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Police numbers up, crime rates down

The effect of police on crime in the Netherlands, 1996-2003

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Abstract in English

In this study, we present evidence on the effect of greater numbers of police personnel on crime and nuisance reduction within the Dutch context. We use a multiple time series design with police regions as the unit of analysis, covering the period 1996-2003. During this period, police resources increased substantially. The growth in additional resources differed greatly between regions, allowing us to use this policy intervention to identify the effect of police on crime and nuisance. We control for regional economic, social and demographic factors and for national trends that might obscure the effect of police on crime. We find significantly negative effects of higher police levels on property crime, violent crime and nuisance. Our estimates suggest that a substantial proportion of the decline in crime and nuisance during the period 1996-2003 is attributable to the increase in police personnel.

Key words: police, crime, nuisance, effectiveness, victimisation survey.

JEL Classification: K4 – Legal procedure, the Legal System, and Illegal Behaviour.

Abstract in Dutch

In deze studie presenteren we schattingen van de effectiviteit van de Nederlandse politie op het gebied van de bestrijding van criminaliteit en overlast. We voeren een panel data analyse uit met gegevens van de 25 politieregio's voor de periode 1996-2003. Gedurende deze periode is de omvang van het politiepersoneel sterk gegroeid. Deze groei verschilde sterk tussen politieregio's, wat ons in staat stelt om aan de hand van deze beleidsinterventie het effect van politie op criminaliteit en overlast te schatten. We houden rekening met regionale trends in economische, sociale en demografische factoren en met nationale trends die de relatie tussen de omvang van het politiepersoneel en de mate van criminaliteit en overlast beïnvloeden. We vinden significant negatieve effecten van meer politiepersoneel op geweldscriminaliteit, vermogenscriminaliteit en overlast. De resultaten suggereren dat een substantieel deel van de daling in de criminaliteit en overlast gedurende de periode 1996-2003 op het conto van de politie kan worden geschreven.

Steekwoorden: politie, criminaliteit, overlast, effectiviteit, slachtofferenquête.

Een uitgebreide Nederlandse samenvatting is beschikbaar via www.cpb.nl.

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Preface

This study looks into a policy instrument to improve public safety: increasing the number of police personnel. Thus far, no firm empirical evidence was available on this topic within the Dutch context. This study fits into a series of evaluation studies within the CPB Netherlands Bureau for Economic Policy Analysis, with earlier work focusing on the effectiveness of welfare-to-work services, R&D subsidies, and many other public policies.

We gratefully acknowledge the Department of the Interior and the Department of Justice for providing survey data from the *Politiemonitor Bevolking* (PMB). Tom van Dijk and Erik Oppenhuis from Intomart provided helpful assistance in working with the data from the PMB. Thanks to Statistics Netherlands for providing data on recorded crime.

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Henk Don, director.

Samenvatting (Dutch summary)

De veiligheid is de laatste jaren sterk verbeterd. Zo is tussen 1994 en 2003 het percentage woningen waar gedurende dat jaar is ingebroken, gehalveerd. Eenzelfde daling geldt het percentage gestolen auto's. Niet alleen de criminaliteit is gedaald, ook de overlast is afgenomen. Het percentage mensen dat zegt dat in hun buurt mensen vaak op straat worden lastiggevalen, daalde in dezelfde periode met 30 procent. De beleving van veiligheid verbeterde ook. Het percentage mensen dat zegt dat ze zich vaak onveilig voelden, is tussen 1994 en 2003 met 40 procent afgenomen. Deze gunstige ontwikkelingen zijn af te leiden uit de nationale slachtofferenquête van het Ministerie van Binnenlandse Zaken en Koninkrijksrelaties en het Ministerie van Justitie, de Politiemonitor Bevolking.

Is de verbetering van de veiligheid te danken aan de gelijktijdige toename van het politiepersoneel? De laatste tien jaar is het politiepersoneel van de regiokorpsen met circa 13.000 fte's toegenomen, wat neerkomt op een groei van meer dan 30 procent. Onderzoek naar wat dit beleid heeft opgeleverd, is schaars. Over het effect van meer politiepersoneel op de veiligheid bestaat tot nog toe veel onzekerheid. Noodgedwongen baseren beleidsmakers beslissingen over het politiebudget daarom vooral op verhalen over de effecten van meer politiepersoneel in plaats van de resultaten van systematisch onderzoek.

Het kabinet heeft de ambitieuze doelstelling de criminaliteit en overlast met 20-25 procent te verlagen. Deze studie vormt een bijdrage voor de afweging hoe de kabinetsdoelstelling het best is te realiseren.

In deze studie analyseren we *het effect van extra politiepersoneel op de kans op slachtofferschap van criminaliteit en overlast in Nederland in de periode 1996-2003*. Na het presenteren van de resultaten gaan we in op de manier waarop we het effect van meer politie op veiligheid bepalen.

Onderzoeksresultaten en lessen voor beleid

Op basis van de uitkomsten van de empirische analyse trekken we de volgende conclusies:

1. *De groei in politiepersoneel in de periode 1996-2003 heeft de kans op slachtofferschap van verschillende vormen van criminaliteit en overlast verlaagd met circa 10 procent.* We konden de grootte van dit effect het meest betrouwbaar inschatten voor woninginbraken, autocriminaliteit (diefstal van en uit auto's, vandalisme van auto's), fietsdiefstal, portemonneediefstal, bedreiging, geweld en overlast van jongeren, openbare dronkenschap en graffiti.

Extra politie draagt bij aan een grotere veiligheid. Hieruit kan nog niet worden geconcludeerd dat uitbreiding van de politie maatschappelijk rendabel is geweest (sinds 1995 is in totaal 700 miljoen euro extra aan de regionale politiekorpsen besteed). Om het maatschappelijk rendement te bepalen is een kosten-batenanalyse vereist. De baten zijn echter moeilijk in beeld te brengen. Zo is de wisselwerking tussen politiebescherming en inspanningen van mensen zelf om criminaliteit te voorkomen onduidelijk. Denk bijvoorbeeld aan de afweging 's avonds niet de deur uit te gaan of om extra hang- en sluitwerk aan te schaffen. Bovendien is naast doelmatigheid, ook rechtvaardigheid een belangrijke overweging, zeker rond het strafrecht.

2. *Meer politiepersoneel heeft een negatief effect op criminaliteit, ondanks mogelijke verplaatsing van criminaliteit binnen politieregio's.* Wanneer politieagenten ergens in een regio verschijnen, kunnen criminelen hun activiteiten verplaatsen naar andere delen van de regio. Ondanks deze mogelijke verplaatsing van criminaliteit binnen politieregio's vinden we een duidelijk effect van meer politie. De politiekorpsen zijn dus door de inzet van meer personeel in staat om het algehele niveau van criminaliteit te verlagen.
3. *Herverdeling van politiepersoneel van landelijke naar stedelijke gebieden draagt bij aan de doelstelling om het nationale niveau van criminaliteit en overlast te verlagen.* Voor verschillende vormen van criminaliteit en overlast vinden we een groter effect van meer politiepersoneel in stedelijke gebieden dan in niet-stedelijke gebieden. Zo is het geschatte effect van meer politie op woninginbraak twee keer zo groot in de Randstad als in de rest van het land en het effect op overlast van jongeren drie keer zo groot. Natuurlijk zijn er ook andere afwegingen bij het verdelen van politiemiddelen, zoals de beschikbaarheid van de politie in landelijke gebieden.
4. *Tot 2010 zijn geen grote uitbreidingen in de omvang van het politiepersoneel gepland. De bijdrage van de politie aan de gewenste verhoging van de veiligheid ligt daarom vooral in een betere inzet van bestaande middelen.* In vergelijking tot vorige jaren vlakt de groei in de omvang van het politiepersoneel *per inwoner* af. Dankzij de 4.000 extra politieagenten die dit kabinet heeft beloofd, blijft het aantal politiemensen per inwoner min of meer stabiel. Betrouwbaar evaluatieonderzoek op het terrein van de Nederlandse politie is schaars. Buitenlands onderzoek laat zien dat innovaties in politiewerk, zoals een betere vergaring, uitwisseling en analyse van informatie tussen korpsen, een verschil kunnen maken. Meer en beter evaluatieonderzoek naar 'wat werkt en wat niet werkt' kan inzicht bieden in de manier waarop de effectiviteit van de politie is te verbeteren.

Aanpak

Hoe onderscheiden we het effect van meer politiepersoneel op criminaliteit van de vele andere factoren die de criminaliteit beïnvloeden? Schrijven we de invloed op de criminaliteit van

bijvoorbeeld de economische opleving van eind jaren '90 niet onbedoeld toe aan de stijging in politiepersoneel?

Het basisidee achter de analyse is het volgende: als meer politiepersoneel effect heeft, dan zal de criminaliteit relatief sterk dalen in een regio waar het politiepersoneel relatief sterk toeneemt. We kijken naar *groeivoeten* en niet naar het historisch gegroeide niveau van criminaliteit en politiepersoneel in een regio. Door te kijken naar *verschillen* in groei tussen politieregio's, laten we nationale trends in politiepersoneel en criminaliteit buiten de analyse. Er bestaat vrij sterke variatie tussen korpsen in de groei in het politiepersoneel. Dankzij deze variatie kunnen we dit onderzoeksontwerp gebruiken om het effect van politie op criminaliteit te bepalen.

De uitdaging van dit onderzoek is te corrigeren voor andere factoren die gerelateerd zijn aan regionale verschillen in groei in politiepersoneel. Stel dat de omvang van het politiepersoneel sterk groeit in regio's waar de werkloosheid zich ongunstig ontwikkelt, dan onderschatten we het effect van meer politie op criminaliteit. Een factor die criminaliteit afremt, meer politiepersoneel, gaat gepaard met een kracht die criminaliteit opdrijft, achterblijvende groei in banen. Als we geen rekening houden met trends in regionale werkloosheid, dan schrijven we het *netto effect* van deze twee factoren toe aan de groei van politiepersoneel in plaats van het effect van politie alleen.

Kortom, als andere factoren samenhangen met regionale verschillen in de groei van het politiepersoneel, dan schrijven we of te veel of te weinig van de criminaliteitsdaling op het conto van de politie.

Een belangrijke manier waarop andere factoren meting van het effect van politie op criminaliteit verstoren, is door de budgetverdeelformule, onderdeel van het budgetverdeelsysteem van de politie. De overheid gebruikt een formule om het nationale politiebudget tussen de regionale korpsen te verdelen. In de formule zitten regionale karakteristieken die de werklast van de korpsen bepalen, waaronder het aantal winkels en de lengte van wegen. Als de groei in politiemiddelen door de budgetverdeelformule vooral terecht komt in regio's met de meest ongunstige ontwikkeling in criminaliteit, dan zullen we de effectiviteit van meer politie onderschatten. In dat geval volgen de middelen de problemen, wat resulteert in een positief verband tussen de groei in politiepersoneel en de groei in criminaliteit.

We laten zien dat het budgetverdeelsysteem niet succesvol is in het volgen van regionale trends in criminaliteit – en dat daarom vertekening in onze schatting van het effect van meer politie niet waarschijnlijk is. Allereerst reageert de middelentoedeling nauwelijks op verschillende ontwikkelingen in de regionale veiligheidssituatie. De meeste variabelen in de formule zijn

slechte voorspellers van criminaliteitstrends en ze worden ook niet regelmatig geactualiseerd (de variabelen in de huidige formule zijn tussen de 5 en 12 jaar oud). Bovendien is er een vertraging van tenminste twee jaar tussen budgetteringsbeslissingen en daadwerkelijke veranderingen in de omvang van het politiepersoneel. Vanwege deze vertraging is er geen oorzakelijke relatie tussen de ontwikkeling in de criminaliteit nu en de groei in politiepersoneel van vorig jaar – de wijze waarop we het effect van politie op criminaliteit schatten.

Om rekening te houden met andere factoren die toevalligerwijs gerelateerd zijn aan verschillen in de groei van regionaal politiepersoneel, nemen we een aantal economische, sociale en demografische factoren op waarvan bekend is dat ze de criminaliteit beïnvloeden.

Executive summary

Public safety has been improving at an amazing rate. Between 1994 and 2003, burglary declined by 50 percent, car theft by 50 percent and harassment in public spaces by 30 percent. The percentage of people saying that they feel frequently unsafe has declined by 30 percent over the same period. These favourable trends have been recorded in the *Politiemonitor Bevolking*, the Dutch crime victimisation survey.

Is the improvement in public safety attributable to the concurrent increase in police personnel? Over the last ten years, police personnel increased by some 13,000 full time equivalents, which amounts to a growth of more than 30 percent. Research into the effects of this policy is scarce. Consequently, policy makers base their decision about the police budget mostly on anecdotal evidence about the effects of more police personnel rather than the results of systematic research.

The Balkenende administration has the ambitious goal to reduce crime and nuisance by 20-25 percent. The results of this study are aimed to contribute to the policy decision how to best achieve this goal.

In this study, we analyse *the effect of additional police personnel on the chance of becoming victim of crime and nuisance in the Netherlands in the period 1996-2003*. After presenting our findings, we discuss the way in which we estimated the effect of more police on public safety.

Findings and lessons for policy

The analysis of changes in police personnel and trends in crime and nuisance led to the following conclusions:

1. *The growth in police personnel decreased the chance on becoming victim of several types of crime and nuisance by some 10 percent during 1996-2003*. We could most reliably estimate the magnitude of the effect on burglary, auto crime (theft of cars, theft out of cars, vandalising cars), bicycle theft, purse theft, threat, violent crime, and on nuisance from youth in public spaces, public intoxication and graffiti.

It is hard to say whether the effect is sufficiently large to justify the additional public expenditures (0.7 billion euro for the regional police forces since 1995). To that end, we should value the benefits of the resulting decline in crime and nuisance. The benefits are hard to identify. For instance, the effect of better police protection on private expenditures on prevention is unclear. When trading off the costs and benefits of expenditures on the police, justice is a factor as well. Society wishes to prosecute and incarcerate people who have

committed certain criminal acts. It is a political choice to trade off the several types of benefits against the costs of more police personnel.

2. *More police has a substantial negative effect on crime, despite possible displacement of crime within police regions.* When police appear in one part of a police region, criminals may flee to another part of the region. Despite this possible displacement of crime within police regions, we find a substantial effect of police on crime. Apparently, the police forces were able to bring down their overall crime rate.
3. *In terms of crime and nuisance reduction, police personnel makes more of a difference in urbanised regions.* For several types of crime and nuisance, we find the effect of more police to be greater in urbanised regions. For instance, we find the impact on crime per police officer to be two to three times larger in the Randstad than in other regions. Therefore, if the government wants to bring down national crime and nuisance rates, then redistributing existing police personnel from rural to urban areas will help towards that goal. Clearly, there are also other considerations when distributing resources, including availability of the police in rural areas.
4. *Until 2010, no major expansions in the number of police personnel per capita are planned: the administration's ambitious plans to lower crime and nuisance mainly rely on improvements in police effectiveness. Research into 'what works, and what doesn't' helps to show the best way of improving police effectiveness.* The promised 4,000 additional police officers ensures that population growth does not bring down the number of police personnel *per capita*. Thus the police's contribution to the target of 20-25 percent reduction in crime and nuisance mainly relies on better use of existing resources. Foreign research shows that innovations in methods of policing can make a difference. So far, research on 'what works, and what doesn't' within the Dutch context is scarce. More and better evaluation research is necessary to provide guidance on how to best improve police effectiveness.

Approach

How do we single out the impact of rising police levels from the many other factors affecting crime (and nuisance) rates? *The basic idea behind this study is as follows: if more police has an effect, then crime should decrease relatively fast in a region that enjoys relatively strong growth in its police personnel.* We focus on *growth rates* and not on historically grown levels of police and crime in a region. Additionally, by focusing on *differences* in growth between regions, we cancel out national trends in police levels and crime rates. There is quite some variation in the growth in police personnel across police regions, allowing us to use this research design to identify the effect of police on crime.

The challenge is to exclude the impact of third factors that are related to regional differences in the growth in police personnel. Say that the growth of police personnel was concentrated in regions with lagging job opportunities, then we are likely to underestimate the effect of police on crime. After all, a force driving down crime, strong growth in police personnel, is combined with a force pushing up crime, lagging growth in job opportunities. Without correcting for this third factor, we will attribute the *net effect* of these two forces to the growth in police personnel rather than the full effect of more police.

Thus, if a third factor affecting crime happens to be related to differences in regional growth in police personnel, then we attribute either too much or too little of the change in crime to the police.

A major way in which third factors may obscure the effect of police on crime is through the budget formula. The budget formula is used to distribute most of the police budget among the police forces. It uses regional characteristics like the number of shops and the length of roadways to indicate the relative need for police resources. If the budget formula shifts the growth in police resources to regions with the most unfavourable trends in crime, then we will underestimate police effectiveness. After all, in that case, police resources follow problems, leading to a positive relation between growth in police personnel and increases in crime.

We show that the budget formula is not successful in following regional trends in crime – and therefore is not likely to bias our estimates of police effectiveness. First of all, the response to differences in regional crime trends is weak, because most variables in the budget formula are poor predictors of crime trends and the variables have not been updated on a regular basis (the variables in the current formula are 5 to 12 years old). Moreover, there is a two-year time lag between budgeting decisions and actual changes in police personnel. Because of this lag, there is no causal relation between current year's crime trend and last year's growth rate in police personnel – the way we estimate the effect of police on crime.

To control for third factors that are coincidentally related to differences in growth of regional police personnel, we include a number of economic, social and demographic factors known to affect crime.

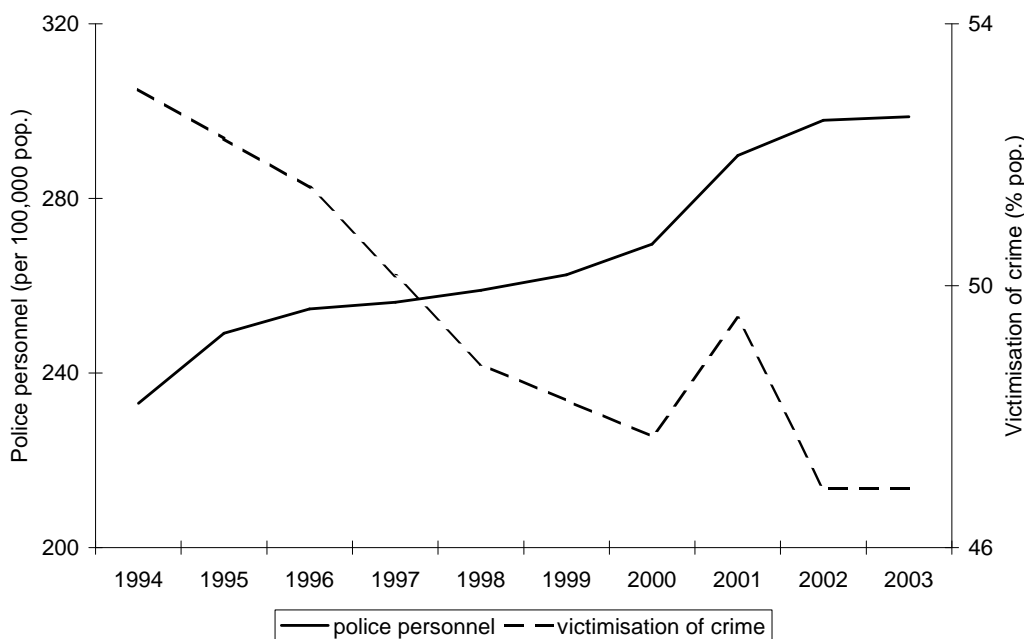
1 Introduction

1.1 Rationale

Crime is on the decline. Between 1994 and 2003, burglary and car theft declined by 50 percent, according to the Politiemonitor Bevolking, the Dutch crime victimisation survey. The percentage of people saying that they feel frequently unsafe has declined by 40 percent over the same period.

A marked expansion in police personnel occurred concurrently with these aggregate crime trends. Over the last ten years, the number of people employed by the regional police forces alone increased by some 13,000, which amounts to a growth of more than 30 percent. With police numbers up and crime rates down, can we conclude that the extra expenditures on the police are paying off?

Figure 1.1 Did the increase in police personnel cause the decline in crime?



Source: Interior Department, Politiemonitor Bevolking, Statistics Netherlands.

Making a direct link between policing and outcomes of police work such as crime is common in the news and in commentaries, but in scientific research it is a rare thing. Surprisingly little is known about the effectiveness of the Dutch police and the factors driving it. Looking through twenty years of (exclusively Dutch) material, Wittebrood en Van Beem (2004) find only two empirical studies on the effects of policing strategies on crime that meet minimum scientific standards (including a treatment and a control group, for instance). Based on Dutch data,

several studies uncovered a relationship between the clearance rate and the crime rate, but none found evidence of an impact of police resources on crime (Van Tulder, 1985). Consequently, claims that more police brings down crime and nuisance are mostly based on anecdotal evidence.

The police play a pivotal role in the Balkenende administration's plans to bring down crime and nuisance by 20 to 25 percent. The police are exempted from the budget cuts facing many other public sectors. The forces receive funds to hire and train 4,000 additional police officers. More 'blue on the streets' is seen as an important way of improving safety. Just how much can we expect from an increase in police resources? The answer to this question is important since the police are competing for the same tax funds as other publicly financed organisations.

There might be a great deal to learn from the influx of police resources over the last ten years. Can we attribute some of the decline in crime to police work or is the decline the result of better labour market opportunities in the second half of the nineties? Using the events of the last ten years as a source of evidence is challenging, since the 'policy experiment' has not been conducted under controlled conditions. In this study, we use econometric methods to discover how far the analysis of this policy intervention gets us in answering basic questions about police effectiveness. By building experience in the empirical analysis of police effectiveness, this study is also a stepping stone for further research into the *determinants* of effectiveness of the Dutch police. Ideally, future research in this area is based on experimental research designs – allowing us to more easily refute alternative hypotheses explaining the same empirical phenomena.

1.2 Research question

In this study, we address the following research question:

What is the impact of increases in the level of police personnel per capita on the chance of becoming a victim of crime and nuisance in the Netherlands in the period 1996-2003?

1.3 Contribution

The only way of knowing how much to spend on the police is to evaluate the effects of police work – and to weigh the costs and benefits against competing ends. This study is a rare contribution to the study of police effectiveness within the Dutch context. This study is a first step since we do not attempt to conduct a cost-benefit analysis of the impact of additional police personnel.

This study contributes to the wider literature on this topic by using victimisation data instead of police statistics on reported crime.¹ The Politieonitor Bevolking (PMB) is unique in its sample size. Each year, some 80,000 respondents are surveyed; more than 1 out of 200 people living in the Netherlands. To compare: the National Crime Victimization Survey, covering all of the US, includes some 75,000 persons. As a result of PMB's sample size, data can be disaggregated to the level of the police region (and, in some areas, to lower levels such as the police district).²

By using victimisation data, we widen the scope of research into police effectiveness to include nuisance. The survey includes nuisance crimes not commonly included in empirical studies, such as graffiti and nuisance from youth.

Additionally, by using victimisation data, we circumvent measurement error common in studies based on reported crime. It is widely known that reported crime is subject to serious measurement error (see also MacDonald, 2002). The effect of more police officers on the percentage of crimes reported may be limited (Levitt, 1998), but there are many other factors that may bias both cross section and time series analysis. Sources of bias in reported crime include changes in policing priorities and recording practices, the introduction of new information technology, and changes in citizen concerns about specific crimes. For instance, if policing priorities in terms of resources allocated to crack down on specific offences also lead the police to more actively record such offences, then the estimated effect of police on crime is biased downwards (domestic violence is a classic example, for a recent Dutch empirical study on violence in nightlife suffering from this type of bias, see Emmaneel et al., 2004).

Constant region-specific differences in recorded crime may be captured by using a single-region time series design or by including fixed effects in a multiple-region time series design, but changes in public and police attitudes are rarely controlled for (Dryden Witte and Witt, 2001). There is some empirical evidence that changes occurred in reporting behaviour (see Catalano, 2004 for trends in the US) and recording behaviour (see MacDonald, 2002 for the UK).

¹ Craig (1987) is a rare exception in using victimisation data for studying police effectiveness. Our study takes his cross-section analysis of victimisation data a step further by using a multiple time-series design.

² Since criminal victimisation is a rare event, large general population samples are required to accurately characterise the population of victims, see Pepper and Petrie (2003).

1.4 Scope

To keep the analysis tractable, we had to decide which issues were truly essential to the research question and which issues are left for future research. There are several areas that would justify separate studies:

- *Analysis of individual victimisation data.* In this study, we aggregate all data to the regional level. An analysis of individual data could better account for heterogeneity within police regions. In a separate study, we are conducting an econometric analysis of micro-data.
- *Determinants of effectiveness.* Next to the question on how successful the police are in bringing down crime and nuisance, the major policy question is how to improve police effectiveness. A more effective use of existing resources is a clear priority of the current administration. Think of the policy to enhance the focus on the arrest of repeat offenders and to improve the visibility of police officers in public spaces. We will assess whether we can extend the analysis to include determinants of effectiveness – using a similar research design and the same source of data.
- *Valuing the impact of police on crime.* A next step is to value the benefits of police work. A cost-benefit analysis is not as straightforward as it seems. An analysis at the regional level does not allow us to study prevention behaviour at the appropriate level of detail, whereas crime prevention measures such as purchasing a burglar alarm or staying home at night are a major cost to society. We plan to use individual data to analyse the effect of police protection on private prevention.
- *Effectiveness in other areas of police work.* The police are active in many ways, and fighting crime and nuisance is just one of them.
- *The effect of other factors on crime.* The strong decline in crime has been attributed to several factors, including increased private prevention and the economic upturn of the 1990s. Our focus is on the effect of the growth in police personnel on crime. We only include control variables such as the welfare caseload in the regression equation to prevent a bias in estimating police effectiveness – not to analyse which factors have contributed to the decline in crime.

1.5 Structure

The study is organised as follows. Chapter 2 describes the theory and existing empirical evidence behind general deterrence. Chapter 3 describes the data. In chapter 4 and 5, we discuss the empirical strategy and estimation results. Chapter 6 concludes.

2 The effect of police on crime: theory and evidence

2.1 Introduction

More police personnel is not necessarily beneficial to social welfare. At some point, society is better off by hiring more teachers rather than police officers, or by no additional public expenditures at all. When trading off alternative ways of spending public money, information on the effect more resources on public goals like crime reduction is an important input. Below, we discuss how the police may have an effect on crime and go into the existing empirical evidence.

2.2 Deterrence and incapacitation

Police reduce crime via deterrence and incapacitation. In the first case, the police prevent people outside prison walls to commit crimes. More police have a deterrent effect if (1) risk perceptions and behaviour of offenders change as a result of a (perceived) increase in the likelihood of being caught (Becker, 1968); or if (2) police have a positive effect on private prevention (e.g. through a publicity campaign updating people's perceptions about the costs and benefits of equipping their house with additional door locks).

In the case of incapacitation, higher police levels contribute to the conviction of a greater percentage of repeat offenders through arrests. While a criminal is imprisoned, he or she is unable to engage in criminal actions that otherwise would have been taken.

2.3 Dependence on the criminal justice system

We are interested in the causal impact of higher police levels – including the effects of accompanying actions 'downstream' in the criminal justice system. Therefore, we do not differentiate between deterrence and incapacitation effects. Clearly, the working of the criminal justice system during the period 1996-2003 may be somewhat unique. Bottlenecks may have hampered police effectiveness; bottlenecks that are not necessarily in place in the future. Thus the possible influence of criminal justice system resources on police effectiveness during the years under investigation is relevant when making predictions about the effects of police levels. In chapter 5, we discuss the impact of the criminal justice system on the estimated effect of police on crime.

2.4 Empirical evidence

For reasons of comparability with the approach taken in this study, we focus on studies using police personnel as deterrence measure. In section 4.2 we discuss why we use police personnel rather than the clearance rate or budget figures. Given the paucity of empirical work based on victimisation data, we do not have much choice other than to review the results of studies relying on police statistics as their source of crime data. To what extent measurement error in recorded crime affects the estimates is unclear – and subject of empirical analysis in chapter 5.

Empirical evidence based on Dutch data

The choice not to review studies using the clearance rate as deterrence measure leaves us with one empirical study. Based on a cross section analysis of municipal police departments, Van Tulder (1985) finds no relation between police resources and reported crime. Thus, so far, empirical work based on Dutch data has not found clear-cut evidence that changes in police resources directly affect crime rates.

International evidence

For a long time, it was thought to be a myth among researchers that the police could actually reduce crime. Bayley (1994) states that ‘one of the best kept secrets of modern life [is that] police do not prevent crime’. Over many years of research, most empirical studies found no or a positive effect of police on crime. Researcher came up with many reasons to explain these somewhat surprising findings: (1) Most police work is not devoted to crime reduction. (2) Only large increases in police levels can produce enough police presence on the streets to actually deter crime. (3) The most common police strategies are poor crime-prevention strategies. (4) If the police does have a deterrent effect, criminals can easily move to other communities or switch to other, less visible types of crime rather than break off their criminal career (Marvell and Moody, 1996).

Recent empirical research contradicts the no-effect hypothesis. Empirical studies since the late 1990s have been addressing specification problems in earlier deterrence research – that is, assumptions concerning the variables to be included in the study and the causal directions among them. Primary concerns were simultaneity and omitted variable bias. These problems cast doubt about the – up to then – common finding that the police had no or a positive effect on crime (see Marvell and Moody, 1996 for a discussion).

Simultaneity problems arise from the fact that most, if not all, deterrence factors are affected by crime, mainly because they are often initiated in response to high or growing crime rates. In such a situation ordinary least squares (OLS) estimates are biased and inconsistent. If resources follow crime, then the OLS estimate of the effect of police on crime will be biased downwards.

Omitted variable bias occurs because factors beside the police variable affect crime rates and might account for their apparent impact (or lack of impact) on crime. Since the researcher can never eliminate the possibility of spurious correlations in regressions, the question is how best to mitigate any omitted-variable problem. For instance, if growth of police personnel was concentrated in regions with lagging job opportunities, then we are likely to *underestimate* the effect of police on crime – if we do not control for the state of the economy. After all, in that case a relatively strong growth in police personnel, a force driving down crime, is combined with relative unfavourable job opportunities, a force pushing up crime (see Gould et al., 2002 for empirical evidence). Without correcting for this third factor, we will attribute the *net effect* of these two forces to the growth in police personnel rather than the full effect of more police.

Based on a review of the recent empirical literature, we identify four different approaches to the simultaneity problem and omitted variable bias:³

1. *Geographical reallocation of police resources after a terrorist attack.* Di Tella and Schargrodsky (2004) use the reallocation of police resources in response to a terrorist attack as a source of exogenous variation in deterrence. Following a terrorist attack on the main Jewish centre in Buenos Aires, Argentina, in July 1994, all Jewish institutions received police protection, leaving some city neighbourhoods relatively unprotected. This event induced a geographical allocation of police forces that can be presumed exogenous in a crime regression. The authors find a large, negative, and highly local effect of police presence on car theft. The estimated elasticity of car theft with respect to police is -0.33 (ibid., p. 124). Thus a one percent increase in police levels leads to a 0.3 percent decline in car theft. This effect does not include the incapacitation effect, since this effect should be observed for all blocks, not just for those occupied by protected Jewish institutions.
2. *Year-to-year changes in police resources related to change in number of fire-fighters.* Levitt (2002) tries to escape simultaneity problems by identifying the part of changes in police personnel that is unrelated to crime trends. He claims that the number of fire-fighters is related to the number of police personnel, but not to trends in crime for 122 large U.S. cities over the period 1975-1995. There are several factors that jointly influence the number of fire-fighters and police officers, including the power of public sector unions, citizen tastes for government services, affirmative action initiatives, or a mayor's desire to provide spoils. Supposedly, none of these factors are related to trends in crime. By only using the growth in police personnel that

³ We do not discuss studies using police strikes as an exogenous shock to police presence (see for instance Makinen and Takala (1980) on the effects of the Helsinki police strike on fights in public places and emergency room admissions for assault-related injuries). None of these studies include a comparison group, so in theory it is possible that crimes would have risen dramatically during the strike period even without the strike (Sherman, 1997).

is correlated with growth in fire-fighters, he finds elasticities of -0.4 for violent crime and -0.5 for property crime. Both coefficients are only of borderline statistical significance.⁴

3. *Monthly changes in crime and police resources.* Corman and Mocan (2000) use monthly time-series data for New York City over the period 1970-1996. They argue that the use of monthly data minimizes any simultaneity bias because of lags in the political response to rising crime. If it takes policy makers say three months to increase the police force when crime rises, then annual data will be contaminated by simultaneity, but monthly data will not (assuming that policy makers do not effectively anticipate future changes). They report elasticities of crime with respect to the number of police that range from -0.29 to -1.39 across crime categories, with a median value of -0.45.
4. *Make absence of simultaneity plausible.* Marvell and Moody (1996) employ a Granger-causality approach, demonstrating that increases in police Granger-cause reductions in crime. Using data for 56 cities for 1971-1992, the elasticity of total crime (violent and property crime combined) is estimated to be -0.30. Kovandzic and Sloan (2002) employ a similar design, using county-level data for 1980-1998. Estimated elasticities of total crime, robbery and burglary with respect to police levels are -0.14, -0.21 and -0.19, respectively. Tauchen, Dryden Witte and Griesinger (1994) use a 10 percent random sample of males born in 1945 in the city of Philadelphia and traces their activities from 1964 to 1970. Tests for endogeneity show support for the hypothesis of exogeneity of police resources. They estimate the elasticity of the real police budget per offence with respect to the number of years that an individual will not get arrested to be 0.47.

It is important to note that most of these results are based on historical data – often covering more than 20 years. These studies suggest that a negative effect of police on crime is not just the result of recent innovations in policing.

2.5 Conclusion

Police effectiveness flows from deterrence and incapacitation. For a long time, empirical findings supported the idea that the police could not substantially bring down crime. In line with these findings, empirical work based on Dutch data has not found evidence that changes in police resources affect crime rates. After addressing issues of simultaneity and unobserved variables in improved research designs, recent deterrence research in other countries repeatedly shows a negative effect of police on crime. These findings are predominantly based on (American) historical data, ranging back as far as the 1970s.

⁴ In an earlier attempt, Levitt (1997) used the electoral cycle as an instrumental variable. He finds that increases in the size of police forces in 59 large U.S. cities are disproportionately concentrated in mayoral and gubernatorial election years. After correcting for an error in Levitt (1997), McCrary (2002) concludes that the electoral cycle is too weak an instrument to identify the effect of police on crime.

3 Data

In this chapter, we describe the sources of data used. All data are available upon request (PMB data can be found at www.minbzk.nl). Below, we discuss two essential inputs to the analysis: data on police personnel and on crime and nuisance. In section 3.3, we provide summary statistics for all variables included in the analysis.

3.1 Police personnel

Data on police resources were obtained from the Interior Department. Historical series of police levels are only available at the regional level. For all 25 police regions, growth in police personnel outstripped growth in population in this period. The total number of police personnel in full time equivalents increased from 38,429 in 1995 to 47,964 in 2002. The number of police personnel per 100,000 population grew from 249 to 298 in this period, amounting to a 20 percent increase.

3.2 Crime and nuisance

We use a unique data source on victimisation of crime and nuisance in the 25 police regions, the Politie-monitor Bevolking (PMB). This telephone survey is unique in its sample size (enabling cross-region comparisons), its long history (since 1993), and its broad scope (crime and nuisance, preventative measures, feelings of safety, satisfaction with police services). For every year, respondents have been selected at random from the total population. Per police region (and sometimes smaller areas), the interviewers used stratified sampling. A minimum of 1,000 respondents were interviewed in each police region. Many regions opted for a higher number of respondents: the total number of respondents interviewed in a year was usually around 80,000. The PMB has a high response rate; 72 percent in 2003. When aggregating individual responses to the level of police regions, weights have been used. The weights are used to bring the age and gender distribution of the sample in line with the actual distribution. The victimisation survey does not include data on homicides and crimes against businesses and tourists. Until 2000, the survey was conducted every odd year. Currently, the survey is conducted annually.

Victimisation of crime has been on the decline for almost every region. The decline has been strongest in more rural regions. In the five most urban regions, crime declined by some 5 percent; in the five most rural regions, crime declined by some 15 percent. Most crime categories followed the national trend, with victimisation of violent crime as the only exception.

3.3 Summary statistics

Table 3.1 Summary statistics, 1996-2003

	Mean	Standard deviation	Minimum	Maximum	$\Delta 1996-2003$ (%) ^a
Deterrence measures					
Police personnel (FTEs) per capita (*100,000) (t-1)	252.4	82.9	169.9	580.0	19.6
Capacity justice system per capita ^b (*100) (t-1)	0.7	0.3	0.5	1.8	11.2
Crime and nuisance					
Victimisation of burglary (% residences)	5.5	1.9	2.4	12.8	- 39.0
Victimisation of bicycle theft (% bicycles)	5.3	2.7	2.0	19.4	- 24.2
Victimisation of purse theft (% population)	3.4	1.2	1.1	8.6	- 22.5
Victimisation of auto crime (% cars)	31.7	10.5	13.6	64.6	- 17.1
Victimisation of property crime (% population)	35.6	5.9	23.6	50.0	- 10.7
Victimisation of threat (% population)	5.4	1.4	2.2	9.7	16.3
Victimisation of violent crime (% population)	5.9	1.5	2.4	10.6	12.7
Nuisance from youth in public spaces (% population) ^c	12.2	2.9	7.4	21.8	7.6
Nuisance from public intoxication (% population)	7.4	1.7	3.9	14.6	1.4
Nuisance from drug users (% population)	6.1	2.7	2.6	18.0	- 17.1
Nuisance from graffiti (% population)	13.0	5.4	4.9	32.6	- 30.9
Nuisance from noise (excl. traffic) (% population)	9.0	3.5	3.7	28.2	- 21.1
Recorded property crime per capita (*100,000)	6322.6	2031.1	3404.0	14349.0	3.2
Recorded violent crime per capita (*100,000)	649.6	254.2	239.0	1821.0	58.6
Recorded burglaries per capita (*100,000)	574.4	181.5	301.6	1176.3	3.0
Recorded auto crime per capita (*100,000)	1409.3	733.3	123.2	4048.6	- 18.9
Recorded bicycle thefts per capita (*100,000)	934.1	162.2	492.2	1365.9	- 7.0
Recorded purse thefts per capita (*100,000)	232.3	303.3	39.1	2012.6	41.6
Inclination to report auto crime (% victims)	29.8	3.6	19.9	37.8	- 4.4
Inclination to report burglary (% victims)	45.9	6.8	21.7	63.3	- 11.6
Inclination to report bicycle theft (% victims)	45.4	8.5	18.4	64.3	- 10.1
Inclination to report violent crime (% victims)	16.1	4.4	5.6	31.6	- 6.7
Inclination to report other personal crimes (% victims)	24.5	3.5	14.9	37.1	- 16.1
Control variables					
Welfare caseload (% labour force) ^d	5.9	2.2	2.7	15.8	- 39.7
Single men aged 15-24 (% population)	6.0	0.5	5.1	7.4	- 7.1
Single men aged 25-34 (% population)	4.7	0.8	3.5	8.0	- 2.2
2nd generation non-western immigrants (% population) (t-3)	2.4	1.7	0.6	9.8	53.0
2nd generation non-western immigrants aged 15-30 (% pop.)	0.7	0.6	0.1	3.5	102.1
Divorced men (% population)	2.3	0.5	1.3	4.0	16.7
Number of cars per capita (*1000)	391.9	44.4	245.0	521.0	20.6
Additional hinges and locks (% residences) (t-2)	74.9	4.1	59.6	82.0	7.2
Burglar alarm (% residences) (t-2)	8.3	2.0	3.7	14.1	27.5

Notes: (a) The growth over this seven-year period is also lagged with one or two years when a variable is lagged. (b) Number of convictions weighted with severity of crime. Severity of crime is equal to the number of days someone is on average incarcerated for having committed that crime. (c) Respondents are asked whether they consider a certain type of nuisance to be a frequently occurring event in their neighbourhood. The figure indicates the percentage of people who answered the question affirmative. (d) Includes people receiving unemployment insurance (WW) and people on welfare assistance (Abw).

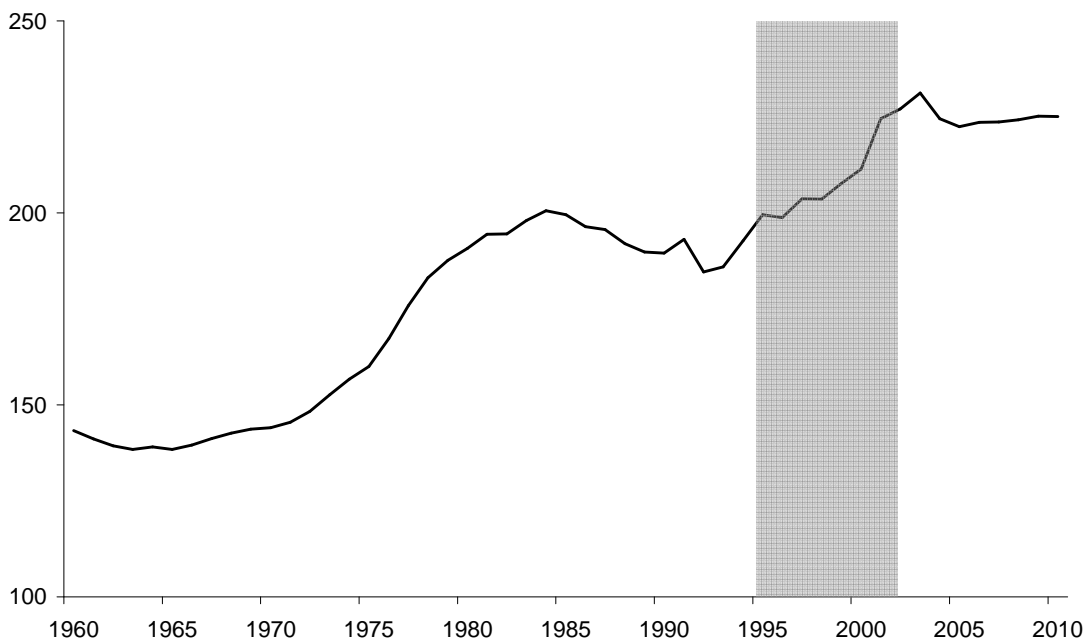
4 Research design

In this chapter, we discuss the design of the empirical analysis. Section 4.1 presents the policy intervention that we analyse. Section 4.2 discusses the basic structure of the analysis. In section 4.3, we discuss how we address simultaneity: crime affects police levels and police levels affect crime.

4.1 The policy intervention

‘More blue on the streets’ was the political ambition of the Kok I (1994-1998) and Kok II (1998-2002) administrations. Polls show that during the period 1992-2000, fighting crime and maintaining public order, already the greatest concern of the public, became an even higher priority (Becker, 2003). The belief in the benefits of having more police officers on the streets translated into a strong rise in police resources. In the last ten years, police personnel per capita in the Netherlands grew at a rate not seen since the 1970s (figure 4.1). The victimisation survey PMB, which commenced in 1993, allows us to closely follow the effects of the inflow of these resources. The combination of a major policy intervention and high-quality data provide good conditions for the analysis of police effectiveness. The analysis commences in 1996 because of data availability.

Figure 4.1 Number of police officers per 100,000 population, 1960-2010



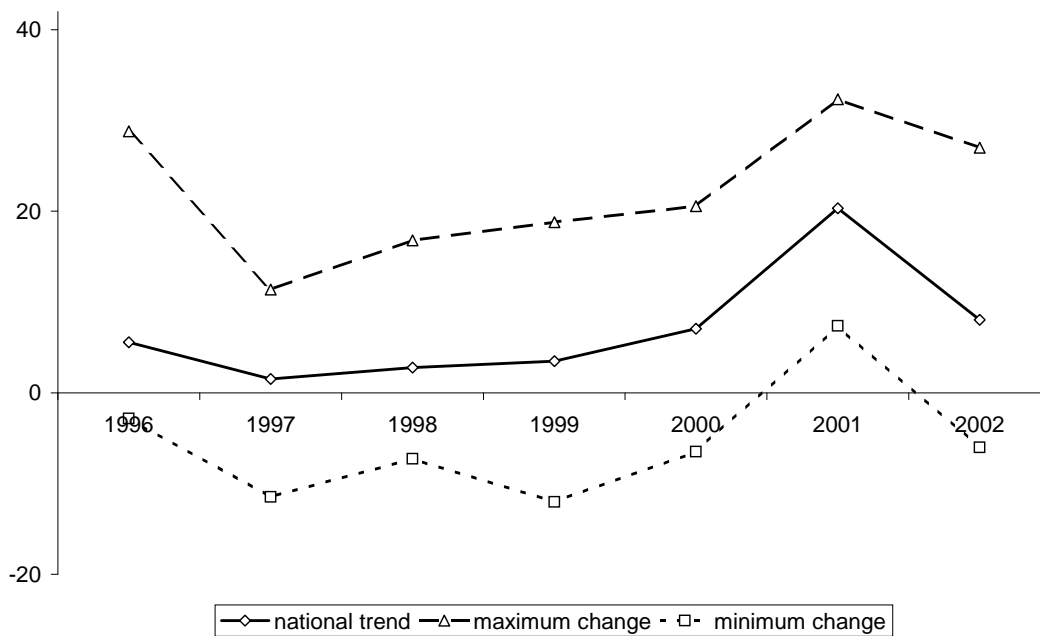
Source: WODC, Department of the Interior, Statistics Netherlands.

Note: The shaded area indicates the years included in the analysis. Non-administrative police personnel only.

4.2 Analysing differences in growth between police regions

Although every police region enjoyed growth in its personnel, there still is quite some variation between the 25 regions. Figure 4.2 shows the national trend in police personnel per capita and the range of growth around the trend (taking the maximum and minimum absolute change in every year – as a note: the same region can enjoy maximum growth in one year and minimum growth in another year).

Figure 4.2 Absolute year-to-year changes in police personnel per 100,000 population



Source: Interior Department, Statistics Netherlands.

A decomposition of the variance in the annual growth rates of regional police personnel per capita shows that only 40 percent of the variance can be explained by the national trend. We use the remaining 60 percent of the variance to identify the effect of police on crime. This variance in the 'intensity of treatment' should teach us whether relatively fast growth in regional police resources leads to a relatively fast decline in regional crime.

We relate the natural logarithm of the (absolute) change in police per capita to the natural logarithm of the percentage point change in crime rates. We assume that each region has its own specific level of police resources and crime that remains constant over time. After all, the relative plenitude of police resources in crime-ridden Rotterdam-Rijnmond compared to rural and safe Zeeland does not imply that more police leads to more crime. By including region-specific effects, our estimates of police effectiveness are based only on *changes* in police levels and crime rates within regions over time.

Additionally, by accounting for year-specific effects, we focus on *differences* in growth between regions. This way, we abstract from nation-wide trends in crime rates and police resources and focus on differences between regions instead.

The basic structure of this so-called difference-in-difference approach is as follows:

$$\ln(Crime_{r,t}) = a \ln(Police_{r,t-1}) + b \ln(Controls_{r,t}) + c \mu_r + d y_t + e_{r,t}$$

$Crime_{r,t}$ is the percentage of population victimised at least once in year t and police region r ;

$Police_{r,t-1}$ is the number of police personnel per 100,000 population in region r and year $t-1$;

$Controls_{r,t}$ are variables driving regional crime trends that are correlated with the regional growth rates in police resources, such as the welfare caseload;

μ_r are the region specific effects;

y_t are the year specific effects;

$e_{r,t}$ is the error term.⁵

Choice of deterrence measure

We use personnel instead of budget figures. The time lag between the impact of more personnel on crime is shorter than between budgets and crime (because of time involved with recruiting, hiring and training). Data on both personnel and budget start no earlier than 1995. A lag of three or more years between budgets and crime (rather than a one-year lag between personnel and crime) would mean that we can only use crime figures as of 1998. Given the small size of our data set, losing observations implies too large a reduction in power.

We prefer police personnel above the clearance rate for three reasons. First, the clearance rate provides too narrow a focus. Next to criminal investigations, emergency assistance and foot patrol are important parts of daily police work. This is of particular concern since we include nuisance in the analysis. Second, the clearance rate is plagued with measurement error (Smit et al, 2003). Third, the clearance rate does not reflect severity of crimes: a lower clearance rate may merely reflect a focus on more severe crimes rather than a lower degree of deterrence (Wiebrens, 2002).

As usual in the empirical literature, we assume a one-year lag in the impact of police on crime. It takes time before police personnel is effective, i.e. actually deters and incapacitates criminals. Based on monthly data for New York, Corman and Mocan (2000, p. 595) estimate that in the case of robberies, motor-vehicle thefts and burglaries it takes some 12, 14 and 21 months respectively for arrest rates to affect crime. Thus it takes at least a year before greater police numbers have an impact.

⁵ A modified Wald test for groupwise heteroscedasticity based on Greene (2000, p. 598) indicates that we have to reject the null hypothesis of no heteroscedasticity. Therefore, we estimate the difference-in-difference equation using feasible generalised least squares (as also suggested by Hausman and Kuersteiner, 2004) under the assumption of heteroscedasticity within panels.

We aim to identify the *marginal* effect of one additional police employee on the rate of crime and nuisance. We have neither theory nor evidence on how the marginal effect of police changes over the level of police personnel per capita. We will follow the literature in assuming constant return to scale. None of the articles discussed in chapter 2 provides leads for modelling increasing or decreasing returns to scale. Consequently, the findings may be specific to the level of police personnel per capita in the Netherlands. Moreover, we do not know whether future expansions in police personnel provide greater or smaller returns than in the past because of scale effects.⁶

Additionally, we focus on the effect across police regions, i.e. we identify the *average* marginal effect. We will allow for a different impact of police in urbanised regions in the robustness analysis in chapter 5.

Our research design is similar to Marvell and Moody (1996). We do not include linear regional trends in the regression equation, however, because of the small size of our panel data set. If police personnel changes are related to specific regional trends in crime, then our model specification could result in estimation bias. Over the time span of 7 years, the regional patterns in factors driving crime are quite similar, however. As we will discuss in the next section, we control for factors driving regional crime trends and changes in police personnel.

Pooling cross-sectional time series data provides us with 150 observations (25 regions and 6 years). The difference in crime trends between regions should be noticeable if we are to believe recent estimates for the US discussed in chapter 3. The elasticity of police resources with respect to property crime is estimated to be around -0.5. The average difference between annual percentage growth in regional police levels per capita and the national trend of 2.2 percent should lead to a difference of about 1.1 percent between regional property crime rates and the national trend in property crime.

4.3 Simultaneity between trends in regional police levels and crime rates

A crucial assumption underlying our research design may not necessarily be met: *annual changes in police personnel do not follow changes in crime trends between regions*. If changes in regional police levels would follow changes in regional crime rates, then we would *underestimate* police effectiveness. In that case, more police is correlated with more crime after all. This is a classic case of simultaneity: crime affects police and police affects crime.

⁶ When conducting the analysis, a quadratic relationship between police and crime appears to provide statistically significant results for some types of crime and nuisance. The estimated impact of police on crime tends to be slightly higher than in the case of a linear relationship. Thus our results are not highly sensitive to alternative specifications – and the impact of further expansions in police personnel are likely to be at least as large as estimated in the linear form.

The absence of simultaneity may sound heroic in the case of this policy intervention. After all, it implies that policy, distributing police personnel between the police regions, is exogenous to the policy problem, trends in regional crime. A closer look at the way police resources have been distributed among the regions shows the likely absence of a causal relation between current year's crime trend and last year's growth rate in police personnel.

In contrast with researchers studying American panel data, we actually *know* how the police budget has been distributed. The Interior Department uses a budget formula to distribute some 95 percent of the police budget among police regions (next to the formula are a number of region-specific extra allowances and deductions). A number of predictors of work load enter into the formula. The work load predictors are defined at the level of the municipality. Each of these predictors is weighted and the result determines the resources per municipality (and, by summing, per police region). Stylised, the formula looks as follows:

$$\text{Police} = f(\text{Projected population, Residences, Length of roadways, Moves, Shops, Non-western second generation immigrants, Housing density} * \text{residences})$$

Since our analysis is on a *per capita* basis, projected population, residences and the product of housing density and residences are outside our scope.⁷ Clearly, most of the other variables are not strongly related to regional trends in crime and nuisance over a ten-year time span. This is very clear for the length of roadways, but also for fluctuations in the number of shops per population and the number of moves per population. Trends in the latter two variables are related to the real estate market and the general economic situation; they have no (statistically significant) relation to regional crime trends.⁸ That leaves us with the number of non-western second generation immigrants as control variable in the regression equation. This control variable from the budget formula is lagged since there is a lagged impact of budgeting decisions on personnel levels (we will explore the length of the lag below).

To exclude any possible remaining simultaneity, we include some other control variables driving regional crime trends, including an indicator of labour market opportunities and an indicator of opportunities for crime. The text box provides an overview.

A substantial part of the variance in regional growth in police personnel is unrelated to regional crime trends for a number of reasons. First, three out of the four crime-related variables in the budget formula are poor predictors of crime *trends*. Their predictive power has been hampered further by the freeze in variables underlying the budget distribution since 2000. This freeze was

⁷ Since relative regional housing density is stable over time, the change in this variable is strongly correlated to the number of residences, and therefore, population.

⁸ Including the number of shops or the number of moves per population in the analysis of the impact of police on crime in chapter 5 does not make a difference (results available upon request from the author).

meant to provide stability in regional police budgets. Second, other considerations also play a role in distributing resources among police regions. Actually, during the '90s, the need for police in the country side was a higher political priority than putting police in urban regions where crime rates are higher (Vollaard, 2004). Not coincidentally, changes in the budget formula played out in the advantage of rural regions. A weak policy response to crime trends is very important for our analysis, because that leaves us with variance in regional growth in police personnel *unrelated* to regional crime trends.

Control variables

To control for the relation between police levels and crime rates, we include two sets of control variables: variables pulled from the budget formula and other known determinants of crime:

- (1) Variable from the budget formula (lagged): *second generation non-western immigrants*.
- (2) Other control variables (determinants of crime):
 - a. *Second generation, non-western immigrants aged 15-30*.
 - b. *Welfare caseload*. Greater labour market opportunities may lower crime rates (see Gould et al., 2002).
 - c. *Divorced men*. Problems during childhood are a major determinant of later delinquent behaviour. A possibly highly disturbing event to children is marital disruption. Increasingly, married couples divorce when they have dependent children. Presently, more than 60 percent of Dutch divorce cases involve dependent children; each year a substantial number of children are affected by divorce (Statistics Netherlands). Using US data, Levitt and Lochner (2001) find that unstable families have a strong effect on juvenile crime.
 - d. *Single men aged 15-24 and 25-34*. These population groups tend to be more criminal than other age groups. They have a lower labour market attachment than older people (Levitt and Lochner, 2001) and they are likely to adopt lenient values towards crime through social interaction (according to a criminological theory, differential association theory, criminal behaviour is learned behaviour via social interaction with others).
 - e. *Number of cars per capita*. Greater wealth produces more opportunities for property crime. One indicator is the number of cars per capita. We include this variable when analysing auto crime and total property crime.

We also know that budget decisions do not have an instantaneous impact on police personnel. The budget formula is in terms of current year variables (except population). When the formula indicates that a region should receive a relatively greater share of the national police budget, it may take considerable time before police personnel is in place. It takes time to hire and train new police personnel after all. Since we have annual data on police personnel and crime, the time lag should be longer than a year to be able to escape simultaneity problems. This condition is most likely to be met, since police school takes two years to complete.

To reiterate, most likely there is a time lag between regional crime trends and budgeting decisions – and the relation between the two is, at best, weak. We will exploit these two facts to avoid simultaneity bias in our estimation of police effectiveness. To be able to do that, we need to conduct two empirical tests: (1) is there a time lag of more than a year between budgeting decisions and changes in police personnel? and (2) is the relation between the two weak?

Table 4.1 How do regional trends in crime and nuisance affect the distribution of police personnel?

Dependent variable: police personnel per capita

	(1)	(2)	(3)	(4)
Victimisation				
Total victimisation (t-1)	- 0.05 (0.04)			
Total victimisation (t-2)	0.06 (0.04)			
Property crime (t-1)		- 0.04 (0.03)		
Property crime (t-2)		0.05 (0.03)*		
Violent crime (t-1)			0.01 (0.01)	
Violent crime (t-2)			0.01 (0.01)	
Nuisances (t-1)				0.05 (0.07)
Nuisances (t-2)				0.04 (0.06)
Lagged dependent variables				
Police personnel (t-1)	0.72 (0.07)***	0.72 (0.07)***	0.72 (0.07)***	0.73 (0.07)***
Police personnel (t-2)	- 0.18 (0.06)***	- 0.19 (0.06)***	- 0.18 (0.06)***	- 0.18 (0.06)***
Control variables				
Second generation immigrants	0.20 (0.07)***	0.19 (0.07)***	0.21 (0.07)***	0.22 (0.07)***
Welfare caseload	- 0.09 (0.03)***	- 0.09 (0.03)***	- 0.11 (0.03)***	- 0.11 (0.03)***
Single men 15-24	- 0.13 (0.10)	- 0.13 (0.10)	- 0.11 (0.09)	- 0.15 (0.10)
Single men 25-34	0.36 (0.11)***	0.35 (0.11)***	0.38 (0.11)***	0.38 (0.11)***
Divorced men	0.22 (0.06)***	0.23 (0.07)***	0.25 (0.06)***	0.24 (0.05)***
Number of cars	0.07 (0.04)*	0.07 (0.04)*		
Wald test for variable groups				
Crime rates (two lags)	3.5	3.4	1.1	1.8
Control variables	73.5***	73.1***	80.5***	82.5***
Year specific effects	175.9***	169.7***	185.6***	179.2***
Region specific effects	118.4***	116.8***	114.6***	115.9***
Sample size	200	200	200	200

Notes: Standard errors in parentheses. All variables are logged. Coefficients estimated using generalised least squares assuming heteroscedasticity within panels. Crime and nuisance rates imputed for 1995, 1997 and 1999. The results for individual region and year fixed effects are not presented. *** Significant at 1%; ** Significant at 5%; * Significant at 10%.

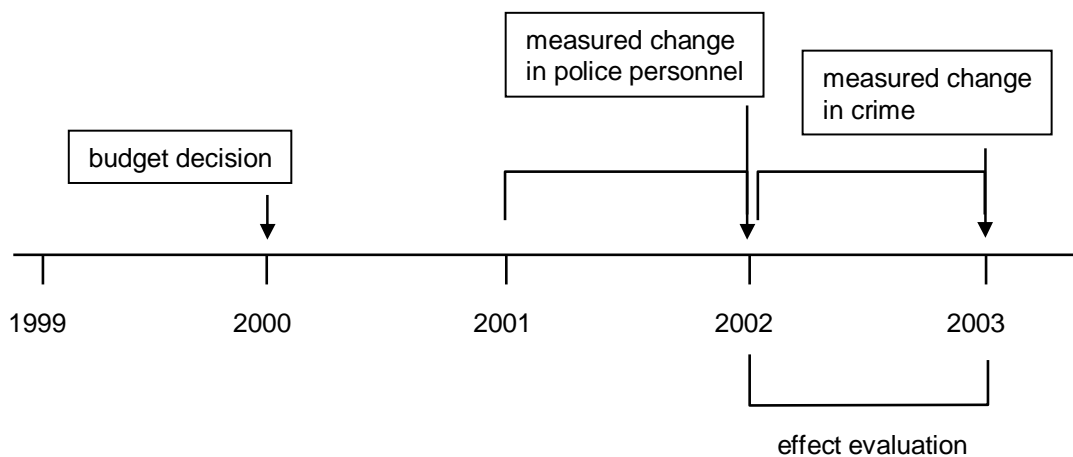
How long is the time lag between budgeting decisions and changes in police personnel?

In line with Marvell and Moody (1996), we use the Granger causality test to show that budgeting decisions do not have an instantaneous impact on personnel levels. The Granger test is used to explore causal direction and determines the ability of one variable to predict another. We regress police levels on crime and nuisance lagged one and two years and on two lags of themselves. Later lags were dropped since they were not significant and they did not result in a decline in the significance level of the Wald test.

Table 4.1 presents the Granger causality test results for total victimisation (1), property crime (2), violent crime (3) and a composite indicator for nuisance (4) on police levels. The results provide evidence that there is at least a two-year lag between budgeting decisions and changes in police levels. The coefficient on the one-year lag is never significant. It is reasonable to

assume that if there is no effect at the one-year lag, there is also no current-year impact. If there was, it would be evident in the one-year lag due to the serial correlation between current-year and prior-year crime levels. The two-year lag is positive, but only statistically significant in the case of property crime.⁹ When we extend the analysis to three lags (not shown), we generally find the second year lag to have the highest value. Thus, the effect of budget decisions on personnel levels are most clearly seen two years later. Figure 4.3 illustrates the two-year lag between the budget decision and the effect evaluation.

Figure 4.3 The time lag between budget decisions, changes in police personnel and the impact on crime



How strong is the relation between budgeting decisions and regional crime trends?

The long term elasticity for total crime with respect to police is equal to 0.1.¹⁰ Thus a one percent increase in victimisation of crime leads to a 0.1 percent increase in police personnel per capita two years later. In the period 1995-2002, the (absolute) difference between the annual trend in regional and national crime was on average 5 percent points. Therefore, the average effect of diverging regional crime trends is a 0.5 percent point difference in growth of police resources. The average (absolute) difference in annual regional growth in police resources compared to the national trend is 2.2 percent point. Thus, on average, more than three quarters of the *difference* in growth in police resources between regions is unrelated to diverging regional crime trends.

The budget formula includes determinants of overall crime trends. Therefore, it is not surprising that total victimisation has a relatively large effect on police levels compared to the effect of specific crime types such as burglary and auto crime (not shown). Consequently, estimating the

⁹ Craig (1987) suggests that budgeting decisions react to reported crime rather than victimisation. Indeed, when we conduct the analysis for reported crime, we find that recorded crime 'Granger-causes' police personnel (i.e. the two lagged crime variables have a significant impact on police personnel), with the first lag insignificant and the second lag significant.

¹⁰ The long term elasticity is equal to $(0.06/(1-0.72+0.18)) = 0.1$.

effect of police personnel on specific crime types also makes the possible estimation bias through simultaneity less likely.

Serial correlation between current crime and police lagged two or more years

Shocks to crime rates may carry over to other years. Such shocks may result from cumulative effects of omitted variables on crime. Omitted variables could include the lagged impact of factors outside the model (the number of drug addicts, for instance) or of any (further) lags of independent variables within the model (the lagged impact of the delinquent behaviour of children in broken families on other children, for instance).

Serial correlation violates the assumption that errors associated with observations in a given year are uncorrelated with errors in ensuing years. If present, we need to control for serial correlation, since it may both affect the estimated standard errors and the point estimates. In the latter case, there is 'indirect simultaneity'. The slow response in police personnel changes is correlated with the lagged effect of shocks that caused crime to rise. As a consequence, the effect of police on crime is biased towards zero.

A test on serial correlation in panel-data models based on Wooldridge (2002, p. 282) suggests that first-order serial correlation is only present in one case: burglary (not shown). The test is based on the correlation between current and lagged residuals from a regression of the first-differences variables. In these two cases, serial correlation is weak. As we will show in the robustness analysis in chapter 5, our results do not change when we address serial correlation.

Conclusion

We conclude that simultaneity is not a major hurdle in estimating the police-crime relationship. It is not likely that we underestimate police effectiveness as a result of simultaneity for the following reasons:

- There is no current year or one-year lagged response of policy to crime trends, since it takes two years before budget decisions affect police personnel. Indirect simultaneity through serial correlation between current crime and police lagged two or more years is weak or not present.
- Following regional crime trends was not a priority in police budgeting. Less than three quarters of the differences in inter-regional growth rates in police personnel was related to diverging regional crime trends.
- When distributing the police budget, the government looks at overall crime rates rather than individual crime types. Therefore, the possible simultaneity bias is even weaker when estimating police effectiveness for individual crime types.

4.4 Conclusion

We exploit differences in regional growth rates of police numbers to identify the impact of police on crime. Although personnel levels increased in every police region, some 40 percent of the variance in regional growth levels is unrelated to the national trend. Moreover, more than three quarters of the difference in growth in police personnel between regions is unrelated to diverging regional crime trends.

Based on an analysis of the timing sequence between hiring police and crime, we conclude that simultaneity bias in estimating police effectiveness is not likely (current-year and one-year lagged impact of crime on police and also serial correlation between current crime and police lagged two or more years). Therefore, we proceed with estimation of the effect of police on crime in the next chapter.

5 Estimation results

In this chapter, we estimate the effect of police on property crime, violent crime and nuisance using the research design discussed in chapter 4. In section 5.4, we discuss the robustness of the results. Section 5.5 discusses competing explanations for our findings. Section 5.6 concludes.

Note that the estimated coefficients for control variables such as the welfare caseload should be interpreted with caution. As stated in chapter 1, our aim is to identify the causal effect of police on crime – the control variables are included for that purpose only. We do not claim that we are able to identify the *causal* effect of any control variable. When discussing the estimation results, we will focus on the *sign* of the coefficient for these variables rather than the exact size.

5.1 Did more police personnel lead to less property crime?

Table 5.1 presents the estimation results for victimisation of several types of property crimes. In all cases, the estimated effect of police has the expected, negative sign.

Table 5.1 The effect of police on property crime, 1996-2003

Dependent variable	(1) Burglary ^a	(2) Auto crime ^b	(3) Bicycle theft	(4) Purse theft ^c	(5) Property crime
Deterrence measure					
Police personnel (t-1)	- 0.55 (0.32)*	- 0.47 (0.20)**	- 0.40 (0.28)	- 0.77 (0.36)**	- 0.17 (0.12)
Economic, social, demographic trends					
Immigrants (t-3)	0.26 (0.44)	0.43 (0.26)*	0.35 (0.38)	1.39 (0.52)***	0.34 (0.13)***
Young immigrants	- 0.14 (0.48)	- 0.70 (0.29)**	- 0.35 (0.38)	0.28 (0.58)	- 0.45 (0.15)***
Welfare caseload	0.14 (0.22)	0.09 (0.15)	- 0.05 (0.20)	0.45 (0.25)*	0.11 (0.08)
Single men 15-24	0.93 (0.90)	1.21 (0.51)**	1.42 (0.68)**	0.08 (1.03)	0.92 (0.28)***
Single men 25-34	1.14 (0.70)*	0.25 (0.45)	0.92 (0.60)	- 0.12 (0.78)	0.01 (0.27)
Divorced men	0.23 (0.47)	- 0.60 (0.32)*	- 0.23 (0.42)	-0.54 (0.61)	- 0.33 (0.19)*
Number of cars		0.41 (0.17)**			0.18 (0.08)**
Wald tests variable groups					
Control variables	10.7*	29.2***	14.4**	24.2***	43.0***
Region-spec. effects	231.2***	527.3***	218.5***	139.8***	445.3***
Year-specific effects	12.8**	37.3***	2.7	15.8***	52.7***
Sample size	150	150	150	150	150

Notes: (a) Includes burglary attempts. (b) Includes car theft, theft of property in car and vandalising cars. (c) Excluding violent theft.

All variables are logged. Estimated using feasible generalised least squares under assumption of heteroscedasticity within panels. Years included: 1996, 1998, 2000, 2001, 2002, 2003. Results for region and year fixed effects not presented.*** Significant at 1%; ** Significant at 5%; * Significant at 10%

Burglary

The estimated effect of more police personnel on burglary is negative and significantly different from zero at the 10 percent confidence level (column 1). A one percent increase in police personnel per capita leads to a 0.55 percent decline in burglary. Given the high standard error, there is quite some variation in the way greater police levels affect burglary rates. In the American literature, researchers tend to find somewhat lower effects. Corman and Mocan (2000), Marvell and Moody (1996) and Kovandzic and Sloan (2002) find elasticities for burglary in New York City, major US cities and Florida counties of -0.41, -0.32 and -0.19, respectively.

The effect of police works partly through private prevention – if people indeed follow prevention advice from the police. The police has been highly active in providing advice to people how to protect their homes and staged some publicity campaigns (including the implementation of ‘Politiekeurmerk Veilig Wonen’, a building code for safe homes). The impact of police levels on burglary rates declines if we control for the presence of burglar alarms and additional hinges and locks (not shown).¹¹ Burglar alarms and hinges and locks are estimated to have a negative impact on burglary, the estimated coefficients are -0.49 and -0.91 and they are significant at the 1 and 12 percent confidence level, respectively. Given the increase in the use of burglar alarms (+28 percent) and additional hinges and locks (+7 percent), together they explain some 20 percent points of the 39 percent decline in burglary over 1996-2003.

Most coefficients of the control variables are not statically significant. A higher number of young, single males is correlated with a higher burglary rate.

Auto crime

The estimation results for auto crime are presented in column 2. Auto crime includes car theft, theft out of cars and vandalising cars. The results suggest that the growth in police personnel had a negative impact on auto crime at the 5 percent confidence level. The elasticity of police personnel with respect to auto crime is -0.47. Thus a one percent increase in police personnel per capita leads to a 0.5 percent decline in auto crime. Given the increase in police personnel of 20 percent in the period 1995-2002, the police may account for 10 percent points out of the 17 percent decline in auto crime.

Estimates in the literature are limited to one element of auto crime: car theft. We estimated the effect of police on auto crime excluding auto theft (i.e. theft out of cars and vandalising cars)

¹¹ In this analysis, prevention has been lagged two years. Many people only take prevention measures after becoming victim of a burglary. For instance, within 12 months after a burglary about a third of the victims purchase additional hinges and locks. Therefore, same-year victimisation rates and prevention measures are highly correlated. Additionally, it takes some time before prevention devices start to pay off.

and found similar results (-0.44). We cannot separately estimate the effect of police on car theft, since the infrequent occurrence of auto theft makes the survey results for this crime type unreliable. Given the similar effects of police on theft out of cars/vandalising cars and auto crime in total, we expect the elasticity with respect to auto theft to be in the same range. The estimated effect on auto crime is in line with the elasticity with respect to car theft of -0.33 reported by Di Tella and Schargrodsky (2004). It is lower than the elasticity of -0.85 reported by Marvell and Moody (1996).

The signs of the statistically significant control variables are as expected, apart from the sign of young second generation immigrants and divorced men. There may be a third factor correlated with both young immigrants and divorced men and burglary rates that obscures the effect, probably the number of drug addicts. If the number of immigrant youth grows relatively fast in areas where the junkie population declines relatively fast, then we will underestimate the effect of immigrant youth on crime.

Bicycle theft

The increase in police personnel is also estimated to have a negative effect on bicycle theft, but the effect is not significant at the 10 percent level (it is at the 15 percent level). There is too much variation in the way an increase in police personnel affects bicycle theft to make general statements.

The sign of the (statistically significant) estimated coefficient for young, single men is as expected. A one percent increase in the share of young, single males in the population leads to a 1.2 percent increase in bicycle theft.

Purse theft

We find higher police levels to have a negative effect on purse theft, the estimated coefficient is statistically significant at the 5 percent level. Based on the elasticity of -0.77, purse theft declined by some 15 percent as a result of greater police personnel levels, which is more than half of the total decline in purse theft over the period 1996-2003.

We find the share of second generation immigrants and welfare caseload to be positively correlated with regional trends in purse theft.

Property crime

We find rising police levels to have a small and insignificant effect on total property crime. An explanation for the insignificant result is the apparent little impact of police on 'other theft' (not shown). Levitt (2002) finds an elasticity for property crime of -0.50, which is in line with the separately estimated elasticities for most types of property crimes.

The estimation results suggest that the increase in police personnel had a substantial negative impact on property crime. For burglary, auto crime and purse theft, we were able to estimate the magnitude of the effect with 90 percent statistical confidence. There is too much variation in the impact of police to make a similar general statement about the effect of police levels on bicycle theft and overall property crime. The effect of police on burglary works also through prevention advice and other activities promoting private prevention. Installing additional locks and hinges and burglar alarms has a large negative effect on burglary rates.

5.2 Did more police personnel lead to less violent crime?

The trend in victimisation of violent crime differs from the trend in property crime. For most years, violent crime has been on the increase. Only since 2001, there has been a decline. Would the trend have been even more unfavourable if there had not been a major increase in police personnel? Table 5.2 presents the estimation results.

Table 5.2 The effect of police on violent crime, 1996-2003

Dependent variable:	Threat	Violent crime
Deterrence measure		
Police personnel per capita (t-1)	- 0.61 (0.28)**	- 0.69 (0.30)**
Economic, social and demographic trends		
Second generation immigrants (t-3)	0.87 (0.42)**	0.45 (0.42)
Second generation immigrants 15-30	1.45 (0.47)***	1.59 (0.46)***
Welfare caseload	- 0.15 (0.20)	- 0.27 (0.21)
Single men 15-24	- 1.88 (0.70)***	- 0.93 (0.72)
Single men 25-34	0.41 (0.59)	0.15 (0.62)
Divorced men	- 0.30 (0.43)	0.14 (0.47)
Wald test variable groups		
Control variables	27.8***	22.9**
Region-specific effects	112.6***	99.2***
Year-specific effects	23.0***	16.7***
Sample size	150	150

Notes: Standard errors in parentheses. Years included: 1996, 1998, 2000, 2001, 2002, 2003. All variables are logged. Estimated using feasible generalised least squares assuming heteroscedasticity within panels. Results for individual region and year fixed effects are not presented. *** Significant at 1%; ** Significant at 5%; * Significant at 10%.

The results suggest that the increase in police levels limited the increase in violent crime. The effect of police levels on threat and overall violent crime is statistically significant at the 5 percent confidence level (we cannot estimate the effect on assault: its infrequent occurrence makes the survey results too unreliable). A one percent increase in police levels results in a 0.6 percent in threat and a 0.7 percent decline in violent crime. To compare: Corman and Mocan (2000) find an elasticity for violent crime of -0.29; Levitt (2002) finds an elasticity of -0.44. As we will discuss in section 5.4, the higher elasticity may be due to the use of victimisation data

rather than police statistics. Especially in the case of violent crime, changes in reporting and recording behaviour may bias the estimated effect of police on crime towards zero.

The share of second generation immigrants in the population is positively and statistically significant correlated with violent crime. When controlling for immigrant youth, we find a negative effect of the group of 15-24 year old men. We also find an negative, although insignificant, sign for the welfare caseload. Levitt (2002, p. 1247) reports a similar result. Raphael and Winter-Ebmer (2001) discuss the factors that may give rise to this result, including the pro-cyclical variation in the degree of interpersonal exposure of possible victims to potential offenders. This greater exposure may result from the fact that when more people are working and away from home, the quantity of encounters with potential offenders increases. The current specification allows such relations between the state of the economy and violent crime.

The growth in police personnel had a significant and substantial impact on violent crime.

5.3 Did more police personnel lead to less nuisance?

In the victimisation survey, people have also been asked whether they consider several types of nuisance as a frequently occurring problem in their own neighbourhood. This variable does not only reflect actual victimisation, but also people's perceptions of nuisance crime.

Table 5.3 The effect of police on nuisance, 1996-2003

Dependent variable:	(1) Youth nuisance	(2) Drugs nuisance	(4) Graffiti	(5) Public intoxication	(6) Noise nuisance ^a
Deterrence measure					
Police personnel (t-1)	- 0.24 (0.21)	- 0.26 (0.33)	- 0.64 (0.26)***	- 0.50 (0.25)**	- 0.24 (0.22)
Economic, social and demographic trends					
Immigrants (t-3)	0.30 (0.31)	0.17 (0.40)	0.42 (0.39)	0.70 (0.36)**	0.45 (0.31)
Young immigrants	- 0.85 (0.31)***	- 0.20 (0.47)	0.01 (0.36)	- 0.22 (0.37)	- 0.81 (0.25)***
Welfare caseload	0.24 (0.16)	0.04 (0.22)	- 0.09 (0.17)	0.08 (0.17)	- 0.01 (0.17)
Single men 15-24	1.85 (0.63)***	1.03 (0.81)	0.53 (0.62)	0.62 (0.70)	1.04 (0.47)**
Single men 25-34	0.41 (0.45)	1.75 (0.65)***	- 0.85 (0.53)	- 0.36 (0.53)	0.74 (0.41)*
Divorced men	- 1.04 (0.32)***	- 1.21 (0.45)***	0.22 (0.39)	- 0.29 (0.36)	0.54 (0.34)
Wald test variable groups					
Control variables	32.7***	23.7**	8.6	10.7*	42.8***
Region-specific effects	509.9***	770.9***	470.5***	235.3***	548.0***
Year-specific effects	31.1***	36.4***	102.5***	17.7***	118.0***
Sample size	150	150	150	150	150

Notes: (a) Excluding traffic noise. Years included: 1996, 1998, 2000, 2001, 2002, 2003. Standard errors in parentheses. All variables are logged. Estimated using feasible generalised least squares assuming heteroscedasticity within panels. Results for individual region and year fixed effects not presented. *** Significant at 1%; ** Significant at 5%; * Significant at 10%.

Table 5.3 presents the estimation results. We find a statically significant impact of police levels on public intoxication and graffiti. The estimated elasticities suggest that the increase in police personnel resulted in a 10 percent decline in the perception of public intoxication as neighbourhood problem and a 12 percent decline in graffiti. The effect of police on youth nuisance, drugs nuisance and noise nuisance is negative, but not statistically significant. The differences in impact of police across regions are too great to reliably estimate the magnitude of the effect.

We find expected effects of all second generation immigrants (t-3) and young single men on nuisance, but a negative effect of second generation immigrant youth and divorced men – as was the case for auto crime. As discussed in section 5.1, the number of drug addicts may be a third factor correlated with nuisance and these two control variables.

The estimation results indicate that the increase in police personnel had a substantial and statistically significant impact on nuisance from public intoxication and graffiti. There is too much variation of the impact on youth nuisance, drugs nuisance and noise nuisance across police regions to reliably estimate the magnitude of the effect.

5.4 Robustness of the results

In Chapter 2, we argued that improvements in research design had a major impact on the study of police effectiveness. Recent studies better account for the simultaneity problem and omitted variable bias. To benefit from these advances, we largely follow the design of Marvell and Moody (1996) – a design also applied by Konvandic and Sloan (2002). In line with the results in the recent literature, we find a negative impact of police on crime.

Within this research design, there still are choices to make when conducting the empirical analysis – although our flexibility is limited by the size of our panel data set. Four major choices are the source of crime data, the way to address serial correlation, how to model differences in the impact of police levels across regions and the functional form.

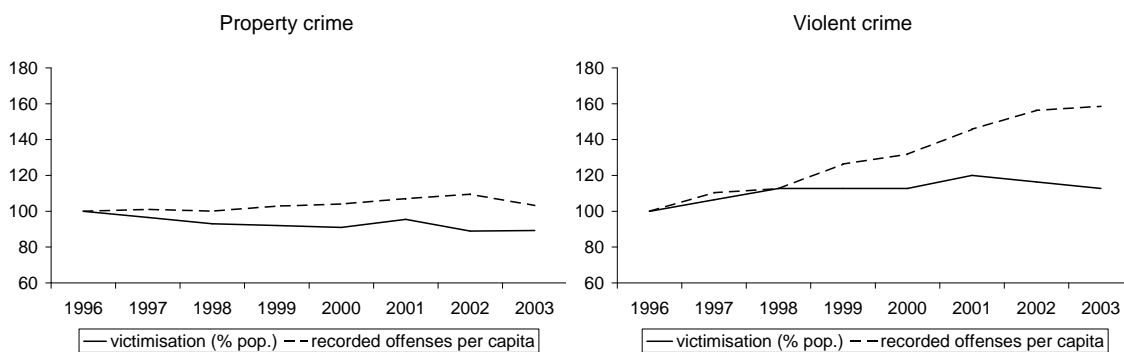
(1) Police statistics as alternative source of crime data

As an additional test on the results, we will also conduct the analysis using police statistics as source of crime data. Recorded crime seems like a logical source of crime data. After all, it excludes offences that are not important enough to report to the police (about 40 percent) or that are solved by the victim (about 10 percent) (POLS, 2004).

In practice, there are many reasons why we prefer to use victimisation data, given the choice between the two sources. First, recorded crime does not reflect all ‘relevant crime’. In 15 percent of the cases, a crime is not reported because people believe that the police will not ‘do

something about it' (POLS, 2004). Second, there is a gap between reported and recorded crime: the police have some freedom in deciding whether to record a reported crime or not. Sometimes, a crime is not recorded because it is not a priority of the police. Third, *trends* in recorded crime may be biased through changes in reporting behaviour and recording practices. In particular in the case of violent crime, recording practices are said to have changed considerably (the inclination to report has declined somewhat according to the PMB). Figure 5.1 illustrates the diverging trends in recorded crime and victimisation. Measurement error through changes in reporting and recording behaviour is hard to control for. Fourth, a major advantage of using victimisation data is a broader scope: next to crime, we also look into several types of nuisance.

Figure 5.1 Trends in victimisation data and police statistics, property crime and violent crime (1996=100)



Source: PolitieMonitor Bevolking, Statistics Netherlands.

Clearly, victimisation data are not the ideal source of crime data either. Offences not worthy to report to the police may have a dynamics of its own – possibly biasing estimation of police effectiveness. Moreover, some crimes are not included in the victimisation survey, including murder and crimes against businesses. Finally, we measure the number of people victimised at least once a year, not the total number of offences. If the degree of multiple victimisation does not change over time, then this fact should not bias our results.

A priori, it is hard to say how the use of recorded crime will affect our findings. There are two contrasting forces. On the one hand, the effect of police on victimisation of crime may be lower because the impact of police on non-reported crime is probably low. On the other hand, if the share of crime reported to the police actually increases with a rise in police resources, then we will underestimate police effectiveness.

Table 5.4 presents estimation results for total property crime, violent crime, and several types of property crimes, using both data sources for roughly the same years. We control for the inclination to report, a variable included the victimisation survey. For property crimes, we also find a negative effect of police on crime. We find very similar results for purse theft and total

property crime. The estimates for burglary, auto crime and bicycle theft are significantly *lower* in the case of recorded crime (even more so since we control for changes in reporting behaviour of the public). More active recording of bicycle theft, auto crime and burglary may explain this result.

Table 5.4 The effect of police on crime: victimisation survey vs. police statistics

	(1a)	(1b)	(2a)	(2b)
Dependent variable:	Burglary ^b Victimisation	Police stats.	Auto crime Victimisation ^c	Police stats. ^d
Police personnel (t-1)	- 0.55 (0.32)*	- 0.12 (0.24)	- 0.47 (0.20)**	- 0.15 (0.27)
Inclination to report ^a		0.21 (0.05)***		0.38 (0.09)***
Dependent variable:	(3a) Bicycle theft Victimisation	(3b) Police stats.	(4a) Purse theft Victimisation	(4b) Police stats.
Police personnel (t-1)	- 0.40 (0.28)	- 0.09 (0.17)	- 0.77 (0.36)***	- 0.87 (0.35)***
Inclination to report ^a		0.20 (0.05)***		- 0.11 (0.11)
Dependent variable:	(5a) Property crime Victimisation	(5b) Police stats.	(6a) Violent crime Victimisation	(6b) Police stats.
Police personnel (t-1)	- 0.17 (0.12)	- 0.15 (0.13)	- 0.69 (0.30)**	0.24 (0.15)
Inclination to report ^a		0.05 (0.05)		0.01 (0.02)

Notes: (a) Inclination to report that specific crime, except for property crime: inclination to report all crime. (b) Excluding private prevention. (c) Including car theft, theft out of cars, vandalising cars. (d) Including car theft, theft out of cars. Results for control variables and region and year fixed effects are not reported. Years included: victimisation of crime: 1996, 1998, 2000-2003 (n=150); recorded crime: 1997-2003 (n=175). Standard errors in parentheses. All variables are logged. Estimated using feasible generalised least squares assuming heteroscedasticity within panels. *** Significant at 1%; ** Significant at 5%; * Significant at 10%.

In the case of violent crime, using police statistics as an alternative source of crime data yields very different results. We actually find a *positive* relation between police levels and violent crime.¹² Since we control for changes in reporting behaviour of the public, changes in recording behaviour of the police most likely explain this finding. It is for this type of measurement error in police statistics that we use victimisation data.

(2) Functional form

As usual in the literature, we estimate the elasticity of police personnel per capita with respect to the crime rate. The advantage of this method is the straightforward interpretation of the coefficients. Alternatively, we can estimate a linear model. In that case, we assume the effect of a unit-increase in police personnel per capita to have a similar *percent-point* effect on the crime rate across time and regions. When the variables are not logged the estimated impact of police

¹² The estimated effect of police personnel on recorded vandalism and disruption of public order is negligible (not shown). This finding provides further evidence that the use of recorded crime may lead to underestimation of police effectiveness.

on crime increases in the case of property crime and nuisance, and is similar in the case of violent crime (table 5.5).

Table 5.5 The effect of police on crime and nuisance: using the linear functional form

	(1) Burglary ^a	(2) Auto crime	(3) Bicycle theft	(4) Purse theft
Police personnel (t-1)	- 0.03 (0.01)***	- 0.10 (0.02)***	- 0.01 (0.01)**	- 0.02 (0.00)***
Implied elasticity ^b	- 1.47***	- 0.79***	- 0.62**	- 1.17***
Base case	- 0.55*	- 0.47**	- 0.40	- 0.77**
	(5) Property crime	(6) Threat	(7) Violent crime	(8) Youth nuisance
Police personnel (t-1)	- 0.03 (0.02)**	- 0.01 (0.01)**	- 0.02 (0.01)**	- 0.02 (0.01)**
Implied elasticity ^b	- 0.22**	- 0.69**	- 0.67**	- 0.49**
Base case	- 0.17	- 0.61**	- 0.69**	- 0.24
	(9) Drugs nuisance	(10) Graffiti	(11) Public intoxication	(12) Noise nuisance
Police personnel (t-1)	- 0.01 (0.01)	- 0.04 (0.01)***	- 0.02 (0.01)***	- 0.04 (0.01)***
Implied elasticity ^b	- 0.27	- 0.86***	- 0.76***	- 1.11***
Base case	-0.26	-0.64***	-0.50**	-0.24

Notes: (a) Excluding private prevention. (b) Evaluated at the mean of police personnel and the crime rate. Years included: 1996, 1998, 2000, 2001, 2002, 2003 (n=150). Results for control variables and region and year fixed effects not reported. Standard errors in parentheses. Estimated using feasible generalised least squares assuming heteroscedasticity within panels. *** Significant at 1%; ** Significant at 5%; * Significant at 10%.

(3) Indirect simultaneity through serial correlation

As stated in chapter 4, the changes in police personnel may be correlated with the lagged effect of shocks that caused crime to rise. In case of such ‘indirect simultaneity’, the effect of police on crime may be biased towards zero. Econometric tests learn that serial correlation is only present in one case: burglary.

Table 5.6 The effect of police personnel on crime, two ways of addressing serial correlation

	(1a)	(1b)
Dependent variable: burglary	Base case	Lagged dependents ^b
Police personnel (t-1)	- 0.55 (0.32)*	- 0.57 (0.32)*

Notes: (a) Excluding private prevention as controls. (b) Dependent variables have been included lagged two and four years since crime and nuisance are observed every even year until 2000; missing observations for 1997 and 1999 have been imputed. Years included: 1996, 1998, 2000, 2001, 2002, 2003 (n=150). Results for control variables and region and year fixed effects not reported. Standard errors in parentheses. All variables are logged. Estimated using feasible generalised least squares assuming heteroscedasticity within panels. *** Significant at 1%; ** Significant at 5%; * Significant at 10%.

As a robustness test, we estimate the model with a common way of dealing with serial correlation: including lagged dependent variables. We only report results for burglary: in all other cases, the differences in the estimates are negligible. Table 5.6 shows that addressing serial correlation does not lead to different conclusions about the effect of police on crime.

(4) Differences in police effectiveness between regions

We have estimated the *average* marginal impact of police personnel. In some cases, there is too much variation in the impact of police across regions to make general statements. It is possible that the impact of police levels on crime would be greater in urbanised police regions. A higher degree of urbanisation allows a police officer to keep an eye on a greater number of people. To test whether there is an additional impact in urban areas, we include an interaction term for police levels in the four most urbanised ‘Randstad’ regions (Amsterdam-Amstelland, Rotterdam-Rijnmond, Haaglanden, Utrecht).

Table 5.7 Did increases in police personnel have a greater impact in the four most urbanised regions?

	(1a)	(1b)	(2a)	(2b)
Dependent variable:	Burglary ^a		Auto crime	
Police personnel (t-1)	- 0.55 (0.32)*	- 0.68 (0.31)**	- 0.44 (0.20)**	- 0.49 (0.20)**
Within Randstad		- 1.45 (0.43)***		- 0.48 (0.35)
Dependent variable:	(3a) Bicycle theft	(3b)	(4a) Purse theft	(4b)
Police personnel (t-1)	- 0.38 (0.27)	- 0.45 (0.28)*	- 0.74 (0.36)**	- 0.75 (0.37)**
Within Randstad		- 0.73 (0.40)*		- 0.11 (0.50)
Dependent variable:	(5a) Property crime	(5b)	(6a) Threat	(6b)
Police personnel (t-1)	- 0.15 (0.12)	- 0.17 (0.12)	- 0.61 (0.28)**	- 0.65 (0.28)**
Within Randstad		- 0.14 (0.15)		0.48 (0.49)
Dependent variable:	(7a) Violent crime	(7b)	(8a) Youth nuisance	(8b)
Police personnel (t-1)	- 0.69 (0.30)**	- 0.73 (0.30)**	- 0.24 (0.21)	- 0.41 (0.20)**
Within Randstad		0.60 (0.50)		- 1.27 (0.30)***
Dependent variable:	(9a) Drugs nuisance	(9b)	(10a) Graffiti	(10b)
Police personnel (t-1)	- 0.26 (0.33)	- 0.28 (0.33)	- 0.64 (0.26)***	- 0.79 (0.25)***
Within Randstad		0.23 (0.38)		- 1.55 (0.35)***
Dependent variable:	(11a) Public intoxication	(11b)	(12a) Noise nuisance	(12b)
Police personnel (t-1)	- 0.50 (0.25)**	- 0.49 (0.24)**	- 0.24 (0.22)	- 0.26 (0.21)
Within Randstad		- 0.93 (0.39)**		- 1.00 (0.40)***

Notes: (a) Excluding private prevention as controls. Years included: 1996, 1998, 2000, 2001, 2002, 2003 (n=150). Results for control variables and region and year fixed effects not reported. Standard errors in parentheses. All variables logged. Estimated using feasible generalised least squares assuming heteroscedasticity within panels. *** Significant at 1%; ** Significant at 5%; * Significant at 10%.

Table 5.7 presents the results. The first column (a), provides the results for the effect across all regions presented in the sections above. The second column (b) presents the results of a test whether the impact of police in Randstad regions is significantly different from other regions (true in 6 out of 12 cases). The impact of police in regions *outside* the Randstad is equal to the first coefficient; the impact *within* Randstad regions is equal to the sum of both coefficients. For

instance, the effect of police levels on bicycle theft is -0.45 for non-Randstad regions and -1.18 for Randstad regions.

For some property crimes (burglary, bicycle theft) and some types of nuisance (youth nuisance, graffiti, public intoxication and noise nuisance), we find the impact of more police to be significantly greater in Randstad regions than in the rest of the country. For violent crime we do not find a (significantly) different impact. If present, the difference in impact is substantial: it is two to three times larger in Randstad regions than in other regions.

The results also show that the estimated nationwide impact of police is not driven by the Randstad regions. After all, the estimated effect in regions outside the Randstad is often highly similar to the nation-wide effect.

Based on an analysis of the robustness of our findings, we conclude the following:

- (1) Using police statistics as alternative source of crime data, we also find a negative effect of police on property crime. The magnitude of the effect is smaller, most likely as a result of changes in recording behaviour of the police. This issue plays a larger role in the case of recorded violent crime: in this case, we even find a positive effect of police levels on crime. Since we cannot control for changes in recording behaviour, we prefer to use the PMB victimisation survey rather than police statistics as our source of crime data.
 - (2) Estimating a linear model rather than a log-log model produces similar results for violent crime and somewhat higher estimates of the effect of police levels on property crime and nuisance.
 - (3) When we allow changes in police personnel to be correlated with the lagged impact of shocks to crime two or more years ago, we do not arrive at different conclusions about police effectiveness.
 - (4) We find no evidence that our results are driven by the impact of police levels in particular regions – we do find evidence of a substantially greater impact of police personnel in highly urbanised regions on several types of property crime and most types of nuisance.
-

5.5 Refuting alternative causal explanations

We aim to estimate the causal effect of additional police personnel on national trends in crime and nuisance. In this section, we refute three alternative explanations that are not in line with this causal interpretation of our findings.

(1) Displacement of crime between regions causes the negative impact of police on crime

When police appear some place, criminals just go somewhere else, keeping overall crime constant. Clearly, this theory of ‘displacement of crime’ does not hold *within* police regions. After all, we find a substantial effect of police on regional crime. Apparently, the police forces are able to bring down their overall crime rate.

But displacement of crime could also be a factor *between* police regions. In that case, criminals keep shifting their activities from regions with relatively strong growth in police personnel to regions with relatively slow growth in police personnel. The *overall* effect of more police on crime is *lower*. In the extreme case, crime was merely redistributed between police regions. Clearly, the extreme case is unrealistic since all regions enjoyed considerable growth in their police resources: there was no ‘safe haven’ for criminals to flee to.

We limit the possibility of displacement by using regions rather than neighbourhoods or cities as unit of analysis. After all, if there is any mobility, a substantial part of it is likely to be within the area of a police region. Additionally, empirical results from the literature show that displacement effects at greater distances tend to be limited. Actually, diffusion effects, whereby the benefits of enforcement spread to other areas, may be substantial (Rogerson et al., 2003). Based on an analysis of Dutch data, Vollaard (2004) finds evidence that crime displacement is, at best, small, since the inflow of resources in rural areas in the Netherlands resulted in a marked and persistent increase in clearance rates in these regions. Thus, displacement effects are unlikely to result in an overestimation of the overall effect of police on crime.

(2) Changing priorities in policing causes the impact of police on specific crimes

Our estimate of the effect of police on specific types of crime may be biased if the allocation of police personnel follows trends in specific crimes.¹³ Say that we want to estimate the effect of police on burglary. Suppose that regions with relatively unfavourable trends in burglary receive relatively generous funding. If these regions allocate a relatively great share of their personnel to bringing down burglary, then we will *overestimate* the effect of additional police personnel on burglary. After all, we only measure the growth in overall regional police personnel, not the growth in regional personnel actually allocated to bring down burglary.

Given the focus on overall crime in funding the police forces (see chapter 4) and the poor state of resource management within police forces (Netherlands Court of Audit, 2003), overestimating police effectiveness as a result of simultaneity between the allocation of police personnel and trends in specific crimes is not likely.

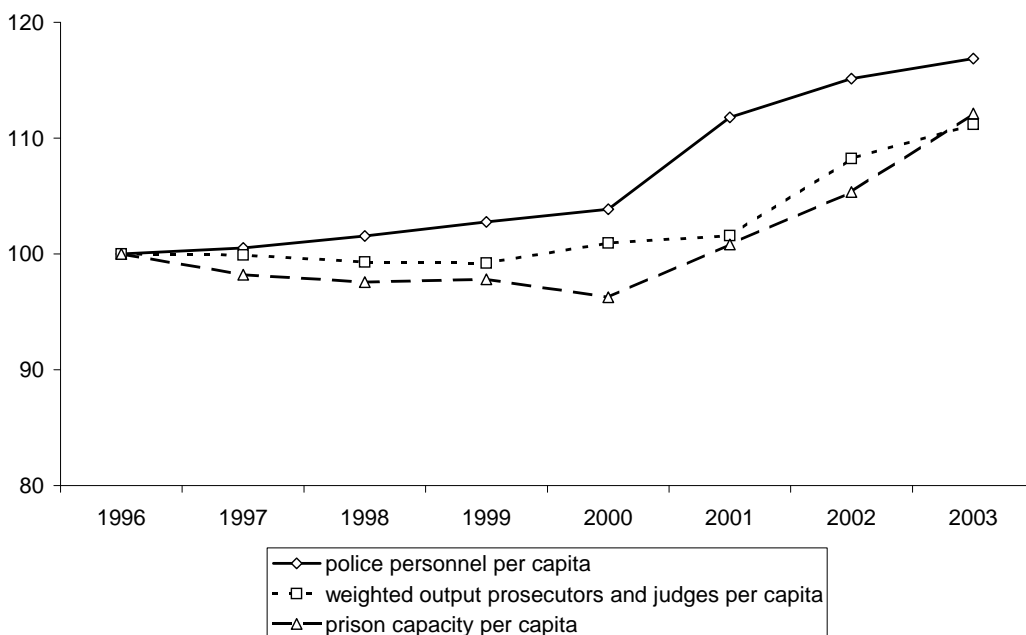
Although we cannot directly test the claim of poor resource management given the lack of data on allocation of police time, our findings provide indirect evidence. Clearly, changing priorities cannot lead to overestimation across the board. If we *overestimate* the effect of police on one specific type of crime, we *underestimate* the effect of police on a different type of crime. We find the police to be effective in multiple areas. The point estimates are often similar; most elasticities are about -0.5.

¹³ Vollaard (2004) provides evidence that the share of regional police resources allocated to fighting crime and nuisance is not affected by differences in crime between police regions.

(3) Expansions in the criminal justice system caused the impact of police on crime

The capacity ‘downstream’ in the criminal justice system (judges, prisons, etc.) is often thought to be the bottleneck in the fight against crime. For instance, some regional prosecution councils have been actively restraining police output because of a lack of capacity (*de Volkskrant*, September 27, 2002). If there are bottlenecks, then an expansion of ‘downstream capacity’ could drive the estimated effect of more police personnel, not the greater number of police personnel itself (like a Leontief production function). Unequal *dynamics* in police capacity and ‘downstream capacity’ may cause a bias in our estimates of the effect of police on crime.¹⁴ In the extreme case, we only find an effect of police on crime if growth in police personnel is correlated with growth in downstream capacity.

Figure 5.2 National trends in police and criminal justice resources, 1996-2003 (1996=100)



Source: Interior Department, Prosecution Council, Statistics Netherlands.

Note: For prison capacity, we take the number of people incarcerated (assuming full and constant occupancy rates).

If we take the number of convictions weighted with the severity of the crime as a proxy for the capacity of prosecutors and district courts, then we see that it has not been keeping pace with the growth in police resources.¹⁵ Figure 5.2 shows correlation between the growth of both organisations – with the a lagging growth in capacity of prosecutors and district courts. Figure 5.2 also shows that prison capacity has not kept pace with the growing number of convictions

¹⁴ A constant ‘bottleneck’ downstream restrains police effectiveness, but does not cause a *bias* in our estimate.

¹⁵ The severity of a crime is equal to the number of days someone is on average incarcerated for having committed that crime. Other sanctions such as financial penalties are translated into ‘prison day equivalents’, following the guide lines of the prosecution council. Thanks to Caspar Wiebrens for providing the data.

either. Although prison capacity is expanding since 2000, the shortage in capacity is still increasing (see Heide and Eggen, 2003, p. 351).

To see whether expansions of capacity within the criminal justice system drive the results, we include an indicator of the capacity of judges and public prosecutors into the analysis.¹⁶ We have a proxy for the capacity of the prosecution council and judges, since we know the number of criminal cases the court dealt with per police region. We weight each conviction with the severity of the offence (see below).

Table 5.8 Police effectiveness - conditional on growth in capacity of prosecutors and judges

Dependent variable: purse theft ^a	(1)	(2)	(3)
Police personnel (t-1)	- 0.93 (0.46)**		- 0.88 (0.45)**
Capacity prosecutors and district courts (t-1)		- 0.25 (0.16)	- 0.18 (0.16)

Notes: (a) Excludes violent theft. Years included: 1998, 2000, 2001, 2002, 2003 (n=125). Results for control variables and region and year fixed effects not reported. Standard errors in parentheses. All variables are logged. Estimated using feasible generalised least squares under the assumption of heteroscedasticity within panels. *** Significant at 1%; ** Significant at 5%; * Significant at 10%.

The estimation results reported in table 5.8 show that ‘downstream capacity’ is not likely to drive our findings. We report results for purse theft; this is the only type of crime for which we do not find simultaneity between trends in regional crime and in capacity of prosecutors and judges (based on a Granger analysis). The first column presents the impact of police on purse theft; the second column the impact of ‘downstream capacity’ on purse theft. The impact of police investigations, prosecutors and judges together (column 2) is smaller than the impact of police work in all its breadth (column 1). Comparing the first and third column, we see that the impact of police is (significantly) lower, when we include dynamics in ‘downstream capacity’. The difference in impact is only 12 percent, however, which is within the range of error.

Displacement of crime between regions, changing priorities in the allocation of police personnel and concurrent growth in criminal justice capacity are not likely alternative explanations of the estimated impact of police on crime.

5.6 Conclusions

We find significantly negative effects of higher police levels on violent crime and on most types of property crime and nuisance. Our estimates suggest that a substantial proportion of the decline in crime and nuisance during the period 1996-2003 is attributable to the increase in police personnel.

¹⁶ We cannot include prison capacity in the analysis since it is determined at the national level. National trends are cancelled out in a difference-in-difference approach.

6 Conclusions

In this final chapter, we return to the research question: what is the effect of increases in the level of police personnel on victimisation of crime and nuisance? We discuss the findings and the relevance of these findings for policy.

6.1 Increase in police personnel substantially decreased chance on victimisation of crime and nuisance

Based on the estimation results, we conclude that the increase of 10,000 FTEs in police personnel during the period 1995-2002 significantly lowered the chance of becoming victim of crime and nuisance at least once during a year. We could most reliably estimate the magnitude of the effect of police on burglary, auto crime, bicycle theft, purse theft, threat, violent crime, and on nuisance from youth in public spaces, public intoxication and graffiti. In the case of nuisance from drug users, we also find a negative effect of more police, but the impact on crime and nuisance differs too much across regions to reliably estimate the magnitude of the effect.

Table 6.1 Impact of additional police personnel on crime and nuisance (%), 1996-2003 ^{a, b}				
	20 percent increase in police personnel per capita resulted in (90 % confidence interval):			Trend 1996-2003
Change in chance on becoming victim of...	Minimum	Best estimate	Maximum	
Burglary	- 1	- 11	- 21	- 39
Auto crime ^c	- 3	- 9	- 16	- 17
Bicycle theft	- 1	- 10	- 19	- 24
Purse theft	- 3	- 15	- 27	- 23
Threat	- 3	- 12	- 21	+ 16
Violent crime	- 4	- 14	- 23	+ 13
Nuisance from youth in public spaces ^d	- 3	- 10	- 17	+ 8
Nuisance from public intoxication ^d	- 2	- 10	- 18	+ 1
Nuisance from graffiti ^d	- 4	- 13	- 21	- 31

Notes: (a) The impact of police is reported in *percent* change in the victimisation rate, not in *percent points* change. (b) For the estimated effect on bicycle theft and nuisance from youth, the results from table 5.7 have been used. (c) Theft of cars, theft out of cars, vandalising cars). (d) Percentage of people saying that this type of nuisance is a frequently occurring problem in the neighbourhood.

Table 6.1 provides an overview of the impact of increases in police personnel on crime and nuisance during 1996-2003. Generally, crime and nuisance declined by some 10 percent as a result of more police. We find the effects of more police to be greatest in urbanised regions. For instance, we find the impact on crime per police officer often to be two to three times larger in the Randstad than in other regions.

When comparing the effect of police on crime and the total change in crime over 1996-2003 (the last column in table 6.1), the results show that there are also many other forces at work. These other factors include the population of young men (declining, therefore negative effect on crime) and of second generation immigrants (increasing; positive effect).

We estimate a one percent increase in police personnel per capita to result in a 0.4 to 0.7 percent decline in crime and nuisance. The estimated effect of police on crime is in line with recent American studies. As Levitt (2002) reports, four different approaches in the American literature have all obtained point estimates in the range of -0.3 to -0.7.

6.2 Lessons for policy

The findings of this study provide the following four lessons for policy:

1. *A higher number of police personnel results in a lower level of crime and nuisance.* This study shows that the increase in police resources had the intended effect. It is hard to say whether the effect is sufficiently large to justify the additional public expenditures (0.7 billion euro for the regional police forces since 1995). To that end, we should value the benefits of the resulting decline in crime and nuisance. The benefits are hard to identify. For instance, the effect of better police protection on private expenditures on prevention is unclear. When trading off the costs and benefits of expenditures on the police, justice is a factor as well. Society wishes to prosecute and incarcerate people who have committed certain criminal acts. It is a political choice to trade off the several types of benefits against the costs of more police personnel.
2. *More police has a substantial negative effect on crime, despite possible displacement of crime within police regions.* When police appear in one part of a police region, criminals may flee to another part of the region. Despite this possible displacement of crime within police regions, we find a substantial effect of police on crime. Apparently, the police forces were able to bring down their overall crime rate.
3. *Redistributing police personnel from rural to urbanised regions helps to reduce the national level of crime and nuisance.* For several types of crime and nuisance, we find the effect of more police to be greater in urbanised regions. For instance, we find the impact on crime per police officer often to be two to three times larger in the Randstad than in other regions. Clearly, there are also other considerations when (re)distributing resources, including availability of the police in rural areas.
4. *Until 2010, no major expansions in the number of police personnel per capita are planned: the administration's ambitious plans to lower crime and nuisance mainly rely on improvements in*

police effectiveness. Research into ‘what works, and what doesn’t’ helps to show the best way of improving police effectiveness. The promised 4,000 additional police officers ensures that population growth does not bring down the number of police personnel *per capita*. Thus the police’s contribution to the target of 20-25 percent reduction in crime and nuisance mainly relies on better use of existing resources. Foreign research shows that innovations in methods of policing can make a difference. So far, research on ‘what works, and what doesn’t’ within the Dutch context is scarce (see Wittebrood and Van Beem, 2004). More and better evaluation research is necessary to provide guidance on how to best improve police effectiveness.

Repeated victimisation surveys like the Politiemonitor Bevolking (PMB) are a welcome input to evaluations of police practices. Since victimisation data are not subjected to changes in reporting and recording behaviour like police statistics, they provide a better picture of crime trends. Moreover, compared to police statistics, the PMB broadens the scope to several types of neighbourhood problems not found in police statistics, including graffiti and nuisance from youth. Thus, continuing the PMB in its current design is of great importance to effective public policy.

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