

Initial Conditions, Institutional Dynamics and Economic Performance:  
Evidence from the American States

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Abstract

Using state-level data from the United States, we find that differences in colonial legal institutions have affected the current quality of state legal institutions. These differences in colonial legal institutions arose because some states were settled by Great Britain, a common law country, and other states were settled by France, Spain, and Mexico, all civil law countries. To explain these findings, we develop a transplant-civil law hypothesis that highlights the disruption associated with large-scale legal transplantation and the possible relative inefficiencies of colonial civil law. We find strong support for the transplant-civil law hypothesis. Our results are robust to the inclusion of additional variables capturing climate, geography, initial population, resource endowments, state level rules, and legal environment. Given the 150-200 year gap between the initial conditions and the measures of the current quality of legal institutions, we provide indirect evidence on the persistence of legal institutions. We then use initial legal systems as a source of exogenous variation in current institutions for providing a series of estimates of their impact on current economic performance.

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## **I. Introduction**

Fifteen American states were originally settled by France, Spain, or Mexico, all countries with civil law legal systems. Shortly after the acquisition of a territory by Great Britain or later by the United States, all of the states except Louisiana adopted common law. Many residents in these former territories were unhappy with the change of legal system. For example, the local French population in Vincennes complained to the local judge that because of the imposition of common law, “laws are too complex, not to be understood and tedious in their operation.”<sup>1</sup> A Missouri resident wrote to then President Thomas Jefferson in 1805, “Many people here do not like the Change & every Law that is pass’d puts them on a Worse Situation than they would have been under the Spaniards is Criticiz’d & the Worst Construction put on.”<sup>2</sup> The adoption of common law engendered significant debate before the first meeting of the California legislature.<sup>3</sup> Elisha Crosby, the Chairman of the Judiciary Committee during this meeting noted, “There was quite an element of Civil Law in the Legislature and many wanted that adopted as a rule.”<sup>4</sup> In the ten states that were acquired after the American Revolution by the United States, there is substantial evidence that the new common law legal systems retained elements of their civil law predecessors.

The different settlement patterns within the United States provide a unique natural experiment that enables us to test whether differences in colonial legal institutions, and subsequent transplantation of common law on civil law legal systems, can explain the variation in the quality of contemporary legal institutions. Contemporary cross-country evidence suggests that common law legal institutions are of higher quality than civil law legal institutions. (La Porta et al. 1998, 1999 and Djankov et al. 2002, 2003 and Botera et. al. 2003). Cross-country evidence also indicates that the process of transplantation can have long-term effects on the quality of legal institutions (Berkowitz, Pistor and Richard 2003).

We use this natural experiment to test two hypotheses. The first hypothesis, which we call the transplant-civil law hypothesis, is that persistent negative shocks to the legal systems

arose from the inferiority of colonial civil law relative to colonial common law, from transplantation of common law onto civil law legal systems, or from both. The alternative hypothesis, which we call the transitory hypothesis, is that the effects of the colonial civil law legal system and the transplantation of common law may have had short-run effects on legal institutions, but that time and mobility rendered these effects transitory.

To test the transplant-civil law hypothesis, we use climate, membership in the Confederacy, and initial settlement by civil or common law countries (as measured by historical land grant data) to control for initial conditions in regressions in which the dependent variable is the quality of state courts in 2001. We have three main findings. First, in regressions using a number of different measures of contemporary legal institutions as the dependent variable, we find strong evidence for the transplant-civil law hypothesis. Having had a civil law legal system after the American Revolution has a significant and large negative effect on contemporary legal institutions. Thus, if we could rewrite history and change the initial legal system of states that had a civil law tradition after the American Revolution, the quality of contemporary state courts would increase roughly one standard deviation, which accounts for the difference between the quality of courts in Virginia and Nevada, or the difference between Nevada and Arkansas.

Second, because of the 150-200 year gap between the initial conditions and available measures of the quality of state legal institutions, legal institutions would have to be very persistent for us to find a measurable effect. Using state budgetary data and corruption data, we provide indirect evidence that suggests that the quality of legal institutions is very persistent.

Third, we argue that the initial legal system is a plausible instrument for measuring the impact of contemporary legal institutions on contemporary economic performance, and we validate this instrumental variables approach with an over-identification test. After controlling for a broad set of covariates that could influence both institutions and economic performance, we find that a change in the initial legal system of a state from civil to common law would be

associated with a statistically and quantitatively significant increase in median household income, and a statistically and quantitatively significant decrease in the poverty rate.

Our paper contributes to analysis of the determinants of good institutions. La Porta et al (1998, 1999) provide cross-country evidence that, conditional on differences in gross national product per capita, common law legal institutions are currently more effective than civil law legal institutions at enforcing rule of law. Several influential cross-country studies, however, stress the importance of initial conditions. Acemoglu, Johnson and Robinson (2001) show that that in former colonies the disease environment at the time of colonization influenced initial institution building and this, in turn, had a long-term effect on institutions. Engerman and Sokoloff (2002) highlight the long-term impact of initial climate, location, and resource endowments. Two potential problems with cross-country analysis of the determinants of good institutions are the substantial unobserved, and difficult to control for, heterogeneity across countries, and the substantial within-country variation in the quality of legal institutions (Jappelli et al 2002, Laeven and Woodruff 2003). We address these problems by using state level data from the United States (see, also, Banarjee and Iyer (2002) for India)

Like the cross-country literature, we find that initial conditions matter. In contrast to Acemoglu et al (2001), however, we find that both climate, which is related to the disease environment in which early settlers lived, and legal families are important determinants of long-run institutions. Our findings about legal families are somewhat different from the findings in La Porta et al (1998, 1999). In their study, countries did not change legal systems, so it is difficult to separate out the initial legal family and any transplantation effects from subsequent influences on the legal family related to culture, levels of economic development, and the rise of mineral and oil based industries, among others.

The paper is organized as follows. Section II describes the initial legal systems in the states and the transition to common law for states that were initially civil law. Section III examines the determinants of the quality of state courts in 2001. Section IV investigates the

robustness of the findings in section III to alternative dependent and independent variables, and provides evidence of institutional persistence. Section V uses initial legal family as an instrument for the quality of state courts to measure the effect of the quality of courts on two economic outcomes, household income and the share of households in poverty. Section VI concludes.

## **II. Initial Legal Conditions**

### *Classification of States by Original Legal Tradition*

Our approach is to classify the forty-eight continental states as having one of four types of initial legal conditions: civil law acquired prior to the American Revolution, civil law acquired after American Revolution, common law, or settler. Civil law states were initially settled by civil law countries such as France, Spain, or Mexico. The subdivision of civil law states reflects the fact that some states were acquired by Great Britain and others by the United States. The distinction between these two types of civil law states would capture any differences associated with Great Britain, the United States, policies towards acquired territory, the timing of settlement, and other factors. Common law states were initially settled by Great Britain, and settler states were settled by the United States. The distinction between the common law states and settler states would capture any differences between having legal institutions that originated with British common law, and legal institutions that originated with American common law.

Classification is complicated by at least two factors. First, we need to distinguish between territory that was nominally held by a country and territory that was actually settled by that country. In particular, for legal institutions to have been persistent, there has to be a reason to believe that there were legal institutions operating in the area. Second, there is a significant time dimension as well. A number of the states on the Eastern Seaboard that one might automatically think of as having originally been British common law, because they were among the original thirteen colonies, were settled or controlled in part by civil law countries such as the Netherlands, Sweden, France, or Spain during the 1600s. By the end of the 1600s, however, the

British controlled the thirteen colonies. The British acquired significant territory from France in the 1700s, and the United States acquired significant territory from France, Spain, Mexico, and Great Britain in the 1800s.

We classified the eighteen states that arose from the original thirteen British colonies as common law states. Some states such as West Virginia and Maine were admitted as states quite late, but were created by subdividing Virginia and Massachusetts. Kentucky was originally part of Virginia, and Tennessee was originally part of North Carolina. Both New York and New Hampshire claimed ownership of Vermont, but Vermont resisted both, creating an independent republic prior to joining the Union. All of these shared the original colony's legal system.

As we noted, however, because four of the eighteen states appear to have had permanent settlements by civil law countries during the seventeenth century, and at least five others had temporary settlements, classification of these states as common law is open to question.<sup>5</sup> The Dutch had settlements in Connecticut, Delaware, New York, New Jersey, and Pennsylvania, although the settlements in Connecticut appear to have been temporary. The population of Dutch New Netherlands was estimated to be 9,000 in 1664, the year that Great Britain acquired the territory.<sup>6</sup> The Swedish had settlements in Delaware and Pennsylvania.<sup>7</sup> Further, there are historical records of Dutch and Swedish courts operating in Delaware, New York, New Jersey, and Pennsylvania.<sup>8</sup> Because of the early nature and relatively short duration of Dutch and Swedish control, we classify these states as common law. In the empirical work, however, we check for robustness by re-classifying the four states with operational courts as civil law.

For later acquisitions, including the territory that Great Britain acquired from France prior to the American Revolution, and all the territory that the United States acquired from France, Spain, Mexico, and Great Britain after the American Revolution, we can use land grant records confirmed by the United States as an indirect measure of settlement by civil law governments. This is an indirect measure, because the processes for submission and the extent of United States government scrutiny of land grants varied considerably over time (Clay 1999). In

Table 1, we list by state the number of confirmed foreign land claims derived from French, Spanish, or Mexican land grants. States that were part of the territory acquired by Great Britain from France prior to the American Revolution: Michigan, Illinois, Indiana, Wisconsin, and Ohio, are included because the United States established land commissions after the American Revolution to incorporate the French land grants into the American system of property rights.

For all of the states with at least 200 confirmed claims, we were able to find additional evidence that confirmed the settlement and operation of a civil law legal system that saw a full range of cases.<sup>9</sup> These states were classified as civil law states. We more carefully scrutinized the five states with fewer than 200 land grants - Wisconsin, Ohio, Arizona, Colorado, and Iowa. The historical evidence suggests that the Colorado and Iowa grants were large speculative grants that were intended to induce, but never actually led to, substantial settlement.<sup>10</sup> Thus we classify Colorado and Iowa as settler states. Wisconsin and Ohio appear to have had sufficient permanent settlement to have had some type of local judicial official, but may not have had fully-functioning courts.<sup>11</sup> Although Arizona had fewer grants than Wisconsin and Ohio, a number of these were pueblo (town) grants and so would have encompassed multiple settlers. Arizona also had strong links to New Mexico, which had a well-developed court system. We classify Wisconsin, Ohio, and Arizona as civil law. In the empirical analysis, we check the robustness of our results by reclassifying these three states as settler states.

All states west of the Mississippi not included in Table 1 were classified as settler states. For the 48 states in the continental United States, Table 2 shows the classification of the states, the dates of statehood, and the date of the first census, and the corresponding state population, state area, and state population density. When compared to the common law states, civil law states and settler states had smaller populations, larger total areas, and much lower population densities at the time of their first census.

### *United States Acquisition of Territory from Foreign Governments*

As the United States acquired land from foreign governments, the issue of bringing the existing land and people into the United States legal system came to the fore. Early on territorial and state legislatures were effectively required to adopt American common law.<sup>12</sup> This posed few problems, since the territory involved was very lightly populated and had been settled largely by British or American colonists.

In territory that had been using civil law, the arrival of large numbers of American settlers eventually “doomed civil law everywhere except Louisiana.” Not all residents in the [post-American Revolution] civil law states were happy with the change, however, nor was the adoption of common law complete. Although Jefferson wanted Louisiana to adopt American common law, civil law was well entrenched and the native population was large.<sup>13</sup> Thus, in 1806 the territorial legislature chose civil over common law. Interestingly, what adopting civil law meant was not entirely clear because the law had not been codified. As a result, the Creole population clamored for a codification of existing law. A compromise was eventually reached, wherein Jefferson permitted the adoption of a civil code in 1808 in return for acceptance of American rule.

During the transitional period, there was a tendency for the legal systems to be hybridized. The hybridization was in part by design. American officials in Missouri wrote that they wanted “to assimilate by insensible means, the habits and customs of the American and French inhabitants; by interweaving some of the regulations of the latter into our Laws, we procure a ready obedience, without violence or complaint.”<sup>14</sup> A few years later, civil law was abolished in Arkansas and Missouri. In Texas, California, New Mexico, and Arizona, however, substantial amounts of civil law survived as it related to marital property and wills. Because the United States Supreme Court recognized property rights regarding land granted by prior governments, civil law also continued to have an effect on property rights in land. Even in Louisiana, some hybridization occurred.<sup>15</sup> For instance, the territorial legislature adopted

common law elements like trial by jury and habeas corpus. And Federal law as it applied to Louisiana was common law.

### **III. Determinants of Current Quality of State Courts**

We begin by describing our dependent variable and the two types of independent variables: initial conditions and contemporary inputs into the legal system. We then present the results of our regressions.

#### *Dependent Variable*

Our primary measure of quality of contemporary institutions is the quality of state courts as measured by the U.S. Chamber of Commerce-States Liability Ranking Survey. The survey was a telephone survey of a nationally representative sample of 824 senior attorneys at companies with annual revenues of at least \$100 million conducted during November and December of 2001.<sup>16</sup> Attorneys evaluated the overall treatment of tort and contract litigation, timeliness of summary judgment/dismissal, discovery, scientific and technical evidence, impartiality of judges, competence of judges, predictability of juries and fairness of juries on a discrete scale of 0 (worst) to 4 (best) for states for which they were familiar.<sup>17</sup> The average attorney evaluated 4.4 states. We use the average score over the 8 categories for each state. For the overall quality, the scores averaged 2.3 and ranged from a low of 1.2 for Mississippi to a high of 3.1 for Delaware. As an alternate measure of the quality of courts, we use the average score for the state for judges' competence. These scores averaged 2.5 and ranged from a low of 1.4, again for Mississippi, to a high of 3.5 for Delaware. Summary statistics are presented in Table 3. Variable definitions and selected state-level data are presented in Appendix Tables 1 and 2.

#### *Independent Variables*

To measure initial conditions, we use five variables. Three are related to the colonial legal system; one is related to the climate; and one captures membership in the Confederacy. As we discussed in the previous section, we divided the forty-eight states into four mutually

exclusive categories based on their colonial legal systems: i) states that only had civil law prior to the American Revolution; ii) states that had civil law after the American Revolution; iii) states did not have a colonial legal system (settler states); and iv) states that had common law. Dummy variables were created for each of the first three categories. In all of our regressions, common law is the omitted variable.

To measure climate, we interact a state's annual average temperature, humidity, and precipitation and then divide by 10,000 to lower the magnitude of this variable.<sup>18</sup> This variable enables us to control for the effects of climate related to disease and to the economic and political systems (see Acemoglu et al, 2001, and Engermann and Sokoloff, 2002). Table 4 shows the strong association between our climate variable and three measures of disease: yellow fever, malaria, and soldier mortality in the 1880s.<sup>19</sup>

Confederate state is a dummy variable that measures membership in the Confederacy during the American Civil War. Much like the climate variable, we include Confederate not to test the transplant-civil law hypothesis, but to control for the effects of slavery and the Civil War. In line with Engerman and Sokoloff (2002), slavery itself could have had a negative effect on the legal system. And even if slavery did not have a negative effect, the disruption in the legal system associated with the Civil War and Reconstruction could have.

To measure inputs into the legal system, we use five variables: three that measure judicial appointment processes and three that are less direct inputs. With respect to how the judiciary arrive on the bench as of 1990, we divide states into three mutually exclusive categories that are ranked in descending order according to the independence it provides for its judges in the highest state courts<sup>20</sup> (see Hanssen, 2002): the merit system, the judicial appointment system, and the partisan election system. The merit system entails the appointment of a candidate by an elected official (usually the governor) from a shortlist of candidates proposed by a nominating commission. The chosen candidate stands for subsequent terms in uncontested retention elections. The judicial appointment system entails initial selection of candidates by the governor, the

legislature, or through non-partisan elections (where judges are not allowed to declare a party affiliation). This category also includes judges selected in partisan elections but then retained by the governor. The partisan system entails selection and retention of judges in partisan elections. Dummy variables were created for the first two categories and are measured relative to partisan elections.

We also include three less direct types of inputs. The first is the log of the annual average state constitutional amendment rate, and this varies across states and is a measure of the power of the legislature to interfere with the activity of the judiciary and courts. In some states, the legislature can do little if the judiciary rules a particular piece of legislation unconstitutional. In other states, the legislature can readily amend the state constitution to make the legislation constitutional and use this to overturn judicial decisions. We also include two other measures: the number of lawyers per 1,000 residents, and a dummy variable for all states that have at least one law school in the top 50 as ranked by experts (judges and lawyers) in the U.S. News & World Report, 2001.<sup>21</sup> The annual state constitutional amendment rate ranged from 0.25 in Vermont to 8.07 in Alabama (because its distribution is skewed upwards, we use its log value); the number of lawyers ranged from 1.847 per 1,000 in South Carolina to 6.862 in Massachusetts, and seventeen of the forty-eight states have a top fifty ranked law school.

### *Regression Results*

In Table 5 we present OLS regressions that examine the determinants of the current quality of state courts. We begin by including the two types of variables: initial conditions and inputs into the legal system, separately. Because we have only forty-eight observations, our estimated standard errors will tend to be large when we include several explanatory variables. Thus, to improve efficiency, for the columns where some variables are insignificant, we apply the general to specific technique as advocated by Hendry (2000), and report test statistics to check for the validity of these joint exclusions.<sup>22</sup>

In column (1), we investigate the relationship between having been a Confederate State during the American Civil War and the current quality of the state's courts.<sup>23</sup> The relationship is negative, economically large and statistically significant. As we noted earlier, there are a number of possible causes. Confederate states may have had poorer quality judicial systems prior to the Civil War for reasons either related or unrelated to the institution of slavery. The Civil War and Reconstruction may also have represented a significant negative shock to the judiciary, independent of the previous quality of the judiciary.<sup>24</sup> The negative effect of being a Confederate state does not, however, survive the inclusion of the civil law and settler state distinctions and the climate variable in columns (2) and (3).

In columns (2) and (3), we use the measures of initial conditions to test the transplant-civil law hypothesis. Consistent with this hypothesis, the coefficient on post-Revolution civil law is negative and significant at the 1-percent level. Climate also has a negative and significant effect, supporting previous findings that hot, humid, and wet climates have negative effects on legal systems. If the effects of the transition and the climate were at least in part transitory, we would have expected the coefficients on the initial legal conditions to all be insignificant or at the very least small in magnitude. After controlling for climate and the other initial state categories, however, a civil law post-Revolution tradition is associated with more than a one standard deviation (0.35 on a scale of 0 to 4) fall in the quality of state courts, which is equivalent to Maryland or Nevada falling to the level of Arkansas.

The insignificance of the post-Revolutionary civil law and settler state and confederate state dummies in column (2) and their elimination in (3), implies that the impact of these initial conditions on current institutions is indistinguishable from a common law tradition. In the case of pre-Revolution civil law states, this is not entirely surprising, since they experienced transplantation earlier and had smaller and sparser populations at the time of transplantation than the post-Revolution civil law states.

In column (4), we examine the effects of current inputs into the legal system and exclude initial conditions. All the coefficients are significant. In terms of how judges reach the bench, states that use a merit system have better quality courts than states that use an appointment system, and both have better courts than states using partisan elections. This is consistent with the idea that judicial independence improves the effectiveness of judges and courts, and the merit system provides the most independence and partisan elections provide the least independence (see Hanssen, 2002). Frequent changes to the state constitutions and the number of lawyers per capita are both associated with lower quality courts, and the number of law schools in the top fifty has a small positive association.

In columns (5) and (6) we combine initial conditions and current inputs in order to test the transplant-civil law hypothesis. Consistent with this hypothesis, the coefficient on civil law post-Revolution remains significant at the 1-percent level, and is still associated with roughly a one standard deviation decline in the quality of courts. In column (5), all of the initial conditions are statistically insignificant except the settler state dummy, which is marginally significant (p-value is 0.085) and quantitatively small. When we apply the step-wise procedure however, all current inputs except the constitution amendment rate survive, and, most importantly, the post-Revolution civil law category remains quantitatively and statistically significant.

The robust effect of the post-Revolution civil law effect raises the question of the channels through which this effect was transmitted. Possible avenues include judges, the law, and norms. Judges typically play a different role under civil law than under common law. Specifically, under civil law, judges are less independent of the legislative branch, because their role is to interpret the code. This relative lack of prestige and power may have led to lower quality judges in the colonial period and persisted after the transition to common law. The need to be able to function under the civil and common law during the transition may also have led to initial, and possibly a persistent problem for these states in attracting high quality judges. As we discussed earlier, the laws themselves differed across the two types of states both before and even

after the transition. These differences may also have had a persistent effect. The academic literature on norms suggests that norms play an important role in reinforcing the law. Norms in civil law states may not have provided the same level of support for the law after the change to common law. Any one of these or some combination of them may have had persistent negative effects on the legal institutions of the post-Revolution civil law states.

#### **IV. Alternative Dependent Variables, Additional Independent Variables, and Persistence**

The significance and the magnitude of the effect of having been settled by a civil law country and adopted common law after the American Revolution in Table 5 are extremely suggestive. A caveat, however, is whether these results are attributable either to some feature of the dependent variable or to the omission of important independent variables. A related issue is whether it is plausible to believe that initial legal conditions would have persisted over a 150-to-200-year period. In this section, we consider alternative dependent variables, additional independent variables, and persistence.

##### *Alternative Dependent Variables*

In this section, we use four additional indirect measures of institutions: judicial competence (taken from the 2001 survey), annual average property crimes and violent crimes per 100,000 population for 1999-2001, and average federal public corruption convictions per 100,000 for 1992-2001.<sup>25</sup> We compared the correlations of the quality of the courts and judicial competence in the 2001 survey with the four indirect measures. Public corruption is negatively correlated with quality of state courts and judicial competence (correlation coefficients are  $-0.475$  and  $-0.465$ , respectively; the correlations for property crimes and violent crimes are weaker but have the expected negative sign).

In Table 6 we investigate whether our regression results are robust to these alternative measures. Because it is unclear how inputs into the legal system enter into the four indirect measures, we only include variables related to initial conditions. The regression for the quality of

courts is presented for comparison purposes. In the case of corruption, climate is the only significant initial condition. In the alternative classification of states (see footnote 20), however, civil law post-Revolution replaces climate as the only significant variable. Civil law post-Revolution is significant and has the expected sign for judicial competence, property crime, and violent crime, and its significance is robust to the alternative classifications of states.

#### *Additional Determinants of Institutions*

In this section we check if our tests of the transplant-civil law hypothesis are robust to the inclusion of additional potential determinants of institutions such as natural resources (measured by whether or not a state had significant petroleum and natural gas production, and significant mining as of 1919), initial population density and geography (including state latitude and longitude, coastal or non-coastal location, and the share of counties in a state that are close to an ocean, close to a major river, close to a navigable river and close to a lake). Table 7 reports regressions of the quality of state courts on our baseline initial conditions (pre- and post-Revolutionary civil law states, settler states, confederate states, and climate) and these additional variables. To save on degrees of freedom, we report only the results obtained using the step-wise procedure (again, joint exclusions are fully validated with the F-test reported in this table).

Column (1) in Table 1 includes natural resources and initial population density, column (2) includes geography variables and column (3) includes both. It is notable that in all cases the post-Revolution civil law effect remains significant at the 1-percent level and is still associated with roughly a one standard deviation decline in court quality; and, the civil-law pre-Revolution, settler, and confederate state categories remain statistically insignificant. Additionally, the strong negative association between petroleum and natural gas resources and legal institutions could arise from a number of sources, but it is plausible that the discovery of these resources led to the corruption of the judiciary.

### *Persistence*

For initial conditions 150-200 years earlier to affect the current quality of institutions, however, the quality of legal institutions would have to have been highly persistent over time. To test this, we would ideally have frequent measures of the quality of the courts over the 150-200 year period. Unfortunately, such data do not exist. We investigate persistence in two ways: by examining the effect of medium-term conditions on the quality of courts, and by presenting indirect evidence suggesting that state corruption and state expenditures on judicial institutions were persistent during the twentieth century.

Our first approach to investigating persistence draws on data from medium-term conditions. The five variables we use: migration, relative income per capita, black and white lynching, and voter participation, are all plausibly related to the quality of the state courts in the late nineteenth and early twentieth centuries. Migration is the inter-censal movement in (positive) and out (negative) of a state during 1870-1920 as a share of 1880 state population. In general, the story has been one of migration out of states with low economic opportunity (particularly for blacks) and to states with greater economic opportunity. If people are also motivated by the quality of the legal system or if the quality of the legal system is correlated with economic opportunity, this variable may be related to the current quality of courts. We also include initial state per capita income as a share of the United States per capita income averaged over the years 1929 and 1940. If wealthier states can afford to provide higher quality legal systems, then current institutional quality may be linked to past economic prosperity. Black and white lynching per 1,000 between 1889 and 1918 can be thought of as a measure of the rule of law.<sup>26</sup> The average of the voter participation rate in 1916 and 1920 reflects the extent to which civil participation is broad-based.

In Table 8-Panel A., we explore the effects of medium-term institutions on the current quality of courts. Black lynching is the only variable that is significant in column 4 and the only variable surviving from the general to specific test in column 5.<sup>27</sup> In Panel B, we explore the

relationship between black lynching and the initial conditions. Controlling for the black share of the population, black lynching per capita is statistically and quantitatively higher in post-Revolution civil law states. Settler states and climate are also positive and statistically significant. This suggests that initial legal traditions and climate had a negative effect on medium-term institutions, and through these institutions on the contemporary quality of courts.

Our second approach to investigating persistence draws on judicial expenditure data for selected years between 1902-1937, annual judicial expenditure data for 1951-2000, and annual state level data on prosecution of public officials for corruption covering 1976-2001. We compare four state categories and two aggregate categories: overall common law (common law and settler states) and overall civil law (pre- and post-Revolutionary) states. Corruption is the rank of the state for per capita federal convictions of public officials for corruption per 100,000 (population). Although corruption data are available yearly, they are extremely noisy with many states, particularly smaller states, having zeros in many years. Following the United States Department of Justice convention, we have ranked the states based on their decadal average for 1976-1981, 1982-1991, and 1992-2001. For 1902-1937, the state court expenditure is spending on courts as a share of total expenditures for payments. For 1951-2000, the expenditure share is defined as the courts (criminal and civil) and activities associated with courts, legal services, and counseling of indigent or other need persons as a share of total state expenditures.

Table 9-Panel A suggests that judicial expenditure and corruption have differed historically across common and civil law states. For court expenditure shares and absence of corruption, the overall common law states are ranked on average 7 and 6.4 positions higher than overall civil law states. These differences are significant at a 1-percent level. Differences in expenditure shares have two, possibly complimentary, interpretations. States vary in the duties assigned to the courts. Lower expenditure shares could, therefore, reflect that courts in civil law states have more narrowly defined duties and require lower levels of funding. Alternatively, these states may fund their court systems less generously, leading to lower quality courts.

Table 9-Panel B examines the persistence of judicial expenditure and corruption. In the case of public corruption, we have only 3 periods of data, so we run an OLS regression. The coefficient on lagged rank of corruption, 0.514, is significant at the 1-percent level. To convert this association from a decadal to annual effect, we compute its tenth root:  $(0.514)^{1/10} \approx 0.936$ . Given the standard errors on the point estimate, the range of persistence is from 0.11 to 0.48 twenty years later and from 0 to 0.16 fifty years later. Thus, corruption during the 1976-2001 period appears to have been persistent. Given the fairly short time period, it is difficult to determine whether the effects would have been persistent over a 150-200 year horizon.

In the case of judicial expenditure shares, we check for persistence using the panel unit root test of Levin et al (2003). We limit the analysis to the period 1951-2000 in which data is reported annually and estimate the following model:

$$JEXPshare_{i,t} = \alpha JEXPshare_{i,t-1} + u_{i,t} \quad (1)$$

where  $t = 1 \dots 50$  denotes a particular year;  $i = 1 \dots 48$  denotes a particular state,  $JEXPshare_{i,t}$  denotes the share of the state budget devoted to judicial expenditure share in the  $i^{\text{th}}$  state in the  $t^{\text{th}}$  year, and  $u_{i,t}$  (the error term) is distributed independently across states and may exhibit serial correlation (an auto-regressive moving average process).

We test for the null of null hypothesis of unit root:  $\alpha = 1$ , against the alternative:  $\alpha < 1$ . In the columns under the heading of judicial expenditure shares, we report the point estimate for  $\alpha$  and test statistics for the hypothesis of a unit root against the alternative with one, two, and three lags in the differenced dependent variable. Point estimates are very close to one, and the lowest p-value that we obtain is 0.377; thus we do not reject the null.<sup>28</sup>

For the period 1902-1937, we cannot test for a unit root because there are too many missing years. However, the high correlation of 0.899 between rank judicial expenditure shares and lagged rank judicial expenditure shares during this period, and the high correlation during 1902-2000 of 0.949 is suggestive of persistence. Taken together with the evidence on unit roots

in expenditure during 1951-2000, this suggests that state-level patterns of spending on the judiciary across common and civil law states have been persistent over past century.

## **V. Legal Institutions and Economic Performance**

In this section we estimate the impact of contemporary institutions on performance. Because there may be feedback, we need a source of exogenous source of variation in contemporary institutions. We have shown that there is a strong association between the post-Revolutionary civil law category and quality of state courts that is consistent with a transplant-civil law hypothesis, and that this relationship exhibits persistence. In this section we invoke the transplant-civil law hypothesis and exclude the post-Revolution civil law category from the second stage-structural estimates of economic performance in order to identify the impact of institutions on performance. We, however, fully validate this identifying exclusion restriction using over-identification test strategy.

We use two measures of current economic performance. The first is the log of the state's median household income in 2001. This averaged 10.64, ranging from a low of 10.32 (\$30,342) in West Virginia to a high of 10.92 in Maryland (\$55,042). The second is the share of the population living under the poverty line in 2001. It averaged 12 percent, ranging from 6 percent in New Hampshire to 19 percent in Louisiana.

In Table 10, we explore the relationship between the log of median household income and the quality of the courts, and the relationship between poverty and the quality of the courts after controlling for variables capturing initial conditions including initial population, climate, geography, and natural resources. In Table 11, we explore the relationship between institutions and economic performance controlling for the relevant initial conditions and intermediate and contemporaneous conditions. Both tables have five panels. In Panel A we report structural estimates of performance using the 2SLS procedure in which the post-Revolution civil law category is excluded for identification; and, in Panel B we list covariates (besides the post-

Revolution civil law category) excluded from the structural equation and test statistics that validate these exclusions. In Panel C we report OLS estimates of the relationship between institutions and performance that we compare with the 2SLS estimates. In panel D we provide evidence of the strength of the post-Revolution civil law category as an instrument from the reduced form estimates. In Panel E we validate the identifying exclusion restriction in our structural estimates using an over-identification test. Specifically, we use climate as an additional instrument because it is always statistically insignificant in our structural (second stage) estimates, and it is always statistically significant in our reduced form (first stage) estimates; we fail to reject the null hypothesis of no over-identification using the Sargan (1958) test and the Hansen (1982) J-test and, thereby, validate the exclusion of the post-Revolution civil law states.<sup>29</sup>

Consider first the relationship between performance and institutions controlling for initial conditions in Table 10. In both sets of 2SLS structural estimates in Panel A, quality of courts is statistically significant at the 1-percent level and leads to better outcomes. For example, the reduced form estimates in Panel D corresponding to the structural estimates for median household income and poverty show that if we could change history and have a post-Revolution civil law state take on a common law tradition prior to the American Revolution, then court quality would increase by 0.463 and 0.499, respectively (on average, roughly one and a third standard deviations in court quality). This in turn would be associated with a  $(0.245 * 0.463) = 11.3$  percent increase in median income, and a  $(-0.8180 * 0.499) = 4.1$  percentage point fall in the poverty rate. Comparing the 2SLS and OLS estimates of the structural equation (Panels A and C), the point estimates for the impact of courts on performance are very close; thus, there is no evidence of attenuation bias or measurement error. Finally, the test statistics in Panels B and E fully validate the exclusion of initial condition covariates and the identifying exclusion restriction in the 2SLS structural estimates; and the 1-percent significance level of the state court regressor and the partial  $R^2$  statistic in Panel D are evidence of the strength of our identifying instrument.

Because the overall estimated impact of institutions on economic performance appears to be very high when we control for initial conditions, in Table 11 we control for the initial conditions already determined significant for performance (average distance to lakes and oceans for median household income, and longitude for poverty), and all intermediate and contemporaneous conditions, even though these additional covariates could be endogenous. In the 2SLS structural estimates reported in Panel A, the quality of courts is statistically significant at no less than a 5-percent level and leads to better outcomes. Again, the impact of reversing a post-Revolutionary civil law tradition is substantial: median household income increases by  $(0.192 \times 0.378) = 7.3$  percent, and the poverty rate falls by  $(-8.747 \times 0.469) = 4.1$  percentage points. Thus, including these additional covariates lowers the estimated impact of institutions on median household income but has no discernible impact on the estimated impact on poverty. Results in Panels C, B, and E show that, again, the OLS and 2SLS estimates of the impact of institutions on performance are quite similar, the exclusion of the full range of covariates in the structural equations and identifying exclusion restriction are all valid, and the post-Revolutionary civil law category is a strong instrument.

## **VI. Conclusions**

In this paper, we examine the effects of a unique natural experiment in which some states within the United States were settled by civil law countries, others were settled by common law countries, and the civil law states, with the possible exception of Louisiana, adopted common law around the time of statehood. The testable hypothesis is that post-Revolution civil law states had highly developed legal systems around the time of legal transplantation, and this made them vulnerable to transplant-civil law effect. We find, after controlling for climate, that those states that had been settled by a civil law country and then adopted common law after the American Revolution have experienced a sizeable and significant negative effect on the quality of their state legal institutions. This in turn has had a substantial impact on economic outcomes in 2001.

This finding provides important new evidence on the effect of initial conditions on institutional and economic development. Because countries are typically heterogeneous and rarely change legal systems, it can be difficult to disentangle the effects of colonial settlement from the subsequent evolution of the institutions in that country. We present evidence that the effects of initial legal family, the transplantation of common law into civil law states, or both have had very persistent negative effects on legal institutions in the United States context. This differs from prior work that by Acemoglu et al (2001) that has focused primarily on the effect of the disease environment on colonists' willingness to invest in institutions (see Acemoglu et al, 2001) or on the suitability of the climate and soil quality for growing crops that required slave labor (see Engerman and Sokoloff, 2002). Clearly, further work remains to be done on isolating the effects of the colonial legal systems in the United States (and in other contexts) and in better understanding the long-run implications of transplanting one legal system into an area previously governed by a different legal system.

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Table 1: Confirmed Private Land Claims  
[to June 30, 1904]

<i>State</i>	<i>Number of Claims</i>	<i>Area of Claims in acres</i>	<i>Classification</i>
Louisiana	9,302	4,347,891.31	Civil, post-Rev.
Oregon	7,432	2,614,082.24	Common Law
Missouri	3,748	1,130,051.62	Civil, post-Rev.
Mississippi	1,154	773,087.14	Civil, post-Rev.
Washington	1,011	306,795.91	Common Law
Michigan	942	280,672.83	Civil, pre-Rev.
Illinois	936	185,774.37	Civil, pre-Rev.
Florida	869	2,711,290.57	Civil, post-Rev.
Indiana	862	188,303.62	Civil, pre-Rev.
California	588	8,850,143.56	Civil, post-Rev.
New Mexico	504	9,899,021.67	Civil, post-Rev.
Alabama	448	251,602.04	Civil, post-Rev.
Arkansas	248	110,090.39	Civil, post-Rev.
Wisconsin	175	32,778.82	Civil, pre-Rev.
Ohio	111	51,161.14	Civil, pre-Rev.
Arizona	95	295,212.19	Civil, post-Rev.
Colorado	6	1,397,885.78	Settler
Iowa	1	5,760.00	Settler

Source: From the Report of the Public Lands Commission (Coville et al, 1904) <http://memory.loc.gov/gc/amrv/vg57old/vg57.html> Image 84. Oregon and Washington were settled by Great Britain. Utah (60 grants totaling 8,876.80 acres) was not included, because we could not find *any* documentary evidence indicating the source of these land claims. In particular, we could not find any evidence to suggest that they were confirmed as part of the work of the Surveyor General of the New Mexico Territory or the Court of Private Land Claims, which were responsible for addressing claims in all territory acquired from Mexico other than California. Utah is classified as a settler state. Land grants for Texas are not reported because Texas was briefly independent and therefore handled land grants itself (the state retained rights to the land).

Table 2: Classification of States

State	Year of Statehood	First Census			
		Date	Population	Area in Square Miles	Pop. /Sq. Mile
<i>Pre-Revolution Civil Law States</i>					
Illinois	1818	1800	2,458	57914.4	0.04
Indiana	1816	1800	2,632	36417.7	0.07
Michigan	1837	1800	3,757	96716.1	0.04
Ohio	1803	1800	42,159	44824.9	0.94
Wisconsin	1848	1820	1,444	65497.8	0.02
<i>Average</i>	<i>1824</i>	<i>1804</i>	<i>10,490</i>	<i>60,274.2</i>	<i>0.22</i>
<i>Post-Revolution Civil Law States</i>					
Alabama	1819	1800	1,250	52419.0	0.02
Arizona	1912	1860	6,482	113998.3	0.06
Arkansas	1836	1810	1,062	53178.6	0.02
California	1850	1850	92,597	163695.6	0.57
Florida	1845	1830	34,730	65754.6	0.53
Louisiana	1812	1810	76,556	51839.7	1.48
Mississippi	1817	1800	7,600	48430.2	0.16
Missouri	1821	1810	19,783	69704.3	0.28
New Mexico	1912	1850	61,547	121589.5	0.51
Texas	1845	1850	212,592	268580.8	0.79
<i>Average</i>	<i>1847</i>	<i>1827</i>	<i>51,420</i>	<i>100,919.1</i>	<i>0.44</i>

Table 2-continued

State	Year of Statehood	First Census			
		Date	Population	Area	Pop. /Sq. Mile
<i>Settler States</i>					
Idaho	1890	1870	14,999	83570.1	0.18
Iowa	1846	1840	43,112	56271.6	0.77
Kansas	1861	1860	107,206	82276.8	1.30
Minnesota	1858	1850	6,077	86938.9	0.07
Montana	1889	1870	20,595	147042.4	0.14
Nebraska	1867	1860	28,841	77353.7	0.37
Nevada	1864	1860	6,857	110560.7	0.06
North Dakota	1889	1870	2,405	70699.8	0.03
Oklahoma	1907	1890/4	61,834	69898.2	0.88
South Dakota	1889	1860/5	4,837	77116.5	0.06
Utah	1896	1850	11,380	84898.8	0.13
Wyoming	1890	1870	9,118	97813.6	0.09
<i>Average</i>	<i>1879</i>	<i>1860</i>	<i>27,041.4</i>	<i>88,348.8</i>	<i>0.34</i>
<i>Common Law States</i>					
Connecticut					
Delaware	1787	1790	59,096	2489.3	23.74
Georgia	1788	1790	82,548	59424.8	1.39
Kentucky	1792	1790	73,677	40409.0	1.82
Maine	1820	1790	96,540	35384.7	2.73
Maryland	1788	1790	319,728	12406.7	25.77
Massachusetts	1788	1790	378,787	10554.6	35.89
New Hampshire	1788	1790	141,885	9349.9	15.17
New Jersey	1787	1790	184,139	8721.3	21.11
New York	1788	1790	340,120	54556.0	6.23
North Carolina	1789	1790	393,751	53818.5	7.32
Oregon	1859	1850	12,093	98380.6	0.12
Pennsylvania	1787	1790	434,373	46055.2	9.43
Rhode Island	1790	1790	68,825	1545.1	44.55
South Carolina	1788	1790	249,073	32020.2	7.78
Tennessee	1796	1790	35,691	42143.3	0.85
Vermont	1791	1790	85,425	9614.3	8.89
Virginia	1788	1790	691,737	42774.2	16.17
Washington	1889	1850	1,201	71299.6	0.02
West Virginia	1863	1790	55,873	24229.8	2.31
<i>Average</i>	<i>1803</i>	<i>1796</i>	<i>197,125</i>	<i>33036.0</i>	<i>13.71</i>

Table 3: Selected Summary Statistics

Variable	Variable Type	Timing	Avg.	Std. Dev.	Min.	Max.
Civil-law: post-Revolution	Dummy relative to common law state	Status prior to 1776 or at time of statehood	.208	.410	0	1
Civil law: pre-Revolution			.104	.309	0	1
Settler			.271	.449	0	1
Common Law			.417	.498	0	1
Climate	Avg. temp.* humidity* Precip.*(.(0001)	Annual	13.1	7.50	1.99	39.7
Confederate State	Dummy relative to all other states	Status prior to 1863	.229	.425	0	1
Migration, 1870-1920	Estimated net inter-censal migration/ 1880 population	1870-1920	.268	.517	-0.072	2.338
Initial state income	Relative annual average	1929 & 1940	89.1%	31.9%	38.6%	160%
Lynching of blacks	Per 100,000 1910 population	1889-1918	0.029	0.054	0.000	0.214
Lynching of whites			0.016	0.031	0.000	0.199
Voter participation	Avg. participation rate in federal elections	1916 & 1920	55.2%	18.4%	13.05%	80.6%
Log Median Household Income	Annual survey	2001	10.64	0.15	10.32	10.92
Poverty rate			12.0%	3.0%	6.0%	19.1%
Quality of state courts	Survey of senior law firm partners	2001	2.32	0.35	1.2	3.1
Judges' competence			2.47	0.37	1.4	3.5
Public corruption	Per 100,000 1996 population	1992-2001	2.73	1.52	0.415	7.06
Property crimes	Per 100,000 population	1999-2001	3,578	817	2,198	5,393
Violent crimes		1999-2001	424.9	193.9	76.0	821.1
Lawyers per capita, 2001	Per 1,000 inhabitants	2001	3.080	1.193	1.847	6.862
Law School Quality, 2001: Expert opinion	Annual Survey of Judges and Lawyers	2001	.313	.468	0	1
Judicial selection procedures						
Merit system, 1990	Versus partisan elections; dummy variables	1990	0.292	0.459	0	1
Appointment, 1990			0.542	0.504	0	1
State constitution amendment rate	Annual average	Through 1990	1.414	1.387	0.25	8.07

Table 4-Association Between Climate, Disease and Soldier Mortality			
Dependent Variable	Yellow Fever dummy, 1700s&1800s; probit regression	Malaria dummy, 1912; probit regression	Soldier Mortality in 1800s; OLS regression
Climate	0.171* (0.060)	0.141* (0.041)	0.078* (0.016)
Constant	-3.79* (1.04)	-2.05* (0.567)	1.12* (0.25)
Pseudo R <sup>2</sup>	0.427	0.275	
R <sup>2</sup>			0.327

Notes: In this table and all other tables, \*, \*\* and \*\*\* denote significance at the 1-percent, 5-percent and 10- percent levels. Standard errors are reported in parentheses. Malaria dummy = 1 if state had outbreaks during 1912, 0 otherwise. Earliest data from 1881 is not used because almost all of the states were afflicted (Source is Pan American Health Organization, 1969). Yellow fever dummy = 1 if there were 5 or more major outbreaks during the 1700s and 1800s, 0 otherwise (robust if we use 3 or more major outbreaks; source is World Health Organization, Geneva, 1998, Appendix I). Solder mortality is the average annual share of solder dying as a share of soldier strength during 1929-1838 and 1839-1854 and then averaged and was computed in Mitchener and McLean, 2003.

Table 5-Determinants of Institutions  
 Dependent Variable is Quality of State Courts, 2001

	1	2	3	4	5	6
Civil-law state: post-Revolution		-0.399* (0.116)	-0.382* (0.102)		-0.438* (0.116)	-0.386* (0.090)
Civil-law: pre- Revolution		-0.026 (0.143)	X		-0.119 (0.131)	X
Settler state		-0.074 (0.117)	X		-0.198*** (0.112)	X
Confederate State	-0.360* (0.107)	0.005 (0.149)	X		0.024 (0.148)	X
Climate		-0.017*** (0.009)	-0.015* (0.006)		-0.007 (0.008)	X
Judges: Merit vs. partisan elections				0.641* (0.122)	0.615* (0.139)	0.561* (0.109)
Appointment vs. partisan elections				0.602* (0.116)	0.433* (0.133)	0.423* (0.110)
Constitution amendment rate, logged				-0.109** (0.051)	-0.019 (0.058)	X
Lawyers per capita, 2001				-0.076*** (0.038)	-0.085** (0.038)	-0.073** (0.033)
Law School Quality, 2001				0.173*** (0.092)	0.199** (0.089)	0.199** (0.080)
Constant	2.405* (0.107)	2.657* (0.131)	2.601* (0.080)	1.995* (0.124)	2.367* (0.189)	2.175* (0.117)
P-value for exclusion of X from unrestricted regression			0.938			0.481
Adjusted R <sup>2</sup>	0.179	0.346	0.383	0.409	0.540	0.545

Table 6: Alternative Dependent Variables

State classification	Baseline					Robustness check 3
Dependent Variable	Courts Quality	Judicial Competence	Property Crimes	Violent Crimes	Corruption	Corruption
Civil-law state: post-Revolution	-0.382* (0.102)	-0.345* (0.113)	723** (284)	156** (64.4)	X	1.19** (2.50)
Civil-law: pre-Revolution	X	X	X	X	X	X
Settler state	X	X	X	-120** (58.9)	X	X
Confederate state	X	X	459*** (275)	X	X	X
Climate	-0.015* (0.006)	-0.017** (0.006)	X	X	0.064** (0.028)	X
Constant	2.657* (0.131)	2.760* (0.089)	3322* (121)	425* (34.4)	1.891* (0.428)	2.503* (0.234)
P-value for exclusion of X from unrestricted regression	0.938	0.598	0.647	0.604	0.685	0.354
Adjusted R <sup>2</sup>	0.383	0.321	0.235	0.211	0.079	0.076

Table 7- Natural Resource, Population, Geography and Institutions  
 Dependent Variable is Quality of State Courts, 2001

Additional Controls	Natural Resources and Initial Population	Geography	Nat Resources, Initial Population and Geography
Civil-law: post-Revolution	-0.367* (0.096)	-0.352* (0.099)	-0.347* (0.095)
Climate	-0.015* (0.005)	-0.019* (0.006)	-0.018* (0.005)
Petroleum and Natural Gas Dummy	-0.218** (0.085)		-0.187** (0.086)
Share of counties close to the ocean		0.248** (0.119)	0.192 (0.117)
Constant	2.653* (0.078)	2.588* (0.077)	2.636* (0.077)
Variables excluded:			
Initial Conditions	Civil-law: pre-Rev, Settler, Confederate	Civil-law: pre-Rev, Settler, Confederate	Civil-law: pre-Rev, Settler, Confederate
Geography		State Latitude, State Longitude, Coastal Dummy, Share of counties close to major river, navigable river, lake	State Latitude, State Longitude, Coastal Dummy, Share of counties close to major river, navigable river, lake
Natural Resources and Population	Initial Population Density, Mining Dummy		Initial Population Density, Mining Dummy
P-value for F-test of joint exclusions from unrestricted regression	0.961	0.964	0.999
Adjusted R <sup>2</sup>	0.451	0.426	0.471

Table 8-Panel A Persistence of Intermediate Institutions Dependent Variable is Quality of State Courts, 2001		
	1	2
Migration, 1870-1920	0.023 (0.094)	X
Initial income, 1929&40	0.133 (0.160)	X
Lynching 1889-1918, of Blacks	-3.371** (1.324)	-3.758* (0.764)
Lynching, 1889-1918, of Whites	-0.201 (1.552)	X
Voter participation, 1916&20	-0.000 (0.0004)	X
Constant	2.317* (0.266)	2.432* (0.046)
P-value for exclusion of X from unrestricted regression		0.946
Adjusted R <sup>2</sup>	0.279	0.331

Table 8-Panel B Determinants of Medium Term Institutions		
Dependent Variable	Black Lynching, 1889-1918	Voter Participation, 1916&1920
Civil-law: post-Revolution	0.0388* (0.0091)	X
Civil-law: pre-Revolution	X	X
Settler state	0.020** (0.009)	X
Confederate state	X	-37.7* (3.11)
Climate	.0017* (0.0008)	X
Black population share, 1910	0.203* (0.034)	X
Constant	-0.0289* (0.0106)	63.9* (1.49)
P-value for exclusion of X from unrestricted regression	0.719	0.467
Adjusted R <sup>2</sup>	0.821	0.756

Paired Category	Rank of State Court Expenditures <sup>b</sup>	Rank of Public Corruption <sup>b</sup>
Common Law – Settler	5.6* (0.000)	-5.9** (0.041)
Common Law – Pre-Revolutionary	15.1* (0.000)	3.0 (0.441)
Common-Law – Civil: Post-Revolution	6.1* (0.000)	4.6 (0.113)
Settler – Civil: Pre-Revolution	9.5* (0.000)	8.9** (0.045)
Settler – Civil: Post-Revolution	0.69 (0.289)	10.6* (0.002)
Civil: Pre – Civil: Post-Revolution	-8.8* (0.000)	1.7 (0.681)
Overall Common Law-Civil Law	7.0* (0.000)	6.4* (0.001)

<sup>a</sup> Standard errors are in parentheses. <sup>b</sup> A two-sided two-sample t test with equal variances is performed and its P-values are reported in parentheses for the null hypothesis that there the difference in ranked means is zero; \*, \*\* and \*\*\* denote significance at the 1-percent, 5-percent and 10-percent levels. The court expenditure data cover 14 years between 1902 and 1937 all 50 years during 1951-2000. For 1902-1937, the share is spending on courts as a share of total expenditures for payments. For 1951-2000, the expenditure share is defined as the courts (criminal and civil) and activities associated with courts, legal services and counseling of indigent or other need persons as a share of total state expenditures.<sup>30</sup> The data cover for public corruption cover three periods: 1976-81, 1982-1991 and 1992-2001.<sup>31</sup>

Dependent Variable	State Corruption	Judicial Expenditure Shares		
Test	OLS	Unit root test		
Lagged Dep. Variable, decadal	0.514* (0.0885)			
Annual	0.936	0.999	0.999	1.001
Lags	0	1	2	3
Observations	96	2306	2260	2214
Test statistics for Unit Root Hypothesis Model: $\text{jud exp shares}_{i,t} = \alpha \text{jud exp shares}_{i,t-1} + u_{i,t}$ , where $I = 1, \dots, 48 = \text{state}$ $T = 1950 \dots 2000 = \text{year}$				
Null and Alternative Hypotheses		Null: $\alpha = 1$ Alternative: $\alpha < 1$		
P-value for null versus alternative; Levin et al (2003)		0.425	0.377	0.552

Table 10: Institutions and Economic Performance with Controls for Initial Conditions (Climate, Population, Geography and Natural Resources)		
Panel A- Second Stage 2SLS Structural Estimates		
Dependent Variable	Log Median Household Income	Poverty Rate
Quality of state courts, instrumented	0.245* (0.082)	-8.180* (1.454)
Share of counties close to an ocean	0.169* (0.048)	X
Share of counties close to a lake	0.183 (0.109)	X
State longitude	X	-0.048** (0.020)
Panel B-Hypothesis tests for 2SLS Structural Estimates		
Variables excluded from the structural equation besides Civil-law: post-Revolution	Climate, confederate, log init pop density, mining, petroleum and natural gas, latitude, longitude, coastal, close to major river, and navigable river	Climate, confederate, log init pop density, mining, petroleum and natural gas, latitude, coastal, close to lake, ocean, major and navigable river
P-value for exclusions	0.932	0.964
Panel C-OLS Regressions of Structural Equation (Covariates are not reported)		
Dependent Variable	Log Median Household Income	Poverty Rate
Quality of state courts	0.228* (0.045)	-6.610* (0.811)
Adjusted R <sup>2</sup>	0.494	0.595
Panel D-Reduced Form Estimates Dependent Variable is Quality of State Courts (Covariates are not reported)		
Civil-law: post-Revolution	-0.463* (0.105)	-0.499* (0.104)
Partial R <sup>2</sup> (Excluded instruments)	0.304	0.337
Panel E- Over-identification Test Post-Revolution civil law and climate are the excluded instruments P-values for null hypothesis of no over-identification		
Sargan test	0.483	0.656
Hansen's J test	0.425	0.656

Table 11: Institutions and Economic Performance with Controls for Initial, Intermediate and Contemporaneous Conditions		
Panel A- Second Stage 2SLS Structural Estimates		
Dependent Variable	Log Median Household Income	Poverty Rate
Quality of state courts, instrumented	0.192** (0.086)	-8.747* (1.493)
Initial state income, 1929&40	0.169** (0.080)	X
Migration, 1870-1920	-0.061** (0.025)	1.027*** (0.568)
Lawyers per capita, 2001	0.029*** (0.017)	-0.724* (0.238)
Panel B- Hypothesis tests for 2SLS Structural Estimates		
Variables excluded from the structural equation besides Civil-law: post-Revolution	Distance to lake, distance to ocean, black lynching, white lynching, early voter participation, judicial merit and appointment systems, amendment rate, law school quality	Distance to lake, distance to ocean, black lynching, white lynching, early voter participation, initial state income, judicial merit and appointment systems, amendment rate, law school quality.
P-value for exclusions	0.882	0.998
Panel C-OLS Regressions of Structural Estimates (Covariates are not reported)		
Dependent Variable	Log Median Household Income	Poverty Rate
Quality of state courts	0.218* (0.043)	-6.714* (0.771)
Adjusted R <sup>2</sup>	0.660	0.642
Panel D-Reduced Form Results Dependent Variable is Quality of State Courts (Covariates are not reported)		
Civil-law: post-Revolution	-0.378* (0.100)	-0.469* (0.106)
Partial R <sup>2</sup> (Excluded instruments)	0.249	0.309
Panel E- Over-identification Test Post-Revolution civil law and climate are the excluded instruments P-values for null hypothesis of no over-identification		
Sargan Test	0.860	0.683
Hansen's J test	0.847	0.718

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<sup>1</sup> Friedman (1973), p. 169.

<sup>2</sup> Banner (2000), p. 95 quoting J. B. C. Lucas to Thomas Jefferson, 10 Dec 1805, Lucas Papers, box 2, Missouri Historical Society.

<sup>3</sup> See Bancroft (1888, reprinted 1970), p. 317, footnote 11 and J. Ross Browne (1850, reprinted 1973), David Langum (1987) also provides some interesting quotes from American traders who lived in Mexican California about their dislike of civil law. See Chapter 5, especially pp. 146ff.

<sup>4</sup> Crosby (1945) p. 58

<sup>5</sup> South Carolina had both Spanish and French settlements. These settlements failed, however, and surviving colonists left. Georgia had Spanish missions. See Gannon (1992) on the Spanish settlement of Santa Elena and the Spanish missions in Georgia. There were early French settlements in Maine and Vermont, but these appear not to have been permanent. Much of the French-American presence in Maine and Vermont dates from migration into Canada during the late eighteenth century. Calloway (1990), Coolidge (1938, reprint 1989) and Hatch (1919).

<sup>6</sup> See Rink (1986). In 1673, the Dutch temporarily regained control of New Netherland. The land was officially ceded to England in 1674.

<sup>7</sup> Johnson (1919, reprinted 1996).

<sup>8</sup> For court records pertaining to New York and New Jersey, see Van Laer (1974) for court records of the Director General and Council of New Netherland (the highest court, covering all of New Netherland) 1638-1664; and O'Callaghan and Fernow (1897, reprinted 1976) for court records of the Courts of Schouts and schepens for New Amsterdam. For court records pertaining to Pennsylvania and Delaware, see Brodhead and O'Callaghan (1853), volume 12 for Dutch minutes of court actions, 1655-1657; Armstrong (1969) for records of the Upland Court (Chester County, Pennsylvania), 1676-1681 which was a Dutch court that continued to operate after English acquisition; Gehring (1981) has records for the Dutch 1648-1664; and Johnson (1930) for some court minutes for New Sweden, 1643-1644.

<sup>9</sup> The large number of land grants in Illinois (936 grants), Indiana (862), and Michigan (942), suggests that the population was significant. Further, records from the village assemblies, which governed many aspects of village life, and records of disputes that made it to New Orleans suggest that there was something similar to a formal judicial system. For more on French Illinois, see Ekberg (1998) and Briggs (1990).

Unfortunately, there was only rarely a notary in the Illinois country, and what notarial records there may have been have not survived. There has been an assumption by some historians that there was no legal system in some colonies prior to the American legal system. Book length legal histories that cover the colonial period exist for Arkansas, California, Florida, Missouri, Louisiana, New Mexico, and Texas. See Arnold (1985) on Arkansas, Banner (2000) on Missouri, Cutter (1995) on Texas and New Mexico, Fernandez (2001) on Louisiana, Langum (1987) on California, and Matthews (1987) on Florida. On Natchez, Mississippi, see Holmes (1963) and on Mobile, Alabama, see Hamilton (1910). For West Florida, see also *Archives of Spanish Government of West Florida, 1782-1816*. National Archives T1116.

<sup>10</sup> Gates (1968).

<sup>11</sup> Ekberg (1998) and Briggs (1990) do not mention courts, but the legal system is not the main topic of their work.

<sup>12</sup> Congress also appointed land commissions to bring land grants made by prior governments into the United States system of property rights. See Clay (1999).

<sup>13</sup> This paragraph draws heavily on Dargo (1975).

<sup>14</sup> Friedman (1973), p. 168, quoting Judge John Coburn to Secretary of State James Madison, 1807.

<sup>15</sup> See Friedman (1973), pp. 171-176.

<sup>16</sup> This survey was repeated in 2003 and the average rankings across the two surveys are highly correlated (0.986). We use only the 2001 survey because we do not have comparable economic data for 2003.

<sup>17</sup> We exclude treatment of class action suits and punitive damages in our calculated average because these two categories cannot be determined in several states.

<sup>18</sup> Our results are robust if we use several alternative measures including annual temperature multiplied by annual humidity, and annual humidity multiplied by annual precipitation.

<sup>19</sup> Yellow fever is a dummy variable that measures whether the state had at least five reported yellow fever epidemics during the 1700s and 1800s. Malaria is a dummy variable that measures whether malaria was considered to be endemic to the state in 1912. In the 1800s malaria existed in a number of other states, but

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the malaria problem was reportedly not as severe in those states as it was during 1912. The climate variable is also positively associated with soldier mortality in the 1800s, which are overall deaths as a share of soldier strength (see Mitchener and McLean, 2003).

<sup>20</sup> This coding is taken from Hanssen (2002), who notes that the selection and retention are similar for lower level state judges.

<sup>21</sup> We also used the popular survey, but found that this had no explanatory power.

<sup>22</sup> Except where specifically noted, these results and all other reported results in this section are robust to the alternative state classifications discussed in section III. The three reclassifications are: 1) Wisconsin, Ohio, and Arizona are moved to settler states; 2) Delaware, Pennsylvania, New York, and New Jersey are moved to civil law pre-Revolution states; 3) starting from the baseline classification, the reclassifications in 1) and 2) are combined. The results are also robust to the inclusion of a separate dummy variable for Louisiana as it is arguable that it was subject to only the civil law effect and it was not subject to a transplant effect.

<sup>23</sup> We ran a similar regression in which Confederate states were replaced with Slave states. Slave states included the eleven Confederate states, and Delaware, Maryland, Kentucky, and Tennessee. In part because of the inclusion of Delaware, the coefficient on slave state was smaller and the adjusted R-squared was much lower. The coefficient may also have been smaller, because these states experienced fewer negative affects related to the Civil War

<sup>24</sup> For example, Wahl (1998) shows that antebellum legal institutions effectively protected the value of rented slaves.

<sup>25</sup> These measures are indirect because they measure outcomes related to the judicial system, rather than the quality of the judicial system itself. Property and violent crimes are further complicated by the fact that they are subject to reporting biases at two levels. Individuals must report the crimes to government agencies, and these agencies must report them to the FBI. Corruption data is complicated by the fact that these are convictions, and so reflect both the propensity to prosecute and the actual conviction rate, both of which may vary over time.

<sup>26</sup> An important source on lynching data is Elizabeth Hines and Eliza Steelwater, Project HAL: Historical American Lynching Data Collection Project, <http://people.uncw.edu/hinese/HAL/HAL%20Web%20Page.htm>

<sup>27</sup> We might have expected early voter participation and relative income to be significant, since they are correlated with the dependent variable. These variables are, however, also correlated with black lynching. These results are robust if we use corruption or judicial competence as our measure of institutions.

<sup>28</sup> A shortcoming of the Levin, Lin, and Chu test is that it assumes that all the state rank expenditure series are stationary under the alternative. To address this issue, we employ the Im, Pesaran and Shin (2003) test, which is consistent with the alternative that a fraction of the rank expenditures are stationary. Again, we fail to reject the null of a unit root.

<sup>29</sup> These tests hold as null hypotheses that the post-Revolution civil law category and climate are uncorrelated with the error term of the structural equation for economic performance under the assumptions of homoskedasticity and heteroskedasticity.

<sup>30</sup> Sources for the period 1951-2000 are U.S. Census Bureau, Annual Survey of State and Local Government Finances and Census of Governments (various years), and <http://www.census.gov/govs/www/class.html> [http://www.census.gov/govs/www/class\\_ch4\\_chartb.html](http://www.census.gov/govs/www/class_ch4_chartb.html). Sources for 1902-37 include Department of Commerce and Labor, Wealth, Debt and Taxation Special Reports of the Census Office, U.S. Government Printing Office; and Department of Commerce, Bureau of the Census, Financial Statistics of States (for the years including 1915, 1916, 1917, 1918, 1924, 1925, 1926, 1927, 1928, 1929, 1930, 1931, 1937), Washington Government Printing Office.

<sup>31</sup> We use state population in 1979, 1987, and 1996 to convert overall federal convictions of corruption public officials into convictions per 100,000 in the 1976-81, 1982-91 and 1992-2001, respectively. Sources for public corruption are the Public Integrity Section, Criminal Division, United States Department of Justice, Report to Congress on the Activities and Operations of the Public Integrity Section (for the years 1981, 1991, and 2001). Population data are from U.S. Department of Commerce, Bureau of the Census, State and Metropolitan Area Data Book, 1979, U.S. Bureau of the Census, 1990 Census of Population and Housing, Population and Housing Unit Counts, and U.S. Census Bureau, Statistical Abstract of the United States, 2002.

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