and Sarafidis (2011) results provide the best evidence to date that each of P_A, $P_{C|A}$, $P_{I|C}$ and S has a suppression effect on crime, with the strongest effects being carried by risk of arrest (P_A). However, although Kelaher and Sarafidis (2011) go to considerable lengths to avoid the pitfalls of many earlier studies, their study has four significant limitations. In the next section we discuss these limitations and how we propose to overcome them in the current study.

THE CURRENT STUDY

The first limitation with the Kelaher and Sarafidis (2011) study concerns their measure of non-violent crime, which included both crimes recorded by police that have been reported to them (reported offences) and crimes recorded by police when they went out looking for them (detected offences). Trends in 'detected' offences are often a poor guide to trends in crime. Trends in arrests for drink-driving, for example, probably tell us more about the police activity in relation to this offence than about the actual incidence of drink-driving. The inclusion of detected offences in the Kelaher and Sarafidis (2011) measure of 'non-violent crime' raises questions about the extent to which that measure reflected variations in crime as opposed to variations in police activity.

A second related problem is that the arrest rate for detected offences (defined as the number of persons arrested divided by the number of recorded offences) does not provide a meaningful measure of the risk of apprehension. As already noted, detected offences are, in the great majority of cases, recorded in the context of an arrest. As a result, the arrest rate for detected offences is always close to 100 per cent even when only a small fraction of the actual offences in question result in apprehension and arrest. In short, Kelaher and Sarafidis's (2011) analysis of non-violent crime mismeasures both the crime variable and a critical criminal justice variable, namely the probability of arrest. The mismeasurement is substantial. Over the period from 1996 to 2008, nearly 30 per cent of the offences on average included in their category of non-violent crime (including drug offences, driving offences, against justice procedures, disorderly conduct, betting and gaming offences, liquor offences, pornography offences, prostitution offences, receiving or handling stolen good, transport regulatory offences, and other offences) involve detected as opposed to reported offences.

The third problem concerns their view that a fully specified model should include a variable that reflects the probability of conviction given arrest. This notion, derived from Ehrlich (1973), assumes that offenders are differentially sensitive to the likelihood of capture and the likelihood of conviction given capture. This assumption would make sense if all or most offenders pleaded not guilty and there was a realistic prospect of acquittal. In practice, the vast majority of offenders who are arrested and charged by police either plead guilty or are found guilty. In New South Wales (NSW), for example, 63 per cent of offenders in the Higher Criminal Courts plead guilty while in the Local Court (where more than 90 per cent of cases are dealt with), 76 per cent of offenders plead guilty (NSW Bureau of Crime Statistics and Research, 2011a). When guilty pleas are added to persons found guilty following a not-guilty plea, 92 per cent of those charged with criminal offences end up convicted. From the vantage-point of the offender, conviction following arrest can be taken as a given.

The fourth problem in the Kelaher and Sarafidis (2011) study is a significant omitted variable problem. Around Christmas 2000 (i.e. during the period covered by the Kelaher and Sarafidis study) Australia experienced a severe heroin shortage (Degenhardt, Day, & Hall, 2004). The shortage resulted in a steep rise in heroin prices and a sharp fall in the purity of heroin, particularly in NSW (Degenhardt et al., 2004; Weatherburn, Jones, Freeman & Makkai, 2003). The fall in heroin use appears to have triggered a sharp fall in heroin related property crime (Degenhardt et al., 2004; Moffatt, Weatherburn, & Donnelly, 2005). To make matters even more complicated, over the period in which heroin use declined, the proportion of suspected offenders refused bail and the proportion of convicted offenders given sentences of imprisonment increased (Lulham & Fitzgerald, 2008). It is possible, then, that the variable measuring the probability of imprisonment given conviction in Kelaher and Sarafidis (2011) was actually functioning as a proxy for other unmeasured variables associated with changes in the consumption of heroin.

Our aim in this study is to build on the work carried out by Kelaher and Sarafidis (2011) by addressing the problems just raised. We address the first two problems by restricting our analysis to violent and property crime. We explicitly exclude those categories of crime that are likely to be reflective of police activity. We address the third problem by focusing on just three criminal justice variables: risk of arrest, risk of imprisonment given arrest and sentence length. We address the fourth problem by including proxy measure of heroin use in our analysis. The central questions of interest in the current study are:

1. Does a higher risk of arrest reduce rates of property and/or violent crime?
2. Does a higher risk of imprisonment reduce rates of property and/or violent crime?

3. Do longer sentences reduce rates of property and/or violent crime?

We examine the relationships between crime and criminal justice variables using a panel of 153 Local Government Areas (LGAs) in NSW from 1996 to 2008. The Australian Standard Geographic Classification (ASGC) defines the LGA as the third lowest level of aggregation, following the Census Collection District (CD) and Statistical Local Area (SLA) (ABS, 2006). We explicitly control for two factors (income and heroin use) that are known from previous research (Moffatt et al., 2005) to have influenced property crime over the period covered by this study. An attempt was also made to control for unemployment but for reasons explained below, unemployment was not included in the final model.

We follow Kelaher and Sarafidis (2011) and analyse the data using GMM. However, instead of adopting the system GMM, we apply the first-differenced GMM by including the lagged values of the dependent variable and independent variables as instrumental variables. As Kelaher and Sarafidis (2011) point out, the GMM approach mitigates the risk of omitted variables, simultaneity and ratio bias and it avoids full specification of the serial correlation and heteroskedasticity properties of the error, or indeed any other distributional assumptions. Our model specifies all explanatory variables as endogenous.

Because the trends in property and violent crime in NSW (and Australia) between 1996 and 2008 have been quite different (see Figures 1 and 2), separate analyses were conducted for property crime and violent crime. The panel data for the study consisted of 10 variables across 153 LGAs in NSW. Each variable corresponds to a time series containing thirteen yearly counts during the period from 1996 to 2008. These 10 variables comprised the (property or violent) crime rate, the (property or violent) arrest rate, the (property or violent) imprisonment rate, the (property or violent) average sentence length, average wage and salary income and the arrest rate for use or possession of narcotics (to account for the effect of the heroin shortage). The crime rate variables are the dependent variables of interest. The details and the sources of these 10 variables are described below.

METHOD

DEPENDENT VARIABLES

The two dependent variables were the rate of property and violent crimes, which were obtained by dividing the number of property and violent crime incidents recorded by police with the total population in each LGA. The denominator, the total population in a LGA, was the estimated resident regional population, an official estimate of the Australian regional population in the middle of each year recorded by the ABS (ABS, 2007a; 2011b). For the numerator, a criminal incident in any LGA was defined as an activity detected by or reported to police which involved the same offender(s) and victim(s), occurred within the LGA, during one uninterrupted period of time, fell into one offence category and fell into one incident type (for example, 'actual', 'attempted', 'conspiracy').

Property crime was defined as any incident of robbery without a weapon, robbery with a firearm, robbery with a weapon not a firearm, break and enter dwelling, break and enter non-dwelling, motor vehicle theft, stealing from motor vehicle, stealing from retail store, stealing from dwelling, stealing from person, stock theft, other theft and fraud. Violent crime was defined as any incident of murder, non-domestic violence related assault, domestic violence related assault, robbery without a weapon, robbery with a firearm, robbery with a weapon not a firearm, sexual assault, indecent assault or act of indecency, or other sexual offence. Note that robbery was counted as both a property and violent offence because it is an acquisitive crime that, by definition, involves actual or threatened violence. Double-counting these offences is unlikely to have any substantive impact on the analyses because robbery averaged 12 per cent of recorded violent incidents and only three per cent of recorded property offences over the period.

INDEPENDENT VARIABLES

Rate of arrest

The arrest rate was defined as the number of persons of interest (POIs) who were proceeded against to courts divided by the number of crime incidents recorded by police at each LGA. A POI is an alleged offender recorded by police in connection with a criminal incident. POIs are not a count of unique offenders. Where an individual is involved in multiple criminal incidents throughout the year they will appear as a POI multiple times. Correspondingly, no person of interest information will be recorded for criminal incidents in which there is no known suspect. This is very common among incidents of property crime that have a low clear-up rate.

Rate of imprisonment

The imprisonment rate was defined as the number of offenders receiving prison sentences for their principal offence (property or violent) divided by the number of POIs who were proceeded against to courts for property and violent offences. The principal offence at a court appearance was defined as that which attracted the highest penalty. The number of persons who received a prison sentence was counted at the LGA where that person lived.

Average sentence length

The average sentence length was defined as the mean length of time in months of the non-parole period (or the total term if no non-parole period was specified) imposed on the offender for the principal offence. It was obtained by dividing the total length of non-parole periods imposed on offenders within an LGA by the number of offenders sentenced to imprisonment within that LGA. This was calculated for offenders sentenced to prison for property and violent crimes separately.

Income

The income variable at each LGA referred to the average wage and salary income for all persons who resided in that LGA, who were aged 15 years and over, who submitted an individual income tax return and for whom wage and salary income was the main source of income for the financial year. This variable was a measure of the *nominal* income, not adjusted for the effects of inflation on purchasing power. The effects of inflation are captured in the year fixed effects (see Equation (1)). The definition did not account for whether wage and salary earners work on a full-time or part-time basis.

It should be noted that the definition of wage and salary income varied slightly in different years across the 1996 to 2008 period. For example, attributed personal services income was included in the wages and salaries income after 2001; and government pensions and allowances were excluded after 2001. These changes, which were consistent across all LGAs, were accounted for by specifying a fixed year effect in the model (see Equation (1)).

Unemployment

Economic theories of crime generally predict that crime rates will be higher during periods of unemployment. An effort was therefore made to control for unemployment. The ABS defines the unemployment rate as the percentage of unemployed persons in the labour force at each LGA. The number of unemployed persons and the labour force have been collected from the Labour Force Survey conducted by the ABS on a monthly basis since 1978. The labour force referred to the number of employed persons aged 15 years or older and involved in paid employment or self-employment. Unemployed persons were those aged 15 years or older who were not employed but had actively looked for full-time or part-time work and were currently available for work. Note that the numbers of unemployed and employed persons were smoothed by averaging the data over four quarters in each year to dampen the variability in the original quarterly data.

Proxy variable for heroin use

The arrest rate for use or possession of narcotics was used as a proxy variable for heroin use in Australia. The control was included because of past evidence linking the rise and fall in burglary and robbery in NSW to the rise and fall in heroin use (Donnelly, Weatherburn, & Chilvers, 2004; Moffatt et al., 2005). It was defined as the number of incidents for use or possession of narcotics divided by the total population in each LGA (expressed as a rate per 1,000 residents).

Data sources

The panel data were obtained from different sources including the NSW Police Force's Computerised Operational Policing System (COPS), the Bureau's Reoffending Database (ROD), and the ABS.

First, the number of criminal incidents for property crime, violent crime and use or possession of narcotics, and the number of alleged offenders proceeded against to courts were sourced from COPS. COPS holds a unique record of all criminal incidents reported to, or detected by, police in NSW.

Second, information on the number of offenders given prison sentences, by principal offence, and information on the mean length of non-parole periods was sourced from the Reoffending Database (ROD), which is maintained by the NSW Bureau of Crime Statistics and Research. This database holds records on all court appearances by unique offenders between 1994 and the present time (Hua & Fitzgerald, 2006).

The estimated resident population and the average wage and salary income, by LGA, were both obtained from the ABS website. The former is available in the *Regional Population Growth, Australia* report (ABS, 2007a; 2011b) while the latter is available from the *Regional Wage and Salary Earner Statistics, Australia* report prior to 2004 (ABS, 2003; 2007b) and the *Regional Wage and Salary Earner Statistics for Small Areas* report in and after 2004 (ABS, 2010). However, it should be noted that the ASGC is updated on an annual basis. Over the period from 1996 to 2008, continuous modifications have been made to the boundaries and names of some LGAs. Also, a few LGAs have been merged over time, which reduced the total number of LGAs in NSW from 178 in 1996 to 153 in 2006. In this analysis, the ASGC 2006 was adopted (ABS, 2006). To ensure the conformity of the data to the ASGC in 2006, any changes in the number and boundary of the LGAs were accounted for using a series of concordance tables.

Lastly, the number of unemployed persons and the labour force were purchased from the Labour Market Research and Analysis Branch of the Department of Education, Employment and Workplace Relations (DEEWR). However, as they were recorded at the level of Statistical Local Area (SLA), a subdivision of a LGA, the SLA data were aggregated to the LGA level and the resulting LGA data was then mapped according to the AGSC 2006.

ANALYSIS

Modelling

A dynamic panel data model with fixed LGA and time effects was adopted to explore the impact of the criminal justice system and some economic factors on property and violent crimes. Efforts were made to include unemployment as a control but, in the presence of income, the effects of unemployment rate were generally small and non-significant. The inclusion of both variables in the model also created problems of multicollinearity. When income was replaced with unemployment, the unemployment variable was non-significant in both property and violent crime models. Given this pattern of findings and the fact that income is probably capturing the effects of unemployment, unemployment was dropped from the modelling.

The model specification was given as follows:

$$\ln crm_{it} = a \ln crm_{i,t-1} + b_1 \ln arr_{it} + b_2 \ln impr_{it} + b_3 \ln avesen_{it} + b_4 \ln income_{it} + b_5\, narr_{it} + u_i + d_t\, yr_t + e_{it}$$

$$\text{where } Corr(e_{it}, e_{kt}) = 0 \text{ for } i \neq k. \qquad (1)$$

In Equation (1), the dependent variables denoted by crm_{it} are property or violent crime rates, the subscript i represents the i^{th} LGA (i=1,…,153) while t stands for the t^{th} year (t=1,…,13). The explanatory variables in the dynamic models include the lagged crime rates in the previous month ($crm_{i,t-1}$), arrest rate (arr_{it}), imprisonment rate ($impr_{it}$), average sentence length ($avesen_{it}$), income (in thousands) ($income_{it}$), and arrest rate for use or possession of narcotics ($narr_{it}$). The symbols $a, b_1, \ldots, b_5$ are the coefficients for these explanatory variables and the yr_t are year dummies such that yr_t = 1 at year t and 0 otherwise.

The dependent variables and all explanatory variables except arrest rate for use or possession of narcotics were natural logged to alleviate the problem caused by the skewed distributions of some variables, such as average sentence length. Another advantage of doing so was to simplify the calculation of the percentage change of crime rates for a one percent change in each explanatory variable (elasticity). The arrest rate for use or possession of narcotics was not logged as the time series contained a substantial amount of zeros. The dynamic model also included the fixed LGA effects denoted by u_i that may be correlated with other explanatory variables. These fixed LGA effects can account for some time-invariant LGA characteristics which were omitted in the model but had an impact on crime rates over years. Ignoring these LGA effects may lead to an aggregation bias and result in misleading inferences. Furthermore, the fixed year effects d_t captured the common variations in crime rates across LGAs and removed the correlation amongst LGAs. For instances, the use of nominal income or real income did not alter the coefficients of the explanatory variables because the year effects absorbed the effects of inflation on nominal income across all LGAs in NSW. The term e_{it} represented the idiosyncratic disturbances of each observation.

Estimation

To estimate the dynamic panel data model, the first-differenced GMM procedure was adopted. As explained earlier, this procedure mitigates the problems caused by having multiple endogenous explanatory variables in the model and the problems caused by having the numerator of the dependent variable (crime rate) as the denominator of the explanatory variable (arrest rate). Under the GMM approach, the idiosyncratic disturbances e_{it} were assumed to be uncorrelated across LGAs, i.e., $Corr(e_{it}, e_{kt})=0$ for $i \neq k$. Furthermore, first-differenced GMM was suitable for our analysis because it is particularly designed for a model: (1) which may be dynamic, with current values of the dependent variable (crime rate) influenced by past values; (2) where there is a linear functional relationship between the dependent variable and the explanatory variables; (3) with fixed individual effects; (4) with relatively few time periods and a large number of panels in the data; (5) with heteroskedasticity and autocorrelation within LGA in the idiosyncratic disturbances; and (6) with the dependent variable time series being stationary (i.e. no unit root). Most of these properties are implicit given the nature of the crime data utilised in the analyses. Some of these properties are also testable and these will be verified in the next section on diagnostic checking.

In first-differenced GMM, the lagged endogenous explanatory variables were used as instruments in the first-difference equation:

$$\Delta \ln crm_{it} = a\, \Delta \ln crm_{i,t-1} + b_1 \Delta \ln arr_{it} + b_2 \Delta \ln impr_{it} + b_3 \Delta \ln avesen_{it} + b_4 \Delta \ln income_{it} + b_5 \Delta narr_{it} + d_t \Delta yr_t + \Delta e_{it}\,. \qquad (2)$$

The appropriate number of lags for the instruments was determined by using Arellano and Bond's (1991) test of autocorrelation in the differenced idiosyncratic disturbances. An insignificant autocorrelation in the idiosyncratic disturbances in differences at lag $l+1$ indicated an insignificant autocorrelation in the idiosyncratic disturbances in level at lag l, and hence endogenous explanatory variables starting from lag $l+1$ and so on can be used as instruments. In our models, variables up to lag 3 only were used as instruments and therefore test of autocorrelation was conducted up to lag 3. It was crucial that all instruments should be exogenous such that the model was correctly specified. Two-step GMM estimators were used with Windmeijer (2005) finite-sample correction to prevent downward bias in the standard errors of the estimates. Robust Windmeijer-corrected standard errors were reported which were valid under arbitrary forms of heteroskedasticity and autocorrelation in the idiosyncratic disturbances. All analyses were carried out using Stata v11.1.

Diagnostic checking

Properties (1), (2), and (4) from the previous section were implicitly met given the nature of the panel data employed in this study. The first-differenced GMM is a more conservative method of parameter estimation than system GMM. Nevertheless a number of diagnostic tests were undertaken to ensure that first-differenced GMM was appropriate given the remainder of the criteria mentioned in the previous section. We first fitted a dynamic model (given by Equation (1)), which linearly related the dependent variable with the lagged dependent variable and other explanatory variables, using the within-regression estimator for both property and violent crimes. The lagged dependent variable was significant in both models (p<.001) and helped account for the omitted variable bias.

Two F-tests were then performed to test if the fixed LGA effects and fixed year effects as a whole were significant (criterion 3 in the previous section). The small p-values (p<.001) on the F-test (H_0: all fixed effects are zero) for both property and violent crimes suggested the inclusion of these fixed effects in the dynamic models was appropriate. The Wooldridge (2002) test, developed for the within-regression estimator, was conducted to test for the presence of autocorrelation in the idiosyncratic disturbances (criterion 5 in the previous section). The small p-values (p<.001) on the Wooldridge test (H_0: no autocorrelation in the idiosyncratic disturbances at lag 1) indicated the presence of autocorrelation for both property and violent crimes and supported the use of GMM. While no tests were conducted to identify whether there was heteroskedasticity in the disturbance terms, it was assumed that this was the case. First-differenced GMM allows for this heteroskedasticity.

Finally, the Im-Pesaran-Shin test for unit roots in panel data (Im, Pesaran, & Shin, 2003) was used to test if the dependent variable was non-stationary (criterion 6 in the previous section - presence of unit roots). This test is particularly designed for panel data with fixed time periods and a large number of panels. It also allows for heterogeneous variances across panels. The test was performed using xtunitroot which was built-in in Stata v11.1 and the option 'demean' mitigated the impact of cross-sectional dependence on the test statistic. The null hypothesis assumed that all series were non-stationary against the alternative that at least one series in the panel was stationary. Both property and violent crime rates were verified to be stationary (p<.001) and these suggested the appropriateness of using first-differenced GMM instead of system GMM.

Expectations

Since our primary interest is to investigate the impact of arrest, imprisonment and sentence length on crime after other important factors have been controlled, the key parameters of interest are the coefficients $b_1, \dots, b_3$. A negative coefficient on b_1, b_2, and/or b_3 would indicate that whenever the arrest rate, imprisonment rate and/or average sentence length go up, property and violent crime rates fall. We therefore expect negative coefficients on $b_1, \dots, b_3$. Because the attractions of crime increase as income decreases, we expect a negative coefficient on b_4. Because past research indicates that crime rates go up with the level of heroin dependence, we expect a positive coefficient on b_5. The coefficient a determines the speed of adjustment in crime rates. We expect this to be positive because we expect an increase in crime during one period to extend into the adjacent period. The smaller the value of a, the faster crime rates return to their equilibrium levels. The parameters b_j, $j=1,\dots,4$ represent the short-run elasticities between independent and dependent variables, that is, the percentage change in crime rates associated with a one per cent instantaneous change in one of the explanatory variables while holding the rest constant. If the one per cent change in the explanatory variable is sustained permanently, the total effect on crime rates, also known as the long-run elasticity (E_{LR}) is given by:

$$\mathrm{E_{LR}} = \frac{b_j}{1-a}, \quad j = 1, \dots, 4. \tag{3}$$

And the standard error of the long-run elasticity is approximated by (Kelaher & Sarafidis, 2011):

$$SE(\mathrm{E_{LR}}) = \sqrt{\frac{b_j}{(1-a)^2} Var(b_j) + \frac{b_j^2}{(1-a)^4} Var(1-a) + \frac{2b_j}{(1-a)^3} Cov(b_j, 1-a)},$$

$$j = 1, \dots, 4. \tag{4}$$

The long-run effect is expected to be larger than the short-run effect because it takes time for changes in law enforcement policies to exert their effects. Note that the series of narcotics arrest rates was not naturally logged due to the presence of the large number of zero observations. The short-run and long-run estimates therefore reflected, respectively, the percentage change in the crime rates per unit instantaneous and permanent change in narcotics arrest rates.

RESULTS

Before presenting the results of the analysis we present descriptive data on the variables included in the study and examine their cross-correlations. Figure 1 displays the overall trends in crime, arrest and imprisonment rates for property crime, aggregated across all 153 LGAs in NSW from 1996 to 2008.

The property crime rate rose gradually from 1996 to 2001 and then began to fall sharply until around 2005, when the decline began to slow down. The property crime arrest rate peaked in 1996, was relatively stable until 1999 and then declined sharply in 2000 and 2001 before stabilising again. The imprisonment rate for property crime rose when the property crime rate rose from 1996 to 2001. After 2004, the imprisonment rate declined gradually with the crime rate but rebounded almost to its peak level in 2008. The arrest rate for use or possession of narcotics doubled between 1996 and 1998, remained stable for a year and then declined dramatically between 1999 to 2001, before slowly declining to level off around 2006 to 2008.

Figure 2 displays the overall trends of the rates of crime, arrest and imprisonment for violent crime aggregated across all 153 LGAs in NSW from 1996 to 2008. Inspection of Figure 2 shows that the violent crime rate grew moderately from 1996 to 2001 but fell slightly between 2001 and 2004 and flattened out afterwards. The change was faint compared with that of the property crime rate. On the other hand, the arrest rate decreased gradually from 1996 to 2004, trended upwards slightly until 2007 and remained stationary in 2008. The imprisonment rate for violent crime remained quite stable over the thirteen-year period. It rose from 0.044 in 1996 to 0.051 in 2008 with little fluctuation.

Table 1 summarises the descriptive statistics of all the ten variables for both property and violent crimes averaged over 153 LGAs in NSW and over the period from 1996 to 2008.
As expected, the crime rate was higher for property crime while the arrest rate was much lower compared with that of violent crime. The imprisonment rate was a little higher for property crime but the median sentence length for violent crime was much higher than that for property crime. While the average income may appear quite low ($33,800), this estimate includes part-time

Figure 1. Rates of crime, arrests and imprisonments for property crime and rate of arrests for use/possess narcotics aggregated across 153 LGAs in NSW, 1996 to 2008

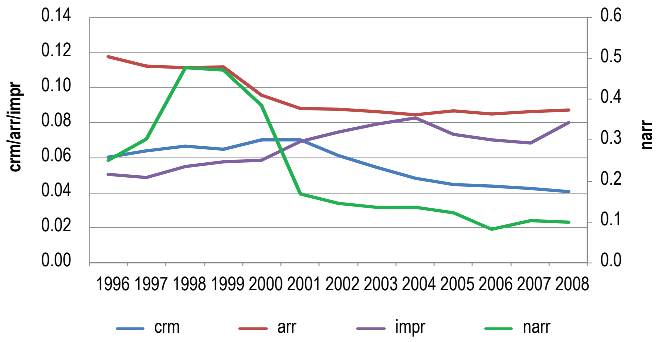


Figure 2. Rates of crime, arrests and imprisonments for violent crime and rate of arrests for use/possess narcotics aggregated across 153 LGAs in NSW, 1996

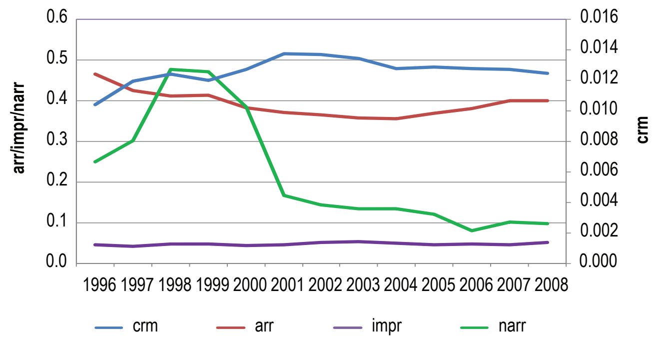


Table 1. Descriptive statistics for study variables aggregated across LGAs and years

Variable	Crime type	Mean	Standard deviation	10th percentile	90th percentile
Crime rate	Property	0.048	0.030	0.023	0.076
	Violent	0.015	0.014	0.006	0.021
Arrest rate	Property	0.117	0.061	0.058	0.192
	Violent	0.469	0.140	0.299	0.663
Imprisonment rate	Property	0.075	0.090	0.021	0.130
	Violent	0.055	0.050	0.020	0.097
Income ($ '000)		33.8	9.573	25.12	44.10
Arrest rate for use or possession of narcotics (rate per 1,000 pop.)		0.097	0.333	0.000	0.215
		Median	**IQR**	**10th percentile**	**90th percentile**
Average sentence length^ (month)	Property	8.667	6.000	3.641	15.44
	Violent	14.3	16.8	4.170	41.92

^ Median and interquartile range (IQR) were reported for average sentence length because its distribution is highly skewed with large outlying observations.

Table 2. Contemporaneous correlation of crime rate and explanatory variables for property crime

	ln crime rate	ln arrest rate	ln imprisonment rate	ln average sentence length	ln income	narcotics arrest rate
ln* crime rate	1.000					
ln arrest rate	-0.014	1.000				
ln imprisonment rate	-0.180	-0.256	1.000			
ln average sentence length	0.098	-0.191	0.102	1.000		
ln income	-0.017	-0.563	-0.099	0.282	1.000	
narcotics arrest rate	0.315	-0.005	0.039	0.057	-0.041	1.000

* 'ln' refers to natural logged
Note. n=1599

Table 3. Contemporaneous correlation of crime rate and explanatory variables for violent crime

	ln crime rate	ln arrest rate	ln imprisonment rate	ln average sentence length	ln income	narcotics arrest rate
ln* crime rate	1					
ln arrest rate	0.235	1				
ln imprisonment rate	0.028	0.068	1			
ln average sentence length	-0.03	-0.197	0.078	1		
ln income	-0.286	-0.574	-0.253	0.117	1	
narcotics arrest rate	0.099	-0.16	-0.037	0.131	-0.022	1

* 'ln' refers to natural logged
Note. n=1680

employees. The arrest rate for use or possession of narcotics was estimated to be 0.097 arrests per 1,000 residents.

An adequate dynamic panel data model should be free of multicollinearity and correctly specified with exogenous instruments. Tables 2 and 3 show the contemporaneous correlations between all pairs of variables within the property and violent crime models, respectively.

The correlation matrix for property crime (Table 2) indicated a moderate correlation between arrest rate and income (-0.563) but the correlations between these two variables and other variables in the model was comparatively different. Table 3 shows that there was also a significant negative correlation between the violent crime arrest rate and income (-0.574). Once again, however, these variables were differently correlated with other variables in the violent crime model. Multicollinearity is therefore unlikely to be a significant problem in either of the property crime or violent crime models.

Table 4 shows the parameter estimates and standard errors for both the property and violent crime models. The large p-values on Hansen's (1982) test indicated that both models were correctly specified with exogenous instruments. The large p-values on Arellano and Bond's test on the differenced idiosyncratic disturbances at lag 2 and lag 3 verified that there was no autocorrelation in the idiosyncratic disturbances in levels at lag 1 and lag 2 and therefore lagged explanatory variables and lagged dependent variables starting from lag 2 can be used as instruments.

The substantive results in Table 4 can be summarized as follows. The arrest rate and imprisonment rate each have significant negative relationships with both property and violent crime, over both the short and long run. Variations in the average sentence impose no short-run or long-run effects on property crime or violent crime. The effects of arrest rate are generally stronger than those of imprisonment rate, however the differences are insignificant for both crimes, as shown at the bottom of Table 4. In terms of long-run (equilibrium) effects, a 1 per cent increase in the arrest rate produces a .135 per cent reduction in property crime and a .297 per cent reduction in violent crime. By comparison, a 1 per cent increase in imprisonment rate produces a .115 per cent reduction in property crime and a .170 per cent reduction in violent crime. The proxy heroin use measure and the income variable are also significant. As expected, higher rates of heroin use and lower levels of income are associated with higher rates of both property and violent crimes. The fact that heroin use appears to affect violent crime might appear surprising but

Table 4. Estimates and standard errors of short-run and long-run elasticities of dynamic panel data model for property and violent crimes

		Property						Violent					
Elasticity		Short-run			Long-run			Short-run			Long-run		
Variables	**Coefficient**	Estimate	SE	p	Estimate	SE	p	Estimate	SE	p	Estimate	SE	p
Lagged crime rate	a	0.241*	0.085	.005	-	-	-	0.370*	0.080	<.001	-	-	-
Arrest rate	b1	-0.103*	0.045	.022	-0.135*	0.061	.027	-0.187*	0.068	.006	-0.297*	0.096	.002
Imprisonment rate	b2	-0.087*	0.035	.013	-0.115*	0.047	.014	-0.107*	0.025	<.001	-0.170*	0.046	<.001
Average sentence length	b3	0.022	0.027	.420	0.028	0.036	.430	0.015	0.014	.265	0.024	0.022	.269
Income	b4	-1.438*	0.404	<.001	-1.894*	0.464	<.001	-0.920*	0.359	.010	-1.460*	0.522	.005
Arrest rate for use or possession of narcotics^	b5	0.049*	0.020	.016	0.064*	0.027	.016	0.053*	0.024	.028	0.083*	0.037	.026
Year 1997	d1	-0.239	0.150	.112	-	-	-	-0.350*	0.139	.012	-	-	-
Year 1998	d2	-0.130	0.133	.328	-	-	-	-0.325*	0.122	.008	-	-	-
Year 1999	d3	-0.102	0.118	.385	-	-	-	-0.312*	0.108	.004	-	-	-
Year 2000	d4	-0.007	0.100	.941	-	-	-	-0.243*	0.097	.012	-	-	-
Year 2001	d5	0.044	0.084	.603	-	-	-	-0.154	0.080	.052	-	-	-
Year 2002	d6	-0.006	0.069	.926	-	-	-	-0.118	0.065	.071	-	-	-
Year 2003	d7	-0.016	0.057	.782	-	-	-	-0.100	0.052	.054	-	-	-
Year 2004	d8	-0.123*	0.061	.043	-	-	-	-0.170*	0.060	.005	-	-	-
Year 2005	d9	-0.120*	0.045	.008	-	-	-	-0.107*	0.046	.020	-	-	-
Year 2006	d10	-0.073*	0.035	.036	-	-	-	-0.070*	0.032	.028	-	-	-
Year 2007	d11	-0.024	0.023	.299	-	-	-	-0.037	0.020	.065	-	-	-
Year 2008	d12	dropped	-	-	-	-	-	dropped	-	-	-	-	-
Tests		p-value						p-value					
Hansen's test		0.331						0.659					
Arellano and Bond's test	Lag 1	0.000						0.000					
	Lag 2	0.141						0.918					
	Lag 3	0.895						0.643					
Chi-square test H0: b1=b2		0.698						0.267					

* indicate significance at 5 per cent level using two-tail tests
^ Arrest rate for use or possession of narcotics was not logged because most LGAs had zero incident rate. Thus, the estimate b7 gives the percentage change in crime rate per unit instantaneous change in arrest rate for use or possession of narcotics. The long-run effect refers to the percentage change in crime rate per unit permanent change in arrest rate for use or possession of narcotics.

it should be remembered that, for the purposes of the present study, robbery is classified as both a property and violent crime. Robbery was strongly affected by the heroin shortage (Moffatt et al., 2005).

There is one other point to note about Table 4. Firstly, the significant positive estimate of the lagged crime rate for the crime rate variables indicate that changes in crime tend to persist. The long-run effect of arrest, imprisonment and income on crime is further demonstrated in Figures 3 and 4. These figures display the dynamic path of the change in property and violent crime rates following a one per cent increase in arrest rate, imprisonment rate and income. The slopes of the curves for arrest and imprisonment on property crime (Figure 3) tend to flatten out, or reach a point of equilibrium after approximately 1.8 years. The slopes for arrest and imprisonment on violent crime (Figure 4), on the other hand, tend to take longer to reach the point of equilibrium (around 2.5 years for both). Put another

Figure 3. Elasticities of arrest, imprisonment and income on property crime in NSW

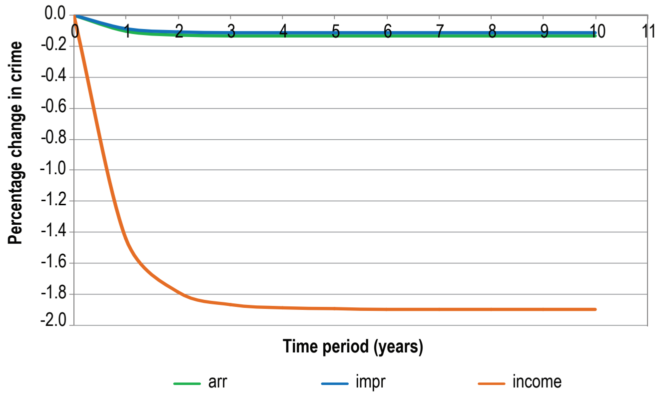
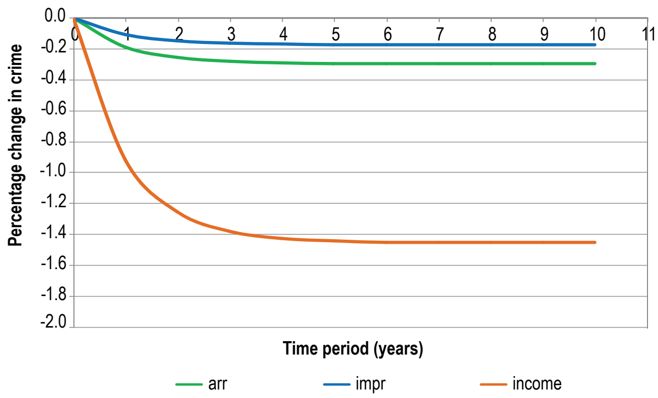


Figure 4. Elasticities of arrest, imprisonment and income on violent crime in NSW



way, the effects of changes in the criminal justice system and economic conditions tend to persist longer on violent crime than those same effects on property crime. This explains why most of the long-run effects are larger for violent crime as shown in Figures 3 and 4 and Table 4. The exception is income, which has higher short-term and long-term effects on property crime than it has on violent crime.

SENSITIVITY

To examine the sensitivity of parameter estimates to omitted variables, we re-estimated the models by discarding the income variable and arrest rate for use or possession of narcotics respectively. When income is excluded, our results indicate that the short-run and long-run estimates of the criminal justice variables are robust in the models for both property and violent crimes. For property crime, the short-run and long-run effects of arrest and imprisonment rates remain significant and inflated slightly. For violent crime, only the short-run and long-run effects of arrest rate get larger while the proxy for heroin use becomes marginally insignificant. The income effect may indeed be embedded into the fixed LGA effects and/or year effects in both models.

When the arrest rate for use or possession of narcotics is excluded, the short-run elasticity (b_4) for the income variable in both property crime and violent crime models became marginally insignificant. The long-run elasticity for income in the violent crime model also turned out to be insignificant. The estimates for the rest of the explanatory variables slightly changed in magnitude but remained significant while the fixed LGA effects and/or year effects seemed to soak up some of the heroin shortage effect. These results indicated the importance of including narcotics arrests as a proxy for heroin shortage in the model, which is known to have affected burglary and robbery during the observation period (Moffatt et al., 2005).

DISCUSSION

There has been much debate in Australia as elsewhere about the effectiveness of the criminal justice system in controlling crime. The idea that the criminal justice system might help control crime has been the subject of considerable scholarly debate. State and Territory Governments have generally acted as if the best way to control crime is to appoint more police and put more offenders in prison for longer (for examples, see Weatherburn, 2004) but policies directed toward this end have rarely if ever been defended on the basis of evidence. The need for more Australian research on the effectiveness of the criminal justice system in controlling crime has never been more acute.

The current study is one of only a handful of Australian studies on the effects of the criminal justice system on crime. It is one of only two studies to examine the joint effects of arrest risk, imprisonment risk and sentence length. Unlike many previous studies, we make explicit provision for the reciprocal effects of crime and the criminal justice system on each other. We include explicit control for income and heroin use, two variables found in previous research to have influenced crime trends in NSW over the last decade. As a further defence against omitted variable bias we use lagged values of the crime variable as a control and fixed LGA and year effects to account respectively for any omitted time-invariant LGA characteristics and common unobserved variations across LGAs. Finally, the effect of arrest risk, imprisonment risk and sentence length are estimated using a dataset and a method that minimises the simultaneity problem, as well as the risk of aggregation and ratio bias.

Our results suggest that the criminal justice system does exert a significant effect on crime but some elements of the criminal justice system exert much stronger effects than others. Increasing the risk of arrest or the risk of imprisonment reduces crime while increasing the length of prison sentences exerts no measurable effect at all. At first sight, increasing the risk of arrest appears to be more effective in reducing crime than increasing the risk of imprisonment. Thus, whereas a one per cent increase in the risk of arrest in the long run produces a 0.135 per cent reduction in property crime, a one per cent increase in the imprisonment risk produces only a 0.115 per cent reduction in property crime. Similarly, whereas a one per cent increase in the risk of arrest for violent crime produces a 0.297 per cent reduction in violent crime, a one per cent increase in the risk of imprisonment produces only a 0.170 per cent reduction in violent crime. These differences are consistent with the economic theory of crime (Becker, 1968; Ehrlich, 1973) and with many individual-level studies of deterrence (Nagin, 1998) but in our case they were found not to be significantly different. On the evidence gathered here, then, changes in the risk of arrest and changes in the risk of imprisonment given arrest exert comparable effects on property and violent crimes.

Arrest and imprisonment appear to exert stronger effects on violent crime than on property crime. Whereas a one per cent increase in the risk of arrest in the long run produces a .135 per cent reduction in property crime, a one per cent increase in the risk of arrest for violent crime produces a .297 per cent reduction in violent crime. Again, whereas a one per cent increase in the imprisonment risk produces only a .115 per cent reduction in property crime, a one per cent increase in the risk of imprisonment produces a .170 per cent reduction in violent crime. The stronger effect for violent crime may be at least partly due to the higher risk of arrest for violent crime relative to property crime. The 30 day clear-up rate for non-domestic assault, for example, is 21.7 per cent, compared with 3.7 per cent for burglary (NSW Bureau of Crime Statistics and Research, 2011b). Offenders may be more sensitive to variations in the risk of arrest when the risk of arrest is high than when it is very low.

The findings on income suggest that its effects are much stronger than those of the criminal justice variables. The long-term elasticity of property crime with respect to income (-1.894) is more than 14 times larger than the effect of a one per cent increase in the probability of arrest (-0.135). The long-term elasticity of violent crime with respect to income (-1.460) is nearly five times larger than a one per cent increase in the risk of arrest for violent crime (0.297). Although we would expect property crime to be more responsive to changes in income than violent crime, these are both very strong effects. One reason for their strength may be that the income variable is picking up both the effect of an increase in real average weekly earnings and the effect of falling unemployment. The latter variable, it will be recalled, was dropped from the model because it was found to be insignificant in the presence of income. During the period marked by falling property crime (i.e. between 2000 and 2008), unemployment was falling and real average weekly earnings were rising (Moffatt et al., 2005). Another possible explanation is that the effects of income are quite pronounced in areas of high socioeconomic disadvantage, such as regions of high indigenous concentration, and the low level of spatial aggregation employed in the current study allows us to pick up effects that might not be so visible at higher levels of spatial aggregation.

It is interesting to compare our results with those of Kelaher and Sarafidis (2011) since both studies examined the effect of arrest and imprisonment on violent crime using similar data and similar methods. The general pattern of results is fairly similar in both studies. Both find negative effects for all criminal justice variables on crime but both find stronger effects for arrest and imprisonment probability than for sentence length. Nonetheless there are some differences. Our short and long run estimates of the effect of arrest on violent crime (-0.187 and -0.297) are much lower than those obtained by Kelaher and Sarafidis (2011) (-0.258 and -0.720). Our estimates of the short and long-run effects of imprisonment probability on violent crime (-0.107 and -0.170) are in the same direction as theirs, however whereas they found no significant effect of imprisonment probability on violent crime we found significant, albeit small effects. The smaller short run effect for arrest may be due to the lack of any measure of conviction risk or unemployment in our model. The most likely explanation for the long-run difference between our results on arrest and those of Kelaher and Sarafidis (2011) relates to our use of difference GMM rather than system GMM. The coefficient on our lagged crime rate is smaller than the coefficient on their lagged crime rate (0.370 compared with 0.642). The denominator for risk of arrest in the long run is one minus the lagged crime rate. The lower coefficient on our lagged crime rate variable will therefore result in a lower coefficient on the arrest variable over the long term.

There are four important points to make about our findings, which deserve emphasis in conclusion. The first is that the panel methods used here may be the ideal vehicle for testing causal claims about the effect of the criminal justice system on crime but this is only because experiments in this context are impossible. Correlational methods can never provide the same assurance about causal relationships provided by randomised controlled trials. The second point is that our results concern marginal rather than absolute effects. In other words, the estimated effects of arrest risk, imprisonment risk and sentence length are the effects obtained when these variables are increased *above their current levels*. This is an important qualification because it does not follow from our results that the risk of imprisonment could be substantially reduced without any adverse effect on crime. The second is that our capacity to pick up marginal effects of the criminal justice system on crime depends critically on there being significant variation in our criminal justice variables over the time period covered by this study. The variation in average sentence length over this period was not especially large[3] and it may be that sentence length exerts effects that we were unable to detect. The third is that the effects of income on crime are far larger than those of the criminal justice system. This suggests that measures that affect the economic well-being of the community provide more potential leverage over crime than measures that influence the risk of arrest or the severity of the punishments imposed on offenders.

NOTES

1 In fact a negative correlation will exist even if there is no measurement error. To see this, denote the crime rate by C/P and arrest rate by A/C. Then the correlation between crime rate and arrest rate is given by

$$r_{\left(\frac{C}{P}\right)\left(\frac{A}{C}\right)} = -\frac{1-r_{CP}-r_{CA}+r_{AP}}{2\sqrt{(1-r_{CP})(1-r_{CA})}}.$$

Even if all the original variables are uncorrelated (r_{CP}, r_{CA}, r_{AP} = 0), it can be shown that the correlation would be equal to -0.5, which would falsely indicate that as arrests increase, crime decreases (Dunlap, Dietz, & Cortina, 1997).

2 Clear-up rates, for example, can be manipulated by putting pressure on offenders to 'admit' to more offences.

3 The median of the average sentence length for property crime stayed constant over the period from 1996 to 2008 at around 14 months. The interquartile range fluctuated at about 17 months over the entire period with little variation. The mean of the average sentence length for violent crime was stable at approximately 9 months from 1996 to 2007 but rose to 11.5 months in 2008. The interquartile range was however steady at roughly 6 months over the 13 years.

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