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

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Abstract

This paper compares actual US crime and incarceration rates to predicted rates from cross-country regressions. Global cross-country regressions of crime and incarceration on background characteristics explain much of the variation between other countries. But the estimated models predict only one-fourth of US incarceration and not all of US crime. The coincidence of the non-negative US crime residuals with the very large positive US incarceration residual constitutes a puzzle. The two pieces fit together only if the residual US incarceration does not contribute to a reduction in crime, except to the extent an omitted criminogenic factor pushes up US crime. The paper quantifies this relationship. Drawing on additional evidence from comparative and US-specific data, it argues that the puzzle's most plausible solution combines low effectiveness of mass incarceration with omitted criminogenic factors such as US neighborhood segregation.

1 Introduction

US crime rates are high relative to peer countries. Within the OECD, the US is a high outlier for homicides and serious drug abuse and above average for other crimes (table 1). At the same time, the US incarcerates five times more people per capita than the OECD average, more than any other country in the world. Figures 1 and 2 illustrate this situation on log scales for readability; but if plotted in levels, the gap between the US and the other OECD countries would look much more dramatic.[1]

This paper shows that known cross-country determinants of crime and incarceration do not explain the high US rates. Global cross-country regressions of crime and incarceration on background characteristics explain much of the variation between other countries. But the estimated models predict only one-fourth of US incarceration and not all of US crime.[2] The coincidence of the non-negative US crime residuals with the very large positive US incarceration residual constitutes a puzzle. The two pieces fit together only if the residual US incarceration does not contribute to a reduction in crime, except to the extent an omitted criminogenic factor pushes up US crime. Put differently, the larger incarceration's crime-reducing effects, the larger the omitted criminogenic factor has to be. The paper quantifies this relationship, making due allowance for estimation error. Drawing on additional evidence from comparative and US-specific data, it argues that the puzzle's most plausible solution combines low effectiveness of mass incarceration with omitted criminogenic factors such as US neighborhood segregation.

Accounting for background characteristics is extremely important in assessing US mass incarceration's effectiveness by comparing the US to other countries. For example, the US also has the highest income inequality and teen birth rates among Western OECD countries, both of which increase crime.[3] When crime is elevated for exogenous reasons, however, so is its product with expected prison time per crime, namely the incarceration rate.[4] Moreover, the policy response to an elevated crime threat may well be to increase expected prison

[1] In the log-log plot, countries with equal numbers of prisoners per crime (roughly equal to punishment per crime, see footnote 4) but unequal crime rates lie on a straight line with slope one that intersects the incarceration axis at log punishment per crime. By contrast, countries with unequal punishment per crime but equal crime rates lie on a horizontal line at distances equal to the log differences in punishment per crime. Of course, the economic theory of crime predicts that countries with unequal punishment per crime should not have equal crime, everything else being equal. Punishment should decrease crime and thus push the more punitive country south, and the more so the stronger deterrence and incapacitation. This is why it is surprising that the US as a whole and almost all its constituent states lie to the *north*east of the OECD countries in figures 1 and 2.

[2] In practice, I combine the estimation and prediction steps by including the US in the sample but inserting a US dummy. The coefficient on the dummy is algebraically identical to the difference (between the prediction and actual US rates) one would obtain in two steps.

[3] OECD (2013) and Kearney and Levine (2012) document the raw disparities. Fajnzylber et al. (2002), Messner et al. (2002), and Hunt (2006) provide comparative evidence of their criminogenic effects.

[4] This accounting identity (incarceration rate = crime rate X expected prison time per crime) holds in steady state and abstracting from wrongful convictions. These seem reasonable first-order approximations. Sections 5.3.1 and 6.1.2 discuss deviations from this simple model.

per crime, which will further increase incarceration even if deterrence and incapacitation are effective in reducing crime relative to where it would have been without the policy response.[5] This simultaneity (mutual causation) is also the reason why it is not sensible to "control for" incarceration in the crime regression, or vice versa. Decomposing and interpreting the reduced form residuals of crime and incarceration can account for the mutual causation much more cleanly and transparently.

There are two reasons to estimate the prediction models on a global sample rather than a smaller, superficially more similar group of rich countries. First, rich countries, particularly rich Western countries, are not a good comparison for the United States on many relevant dimensions. For example, to apply estimates of the effect of inequality from a sample of only rich Western countries to the US would necessarily extrapolate beyond the estimation support for this variable. Second, most comparative 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 assess the validity of these theories, or more to the point, to avoid over-fitting and to build a reliable model for predicting US rates.

The paper's analysis uses the incarceration rate because it is the only reliable measure of punishment available for more than a handful of countries.[6] It is admittedly a coarse measure: it confounds crime and prison time per crime, types of crimes, as well as sentence length and conviction rates, and it omits all dimensions of punishment other than prison time (such as prison conditions or the death penalty). But these problems are unlikely to affect the main results. First, it is straightforward to decompose the results from incarceration regressions into crime and (expected) prison time per crime (see section 5.1). Second, the results hold for a broad spectrum of crimes, suggesting that composition effects are not an issue. Third, the incarceration rate is positively correlated with and hence a proxy for other dimensions of punishment; in any event, the US is unusually harsh on those other dimensions as well (Tonry 2001; Whitman 2003, 2005; Tonry and Melewski 2008; cf. section 3.1.2 below). Section 6.1.2 addresses the respective roles of sentence lengths and admission rates.

The paper proceeds as follows. Section 2 situates the present paper in the literature. Section 3 describes the data and regression specifications. Section 4 presents the results, including robustness to model specification and over time. Section 5 derives joint bounds

[5] This holds except in the unlikely case that crime decreases more than proportionally and thus offsets the increase in expected prison per crime. In technical terms, an increase in expected prison per crime will increase incarceration provided the crime response to prison is inelastic. This is commonly assumed in the theoretical literature, and borne out by the empirical evidence. Becker (1968, 183) derives it as a condition of optimal enforcement.

[6] In particular, there are no comparative data on punishment per crime, let alone expected punishment per crime. Nor could they be easily collected from statutes or other moderately accessible information. Countries differ in their definitions of crimes and in their norms for sentencing within the statutory or otherwise publicized range. Moreover, measuring expected prison time would also require knowledge of clearance rates.

on the crime-punishment elasticity and the size of omitted criminogenic factors. Section 6 discusses the plausibility of various explanations. Section 7 concludes.

2 Related literature

The papers closest to the present one are Dills et al. (2010) and McCrary and Sanga (2012). They compare *changes* in crime and punishment in the US with those in a small number of other countries. McCrary and Sanga assume parallel crime trends (cf. Durlauf 2012), and conclude that the five-fold increase in US incarceration since the early 1970s reduced crime modestly at best. Dills et al. juxtapose changes in a handful of background characteristics and crime policy, and argue that no clear patterns emerge. Buonanno et al. (2011) also use data from multiple countries, but allow for flexible, unobserved country-specific trends to estimate a crime-incarceration elasticity of -0.4 from shocks to imprisonment (amnesties) that they argue are uncorrelated with other drivers of crime. In line with other comparative economic work on crime (e.g., Soares 2004, Lin 2007), these analyses thus eliminate time-invariant heterogeneity to identify causal effects of time-varying variables. By contrast, this paper focuses precisely on the much larger differences in *levels* across a much larger number of countries.[7] It also does not attempt to estimate directly the effect of punishment on crime, which is not identified in cross-country data.[8] Rather, this paper's accounting exercise attempts to shed light on the crime-punishment nexus indirectly by exposing the large gap between existing micro estimates' prediction of US crime given US incarceration rates, and the actual US crime rates.

Modern micro-econometric work has made much progress in the direct examination of the crime-punishment nexus. Its quasi-experimental settings can credibly identify causal effects of deterrence and incapacitation. There are at least two reasons, however, to complement the quasi-experimental micro studies with an observational macro perspective.[9] First, micro studies cannot identify macro effects such as neighborhood disruption or the removal of stigma effects (McCrary and Sanga 2012). Second, quasi-experiments estimate a local average treatment effect, and this estimate can vary widely from setting to setting. For example, estimates of the effect of punishment range from close to zero (Helland and Tabarrok 2007, Lee and McCrary 2009, Abrams 2012) to rather large (Levitt and Kessler 1999 and Drago

[7] The cross-country standard deviation is at least twice as large as the within-country standard deviation for key variables such as the incarceration rate, the homicide rate, income inequality, and teen births.

[8] In particular, there is no credible instrument (cf. Spamann 2015). As shown in the appendix, all variables structurally affecting one may also plausibly affect the other, violating the exclusion restriction. The one possible exception is demographics, in particular the share of young males, which could plausibly be structurally unrelated to punitiveness. It would be a weak instrument, however, and being correlated with the distribution of crime types, it would be correlated with the measurement error in any punishment-per-crime variable that one could construct from generic incarceration divided by specific crime rates.

[9] These reasons are related to the general concern that (quasi-)experiments trade off high internal validity for possibly low external validity (e.g., Rodrik 2009).

et al. 2009 on deterrence; Owens 2009, Buonanno and Raphael 2013, and Barbarino and Mastrobuoni 2014 on incapacitation; Buonanno et al. 2011 on imprisonment generally). An observational study can help triangulate which of the estimates is more representative. In particular, the present study suggests that the low estimates, which are all from US settings, are more representative of the US situation than the high estimates, many of which come from Europe.

Methodologically, the paper is part of a much broader comparative literature that attempts to gain insights from synthetic counterfactuals constructed from comparative data. That is, this paper extends the approach of comparing the US to one similar country, usually Canada (e.g., Cook and Khmilevska 2005), to a model-based comparison that allows for closer approximation of the relevant covariates. 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, however, this method is unavailable here.

3 Data and Specifications

This section describes the paper's data and regression specifications. Table 1 shows means, medians, standard deviations, US values, and OECD means excluding the US for all the dependent and independent variables.[10]

3.1 Dependent variables

3.1.1 Crime

The paper uses all three series of crime data that are reliable yet available for large cross-sections.[11]

Homicide rates (WHO/GBD). The most commonly used comparative crime data is the homicide rate. It is considered reliable as homicides are difficult to conceal. There are two comparative data series in wide use: data from police statistics as compiled by the United Nations Office on Drugs and Crime (UNODC), and data primarily from death classifications

[10] I merge Guernesey and Jersey into Channel Islands, and England and Wales, Northern Ireland, and Scotland into the UK.

[11] 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 of development or inequality (Soares 2004), and is not constant over time (Gibson and Kim 2008; Vollaard and Hamed 2012). Police-reported official crime data will thus paint a very misleading comparative picture. For example, Buannano et al. (2011, web appendix) show that relatively low reporting rates in the US bias US police-reported crime rates downwards relative to other wealthy countries. Even domestically, police reported data can be misleading. For example, more reliable victimization data show a break in US crime trends much earlier and more dramatically than police reports (cf. US Department of Justice 2013). INTERPOL (1999) even explicitly warns against using its data for comparative purposes.

by medical practitioners compiled by the WHO (Newman and Howard 1999). This paper uses the latter because the former contain many clear reporting errors and cover less than two thirds as many countries.[12] The paper's main cross-sectional tests use a recent overhaul of the WHO Global Burden of Disease (GBD) data for 2005, which offers the highest data quality and country coverage ($N = 187$) (IHME 2013). The subsequent discussion of time trends also uses the other two years of updated GBD data (1990 and 2010), and data from the standard WHO mortality database available since the 1960s.[13] The correlation between the log-transformed GBD and WHO rates is 0.85.

Different years of the standard WHO data were collected under different versions 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, regressions with standard WHO data include dummies for each version of the ICD.

Victimization rates for common crimes (ICVS). The second reliable series of comparative 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 et al. 2007; van Kesteren 2007) (hereinafter collectively referred to as ICVS). As its interest is in country-level determinants, the paper uses country averages rather than individual data (following Wooldridge 2003).[14]

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). This paper appears to be the first to pool data from all five sweeps, including city surveys from developing countries, yielding a sample of 75 countries. The paper excludes data from socialist transition countries from the most tumultuous years 1989-1992; all of the affected countries offer data for later, more comparable years. For steps to adjust for the unbalanced nature of the data, see Section 3.3

[12] In particular, many countries' values jump by an order of magnitude from one year to the next, or diverge by up to an order of magnitude from domestic statistics. In any event, I have verified that the US results would not change with the UNODC homicide data.

[13] http://www.who.int/healthinfo/mortality_data/en/. In fact, the WHO provides some data even in the 1950s, but data on key independent variables, particularly the Gini coefficient and teen birth rates, is not available for those years.

[14] The country averages are calculated using the ICVS survey weights that neutralize over- or undersampling of certain demographic groups within countries.

below.

The primary variable of interest is the one-year prevalence rate of victimization by 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.[15] This measure is commonly emphasized in the comparative literature as a proxy for overall crime (e.g., van Dijk et al. 2007), it has sufficiently many non-zero individual observations to estimate country averages reliably, and its focus on less serious crimes provides a useful counterpoint to the homicide measure. Peru and Tanzania lack information on at least one of these crimes, and hence are omitted. This leaves 73 countries with observations for at least one sweep. Table 4 also shows results for major component crimes.

Drug use prevalence and death rates (WDR/GBD) Much criminal law enforcement in the US over the last decades has been dedicated to the "war on drugs." About a quarter of US prisoners serve time for drug possession or trafficking.[16] When drug-related crimes such as dealer warfare are included, the number is presumably much higher. It thus seems imperative to include some measures of drug abuse in the analysis.

The best available measure is the GBD measure of deaths caused by drug-use disorders in 2005. As noted above, the GBD measures are considered very reliable. At the same time, drug-related deaths are only the tip of the iceberg, and surely the "war on drugs" is also concerned with less dramatic drug abuse. Moreover, the GBD measure does not distinguish abuse of illegal drugs and prescription drugs such as opioids. Table 5 therefore also shows results using the percentage of annual drug use prevalence for opiates, cocaine, and ecstasy from the UN's World Drug Report 2012 (UNODC 2012a). These data should be interpreted with caution, however, as they derive mainly from questionnaires submitted by UNODC member states (UNODC 2012b).

3.1.2 Punishment: Incarceration Rate

The main punishment data is the incarceration rate per 100,000 inhabitants compiled by the International Center for Prison Studies (ICPS) in its first nine World Prison Reports (e.g., Walmsley 2012). The ICPS data are very reliable (cf. Neapolitan 2001; Lappi-Seppälä 2008) and offer nearly universal country coverage. Where the ICPS has not already done so, the paper fills in missing data for individual years by linear interpolation.

[15] 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.

[16] For example, BJS (2013) reports that a drug offense was the most serious offense of about 50% of federal prisoners throughout the 2000s (appendix table 11) and of between 16.6% (2011) and 23% (1991) of state prisoners (table 3), and federal prisoners comprise about 15% of the total inmate population reported there.

ICPS data are not available before the mid-1990s. The examination of time trends therefore also uses UNODC data going back to 1970 but with much smaller country coverage. The correlation of the UNODC and ICPS log-transformed measures is 0.93.

Other reliable data on punishment – prison conditions, probation, parole, etc. – do not seem to be available for larger samples. There is data on the application of the death penalty, but it offers little cross-country variation as only a quarter of the world's countries retain the death penalty and only ten percent carry it out. In any event, the US is such a clear outlier on this dimension that considering the death penalty would only reinforce this paper's conclusions.[17] On institutionalization of the mentally ill, see section 6.2.1 below.

3.2 Independent variables

As independent variables, the paper attempts to use all of the main variables suggested in the comparative literature on crime and punishment, provided they are exogenous and available for the large cross-section.[18] In particular, and subject to the aforementioned proviso, this includes all of the variables suggested in the cross-country regression literature on crime[19] and punishment[20], or close substitutes thereof. The twenty independent variables thus selected fall into four broad categories:

1. Development: log and level of GDP per capita, PPP-adjusted;

2. Institutions: legal origin (common law, socialist, or other), federalism, democracy, proportional voting, and freedom;

3. Demographics: the population shares of main religious groups (Protestant, Catholic, Muslim, or other), descendants of former slaves, immigrants, urban population, and men aged 15-19, respectively; ethnic fractionalization; and the share of teen births among all births;

4. Social: Gini coefficient, employment protection (as a proxy for social policies), and the unemployment rate.

The appendix describes data sources and briefly summarizes the voluminous literature motivating the variables.

[17] The US is the only Western country, one of very few developed countries, and one of only 58 countries worldwide to retain the death penalty; it is one of only 21 countries to have carried out an execution in 2012 (Amnesty International 2013).

[18] 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. Dills et al. (2010) regress crime on a large set of criminal justice variables, arguing that the coefficients provide important information in spite of the endogeneity concerns.

[19] Messner et al. (2002); Fajnzylber et al. (2002); Soares (2004); Hunt (2006); Lin (2007).

[20] Neapolitan (2001); Jacobs and Kleban (2003); Ruddell (2005); Anckar (2006); Downes and Hansen (2006); Greenberg and West (2008).

As is standard with international data (Durlauf et al. 2005), the paper linearly interpolates missing data on religion, urbanization, and migration, which are only provided at five-year intervals; freedom (which has one gap year in 1981 due to changing measurement periods); and on the share of teen births, which have fewer and irregular gaps. The Gini coefficient is also extrapolated from earlier or later measurements and interpolated from a separate data series; details are in the appendix.

Perhaps the most conspicuous omission in the list above is gun ownership. The reason for the omission is that gun ownership is not plausibly exogenous. Increased crime might lead citizens to arm themselves in defense. Section 6.2.2 will return to the gun issue. While some other variables, particularly in the fourth group, might also be affected by crime and perhaps incarceration, any such effect is likely to be small.

Not all of the variables listed above are likely to have an equally strong direct effect on both crime and expected punishment. There are three reasons, however, to use all of them for predicting both crime and incarceration. First, the incarceration rate is not a pure measure of expected punishment but rather its product with the crime rate. Second, the core of the economic model of crime is that expected punishment and the crime rate are simultaneously determined, so that any variable structurally influencing one of them will at least predict the other as well. Third, as shown in the appendix, almost all of the independent variables plausibly have at least some direct structural influence on both crime and expected punishment, or are correlated with an unobserved variable that does.

3.3 Regression specifications[21]

The basic specification is a simple cross-sectional regression of the form

$$y_{it} = \alpha_t + \boldsymbol{\beta}'\mathbf{x}_{it} + \gamma \mathbf{1}_{i=USA} + \varepsilon_{it}, \tag{1}$$

where y_{it} is the log[22] of a crime or incarceration rate in country i and year t as described in section 3.1, $\mathbf{x}_{it}$ is the vector of $K = 20$ independent variables described in section 3.2,[23] and ε_{it} is the country-year-specific error term. The tables report Huber/White/sandwich robust standard errors.

The coefficient of interest is γ. This US dummy coefficient captures the log difference between the actual US rate and the rate predicted by the model. The US data do not influence the prediction itself (i.e., the estimation of the rest of the model) because they

[21] An extended explanation of the regression specifications is available as an online appendix.

[22] The log-transformation of the dependent variables recommends itself because the effects of the independent variables are most plausibly multiplicative. It also facilitates the residuals' use for elasticity calculations (see infra section 5) and reduces the weight of outliers.

[23] It is worth emphasizing that $\mathbf{x}_i$ does *not* contain crime or incarceration rates. Given the simultaneous determination of these rates, "controlling" for one in a regression of the other would bias the coefficients even for the exogenous predictors. I account for the mutual influences in section 5 below.

are absorbed by the dummy. The robust standard error on the US dummy is algebraically identical to the finite sample estimation error of the prediction model (i.e., the extent to which the estimated model is likely to deviate from the "true" linear prediction).[24]

In the main homicide, incarceration, and drug death regressions, $t = 2005$ for all data points. Using 2005 ensures comparability of the various estimates, as this is the last year for which the victimization data are available. The results would likely be almost identical with more recent data because the cross-sectional variation is rather stable and much larger than the inter-temporal variation (cf. sections 4.2.3 and 5.3.1). In the drug use regressions, $t \in \{2000, ..., 2011\}$ because UNODC (2012a) measured drug use for different countries in different years.

While 2005 is the last year with ICVS data, attaining considerable cross-country coverage requires perusing ICVS data from all five sweeps going back to 1989, as explained in subsection 3.1.1 above. Here using separate intercepts by sweep s rather than year t preserves degrees of freedom while also accounting for any changes in survey design. An indicator for capital surveys accounts for the fact that some surveys were only conducted in capital cities. The regression equation thus becomes

$$ICVSvar_{ist} = \alpha_s + \boldsymbol{\beta}'\mathbf{x}_{it} + \gamma_s \mathbf{1}_{i=USA} + \kappa \mathbf{1}_{capitalsurvey,ist} + \varepsilon_{ist}, \qquad (2)$$

and the coefficient of interest is γ_5, corresponding to the US measurement in the fifth sweep taken in 2004. Each country-year observation is weighted by the inverse of the number of years for which the country has data.[25] Standard errors are clustered at the country level.

As shown in table 2, in each regression, about half the observations have a missing value for at least one independent variable, most frequently for the lagged teen birth rate. Consequently, only half the sample would be available with casewise deletion, ignoring much information and introducing potential bias (Little and Rubin 2002). To avoid this, the paper uses two standard methods from statistics (multiple imputation [MI] and full-information maximum likelihood [FIML]) as well as the labor economics standard, which is to replace missing values with zeroes while adding a set of dummies indicating missing values (abbreviated as OLS+). See the online appendix for a technical description of these methods. Table 3 (homicide and incarceration) reports all three sets of coefficients along with the naive OLS estimates for the main incarceration and homicide regressions. As will be seen, all three methods yield results that are essentially identical to one another but moderately different from the naive OLS results. Tables 4 (ICVS) and 5 (drugs) report only MI estimation.

[24] By contrast, the classical (homoskedastic) standard error on the dummy would be equal to the standard error of the forecast. I have verified that the robust standard errors are otherwise appropriate, i.e., generally larger than, and in any event not meaningfully different from, the classical standard errors.

[25] The consequence of this is that each country carries equal weight in the regression, regardless of the number of times its victimization rate was measured.

4 Results

4.1 Basic results

Tables 3 through 5 present the basic results. Residual US crime rates are either positive or statistically indistinguishable from zero, while the residual US incarceration rate is positive and very large, both economically and statistically: Depending on the way of dealing with missing data, the actual US incarceration rate is between 1.35 and 1.44 log points or approximately $e^{1.4} \approx 4$ times higher than the predicted rate, and even the lower 95% confidence bound is 2.5 times.

The point estimates for the residual crime rate differ by type of crime. The residual homicide rate is about 0.6 points on the log scale. That is, the actual US homicide rate is about $e^{0.6} = 1.82$ times higher than predicted by the model. The residual overall victimization crime rate is -0.04 points on the log scale, i.e., actual US overall victimization is $e^{-0.04} = 0.96$ of the prediction. This estimate is quite noisy, however, with a standard error of 0.19. For component victimization rates, the estimates are even noisier, reflecting the larger sampling error (cf. section 3.1.1 above). The point estimates are negative for car theft, theft, robbery, and assault, but positive for burglary; none of these is statistically significant. Estimates for drug crimes are similarly noisy and mixed. The estimates for the most serious drug crimes or rather manifestations thereof, drug-related deaths and opiate use, are positive, however, and even statistically significant at the 10% level.

The large US residuals stand out, as the explanatory power of the models is otherwise very high. The models explain more than half of the cross-country variance, as measured by the R^2 in the OLS+ specifications of table 3 (0.62 in model 2, and 0.53 in model 6). Comparable specifications with the victimization rate yielded an R^2 of 0.5 (not shown). (For MI and FIML, R^2 is not a meaningful measure.) The joint p-value (F-test) for the twenty explanatory variables is less than 0.0005 in most models and less than 0.01 in all but the model for common theft ($p = 0.22$).

Figure 3 (the residual counterpart to figure 1) visualizes the key results. It shows that the US remains an extreme outlier with respect to incarceration even after partialling out covariates, and even while the residual homicide rate remains positive. It also visualizes the high explanatory power of the model, as the "cloud" in figure 3 is only two log points long and wide, compared to four in figure 1.

While the puzzle thus persists in the residuals, it is worth pointing out that it is smaller than in the raw data. This is true even for the US incarceration rate: it exceeds the model's prediction by a factor of four, but it exceeds the mean OECD rate by a factor of five and the rate of many conventional peer countries (i.e., Western OECD countries) by at least as much and up to a factor of ten. Similarly, the US homicide rate exceeds the prediction by a factor of 1.82, but it exceeds that of its conventional peers by a factor of three to ten. Finally, the

US overall victimization rate is on the high end among its conventional peers, but actually slightly below the prediction, i.e., the synthetic comparison country.

The main variables that predict high US homicide rates relative to other OECD countries are the high teen birth rate, high income inequality, lax labor laws, high ethnic fractionalization, and young (male) population. The products of their MI coefficients times the difference between their US values and the OECD means are .33, .17, .12, .11, and .11, respectively, suggesting they collectively account for .84 log points of additional homicides, or more than a doubling of homicides ($e^{0.84} = 2.32$), in the US relative to the OECD mean. The variables that predict an elevated US incarceration rate are the high teen birth rate and the absence of proportional democracy, which respectively add .38 and .19 log points to the US prediction relative to the OECD mean. For the most part, these coefficients are also relatively precisely estimated, suggesting that these are not mere fluke findings.

4.2 Robustness

4.2.1 Missing data methods

As already mentioned, nothing substantive hinges on the choice between the three methods for dealing with missing data. For comparison, table 3 also shows results using only complete observations (models 1 and 5). These naive estimates of the US dummy are about one standard error larger, which would make this paper's conclusions even stronger. Unreported tests obtained similar US results using "naive" model selection, where the final regression contained only variables that achieved a t-statistic of at least 1.64 in preliminary regressions with small, related blocks of explanatory variables.

4.2.2 Non-linearities and interactions

A more complex functional form does not seem to explain the US position better, as much as the data allow such a test.

Any functional form can be (locally) approximated by polynomials. The test performed chose the "best" predictors from up to third order polynomial interactions of all variables, using separate dummies for all possible combinations of binary variables. Of course, there are far too few observations to include all of approximately 8,000 generated interactions in the regressions. To deal with this problem, the Least Absolute Shrinkage and Selection Operator (LASSO) was used to select small numbers of predictors separately for the dependent variables (the incarceration and homicide rates, respectively) and for the US dummy (here no predictor is selected). The dependent variables are then regressed on the US dummy and the selected predictors. Belloni et al. (2012) have shown that this "Post-LASSO" method yields 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).

The estimate for the US "treatment" effect using the Post-LASSO on the complete data is .96 (.76) for the log homicide rate and 1.68 (.53) for the log incarceration rates (standard errors in parentheses). How, if at all, the Post-LASSO could be used with multiply imputed data is an open question. Point estimates from a single set of imputed data were .83 (.84) and 1.12 (.64), respectively (standard errors in parentheses).[26]

4.2.3 Trends over time

At least in broad outline, the results are also robust over time. In particular, both the US crime residual and the US incarceration residual were consistently positive over all four decades for which we have data. In the past, the crime residual was larger while the incarceration residual was smaller. But as explained in section 5.1, it is the weighted average of the two that constitutes the US crime puzzle, and that weighted average may well have been constant.

To establish a baseline, figure 4 shows time series of US data without regression adjustment. The upper panel shows levels of the US incarceration, homicide, and victimization rates for all years available.[27] The lower panel shows those same rates in logs net of the constant-sample world mean, and smoothed by fitting a local polynomial.[28] Two features stand out. First, the US had comparatively high homicide and incarceration rates for as far back as we have data (the 1950s and 1970s, respectively). The US was always at least half a log point above the annual constant-sample world average. This is worth emphasizing because it is often said that US incarceration rates were hovering around 100 per 100,000 population in the early 1970s, comparable to other countries. That low estimate seems based on a narrow and misleading focus on the im*prison*ment rate, however, as the rate including *jails* stood at around 200 even in the 1960s (other countries do not distinguish jails and prisons). Only the US victimization rate has been closer to and recently at the world mean. Second, US incarceration rates steadily increased since the early 1970s, while US crime rates came down, if not always steadily or in exact synchronization (cf. McCrary and Sanga 2012).

[26] These standard errors are biased downwards because they do not account for the imputation variance.

[27] The data are from the US Bureau of Justice Statistics, the FBI's Uniform Crime Reports, and the ICVS, respectively. Reliable victimization data are unavailable for earlier periods. The National Crime Victimization Survey was fundamentally redesigned in 1992 and older data are not currently (2014/15) available from the Bureau of Justice Statistics, cf. http://perma.cc/ED73-EUCG and http://perma.cc/PSV6-HVQ2. Other data series, in particular the Uniform Crime Reports, are not reliable for earlier years, cf. the comparison of trends of victimization data against crimes reported to law enforcement in US Department of Justice (2013), and cf. Vollaard and Hamed (2012) for similar problems with British data.

[28] That is, the lower panel shows local polynomial smoothed plots over t of $\alpha_{USA} + \varepsilon_{USA,t}$ estimated from $y_{it} = \alpha_i + \mu_t + \boldsymbol{\kappa}' \sum_{\kappa} \mathbf{1}_{surveytype_{it}=\kappa} + \varepsilon_{it}$ using all available data on y_{it}. The survey types (one of which will be an omitted base category) are capital or national for ICVS and the various ICD versions for WHO data; there are no survey type dummies in the UNCTS regression.

Figure 5 shows residual US rates after partialling out the covariates, along with 95% confidence intervals.[29] The upper panel draws on the same data sources as the main regressions above but uses all available years of data.[30] The lower panel uses homicide (WHO), incarceration (UNODC), and inequality (UTIP) data of lower quality but longer coverage, and drops three covariates for which historical data is mostly unavailable before the 1990s.[31] The results from the two panels are consistent. The residual US incarceration rate steadily rose from just above zero in the 1970s to its current high level. The near-zero residual in the 1970s is noteworthy because, as just shown, the raw US incarceration rate was far above the world mean even back then. During that same time period, US residual homicide rates appear to have declined, but unsteadily and perhaps not significantly.

5 Joint Bounds for Explanations

As mentioned in the introduction, these results imply major omitted sources of US crime or ineffectiveness of residual US incarceration. This section formalizes this argument. It derives joint bounds on the crime-punishment elasticity and omitted criminogenic factors, accounting for estimation error. As is common in the literature, the model and bounds assume that the elasticity η of the crime rate C with respect to expected punishment per crime Π is constant, both within and across countries.[32] Subsequent discussion will consider more general models.

5.1 Model

The only functional form consistent with the constant elasticity assumption is $C = K\Pi^{\eta}$, where K is a country-specific constant that determines the level of crime for a given punishment intensity and elasticity. Mechanically, the overall steady-state rate of punishment (incarceration) is then $P = \Pi C = K\Pi^{1+\eta}$. K is a latent variable (i.e., it is not directly observable), and so it will henceforth be called "latent crime."

[29] The underlying standard errors are clustered at the country level.

[30] The victimization residuals are simply the full series of US dummy coefficients γ_s that were previously unreported in model 1 of table 4 (i.e., from estimating equation 2 with MI). The homicide and incarceration residuals come from panel extensions of models 2 and 6 of table 3, i.e., with added annual US dummy coefficients γ_t and estimated using all country-year observations with data on the dependent variable. The homicide regression also contains year dummies while the incarceration regression contains a quadratic time trend. The online appendix reports these regression equations in full.

[31] The three independent variables that are missing are labor laws, unemployment, and the lagged teen birth rate. Given the lagged teen birth rate's contribution to explaining high US crime rates, its omission may be responsible for at least some of the high US homicide residual in this panel.

[32] Any use of LATE estimates for society-wide policy analysis implicitly assumes that the elasticity is constant within a country, and any use of foreign estimates implicitly assumes that the elasticity is constant across countries. Constant elasticity is not an unreasonable assumption; in particular, it is compatible with (strongly) diminishing returns to punishment (as in the Italian data of Buonanno and Raphael 2013).

Denote the natural logarithms of P, C, K and Π by p, c, k, and π, their linear predictions by p^*, c^*, k^*, and π^*, and the difference between the two (i.e., the prediction error or residual) by ε_p, ε_c, ε_k, and ε_π, respectively. The regressions' US dummy coefficients are estimates $\left(\hat{\varepsilon}_p^{USA}, \hat{\varepsilon}_c^{USA}\right)$ of $\left(\varepsilon_p^{USA}, \varepsilon_c^{USA}\right)$, and the former's standard errors can be used to construct confidence bounds for functions of the latter.

By definition and $C = K\Pi^\eta$,

$$
\begin{aligned}
c^* &= k + \eta\pi - \varepsilon_c && (3)\\
p^* &= k + (1+\eta)\,\pi - \varepsilon_p, && (4)
\end{aligned}
$$

where the residual crime and incarceration rates can be decomposed as

$$
\begin{aligned}
\varepsilon_c &= \varepsilon_k + \eta\varepsilon_\pi && (5)\\
\varepsilon_p &= \varepsilon_k + (1+\eta)\,\varepsilon_\pi. && (6)
\end{aligned}
$$

It follows that

$$\varepsilon_k = (1+\eta)\,\varepsilon_c - \eta\varepsilon_p. \qquad (7)$$

That is, residual latent crime ε_k is a weighted average of residual observed crime ε_c and residual incarceration ε_p, with weights depending on η. In terms of this model, the US crime puzzle is that if ε_p^{USA} is positive while ε_c^{USA} is non-negative, then ε_k^{USA} must be positive – there is unexplained US crime – under the maintained assumption that $\eta < 0$.

5.2 Estimates[33]

Figure 6 graphs $\hat{\varepsilon}_k^{USA} = (1+\eta)\,\hat{\varepsilon}_c^{USA} - \eta\hat{\varepsilon}_p^{USA}$ as a function of η along with 95% confidence bounds for the estimation error.[34] The estimate of ε_c^{USA} depends on the measure of C, i.e.,

[33] The graphs and discussion to follow use slightly modified estimates relative to tables 3 and 4 to produce a joint covariance matrix for $\hat{\varepsilon}_p^{USA}$ and $\hat{\varepsilon}_c^{USA}$. The right panel and corresponding discussion uses MI estimates for 2005 from only the countries where both incarceration and homicide data are available ($N = 170$). MI is unsuitable for the victimization data, however, because the imputation model should include both dependent variables while victimization and incarceration data are rarely available for the same country-year. Instead, the estimates underlying the left panel derive from regressions with dummies indicating missing values (OLS+), and use all incarceration data for 2005 and the latest ICVS measure available for each country, if any. In both cases, the US point estimates are very similar to tables 3 and 4. Robust standard errors and confidence bounds are adjusted for small samples.

[34] One can also calculate $\hat{\varepsilon}_\pi^{USA} = \hat{\varepsilon}_p^{USA} - \hat{\varepsilon}_c^{USA}$, which yields $\hat{\varepsilon}_\pi^{USA} = 1.41$ using victimization as the crime measure, and $\hat{\varepsilon}_\pi^{USA} = 0.75$ using homicides as the crime measure. The comparative macro data thus suggest that expected prison sentences in the US are between two and four times longer than predicted, i.e., than in the synthetic comparison country. This accords with anecdotal evidence (e.g., Tonry 2001; Blumstein et al. 2005; Lynch and Pridemore 2011).

the type of crime used in the estimation. To provide upper and lower bounds, the figure focuses on the lowest and the highest among the more reliably (MI) estimated residual US crime rates, namely those for overall victimization from smaller crimes (left panel) and homicides (right panel).

If homicides were a good proxy for overall crime C (right panel), the US crime puzzle would be very deep indeed. As the right panel shows, any $\eta < 0$ would then imply large unexplained crime ε_k^{USA} even at the lower 95% confidence bound of estimation error.

If the overall victimization rate were a better proxy of C (left panel), the puzzle would be smaller but not resolved. In particular, the higher elasticities estimated in the literature would still imply a very high US latent crime residual. For example, $\eta = -0.74$ from deterrence alone (Drago et al. 2009) would imply a lower 95% confidence bound for ε_k^{USA} of 0.68 even if incapacitation were completely inoperative. The point estimate for ε_k^{USA} is zero only if $\eta = 0$. To be sure, the 95% confidence bound for ε_k^{USA} derived using the victimization rate (barely) includes zero if $\eta = -0.25$, as suggested in a literature summary by Abrams (2013:961n219). One might therefore believe that any appearance of a puzzle for smaller crimes is merely an artefact of estimation error. Importantly, however, this would leave intact the puzzle for other types of crime, specifically homicides and serious drug crimes.

5.3 Relaxing assumptions

5.3.1 Steady state vs. adjustment path

The foregoing analysis assumed a system in steady state. In reality, crime and criminal justice are constantly changing. Precisely accounting for the transition dynamics would be very complicated and perhaps not possible: different shocks of unknown origin may propagate through the system simultaneously, and even single-shock adjustment paths may be non-monotonic and depend on many unknown factors, in particular the relative importance of deterrence and incapacitation. As McCrary and Sanga (2012) point out, these complications are a major problem for inference from changes of crime and incarceration rates over time. Cross-sectional results will be much less affected by these complications, however, since the intertemporal differences are small relative to the cross-sectional differences. Concretely, there are three reasons to think that transition dynamics are of minor importance for the results presented above.

First, US crime, punishment, and incarceration were relatively stable in the decade around 2005, the year chosen for the main analysis above (cf. figure 4). US incarceration rates shot up between 1980 and 2000 but were fairly flat thereafter, peaking in 2009. Flows (admissions and releases) were approximately stable during that decade as well, including the shares of various offenses and sentence lengths; the minimum admissions and releases were 12% and 14%, respectively, below their maximum (BJS 2013). This relative stability reflects the fact that the major tough-on-crime reforms in the US had occurred earlier, namely in the 1980s and 1990s (e.g., Kearney et al. 2014, ch. 1 and fig. 5). While crime rates kept falling between 2000 and 2010 in the US (and other rich countries, see UN Economic and Social Council 2014), the rates of decline were lower than in the 1990s (cf. BJS 2011, 3).

Second, the changes that did occur are small relative to the levels and their estimation error that form the basis for the analysis above. For example, the difference between the minimum US log-victimization residual over the period 1992-2004 ($-.15$ for the year 2000)[35] and the one used in the calculations above ($-.04$) is only .11, less than 0.7 standard errors. This difference would be noteworthy in the national context, but it would move the estimate of the implied residual latent crime rate (left plot of figure 6) down by only about a sixth of the width of the confidence interval. The differences in the log-incarceration residual over the period 1997-2009 are equally small. The changes in the log-residual homicide rate over the period 1990-2010 are larger, but even here using the minimum over the period (.58 in 2010) instead of the 2005 value of .72 would not qualitatively affect the results displayed in the right plot of figure 6; at an assumed elasticity of zero the 95% confidence interval would now barely include 0 but the 90% interval would not.

In sum, the steady state assumption seems at least a reasonable approximation of the US situation in 2005.[36]

5.3.2 Crime-specific incarceration rates and elasticities

Ideally, one would perform entirely separate analyses for different types of crimes. This would require data on punishment by crime type, however, which are not available for large samples.

As a first approximation, it seems reasonable to assume that US residual incarceration rates and, more to the point, punishment per crime are uniformly high across crime categories. Accounts of US "punitiveness" do not distinguish different sorts of crime (e.g., Whitman 2003). This is true even at the top end of the scale. The US is one of the few

[35] The US participated in the 1989 sweep but this observation drops out because indispensable (binary) covariates are missing for that year.

[36] Besides, the violation of the steady state assumption by falling crime rates in the 2000s adds only a temporal perspective but does not fundamentally undermine the conclusions derived above. The most straightforward explanation for the combination of falling crime rates and a flat incarceration rate is a drop in the latent crime rate. The resulting instantaneous drop in the flow of crimes committed will only gradually reduce the stock of criminals caught and incarcerated (Johnson and Raphael 2012; cf. McCrary and Sanga 2012). Along the transition path, ε_p is larger than the incarceration residual that will prevail in the new steady state, which in turn exaggerates the *current* latent crime residual when applying the steady state formula $\varepsilon_k = (1+\eta)\,\varepsilon_c - \eta\varepsilon_p$ (provided $\eta < 0$). At the same time, the latent crime residual thus calculated along the transition path is *less* than the *initial* latent crime residual (because ε_p is weakly smaller and ε_c is strictly smaller than initially). To the extent this dynamic is at work in the 2000s, the estimates and confidence intervals reported above exaggerate ε_k^{USA} for 2005 but actually understate ε_k^{USA} for previous years.

countries in the world to continue use of the death penalty, and its practice of imposing life without parole is virtually unheard of elsewhere, even for mass killings (Lerner 2013).[37]

If US punishment were particularly harsh for some crimes, then the puzzle for those crimes would be larger, while the puzzle for other crimes would be smaller. If one also believed that elasticities are larger for some crimes than others, then differentiated US punitiveness could deepen or resolve the puzzle. The former would occur if the US punished high elasticity crimes relatively more harshly, and the latter if it punished them less harshly. Empirically, there is very mixed evidence for differentiated elasticities. While Johnson and Raphael (2013) find that the crime-prison elasticity is higher for property crimes than for violent crimes, Levitt (1996) and Buonanno et al. (2011) find the opposite.

5.3.3 Diminishing elasticity

The elasticity might also vary with the level of punishment. In particular, punishment might exhibit more than proportionally diminishing returns. At the relatively low levels of incarceration observed in Western Europe today and in the US in the past, the elasticity might be high, but at the levels of contemporary US mass incarceration, it might be low. This would explain why elasticities estimated on foreign data (Drago et al. 2009; Buonanno et al. 2011; Buonanno and Raphael 2013; Barbarino and Mastrobuoni 2014) tend to be much higher than elasticities that most researchers have found in the US (e.g., McCrary and Lee 2009; Abrams 2012; Johnson and Raphael 2013). It would also explain why within the US, Johnson and Raphael (2013) find a higher elasticity in 1978-1990 than in 1991-2004.

It would not explain, however, why micro-estimates of the elasticity for the contemporary US are still non-zero. In fact, it would make those estimates harder to reconcile with the comparative data. The reason is that, as just shown, zero residual latent US crime rates are barely compatible with crime-punishment elasticities around -0.25 if those elasticities are constant (and even then only for smaller crimes). If elasticities were actually larger in absolute value at lower levels of punishment, then the overall effect of US punishment (the integral of the elasticity from zero to US mass incarceration) would be larger as well, and the US crime rate should be concomitantly lower. Diminishing elasticities can thus explain the US crime puzzle only if elasticities at high levels of contemporary US punishment are essentially zero, as indeed estimated in Lee and McCrary (2009).[38] Cf. section 6.1.1 below.

[37] For example, Anders Breivik received only 21 years in Norwegian prison for killing 76 people. In the United States, he would almost certainly have been sentenced to life in far less pleasant prison conditions, and quite possibly have been executed (Mary Slattery, Why is Breivik Facing a Maximum Sentence of Just 21 Years?, *New Republic* 8/1/2011).

[38] Other low estimates of the crime-punishment elasticity from deterrence include Helland and Tabarrok (2007) (from whose estimates Lee and McCrary [2009, p. 6] calculate an elasticity of -0.07) and Abrams (2012) (finding an elasticity of -0.10).

6 Discussion

This section discusses the plausibility of the two possible solutions of the US crime puzzle: the ineffectiveness of excess imprisonment, and the presence of omitted criminogenic factors. Unlike the other outlier countries, the US does not have one obvious country-specific explanation.[39]

6.1 Ineffectiveness of US mass imprisonment

There are several hypotheses why incarcerating many more people than comparable countries may contribute little to crime control in the contemporary US.[40]

6.1.1 Limits of imprisonment in general

Few would doubt that some well-targeted imprisonment is effective for crime control. But imprisonment's effectiveness may rapidly decrease as expected sentences increase (cf. section 5.3.3 above). Expected sentences equal the product of sentence length and imprisonment probability. Long sentences may be ineffective because incapacitation benefits decrease strongly as convicts age, and deterrence is blunted by criminals' (beta-)discounting of distant prison time (Lee and McCrary 2009). High imprisonment probabilities may mean that many spend short times in prison, which sharply increases recidivism rates relative to milder sanctions such as monitoring (di Tella and Schargrodsky 2013; Aizer and Doyle 2015; cf. Ganong 2012).

Moreover, even if high expected sentences worked for deterrence and incapacitation at the individual level, they might be counterproductive at the macro level (see, e.g., Western 2006; McCrary and Sanga 2012; National Research Council 2014). Mass incarceration may disrupt communities (Clear 2008) and remove the stigma effect of incarceration. It has collateral, possibly criminogenic effects on prisoners' children (Murray and Farrington 2008). These macro effects are difficult to test in cleanly identified designs, but they may be extremely important.

6.1.2 US-specific implementation issues

Another possibility is that the US inefficiently targets and administers its imprisonment.

US targeting is undoubtedly imperfect.[41] For example, wrongful convictions occur even

[39] For example, Rwanda has the largest residual incarceration rate in 2005 because it still imprisoned a large number of accused and convicted participants of the 1994 genocide (Walmsley 2012).

[40] This is a stronger set of hypotheses than the related hypothesis that the cost-savings of reducing punitiveness would outweigh any increase in crime (e.g., Cook and Ludwig 2011; Abrams 2013).

[41] The US war on drugs may also be a misallocation in a welfare sense, but not one that would explain the findings of this paper. Harsh persecution of drug crimes should at least reduce drug crimes, but as shown above, hard drug abuse and drug related deaths are still higher than predicted in the US.

while most murderers in inner cities are never even charged.[42] The less precise the punishment, the less are its deterrence and incapacitation effects. The question is, however, if US law enforcement is less precise than possible or, more to the point, than in peer countries. While there do not appear to be comparative data suggesting this, there are factors that could induce such differences.[43] In particular, elected judges, prosecutors, and sheriffs are virtually unique to the US and might be less precise than their professional counterparts in peer countries.[44] More generally, the same populist politics that arguably drove the expansion of US punishment may also have deflected it away from efficient crime control (e.g., Jacobs and Jackson 2010). Such politics have been absent in Western Europe (e.g., Hammel 2010).

As regards the administration of imprisonment, the US treats inmates unusually harshly by Western standards in and after prison (e.g., Tonry 2001; Whitman 2003, 2005; Tonry and Melewski 2008).[45] This may increase recidivism rates. For example, criminal records are publicly accessible in the US but not in other countries, hindering reintegration (Western 2006).

There is some controversy whether the high US incarceration rate is driven by long sentences (e.g., Young and Brown 1993; National Research Council 2014) or high prison admission rates (Langan 2005; Pfaff 2011). Because of discounting and perhaps risk-loving behavior, long sentences generate less deterrence for a given level of expected punishment (Durlauf and Nagin 2011). There is also evidence that sentence lengths are not effectively communicated to the relevant populations in the US (Hjalmarsson 2009; Bushway and Owens 2013).

[42] For examples and statistics of wrongful convictions, see, e.g., innocenceproject.org. While the overall homicide clearance rate in the US is around 60%, it was for example in the low single digits in Detroit until the city's bankruptcy (NPR, Open Cases: Why One-Third Of Murders In America Go Unresolved, 3/30/2015, available at http://perma.cc/S9QT-SVD7).

[43] In particular, there are no reliable data on comparative clearance rates. The United Nations reports clearance rates by region but not by country (UNOCD 2014, 92), and it references a data source (UN-CTS) that does not in fact make the clearance rates available to the public (cf., e.g., http://perma.cc/WX7Z-V9RV), presumably in recognition of formidable measurement problems arising from self-reporting. Differences in clearance rates may also arise endogenously from the number and type of crimes committed (see, e.g., the explanation of Japan's almost perfect clearance rate in Roberts 2008).

[44] Berdejo and Yuchtman (2013) show that elected judges punish more harshly before elections, implying at the very least suboptimal variance in sentencing. Clearly, the Ferguson Police Department's focus on generating revenue (US Department of Justice 2015) was at odds with optimal crime control. Policing is demonstrably very important (e.g., di Tella and Schargrodsky 2004; DeAngelo and Hansen 2014; cf. Levitt 2004).

[45] Compare, e.g., Jessica Benko, The Radical Humaneness of Norway's Halden Prison, with Mark Binelli, Inside America's Toughest Federal Prison, both *New York Times Magazine* 3/26/2015 (http://nyti.ms/1HMmyZ2 and http://nyti.ms/1GsrGDt).

6.2 Omitted criminogenic factors

The predictive regressions omitted many factors with a potential influence on crime, such as school hours and lead paint (e.g., Kleiman 2009). Only three seem sufficiently important and variable between countries, however, to be able to explain the US outlier position: deinstitutionalization, guns, and segregation.

6.2.1 Deinstitutionalization

The US released most patients from its mental health hospitals in the second half of the 20th century (e.g., Harcourt 2011). This raises the question whether US crime might be higher than in other countries because the US now institutionalizes fewer mentally ill than other countries. More to the point, the incarceration rate might be a misleading indicator if what really mattered were the overall institutionalization rate, i.e., the combined rate of commitment to prison, jail, or a mental institution, as argued in Harcourt (2011). Unlike its incarceration rate, the US institutionalization rate might be normal. The data, however, do not bear out either of these conjectures.

First, other countries have much lower institutionalization rates than the US.[46] In 2010/11, the US institutionalization rate of 803 per 100,000 inhabitants was by far the highest in the world (the number two being Russia's 677). The US rate is almost four standard deviations above the OECD mean of 245 per 100,000 (recall that the US incarceration rate is 4.5 standard deviations above the OECD mean). The reason is that the current US mental hospitalization rate of 56 per 100,000 is only a half standard deviation below the OECD median of 70 (the mean being 75). This offsets only a very small part of the very large surplus US incarceration rate. It is also unlikely to account for any additional crime given that a large number of other mentally ill people are serving time in US prisons (Raphael and Stoll 2013).

Second, using demographic data, Raphael and Stoll (2013) estimate that only 4 to 7% of the growth of the US prison population in the 1980-2000 and none before can be explained by "transinstitutionalization" from mental hospitals and facilities into prisons and jails. This argues strongly against treating these populations as interchangeable.

6.2.2 Guns

In the US, firearms are notoriously easy to procure, and most US murders are committed with a gun (Donohue 2013; Webster and Wintemute 2015; Wintemute 2015). The regressions did not control for this. This was intentional. Gun ownership is endogenous if and because

[46] I calculated these rates as the sum of 2010 incarceration rates (from ICPS) and 2011 rates of beds for mental health patients in mental hospitals, general hospitals, and community residential facilities (from the WHO's Global Health Observatory Data Repository, accessed 4/28/2015).

citizens acquire guns to protect themselves from crime.

Unreported regressions of incarceration and violent crime (homicide, robbery, and assault) did include a control for the number of firearms per population.[47] This only made the US residual larger in all but the robbery regression. To be sure, the introduction of an endogenous regressor might negatively affect the quality of the prediction. But the coefficients on the other regressors did not significantly change, suggesting that the endogeneity problem is not that important.

Substantively, the limited impact of the firearm regressor is driven by the fact that many Western European countries with very low homicide rates have very high gun ownership rates as well (e.g., Finland, France, Germany, Norway, Sweden, and Switzerland), while many countries with low to medium gun ownership rates, particularly in Africa and Latin America, have very high homicide rates. How many guns there are may not matter nearly as much as who has them.

The foregoing empirical analysis is subject to important caveats (cf. Donohue 2013). The available data do not distinguish between handguns and long guns, or the type of long guns. For crime, handguns and assault rifles are most problematic. The data also do not control for various ways of regulating gun ownership beyond mere numbers (cf. Braga and Weisburd 2015; Webster and Wintemute 2015). In general, the data quality is low. The comparative data thus cannot rule out an important role for guns and gun regulation in explaining US crime and punishment. But they also do not provide support for it.

6.2.3 Minorities and segregation

Crime and punishment in the US are heavily concentrated among minorities, particularly (young) black males (Tonry and Melewski 2008; BJS 2013, table 18; BJS 2014, table 9). Relative to non-hispanic white males, black males are incarcerated at seven times the rate (National Research Council 2014, 60) and murdered at ten times the rate in any given year (CDC 2013, table 15).

Important as these racial disparities are, they do not imply that the US crime puzzle is merely a black crime puzzle. Even non-hispanic white Americans are incarcerated at 2.8 times the OECD average and murdered twice as often as the OECD median.[48] More to

[47] The firearms data are from the Small Arms Survey (Karp 2007). I also used an additional dependent variable, gun homicides in 2005 (from IHME 2013). Here, the other coefficient estimates do change considerably from the overall homicide regression. In particular, the US residual is considerably larger (1.87) than in the regressions with all homicides. Controlling for firearm ownership rates now reduces the US coefficient a little to 1.17, but it remains above the US coefficient in the basic homicide regression.

[48] Both comparisons are for 2010, or close by. The incarceration rate for non-hispanic white Americans was 385 per 100,000 in 2010 (cf. BJS 2011, 8), while the non-US OECD mean incarceration rate was 138 in 2008 (ICPS). In 2010, the homicide death rate for non-hispanic white Americans was 2.5 (CDC 2013, 69), while the non-US median homicide rate was 1.32 (IHME-GBD; the non-US *mean* of 2.34 is dominated by the outliers of Mexico (18.3), Estonia (7.7), and Chile (5.9)).

the point, the log-differences between the rates for all Americans and non-hispanic white Americans are smaller or even much smaller than the estimated US residuals.[49]

That being said, there are aspects of US race relations that the regressions did not capture and that might explain at least part of the puzzle. A striking characteristic of the US is the high level of segregation along socio-economic and particularly racial lines. Other countries like Singapore have similarly high levels of income inequality and ethnic fractionalization. But while Singapore forces integration in mostly government-built housing and publicly funded schools (OECD 2011), neighborhoods and schools in the US are highly segregated by income and ethnicity. In cities like Chicago, crime and punishment are heavily concentrated in just a few overwhelmingly African American neighborhoods that are also suffering in other dimensions (e.g., Sampson and Loeffler 2010; Sampson 2013). To the extent such concentration is merely a regrouping of otherwise constant characteristics and behavior, it would not affect any averages and hence would not yield any new predictions. But if the concentration of crime and punishment is the result of negative dynamics in these neighborhoods, higher levels of de facto racial and social segregation in the US than elsewhere might increase US crime relative to other countries. There is some micro-econometric evidence for such dynamics (Damm and Dustmann 2014).

While comparative data on segregation are not available, domestic US data are consistent with the hypothesis. Unreported regressions show that US homicide and incarceration are indeed concentrated in areas with high levels of segregation, high fractions of African Americans, and low levels of social capital (which are highly correlated with one another).[50] These areas tend to be in the American South. As figure 2 shows, homicide and incarceration rates tend to be much higher in the South than in the rest of the US.[51] At the same time, it also shows that there is something special about the United States as a whole. Even the New England states incarcerate a higher fraction of the population than any OECD country except Estonia, and their homicide rates rank on par with the highest ones observed in Western Europe.

7 Conclusion

This paper has shown that standard comparative theories of crime and punishment only partially explain the high crime and incarceration rates of the United States relative to other

[49] US log-residual homicide and incarceration are still 0.3 and 0.8, respectively, when calculated using the non-hispanic white rates. Moreover, these residuals understate the US "white crime puzzle" because predicted US rates are elevated by US background characteristics such as income inequality or ethnic fractionalization that are heavily influenced by the presence of minority populations.

[50] I use data from Chetty et al. (2014), collapsing the data at the state level to match with incarceration data. Relatedly, Kearney and Levine (2012) find that US teen birth rates are particularly high among minorities and in states with high income inequality.

[51] The homicide rates are averaged over the three years 2004-06 to avoid small sample noise in small states.

developed nations. Regressions estimated on the largest cross-country data set predict some of the high US crime rates, but they predict only one-fourth of the actual US incarceration rate. Either US residual punishment is not working well, or some omitted factor, such as segregation, pushes up US crime. Most plausibly, both are true.

An accounting exercise such as the present one cannot conclusively sort out these explanations. What the accounting exercise does demonstrate, however, is that commonly used estimates of the crime-punishment elasticity and common descriptions of the comparative drivers of crime and punishment do not add up in the contemporary US. Like similar findings in other areas of economics (e.g., Mehra and Prescott 1985), this points to important research opportunities. Future research might investigate possible reasons why US mass incarceration may not work or identify idiosyncratic factors that push up US crime. The findings of this paper suggest that it may be worthwhile to shift attention from tougher punishment to other levers of crime control, including better punishment.[52]

[52] See Cook and Ludwig (2011) for a discussion of the cost-effectiveness of various levers.

Appendix: Theory and Sources of Independent Variables

This appendix describes the data sources for the independent variables, and briefly summarizes the voluminous literature motivating them. For reviews of this literature, see Neapolitan (1997), Whitman (2005), Tonry (2007), Lappi-Seppälä (2008), and Lynch and Pridemore (2011).

Development. All of the cited cross-country studies control for the level of development, usually operationalized as GDP per capita.[53] In fact, the impact of development is so fundamental that presumably most other theories implicitly hold the level of development constant, and all the regressions control for it.[54] 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, or be affected by, 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, the homicide and incarceration regressions control for the level of development non-linearly, using both the level of GDP per capita and its natural logarithm. PPP-adjusted data come from the Penn World Tables 7.1 (Heston et al. 2012).

Income inequality and social policy. At least since Ehrlich (1973), another major focus of the prior literature has been income inequality and the policies that influence it (e.g., Kelly 2000; Fajnzylber et al. 2002; Messner et al. 2002; Choe 2008; Dahlberg and Gustavsson 2008). 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; Foley 2011). 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 (e.g., Downes and Hansen 2006). 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. Di Tella and Dubra (2008) generate similar coincidences in an economic model. The regressions control for income inequality using the Gini coefficient, and for labor regulation using the World Bank's index of the ease of hiring

[53] 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.

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

and firing a worker for 2007 (World Bank 2007).[55]

Gini data preferably come from the OECD Income Distribution database, and otherwise from the World Bank's World Development Indicators (WDI). Both measure post-tax, post-transfer (i.e., disposable) income inequality, which fits the above theories.[56] Some WDI 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, missing years are first linearly interpolated. Remaining missing values are predicted from a regression of the interpolated OECD/WDI data on a Gini measure constructed from pay data by the University of Texas Inequality Project (UTIP) (Galbraith and Kim 2005) (the UTIP measure is used directly in panel 3 of figure 5). Finally, the cross-sectional tests fill in remaining missing values with 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).

Political structure. In the aforementioned criminological literature, differences in social policy are usually viewed in a broader context of different political systems. This literature tends to include in the analysis 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). Since these classifications are only available for relatively small groups of countries, however, this paper instead uses proportional voting, which is often viewed as conducive to, or even a hallmark of, social democracies or consensus systems.

The measure of proportional voting is constructed from the World Bank's Database of Political Institutions (Beck et al. 2001) using the formula of Pagano and Volpin (2005).[57] Since proportional voting only matters in democracies, a democracy dummy is included as well, and the proportional voting variable "switches on" only when the dummy is equal to one. To the extent possible, missing data are filled in 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. Like Lin, the paper uses the Fraser Institute's political rights and civil liberties scores (Gwartney et al. 2012) to control for democracies and liberty more broadly, adding the two subscores and rescaling

[55] The index was discontinued, and is not available in the World Bank's electronic databases.

[56] The WDI data descriptions do not say so explicitly. But they are almost perfectly correlated with the OECD post tax and transfer measures, much less so with the pre tax and transfer measures. They are also very closely correlated with post tax/transfer measures from the World Income Inequality Database of the World Institute for Development Research (WIDER).

[57] Concretely, I use DPI 2012 revised January 2013. The formula is (PR - PLURALTY - HOUSESYS + 2)/3.

them so that higher values correspond to more freedom.[58] Data for one gap year are linearly interpolated (1981).

Finally, Jacobs and Kleban (2003) have argued that federal systems should have more prisoners because relevant political decisions will be less remote from the population and hence more subject to populist pressures for harsh punishment. A dummy federalism variable supplied by Tom Ginsburg of the Comparative Constitutions Project (Elkins et al. 2014) controls for this.[59]

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. This paper uses 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 associations of crime with young adults born to teen mothers, particularly when they reach age 25-29. The US teen birth rate is exceptionally high among developed nations (Kearney and Levine 2012). The regressions 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). The regressions do so using the population share living in urban areas in the relevant year (WDI).

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. Relatedly, outsiders such as immigrants might be more inclinded to engage in crime for lack of alternatives or because of lower informal social control.[60] To control for this, the regressions use the index of ethnic fractionalization from Alesina et al. (2003), and the percentage of foreign born inhabitants (WDI).

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 (Western 2006; Tonry and Melewski 2008). In an attempt to account for this, the regressions include a measure

[58] Lin (2007) uses the political rights score (and the civil liberties score as an instrument for it).

[59] The variable was constructed from a prior version of the database and takes value 1 if the constitution explicitly labels the country federal or confederal, or if it grants residual or superior lawmaking powers to subnational levels.

[60] Most empirical evidence does not support this conjecture, or only to a very limited extent, see, e.g., Bianchi et al. (2012); Chalfin (2014); Spenkuch (2014).

of slavery legacy in non-African countries constructed as the percentage of the population descendant from former African slave exporter countries. Measures of ancestry come 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, to avoid 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, and in any event they would hardly be exogenous. 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.[61]

This ties into an important literature in economics that 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. "Social control" is broader, however, and may affect latent crime (K) through other channels as well.

For continuity with the economics literature, this paper employs 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). Data on twelve additional jurisdictions come from Klerman et al. (2011).[62]

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

[61] 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.

[62] The added jurisdictions are Congo-Brazzaville, French Guiana, French Polynesia, and Timor-Leste (French); the Channel Islands, British Virgin Islands, Palau, and Gibraltar (Common Law); East and West Germany prior to reunification (socialist and German, respectively); and Serbia, Montenegro, and Kosovo (socialist).

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). This is why culture variables are not included in this paper.

The paper does, however, use 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." For example, religion could reduce crime through informal social control, or modulate criminal law enforcement according to notions of mercy or retribution. 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, it is probably not possible to disentangle their influence on the death penalty in a cross-country regression. Focusing on the main world religions, the paper employs 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 (e.g., Altindag 2012), and the economic growth rate. The latter is subject to too frequent fluctuations, however, to explain the huge, stable cross-country differences in crime and punishment, and is thus omitted. For the former, the paper uses total unemployment rates estimated by the International Labor Organization (WDI). 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.

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Figure 1: Homicide and incarceration

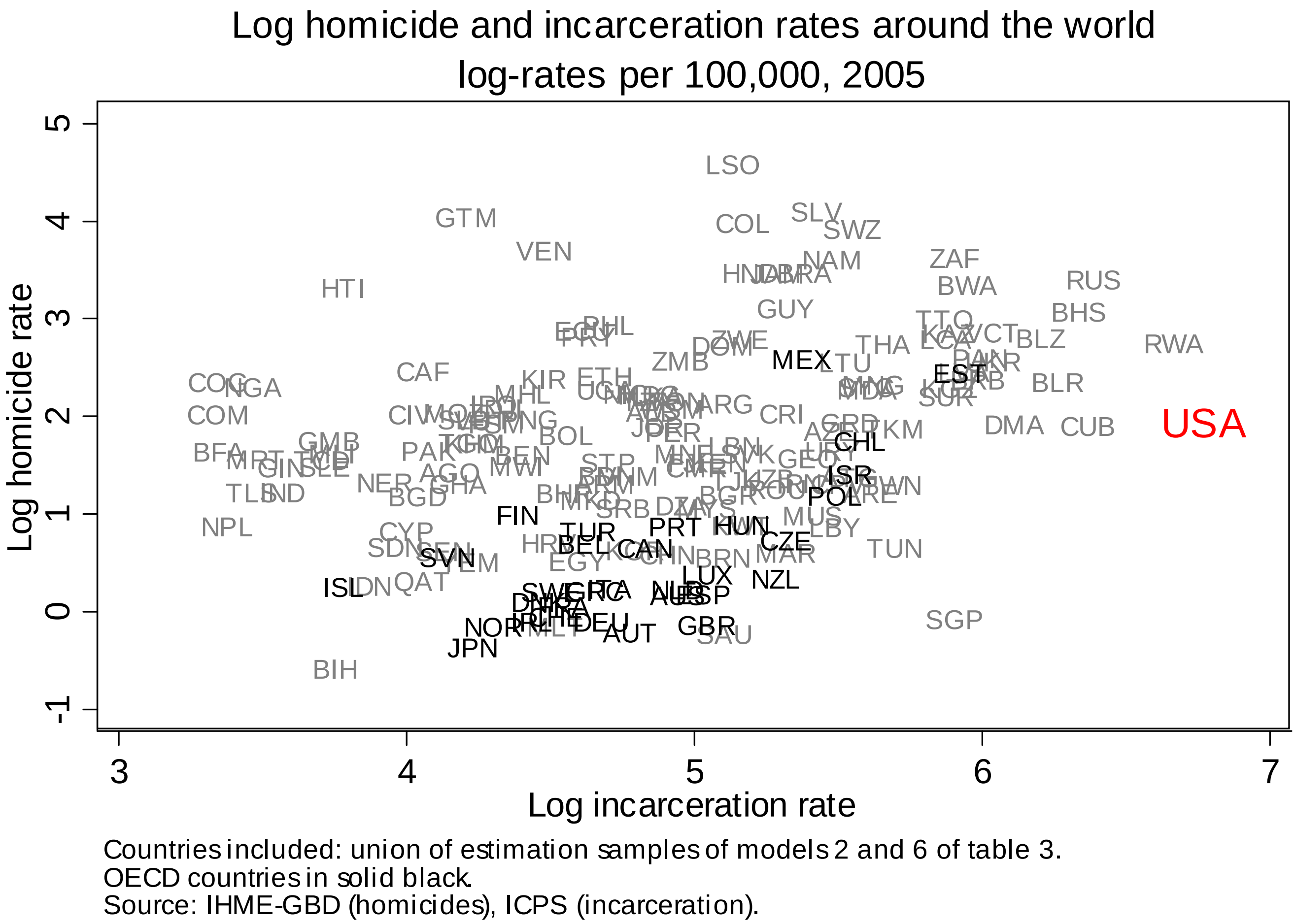
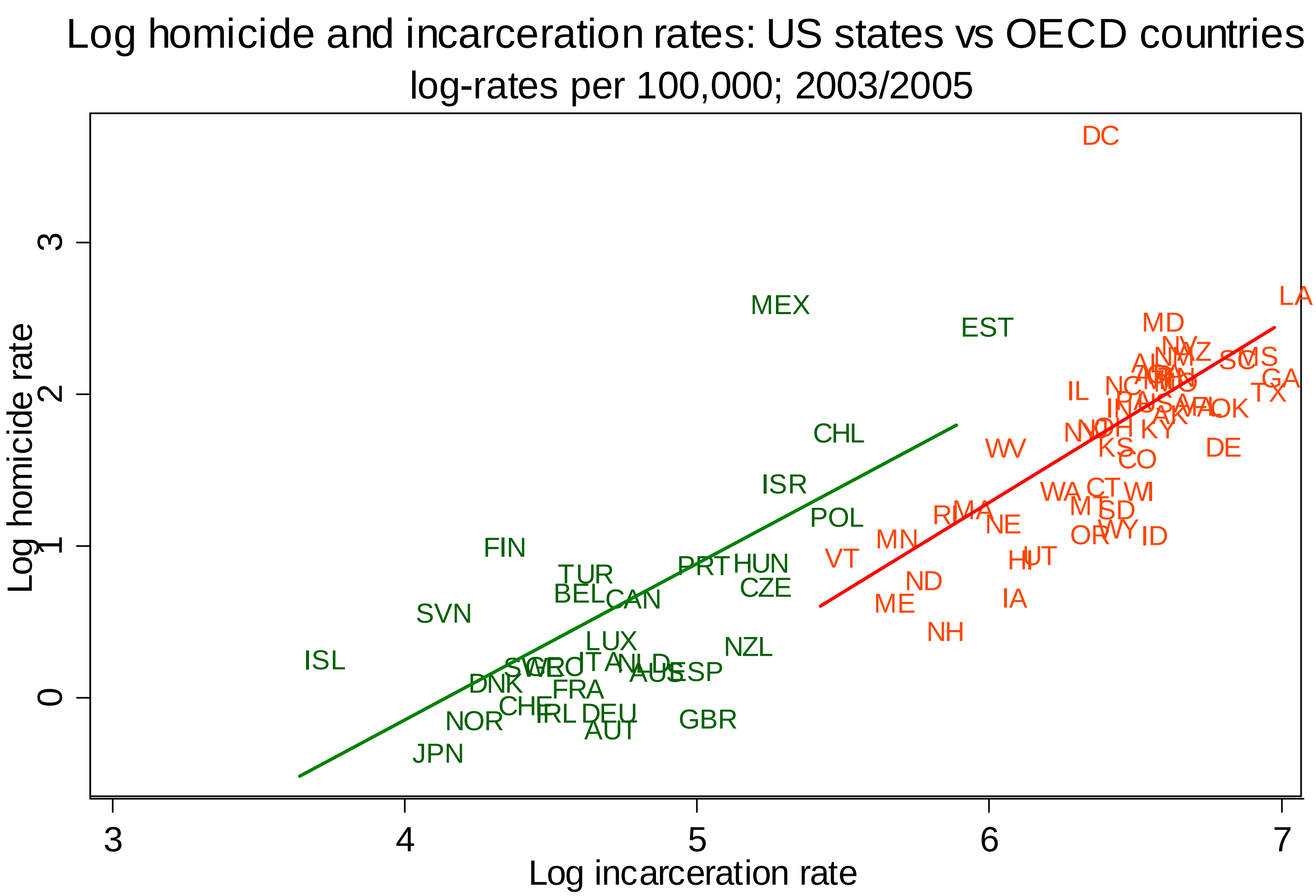
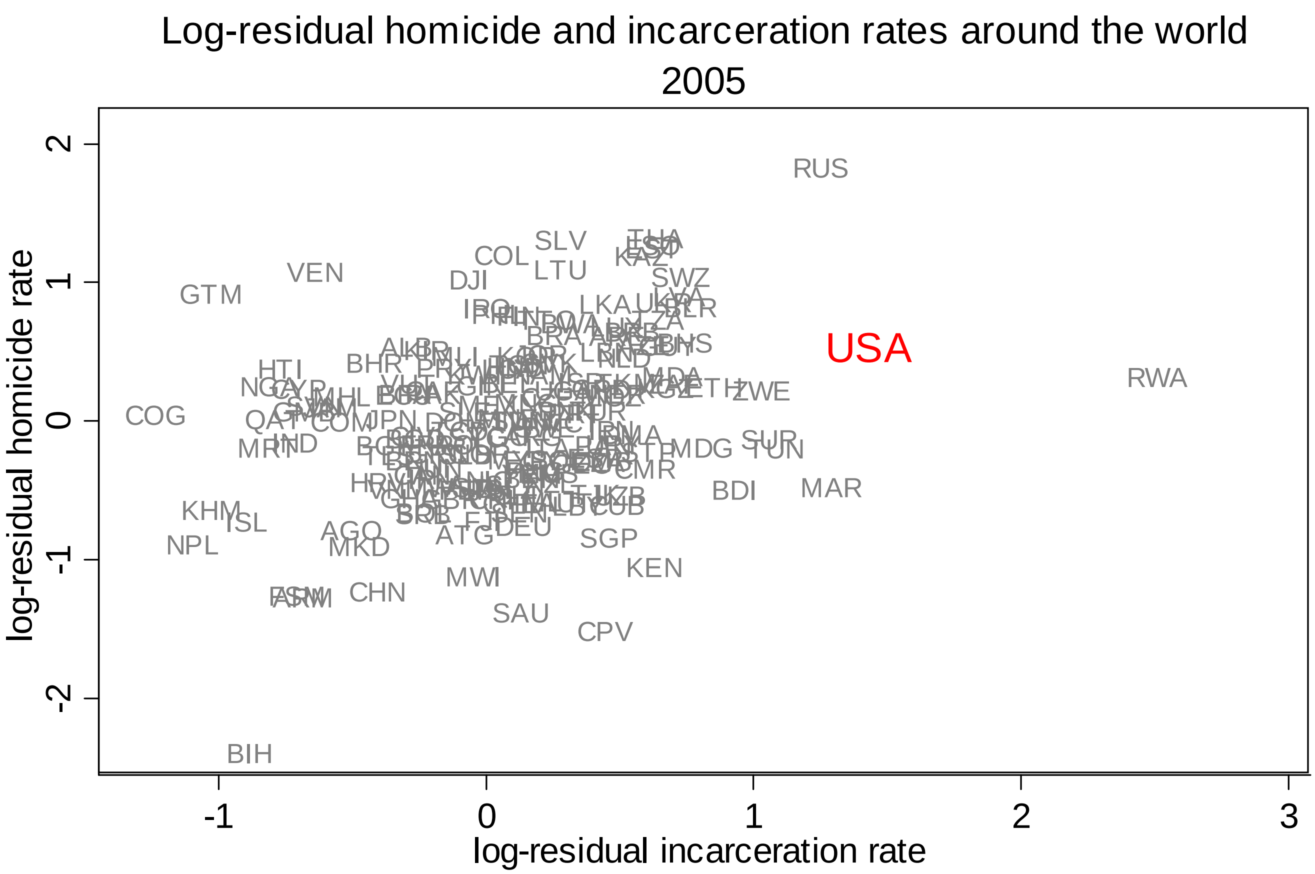


Figure 2: Individual US states vs OECD countries



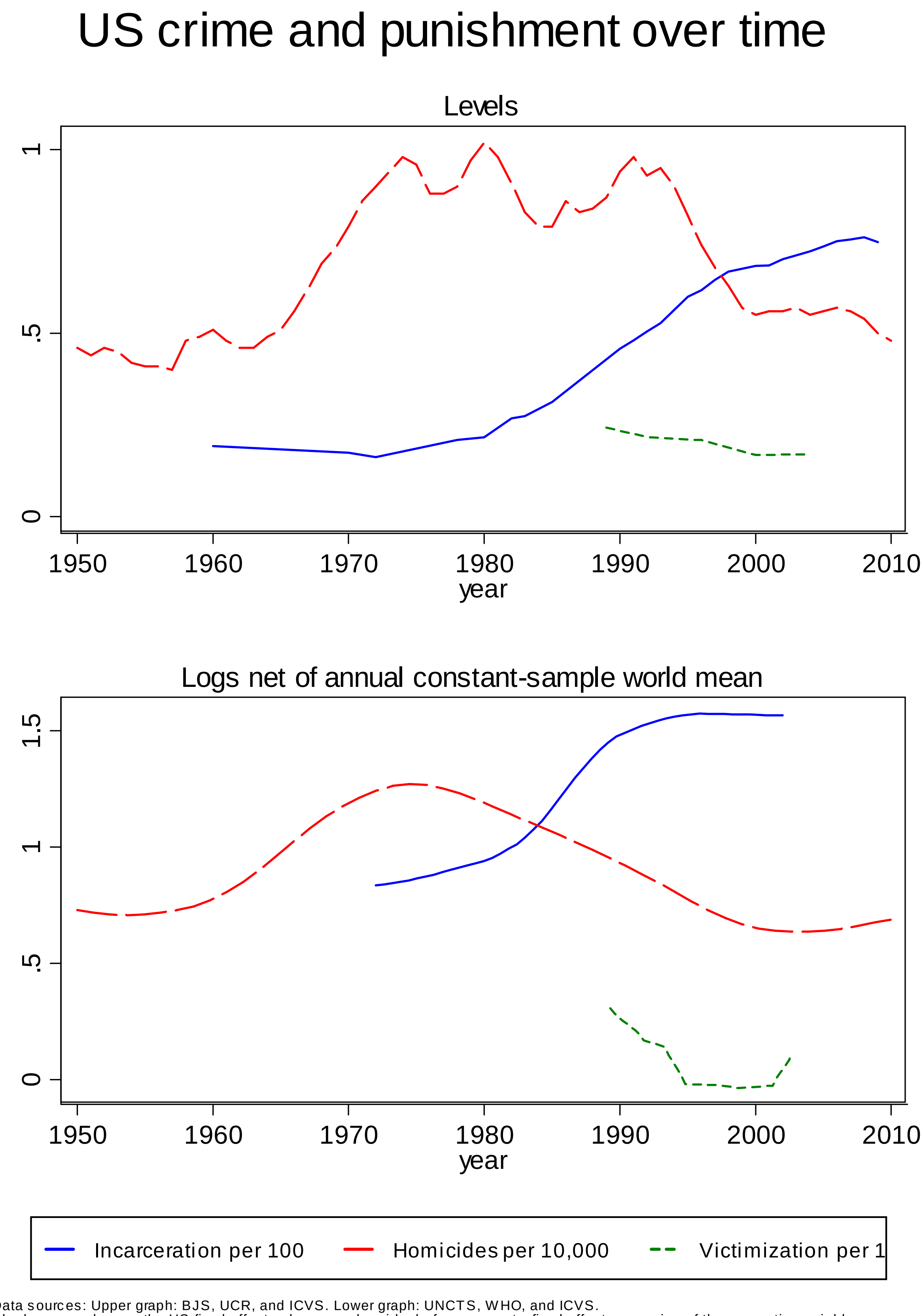
Source: UCR and BJS (US states); IHME-GBD and ICPS (OECD). Linear trend lines.

Figure 3: Residual homicide and incarceration



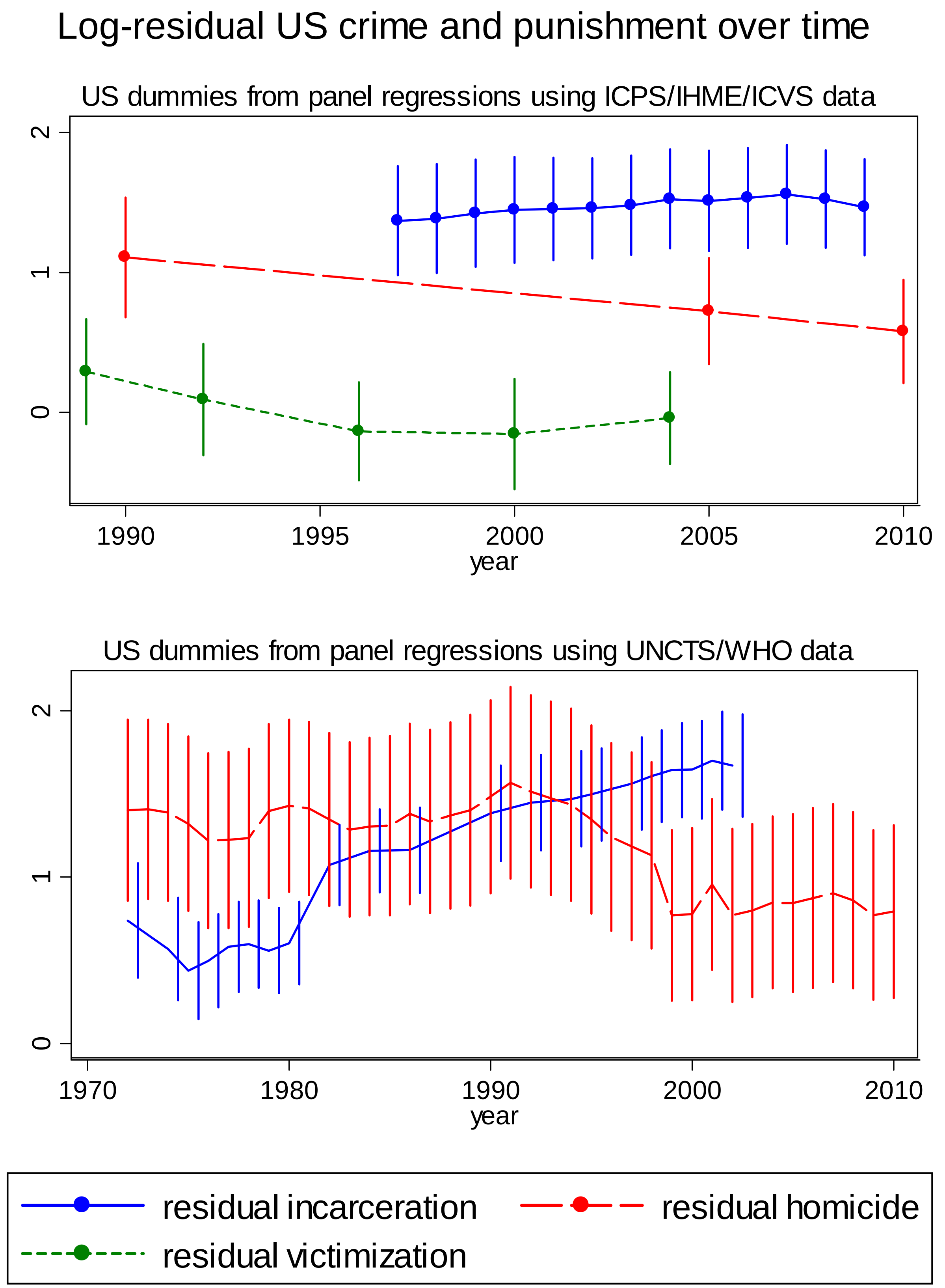
Residuals from models equivalent to models 2 and 6 of table 3 except that they do not contain a separate US dummy.

Figure 4: Raw US crime and punishment over time



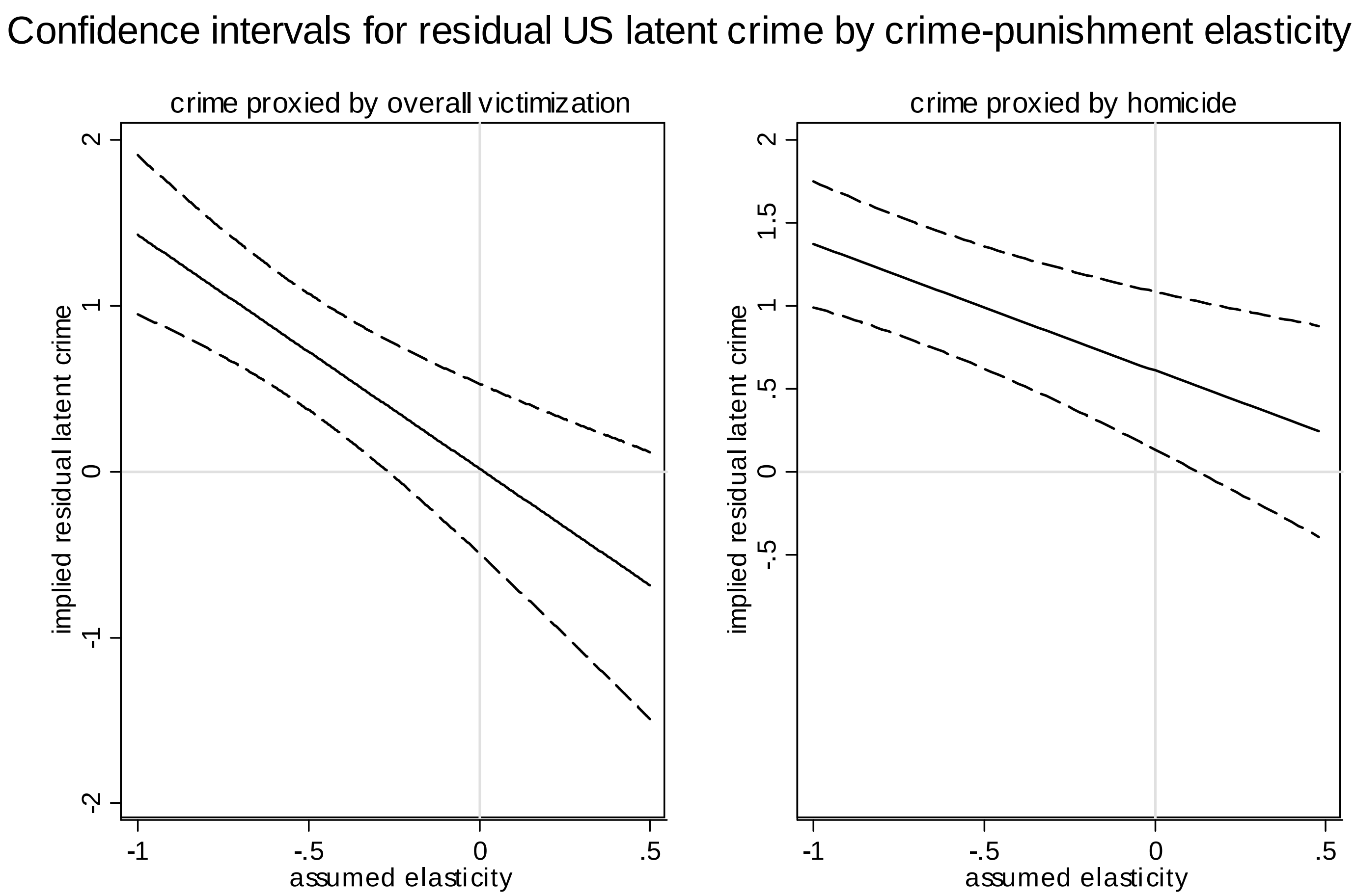
Data sources: Upper graph: BJS, UCR, and ICVS. Lower graph: UNCTS, WHO, and ICVS.
The lower graph smooths US fixed effects plus annual residuals from a country fixed effect regression of the respective variable, log-transformed, on year dummies and, in the case of WHO and ICVS, ICD version and sweep dummies, respectively.

Figure 5: Residual US crime and punishment over time



Estimated US dummies and 95% confidence intervals from panel regressions with missing value dummies (OLS+) and a quadratic trend. Standard errors are clustered at the country level. All scales are in logs. The independent variables are the same as in tables 3 and 4 except that graph 2 omits labor laws, unemployment, and teen birth rates, and uses Gini data from UTIP instead of the OECD or World Bank.

Figure 6: Confidence intervals: epsilon_k over eta



Point estimates and 95% confidence intervals from linear combinations of regression coefficients (see formula in section 5.2).
The right plot has been drawn using MI estimates. The left plot derives from regressions with dummies indicating missing values.

Table 1: Main variables

Summary statistics, US values, OECD means excluding US

Variable (source)	N	Mean	Median	S.d.	US	OECD\US
<u>Dependent variables</u>						
Prisoners per 100,000 (ICPS)	199	165	125	132	740	126
Homicides per 100,000 (IHME-GBD)	187	9.09	5.18	12.11	6.99	2.43
Victimization, 1-yr prevalence rate, 1989-2005 (ICVS)						
Any	162	0.23	0.21	0.10	0.20	0.18
Car theft	166	0.01	0.01	0.01	0.02	0.01
Burglary	165	0.03	0.02	0.03	0.03	0.02
Robbery	166	0.07	0.05	0.05	0.05	0.04
Theft	165	0.02	0.01	0.02	0.01	0.01
Assault	165	0.04	0.03	0.02	0.05	0.03
Drug deaths per 100,000 (IHME-GBD)	187	1.54	0.97	1.69	5.94	1.96
Drug use, 1-yr prevalence %, 2000-2011 (WDR)						
Cocaine	99	0.69	0.60	0.63	2.16	0.89
Ecstasy	101	0.52	0.30	0.55	1.20	0.79
Opiates	103	0.35	0.20	0.43	0.57	0.23
<u>Independent variables</u>						
GDP per capita PPP $1,000s (PWT 7.1)	189	12.1	6.3	14.4	42.5	29.9
English legal origin (LLSV 1999)	219	0.34	0	0.48	1	0.19
Socialist legal origin (LLSV 1999)	219	0.18	0	0.38	0	0.16
Federalism (CCP)	197	0.16	0	0.37	1	0.26
Democracy (derived from WB DPI)	214	0.74	1	0.44	1	1
Proportional democracy (id.)	214	0.36	0	0.44	0	0.70
Freedom (Fraser Institute)	194	0.63	0.67	0.33	1	0.98
Protestant/pop. (ARDA)	199	0.14	0.05	0.21	0.40	0.21
Catholic/pop. (ARDA)	199	0.28	0.14	0.31	0.26	0.40
Muslim/pop. (ARDA)	199	0.24	0.03	0.36	0.01	0.06
Slave descendants/pop. [see appendix]	165	0.04	0.00	0.13	0.05	0.00
Ethnic fractionalization (Alesina et al. 2003)	190	0.44	0.43	0.26	0.49	0.24
Immigrants/pop. (WDI)	209	0.11	0.04	0.17	0.13	0.11
Urban/pop. (WDI)	210	0.56	0.56	0.24	0.81	0.76
Males 15-19/pop. (US Census IDB)	224	0.05	0.05	0.01	0.04	0.03
Teen/total births (t-25) (UN Demographic YB)	108	0.13	0.12	0.07	0.16	0.08
Gini (OECD, WDI)	182	0.41	0.40	0.09	0.38	0.32
Difficulty firing worker 2007 (Doing Business)	178	0.31	0.30	0.23	0.00	0.31
Unemployment rate (ILO/WDI)	174	0.09	0.08	0.06	0.05	0.07

Unless otherwise indicated, values are for 2005.

For more detailed explanations of the sources, see section 3.1 and the appendix.

Table 2: Missing Values of Independent Variables in the Regression Samples

Dependent variable	Incarce-ration	Homicides / Drug deaths	ICVS	Cocaine	Ecstasy	Opiates
Total number of countries (observations) in the regression sample	172	183	72 (153)	88	92	96
Countries (obs.) with missing values	105	116	32 (52)	37	42	56
Slave descendants/pop.	*24*	*23*	*2 (2)*	*11*	*11*	*4*
Ethnic fractionalization	*4*	*1*		*1*	*1*	*0*
Immigrants/pop.	*1*	*1*		*1*	*1*	*1*
Urban/pop.	*1*	*1*		*1*	*1*	*1*
Males 15-19/pop.	*1*	*1*	*1 (11)*	*0*	*0*	*0*
Teen/total births (t-25)	*88*	*100*	*28 (48)*	*27*	*32*	*52*
Gini	*12*	*10*		*3*	*5*	*1*
Difficulty firing worker	*10*	*11*	*2 (3)*	*5*	*5*	*5*
Unemployment rate	*17*	*15*	*0 (12)*	*6*	*6*	*1*

This table shows the number of missing values imputed or otherwise dealt with as described in section 3.3.

In the ICVS regressions, one country can have multiple country-year observations.

Table 3: Log-Homicides and Log-Incarceration

	Log-Homicide rate (2005)				Log-Incarceration rate (2005)			
	OLS	OLS+	MI	FIML	OLS	OLS+	MI	FIML
USA dummy	0.84***	0.62***	0.62**	0.55**	1.57***	1.44***	1.37***	1.35***
	(0.26)	(0.23)	(0.26)	(0.22)	(0.17)	(0.20)	(0.20)	(0.18)
GDP per capita	-0.02	-0.02	-0.01	-0.01	-0.01	-0.01	-0.00	-0.00
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
ln(GDP per capita)	0.31	-0.06	0.01	0.02	0.27*	0.30***	0.33**	0.35***
	(0.26)	(0.11)	(0.14)	(0.13)	(0.14)	(0.10)	(0.16)	(0.13)
English legal origin	-0.35	0.00	-0.14	-0.16	0.06	0.08	-0.05	-0.07
	(0.26)	(0.16)	(0.18)	(0.17)	(0.18)	(0.15)	(0.16)	(0.15)
Socialist legal origin	0.41	0.38*	0.11	0.04	0.67***	0.66***	0.30	0.27
	(0.31)	(0.21)	(0.26)	(0.26)	(0.21)	(0.18)	(0.21)	(0.19)
Federal state	0.18	0.20	0.12	0.13	-0.15	-0.11	-0.15	-0.15
	(0.22)	(0.18)	(0.19)	(0.18)	(0.13)	(0.13)	(0.13)	(0.12)
Democracy	0.79**	0.14	0.36	0.45*	-0.33	-0.12	0.07	0.10
	(0.31)	(0.16)	(0.25)	(0.24)	(0.22)	(0.17)	(0.18)	(0.16)
Proportional voting	0.10	-0.08	-0.05	-0.06	-0.21	-0.24*	-0.28*	-0.27**
	(0.20)	(0.16)	(0.17)	(0.16)	(0.15)	(0.13)	(0.14)	(0.13)
Freedom	-1.71***	-0.18	-0.38	-0.34	0.25	0.18	0.15	0.18
	(0.47)	(0.33)	(0.38)	(0.34)	(0.46)	(0.26)	(0.30)	(0.29)
Protestant/pop.	0.42	0.50	0.14	0.04	-0.72**	-0.48	-0.86***	-0.92***
	(0.56)	(0.33)	(0.41)	(0.37)	(0.31)	(0.30)	(0.33)	(0.31)
Catholic/pop.	-0.07	0.27	0.02	-0.07	-0.37	-0.22	-0.40	-0.45*
	(0.41)	(0.25)	(0.31)	(0.31)	(0.28)	(0.22)	(0.25)	(0.24)
Muslim/pop.	-1.07**	-0.70***	-0.79**	-0.79***	-0.64*	-0.51**	-0.56*	-0.57**
	(0.50)	(0.26)	(0.32)	(0.28)	(0.37)	(0.25)	(0.30)	(0.29)
Slave descendants/pop.	0.08	0.49	-0.40	-0.80	-0.03	0.27	-0.75	-0.99
	(1.32)	(0.34)	(0.94)	(0.71)	(0.65)	(0.41)	(0.75)	(0.69)
Ethnic fractionalization	0.47	0.21	0.44	0.44	0.31	0.17	0.27	0.28
	(0.45)	(0.29)	(0.30)	(0.27)	(0.25)	(0.26)	(0.28)	(0.25)
Immigrants/pop.	0.88	0.94	0.52	0.60	0.49	0.47	0.33	0.34
	(1.11)	(0.65)	(0.76)	(0.69)	(0.82)	(0.60)	(0.61)	(0.53)
Urban/pop.	-1.11*	-0.12	0.04	0.05	0.28	0.26	0.44	0.46
	(0.59)	(0.40)	(0.43)	(0.41)	(0.44)	(0.35)	(0.36)	(0.35)
Males 15-19/pop.	25.16	34.39***	31.41**	34.09***	21.86*	21.02**	23.10**	25.32**
	(19.66)	(12.52)	(13.33)	(12.62)	(11.93)	(10.02)	(11.23)	(10.23)
Teen/total births (t-25)	3.80	3.49***	4.51	5.71**	1.21	2.44***	5.21***	5.73***
	(2.30)	(1.23)	(2.74)	(2.42)	(1.37)	(0.93)	(1.65)	(1.46)
Gini	5.23***	3.14***	2.79**	2.66***	1.32	1.19	0.24	0.10
	(1.39)	(0.89)	(1.10)	(0.99)	(1.11)	(0.77)	(0.92)	(0.81)
Difficulty firing worker	-0.26	-0.21	-0.38	-0.35	0.20	0.05	0.07	0.07
	(0.37)	(0.30)	(0.32)	(0.28)	(0.32)	(0.24)	(0.27)	(0.27)
Unemployment rate	2.11	2.63*	3.93*	4.84**	1.93	0.70	2.47	2.91*
	(3.36)	(1.42)	(2.23)	(2.14)	(1.77)	(1.06)	(1.82)	(1.58)
Observations (countries)	67	183	183	183	67	172	172	172
R^2	0.85	0.62			0.69	0.53		
joint *p*- value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Robust standard errors in parentheses. Dependent variables are log-transformed rates per 100,000 (IHME and ICPS).

OLS+ sets missing values to zero and adds a dummy variable equal to one for those observations.

MI uses multiple imputation to fill in missing values. FIML derives the joint likelihood.

Table 4: Log-Victimization (ICVS 1989-2005)

	Overall	Car theft	Burglary	Theft	Robbery	Assault
USA 2004 dummy	-0.04	-0.02	0.34	0.01	-0.45	-0.19
	(0.16)	(0.35)	(0.35)	(0.24)	(0.35)	(0.21)
GDP per capita	-0.00	-0.03	-0.01	0.01	-0.01	0.01
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
ln(GDP per capita)	-0.10	-0.17	0.03	-0.32*	-0.42*	-0.28*
	(0.11)	(0.33)	(0.21)	(0.16)	(0.21)	(0.16)
English legal origin	0.24	1.37***	0.05	0.31	0.33	0.67***
	(0.15)	(0.36)	(0.25)	(0.24)	(0.32)	(0.21)
Socialist legal origin	0.11	0.01	-0.23	0.24	0.33	0.27
	(0.15)	(0.45)	(0.26)	(0.23)	(0.31)	(0.22)
Federal state	0.03	-0.39	-0.32**	0.18	0.02	-0.07
	(0.08)	(0.29)	(0.16)	(0.13)	(0.18)	(0.13)
Democracy	-0.40*	0.12	-0.54	-0.32	-0.05	-0.37
	(0.23)	(0.50)	(0.40)	(0.27)	(0.35)	(0.27)
Proportional voting	0.35***	0.66**	0.24	0.32**	0.57**	0.41**
	(0.12)	(0.29)	(0.22)	(0.16)	(0.25)	(0.17)
Freedom	0.02	-1.44*	0.10	-0.17	-0.83	-0.30
	(0.30)	(0.85)	(0.56)	(0.47)	(0.64)	(0.36)
Protestant/pop.	-0.02	0.62	-0.65	0.39	0.32	0.59*
	(0.19)	(0.48)	(0.41)	(0.44)	(0.47)	(0.33)
Catholic/pop.	-0.15	1.21***	-0.30	0.24	0.51	-0.07
	(0.17)	(0.38)	(0.33)	(0.38)	(0.38)	(0.28)
Muslim/pop.	-0.32	1.36**	-0.37	0.02	0.44	-0.62**
	(0.28)	(0.62)	(0.49)	(0.41)	(0.50)	(0.30)
Slave descendants/pop.	1.74	2.12	-3.14	0.51	0.86	6.69**
	(2.16)	(3.97)	(4.68)	(3.34)	(3.82)	(2.79)
Ethnic fractionalization	0.01	-0.58	0.21	-0.21	-0.073	0.16
	(0.21)	(0.51)	(0.32)	(0.29)	(0.37)	(0.21)
Immigrants/pop.	0.24	2.10	1.25	1.64	2.29	1.76
	(0.63)	(2.20)	(1.39)	(1.45)	(1.42)	(1.11)
Urban/pop.	0.84**	3.98***	0.58	0.87*	1.96**	1.00**
	(0.34)	(0.97)	(0.61)	(0.51)	(0.81)	(0.45)
Males 15-19/pop.	8.31	-31.0	24.0*	8.10	7.54	12.3
	(7.46)	(20.6)	(14.2)	(13.5)	(17.9)	(11.5)
Teen/total births (t-25)	-0.28	6.80*	2.31	2.54	1.43	0.36
	(1.55)	(3.73)	(2.69)	(2.68)	(3.11)	(2.02)
Gini	0.67	-0.12	1.79	-0.47	3.90***	-0.28
	(0.64)	(1.81)	(1.22)	(1.14)	(1.40)	(1.12)
Difficulty firing worker	-0.18	-0.66	-0.57	-0.06	-0.47	-0.03
	(0.20)	(0.44)	(0.35)	(0.30)	(0.42)	(0.29)
Unemployment rate	-0.12	2.21	-0.41	-0.05	-0.12	0.33
	(0.60)	(1.68)	(0.99)	(0.91)	(1.14)	(0.74)
Observations (country X year)	153	153	153	153	153	153
Clusters (countries)	72	72	72	72	72	72
joint *p* -value	0.004	0.000	0.002	0.206	0.000	0.000

All dependent variables are log-transformed. Missing data on independent variables multiply imputed.
Pooled OLS with sweep, surveytype, and annual US fixed effects, and equal country-weighting.
Country-clustered standard errors in parentheses.

Table 5: Drugs

	Log-Deaths (2005)	Log-Prevalence (2000-2011) Cocaine	Ecstasy	Opiates
USA dummy	0.54**	-0.09	-0.14	1.28*
	(0.26)	(0.48)	(0.43)	(0.66)
GDP per capita	0.00	0.03	0.05*	-0.04
	(0.01)	(0.02)	(0.03)	(0.03)
ln(GDP per capita)	0.43***	-0.63*	-0.42	0.15
	(0.14)	(0.34)	(0.38)	(0.37)
English legal origin	0.05	1.49***	0.90**	0.47
	(0.20)	(0.39)	(0.38)	(0.52)
Socialist legal origin	0.44	0.31	1.57***	0.73
	(0.31)	(0.56)	(0.39)	(0.53)
Federal state	0.13	0.34	0.00	-0.18
	(0.18)	(0.23)	(0.27)	(0.42)
Democracy	0.26	0.18	-0.14	0.15
	(0.23)	(0.45)	(0.38)	(0.42)
Proportional voting	-0.23	0.45	-0.01	-0.65
	(0.17)	(0.34)	(0.31)	(0.45)
Freedom	0.10	0.85	0.36	0.68
	(0.37)	(0.58)	(0.56)	(0.73)
Protestant/pop.	1.72***	0.93	-0.44	-1.20
	(0.43)	(0.62)	(0.71)	(1.21)
Catholic/pop.	0.14	1.68***	0.21	-0.86
	(0.34)	(0.45)	(0.48)	(0.85)
Muslim/pop.	0.20	0.57	0.89	0.22
	(0.33)	(1.02)	(1.05)	(0.78)
Slave descendants/pop.	-0.65	0.67	0.78	1.19
	(0.91)	(1.37)	(1.48)	(1.82)
Ethnic fractionalization	-0.18	-0.90*	0.27	0.69
	(0.32)	(0.51)	(0.55)	(0.69)
Immigrants/pop.	1.65**	-0.78	3.20	0.62
	(0.79)	(1.50)	(1.97)	(1.60)
Urban/pop.	-0.72	1.95*	0.27	-0.00
	(0.47)	(0.99)	(0.98)	(0.87)
Males 15-19/pop.	11.31	-39.20	-50.01*	0.76
	(13.47)	(26.00)	(25.86)	(27.67)
Teen/total births (t-25)	1.38	5.53	-2.80	-2.77
	(2.76)	(3.35)	(3.29)	(5.71)
Gini	1.36	0.61	2.82	0.58
	(1.27)	(2.09)	(2.43)	(2.57)
Difficulty firing worker	0.14	0.11	0.05	-0.23
	(0.34)	(0.50)	(0.52)	(0.75)
Unemployment rate	2.77	3.12	0.76	-0.20
	(1.82)	(2.77)	(2.15)	(4.29)
Observations (countries)	183	88	92	96
Joint *p* -value	0.000	0.000	0.000	0.009

Robust standard errors in parentheses. Missing data on independent variables multiply imputed.

Dependent variables in logs: death rates per 100,000 (IHME-GBD); % annual prevalence (WDR).