

WHY THE POLICE HAVE AN EFFECT ON VIOLENT CRIME AFTER ALL EVIDENCE FROM THE BRITISH CRIME SURVEY*

August 10, 2009

BEN VOLLAARD

CentER, TILEC, Tilburg University, The Netherlands. b.a.vollaard@uvt.nl

JOSEPH HAMED

Department for Business, Innovation and Skills, London, UK.

ABSTRACT

Whereas the police have been shown to have a negative effect on property crime, the evidence for an effect of police on violent crime is weak and inconsistent. We provide evidence that measurement error in police statistics on crime explains the divergence in findings for property and violent crime. In particular, the propensity to record violent crime is found to be more susceptible to changes in number of police than police statistics on property crime. Crime data from the British Crime Survey do not suffer from similar measurement error as they relate to victimization of crime irrespective of whether or not the crime is reported to the police. We find the estimated effect of police on violent and property crime to become similar in size once using crime survey data, with a one percent increase in police resulting in a 0.7 percent decrease in crime.

JEL Classification: K42, K14, H50

Keywords: public law enforcement, measurement error

* The authors thank Richard Dubourg, Andy Healy, Steve Machin, Naci Mocan and participants of the Royal Economic Society conference in April 2007 for helpful comments. We thank the Home Office for providing research funding. Crime data from Crown copyright records made available through the Home Office and the UK Data Archive have been used by permission of the Controller of Her Majesty's Stationery Office and the Queen's Printer for Scotland.

1. Introduction

Increases in the number of police have been cited as one explanation of the decline in crime in the 1990s (Levitt 2004; Donahue and Ludwig 2007). These claims are based on recent studies that tend to find a negative effect of police on crime, including Corman and Mocan (2000) and Klick and Tabarrok (2005). The innovation of these studies is to explicitly address reverse causality in the relation between police and crime. Areas with relatively unfavorable crime levels and crime trends tend to receive relatively higher levels of police resources. If not properly controlled for, this policy response attenuates the negative effect the police may have on crime. Exploiting shocks in police levels that are exogenous to crime, for instance changes in police deployment after a terror attack, these recent studies refute findings from earlier research that did not address reverse causality and showed no or a positive effect of police on crime (see Levitt and Miles [2006: 150-153], for an overview of the literature).

The empirical evidence relating to the impact of police on crime is mostly limited to property crime. Most studies find an effect of police on property crime, but no effect on violent crime (e.g. Levitt 1997, 2002; Corman and Mocan 2000, 2005; Klick and Tabarrok 2005; Lin 2009).¹ Often the estimated effect on violent crime is at best of borderline statistical significance in some of the model specifications and the size of the effect tends to be smaller than the effect on property crime. Several explanations have been given for the commonly found divergence in the estimated impact on property crime and violent crime. DiTella and Schargrotsky (2004: 118) suggest that criminals miss the presence of police when concentrating their attention on mobile victims. Car thieves, on the other hand, have the time to gather information on areas in which they intend to commit crimes. Marvell and Moody (1996: 631) suggest a lack of premeditation on the side of violent offenders, assuming that thieves put more thought into their actions and are therefore more susceptible to police oversight.

In this paper, we present and test another explanation for the relatively weak and inconsistent evidence on the impact of police on violent crime: differences in the way data on property crime and data on violent crime are collected. The existing literature is based on police statistics as source of crime data.² As is well known, and has been extensively studied, property crimes tend to find their way into recorded crime statistics more often than violent crimes (Levitt 1998; MacDonald

¹ Levitt (1997, 2002) provides an imprecisely estimated effect on overall violent crime, with only the estimate in the 2002 study being of borderline statistical significance. Lin (2009) presents an estimated effect on overall violent crime that is only statistically significant in some model specifications. In crime-specific regressions, she finds no effect on assault and rape; the effect on robbery is of borderline statistical significance. Corman and Mocan (2000: 600) find no effect on murder and assault; the effect on robbery is statistically significant only in some model specifications. Corman and Mocan (2005) find no effect of police on assault, robbery, rape and murder. Klick and Tabarrok (2005) find no effect of police on overall violent crime. DiTella and Schargrotsky (2004) limit their scope to car theft. Exceptions are the positive effect of police on murder in Lin (2009) and on assault and harassment in Draca, Machin, and Witt (2008): both studies will be discussed in section 2.

² With the exception of Vollaard and Koning (2009), who use crime data from a victimization survey rather than police statistics. We will discuss the results of this study later in the introduction.

2002). Victims of theft, in particular car theft and burglary, more frequently report the offence to the police than victims of violence (Kershaw et al. 2008: 39). Moreover, the police less frequently record a reported violent crime than a reported property crime (Thorpe and Ruparel 2005: 42), with homicide as one of the well-known exceptions. Not only are police statistics on violent crime relatively incomplete, the propensity to report and record violent crime may also be more susceptible to changes in number of police than police statistics on property crime (again with homicide as one of the exceptions, see Levitt [1998] for a discussion). For instance, it is easy to imagine that well-staffed police departments respond quicker to a growing demand from policy makers to better record crime, particularly violent crime, than low-staffed departments. Adding police might also have a stronger effect on crimes with low reporting and recording rates, such as assault, than on crimes with high reporting and recording rates, such as car theft. Thus incomplete police records and victim reporting and police recording biases may have a disproportionately strong effect on estimates of the impact of police on violent crime.

Data on victimization of crime from the British Crime Survey (BCS), a large-scale survey among the English and Welsh population aged 15 and over, provide us with the opportunity to test whether measurement error in police statistics is a major source of estimation bias. Respondents to the BCS are asked whether they became victim of crime in the period preceding the interview, irrespective of whether the crime was reported to the police or not. The survey includes data on victimization of a broad range of property and violent crimes; it excludes data on homicides and victimless crimes such as speeding. Unlike police statistics, data on victimization of crime from the BCS are not subject to changes in reporting behavior of the public and recording behavior of the police. Using both survey data on crime from the BCS and police recorded crime statistics, we are able to compare the effect of police on recorded crime with the effect of police on victimization of crime.

Using a method that is the exact inverse of the instrumental variable approach, we isolate exogenous variation in police levels by modeling the process by which police resources are allocated. In England and Wales, the national police budget is distributed by means of an elaborate funding formula. Based on predictors of police workload like population density and the number of unemployed, the police funding formula distributes police resources across 43 police force areas. Since it takes time to hire and train police personnel, year-to-year variations in funding are smoothed, and police force areas boundaries are sometimes redrawn, actual police levels differ from the levels predicted by the formula for extended periods of time. We use this difference between actual and prescribed police levels as exogenous source of variation in number of police, a research design introduced in Vollaard and Koning (2009).

In contrast to studies relying on major events such as terror attacks as source of variation in police levels, which focus on the effect of temporary changes in visible police presence on street-level crimes in an urban setting, we broaden the

perspective by estimating the effect of more permanent changes in overall police levels across the nation on a broad set of crimes. Compared to the instrumental variable approach, with the number of firefighters (Levitt 2002) and state sales tax revenues (Lin 2009) as examples of instrumental variables, the funding formula has a more direct, certain and transparent impact on police numbers.³

We find the estimated impact of police on violent crime to be attenuated by a positive relation between number of police and the propensity of police to record violent crime. A greater number of police not only deters crime but also increases the share of crime that finds its way into police statistics. The result is that the true effect of police on violent crime is underestimated. We do not find evidence for a similar bias in the estimated effect of police on property crime. Using crime survey data, we estimate a one percent increase in police per capita to result in a decrease in both property and violent crime of 0.7 percent.

Our paper is most closely related to the work by Levitt (1998). Using data from the U.S. National Crime Victimization Survey (NCVS), Levitt provides evidence for an effect of the number of police on the propensity to report crime. Typically, a 10 percent increase in the number of sworn officers per capita corresponds to a 1 percent increase in the reporting rate of violent crime, although the effect is found to be only statistically significant for robbery (ibid.: 67). Comparing city-level trends in police recorded rape, robbery, assault, and theft to police recorded murder, a crime with high and stable reporting and recording rates, Levitt shows that the propensity to record a reported crime is also affected by the number of police. Taken together, higher reporting and recording rates as a result of a 10 percent increase in police levels are estimated to result in a 3 percent increase in some violent crimes, including assault. Surprisingly, these findings have had no impact on the choice of the source of crime data or the research design of later studies into the effect of police on crime – or, for that matter, any notes of caution when reporting estimates based on police recorded crime data.

In a related study, Gibson and Kim (2008) analyze how measurement error in police statistics affects the estimated effect of inequality on crime in a cross-country setting. Comparing estimation results based on crime data from victimization surveys and from police statistics, they find police recorded crime to provide a downward estimation bias, which is particularly strong for assault and robbery. Their findings provide further support for a closer study of how measurement error affects the estimated effect of police on crime. As a first step, Vollaard and Koning (2009) use micro-level data from a victimization survey to estimate the impact of police on crime, and find the effect on property and violent crime to be similar. In this paper, we use both police statistics and victimization survey data on crime for the same period and the same geographical areas to test how the source of data affects the findings. As such, our analysis provides

³ Another successful strategy to isolate the effect of police on crime from the effect of crime on police has been the use of high frequency crime data to escape the (slow) adjustment in allocation of police resources to crime rates (Corman and Mocan 2000, 2005).

the first direct test of how robust the estimated impact of police on crime is to the source of crime data.

The paper is structured as follows. First, we briefly discuss the ways in which measurement error may have affected the findings of previous studies. In the following two sections we outline the data and methodology we use to estimate the effect of police on crime. We then present the estimation results, the sensitivity analysis, and discuss the implications of our findings.

2. Measurement error and the effect of police on crime

The use of police recorded crime data will result in biased estimates of the effect of police on crime if reporting and recording practices are systematically related to police levels. Random measurement error in police statistics will increase the standard error of the estimates, but will not bias the parameter estimates (Hausman 2001). Thus identifying an effect of police on recorded crime could fail for two reasons. First, the effect cannot be estimated with sufficient precision, since, in some random manner, a great number of offences do not make their way into police statistics. Second, the estimated effect is biased upwards as greater police numbers result in a greater number of recorded crimes.

Both factors seem to be at play. Based on a number of recent studies into the effect of police on crime, figure 1 shows the reported t-statistics for 26 estimated elasticities of property and violent crime with respect to police (full details provided in the appendix). Relative to the size of the estimated effects of police on crime, the standard errors of the estimated coefficients tend to be higher for violent crime than for property crime. The relatively low precision of the estimated effect on violent crime suggests that random measurement error is relatively large for police recorded violent crime.

Figure 1 Reported t-statistics for the estimated effect of police on crime

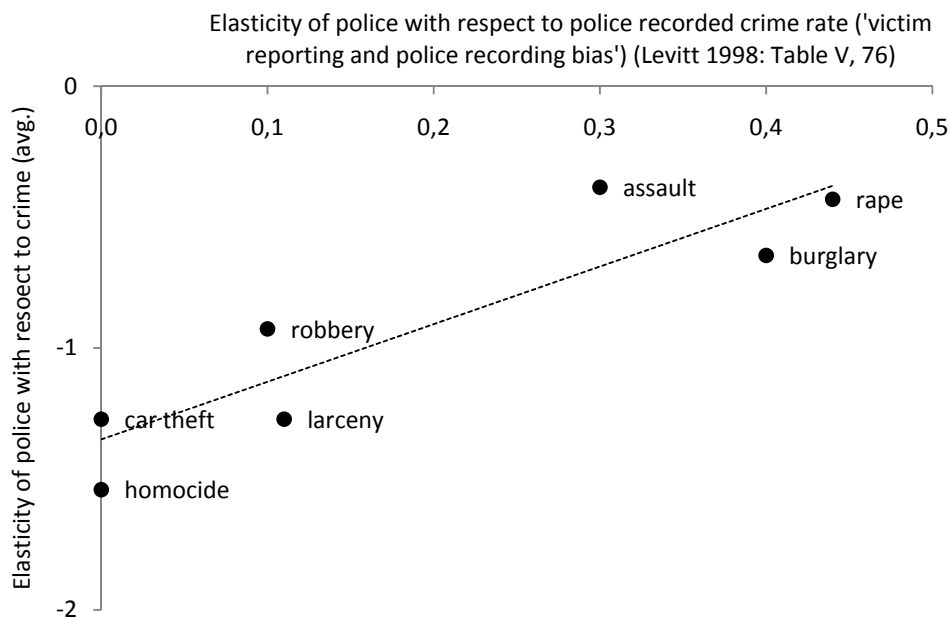


Source: Corman and Mocan (2000; 2005); Levitt (2002); DiTella and Schargrodsky (2004); Klick and Tabarrok (2005); Lin (2009). Individual estimation results provided in the appendix.

Note: Elasticities relate to both individual crime categories and aggregated property and violent crime. Draca, Machin, and Witt (2008) do not report separate elasticities for property and violent crime.

Not only the precision but also the size of the estimated effect of police on crime seems to be affected by measurement error in police statistics. For six crime categories, figure 2 plots Levitt's (1998) estimates of the combined reporting and recording bias in police statistics against the average estimated effect of police on crime (the reporting and recording bias for homicide is taken to be zero). A larger reporting and recording bias is correlated with a lower estimated effect of police on crime. Interestingly, just like not all violent crimes are similarly affected by the reporting and recording bias, not all property crimes are immune to this bias either. Homicide and burglary are the two most striking exceptions. As noted before, homicide has high and stable reporting and recording rates. The resulting low reporting and recording bias explains why Lin (2009) finds a statistically significant effect of police on homicide and not on other violent crimes such as assault. The effect of police on recorded burglary is found to be statistically significant in most studies, but figure 2 suggests that the true effect on burglary is likely to be underestimated.

Figure 2 Estimated reporting and recording bias in police statistics vs. the estimated effect of police on crime



Source: Estimated elasticities of police with respect to crime – irrespective of level of statistical significance – taken from Corman and Mocan (2000; 2005); DiTella and Schargrodsy (2004); Klick and Tabarrok (2005); Lin (2009). Individual estimation results provided in the appendix. Estimated elasticities of police with respect to reporting and recording rate of crime taken from Levitt (1998); elasticity of police with respect to reporting and recording rate of homicide is taken to be zero.

Note: Levitt (2002), Draca, Machin, and Witt (2008) do not report crime-specific elasticities.

The variation across crime types shown in figure 2 provides a stronger indication for the presence of an estimation bias through the use of police statistics than the prior observation that the estimated effect of police on violent crime tends to be relatively small in size. After all, there is no *a priori* reason why the police should have a similar effect on property and violent crime. Figure 2 suggests that the argument is more nuanced: compared to property crimes, the reporting and recording bias is larger for *most* violent crimes. As a result, the estimation bias in the police-crime relation is generally larger for violent crime than for property crime – with the two most noticeable exceptions, homicide and burglary, accounting for only a small part of violent and property crime, respectively.

Draca, Machin, and Witt (2008) are the first to find an impact of police on assault and harassment. Their findings suggest that the degree of measurement error may differ depending on the nature and duration of the changes in police levels studied. Like DiTella and Schargrotsky (2004) and Klick and Tabarrok (2005), Draca, Machin, and Witt (2008) adopt a quasi-experimental approach to identify the effect of police on crime. They analyze what happens to crime before and after a major event, a terror attack in London, induced a change in police presence. Quasi-experimental studies cover a short period of some months. In contrast, time series data covering several decades are used in studies that attempt to single out exogenous variation in naturally occurring changes in police numbers, either by way of an instrumental variable (Levitt 1997, 2002; Lin 2009) or by using high-frequency data (Corman and Mocan 2005).

Clearly, the nature and degree of measurement error in police statistics may differ between the two type of studies, affecting the size of the estimation bias.⁴ Whereas victim reporting may increase in response to a greater number of police in the long-term, in the immediate aftermath of a terror attack reporting practices may change because of increased vigilance on the part of citizens (Klick and Tabarrok 2005: 276). Similarly, in the long run, changes in crime recording through technical or organization innovations may be most pronounced in well-staffed police departments, whereas in the short run changes in the propensity to record crime may be affected predominantly by the greater chance of intercepting a brawl or other crime incident. It is by no means clear, however, how the specific setting affects the nature and degree of measurement error. For once, using a research design similar to Draca, Machin, and Witt (2008), Klick and Tabarrok (2005) find no effect of police on violent crime. What exactly is driving changes in recording practices of the police and how these changes

⁴ The two types of studies may differ on other dimensions as well. Compared to studies based on state-level, long-term data, such as Levitt (2002) and Lin (2009), quasi-experimental studies are more limited in scope. Quasi-experimental studies focus on deterrence, i.e. the effect of oversight in or around public spaces, to the exclusion of incapacitation of offenders. The effects of incarceration and subsequent reduction of the criminal population are also experienced in control areas after all (DiTella and Schargrotsky 2004: 124). Furthermore, both Klick and Tabarrok (2005) and Draca, Machin, and Witt (2008) suggest that studies relying on temporary changes in visible police presence limit the scope to 'street crimes' only. Interestingly, and as an illustration of how little is known about which policing activities affect which crimes, Klick and Tabarrok (2005) find an effect of police on burglary, a crime which Draca, Machin, and Witt (2008) suggest (and find) not to be susceptible to greater visible police presence.

affect recorded crime statistics remains largely unknown, making it impossible to draw any firm conclusions on how measurement error differs from setting to setting. The more or less permanent changes in overall police levels studied in our paper most closely resemble the work by Corman and Mocan (2000, 2005), Levitt (2002) and Lin (2009), who all fail to find a robust effect of police on violent crime.

3. Data

The analysis is based on data at police force area level. The British Crime Survey counts the City of London and the Metropolitan police areas as the same area so our analysis is based on 42 police force areas in England and Wales. On average, a police force employs 3,500 police officers serving 1.2 million inhabitants.

As sources of crime data, we use police recorded crime statistics and survey data from the British Crime Survey (BCS), both provided by the Home Office together with the UK Data Archive. Police recorded crime is defined as all crimes the police came to know about and subsequently recorded; it includes crimes the police found out about by their own efforts rather than by a citizen reporting the incident.

We include five waves of BCS data, from fiscal year 2001/2002 to fiscal year 2005/2006, and police statistics for the same years. The fiscal year runs from April to March. The BCS is a repeated cross-section face-to-face survey. The survey is continuous, with interviews conducted between April and March. Respondents are asked about crime incidents experienced in the 12 months prior to the interview. The center point of the period for reporting crime is the first month of the fiscal year, the only month to be included in all respondents' reference periods. The survey includes some 40,000 adults aged 16 or over living in private households in England and Wales. The sample is equal to 0.1 percent of the population. Given the sample size of the BCS, we can break down victimization rates into property crime and violent crime for each of the police force areas. We use the victimisation incidence rate, which is equal to the number of crime incidents within a 12-months period per population aged 16 or above.

The BCS sample does not allow us to break down police force area-level data on property and violent crime into smaller categories. Only some 6 percent of the population is affected by the most common crimes such as vehicle-related theft, whereas victimization rates hover around 2 to 3 percent for crimes like violence against the person and burglary. Given the relatively infrequent occurrence of crime and a sample of some 1,000 respondents per police force area, further disaggregated crime-specific victimization rates would not provide sufficiently reliable data on area-specific crime levels and trends.

We made the definition of property crime and violent crime for both data sources as comparable as possible, although no one-to-one match is feasible. Police recorded property crime includes burglary in dwellings and other buildings, robbery of personal property, theft of and from vehicles, and theft from person. Victimization of property crime includes snatch theft and theft from the person at the individual level and burglary and theft of and from a vehicle at the household level. We also include all attempts of theft. Police recorded violent crime includes common assault, aggravated assault, child abuse, wounding or other act endangering life, other wounding, possession of weapons, harassment, and threat. Victimization of violent crime includes serious wounding, other wounding, common assault, other common assault, attempted assault, rape, indecent assault and robbery, including attempts.

Using victim feedback from the BCS, we derive the percentage of crimes reported to the police. Some values for the percentage property and violent crime reported have been imputed due to low sample sizes in some areas in some years for some crime types.

Table 1. Summary statistics, 42 police force areas, weighted by population, 2001-2005

	Mean	Standard deviation
Deterrence measure		
Police officers per 10,000 population	25.91	7.31
Recorded crime per 1,000 population		
Property crime	36.28	12.41
Violent crime	17.18	5.56
Victimization of crime per 100 population/households		
Property crime	0.14	0.04
Violent crime	0.05	0.01
Propensity to report crime		
Property crime	0.44	0.05
Violent crime	0.42	0.10
Police funding		
Street Crime Initiative funding per capita (£)	0.26	0.49
Community Safety Officers funding per capita (£)	0.42	0.65
South East Allowance per capita (£)	0.50	1.05
Police council tax per household (£)	85.57	23.36
Additional crime-related variables		
Proportion of population of non-white ethnicity	0.09	0.10
Proportion of population that visits nightclubs frequently	0.04	0.01
Proportion of population reporting that lack of school discipline are problems or causes of crime	0.73	0.05
Proportion of population reported that drugs use or drug dealing are problems or causes of crime	0.35	0.05

Data on the number of police officers in FTEs per 10,000 population as at the end of the fiscal year were obtained from the summary police statistics published

by the Chartered Institute of Public Finance Accountants. Police officers do not include traffic wardens and civilian staff (including community support officers who do not have the power to arrest).

The Home Office provided data on supplementary grant funding of the police authorities. Data on the average council tax for police services levied on 'Band D' households were obtained from the Department for Communities and Local Government for England and from the Welsh Assembly Government for Wales. In addition to the variables from the police funding formula, which will be introduced in the next section, we will use a number of additional variables, each of them based on survey data from the BCS. Summary statistics for all variables used in the analysis are provided in table 1.

4. Research design

We use variation in the number of police officers per capita between police force areas to identify the impact on crime:

$$(1) CRIME_{jt} = \alpha \ln p_{jt-1} + FORMULA_{jt-2} \beta + SUPPLEMENTARY_{jt-2} \gamma + \delta LOCAL_{jt-2} + \zeta_t + \varepsilon_{jt}$$

In this estimation equation, $CRIME_{jt}$ can represent both the average probability of victimization of crime and the number of recorded crimes per population in police force area j and time t . Year t relates to the fiscal year running from April in year t to March in year $t+1$. We use a linear probability model to estimate the effect on crime.⁵ $\ln p_{jt-1}$ stands for the natural logarithm of police officers per capita in police force area j in period $t-1$. We lag police levels by one year as the figures for the number of police officers relate to the end of the fiscal year. α is the parameter of interest, describing the effect of police on crime. $FORMULA_{jt-2}$ is a vector of police force area characteristics included in the police funding formula; $SUPPLEMENTARY_{jt-2}$ a vector of police authority-specific supplementary grants; $LOCAL_{jt-2}$ a variable representing local authority police funding. Each of the budget variables is lagged by two years as they relate to the level of police resources at the start of fiscal year t , whereas police levels are measured at the end of the fiscal year. We discuss these funding variables below. To control for any national trends in crime levels that might be related to trends in police levels, we include year fixed effects ζ_t . ε_{jt} is the error term in the model. We assume the error term to be identically and independently distributed, with mean zero – also in the case of using survey data for $CRIME$, since the probability of victimisation is a continuous variable.⁶

In England and Wales, the yearly cycle of distributing police resources across police force areas is guided by the use of an elaborate funding formula. The police funding formula is meant to provide an objective basis for the allocation of

⁵ The estimation results are robust to the choice between the linear probability model, probit and logit.

⁶ A modified Wald test for groupwise heteroscedasticity based on Greene (2000: 598) indicates that we have to reject the null hypothesis of no heteroscedasticity. Therefore, we estimate the equation using feasible generalised least squares (as also suggested by Hausman and Kuersteiner [2004]) under the assumption of heteroscedasticity within panels.

the national police budget. The formula includes indicators of need for police services in an area, including the number of residents in lone parent families, the number of unemployed and population density.

Clearly, if all of the variation in police levels were explained by the funding formula, we would not be left with any variation to identify an effect. In that case, the variation in the number of police would be perfectly endogenous. In practice, actual police levels differ from police levels as predicted by the funding formula. It takes a while before new personnel is hired and trained. It takes easily up to two years before changes in funding can be traced in changes in police levels. Additionally, to accommodate the forces, changes in funding are smoothed through floors and ceilings. For instance, in some years a larger budget is allocated to certain police authorities to ensure that no authority receives a smaller budget than last year. Another example is the 'Welsh floor-top up' introduced in 2003 which was meant to cushion a drop in resources for Welsh police authorities. Boundary changes have also a profound effect on local police levels. When Metropolitan Police's area was reduced to that of Greater London in 2000, some 900 police officers were transferred to neighboring police forces. Many of these officers returned to the Metropolitan Police and it took years before the neighboring forces were able to replace them with new recruits. We use the difference between actual police levels and police levels as predicted on the basis of the indicators of need in the police funding formula to identify the effect of police on crime.

By including the funding formula variables in the estimation equation, we are able to control for the fact that police force areas with unfavorable crime rates and trends receive higher levels of police resources. The variables from the funding formula enter the estimation equation linearly, in the same way as they are used in the funding formula:

$$(2) \text{FORMULA}_{jt} = \alpha \text{STRIVING}_{jt} + \beta \text{LONEPARENT}_{jt} + \gamma \text{RENTING}_{jt} + \delta \text{LTUNEMPL}_{jt} + \zeta \text{POPDENS}_{jt} + \eta \text{POPSPARS}_{jt} + 1/\text{RESPOP}_{jt} (\theta \text{ROADS}_{jt} + \psi \text{MOTORWAYS}_{jt} + \kappa \text{SECEXP}_{jt} + \lambda \text{PENSIONS}_{jt}) + \text{DAYTIMEPOP}_{jt}/\text{RESPOP}_{jt} (\mu \text{TERRACED}_{jt} + \nu \text{STRIVING}_{jt} + \xi \text{LONEPARENT}_{jt} + \rho \text{ONEADULT}_{jt} + \varsigma \text{RENTING}_{jt} + \sigma \text{UNEMPL}_{jt} + \tau \text{OVERCROWDED}_{jt} + \omega \text{YNGMUNEMPL}_{jt} + \phi \text{POPDENS}_{jt} + \chi \text{POPSPARS}_{jt})$$

where *STRIVING* stands for the proportion of residents living in areas classified as striving, *LONEPARENT* for the proportion of residents living in a lone parent families, *RENTING* for the proportion of households living in rented accommodation, *LTUNEMPL* for the proportion of residents claiming unemployment-related benefits for over a year, *POPDENS* for population density, *POPSPARS* for the proportion of population living in areas with 0.5 or less residents per hectare, *RESPOP* for the resident population, *ROADS* for built-up road length, *MOTORWAYS* for motorway length, *SECEXP* for expenditure on security-related commitments, *PENSIONS* for pension obligations of police authorities, *DAYTIMEPOP* for the number of population during daytime, *TERRACED* for the proportion of residents living in unshared terraced dwellings,

ONEADULT for the proportion of households containing only one person aged 16 or over, *UNEMPL* for the proportion of population aged 18-64 claiming unemployment-related benefits, *OVERCROWDED* for the proportion of residents living in accommodation with more than one person per room, and *YNGMUNEMPL* for the proportion of male population below 25 claiming unemployment-related benefits. A full description of the funding formula, a definition of all variables and summary statistics are provided in the appendix.

The police funding formula dominates the allocation of police funding. Some 92 percent of the national police budget is directly allocated with the use of the funding formula.⁷ A further 6 percent of the budget, consisting of allowances for capital expenditures, hiring additional uniformed officers, paying bonuses to police staff and some other relatively minor grants, is allocated largely in line with the funding formula.

The remaining 2 percent of the national police budget includes funding for new technologies (including DNA-units and Airwave, a secure radio service), updating of police premises, and, most importantly for our analysis, supplementary funding for specific regional needs, some of which are crime-related. The Metropolitan Police receives an additional allowance in recognition of London's national and capital city functions; neighboring police force areas in the South East of England receive some supplementary funding as well. As of 2002, police authorities can apply for grant funding to employ Community Support Officers, uniformed staff supporting regular police officers in providing a visible presence on the streets. Under the 'Street Crime Initiative', ten police authorities with the highest robbery rates received additional funding from 2002 on.

When targeted at tackling crime, these sources of supplementary funding could introduce endogeneity in the relation between the remaining variation in police levels and crime. To prevent an estimation bias, we include the crime-related supplementary grants – the Street Crime Initiative, the Community Support Officers and the London/South East Allowance – as additional control variables in the estimation equation. In the sensitivity analysis, we test the robustness of the results to including London in the sample. We also test whether other changes in the allocated budget are related to local crime levels and trends. Like the police funding formula variables, we lag the supplementary funding variables by two years:

$$(3) \text{ SUPPLEMENTARY}_{jt} = \alpha \text{ SCI}_{jt} + \beta \text{ CSO}_{jt} + \gamma \text{ SOUTHEAST}_{jt}$$

where *SCI* stands for the allocated Street Crime Initiative budget, *CSO* for the budget for Community Safety Officers, and *SOUTHEAST* for the supplementary

⁷ This 'General Grant' includes the 'Specific Grant' from the Home Office, the 'Revenue Resource Grant' from the Department for Communities and Local Government, and the 'Non-Domestic Rates' from receipts of a local tax on business properties.

allowance for London and neighboring police force areas. All variables are per capita.

Next to the national police budget, police authorities receive some funding from local authorities through a tax on residential properties, the council tax. Local funding makes up some 20 percent of the total budget of a police authority. To make sure that local funding decisions do not introduce an endogeneity in the relation between crime and police either, we include the average council tax for police services levied on a 'Band D' property inhabited by two liable adults *LOCAL* (where 'Band D' stands for properties in the middle range of housing values, the reference group used in budgeting decisions). The council tax variable is also lagged by two years.

Table 2 presents the results of a regression equation explaining variation in police levels with the funding formula variables and the supplementary and local funding. As expected, we find most of the indicators of need for police services that are part of the funding formula to be positively related to the number of police. Exceptions are variables that appear twice, once unweighted and once weighted by the ratio of daytime over resident population. The negative coefficient for one of the two variables that are included twice can be seen as an adjustment factor. The estimated coefficient for built-up road length is also found to be negative, but is very imprecise. The degree to which a pound of supplementary funding is translated into a higher number of police differs between the three sources of supplementary funding. As expected, the coefficient is smaller for funding of temporary programmes such as the Street Crime Initiative than for more permanent funding like the South East Allowance. Together, the funding variables and the year fixed effects explain 98 percent of the variation. We use the remaining 2 percent variation in police strength as exogenous source of variation.

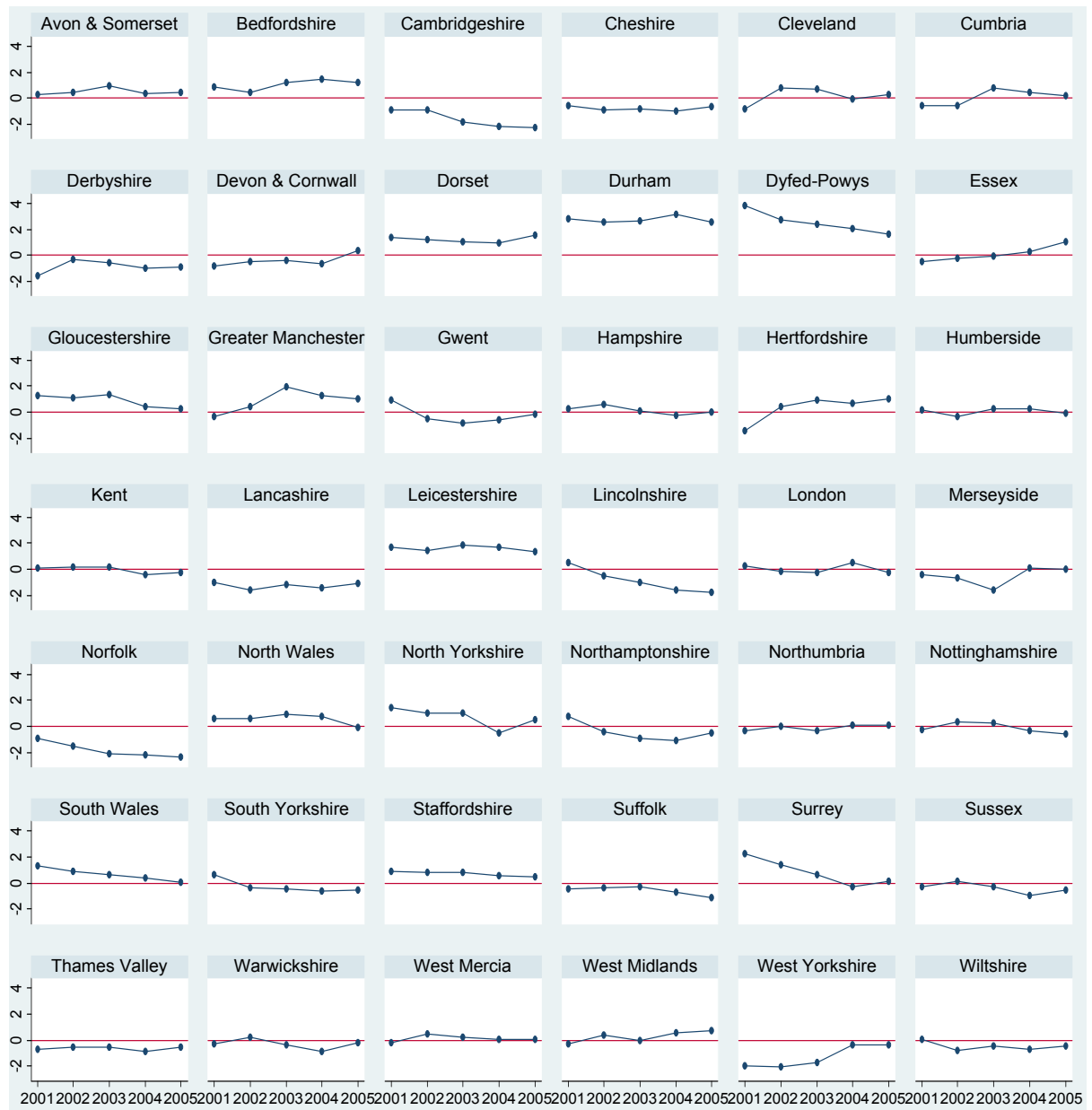
Figure 3 shows the remaining variation in police levels by police force area. Most of the remaining variation in police strength is variation across police force areas rather than variation over time. As evidenced by police force areas such as Durham, Leicestershire and Norfolk, actual police levels can deviate from predicted police levels for extended periods of time. The deviation from the predicted police levels is usually limited to plus or minus 1 police officer per 10,000 population, which equals to some 120 police officers per police force area on average, and reaches 4 police officers per 10,000 population at a maximum, which implies a variation around the prescribed levels of plus or minus 15 percent.

Table 2. The police funding formula and levels of supplementary and local funding as predictors of police levels, 2001-2005

Dependent variable: Police officers per capita		
<i>Funding formula variables (t-1)</i>		
Striving population	-309.46**	(70.51)
Lone parent households	1464.58 ⁺	(785.02)
Households renting	144.75	(162.83)
Long term unemployment	0.78	(6.54)
Population density	-7.45*	(3.39)
Population sparsity	-62.08**	(44.10)
Built-up roads length	-335.68	(528.71)
Motorway length	4516.98	(3302.19)
Security expenditure	8823.61**	(2963.14)
Police pensions	-0.04	(0.09)
Residents in terraced houses*	3.02	(4.38)
Striving population*	356.52**	(86.49)
Lone parent households*	-1266.59 ⁺	(701.72)
One-adult households*	27.66*	(12.88)
Households renting*	-119.83	(149.74)
Unemployment *	36.01	(25.31)
Overcrowded households*	27.72	(30.00)
Young male unemployment *	43.23**	(13.02)
Population density*	6.82**	(2.76)
Population sparsity*	53.04**	(40.75)
<i>Supplementary and local police funding (t-1) (in £ 1,000)</i>		
Street Crime Initiative	222.01	(247.96)
Community Support Officers	318.73	(292.07)
South East Allowance	581.64	(405.57)
Police council tax	29.91**	(10.93)
Year 2002	1.29**	(0.27)
Year 2003	1.78**	(0.40)
Year 2004	1.61**	(0.61)
Year 2005	1.11**	(0.81)
R ²	0.98	
Number of observations	210	

Notes: (*) Per ratio daytime over resident population. Estimated using mid-year population of police force areas as weights. All variables are per population, except for population density and population sparsity. Standard errors between parentheses. + P<.10; * P<.05; ** P<.01.

Figure 3 Remaining variation in police levels by police force area, 2001-2005



Note: On the vertical axis the number of police officers per 10,000 population relative to the level of police predicted by the police funding formula, supplementary and local police funding and year fixed effects.

5. Estimation results

Table 3 provides an overview of the estimated effects based on equation (1). All effects are expressed in terms of elasticities: the percentage change in victimization rates or recorded crime rates as a result of a one-percent increase in police officers per capita.⁸

Table 3. Estimated elasticities of crime with respect to police

Dependent variable:	Recorded crime per population			Victimisation rate
	(i)	(ii)	(iii)	(iv)
Property crime	0.95** (0.05)	-0.76** (0.16)	-0.84** (0.17)	-0.72** (0.20)
Violent crime	0.84** (0.05)	0.40* (0.17)	0.41* (0.17)	-0.68* (0.29)
Incl. funding formula variables	No	Yes	Yes	Yes
Incl. propensity to report crime	No	No	Yes	-
Number of observations	210	210	210	210

Notes: Estimated using mid-year population of police force areas as weights. Full estimation results for column (iv) are included in the appendix. Standard errors between parentheses. Standard errors are adjusted for correlation within police force areas. + P<.10; * P<.05; ** P<.01.

The first column shows the estimated effect on recorded crime without controlling for the police funding formula variables. We find a positive effect of police on both property and violent crime. A positive effect is to be expected since the budgeting process is meant to benefit police force areas with relatively unfavorable levels of crime and trends in crime.

If we control for endogeneity and heterogeneity in the relation between police and crime by including the funding formula variables, the estimated elasticity for recorded property crime becomes negative (column ii). The estimate suggests that an increase in police personnel per capita by 1 percent results in 0.8 percent less recorded property crime per person, confirming the findings of earlier studies explicitly addressing reverse causality in the relation between police and crime, including Corman and Mocan (2000, 2005), Levitt (2002) and Lin (2009). We do not find a negative effect of police on violent crime, rather, the estimated effect is strongly positive. A positive coefficient suggests a particularly strong reporting and recording bias in police statistics on violent crime, something we will discuss below.

To partially control for measurement error in police recorded crime statistics, we include the propensity to report a crime to the police as an additional variable in the third specification (separate for property and violent crime). Information on victim reporting behavior is gained from the BCS, in which respondents are asked whether they reported an offence to the police or not. Keeping the reporting rate constant, we find an effect of police on recorded property crime that is slightly larger (column ii vs. iii). The small change in the estimated effect

⁸ The elasticity is defined as the estimated coefficient for $\ln p$ (as in table A3 in the appendix) divided by the average value of either property or violent crime (as in the summary statistics in table 1).

suggests that a higher number of police does not induce victims to report many more crime incidents to the police. The implied elasticity of reporting property crime with respect to police is 0.01, meaning that a 10 percent increase in police staff results in a 0.1 percent increase in crime reported. This victim reporting bias is much smaller than what Levitt (1998: 67, 71) finds using Uniform Crime Report data from the US.

We find no evidence for a victim reporting bias in the case of violent crime. The estimated elasticity remains unchanged (column ii vs. iii). Apparently, higher police levels do not induce victims to report more violent crimes. As we discuss in the below, the results for violent crime appear to be much more affected by a police recording bias than a victim reporting bias, which is in line with what Levitt (1998: 76) finds for assault.

To also control for changes in recording behavior of the police, we estimate the effect of police on victimization of crime rather than police recorded crime.⁹ As said before, victimization data are neither affected by changes in victim reporting, nor by changes in police recording. Before discussing the results, it should be noted that using victimization data does not only address measurement error in recorded crime data, but may also affect estimation of the effect in other ways. The effect of police on victimization of crime may be lower than the effect on recorded crime, because the police may not be able to do much about incidents that go unreported. Some 60 percent of all crime is not reported to the police. The most frequently mentioned reasons for not reporting crime are that the incident is 'too trivial', 'private', 'no loss' is involved, or that the 'police would not or could not do anything' (Kershaw et al. 2008). Thus, using victimization data rather than police recorded crime may eliminate an upward bias in the estimated effect resulting from measurement error, but may also result in a lower impact of police on crime.

We find a negative impact of police on victimization of property crime (column iv). A one percent increase in police per capita is estimated to result in a 0.7 percent decrease in victimization of property crime. Even when including a great number of unreported and unrecorded thefts in our definition of crime, many of which were not worthy of police attention in the perspective of victims, we find a largely similar elasticity of police with respect to property crime (column iii vs. iv). Apparently, a wider definition of crime does not affect the average marginal effect of one additional police officer, i.e. the police have a similar effect on reported and unreported crime. The similar elasticities for police recorded crime and victimization of crime also imply that not only the victim reporting bias, but also

⁹ The British Crime Survey also includes victim feedback on which reported crimes have been recorded by the police. Besides the many missing observations, these survey responses do not provide a reliable indicator of police recording practices, because victims have a limited understanding of these practices. In many cases, crimes that have reportedly been recorded by the police cannot be traced in police statistics (Schneider et al. 1978). Additionally, some recorded crime has not been reported by the public, but is the result of police efforts. For these reasons, we do not include the police recording rate from the BCS in our analysis.

the police recording bias is close to zero in the case of property crime. Thus, police do not record many more property crimes when their level of resources grows.

We find a large and statistically significant impact of police on victimization of violent crime that is similar to the estimated effect on property crime (column iv). Comparing this estimate with the estimated impact on police recorded violent crime (column iii vs. iv) suggests that recording practices of the police introduced a strong upward bias in the estimated effect. Assuming the police to have a similar impact on recorded violent crime and victimization of violent crime as in Levitt (1998), the implied elasticity of recording violent crime with respect to police is 0.7. Thus a 10 percent increase in police staff results in a 7 percent increase in crime recorded. This police recording bias is about a factor of two bigger than what Levitt (1998: 76) finds for the US. The implied police recording bias is also smaller in previous studies into the effect of police on crime discussed in section 2. After all, the estimated effect of police on recorded violent crime reported in these studies tends to be weak but negative, not positive, as we find (column ii).

A possible explanation for the particularly strong implied positive relation between police levels and police recording of violent crime is an overhaul of police recording practices starting in April 2002 in combination with a greater priority attached to fighting violent crime (Kershaw et al. 2008, 60). Whereas reporting and recording practices for property crime did not change much, the propensity of victims to report violent crime and the propensity of police to record violent crime strongly increased. The percentage of all violent incidents experienced by respondents of the British Crime Survey that are both reported to the police and recorded by the police doubled during 1999-2005. Well-staffed police forces may have been able to pro-actively record more violent crime than low-staffed police forces, resulting in the recording bias.

6. Sensitivity analysis

As a test of the robustness of our findings, we re-estimate our model using a number of alternative specifications. The results are reported in table 4.

Table 4. Sensitivity analysis of the elasticity of victimization of crime with respect to police

	(i) Preferred specification	(ii) Additional control variables	(iii) Incl. actual allocated budget	(iv) Excluding London	(v) Linear functional form
Property crime	-0.72** (0.20)	-0.69** (0.20)	-0.58** (0.22)	-0.51* (0.22)	-0.80** (0.19)
Violent crime	-0.68* (0.29)	-0.73** (0.30)	-0.67* (0.31)	-0.70* (0.31)	-0.70** (0.28)
Number of observations	210	210	210	205	210

Notes: Estimated using mid-year population of police force areas as weights. Standard errors between parentheses. Standard errors are adjusted for correlation within police force areas. + P<.10; * P<.05; ** P<.01.

Although we cannot directly test the assumption of exogeneity of the variation in police levels remaining after controlling for the funding formula variables and levels of supplementary and local funding, we can test whether the results are robust to including additional variables that are commonly associated with crime, but are not directly related to the variables in the police funding formula. If the remaining variation in police levels is truly exogenous, then including additional variables should not affect our results. For this purpose, we selected four additional variables from the British Crime Survey, including the proportion of population that is of non-white ethnicity; the proportion of population that visits nightclubs frequently; the proportion of population reporting that lack of school discipline are problems or causes of crime; and the proportion of population reporting that drug use or drug dealing are problems or causes of crime. Including these additional control variables does not affect our results: the differences in the estimated elasticities are small and not statistically significant (column i vs. ii). Thus our findings are robust to including other determinants of crime that are not part of the police funding formula.

So far we have assumed that the endogeneity in the relation between police and crime is captured by the police funding formula variables – together with levels of supplementary and local funding. It could be, however, that budgeting decisions at the national level that deviate from what the police funding formula prescribes are made in an attempt to better address divergent local crime trends. The funding formula is relatively inflexible. Over the years, adjustments to the budget and the number of special grants have grown in response to demands from police authorities. If these changes in the allocation of the national police budget reflect differences in local crime trends, then we will underestimate the effect of police on crime. In other words: some of the variation in police levels remaining after including the police funding formula variables (and levels of supplementary and local funding) may actually be endogenous. To test how robust our findings are to this possible source of endogeneity, we also include the total budget allocated to police authorities (including adjustments, top-ups, etc.) in the estimation equation. The results in column (iii) show that the elasticities of crime with respect to police remain largely similar when excluding budgetary adjustments from the remaining variation in police levels. So the remaining variation in police levels that we use to identify the effect of police is the result of the process of translating financial resources into police staff rather than budget decisions that deviate from the police funding formula

As a third test of the robustness of our results, we exclude London from the analysis. London is different from other police force areas in many ways, with twice as many police officers per capita as most of the other areas for instance. The estimation results in the fourth column in table 4 shows that the elasticity remains unchanged for violent crime and decreases for property crime when excluding London. The difference in the estimated effect for property crime is not statistically significant.

Additionally, we estimate the effect using a linear rather than a linear-log functional form. The linear-log specification imposes diminishing returns on the relation between police and crime. Thus a proportional increment in the number of officers per population is assumed to have a smaller effect on the natural logarithm of the number of police for a higher base level of police. The estimation results in column (v) show that there are only small and statistically insignificant differences between the linear and linear-log specification. This is not surprising given the small remaining variation in police levels.

7. Conclusions

We show the existing lack of empirical evidence of an impact of police on violent crime to be the result of measurement error in police statistics on crime. In particular, we find the estimated impact of police on violent crime to be attenuated by a positive relation between number of police and the propensity of police to record violent crime. Apparently, a greater number of police not only deters crime but also increases the share of crime that finds its way into police statistics. The result is that the true effect of police on violent crime is underestimated. We do not find evidence for a similar bias in the estimated effect of police on property crime.

The recording bias for violent crime seems to be particularly strong for England and Wales during the period studied, but is by no means exclusive to this region. For the US, Levitt (1998) finds evidence for a similar albeit less strong recording bias in Uniform Crime Report (UCR) data. Based on the same US data, O'Brien (2006) concludes that changes in law enforcement agencies rather than changes in the rates of violent crime incidents determine trends in UCR violent crime rates. Even with a police recording bias halve the size of what we find, which is more in line with Levitt (1998), an estimated elasticity of violent crime with respect to police of for instance -0.5 would increase to -0.9 when keeping the crime recording rate constant. Clearly, the impact of measurement error in police statistics is large, and cannot be ignored when studying the impact of police on crime.

When we use crime survey data rather than police recorded crime data for the same set of police force areas and the same period, we find the estimated effect of police on violent and property crime to become similar in size. We estimate a one percent increase in police per capita levels to result in a 0.7 percent decrease in both victimization of property crime and victimization of violent crime. Crime data from victimization surveys do not suffer from measurement error like police statistics as they relate to victimization of crime irrespective of whether the crime was reported to the police or not. Thus the estimation bias inherent in police recorded crime data is absent when using survey data on crime. We do not find the wider definition of crime in victimization data, which also includes all non-reported crime, to substantially alter the estimated impact of the police, which suggests that the police have a similar effect on reported and unreported crime.

Knowing how police resources are allocated allows us to use only the variation in police levels that was unintended to identify the effect on crime. Although the yearly deviations from staffing levels predicted by the funding formula are small,

we find a clear and substantial effect of police on crime. Our estimation results are robust to including additional variables commonly associated with crime, but not directly related to the variables in the police funding formula, suggesting that the difference between predicted and actual police levels is truly exogenous to crime levels and trends. Our findings are also not found to be affected by budget decisions that deviate from the funding formula, a practice that has become more common over the years.

Again, when explicitly addressing endogeneity, the police are found to be effective in bringing down common crime, which provides further evidence of the benefits of the current large-scale public expenditures on policing. Using a different method of identification in a different setting, our analysis confirms the findings of earlier studies into the effect of more or less permanent changes in overall police levels on (recorded) property crime, including Corman and Mocan (2000, 2005), Levitt (2002) and Lin (2009).

Finding the impact of police likely to be underestimated is most previous studies has the effect of making the benefit-cost ratio of expenditures on the police even more favorable (compare Levitt 1997).¹⁰ The results of our study should not be read as a plea for expanding the number of police, however. Improving police effectiveness – by better targeting of scarce police resources on crime ‘hot spots’, for instance – may well be a more cost effective way of bringing down crime (see Corman and Mocan [2005] for a rare study encompassing the effectiveness of both policy options; see Weisburd and Eck [2004], for a recent overview of studies into the effects of policing strategies). When comparing the costs and benefits of expanding the number of police versus increasing police effectiveness, it should be noted that studies into the impact of alternative policing strategies may well suffer from a similar victim reporting and police recording bias in police statistics as studied in this paper.

References

- Chartered Institute of Public Finance Accountants. 2004. *Police statistics: actuals 2003/2004*. London.
- Corman, Hope, and H. Naci Mocan. 2000. “A time-series analysis of crime, deterrence, and drug abuse in New York City.” *American Economic Review* 90: 584-604.
- Corman, Hope, and H. Naci Mocan. 2005. “Carrots, sticks, and broken windows.” *Journal of Law and Economics* 48: 235-266.
- Di Tella, Rafael, and Ernesto Schargrodsy. 2004. “Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack.” *American Economic Review* 94: 115-133.
- Donahue, John J., and Jens Ludwig. 2007. More COPS. Policy Brief no. 158. Washington D.C.: The Brookings Institution.

¹⁰ Taking estimates of the costs of property and violent crime from Dubourg, Hamed, and Thorns (2005), we find a 1 percent increase in police resources at a cost of 100 million pound to result in a decrease in crime with a value of 190 million pound, although this excludes possible additional ‘downstream’ costs accruing to legal and correctional services.

- Draca, Mirko, Stephen J. Machin, and Robert Witt. 2008. "Panic on the Streets of London: Police, Crime and the July 2005 Terror Attacks." Working Paper no. 3410 (updated version February 2009). IZA Institute for the Study of Labor, Bonn.
- Dubourg, Richard, Joseph Hamed, and Jamie Thorns. 2005. The economic and social costs of crime against individuals and households 2003/04. Home Office Online report no. 30/05. London: Home Office.
- Gibson, John, and Bonggeun Kim. 2008. "The effect of reporting errors on the cross-country relationship between inequality and crime." *Journal of Development Economics* 87: 247-254.
- Greene, William H. 2000. *Econometric Analysis*. Upper Saddle River, NJ.: Prentice Hall,.
- Hausman, Jerry. 2001. "Mismeasured variables in econometric analysis: problems from the right and problems from the left." *Journal of Economic Perspectives* 15: 57-67
- Hausman, Jerry, and Guido Kuersteiner. 2004. "Difference in difference meets generalized least squares: higher order properties of hypothesis tests." Working paper. Massachusetts Institute of Technology, Cambridge, Mass.
- Home Office. 2006a. *Police service strength England and Wales 31 March 2006*. Statistical Bulletin, London: Home Office.
- Home Office. 2006b. *The police grant report 2006/07 (England and Wales)*. London: The Stationary Office.
- Kershaw, Chris, Sian Nicholas, and Alison Walker. 2008. Crime in England and Wales 2007/2008. Findings from the British Crime Survey and police recorded crime. London: Home Office.
- Klick, Jonathan, and Alexander Tabarrok. 2005. "Using terror alert levels to estimate the effect of police on crime." *Journal of Law and Economics* 48: 267-279.
- Levitt, Steven D. 1997. "Using electoral cycles in police hiring to estimate the effect of police on crime." *American Economic Review* 87: 270-290.
- Levitt, Steven D. 1998. "The relationship between crime reporting and police: implications for the use of Uniform Crime Reports." *Journal of Quantitative Criminology*. 14: 61-81.
- Levitt, Steven D. 2002. "Using electoral cycles in police hiring to estimate the effect of police on crime: reply." *American Economic Review* 92: 1244-1250.
- Levitt, Steven D., and Thomas J. Miles. 2006. "Economic contributions to the understanding of crime." *Annual Review of Law and Social Science* 2: 147-164.
- Lin, Ming-Jen. 2009. "More police, less crime: evidence from US state data." *International Review of Law and Economics* 29: 73-80.
- MacDonald, Ziggy. 2002. "Official crime statistics: their use and interpretation." *The Economic Journal* 112: F85-F106.
- O'Brien, Robert M. 2006. "Police productivity and crime rates: 1973-1992." *Criminology* 34: 183-207.
- Schneider, Anne L., William R. Griffith, David H. Sumi, and Janie M. Burcart. 1978. Portland forward records check of crime victims. National Institute of Law Enforcement and Criminal Justice. Washington D.C.: U.S. Department of Justice.

Thorpe, Katharine, and Chandni Ruparel. 2005. Reporting and recording crime. Chapter 3 in: Sian Nicholas, David Povey, Alison Walker, and Chris Kershaw, *Crime in England and Wales 2004/2005*. London: Home Office. 35-47.

Vollaard, Ben, and Pierre Koning. 2009. "The effect of police on crime, disorder and victim precaution. Evidence from a Dutch victimization survey." *International Review of Law and Economics* 29 (forthcoming).

Weisburd, David, and John E. Eck. 2004. "What can police do to reduce crime, disorder and fear?" *The Annals of the American Academy of Political and Social Science* 59: 42-65.

Appendix

Findings from some previous studies into the impact of police on crime

Table A1 The elasticity of crime with respect to police; estimation results from six recent studies

Property crime		coefficient	t-stat.	Violent crime		coefficient	t-stat.
Corman and Mocan 2005	burglary	-0,3	-1,2	Corman and Mocan 2000	assault	-0,3	-1,3
Lin 2009	burglary	-1,6	-2,0	Corman and Mocan 2005	assault	-0,3	-1,4
Klick and Tabarrok 2005	burglary	-0,3	-1,9	Lin 2009	assault	-0,6	-0,8
Corman and Mocan 2000	burglary	-0,4	-3,0	Corman and Mocan 2000	murder	-1,4	-1,5
DiTella and Schargrodsky	car theft	-0,3	-2,6	Corman and Mocan 2005	murder	-0,5	-0,5
Corman and Mocan 2005	car theft*	-0,6	-2,3	Lin 2009	murder	-2,7	-2,1
Lin 2009	car theft	-4,1	-2,3	Corman and Mocan 2005	rape	-0,1	-0,3
Klick and Tabarrok 2005	car theft	-0,9	-2,3	Lin 2009	rape	-0,7	-0,9
Corman and Mocan 2000	car theft*	-0,5	-1,4	Corman and Mocan 2005	robbery	-0,4	-0,9
Corman and Mocan 2005	grand larceny	-0,7	-2,7	Lin 2009	robbery	-1,9	-1,7
Lin 2009	larceny	-1,9	-1,9	Corman and Mocan 2000	robbery	-0,5	-1,6
Lin 2009	property crime	-2,2	-2,3	Lin 2009	violent crime	-1,1	-1,5
Levitt 2002	property crime	-0,5	-2,1	Levitt 2002	violent crime	-0,4	-1,9

Note: (*) Reported elasticity relates to motor vehicle theft rather than car theft only.

Description of the police funding formula

For fiscal year 2005/06, the formula has been defined as follows (Home Office 2006b):

$$D = ((A + B + C) * \text{COSTADJ}) + \text{SECEXP} + \text{PENSIONS} * \text{GRANTRATE} * 1.0000862369$$

$$A = \text{DAYTIMEPOP} * (30.49 + 0.85 * \text{INDEX1} + 1.28 * \text{INDEX3} + 32.49 * \text{RENTING} + 107.33 * \text{OVERCROWDED} + 5.47 * \text{STRIVING} + 116.36 * \text{YNGMUNEMPL} + 0.79 * \text{POPDENS} - 2.84 * \text{POPSPARS})$$

$$B = \text{RESPOP} * (37.76 + 1.24 * \text{INDEX2} + 2.43 * \text{POPDENS} + 4.69 * \text{POPSPARS})$$

$$\text{INDEX1} = (\text{TERRACED} - 0.3079) / 0.0627 + (\text{STRIVING} - 0.2019) / 0.0894 + (\text{LONEPARENT} - 0.0614) / 0.0145 + (\text{ONEADULT} - 0.3024) / 0.0342$$

$$\text{INDEX2} = (\text{STRIVING} - 0.2019) / 0.0894 + (\text{LONEPARENT} - 0.0614) / 0.0145 + (\text{RENTING} - 0.3213) / 0.0632 + (\text{LTUNEMPL} - 0.1509) / 0.0376$$

$$\text{INDEX3} = (\text{TERRACED} - 0.3079) / 0.0627 + (\text{STRIVING} - 0.2019) / 0.0894 + (\text{LONEPARENT} - 0.0614) / 0.0145 + (\text{RENTING} - 0.3213) / 0.0632 + (\text{ONEADULT} - 0.3024) / 0.0342 + (\text{UNEMPL} - 0.0255) / 0.0095$$

$$C = 1388.76 * \text{ROADS} + 13908.75 * \text{MOTORWAYS}$$

The GRANTRATE reflects the proportion of police revenue expenditure for 2005/06. It is a scalar and excluding it does not affect the estimated effect of police on crime. Since we focus on number of police personnel rather than police expenditures, we abstract from the area cost adjustment (COSTADJ). This adjustment reflects differences in the costs of providing police across the country.

If we rearrange the elements of the funding formula into one expression, we find equation (2), which is included in section 4. The equation includes twenty individual variables. Five variables appear twice, once unweighted and once weighted by the (time varying) ratio of daytime population over resident population: STRIVING, LONEPARENT, RENTING, POPDENS, POPSPAR.

Summary statistics for police funding formula variables

Table A2 defines all variables included in the funding formula and provides summary statistics.

Variable	Full name	Definition	Mean	Standard deviation
DAYTIMEPOP	Daytime population	Resident population + persons working but not resident in area - persons resident but working outside area + overnight visitors + day visitors (mln.)	1.40	1.35
LONEPARENT	Residents in lone parent families	Proportion of household residents living in lone parent family	0.07	0.02
LTUNEMPL	Long term unemployment benefit claimants	Proportion claiming benefits for over one year	0.19	0.05
MOTORWAYS	Motorway lengths	Length of trunk and principal motorways	86.65	59.66
ONEADULT	One adult households	Proportion of households containing only one person aged 16 yrs. or over	0.34	0.06
OVERCROWDED	Residents in overcrowded households	Proportion of household residents living in accommodation with more than one person per room	0.05	0.03
PENSIONS	Police pensions	Pension obligations (mln.)	66.1	86.0
POPDENS	Population density	Number of residents per hectare	5.80	3.23
POPSPARS	Population sparsity	Ratio resident population in authorities with 0.5 or less residents per hectare over total resident population	0.20	0.17
RENTING	Households renting	Proportion of households living in rented accommodation	0.36	0.09
RESPOP	Resident population	Resident population (mln.)	1.25	1.09
ROADS	Built-up roads lengths	Length of roads subject to speed limit not exceeding 40 mph (thousand)	5.79	3.74
SECEXP	Security expenditure	Expenditure on security-related commitments	282	598
STRIVING	Striving population	Proportion of residents living in areas classified as striving	0.23	0.10
TERRACED	Residents in terraced accommodation	Proportion of household residents living in unshared terraced dwellings	0.34	0.08
UNEMPL	Unemployment benefit claimants	Ratio benefit claimants over population 18-64 yrs.	0.03	0.01
YNGMUNEMPL	Young male unemployment benefit claimants	Proportion of benefit claimants who are male and aged under 25 yrs.	0.19	0.03

Note: All variables, excluding resident and daytime population, are weighted by population.

Full estimation results

The table below includes the full estimation results for the estimated effect of police on victimization of crime reported in table 3 (column iv).

Table A3. Estimated effect of police on victimization of crime

Dependent variable:	Property crime		Violent crime	
ln (Police personnel, t-1)	-0.10**	(0.03)	-0.03*	(0.01)
<i>Funding formula variables (t-2)</i>				
Striving population	2,14 ⁺	(1,14)	-0,45	(0,63)
Lone parent households	-2,33	(10,55)	9,29 ⁺	(5,64)
Households renting	-3,55 ⁺	(2,06)	-1,84	(1,22)
Long term unemployment	-0,30**	(0,09)	-0,01	(0,04)
Population density	0,22**	(0,04)	0,02	(0,02)
Population sparsity	1,19*	(0,58)	0,21	(0,34)
Built-up roads length	-4,87	(4,74)	-1,34	(2,55)
Motorway length	136,85**	(30,92)	14,05	(22,97)
Security expenditure	76,89 ⁺	(42,58)	41,42 ⁺	(24,89)
Police pensions	0,00	(0,00)	0,00	(0,00)
Residents in terraced houses*	-0,08 ⁺	(0,04)	0,01	(0,02)
Striving population*	-2,22*	(1,04)	0,46	(0,58)
Lone parent households*	1,90	(9,46)	-8,44 ⁺	(5,71)
One-adult households*	0,75**	(0,16)	0,14	(0,09)
Households renting*	3,62 ⁺	(1,88)	1,58	(1,11)
Unemployment*	0,19	(0,46)	-0,52*	(0,22)
Overcrowded households*	1,71**	(0,25)	0,08	(0,13)
Young male unemployment*	0,56**	(0,15)	0,08	(0,09)
Population density*	-0,19**	(0,04)	-0,02	(0,02)
Population sparsity*	-1,15*	(0,53)	-0,19	(0,31)
<i>Supplementary and local funding (t-2) (in £ 1,000)</i>				
Street Crime Initiative	4.53	(4.84)	-5.57*	(2.64)
Community Support Officers	-9.03 ⁺	(4.90)	-0.34	(2.41)
South East Allowance	27.94**	(4.57)	-1.19	(2.51)
Police council tax	0.07	(0.17)	-0.29**	(0.08)
Year 2002	0.00	(0.00)	-0.00 ⁺	(0.00)
Year 2003	-0.02**	(0.01)	-0.00 ⁺	(0.00)
Year 2004	-0.04**	(0.01)	-0.01	(0.00)
Year 2005	-0.04**	(0.01)	-0.01*	(0.00)
Number of observations	210		210	

Notes: (*) Per ratio daytime over resident population. All variables per population, except for population density and sparsity. Observations are weighted by midyear population and parameters are estimated using generalised least squares. Standard errors are adjusted for correlation within police force areas. Standard errors between parentheses. + P<.10; * P<.05; ** P<.01.