LAW AND ECONOMICS SEMINAR **Professor Polinsky**
Winter Quarter 2014

January 23, 2014
4:15 - 5:45 p.m.
Stanford Law School
Room 285

"The US Crime Puzzle: A Comparative Perspective on US Crime & Punishment"

by

Holger Spamann

(Harvard Law School)

Note: **It is expected that you will have reviewed the speaker's paper before the Seminar.**

The US Crime Puzzle: A Comparative Large-N Perspective on US Crime & Punishment

Holger Spamann*
Harvard Law School

January 17, 2014

Abstract

I generate out-of-sample predictions for US crime and punishment from cross-country regressions, and compare them to the actual US values. While a few covariates can explain a large part of the international variation, they leave much US crime and punishment unexplained. Since the 1970s, the US has gone from having mostly an unexpectedly high crime rate to having mostly an unexpectedly high incarceration rate. Increased punishment may be working, but the US starting position remains a puzzle.

PRELIMINARY AND INCOMPLETE. PLEASE DO NOT CITE OR DISTRIBUTE.

1 Introduction

The United States exhibits astonishingly high crime and punishment among developed countries. For example, in 2005, the Global Burden of Disease project recorded seven murders per 100,000 inhabitants in the US, but only 0.9 in Germany (IHME 2013). At the same time, the US incarcerated 747 people per 100,000 inhabitants – the most of any country in the world –, while Germany incarcerated only 85 (ICPS 2011; data for 2009 and 2010, respectively).

*hspamann@law.harvard.edu. A predecessor of this draft circulated under the name "The Global Cross-Section of Crime and Punishment." I am grateful to Ruchir Agarwal, Lucian Bebchuk, Bernard Black, Jeffrey Fagan, Andreas Fuster, Martin Gelter, Yehonatan Givati, Edward Glaeser, Andrew Hammel, Louis Kaplow, Dan Klerman, Justin McCrary, Eduardo Morales, Nathan Nunn, Mark Ramseyer, Mark Roe, Andrei Shleifer, Tom Vogl, and workshop participants at Harvard University, the University of Texas at Austin, the 2009 Annual Meeting of the American Law and Economics Association (San Diego), and the Fourth Annual Conference on Empirical Legal Studies (Los Angeles 2009) for helpful comments and suggestions. Thanks to John van Kesteren and Gallup Europe for granting me access to the ICVS and EUICS data, respectively, to Tom Ginsburg for access to data from the Comparative Constitutions Project, and to Roy Walmsley for making early editions of the World Prison Brief available to me. I gratefully acknowledge financial support from a Terence M. Considine Fellowship in Law and Economics provided through the John M. Olin Center for Law, Economics, and Business at Harvard Law School.

Even the dramatic drop in US crime during the 1990s by about 40% (Levitt 2004) is small relative to the cross-country differences sampled in Table 1. Figure 1 shows that the US is still an outlier when considering a much larger group of countries.

Table 1: Crime and Punishment in Major Developed Countries

	US	Canada	UK	France	Germany	Japan
homicide per 100,000, 2005[a]	7.0	1.9	0.9	1.1	0.9	0.7
victimization rate (10 common crimes), 2004/5[b]	0.18	0.17	0.21	0.12	0.13	0.10
prisoners per 100,000, approx. 2009[c]	747	118	138	93	85	58
death penalty, 2012[d]	Yes	No	No	No	No	Yes

sources: [a] IHME 2013; [b]van Dijk et al. 2007; [c] ICPS 2011 (latest available); [d]Amnesty International 2013

The US combination of high crime *and* punishment up to an order of magnitude above its peers is puzzling. To be sure, a higher crime rate mechanically induces a higher incarceration rate, assuming constant expected sentences per crime (Listokin 2003). But why would the US have so much higher crime? Moreover, the criminological literature suggests that expected sentences *per crime* in the US are in fact considerably harsher than in most other countries [cite]. While this mechanically increases the incarceration rate for a given crime rate, it only deepens the puzzle concerning the crime rate itself. Harsher sentences should reduce crime through deterrence and incapacitation, as several well-identified papers demonstrate [cite. Abrams 2012 provides a survey].

Can the US outlier position be explained by other US peculiarities among developed nations, such as high rates of immigration and income inequality? In this paper, I analyze largest data set of crime and punishment around the world to argue the answer is no. US crime today, while high in the developed world, looks in fact normal given certain unusual background characteristics (notably high income inequality and demographics, including the high teen birth rate). But the US incarceration rate is over four times larger than what one would expect based on other countries' data. Figure 2 captures this result, plotting residuals from regressions of homicide and incarceration rates on country characteristics.

This means one of two things. Either US *residual* punishment does not contribute to crime control. Or the US has in fact extremely high *latent* crime, that is, even higher than the observed levels, which are already high by developed world standard. The latter explanation finds some suggestive support in the opposing trends of unexplained crime and punishment shown in figures 3 and 4. Since the 1970s, the US has gone from having mostly an unexpectedly high crime rate to having mostly an unexpectedly high incarceration rate (Figures 3 and 4). Increased punishment may be working, but then the US starting position remains a puzzle.

While most of my analysis focuses on homicides, the cross-sectional results also hold for smaller crimes covered by the five sweeps of the International Crime Victimization Survey

(ICVS), such as theft, burglary and robbery, and drug use prevalence as recorded in the UN World Drug Report 2012. I regress these variables on a US dummy and all the major variables that have been hypothesized to predict crime and punishment and that are plausibly exogenous to both. It turns out that a few covariates can explain a large part of the international variation. Conceptually, the US dummy captures the difference between the actual US numbers and out-of-sample predictions derived for the US from other countries' data.

The paper's contribution to the literature is the assembly and analysis of a large cross-country data set for crime *and* punishment in parallel.[1] As I explain in section 3, I gather the broadest possible data both in terms of country coverage and in terms of included variables. Nevertheless, like most cross-country research I confront a problem of pervasively missing data and hence limited degrees of freedom. In section 4, I explain various techniques that I use to fill in or deal with missing data. In section 2, I explain why crime and punishment must be studied in parallel but preferably not in the same regression. Crime and punishment are to some extent fungible such that one needs to study both to determine how background characteristics influence either.

In attempting to explain crime and punishment in the US, only a large-N approach can capture sufficient variation in the explanatory variables to avoid the worst forms of extrapolation. For example, the United States has by far the highest income inequality among rich countries (OECD 2013). To apply estimates of the effect of inequality from only a sample of rich countries to the US would thus necessarily extrapolate beyond the support for this variable. Moreover, most theories regarding crime and punishment have been developed and tested on essentially the same small group of rich countries. Extending the sample is important to asses the validity of these theories, or more to the point, build a reliable model to derive the US prediction.

To be sure, this extension comes at the cost of losing many details that are simply not available for larger samples. In particular, the large sample contains no information on the composition of the incarceration rate, i.e., sentence length vs. frequency of new admissions, and which crimes inmates are serving time for. Data on other dimensions of punishment, such as prison conditions, are also lacking at the large sample level. The latter only biases my results against my ultimate finding, however, as prison conditions and other non-time aspects of punishment, such as shaming or the death penalty, are by all accounts unusually harsh in the US by Western standards and generally positively correlated with the incarceration rate (e.g., Whitman 2003, 2005; Tonry and Melewski 2008). Moreover, by considering a broad spectrum of crime, I leave little room for the possibility that the results are driven by some omitted type of crime that the US would (successfully) target very severely at the

[1] Neapolitan (2001) and Ruddell (2005) also study both but in the same regression. See section 2 for a discussion of this approach.

expense of raising its incarceration rate. In any event, the present study is not a substitute for, but a complement to, the excellent small-N literature, some of which I will return to in the discussion part.

The paper is closest in spirit to McCrary and Sanga (2012). They compare *changes* in crime and punishment in 34 countries and conclude that the US shift to harsher punishment since succeeded in reducing crime relative to other countries. In general, the economic literature using comparative crime data has tended to prefer panel data with country fixed effects (FE) over purely cross-sectional estimation (e.g., Soares 2003, Lin 2007). The underlying motivation is to eliminate unobserved time-invariant heterogeneity to identify causal effects of time-varying variables. By contrast, the present paper is precisely concerned with explaining – or highlighting – the much larger differences in *levels*, and considers changes only as a second step.[2] Moreover, the present paper attempts to model differences between countries directly, thus avoiding the need to assert that countries' crime would have moved in parallel but for a change in imprisonment (cf. Durlauf 2012). Both approaches are of course complementary.

Methodologically, the paper is part of a much broader comparative literature that attempts to gain insights from "synthetic" counterfactuals constructed from comparative data. Abadie et al. (2010) formalize this method in a panel setup that can deal with unknown factor loadings and avoid extrapolation. For lack of data and a clearly defined treatment, this method is unavailable here.

2 The relationship of observed crime and punishment to criminogenity, latent crime, and punitiveness

This section reviews a very basic model of crime and punishemtn to clarify three points. First, and not surprisingly, crime and punishment are too closely related to identify "criminogenity" or "latent crime" with data on observed crime alone, or "punitiveness" with data on observed punishment alone. Second, in particular, comparing only crime rates will tend to understate differences in criminogenity if countries counter increases in criminogenity with harsher punishment. Third, "controlling for" punishment in the crime regression by adding punishment as an independent variable, and vice versa, is not a solution given the reciprocal relationship between the two. Absent credible instruments, the only way of dealing with the problem seems to be to acknowledge the limits of the reduced form estimates and to address the crime-punishment nexus as well as possible in a contextualized interpretation of the reduced form estimates.

[2] For key variables such as the incarceration rate, homicide rate, income inequality, or teen births, the cross-country standard deviation is at least twice as large as the within-country standard deviation.

2.1 Three basic connections

Crime and punishment are connected in three basic ways:

1. a mechanical relationship translating higher expected sentences p into higher incarceration rates P for a given crime rate C: $P = pC$ (Listokin 2003);

2. deterrence and incapacitation, which imply $C = f(K, p)$ with $f_1 > 0, f_2 < 0$, where K incorporates differences in "latent crime," i.e., crime at a given level of punishment;

3. a policy response $p = g(C(p), J)$ with $g_2 > 0$, (and presumably $g_1 > 0$), where J incorporates differences in "punitiveness," i.e., differences in preferred punishment at any given level of crime.

If comparative data are to shed any light on these relationships, at least their functional form should be constant across countries. In particular, if the crime-punishment elasticity $\varepsilon \equiv \frac{\partial C}{C} / \frac{\partial p}{p}$ is assumed to be constant across countries, the deterrence/incapacitation response must be of the form $f(K, p) = K \cdot p^{\varepsilon}$. The factors that vary by country i in function of some observed background factors X_i and unobserved factors μ_i and η_i are then $K_i \equiv K(X_i, \mu_i)$ and $J_i \equiv J(X_i, \eta_i)$.

2.2 Observed crime (punishment) is not necessarily the best proxy for latent crime (punitiveness)

Empirically, researchers tend to be interested in the functions K and J, but we only observe C and P (or, in smaller samples, p). Importantly, C (P) is not necessarily the best proxy for K (J). A special case of the model immediately establishes this fact.

Consider $\varepsilon = -1$ and a social welfare function $W = -C - \alpha_i p - \beta P$, i.e., society cares not only about (low) crime but also about the number of prisoners (either out of concern for the prisoners, or for mere financial reasons) and punishment per se out of "fairness" concerns (note that α_i could be negative).[3] Countries differ only in their fairness attitude towards punishment α_i. To maximize W, the policymaker chooses $p_i^* = \sqrt{\frac{K_i}{\alpha_i}}$, such that the observed $C^* = \sqrt{a_i K_i}$ and $P^* = K_i$. That is, under these assumptions, a country's latent crime K_i manifests in pure form only in the incarceration rate (!), while the crime rate is contaminated by the country's fairness attitude towards punishment, α_i.

[3] Under this simple formulation, crime could be reduced to near zero by setting maximum penalties, because the length of punishment and the reduction of crime exactly offset each other and total imprisonment is constant. In this model, the only thing that prevents society from setting such high punishment is $\alpha > 0$.

2.3 Differences in observed crime rates can under- or overestimate differences in latent crime

In general, differences in observed crime rates can over- or underestimate differences in latent crime, depending on whether they are driven by real differences in latent crime K or differences in punitiveness J. Correspondingly, differences in observed crime rates alone cannot even identify the direction of the criminogenic effects of background characteristics X.

From $C = Kp^{\varepsilon}$, we obtain for two countries i and j

$$\frac{K_i}{K_j} = \frac{C_i}{C_j}\left(\frac{p_i}{p_j}\right)^{-\varepsilon}.$$

Since $\varepsilon < 0$, this equation implies that differences in *observed* crime rates $\frac{C_i}{C_j}$ understate (overstate) differences in *latent* crime rates $\frac{K_i}{K_j}$ if the country with the higher crime rate $C_i > C_j$ also applies harsher punishment $p_i > p_j$. This will be the case when the root cause of the differences between the two countries is $K_i \neq K_j$, because the country with the higher K will be driven also to apply harsher punishment. By contrast, if the root cause is differences is in $J_i \neq J_j$, one country will apply laxer punishment and hence experience higher crime, which a comparison of observed crime rates alone might erroneously attribute to differences in latent crime. These biases are increasing in ε.

If we do not know $p_{i,j}$, we can substitute from $P = pC$ to obtain

$$\frac{K_i}{K_j} = \frac{C_i}{C_j}\left(\frac{P_i/P_j}{C_i/C_j}\right)^{-\varepsilon}.$$

The right hand side is empirically observable with the exception of ε. Abrams (2012) reviews all of the published empirical literature and argues that $\varepsilon = -0.25$ is the best estimate for the combined effect of deterrence and incapacitation, at least in the US. Using that number, a comparison of US and Canadian crime rates alone would underestimate the ratio of latent crime between these two countries by 15% or 56%, depending on whether one uses homicide or victimization rates to proxy overall crime rates.

2.4 Reduced form regressions

Finally, the reciprocal relationships between crime and punishment imply that reduced form regressions of crime C and punishment P or p, respectively, on background characteristics X does not identify $\frac{dC/dK}{dK/dX}$ and $\frac{dp/dJ}{dJ/dX}$, much less dK/dX or dJ/dX. The reason is that X enters $C = f(K(X, \mu), p)$ also through $p = g(C(p), J(X, \eta))$, and vice versa. This system of equation also implies that it is not possible to "control for" the level of crime or punishment in regressions of the other, as they are correlated with the structural equation's error term. The reduced form is the best one can do at the large sample level. This leaves open the possibility of a more nuanced interpretation of individual observations residuals using more contextual information.

3 Variables

I now describe in more detail the variables used in this paper, and, for the independent variables, the theories and prior literature motivating their inclusion.

3.1 Independent variables

The choice of four independent variables is dictated by the need to achieve considerable country coverage without sacrificing too much in terms of reliability.

3.1.1 Crime

Reliably measuring crime is notoriously difficult, since much crime is not reported. Importantly, the propensity to report crime covaries with certain variables of interest, such as the level ofdevelopment or inequality, so that official crime data will paint a very misleading comparative picture (Soares 2004; Gibson and Kim 2008). INTERPOL (1999) explicitly warns against using its data for comparative purposes.

There are three series of crime data available for large cross-sections, however, that are considered reliable, and I use all of them in this paper.

WHO/GBD and UNODC homicide rates. The first is the log of the homicide rate, because homicides are less easily concealed. There are two comparative data series in wide use, UNODC data from police statistics, and WHO Global Burden of Disease (GBD) data primarily from death classifications by medical practitioners (Newman and Howard 1999). The GBD data have recently been updated and improved by the Institute for Health Metrics and Evaluation. I use primarily these latter data because it has substantially greater country coverage ($N = 187$) than the UNODC data, which is available for at most 102 countries in any given year. On the downside, the GBD data are only available for 1990, 2005, and 2010, while the UNODC data go back to 1950 (albeit with diminishing reduced country coverage). I thus use the UNODC data when examining the stability of the estimates and the evolution of the US position in decades past. The correlation between the two log-transformed rates is 0.85.

The UNODC data present one complication. Earlier data were classified according to a different version of the International Classification of Diseases (ICD). Accordingly, the definition of "homicides" (in truth, a composite of a variety of smaller categories) is not constant in decades past. To account for this, I include dummies for each version of the ICD used.

ICVS prevalence rates for common crimes. The second reliable series of crime data come from victimization studies, i.e., representative surveys eliciting experiences of victimization by various crimes (Tonry and Farrington 2005; Lynch 2006). Standardized comparative data on ten common property and contact crimes have been collected in five sweeps of the International Crime Victims Survey between 1989 and 2005, including the European Survey on Crime and Safety (van Dijk, van Kesteren, and Smit 2007; van Kesteren 2007) (hereinafter collectively referred to as ICVS). As my interest is in country-level determinants, following Wooldridge (2003) I work with weighted country-level averages rather than individual data.[4]

The major shortcoming of the ICVS data is low coverage in any given sweep. Although 75 countries participated in at least one of the five sweeps, any given sweep covered far fewer. For example, the 2004-05 sweep contained only 27 country surveys (essentially all and only OECD countries). Consequently, papers using these measures in the past have had only about 40 observations to work with (e.g., Soares 2004). To my knowledge, I am the first to pool data from all five sweeps, including city surveys from developing countries, which gives me 75 countries or around 300,000 individual responses (after eliminating duplicates) to work with. (I take appropriate steps to adjust for the unbalanced nature of the data, see Section 5.2 below.)

I use the one-year prevalence rate of victimization by any of nine common crimes (burglary; attempted burglary; personal theft; theft of a car, theft from a car; theft of a bicycle; theft of a motorcycle; assault; and robbery), i.e., the probability of being the victim of any of these nine crimes at least once in the year before the survey.[5] This measure is commonly emphasized in the comparative literature as a proxy for overall crime (e.g., van Dijk, van Kesteren, and Smit 2007), it has sufficiently many non-zero observations to estimate country averages reliably, and its focus on less serious crimes provides a useful counterperspective to the homicide measure. Peru and Tanzania lack information on at least one of these crimes, and I omit them. This leaves me with 73 countries with observations for at least one sweep.

UNODC World Drug Report use prevalence. Much criminal law enforcement in the US over the last decades has been dedicated to the war on drugs. An estimated [X]% of US prisoners serve time for drug-related offenses [cite]. To capture this important dimension and crime and punishment, I also use data on drug use prevalance for opiates, cocaine, and ecstasy from the UNODC World Drug Report 2012. These data refer to years between 2000

[4] The ICVS supplies survey weights that neutralize over- or undersampling within countries.

[5] I do not include sexual offenses against women in this count because this question was not asked in all surveys, and in any event would presumably yield answers that are not necessarily comparable across countries.

and 2011.

Drug-related deaths To obtain a measure of more serious drug problems, I also use the GBD measure of deaths caused by drug-use disorders in 2005.

3.1.2 Punishment: Incarceration Rate

For punishment data, I focus on the log of the incarceration rate per 100,000 inhabitants. These data are very reliably measured (cf. Neapolitan 2001; Lappi-Seppälä 2008) and offer nearly universal country coverage.

My main data series is the nine World Prison Reports issued by the International Center for Prison Studies (ICPS). Data are available for 214 countries and territories.[6]

The only shortcoming of these data is that they are not available before the mid-1990s. I again use UNODC data going back to 1970 but covering far fewer countries for regressions using past decades' data. The correlation of the UNODC and ICPS log-transformed measures in overlapping periods is 0.84.

3.2 Independent variables

As independent variables, I attempt to use all of the main variables suggested in the comparative literature on crime and punishment, in as far as they are amenable to testing in the large cross-section and are not simultaneously determined with crime and punishment.[7] In particular, and subject to the previous caveat, I use all of the variables suggested in the cross-country regression literature on crime (Messner, Raffalovich and Shrock 2002; Fajnzylber, Lederman and Loayza 2002; Soares 2004; Hunt 2006; Lin 2007) and punishment (Neapolitan 2001; Jacobs and Kleban 2003; Ruddell 2005; Anckar 2006; Downes and Hansen 2006; Greenberg and West 2008), or close substitutes thereof. The theoretical literature motivating these variables is voluminous; for excellent reviews, see Neapolitan (1997), Whitman (2005), Tonry (2007), Lappi-Seppälä (2008), and Lynch and Pridemore (2011). Here I only give very brief descriptions of each variable and its possible relevance.

Some of the variables mentioned below could themselves be affected by crime and punishment rates, particularly the level of development, unemployment, income inequality, and social policy. I expect such effects to be small, but acknowledge that such concerns could mar the interpretation of the results for those variables. At least for income inequality, researchers have found that the link to crime is robust to methods controlling for potential

[6] To match the data with other variables, I merge Guernesey and Jersey into Channel Islands, and England and Wales, Northern Ireland, and Scotland into the UK.

[7] Independent variables that have been used in the comparative literature but are almost certainly simultaneously determined with crime and (official) punishment are extrajudicial killings (Neapolitan 2001), and crime and official punishment themselves.

endogeneity (Fajnzylber et al. 2002).

Not all of the variables listed below are likely to have a direct effect on both crime and punishment. They nevertheless belong into the reduced form regressions for both because they can have an indirect effect on either dependent variable through the other.

As a robustness check, I will also present regressions using gun ownership (from Small Arms Survey 2007) as a regressor in table 7. Clearly, gun ownership has been singled out as a main determinant of US violent crime. Nevertheless, I do this as a robustness check only because gun ownership is not plausibly exogenous to crime, as increased crime might lead citizens to arm themselves in defense. In any event, this would bias the coefficient on arms upwards, while I find a negative or zero coefficient.

Development. All of the aforementioned studies control for the level of development, usually operationalized as GDP per capita.[8] In fact, the impact of development is so fundamental that presumably most other theories implicitly hold the level of development constant, and I control for it in all of the regressions.[9] The level of development affects the opportunity set of potential criminals, and the institutional capacity of public law enforcement. More subtly, the level of development may also affect social structures that informally suppress or encourage crime, and steer human behavior more generally. Finally, a "civilization" effect may lead to less severe punishment in more developed societies. To account for the possibility of the latter effect, I control for the level of development non-linearly in the homicide and incarceration regressions, using both the level of GDP per capita and its natural logarithm. PPP-adjusted data come from the Penn World Tables 7.1.

Income inequality and social policy. Another major focus of the prior literature is income inequality and the policies that influence it. The economic literature on crime mostly emphasizes the effect of income inequality on the opportunity set of potential criminals (e.g., Burdett et al. 2003). In the criminological and sociological literature on punishment, income inequality is also viewed as a proxy for, and consequence of, social policies defining the relationship between the well-off and the less well-off, which are the major focus of that literature. In that view, societies that support the poor with generous welfare spending, and that support employees with protective labor regulation, are also likely to employ only moderate punishment. I control for income inequality using the Gini coefficient from the World

[8] Some authors, such as Neapolitan (2001), use instead the Human Development Index, which combines GDP per capita, life expectancy, and educational achievement; I used it in some regressions with identical results. Other authors, such as Soares (2004), separately include the level of education. I found that coefficients on a variable of primary school enrollment or adult literacy have the same sign as those for GDP per capita (less crime, more punishment) without adding explanatory power or altering the results for other variables. Since I do not see a theoretical reason for adding this separate variable, and to conserve degrees of freedom, I do not report results with this variable.

[9] Soares (2004) provides a full review of the relevant empirical literature.

Development Indicators, and for labor regulation using the World Bank's (now discontinued) Doing Business (DB) index of the ease of hiring and firing a worker for 2007.

Coverage of both measures is limited. The DB index is only available for a couple years in the 2000s. Hence I cannot fruitfully use it in the historical regressions (labor regulation seems to undergo too many legislative changes to assume stability over the course of several decades). Some Gini data go back to 1978, but they become quite sparse long before that, and many countries only have a measure for one year, usually in the late 1990s. To deal with this, I linearly interpolate data for missing years and, where necessary, using the latest or earliest measurement available. This seems appropriate because income inequality is quite stable over time. Its year-to-year autocorrelation is 0.97, and the within-country standard deviation (3.5) is only 37% of the between-country standard deviation (9.5).

In addition, in the historical regressions I use another Gini measure from the University of Texas Inequality Project (UTIP) (Galbraith & Kum 2005). The UTIP data go back further in time and are less sparse in decades past. I linearly interpolate the data for missing intervening years only.

Political structure. In the aforementioned criminological literature, differences in social policy are usually viewed in a broader context of different political systems, and classifications such as corporatism (Jacobs and Kleban 2003), social democracies vs. neoliberal systems vs. conservative corporatist systems (e.g., Cavadino and Dignan 2006a/b), or consensus vs. conflict political systems (Lappi-Seppälä 2008) are directly included in the analysis. Since these classifications are only available for relatively small groups of countries, however, I instead use a year-matched measure of proportional voting, which is often viewed as conducive to, or even a hallmark of, social democracies or consensus systems.[10] Since proportional voting only matters in democracies, I also include a democracy dummy, and "switch on" the proportional voting variable only when the dummy is equal to one. These data come from the World Bank's Database of Political Institutions. To the extent possible, I fill missing data with hand-collected data from Wikipedia.

In fact, Lin (2007) and others have suggested that democracies punish minor crimes less harshly and hence have more of it, and inversely for major crime. To control for democracies and liberty more broadly, I combine the Freedom House political rights and civil liberties scores for the relevant year, and rescale them so that higher values correspond to more freedom.[11] I interpolate data for one gap year (1981).

Finally, Jacobs and Kleban (2003) have argued that federal systems should have more

[10] I construct the measure from the World Bank's Database of Political Institutions (revision 4) using the formula of Pagano and Volpin (2005): PR - PLURALTY - HOUSESYS + 2.

[11] Lin (2007) uses the political rights score (and the civil liberties score as an instrument for it). Using only the political rights score does not change my results. Nor does using the Polity IV score instead of the Freedom House measures.

prisoners because relevant political decisions will be less remote from the population and hence more subject to populist pressures for harsh punishment. I control for federalism using a dummy variable supplied by Tom Ginsburg of the Comparative Constitutions Project.

Population structure. The last major complex of variables considered in the literature is the structure of the population. It is well known that young males are particularly prone to criminal activity (Hirschi and Gottfredson 1983), and many cross-country studies attempt to control for this. I do so using the share of 15-19 year old males in the population, as reported by the US Census International Data Base.

Hunt (2006) has drawn attention to the particular importance of young adults born to teen mothers, particularly when they reach age 25-29. I control for this using the share of children born to teen mothers out of all children born 25 years prior to the relevant year, calculated from the United Nations Demographic Yearbook Historic Supplement 1948-1997. (An alternative interpretation this variable is as a proxy for broader social dysfunctions, since it is highly correlated with the *current* share of teen births (calculated from the same sources).)

Many papers also control for the level of urbanization since crime tends to be more prevalent in cities (Glaeser and Sacerdote 1999). I do so using the percentage of the population living in urban areas in the relevant year (from World Development Indicators).

Another important aspect of population structure considered in the literature is its homogeneity or heterogeneity (e.g., Ruddell 2005). Group differences might breed conflict and hence crime, and a government dominated by one group might have less reservations about punishing members of the other group. To control for this, I use the index of ethnic fractionalization from Alesina et al. (2003), and the percentage of foreign born inhabitants (from World Development Indicators).

More specifically, many observers view the experience of slavery and its legacy of charged race relationships as a major factor of US crime and crime policy (Tonry and Melewski 2008). In an attempt to account for this, I construct a measure of slavery legacy in non-African countries as the percentage of the population descendant from former African slave exporter countries. I obtain measures of ancestry from Putterman and Weil (2009), and a list of African slave exporting countries from Nunn (2008) (counting only those that exported at least 250,000 slaves, for fear that I might otherwise end up counting non-slave migration).

Legal system. The legal system is obviously of the utmost importance for crime and punishment, as it determines the government-administered part of the latter. Comparative data on legal aspects of punishment are not available for large groups of countries. There is evidence suggesting, however, that the historic origin of a legal system may play a role in punishment. Greenberg and West (2008), using the classification of Mukherjee and Reichel

(1999), report that common law countries are significantly more likely than others (except Islamic law countries) to retain the death penalty.[12]

This ties into an important literature in economics which has documented pervasive correlations between "legal origins," i.e., common and civil law, and economic regulation and outcomes in areas ranging from investor protection to conscription (La Porta et al. 2008). While this literature has not specifically considered criminal law, it has found that common law countries tend to have more severe criminal sanctions, at least "on the books," for breaches of securities (La Porta et al. 2006) and corporate law (Djankov et al. 2008). Moreover, in a recent survey, La Porta et al. (2008:286) characterize "legal origin as a style of social control of economic life (and maybe other aspects of life as well)." Criminal law enforcement, however, is the archetype of social control in modern societies.

For continuity with the economics literature, I employ the legal origin classification from La Porta et al. (1999), maintaining socialist legal origin as a separate category to capture the special position of the transition economies with respect to crime and crime policy (cf. Neapolitan 2001; Lappi-Seppälä 2008). I added [six] jurisdictions for which data was missing.[13]

Culture and religion. Some contributions place great emphasis on cultural factors. For example, Lappi-Seppälä (2008) argues that higher levels of trust are associated with less harsh punishment practices. Unfortunately, good measures of culture are notoriously difficult to obtain, and those that exist are available only for medium sample sizes. Moreover, some measures, such as the World Value Survey measures of trust in other people and the government used by Lappi-Seppälä (2008), are likely to be simultaneously determined with crime and punishment, as more crime presumably reduces trust in other people and the government (cf. Blanco and Ruiz 2013). I therefore do not report any results with culture variables.

I do, however, report results with a closely related set of variables available for large samples, namely religion. Whitman (2005:27-8) notes that studies of the role of religion in punishment "cry out to be done." Anckar (2006) and Greenberg and West (2008) find that higher percentages of Buddhist and perhaps Muslim inhabitants are associated with a higher likelihood of retaining the death penalty, while Catholics may be associated with a lower likelihood. Given the low number of countries with sizeable groups of Buddhists, I

[12] Ruddell (2005) finds that common and civil law systems have, on average, higher incarceration rates than communist, mixed, and Islamic systems. His coefficient for common law systems is larger than for civil law systems, but he does not report tests of statistical significance of this difference. Related to legal origin, Jacobs and Kleban (2003) find higher incarceration rates in English-speaking countries, and Anckar (2006) finds that use of the death penalty in former colonies differs by the last colonizing power.

[13] The added jurisdictions are Congo-Brazzaville, French Guiana, and French Polynesia (French); the Channel Islands (Common Law); and East and West Germany prior to reunification (socialist and German, respectively).

am skeptical about the possibility of disentangling their influence on the death penalty in a cross-country regression. I focus on the main world religions, employing measures of the percentage of the population identified as Muslim, Catholic, or Protestant, respectively, from the Association of Religion Data Archives' World Religions Dataset (Maoz and Henderson 2013).

Other independent variables. Many other variables have been discussed in the theoretical literature. Only two of them, however, have found application in many empirical studies, namely the unemployment rate, and the economic growth rate. I do not use the latter because of its frequent fluctuations, which make it seem unlikely that differential growth rates could explain much of the huge cross-country differences in crime and punishment. I do use total unemployment rates from the World Development Indicators. A caveat here is that from the perspective of some criminological theories that argue for its importance, the unemployment rate is endogenous, because those theories argue that criminal punishment is used to control excess labour.

4 Regression specifications: Dealing with the degrees of freedom problem

The basic cross-sectional regression setup is straightforward. I regress the outcome variable on a US dummy and a set of controls. The US dummy captures the deviation of the US outcome from the outcome predicted by the model. The US data do not influence estimation of the rest of the model because they are absorbed by the dummy.[14]

There are, however, a numer of subtleties relating to dealing with limited degrees of freedom. I do four things to deal with the perennial degrees-of-freedom problem in cross-country regressions.

4.1 Interpolating missing values

First, I linearly interpolate data to fill in missing values of independent variables. As the independent variables are slow-moving, this should introduce only minor measurement error. Without interpolation, country coverage would be considerably smaller due to non-overlapping gaps in series of different independent variables.

In addition to the variables already mentioned above (the Gini coefficient and the Freedom House indicator), I interpolate data on religion, urbanization, and migration, which are only

[14] If there is more than one US observation in the multi-year sample, US variation over time will affect the other estimated coefficients. This is relevant only in table 5.

provided at five-year intervals, and on unemployment and the share of teen births, which have fewer and irregular gaps.

To simplify the presentation, I also interpolate the incarceration rate for some of the tables and graphs (as ICPS does, too).

4.2 Pooling observations from different years

Second, I pool observations from different years to maximize country coverage on the dependent variable when it is not available for all countries in the same year. That is, I run the main regressions either on decade means ("between" regressions) or as pooled cross-sections with annual data for the entire decade. The first approach is my favorite, as it tends to smooth out measurement error and does not require further manipulations beyond inclusion of a linear time trend.

The UNODC homicide data are not immediately comparable from one year to the next, however, as changes in the classification scheme discussed above may affect the reported rates. Similarly, some observations from the ICVS are derived from city-only rather than national surveys and survey instruments differed slightly. For these variables, I hence pursue the second approach, as it allows me to control for these effects by including dummies for classification type, survey type, etc. In that case, I also include year/sweep dummies, and I weight the data for each country by the inverse of the frequency with which it shows up in the data in order to give each country equal weight. I cluster standard errors at the country level.

4.3 Model selection

Even with interpolation and pooling, however, different sample sizes and imperfect sample overlap mean that complete data on all independent variables is available only for a relatively small number of countries, certainly small relative to the number of potentially relevant regressors (cf. models 1 and 5 of table 4, which include all regressors). Thus the third thing I do is model selection, both naive and systematic.

4.3.1 Naive model selection

The naive approach is to focus initially on small, related blocks of explanatory variables (models 1-5 of tables 2-4). In a second step, I run against one another those variables that cleared the first hurdle (defined as a t-statistic of at least 1.64) (model 6 of tables 2-4). In this regard, I treat the two legal origin dummies, the three religious group variables, and the democracy and proportional voting variables, respectively, as inseparable because omitting one of them would change the reference category for the other. At the bottom of each table, I report F-tests for the joint null for those blocks in addition to individual coefficients and t-statistics.

The naive approach is reasonable to the extent one believes that any relationship of first-order importance should manifest in a simple setup controlling only for the level of development and the most obvious confounding factors. One may also view these naive regressions as an exploratory analysis. But as is well known, the naive approach is statistically problematic. It may lead to omitted variable bias, overfitting, and understatement of standard errors.

4.3.2 Post-LASSO

As a rigorous alternative, I present estimates using the Post-LASSO method for model selection and inference of Belloni et al. (2012) in models 2 and 6 of table 5. This method uses the LASSO procedure for the selection of control variables, and then obtains a point estimate and standard error for the "treatment effect" in a regression of the outcome on treatment and selected controls. This method produces valid standard errors (only) for the treatment on the assumption that the correct model is approximately sparse (i.e., it contains only few regressors, even if their identity is initially unknown).[15] In the present context, the "treatment" is the US effect.

It is debatable whether the appropriate model for crime and punishment is approximately sparse. The true model may very well be very complex, certainly relative to the 60 or so observations that enter the Post-LASSO estimation here. Moreover, data for the remaining 120 countries may not be missing completely at random. This would induce bias that the Post-LASSO cannot address.

4.4 Missing data techniques: MI and FIML

I therefore confront the missing data problem head on with multiple imputation (MI) and full-information maximum likelihood (FIML) in models 3-4 and 7-8 of table 5, my preferred specifications. These techniques assume that the data are missing at random (MAR) conditional on the non-missing data. This is plausible as the main reason for missing data is presumably poverty, which I control for using GDP and log GDP, as well as perhaps some political variables that I also control for. In any event, if MAR fails, the naive and Post-LASSO model selection approaches that do not address the issue at all will do even worse (Little and Rubin 2002). Similarly, while MI and FIML with arbitrarily missing data patterns technically rely on multivariate normal distributions, they typically handle deviations from this assumption better than the alternatives (Graham 2009).

[15] The standard errors for the controls themselves, while shown in table 5, are not statistically valid.

I implement MI and FIML using STATA's MI and SEM routines, respectively. I impute 100 samples each for the homicide and incarceration regressions using chained linear regression equations and bootstrapped samples. I restrict the sample to observations where the dependent variable is observed (that is, I do not impute the dependent variable, although I do use it for imputation of others). To avoid the most severe complications from non-normality, I exclude a very small number of observations for which categorical variables (legal origin and democracy) are missing.

5 Results

5.1 Naive model selection with missing data

Tables 2 through 4 present the basic results from naive model selection for the log homicide rate (GBD, 2005), the victimization rate by one of the nine common crimes (ICVS, 1989-2005), and the log incarceration rate (ICPS, pooled 2000-2010). Three features stand out.

First, the explanatory power of the regressors is quite high. To be sure, the very high F-statistic and R^2 (of 0.61 or higher) in models 6 overstates the goodness of fit because of the naive model selection. But even in the other regressions the R^2 is generally quite high given the small number of regressors, and the joint null is generally rejected.[16] The regressors, developed in work on much smaller samples, thus pass their out-of-sample test.

Second, the US victimization rate, while high, is indistinguishable from the cross-country prediction. By contrast, homicide rates are about twice as high as predicted, and incarceration rates even five times as high.

Third, the number of (independent) observations drops dramatically as more regressors are included. For example, the basic incarceration regressions has 184 observations, but this number drops to 70 by the time all the "significant" regressors are included. The problem here is non-overlapping missingness, such that each additional variable eliminates more observations from the sample. This not only reduces power but can potentially introduce severe biases. We must therefore consider more principled approaches.

5.2 PLASSO, MI, and FIML

Table 5 shows these favored estimates for both the homicide rate and the incarceration rate. As a baseline, it first presents the full OLS results with case-wise deletion, i.e., the results from including all regressors in a simple regression and excluding all observations that have missing data for at least one variable. The sample shrinks to 61 observations, and standard errors are large.

[16] The R^2 overstates the fit slightly because it counts the US as fully explained. But this effect is small, and does not affect the F-statistic.

PLASSO reduces the number of regressors drastically, but probably too drastically. The problem is presumably that PLASSO selects regressors from two first stage regressions. Here one of these, namely the one of the US dummy on the controls, will hardly select any regressors. In any event, PLASSO cannot address the bias from non-random missingness, and hardly moves the US estimate compared to OLS.

MI and FIML do address missingness assuming MAR, and they reduce the US coefficient considerably for the homicide rate. To be sure, the difference is not statistically significant. But the low coefficient in these preferred specifications just reinforces the result from OLS and PLASSO that the US is not statistically distinguishable from the model prediction for the homicide rate. By contrast, the US incarceration rate is estimated to be at least four times higher than the prediction, and the 95% confidence interval lower bound is still more than 30% above the prediction.

[discussion of F-tests and so on to come]

6 Discussion

[to be added. Major points only]

- Is it because of the war on drugs? No, or if it is, then we are losing it. See table 6 [naively selected models – MI/FIML estimates to come]

- Is it because of the availability of firearms? No – see table 7 [again, MI/FIML to come]. This result is particularly striking because any endogeneity bias (when crime rises, people buy more guns to defend themselves) would bias the arms coefficient upwards, not downwards.

- Can there be features outside the regression model? Absolutely. But the results are still important to tell us where (not) to look.

References

1. Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. 2010. Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. *Journal of the American Statistical Association* 105:493-505.

2. Abrams, David. 2012. The Imprisonner's Dilemma: A Cost-Benefit Approach to Incarceration. *Iowa Law Review* 98:905-969.

3. Alesina, Alberto, Arnaud Devleeschauwer, William Easterly, Sergio Kurlat, and Romain Wacziarg. 2003. Fractionalization. *Journal of Economic Growth* 8:155-194.

4. Anckar, Carsten. 2006. *Determinants of the Death Penalty - A comparative study of the world.* London and New York: Routledge.

5. Belloni, Alexandre, Victor Chernozhukov, and Christian Hansen. 2012. Inference on Treatment Effects after Selection amongst High-Dimensional Controls. Working paper.

6. Blanco, Luisa, and Isabel Ruiz. 2013. The Impact of Crime and Insecurity on Trust in Democracy and Institutions. *American Economics Review: Papers & Proceedings* 103:284-288.

7. Blumstein, Alfred, Michael Tonry, and Asheley van Ness. 2005. Cross-National Measures of Punitiveness. In *Crime and Punishment in Western Countries, 1980-1999*, ed. Michael Tonry and David P. Farrington, 347-376. Crime and Justice: A Review of Research 33. Chicago: University of Chicago Press.

8. Burdett, Kenneth, Ricardo Lagos, and Randall Wright. 2003. Crime, Inequality, and Unemployment. *American Economic Review* 93:1764-1777.

9. Cavadino, Michael, and James Dignan. 2006a. *Penal Systems: A Comparative Approach.* London, Thousand Oaks, and New Delhi: SAGE.

10. ——. 2006b. Penal Policy and Political Economy. *Criminology & Criminal Justice* 6:435-456.

11. Clear, Todd R. 2008. The Effects of High Imprisonment Rates on Communities. *Crime and Justice: A Review of Research* 37:97-132.

12. Deininger, Klaus, and Lyn Squire. 1996. A New Data Set Measuring Income Inequality. *World Bank Economic Review* 10:565-591.

13. Djankov, Simeon, Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer. The law and economics of self-dealing. *Journal of Financial Economics* 88:430-465.

14. Downes, David, and Kirstine Hansen. 2006. Welfare and Punishment in Comparative Perspective. In *Perspectives on Punishment*, ed. Sarah Armstrong and Lesley McAra, 133-154. Oxford: Oxford University Press.

15. Durlauf, Steven. 2012. Comment. *Cato Papers on Public Policy*, 13:195-201.

16. Fajnzylber, Pablo, Daniel Lederman, and Norman Loayza. 2002. Inequality and Violent Crime. *Journal of Law and Economics* 45:1-40.

17. Galbraith, James K., and Hyunsub Kim. 2005. Estimating the Inequality of Household Incomes: A Statistical Approach to the Creation of a Dense and Consistent Global Data Set. *Review of Income and Wealth* 51:115-143.

18. Gibson, John, and Bonggeun Kim. 2008. The effect of reporting errors on the cross-country relationship between inequality and crime. *Journal of Development Economics* 87:247-254.

19. Graham, John. 2009. Missing Data Analysis: Making it Work in the Real World. *Annual Review of Psychology* 60:549-576.

20. Greenberg, David F., and Valerie West. 2008. Siting the Death Penalty Internationally. *Law & Social Inquiry* 33:295-343.

21. Gwartney, James, and Robert Lawson, with Russell S. Sobel and Peter T. Leeson. 2007. *Economic Freedom of the World: 2007 Annual Report.* Vancouver, B.C.: Fraser Institute. Data retrieved from www.freetheworld.com.

22. Hirschi, Travis and Michael Gottfredson. 1983. Age and the Explanation of Crime. *American Journal of Sociology* 89:552-584.

23. Hunt, Jennifer. 2006. Do teen births keep American crime high? *Journal of Law and Economics* 49:533-566.

24. International Center for Prison Studies. 2008. World Prison Brief. http://www.kcl.ac.uk/depsta/law/research/icps/worldbrief/index.php?search=All, visited 4/24/08.

25. INTERPOL. 1999. *International Crime Statistics for 1999.* Saint Cloud: INTERPOL.

26. Klerman, Daniel, Paul Mahoney, Holger Spamann, and Mark Weinstein. 2011. Legal Origin or Colonial History? *Journal of Legal Analysis* 3(2).

27. La Porta, Rafael, Florencio Lopez-de-Silanes, and Andrei Shleifer. 2006. What works in securities laws? *Journal of Finance* 61:1-32.

28. ________. 2008. The Economic Consequences of Legal Origins. *Journal of Economic Literature* 46:285-332.

29. ________, and Robert Vishny. 1999. The Quality of Government. *Journal of Law, Economics, and Organization* 15:222-279.

30. Lappi-Seppälä, Tapio. 2008. Trust, Welfare, and Political Culture: Explaining Differences in National Penal Policies. *Crime and Justice: A Review of Research* 37:313-387.

31. Levitt, Steven D. 2004. Understanding Why Crime Fell in the 1990s: Four Factors that Explain the Decline and Six that Do Not. *Journal of Economic Perspectives* 18:163-190.

32. Lin, Ming-Jen. 2007. Does democracy increase crime? The evidence from international data. *Journal of Comparative Economics* 35:467-483.

33. Listokin, Yair. 2003. Does more crime mean more prisoners? *Journal of Law and Economics* 46:181-206.

34. Little, Roderick, and Donald Rubin. 2002. Statistical Analysis with Missing Data. 2nd ed. Hoboken, NJ: Wiley.

35. Lynch, James P. 2006. Problems and Promise of Victimization Surveys for Cross-National Research. In *Crime and Justice: A Review of Research* 34, ed. Michael Tonry, 229-287. Chicago: University of Chicago Press.

36. Lynch, James P., and William A. Pridemore. 2011. Crime in International Perspective. In *Crime and Public Policy*, ed. James Q. Wilson and Joan Petersilia, 5-52.

37. Maoz, Zeev, and Errol A. Henderson. 2013. The World Religion Dataset, 1945-2010: Logic, Estimates, and Trends. *International Interactions* 39(3).

38. McCrary, Justin, and Sarath Sanga. 2012. General Equilibrium Effects of Prison on Crime: Evidence from International Comparisons. *Cato Papers on Public Policy*, 13:165-193.

39. Messner, Steven F., Lawrence E. Raffalovich, and Peter Shrock. 2002. Reassessing the Cross-National Relationship between Income Inequality and Homicide Rates: Implications of Data Quality Control in the Measurement of Income Distribution. *Journal of Quantitative Criminology* 18:377-395.

40. Mukherjee, Satyanshu, and Philip Reichel. 1999. Bringing to Justice. In *Global Report on Crime and Justice*, ed. Graeme Newman, 65-88. New York and Oxford: Oxford University Press.

41. Murray, Joseph, and David P. Farrington. 2008. The Effects of Parental Imprisonment on Children. *Crime and Justice: A Review of Research* 37:133-206.

42. Neapolitan, Jerome L. 1997. *Cross-National Crime: A Research Review and Sourcebook*. Westport CT and London: Greenwood Press.

43. ________. 2001. An Examination of Cross-National Variation in Punitiveness. *International Journal of Offender Therapy and Comparative Criminology* 45:691-710.

44. Newman, Graeme, and Gregory J. Howard. 1999. Introduction: Data sources and their use. In *Global Report on Crime and Justice*, ed. Graeme Newman, 1-23. New York and Oxford: Oxford University Press.

45. Nunn, Nathan. 2008. The Long-Term Effects of Africa's Slave Trades. *Quarterly Journal of Economics* 123:139-176.

46. OECD. 2013. Crisis squeezes income and puts pressure on inequality and poverty. Available at http://www.oecd.org/els/soc/OECD2013-Inequality-and-Poverty-8p.pdf, visited 6/8/2013.

47. Pagano, Marco, and Paolo F. Volpin. 2005. The Political Economy of Corporate Governance. *American Economic Review* 95:1005-1030.

48. PEW Center on the States. 2009. One in 31: The Long Reach of American Corrections. Washington, DC: The Pew Charitable Trusts (March).

49. Putterman, Louis, and David N. Weil. 2009. World Migration Matrix 1.1. Available at http://www.econ.brown.edu/fac/louis_putterman/world%20migration%20matrix.htm, visited 4/10/2013.

50. Ruddell, Rick. 2005. Social Disruption, State Priorities, and Minority Threat. *Punishment & Society* 7:7-28.

51. Soares, Rodrigo R. 2004. Development, crime, and punishment: accounting for the international difference in crime rates. *Journal of Development Economics* 73:155-184.

52. ________. 2004b. The welfare cost of violence across countries. *Journal of Health Economics* 25:821-846.

53. Steinhauer, Jennifer. 2009. To Cut Costs, States Relax Prison Policies. *New York Times*, March 25, 2009.

54. Tonry, Michael. 2007. Determinants of Penal Policies. In *Crime, Punishment, and Politics in Comparative Perspective*, ed. Michael Tonry, 1-48. Crime and Justice: A Review of Research 36. Chicago: University of Chicago Press.

55. ________ and David P. Farrington. 2005. Punishment and Crime across Space and Time. In *Crime and Punishment in Western Countries, 1980-1999*, ed. Michael Tonry and David P. Farrington, 1-39. Crime and Justice: A Review of Research 33. Chicago: University of Chicago Press.

56. ________ and Matthew Melewski. 2008. The Malign Effect of Drug and Crime Control Policies on Black Americans. *Crime and Justice: A Review of Research* 37:1-44

57. van Dijk, Jan, John van Kesteren, and Paul Smit. 2007. *Criminal Victimization in International Perspective: Key findings from the 2004-2005 ICVS and EU ICS.* The Hague: WODC.

58. van Kesteren, John. 2007. Integrated Database from the International Crime Victim Surveys (ICVS) 1989-2005, Data and Codebook. Tilburg: INTERVICT. Available at http://easy.dans.knaw.nl., database ID P1749.

59. Whitman, James Q. 2003. *Harsh Justice: Criminal Punishment and the Widening Divide between America and Europe.* Oxford and New York: Oxford University Press.

60. ——. 2005. The Comparative Study of Criminal Punishment. *Annual Review of Law and Social Science* 1:17-34.

61. Wooldridge, Jeffrey M. Cluster-Sample Methods in Applied Econometrics. *American Economic Review (papers & proceedings)* 93:133-138.

62. World Health Organization. 2009. Mortality and Burden of Disease Estimates for WHO Member States in 2004. Available at http://www.who.int, visited 5/05/2009.

63. Young, Warren, and Mark Brown. 1993. Cross-National Comparisons of Imprisonment. In *Crime and Justice: A Review of Research*, ed. Michael Tonry, 17:1-45. Chicago: University of Chicago Press.

Figure 1: Homicide and incarceration: raw

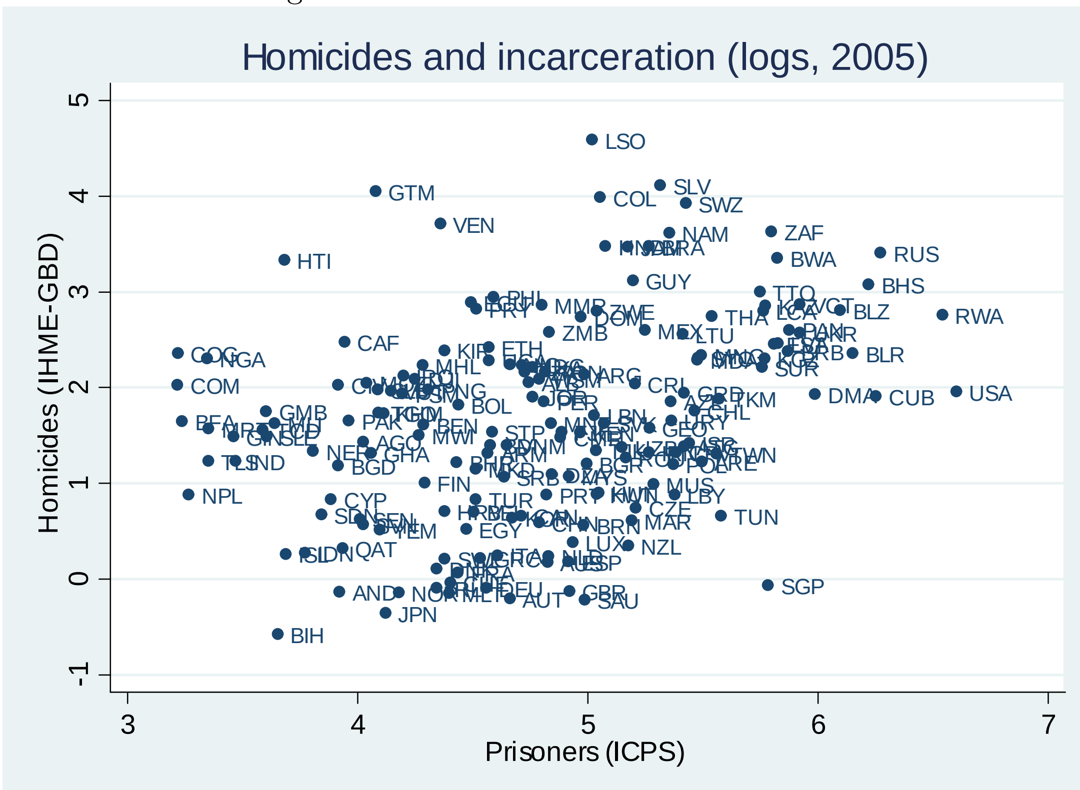

Figure 2: Homicide and incarceration: residuals

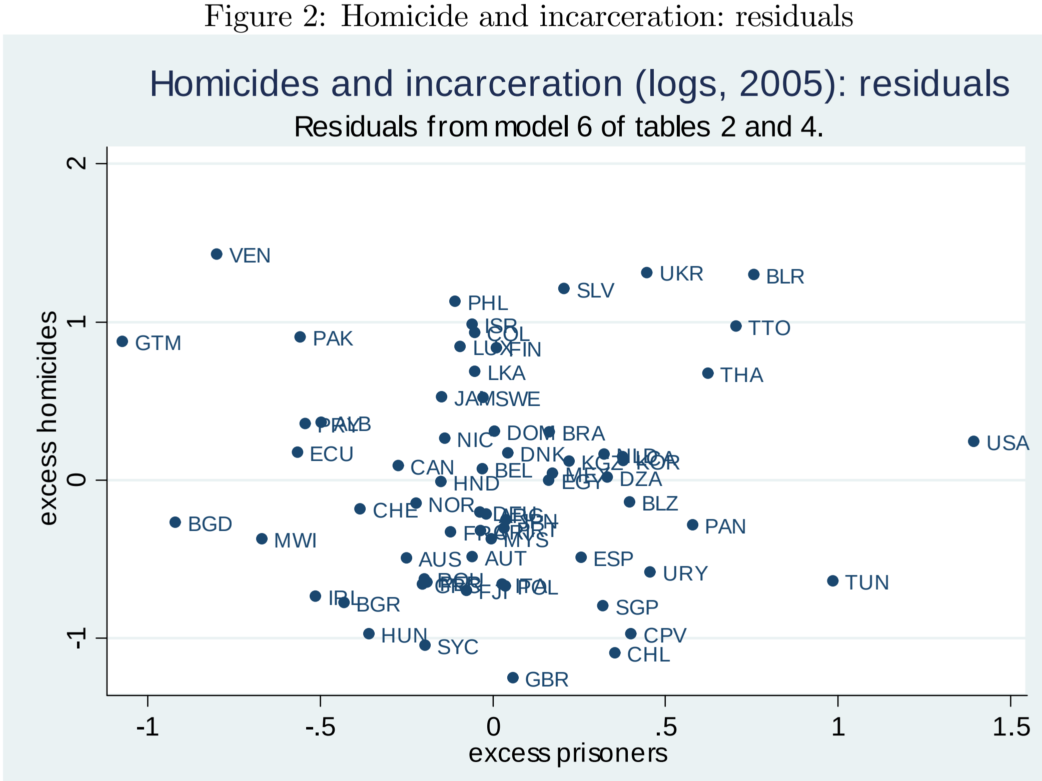

Figure 3: US excess crime over time

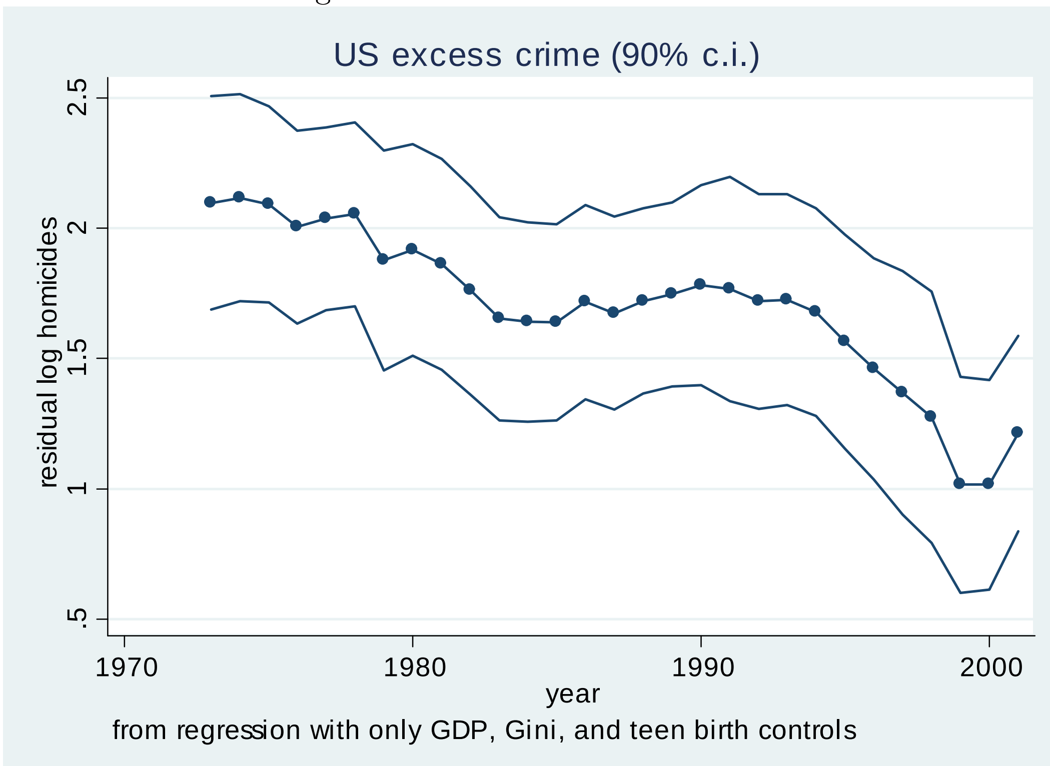

Figure 4: US excess incarceration over time

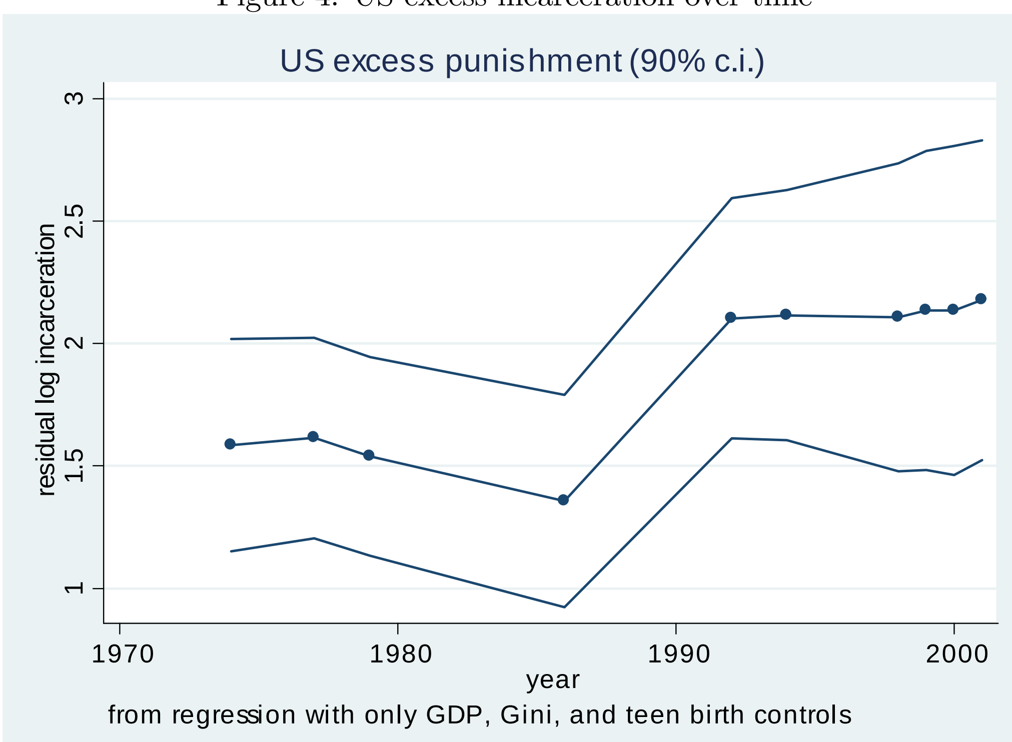

Table 2: ln(Violent deaths per 100,000) 2005 (GBD 2013)

	(1)	(2)	(3)	(4)	(5)	(6)
USA dummy	1.46***	0.98***	1.18***	0.92***	0.93***	0.84**
	(0.18)	(0.26)	(0.27)	(0.24)	(0.29)	(0.36)
ln(GDP per capita)	0.088	0.041	0.084	0.13	0.034	0.13
	(0.085)	(0.10)	(0.093)	(0.25)	(0.18)	(0.26)
GDP per capita	-0.044***	-0.042***	-0.044***	-0.026	-0.036**	-0.024
	(0.0095)	(0.012)	(0.010)	(0.016)	(0.018)	(0.019)
English legal origin		0.30*				-0.28
		(0.18)				(0.22)
Socialist legal origin		0.20				0.43
		(0.21)				(0.26)
Protestant pop.		0.62				0.14
		(0.40)				(0.49)
Catholic pop.		0.76**				-0.21
		(0.30)				(0.30)
Muslim pop.		-0.34				-1.03*
		(0.25)				(0.55)
Ethnic frac.		0.63*				0.92**
		(0.35)				(0.42)
Former slaves		0.93*				0.070
		(0.56)				(1.64)
Freedom			0.28			
			(0.31)			
Democracy			0.025			
			(0.19)			
Prop. voting			-0.091			
			(0.066)			
Federal state			0.11			
			(0.21)			
Immigrants/pop.				0.52		
				(0.77)		
Urban/pop.				0.20		
				(0.60)		
Males 15-19/pop.				51.4***		43.7*
				(16.5)		(23.6)
Teen/total births (t-25)				6.22***		3.14
				(1.44)		(2.53)
Gini					0.061***	0.037*
					(0.0083)	(0.021)
Difficulty firing worker					-0.93**	-0.044
					(0.38)	(0.46)
Unemployment rate					-0.0084	
					(0.013)	
R^2	0.24	0.41	0.26	0.62	0.54	0.79
Observations	183	158	183	83	113	64
Extra controls: joint F		5.57	0.85	13.13	22.22	7.77
Extra controls: joint p	.	0.00	0.49	0.00	0.00	0.00

OLS regressions with a constant

Robust standard errors in parentheses

Table 3: Victimization from 9 common crimes (ICVS, all 5 sweeps)

	(1)	(2)	(3)	(4)	(5)	(6)
USA dummy	0.029***	-0.046	0.036	-0.0027	-0.025	0.0056
	(0.0092)	(0.041)	(0.024)	(0.026)	(0.021)	(0.023)
ln(GDP per capita)	-0.036**	-0.031**	-0.030	-0.031	-0.035**	-0.014
	(0.015)	(0.014)	(0.021)	(0.039)	(0.016)	(0.016)
GDP per capita	0.00072	0.00028	0.00018	0.0014	0.0010	0.00028
	(0.00092)	(0.00090)	(0.00098)	(0.0015)	(0.00081)	(0.00079)
English legal origin		0.037				
		(0.024)				
Socialist legal origin		-0.0077				
		(0.025)				
Protestant pop.		0.0065				-0.023
		(0.033)				(0.029)
Catholic pop.		-0.014				-0.033
		(0.033)				(0.028)
Muslim pop.		-0.092*				-0.068
		(0.051)				(0.041)
Ethnic frac.		0.011				
		(0.040)				
Former slaves		0.80				
		(0.58)				
Freedom			0.064			
			(0.065)			
Democracy			-0.083*			-0.083**
			(0.044)			(0.035)
Prop. voting			0.020**			0.023***
			(0.0092)			(0.0075)
Federal state			0.052**			0.035*
			(0.023)			(0.019)
Immigrants/pop.				-0.085		
				(0.14)		
Urban/pop.				0.16		
				(0.11)		
Males 15-19/pop.				1.62		
				(1.81)		
Teen/total births (t-25)				0.34		
				(0.24)		
Gini					0.0026***	0.0030***
					(0.00098)	(0.00084)
Difficulty firing worker					-0.076*	-0.058
					(0.042)	(0.039)
Unemployment rate					-0.00021	
					(0.0016)	
Constant	0.25***	0.24***	0.22***	-0.014	0.19***	0.17***
	(0.035)	(0.045)	(0.043)	(0.13)	(0.057)	(0.046)
R^2	0.39	0.50	0.48	0.47	0.53	0.61
Clusters (Countries)	73	70	70	44	62	67
Observations	161	153	153	96	140	148
Extra controls: joint F		2.1	2.6	1.2	3.1	3.7
Extra controls: joint [illegible]		0.05	0.04	0.31	[illegible]	[illegible]

Table 4: ln(Prisoners per 100,000) 2000-2010 (ICPS)

	(1)	(2)	(3)	(4)	(5)	(6)
USA dummy	1.69**	1.65**	1.82***	1.63***	1.62***	1.50***
	(0.67)	(0.64)	(0.66)	(0.50)	(0.58)	(0.41)
ln(GDP per capita)	0.50***	0.40***	0.53***	0.36***	0.46***	0.38***
	(0.068)	(0.078)	(0.072)	(0.13)	(0.086)	(0.10)
GDP per capita	-0.030***	-0.024***	-0.034***	-0.027***	-0.034***	-0.022**
	(0.0061)	(0.0066)	(0.0059)	(0.0089)	(0.0085)	(0.0086)
English legal origin		0.18				0.23*
		(0.13)				(0.13)
Socialist legal origin		0.48***				0.86***
		(0.16)				(0.18)
Protestant pop.		-0.17				
		(0.36)				
Catholic pop.		0.033				
		(0.23)				
Muslim pop.		-0.25				
		(0.20)				
Ethnic frac.		0.22				
		(0.23)				
Former slaves		0.56				
		(0.38)				
Freedom			0.029			
			(0.22)			
Democracy			-0.23			
			(0.16)			
Prop. voting			-0.052			
			(0.048)			
Federal state			-0.11			
			(0.13)			
Immigrants/pop.				1.63***		1.45**
				(0.50)		(0.63)
Urban/pop.				-0.087		
				(0.31)		
Males 15-19/pop.				2.89		
				(8.88)		
Teen/total births (t-25)				3.86***		1.39
				(0.86)		(1.04)
Gini					0.017***	0.027***
					(0.0062)	(0.0078)
Difficulty firing worker					-0.59**	0.080
					(0.23)	(0.21)
Unemployment rate					-0.00030	
					(0.0084)	
Constant	9.78	21.1	19.5	-123.1	53.9	-65.4
	(78.7)	(93.7)	(75.6)	(75.1)	(79.3)	(69.5)
R-sq	0.27	0.35	0.32	0.45	0.37	0.62
Observations	826	699	786	431	545	346
Countries	184	153	178	91	118	70
Extra controls: joint F		2.73	2.17	9.13	5.16	7.72

Table 5: Better methods to deal with small N / missing data

	Homicide rate (2005)				Incarceration rate (2005)			
	OLS	PLASSO	MI	FIML	OLS	PLASSO	MI	FIML
USA dummy	0.828	1.057	0.506	0.494	1.554***	1.711**	1.467*	1.409**
	(1.31)	(1.49)	(0.68)	(0.70)	(3.65)	(3.32)	(2.56)	(2.61)
GDP per capita	0.00151	-0.0229*	-0.0185	-0.0186	-0.00893		-0.0116	-0.00837
	(0.08)	(-2.56)	(-1.69)	(-1.66)	(-0.73)		(-1.33)	(-0.87)
ln(GDP per capita)	-0.121		0.102	0.0914	0.225		0.404***	0.402***
	(-0.46)		(0.78)	(0.78)	(1.28)		(3.67)	(3.74)
English LO	-0.301		-0.0658	-0.0787	-0.0952		0.0223	-0.00723
	(-1.13)		(-0.35)	(-0.45)	(-0.53)		(0.14)	(-0.05)
Socialist LO	0.306		0.167	0.152	0.613**		0.428*	0.387
	(0.98)		(0.63)	(0.62)	(2.91)		(2.02)	(1.95)
Protestant	0.103		0.173	0.152	-0.412		-0.671*	-0.745*
	(0.21)		(0.42)	(0.39)	(-1.25)		(-2.04)	(-2.46)
Catholic	-0.368		0.160	0.0745	-0.563		-0.317	-0.400
	(-0.87)		(0.56)	(0.28)	(-1.97)		(-1.30)	(-1.83)
Muslim	-1.886**		-0.594	-0.643*	0.0924		-0.412	-0.513
	(-2.99)		(-1.67)	(-2.04)	(0.22)		(-1.15)	(-1.81)
Ethnic frac.	0.460		0.445	0.462	0.452		0.220	0.231
	(1.06)		(1.49)	(1.65)	(1.55)		(0.88)	(1.02)
Former slaves	0.759		-0.596	-0.907	0.0755		-0.543	-1.098
	(0.59)		(-0.56)	(-0.93)	(0.09)		(-0.57)	(-1.24)
Freedom	-1.485		-0.430	-0.402	0.983		0.184	0.168
	(-1.83)		(-1.06)	(-1.11)	(1.79)		(0.51)	(0.55)
Democracy	0.842*		0.358	0.366	-0.510		0.0206	0.0577
	(2.18)		(1.37)	(1.57)	(-1.95)		(0.11)	(0.34)
Prop. voting	0.0195		-0.0292	-0.0360	-0.0420		-0.0851	-0.0952*
	(0.25)		(-0.47)	(-0.63)	(-0.80)		(-1.73)	(-1.99)
Federal	0.192		0.0740	0.0913	-0.133		-0.151	-0.143
	(0.94)		(0.45)	(0.58)	(-0.96)		(-1.16)	(-1.17)
Immigrants	-0.00682		0.674	0.604	1.461		0.156	0.0826
	(-0.00)		(0.81)	(0.79)	(1.33)		(0.24)	(0.14)
Urban	-0.812		0.0630	0.120	-0.0763		0.488	0.491
	(-1.40)		(0.14)	(0.30)	(-0.19)		(1.37)	(1.50)
Males 15-19	41.41	26.09	31.54*	32.14*	6.060		21.44	25.77*
	(1.88)	(1.78)	(2.18)	(2.50)	(0.41)		(1.97)	(2.46)
Teen births	2.384		4.564	4.951*	1.295		4.072*	5.001**
	(1.22)		(1.89)	(2.30)	(0.98)		(2.09)	(2.82)
Gini	0.0457*	0.0654***	0.0271	0.0272*	0.0343*		0.00477	0.00258
	(2.30)	(5.33)	(1.83)	(2.17)	(2.55)		(0.42)	(0.25)
D. firing	-0.0716		-0.439	-0.435	0.139		0.0307	0.0382
	(-0.21)		(-1.38)	(-1.54)	(0.60)		(0.12)	(0.16)
Unemployment	0.0401		0.0118	0.0168	0.00712		0.00327	0.0120
	(1.27)		(0.57)	(0.88)	(0.33)		(0.15)	(0.61)
Observations	61	61	186	186	61	61	172	172

t statistics in parentheses

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table 6: Drug use prevalence (World Drug Report 2012) / deaths (GBD 2013)

	(1) Cocaine	(2) Ecstasy	(3) Opiates	(4) ln(deaths)
USA dummy	0.92***	-0.16	0.19	0.39
	(0.31)	(0.36)	(0.16)	(0.24)
ln(GDP per capita)	0.070	-0.18	0.054	0.51***
	(0.17)	(0.18)	(0.063)	(0.16)
GDP per capita	0.0060	0.031**	-0.0081	0.0027
	(0.0087)	(0.015)	(0.0062)	(0.011)
English legal origin	0.43*	0.74***	0.21	0.36
	(0.25)	(0.25)	(0.15)	(0.23)
Socialist legal origin	-0.16	0.88***	0.29*	0.50*
	(0.22)	(0.22)	(0.16)	(0.27)
Protestant pop.	-0.52	-0.56	-0.015	1.67***
	(0.41)	(0.39)	(0.35)	(0.47)
Catholic pop.	0.37	0.36	-0.13	0.29
	(0.24)	(0.26)	(0.20)	(0.33)
Muslim pop.	-0.18	0.55*	0.16	0.47
	(0.24)	(0.31)	(0.22)	(0.36)
Democracy	0.32	0.069	0.19	0.17
	(0.23)	(0.15)	(0.12)	(0.26)
Prop. voting	0.0016	0.014	-0.042	-0.018
	(0.060)	(0.063)	(0.039)	(0.061)
Urban/pop.	0.48	1.09**	0.28	-0.54
	(0.55)	(0.49)	(0.27)	(0.54)
Unemployment rate	0.016	-0.0050	-0.0066	0.013
	(0.012)	(0.010)	(0.0067)	(0.011)
R^2	0.56	0.54	0.44	0.45
Observations	78	81	72	131

OLS regressions with year dummies (1-3) / constant (4)

Robust standard errors in parentheses

${}^{*}p < .1$, ${}^{**}p < .05$, ${}^{***}p < .01$

Table 7: Crime regressions with control for firearms per capita

	Homicides		Assault		Robbery
	(1)	(2)	(3)	(4)	(5)
USA dummy	2.52***	1.32***	0.0013	-0.0050	0.014
	(0.27)	(0.36)	(0.012)	(0.012)	(0.012)
Arms/pop. (2004)	-1.68***	-0.51	0.018	0.0058	-0.012
	(0.38)	(0.53)	(0.018)	(0.020)	(0.018)
ln(GDP per capita)	0.049	0.11	-0.014***	-0.0031	-0.0035
	(0.069)	(0.17)	(0.0045)	(0.0075)	(0.0044)
GDP per capita	-0.030***	-0.027***	0.00067*	0.00015	-0.00047
	(0.0081)	(0.010)	(0.00034)	(0.00020)	(0.00034)
English legal origin		-0.15		0.030***	
		(0.15)		(0.0057)	
Socialist legal origin		0.45**		0.0067	
		(0.18)		(0.0070)	
Protestant pop.		0.21		0.0080	
		(0.31)		(0.0084)	
Catholic pop.		0.072		-0.0069	
		(0.18)		(0.0069)	
Muslim pop.		-0.98***		-0.0038	
		(0.29)		(0.0097)	
Ethnic frac.		1.05***			
		(0.27)			
Former slaves		0.11			
		(0.97)			
Males 15-19/pop.		36.8***			
		(11.7)			
Teen/total births (t-25)		2.67*		-0.023	
		(1.60)		(0.035)	
Gini		0.028**		0.00061	
		(0.012)		(0.00058)	
Difficulty firing worker		0.13		0.0022	
		(0.26)		(0.011)	
Democracy				-0.0044	
				(0.0061)	
Prop. voting				0.0055**	
				(0.0026)	
Federal state					
Urban/pop.					
Constant	2.01***	-1.56**	0.051***	0.0038	0.034***
	(0.055)	(0.65)	(0.0091)	(0.027)	(0.0100)
R^2	0.22	0.76	0.27	0.47	0.21
Clusters			73	41	73
Observations	499	163	163	102	163

Homicides (2005, GBD): OLS regressions with a constant, robust standard errors in parentheses

Assault and robbery (1989-2005, ICVS): Pooled OLS with constant, sweep, and survey dummies; countries equally weighte

$^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$