Police numbers and crime rates – a rapid evidence review

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Summary

1. Up until the mid-1990s there was very little evidence that increasing the number of police officers might result in a reduction in crime – or that reducing the number of officers might lead to an increase in crime.
2. However more recent studies, using more robust methodologies, have suggested that there is indeed a link between the two.
3. The weight of evidence is strengthened by the fact that the extant studies use a variety of methods. However the causal claims made by many of them are somewhat doubtful, and care should be taken when interpreting the results.
4. Most of these recent studies converge on two key findings:
    a. Higher levels of police are linked to lower levels of property crime. Evidence for an association between police numbers and violent crime is weaker.
    b. A summary of existing studies would put the elasticity of property crime in relation to police numbers at approximately -0.3 – that is, a 10 per cent increase in officers will lead to a reduction in crime of around 3 per cent (and vice versa).

Introduction

The current reductions in public expenditure will inevitably reduce police numbers – and thus probably reduce the numbers of police on the streets. Thus an important question to address is whether these reductions will result in more crime.

Demonstrating an effect of police numbers on crime is an issue beset with problems of causality, because there are many issues that might affect both police numbers and the crime, such as economic cycles or social change. Furthermore, it is entirely possible - indeed likely - that more crime actually causes more police, in the sense that all else equal, an increase in the rate of crime will lead to more officers being hired. In such cases it may look as if higher numbers of police cause more crime, and it is important to examine temporal sequences.

Three relatively recent summaries of the literature – Cameron (1988), Marvell and Moody (1996) and Eck and Maguire (2000) – concluded that most research finds either no link or evidence of a positive association between police and crime levels (i.e. more police leads to more crime). Marvell and Moody (1996), for example, accompanied their own empirical study with a review of 36 previous papers that examined the association between police numbers and crime rates. Only 10 of these found evidence of a negative

association between police numbers and crimes of any type. Some 15, however, found a positive association between crime rates and police numbers. This led Sherman and Eck (2002) to conclude that while there is consistent evidence that having *no* police (for example during police-strikes) significantly increases crime, the evidence of a marginal effect of increasing police numbers on crime is weak indeed.

There are however exceptions to this pattern, many of which have appeared in the literature since Sherman and Eck's (2002) review. These studies have tended to use stronger methodologies than was previously the case. They are summarised in Table 1 below.

Methodology

This note is not intended to be an exhaustive review of research on the relationship between police numbers and crime rates. Rather, it attempts to bring together the most relevant recent empirical studies on this issue. As such the starting point was taken to be Marvell and Moody's 1996 paper, which provided a thorough review of the literature up until that time. To collate papers from later years searches were conducted on the ISI 'Web of Knowledge', with search terms such a 'Police AND numbers AND crime'. Relevant studies located in this manner were crosschecked against each other and papers they themselves referenced were added to the review. Research looking at the relationship between police numbers and/or arrest rates and crime was 'counted in', and in total 13 studies, from Marvell and Moody 1996 onwards, were identified.

This review is not therefore systematic, and caution should be taken in generalising from the findings discussed below. However, the list of studies presented concurs with those noted in the literature reviews of the most recent papers (for example Vollaard and Konig 2009), suggesting some level of agreement as to what constitutes the currently relevant literature in this area.

Table 1: Summary of studies looking at the relationship between police numbers and/or arrests and crime			
Study	**Country and Sample**	**Method**	**Findings**
Marvell and Moody 1996	US. Pooled data from 49 states and 56 cities, 1973-1992.	Observational study. Multiple Time Series with fixed effects (with Granger causality test). Main explanatory variable: police numbers per capita. Response variable(s): crime rate (incidence divided by population).	Causality in both directions, with stronger effect of police numbers on crime. This effect was much stronger at the city rather than the state level. At the state level, significant negative association between police numbers and homicide, robbery and burglary. At city level, statistical effects for homicide, robbery, burglary, larceny, auto theft and total crime all significant.
Levitt 1997	US. Panel of 59 cities, 1970-1992.	Observational study. Two-stage least squares using electoral cycle (election year or not) as an instrumental variable[2]. Main explanatory variable: 'sworn officers' per capita. Response variable: crime rate.	Increases in police reduce crime. Estimates elasticity of crime to be -0.3: however, see McCrary's (2002) critique, which nullified these findings[3].
Corman and Mocan 2000	US. Monthly time series data from New York, 1970-1996.	Observational study. Regression analysis with lagged effects. Main explanatory variables: number of arrests and number of police officers. Response variable: Absolute numbers of crimes.	Significant effect of arrests on murders, robberies, burglaries and motor-vehicle thefts; significant effect of police numbers on robberies and burglaries but not on murder or motor vehicle theft. Reports average elasticity of crime with respect to police numbers as -0.45.
McCrary 2002	US. Panel of 59 cities, 1970-1992.	Observational study. Re-analysis of Levitt (1997).	Identifies problems in Levitt (1997) that effectively nullify the earlier paper's findings.

Levitt 2002	US. Panel of 122 cities, 1975-1995.	Observational study. Two-stage least squares using number of firefighters per capita as an instrumental variable. Main explanatory variable: police per capita. Response variables: violent crime rate and property crime rate.	Negative effect of police numbers on both violent and property crime rates. Evidence is strongest for murder and robbery.
Kovandzic and Sloan 2002	US. Yearly data from Florida counties, 1980-1998.	Observational study. Multiple Time Series with fixed effects (with Granger causality test). Main explanatory variable: police per capita. Response variables: crime rates.	'Significant and substantial' impacts of police levels on robbery, burglary and larceny and total crime (with relatively small elasticities – 0.14 for total crime). However no effect of police numbers on aggravated assault or murder.
Di Tella and Schargrodsky 2004	Argentina. City blocks in Buenos Aires.	Natural experiment. A terrorist bomb led to police guard being placed on every block containing a Jewish institution in Buenos Aires. A difference-in-difference approach is used to analyse the impact on car-theft in those blocks. Main explanatory variable: dummy variables representing police presence at or near block. Response variable: Absolute number of car thefts.	Find 'a large, negative and highly local effect of police presence on car theft'. Note that this effect is highly local and this paper is more akin to those that look at 'hot-spot' policing.

Corman and Mocan 2005	US. Monthly time series data from New York, 1974-1999.	Observational study. Regression analysis with lagged effects. Main explanatory variables: absolute numbers of police officers and arrests. Response variable: absolute number of crimes.	Significant associations between felony arrests and murder, burglary, assault, robbery, motor-vehicle theft, grand larceny and rape. Significant associations between misdemeanor arrests and robbery, motor-vehicle theft and grand larceny.
Klick and Tabarrok 2005	US. Washington DC.	Natural experiment. Uses changes to terror alert status to examine potential effect on crime of police mobilisation on high-alert days. Main explanatory variable: dummy variable representing high alert day. Response variable: Absolute number of reported crimes.	Report that non-violent crime, particularly auto-theft and theft from auto was significantly reduced on high-alert days. Suggest that overall elasticity of crime in relation to police presence of about -0.3.
Machin and Marie 2005	UK. Police Force Areas in England and Wales.	Quasi-experiment. Used the introduction of the Street Crime Initiative (SCI) in 2002 to examine the impact of increased police expenditure on robberies. Main explanatory variable: dummy representing 'policy on'. Response variable: Robbery rate per capita.	Introduction of the SCI appeared to significantly reduce the number of robberies in the areas it was introduced.
Draca *et al.* 2008	UK. Central London Boroughs.	Natural experiment. Deployment of extra police in central London after 7/7 bombings used to examine impact on crime rates of increased police	Conclude that 'susceptible' crime – violence and sexual offences, theft and handling and robbery – fell significantly in the treatment areas compared with control areas. 'Non-susceptible' crime – burglary and criminal

		numbers in certain areas. Main explanatory variable: police deployment (hours worked per 1,000 population). Response variable: crime rate.	damage – not affected. Note that this distinction seems rather arbitrary. The authors state that burglary and criminal damage are less susceptible in this context because they occur more in residential areas or at night, but provide no supporting evidence in this regard. Estimate an elasticity of crime with respect to the police of approximately -0.3.
Vollaard and Koning 2009	Netherlands. Five waves of PMB victimisation survey that covers the entire country.	Observational study. Combines survey data on victimisation and precaution-taking with data on police expenditure and numbers. Main explanatory variable: police per capita. Response variable: reported victimisation.	Conclude that there are significant negative effects of higher police levels on property and violent crime, public disorder, and precaution taking. Elasticitites range from -0.2 to -0.5. No effect of police numbers on assault or robbery with violence.
Lin 2009	US. Panel of '51' States, 1970-2000.	Observational study. Two-stage least squares using state sales tax as an instrumental variable. Main explanatory variable: police numbers per capita. Response variables: crime rate	Significant negative associations between numbers of police and levels of property crime, murder, robbery, burglary, larceny and auto theft. Estimates elasticity for property crime of about -0.9.

Discussion

A summary of the 13 pieces of research reported above might conclude that: (a) there is a relatively robust negative association between numbers of police officers and property crimes, broadly defined; (b) evidence of an association between police numbers and violent crime is weaker and sometimes contradictory; and (c) the elasticity of property crime in relation to police numbers might be relatively conservatively estimated at approximately -0.3; that is, a 10 per cent increase in officers will lead to a reduction in crime of around 3 per cent (and vice versa).

Taken individually none of the studies outlined in Table 1 would come close to 'proving' that higher numbers of police leads to less crime. Despite improvements compared with earlier years almost all suffer from potentially significant methodological and conceptual flaws. For example, some of the observational studies that use lagged effects to estimate the effect of police numbers or arrests on crime rate (e.g. Corman and Mocan 2005) appear to vary the length of the lag used purely to maximise the significance of the association, without giving much if any thought as to why the time spans involved should vary by crime type.

Many of the natural or quasi-experiments rely on what are highly unusual and probably unsustainable deployment patterns that resulted from 'shocks', such as terrorist attacks, which are thankfully highly unusual. Furthermore, such events may initiate a context for policing far removed from the 'day-to-day', and the policing response to such emergencies may acquire a specific and unique symbolic meaning. The rather general claims the papers make about the potential effect of police numbers on crime may be undermined by the specific context of the events they describe. Despite the fact that natural or quasi-experiments usually allow firmer causal conclusions to be drawn than observational studies, in this instance better evidence for a 'general' link between police numbers and crime rates is perhaps provided from the latter. It is also worth mentioning that most of these natural experiments to date have involved sharp *increases* in police presence, and not *decreases* – a point discussed below.

Yet, the observational studies are likely to suffer from all the usual problems around the recording of crime and officer's activities. For example, most are forced to use the recorded rate of crime as a proxy for the real rate (although Vollaard and Koning 2009 use self-reported victimisation). This may or may not be a valid approach, but in the absence of firm evidence either way some care is need when making claims that more police lead to less crime. Most of the studies that are not natural or quasi-experiments take no account of what police officers actually do, and how this might vary from place to place and over time, while, as noted, the experimental studies rely on highly unusual events and police deployment patterns.

Many of the observational studies also suffer from two further problems. The first is a possibly excessive faith in instrumental variables (IVs) – in many of the studies listed the robustness of the IV used is far from proven, meaning that issues of endogeneity may

not have been as fully dealt with as the authors claim. That is, while the authors believe they have solved any problems created by the possibility that the crime rate affects the number of police as much as vice versa, this may not actually be the case.

The second problem is that many of the papers rely on a rather simplistic 'rational choice' model of criminal behaviour. They postulate that the commission of crime is vitally influenced by considerations of the risk of sanction weighed against potential reward, with the number of police representing one element of the risk calculation. If this is not a valid theory of crime causation, or at least one that tells only part of the story (Downes and Rock 2007), much of the work outlined above lacks a plausible causal mechanism to explain the associations it uncovers. Causal claims – that higher police leads to less crime – made on the back of observational data are significantly weakened by the absence of such a mechanism.

However taken *together* the studies that have appeared over the last 15 years do indeed suggest that there is a significant negative association between the numbers of police (and/or the number of arrests made) and the level of at least some forms of recorded crime. Two things in particular support this conclusion. First there is the striking extent of agreement between most of the studies listed in Table 1. The research has varied considerably in terms of methodologies, time-spans, and countries, but has still generated broadly similar findings. This kind of triangulation adds some weight to the idea that there is a real effect of police numbers on some types of crime.

Second, there is the fact that the purported effect of police numbers on crime is more consistently found in relation to property crime than violent crime. This adds considerable 'face validity' to the overall findings. At least a proportion of overall property crime probably *is* committed by individuals who weigh up the relative risks and rewards involved, and who may even pay conscious attention to the presence or potential presence of police. Much violent crime, however, is conducted in the heat of the moment in pubs or on the street, or behind closed doors in the home. In neither case would one expect consideration or even awareness of potential police attention to come into play. A uniform association between police numbers on crime might in these terms be suspicious – one would expect the potential effect of the number of officers on the rate of crime to vary by crime type. As Dracin *et al.* (2008) note, for example, only some types of crime are susceptible to an increase of officers on the streets.

An important thing to bear in mind is that, on the accounts of most of the papers listed above, any effect of police numbers or arrest rates on crime must necessarily be mediated by potential offenders' perceptions. They must notice changes in the number of police, or in their activity, and construe these as having meanings or implications for their own behaviour. The meanings they attach to greater police presence may or may not relate to perceptions of risk. And – in the current economic climate, where reductions in police presence are in prospect, it should be remembered that increased police presence may be more salient than reduced police presence. Very large numbers

of patrols are hard not to notice; fewer patrols than usual may go unnoticed. In other words, the relationship between police numbers and crime may in fact be asymmetrical.

Conclusion

'Conclusion' at this stage is a misnomer. Despite the apparent consistency of recent research it is too early to say, for all the reasons given above, that there is a direct causal link between higher numbers of police and lower crime. Considerably more work would need to be done before such a claim could be made. In particular, more work is needed on the difference in the (potential) effect of specific, large-scale changes in deployment patterns due to terrorist attacks and other shocks, and that of general numbers of police or arrest rates averaged across a large number of areas. A related task is to locate the boundary between marginal changes in numbers – which go unnoticed – and gross changes – which can have a marked impact on crime. What seems fair to say, however, is that there is relatively strong evidence *for the potential* of an effect of police numbers on crime, particularly with regard to property and other acquisitive forms of offending.

Notes

1. My thanks to Mike Hough of ICPR for his many useful comments on an earlier draft of this paper.
2. Instrumental variables are used in an attempt to deal with issues of bi-directional causality in statistical models (in this case, that crime rates might affect police numbers as much as police numbers might affect crime rates). A third variable is identified – an example in the research considered here is fire-fighters in Levitt (2002) – that is supposed to be correlated with the number of police but not with the level of crime. This third variable is used to predict the number of police in the first stage of the 'two stage least squares regression'. The predicted values (number of police) from this first regression are then used as the explanatory variable in the second regression, which predicts crime rates. The idea is that the part of police numbers predicted by number of fire-fighters is distinct from the part predicted by the level of crime, instead being related to external factors such as local funding decisions and so forth. Whether this approach is actually valid in any particular case is, however, often a moot point.
3. McCary identified a weighting error in Levitt's original programming. Once this was corrected there was no significant association between police numbers and crime rates (either property or violence) in the 2SLS models. As always, research is open to bias introduced by human error.

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