

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

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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. Using both police statistics and a victimization survey as sources of crime data, we provide evidence that measurement error in police statistics on crime explains the divergence in findings. In particular, the rate at which violent crime is recorded is found to be more susceptible to police levels than the recording rate for property crime. Unlike police statistics, data on victimization of crime from the British Crime Survey are not affected by public recording or police recording. Using deviations from funding formula-predicted police levels as instrument for police, we find the police to have an effect on victimization of both property and violent crime, with a 1 percent increase in police resulting in a 0.6 percent decrease in property crime and a 0.9 percent decrease in violent crime.

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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 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 terrorist 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).

Whereas the effect of police on property crime is consistently found to be negative, the evidence for an effect of police on violent crime is weak and inconsistent. Most studies find an effect of police on property crime, but no effect on violent crime, including Levitt (1997, 2002), Corman and Mocan (2000, 2005), Klick and Tabarrok (2005), and Lin (2009).¹ If an effect on violent crime is identified, it is often found to be only 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. Of the 22 recent estimates of the effect of police on

violent crime reviewed in Appendix A of this paper, we found only 5 to be statistically significant at the 5 percent confidence level or higher. In contrast, of the 19 reviewed estimates of the effect of police on property crime 14 were statistically significant at this confidence level.

A number of explanations have been advanced to account for the commonly found divergence in the estimated impact on property crime and violent crime. 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. DiTella and Schargrotsky (2004, 118) suggest that property crimes like car theft are more susceptible to police presence than other crimes since a parked car gives criminals time to gather information on areas in which they intend to commit crimes. Other crimes, including violent crime, may not provide similar favorable conditions for offender planning, including strategies to avoid the police.

In this paper, we present and test a different explanation for the relatively weak and inconsistent evidence for the impact of police on violent crime: measurement error in police recorded crime. The existing literature is exclusively based on police statistics as source of crime data.² As is well known, and has been extensively studied, police statistics provide an imperfect picture of crime, and of crime trends in particular. Police recorded crime excludes all crime incidents that victims do not report to the police and all those incidents the police do not record,

altogether some 70 percent of all crime incidents reported by victims in surveys (Kershaw, Nicholas, and Walker 2008). Changes in public reporting and police recording have been shown to have a profound impact on the rate of recorded crime (O'Brien 1996; MacDonald 2002; Tarling and Morris 2010). If reporting and recording of crime are affected by the number of police, as Levitt (1998) finds when comparing police statistics with victimization survey data, then the estimated effect of police on crime will be biased.

Police manpower and crime recording may be positively related for a number of reasons. First, greater police presence makes it more likely for crime to be observed by the police. Second, the availability of staff at the police station may affect the likelihood that citizen incident reports are officially recorded in the crime statistics. In addition, if greater police levels boost public confidence in police, then reporting rates may go up as well: MacDonald (2001) finds victims who hold positive attitudes toward the police to be more likely to report their victimization. Third, it is easy to imagine that well-staffed police departments are in a better position to improve their recordkeeping than low-staffed departments. The technical or organizational innovations underlying better recording practices may be most easily implemented in well-staffed police departments. As noted by O'Brien (1996), much of the increase of police numbers in the 1970s to 1990s in the US occurred in areas of dispatching, recordkeeping, and service to those reporting criminal incidents. Consequently, increases in police are likely to have resulted in a higher rate at which crime incidents are recorded.

Unlike police statistics, data on victimization of crime from the British Crime Survey (BCS) are not affected by public recording or police recording practices. 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. Using both survey data on crime from the BCS and police recorded crime statistics, we test whether estimates of the crime reducing effect of police are biased and whether the bias differs between violent crime and property crime.

We identify variation in police that is exogenous to crime by exploiting knowledge of the budgeting process. In England and Wales, the national police budget is distributed among the police forces with the use of a funding formula. The formula includes a number of indicators of need for police services, including the rate of unemployment. The formula establishes a firmly positive relation between police levels and regional crime rates. In practice, police levels deviate from formula-predicted police levels as changes in personnel do not follow budget decisions instantaneously – for reasons that are unrelated to crime. It takes considerable time to hire and train police officers, for instance. Arguably, unintended variation in police levels is exogenous to crime. We use the temporary deviations as instrument for police.

Studies relying on major events such as terrorist attacks as source of variation in police levels 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. Our approach is related to Evans and Owens (2007) who use federal law enforcement grants as an instrument for police. Compared to instrumental variables such as the number of firefighters (Levitt 2002) and state sales tax revenues (Lin 2009), instruments based on the police budgeting process have a more direct, certain and transparent impact on police numbers.

We find a greater number of police to not only deter crime but also to increase the share of violent 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 of 0.6 percent and violent crime of 0.9 percent.

Our paper is most closely related to the work by Levitt (1998). Using data from the FBI's Uniform Crime Reports (UCR) and the U.S. National Crime Victimization Survey (NCVS), Levitt provides evidence for an effect of the number of police on public reporting of 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 police recording of crime is also affected by police numbers. Taking the effect on public reporting and police recording together, a 10 percent increase in police levels is estimated to result in a 3 percent increase in recording of some violent crimes, including assault.

Measurement error in police recorded crime has been found to bias other tests of the economic model of crime as well. MacDonald (2001) finds that individuals who are not in the labor market are less likely to report property crimes compared to individuals who are in work. Given the negative relation between economic conditions and victim reporting, the effect of unemployment on crime is likely to be underestimated when using police statistics as source of crime data. 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. Gibson and Kim (2008) are innovative in providing a direct test of the effect of measurement error in police statistics on the outcome of interest.

The finding that crime reporting and recording vary systematically with determinants of crime, including deterrence variables, provides a rationale for a direct test 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 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. In Section 2, we discuss the ways in which measurement error may have affected previous empirical work. Section 3 outlines the data. Section 4 introduces the research design and presents the estimation results. In Section 5, we conduct direct tests of the estimation bias resulting from measurement error in police recorded crime. We present a number of sensitivity tests in Section 6 and discuss the broader implications of our findings in the concluding section.

2. Measurement error and the effect of police on crime

As discussed in the introduction, higher staff levels could affect reporting and recording of crime in multiple ways, including better capabilities of relatively well-

staffed police departments to record crime, growth in staff levels enabling a police department to deal with more citizen's incident reports, and greater police presence going together with a greater likelihood of catching an offender in the act. The use of police recorded crime data results in biased estimates of the effect of police on crime if reporting and recording practices are systematically related to police levels (Hausman 2001).

[TABLE 1]

Levitt (1998) provides empirical evidence for a relation between police numbers and crime reporting and recording. Using city-level data for 1971-1992, Levitt (1998) estimates how strongly reporting and recording of several types of crime are affected by police numbers. The crime recording bias varies from 0 in the case of car theft (and presumably homicide) to 0.3 to 0.4 for rape and, somewhat surprisingly, burglary, meaning that a 1 percent increase in police officer per capita is estimated to result in an increase in recorded crime of 0.3 to 0.4 percent.³ When we plot the estimated biases against the average estimated effects of police on crime reported in a number of recent studies reviewed in Table 1, we find a strong positive relation between the two. Figure 1 shows that a larger reporting and recording bias goes together with a lower estimated effect of police on recorded crime, suggesting that measurement error is a source of estimation bias.

[FIGURE 1]

Figure 1 shows no simple divide between property and violent crimes. Just like not all violent crimes are similarly affected by the reporting and recording bias, with homicide as the obvious exception, not all property crimes are immune to this bias either. The figure suggests that the true effect on burglary may be underestimated when using police statistics on crime.

The variation across crime types shown in Figure 1 provides a stronger indication for the presence of an estimation bias through the use of police statistics than the 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 1 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.

In a few cases, a statistically significant effect is found for violent crimes that Levitt (1998) finds to be susceptible to a reporting and recording bias, including assault in Evans and Owens (2007) and in Draca, Machin, and Witt (forthcoming). If the estimated effects in these papers are biased, then the

recording bias did not preclude statistically significant results. Levitt's (1998) findings do imply that many of the reported elasticities are underestimates of the true effect of police on crime, however. The bias can be substantial. For instance, if we take Levitt's (1998) estimated bias in police recording of assault, the true effect of police on assault could be twice as large when addressing measurement error in police recorded crime.

The relationship in Figure 1 does not imply that in all studies the channels by which police levels affect public reporting and police recording are exactly the same. After all the studies differ greatly in the nature and duration of changes in police levels studied. Some studies rely on long-term changes in overall police levels covering several decades, and using instrumental variables (Levitt 2002; Lin 2009) or high-frequency data (Corman and Mocan 2005) to identify exogenous variation in police levels. Evans and Owens (2007) follow a similar approach, albeit with data spanning a period of 12 years rather than decades. Other studies analyze what happens to crime before and after a major event induced a temporary change in visible police presence (DiTella and Schargrotsky 2004; Klick and Tabarrok 2005; Draca, Machin, and Witt forthcoming). Typically, these studies cover a period of a couple of months.⁴

The studies based on data for multiple years or even decades are most likely to be affected by the technical and organizational changes behind the increasing rate at which police departments have been improving their recording of violent

crime (O'Brien 1996). The studies of short-term shocks in police levels are more likely to be affected by the greater chance of intercepting a brawl or other crime incident as a result of greater police presence. In addition, in the immediate aftermath of a terrorist attack, reporting practices may change because of increased vigilance on the part of citizens (Klick and Tabarrok 2005, 276). Despite these differences, the positive relation in Figure 1 holds true even for the subset of studies focusing on short-term shocks in police. Exactly how staffing levels affect public reporting and police recording remains largely unknown, however, making it impossible to draw firm conclusions on how measurement error differs from setting to setting. The more or less permanent changes in overall police levels studied in this paper most closely resemble the work by Corman and Mocan (2000, 2005), Levitt (2002), Evans and Owens (2007) and Lin (2009).

3. Data

The analysis is based on data at the police force area level. In England and Wales the police are organized in regional forces, loosely based on a county structure. On average, a police force employs 3,500 police officers serving 1.2 million inhabitants. We take the City of London Police and the Metropolitan police to be one force, which leaves us with 42 police force areas.

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. We include data for fiscal year 2001/2002 to fiscal year 2006/2007. The fiscal year runs from April to March. Police officers do not include traffic wardens, civilian staff or community support officers who do not have the power to arrest. The Home Office provided data on supplementary funding (or: specific grants) of the police forces. As is shown in Figure 2, England and Wales experienced strong growth in police levels during the period of the analysis. The total number of police increased by 12 percent during 2001/2002 to 2006/2007. Data on the council tax for police services were obtained from the Department for Communities and Local Government for England and from the Welsh Assembly Government for Wales.

[FIGURE 2]

As sources of crime data, we use police recorded crime statistics and survey data from the 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 six waves of BCS data, from fiscal year 2001/2002 to fiscal year 2006/2007, and police crime statistics for the same years. 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 victimization 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. Given incidence rates of some 2 to 6 percent for most crimes 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

simple 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, simple assault, other simple assault, attempted assault, rape, indecent assault and robbery, including attempts. Summary statistics for all variables used in the analysis are provided in Table 2.

For property crime, reporting and recording practices were more or less stable during the period of the analysis. In contrast, the percentage of all violent incidents experienced by respondents of the BCS that are both reported to the police and recorded by the police almost doubled during 2001-2005 (Kershaw, Nicholas, and Walker 2008). Political pressure on the police to better address violent crime has been the key driver behind the change in police recording (*ibid.*, 60). The political attention also led to an overhaul of police recording standards as of April 2002. The new crime recording standard primarily impacted statistics on violent and sexual crime. For instance, recording of violence against the person incidents increased by some 20 percent in the first year of the implementation of the standard (Simmons, Legg, and Hosking 2003).

[TABLE 2]

4. Estimating the effect of police on crime

In England and Wales, the national police budget is distributed among the regional police forces with the use of a formula. The regular budget is directly distributed with the formula, and, as discussed below, most supplementary funds are also distributed in line with the funding formula.⁵ The funding formula includes a number of indicators of need for police services, including the percentage of residents living in a lone parent family and the percentage of unemployed claiming benefits for over one year.⁶ All the variables in the funding formula are strong predictors of the local crime rate. Hence the formula establishes a firmly positive relation between police numbers and regional crime rates.

Actual police levels often deviate from funding formula-predicted police levels for reasons that are unrelated to crime, however. First of all, there is a considerable time lag between budget decisions and changes in personnel. It takes time to hire and train police officers, to have their number reduced or to have them transferred. As a result, it can easily take up to two years before changes in funding can be traced in police levels. In other words, budgeting decisions follow the funding formula instantaneously – although police forces sometimes receive transitional funds to absorb major shocks in funding – but the number of police does not. Changes in the boundaries of police force areas are another source of unintended variation in police levels. When the 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. Arguably, unintended variation in actual police levels is exogenous to crime. Hence we can use these temporary deviations from funding formula-predicted police levels as an instrument for police.

We identify deviations from funding formula-predicted police-levels using the following estimation equation:

$$\text{POLICE}_{jt} = Y_{jt} \beta + \gamma \text{Formula grant}_{jt-1} + \zeta_t + \varepsilon_{jt} \quad (1)$$

where *Formula grant* is the level of resources distributed to police force area j in fiscal year t based on the funding formula. The formula grant is lagged by one year as the grant relates to the level of resources at the start of fiscal year t , whereas police levels are measured at the end of the fiscal year. Y is a vector of two sources of funding that are distributed without the funding formula and that may be related to regional crime rates. First, some supplementary funds for the police forces are explicitly targeted at high-crime areas, including funding for the Street Crime Initiative (targeted at ten police force areas with high robbery rates, see Machin and Marie forthcoming) and the special allowance for London and eight neighboring police force areas. We also include the rural policing grant that is targeted at rural, low-crime areas, as it may establish a negative relation between levels of funding and regional crime rates. All other supplementary funds are distributed in line with the funding formula. In the sensitivity analysis, we show our estimates to be robust to including all other sources of national

funding. Second, we control for levels of local funding. The forces receive some 20 percent of their funding from local authorities through a tax on residential properties, the council tax. The local crime rate may feed into debates on changes in this tax rate, which is why we control for this source of funding. Levels of both sources of funding are lagged by one year. Year fixed effects account for changes in the overall level of police funding. The idiosyncratic term ε_{jt} denotes the deviations from police levels predicted by the budgeting process.

[TABLE 3]

The formula grant, together with levels of local and crime-related supplementary funding, explain some 94 percent of the variation in police levels, as shown in Table 3. We use the remaining 6 percent variation in police levels to identify the effect of police on crime. The estimated coefficient for the formula grant in the first row of Table 3 implies that every £40,000 of formula grant funding is related to having one more police officer. In 2005, the annual wage of an police officer was some £35,000. The grant excludes capital funding, which may explain the low level of funding per police officer. The coefficient for local police funding in the second row is negative, which can be explained by the practice of local authorities to make the rate of the police council tax dependent on budget decisions at the national level (Simper 2002). If we exclude the formula grant from equation (1), the coefficient for local police funding becomes positive. In other words: a lower formula grant tends to be offset by a higher police council

tax and vice versa. The relation between crime-related supplementary funds and police levels is not found to be statistically significantly different from zero.

To identify the effect of police on crime, we relate the deviations from formula-predicted police levels from equation (1) to the regional crime rate. We estimate a reduced-form relationship between police levels and crime rates as

$$CRIME_{jt} = X_{jt} \beta + \alpha \text{ Deviations from formula-predicted police levels}_{jt-1} + \zeta_t + \varepsilon_{jt} \quad (2)$$

where $CRIME_{jt}$ represents the number of recorded crimes per population or the average probability of victimization of crime in police force area j and fiscal year t . X is a vector of time-varying economic and demographic characteristics for the police force area, ζ_t are year fixed effects and ε_{jt} is the idiosyncratic error.

As our instrument is already expressed in terms of police numbers, we estimate the reduced-form relationship (2) as our baseline model. Alternatively, we could derive a 2SLS estimate by using deviations from formula-predicted police levels as an instrument for police. As shown in the sensitivity analysis, the 2SLS estimates are highly similar to the baseline estimates.

[TABLE 4]

Table 4 presents the estimation results based on equation (2). All estimates are also expressed in terms of elasticities: the percentage change in crime as a result of a one-percent increase in police per capita.⁷ We find a statistically significant effect of police on recorded property crime. The estimate in column 1 suggests that an increase in police personnel per capita by 1 percent results in 0.75 percent lower rate of recorded property crime. The result is within the range of estimates in previous studies although at the high end (see Table 1). We find no effect of police on violent crime. As discussed previously, finding an effect of police on property crime but no effect on violent crime is typical when using police statistics as source of crime data.

To see whether public reporting and police recording of crime bias these estimates, we use victimization of crime as dependent variable in the second column.⁸ The BCS provides a source of crime data that is not affected by reporting or recording practices. Before discussing the results, it should be noted that using victimization data does not only address measurement error in crime data, but may also affect our results in other ways. The effect of police on victimization of crime may be different from 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, Nicholas, and Walker 2008). In addition, the definitions of

property and violent crime differ slightly between the two sources of crime data (see Section 3).

With this said, we find police to have a strong negative impact on victimization of property crime (column 2). A one percent increase in police per capita is estimated to result in a 0.6 percent decrease in victimization of property crime. The effect is statistically significant at the 95 percent confidence level. The elasticity of property crime with respect to police is somewhat lower when using victimization data rather than police recorded crime, but the difference is not statistically significant. In Section 5 we conduct a direct test of the presence of an estimation bias through reporting or recording of property crime.

Using the British Crime Survey as source of crime data, the negative effect of police on violent crime becomes statistically significantly different from zero (column 2). A one percent increase in police is estimated to result in a 0.9 percent decrease in violent crime, which is higher than found in most previous studies. The difference between the implied elasticities in column 1 and 2 is statistically significant. The large difference indicates that public reporting and police recording of violent crime 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, the implied elasticity of recording violent crime with respect to police is 0.85. Thus a 1 percent higher level of police results in a 0.85 percent higher level of reported and recorded crime, which is higher than

Levitt (1998, 76) finds for the US. Below, we analyze further what is driving the estimation bias.

5. What is driving the bias in police recorded crime?

As a first step in explaining the estimation bias resulting from police statistics on crime, we focus on the relation between public reporting and levels of police. The propensity to report crime can be gained from the British Crime Survey. Respondents are also asked whether they reported crime incidents to the police. Some two out of five of all crime incidents are said to be reported to the police, with reporting rates for property crime stable and reporting rates for violent crime increasing during the period of the analysis (Section 3). The victim feedback allows us to analyze to what extent the bias in the estimated effect for violent crime can be explained by victim reporting behavior. We include the reporting rates for property and violent crime as additional control variables in equation (2). Due to low sample sizes, data on the rate of reporting crime are not complete for all years. To see how much of an impact reporting rates have on the estimated elasticity of crime with respect to police, we exclude observations for which the reporting rate is missing in the baseline model without the reporting rate as well.

Table 5 presents the results. Once controlling for public reporting of property crime, the elasticity of property crime with respect to police becomes slightly larger. The difference between the two estimates is not statistically significant. These findings suggest that a higher level of police has little to no effect on the

reporting behaviour of victims of property crime. The estimated effect of police on recorded violent crime is not at all affected by public reporting of violent crime (column 3 vs. 4). A higher number of police does not induce victims to report more violent crime incidents to the police. In other words, the measurement error in police recorded violent crime is driven by police recording rather than public reporting, which is in line with what Levitt (1998, 76) finds for assault.

[TABLE 5]

To see what was driving the combined public reporting and police recording bias, we conduct a direct test of the relation between police levels and overall crime recording in Table 6. As dependent variable we use the number of police recorded offenses divided by the number of crime incidents from the BCS. The higher this ratio, the more crime incidents experienced by victims are recorded by the police.⁹ Again, we use deviations from funding formula-predicted police levels as source of exogenous variation in police. Hence we test whether quasi-experimental variation in police levels affect the rate at which crime is recorded.

The results in column 1 of Table 6 show that the level of police is unrelated to the share of property crime incidents that are both reported and recorded. The coefficient is small and statistically insignificant. Again, we find no evidence for an estimation bias stemming from the use of police recorded property crime data.

Both public reporting and police recording of property crime do not seem to be responsive to police numbers.

In contrast, the combined reporting and recording bias in violent crime is found to be large (column 2). We find a 1 percent increase in police per capita to result in a 0.4 percentage-point increase in the ratio of crime recorded (coefficient*0.25). That is equivalent to the 0.85 percent increase in recorded violent crime as a result of a 1 percent higher level of police that could be deduced from comparing the effect of police on recorded crime and victimization of crime in Table 3. The bias is fully driven by police recording: we found public reporting of violent crime not to be responsive to police levels in Table 5.

Apparently, in a period of drastic change in police recording, a relatively high level of police per capita was an important determinant of the rate at which violent crime incidents were recorded. Well-staffed police forces could probably more easily implement the organizational and technical changes necessary to respond to the call for more complete recording of violent crime incidents.

[TABLE 6]

6. Sensitivity analysis

As a test of the robustness of our findings, we re-estimate our baseline model using a number of alternative specifications.

Although the funding formula is leading in distributing the national police budget, some minor adjustments are often made to the police forces' budgets. Ad hoc amendments are made for a host of reasons, including funds to cushion drops in budgets and to help police forces adjust to boundary changes. Although very small in terms of the overall budget, these auxiliary budget decisions may introduce endogeneity in the relation between police and crime that equation (1) does not account for. To test the robustness of our results to these amendments, we include total national funding in equation (1) rather than the formula grant and the crime-related supplementary funds. The results in row (2) of Table 7 show that the estimated effect of police on crime can be less precisely estimated, which is to be expected since the remaining variation in police levels is smaller, but that the size of the effect is hardly affected. Thus our results are robust to budgeting decisions that are not explicitly modelled in equation (1).

As the time dimension of our panel data is limited, we exploit both cross-sectional and time variation to identify the effect of police on crime. We address the issue of reverse causality by explicitly modelling the process of distributing police resources across the police force areas. As an additional test on whether our results are biased through some unaccounted for simultaneity in the budgeting process, we include region-fixed effects in equation (2). The results in the third row of Table 7 show that limiting the analysis to variation within police force areas leads to similar estimates that are only less precise. These results provide

evidence that both the cross-sectional and time variation are of similar nature and can be captured by modelling the budgetary process.

As stated in section 4, rather than estimating the reduced form relationship between police and crime as in equation (2), we can derive 2SLS estimates using deviations from funding formula-predicted police levels as an instrument for police. As shown in row (4) of Table 7, the 2SLS estimates are slightly higher than the reduced form estimates, but the differences with the baseline estimates are not statistically significant.

As a last 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 the number of police officers per capita as most other police force areas for instance. The parameter estimates in row (5) in Table 7 show that the differences in the estimated effects on crime are small and not statistically significant.

[TABLE 7]

7. Conclusions

We provide evidence that a greater number of police not only deters crime but also increases the share of violent 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.

Our findings provide an explanation for the contrasting findings for property and violent crime in many studies into the effect of police on crime. Often the police are found to have a robust negative effect on property crime, but no or a weak effect on violent crime. Measurement error in police statistics on violent crime is shown to be one of the factors driving this divergence in findings. It should be noted that at lower levels of aggregation, some crime types do not fit the simple divide between property and violent crime, including homicide and, as suggested by Levitt (1998), burglary. These exceptions make up only a small part of overall violent and property crime, giving rise to the contrasting evidence for the two broad crime categories.

Our paper is unique in providing a direct test of how robust the estimated effect of police on crime is to the source of crime data. We use both police recorded crime statistics and crime data from a victimization survey for the same set of areas, the same period, and similar crime categories. Crime data from victimization surveys relate to victimization of crime irrespective of reporting and recording of crime. Thus the estimation bias inherent in police recorded crime data is absent when using survey data on crime.

We find the crime recording bias to be substantial. Keeping everything else constant, including the effect of police on the actual crime rate, a one percent higher level of police staff per capita is estimated to result in a 0.85 percent higher rate of recorded violent crime. This is equivalent to a 0.4 percentage-point increase in the rate at which violent crime incidents experienced by respondents of the British Crime Survey are recorded by the police. Further analysis of our findings suggest that well-staffed police forces showed a stronger response to the growing pressure on the police to more vigorously address violent crime and to better record violent crime than low-staffed police forces.

The recording bias found in this paper is relatively large compared to previous studies, including Levitt (1998), which may be explained by the major overhaul of police recording practices during the period of the analysis. Although extreme in its impact, a recording bias in police statistics on crime is by no means exclusive to England and Wales. For the US, Levitt (1998) finds evidence for a similar albeit less strong recording bias in Uniform Crime Report (UCR) data, the source of data most commonly used in studies into the effect of police on crime. The evidence suggests that 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, we find a 1 percent increase in police to result in a 0.6 percent decrease in victimization of property crime and a 0.9 percent decrease in victimization of violent 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 on property crime, which suggests that the police have a similar effect on reported and unreported crime.

In line with studies using changes in police budgets that are unrelated to local crime rates, as in Evans and Owens (2007), we exploit knowledge of the budgeting process to identify the effect of police on crime, albeit in a different way. We identify exogenous variation in police levels by differencing actual police levels from police levels a police force is meant to have based on the funding formula. Our findings are robust to excluding cross-sectional variation and to other considerations behind budget decisions at the national level that are not reflected in the baseline model.

Again, when explicitly addressing endogeneity, the police are found to be effective in bringing down 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 property crime, including Corman and Mocan (2000, 2005), Levitt (2002), Evans and Owens (2007) and Lin (2009).

Finding a higher impact of police on crime once addressing measurement error in crime data has the effect of making the benefit-cost ratio of expenditures on the police even more favorable (compare Donahue and Ludwig 2007).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 into 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 crime recording bias in police statistics as studied in this paper.

FOOTNOTES

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Table 1

Estimated elasticity of crime with respect to police, review of recent studies

Property crime		elasticity	Violent crime		elasticity
Corman, Mocan 2005	burglary	-0.3	Corman, Mocan 2000	assault	-0.3
Lin 2009	burglary	-1.6	Corman, Mocan 2005	assault	-0.3
Klick, Tabarrok 2005	burglary	-0.3	Lin 2009	assault	-0.6
Corman, Mocan 2000	burglary	-0.4	Evans, Owens 2007	assault	-1.0
Evans, Owens 2007	burglary	-0.6	Draca et al. forthcoming	assault	-0.4
Draca et al. forthcoming	burglary	-0.1	Corman, Mocan 2000	murder	-1.4
DiTella, Schargrotsky 2004	car theft	-0.3	Evans, Owens 2007	murder	-0.8
Corman, Mocan 2005	car theft	-0.6	Corman, Mocan 2005	murder	-0.5
Lin 2009	car theft	-4.1	Lin 2009	murder	-2.7
Klick, Tabarrok 2005	car theft	-0.9	Corman, Mocan 2005	rape	-0.1
Evans, Owens 2007	car theft	-0.9	Lin 2009	rape	-0.7
Corman, Mocan 2000	car theft	-0.5	Evans, Owens 2007	rape	-0.4
Corman, Mocan 2005	larceny	-0.7	Draca et al. forthcoming	rape	-0.2
Lin 2009	larceny	-1.9	Corman, Mocan 2005	robbery	-0.4
Evans, Owens 2007	larceny	-0.1	Lin 2009	robbery	-1.9
Draca et al. forthcoming	larceny	-0.4	Corman, Mocan 2000	robbery	-0.5
Evans, Owens 2007	property crime	-0.3	Evans, Owens 2007	robbery	-1.3
Lin 2009	property crime	-2.2	Draca et al. forthcoming	robbery	-0.4
Levitt 2002	property crime	-0.5	Klick, Tabarrok 2005	violent crime	-0.0
			Evans, Owens 2007	violent crime	-1.0

Lin 2009	violent crime	-1.1
Levitt 2002	violent crime	-0.4

Note. Reported estimates include those that were not statistically significant at conventional levels.

Reported elasticity for car theft in Corman and Mocan (2000, 2005) relates to the broader category of motor vehicle theft; reported elasticity for rape in Draca, Machin, and Witt (forthcoming) relates to the broader category of sexual offenses; reported elasticity for larceny in Corman and Mocan (2005) relates to grand larceny.

Table 2

Summary statistics, 42 police force areas, weighted by population, 2001/02-2006/07

	Mean	Standard deviation
Police resources		
Police officers per 10,000 population	25.48	7.15
Formula grant per capita	76.72	2.92
Crime-related supplementary national funding per capita	1.07	1.37
Police council tax per capita	30.99	12.91
Recorded crime per 10,000 population		
Property crime	3.63	1.24
Violent crime	1.72	0.56
Victimization of crime (% population 16+)		
Property crime	14.35	3.80
Violent crime	4.99	1.45
Economic and demographic characteristics		
Proportion of population of non-white ethnicity	0.09	0.09
Propensity to report property crime (% population 16+)	43.51	5.43
Propensity to violent property crime (% population 16+)	41.94	10.47
Proportion of male benefit claimants under 25 years of age	0.19	0.03
Proportion of unemployed claiming benefits for over one year	0.20	0.05
Proportion of one-adult households	0.30	0.03
Proportion of households living in striving areas	0.20	0.09
Population density	12.67	16.12

Table 3

The formula grant as predictor of police levels, 2001/2002-2006/2007

Dependent variable: Police officers per 10,000 population		
Formula grant per population	0.25**	(0.01)
Local funding per population	-0.10**	(0.04)
Crime-related supplementary funds per population	-0.03	(0.36)
R ²	0.94	
Number of observations	252	

Note. Other covariates include year fixed effects. Estimated using mid-year population of police force areas as weights. Standard errors between parentheses. + P<.10; * P<.05; ** P<.01.

Table 4

Estimated effect of police on crime (reduced form estimates)

	Police recorded crime per 10,000 population	Victimization of crime, percentage of population
<i>Property crime</i>		
Police per 10,000 population	-1.04* (0.52)	-0.33* (0.16)
Implied elasticity	-0.75	-0.60
<i>Violent crime</i>		
Police per 10,000 population	-0.03 (0.22)	-0.17** (0.06)
Implied elasticity	-0.04	-0.89
Number of observations	252	252

Note. 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. Other covariates include fraction non-white, fraction of male benefit claimants under 25, fraction long-term unemployed, fraction one-adult households, fraction households living in striving areas, population density, and year-fixed effects. + P<.10; * P<.05; ** P<.01.

Table 5

The impact of public reporting on the effect of police on crime

Dependent variable:	Recorded property crime		Recorded violent crime	
	(1)	(2)	(3)	(4)
Police per 10,000 population	-1.03 ⁺ (0.55)	-1.24 ^{**} (0.47)	-0.05 (0.22)	-0.06 (0.23)
Implied elasticity	-0.70	-0.84	-0.07	-0.09
Propensity to report crime	No	Yes	No	Yes
Number of observations	216	216	241	241

Note. 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. Other covariates include fraction non-white, fraction of male benefit claimants under 25, fraction long-term unemployed, fraction one-adult households, fraction households living in striving areas, population density, and year-fixed effects. + P<.10; * P<.05; ** P<.01.

Table 6

Estimated effect of police on recording of crime

Dependent variable:	Ratio recorded property crime / victimization of property crime	Ratio recorded violent crime / victimization of violent crime
Police per 10,000 population	-0.10 (0.28)	1.77** (0.68)
Number of observations	252	252

Note. 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. Other covariates include fraction non-white, fraction of male benefit claimants under 25, fraction long-term unemployed, fraction one-adult households, fraction households living in striving areas, population density, and year-fixed effects. + P<.10; * P<.05; ** P<.01.

Table 7

Robustness checks

	Victimization of property crime	Victimization of violent crime
1. Baseline	-0.33* (0.16)	-0.17** (0.06)
2. Equation (2) but use total national funding	-0.30 ⁺ (0.18)	-0.13 ⁺ (0.07)
3. Include region-fixed effects	-0.33* (0.16)	-0.13 (0.16)
4. 2SLS estimate	-0.43 ⁺ (0.20)	-0.23** (0.07)
5. Excluding London	-0.40* (0.19)	-0.18** (0.06)

Note. All estimates based on 252 observations, except specification 5 (n=246). 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. Other covariates include fraction non-white, fraction of male benefit claimants under 25, fraction long-term unemployed, fraction one-adult households, fraction households living in striving areas, population density, and year-fixed effects. + P<.10; * P<.05; ** P<.01.

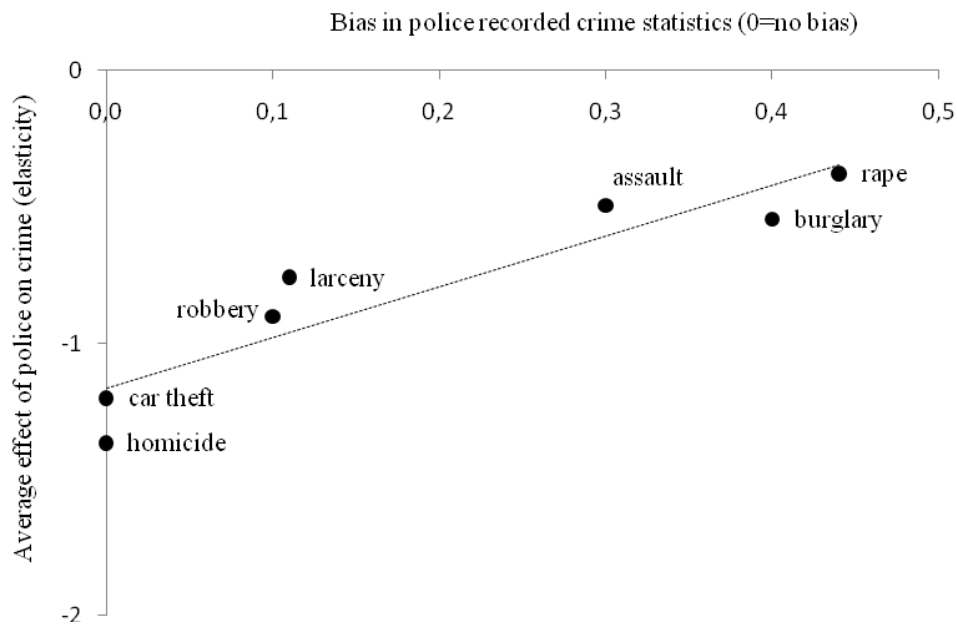


Figure 1. Bias in police recorded crime vs. the effect of police on crime

Note. Bias in police recorded crime statistics is defined as the elasticity of police with respect to police recorded crime rate (alternatively: victim reporting and police recording bias), and is taken from Levitt (1998, 76, Table V). Bias in police recorded homicides is taken to be zero. Average estimated elasticities of police with respect to crime – irrespective of level of statistical significance – based on results reported in Corman and Mocan (2000, 2005); DiTella and Schargrodsky (2004); Klick and Tabarrok (2005); Evans and Owens (2007); Lin (2009); Draca, Machin, and Witt (forthcoming). Levitt (2002) does not report crime-specific elasticities. Individual estimation results provided in Appendix A.

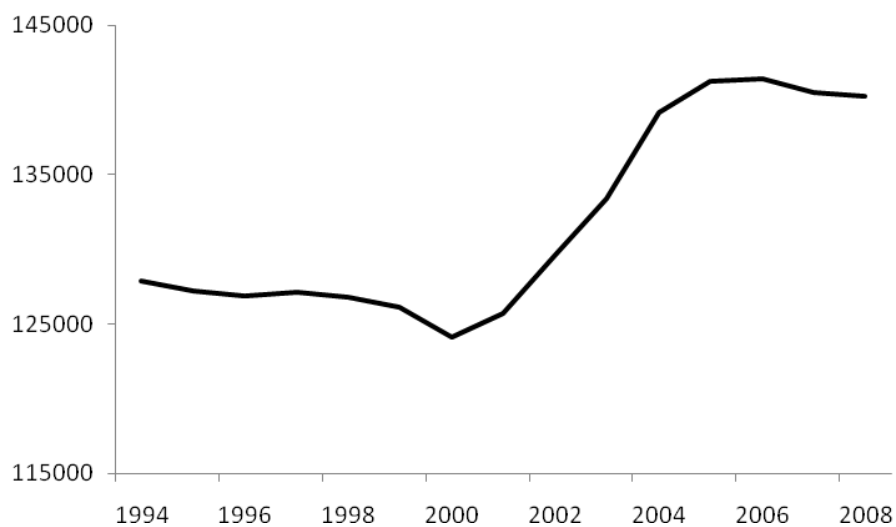


Figure 2. Number of police officers, England and Wales, 1994-2008

Source. Police Service Strength, England and Wales, various years.

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 Schargrodsky (2004) limit their scope to car theft. Exceptions are the positive effect of police on murder in Lin (2009), on robbery and assault in Evans and Owens (2007), and on assault in Draca, Machin, and Witt (forthcoming). We discuss these findings 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 discuss this study later in the introduction.

³ We ignore the highly imprecise estimates that are based on instrumented police variables (Levitt 1998, 76, Table V, bottom row). The instrument used – the timing of mayoral and gubernatorial elections – was later found to be too weak to identify meaningful exogenous variation in police levels (McCrary 2002).

⁴ The two types of studies may differ on other dimensions as well. Compared to studies based on state or city-level, long-term data, studies of temporary shocks to police presence may be more limited in scope. The latter studies focus on deterrence, i.e. the effect of oversight in or around public spaces, to the exclusion of incapacitation of offenders. Control areas may also experience the effects of incarceration and subsequent reduction of the criminal population after all (DiTella and Schargrodsky 2004, 124). Furthermore, both Klick and Tabarrok (2005) and Draca, Machin, and Witt (forthcoming) suggest that studies relying on temporary changes in visible police presence limit the scope to ‘street crimes’. 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 (forthcoming), using a very similar research design, suggest and find not to be susceptible to greater visible police presence.

⁵ The General Grant distributed with the police funding formula includes the 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.

⁶ The full list of variables in the police funding formula includes: fraction of household residents living in lone parent family, fraction of unemployed claiming benefits for over one year, length of trunk and principal motorways, fraction of households contained only one person aged 16 years or over, fraction of households living in accommodation with more than one person per room, population density, population sparsity, fraction of households living in rented accommodation, length of roads subject to speed limit not exceeding 40 mph, fraction of residents living in areas classified as striving, fraction of household residents living in unshared terraced dwellings, fraction of population aged 18 to 64 years claiming benefits, fraction of males aged under 25 claiming unemployment benefits.

⁷ To compute the elasticities, we multiply the parameter estimate with 0.25, which is equal to a one percent increase in police, and divide the result by the average crime rate reported in the summary statistics.

⁸ We do not use feedback from BCS respondents which reported crimes have been recorded by the police. Besides the many missing observations, the survey responses do not provide a reliable indicator of police recording practices. Most people have a very limited understanding of whether a reported crime incident was recorded by the police (Averdijk and Elffers 2010). Additionally, some recorded crime has not been reported by the public, but is the result of police efforts.

⁹ The crime categories included in the two sources of crime data do not perfectly overlap. The used ratio is roughly similar to the percentage of crime incidents recorded (see Kershaw, Nicholas, and Walker 2008).

¹⁰ Taking estimates of the costs of property and violent crime from Dubourg, Hamed, and Thorns (2005), we find a one percent increase in police resources at a cost of £ 100 m. to result in a decrease in crime with a value of £ 190 m. The actual costs to society may be somewhat higher if downstream costs accruing to legal and correctional services are included.