Crime, apprehension and clearance rates: Panel data evidence from Canadian provinces

Philip A. Curry *University of Waterloo*
Anindya Sen *University of Waterloo*
George Orlov *University of Western Ontario*

Abstract. The Becker (1968) model of crime establishes the importance of the probability of apprehension as a key factor in a rational individual's decision to commit a crime. In this respect, most empirical studies based on US data have relied on variation in the number of police officers to estimate the impact of the probability of apprehension or capture. We measure the probability of apprehension by clearance rates and study their effects on crime rates, employing a panel of Canadian provinces from 1986 to 2005. OLS, GMM, GLS and IV estimates yield statistically significant elasticities of clearance rates, ranging from −0.2 to −0.4 for violent crimes and from −0.5 to −0.6 for property crimes. These findings reflect the importance of police force crime-solving productivity.

Résumé. Crime, arrestation et taux d'incidents élucidés par la police: résultats à partir de données de panels pour les provinces canadiennes. Le modèle de crime de Becker (1968) établit l'importance de la probabilité d'arrestation en tant que facteur important dans la décision de commettre un crime d'un individu rationnel. La plupart des études empiriques basées sur des données américaines ont utilisé le nombre de policiers pour évaluer la probabilité d'arrestation. Les auteurs proposent d'utiliser le taux des crimes élucidés, et étudient leurs effets sur les taux de criminalité à l'aide de données pour un panel de provinces canadiennes de1986 à 2005. Des calibrations obtenues par diverses méthodes (moindres carrés ordinaires, moindres carrés généralisés, méthode des moments généralisés, IV) donnent des élasticités significatives des taux de crime par rapport aux taux d'élucidation: entre −0.2 et −0.4 pour les crimes violents, et entre −0.5 et −0.6 pour les crimes contre la propriété. Ces résultats montrent l'importance de la productivité des corps policiers dans l'élucidation des crimes.

JEL classification: K14, K42

We are grateful to Peter Carrington for many illuminating discussions. We thank Salvador Navarro, Fraser Summerfield and two anonymous referees for their feedback. We also thank Corey Van De Waal for his help in the data collection.
Corresponding author: Phil Curry, pacurry@uwaterloo.ca

Canadian Journal of Economics / Revue canadienne d'économique, Vol. 49, No. 2
May 2016. Printed in Canada / Mai 2016. Imprimé au Canada

0008–4085 / 16 / 481–514 / © Canadian Economics Association

1. Introduction

Becker's (1968) seminal theory of crime hypothesizes that criminals rationally evaluate the benefits of crime against the probability of being caught (apprehension) and the severity of punishment. Early empirical studies[1] often used police-reported clearance rates as a measure of the probability of apprehension. As noted by Chalfin and McCrary (2013), however, most recent econometric studies have focused on the effects of the number of police officers on crime. From a policy standpoint, using the number of police officers is appealing because policy-makers cannot choose the probabilities of apprehension directly, but they can choose the size of the police force. Given that labour is an input into the apprehension production function, understanding the effect of police on crime, even in a reduced form, is valuable.

From a researcher's standpoint, the number of police officers is easy to observe. Further, if changes to the size of a jurisdiction's police force are exogenous and not an artifact of trends in crime rates or unobserved shocks, then OLS estimates will yield unbiased and consistent impacts that can be used in policy evaluation. A statistically significant correlation between more police officers and less crime also yields some very clear options for policy-making.

However, even in such ideal circumstances for empirical research, a correlation between more police officers and less crime does not offer insight into how deterrence is achieved. Presumably, the probability of apprehension is the "output" produced by the police force and the number of police officers is the labour input for this production function. However, an increase in the number of police officers may result in reduced crime simply because of higher visibility rather than an increased probability of apprehension. Furthermore, as discussed in the data section below, the relationship between the number of police officers and clearance rates is murky at best. As such, the exact channel through which the number of police officers affects crime rates is not obvious.

We attempt to complement the existing studies on police and crime by focusing on the effects of clearance rates on crime using panel data for Canadian provinces (1986 to 2005) while including the number of police officers. In the majority of cases, a police-reported incident of crime is said to be "cleared" if an individual associated with the specific criminal act is apprehended by the police. There is a reasonable chance that the deterrence effects associated with an enhanced probability of apprehension will be more precisely captured through clearance rates than the per capita number of police officers or arrests. Further, the importance of clearance rates has been acknowledged by criminologists for some time.

The primary objective of our research is to empirically evaluate the relationship between changes in the probability of apprehension—as captured through clearance rates—and corresponding trends in violent and property crime rates while controlling for the number of police officers. Our empirical strategy is motivated by an extension of the theoretical model developed by Polinsky and Shavell

1 See, for example, Carr-Hill and Stern (1973), Ehrlich (1973), Thaler (1977) and Wolpin (1978).

(2000), which allows us to link spending on police services to corresponding changes in clearance rates and crime and serves as a foundation for constructing plausible instruments. The model also offers some insight on the relative benefits of focusing on clearance rates as opposed to the number of per capita police officers.

There are very few studies based on panel data that have investigated the effects of apprehension on crime rates in Canada.[2] From a general perspective, a study of Canadian trends should be of interest to policy-makers in the US, given similarities in movements in crime rates over time. Specifically, Canada experienced the same dramatic decline in crime during the 1990s that also occurred in the US and that has continued to baffle academics and policy-makers.[3] Figure A1 shows that per capita violent crime rates are higher in Canada and that violent crime fell in both countries from the early 1990s onward. Figure A2 suggests a closer correspondence in property crime rates between the two countries, with a similar persistent drop from the early 1990s. Using Canadian data also allows a rather clean identification of the effects of the probability of apprehension through clearance rates because legislative penalties for violent crime (and most property crimes) are at the federal level and thus can be accounted for through the use of year-specific dummies. On the other hand, there are rather significant and complex state-specific differences in penalties in the US. The presence of unobserved state-varying and time-specific determinants of crime, such as the well-documented crack cocaine epidemic from the mid-1980s to the early 1990s, makes it difficult to ensure unbiased coefficient estimates of clearance rates based on US data. In the absence of proper controls, empirical estimates based on US data may then be confounded if variation in clearance rates coincides with amendments to penalties implemented by state legislatures or unobserved factors such as the crack cocaine epidemic.[4]

OLS estimates yield statistically significant elasticities of clearance rates, ranging from −0.2 to −0.4 for violent crimes and from −0.5 to −0.6 for property crimes. These estimates are robust to the use of a wide array of controls, province and year fixed effects and province-specific linear trends. Comparable results are obtained from generalized least squares (GLS), first difference, generalized method of moments (GMM) and instrumental variables (IV) regressions. In contrast, coefficient estimates of the per capita number of police officers are not always significant or possess the wrong sign. We think that these findings reflect the importance of police force productivity in terms of solving crimes and apprehending criminals linked to specific crimes. However, we note that our instruments are weakly correlated with clearance rates. Therefore, appropriate caution should be used in interpreting the results of this study.

2 Early empirical work on apprehension and Canadian crime is based primarily on time-series data. Examples are Avio (1973), Avio and Clark (1976), Avio and Clark (1978) and Avio (1979).
3 See Levitt (2004) for further details.
4 Cheung and Erikson (1997) suggest that crack cocaine use in Canada was quite insignificant relative to corresponding trends in the US. Fryer et al. (2005) provide a detailed account of the crack cocaine epidemic in the US along with associated costs.

The remainder of the paper is organized as follows. Section 2 contains a review of the relevant literature. Section 3 describes the data and trends in key variables. The theoretical model is presented in section 4. Our empirical model is outlined in section 5. Empirical estimates are detailed in section 6. Section 7 concludes with a summary of our main findings and associated policy implications.

2. Previous literature

As noted, most empirical research has focused on the effects of the number of police officers on crime.[5] Chalfin and McCrary (2013) offer a detailed review of recent studies. We focus on papers that study the effects of arrest or clearance rates. In this respect, the empirical literature on the effects of apprehension is much thinner, and very few of these studies have accounted for potential simultaneity bias in coefficient estimates of arrest rates with respect to crime through the use of instrumental variables or structural models. Key results and measurement of arrest rates from previous studies are detailed in table 1. For the sake of brevity and direct relevance, we restrict our discussion to studies based on US data and that have relied on panel data across jurisdictions and over time in order to evaluate the impacts of arrest or clearance rates.[6,7]

5 Recent studies employ varying strategies to address the simultaneity bias of coefficient estimates of police on crime, which occurs when the size of the police force increases as a response to a corresponding upward trend in local crime rates. Evans and Owens (2007) study increases in the number of police officers generated by grants from the Community Oriented Police Service (COPS) program. Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005) and Draca et al. (2011) identify the impacts of police on crime based on sudden exogenous terror events that require enhanced police presence. Levitt (1997) analyzes data on 59 large US cities with directly elected mayors, 1970 to 1992. His instrumental variables are constructed from mayoral and gubernatorial elections. Levitt's estimates suggest that an increase in the size of the police force reduces violent crime but does not significantly impact property crime. However, McCrary (2002) finds that Levitt's IV estimates are much less precise once some specific programming errors are corrected. In a rejoinder, Levitt (2002) uses a different dataset and obtains results that are comparable to his original 1997 AER paper. Chalfin and McCrary (2013) note that simultaneity between crime rates and police numbers is much weaker than assumed in previous literature, at least at the municipal level (due to institutional constraints). They correct measurement error by using data from both Uniform Crime Reports (UCR) and Annual Survey of Government (ASG) for 242 US cities with populations over 50,000 during the period from 1960 to 2010. The authors use the ASG measure of police as an IV for UCR data and models using UCR data as IV for ASG measures. Like Levitt (1997), their results suggest an array of elasticities across different categories, with violent crime rates being more responsive than property crime rates to changes in police force size.

6 There are, of course, studies that have not relied on panel data. Lochner (2007) uses the National Longitudinal Survey of Youth 1997 Cohort and the National Youth Survey to examine which factors affect an individual's perceived probability of arrest. His results suggest that a 10% increase in the perceived probability of arrest for auto theft reduces auto theft by 7% and major theft by close to 4%. In terms of panel-based Canadian studies, Sen (2007) employs clearance rates as a control, but the emphasis of the study is on the effects of abortion and fertility on crime. Similarly, the focus of Joyce (2009) is on the effects of abortion on US crime. He replicates the results from Donohue and Levitt (2008) and finds that higher arrest rates have a significant and negative impact on some types of violent crime.

7 A number of panel-based studies focus on the effects of the existence of a death penalty on murder rates. Zimmerman (2004, replicated in 2009) uses state-level panel data from 1978 to

TABLE 1
Crime elasticities with respect to arrests

	All	Property crimes				Violent crimes					Measurement
		All	Burglary	Auto theft	Larceny	All	Robbery	Murder	Rape	Assault	
Thaler (1977)[a]		−0.0017 (2.24) −0.00064 (0.165)									Police proportion of solved crimes; arrests over numbers of crime
Craig (1987)	−0.57[b]										Clearances as a proportion of crime
Mathur (1978)[c]			0.055 (0.310) −0.256 (−2.36)	−0.541 (−2.72) −0.505 (−3.06)	0.626 (1.73) 0.486 (2.18)		−3.07 (−2.03) −1.58 (−2.54)	−1.09 (−1.07) −0.094 (−0.217)	−3.21 (−1.34) −1.10 (−1.28)	−0.909 (−1.56) −0.91 (−1.83)	Admissions to prison for the offence divided by reported crimes
Cornwell and Trumbull (1994)[d]	−0.455 (0.618)										Arrests to offences ratio
Levitt (1998)[e]			−0.272 (0.036)	−0.087 (0.028)	−0.204 (0.030)		−0.339 (0.053)	−0.071 (0.072)	−0.119 (0.030)	−0.201 (0.047)	Arrests divided by crimes
Gould et al. (2002)[f]	−0.002 (0.002)	−0.01 (0.002)	−0.01 (0.003)	−0.01 (0.001)	−0.01 (0.001)	−0.004 (0.0003)	−0.006 (0.0005)	−0.002 (0.0002)	−0.004 (0.001)	−0.003 (0.0003)	Arrests to offences ratio
Mustard (2003)[g]			−0.0123 (0.00267)	−0.0052 (0.00108)	−0.0072 (0.00178)		−0.0016 (0.00036)	−0.0035 (0.00025)	−0.0026 (0.00091)	−0.0019 (0.00049)	Arrests divided by offences
Mustard (2003)[h]			−0.0102 (0.00304)	0.0003 (0.0008)	−0.0046 (0.00166)		−0.0035 (0.00076)	0.0000 (0.00034)	−0.0031 (0.00075)	−0.0038 (0.00049)	Lagged arrests divided by lagged offences
Corman and Mocan (2005)[i]			−0.32 −0.27	−0.51 −0.50	−0.14 −0.10		−0.57 −0.59	−0.40 −0.39	−0.32 −0.30	−0.20 −0.24	Number of arrests
Spelman (2005)[j]		−0.1206 (0.0504)				−0.2376 (0.1057)					Arrests per 1,000 people (UCR data)
Agan (2011)									−0.066 (0.024)		Arrests divided by incidents
Garett and Ott (2011)[k]			−0.009	−0.002	−0.175		−0.070	−0.557	−0.037	−0.006	Number of arrests (UCR data)

NOTES: [a]t-statistics are in parentheses. First row reports the result for the police clearance rate. Third row reports the coefficient for the arrests over the number of crimes. [b]Urban type 1 crimes are the focus of the study. [c]t-statistics in parentheses. The first row of elasticities is for 1960, the second is for 1970. [d]From a log-linear specification. [e]First difference estimates with lag of arrest rates included. [f]Marginal effects reported instead of elasticities. [g]Semi-elasticities from the regression of the natural log of crime rates on arrest rates. [h]Semi-elasticities from the regression of the natural log of crime rates on lagged arrest rates. [i]Second row estimates use the average year-to-year growth of arrests in NYC (reported in C&M (2005) Table 3). [j]These elasticities are with respect to prison populations by metropolitan area. [k]Pooled city elasticities are reported. Only robbery is significant at 5%.

Early empirical work on crime and deterrence focused on the effects of clearance rates as a measure of the probability of apprehension. Thaler (1977) uses 1972 census-tract-level data from Rochester, New York, to estimate the effect of deterrence on neighbourhood crime. He emphasizes the importance of measuring the probability of apprehension through an "arrest clearance rate." On page 41, he specifically states "[f]or this measure a crime is only considered cleared if the criminal was arrested specifically for that crime." Carr-Hill and Stern (1973) also rely on clearance rates in their study of crime in police districts in England and Wales in 1961 and 1966, and they are quite clear on the emphasis that should be placed on clearance rates relative to the number of per capita police officers. In particular, they state (pp. 289–90):

> Deterrence theories indicate that this offence rate should depend on the proportion of crimes 'cleared-up' (or clear-up rate), if this reflects perceived probabilities of apprehension. Such theories might also focus on the number of policemen per capita and a measure of the equipment available to each officer.

Craig (1987) uses 1972 data from Baltimore and obtains a −0.57 elasticity of crime with respect to actual clearance rates (generated from 3SLS). Wolpin (1978) obtains comparable results (with respect to clearance rates) based on time-series data from 1955 to 1971 for England, Japan and California.

However, studies from the 1990s focus on the relationship between police-reported crimes and per capita arrests (relative to either the number of reported crimes or population). Cornwell and Trumbull (1994) exploit variation across counties in North Carolina and obtain OLS elasticities of arrest rates (arrests/crimes) with respect to FBI Index crimes ranging from −0.35 and −0.68. On the other hand, 2SLS estimates with fixed effects are statistically insignificant. Lott and Mustard (1997) employ county-level data between 1977 and 1992 and focus on the effects of legislation pertaining to the right to carry concealed handguns. Their reduced form estimates suggest that a higher probability of arrest is linked with lower crime rates. 2SLS estimates of arrest rates (measured by arrests divided by crimes) instrumented by lagged crime rates also result in negative and significant coefficient estimates. However, in their analysis of the Mustard-Lott dataset, Black and Nagin (1998) are unable to replicate the 2SLS findings and question their credibility. Dezhbakhsh and Rubin (1998) note that Mustard and

1997. Dezhbakhsh et al. (2003) employ a panel of county-level data covering the years from 1977 to 1996. The same data are used by Shepherd (2005). Shepherd (2004) relies on panel data at the state level and with monthly observations. Durlauf et al. (2010) and Durlauf et al. (2012) employ similar data to these studies but rely on structural econometric models. Another strand of literature focuses on imprisonment rates. Spelman (2005) uses Texas county-level data to examine the effect of prison population on crime rates. Similar to Corman and Mocan (2005), Spelman finds evidence that public order arrests reduce property crime (counties with zero-tolerance and community-policing policies substantially decreased their crime rates). Ehrlich (1973), Levitt (1996) and Marvel and Moody (1994, 1996, 1998) are other important references on crime and imprisonment.

Lott emphasize OLS findings, which do not correct for the endogeneity of the arrest rate. In contrast, their research suggests coefficient estimates of arrest rates that are less statistically precise and smaller in magnitude than those suggested by Lott and Mustard.[8]

Levitt (1998) exploits data across 59 cities from 1970 to 1992 and studies the effects of arrest rates on the following crimes: murder and non-negligent manslaughter, forcible rape, robbery, aggravated assault, burglary, larceny and motor vehicle theft. His results suggest elasticities between −0.03 and −0.319.[9] However, he cautions on his inability to control for potential endogeneity bias and concludes that, especially for property crime, deterrence is the most likely factor behind the observed negative relationship between crime and arrest rates, as opposed to incapacitation and measurement error.

Shepherd (2002) uses panel data based on all 58 California counties from 1983 to 1996 to explore the deterrent effects of that state's two- and three-strike legislation. Her 2SLS estimates reveal that arrest rates (number of arrests/crimes) have a consistently negative impact on all categories of violent and property crime. Gould et al. (2002) focus on the effects of labour market conditions on crime rates, employing aggregated data at the county level between 1979 and 1997, but do include arrest rates in some specifications. In most cases, an increase in the arrest rate is significantly correlated with reductions in different types of crime. Like Levitt (1998), they also specifically acknowledge the difficulty of finding instruments for arrest rates. Mustard (2003) employs county-level data from New York, Oklahoma, Oregon and Washington from 1977 to 1992 and finds that sentence length has little effect on crime. However, conviction rates have a statistically significant negative effect on crime rates, with coefficients on arrest rates ranging from −0.0016 to −0.012, implying a 0.16% to 1.2% drop in crime rates in response to a 1% increase in the corresponding arrest rates.

Corman and Mocan (2005) analyze the effect of economic conditions and deterrence measures on crime as well and verify the validity of the "broken-windows" hypothesis, which suggests that reduced tolerance towards minor misdemeanours leads to an overall reduction in crime. They employ monthly time-series data on crime levels (for murder, assault, robbery, burglary, motor vehicle theft, grand larceny and rape) in New York City from 1974 to 1999. Besides obtaining evidence in support of the "broken-windows" hypothesis, the authors find that the size of the police force has an effect only on auto theft and grand larceny, while the number of arrests has a statistically significant impact across all types of crime. The authors note that the use of monthly data reduces

8 In a much-cited paper, Duggan (2001) uses county- and state-level data over time and finds that the recent decline in gun ownership explains about one-third of the decline in gun homicides (relative to non-gun homicides). His results suggest that a 10% increase in gun ownership translates into a 1.42% increase in the homicide rate the following year.

9 Calculated as arrests divided by the number of crimes.

the possibility of simultaneity bias of coefficient estimates of the number of police officers, as it usually takes six months to hire more police officers.

In summary, we attempt to complement the existing literature in the following ways. First, despite early studies that clearly emphasized the importance of clearance rates as a measure of the probability of apprehension, we have not been able to locate any recent papers that econometrically investigate the effects of clearance rates on crime based on data across jurisdictions and over time.[10] We think that using clearance rates is important for reasons that are more fully discussed in the next section. Second, employing Canadian data is interesting given the similarities to trends in US violent and property crime observed over the sample period. Third, we assess the sensitivity of our estimates with instrumental variables. As discussed, this is a challenge that has been noted by previous studies, and very few have actually attempted to instrument arrest or clearance rates.[11]

3. Trends in crime rates

Most of our data were downloaded from CANSIM, Statistics Canada's database of socio-economic variables that are publicly available. Table 2 details the table numbers, sources and summary statistics for each variable employed in this study. The key variables are the number of police-reported incidents of different types of crimes and the number of incidents cleared for each of these categories. An incident is cleared when a suspect linked to the crime has been identified by the police. Accordingly, the empirical measure of the probability of apprehension that we employ is the number of incidents cleared *divided* by the number of police-reported incidents.[12] We view the total proportion of incidents cleared as the relevant measure of apprehension because a suspect needs to be identified, irrespective of whether an incident is cleared by charge or otherwise.

An alternative strategy is to use the arrest rate, which is the number of arrests per 100,000 of population. As discussed above, at some point, most empirical studies began to use such arrest rates—or the number of arrests divided by the

10 Mastrobuoni (2013) uses detailed micro-level data on robberies and deployment of two police forces in the city of Milan, but focuses on the effects of local police presence on the likelihood of clearing cases.

11 Numerous studies (Ehrlich 1973; Thaler 1977; Mathur 1978; Craig 1987) from the 1970s and 1980s did use either 2SLS or 3SLS in order to correct for simultaneity bias. However, it is difficult to assess the success of their efforts given the absence of *F-test* statistics and first-stage regression results.

12 An incident can be cleared by charge or otherwise. A suspect needs to be arrested in order for an incident to be cleared by charge. There are many reasons why an incident may be cleared otherwise. Examples include the death of a suspect or the dropping of a charge to a less serious offence. We are indebted to Peter Carrington for some very insightful discussions. Please see Carrington and Schulenberg (2008) for further discussion on clearance rates.

TABLE 2
Data sources and summary statistics

Variable	CANSIM table	Mean	Standard deviation	Min.	Max.
Median income of welfare recipients	2020404	44,111	4,918.2	37,000	58,300
Minimum wage		5.51	1.02	3.65	8.00
Average transfers to poorest quintile	2020301	7,738.5	994.59	5,300.0	10,300
Per capita number of new immigrants	510011	428.69	354.54	60.45	1385.7
Incarceration rate per 100,000 adults	2510005	94.77	33.72	48.00	183.00
Proportion of males aged 15–24	510001	0.08	0.01	0.07	0.10
Police officers per 100,000 of population	2540002	174.94	17.995	136.80	209.20
Provincial population	510001	0.29248E+07	0.34000E+07	0.12841E+06	0.12565E+08
Property crimes per 100,000 of population	2520013	4749.7	1,642.0	2,342.4	9,007.8
Clearance rate for property crimes	2520013	0.26	0.06	0.13	0.42
Employment rates 15 years and over	510001	89.89	3.75	79.94	96.07
Violent crimes per 100,000 of population	2520013	1,040.7	316.19	478.33	2,059.4
Clearance rate for violent crimes	2520013	0.71	0.062	0.52	0.89
Homicides per 100,000 of population	2520013	1.9967	0.97674	0	4.33
Clearance rate for homicides	2520013	0.85	0.29	0	2.53
Attempted murders per 100,000 of population	2520013	83.69	102.52	0	408.00
Clearance rate for attempted murder	2520013	0.82	0.26	0	2.00
Sexual assaults per 100,000 of population	2520013	107.78	40.07	28.39	227.40
Clearance rate for sexual assaults	2520013	0.66	0.15	0.41	1.34
Physical assaults per 100,000 of population	2520013	908.66	274.83	374.53	1753.2
Clearance rate for physical assaults	2520013	0.75	0.07	0.53	0.89
Robberies per 100,000 of population	2520013	78.46	53.18	4.87	196.59
Clearance rate for robberies	2520013	0.41	0.15	0.24	1.41
Motor vehicle thefts per 100,000 of population	2520013	427.21	266.93	82.860	1,359.5
Clearance rate for motor vehicle thefts	2520013	0.22	0.099	0.047	0.52
Breaking & entering per 100,000 of population	2520013	1,157.4	421.98	515.39	2,097.0
Clearance rate for breaking & entering	2520013	0.19740	0.62216E−01	0.77497E−01	0.54948
Real police expenditures per capita of population	2540002	155.03	34.643	80.826	235.67
Real police expenditures per 100,000 of police	2540002	87,931	13,776	57,981	0.12637E+06

NOTE: Minimum wage data were extracted from the Government of Canada minimum wage database, available at srv116.services.gc.ca/dimt-wid/sm-mw/menu.aspx?lang=eng.

number of crimes—as measures of the probability of apprehension. However, per capita arrest rates do not clearly yield a probability of apprehension because states or provinces with more crime are mechanically more likely to have higher arrests per 100,000 of population. Further, unlike clearance data, the number of arrests per capita of population is not linked to specific police-reported crime, and therefore, in our opinion, is a weaker empirical measure of the probability of apprehension than the clearance rate.[13,14]

Statistics Canada collects data on police-reported incidents and the number of crimes cleared through the Uniform Crime Reporting (UCR) Survey, which was established in 1962. The scope of these data is comprehensive; they include all *Criminal Code* offences and other federal statutes that have been reported to all federal, provincial and municipal police services in Canada and that have been substantiated through investigation by these services. As noted on the Statistics Canada website, "[c]overage of the UCR aggregate data reflects virtually 100% of the total caseload for all police services in Canada." It is important to note that the number of incidents is based upon severity, and therefore, the most serious offence.[15] Applying this concept to clearance rates means that, for example, the clearance of a homicide, robbery or breaking and entering receives a higher weight than the clearance of less serious offences such as minor theft, mischief and disturbing the peace.[16]

It is useful to note some differences between US and Canadian crime trends. As discussed, figure A1 demonstrates that per capita violent crime rates are much higher in Canada than in the United States. However, it is important to note that violent crime is not defined similarly across both countries. For example, sexual assault in the US requires forcible intercourse by a male against a female. In comparison, the same offence in Canada does not require sexual penetration and is not gender-specific. That is one reason why Canadian crime rates seem higher in figure A1.[17] Further, there are considerable differences in the distribution of crimes by offence. As shown in figures A3 and A4, murder rates in the US were roughly three times higher than in Canada during the 2000s and robbery rates were one and a half times higher in the US during the same period. On the other hand, although not directly comparable, forcible rape rates in the US (figure A5) were much lower than sexual assault rates in Canada. As discussed, this is because

13 However, an arrest rate defined as the number of arrests divided by number of crimes obviously does have a direct connection to crime trends.

14 A relevant concern is the calculation of clearance rates when crimes committed during a specific year are cleared during subsequent years. Our understanding from discussions with criminologists is that roughly 95% of all crimes are solved within the calendar year.

15 "Police-reported crime statistics in Canada, 2011," by Shannon Brennan, available at statcan.gc.ca/pub/85-002-x/2012001/article/11692-eng.htm.

16 See table 9-11, Police personnel in municipal police services – Yukon, 2011, in "Police resources in Canada" (Statistic Canada catalogue no. 85-225-X), available at statcan.gc.ca/pub/85-225-x/2011000/t031-eng.htm.

17 For further details please see Gannon (2001).

TABLE 3
Pearson correlation coefficients between clearance rates, per capita police officers and crime rates (all in natural logarithms)

Violent crime	1.0000				
Property crime	0.55690	1.0000			
Clearance rate (violent crime)	−0.12262	0.06996	1.0000		
Clearance rate (property crime)	−0.29707	−0.43087	0.51906	1.0000	
Per capita police officers	0.29754	0.50426	0.19985	−0.33855	1.0000

SOURCE: CANSIM

of the relatively broad definition of sexual assault in Canada. Finally, figures A6 and A7 offer scatterplots of violent and property crime rates against corresponding clearance rates. The graphs depict a visible negative relationship between crime and clearance rates, with a steeper slope for property crime.

Are correlations between clearance rates and crime rates different from correlations between clearance rates and the number of per capita police officers? Table 3 contains relevant Pearson correlation coefficients as well as the correlation coefficient between clearance rates and the number of per capita police officers, with all variables in natural logarithms. The correlation coefficients are consistent with the above graphs, revealing a −0.122 correlation coefficient between violent crime rates and corresponding clearance rates and a stronger correlation (-0.43) with respect to property crimes and clearance rates for such crimes. On the other hand, Pearson correlation coefficients between the number of per capita police officers and crime rates are positive (from 0.2 to 0.5). Further, correlation coefficients between the number of per capita police officers and clearance rates for violent and property crimes are quite different, with a value of 0.19 for violent crime clearance rates and −0.34 for property crime clearance rates. At the very least, these simple statistics suggest that the variation in clearance rates across provinces and over time is different from corresponding movements in the number of per capita police officers. Therefore, assessing whether clearance rates have different deterrence rates in comparison to the number of police officers becomes a worthwhile exercise.

4. Theoretical model

In the Polinsky and Shavell (2000) model of crime, potential criminals make their decision to commit crime based on expected benefits and costs. The expected costs are determined by policy variables p and s, where p is the probability of being apprehended and convicted and s is the sanction that comes with conviction. Individuals will then commit crime if their assessment of the benefits, b, outweighs the expected costs (i.e., if $b > ps$ for risk-neutral individuals).

If we suppose that, for any given criminal opportunity, the benefits associated with crime are determined by a draw from a distribution $f(b)$, then the probability of a crime occurring is $1 - F(ps)$, where $F(.)$ is the associated cumulative density function. The number of crimes committed in a given time period, therefore, would be a function of this probability and the number of criminal opportunities per time period, N. In addition, the distribution of benefits or the number of criminal opportunities may depend on various demographic and economic variables, such as the proportion of young males, population size, economic conditions and welfare transfers, among others. If we denote the vector describing these variables by X, then the expected number of crimes in a given period is $N(X)^*[1 - F(ps|X)]$, which we call the supply of crime, $S(ps, X)$.

We build on this model by assuming the probability of apprehension to be a function of labour, l, and capital, k, so that we have $p(l, k)$. This allows for the following comparative statics. First, the effect of an increase in labour on the expected number of crimes is given by $-N^*f(ps)^*\partial p/\partial l^*s < 0$, while the effect of an increase in the probability of apprehension is $-N^*f(ps)^*s < 0$. The model offers some insight into differences between examining the effect of an increase in labour versus the effect of an increase in the probability of apprehension. Specifically, it is worth noting that the difference between the two is just one additional step. When looking at the effects of more labour, there is an additional term in the comparative static $-\partial p/\partial l$. Intuitively, an increase in labour should result in a corresponding rise in the probability of apprehension.

A key question is: How do we measure l? Policing is a skilled profession, and police officers vary in the amount of human capital that they have. As a result, the number of police officers is a noisy measure of what l is trying to capture in the model. An alternative is to use spending on wages and salaries paid to police personnel. If the labour market for police officers operated efficiently, then there would be reason to believe that an individual's salary would reflect their human capital, and expenditures would be a good measure. However, police officers are generally unionized, and salaries generally reflect seniority rather than productivity, so expenditures are also a noisy measure of l. Thus, while there may be measurement issues associated with looking at clearance rates as a proxy for the probability of apprehension, p, in the model, such measurement issues also exist when considering the effect of an increase in labour, l (measured through police officers). In addition, there are reasons to believe that incomplete data imply that changes in l are not holding all else fixed, meaning that any empirical analysis will not capture the desired partial derivative. However, this would not be the case when looking at the probability of apprehension.

Consider the following example of how changes in l may occur with changes in other variables that are unobserved in the data. Police budgets are set on an annual basis, with expenditures made on both labour and capital. Therefore, once the budget has been set, an increase in expenditures on labour necessarily implies a decrease in expenditures on capital.[18] As a result, changes in expenditures

18 This is in fact how the budgetary process works in Ontario. Future budgets for police services are set by the municipal police services (after consultation with the chief of police services) and must then be approved by the municipality.

on police officers in the data do not necessarily capture the partial derivative expressed above.

There is supportive evidence that this is the case. Recent media coverage has documented the significant number of Toronto police officers who earn more than a $100,000 in annual income, primarily because of overtime pay.[19] Therefore, it is unsurprising that roughly 90% of the police services budget for the City of Toronto is consumed by salaries and benefits.[20] It is also important to note these trends may not be exclusive to Toronto. Figures A8 and A9 plot, respectively, the number of police officers per 100,000 of population and police expenditures (in real dollars) on salaries and wages, benefits, accommodation costs, fuel and maintenance across provinces and over time.[21] While there are differences across provinces, time-series movements are comparable. The number of police officers declined with the observed drop in crime rates through most of the 1990s, but then started to increase during the following decade. On the other hand, per capita police expenditures (in real dollars) on salaries and wages have steadily increased over time for all provinces. These trends suggest that the crowding out of capital expenditures by an increase in wages and salaries is a distinct possibility.

The model suggests other benefits from focusing on clearance rates as opposed to the number of police officers. Specifically, the data on the number of officers must be aggregated across all crimes, while clearance rates are crime-specific. Suppose that there are J different crimes, or classes of crimes, in the data. Further suppose that the probability of solving crimes of type j is a function of the resources devoted specifically to them, l_j and k_j, so that there exists a crime-specific probability of apprehension function, $p_j(l_j, k_j)$. Finally, suppose that the distribution of benefits also varies across crimes, so that there are J distributions, $f_j(b)$. The effect of an increase in expenditures devoted to solving crime j, would therefore be $-N^* f_j(p_j(l_j, k_j)s)^* \partial p_j / \partial l_j$, while the effect of an increase in the probability of apprehension for crime j is $-N^* f_j(p_j(l_j, k_j)s)$. Since data are not available for the amount of resources devoted to each type of crime, it is not possible to cleanly disaggregate crime rates into groups of crimes or individual crimes for regression analysis. However, this can be done with the probability of apprehension because we do have data for p_j. Simply put, employing clearance rates is informative because data are available for different categories of crime, which may (with appropriate caveats) capture deterrence effects associated with resources devoted to reducing different types of crimes as opposed to estimating

19 See "Sunshine List: More than a third of Toronto's police officers earned $100,000 in 2013," by Jennifer Pagliaro, published by the *Toronto Star* on Friday, March 7, 2014, and available at thestar.com/news/gta/2014/03/07/sunshine_list_more_than_a_third_of_torontos_police_officers_earned_100000_in_2013.html.

20 See "Rising police budget draws few questions from councillors," by Betsy Powell, published by the *Toronto Star* on January 30, 2014, and available at thestar.com/news/city_hall/2014/01/30/rising_police_budget_draws_few_question_from_councillors.html.

21 The data do not allow us to specifically isolate wages and expenditures.

the effects of the same number of police officers with respect to different types of crimes.

Finally, there may be an argument that police expenditures directly impact crime rates. Indeed, some papers have used reduced form regressions to estimate the impact of police expenditures on crime rates.[22] However, we think that relying on estimates of expenditures on crime is not very informative and inconsistent with the classic Becker (1968) and subsequent models of crime, which clearly specify that individually rational criminals respond to incentives generated by changes in the probability of apprehension and severity of penalties. Changes in police expenditures should have an indirect effect on crime rates, conditional on how such spending is allocated and the marginal deterrence impacts of such policies.

5. Econometric model

We test the effects of clearance rates on different types of crime through a parsimonious reduced form specification comparable to recent US studies:

$$\ln Y_{\mathrm{it}} = \beta_0 + \beta_1 \ln X_{\mathrm{it}} + \beta_2 \ln \psi_{\mathrm{it}} + \beta_3 \ln \theta_{\mathrm{it}} + \alpha_{\mathrm{i}} + \gamma_{\mathrm{t}} + \varepsilon_{\mathrm{iut}}, \tag{1}$$

where $\ln Y_{\mathrm{it}}$ is the natural logarithm of the annual crime rate per 100,000 of population of province i at time t, X_{it} is our measure of the probability of apprehension, $\psi_{\mathrm{it}}$is a vector of government policies that might plausibly impact trends in crime rates, θ_{i} is a vector of other time-varying demographic and province-specific factors, $\alpha_{\mathrm{i}}$are province fixed effects and γ_{t} is a vector of year dummy variables. The error term, ε_{it}, is assumed to be independently and identically distributed. Most regressions based on this model are run with data from all 10 provinces from 1986 to 2005.

The focus of our study is estimating β_1, which is the coefficient estimate of $\ln X_{it}$. Hence, $\beta_1$is the elasticity of crime with respect to apprehension and X_{it} is measured by the number of incidents cleared by the police divided by the number of police reported incidents. We evaluate the sensitivity of coefficient estimates by including the number of police officers per 100,000 of population. Deterrence may also be captured by incarceration rates, and we measure these through the number of prisoners per 100,000 of population. In such regressions, observations for British Columbia are dropped because data on imprisonment rates are unavailable for that province.

Province-specific policies denoted by ψ_{it} are the hourly minimum wage (in real dollars) and average annual government transfers (in real dollars) to the poorest quintile of population. The intuition is that an increase in the minimum wage or average government transfers acts as an income effect, which reduces

22 Pogue (1975) is an early example. Ajilore and Smith (2010) is a more recent study. Shoesmith and Klein (2013) offer a nice summary of these papers.

the incentive to engage in illegitimate activities. The use of Canadian data offers some pronounced cross-province and time-series variation in order to identify the impacts of social assistance transfers on crime.[23] There also exists significant time-series and cross-province variation in the Canadian minimum wage laws relative to US legislation.[24]

θ_i consists of covariates for the employment rate for prime-aged adults aged 15 and over, the province population, males aged 15 to 24 years as a proportion of the total population, the median income of social assistance recipients and the total number of new immigrants per 100,000 of population. Among these controls, the employment rate and the proportion of young adults have been identified as important determinants of crime rates. Controlling for all else, a more prosperous economy with a higher probability of employment reduces the incentive to commit crimes in order to earn income. Most crimes are committed by young males. Therefore, a province with a higher proportion of young males may experience an increase in crime levels. We think that the use of these covariates results in empirical specifications that are comparable to models used by Levitt (1997, 1998).[25]

In summary, we are identifying the effects of clearance rates, welfare transfers and the minimum wage by relying on within-province time-series variation while controlling for province-specific differences that are constant through time and year-specific shocks that are common across jurisdictions. We also evaluate the sensitivity of our results by running first differences regressions where all variables are transformed by subtracting lagged values from corresponding current values, resulting in growth rates. The estimable framework then becomes:

$$\begin{aligned}\ln Y_{it} - \ln Y_{i,t-1} = {} & \alpha_0 + \alpha_1(\ln X_{it} - \ln X_{i,t-1}) + \alpha_2(\ln \psi_{it} - \ln \psi_{i,t-1}) \\ & + \alpha_3(\ln \theta_{it} - \ln \theta_{i,t-1}) + \gamma_t + \varepsilon_{iut}.\end{aligned} \tag{2}$$

As noted by Chalfin and McCrary (2013), estimating these first-differences specifications is typical in the literature in order to remove noise and unobserved

23 Sen and Ariizumi (2013) report that some Canadian provinces implemented significant reductions to social assistance transfers during the 1990s. In its February 1994 budget, the Conservative Government of Alberta specifically outlined a 19.3% cut in social services. The Ontario Progressive Conservative government followed Alberta's lead, slashing welfare benefits by roughly 22% in 1996. Perhaps more importantly, the amendments implemented through the Ontario Works Act (enacted in 1996) not only reduced the generosity of welfare income but also increased the costs to welfare participation.

24 As documented by Sen et al. (2011), 1992 to 2005 witnessed several significant legislative changes, with 11 amendments to the minimum wage enacted by Quebec, nine by Nova Scotia, seven by British Columbia and Manitoba, six by New Brunswick, five by Saskatchewan and Alberta and four by Ontario. As discussed, over the sample period of our study, the minimum wage set by the federal government supersedes the wage set by state government for many states (in the US), resulting in mostly time-series variation.

25 At the Municipal Statistical Area (MSA) level, Levitt (1997, 1998) uses percent of black households, percent of female-headed households and percent of population aged 15 to 24. At the state level, he employs state unemployment rates and real per capita state and local spending on education and public welfare.

jurisdiction-specific characteristics, which are time-invariant. Differencing the data removes the between-jurisdiction variation but does not eliminate the potential confounding effects of unobserved national specific shocks, which are soaked up through year dummies. Our benchmark estimates of equations (1) and (2) are based on OLS regressions. Following Chalfin and McCrary (2013), we also use GMM to estimate equation (2). The benefit of relying on GMM is that we are able to assess the sensitivity of our findings because GMM does not restrict the empirical model to a single specific parametric specification. The kernel and the bandwidth are chosen using the methods proposed by Newey and West (1987). Finally, given the long time period of the data, we also GLS in order to account for serial correlation. The GLS estimates are based on the cross-sectionally heteroskedastic and time-wise autoregressive model for pooled cross-sections of time series initially developed by Parks (1967). None of these methods accounts for endogeneity bias or measurement error. Our IV strategy to tackle the corresponding bias in OLS estimates is discussed in the next section.

6. Results

6.1. Baseline estimates

Table 4 contains empirical estimates of the effects of violent crime (columns (1) to (3)) and property crime (columns (4) to (6)) clearance rates on corresponding crime rates, controlling for other factors. Columns (1) and (3) contain results conditioned on the use of covariates and province and year fixed effects; columns (2) and (5) evaluate the effects of adding the number of per capita police officers; and columns (3) and (6) include the number of per capita police officers and province-specific trends. Panel A reports estimates based on 10 provinces from 1986 to 2005, panel B consists of results from the same provinces but from 1988 to 2005 in order to accommodate one- and two-year lagged clearance rates and panel C contains estimates from nine provinces between 1986 and 2005, which are conditioned on the use of per capita incarceration rates. Given the long time-series of the data, we focus on issues that are a consequence of autocorrelation and heteroskedasticity. Therefore, standard errors of coefficient estimates are White and Newey–West corrected for second-order autocorrelation and unknown heteroskedasticity.[26] For the sake of brevity, we report only coefficient estimates of clearance rates, the number of per capita police officers (when employed) and incarceration rates.

First, coefficient estimates of clearance rates are negative and statistically significant at the 1% or 5% levels across almost all columns. With respect to violent

26 Another option would be to cluster the standard errors by province. However, the number of provinces (10)—and therefore clusters—would be quite small. Conversations with Jeff Wooldridge suggest that, in such cases, using a Newey–West correction for autocorrelation may be a better strategy (econometrics course offered by the Canadian Economics Association Meetings, Ryerson University, May 26 to May 28, 2009).

TABLE 4
OLS estimates with respect to violent and property crime rates: Province–year data

	Violent crime			Property crime		
	Base (1)	Police (2)	Police and province-specific trends (3)	Base (4)	Police (5)	Police and province-specific trends (6)
A. 10 provinces, 1986–2005						
Clearance rate (per incident)	−0.245 (0.116)^{b}	−0.075 (0.072)	−0.334 (0.101)^{a}	−0.592 (0.086)^{a}	−0.563 (0.073)^{a}	−0.516 (0.048)^{a}
Per capita police officers per 100,000 of population		1.505 (0.199)^{a}	0.843 (0.544)		0.542 (0.214)^{b}	−0.345 (0.196)^{c}
Adjusted R-squared	0.9298	0.9497	0.9695	0.9639	0.9657	0.9807
B. 10 provinces, 1988–2005						
Clearance rate (per incident)	−0.300 (0.119)^{b}	−0.134 (0.109)	−0.306 (0.08)^{a}	−0.498 (0.06)^{a}	−0.486 (0.056)^{a}	−0.431 (0.043)^{a}
One-year lagged clearance rate (per incident)	0.0157 (0.116)	0.0484 (0.112)	−0.0297 (0.075)	−0.074 (0.052)	−0.054 (0.050)	−0.097 (0.044)^{b}
Two-year lagged clearance rate (per incident)	−0.067 (0.136)	0.0184 (0.126)	−0.209 (0.094)^{b}	−0.0002 (0.073)	0.003 (0.067)	−0.065 (0.049)^{c}
Per capita police officers per 100,000 of population		1.437 (0.247)^{a}	0.870 (0.209)^{a}		0.432 (0.223)^{b}	−0.304 (0.182)
Adjusted R-squared	0.9275	0.9446	0.9723	0.9686	0.9696	0.9847
C. 9 provinces, 1986–2005						
Clearance rate (per incident)	−0.371 (0.119)^{a}	−0.232 (0.095)^{b}	−0.362 (0.097)^{a}	−0.543 (0.075)^{a}	−0.536 (0.067)^{a}	−0.510 (0.053)^{a}
Per capita police officers per 100,000 of population		1.552 (0.183)^{a}	0.923 (0.279)^{a}		0.469 (0.219)^{b}	−0.189 (0.206)
Incarceration rates per 100,000 of population	−0.125 (0.069)^{c}	−0.002 (0.057)	−0.0575 (0.077)	−0.148 (0.063)^{b}	−0.109 (0.066)	−0.083 (0.056)
Adjusted R-squared	0.9302	0.9516	0.9660	0.9583	0.959	0.9598
Province fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Province linear trends	No	No	Yes	No	No	Yes

NOTES: Estimates in columns (1) to (3) are with respect to violent crime, and results in columns (4) to (6) are with respect to property crime. Standard errors are White and Newey–West corrected for second-order autocorrelation. Other covariates that are not reported but are included in all regressions are the minimum wage, average government transfers, employment rates, population, the proportion of young males aged 15 to 24, the number of new immigrants per 100,000 of population and average income of welfare recipients. With the exception of fixed effects, all variables are in natural logarithms. Superscripts a, b and c refer to statistical significance at the 1%, 5% and 10% levels, respectively.

crime, estimates of clearance rates are significant in column (1) across all panels. The addition of the number of police officers in column (2) removes the statistical significance of clearance rates in panels A and B. However, the inclusion of province-specific trends in column (3) results in statistically significant (at the 1% level) estimates of clearance rates across all panels with an elasticity of roughly −0.3. Coefficient estimates of per capita police remain positive and are significant in all panels. Lagged values of clearance rates are, in most cases, statistically insignificant.[27] Finally, while the coefficient estimate of incarceration rates is negative and statistically significant (at the 10% level) in column (1), it becomes insignificant with the inclusion of police officers and trends in column (3).

Estimates in columns (4) to (6) imply that a 10% increase in the clearance rate for property crime is robustly correlated (at the 1% levels) with a roughly 4% to 5% reduction in property crime rates. Coefficient estimates of per capita police rates are positive in column (5) but become negative with an implied elasticity of −0.3 to −0.345 in column (6) (panels A and B) with the employment of province linear trends. However, only the estimate in panel A is statistically significant. One- and two-year clearance rates (in panel B) are negative and statistically significant (at the 5% or 10% levels) but of much lower magnitude than current clearance rates. Finally, the estimate of incarceration rates (in panel C) is statistically insignificant.

In summary, the important result is the relative stability of coefficient estimates of clearance rates. Clearance rates are negatively correlated with violent and property crime rates across all columns and also statistically significant (in most cases). When included in tandem with clearance rates, per capita police rates either do not have the correct sign or are statistically insignificant. However, coefficient estimates of clearance rates remain statistically significant and possess negative signs. These findings suggest that previous studies, which focus exclusively on the number of per capita police officers to capture the impacts of the probability of apprehension, may understate the overall deterrence effects of enforcement by police.

Table 5 explores the deterrent effects of different measures of apprehension in some more detail. Specifically, column (1) contains the results of focusing on the effects of the per capita number of police officers on property crime in isolation from other deterrence measures. Columns (2) and (3) contain results on the impacts of arrest rates per 100,000 of population and police expenditures per capita of population, respectively.[28] Column (4) assesses the effects of including

27 The inclusion of one- and two-year lagged values of clearance rates is intended to evaluate the possibility that information on actual clearance rates, and therefore the likelihood of apprehension, may take some time to reach criminals before they take them into account in their cost-benefit decisions.

28 Data on provincial expenditures on police services are available from the Police Administration Survey conducted by Statistics Canada. For further details of this survey, see www23.statcan.gc.ca/imdb/p2SV.pl?Function=getSurvey&SDDS=3301. The Police Administration Survey collects data on police personnel and expenditures from each municipal, provincial and federal (RCMP) police service in Canada. As detailed by Hutchins (2014), police expenditures are actual operating expenditures on salaries and wages, benefits, accommodation costs, fuel, maintenance and so forth. Unfortunately, capital expenditures are not included.

TABLE 5
OLS estimates of different measures of apprehension: province–year data

	Property crime				Violent crime			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Clearance rate (per incident)				-0.493 (0.062)^{a}				-0.441 (0.080)^{a}
Per capita police officers per 100,000 of population	-0.390 (0.239)^{c}			-0.255 (0.174)	0.925 (0.267)^{b}			0.449 (0.131)^{a}
Arrest rate per 100,000 of population		0.409 (0.07)^{a}		0.379 (0.057)^{a}		0.474 (0.038)^{a}		0.486 (0.029)^{a}
Police expenditures per capita of population			-0.17 (0.164)	-0.0701 (0.11)			0.368 (0.143)^{a}	0.0278 (0.098)
Other exogenous covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province/year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province linear trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.9703	0.9766	0.9701	0.9863	0.9667	0.9821	0.9648	0.9884
Province linear trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

NOTES: Estimates in columns (1) to (4) are with respect to property crime, and results in columns (5) to (8) are with respect to violent crime. Standard errors are White and Newey–West corrected for second-order autocorrelation. Other covariates that are not reported but are included in all regressions are the minimum wage, average government transfers, employment rates, population, the proportion of young males aged 15 to 24, the number of new immigrants per 100,000 of population and average income of welfare recipients. With the exception of fixed effects, all variables are in natural logarithms. Superscripts a, b and c refer to statistical significance at the 1%, 5% and 10% levels, respectively.

all these measures. Columns (5) to (8) are similarly organized and contain estimates of apprehension measures with respect to violent crime.

Broadly speaking, the results in table 5 are similar to corresponding estimates in table 4. The coefficient estimate of the number of police officers with respect to property crime is negative and statistically significant (at the 10% level) in column (1) but becomes insignificant in column (4) with the inclusion of other measures of apprehension. The arrest rate covariate is statistically significant (at the 1% level) in columns (2) and (4) but positive. Per capita police expenditures are negatively associated with property crime but statistically insignificant. On the other hand, the coefficient estimate of clearance rates (in column (4)) possesses a negative sign and is statistically significant at the 1% level. The coefficient estimate of −0.49 is comparable to results in table 4. Higher clearance rates are also correlated with a reduction in violent crime rates (at the 1% level). Coefficient estimates of arrest rates and the number of police officers are statistically significant but possess counterintuitive signs. The per capita police expenditure covariate is significant in column (7), but becomes statistically insignificant with the inclusion of other measures of apprehension.

A relevant question is whether these estimates are comparable with the US-based estimates. Our results with respect to the effect of arrest rates and clearance rates on violent crime cannot be compared because many US studies obtain coefficient estimates with negative signs while our regressions reveal arrest rates with positive coefficients. However, our coefficient estimates of the number of police officers with respect to property crime are consistent with the −0.5 elasticity obtained by Levitt (2002) and a bit larger than the preferred estimate of −0.17 reported by Chalfin and McCrary (2013). Further, our estimates of the effects of an increase in clearance rates are comparable to those obtained by Craig (1987) with respect to all crimes. As discussed, there is very little empirical research on the effects of clearance rates.

In summary, coefficient estimates of clearance rates remain robust and statistically significant even after the inclusion of other plausible measures of apprehension. Given the presence of either weak statistical significance or counterintuitive signs, we do not use per capita arrest rates, the number of police officers or per capita police spending as covariates in further regressions. This strategy allows us to focus on obtaining robust estimates of the impacts of clearance rates. However, we acknowledge that omitting these plausible measures of apprehension might induce some bias in coefficient estimates of clearance rates, the magnitude of which is a function of the correlation between these measures and clearance rates. Therefore, coefficient estimates of clearance rates should not be interpreted as causal relationships and should be treated with appropriate caveats. Further, we do not employ province-specific linear trends in the remaining regressions because coefficient estimates of clearance rates remain relatively stable after their inclusion (with other covariates and province and year fixed effects). This allows the other covariates to be identified by time-series variation within provinces.

Table 6 evaluates the sensitivity of coefficient estimates of clearance rates through alternative estimation strategies. Columns (1) to (3) contain estimates with respect to violent crime. Column (1) contains OLS first differences estimates based on equation (2); column (2) reports the results of GMM estimation based on equation (2); and column (3) contains results from a GLS regression based on equation (1). Columns (4), (5) and (6) are organized similarly, but with respect to property crime rates. Broadly speaking, the estimates are quite comparable to results in the previous table. The coefficient estimate of the clearance rate with respect to violent crime from the first differences model is −0.126 and statistically significant at the 10% level. Corresponding GMM and GLS estimates are −0.241 and −0.244, respectively, and statistically significant at the 5% and 1% levels, respectively. Coefficient estimates of the effects of clearance on property crime rates are also significant (at the 1% level) and range from −0.3 to −0.6. In terms of other covariates, levels and GLS estimates of the minimum wage with respect to violent crime are negative and statistically significant at the 1% and 5% levels. While GMM and GLS estimates of the minimum wage with respect to property rates are significant, they possess counterintuitive positive signs. First differences and GMM estimates of employment rates are negative and significant with respect to property crime but possess implausibly large coefficient estimates.

6.2. *Endogeneity bias and instrumental variables*

Reduced form estimates are premised on the assumption that changes in clearance rates exogenously impact crime trends. However, as discussed above, OLS estimates are likely to be biased because of reverse causality. It might very well be the case that increases in crime result in public pressure for rapid arrests of perpetrators. In that case, OLS estimates of clearance rates will be biased and confounded. There are other possible sources of bias in estimates of the true impacts of clearance rates. First, the number of police-reported incidents enters the left- and right-hand side of the econometric model. This is similar to the classic measurement error noted by Borjas (1980) with respect to estimating the effects of average weekly (or annual) wages on weekly (or annual) hours of work. The problem is that hours of work enter both the right- and left-hand side of the equation, resulting in a downward bias in coefficient estimates of the effects of changes to average wages. Second, another type of measurement error arises from the fact that some crimes are not reported to the police, which means that clearance rates overstate the true probability of apprehension. Therefore, estimates of the effects of the clearance rate on crime will be upward biased with regards to the true crime rate, but accurate for the recorded crime rate. Third, as explained by Cook (1979), coefficient estimates of clearance rates may also be biased if rational criminals respond to an increase in clearance rates by committing crimes that are more difficult to solve.

In order to evaluate the magnitude of bias in OLS estimates, we construct political party dummy variables as well as instruments based on the proportion of seats held by different parties, which can arguably identify trends in clearance

TABLE 6
First differences, GMM and GLS estimates with respect to violent and property crime rates: Province–year data 1986–2005

	Violent crime (1) First differences	(2) GMM	(3) GLS	Property crime (4) First differences	(5) GMM	(6) GLS
Clearance rate	−0.126 (0.074)[c]	−0.241 (0.115)[b]	−0.244 (0.084)[a]	−0.339 (0.049)[a]	−0.611 (0.08)[a]	−0.583 (0.058)[a]
Minimum wage	−0.086 (0.117)	−0.269 (0.169)	−0.276 (0.117)[b]	−0.076 (0.101)	0.39 (0.13)[a]	0.325 (0.103)[a]
Average government transfers	0.092 (0.058)[c]	−0.130 (0.116)	−0.138 (0.108)	−0.013 (0.059)	0.026 (0.077)	−0.029 (0.091)
Employment rate	1.100 (0.447)[b]	0.227 (1.024)	0.53 (0.55)	−1.01 (0.54)[a]	−2.176 (0.959)[a]	−0.533 (0.469)
Province fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared/Log likelihood	0.4402		247.325	0.5775		280.825

NOTES: Results are based on data for 10 provinces from 1986–2005 (200 obs.). Estimates in columns (1) to (3) are with respect to violent crime, and results in columns (4) to (6) are with respect to property crime. Standard errors of coefficient estimates of first difference regressions are White and Newey–West corrected for second-order autocorrelation. For GMM regressions, the kernel and the bandwidth are chosen using the methods proposed by Newey and West (1987). The GLS estimates are based on the cross-sectionally heteroskedastic and time-wise autoregressive model for pooled cross-sections of time series initially developed by Parks (1967). Other covariates that are not reported but are included in all regressions are population, the proportion of young males aged 15 to 24, the number of new immigrants per 100,000 of population and average income of welfare recipients. With the exception of fixed effects, all variables are in natural logarithms. Superscripts a, b and c refer to statistical significance at the 1%, 5% and 10% levels, respectively.

rates and not be correlated with the right-hand side error term of equation (1). The intuition is that changes in the governing party at the province level might impact the allocation of resources to law enforcement agencies. This approach is consistent with Besley and Case (2000), who suggest that variation in political variables can exogenously identify trends in policy variables and not have any impact on the outcome of interest. The use of political variables as instruments is also comparable to the strategy used by Levitt (1997), who relied on variation in mayoral and gubernatorial electoral cycles to instrument police rates.

The presence of similar parties allows us to create the same political party fixed effects across provinces. The major political parties in most Canadian provinces are similar: the Liberal Party, the Conservative Party and the New Democratic Party. In addition, British Columbia has the Social Credit Party, and the Parti Quebecois is a major political force in Quebec.[29] Therefore, we construct three dummy variables that take a value of 1 if one of the major political parties (Liberals, Conservatives or NDP) is in power. Hence, the omitted category is the presence of a ruling political party that does not have a national presence. As mentioned, we also construct instruments based on the proportion of seats held by each party.[30] These variables are intended to reflect the ease with which governing parties can implement policy reforms and corresponding changes in government spending. A greater proportion of legislature seats might imply that a political party with a "tough on crime" agenda will have greater flexibility in increasing government spending on specific anti-crime policies at the expense of reduced expenditures on other items. We also use three- and four-year lagged clearance rates that may impact trends in clearance rates but do not share a statistically significant relationship with current crime rates. We interact these lagged clearance rates with each of the political party dummy variables as a crude proxy for variation in clearance rates generated by changes to political regimes that might result in shifts in provincial funding. Finally, consistent with our theoretical model, we employ police expenditures as another instrument that should impact trends in clearance rates.[31] Given the ambiguities of empirically defining per capita police expenditures, we define per capita expenditures in terms of population and the number of police officers.[32]

Table 7 contains first- and second-stage estimates from a variety of instrumental variables regressions. Columns (1) to (5) document estimates with respect to violent crime while columns (6) to (10) report corresponding results for property

29 In terms of ideology, the Conservatives are considered by most to be on the right end of the political spectrum, the Liberals are positioned at the centre left, and the NDP is on the far left.
30 Information on the governing political party and the number of seats held by each party was obtained from the websites of the legislative assemblies of each province, the Social Credit Party and the Parti Quebecois.
31 We are grateful to an anonymous referee for recommending this.
32 Employing police expenditures as an alternate instrument is also a useful sensitivity check, given the possibility that changes in the governing political party or the seats held by it could indirectly affect crime rates because a different party might result in significant shifts in areas that affect crime, such as employment, public housing and education policies. We thank an anonymous referee for pointing this out.

TABLE 7
Instrumental variables estimates with respect to violent and property crime rates: Province–year data

	Violent crime (1)	Violent crime and first differences (2)	Violent crime – police exp. per capita of population (3)	Violent crime – police exp. per capita of population (4)	Violent crime – police exp. per capita of police (5)	Property crime (6)	Property crime and first differences (7)	Property crime - police exp. per capita of pop. (8)	Property crime – police exp. per capita of pop. (9)	Property crime – police exp. per capita of police (10)
A. First stage										
Per capita police expenditures			−0.421 (0.142)[a]	−0.382 (0.147)[a]	−0.405 (0.173)[a]			−0.561 (0.15)[a]	−0.568 (0.15)[a]	−0.658 (0.15)[a]
Instruments	Political party dummies	Political party dummies and party dummies interacted with three- and four-year lagged clearance rates	Political party dummies and per capita police exp. (police exp. = per capita of pop.)	Political parties, share of seats by political parties and per capita police exp. (police exp. = per capita of pop.)	Political party dummies and per capita police exp. (police exp. = per 100,000 of police officers)	Political party dummies	Political parties and political party dummies interacted with three- and four- year lagged clearance rates	Political party dummies and per capita police exp. (police exp. = per capita of pop.)	Political parties, share of seats by political parties and per capita police exp. (police exp. = per capita of pop.)	Political party dummies and per capita police exp. (police exp. = per 100,000 of police officers)
F stat, *P* value	5.215, 0.00184	3.036, 0. 0027	7.272, 0.00002	5.355, 0.00002	4.508, 0.00013	0.6208, 0.603	4.929, 0. 00001	5.386, 0.0004	3.648, 0.001	4.047, 0.0004
Adjusted R-squared	0.5694	0.6094	0.614	0.6205	0.6099	0.6137	0.8966	0.898	0.898	0.8959
B. Second stage										
Clearance rate	−0.519 (0.245)[b]	−0.54 (0.32)[c]	−0.689 (0.202)[a]	−0.726 (0.194)[a]	−0.496 (0.1909)[a]	−0.268 (0.667)	−0.599 (0.202)[a]	−0.831 (0.2)[a]	−0.725 (0.18)[a]	−0.544 (0.188)[a]
Adjusted R-squared	0.9262	0.3036	0.9203	0.9186	0.9268	0.9585	0.4977	0.961	0.963	0.9638

NOTES: Results are based on data for ten provinces from 1986–2005 (200 obs.) for all columns, excepting (2) and (7), which are based on the same provinces but from 1990–2005 (160 obs.). Other covariates that are not reported but are included in all regressions are the minimum wage, average government transfers, employment rates, population, the proportion of young males aged 15 to 24, the number of new immigrants per 100,000 of population, average income of welfare recipients and province and year fixed effects. With the exception of fixed effects, all variables are in natural logarithms. Superscripts a, b and c refer to statistical significance at the 1%, 5% and 10% levels, respectively.

crime. Panel A (B) contains first-stage (second-stage) results. Columns (1) and (6) in table 6 contain estimates of the effects of clearance rates using political party dummies as instruments. Results in columns (2) and (7) are from an estimable model in first differences with second stage IV estimates based on political party dummies and political party dummies interacted with three- and four-year lagged clearance rates. Columns (3) and (8) report results from a regular log-log model with second-stage results identified by political party dummies and police expenditures per capita of population as instruments. Columns (4) and (9) contain results obtained from adding the proportion of seats held by political parties as instruments (in addition to political party dummies and police expenditures per capita of population). Finally, estimates from employing political party dummies and police expenditures per 100,000 of police officers as instruments are detailed in columns (5) and (10).

The availability of multiple instruments allows us to conduct tests of over-identifying restrictions and evaluate the sensitivity of findings to the use of different instrumental variables. Results from standard Sargan and Hansen tests of over-identifying restrictions yield test statistics that do not reject the null hypothesis of over-identification.[33] With the exception of property crime clearance rates instrumented by political party dummies (column (6)), we can reject the null hypothesis (at the 1% level) that the coefficient estimates of instruments are equal to zero. However, in all cases, the F statistic from the joint test of statistical significance of all instruments is less than 10 in value.

Instrumental variables estimates are quite similar to the above reduced form estimates. Second-stage coefficient estimates of clearance rates on violent crime (in panel B) range from -0.5 to -0.7 and are statistically significant at the 1% or 10% levels. With the exception of column (6), second-stage coefficient estimates of clearance rates with respect to property crime are roughly from -0.3 to -0.7 and are significant at the 1% level. Another key point is the statistical significance and the negative sign of coefficient estimates of per capita police expenditures across all columns and irrespective of whether police expenditures are measured per capita of population or per 100,000 of police officers. Coefficient estimates of per capita police expenditures range from roughly -0.4 to -0.6 and imply that an increase in such expenditures is associated with lower clearance rates for violent and property crimes.

While a negative relationship may seem odd, we suspect that it is driven by either an inability to measure capital expenditures or by the fact that expenditures may be a poor measure of quality units of labour, as discussed earlier.[34]

33 These are available on request.

34 In particular, the latter may cause this negative relationship because our sample period covers the tail end of a 30-year increase in crime levels and most of the current 20-year decline in crime. We thus have, in our sample, police forces that have gone through extensive hiring booms and generally cannot let officers go due to strong unions. The result is aging police forces that get more expensive without necessarily getting more productive.

For example, if a higher proportion of a police force's budget is being devoted to salaries and wages, this implies less spending on capital expenditures. At some point, it is possible that diminishing returns set in, and any incremental increase in the marginal deterrence gained from more spending on wages and salaries is outweighed by the decline in deterrence associated with the corresponding decrease in relative spending on capital expenditures. From an empirical perspective, the result is negative signs of coefficient estimates of the effects of spending on wages and salaries and variable cost items (such as fuel expenses).

There may be concerns that the use of police expenditures as an instrument is invalid because of potential simultaneity between crime or clearance rates and police expenditures. An increase (decrease) in crime (clearance) rates may result in public demand for higher government expenditures on police services. To assess this possibility, we ran three-stage least squares regressions assuming the following: in the third stage, crime rates are functions of clearance rates; in the second stage, clearance rates are identified by police expenditures per capita; and in the first stage, police expenditures are identified by the political party dummies and the share of seats held by political parties. Relative to our earlier instrumental variables approach, we now evaluate whether changes in government and/or in the distribution of seats among political parties impact spending on police services.

Our results remain largely unchanged.[35] In the first stage, the coefficient estimates of the political variables with respect to per capita police expenditures are statistically significant, with an F statistic and P value (from a joint test of significance) equal to 2.72 and 0.015, respectively. The impact of police expenditures on clearance rates has already been discussed. The third-stage coefficient estimates of clearance rates with respect to violent and property crime are -0.633 and -0.308, respectively, and statistically significant at the 1% and 10% levels, respectively.[36]

In summary, we do not claim that the IV estimates are successfully purged of reverse causality or measurement error. However, as discussed above, this is an extremely difficult accomplishment, and very few papers in the literature have actually attempted some type of correction. The relative similarity between OLS and IV estimates suggests that potential bias in OLS estimates of clearance rates may not be significant. However, it is also important to acknowledge that IV estimates are biased towards OLS estimates when multiple instruments are weak.[37] As a consequence, it is prudent to treat our findings with appropriate caution.

35 Each of the three regressions have the minimum wage, average government transfers, employment rates, population, the proportion of young males aged 15 to 24, the number of new immigrants per 100,000 of population, average income of welfare recipients and province and year fixed effects as right-hand side variables.

36 All these estimates are available on request.

37 We are very grateful to an anonymous referee for pointing this out. An excellent exposition of this point is available from "Weak Instruments – EC533: Labour Economics for Research Students" by Jörn-Steffen Pischke, available at econ.lse.ac.uk/staff/spischke/ec533/Weak_IV.pdf.

TABLE 8
OLS estimates with respect to violent and property crime rates: Province–year data

	Violent crime diff.-in-diff. (1)	Property crime diff.-in-diff. (2)	Violent crime 1990–2001 (3)	Property crime 1990–2001 (4)
Clearance rate (per incident)	−0.215 (0.133)	−0.557 (0.097)[a]	−0.398 (0.148)[a]	−0.446 (0.06)[a]
Clearance rate (per incident) * year dummy 1990–2001	−0.112 (0.181)	−0.066 (0.051)		
Province fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Adjusted R-squared	0.9296	0.9641	0.9383	0.9756

NOTES: Results in columns (1) and (2) are based on data for 10 provinces from 1986–2005 (200 obs.). Estimates in columns (3) and (4) are based on data for the same provinces from 1990–2001 (110 obs.). Standard errors are White and Newey–West corrected for second-order autocorrelation. Other covariates that are not reported but are included in all regressions are the minimum wage, average government transfers, employment rates, population, the proportion of young males aged 15 to 24, the number of new immigrants per 100,000 of population and average income of welfare recipients. With the exception of fixed effects, all variables are in natural logarithms. Superscripts a, b and c refer to statistical significance at the 1%, 5% and 10% levels, respectively.

Finally, table 8 offers some further sensitivity analyses through models designed to evaluate whether changes in clearance rates during the 1990s possessed different marginal impacts relative to other years in our sample. The motivation of this exercise is to investigate whether clearance rates might be one of the contributing factors behind the significant decline in crime rates observed during the 1990s.[38] Columns (1) (violent crime) and (2) (property crime) contain difference-in-differences regressions based on the entire sample (1986 to 2005), with an additional clearance rate covariate interacted with a dummy that takes a value of 1 for all observations from 1991 to 2000 and is 0 otherwise. The coefficient estimate of this dummy variable reflects the marginal effect of clearance rates during this time period relative to other years. Columns (3) (violent crime) and (4) (property crime) are based on an alternative approach in which regressions are based on a reduced sample (1991 to 2000) during which significant reductions in crime were observed.

Consistent results emerge across columns. The coefficient estimate of the clearance rate interacted with the year dummy with respect to violent crime (column (1)) is statistically insignificant, while the coefficient estimate of the violent crime clearance rate in column (3) is statistically significant but not that different in magnitude from estimates in table 4. The interacted term with respect to property crime (in column (2)) is negative but statistically insignificant. The coefficient estimate of the property crime clearance rate in column (4) is negative and statistically significant and quite comparable to previous results. In summary, while these results offer further evidence on the importance of clearance rates, we cannot conclude with certainty that they were a significant determinant of the observed decline in crime during the 1990s.

38 Levitt (2004) cites increased police hiring as one of the factors but does not mention clearance rates.

7. Conclusion

Relative to the vast literature on crime and the effects of more police officers, the number of studies that have focused on the effects of clearance rates on crime is quite limited. This is unfortunate, as we think that the clearance rate is a reasonable approximation of Becker's probability of apprehension. Indeed, early empirical studies on crime and deterrence focused on the effects of clearance rates rather than changes to the size of a jurisdiction's police force. We evaluate the importance of clearance rates with respect to crime by using data across Canadian provinces from 1986 to 2005. The use of Canadian data is informative from a general perspective, given the correlation between US and Canadian crime rates over time. Exploiting Canadian data is also useful given that penalties for *Criminal Code* offences are set at the federal level, yielding some reassurance that estimates of the impacts of clearance rates are not biased by variation in local penalties, which reflect changes to the severity of penalty rather than the probability of apprehension. Finally, we note that Canada did not experience the adverse consequences associated with the crack cocaine epidemic that occurred over the sample period.

In terms of other contributions, we develop a simple model that links labour and capital to the probability of apprehension and the incentive to commit crime. The model allows us to construct instruments, such as per capita police expenditures that proxy effective labour units of policing and enable us to assess the sensitivity of OLS estimates. OLS, GMM, GLS and IV estimates yield very comparable results. All else being equal, an increase in the clearance rate is correlated with a reduction in crime; marginal effects are higher with respect to property crime rates. These results are robust to the use of police force size and a wide array of other covariates, fixed effects and provincespecific linear trends. However, the similarity between the IV and OLS estimates might be an artefact of the relative statistical weakness of multiple instruments. Hence, our estimates should be treated with suitable caveats.

Further, our IV estimation has been conducted only with respect to clearance rates and ignores potential endogeneity bias in coefficient estimates for other covariates, such as the number of police officers (one measure of labour). Therefore, we cannot say that higher arrest rates or hiring more police officers does not result in lower crime rates. It is possible that these other measures of apprehension are significantly associated with lower crime rates and that we have been unsuccessful in purging coefficient estimates of endogeneity bias. Given these caveats, we interpret the consistent statistical significance of clearance rates as cautious evidence of the importance of the probability of apprehension, but with the possibility of bias, taking into account the results with respect to other measures of apprehension. In future research we hope to better understand the reasons for the positive correlation between crime and per capita police rates, possibly through the use of more structural methods of estimation.

Appendix

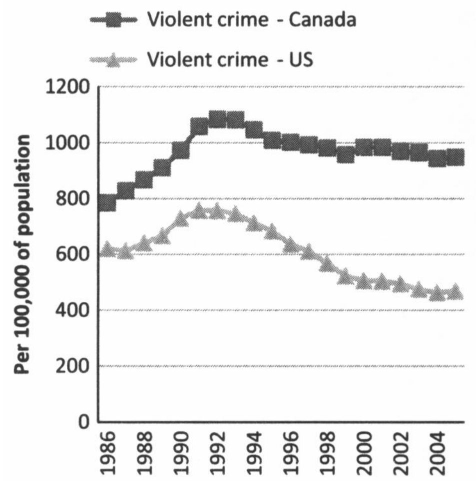

FIGURE A1 A comparison of violent crime in Canada and the US
SOURCES: Canadian data are from CANSIM tables 252-0013 and 252-0051. US data are from the Federal Bureau of Investigation website (www2.fbi.gov/ucr/05cius/data/table_01.html).

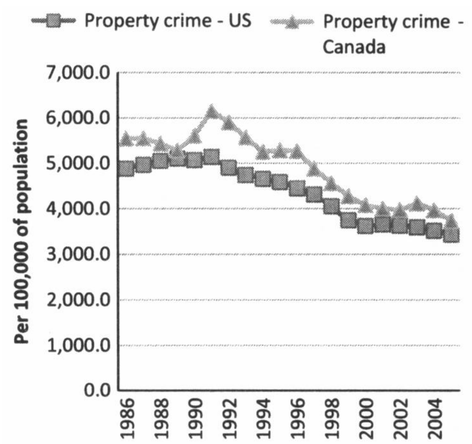

FIGURE A2 A comparison of property crime in Canada and the US
SOURCES: Canadian data are from CANSIM tables 252-0013 and 252-0051. US data are from the Federal Bureau of Investigation website (www2.fbi.gov/ucr/05cius/data/table_01.html).

FIGURE A3 A comparison of murder rates in Canada and the US
SOURCES: Canadian data are from CANSIM tables 252-0013 and 252-0051. US data are from the Federal Bureau of Investigation website (www2.fbi.gov/ucr/05cius/data/table_01.html).

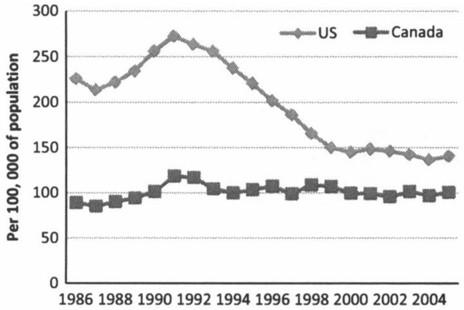

FIGURE A4 A comparison of robbery rates in Canada and the US
SOURCES: Canadian data are from CANSIM tables 252-0013 and 252-0051. US data are from the Federal Bureau of Investigation website (www2.fbi.gov/ucr/05cius/data/table_01.html).

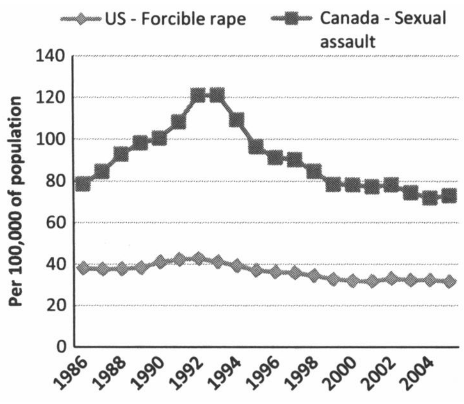

FIGURE A5 A comparison of sexual assault rates in Canada and the US
SOURCES: Canadian data are from CANSIM tables 252-0013 and 252-0051. US data are from the Federal Bureau of Investigation website (www2.fbi.gov/ucr/05cius/data/table_01.html).

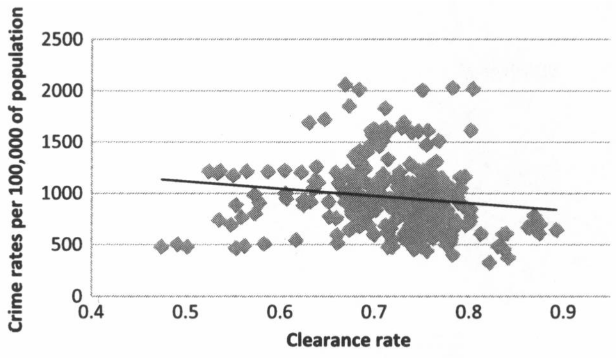

FIGURE A6 A scatterplot of violent crime rates and clearance rates for Canada
SOURCE: CANSIM table 252-0013

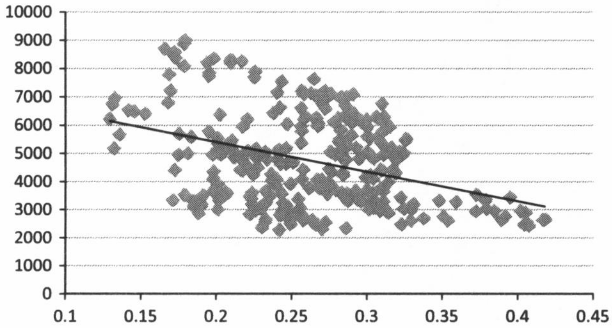

FIGURE A7 A scatterplot of property crime rates and clearance rates for Canada
SOURCE: CANSIM table 252-0051

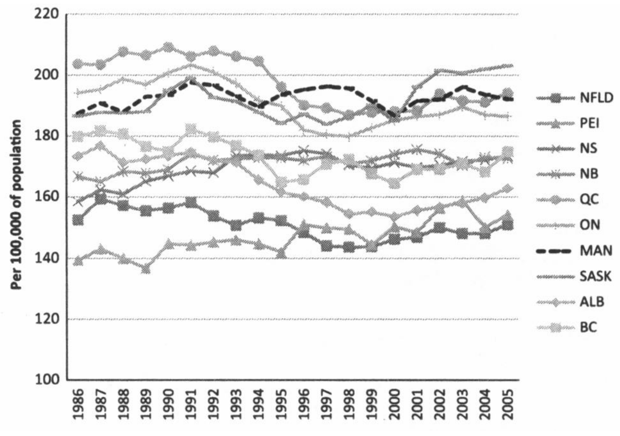

FIGURE A8 Numbers of police officers by province, by year
SOURCE: CANSIM table 254-0002

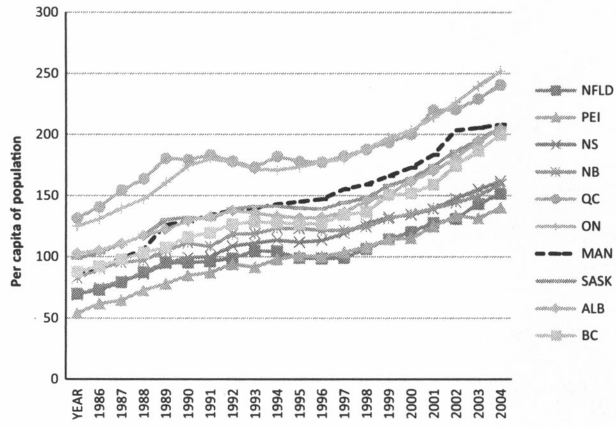

FIGURE A9 Per capita police expenditures by province, by year in real $
SOURCE: CANSIM table 254-0002

References

Ajilore, O., and J. Smith (2010) "Ethnic fragmentation and police spending," *Applied Economic Letters* 18(4), 329–32

Avio, K. L. (1973) "An economic analysis of criminal corrections: The Canadian case," *Canadian Journal of Economics* 6(2), 164–78

——— (1979) "Capital punishment in Canada: A time-series analysis of the deterrent hypothesis," *Canadian Journal of Economics* 12(4), 647–76

Avio, K. L., and C. S. Clark (1976) *Property Crime in Canada: An Econometric Study*, Toronto: University of Toronto Press

——— (1978) "The supply of property offences in Ontario: Evidence on the deterrent effect of punishment," *Canadian Journal of Economics* 11(1), 1–19

Becker, G. S. (1968) "Crime and punishment: An economic approach," *Journal of Political Economy* 76(2), 169–217

Besley, T., and A. Case (2000) "Unnatural experiments: Estimating the incidence of endogenous policies," *The Economic Journal* 110(467), 672–94

Black, D. A., and D. S. Nagin (1998) "Do right-to-carry laws deter violent crime?," *Journal of Legal Studies* 27(1), 209–19

Borjas, G. J. (1980) "The relationship between wages and weekly hours of work: The role of division bias," *The Journal of Human Resources* 7(3), 409–23

Carr-Hill, R. A., and N. H. Stern (1973) "An econometric model of the supply and control of recorded offences in England and Wales," *Journal of Public Economics* 2(4), 289–318

Carrington, P. J., and J. L. Schulenberg (2008) "Structuring police discretion: The effect on referrals to youth court," *Criminal Justice Policy Review* 19(3), 349–67

Chalfin, A., and J. McCrary (2013) "The Effect of Police On Crime: New Evidence from U. S. Cities, 1960–2010," NBER working paper no. 18815

Cheung, Y. W., and P. G. Erickson (1997) "Crack use in Canada: A distant American cousin." In *Crack in America: Demon Drugs and Social Justice*, pp. 175–193, eds. H. G. Levine and C. Reinarman. Berkeley: University of California Press

Cook, P. J. (1979) "The clearance rate as a measure of criminal justice system effectiveness," *Journal of Public Economics* 11(1), 135–42

Corman, H., and N. Mocan (2005) "Carrots, sticks, and broken windows," *The Journal of Law & Economics* 48(1), 235–66

Cornwell, C. N., and W. N. Trumbull (1994) "Estimating the economic model of crime with panel data," *The Review of Economics and Statistics* 76(2), 360–66
Craig, S. G. (1987) "The deterrent impact of police: An examination of a locally provided public service," *Journal of Urban Economics* 21, 298–311
Dezhbakhsh, H., and P. H. Rubin (1998) "Lives saved or lives lost? The effects of concealed-handgun laws on crime," *The American Economic Review* 88(2), 468–74
Dezhbakhsh, H., P. H. Rubin, and J. M. Shepherd (2003) "Does capital punishment have a deterrent effect? New evidence from postmoratorium panel data," *American Law and Economics Review* 5(2), 344–76
Di Tella, R., and E. Schargrodsky (2004) "Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack," *The American Economic Review* 94, 115–33
Donohue, J. J., and S. D. Levitt (2008) "Measurement error, legalized abortion, and the decline in crime: A response to Foote and Goetz," *The Quarterly Journal of Economics* 123(1), 425–40
Draca, M., S. Machin, and R. Witt (2011) "Panic on the streets of London: Police, crime and the July 2005 terror attacks," *The American Economic Review* 101(5), 2157–81
Duggan, M. (2001) "More guns, more crime," *Journal of Political Economy* 109(5), 1086–114
Durlauf, S. N., C. Fu, and S. Navarro (2012) "Assumptions matter: Model uncertainty and the deterrent effect of capital punishment," *The American Economic Review* 102(3), 487–92
Durlauf, S. N., S. Navarro, and D. A. Rivers (2010) "Understanding aggregate crime regressions," *Journal of Econometrics* 158(2), 306–17
Ehrlich, I. (1973) "Participation in illegitimate activities: A theoretical and empirical investigation," *Journal of Political Economy* 81(3), 521–66
Evans, W. N., and E. G. Owens (2007) "COPS and crime," *Journal of Public Economics* 91(1–2), 181–201
Fryer, R. G., P. S. Heaton, S. D. Levitt, and K. M. Murphy (2005) "Measuring the impact of crack cocaine," NBER working paper no. 11318
Gannon, M. (2001) "Crime comparisons between Canada and the United States," *Juristat* 21(11), 1–12
Garrett, T. A., and L. S. Ott (2011) "Crime and arrests: deterrence or resource reallocation?," *Applied Economics Letters* 18(12), 1171–75
Gould, E. D., B. A. Weinberg, and D. B. Mustard (2002) "Crime rates and local labor market opportunities in the United States: 1979–1997," *The Review of Economics and Statistics* 84(1), 45–61
Hutchins, H. (2014) "Police resources in Canada, 2013," Juristat, catalogue no. 85-002-X
Joyce, T. (2009) "A simple test of abortion and crime," *The Review of Economics and Statistics* 91(1), 112–23
Klick, J., and A. Tabarrok (2005) "Using terror alert levels to estimate the effect of police on crime," *The Journal of Law & Economics* 48(1), 267–79
Levitt, S. D. (1996) "The effect of prison population size on crime rates: Evidence from prison overcrowding litigation," *The Quarterly Journal of Economics* 111(2), 319–51
——— (1997) "Using electoral cycles in police hiring to estimate the effect of police on crime," *The American Economic Review* 87(3), 270–90
——— (1998) "Why do increased arrest rates appear to reduce crime: Deterrence, incapacitation, or measurement error?," *Economic Inquiry* 36(3), 353–72
——— (2002) "Using electoral cycles in police hiring to estimate the effect of police on crime: Reply," *The American Economic Review* 92(4), 1244–50
Levitt, S. D. (2004) "Understanding why crime fell in the 1990s: Four factors that explain the decline and six that do not," *The Journal of Economic Perspectives* 18(1), 163–90

Lochner, L. (2007) "Individual perceptions of the criminal justice system," *The American Economic Review* 97(1), 444–60
Lott, J. R., and D. B. Mustard (1997) "Crime, deterrence, and right-to-carry concealed handguns," *Journal of Legal Studies* 26(1), 1–68
Marvell, T. B., and C. E. Moody (1994) "Prison population growth and crime reduction," *Journal of Quantitative Criminology* 10(2), 109–40
——— (1996) "Specification problems, police levels, and crime rates," *Criminology* 34(4), 609–46
——— (1998) "The impact of out-of-state prison population on state homicide rates: Displacement and free-rider effects," *Criminology* 36(3), 513–36
Mastrobuoni, G. (2013) "Police and clearance rates: Evidence from recurrent redeployments within a city," Collegio Carlo Alberto working paper
Mathur, V. K. (1978) "Economics of crime: An investigation of the deterrent hypothesis for urban areas," *The Review of Economics and Statistics* 60(3), 459–66
McCrary, J. (2002) "Using electoral cycles in police hiring to estimate the effect of police on crime: Comment," *The American Economic Review* 92(4), 1236–43
Mustard, D. B. (2003) "Reexamining criminal behavior: The importance of omitted variable bias," *The Review of Economics and Statistics* 85(1), 205–11
Newey, W. K., and K. D. West (1987) "A simple, positive semi-definite, heteroskedasticity and autocorrelation consistent covariance matrix," *Econometrica* 55(3), 703–8
Parks, R. W. (1967) "Efficient estimation of a system of regression equations when disturbances are both serially and contemporaneaously correlated," *Journal of the American Statistical Association* 62(318), 500–9
Pogue, T. F. (1975) "Effect of police expenditures on crime rates: Some evidence," *Public Finance Review* 3(1), 14–44
Polinsky, A. M., and S. Shavell (2000) "The economic theory of public enforcement of law," *Journal of Economic Literature* 38(1), 45–76
Sen, A. (2007) "Does increased abortion lead to lower crime? Evaluating the relationship between crime, abortion, and fertility," *The B.E. Journal of Economic Analysis & Policy* 7(1), 1–38
Sen, A., K. Rybczynski, and C. Van De Waal (2011) "Teen employment, poverty, and the minimum wage: Evidence from Canada," *Labour Economics* 18(1), 36–47
Sen, A., and H. Ariizumi (2013) "Teen families, welfare transfers, and the minimum wage: Evidence from Canada," *Canadian Journal of Economics* 46(1), 338–60
Shepherd, J. M. (2002) "Fear of the first strike: The full deterrent effect of California's two- and three-strikes legislation," *The Journal of Legal Studies* 31(1), 159–201
——— (2004) "Murders of passion, execution delays, and the deterrence of capital punishment," *The Journal of Legal Studies* 33(2), 283–321
——— (2005) "Deterrence versus brutalization: Capital punishment's differing impacts among states," *Michigan Law Review* 104(2), 203–56
Shoesmith, B., and C. Klein (2012) "An examination of the impact of police expenditures on arrest rates," *Explorations* (Mathemathics and Economics) VII, 106–17
Spelman, W. (2005) "Jobs or jails? The crime drop in Texas," *Journal of Policy Analysis and Management* 24(1), 133–65
Thaler, R. (1977) "An econometric analysis of property crime: Interaction between police and criminals," *Journal of Public Economics* 8(1), 37–51
Wolpin, K. I. (1978) "An econometric analysis of crime and punishment in England and Wales, 1894–1967," *Journal of Political Economy* 86(5), 815–40
Zimmerman, P. R. (2004) "State executions, deterrence, and the incidence of murder," *Journal of Applied Economics* 7(1), 163–93
——— (2009) "Statistical variability and the deterrent effect of the death penalty," *American Law and Economics Review* 11(2), 370–98