Judicial Independence, Elections, and Minority Interests

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Abstract

Many scholars and policy makers argue that judicial elections weaken judicial independence. In this paper we develop two tests to check for whether eliminating elections increases the independence of state-supreme court judges in the United States. The first test is that if judges become more independent after elections are eliminated, then they should receive additional state resources. We find that this is the case: state courts received substantial increases in state budget allocations during 1961-1999 after elections were removed, and these budget allocations were associated with increases in judicial salaries, and payrolls, employment levels and current operating expenditures. The second test is that if judges become more independent, then they become more willing to make politically unpopular decisions that protect the interests of minorities. We focus on education, because state judges were involved in ensuring that students with disabilities received special programs in public schools following the passage of the 1975 Individuals with Disabilities in Education Act (IDEA). We find that following the removal of elections, judges protected minority interests of special needs students, and that these protections were concentrated largely in states that had constitutional provisions ensuring that there should be within state uniformity in the quality of public education.

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I. Introduction

At the beginning of the twentieth century most states in the United States of America used elections for selecting and retaining their high level judges. By the end of the century, however, many states had elections with merit systems or appointment systems. Hanssen (2004a) argues that this shift reflects learning by political reformers that the election of judges, while originally intended to promote judicial independence in fact had the exact opposite effect. One reason for this is that judges that must stand for election or run for re-election are captured by political groups that raise money for their campaigns (Becker and Reddick 2003, American Bar Association 2003). Another reason is that judges who are elected have a strong incentive to make populist decisions that will help them get re-elected (Hanssen 2004b, Besley and Payne 2003, Hall 1995). While there is good evidence that elections are bad for judicial independence, it is not entirely obvious that replacing elections with appointment or merit systems in fact promotes judicial independence for three reasons. First, even though judges are no longer elected, they may lack the resources to enforce the law. Second, judges who are no longer elected may still be under pressure to make court rulings that are designed to win favor from the executive or legislative body that oversees his/her appointment. Third, elected judges do not necessarily pander to the electorate for some issues, so that eliminating elections would not always change the nature of their rulings.

In this paper, we propose two simple tests for whether shifting out of elections increases the independence of the judiciary using the case of state-supreme court judges in the United States. The first test is based on the idea that an independent judiciary must also have substantial state resources. While a selection and retention system that does not

rely on elections may be necessary for empowering judges, it is clearly not sufficient for creating a judiciary that is able and willing to make socially efficient laws.[1] Judges who are poorly paid and have meager resources to pay staff and to fund operating expenditures are easily bullied and subverted by members of the business community, high-level government officials and criminals (Glaeser, Scheinkman, and Shleifer 2003). Thus, if shifting out of elections in fact promotes judicial independence, we would expect that this shift is accompanied by substantial and sustained increases in state resources to cover judicial salaries, operating expenses and payrolls for staff working in the judicial and legal sector.

Our second test is based on the idea that an independent judiciary makes rulings that protect minority rights. If a standing judge does not have to worry about getting re-elected, then he/she can make what he/she believes is the correct ruling without worrying about alienating the broad electorate or an important campaign financier. Nevertheless, it is not clear that eliminating elections effectively changes judicial behavior in all cases because some judges only care about their judicial legacy and do not pander to public opinion (Maskin and Tirole 2004). Also, judges who no longer have to worry about standing for re-election still must worry about pandering to the group that over-sees their re-appointment, whether that be the state legislature or the governor. We test this idea by checking whether eliminating elections caused state judges to protect the interests of students with disabilities.

The influence of state judges on state education policy is a useful case study for several reasons. First, education is the largest component of the state budgets, and accounts for roughly 20.1-percent of direct state expenditures during 1961-1999**.** Second,

[1] We are thus referring to common law judges whose decisions set precedents that effectively create laws.

state court judges have been under pressure to ensure that students with disabilities receive programming in public elementary and secondary school following the Individuals with Disabilities Education Act (IDEA) initially passed by Congress in 1975 (and modified as recently as 2004). This law "requires public schools to make available to all eligible children with disabilities a free appropriate public education in the least restrictive environment appropriate to their individual needs" (U.S Department of Justice 2005). While the federal government has provided federal money in support of this act, the money has not been sufficient to cover all the costs of implementing IDEA (National Education Association 2002; Rotherham 2002). Thus, the states have been left to make up the difference. However, educating children with disabilities can be quite costly, and states (and schools) have a financial incentive to try and cut corners in their compliance with IDEA. Whether or not states (and schools) are in compliance is ultimately a judicial question, and state courts have been forced to make rulings on this topic.[2] There is little doubt that students with disabilities are a minority, and this is another area in which state courts have been asked to protect minority rights in terms of education.

Regarding our first test, we find that shifting out of elections is associated with a substantial and sustained increase in state funding to support state courts. Specifically, we find that the elimination of judicial elections for court of last resort judges is associated with an immediate increase of state budget allocations to the judiciary in the range of 15.5% to 17.3% compared to states that continued to elect their court of last resort judges. It is also notable that this budgetary increase is sustained after the reform and is strongly

[2] This is true even though IDEA is a federal law, and thus one might think the cases would go to federal court, as opposed to state court. According to the U.S. Department of Justice (2005), "If parents disagree with the proposed IEP [Individualized Education Programs], they can request a due process hearing and a review from the State educational agency if applicable in that state. They also can appeal the State agency's decision to State or Federal court."

positively associated with spending that promotes the power of the judiciary, including higher salaries to court of last resort judges, the emergence of intermediate appellate courts that enable state court of last resorts to control their docket, increases in full time employees, payrolls and operating expenditures for court activities.

Regarding our second test, we find that the elimination of elections is associated with major increases in the enrollment of student with disabilities in public school elementary and secondary schools. Moreover, these enrollment increases are concentrated in states that have constitutional provisions ensuring state-wide uniformity in the provision of public education. Specifically, after controlling for state fixed effects, year effects and a host of relevant covariates, we find that the elimination of elections is associated with a 21-25% increase in enrollments compared to states that do not reform.

Our paper is related to several literatures. Besley and Payne (2003) and Hanssen (2004a, 2004b) use judicial elections or partisan judicial elections as a measure of judicial independence and Berkowitz and Clay (2005) argue that these measures are related to quality of courts. Our paper provides more statistical justification for their arguments. There has been a great deal of attention paid to just how state court rulings influenced the equity of within-state spending following the Rodriguez ruling in 1973. Evans, Murray and Schwab (1997) and Card and Payne (2003) show that state spending per pupil became more equitable across school districts after state supreme courts ruled against the constitutionality of its state's education financing scheme. Just how state courts have influenced policies for students with disabilities has been ignored and is an important case for several reasons. First, students with disabilities represent a small minority versus students in relatively poor school districts. Depending upon the "political winds" in a

particular state at a particular point in time, judges could strengthen or weaken their popularity by making policies that shift resources to poor school districts. However, providing accommodations to people with disabilities is not a popular political position (e.g., Ward 2004). Finally, Figlio et al (2004) show that that education uniformity provisions in state constitutions are associated with a relatively equitable allocation of state education spending across school districts within states. We show that these provisions are also associated with the better treatment of students with disabilities. We propose a constitutional interpretation of this result, which is that the IDEA legislation coupled with pro-education state constitutional provisions more clearly defined the minority rights of disabled children.

The rest of this paper is organized as follows. The next section presents an overview of the theory and evidence that judges who are elected lack independence. Section III proceeds with the first test of whether removing elections increases judicial independence by presenting estimates of whether removing elections is associated with additional allocation of resources to state courts. Section IV discusses how the IDEA legislation passed by the Congress in 1975 gave state judges substantial responsibility to determine just which students were entitled to individual education programs. Section V contains results of the second test of the relationship between election and judicial independence by presenting estimates of the impact of judicial reform on the treatment of students with disabilities. Section VI contains a conclusion.

II. Judicial Independence and Methods of Retention

One of the most contentious issues surrounding state courts deals with the issue of judicial independence. Definitionally, "judicial independence relies on the idea that judges are not subject to the influence of some other actor(s); they are the authors of their own decisions" (Kornhauser 2002, 48). While it is true that no one method of retention (or selection) truly provides for independence[3], it is the case that appointed systems better insulate judges from reprisals from the public.[4] In state supreme courts, judges are retained in a variety of ways: appointment by the governor, appointment by the legislature, victory in a merit election, victory in a partisan election, and victory in a nonpartisan election. While the precise details of each retention scheme are not relevant here, it is the case that two of the methods of retention subject judges to electoral vulnerability: partisan elections and nonpartisan elections. Indeed, Bonneau (2005) shows that incumbent judges were more likely to be defeated in their bids for election than incumbent members of the U.S. House and Senate. Not only are these judges at risk for electoral defeat, but they are also aware of this fact. Scholars have demonstrated that judges who are up for election are more likely to change their voting behavior to make it more in line with their constituents as opposed to their own personal policy preferences (Hall 1995; Brace and Hall 1997). In contrast, no such evidence has been found with

[3] Here, we look at method of retention as opposed to method of selection. The reason we make this choice is because what is important for our purposes is the manner in which judges are able to retain their offices and not the method by which they initially obtain them. Of course, the relationship between formal method of selection and method of retention is quite strong—only Illinois, New Mexico, and Pennsylvania have differences in our data.

[4] As several studies of the U.S. Supreme Court have shown (Mishler and Sheehan 1993; McGuire and Stimson 2004), the justices are somewhat receptive to public opinion, even though they are appointed for lifetime terms of office. However, what is important here is the *degree* to which judges may be held accountable for their decisions. The more insulated the judge, the more independent she will be, and thus we expect judges who are retained by appointments to be more independent than judges who must face the electorate to retain their jobs.

either appointed schemes or with merit retention (where judges are subject to a vote before the electorate, but they are unopposed and voters are simply asked whether a judge should be retained). Between 1990-2000, only 3 of 177 judges who stood for merit retention were defeated (Bonneau 2004).

Numerous scholars and public officials, including some judges themselves (Glaberson 2000; Davidson 2001; Phillips 2002), have publicly opposed the election of judges. Moreover, calls for reform have permeated the media in states where judges are elected (Bell 2001; Dickerson 2001; Pittsburgh Post-Gazette 2001; Glaberson 2001). Calls for reform, though they have become louder and more widespread, are not new. As early as 1906, the renowned legal scholar Roscoe Pound in his address to the American Bar Association argued that "putting courts into politics, and compelling judges to become politicians in many jurisdictions. . . [has] almost destroyed the traditional respect for the bench" (Pound 1937). The American Bar Association (ABA) was instrumental in the development of merit plans in the 1930s and in their adoption in some states beginning in the 1940s. The ABA is also on record as opposing both partisan and nonpartisan judicial elections.[5]

The existence of variation in judicial selection over time and across states has led to a substantial empirical literature on the effect of judicial selection and retention on outcomes. Simply put, elections have been associated with receptivity to public desires, a perception of impropriety (since the judges must obtain campaign contributions from individuals and businesses who may appear before the court), and an overall erosion of

[5] "BE IT RESOLVED, that the American Bar Association urges state, territorial, and local bar associations in jurisdictions where judges are elected in partisan or non-partisan elections to work for the adoption of merit selection and retention, and to consider means of improving the judicial elective process."." www.abanet.org/govaffairs/judiciary/rappd.html

judicial independence. Moreover, it seems to be *elections themselves* that have these effects—the difference between partisan and nonpartisan does not appear to be at issue (Becker and Reddick 2003).

If it is true that electing judges leads to a loss of independence, then if a state were to change its method of retention from election to some other means (appointed, lifetime tenure, or merit retention) then we should see the behavior of judges change in a way that would indicate that they are free to decide cases in a way free from punishment.[6] Also, if eliminating elections makes the judges more independent, we would also expect these judges to command more resources from the state, other things being equal. Making the judiciary more independent increases the power given to the judiciary, and we would expect that this more powerful judiciary would place more demands (financial and otherwise) on the state government. In sum, then, we have two hypotheses that we will test in this paper:

Judicial Resources Hypothesis: States that switch their method of retaining judges from elections (partisan or nonpartisan) to some other scheme will also have an increase on expenditures for the judiciary.

Independent Decisions Hypothesis: Judges in states that switch their method of retaining their judges are more likely to make decisions that protect minority interests than they were prior to the switch.

[6] Again, it is important to note that we are not arguing that judges are ever *completely* independent. The work on the U.S. Supreme Court, an institution that was specifically designed to promote maximum independence, has shown that even these justices are (at least somewhat) receptive to public opinion. Rather, we are arguing that moving from an elected system to some other system *increases* the independence of judges.

III. The Judicial Resources Hypothesis

This section tests the hypothesis that states that switch their method of retaining judges from elections (partisan or nonpartisan) to some other scheme will also have an increase on expenditures for the judiciary. State level financing for the court system is recorded in the judicial and legal expenditures category of the annual state budget (U.S. Census Bureau, 2001). This contains state expenditures to all state criminal and civil courts and includes salaries for judges, court reporters, payments for witness fees, court activities of sheriff offices (bailiffs and probate activities), and payments to legal departments, general counsels, solicitors, prosecuting and district attorneys, and so forth. As of 1982, this category was expanded from court activities to also include legal services and public defense.

In order to compare judicial and legal expenditures over time, we deflate them using 2000 as the base year. In order to compare judicial and legal expenditures across space, we divide them by state population. For brevity, we denote state deflated judicial and legal expenditures per capita as judicial and legal spending. Between 1961 and 2000 judicial and legal spending increased from $3.53 to $48.31, which represents an annual average growth rate of almost 7%. However, 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 versus 2000 is 0.51.

Table 1 categorizes states into three groups: those that eliminated retention elections (partisan and non-partisan) for their court of last resort judges during 1950-2000; those states that always had elections, and those that never had elections.

Figure 1 compares average judicial and legal spending for the group of states that eliminated elections (denoted reform) with those that never eliminated elections (denoted no-reform). The jump in average spending for both groups in the early 1980s reflects that inclusion of legal services and public defense in the judicial and legal expenditures as of 1982. This figure suggests that reform is associated with higher judicial and legal spending, especially starting in the late 1970s.

Table 2 provides a rough sense of some of the important components of judicial and legal spending. We match the Bureau of Justice Statistics surveys (U.S. Department of Justice, 1996 and 1997-2004) on employment and spending in state courts conducted during 1971-79 and then during 1982-2001 to generate measures of full time employees, payrolls for full time employees and current operating expenditures (all of these figures are deflated and presented in per capita terms; results on part-time employees are available upon request). Starting in the 1950s, states began to introduce intermediate appellate courts, which are to play the role of hearing cases before they go to the court of last resort and help the court of last resort control its docket. Thus, we would expect that intermediate appellate courts would require some additional resources. Finally, we also consider the average salaries for court of last resort judges (these are deflated).

Table 2 reports raw correlation coefficient for judicial and legal spending, court of last resort salaries, whether or not there are intermediate appellate courts, the number of full time employees, full time payrolls and operating expenditures. The first column

shows that full time employees and operating expenditures have very high correlation coefficients of 0.92 and 0.93 with respect to judicial and legal spending. Full time employees also have a relatively high correlation coefficient of 0.65, while the existence of intermediate appellate courts and court of last resort salaries have low correlation coefficients of 0.14 and 0.10. Thus, it would appear that employment, payrolls and operating expenditures account for a large share of judicial and legal spending.

Figures 2-6 then compares average salaries for courts of last resort judges, average share of sates with intermediate appellate courts, average employment, average payrolls and average operating expenditures for the group of reform and no-reform states. What is striking is that the higher spending in the reform vs. no-reform states is most pronounced in judicial and legal employment, judicial and legal payrolls and operating expenditures, and these components are most highly correlated with judicial and legal spending.

In order to obtain more precise estimate of just how reform affects judicial and legal spending, we employ regression analysis and estimate the following model:

$$\ln J\,\&\,L_{st} = \alpha_s + \gamma_t + \delta REF_{st} + \varepsilon_{st} \qquad (1)$$

This is a fixed effects model, where **s** denotes the state, **t** = 1961, 1963, 1965,…1999 denotes an odd numbered year, $\mathbf{\ln J\&L_{st}}$ denotes the log of judicial and legal spending in state s in year t, $\boldsymbol{\alpha_s}$ is the state fixed effect, $\boldsymbol{\gamma_t}$ is a time effect, and $\boldsymbol{\varepsilon_{st}}$ is a stochastic error term. The term $\mathbf{REF_{st}}$ is the post-passage dummy and equals 0 in state s in years when there are elections, and equals one when elections have been eliminated. Thus, the

coefficient **δ** (post-passage dummy) measures the impact of reform on the percentage change in judicial and legal spending.

There are several caveats with using this model to estimate the influence of reform on judicial and legal spending. First, if we want to compute the impact of eliminating elections on judicial and legal spending, then this reform must be a random treatment, i.e., an exogenous event, rather than being part of a system that is both influenced by the balance of power between the state judiciary and state legislature, state political culture, state income, and then influences judicial and legal spending. However, it does not matter for our purposes whether reforms were designed to also increase spending on the judiciary or whether a judiciary that did not have to worry about being retained in elections could more effectively persuade its legislature to increase spending on the judiciary. What is important is to check if there is a strong association between reform and subsequent spending, so we do not need to develop a strategy to correct for the potential endogeneity of reform. A second problem, is that while the specification in (1) accounts for how fixed state factors (for example, climate and initial conditions) and national effects such as business cycles influence judicial and legal spending, there may be factors that vary across both states and time such state level income and population dynamics that can influence judicial and legal spending. Thus, we need to account for some of these potentially relevant omitted variables. Third, the impact of reform may vary over time, for example, it may have an initially strong effect on judicial and legal spending that either dissipates or increases over time. We first estimate the post-passage dummy variable in equation (1) and then address the second and third potential problems.

Table 3a contains results for our estimates for the reform regressor (the post-reform dummy) on legal and judicial spending during 1961-1999 using the simple model in (1). The estimated effects of reform are statistically significant at the 1% level and the quantitative impact is large: that is, the elimination of elections is associated with a 13.7% increase in judicial and legal spending.

The specification in (1) assumes that spending remained fixed after reform. However, it may also be the case that reform is followed by an increase in spending that subsequently falls off or accelerates. To check if our results in Table 3a are robust to these possibilities, we estimate the following extension of equation (1):

$$\ln J\ \&\ L_{st} = \alpha_s + \gamma_t + \delta REF_{st} + \eta REF_{st} \min(Tref, t - tref_{st}) + \varsigma X_{st} + \varepsilon_{st} \qquad (2)$$

In this model, we add an interaction of the post-reform dummy with a time effect, where $\mathbf{tref_{st}}$ is the year that a state eliminates elections, **t** is any year after elections have been removed, and $\mathbf{T_{ref}}$ is the period during which we allow some adjustment (trend) in state spending patterns following reform. This is the hybrid model as discussed by Ayres and Donohue (2003), and we estimate six and twelve year trend effects.[7] We also include a vector of state level time varying control variables, $\mathbf{X_{st}}$, that includes logged population, logged annual real per capita income and whether or not a state has an intermediate appellate court.

Table 3b reports three different estimates of the association between reform and spending on the judiciary. The dummy variable model includes the additional controls

[7] Donohue and Ayres put no limit on the number of years that the trend effect may occur. Because we are using a much longer panel, we impose a six and twelve year limit.

but ignores trend effects. The medium term and long term models include the additional controls and either the six year or the twelve trend effects. In each case, the association between reform and spending on the judiciary remain statistically significant at the 1% percent level. Most notably, trend effects are statistically insignificant and the association between reform and judicial spending ranges between 14.8% and 16.9% and is much stronger than the baseline estimate in Table 3a.

As an additional robustness check for the results in Table 3b, we estimate equation (2) using operating expenditures (deflated and per capita) in the judicial and legal sector as the dependent variable. This measure is only available during selected years where detailed survey of employment and expenditures were conducted during 1971-99. Moreover, it is highly correlated with overall judicial spending as well as employment and payrolls in the judicial and legal sector. Table 3c reports estimates for the dummy variable model, and the six and twelve trend effects model. In each case, the post-reform dummy is statistically significant at no greater than the 5-percent level; and its association with operating expenditures is stronger than its association with judicial spending. Furthermore, the time six year and twelve year trend effects remain small in size and statistically insignificant. Thus, there is substantial evidence that supports the judicial resources hypothesis.

IV. The Independent Decisions Hypothesis

This section tests the hypothesis that judges in states that eliminate retention elections are more likely to make decisions that are unpopular and protect minority rights after the reform than they were prior to reform. To test this hypothesis we focus on how

court of last resort judges influence state education policy following the enactment of IDEA in 1975. IDEA mandated "a free appropriate public education in the least restrictive environment appropriate to their individual needs" (U.S. Department of Justice 2005). Thus, school districts were able to determine what was an "appropriate" education "in the least restrictive environment appropriate" to the needs of each individual student. If the parents of a child disagreed with the schools assessment, this could be appealed through the state judicial system. "Mainstreaming" children with disabilities (in which children with disabilities are educated in the same classes as children with disabilities) is an unpopular political position that promotes minority interests. Judges who face popular elections will be less likely to do this (since they need to face the electorate for their reelection), and thus we would expect that enrollments in these special programs to be lower in states where judges are elected than in other states.

In this section we test this idea econometrically, and estimate the following fixed effects model:

$$\ln IDEA_{st} = \alpha_s + \gamma_t + \delta REF_{st} + \varsigma X_{st} + \varepsilon_{st} \qquad (3)$$

In this specification **t** = 1977, 1979, …1999 denotes an odd numbered year, **ln IDEA$_{st}$** denotes log enrollments of students with disabilities in state programs under IDEA-B and chapter 1 of ESEA in state s in year t, and X_{st} is a vector of covariates including (as before) log population and log real per capita income and whether or not a state has an intermediate appellate court.

Specifications (1) and (2) in table 4 summarize estimates of the model in (3) during 1977-1999. The estimated effects of reform are statistically significant at the 1% level and their impact is substantial: eliminating elections is followed with roughly a 14% increase in enrollments whether or not we control for population, income and intermediate appellate courts.

Judges are also under more pressure to promote equitable school finance across school districts in those states that have constitutional provisions guaranteeing the right to an equitable and free education (see Figlio et al, 2004). This constitutional language can make it easier for plaintiffs to challenge the constitutionality of existing financing schemes, and make it harder for judges to uphold the status quo when there are substantial disparities in education financing across school districts. Table 5 lists states that had such provisions on their constitutions during 1960-2000. In 1960 all seventeen of elected their court of last resort judges; by 2000 seven of these state eliminated elections. We would also expect that there would be a stronger case to make for students with disabilities and that judges would have less discretion to rule against students with disabilities in state that have constitutions with these uniformity provisions. To check for this, we estimate:

$$\ln IDEA_{st} = \alpha_s + \gamma_t + \delta_1 REF_{st} + \delta_2 D * REF_{st} + \varsigma_1 X_{st} + \varsigma_2 D * X_{st} + \varepsilon_{st} \quad (4)$$

where D is an interaction term equal to one for states that have education uniformity provisions on their constitutions, and zero otherwise. Thus, the regressor δ_1 measures the impact of eliminating elections in those state do not have uniformity provisions, and δ_2

measures the additional impact of reform in those states that have these constitutional provisions.

Specifications (3) and (4) in table 4 report the estimates for the post-passage dummy. What is striking is that most all of the enrollment increase occurs in those states that have education uniformity provisions in their constitutions. For example, if we control for population, per capita income and intermediate appellate courts, there is no statistically or quantitatively significant increase following reform in those states that do not have education uniformity provisions in their constitutions, but there is a 0.9% + 21.2% ≈ 22% increase in those states that have these provisions. These results provide evidence that the elimination of retention then encourages judges to make unpopular decisions that protect minority rights.

These results also show the importance of state constitutional protections for minorities (in this case, children with disabilities). In states that require education (especially education spending) to be uniform, children with disabilities fare much better. The constitutional requirement of uniformity removes some of the discretion judges have in deciding these cases and forces them to decide cases differently that judges in states without this provision. Indeed, this is the single-most important factor in explaining the enrollment increase in programs for children with disabilities.

The estimates of equations (3) and (4) assume that once elections are removed, the impact of this reform is constant. However, it may also be the case that the initial burst of enrollments following reform can subside or even accelerate. To check for this, we interact the post-passage dummy with a time effect and estimate six and twelve trend effects for states with and without education uniformity provisions in their constitutions.

All of our results are robust to these considerations (see Appendix, Table A1) ; i.e., the post-passage continues to be large and statistically in those states that uniform education provisions on their constitutions.

V. Conclusion

Our results here are both novel and interesting. First, we have found that after the elimination of elections, judges receive additional resources. These additional resources serve to make them more independent from the legislature. Second, we find that this independence makes judges more likely to protect the interests of minorities (specifically, children with disabilities). However, these protections did not occur in all states; rather, they only occurred in those states where equity in education was required by the state constitution. Thus, this requirement of equity is an important factor in understanding judicial behavior in these cases.

There are two key implications of our findings. First, there are two components to achieving judicial independence. The elimination of elections alone is not enough. Rather, this elimination must also be accompanied with an increase in resources to allow judges to utilize their independence.

Second, and more importantly, even after elections are eliminated and resources increased, judges are still constrained by the laws of their state. Looking at children with disabilities, we found that judges in states with constitutions that required uniformity in education behaved differently and were more protective of these children than judges in states without such constitutional requirements. Judicial independence is about more than just elections and resources. It is also about the requirements of the constitution.

Even if a state were to end elections and give the judiciary more resources, judges would still be constrained by the language of the constitution of the state. This finding should give comfort to those who fear that giving judges too much independence will lead to these judges freely disregarding the constitution of the state. Our analysis indicates that, to the contrary, judges will continue to be faithful to the constitution, even if there are no immediate repercussions if they are not.

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Table 1: Retention Procedures in State Courts of Last Resort, 1960-2000

Eliminated Partisan and Non-Partisan Elections

State	Year of reform
Arizona	1974
Colorado	1966
Florida	1972
Illinois	1971
Indiana	1968
Iowa	1962
Maryland	1976
Nebraska	1962
New Mexico	1989
New York	1979
Oklahoma	1967
Pennsylvania	1968
South Dakota	1981
Tennessee	Revert to elections in 1966, and reform in 1995
Utah	1967
Wyoming	1972

Always Had Elections: Alabama, Arkansas, Georgia, Idaho, Kentucky, Louisiana, Michigan, Minnesota, Mississippi, Montana, Nevada, North Carolina, North Dakota, Ohio, Oregon, Texas, Washington, West Virginia, Wisconsin

Never Had Elections: California, Connecticut, Delaware, Maine, Massachusetts, Missouri, New Hampshire, New Jersey, Rhode Island, South Carolina, Vermont, Virginia

Date source: The Book of the States, various years.

Table 2: Determinants of Judicial and Legal Spending, 1971-79 and 1983-2000

	Judicial and Legal Spending	Court of last resort salaries	Intermediate Appellate Courts	Full time employees	Full time payrolls	Operating Exps
Jud and Leg Spending	1.00					
Court of last resort salaries	0.10	1.00				
Intermediate Appellate	0.14	0.47	1.00			
Full time employees	0.65	0.59	0.55	1.00		
Full time payrolls	0.92	0.18	0.14	0.68	1.00	
Operating Exps.	0.93	0.18	0.14	0.64	0.98	1.00

Data sources: The Book of the States, various years; and U.S. Department of Justice, Bureau of Justice Statistics. 1996, and 1997-2004.

Table 3a: The Estimated Impact of Judicial Reform on Judicial and Legal Spending, Controlling only for State and Year-Effects

Time period, 1961-1999	
Post-passage dummy	13.7%*** (4.6%)
Adjusted R^2	0.916
Observations	960

Table 3b: The Estimated Impact of Judicial Reform, with a Broader Group of Controls

Time period, 1961-1999	
1. Dummy Variable Model:	
Post-passage dummy	16.9%*** (4.7%)
Adjusted R^2	0.920
2. Medium Term Trend Model:	
Post-passage dummy	16.8%*** (5.0%)
Six-year trend effect	0.1% (1.4%)
Adjusted R^2	0.920
3. Long Term Trend Model:	
Post-passage dummy	14.8%*** (5.1%)
Twelve-year trend effect	0.6% (0.6%)
Adjusted R^2	0.920
Observations in all 3 specifications	960
Description of Additional Controls: ln (state population), ln(real per capita income) and whether or not a state has an intermediate appellate court	

Table 3c: The Estimated Impact of Reform on Operating Expenditures

Time period, 1971-1999	
1. Dummy Variable Model:	
Post-passage dummy	24.0%*** (8.3%)
Adjusted R^2	0.971
2. Medium Term Trend Model:	
Post-passage dummy	22.0%** (8.9%)
Six-year trend effect	1.3% (1.6%)
Adjusted R^2	0.971
3. Long Term Trend Model:	
Post-passage dummy	20.6%** (9.0%)
Twelve-year trend effect	0.8% (0.6%)
Adjusted R^2	0.971
Observations in all 3 specifications	669 (there are 3 missing cells)
Description of Controls in each specification: State-fixed effects; year-fixed effects; ln (state population); ln (real per capita income), whether or not a state has an intermediate appellate court	

Notes: The dependent variable is ln (judicial and legal expenditures spending), where this is expenditures per capita and deflated using the 2000 cpi. Because the judicial retention procedure is reported once every two years, the regressions are run on all odd years between 1960 and 2000. Between 1960 and 1981 judicial and legal expenditures are solely for courts; as of 1982 this category includes expenditures for legal defense and public prosecutors. In this and all subsequent tables, standard errors are reported in parentheses and are corrected for heteroskedasticity, and the notation ***, ** and * denotes coefficients that are significant at the 1%, 5% and 10% levels. See Ayres and Donohue (2003) for a discussion of the models that include the six and then twelve year trend effects.

Table 4: Estimated Impact of Judicial Reform on Services for Students for Disabilities in public schools, 1977-99 (every other year)

Independent Variables	**Dependent Variable is the logged enrollment of children with disabilities in state programs under IDEA-B and chapter 1 of ESEA**			
Specification	(1)	(2)	(3)	(4)
Post-passage dummy	14.4*** (4.1%)	14.1%*** (3.9%)	-1.0% (5.0%)	0.9% (4.1%)
*Post-passage dummy * education uniformity*	X	X	25.8%*** (6.1%)	21.2%*** (5.5%)
Controls	State-fixed effects and year effects			
Additional Controls	X	ln(state population); ln(real per capita income); intermediate appellate courts	X	ln(state population); ln(real per capita income); intermediate appellate courts, differentiated by education uniformity
Number of observations	576	576	576	576
Adjusted R^2	0.758	0.765	0.766	0.779

Notes: Standard errors are heteroskedasticity corrected, and ***, ** and * denotes significance at the 1%, 5% and 10% levels.

Data sources: U.S. Department of Education. National Center for Education Statistics. 1998, table 14; and, U.S. Department of Education, Office of Special Education and Rehabilitative Services, various years**.**

Table 5: States where the Constitution Requires a Uniform Education System, 1960-2000

State	Eliminated Elections	Never had Elections	Always had Elections
Arizona	**1974**		
Colorado	**1966**		
Florida	**1972**		
Idaho			**Yes**
Indiana	**1968**		
Louisiana			**Yes**
Minnesota			**Yes**
Montana			**Yes**
Nevada			**Yes**
New Mexico	**1989**		
North Carolina			**Yes**
North Dakota			**Yes**
Oregon			**Yes**
South Dakota	**1981**		
Washington			**Yes**
Wisconsin			**Yes**
Wyoming	**1972**		

Data Sources: The Book of the States, various years; Corcoran et al (2004); Minorini and Sugarman (1999).

Figure 1: State Judicial and Legal Expenditures

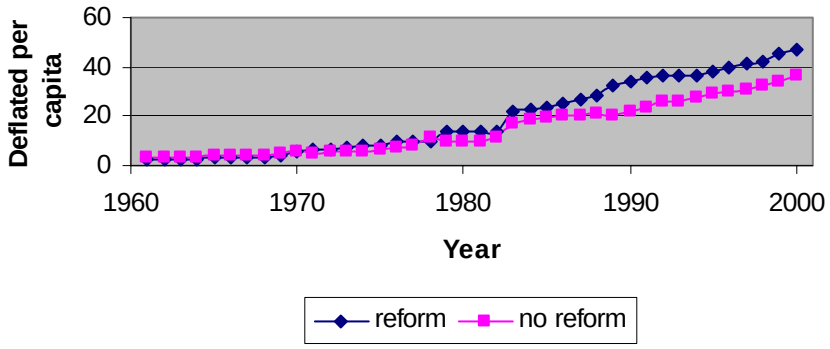


Figure 2: Salaries for State Court of Last Resort Judges

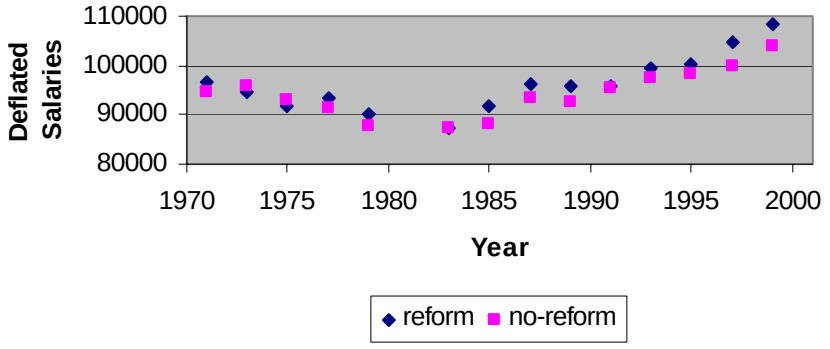


Figure 3: States with Intermediate Appelate Courts

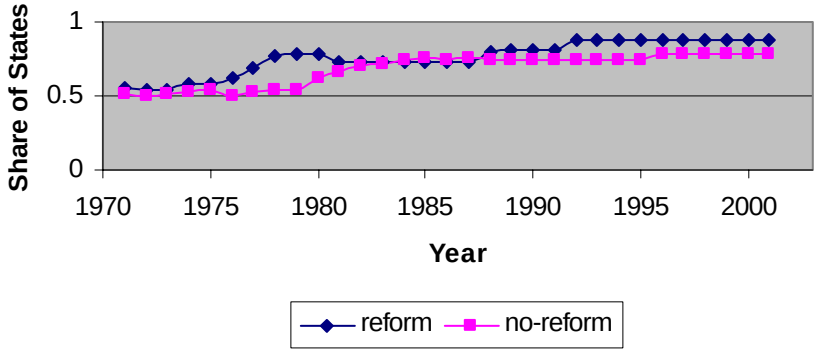


Figure 4: Judicial and Legal Employment

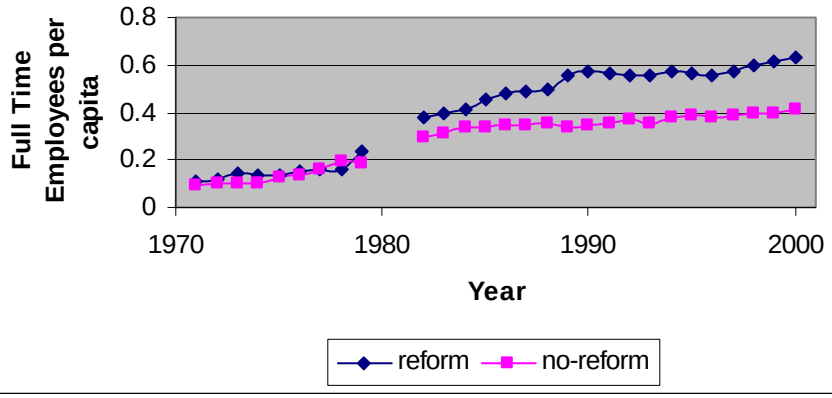


Figure 5: Judicial and Legal Payrolls

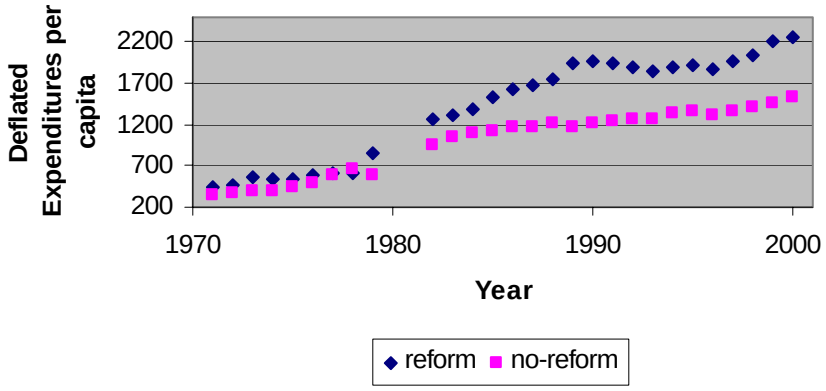
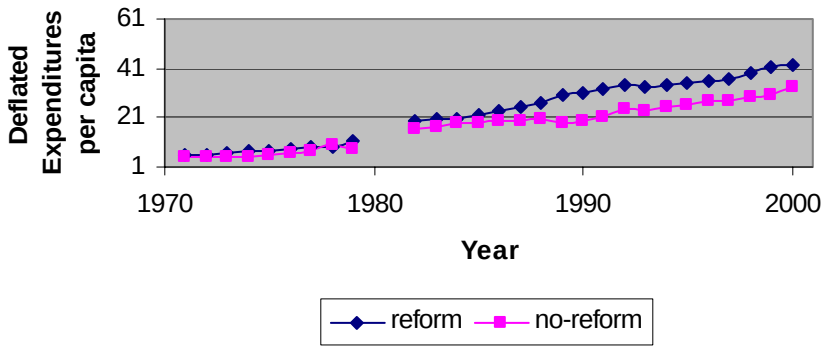


Figure 6: Judicial and Legal Operating Expenditures



Appendix

Table A1: Estimated Impact of Judicial Reform on Services for Students for Disabilities, Controlling for Timing of Reform

Time period, 1961-1999	
1. Dummy Variable Model:	
Post-passage dummy	-1.0% (5.0%)
*Post-passage dummy*education uniformity*	25.8%*** (6.1%)
Adjusted R^2	0.766
2. Medium Term Trend Model:	
Post-passage dummy	1.8% (4.8%)
Six-year trend effect	-0.4% (1.2%)
*Post-passage dummy*education uniformity*	19.0%*** (6.1%)
*Six-year trend effect*education uniformity*	1.4% (1.5%)
Adjusted R^2	0.779
3. Long Term Trend Model:	
Post-passage dummy	-0.8% (4.5%)
Twelve-year trend effect	0.5%* (0.3%)
*Post-passage dummy*education uniformity*	21.7%*** (5.9%)
*Twelve-year trend effect*education uniformity*	-0.3% (0.4%)
Adjusted R^2	0.780
Observations in all 3 specifications	576
Description of Additional Controls: ln (state population), ln(real per capita income) and whether or not a state has an intermediate appellate court, and these are differentiated by education uniformity in the medium term and long term trend models	

Notes: Standard errors are heteroskedasticity corrected, and ***, ** and * denotes significance at the 1%, 5% and 10% levels.