

Legal Origins and the Evolution of Institutions:

Evidence from American State Courts

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Abstract

Several important studies of institutions assume that the quality of institutions is persistent following some formative historic event. The assumption of institutional persistence, however, begs the question of how these institutions persisted. To better understand this issue, this paper examines the evolution of state courts in the United States. We begin by reviewing the evidence that France, Spain, and Mexico operated civil-law legal systems in territory that would later make up thirteen states. One important philosophical difference between civil-law and common-law legal systems arises from differences in their beliefs regarding the appropriate degree of judicial independence. To show how these beliefs, if persistent, would manifest themselves, we present a model in which legislatures allocate budgets to their judges. In the model, common and civil-law legislatures have different preferences regarding the level of judicial independence. Our model predicts civil-law legislatures will give fewer discretionary resources to their judges when judicial elections are replaced by a system of appointments. We confirm this prediction using state-level data for the period 1961-1999. Finally, we argue that one important reason why civil-law preferences for a weak judiciary appear to have persisted in the American states is that the political culture within state legislatures is slow-moving.

1. Introduction

There is a growing body of evidence documenting that the quality of institutions that protect property rights can explain the large variations in long term growth, financial market development, and other important outcomes across countries. Many of these studies assume that the quality of institutions does not change very much after some formative historic event. Acemoglu, Johnson and Robinson (2001), for example, argue that the way in which early settlers of former colonies built institutions that protected property rights during the seventeenth, eighteenth and nineteenth centuries was formative. La Porta, Lopez-de-Silanes, Shleifer and Vishny (1998), Djankov, La Porta, Lopez-de-Silanes and Shleifer (2002) and La Porta, Lopez-de-Silanes, Pop-Eleches and Shleifer (2004) argue that the development of common law and civil law in the Middle Ages and the transplantation of these legal codes around the world during the nineteenth and early twentieth century was also formative for institutions that affect finance, entry, and regulation.¹

The assumption of institutional persistence, however, begs the question of how these institutions persisted. To better understand the mechanisms through which institutions persist, this paper examines the evolution of state courts in the United States during the twentieth century. We are interested in state courts because they are responsible for evaluating, enforcing and even creating law.² American states provide a useful laboratory, because they share a common language and many other attributes. Yet they are also characterized by diverse geographic, cultural, and, importantly for this paper, legal initial conditions.

¹ Other relevant studies include Engerman and Sokoloff (2001, 2002), Rodrik, Subramanian and Trebbi (2004), and Berkowitz, Pistor and Richard (2003).

² For a good overview on evolution of the state courts, see G. Alan Tarr, (1996, 1998).

We begin by reviewing the evidence that France, Spain, and Mexico established and operated civil-law legal systems in territories that would later make up thirteen states. The remaining thirty-five states in the continental United States were settled by England or the United States and always had common law. All of the civil-law states, with the exception of Louisiana, adopted common law around the time they entered the Union.³ One important philosophical difference between civil-law and common-law legal systems arises from differences in their beliefs regarding the appropriate degree of judicial independence. Common-law legal systems prefer more independent judges, and civil-law legal systems prefer less independent judges (Merryman 1985).

To show how the attitudes in legislatures about judicial independence, if persistent, would manifest themselves, we present a model in which common-law and civil-law legislatures have different preferences regarding the level of independence of the judiciary. The model builds on Maskin and Tirole (2004) and predicts that once judicial elections are replaced with appointments, civil-law legislatures will make larger cuts in their judges' discretionary budgets than common-law legislatures. We confirm these predictions using data from the period 1961-1999. In a set of falsification exercises, we show that the observed effect is attributable to differences in historic legal systems and not to historic differences associated with slavery. To support our assumption that civil-law and common-law preferences for judicial independence could plausibly have persisted for long periods of time, we provide evidence that the political culture in state legislatures was persistent during the twentieth century.

³ Despite the fact that only one of the thirteen states continues to have a civil-law legal system, we will, for convenience, refer to states that initially had civil law as civil-law states.

Our paper is closely related to Hansen (2004b), who argues that state legislatures optimally set the level of judicial independence by choosing retention methods for their judges. Drawing on Landes and Posner (1975) and Ramseyer (1994), Hanssen argues both theoretically and empirically that state legislatures will eliminate retention elections and effectively strengthen judicial independence in periods when within-state political competition is sufficiently strong. In practice, however, retention procedures rarely change, in part because state constitutions often have to be amended or replaced. Moreover, change appears to have been largely driven by the lobbying efforts of outside actors such as the American Bar Association and the League of Women Voters and not by the legislatures themselves.

In contrast to changes in judicial retention, legislatures determine judicial budgets annually.⁴ The judicial budget is much less closely scrutinized than other more salient issues such as the education budget or the passage of high-profile laws. We argue that the way in which state legislatures set judicial budgets after the removal of elections is, in fact, indicative of preferences within state legislatures for a more or less independent judiciary.

The rest of this paper is organized as follows. In the next section, we provide background information on legal origins in the American states and on the role of judges in common-law and civil-law legal systems. In section 3, we build a model of how common-law and civil-law legislatures set budgets. In section 4, we take the model's predictions to the data. In section 5, we examine the likely mechanism through which civil-law norms may have persisted. In section 6, we conclude.

⁴ In effect, we are arguing that judicial selection and retention procedures are slow moving, while judicial budgeting is relatively fast moving. For more on fast-moving and slow-moving institutions, see Roland (2004)..

2. Initial Legal Conditions

In the next section, we present a model in which differences in state legislatures' preferences regarding the independence of the judiciary lead to differences in outcomes. In this section we lay the foundation for the model by presenting evidence to justify two key assumptions in the model. The first is that American states had different legal initial conditions, namely whether they were initially settled by a common-law or a civil-law country. The second is that civil-law and common-law legal systems differ in their views regarding the appropriate degree of judicial independence. We present evidence that common-law legal systems are designed to have, and empirically have, more independent judges than civil-law systems. We show that this is consistent with how judges in civil and common-law states within the United States have been selected.

Common and Civil-law Legal Systems in the United States

From the time of Christopher Columbus's arrival in North America in 1492, European powers vied for footholds. By the end of the seventeenth century, England had acquired control of the Dutch and Swedish settlements in the mid-Atlantic, consolidating their control of a large stretch of the Atlantic seaboard. The eighteenth century was marked by British conflict with the Spanish to the South and the French to the North and West of the British colonies. By the late eighteenth century, the French were largely absent from North America. And the Spanish were located largely in Florida, parts of the Gulf coast, and territory west of the Mississippi River.

With the War of American Independence and the Treaty of Paris in 1783, the newly founded United States came to control many of the British possessions in North America. In 1803 large amounts of land that had been recently controlled in most cases by both the

French and the Spanish came into United States possession through the Louisiana Purchase. Additional land was added by the purchase of Florida in 1821. In the far West, Russia established short-lived settlements in California at Fort Ross and later in Washington and Oregon. Ongoing American settlement in British-controlled Oregon and Washington and the election of James Polk (an expansionist, whose slogan was "Fifty-four Forty or Fight!") led to the Treaty of Washington in 1846. Conflict with Mexico in Texas and elsewhere led to war and the acquisition of additional territory through the Treaty of Guadalupe Hidalgo in 1848. The final territory in the continental United States was acquired through the Gadsden Purchase in 1853.

Many states had settlements by civil-law countries at some point during their history. When classifying states as civil or common-law, however, we will restrict attention to the states that have evidence of permanent settlement and operation of a civil-law legal system during the eighteenth century. In Berkowitz and Clay (2006), we use population estimates, later census data, land claims, and evidence on the operation of courts to classify states as having originally had a civil or common-law legal system. Based on this evidence, we define thirteen states –Alabama, Arizona, Arkansas, California, Florida, Illinois, Indiana, Louisiana, Michigan, Mississippi, Missouri, New Mexico, and Texas – as civil-law states. The remaining thirty-five states in the continental United States are classified as common-law states.

For our purposes, it is important to note that by the mid-eighteenth century, a legal system based on English common-law was operating in the English colonies: lawyers pled cases before judges, there was trial by jury, judges adjudicated based on precedent and English common-law was influential in substantive laws including real estate,

inheritances, marriage and divorce, separation of Church and State, criminal law and in procedures (Hoffer, 1992).

Similarly civil-law legal systems were operating in the Spanish, French, and Mexican colonies. Civil procedures were in use and parts of the law had been codified. Further, book-length legal histories of Arkansas, California, Florida, Missouri, Louisiana, New Mexico, and Texas, and articles about Alabama and Mississippi indicate that colonial judges served that same basic function as they did in the home country. In Illinois, Indiana, and Michigan, records from the village assemblies, which governed many aspects of village life, and records of disputes that made it to New Orleans, which was the administrative center for France and later Spain, suggest there was something approaching a formal judicial system in these three states.⁵

Civil and Common-law Attitudes towards Courts

As noted by Merryman (1985), judges play very different roles in common and civil-law legal systems. In civil-law legal systems, the judiciary is considered to be primarily an enforcement arm of the state, whereas in common-law legal systems the judiciary protects citizens from the state. In particular, common-law judges check the power of the executive and legislative branches of the state. In civil-law legal systems judges primarily interpret existing statutes, while in common-law legal systems judges can create law within the system of precedents. Thus, if civil-law and common-law attitudes towards the judiciary persist, we should observe a less independent judiciary in civil-law states.

La Porta et al (2004) provide detailed cross country evidence that judges are less independent in civil-law countries than in common-law countries for two reasons. First, they find that the tenure of Supreme Court judges and administrative judges is longer in

⁵ On French Illinois, see Ekberg (1998) and Briggs (1990).

English common-law countries than in French civil-law countries. Judges with relatively long tenures are less vulnerable to political pressure. Second, La Porta et al (2004) provide evidence that judicial decisions are more likely to become a source of law in English common-law countries than in French civil-law countries. Judges who are bound by judicial precedent are less likely to be influenced by the executive and legislative branches when ruling in particular cases. Mahoney (2001) argues that high-level common-law judges tend to be more effective in checking the executive branch, because they have a strong exit option. The typical high-level common-law judge was an independent and powerful lawyer before becoming a judge, while the typical high-level civil-law judge spent his or her prior career advancing through the civil service.

In related work (Berkowitz and Clay 2006), we document that state judges in the American civil-law states are less independent than state judges in common-law states at the end of the twentieth century. Of the methods of judicial retention, partisan elections now are widely considered to give officials in the state legislative and executive branches the most leverage over judges. Controlling for membership in the Confederacy, civil-law states were more likely to retain their high-court state judges through partisan elections. Further, civil-law states provide smaller budgets to their judges, remove their judges more frequently, and amend and replace their constitutions more frequently.

Because partisan retention elections are considered to be one of the most significant impediments to judicial independence, it is useful to review how common and civil-law states have employed this procedure throughout the twentieth century.⁶ Partisan elections were originally widely adopted in the nineteenth century in response to the perception

⁶ We focus on retention because this appears to have a stronger influence than selection on how judges rule: for example, see Berkowitz, Bonneau and Clay (2006) and Besley and Payne (2003). However, most states have similar retention and selection procedures.

that state legislatures had too much – and common people had too little – control over the judiciary. Beginning in the early twentieth century, states began to move away from partisan elections as their negative effects became apparent. Figure 1 illustrates that during 1910-2000 partisan retention elections for court of last resort judges have been replaced by other methods. During 1910-1959, 20 states eliminated partisan retention elections. During 1960-2000, 15 more states followed. Moreover, controlling for membership in the Confederacy, civil-law states were much slower to remove partisan retention elections than common-law states.⁷ That is, Civil-North states were slower to remove partisan retention elections than Common-North states, and Civil-South states were slower to remove partisan retention elections than Common-South states.

Why have civil-law states been slower to remove partisan retention elections?

Hanssen (2004b) argues that retention elections were removed during periods of intense within-state political competition. His interpretation is that the dominant political party chose to commit to an independent judiciary to preserve their legislative policies when faced with the real possibility of losing control of the state legislature. If this is correct, civil-law states should have had lower levels of political competition.

In fact, civil-law states have had lower levels of political competition. To measure political competition, we use a version of the Ranney index that captures the extent to which either the Democratic or Republican Party dominates the upper and lower houses of the state legislatures.⁸ This Ranney index is computed as the minus the product of the

⁷ The data on dates of changes are drawn from Hanssen (2004a) and individual state constitutions. The latter are available at <http://www.stateconstitutions.umd.edu/index.aspx>. The way in which states select their high-court judges tends to be indicative of how they select their lower courts judges: see Berkowitz, Bonneau and Clay (2006) for evidence on trial judges and Hanssen (2004a) for evidence on appellate judges.

⁸ The measure is computed as the minus the product of the absolute values of the share of Democrats in the upper house minus 0.5 times the share of Democrats in the lower house. See Besley and Case (2003). This

absolute values of the share of Democrats in the upper house minus 0.5 times the share of Democrats in the lower house. If, for example, the Democrats held 90 percent of the seats in the upper and lower houses, the Ranney index then equals:

$-abs(0.9 - 0.5) * (0.9 - 0.5) = -0.16$. The Ranney index can vary from -0.25 to 0 and higher values imply more competition. During 1953-2000 the Ranney index was -0.080 in civil-law states and -0.041 in common-law states. We reject the null that this difference is zero at the 1-percent level.

Another explanation is that legislatures in civil-law and common-law states have different preferences regarding judicial independence. Specifically, if the civil-law preferences for a weak judiciary were preserved long after common-law replaced civil law in the American states, then we would expect legislatures in civil-law states would be more likely to withstand attempts to move from elections to appointment than legislatures in common-law states.⁹ Figure 1 is consistent with this interpretation.

We can reconcile these two views using a simple econometric model. Let judicial independence equal 0 when there are partisan elections, and equal 1 otherwise. We estimate the probability of judicial independence in state s ($\text{prob}(\text{ref}_s(t) = 1)$) in year t using a standard probit model.

Columns (1) and (2) in Table 1 verify Hanssen's theory: political competition is positively associated with judicial independence at the 1-percent level controlling for national time effects. When we also control for population and real per capita income (2000 is the base year), the coefficient on political competition remains positive. It

measure is highly correlated during 1970-90 with the traditional Ranney index that includes the state executive branch.

⁹ Again, the exception is Louisiana. However, Fernandez (2001) argues that shortly after Louisiana entered the Union judges in Louisiana had the power to rule based on precedents and to create precedents.

shrinks, however, to roughly one-fifth its original size, and its significance falls to the 10-percent level.

In columns (3) and (4) we also test for whether political competition has the same association with judicial independence in civil-law and common-law states. We include political competition interacted with civil-law as an explanatory variable and test the null that this interaction variable is insignificant. In specification (3) we control for national time effects and differential time effects in civil-law states. We reject that null at the 1-percent level. In specification (4) we also control for real per capita income and population again allowing for differential effects in civil-law states. We reject the null at the 5-percent level.

Thus, common and civil-law state legislatures behave differently in the sense that they change judicial selection and retention procedures under different conditions. Common-law legislatures do not need high levels of political competition to remove elections. Civil-law legislatures, on the other hand, tend to eliminate elections in periods when political competition is intense. There is evidence that agitation for changes in judicial selection and retention almost invariably come from parties outside the state legislature, including the American Bar Association, the League of Women Voters and informed citizens (see American Bar Association, 2003; Becker and Reddick, 2003; and Hanssen, 2004a). Thus, it is likely that these groups are successful in civil-law states precisely when the dominant political party is vulnerable. In contrast, these groups are successful in common-law states, even in periods when the level of political competition is not unusually high. This is consistent with common-law legislatures preferring a more independent judiciary.

The theory that we develop in the next section posits that state legislatures differ in their preferences for independence because of their legal initial conditions. Because there are many actors involved in setting judicial selection and retention policy, in any year state legislatures take retention policy as given. The way in which legislatures set budgets provides an empirical test for the whether common-law and civil-law state legislatures differ in their preferences.

3. The Model

In this section we model a three-stage game in which common and civil-law legislatures differ in their preferences regarding judicial independence. Legislatures first choose a judicial appointment system, then legislatures choose judicial budgets and then judges make rulings. In practice, the first period is plausibly exogenous since, as we have previously argued, it is hard for legislatures to change retention procedures.

We extend the model developed by Maskin and Tirole (2004). There are two periods, denoted $t = 1$ and 2 and, in each period, a judge can take one of two possible actions (rulings), which we denote $r = a, b$. These actions, while carrying the same labels for notational convenience, are almost certainly different in the two periods.

In each period, there is a socially efficient ruling, which may be a or b . The probability that the socially efficient ruling is the popular ruling a is $p > 0.5$. The value of p is common knowledge among the electorate. Moreover, it is what the electorate would choose if they were collectively the judge. Therefore, the ruling a is always the most popular ruling. If b is the socially efficient ruling and the judge rules for b , the

ruling will be unpopular. The variable p is exogenous and is a proxy for how well informed the populace is about a particular legal issue.

Let $c \geq 0$ denote the costs that a judge needs to incur to find out whether ruling a or ruling b is efficient in any period. In the model, c is exogenously determined, but judges will make endogenous decisions about whether to become informed. If the judge incurs costs c , we will call her ruling informed. Loosely speaking, c can be thought of as costs associated with research inputs, such as the number and quality of judicial clerks and research related staffs, any costs of accessing research related materials, and any other support costs such as computers related to judicial clerks and research related staffs. These resources exceed the normal level of resources supplied by the legislature to cover salaries and capital budgets.¹⁰ Further, judges do not fund these resources with their own money or with money raised from political groups. Another way of thinking about c is as the additional resources judges need to make a careful decision. Without these resources judges will remain uninformed.

The probability that a judge is congruent, that is, prefers that efficient ruling when she is fully informed is π . The selection process is better than random: $\pi > 0.5$. The probability that a judge is incongruent is $1-\pi$. Thus, π is proxy for the quality of the judicial selection process.

In Table 2 panel A, we list how a judge's preferred ruling depends both on whether or not she is informed and on the state of nature. The first column contains the judge's preferred choices when she is fully informed. The second column contains the corresponding probabilities of these outcomes. The third column contains the judge's

¹⁰ Because the transaction costs for the legislative and judicial branches to negotiate this additional budget on a case by case basis are high, we assume that Coasian bargains cannot be made.

preferred choices when she is uninformed and the fourth column contains corresponding probability of these outcomes.

An informed judge prefers ruling a either because a is efficient and the judge is congruent, or because ruling b is efficient and the judge is incongruent. Similarly, an informed judge prefers ruling b , either because she is incongruent and a is efficient, or because she is congruent and b is efficient. If she is congruent and thus public opinion is her best source of information, an uninformed judge prefers ruling a . If she is incongruent, an uninformed judge prefers ruling b .

The legislature sets the judicial budget. Legislatures want to promote social welfare. Common-law legislatures, however, prefer a more independent judiciary, while civil-law legislatures prefer a less independent judiciary. Moreover, the median citizen in a particular state s varies in their willingness to pay for an efficient outcome, denoted $v^s > 0$, and this variation is driven by state-level fundamentals such as income, education, religion, etc. The willingness of the median citizen to pay for the inefficient outcome in each state is normalized at 0.

To capture the influence of political culture in common and civil-law state legislatures, we assume that when an appointed judge makes a socially efficient ruling, the common-law legislatures receive payoff, $\gamma^{common} v^s > v^s > 0$, because this confirms their belief in the inherent value of an independent judiciary. And, when an appointed judge makes an efficient ruling, the civil-law legislature receive payoff $0 < \gamma^{civil} v^s < v^s$, because this undermines their beliefs. Thus, $\gamma^{common} > 1 > \gamma^{civil} > 0$. Both types of legislatures receive a payoff of 0 after an inefficient ruling is made.

The legislatures are risk neutral: when judges are appointed, in state s a legislature

$L = \{\text{com}, \text{civ}\}$ receives a payoff of $2v^s\gamma^L$, $v^s\gamma^L$ and 0 when there are two, one and zero efficient rulings over two periods. And, when judges are elected, legislatures receive payoffs of 2, 1 and 0 when there are two, one and no efficient rulings over the two periods.

Appointed Judges

Following Maskin and Tirole (2004), let $G > 0$ and 0 denote the judge's payoff from making her preferred and least preferred rulings in the first period. The judge makes her most preferred ruling in the second period. $X = \delta(G + R)$ denotes her payoffs, $\delta > 0$ is the discount factor and $R \geq 0$ are rents associated with holding office.

Because an appointed judge can remain in office both periods, in each period she decides either to spend c and make an informed decision, or to not spend c and make the popular ruling, a . An informed judge receives payoffs $G - c$ and $X - \delta c$, in periods 1 and 2, and a uninformed (populist) judge receives payoffs pG and $X - \delta(1-p)G$. In each period, a judge makes an informed ruling when c is sufficiently low.

Proposition 1: An appointed judge is informed if and only if $J^{app} > c$, where

$$J^{app} \equiv G(1-p)$$

The legislature receives payoffs of $\gamma^L\pi v^s - c$, if the judge makes an informed ruling, or $\gamma^L p v^s$, if the judge makes an uninformed ruling. A legislature L is willing to make additional payments to their judges to be informed if and only if $L^{app} > c$, where

$$L^{app} \equiv \max[\gamma^L v^s (\pi - p), 0],$$

where $L_{com}^{app}/v^s \geq L_{civ}^{app}/v^s$ since $\gamma^{com} > \gamma^{civ}$.

Proposition 1 has several implications. First, if $\pi \leq p$ so that popular opinion is at least as likely on average as an informed and appointed judge to determine the efficient ruling, both common-law and civil-law legislatures budget for a populist judiciary.

Second, if we assume

$$(A1) \pi > p,$$

then the legislature can always implement either a populist or informed judiciary.¹¹ The results are summarized in the Table 2b. Two things are worth noting. The first thing is that the judicial constraint is not binding on the legislature. If $J^{app} \leq c$, an additional budget equal to $J^{app} + \varepsilon$, where ε is small, coupled with a direct payment of $c - J^{app} - \varepsilon$ to reduce costs also implements an informed judiciary.¹² The second thing is civil-law and common-law legislatures would, under some conditions, prefer different information structures given the same values of c , π , and p . This arises if $v^s \gamma^{common} > c/(\pi - p) > v^s \gamma^{civil}$. In this case, a common-law legislature will provide more total funding to the judiciary than a civil-law legislature in state s .

Third, in principle, the model has cross-sectional implications, but due to differences in the way that budgets are measured and our inability to control for all relevant state-specific variables, we will ignore these predictions. In the next sub-section we will show how the model can be used to generate testable time-series predictions.

¹¹ Assumption (A1) captures Hanssen's (2004a) interpretation of the early twentieth century where it became common knowledge that elected judges were populist and appointed judges were likely to be competent and responsible.

¹² This is the smallest amount of the social surplus that the legislature must pay to implement an informed judiciary. The legislature does not need to share any more of the social surplus since it is essentially writing an ex ante contract for the judges.

Elected Judges

In a system of elections, judges who stand tall, that is, choose their most preferred policy may fail to be re-elected. If the electorate makes inferences about the judges based on rulings made in the first period, then the posterior probability that a judge is congruent after making the popular ruling a is:

$$\frac{\pi p}{\pi p + (1-\pi)(1-p)} > \pi$$

And, if the posterior probability that a judge is congruent after she makes ruling b is:

$$\frac{\pi(1-p)}{\pi(1-p) + (1-\pi)p} < \pi$$

Both of these inequalities for the posterior probabilities hold since $p > 0.5$. This implies that a judge is re-elected if and only if she makes ruling a in period 1.

We denote the critical payoff for elected judges

$$J^{elec} = \max \left[\frac{(G - X)(1-p)}{1 - \delta(1-p)}, 0 \right]$$

Since $X = \delta(G + R)$, where δ is the discount factor and R denotes rents from holding office in the second period, it is straightforward to show that an appointed judge is more willing to become informed:

$$0 \leq J^{elec} < J^{app}.$$

Proposition 2: If $0 \leq c < J^{elec}$, then a judge stands tall and makes her preferred ruling in period 1, and, if re-elected, makes her preferred ruling in period 2;

if $J^{elec} \leq c < J^{app}$, then a judge is a partial populist: she makes ruling a in period 1 to retain office, and then makes her preferred ruling in period 2;

if $J^{app} \leq c$, then a judge is a populist and chooses the popular ruling always.

Proof: See the appendix.

The payoffs for either legislature in some state s when there are stand tall judges, partial populist judges and populist judges are $v^s\pi(1 + \pi + 2p(1-\pi)) - 2c$, $v^s(p + \pi) - c$ and $2v^sp$, respectively, and the critical payoffs are

$$L^{elec} \equiv \text{Max}[\pi(1+\pi)+2p(\pi(1-\pi)) - 1, 0]$$

Proposition 3: A legislature in state s prefers a stand tall judiciary when $c < L^{elec}$, and a populist judiciary when $L^{elec} \leq c$, and never prefers partial populists.

Proof: See the appendix.

Suppose that $L^{elec} > 0$, so that

$$(A2) \quad \pi(1+\pi) - 2p(1-\pi + \pi^2) > 0$$

Then, both legislatures are willing to pay for a stand-tall judiciary if costs are low enough. A legislature sets the budget equal to $c \forall c \in [0, L^{elec}]$, and 0 otherwise.

Our model has time series implications. Let $\Omega(c, \gamma^{com}) = \int_{L_{com}^{elec}}^{L_{com}^{app}} cf(c)dc$ and

$\Omega(c, \gamma^{civ}) = \int_{L_{civ}^{elec}}^{L_{civ}^{app}} cf(c)dc$ denote a legislature's willingness to pay for an appointed

judge and an elected judge when the upper bound of the distribution of c exceed all critical legislative payoffs in each state.

Proposition 4: Suppose that (A1) and (A2) hold so that legislatures are willing to pay for an informed appointed judiciary and a stand-tall elected judiciary when costs are small.

Moreover, suppose that we assume:

(A3) c is uniformly distributed over the interval $[0, C^*]$: $C^* > \max[L^{app}, L^{elec}]$.

Then $\Omega(c, \gamma^i)$ is separable in v and $\Omega(c, \gamma = 1) < 0$ and $\partial\Omega(c, \gamma^i) / \partial\gamma^i$. This implies:

$$(1) \Omega(c, \gamma^{civil}) < 0; \text{ and } \Omega(c, \gamma^{civil}) < \Omega(c, \gamma^{common}).$$

Proof. See the appendix.

Equation (1) predicts that civil-law legislatures cut their budgets following the removal of judicial elections, and common-law legislatures either make smaller cuts or even increase their budgets.

To understand this, first suppose that state legislatures are concerned only with social welfare ($\gamma = 1$). In this case, legislatures will pay more for an elected judiciary that stands tall than an appointed judiciary. While both kinds of judges make the efficient ruling with probability π in the first period, elections enable voters to replace judges whose rulings suggest they are non-congruent (see Maskin and Tirole, 2004, p.1041). Hence, social welfare – and the legislature’s budget allocation – is higher under elections than appointments.

A common-law preference for judicial independence, $\gamma^{common} > 1$, increases the payoff from appointments, and a civil-law preference, $\gamma^{civil} < 1$, lowers the payoff from appointments. Thus, civil-law legislatures cut judicial budgets when elections are removed; common-law legislature make smaller cuts and may even increase budgets if $\gamma^{common} > \gamma^{**} = .5(1 + \pi) + p(1 - \pi) > 1$ (see the Appendix).

Our model can also incorporate the well documented finding that elected judges often make decisions that are popular and not necessarily efficient. For example, there is evidence that elected judges (compared to appointed judges) are lax in enforcing constitutional restrictions on deficit finance and lax in calling hearings for public utility dispute cases. They are pro-labor in employment discrimination cases, and consider public opinion when making rulings in cases involving death penalties (Hall 1995; Bohn and Inman 1996; Hanssen 1999; Besley and Payne 2003).

Suppose that after the elimination of elections, the probability of that a judge is congruent increases to $\pi^{app} = \eta\pi$, where $\eta > 1$. Then, the common-law and civil preferences, adjusted for this gain in judicial quality, become $\eta\gamma^{common} > 1$ and $\eta\gamma^{civil} < \text{or } \geq 1$. This implies that if η is sufficiently large: $\eta\gamma^{civil} > .5(1 + \pi) + p(1 - \pi) > 1$, civil-law legislatures increase budgets when elections are eliminated. Thus, whether or not civil-law legislatures cut budgets is an empirical question. However, the theoretical prediction that civil-law legislatures give less to their courts than common-law legislatures is robust.

4. Empirical Results on State Judicial Budgets

In this section, we test the model's prediction that civil-law legislatures cut their judges' discretionary budget more than common-law legislatures following the elimination of judicial elections. In this section we describe state judicial budgets, we explain how this data can be used to capture discretionary budgets for judges, and then we test our theory.

Budgets for state courts are recorded in the judicial and legal expenditures category of the annual state budget (U.S. Census Bureau, 2001). This variable includes expenditures on all state criminal and civil courts and includes salaries for judges, court reporters, payments for witness fees, payments to legal departments, general counsels, solicitors, and prosecuting and district attorneys. In 1982, this variable began to include payments for legal services and public defense.

We deflate judicial and legal expenditures using 2000 as the base year and divide them by state population. For brevity, deflated judicial and legal expenditures per capita are denoted judicial budgets. Between 1961 and 2000 judicial budgets increased in the average continental state from \$3.53 to \$48.31, which represents an annual average growth rate of almost 7 percent. The dispersion in spending across states has been relatively stable: in 1961 spending on courts in the top ranked state (Vermont at \$13.28) was 14.8 times greater than in the lowest ranked state (Michigan at \$0.90). In 2000 spending in the top ranked state (Connecticut at \$116.69) was roughly 13.7 times greater than in the lowest rank state (Washington at \$3.53). There has been some change in the rank of state spending as the correlation coefficient for the rank of judicial and legal spending in 1961 and 2000 is 0.51.

We use a differences-in-differences framework to test our theoretical prediction that civil-law legislatures cut judicial discretionary budgets more than common-law legislatures following the elimination of elections.¹³ To consistently estimate the differential impact of reform in civil and common-law states, we control for state fixed effects and annual national level time effects:

¹³ We use elections which include partisan and non-partisan retention elections as this matches our model. Our results are qualitatively similar if we use only retention elections. See Tables 1 and 2 in the appendix for a listing of reform in retention elections and partisan retention elections during 1960-2000.

$$(2) \quad \ln JUD_{it} = \alpha_0 + \sigma_i F_i + \beta_1^1 year_t + \beta_1^2 year_t * civil + \gamma_{it}^1 X_{it} + \gamma_{it}^2 civil * X_{it} + \alpha_1 ref_{it} + \alpha_2 ref_{it} * civil + \varepsilon_{it}$$

Controlling for state-fixed effects enables us to net out baseline judicial expenditures that are driven by operating expenditures and employment levels in the state court system and thus enables us to measure something conceptually similar to the legislature's allocation of discretionary resources to its judges.¹⁴

In equation (2), the subscripts i and t denote a particular state and a particular year; $\ln JUD_{it}$ denotes logged judicial budgets and is the dependent variable; F_i is a vector of state fixed effects, $year_t$ is a vector of national level time effects, X_{it} is a vector of time varying state-level observables, ref_{it} dummy variable equal to 0 when judges are elected and 1 when they are appointed and ε_{it} is an error term. The civil dummy is interacted with the variables ref_{it} , $year_t$ and X_{it} to test for the differential impact of civil-law. Our specification effectively considers a separate group of common-law and civil-law state because it allows for differential time effects and differential effects of the time-varying state-level observables. All standard errors are corrected for heteroskedasticity and are clustered at the state-level.

The differences-in-differences estimator is α_2 . We test the hypothesis that $\alpha_2 = 0$ (i.e. $ref_{it} * civil$ has no significant impact): a rejection of the null against the alternative that $\alpha_2 < 0$ is consistent with our theory.

¹⁴ This econometric approach will not work if state judicial budgets follow a unit root. Fortunately, the state-level time series plots are not consistent with unit roots. The typical trajectory starts with an initially flat growth rate followed by a switch to a rapid growth trajectory, where the timing of the switch varies across states. We cannot formally test for a unit root, because we have only 20 observations per state. We thank Roman Liesenfeld for help with this point.

Table 3 reports results from several specifications of equation (6). In specification (1) we control for time effects and state fixed effects. Specification (2) is our baseline: here we control for several plausible state-level time-varying determinants of judicial budgets including logged state population, logged state annual real-per-capita income, whether or not a state has an intermediate appellate court and a reform dummy interacted with a six-year time-trend effect that allows for some acceleration or deceleration in spending for six years after the reform.^{15, 16} In each specification we reject the null at the 1-percent level that $ref_{it} \times civil$ is insignificant. Further, the sign of $ref_{it} \times civil$ is negative in both specifications. This confirms our theoretical prediction that that civil-law legislatures spend less than common-law legislatures on their judges after reform.

Specification (3) is a “horse race” between our theory of civil-law preferences and Hanssen’s theory that political competition drives judicial independence. Since judicial budgets plausibly measure judicial independence and discretion (Glaeser, Scheinkman and Shleifer, 2003) a positive regressor for political competition and a non-negative regressor for political competition interacted with civil-law would be consistent with Hanssen’s theory. Moreover, Hanssen’s theory would dominate if including political competition eliminates the impact of $ref_{it} \times civil$ and ref_{it} . In specification (3) we include political competition by itself and interacted with civil-law in our baseline (2): and, we still find that $ref_{it} \times civil$ remains negative, ref_{it} remains positive, and both are significant at the 1-percent level. Moreover, the political competition variables are insignificant.

¹⁵ Intermediate appellate courts allow state courts of last resorts to control their dockets. Langer (2002) argues that the introduction of intermediate appellate courts is indicative of the level of development of a state court system.

¹⁶ We have also experimented with a 12-year trend and obtained similar results

Another concern is that a state having been a member of the Confederacy is potentially a fundamental determinant of rule of law in the United States (Wahl 1998 and Morris 1996). If this is true, our distinction between common-law and civil-law may be just picking up the persistent poor treatment of judges in the South. To deal with this concern, in specification (4) we add to our baseline specification the South dummy interacted with ref_{it} , all of the covariates and the national level time effects. Our theory of civil-law norms still holds, since $ref_{it} \times civil$ remains significantly negative at the 1-percent level. Moreover, it is striking that the differential effect of reform in the South, $ref_{it} \times South$, is positive and statistically significant at the 1-percent level. This suggests the influence of slavery is very different than the influence of civil-law origins.

Because the baseline specification (2) contains the most controls, it is arguably represents the most plausible estimates of the differential impact of civil-law. In this case, civil-law states cut their budgets 37 percent more than common-law states following reform. Common-law states increase budgets roughly 19 percent and civil-law state cut them roughly 18 percent.¹⁷ These seemingly large point estimates are reasonable considering the judicial budgets (deflated in 2000 dollars and in per capita terms) increased almost 13-fold during 1961-99. During this period, an average state in an average year spent \$19 per capita on its courts. The sample standard deviation is large (\$19.20) and spending ranges from \$0.90 in Mississippi in 1963 to \$109.56 in Delaware in 1999. If we adjust for this variation, then the average common-law state increases its budget by roughly \$3.61 and the average civil-law state decreases its budget by \$3.42, which is 0.19 and 0.18 of the sample standard deviation.

¹⁷ The stand-alone estimator of the impact of judicial reform relates to common-law; the sum of the stand-alone and interacted estimators relates to the civil-law, which is significant at the 1-percent level.

Because we use 20 years of data during 1961-99, we must be cautious about drawing inferences for two reasons. The dependent variable may be serially correlated, and the judicial reform dummy is repeatedly either 0 or 1.¹⁸ Thus, the estimated standard errors can be too small so that we could be over-rejecting the null (see Bertrand, Dufflo and Mullainathan, 2004). To address this concern, we use only five years of data (1961, 1971, 1981, 1991 and 1999). Because our sample size is reduced by 75 percent, we lose statistical power. Nevertheless, in results reported at the bottom of Table 3 all of the estimates of the differential effect of civil-law are negative. In specification (1) where we exclude all time-varying state-level covariates except judicial reform, the estimator is not statistically significant. In the baseline (2), we reject the null at the 10-percent level. In specifications (3) and (4) where we include political competition and the South, we reject the null at the 5-percent level.

In Table 4 we use the relative judicial budgets (the ratio of the judicial and state legislative budgets) as an alternative measure of judicial discretion. Here the argument is that when legislatures increase judicial budgets relative to legislative budgets, this picks up additional discretionary budget. The average relative judicial budget grew from 2.2 in 1961 to 6.0 in 1999. We use specifications (1)-(4) from Table 3.¹⁹ In specification (1) $ref_{it} \times civil$ is significant and negative at the 10-percent level. In specifications (2)-(4), it is significant and negative at the 1-percent level. Moreover, when we cut the sample size by 75 percent, $ref_{it} \times civil$ is always negative. Further, it is significant at the 10-percent-level in specifications (2)-(4). When we check for the power of our theory against the political competition theory in specification (3), political competition is never significant. And,

¹⁸ It is hard to either confirm or reject serial correlation as there are only 20 observations per state.

¹⁹ These specifications are sensible since the state-level trajectories do not follow a unit root.

when we check for the power of the civil-law theory against the South in specification (4), $ref_{it} \times south$ is statistically significant at the 1-percent level and has the opposite sign of $ref_{it} \times civil$.

The point estimates in the baseline specification (2) are striking: following reform civil-law states cut the relative judicial budget by 51 percentage points more than common-law states. In common and civil-law states, there was a 30 percent gain and a 22 percent loss in the relative budget, respectively. Because the relative judicial budget on average is 3.3 with a sample standard deviation of 2.8, the average common-law increased it by 34 percent of a sample standard deviation, and the average civil-law state decreased it by 26 percent.

5. Persistence and State Politics

In the previous section, we presented evidence that civil-law legislatures cut budgets more than common-law legislatures when judicial retention were changed from elections to appointment. This is consistent with civil and common-law state legislatures having different preferences regarding judicial independence. This raises the question: How would such preferences have persisted? In this section, we document that both political competition and legislative professionalism are quite persistent within state legislatures. Since political competition and professionalism are both indicative of a persistent culture within state legislatures, state level political culture is a plausible channel for transmission of civil and common-law norms. Moreover, our previous discussion in section 2 (see Table 1) about the differential response of civil and common-law

legislatures to political competition provides additional evidence of this persistent culture with state legislatures.

Clay (2006) argues that climate at time of colonial settlement was related to the development of political competition in state institutions in the antebellum period.²⁰ The idea is that states that had a rainy and warm climate, limited flooding and drought and deep soil were highly suited to farming, and the rich farmers used political institutions to promote their narrow interests.²¹ States that were colder and had less rain, more droughts, and more flooding and shallower soil were less well suited for farming. Their economic elites were drawn from more diverse occupations and so had less ability to use political institutions to promote their narrow interests. Thus, if political competition is persistent, we would expect it to be correlated with climate. Clay (2006) also notes that legal origin, initial transport and the culture of the settlers may have also plausibly influenced the state politics.

The top panel in Table 5 reports how political competition in 1910, 1930, 1950, 1970 and 1990 is influenced by initial conditions.²² We find that climate is always negatively associated with political competition at the 5-percent level. In the bottom panel political competition in 1910, 1930, 1950, 1970 and 1990 is regressed on lagged political competition and the full set of initial conditions. Lagged political competition is positively associated with political competition at the 1-percent level in each

²⁰ Ideally, political competition during the colonial period would be documented. However, sufficient data is only available as far back as the antebellum period.

²¹ Our climate measure is weighted average of five key variables: $\text{climate} = 0.8445 * \text{std}(\text{temperature}) + 0.8232 * \text{std}(\text{precipitation}) - 0.8172 * \text{std}(\text{flood frequency}) + 0.8262 * \text{std}(\text{depth of soil}) - 0.5880 * (\text{months of drought per decade})$, where all of the explanatory variables are converted to have mean 0 and variance 1 and std denotes the standard deviation for a particular state. See Clay (2006) for the details.

²² In order to avoid measurement errors, political competition is averaged over ten year periods. So, political competition in 1990 is the Ranney index averaged during 1980-90.

specification and climate is negatively associated at the 5-percent level in two of the specifications. Thus, there is strong evidence of persistence in the twentieth century.

We use the measures of professionalism in state legislatures developed by Squire (2006a, 2006b) for additional evidence of persistence. The Squire index compares the average pay, average staff size and average number of days in sessions of a member of a state legislative body with his/her counterpart in the United States Congress. “In essence, the measure shows how closely a legislature approximates these characteristics of Congress on a scale where 1.0 represents perfect resemblance and 0.0 represents no resemblance” (Squire 2006b, p.4). When the Squire index is close to 0.0, members of state legislators have relatively low and small staffs and they meet relatively infrequently. This can be associated with a culture where legislators are pressed to find alternative income sources and where they are poorly informed about technical aspects of issues.

The top panel of Table 6 reports conditional associations between the Squire index during 1935, 1945, 1960, 1979, 1996 and 2003 and initial conditions.²³ The evidence here is somewhat weaker: traditional culture (Elazar T) is negatively associated with the Squire index in all specifications. It is significant at the 10-percent level in three. Transport is positively associated with the Squire index and significant at the 10-percent level in only three. The evidence for persistence is stronger when we control for the lagged Squire index in the bottom panel. The lagged Squire index is positive and statistically significant at the 1-percent level in each reported year except for 1935. Other variables associated with initial conditions are no longer consistently statistically significant. Further, the lagged Squire index dramatically increases the fit of each

²³ The index is available during various years between 1910 and 2003. Because we use lags to test for persistence, we start with explaining political professionalism in 1935 which is the second earliest year for which the index is available.

regression: for example, the R-square without and with lagging is 0.281 and 0.764 in 1945, and 0.243 and 0.910 in 2003. Thus, professionalism in state legislatures was persistent, at least during 1935-2003.

6. Conclusion

There is a growing body of evidence that initial conditions such as climate, latitude, early disease environment, legal family and the process of legal transplantation have a persistent and long term effect on the quality of institutions around the world. Just how these initial conditions have left their imprint after many decades and even centuries is difficult to document and, therefore, challenging to explain. In this paper, we build on the finding that the establishment of civil-law systems by French, Mexican and Spanish explorers in North America during the seventeenth, eighteenth and nineteenth centuries has left its imprint on courts in the American states at the turn of the twenty-first century (Berkowitz and Clay, 2005 and 2006). States that have civil-law origins have less independent judges and lower quality courts than states with common-law origins. This pattern is consistent with the treatment of judges in civil-law and common-law countries around the world (La Porta et al, 2004 and Mahoney, 2001). Our major finding is that when setting the judicial budget, civil and common-law state legislatures behave in ways that are consistent with their having different preferences regarding judicial independence. We have also documented that the political culture in state legislatures has been slow-moving during much of the twentieth century, so that is plausible to posit stable state-legislative preferences.

It is clear from many studies including North (1990) that courts run by an independent judiciary are one of the central economic institutions that protect property rights and enforce contracts. Our work shows that initial legal families have had a persistent and long term influence on courts in the United States. It also identifies channels through which initial condition influence contemporary institutions. By better understanding these channels, policy makers can then design policies that overcome the negative influence of initial conditions.

Figure 1: Partisan Retention Elections

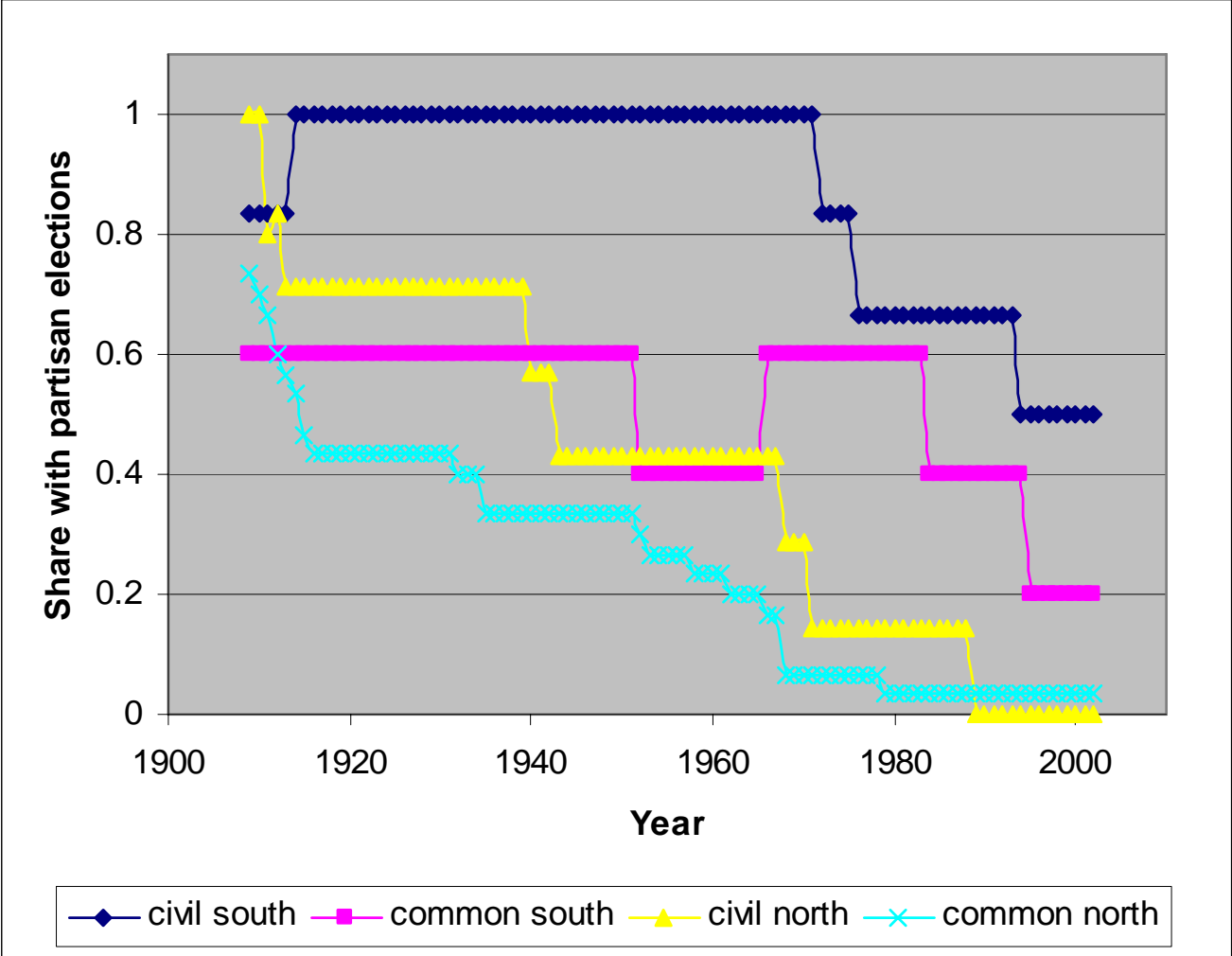


Table 1: Dependent Variable is a Judicial Independence, 1953-2000
 (1 = no partisan elections, 0 = partisan elections)

Specification	(1)	(2)	(3)	(4)
Ranney index	8.89*** (0.704)	1.83* (1.06)	5.30 (1.02)	-2.14 (1.52)
Ranney x Civil			7.36*** (1.78)	6.01** (2.73)
Log likelihood	-442.04	-337.38	-402.71	-300.33
Pseudo R ²	0.213	0.399	0.283	0.465
Civil-law	No	No	Yes	Yes
National time effects	Yes	Yes	Yes	Yes
National time effects x Civil-law	No	No	Yes	Yes
Real per capita income and population	No	Yes	No	Yes
Both real per capita income and population x Civil-law	No	No	Yes	Yes

Notes: Estimates are based on probit regressions for all odd years. In the 1950s we have 1953 and 1957 only; then we report the 20 odd years from 1991 through 1999. The estimate of the constant is not reported. The notation ***, ** and * denotes significance at the 1 percent, 5 percent and 10 percent levels. There are always 1025 observations: we drop Nebraska because it has one legislative house so we cannot measure political competition there and there are nine years when political competition is not reported in Minnesota. Since we have 22 years, 47 states and nine missing observations, then $47 \times 22 - 9 = 1,025$.

Table 2A: Judicial Rulings, Information and States of Nature

Informed Judge's preferred choice	Probability of this outcome	Uninformed Judge's preferred choice	Probability of this outcome
a is efficient; judge prefers a	$P \pi$	It is not known with certainty what is efficient; judge prefers a	Π
b is efficient; judge prefer a	$(1-p)(1-\pi)$		
a is efficient; judge prefers b	$P (1 - \pi)$	It is not known with certainty what is efficient; judge prefers b	$(1- \pi)$
b is efficient; judge prefers b	$(1-p) \pi$		

Table 2B: Legislatures and Implementation of Judicial Policy

	$L_{civ}^{app} \leq c$	$L_{civ}^{app} > c$	$L_{com}^{app} \leq c$	$L_{com}^{app} > c$
$J^{app} > c$	Populist	Informed	Populist	Informed
$J^{app} \leq c$	Populist	Informed	Populist	Informed

Table 3: Judicial Budgets and Judicial Reform

Specification	(1)	(2)	(3)	(4)
	Dependent variable is log judicial and legal expenditures, for every odd year during 1961-99			
<i>Judicial reform dummy</i>	0.214*** (0.058)	0.189*** (0.059)	0.191*** (0.061)	0.156** (0.070)
<i>Judicial reform x Civil</i>	-0.199** (0.088)	-0.369*** (0.095)	-0.370*** (0.098)	-0.392*** (0.096)
<i>Political Competition</i>			-0.059 (0.379)	
<i>Political Competition x Civil</i>			0.090 (0.641)	
<i>Judicial reform x South</i>				0.394*** (0.120)
<u>Controls</u> ; state fixed effects; national time effects, national time effects x civil	Yes	Yes	Yes	Yes
<u>Additional Controls</u> ; real income, population, intermediate appellate courts, 6 year time trend (and interacted with civil)	No	Yes	Yes	Yes
<u>Variables interacted with the South</u> : real income, population, intermediate app. courts, 6 year trend	No	No	No	Yes
Adjusted R ²	0.894	0.904	0.905	0.904
Observations	960	960	933	960
	Robustness check: Sample limited to 1961, 1971, 1981, 1991 and 1991			
<i>Judicial reform x Civil</i>	-0.177 (0.199)	-0.390* (0.215)	-0.464** (0.219)	-0.481** (0.216)
<i>Political Competition</i>			0.310 (0.751)	
<i>Political Competition x Civil</i>			-1.40 (1.28)	
<i>Judicial reform x South</i>				0.561** (0.251)

Notes: Standard errors are heteroskedasticity corrected, and ***, ** and * denotes significance at the 1-percent, 5-percent and 10-percent levels. In 1961 eleven of the thirteen civil-law states and twenty-three of the common-law states had retention elections. By 1999, five of the civil-law states and ten of the common-law states had eliminated elections. Moreover, Tennessee (a common-law state) had elections appointments in 1961, reinstated elections in 1966 and removed them again in 1995. We obtain qualitatively similar results when we use partisan retention elections.

Table 4: Relative Judicial Budgets and Judicial Reform

Specification	(1)	(2)	(3)	(4)
	Dependent variable is log judicial power, for every odd year during 1961-99			
<i>Judicial reform dummy</i>	0.278*** (0.083)	0.293*** (0.081)	0.295*** (0.081)	0.209** (0.085)
<i>Judicial reform x Civil</i>	-0.205* (0.121)	-0.510*** (0.121)	-0.535*** (0.120)	-0.528*** (0.118)
<i>Political Competition</i>	0.073 (0.089)		-0.664 (0.619)	
<i>Political Competition x Civil</i>			-0.317 (0.844)	
<i>Judicial reform x South</i>				0.624*** (0.181)
<u>Controls</u> ; state fixed effects; national time effects, national time effects x civil	Yes	Yes	Yes	Yes
<u>Additional Controls</u> ; real income, population, intermediate appellate courts, 6 year time trend (and interacted with civil)	No	Yes	Yes	Yes
<u>Variables interacted with the South</u> : real income, population, intermediate app. courts, 6 year trend	No	No	No	Yes
Adjusted R ²	0.633	0.653	0.660	0.653
Observations	960	960	933	960
	Robustness check: Sample limited to 1961, 1971, 1981, 1991 and 1991			
<i>Judicial reform x Civil</i>	-0.119 (0.260)	-0.473* (0.275)	-0.569** (0.278)	-0.522* (0.275)
<i>Political Competition</i>			-0.136 (1.43)	
<i>Political Competition x Civil</i>			-1.04 (1.88)	
<i>Judicial reform x South</i>				0.932 (0.346)

Table 5: Political Competition

Dependent Variable is the Modified Ranney Index

	1910	1930	1950	1970	1990
With Initial Conditions					
Climate	-0.013** (0.005)	-0.016*** (0.005)	-0.017*** (0.004)	-0.017*** (0.003)	-0.011*** (0.003)
Transport	0.027 (0.003)	0.006 (0.031)	0.062** (0.027)	0.050*** (0.017)	0.022 (0.019)
Civil	-0.024 (0.024)	-0.015 (0.024)	0.003 (0.021)	0.003 (0.016)	0.010 (0.0131)
Elazar M	-0.076*** (0.025)	-0.109*** (0.025)	-0.077*** (0.020)	-0.020* (0.011)	0.000 (0.013)
Elazar T	-0.056* (0.032)	-0.048 (0.036)	-0.096*** (0.032)	-0.064*** (0.022)	-0.020 (0.021)
Obs	48	48	47	46	47
R-squared	0.490	0.457	0.664	0.782	0.485
	1910	1930	1950	1970	1990
With Initial Conditions and Lagged Dependent Variable					
Lagged Ranney Index	0.754*** (-0.132)	0.853*** (0.122)	0.474*** (0.142)	0.336*** (0.112)	0.667*** (0.151)
Climate	-0.002 (0.004)	-0.005 (0.004)	-0.011** (0.004)	-0.011** (0.004)	0.001 (0.004)
Transport	-0.021 (0.023)	-0.016 (0.014)	0.052** (0.022)	0.029* (0.017)	-0.011 (0.015)
Civil	-0.040** (0.016)	0.006 (0.015)	0.015 (0.022)	0.002 (0.015)	0.008 (0.010)
Elazar M	-0.044* (0.025)	-0.045** (0.022)	-0.040* (0.021)	0.004 (0.014)	0.014 (0.010)
Elazar T	-0.054** (0.025)	-0.001 (0.024)	-0.068** (0.031)	-0.032* (0.017)	0.023 (0.020)
Obs	40	48	47	46	46
R-squared	0.709	0.764	0.747	0.831	0.693

Notes: The notation ***, ** and * denotes significance at the 1 percent, 5 percent and 10 percent levels; standard errors are corrected for heteroskedasticity. The modified Ranney index is averaged over ten years, so in 1990, for example, it is the average for each state during 1980 through 1990. The top panel includes initial conditions and the bottom panel also includes the lagged dependent variable, where 1970 is the lag for 1990; 1950 is the lag for 1970; 1930 is the lag for 1950, 1910 is the lag for 1930 and 1890 is the lag for 1910. We do not report estimates for the constant term.

Table 6: Legislative Professionalism

Dependent Variable is Squire's Legislative Professionalism Index

	1935	1945	1960	1979	1996	2003
With Initial Conditions						
Climate	0.001 (0.006)	0.002 (0.003)	0.003 (0.005)	-0.004 (0.007)	-0.012 (0.008)	-0.011* (0.007)
Transport	0.077 (0.047)	0.010 (0.024)	0.071* (0.039)	0.090 (0.054)	0.198*** (0.060)	0.151*** (0.054)
Civil	-0.012 (0.030)	0.011 (0.014)	0.001 (0.029)	0.074 (0.047)	0.095* (0.049)	0.080 (0.051)
Elazar M	-0.052 (0.035)	-0.041* (0.020)	-0.034 (0.036)	-0.006 (0.043)	-0.007 (0.049)	-0.007 (0.051)
Elazar T	-0.068* (0.038)	-0.065*** (0.020)	-0.089*** (0.030)	-0.063 (0.040)	-0.038 (0.037)	-0.031 (0.035)
Obs	35	48	48	48	48	48
R-squared	0.246	0.281	0.287	0.213	0.345	0.243
With Initial Conditions and Lagged Dependent Variable						
Lagged Legislative Profess.	0.028 (0.092)	0.428*** (0.062)	1.29*** (0.228)	0.888*** (0.177)	0.919*** (-0.079)	0.944*** (0.078)
Climate	0.007 (0.007)	0.001 (0.002)	0.001 (0.005)	-0.007 (0.005)	-0.008** (0.004)	0.000 (0.003)
Transport	0.002 (0.048)	-0.023 (0.015)	0.058* (0.032)	0.027 (0.035)	0.115*** (0.030)	-0.036 (0.023)
Civil	-0.002 (0.040)	0.016* (0.009)	-0.013 (0.025)	0.073** (0.029)	0.028 (0.017)	-0.010 (0.019)
Elazar M	-0.005 (0.027)	-0.019* (0.011)	0.019 (0.024)	0.025 (0.029)	0.013 (0.026)	-0.014 (0.019)
Elazar T	-0.045 (0.038)	-0.036** (0.015)	-0.005 (0.032)	0.015 (0.030)	0.020 (0.019)	0.004 (0.015)
Obs	35	48	48	48	48	48
R-squared	0.068	0.764	0.678	0.611	0.812	0.910

Notes: The notation ***, ** and * denotes significance at the 1 percent, 5 percent and 10 percent levels; standard errors are corrected for heteroskedasticity. The top panel includes initial conditions and the bottom panel also includes the lagged dependent variable, where 1996 is the lag for 2003; 1979 is the lag for 1996; 1960 is the lag for 1979; 1945 is the lag for 1960; 1935 is the lag for 1945; and 1910 is the lag for 1934. We do not report estimates for the constant term. Data on legislative professionalism is taken from Squire (2006a, 2006b).

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Appendix

Table 1A: Retention of Court of Last Resort by Elections: 1960-2000
States that eliminated partisan and non-partisan elections

Arizona	1974
Colorado	1966
Florida	1972
Illinois	1971
Indiana	1968
Iowa	1962
Maryland	1976
Nebraska	1962
New Mexico	1989
New York	1979
Oklahoma	1968
Pennsylvania	1968
South Dakota	1981
Tennessee*	1995
Utah	1967
Wyoming	1972

Table 1B: Retention by Partisan Elections: 1960-2000
States that eliminated partisan

Colorado	1966
Florida	1972
Georgia	1984
Illinois	1971
Indiana	1968
Kentucky	1968
Louisiana	1976
Mississippi	1994
New Mexico	1989
New York	1979
Oklahoma	1968
Pennsylvania	1968
South Dakota	1981
Tennessee*	1995

Sources: The Book of the States, various years and Hanssen (2004a)

*Tennessee appoints as of 1960, introduces partisan elections in 1966 and then removes partisan elections in 1995; all other states remove elections once and for all during 1960-2000.

Appendix-Proofs

Proposition 1 Proof: An informed judge receives pay-offs $G - c$ and $X - \delta c$ in periods 1 and 2; and, an uninformed judge receives pay-offs pG and $X - \delta(1-p)G$. Then, in each period the judges are indifferent to being informed and uninformed when $G - c = pG$, and $X - \delta c = X - \delta(1-p)G$, so that $J^{app} \equiv G(1-p) = c$ and an appointed judge is informed $\leftrightarrow J^{app} > c$.

The legislature in state s is indifferent between an informed and uninformed judge when $\gamma^L \pi v^s - c = \gamma^L p v^s$: $\gamma^L v^s (\pi - p) = c$. Since, however, c is non-negative then the legislature is willing to make additional payments for an informed judge $\leftrightarrow L^{app} \equiv \max[\gamma^L v^s (\pi - p), 0] > c$.

Since $L^{app} \geq 0$ and $\gamma^{com} > \gamma^{civ}$, then $L_{com}^{app}/v^s \geq L_{civ}^{app}/v^s$.

Proposition 2 Proof: In the second period, the judge does not worry about re-election and so is informed $\leftrightarrow J^{app} > c$. If $G \geq X \equiv \delta(G + R)$, then a judge is never willing to stand-tall, and will always choose the popular position in the first period.

Suppose $G > X$. In the second period, the judge does not worry about re-election and so is informed $\leftrightarrow J^{app} > c$. Therefore, the judge is uninformed $\leftrightarrow J^{app} \leq c$.

Consider the first period. If the judge is congruent and chooses to stand-tall, then she receives first period pay-off $G - c$ and is re-elected with probability p and the total pay-off from standing tall is $G - c + p(X - \delta c)$. If the judge is a partial populist and makes the uninformed (and popular) ruling in the first period, her total pay-off is

$pG + X - \delta c$. Therefore, a judge is indifferent between standing tall and partial populism

$$\text{when } c = \frac{(G - X)(1-p)}{1 - \gamma(1-p)}.$$

Denoting $J^{elec} = \max\left[\frac{(G - X)(1-p)}{1 - \gamma(1-p)}, 0\right]$, Proposition 2 holds for the congruent

judge. It is trivial to show this argument applies to the non-congruent judge.

Proposition 3 Proof: Let $L(ST)$, $L(PP)$ and $L(POP)$ denote the a legislature's pay-off from a stand-tall, partial populist and populist judge. Note that we draw no distinction between common and civil-law legislatures, because they have the same pay-offs when there are elections (for simplicity, pay-offs are different for civil and common legislatures under appointments).

If a judge stands tall, she makes the socially efficient decision with probability π in the first period and welfare is $\pi - c$. Elected judges rule efficiently in the second period in three possible states of nature. First, with probability πp the first term judge is congruent and is re-elected; second, with probability $\pi(1-p)\pi = \pi^2(1-p)$ the first term judge is congruent, is voted out of office, and is replaced with another congruent judge; and, third, with probability $(1-\pi)p\pi$, the first term judge is incongruent, voted out of office and replaced with a congruent judge. Moreover, in the second term, if an incongruent judge emerges, she will still be informed. Therefore, in the second term welfare is

$\pi(p + (1-p)\pi + (1-\pi)p) - c = \pi(\pi + 2p(1-\pi)) - c$. The legislature's two-period pay-off is:

$$L(ST) = v^s \{ \pi(1 + 2p(1 - \pi)) + \pi \} - 2c$$

Under partial populism, the probability of an efficient ruling in the first period is p (and the judges is uninformed) and in the second period is π , and

$$L(PP) = v^s (\pi + p - c)$$

Under populism, the judge is always uninformed and the probability of an efficient ruling is p in each period, so that

$$L(POP) = v^s (2p)$$

The proof is completed in two steps. Step 1 shows if $L(PP) > L(POP)$, then standing tall dominates: $L(ST) > L(PP) > L(POP)$. Step 2 shows if $L(POP) \geq L(PP)$, then $L(ST)$ dominates when $c < L^{elec}$, and $L(POP)$ dominates when $c \geq L^{elec}$.

Step 1: By simple manipulation:

$$(i) \text{ If } L(PP) - L(POP) > 0 \rightarrow \pi - p - c > 0.$$

Moreover,

$$(ii) L(ST) - L(PP) = 2p\pi - 2p\pi^2 + \pi^2 - p - c$$

Therefore,

$$(iii) \text{sgn } L(ST) - L(PP) = \text{sgn } \{2p\pi - 2p\pi^2 + \pi^2\} - p - c.$$

Note that:

$$(iv) \{2p\pi - 2p\pi^2 + \pi^2\} > \pi,$$

since this can be rewritten :

$$2p\pi(1 - \pi) > \pi(1 - \pi), \text{ which holds since :}$$

$$p > 0.5$$

Therefore, if $L(PP) - L(POP) > 0$ so that $\pi - p - c > 0$, the equation (iv) implies that $\text{sgn } L(ST) - L(PP) > 0$.

Step 2: Suppose $L(POP) \geq L(PP)$. Then,

$$(i) \pi - p - c \leq 0.$$

Therefore,

$$(ii) \text{sgn } L(\text{ST}) - L(\text{POP}) = \text{sgn } 0.5 \{ \pi(1 + \pi) + 2p(\pi(1 - \pi) - 1) \} - c$$

By inspection, $\text{sgn } L(\text{ST}) - L(\text{POP})$ is decreasing in c , and the sgn is 0 when $0.5 \{ \pi(1 + \pi) + 2p(\pi(1 - \pi) - 1) \} = c$.

$$\text{Define } L^{\text{elec}} \equiv \text{Max}[\pi(1 + \pi) + 2p(\pi(1 - \pi)) - 1, 0].$$

It follows that the legislature in state s prefers a stand tall judiciary when $c < L^{\text{elec}}$, and a populist judiciary when $L^{\text{elec}} \leq c$. Moreover, the legislature never implements a partial populist judiciary.

Proposition 4 Proof: To construct $\Omega(c, \gamma^i) = \int_{L_i^{\text{elec}}}^{L_i^{\text{app}}} cf(c)dc$, then we can exploit the

properties of the uniform distribution in assumption (A3):

$$(i) \quad \Omega(c, \gamma^i) = ((v^s)^2 / 2C'')((L^{\text{app}})^2 - (L^{\text{elec}})^2)$$

Plugging in for L^{app} and L^{elec} , and recalling that by (A2) $L^{\text{elec}} > 0$, then

$$(ii) \quad \Omega(c, \gamma^i) = (v^s \pi)^2 (2C'')^{-1} \{ 4(\gamma^i)^2 - (1 + \pi + 2p(1 - \pi))^2 \}$$

By inspection of (ii), $\Omega(c, \gamma^i)$ is separable in v^s and increasing in γ^i . Moreover, when $\gamma^i = 1$ and since $p > 0.5$, then

$$(iii) \quad \text{sgn} \Omega(c, \gamma = 1) = \pi(1 - \pi - 2p(1 - \pi)) = \pi(1 - 2p)(1 - \pi) < 0$$

Since $\gamma^{\text{civil}} < 1$, then $\Omega(c, \gamma^{\text{civil}}) < 0$. Moreover

$$\Omega(c, \gamma^{\text{civil}}) - \Omega(c, \gamma^{\text{civil}}) =$$

$$\Omega(c, \gamma^{\text{civil}}) - \Omega(c, \gamma^{\text{common}}) = 2(v^s \pi)^2 (C'')^{-1} \{ (\gamma^{\text{civil}})^2 - (\gamma^{\text{common}})^2 \} < 0$$

Follow up points to Proposition 4.

Using equation (ii) from the proof of Proposition 4 and recalling that $p > 0.5$, let

(iv) $\gamma^{**} = .5(1 + \pi) + p(1 - \pi) > 1$, where

$$\Omega(c, \gamma^{**}) = 0.$$

Then $\forall \gamma^{common} > \gamma^{**} > 1$, $\Omega(c, \gamma^{common}) > 0$, and

$$\forall 1 < \gamma^{common} < \gamma^{**}, \Omega(c, \gamma^{common}) < 0.$$

Now, suppose that after the elimination of elections, the probability that a judge is congruent increases to $\pi^{app} = \eta\pi$, where $\eta > 1$. It then follows:

$$\Omega(c, \gamma^i, \eta) = (v^s \pi)^2 (2C'')^{-1} \{4(\eta\gamma^i)^2 - (1 + \pi + 2p(1 - \pi))^2\}, \text{ where}$$

$\Omega(c, \gamma^i, \eta)$ is increasing in $\eta\gamma^i$,

$$\Omega(c, \gamma^i, \eta) = 0 \quad \forall (\eta\gamma)^{**} = .5(1 + \pi) + p(1 - \pi) > 1, \text{ and}$$

$$\forall \eta\gamma^i > (\eta\gamma)^{**} > 1, \Omega(c, \gamma^i, \eta) > 0, \text{ and}$$

$$\forall \eta\gamma^i < (\eta\gamma)^{**}, \Omega(c, \gamma^i, \eta) < 0.$$