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What Do Economists Know about Crime?

Angela K. Dills, Jeffrey A. Miron, and Garrett Summers

8.1 Introduction

Since Gary Becker's (1968) seminal article on the economic model of crime, economists have devoted considerable effort to determining its empirical validity. Much of this research examines deterrence, the idea that policy can reduce crime by raising the expected costs. This literature focuses in particular on arrest and incarceration rates, policing levels, and punishments like the death penalty. Considerable additional research, while not directly focused on deterrence, considers hypotheses derived from economic models and uses statistical techniques commonly employed by economists. Such hypotheses include, among others, the roles of abortion legalization, gun laws, guns, lead, and drug prohibition in causing crime.

In this chapter we evaluate what economists have learned over the past forty years about the major determinants of crime. We consider both the policy variables related to deterrence and the more unconventional factors examined by economists. We rely on two kinds of evidence: an examination of aggregate data over long time periods and across countries, and a critical review of the literature.

Our empirical strategy consists mainly of plots that, one by one, compare crime rates with potential determinants; that is, we examine the univariate,

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"noninstrumented," relation between crime and possible explanatory factors. This approach suffers two large defects: the right model of crime is undoubtedly multifactorial, and the raw correlation between crime and a potential determinant can be misleading in the presence of endogeneity.

We nevertheless argue that examining the data in this fashion sheds considerable light on which determinants of crime are empirically important. This approach shows that the raw correlations in the data are frequently the wrong sign relative to standard models or claims. This does not prove a multivariate, "instrumented" analysis would not uncover a different effect, but it suggests this outcome is not especially likely and that any instrument would have to exert a strong impact on the endogenous variable to reverse the raw correlation. We also show, moreover, that a multivariate approach (admittedly, one that is not well-identified) fails to display the "correct" correlations in the vast majority of cases.

Based on this evaluation, we argue that economists know little about the main factors identified in the economics of crime literature as key determinants of crime. This conclusion applies both to policy variables like arrest rates or capital punishment and to indirect factors such as abortion or gun laws. The reason is that even hypotheses that find some support in U.S. data for recent decades are inconsistent with data over longer horizons or across countries. Thus, these hypotheses are less persuasive than a focus on recent U.S. evidence might suggest.

The hypothesis that drug prohibition generates violence, however, is consistent with the long time series and cross-country facts. Previous research has considered this hypothesis; we focus on a broader set of data and show the potentially large role that drug prohibition plays in determining violence. The evidence we present is only suggestive, but it indicates this hypothesis deserves further exploration. This analysis is also consistent with a general perspective in which government policies that affect the amount and nature of dispute resolution play an important role in determining violence.

The chapter proceeds as follows. In section 8.2 we present basic facts about crime over the past century in the United States and other countries. Section 8.3 examines whether individual deterrence variables appear to explain crime. Section 8.4 considers alternative determinants of crime, while Section 8.5 considers the role of drug prohibition, still focusing on explanatory variables one by one. Section 8.6 presents ordinary least squares (OLS) regressions that potentially account for multiple determinants of crime. Section 8.7 concludes.

8.2 Stylized Facts

Recent research on crime typically uses the past several decades of U.S. data. This is in part because data on crime and its possible determinants are

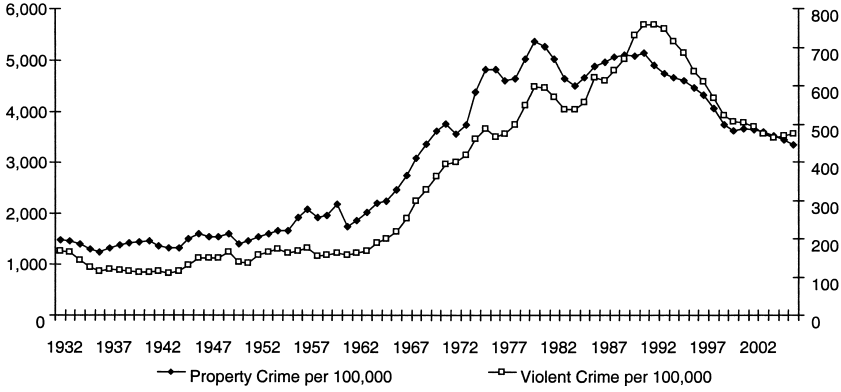

Fig. 8.1 Violent and property crime rates per 100,000: 1932 to 2006
Source: FBI Uniform Crime Reports (UCR) (various years).

more readily available for this period, in part because crime data for other countries are less readily available or difficult to compare to U.S. data. This focus also occurs because crime has fluctuated substantially during the past several decades in the United States, making it an inherently interesting period.

It is nevertheless possible and interesting to consider longer time series and, in some cases, data from other countries. Crime varies substantially across time and space, so a broader perspective can support or refute hypotheses of interest. We begin, therefore, by establishing key facts about crime over the long run and across countries.

Figure 8.1 presents the violent crime and property crime rates in the United States over the period 1932 to 2006. The data are from the FBI's Uniform Crime Reports. All data are in per capita terms.

Violent crime began to rise in the mid-1960s and increased overall until the early 1990s, when it began a persistent decline. The decline was substantial, but even by 2006 violent crime was still well above its level in the 1940s and 1950s. Some of the upward trend might reflect changes in crime reporting, so the measured difference between 2006 and the 1950s may overstate the true change.

Property crime behaves similarly to violent crime. Property crime increased beginning in the mid-1960s and remained substantially higher from the 1970s through the present compared to the 1940s and 1950s. Violent crime and property crime differ somewhat after 1970, with property crime peaking in 1980 rather than in the early 1990s. Property crime did increase during the latter half of the 1980s, but less so than violent crime.

Figure 8.2 presents the murder rate and the homicide rate. The murder rate is from the same source as the violent and property crime rates; the homicide

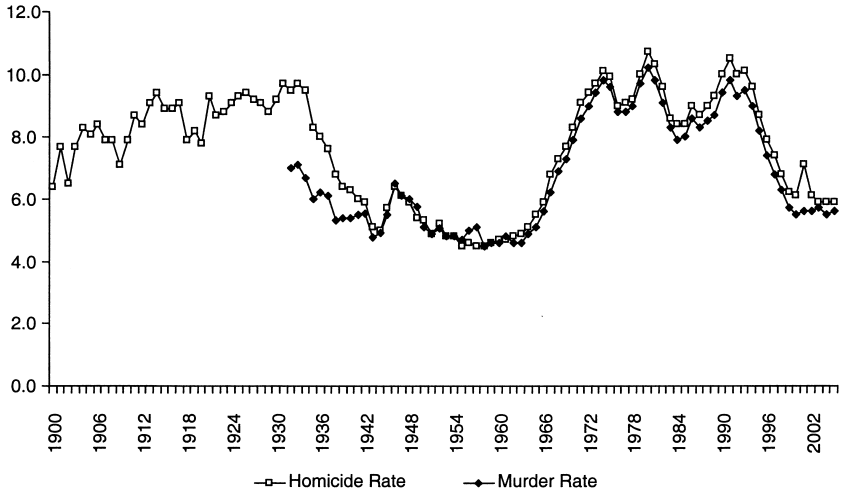

Fig. 8.2 Vital statistics homicide and UCR murder rate per 100,000: 1900 to 2005
Source: CDC, National Vital Statistics; Eckberg (1995); FBI UCR.

rate is from vital statistics sources and is available for a longer time period.[1] Murder behaves similarly to the overall violent crime rate. The murder rate increased substantially in the 1960s and early 1970s, displayed a marked increase in the 1980s, and a dramatic decline in the 1990s. The murder rate declined between the 1930s and the 1950s, however, while the violent crime rate did not. More importantly, the decline in the murder rate during the 1990s returned to virtually its 1960s level, while violent crime remains well above its 1960s levels. The decline in the murder rate also appears to have abated by the end of the sample. The homicide rate behaves similarly to the murder rate, but the pre-1950s decline is more apparent.[2] In addition, the homicide rate displays a persistent increase over the first three decades of the century and an enormous decline during the 1930s.

What these data show is that U.S. crime exhibited substantial variation well before the 1980s and 1990s, with fluctuations in both directions. The homicide rate does not show an overall upward trend, although this might reflect changing medical technology in the treatment of gun shots and other serious injuries.[3] A convincing explanation of crime can ideally account for all of this variation, not just the past two decades.

We turn next to data for other countries. We focus on vital statistics mea-

1. Data for the early years are problematic due to changes in the coverage area and underreporting of homicide, so we employ an adjusted series for 1900–1932 (Eckberg 1995).

2. Deaths resulting from the 9/11 terrorist attacks are recorded as homicides but not as murders.

3. See Harris et al. (2002). This decline in murder would tend to increase reported assaults since some of those assaulted do not die.

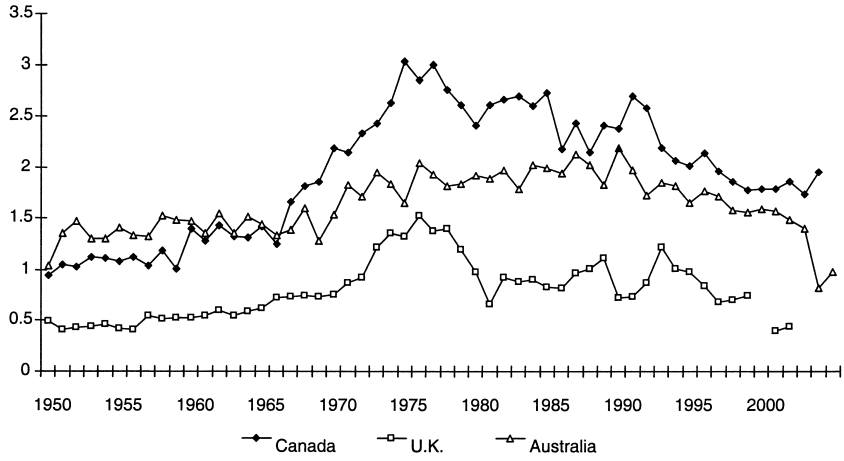

Fig. 8.3 Cross-country homicides per 100,000: 1950 to 2005
Source: World Health Organization (WHO).

sures of the homicide rate, since this series is more accurately and consistently reported than most crime statistics.

Figure 8.3 shows homicide rates for Canada, the United Kingdom and Australia over time. The average level of homicide has been substantially lower in these three countries than in the United States over the same period. Homicide nevertheless displays substantial variation, and this variation is broadly similar to that in the United States. Homicide rates rose during the 1960s and early 1970s but then declined over the remaining part of the sample. This similarity raises doubt about U.S.-specific explanations for the major fluctuations in crime.

Table 8.1 compares homicide rates across countries. Among Organization for Economic Cooperation and Development (OECD) countries, the United States is a major outlier, with a homicide rate in 2001 of 7.06, compared to the OECD average of 1.62. Compared to a broader set of countries, however, the U.S. homicide rate is less unusual. The average for all countries in the sample is 6.28, and nineteen countries have rates in excess of the U.S. rate, sometimes by substantial amounts. The most extreme case is Colombia, where the homicide rate is 62.38.

A convincing account of the determinants of homicide, therefore, should explain not only the fluctuations over recent decades in the United States but also longer term fluctuations in the United States and in other countries. Likewise, a complete account should explain the differences in homicide across countries. In particular, this account should explain why the United States has a higher rate than most similar countries but a lower rate than a broad range of other countries.

Table 8.1 Homicides per 100,000 population, various countries, around 2001

United States	7.06				
OECD countries					
Australia	1.57	Hungary	2.43	New Zealand	1.43
Austria	0.95	Iceland	0.70	Norway	0.73
Belgium	1.74	Ireland	1.04	Poland	1.72
Canada	1.49	Italy	0.97	Portugal	1.30
Czech Republic	1.32	Japan	0.58	Slovakia	2.06
Denmark	1.26	Korea	1.59	Spain	1.03
Finland	2.97	Luxembourg	2.04	Sweden	0.97
France	0.83	Mexico	10.10	Switzerland	1.13
Germany	0.68	Netherlands	1.26	United Kingdom	0.40
Greece	1.05				
OECD average	1.62				
Other countries					
Albania	7.17	Croatia	1.96	Romania	3.49
Argentina	6.93	Cuba	5.38	Russian Federation	29.85
Armenia	1.76	Estonia	15.17	Serbia and Montenegro	2.92
Azerbaijan	2.59	Georgia	3.92	Singapore	0.75
Bahamas	20.79	Hong Kong	0.77	Slovenia	0.80
Barbados	10.47	Israel	5.64	Tajikistan	2.47
Belarus	11.23	Kazakhstan	15.52	Macedonia	6.44
Brazil	26.37	Kuwait	1.74	Thailand	5.65
Bulgaria	3.08	Kyrgyzstan	6.72	Trinidad and Tobago	8.52
Cayman Islands	11.51	Latvia	12.31	Turkmenistan	7.07
Chile	9.98	Lithuania	10.23	Ukraine	12.65
China	1.98	Malta	2.29	Uruguay	5.54
Colombia	62.38	Mauritius	2.78	Uzbekistan	3.13
Costa Rica	6.05	Moldova	11.21	Venezuela	26.23
Other average	9.37				

Sources: WHO; most figures constructed from WHO Mortality database. Data for Mauritius and Denmark are for 2000; data for China for 1999; data for Belgium (1997). Some figures constructed from table 1 of WHOSIS: these include Mexico, New Zealand (2000), Argentina, Bahamas (2000), Barbados (2000), Brazil (2000), Cayman Islands (2000), Chile, Colombia (1999), Costa Rica, Cuba, Thailand (2000), Trinidad and Tobago (1998), Turkmenistan (1998), Uruguay (2000), Uzbekistan (2000), and Venezuela (2000). No data were available for a nearby year for Turkey. Population for the Cayman Islands from the *CIA World Factbook.*

8.3 Determinants of Crime: Deterrence Variables

In this section we explore the crime-reducing effect of deterrence variables like the arrest or incarceration rate. These variables are in principle controllable by policy, and they affect crime either by raising the expected cost of crime or by incapacitating criminals. We consider these variables in the context of the stylized facts above.[4]

4. For an early attempt to develop and test the implications of the deterrence model, see Ehrlich (1973). For reviews of the literature and related work on deterrence, see Cameron (1988), Benson, Kim, and Rassmussen (1994), Ehrlich (1996), Nagin (1997), Freeman (1999), and Levitt (2004).

The first variable of interest is arrests per capita, which is a proxy for the probability that a criminal offender is caught by the criminal justice system and subjected to punishment. Other things equal, a higher probability of arrest should imply a lower incidence of crime. The ideal proxy would be arrests per crime, and part of the literature considers this measure. Given the likelihood of measurement error in both crime and arrest counts, however, arrests per crime and crime per capita almost certainly show a strong negative correlation due to "ratio bias," regardless of the true relation. We focus, therefore, on arrests per capita.

Figures 8.4a–8.4c show arrest rates over time for violent crime, property crime, and murder, along with the related crime rate. The first-order fact is that arrest rates and crime correlate positively, contrary to what should occur if the main operative mechanism is that higher arrest probabilities reduce crime. This positive correlation does not prove the arrest probability has no deterrent effect, nor does it imply that higher arrests increase crime. Instead, it likely indicates that a third factor is simultaneously driving both crime and arrests. For example, political economy considerations could cause arrest rates to respond positively to crime rates. Alternatively, the strong positive relation might be a reporting effect, since recording of crime and arrests tends to go together. The fact that the correlation is positive, however, raises doubt as to whether arrests have a major deterrent effect, and it suggests that an appropriate instrument would have to be powerful to overcome the inherent endogeneity in the arrest rate.

A second key deterrence variable is the size of the police force. Though it might seem obvious that police should reduce crime, this implication is not immediate. The standard model of crime identifies the probabilities of arrest and conviction, along with the expected punishment, as the primary deterrence variables. If these are held constant, police *per se* should have no additional impact. In practice, comparing police and crime makes sense for several reasons. Counting the number of police is easier than assessing arrest probabilities, and the probability of arrest is plausibly increasing in the size of the police force. Also, the relation between police and crime does not suffer from ratio bias. Despite the plausibility of the hypothesis, however, a long literature has found it difficult to confirm an effect of police on crime.[5] This is plausibly because jurisdictions with high crime rates tend to hire more police. In principle one can address this by finding an appropriate instrument, but in practice such instruments are rare.[6]

Figure 8.5 plots police personnel and police officers per capita in the United States from 1934 to 2006. The number of police crept upward in the

5. See the discussion in Cameron (1988), Cornwell and Trumbell (1994), or Moody and Marvell (1996).

6. Levitt (1997) attempts to solve the endogeneity problem using the timing of mayoral and gubernatorial election cycles and finds a strong negative impact of police on crime. The results are sensitive, however, to a coding error (McCrary 2002).

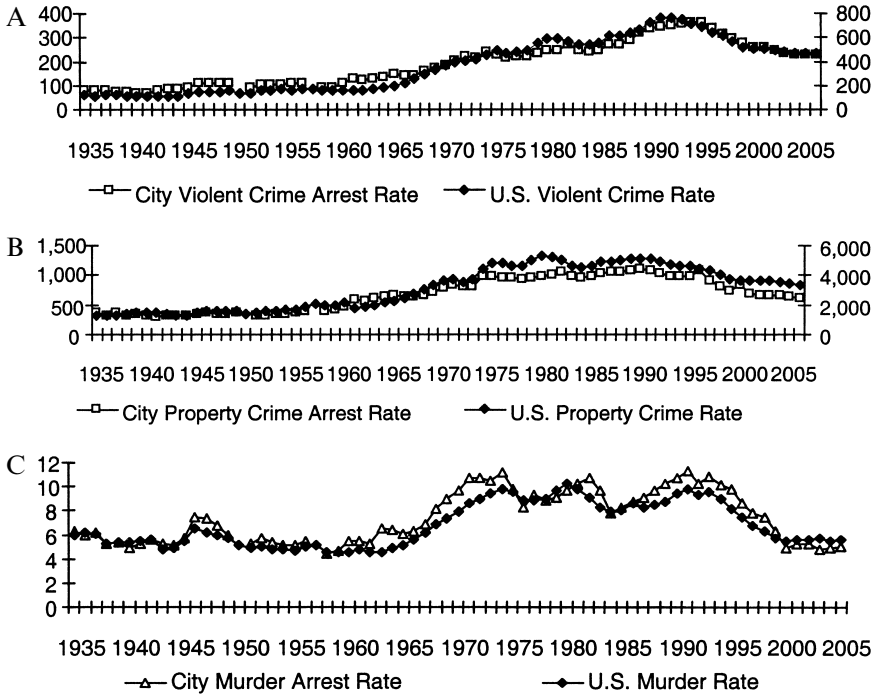

Fig. 8.4 Crime rates and arrest rates in cities with over 2,500 (1935 to 2006): *A*, violent crimes; *B*, property crimes; *C*, murder
Source: FBI UCR (various years).

early part of the period, increased more rapidly in the late 1960s, flattened out in the 1970s, and increased through 1999. The number of police officers increased less rapidly than police employees although a similar peak occurs in 1999. The steady, slight increase in police occurred simultaneously with large fluctuations in crime rates. Police and crime both increase in the 1960s; declines in crime begin years before the decline in police at the turn of the twenty-first century. Thus, the overall correlation between police and crime is the opposite of what is implied by the deterrence hypothesis.

The perverse correlation does not prove deterrence is wrong or that police increase crime. These data, however, make clear the hurdle any structural estimation must overcome: the dominant variation in the data is in the wrong direction, so any instrument would need to be powerful to find a negative effect of police on crime. The perverse correlation also suggests skepticism about the effectiveness of this aspect of the deterrence hypothesis.

A third key deterrence variable is the incarceration rate. Incarceration can affect crime through several channels. First, the threat of incarceration

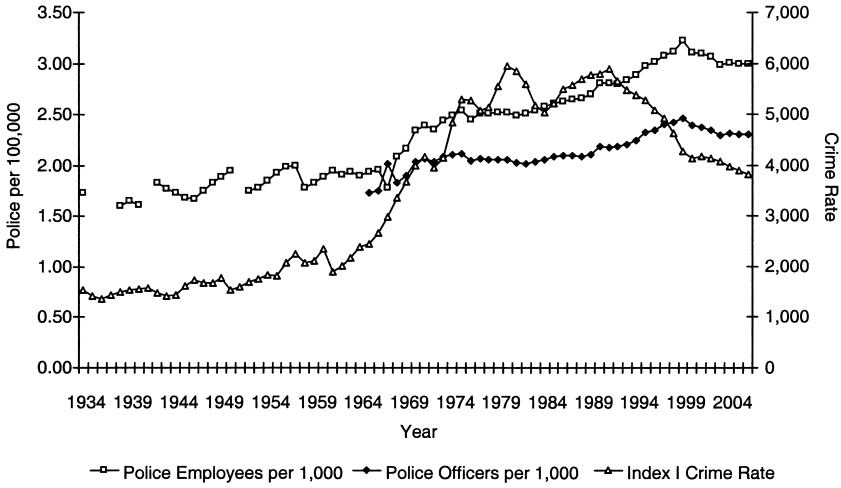

Fig. 8.5 Police employees per 1,000 and the Index I crime rate, 1934 to 2006
Sources: FBI UCR (various years).

can raise the expected punishment for crime. Second, persons in prison cannot commit crimes against the nonprison population. Both channels imply that greater incarceration leads to less crime. A possibly offsetting effect is that incarceration might make some persons more criminogenic due to lost human capital, peer effects (hardening), or increased criminal capital.[7] Thus, the magnitude and possibly the sign of any incarceration effect can only be determined empirically.

Much of the literature suggests a small, crime-reducing effect of incarceration on net, but some research suggests a zero or even perverse effect.[8] One possible interpretation is that the true effect is negative but reverse causation biases the coefficient toward zero, since a higher crime rate likely implies more incarcerations. As in the arrests and police literature, the key empirical issue is therefore whether an appropriate instrument exists.[9]

Figure 8.6 presents the incarceration rate for the United States from 1925 to 2006. After fluctuating in a relatively narrow range from 1925 to 1970, the incarceration rate began increasing in the early 1970s at a steady rate

7. Chen and Shapiro (2004), for example, provide evidence of a hardening effect.

8. See Johnson and Raphael (2006) for a recent review of the literature. Abrams (2006) provides evidence based on add-on gun laws that incarceration deters crime, while Kessler and Levitt (1999) document both a deterrent and an incapacitation effect based on sentence enhancements. Webster, Doob, and Zimring (2006) critique Kessler and Levitt (1999); Levitt (2006) responds.

9. Levitt (1996) uses lagged judicial decisions to circumvent this problem. This instrument does not satisfy the exclusion restriction, however, if the error term is serially correlated. Also, the first stage *F*-statistics are well below 10 in many cases. Johnson and Raphael (2006), using

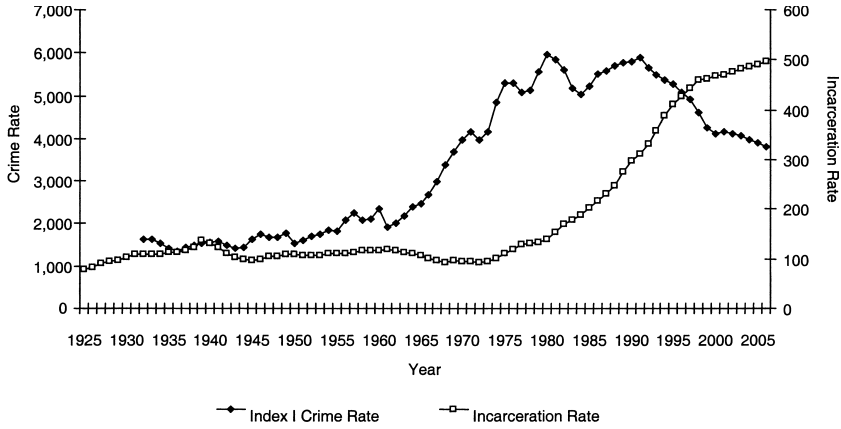

Fig. 8.6 Incarceration rate per 100,000 and Index I crime rate, 1925 to 2006
Sources: Index I Crime Rate—FBI UCR (various years). *Sourcebook of Criminal Justice Statistics.* Available online at www.albany.edu/sourcebook/wk1/t628.wk1.

and has now reached a level almost five times the average rate over the pre-1970 period.[10]

This figure poses a challenge for the view that incarceration reduces crime. Starting around 1990, the incarceration rate climbs while the crime rate falls drastically, which is consistent with the view that incarceration reduces crime. During the early part of the sample, however, crime fluctuated substantially while the incarceration rate did not. In the 1970s and early 1980s, both rates climbed simultaneously, the opposite of what deterrence and incapacitation imply. The average level of the incarceration rate, moreover, is much higher in the past several decades, so even a small causal effect of incarceration in reducing crime implies much lower crime than now observed.

Further evidence on the relation between incarceration and crime comes from cross-country comparisons. Figures 8.7a–8.7c show scatter plots of homicide and incarceration rates for several groups of countries. The first consists of the fifty-eight countries with available data on both incarceration rates and homicide.[11] This figure suggests a positive correlation between incarceration rates and homicide rates, but it is dominated by a few countries that plausibly differ across multiple dimensions from the remaining

a different identification scheme, find evidence consistent with Levitt but show the effect of incarceration has become much weaker in recent years.

10. The average incarceration rate from 1925–1969 is 108 prisoners per 100,000. The incarceration rate in 2006 was 497 per 100,000. The average incarceration rate from 1970–2006 is 280.

11. Walmsley (2004) provides incarceration rates for 205 independent countries and dependent territories and reports that 62.5 percent of countries have incarceration rates below 150 per 100,000.

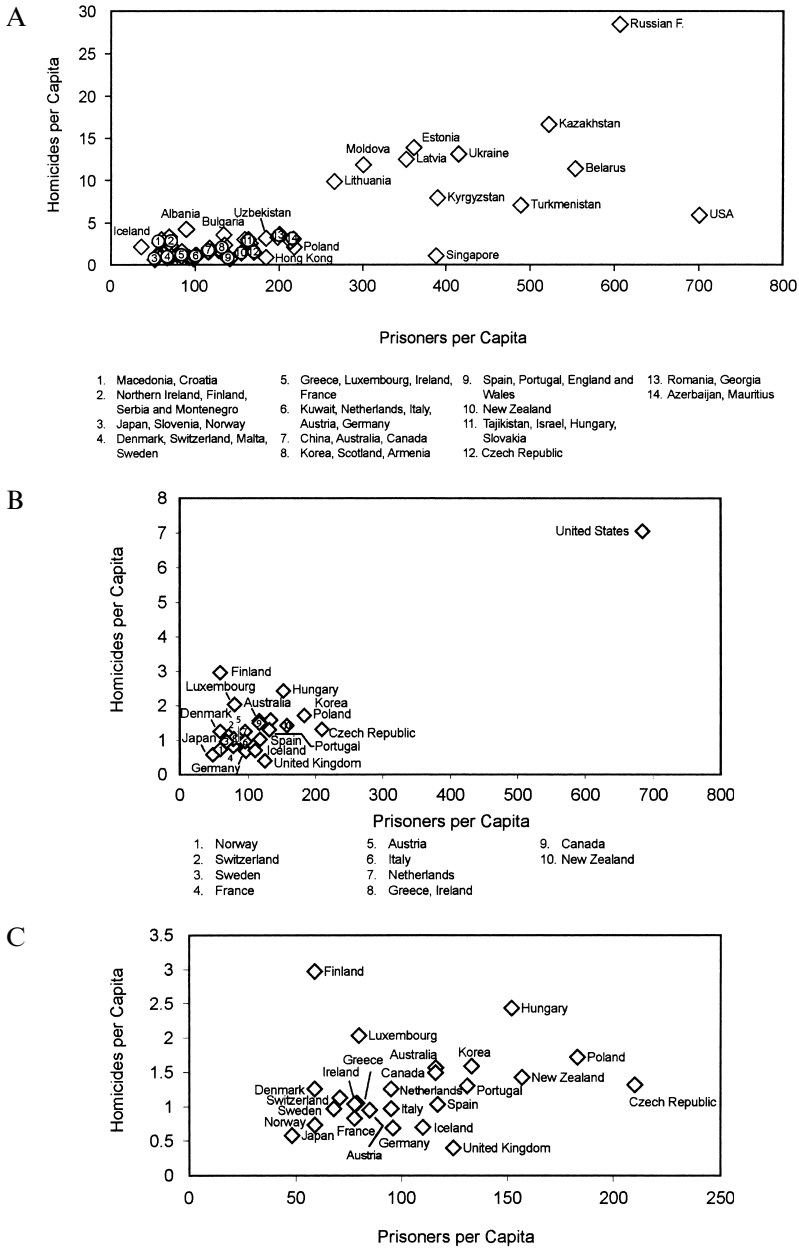

Fig. 8.7 Homicides per capita and prisoners per capita 2001: *A*, fifty-eight countries; *B*, OECD; *C*, OECD excluding the United States

Sources: International prisoners data from OECD Factbook 2007: Economic, Environmental and Social Statistics and Walmsley (2004). Homicides from WHO.

countries. The second figure shows data for the OECD countries. These again suggest a strong positive relation between homicide and incarceration, but the United States is an extreme outlier. The third figure excludes the United States and still shows a significant, positive correlation. Yet again one cannot draw a structural conclusion, but the strong, perverse correlation between homicide and incarceration should make one cautious in accepting the proposition that increased incarceration reduces crime.

A final deterrence variable that economists link to crime, especially homicide, is capital punishment. According to the deterrence model, imposing capital punishment rather than life imprisonment increases the expected cost of crime, thereby reducing crimes like murder. Initial empirical work based on aggregate U.S. data appeared to suggest that capital punishment reduces homicide (Ehrlich 1975, 1977), but subsequent work raised doubts about this conclusion (e.g., Passell and Taylor, 1977).[12] Due to the Supreme Court's reinstatement of the death penalty in 1976 and the subsequent increase in executions during the 1980s and 1990s, as well as renewed political interest in the death penalty, several papers have reexamined the issue using post-moratorium, state-level data. Most of these papers claim to find a significant deterrent effect, but many of the results are not robust to reasonable modifications of the statistical specification.[13] More broadly, Donohue and Wolfers (2005) argue that the evidence does not support a substantial deterrent effect of the death penalty.

We review here the first-order facts related to capital punishment. Figure 8.8 plots the number of executions in the United States against the homicide rate.[14] In some sub-periods the correlation is negative, as implied by deterrence hypothesis, but in other sub-periods the correlation is positive. For example, executions increased rapidly while murder fell during the 1990s, but from 1900 to the early 1960s executions and homicide moved together. During the moratorium period, murder fluctuated considerably even though the number of executions was zero.[15]

12. The single most important critique is that the results are highly sensitive to sample period and/or functional form. This is because the last few years of the sample had zero executions. If one excludes these, as seen below, the correlation between executions and homicide is positive. If one includes these in a linear specification, the correlation is still positive. The original Ehrlich paper, however, used the log of executions and assumed a small number of executions to avoid taking the log of zero.

13. See, for example, Dezhbakhsh, Rubin, and Shepherd (2003); Mocan and Gittings (2003); Shepherd (2004); and Dezhbakhsh and Shepherd (2006). Dezhbakhsh, Rubin, and Shepherd and Shepherd fail to use state trends and/or clustered standard errors. Dezahbakhsh and Shepherd's results are sensitive to measuring executions in levels rather than adjusted for population. Mocan and Gittings results are sensitive to the assumed lag between executions and murders. Mocan and Gittings (2006) argue that the theoretically sound assumption about this lag supports an effect of the death penalty, but their case for the assumed lag is not persuasive. Katz, Levitt, and Shustorivich (2003) fail to find an economically or statistically significant effect of the death penalty. Cohen-Cole et al. (2007) find limited evidence for a deterrent effect.

14. For the homicide rate we use vital statistics homicides, adjusted by Eckberg (1995).

15. For 1900–2005 (full sample) the correlation is –0.11, p-value = 0.28; for 1900–1955 it is 0.34, p-value=0.012; and for 1956–2005 it is –0.60, p-value=0.000.

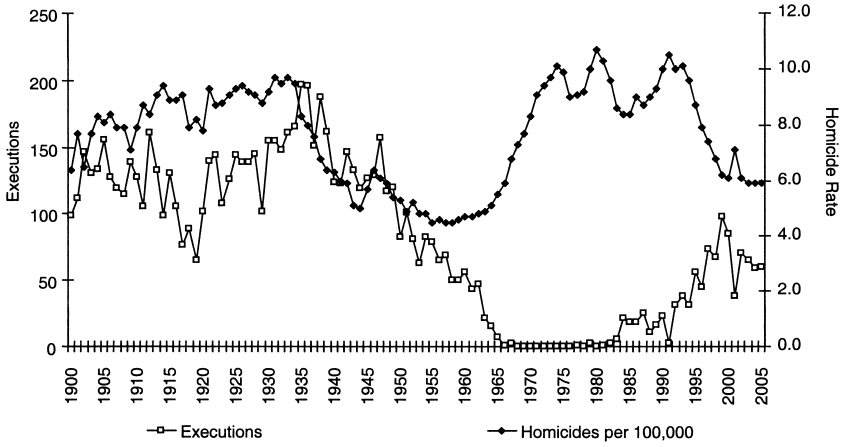

Fig. 8.8 Executions and homicide rate per 100,000: 1900 to 2005

Source: Espy, M. W., and J. O. Smykla. *Executions in the United States, 1608–2002: The Espy file* [Computer file]. 4th ICPSR ed. Compiled by M. Watt Espy and John Ortiz Smykla, University of Alabama. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [producer and distributor], 2004. Vital Statistics Homicides adjusted by Eckberg (1995).

Figure 8.9 graphs similar data for Canada from 1926 through 2005. The first forty years showed substantial variation in the number of executions while the homicide rate remained relatively stable. The correlation for 1926 to 2005 is significantly negative (–0.59, p-value 0.00), consistent with a deterrent effect of capital punishment. For the time period 1926 to 1962, however, the correlation is significantly positive (0.34, p-value = 0.039.)[16] In the last forty years, homicide has fluctuated substantially while the execution rate has been constant at zero. Thus, the overall negative correlation is an artifact of two very different regimes; in one the correlation is perverse, and in the other it is zero.

Figure 8.10 demonstrates a similar result for the United Kingdom. Homicides per capita fluctuated substantially over the past forty years despite an almost complete absence of executions. Thus, the data provide little affirmative evidence that is consistent with the standard deterrence story.

8.3.1 Discussion

The previous results present a challenge to key aspects of the deterrence model of crime. An important feature of this model is the implication that policy can reduce crime by manipulating variables such as arrest rates, policing levels, and so on. The aggregate data do not provide much confirmation for this perspective, at least not in a simplistic, "more X, less crime" kind of way.

16. For the time period 1926–1966, the correlation becomes an insignificant 0.24, p-value = 0.128.

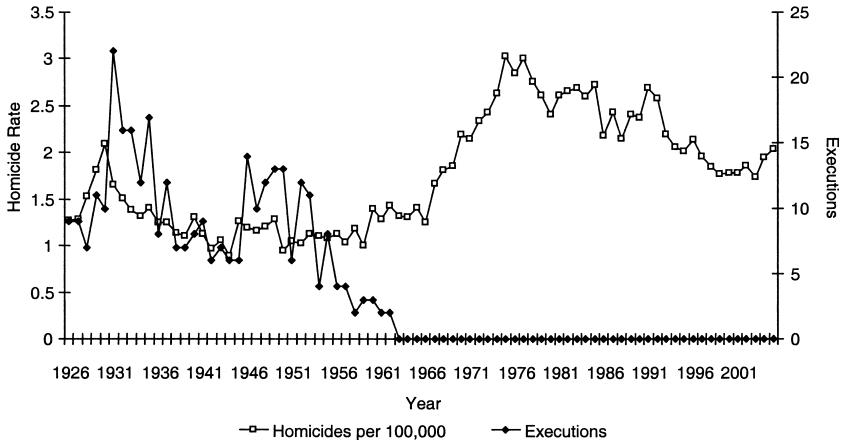

Fig. 8.9 Canada homicides per 100,000 and executions, 1926 to 2005

Sources: Canadian executions: 1926–1960 from *Historical Statistics of Canada;* 1961–1962 from http://geocities.com/richard.clark32@btinternet.com/canada.htm. Extended time series of homicides for Canada also from *Historical Statistics of Canada.* Social Science Federation of Canada and Statistics Canada.

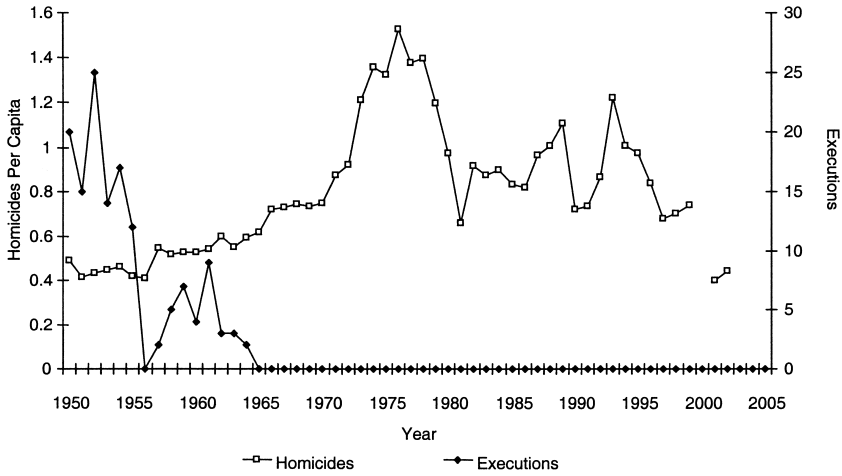

Fig. 8.10 U.K. homicides per capita and executions, 1950 to 2005

Source: U.K. executions from http://www.murderfile.net.

This conclusion in no way suggests that criminals are not rational or that they fail to respond to incentives. On the one hand, the analysis above is too broad brush to support such a specific claim. On the other hand, numerous analyses do find that criminals respond to incentives in roughly rational ways; this research, however, focuses on responses that are local or in response to specific and unambiguous changes or differences in the incen-

tives faced by criminals.[17] So, we do not mean to throw out the baby with the bath water by arguing against the deterrence model at a general level or minimizing its usefulness as an organizing device. We are instead focusing on specific, policy-relevant implications, and we are addressing the naïve, "if we hire more police, or make more arrests, or lock more people up, that will necessarily reduce crime" perspective. This application of the deterrence model is not supported by the data. A key question, then, is why we do not find more evidence of deterrence with respect to these variables.

One possibility is that data limitations make it difficult to test these hypotheses in the settings where they are most relevant. For example, it could be that going from no police force to some police force indeed reduces crime, but the observed variation in the size of modern police forces is over a range where diminishing returns have set in so that additional police have little impact. Thus, our evidence cannot rule out the possibility that adding more police is unlikely to lower crime, but substantially reducing police might increase crime.

A different hypothesis is that most existing analyses, even those using city- or state-level data, are too aggregated with respect to time period, place, type of crime, and type of police effort to uncover a number of effects that might operate in practice. Thus, while putting more police on the street in a specific neighborhood and instructing them to target a particular kind of crime might indeed reduce that kind of crime in that neighborhood, especially in the short term, simply hiring more police who sit behind desks pushing paper is unlikely to reduce any kind of crime.

A third hypothesis is that deterrence variables might have counterproductive effects as well as some beneficial effects. Some expansions of police might increase crime, as we will subsequently discuss, because the police work at enforcing drug prohibition or other counter-productive activities.[18] In the case of capital punishment, the reality might be that criminals pay considerable attention to the expected costs but that executions are rare, so it is approximately rational to ignore the possibility of being executed.[19] Regarding the effect of incarceration via deterrence, it might be that some criminals do not regard prison as worse than life outside or view the negative impacts on their future lives once released as small. Kling (2006), for example, finds that incarceration has minimal impact on post-incarceration employment and earnings.

A different reason the key policy variables in the deterrence model may

17. See, for example, Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005), and Iyengar (2008).

18. For example, Iyengar (2007) finds that laws mandating arrest for reports of domestic violence increase intimate homicides, plausibly because the laws reduce reporting of initial violent acts.

19. In the U.S. in recent years, the number of executions per year has been less than 100 while the number of homicides has been over 20,000.

have small effects on crime is that the underlying behavioral parameters of criminals do not imply a big response. Criminals may be people with high discount rates and/or myopic preferences, in which case the threat of future punishment should play a relatively small role.[20]

Thus, while one interpretation of our deterrence results is that economists do not have the right data to find these effects, an alternative, reasonable interpretation is that increases in the standard deterrence variables have small or perverse effects over the relevant range.

8.4 Alternative Determinants of Crime

Perhaps due to the difficulties in establishing links between policy variables and crime, economists have considered a number of alternative explanations. In this section we examine several that have received significant attention in recent years. Our assessment is that none provides a convincing explanation for a substantial fraction of the variation in crime.

In a provocative paper, Lott and Mustard (1997) (henceforth, LM) argue that laws protecting the right to carry a concealed gun (RTC laws) increase concealed carry and therefore reduce crime by causing criminals to worry their intended targets might be armed. Lott and Mustard also suggest that RTC laws cause criminals to substitute away from crimes against persons, like murder, rape, robbery, and assault, to crimes "against" property, like burglary, larceny, and auto theft. The original LM analysis appeared to find that RTC laws reduce violent crime and increase property crime, consistent with the *a priori* considerations. Subsequent research, however, has raised questions about the LM conclusions, while Lott has defended these conclusions.[21]

We reconsider the LM hypothesis using long-run, aggregate data. Figure 8.11 shows the fraction of the U.S. population living under RTC laws in each year since 1900. The fraction rose steadily and substantially starting in the mid-1980s, yet violent crime does not fall until the 1990s. Despite an enormous change in the fraction of the population living in a state covered by an RTC law, crime has neither skyrocketed nor plummeted over the sample. The homicide rate, in particular, shows no particular trend, and this is a crime category that is most plausibly affected by RTC laws. Thus neither the view that RTC laws increase crime, nor the view that RTC laws decrease crime, receives obvious support from the aggregate data.

In contrast to the hypothesis that gun laws reduce crime, a different litera-

20. Lee and McCrary (2005) provide evidence consistent with this view. Lochner (2003) finds that individual beliefs about the probability of arrest are substantially idiosyncratic and not responsive to local neighborhood conditions. These beliefs do adjust in response to individual arrest experience, however, and these beliefs do correlate with criminal behavior.

21. For critiques of the LM analysis, see Black and Nagin (1998), Ayres and Donohue (2003a, 2003b), and Helland and Tabarrok (2004). For responses, see Lott (1998, 2003) and Plassmann and Whitley (2003).

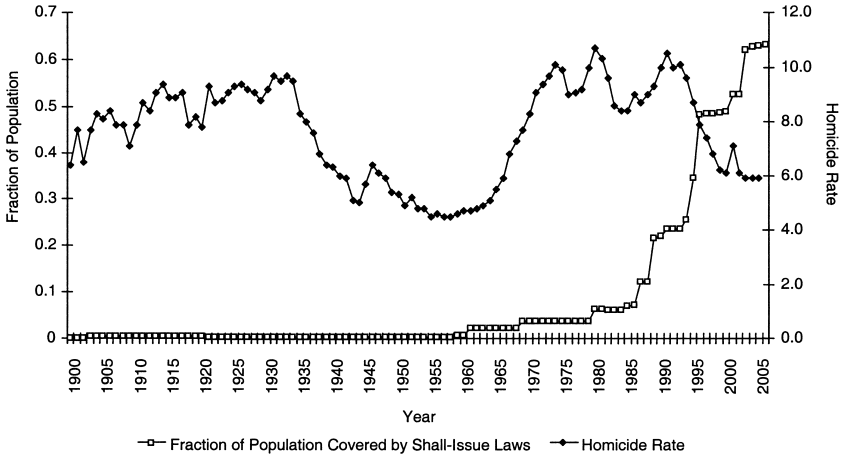

Fig. 8.11 Fraction of U.S. population covered by Shall-Issue Laws and homicide rate, 1900 to 2006

Source: Grossman and Lee (2008) and annual state population estimates from the Census Bureau. Vital Statistics homicides adjusted by Eckberg (1995).

ture considers whether guns themselves cause crime.[22] We again confront this hypothesis with aggregate level data. Figure 8.12 shows the stock of guns over time in conjunction with the violent crime rate. Crime declines numerous times, including a major decline from 1990 onward, despite a large and ever-increasing stock of guns. This makes it implausible that guns per se play a large role in causing crime, although it does not rule out some role.

A different hypothesis that has received enormous attention is that legalization of abortion in the early 1970s caused a substantial fraction of the decline in crime during the 1990s (Donohue and Levitt 2001). The logic is that access to legal abortion allows women to have children in environments less likely to produce future criminals. Thus, cohorts born after legalization should have a lower propensity to commit crime. Further, crime should have begun falling about fifteen years after legalization and continued falling as successive cohorts born after legalization entered their high crime years. Donohue and Levitt (2001), (henceforth DL) provide evidence that appears consistent with their hypothesis, stating in particular that "legalized abortion appears to account for as much as 50 percent of the recent drop in crime" (p. 379). Several authors, however, have disputed the DL conclusion, while DL have responded with evidence that claims to validate their original position.[23]

22. For a recent summary and critique of this literature, see National Research Council (2005).

23. See Joyce (2003, 2009), Lott and Whitley (2007), Foote and Goetz (2008), and Dills and Miron (2006), for critiques, and Donohue and Levitt (2004, 2008) for responses.

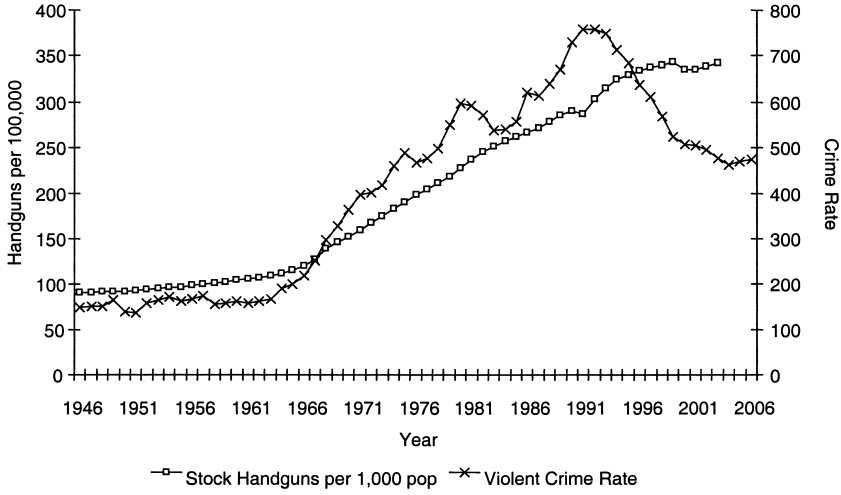

Fig. 8.12 Stock of handguns in the U.S. and the violent crime rate, 1946 to 2006
Sources: Gun data from Kleck (1997, updated with additional data from author). FBI UCR (various years) for the Index Crime Rate.

We revisit this issue using aggregate and cross-country data. Figures 8.13a–8.13c compare the abortion rate in the United States lagged twenty years with the violent crime, property crime, and homicide rates. The figures show visually what we know from history: no legal abortions occurred before 1970, and the legalization in 1970 to 1973 could not have affected crime rates before the late 1980s. Nevertheless, major fluctuations in crime occurred before 1990. This does not mean abortion legalization played no role in the declines in the past two decades, but the magnitude of legalized abortion's impact on crime is likely modest or confined to only the most recent period.[24]

Figure 8.14 shows the homicide rates in a number of other countries that have legalized or substantially liberalized abortion access. The evidence from these countries provides little support for hypothesis that legalizing abortion reduces crime. While the data from some countries are consistent with the DL hypothesis (e.g. Canada, France, Italy), several countries' data show the opposite correlation (e.g. Denmark, Finland, Hungary, Poland). In other cases crime was falling before legalization and does not decline any more quickly (twenty years) after legalization (e.g. Japan, Norway).[25]

24. We note that Donohue and Levitt emphasize the role of abortion legalization in explaining the decline in crime in the 1990s; they do not assert that abortion policy explains the longer time series. They do claim that legalization explains as much as half of the decline in the 1990s. Our view is therefore that if several other substantial declines in crime have occurred, during periods when abortion legalization could not have played a role, this should introduce some caution about the quantitative importance during recent decades.

25. For a related analysis that reaches a similar conclusion, see Kahane, Paton, and Simmons (2008).

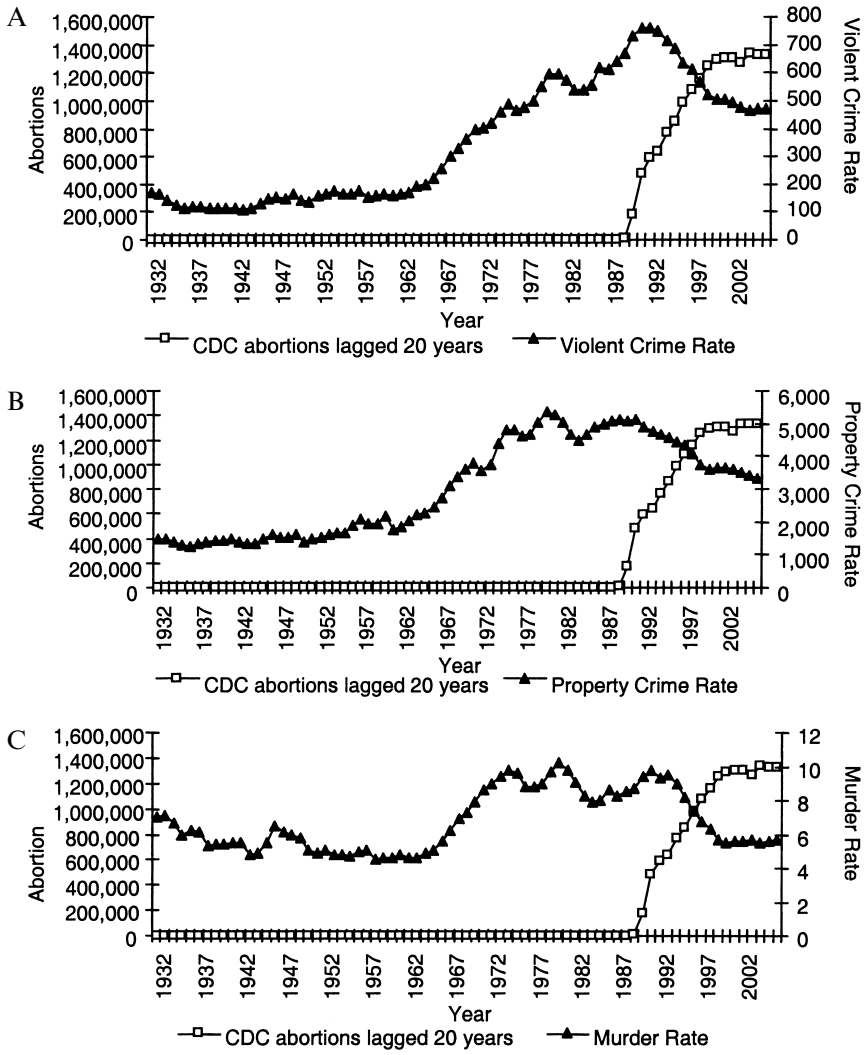

Fig. 8.13 U.S. crime rates and abortions lagged 20 years, 1932 to 2006: *A*, violent crime; *B*, property crime; *C*, murder

Source: CDC abortions' figures from "Abortion Surveillance" (various years), published in various issued of the MMWR.

A final hypothesis recently advanced to explain the variation in homicide rates is exposure to lead through paint or gasoline. According to this hypothesis, this exposure generates aggressive, antisocial behavior and violent tendencies later in life. Reyes (2007) and Nevin (2007) argue that laws restricting the use of lead have reduced this exposure and contributed to the recent decline in violent crime.

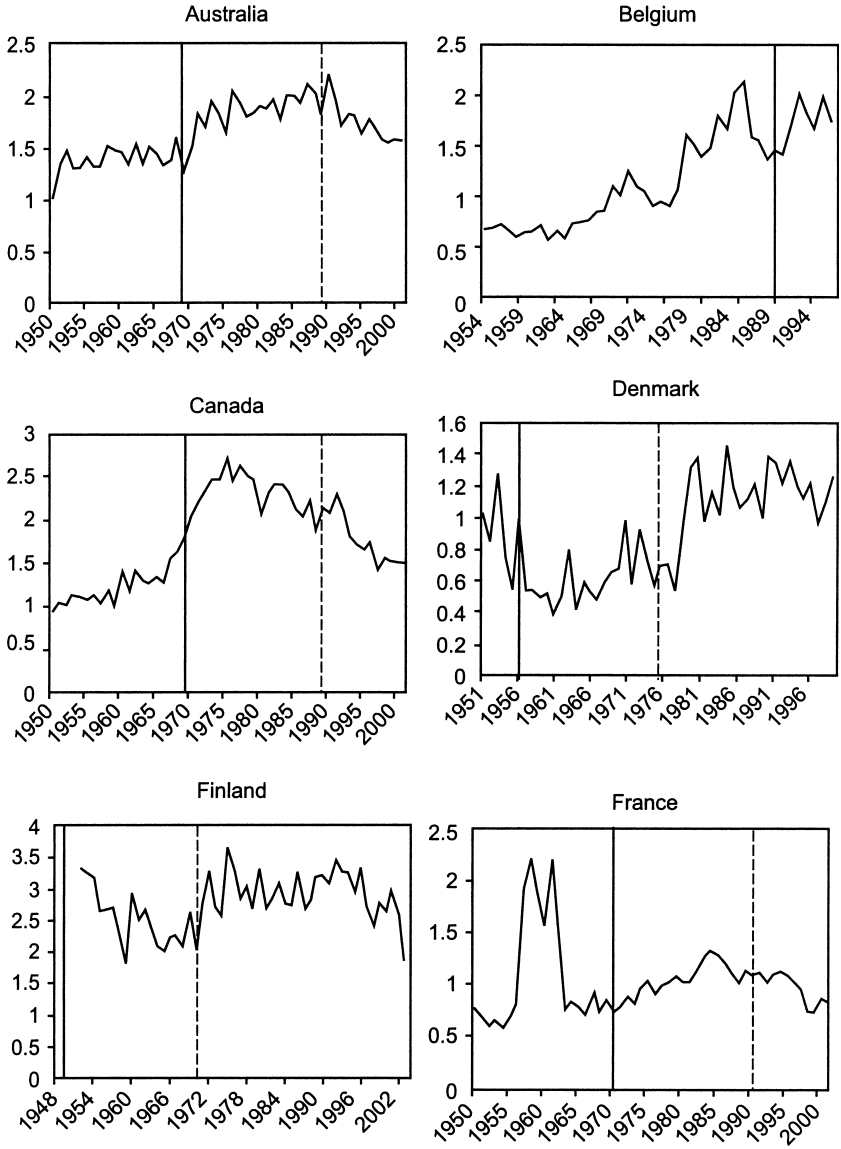

Fig. 8.14 Homicide rate time series and legalization of abortion

Sources: Homicide figures from WHO.

Note: Black vertical line indicates legalization of abortion; red vertical line indicates twenty years later.

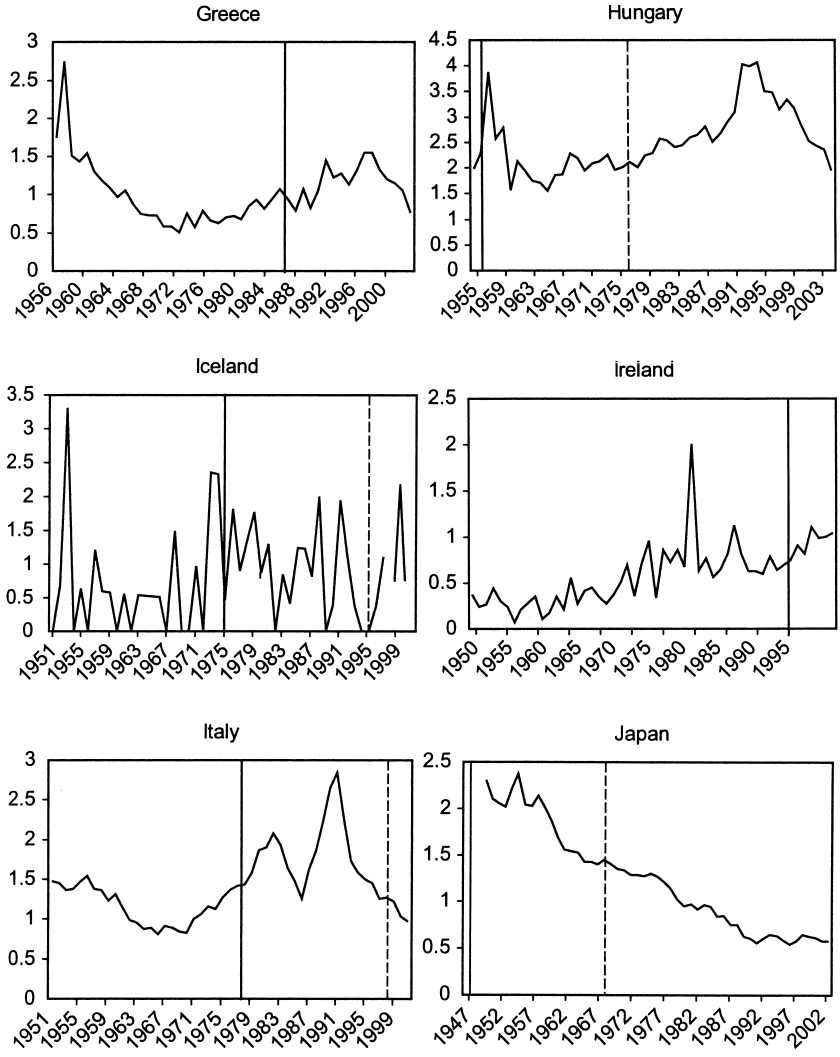

Fig. 8.14 (cont.)

Figure 8.15 plots the U.S. murder rate and a measure of environmental lead exposure lagged twenty years for 1961 through 2005. During some parts of the sample, the correlation is positive, consistent with the Reyes/Nevin hypothesis, but in other time periods, especially the late 1960s to the mid-1980s, the correlation is negative or zero.[26] Further, lead exposure falls

26. For 1961–1974, the correlation is –0.5762 (p-value = 0.0310). Between 1975 and 1992 the correlation is 0.2181 (p-value = 0.3846) and between 1993 and 2005 the correlation is 0.3331 (p-value = 0.2661).

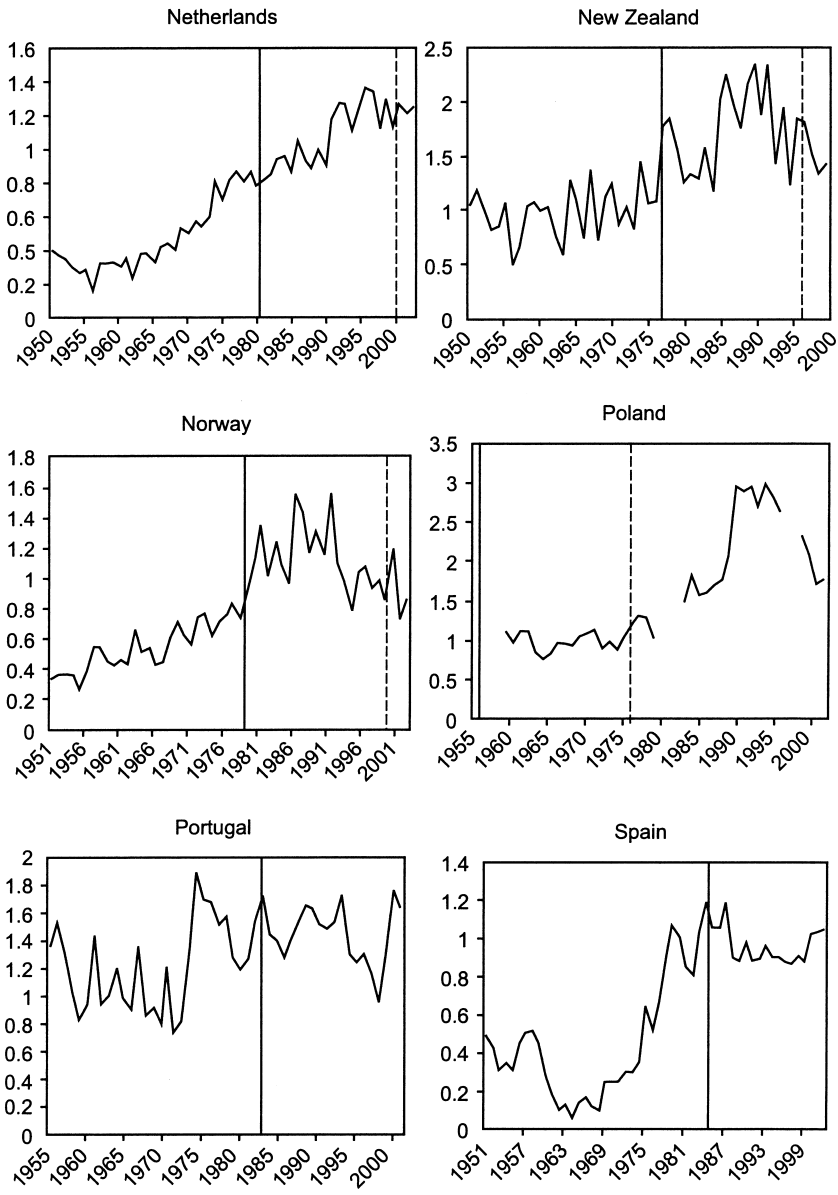

Fig. 8.14 (cont.)

to virtually nil by the end of the sample, yet crime appears to have leveled off. This makes the case for the lead hypothesis highly dependent upon the time period examined.

Figure 8.16 shows alternative measurements of lead exposure based on motor vehicle or motor fuel data. All proxies for lead increased dramati-

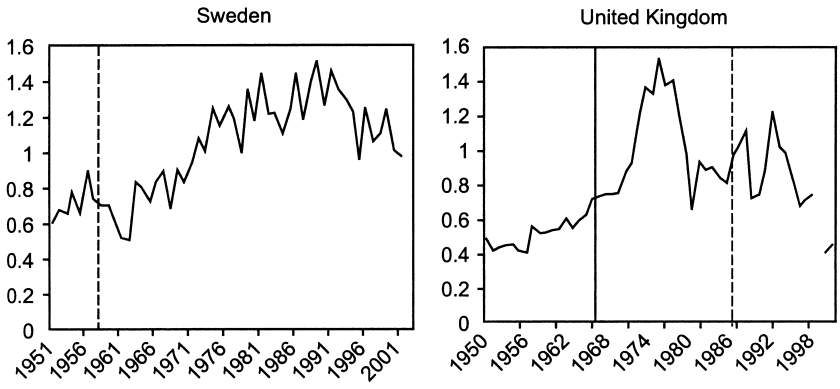

Fig. 8.14 (cont.)

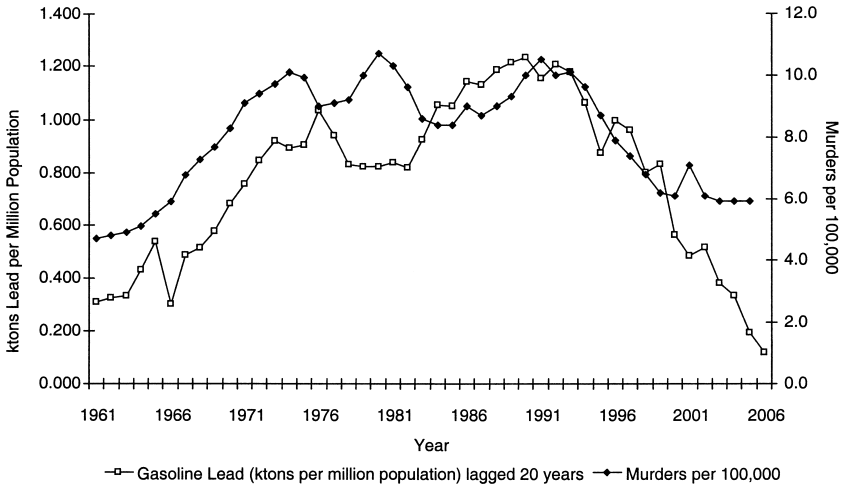

Fig. 8.15 Lead lagged twenty years and murders per 100,000: 1961 to 2006
Sources: Aggregate lead data from Reyes (2007).

cally from around 1910 through 1970. If the lead hypothesis is correct, then crime should have displayed a measurable increase between 1925 and 1985. The U.S. murder rate, however, decreased between the 1930s and 1950s. The murder rate does rise from the 1960s through the mid-1970s, but much unexplained variation remains between the mid-1970s and mid-1980s.

Our bottom line assessment on all these hypotheses is therefore as follows: each may contain an element of truth, and nothing we have done proves they are false. Any claim that these hypotheses explain large components of the fluctuations in crime, however, does not seem consistent with the aggregate data.

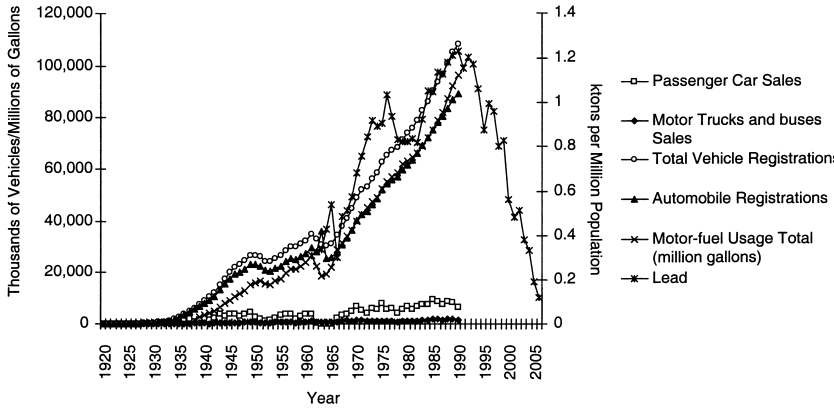

Fig. 8.16 Alternate measures of lead exposure lagged twenty years, 1920 to 2006
Sources: Automobile/fuel usage data are from the Historical Statistics of the United States.

8.5 Drug Prohibition

The previous analysis leaves a puzzle: why has crime fluctuated over the past several decades, and why are crime rates so different across countries? One possibility is that reporting conventions differ over time and place, and this undoubtedly plays a role for many crime categories and perhaps even for the most consistently reported crime series, homicide. The differences across time and space, however, seem too dramatic for this to be the main story.

We believe instead that a different mechanism, in both a general and a specific form, explains much of the variation in crime—especially violent crime—both over time and across countries. This hypothesis is that violence is more common when and where mechanisms for nonviolent dispute resolution are not readily available. This view has two implications.

At a general level, countries with weak systems for defining and enforcing property rights are likely to see elevated violence because market participants cannot use courts or nonviolent adjudication methods backed up by the courts. This is consistent with the fact that many former Soviet republics and some developing economies have high homicide rates. In these countries the rules are not always clear, and the government system for enforcing the rules is not effective. Countries with weak systems for defining and enforcing property rights are plausibly also ones in which property crimes like theft would be common.

At a specific level, the view that violence occurs when alternative dispute resolution mechanisms are not available implies that if government forces a market underground, participants substitute violence for other dispute-resolution mechanisms. The best example is drug prohibition; by forcing

drug markets underground, prohibition encourages the use of violence to resolve disputes. In particular, greater enforcement of prohibition makes it harder for suppliers and demanders to circumvent the prohibition legally (for example, by obtaining the prohibited goods through medical channels). Enforcement also reallocates property rights and upsets reputations, so the ability of private parties to establish nonviolent dispute resolution diminishes with greater enforcement, while the number of disputes likely increases. Thus, more enforcement means a larger black market and more scope for violent dispute resolution.[27]

This hypothesis is related to, but partially distinct from, the "crack cocaine" hypothesis advanced in Fryer, et. al. (2005) (FHLM). They suggest that the major upturn in violence in the 1980s and the subsequent decline in the 1990s resulted from crack's introduction and spread. When crack arrived in cities beginning in the early 1980s, the property rights to distribution (e.g., street corners) were not assigned, and since crack dealers could not use advertising or lawsuits to capture market share or property rights, they used violence instead. Over time, according to FHLM, these property rights evolved (*de facto*), so violence subsided.

We agree with this hypothesis as far as it goes, but we argue it is incomplete. First, disputes arise in markets for many reasons beyond the initial assignment of property rights, and these disputes would presumably continue as long as a market operates. Second, the FHLM hypothesis does not explain fluctuations in violence outside the sample of the 1980s and early 1990s, or in other countries.

The "drug enforcement-generates-violence" hypothesis, however, potentially explains differences in crime across a broad range of time and space. Drug and alcohol prohibition enforcement have varied substantially over the past eighty years, which might explain fluctuations in U.S. violence over that time period. Similarly, prohibition enforcement differs across countries in ways that potentially explain differences in violence.

Drug enforcement might also explain variation in crime categories other than murder and assault. An increase in drug prices from increased enforcement may cause income-generating crime such as robbery, larceny, burglary, or auto theft. If increased police effort to enforce drug prohibition comes at the expense of other activities, deterrence for nondrug crimes might decline as enforcement increases. Further, increased incarceration of drug prisoners might overcrowd prisons and cause early release of nondrug prisoners, implying additional nondrug crime. Finally, incarceration of drug prisoners might cause people with generally low criminal proclivity to become more criminogenic.[28]

27. See Goldstein (1985), Goldstein et al. (1989) and Miron (2001) for further discussion.

28. An effect might also operate in the other direction; locking up people who commit both drug crime and nondrug crime might lower general crime (Kuziemko and Levitt 2004).

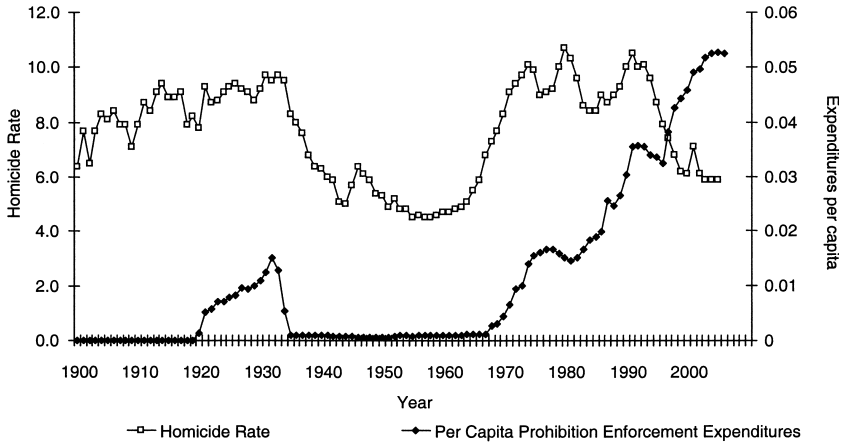

Fig. 8.17 Expenditures on prohibitions per capita and homicides per 100,000: 1900 to 2006

Sources: Homicide rate from FBI UCR (various years). Projected prohibition enforcement expenditures based on Miron (1999) with data from the Budget of the United States Government (various years).

Figure 8.17 presents data on expenditure for enforcement of alcohol and drug prohibition over time in the United States.[29] These data are broadly consistent with the view that differences in violence and perhaps other crime as well, result to a significant degree from differences in enforcement of alcohol and drug prohibition. The variations over time correlate well with the murder and homicide rates and, to a lesser degree, with the overall violent and property crime rates.[30] The relatively high rate of homicide in the United States compared to Europe is consistent with the fact that European countries enforce drug prohibition to a far lesser degree (Miron 2001). The relatively high homicide rates in various drug source countries (Colombia, Mexico) is further evidence consistent with the enforcement hypothesis.[31]

29. The data are based on those in Miron (1999).

30. The increases in crime in the 1960s seem to predate the increases in enforcement. One possibility is that the baby boom generation was entering its teens and twenties, which in part caused the increase in both crime and enforcement. In the regressions reported in section 8.6, we control for demographics.

31. Fajnzylber, Lederman, and Loayza (2002) use panel data for a large sample of developed and developing countries to examine the determinants of violent crime. They find some evidence that being a drug producer or having a high drug possession arrest rate is associated with more violence. Grogger and Willis (2000) find that rise in urban crime rates in the 1980s is strongly correlated with the introduction of crack cocaine. Some interpret this as suggesting crack has an independent, psychopharmacological effect in causing crime, but the correlation is more consistent with the FHLM story (since crack remained but crime declined). Goldstein et al. (1989) provide micro evidence that crime during the crack epidemic in New York City in 1988 resulted from disputes related to the drug trade. Dobkin and Nicosia (2009) provide evidence against the view that drug use per se causes crime. Prasad (2009) finds that economic

8.6 An Accounting Exercise

The discussion so far has considered potential crime determinants one by one, and it has relied on raw rather than instrumented correlations. In this section we address the first of these issues by considering regressions of crime rates on a multitude of possible determinants. We make no attempt to address the second issue because we are pessimistic that valid instruments exist at this level of aggregation.[32] Indeed, even finely tuned or clever instruments are often not compelling in practice, either because they have low explanatory power or are not really themselves exogenous.[33] Further, calculating accurate standards error is often difficult in standard panel regressions (Bertrand, Duflo, and Mullainathan 2004).

Table 8.2 shows regressions of various crime rates on measures of arrests, police, incarceration, executions, RTC laws, abortion, lead, and drug and alcohol prohibition enforcement.[34] The regressions also control for several factors that we have not examined explicitly: the age structure, economic conditions, and education. The existing literature provides some confirmation that each of these plays a role, although this evidence is less consistent than one might like.[35] We consider two samples: a shorter one (1961 to 2004) that includes all these variables, and a longer one that includes the variables available for the entire sample of the dependent variable. This is 1932 to 2005 for all four series, plus 1900 to 2005 for homicide.

The results are consistent with the conclusions derived above from the univariate correlations. The arrest rate enters significantly but with the wrong sign. Police per capita never enters significantly and is typically the wrong

liberalization in India, which brought much informal activity into the legal sector, was accompanied by a large decrease in violence.

32. Fisher and Nagin (1978) is the classic reference on the difficulty of identifying the crime function.

33. On the problems created by weak instruments, see Staiger and Stock (1997).

34. The regression includes a time trend, logged GNP per capita measured in 1992 dollars from the Bureau of Economic Analysis, the fraction of the population aged fifteen to twenty-four from the Census Bureau, and the fraction of the population with at least a high school diploma from the Census Bureau. Most other factors are lagged one year. These include real prohibition expenditures per capita (from the Office of Management and Budget of the United States), the percent of the population covered by right to carry laws (from Grossman and Lee [2008]) and the Census Bureau), the incarceration rate (from the Sourcebook of Criminal Justice Statistics), the arrest rate (from the FBI's UCR from various years), police employees per capita (from the FBI's UCR), and the stock of handguns per capita (from Kleck, 1997). CDC abortions are included measured in thousands and lagged 20 years. The Reyes (2007) measure of lead exposure in kilotons per million population is included lagged 20 years. In a few cases, a variable was missing data for a limited number of years. We linearly interpolated values for these missing observations. In particular, we interpolated values for police in 1936–1938, 1942, and 1952; for the arrest rates in 1950 and 1957; and the percent high school graduate for 1956, 1957, 1959, 1962, and 1964. We estimate Newey-West standard errors allowing for first order autocorrelation.

35. On the role of the age structure, see Levitt (1999) and De Mello and Schneider (see chapter 6, this volume). For recent evidence on the role of education, see Lochner (2004).

Table 8.2 **Multivariate regressions of U.S. crime rates on potential determinants**

	Property crime		Violent crimes		Murders		Homicides		
	1961–2004 (1)	1932–2005 (2)	1961–2004 (3)	1932–2005 (4)	1961–2004 (5)	1932–2005 (6)	1961–2004 (7)	1932–2005 (8)	1900–2005 (9)
Trend	0.0317	0.0369	0.0546	0.0448	0.0276	0.0178	0.0248	0.0241	0.0316
	(1.49)	(9.64)	(1.81)	(6.90)	(0.94)	(3.05)	(0.77)	(4.06)	(3.32)
ln(GNP per capita)	0.2625	–0.3541	0.8808	–0.4337	0.7434	–0.1787	0.8491	–0.6114	–1.1302
	(0.52)	(2.83)	(1.45)	(2.01)	(1.41)	(0.98)	(1.48)	(3.52)	(3.16)
% aged 15–24	1.2754	5.6154	–3.7314	4.3692	–5.7484	5.2577	–6.7234	7.9815	5.6972
	(0.51)	(6.07)	(1.44)	(2.85)	(2.09)	(4.76)	(2.30)	(6.75)	(3.86)
Prohibition expenditures per capita$_{t-1}$	3.2995	7.5471	–0.1931	21.6664	9.8620	15.6705	2.7608	14.6486	21.3226
	(0.81)	(1.55)	(0.04)	(2.56)	(2.14)	(2.62)	(0.48)	(2.27)	(3.51)
% covered by right to carry$_{t-1}$	–0.3932	–1.7027	–0.2082	–2.4350	–0.4089	–1.6177	–0.4830	–1.8579	–2.2918
	(1.53)	(5.79)	(0.69)	(4.52)	(1.19)	(3.53)	(1.15)	(2.70)	(4.64)
Incarceration rate$_{t-1}$	–0.0036	–0.0002	–0.0061	–0.0004	–0.0066	–0.0008	–0.0060	–0.0001	
	(2.22)	(0.31)	(3.80)	(0.42)	(3.89)	(0.92)	(3.10)	(0.13)	
Executions per capita$_{t-1}$	–441.6479	–43.7983	–263.9329	80.5164	–531.6189	158.1099	–376.5246	123.3264	
	(1.79)	(0.66)	(0.71)	(0.67)	(1.76)	(1.46)	(1.02)	(1.16)	
Abortion (thousands)$_{t-20}$	0.000304		0.000364		0.000525		0.000624	0.000022	
	(1.69)		(1.83)		(2.76)		(2.65)		(0.08)
Arrest rate$_{t-1}$	0.0005		0.0030		0.0468		0.0417		
	(2.36)		(3.15)		(3.51)		(2.68)		
Police per capita$_{t-1}$	–0.0168		0.0304		0.0127		0.0835		
	(0.10)		(0.16)		(0.07)		(0.52)		
Guns per capita$_{t-1}$	0.0052		0.0035		0.0068		0.0012		
	(2.47)		(1.54)		(2.46)		(0.27)		
% high school graduate or more	–0.0309		–0.0320		–0.0448		–0.0094		
	(1.27)		(1.33)		(1.92)		(0.34)		
Lead$_{t-20}$	0.1091		0.0707		0.0503		0.0526		
	(1.10)		(0.55)		(0.43)		(0.43)		
Constant	–56.1936	–62.1396	–108.3916	–78.9484	–56.8818	–32.3040	–53.4762	–40.9172	–50.2325
	(1.37)	(9.45)	(1.92)	(7.02)	(1.06)	(3.20)	(0.91)	(3.95)	(3.28)
Observations	44	74	44	74	44	74	44	74	105

Note: Newey-West *t*-statistics in parentheses.

sign. The execution rate never enters significantly and is sometimes the wrong sign. Greater incarceration does correlate with lower crime in the shorter sample but not in the longer samples, consistent with the graphical evidence above.

Right to carry laws are consistently correlated with lower crime, but the correlation is about as negative for property crime as for violent crime, which is not consistent with the LM hypothesis. Guns are positively correlated with crime for all four crime series, but again the correlation is particularly strong for property crime, which is counterintuitive. Abortion enters with the wrong sign, sometimes significantly. Lead enters positively, consistent with the Reyes/Levin view, but the effect is never significant. It is also largest in magnitude for property crime, which is the opposite of the Reyes result and inconsistent with the view that lead poisoning promotes aggression in particular.

Enforcement of prohibition is associated with greater crime in all but one regression, and most of the coefficients are significant. In particular, for overall violent crime and murder/homicide in the longer samples, the coefficients are consistently positive and significant.

As noted, one should not draw structural conclusions from these regressions, but they are not consistent with the view that standard deterrence variables, as well as other factors recently addressed in the economics of crime literature, are robust determinants of crime. At the same time, they are consistent with the view that drug prohibition enforcement plays an important role, especially for violent crime categories.

8.7 Conclusion

The economic model of crime first posited in 1968 has spurred a large literature that tries to estimate the empirically relevant determinants of crime. This chapter has examined aggregate data for first-order evidence on these determinants; these data, however, provide little confirmation that these determinants have a significant impact on long-run trends or major differences across countries. While this does not mean these theories are wrong, we believe that any estimation finding a strong effect not seen in the aggregate data deserves careful scrutiny.

We do find one theory that is consistent with the aggregate time series and cross-country data on crime: the view that enforcement of drug prohibition encourages violent dispute resolution.

This perspective is also consistent with phenomena beyond those typically labeled crime. Acts of terrorism, civil unrest, and civil war can be thought of as dispute resolution. These activities are more likely when, on the one hand, more disputes exist because of policies that provide the basis for such disputes (e.g., laws mandating one religion or language). These activities are also more likely when, on the other hand, groups that disagree with

government policies have few nonviolent mechanisms for expressing their grievances. Thus, an alternative perspective to the deterrence model, at least for much violent crime, is one that examines how policies affect both the amount and nature of dispute resolution.

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Comment

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The chapter by Dills, Miron, and Summers (hereafter "DMS") grabs attention through provocative claims about economists' ignorance when it comes to "the main factors identified in the economics of crime literature as key determinants of crime." The exception offered to this nihilistic conclusion is a finding that drug prohibition generates violence, a result that has been documented by (among others) one of the authors of this chapter, Miron.

DMS's claim that forty years of empirical research by economists has been unproductive rests less on a careful review of the literature (see, e.g., Cook 1980; Nagin 1998; Levitt and Miles 2007) than on several time-series plots of national crime rates juxtaposed with a potentially causal variable. Two of these causal variables relate to the core issue in the economics of crime—the deterrent effect of the threat of criminal sanctions—and are plausibly important: the arrest rate, and the size of the police force. In my comments I will focus on these two variables. Two other variables, the execution rate and the imprisonment rate, are relevant to deterrence but of less interest. Execution is a very rare sanction in practice and the execution rate tells us very little about the likelihood or severity of punishment for the typical murder (Cook, in press). The imprisonment rate has a theoretically ambiguous relationship to crime.[1]

The method of empirical inquiry by which DMS reach their damning conclusion is, ironically, far less sophisticated than the literature they critique. From the first econometric studies that were published on the effect of sanction threat on crime (Ehrlich 1973; Carr-Hill and Stern 1973; Sjoquist

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1. As demonstrated by Blumstein and Nagin (1978), the relationship between crime and the imprisonment rate is not monotonic. At a low probability, an increase in the probability is likely to generate an increase in the imprisonment rate; at a high probability, an increase in probability may well generate a reduction in the imprisonment rate. The logic is identical to the relationship between price and revenue along a demand curve.