

"Legal Origins" of Crime and Punishment

Holger Spamann*

Harvard University

November 25, 2008

Abstract

[Draft - preliminary and incomplete]

This paper shows a robust correlation between legal origin and punitiveness in a global cross-section of 213 countries and territories. English legal origin is associated with half a standard deviation more inmates per capita than civil law origin. Moreover, English legal origin countries are twice as likely as other countries to retain the death penalty. Nevertheless, they also appear to have higher crime rates. This appears to be a cost associated with English legal origin. The finding suggests a reinterpretation of "legal origins" as different priorities in social and economic policies.

1 Introduction

The incidence and the intensity of crime and punishment vary enormously even between countries with similar economic development. For example, in 2002, the WHO recorded 5.4 murders per 100,000 inhabitants in the US, but only .68 in France (WHO 2004). Incarceration rates – measured as inmates per 100,000 inhabitants – now range from the global maximum of 751 in the US to 91 in France to 36 in Iceland, not far from the Republic of Congo's global minimum of 22 (Liptak 2008). And while many US states and foreign countries retain the death penalty, other countries vehemently oppose it, notably in Europe (Anckar 2006).

This paper explores the pattern of these differences in the broadest possible cross-section of countries. It finds only three factors that are more or less consistently correlated with crime and punishment around the world: the level of economic development, income inequality, and – legal origins. In the 213 countries and territories for which incarceration rates are known, English legal origin is associated with on average half a standard deviation more inmates per

*hspamann@law.harvard.edu. I am grateful to Lucian Bebchuk, Martin Gelter, Andrew Hammel, Louis Kaplow, Dan Klerman, Mark Ramseyer, Mark Roe, Andrei Shleifer, Tom Vogl, and participants at Harvard Law School's Law and Economics seminar and SJD colloquium for helpful comments and suggestions.

capita than civil law. Similarly, as reported in Green and West (2008), in a sample of 186 countries for which Amnesty International tracks use of the death penalty, English legal origin countries are twice as likely as other countries to retain the death penalty. The relationship holds in both the developed and the developing world, and is robust to excluding outliers and controlling for various other determinants suggested in the literature. Still, English legal origin countries appear to have more crime, although this relationship is less robust.

While the prior literature had long theorized and analyzed the level of development and income inequality as determinants of crime and punishment (e.g., Messner, Raffalovich, and Shrock 2002; Soares 2004; Whitman 2005; Downes and Hansen 2006a/b; Gibson and Kim 2008), it had not appreciated the role of legal origins. This is all the more surprising because no other factors that the literature has focused on are robustly correlated with crime rates, incarceration rates, or the death penalty, as this paper shows.

Two hitherto entirely separate literatures motivate the exploration of legal origins as a determinant of crime and punishment. Criminologists studying smaller samples of mainly OECD countries have consistently observed higher incarceration rates in Anglo-Saxon countries than elsewhere, especially Scandinavia (Tonry and Farrington 2005; Cavadino and Dignan 2006a/b; Tonry 2007:30). They have also found higher crime rates in these countries, without necessarily drawing a link (e.g., van Dijk, van Kesteren, and Smit 2007). Recently, Greenberg and West (2008) found in a larger sample that common law countries are significantly more likely to retain the death penalty. An important question was whether the Anglo-Saxon effect is limited to developed countries and/or the death penalty, or whether it extends beyond the small samples analyzed so far.

Economists, on the other hand, have documented pervasive correlations between legal origin, legal rules, and economic outcomes in up to 152 countries in areas ranging from civil procedure (Djankov et al. 2003b) to military conscription (Mulligan and Shleifer 2005a/b). Formally, legal origin is defined as the descent of a country's legal system from one of the major legal systems of Europe. The main distinction is between those countries that were influenced by English common law, and those influenced by continental civil law (comprising the French, German, and Scandinavian subfamilies).¹ Western domination over the last centuries diffused the European legal models throughout the world, even though the degree of influence varies considerably from country to country. Because of this history, however, English legal origin is almost perfectly correlated with Anglo-Saxon (colonial) heritage. Unknowingly, economists and criminologists may have identified different consequences of the same underlying phenomenon, and both might benefit from viewing their results in light of the others'.

The legal origins literature itself strongly suggests that legal origins should matter for crime policy. Interpreting the available evidence in a recent survey, the leading authors La

¹Socialist countries followed a different legal path and were therefore classified as a separate category.

Porta, Lopez-de-Silanes, and Shleifer (henceforth LLS) (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 a modern society. The link between legal origins and the criminal justice system is therefore congenial to the "legal origins theory" of LLS (2008). Yet this paper is the first to study it.²

In its empirical part, this paper emphasizes incarceration rates over crime rates and the death penalty for two reasons.³ First, as compared to crime rates, incarceration rates are relatively easily measured, and in fact available for almost the entire population of countries. By contrast, reasonably reliable crime data are available for only 75 countries, and they are a much less valid measure of crime than incarceration rates are of punitiveness (see Section 6 below). Second, as compared to the death penalty, incarceration is much more important, at least quantitatively speaking. Even in the US, one of only a handful of countries to execute sizeable numbers of people, only 42 people were executed in 2007, while the prison population stood at 2.3 million. Moreover, the correlation between common law and retention of the death penalty was already discovered and discussed by Green and West (2008). [Note to readers of this draft: This draft has even less data on the death penalty and crime rates than it should. I obtained the relevant data only very recently, and have not yet been able to perform all the tests that should be in the paper.]

As with all cross-sectional estimates, the interpretation of this paper's results is subject to important limitations. In general, cross-sectional estimates do not permit causal inference. The results shown in this paper are correlations, and should not be confused with causal relationships. In fact, with respect to the crime-punishment nexus proper, the problem of reverse causality (Levitt and Miles 2007) would be so severe that almost all the regressions shown in this paper do not include either crime or punishment as a possible determinant of the other; including them would not only yield inconsistent estimates for that regressor but could also bias the estimates for the other variables (in any event, unreported regressions did include them as regressors, with essentially unaffected results). This being said, the cross-sectional estimates can be informative for variables with less severe reverse causation concerns, such as income inequality, or the level of development. A robust cross-sectional correlation makes a causal link more plausible; inversely, the lack of a correlation should at least raise doubts whether some factor could be of first-order importance. In this sense, cross-sectional estimates are an indispensable complement to other approaches. While other empirical strategies allow causal inference (e.g., Abrams 2007), their external validity is

²The only prior glimpses at crime policy through the lense of legal origins are comparisons of the severity of criminal sanctions on the book for breaches of securities (LLS 2006) and corporate law (Djankov et al. 2008). The evidence is mixed, but English legal origin tends to be associated with harsher punishment threats, in line with the findings of the present paper.

³There are other variables of interest for assessing punitiveness in relation to prisons, such as the prison admission rate (cf. Young and Brown 1993, Pease 1994, Blumstein et al. 2005), but these are not nearly as widely available and perhaps not as interesting.

low. The estimated effect is local and cannot be extrapolated to other situations without a structural model, which is far beyond the current research frontier.

For legal origin in particular, a causal interpretation of its correlation with crime and punishment is plausible because legal origin is almost certainly exogenous. The problem, however, is that the meaning of legal origin itself is highly uncertain. As mentioned above, while legal origin is *defined* by reference to the legal system's lineage, its actual empirical *implementation* as a dummy variable denoting this lineage is for all practical purposes indistinguishable from any other influence by the colonial power, be it cultural, political, economic, or otherwise.⁴ Other authors have discussed specificities of Anglo-Saxon countries without drawing a link to the common law (e.g., Esping-Andersen 1990; Hall and Soskice 2001; Soskice and Iversen 2008). Section 7 will return to this question. In the meantime, the expression legal origin will be used in accordance with common usage in economics, but it should be remembered that *legal* origin need not be understood literally.

[Full review of empirical cross-country crime and punishment literature to be added.] Two prior papers have empirically investigated the connection between legal families and penal policy without, however, drawing a connection to the legal origin literature. Greenberg and West (2008) discovered that by Mukherjee and Reichel's (1999) classification of legal systems, common law countries were significantly more likely than others (except Islamic law countries) to have the death penalty. The present paper confirms this finding with the classification from La Porta et al. (1999) and a different set of control variables. Ruddell (2005) regressed incarceration rates on Mukherjee and Reichel's (1999) legal family dummies and other variables, and found that the coefficients for both common law and civil law were positive, the former being larger. However, Ruddell (2005) did not report statistical tests for the difference between the two coefficients, restricted the sample to the world's 100 richest countries, and included a number of endogenous regressors raising econometric concerns (e.g., the homicide rate). The comparison between common and civil law effects on incarceration rates therefore remained an open question.

2 Description of the main data and the sample

The paper employs the broadest available cross-country datasets for legal origins, crime, and punishment.

The classification of countries by legal origin is from La Porta et al. (1999). While some of the codings are controversial (e.g., of mixed systems like South Africa's as common law, or certain Asian countries like Japan as German civil law), using this dataset has the major advantage of continuity with prior research. For purposes of this paper, the data were

⁴Nevertheless, Rostowski and Stacescu (2006) and Licht, Goldschmidt, and Schwartz (2007) attempt to disentangle these two.

updated for 13 countries and territories not in the original dataset, using new editions of the sources used in La Porta et al. (1999) (CIA 2008; Reynolds and Flores 2008) and classifying as socialist legal origin successor states of countries classified as socialist in La Porta et al. (1999) (this was relevant for the former Yugoslavia). Unlike LLS (2008), the present paper maintains socialist legal origin as a separate category to capture the special position of the transition economies with respect to crime and crime policy (cf. Lappi-Seppälä 2008). Alternatively, one could have reclassified formerly socialist countries as civil law countries but added a dummy for transition countries. Of the 214 observations for which data on at least one dependent variable is available, only Palau could not be assigned a legal origin with these sources, owing to Palau's checkered colonial history.⁵

The International Center for Prison Studies (ICPS) at King's College, London, provides the data on the number of prisoners, including pre-trial detainees, per 100,000 population, in 2007 or the latest date available. Most of these data come directly from national prison authorities. Cross-checks against other data sets have confirmed the data's high reliability (Neapolitan 2001; Lappi-Seppälä 2008). The data set covers an unrivalled 217 countries and territories. To match the unit of observation of other variables, prisoner data were combined for the UK (population-weighted average of England and Wales, Scotland, and Northern Ireland) and the Channel Islands (population-weighted average of Guernsey and Jersey). This left 214 observations. One of the 214 was Palau; for all others, legal origin data is available as well.

Data on the death penalty in 2007 for 186 countries come from Amnesty International's (AI) website and are widely used in the literature (e.g., Anckar 2006; Greenberg and West 2008). AI provides data for three different definitions of whether a country has the death penalty, as well as numbers on the frequency of its actual application. The results do not depend on the choice of data series, and Section 5 only provides estimates for the most relevant one, which is whether the death penalty continues to be applicable in general criminal cases.

As already mentioned above, crime data are the most difficult to come by. Usual crime statistics come from police reports. But many, perhaps most, crimes are not reported, particularly in places where the police is ineffectual or even engaged in criminal activity itself. As a result, the measurement error in these data is likely to be correlated with independent variables of interest (Gibson and Kim 2008). Hence these statistics cannot be used for comparative purposes (INTERPOL 1999; Newman and Howard 1999; Tonry and Farrington 2005). Homicides, being more severe and more visible, tend to be more thoroughly reported. It is therefore standard to use homicide rates as proxies for crime

⁵According to the CIA (2008), 27 of these 213 observations are dependent territories (such as the Channel Islands [UK], Aruba [Netherlands], and Hong Kong [China]). This might raise statistical concerns since the independence assumption is even more problematic for these territories than usual in cross-country regressions. However, dropping the dependent territories from the sample does not affect the results.

rates in comparative analyses (e.g., Neapolitan 2001; Greenberg and West 2008). However, the correlation of different measures of homicide (UNODC data from police statistics, and WHO data from death classifications by medical practitioners (Newman and Howard 1999)) with one another ($\rho = .69$) and other measures of crime (between $-.31$ and $.48$) is not particularly high, suggesting that after all the homicide measures may not be so reliable or valid, respectively. In this paper, they are used as a control in a few incarceration regressions, and as dependent variable in unreported regressions discussed in Section 6.

The best available measures of the comparative incidence of crime come from victimization studies, i.e., representative surveys eliciting experiences of victimization by various crimes (Tonry and Farrington 2005; Lynch 2006). Standardized comparative data have been collected in five sweeps of the International Crime Victims Survey (ICVS) between 1989 and 2005. The major shortcoming of these data is low coverage in any given sweep. Although 74 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). This paper therefore employs data from all five sweeps, controlling for the survey sweep to account for trends in crime over time. To get the most efficient estimates and avoid aggregation bias (Lynch 2006), the paper uses data from over 300,000 individual survey respondents rather than country averages, which allows controlling for individual-level covariates such as age or town size. The data measure whether an individual was the victim of certain types of crimes during the year prior to the survey.

Tables 1 and 2 show definitions, sources, and summary statistics for all the variables used in this paper [currently only for those used in the incarceration regressions].

3 Incarceration rates: Tests of means

Table 4 presents means and tests-of-means (t -statistics) by legal origin for the natural log of the incarceration rate. These numbers confirm that English legal origin countries are significantly more punitive than civil law countries. The data are log transformed (i.e., the means are geometric means) because the t -tests assume normal distributions while the raw data is bounded below at zero and right-skewed; using the raw data would yield even stronger results.⁶

In the full sample, English legal origin is associated with about 45% more prisoners per capita than civil law, an excess of about half a standard deviation. This difference is statistically significant at the 1% level. Removing the US outlier from the sample (column 2) does not materially affect the result - the difference is still .44 standard deviations (42%) and statistically significant at the 1% level. Perhaps most importantly, the result holds even

⁶In particular, with raw data, the difference between English and other legal origin appears significant in all but the OECD subsample.

if the sample is restricted to non-OECD countries (column 4) (difference of 41%, statistically significant at the 5% level). As noted above, the prior criminological literature had identified a correlation between Anglo-Saxon influences and punitiveness only in small samples of developed countries. The results presented here show that the association holds more broadly, even though the difference is almost twice as large in the OECD (77% = .84 standard deviations) – the effect of legal origins seems to get attenuated as one moves from the core to the periphery. Finally, the result also holds if one excludes small countries and territories (column 5) or dependent territories (column 6).

Comparisons of English legal origin to any of the civil law subgroups (French, German, Scandinavian) yield the same picture. As expected, Scandinavian (legal origin) countries have the lowest incarceration rates, over a full standard deviation below English legal origin. The French legal origin mean is highest among civil law subgroups but still over .4 standard deviations below English legal origin, and this difference is statistically significant at the 1% level in the full sample and at the 5% level otherwise (except in the OECD subsample). German legal origin falls between the French and Scandinavian, but because of the much smaller sample ($N \leq 7$) the difference reaches conventional significance thresholds only in the OECD subsample.

By contrast, the mean incarceration rate for socialist legal origin exceeds English legal origin's by about .2 standard deviations (although the difference is not statistically significant except in the independent countries subsample). Still, the difference between English and non-English legal origin, which confounds civil law and socialist legal origin, is positive (around 25%, or a quarter standard deviation) and statistically significant at the 5% (10%) level in the full sample (in the sample excluding the US). In the OECD subsample, the difference is even larger (63%, or .62 standard deviations) and again statistically significant at the 10% level. However, in the other subsamples (columns 4 to 6), the difference is smaller (around 20%) and falls short of the 10% significance threshold (p between .12 and .2).

4 Incarceration rates: Multivariate regressions

This Section reports results from regressing incarceration rates on legal origin and a panoply of control variables (Tables 4 to 7). Irrespective of the controls, and as in the univariate tests, English (socialist) legal origin remains associated with around a half (full) standard deviation more inmates per capita than civil law. In most of the empirical models, these point estimates are statistically different from zero at the 1% level, otherwise at the 5% level.

The results are informative in two distinct ways. First, the correlation between legal origin and incarceration rates does not appear to be spurious, i.e., legal origin is not merely a proxy for some exogenous third variable like religion that drives incarceration rates and happens to be correlated with legal origin in the sample. Second, none of the proposed

control variables captures the channel through which legal origin affects incarceration rates. The second point deserves to be separately mentioned because many of the control variables may in fact themselves be affected by legal origin. For example, Mahoney (2001) has argued that English legal origin causes higher GDP growth, and Botero et al. (2004) have argued that English legal origin is associated with less restrictions on dismissing workers, both of which are variables used below. Likewise, juries are associated with English legal origin both in popular perception and in the data (Voigt 2008).

[NB: The following regressions are not yet in the tables and the main text: alongside $\ln(\text{GDP per capita})$ and legal origins, controls for corruption, migration (both stock and flow), case law, and GDP growth. None of these affects the estimate for legal origin or $\ln(\text{GDP per capita})$. More corrupt countries have more prisoners, and countries which recognize case law have less. Migration and GDP growth have no significant correlation with the incarceration rate.]

4.1 Regression specifications

All the relationships are estimated using OLS with heteroskedasticity-robust standard errors (Huber/White). The dependent variable in all reported regressions is the natural logarithm of inmates per 100,000 population (in a Zarembka test, the equivalence of the level specification is rejected at the .1% level). As is customary in the legal origin literature, the reported regressions control for the natural logarithm of GDP per capita (2006) to hold the level of development fixed. The legal origin dummies are English and socialist, with civil law as the omitted category.

The control variables include proxies for all of the major theories of comparative punishment that are amenable to testing in the large cross-section. Among others, they include all the main variables from the prior cross-country quantitative work of Neapolitan (2001), Ruddell (2005), Downes and Hansen (2006a/b), and Lappi-Seppälä (2008) (or close substitutes if these are not available for larger samples⁷). Excellent reviews of these theories and prior empirical work are Whitman (2005), Tonry (2007), and Lappi-Seppälä (2008).

In particular, some regressions control for the level of crime, using data on homicide rates

⁷The main difference is that the classification of political systems – consensus vs. conflict democracies – from Lijphart (1999) used in Lappi-Seppälä (2008) is available for only 36 countries. In its stead, this paper uses a cruder variable of proportional vs. majority voting constructed as in Pagano and Volpin (2005). The lack of significant results may be due to the crudeness of this measure. By contrast, in a regression of $\ln(\text{inmates per capita})$ on $\ln(\text{GDP per capita})$, legal origin, and Lijphart’s index (for 1971-96), the latter’s coefficient is statistically significant at the 5% level and would imply that going from the most party-oriented system (Switzerland) to the most executive-oriented system (Jamaica) would almost triple the number of prisoners per capita. As in the large sample, no significant results obtain with the proportional voting variable in this small sample ($p = .24$). The English legal origin coefficient is around .5 in both regressions ($p = .076$ with proportional voting, and $p = .131$ with the Lijphart index) (of course, English legal origin is a very strong predictor of the Lijphart index, explaining on its own 28% of the variation of this index!). Adding Lijphart’s second dimension to these regression has no effect.

for the reasons discussed above. As mentioned in the introduction, crime rates, and some of the other control variables, may of course be simultaneously determined with incarceration rates, and OLS would not be proper for estimating the effect of crime on incarceration rates (Levitt and Miles 2007; Spelman 2008). However, as far as legal origin is concerned, including potentially endogenous control variables will merely bias the estimates for legal origin towards zero and make for a very strict test for legal origin.

Since different control variables are missing different observations, the intersection of the samples gets relatively small if all control variables are included simultaneously. Moreover, overcontrolling with simultaneously determined control variables will adversely affect the estimates for the other variables as well. Tables 5 to 7 therefore start by including the various control variables one by one. Table 8 then reports results from regressions with all controls available for at least 180 or 130⁸ countries, with and without two controls subject to strong simultaneity concerns (homicide rate, extrajudicial killings).⁹ In addition, column (7) of Table 5 reports results from a regression with all controls that are certainly exogenous to legal origin, and available for more than 65 countries (hence excluding the culture variables).

4.2 Results

The regressions confirm the results of the univariate tests, including the estimated economic magnitude of legal origin’s effect.

Regardless of the controls used, English legal origin is associated with almost half a standard deviation, or 40%, more prisoners per capita than civil law. The difference is statistically significant at the 1% level in most specifications, otherwise at the 5% level, dropping to $5\% \leq p \leq 10\%$ only with the individual religious fractionalization and transfers and subsidies variables, and the full set of controls (Table 8 column 3).¹⁰ The stability of the coefficient across specifications, and the match with the univariate tests, gives added confidence that the result is robust. The only control variables that decrease the coefficient estimate for English legal origin are the social policy variables on their own (Gini, transfers and subsidies, restrictions on firing workers); however, this effect is small (around one seventh), and it does not persist in the multiple control regressions. Of particular note is the result for homicide rates in Table 7 column 9: legal origin’s effect on incarceration rates appears to be orthogonal to crime rates, i.e., even holding the level of crime constant, English

⁸The next possible threshold would have been 100, which would have brought transfers and subsidies (109 observations) and the unemployment rate (103 observations) into the specification. However, this would have (1) brought the sample size down to 65, (2) included one variable with strong simultaneity problems (unemployment rate), and (3) included another (transfers and subsidies) that is the main cause of, and highly correlated with, a third (Gini). In any event, the results would not have materially changed.

⁹Following Alesina et al. (2003), these regressions include only one fractionalization measure at a time (those reported are for ethnic fractionalization).

¹⁰When the homicide rate and the extrajudicial killings variables are included, $p = .101$, but as explained above, the inclusion of these variables is econometrically improper.

(and socialist) legal origin countries imprison significantly more people.¹¹

The estimated effect for socialist legal origin varies a little from specification to specification but is generally almost one standard deviation (i.e., twice as many prisoners). The estimate is statistically significant at the 1% level in almost all specifications, otherwise at the 5% level.

The one odd set of results is Table 6, covering culture variables, including trust, religious intensity, and attitudes towards markets and welfare states. The inclusion of the culture variables does not reduce, and even increases, the estimates for legal origin, as shown by a comparison of columns 1, 3, 5, 7, and 9 with columns 2, 4, 6, 8, and 10, respectively. However, in the particular subsamples for which these culture variables are available, the effect of legal origin and even $\ln(\text{GDP per capita})$ is reduced, and dramatically so for the World Values Survey subsamples. This is odd because, controlling for $\ln(\text{GDP per capita})$, (English) legal origin does have a significant effect both in the smaller OECD subsample (and these countries are almost all in the culture sample) and in larger subsamples such as those of Table 8 (not reported). Hence the results in Table 6 seem to be due to an unusual sample draw.

Of the control variables, only $\ln(\text{GDP per capita})$, the social policy variables (Gini, transfers and subsidies as percentage of GDP), and perhaps the religious groups seem to be significantly correlated with incarceration rates. As in Lappi-Seppälä (2008), there is some indication that trust matters (an F-test returns $p = .01$ for a joint zero coefficient for the two trust variables). Results for attitudes towards income inequality and competition appear statistically significant in Table 5, but this is not robust to different specifications (using binary indicators, or medians, instead of means), and in any event the results would seem hard to interpret since conceptually the two significant coefficients point in opposite directions (this is so even when they are used separately).

[NB: The relationship between the level of development and incarceration rates is not linear. The positive coefficient for $\ln(\text{GDP per capita})$ disappears if one restricts the sample to the richer half of the world. Similarly, a control for the level of GDP has a negative coefficient in all these regressions, and is highly statistically significant. Future revisions of the paper will show that in the tables.]

4.3 Robustness

These regression results are robust to a battery of unreported robustness checks.

¹¹The result is qualitatively the same if one controls for the various measures of victimization introduced in Section 6.

4.3.1 Data from the 1970s

Unreported regressions verified that the correlation between legal origins and incarceration rates is not an artifact of the time period under consideration. The first and second UN surveys on crime provide incarceration data for the years 1970-1980. Filling missing observations by averaging over the available years, one obtains data for 56 countries. Again, English legal origin is economically and statistically highly significantly correlated with the natural logarithm of the incarceration rates in a regression controlling for $\ln(\text{GDP per capita})$.

4.3.2 Alternative variables and data

Using levels instead of the logarithm as dependent variable slightly reduces the R^2 but produces substantively identical results.¹²

Omitting the $\ln(\text{GDP per capita})$ control in the regressions of Tables 5 to 7 does not materially affect the results for legal origin. The fractionalization indices then pick up some of the development effect and enter significantly *negatively*, being strongly negatively correlated with GDP per capita (cf. Alesina et al. 2003, Fearon 2003).

The choice of fractionalization measure – ethnic, religious, or linguistic in the multiple control regressions (Table 8), or using alternative measures of fractionalization from Fearon (2003) instead of Alesina et al. (2003) – does not affect any of the results.¹³ Nor does using the revised Polity IV index instead of the freedom variable.¹⁴

Finally, the results for religious intensity – but not legal origin – in Table 6 column 11 do depend on the measure of religiosity used.¹⁵

4.3.3 Subsamples

The log transformation of the dependent variable reduces the impact of outliers in the reported regressions. This is illustrated in Table 8: the results with the US (columns 1-4) are not materially different from those without the US (columns 5-8).

¹²The homicide rate then enters significantly positively, while proportional voting enters significantly negatively when included alone.

¹³In the multiple control regressions, the linguistic fractionalization measure enters insignificantly negatively, while the religious fractionalization measure is not significant either and switches sign from one regression to the other. For the Fearon indices, the estimated coefficients are consistently negative (but significant only in the equivalent of Table 5, i.e., without all the other controls).

¹⁴However, the revised Polity IV index is available only for a smaller sample ($N = 150$), and in this smaller sample the coefficient for English legal origin is reduced by one third (but still significant at 10% - $p = .059$) even without either the freedom or the Polity IV variable.

¹⁵Among those suggested in McCleary and Barro (2006), the percentage of the population praying or attending religious services with some minimum frequency, or believing in an afterlife, yields similar insignificant results as those reported for self-reported religiosity, while the percentages of the population believing in God, hell, or heaven do yield significantly positive results (which would suggest that fully believing societies are about one standard deviation more punitive than fully non-believing societies).

There are at most seven dependent territories in any of the regressions (because most data is not available for dependent territories anyway). Eliminating those observations does not affect any of the results; in fact, the results are even slightly stronger with only independent countries.

The OECD subsample is too small ($N \leq 30$) for sensible regressions. In the non-OECD subsample, the coefficients for legal origin are generally reduced by one third to one half, and the statistical significance for English legal origin depends on whether the dependent variable is in levels or logs: with levels, the results remain significant almost throughout, while with logs they mostly fall short of conventional significance levels (as do almost all the controls). Some of this reduction may be due to the lower data quality in poorer countries, which biases coefficients downwards. But most of it seems attributable to an attenuation of the legal origin effect further from its core, which was already observed in the univariate tests above.

4.3.4 English vs. other legal origin; English legal origin vs. civil law subgroups

The socialist legal origin dummy is important for fitting the regressions. Without it, the R^2 goes down by about one half, and the estimates for other variables are reduced, especially for the Gini coefficient, and transfers and subsidies (which tend to be particularly low and high, respectively, in transition economies). In particular, the estimate for English legal origin, now in comparison to any other legal origin including socialist, is reduced by one third to one half and is usually not statistically significant.

Adding German and Scandinavian legal origin dummies to the regression – making French legal origin the omitted category – confirms the results from the univariate tests and the prior literature that Scandinavian (legal origin) countries are the least punitive, followed by German legal origin. These dummies fully absorb the effect of the Protestant variable. According to the point estimates, Scandinavian (German) legal origin is associated with over a full (half) standard deviation less inmates per capita than French legal origin, and this difference is statistically significant almost throughout at the .1% (1%) level.¹⁶ Concomitantly, the estimates for English and socialist legal origin are now about .1 smaller in the horseraces and .2 in the multiple control regressions, while the standard errors are unaffected, and this reduces the t -statistics for English legal origin below conventional levels of significance in about half the horseraces and the multiple control regressions. However, with levels as dependent variable the results for English legal origin remain significant throughout.

¹⁶There are a few exceptions. In the multiple controls regression (Table 8), the Scandinavian legal origin coefficient drops to just over half a standard deviation and becomes statistically insignificant when Gini and proportional voting are included. For German legal origin, the estimates are only marginally significant, or not at all, in the culture variable subsamples (Table 6), and drop to the 5 or 10% level of statistical significance when certain social policy or proportional voting variables are included in the regression.

5 Death penalty

As Greenberg and West (2008) discovered, English legal origin is also associated with a higher likelihood of retaining the death penalty. While Greenberg and West (2008) used a different classification of legal families (Mukherjee and Reichel 1999), Table 8 shows that this association also holds with the legal origin classification of La Porta et al. (1999).

In the sample of 186 countries for which Amnesty International collects data, English legal origin countries are twice as likely as other countries to retain the death penalty in ordinary criminal cases (the results are the same if one uses death penalty in any cases, or death penalty on the books but not applied). The respective fractions are 45 and 23%, and the difference is statistically significant at the 1% level in a simple two-sample test of proportion. The result is the same if socialist legal origin countries are excluded, i.e., if the comparison is between English and civil legal origin countries; it is even stronger if the German and Scandinavian civil law subgroups are added as separate regressors.

English legal origin is not just a proxy for other plausible explanatory variables. Adding controls for the level of development ($\ln(\text{GDP per capita})$), freedom, ethnic fractionalization, religious groups, or inequality does not affect the basic result, neither economically nor statistically.¹⁷ If anything, the difference between English and civil legal origin becomes larger once these other variables are controlled for. Interestingly, socialist legal origin is associated with an even lower likelihood of retaining the death penalty than civil law, and the difference between the two is statistically significant in most models. Excluding the US and the countries of the European Union from the sample (models 5 through 8) does not change any of the results.

As before, the effect of legal origin on the outcome variable is weaker in the peculiar sample for which the World Value Survey data on trust is available, but the trust data itself does not affect the legal origin estimate noticeably. Counter to expectation, a higher level of trust is associated with a higher likelihood of having the death penalty in these regressions, and the estimate is statistically significant for trust in people.

When one estimates the effect of legal origin and other controls on the number of actual executions in a tobit model, the results are very unstable, switching signs as a result of only slight changes in model specification. The reason is that the number of countries that actually carry out executions in any given year is very small. [To be added: tobit regressions with numbers of executions over multi-year periods.]

¹⁷All of the results for these other controls also hold if the $\ln(\text{GDP per capita})$ control is omitted.

6 Crime rates

An obvious question is whether civil law countries pay for lower incarceration rates and the lack of death penalty deterrence with higher crime rates. The allegedly better protection of property rights in English legal origin countries (La Porta et al. 1999; LLS 2008) might manifest itself in stronger efforts to combat (property) crime, leading to lower crime rates but higher incarceration rates in these countries. However, the cross-country regressions of crime and victimization rates on legal origin reported below tend to show the opposite. If anything, English legal origin is associated with higher crime rates.¹⁸

The regressions for which results are reported in Table 9 are weighted logit regressions of victimization in the year prior to the survey, with standard errors clustered at the country level. The ICVS provides survey weights to compensate for over- or undersampling of particular population groups within a country. For purposes of this paper, these weights were divided by the number of observations available for the respective country. This ensures that each country is weighted equally in the regressions, which is appropriate because the effects of interest are country-level effects, and the cross-country approach implicitly treats each country as a random draw from an infinite pool of countries. In addition to the country-level covariates for which marginal effects and standard errors are shown in the table, the regressions contain dummies for the survey sweep, as well as for those individual level controls that were identified as relevant in the prior literature on the ICVS data (Lynch 2006): the respondent's age group, the respondent's gender, and the size of the respondent's hometown.

The results for legal origin are mixed. Citizens of English legal origin countries are about 1.5% more likely to be victims of a burglary or an assault in any given year, but about .6% less likely to be robbed. These estimates differ a little between specifications but are generally statistically significant with p -values around .05. Economically, these estimates are quite meaningful given the low average probabilities of victimization shown in the last row of the table.

Most publications based on the ICVS (e.g., van Dijk, van Kesteren, and Smit 2007) emphasize the overall victimization measure, i.e., a measure of whether a respondent was victimized at least once over the last year by any of the following 9 crimes: car theft, theft from a car, bicycle theft, motorcycle theft, theft of personal property (like pickpocketing), burglary, attempted burglary, robbery, and assault.¹⁹ Depending on the regression specification, the point estimate for this probability is between 1.5% and 3.7% higher in English legal origin countries, and in one of the three specifications the t -statistic approaches 2.

In unreported regressions of the homicide rate (as measured by UNODC or the WHO)

¹⁸Pointing in the same direction is the positive if insignificant coefficient for homicide rates in the incarceration regressions above – higher crime and incarceration rates seem to go together, on average.

¹⁹Often the measure also counts sexual offenses. However, surveys were not consistent as to whether men were also asked about this crime. In order to use the entire data set, this paper therefore excluded sexual offenses.

on legal origins and controls for $\ln(\text{GDP per capita})$ and the Gini coefficient, the results were similarly mixed, with three out of six regressions yielding a negative coefficient for English legal origin, and the other three a positive coefficient (none of which was statistically significant).

While these results for English legal origin are somewhat inconclusive, it deserves to be emphasized that none of the other country-level controls is robustly correlated with the likelihood of victimization either. The only exception is income inequality as measured by the Gini coefficient, which is statistically significantly correlated with higher victimization rates in 6 out of 8 specifications.

[This section to be expanded, with more regressions for different types of crime and different controls.]

As repeatedly emphasized, it is not the purpose of the present analysis to evaluate prison's effectiveness in suppressing crime through either deterrence or incapacitation. Cross-sectional evidence is not fit for that purpose (Levitt and Miles 2007). In particular, the positive correlation of English legal origin with both crime and incarceration rates does not mean that incarceration is counterproductive in the fight against crime. A more plausible interpretation is that something about English legal origin makes these countries more prone to crime, prompting higher crime, higher anti-crime measures such as incarceration, or both. The following Section will return to this question.

7 Discussion – The meaning of "legal origins"

How should one interpret these empirical correlations? It is easy to speculate about the positive correlation between income inequality on the one hand and crime and incarceration on the other – the greater the inequality, the more tempted poorer parts of the population are likely to be to try to take from the rich, who in turn fight back with harsher punishment (cf., e.g., Whitman 2005; Tonry 2007). Likewise, the concave relationship between economic development and incarceration rates makes intuitive sense – while poor countries do not have the necessary bureaucratic structures to implement large-scale incarceration, rich countries have attained a stable equilibrium in which harsher punishment is less necessary; in between, high levels of incarceration prevail.

7.1 Failure of existing theories of "legal origin"

By contrast, the link between legal origins on the one hand and crime and punishment on the other is a puzzle. The characterization of legal origin as a "style of social control" in LLS (2008) sufficed to motivate the empirical analysis. But it would be a tautology as an explanation of the results. To be sure, LLS (2008:286) argue that "common law stands for

the strategy of social control that seeks to support private market outcomes, whereas civil law seeks to replace such outcomes with state-desired allocations." Yet this summary of the prior empirical evidence hardly elucidates the findings for crime and punishment. Criminal law enforcement can support market transactions or state allocations – some crimes, such as theft and embezzlement, would be persecuted in either case (even though the typical crime may look different in differently organized economies), while others, such as violations of financial disclosure rules or state rationing rules would be prosecuted only in a market or state-directed economy, respectively.

Other authors have also stressed the greater market orientation of Anglo-Saxon countries, and often argued that it explains greater income inequality in these countries (e.g., Esping-Andersen 1990; Hall and Soskice 2001; Soskice and Iversen 2008). Indeed, unreported regressions show that English legal origin is associated with a half standard deviation higher Gini coefficient ($p = .015$), although the result is moderately sensitive to the choice of specification and subsample. However, the regressions of the crime and punishment variables on legal origin already controlled for income inequality. Legal origin seems to have an effect on crime and punishment above and beyond any effect it may have through greater income inequality.

Going back to a specifically legal interpretation of "legal origins", can one find aspects of the common law that point towards greater punitiveness? It is hard to think of any. The most distinct features of the common law systems are believed to be the existence of the jury and case law. One could argue that the former would lead to harsher and less targeted punishment if and because juries take revenge on criminals beyond what would be optimal from a crime control perspective. However, as shown in models 10 and 11 of Table 6, the existence of juries or other lay participation does not make a dent in the estimated effect of English legal origin. Unreported regressions verified that there is also no effect of the existence of case law (as coded by La Porta et al. 2004). In general, it is hard to see how anything relating to the operation of the legal system in a narrow sense which is the traditional concern of comparative lawyers could explain the empirical evidence. This evidence now includes aspects of society as far removed from judging and legal doctrine as military conscription (Mulligan and Shleifer 2005a/b). Relatedly, it was pointed out that most of the legal rules examined in the legal origins literature were, in fact, statutory even in common law jurisdictions (Roe 2006).

Hayek (1960, 1973) famously drew a connection between institutions of the common law and liberty. In the most simple reading, this claim would predict the precise opposite of what we find, namely higher incarceration rates – deprivation of liberty – in common law countries. Of course, a more sophisticated reading might concede that strict criminal law enforcement against those who disobey the rules of the (private) game might be necessary

to support a society that is otherwise relatively free of state interference.²⁰ While plausible, such a reading is again not borne out by the data. Variables proxying for the degree of freedom in a society, be it political or economic, did not absorb the English legal origin effect.

In sum, none of the existing theories of "legal origin" is able to explain the findings of this paper, or of many of the other papers in the literature, for that matter.

7.2 From "legal" to other origins

The search for an explanation might benefit from dispensing with the "legal" in "legal origins". As outlined above, there appears to be no plausible explanation, or empirical evidence, that would situate the legal system at the source of the cross-country differences, rather than seeing it only as an instrument through which differences determined elsewhere are implemented. Hence to jettison the "legal" is to free the view on other aspects that may be more relevant.

LLS themselves appear to be drawn to a cultural explanation, arguing that legal origins stand for different ideologies or cultures understood as (2008:308, emphasis added) "beliefs about how the law *should* deal with social problems".²¹ However, they oscillate between this cultural view and a "toolkit" view (2008:308), which assumes that civil law countries simply *cannot* follow in the common law countries' footsteps. Perhaps it seemed implausible to them that countries might *choose* the "civil law approach" because they unambiguously concluded that (2008:327) "in the last quarter century ... the common law approach to social control of economic life performs better than the civil law approach." If it turns out, however, that the "common law approach" entails a cost, be it in the form of higher incarceration and crime rates, higher obesity (Cutler, Glaeser, and Shapiro 2003), or something else, then legal origins embody a trade-off. (And at least Europeans generally think that incarceration rates as high as those of the US are a very high cost (Whitman 2003).) In turn, this trade-off would make plausible a theory that conceives of legal origins as different priorities in social and economic policies.

With these revisions, the theory of "legal origins" would resemble even more closely that painted by political scientists and sociologists comparing Anglo-Saxon and continental developed economies, who in various forms emphasize the latter's more egalitarian distribution of resources (e.g., Esping-Andersen 1990; Hall and Soskice 2001; Soskice and Iversen 2008). As a cultural theory, the most pressing question it would need to answer would be how the

²⁰Relatedly, English legal origin does not stand for the absence of regulation, but a different kind of regulation (cf. Beny 2005, LLS 2006, and Jackson and Roe 2008, which all provide evidence of stronger, disclosure- and enforcement-oriented financial regulation in English legal origin countries).

²¹Crucially for empirical tests of this proposition, these aspects are not captured by conventional measures of culture such as the Hofstede and Schwartz measures used above (LLS 2008). These had been suggested as an explanation of the legal origins effect by Licht, Goldschmidt, and Schwartz (2007).

cultural differences persist and, most importantly, spread from the UK not just to white settler colonies like the US and Australia but also the rest of the vast British Empire. If it were recast as a political theory (as in Pagano and Volpin 2005; Roe 2006), it would need to explain which features of the political system predispose one set of countries to pursue the "common law approach" and others the "civil law approach". Intermediate theories based on self-sustaining beliefs are possible (e.g., di Tella and Dubra 2008).

8 Conclusion

This paper identified a strong positive correlation between English legal origin and incarceration rates in a sample of 213 countries and territories. The finding is robust to myriad control variables and specifications. It also replicates with different specifications Greenberg and West's (2008) finding that English legal origin countries are more likely to retain the death penalty. Finally, the paper shows that crime rates are not lower and perhaps higher in English legal origin countries as well.

The finding confirms and extends criminologists' observations from smaller samples that Anglo-Saxon countries are more punitive and yet tend to have higher crime rates. But perhaps more importantly, the finding sheds new light on the meaning of legal origins. The results for crime and punishment show that English legal origin, whatever its other advantages, may come at a cost. Consequently, legal origins may stand for different trade-offs between competing social goals. In turn, this makes it plausible that these differences could persist by choice rather than institutional-"technological" constraints.

These results derive from cross-country regressions, and as such should be interpreted with caution. This being said, at least for incarceration rates and the death penalty, most of the usual problems of cross-country regressions do not afflict the results.²² The dependent variable is relatively easily measured and comparable across countries. The sample coverage is close to universal ($N = 213$ or $N = 186$, respectively), so sample selection is not an issue. Most importantly, the main independent variable, legal origins, is clearly exogenous at least for former colonies, which constitute the vast majority of the sample. Hence even though cross-sectional regressions do not allow for causal inference properly speaking, it is reasonable to give the results a causal interpretation: Legal origins seem to cause differences in incarceration, the death penalty, and perhaps crime rates. The key question for future research is: how?

²²Besides the advantages mentioned in the main text, correlated errors seem to be much less of a problem for incarceration than, e.g., growth rates: for example, while Canada and the USA may be heavily interdependent economically, their incarceration rates (108 vs. 751) are radically different (Tonry and Farrington 2007).

References

1. Abrams, David. 2007. More Time, Less Crime? Estimating the Deterrent Effect of Incarceration using Sentencing Enhancements. Working paper, University of Chicago (April).
2. Alesina, Alberto, Arnaud Devleeschauwer, William Easterly, Sergio Kurlat, and Romain Wacziarg. 2003. Fractionalization. *Journal of Economic Growth* 8:155-194.
3. Anckar, Carsten. 2006. *Determinants of the Death Penalty - A comparative study of the world*. London and New York: Routledge.
4. Beny, Laura Nyantung. 2005. Do Insider Trading Laws Matter? Some Preliminary Comparative Evidence. *American Law and Economics Review* 7:144-183.
5. Blumstein, Alfred, Michael Tonry, and Asheley van Ness. 2005. Cross-National Measures of Punitiveness. In *Crime and Punishment in Western Countries, 1980-1999*, ed. Michael Tonry and David P. Farrington, 347-376. *Crime and Justice: A Review of Research* 33. Chicago: University of Chicago Press.
6. Botero, Juan C., Simeon Djankov, Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer. 2004. The Regulation of Labor. *Quarterly Journal of Economics* 119:1339-1382.
7. Cavadino, Michael, and James Dignan. 2006a. *Penal Systems: A Comparative Approach*. London, Thousand Oaks, and New Delhi: SAGE.
8. ——. 2006b. Penal Policy and Political Economy. *Criminology & Criminal Justice* 6:435-456.
9. CIA. 2008. *World Factbook 2008*. www.cia.gov/library/publications/the-world-factbook, visited 6/01/2008.
10. Cutler, David M., Edward L. Glaeser, and Jesse M. Shapiro. 2003. Why Have Americans Become More Obese? *Journal of Economic Perspectives* 17(3):93-118.
11. Di Tella, Rafael, and Juan Dubra. 2008. Crime and punishment in the "American Dream". *Journal of Public Economics* 92:1564-1584.
12. Djankov, Simeon, Edward Glaeser, Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer. 2003a. The new comparative economics. *Journal of Comparative Economics* 31:595-619.

13. Djankov, Simeon, Oliver Hart, Caralee McLiesh, and Andrei Shleifer. 2007. Debt Enforcement Around the World. ECGI Finance Working Paper 147.
14. Djankov, Simeon, Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer. 2003b. Courts. *Quarterly Journal of Economics* 118:453-517.
15. ——. 2008. The law and economics of self-dealing. *Journal of Financial Economics* 88:430-465.
16. Downes, David, and Kirstine Hansen. 2006a. Welfare and Punishment in Comparative Perspective. In *Perspectives on Punishment*, ed. Sarah Armstrong and Lesley McAra, 133-154. Oxford: Oxford University Press.
17. ——. 2006b. Welfare and punishment: The relationship between welfare spending and imprisonment. Crime and Society Foundation briefing 2, November 2006. London: Crime and Society Foundation.
18. Esping-Andersen, Gosta. 1990. *The three worlds of welfare capitalism*. Princeton: Princeton University Press.
19. Fearon, James D. 2003. Ethnic and Cultural Diversity by Country. *Journal of Economic Growth* 8:195-222.
20. Gibson, John, and Bonggeun Kim. 2008. The effect of reporting errors on the cross-country relationship between inequality and crime. *Journal of Development Economics* 87:247-254.
21. Greenberg, David F., and Valerie West. 2008. Siting the Death Penalty Internationally. *Law & Social Inquiry* 33:295-343.
22. Gwartney, James, and Robert Lawson, with Russell S. Sobel and Peter T. Leeson. 2007. *Economic Freedom of the World: 2007 Annual Report*. Vancouver, B.C.: Fraser Institute. Data retrieved from www.freetheworld.com.
23. Hall, Peter A., and David Soskice. 2001. An Introduction to Varieties of Capitalism. In *Varieties of Capitalism: the institutional foundations of comparative advantage*, ed. Peter A. Hall and David Soskice, 1-68. Oxford and New York: Oxford University Press.
24. Hayek, Friedrich A. 1960. *The Constitution of Liberty*. Chicago: University of Chicago Press.

25. ———. 1973. *Law, legislation, and liberty: a new statement of the liberal principles of justice and political economy*. Volume 1. *Rules and Order*. Chicago: University of Chicago Press.
26. International Center for Prison Studies. 2008. World Prison Brief. <http://www.kcl.ac.uk/depsta/law/> visited 4/24/08.
27. INTERPOL. 1999. *International Crime Statistics for 1999*. Saint Cloud: INTERPOL.
28. Jackson, Howell E., and Mark J. Roe. 2008. Public and Private Enforcement of Securities Laws – Resource-Based Evidence. Mimeo, Harvard University.
29. La Porta, Rafael, Florencio Lopez-de-Silanes, Cristian Pop-Eleches, and Andrei Shleifer. 2004. Judicial Checks and Balances. *Journal of Political Economy* 112:445-470.
30. La Porta, Rafael, Florencio Lopez-de-Silanes, and Andrei Shleifer. 2006. What works in securities laws? *Journal of Finance* 61:1-32.
31. ———. 2008. The Economic Consequences of Legal Origins. *Journal of Economic Literature* 46:285-332.
32. ———, and Robert Vishny. 1999. The Quality of Government. *Journal of Law, Economics, and Organization* 15:222-279.
33. Lappi-Seppälä, Tapio. 2008. Trust, Welfare, and Political Culture: Explaining Differences in National Penal Policies. In *[to be added]*, ed. Michael Tonry, xx-yy. *Crime and Justice: A Review of Research* 37. Chicago: University of Chicago Press.
34. Levitt, Steven D., and Thomas J. Miles. 2007. Empirical Study of Criminal Punishment. In *Handbook of Law and Economics*, ed. A. Mitchell Polinsky and Steven Shavell, 1:455-495. Amsterdam: North-Holland.
35. Licht, Amir N., Chanan Goldschmidt, and Shalom H. Schwartz. 2007. Culture Rules: The foundations of the rule of law and other norms of governance. *Journal of Comparative Economics* 35:659-688.
36. Lijphart, Arend. 1999. *Patterns of Democracy: Government Forms and Performance in Thirty-Six Countries*. New Haven and London: Yale University Press.
37. Liptak, Adam. 2008. Inmate Count in U.S. Dwarfs Other Nations'. *New York Times*, April 23, 2008.
38. Lynch, James P. 2006. Problems and Promise of Victimization Surveys for Cross-National Research. In *Crime and Justice: A Review of Research* 34, ed. Michael Tonry, 229-287. Chicago: University of Chicago Press.

39. Mahoney, Paul G. 2001. The Common Law and Economic Growth: Hayek might be right. *Journal of Legal Studies* 30:503-525.
40. McCleary, Rachel M., and Robert J. Barro. 2006. Religion and Economy. *Journal of Economic Perspectives* 20(2):49-72.
41. Messner, Steven F., Lawrence E. Raffalovich, and Peter Shrock. 2002. Reassessing the Cross-National Relationship between Income Inequality and Homicide Rates: Implications of Data Quality Control in the Measurement of Income Distribution. *Journal of Quantitative Criminology* 18:377-395.
42. Mukherjee, Satyanshu, and Philip Reichel. 1999. Bringing to Justice. In *Global Report on Crime and Justice*, ed. Graeme Newman, 65-88. New York and Oxford: Oxford University Press.
43. Mulligan, Casey B., and Andrei Shleifer. 2005a. Conscriptio as Regulation. *American Law and Economics Review* 7:85-111.
44. ———. 2005b. The Extent of the Market and the Supply of Regulation. *Quarterly Journal of Economics* 120:1445-1473.
45. Neapolitan, Jerome L. 2001. An Examination of Cross-National Variation in Punitiveness. *International Journal of Offender Therapy and Comparative Criminology* 45:691-710.
46. Newman, Graeme, and Gregory J. Howard. 1999. Introduction: Data sources and their use. In *Global Report on Crime and Justice*, ed. Graeme Newman, 1-23. New York and Oxford: Oxford University Press.
47. Pagano, Marco, and Paolo F. Volpin. 2005. The Political Economy of Corporate Governance. *American Economic Review* 95:1005-1030.
48. Pease, Ken. 1994. Cross-National Imprisonment Rates: Limitations of Method and Possible Conclusions. *British Journal of Criminology* 34(special issue):116-130.
49. Reynolds, Thomas, and Arturo Flores. 2008. *Foreign Law Guide: Current Sources of Codes and Basic Legislation in Jurisdictions of the World*. www.foreignlawguide.com, visited 6/2/2008.
50. Roe, Mark J. 2006. Legal Origins and Modern Stockmarkets. *Harvard Law Review* 120:460-527.

51. Rostowski, Jacek, and Bogdan Stacescu. 2006. The Wig and the Pith Helmet – the Impact of "Legal School" versus Colonial Institutions on Economic Performance. *CASE Studies & Analyses* 300.
52. Ruddell, Rick. 2005. Social Disruption, State Priorities, and Minority Threat. *Punishment & Society* 7:7-28.
53. Soares, Rodrigo R. 2004. Development, crime, and punishment: accounting for the international difference in crime rates. *Journal of Development Economics* 73:155-184.
54. Soskice, David, and Torben Iversen. 2008. Inequality and Redistribution: A Unified Approach to the Role of Economic and Political Institutions. Mimeo, Oxford University and Harvard University.
55. Spelman, William. 2008. Specifying the Relationship Between Crime and Prisons. *Journal of Quantitative Criminology* 24:149-178.
56. Tonry, Michael. 2007. Determinants of Penal Policies. In *Crime, Punishment, and Politics in Comparative Perspective*, ed. Michael Tonry, 1-48. *Crime and Justice: A Review of Research* 36. Chicago: University of Chicago Press.
57. ——— and David P. Farrington. 2005. Punishment and Crime across Space and Time. In *Crime and Punishment in Western Countries, 1980-1999*, ed. Michael Tonry and David P. Farrington, 1-39. *Crime and Justice: A Review of Research* 33. Chicago: University of Chicago Press.
58. Jan van Dijk, John van Kesteren, and Paul Smit. 2007. *Criminal Victimization in International Perspective: Key findings from the 2004-2005 ICVS and EU ICS*. The Hague: WODC.
59. Voigt, Stefan. 2008. The (Economic) Effect of Lay Participation in Courts – A Cross-Country Analysis. CESifo Working Paper 2365.
60. Whitman, James Q. 2003. *Harsh Justice: Criminal Punishment and the Widening Divide between America and Europe*. Oxford and New York: Oxford University Press.
61. ———. 2005. The Comparative Study of Criminal Punishment. *Annual Review of Law and Social Science* 1:17-34.
62. World Health Organization. 2004. Causes of death, series W158 (per capita). <http://www.who.int/en> visited 5/07/08
63. Young, Warren, and Mark Brown. 1993. Cross-National Comparisons of Imprisonment. In *Crime and Justice: A Review of Research*, ed. Michael Tonry, 17:1-45. Chicago: University of Chicago Press.

Table 1: Main variables: definitions and sources

Variable	Definition	Source
Inmates per capita	Prisoners (including pre-trial detainees) per 100,000 population, 2007 or most recent available	International Center for Prison Studies
Death penalty	Dummy variable indicating if death penalty is still applied in ordinary criminal cases (2007)	Amnesty International
Legal origin	Dummy variable indicating English Common, French Civil, German Civil, Scandinavian Civil, or Socialist Law	La Porta et al. (1999), CIA (2008), and Reynolds and Flores (2008)
(ln) GDP per capita	(natural log of) GDP per capita 2007 in constant 2000 dollars	World Development Indicators online
Muslim, Catholic, Protestant pop.	Percentage of population identified as Muslim, Catholic, Protestant in 1980	La Porta et al. (1999)
Ethnic, linguistic, and religious fractionalization	Indices of fractionalization, 0 (fully homogenous) to 1 (fully heterogenous)	Alesina et al. (2003)
Proportional voting	Index of proportional voting, 3 (proportional) to 0 (majoritarian); defined in Pagano & Volpin (2005)	World Bank DPI rev. 4
Hofstede power distance index individualism masculinity	acceptance of unequal power distribution individuals' non-integration into groups competitive and assertive elements in men's values	www.geert-hofstede.com
Schwartz hierarchy egalitarianism	Emphasis on obeying role obligations ... transcendence of selfish interests	Licht, Goldschmidt, and Schwartz (2007)
Trust in people	Fraction of survey respondents that answer "most people can be trusted", as opposed to "can't be too careful"	World Value Survey item A165, 1999-2004 sweep (if missing, 1994-1999)
Trust in police and parliament	Average of trust in parliament and police (individual answers weighted 3 to 0)	ibid., items E074 and E075
Religious intensity	Fraction of survey respondents that self-identifies as religious	ibid., item F034
Days required to enforce a contract	Time in calendar days required to resolve a contract dispute in court, from filing the lawsuit to receipt of payment	Doing Business in 2008
Extrajudicial killings occur	Dummy variable indicating whether police or vigilantes practice extrajudicial killings	US Department of State, 2007 Country Reports on Human Rights Practices
Gini	Gini coefficient, 2007 or most recent available 1992-2006, 0 (very equal) to 1 (very unequal)	World Development Indicators online
(Transfers + Subsidies)/GDP, %	Transfers and subsidies as a share of GDP, 2005	Gwartney and Lawson (2007)
Unemployment rate	Total unemployment rates, average of 2005 and 2006 available data	ILO
Difficulty of firing a worker	Index of difficulty of firing a worker 2007, 0 (minimum restrictions) to 1 (maximum restrictions)	Doing Business in 2008
Freedom	Standardized average of political rights and civil liberties 2007, 0 (least freedom) to 1 (most ~)	Freedom House, Freedom in the World 2008
Homicide rate	Deaths by violence ²⁴ (intentional injuries) per 100,000 population, 2002	WHO, causes of death series W158

Table 2: Main variables: summary statistics

Variable	Obs.	Mean	St. Dev.	Min	Max
Inmates per capita	213	162.28	128.97	22	751 (USA)
Death penalty	195	.31	.46	0	1
Legal origin	213				
English		.35			
German		.03			
Scandinavian		.02			
Socialist		.17			
ln(GDP per capita)	182	3.40	.69	1.96	4.73
Muslim population, %	201	21.16	35.05	0	99.90
Protestant population, %	199	15.18	23.60	0	99.80
Catholic population, %	201	33.27	36.35	0	99.10
Fractionalization					
ethnic	181	.43	.26	0.00	.93
linguistic	191	.39	.28	0.00	.92
religious	203	.44	.23	0.00	.86
Proportional voting	147	1.40	1.31	0	3
Hofstede	65				
PDI		58.63	22.04	11	104
IDV		44.11	24.35	6	91
MAS		50.69	19.06	5	110
Schwartz	51				
Hierarchy		2.26	.50	1.41	3.63
Egalitarianism		4.81	.29	4.25	5.40
Trust in people	83	.28	.15	.03	.67
Trust in police and parliament	79	.48	.11	.28	.87
Religious intensity	79	.73	.19	.15	.99
Favor income inequality	71	5.89	1.15	3.54	8.24
Favor welfare state	80	5.77	1.07	3.26	7.86
Dislike competition	68	3.67	.57	2.47	4.80
Days to enforce contract	172	603.58	308.54	120	1800
Extrajudicial killings	183	.28	.45	0	1
Gini	130	40.61	9.47	24.70	74.33
(Transfers and subsidies)/GDP, %	109	10.15	7.18	0	28.78
Unemployment rate	103	9.68	7.53	1.30	40.45
Difficulty of firing worker	172	31.28	22.60	0	100
Freedom	189	.63	.32	0	1
Homicide rate	183	9.16	10.55	.23	72.37
Juries	76	.36	.48	0	1
Lay assessors	76	.46	.50	0	1

The sample contains all countries and territories for which data on inmates per capita is available from ICPS, except that (1) to match other datasets, data were combined for the UK (population-weighted average of England and Wales, Scotland, and Northern Ireland) and the Channel Islands (population-weighted average of Guernsey and Jersey), and (2) Palau was omitted because its legal origin could not be determined with the standard sources.

Table 3: Natural logarithm of (inmates per 100,000 population), by legal origin

	(1)	(2)	(3)	(4)	(5)	(6)
	full sample	\USA	OECD	\OECD	pop. $\geq 10^5$	independent
Observations	213	212	30	183	186	186
Means by subsample and legal origin (number of observations)						
English	4.947 (74)	4.924 (73)	5.131 (6)	4.931 (68)	4.893 (58)	4.832 (61)
non-English	4.721 (139)	4.721 (139)	4.640 (24)	4.738 (115)	4.727 (128)	4.679 (125)
Socialist	5.124 (37)	5.124 (37)	5.221 (4)	5.113 (33)	5.147 (36)	5.147 (36)
Civil Law	4.574 (102)	4.574 (102)	4.524 (20)	4.587 (82)	4.563 (92)	4.489 (89)
French	4.610 (90)	4.610 (90)	4.759 (10)	4.592 (80)	4.586 (81)	4.518 (77)
German	4.423 (7)	4.423 (7)	4.442 (5)	4.376 (2)	4.605 (6)	4.423 (7)
Scandinavian	4.136 (5)	4.136 (5)	4.136 (5)	NA (0)	4.136 (5)	4.136 (5)

Tests of means (t-statistics)

English vs. non-English	2.01**	1.82*	2.05*	1.55	1.39	1.27
English vs. Socialist	-1.08	-1.24	-0.21	-1.03	-1.54	-1.84*
English vs. Civil Law	3.14***	2.96***	2.64**	2.56**	2.66***	2.72***
English vs. French	2.71***	2.54**	1.36	2.52**	2.35**	2.37**
English vs. German	1.53	1.48	1.87*	0.85	0.82	1.17
English vs. Scandinavian	2.03**	2.01**	2.61**	NA	1.97*	1.71*
Socialist vs. Civil Law	4.22***	4.22***	3.48***	3.50***	4.52***	5.17***
Socialist vs. French	3.82***	3.82***	2.67**	3.48***	4.16***	4.76***
Socialist vs. German	2.64**	2.64**	4.71***	1.41	1.99*	2.74***
Socialist vs. Scandinavian	3.34***	3.34***	5.31***	NA	3.44***	3.44***
French vs. German	0.68	0.68	2.14*	0.40	-0.07	0.37
French vs. Scandinavian	1.49	1.49	3.78***	NA	1.45	1.28
German vs. Scandinavian	0.93	0.93	1.79	NA	1.98*	0.93

* $p < .1$, ** $p < .05$, *** $p < .01$ (two-sided)

Table 4: Regressions with religion, fractionalization, and proportional voting variables

	Dependent variable: ln(inmates per 100,000 population)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
English legal origin	0.43*** (0.12)	0.34*** (0.11)	0.31*** (0.11)	0.24* (0.12)	0.30** (0.13)	0.36** (0.14)	0.37** (0.16)
Socialist legal origin	0.74*** (0.17)	0.72*** (0.13)	0.72*** (0.14)	0.67*** (0.14)	0.75*** (0.15)	0.77*** (0.14)	0.76*** (0.19)
ln(GDP per capita)	0.45*** (0.068)	0.48*** (0.081)	0.43*** (0.074)	0.47*** (0.069)	0.43*** (0.078)	0.39*** (0.081)	0.41*** (0.086)
Muslim pop., %	-0.00064 (0.0020)						-0.0023 (0.0025)
Protestant pop., %	-0.0035 (0.0027)						-0.0059** (0.0027)
Catholic pop., %	0.0029 (0.0020)						0.00024 (0.0022)
ethnic fractionalization		0.11 (0.22)			0.67*** (0.25)		0.62** (0.27)
linguistic frac.			-0.26 (0.18)		-0.74*** (0.22)		-0.67*** (0.23)
religious frac.				0.38 (0.24)	0.41* (0.25)		0.29 (0.28)
Proportional voting						-0.043 (0.047)	-0.056 (0.054)
Constant	2.95*** (0.31)	2.87*** (0.33)	3.17*** (0.29)	2.81*** (0.26)	2.85*** (0.34)	3.25*** (0.27)	3.17*** (0.45)
R^2	0.30	0.28	0.30	0.29	0.34	0.28	0.36
Observations	177	171	171	179	163	145	136

Robust standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 5: Regressions with culture variables

	Dependent variable: ln(inmates per 100,000 population)										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Hofstede sample	Schwartz sample	WVS sample 1	WVS sample 2	WVS sample 3	WVS sample 1	WVS sample 2	WVS sample 2	WVS sample 2	WVS sample 3	WVS sample 3
English legal origin	0.25 (0.20)	0.38** (0.19)	0.28 (0.21)	0.23 (0.22)	0.0026 (0.25)	0.067 (0.25)	0.038 (0.23)	0.037 (0.23)	0.14 (0.25)	0.31 (0.28)	0.42 (0.29)
Socialist legal origin	0.52*** (0.19)	0.61*** (0.20)	0.72*** (0.25)	0.68** (0.26)	0.47*** (0.16)	0.41** (0.16)	0.53*** (0.16)	0.56*** (0.18)	0.48** (0.20)	0.59*** (0.19)	0.62*** (0.20)
ln(GDP per capita)	0.16 (0.15)	0.39* (0.20)	0.29 (0.19)	0.39* (0.21)	0.10 (0.13)	0.18 (0.16)	0.13 (0.13)	0.17 (0.16)	0.077 (0.15)	0.085 (0.18)	0.17 (0.23)
Hofstede											
power distance		0.0047 (0.0050)									
individualism		-0.0037 (0.0045)									
masculinity		-0.0044 (0.0037)									
Schwartz											
hierarchy			0.12 (0.30)								
egalitarianism			-0.21 (0.45)								
Trust in people						-0.80 (0.59)					-1.11 (0.75)
Trust in police, parliament						-0.75 (0.59)					-0.96 (0.65)
Religious intensity							0.0028 (0.0039)				0.00082 (0.0049)
Favor income inequality										0.18** (0.082)	0.18** (0.081)
Favor welfare state										-0.0054 (0.077)	-0.066 (0.077)
Dislike competition										0.31** (0.16)	0.33** (0.14)
Constant	4.15*** (0.59)	3.35*** (0.89)	3.48*** (0.76)	3.83 (2.58)	4.33*** (0.53)	4.63*** (0.71)	4.23*** (0.51)	3.87*** (0.82)	4.49*** (0.61)	2.22*** (1.06)	2.90* (1.51)
R^2	0.10 65	0.15 65	0.18 51	0.19 51	0.11 79	0.18 79	0.13 79	0.14 79	0.10 59	0.24 59	0.34 58
Observations											

Robust standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 6: Regressions with other independent variables

	Dependent variable: ln(inmates per 100,000 population)										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
English legal origin	0.33*** (0.11)	0.35*** (0.12)	0.36*** (0.11)	0.26** (0.12)	0.27* (0.15)	0.44*** (0.16)	0.29** (0.12)	0.35*** (0.11)	0.38*** (0.11)	0.28 (0.19)	0.25 (0.21)
Socialist legal origin	0.67*** (0.13)	0.64*** (0.14)	0.70*** (0.13)	0.92*** (0.15)	0.79*** (0.14)	0.35** (0.15)	0.63*** (0.13)	0.68*** (0.13)	0.75*** (0.13)	0.75*** (0.17)	0.79*** (0.17)
ln(GDP per capita)	0.47*** (0.071)	0.47*** (0.076)	0.45*** (0.082)	0.50*** (0.081)	0.41*** (0.12)	-0.095 (0.12)	0.44*** (0.074)	0.48*** (0.079)	0.51*** (0.076)	0.31*** (0.12)	0.30*** (0.12)
Days to enforce contract		-0.000054 (0.00021)									
Extrajudicial killings			-0.0076 (0.11)								
Gini				0.030*** (0.0057)							
(Transf. + Subs.)/GDP, %					-0.031*** (0.0097)						
Unemployment rate						-0.0052 (0.011)					
Difficulty of firing worker							-0.0036 (0.0024)				
Freedom								-0.069 (0.18)			
Homicide rate (WHO)									0.0078 (0.0050)		
Juries										0.16 (0.16)	
Lay assessors										-0.14 (0.15)	
Constant	2.96*** (0.25)	2.98*** (0.32)	2.99*** (0.30)	1.60*** (0.45)	3.46*** (0.36)	5.21*** (0.47)	3.17*** (0.28)	2.95*** (0.25)	2.73*** (0.28)	3.44*** (0.46)	3.49*** (0.44)
R^2	0.28	0.28	0.28	0.36	0.24	0.13	0.29	0.28	0.29	0.22	0.24
Observations	182	170	173	130	109	94	170	177	173	76	76

The sample of model 10 is restricted to match that of model 11.

Robust standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 7: Multiple control regressions

	Dependent variable: ln(inmates per 100,000 population)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	full sample				without the USA			
English LO	0.37*** (0.14)	0.38*** (0.14)	0.31* (0.17)	0.29 (0.18)	0.35** (0.14)	0.36** (0.14)	0.28* (0.17)	0.27 (0.17)
Socialist LO	0.64*** (0.17)	0.63*** (0.17)	0.79*** (0.17)	0.74*** (0.18)	0.64*** (0.17)	0.63*** (0.17)	0.78*** (0.17)	0.73*** (0.18)
ln(GDP pc)	0.46*** (0.086)	0.44*** (0.093)	0.55*** (0.11)	0.48*** (0.12)	0.43*** (0.083)	0.41*** (0.090)	0.51*** (0.11)	0.44*** (0.11)
Days to enforce	-0.00010 (0.00025)	-0.00012 (0.00027)	-0.00035* (0.00020)	-0.00040* (0.00021)	-0.000074 (0.00025)	-0.000095 (0.00027)	-0.00032 (0.00020)	-0.00038* (0.00021)
Muslim, %	-0.0027 (0.0021)	-0.0030 (0.0023)	-0.0018 (0.0024)	-0.0020 (0.0026)	-0.0028 (0.0021)	-0.0030 (0.0023)	-0.0021 (0.0024)	-0.0023 (0.0025)
Protestant, %	-0.0050* (0.0030)	-0.0048 (0.0031)	-0.0058** (0.0028)	-0.0056* (0.0029)	-0.0052* (0.0029)	-0.0050 (0.0030)	-0.0062** (0.0027)	-0.0061** (0.0029)
Catholic, %	0.0024 (0.0020)	0.0026 (0.0021)	-0.0013 (0.0021)	-0.00098 (0.0022)	0.0023 (0.0020)	0.0026 (0.0021)	-0.0014 (0.0021)	-0.0011 (0.0022)
ethnic frac.	0.11 (0.24)	0.061 (0.26)	-0.037 (0.27)	-0.091 (0.29)	0.060 (0.24)	0.012 (0.25)	-0.097 (0.27)	-0.15 (0.28)
Difficulty firing	-0.0034 (0.0024)	-0.0039 (0.0026)	-0.0033 (0.0023)	-0.0036 (0.0025)	-0.0032 (0.0024)	-0.0038 (0.0026)	-0.0029 (0.0023)	-0.0032 (0.0025)
Freedom	-0.15 (0.22)	-0.25 (0.24)	-0.39 (0.29)	-0.46 (0.31)	-0.16 (0.22)	-0.26 (0.24)	-0.41 (0.29)	-0.48 (0.31)
Homicide rate		0.0042 (0.0062)		0.00080 (0.0061)		0.0040 (0.0062)		0.00051 (0.0061)
Extrajudicial k.		-0.14 (0.14)		-0.13 (0.15)		-0.14 (0.14)		-0.12 (0.15)
Proportionality			-0.012 (0.055)	-0.0078 (0.058)			0.00044 (0.055)	0.0059 (0.057)
Gini			0.029*** (0.0064)	0.028*** (0.0073)			0.028*** (0.0064)	0.027*** (0.0073)
Constant	3.24*** (0.45)	3.40*** (0.48)	2.30*** (0.64)	2.66*** (0.70)	3.34*** (0.44)	3.50*** (0.48)	2.46*** (0.62)	2.84*** (0.69)
R^2	0.33	0.34	0.44	0.45	0.32	0.33	0.43	0.45
Observations	160	156	113	111	159	155	112	110

Robust standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 8: Death penalty logistic regressions

	Dependent variable: Death penalty dummy									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
	full sample			without USA and EU						WVS sample
English legal origin (d)	0.185** (2.38)	0.210*** (2.63)	0.262** (2.51)	0.267** (2.15)	0.155* (1.85)	0.178** (2.05)	0.276** (2.31)	0.296** (2.04)	0.175 (1.05)	0.132 (0.86)
Socialist legal origin (d)	-0.135 (1.57)	-0.164* (1.91)	-0.256*** (3.67)	-0.205*** (2.87)	-0.119 (1.12)	-0.144 (1.29)	-0.317*** (3.76)	-0.273*** (3.10)	-0.147* (1.70)	-0.0900 (1.05)
ln(GDP per capita)		-0.0430 (0.85)	0.0928 (1.31)	0.0408 (0.54)		0.0412 (0.65)	0.134 (1.55)	0.0483 (0.48)	-0.228*** (2.91)	-0.242** (2.37)
Freedom			-0.493*** (2.80)	-0.496*** (2.69)			-0.469** (2.23)	-0.557** (2.35)		
ethnic fractionalization			-0.0430 (0.26)	0.0308 (0.18)			-0.145 (0.75)	-0.0439 (0.20)		
Muslim pop.			-0.116 (0.75)	-0.224 (1.54)			-0.190 (1.00)	-0.314 (1.64)		
Protestant pop.			-0.506** (2.02)	-0.424 (1.51)			-0.811** (2.37)	-0.695 (1.55)		
Catholic pop.			-0.359** (2.24)	-0.244 (1.55)			-0.461** (2.26)	-0.272 (1.20)		
Gini				-0.00412 (0.76)				-0.00833 (1.12)		
Trust in people									0.807** (2.44)	
Trust in police, parliament									0.344 (1.00)	
Pseudo R^2	0.05	0.07	0.22	0.25	0.03	0.05	0.19	0.21	0.23	0.49
Observations	186	175	167	125	158	147	141	101	78	78

Marginal effects; Absolute t statistics in parentheses

(d) for discrete change of dummy variable from 0 to 1

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 9: Crime regressions - likelihood of victimization in the past year

	(Any of 9 common crimes)			Burglary			Robbery			Assault		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	
English LO (d)	0.037 (0.023)	0.014 (0.022)	0.035** (0.018)	0.016** (0.0068)	0.0043 (0.0058)	0.017*** (0.0053)	-0.0039 (0.0041)	-0.0069** (0.0031)	-0.0059* (0.0035)	0.019*** (0.0054)	0.014*** (0.0051)	
Socialist LO(d)	-0.0072 (0.022)	-0.011 (0.023)	0.047* (0.024)	-0.0043 (0.0073)	-0.0041 (0.0067)	0.0085 (0.0076)	-0.011*** (0.0034)	-0.0066** (0.0033)	0.000065 (0.0034)	0.00047 (0.0044)	0.0048 (0.0050)	
ln(GDP pc)	-0.014 (0.016)	0.0081 (0.020)	-0.0089** (0.0043)	-0.012* (0.0071)	0.00040 (0.0030)	0.0069* (0.0035)	0.00040 (0.0030)	0.00040 (0.0030)	0.0069* (0.0035)	0.00040 (0.0030)	-0.0033 (0.0030)	
GDP pc (1,000)	-0.001 (0.001)	-0.0007 (0.0010)	-0.0002 (0.0004)	0.0003 (0.0004)	-0.0005* (0.0003)	-0.0004* (0.0002)	0.0003 (0.0004)	0.0003 (0.0004)	0.0003 (0.0002)	0.0003 (0.0002)	0.0003 (0.0002)	
GDP growth	-0.13 (0.14)	-0.30** (0.14)	0.0047 (0.021)	0.066 (0.045)	-0.018 (0.017)	-0.049* (0.025)	0.052*** (0.016)	0.068*** (0.015)	0.0061 (0.0063)	0.024 (0.017)	-0.017 (0.031)	
Gini/100	0.12 (0.082)	0.20*** (0.075)	0.056** (0.024)	0.061*** (0.021)	0.0019 (0.0078)	0.0061 (0.0063)	0.0050 (0.024)	0.0050 (0.024)	0.0050 (0.024)	0.0050 (0.024)	0.0050 (0.024)	
Fractional'n			0.041 (0.043)									
foreign pop.			-0.011 (0.12)									
Civil Liberties			0.011 (0.010)									
Male unempl't			-0.18 (0.17)									
N	315564	311648	267615	314304	310388	266399	314369	310453	266448	304706	300790	
Clusters	74	71	57	73	70	56	73	70	56	73	70	
$Pr(y \bar{x})$	0.23	0.23	0.22	0.035	0.033	0.030	0.020	0.018	0.016	0.031	0.030	

Marginal effects; Country-clustered standard errors in parentheses

(d) for discrete change of dummy variable from 0 to 1

* $p < .1$, ** $p < .05$, *** $p < .01$

Weighted logit; the weights compensate for over- and undersampling within countries, as well as for over- and undersampling across countries (i.e., every country has equal weight). All models control for the survey sweep, age, gender, and town size.