

## Why Are Japanese Judges So Conservative in Politically Charged Cases?

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**T**heory suggests that Japanese politicians have weaker incentives than U.S. politicians to keep lower court judges independent. Accordingly, we hypothesize that Japanese lower court judges who defer on sensitive political questions will do better in their careers. To test this, we assemble several new data sets and measure the quality of the assignments received by about 400 judges after deciding various types of cases. We find that judges who deferred to the ruling party in politically salient disputes obtained better posts than those who did not, and that judges who actively enjoined the national government obtained worse posts than those who did not. We also hypothesize that judges with forthrightly leftist preferences do worse in their careers. We measure the speed at which the 500 judges hired during the 1960s moved up the pay scale and find indications that judges who joined a leftist group were promoted more slowly than their peers.

**A**lthough the judiciary is as much a branch of government as the executive and legislature, in most modern democracies it prides itself on its independence from voter preferences. In turn, many voters take pride in their lack of power over the judiciary, at least until it does something they dislike. This is interesting in itself, but of even more interest is how the organization of courts affects their independence.

Modern governments use a variety of ways to structure courts. Some appoint judges for life to a single position, some subject them to elections, and others appoint them at a young age to a judicial bureaucracy that rotates them through a variety of posts. Scholars have studied appointment-for-life regimes most closely, if only because that is the U.S. federal court organization. Indeed, the social scientific literature on U.S. courts is voluminous. Much excellent work focuses on how politicians decide whom to appoint (e.g., Cameron, Cover, and Segal 1990; de Figueiredo and Tiller 1996), how and when members of a court may act strategically with respect to one another (e.g., Cooter and Ginsburg 1996; Segal 1997; Spiller and Gely 1992), and how and when the court as a whole may act strategically with respect to statutory reversal by the legislature (e.g., Atkins and Zavoina 1974; Caldeira, Wright, and Zorn 1999; Easterbrook 1982; Revesz 1997; Songer, Segal, and Cameron 1994).

Outside the United States, by far the most common judicial systems are bureaucratic. In such systems, the

government generally taps young jurists by examination rather than political connection. In the course of their training, their performance is monitored by more experienced judges to prevent slacking or bias. Yet, precisely because senior judges have that power, the courts are potentially vulnerable to indirect political pressure.

We will examine the Japanese judiciary. Unlike many countries with bureaucratic courts, Japan does not send politically charged disputes to special constitutional courts. In cases involving the government, however, Japanese judges routinely validate what the government has done. The Japanese Supreme Court is legendary for seldom voiding statutes. Although lower courts defer slightly less, they also parrot the moderately conservative positions of the longtime incumbent Liberal Democratic Party (LDP).

The reason Japanese Supreme Court justices uphold LDP positions is straightforward: For most of the postwar period they have been recent LDP appointees. Why lower court judges would uphold LDP positions is less obvious, since the government appointed them straight out of law school with relatively little information about their political leanings. All else equal, the government should have found itself saddled with at least a substantial minority of heterodox judges. Yet, heterodox opinions generally did not follow, and we argue that the explanation lies in the career structure of the courts. We know that the Japanese courts use job postings as incentives. Elsewhere, for example, we have found that judges who write administrative law opinions that are reversed receive worse transfers, as do those who acquit criminal defendants on formalistic grounds or who acquit leftist politicians of violating electoral campaign laws (Ramseyer and Rasmusen 1997, 1999b, 2001b).

Using new data on about 400 judges, we will explore the career effect of controversial opinions in a range of politically charged headline-grabbing disputes. We first locate proxies for a judge's seniority, intelligence, effort, and ideology. Holding those proxies constant, we examine the careers of (1) judges who held either the Self-Defense Force (SDF) or U.S. bases unconstitutional; (2) judges who rejected electoral apportionment schemes advantageous to the LDP; and (3)

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We received helpful comments and suggestions from Christine Jolls, Dennis Karjala, Mark Levin, Kip Viscusi, Mark West, the editor of this journal, the anonymous referees, and participants at workshops at the Comparative Law and Economics Forum, Indiana University, the Japan Behavioral Statistics Association, Kyushu University, the Law and Society Association, the University of Michigan, the National Bureau of Economic Research, Ohio State University, Osaka University, the University of Pennsylvania, the University of Tokyo, Waseda University, and the University of Washington. Ramseyer gratefully acknowledges the generous financial assistance of the John M. Olin Program in Law, Economics, and Business, Harvard University.

judges who often enjoined the national government in administrative law suits. Systematically, we find that they suffered in their careers.<sup>1</sup> We conclude by exploring whether judicial salaries are correlated with political affiliation. Using career data on the 500 or so judges hired between 1959 and 1968, we find evidence that leftist judges are indeed promoted more slowly than conservatives.

We know of no other scholars who have used a multivariate approach to test systematically the effect of politics on judicial careers in a bureaucratic system. Moreover, to our knowledge this article is the first to use Japanese career data to study judicial independence in a range of disparate but politically sensitive disputes. It is also the first to use the data to test for a political bias in pay.

## TOWARD A POSITIVE THEORY OF JUDICIAL INDEPENDENCE

### The Puzzle

Before we explain the institutional structure of the Japanese courts and explore the connection between public law opinions and judicial careers, we will outline the conditions under which a government could be expected to keep judges genuinely independent. Although voters elect politicians to deliver policies, they do not expect them to do all the work themselves. They expect them to hire agents—generally, bureaucrats—to see the policies through. They also expect the politicians to prevent the agents from promoting policies they dislike.

Given the desire to remain in office, elected officials monitor their bureaucratic agents with care. Not every tax agent will perform every audit perfectly; the optimal level of agency slack in government is not zero any more than it is in private business. As do managers of private firms, however, politicians devise mechanisms (or “fire alarms,” McCubbins and Schwartz 1984) to alert them to serious slack. In private business, managers do this to increase shareholder profits. In government, politicians do it to deliver policies voters want.

Judges are just another set of agents. They can be elected, as in some U.S. state judiciaries, or appointed by politicians, as in Japan, the U.S. federal judiciary, and most of the rest of the world. Politicians can readily discipline misbehavior by most appointed bureaucrats, subject to the constraints of civil service laws that the politicians themselves pass. The puzzle of judicial independence is why politicians apparently do not discipline judges similarly. Why would politicians find it advantageous to control one set of agents (bureaucrats) but let another set (judges) run free? Why would voters reelect politicians who do nothing to

stop judges from blocking the policies for which they elected the politicians in the first place?

Research suggests several reasons rational politicians might not use career incentives to control judges. First, perhaps politicians find it hard to make their promises credible. Whether in selling regulatory rents to lobbyists or in promising policies to voters, they have an incentive to renege on their commitments after the fact. By delegating dispute resolution to independent judges, they may increase the credibility of their initial promises (Landes and Posner 1975).<sup>2</sup>

Second, perhaps by giving citizens the right to sue misbehaving bureaucrats, politicians can use the courts to keep bureaucrats in line. Suppose politicians worry that bureaucrats may try to deflect this “fire alarm” by leaning on judges. If so, then they may want to keep courts strictly independent (McCubbins and Schwartz 1984).

Third, perhaps politicians hope to mitigate their losses from losing elections. Although they could increase their power as majority politicians if they constrained judges, that power would come at a cost. What they now do to the opposition, the opposition may do to them later (Ramseyer 1994).

All else equal, we therefore expect courts to be less independent if the majority party (1) can credibly commit to policy through means other than the courts, (2) can detect misbehaving bureaucrats through mechanisms other than the courts, and (3) can expect to continue winning elections.

### Why Study Japan?

**The Empirical Problem.** A straightforward way to test these hypotheses would involve regression analysis on data across countries and time, but scholars have not yet collected the necessary information for countries other than the United States. Therefore, we focus on one country: Japan. Data availability is crucial, because official pronouncements cannot be taken at face value. On the issue of judicial independence, modern governments present a united front: They are for it. They maintain a constitutional framework that promises judges independence from politics, and they collect no data that show the contrary. Politicians claim, and most local law professors agree, that the judges are indeed independent.

**The U.S. Federal Example.** We do not wish to exaggerate the risk of political bias. The three hypotheses above largely suggest that U.S. federal politicians would want to keep judges independent, and evidence suggests they usually do. Caveats aside,<sup>3</sup> no matter how

<sup>1</sup> Elsewhere (particularly Ramseyer and Rasmusen 1999a), we explore more fully whether these effects result from the politically charged character of the disputes, from a nonpolitical bureaucratic response to innovation by the judges involved, or from inaccuracy in judicial interpretation. We find that the motivation for punishment is primarily political.

<sup>2</sup> This argument hinges in part, of course, on whether courts interpret statutes in light of the enacting politicians' preferences rather than those of current politicians, a proposition for which empirical studies (e.g., Eskridge 1991) tend not to find evidence.

<sup>3</sup> One can overstate the point. Congress does have some controls (e.g., the reach of court jurisdiction, the number of judges, the timing of increases in judicial salaries; George and Epstein 1992; Wasby 1988) and has manipulated judicial careers on a few occasions (e.g., the Jefferson-Adams battle over the midnight appointments; Rosenb 1992, 380).

U.S. federal judges decide their cases, most will spend the rest of their career deciding the same kinds of cases, sitting in the same cities, and earning no more and no less than their peers. A few dream of promotion to a higher court, but the effect of that incentive remains modest.<sup>4</sup>

**The Japanese Example.** The Japanese Constitution also guarantees independence: “All judges are independent in the exercise of their conscience and bound only by this constitution and the laws” (Art. 76, Sec. 3). Most observers would agree with Japanese law scholar John Haley (1998, 98) that “the political branches of government have long ignored the courts and the judge-administrators of the system have worked hard to preserve that judicial autonomy.”

Yet, each of the three hypotheses suggests that Japanese politicians would seek to constrain judges. First, the majority LDP maintains an internal structure that readily enables it to make its commitments credible: Party affairs are centralized under the control of senior politicians from safe districts, control over policy is delegated to them, and they are regularly paid enormous amounts of legal and illegal cash. The result was a majority party controlled by leaders who earn efficiency wages in a long-term indefinitely repeated game (Ramseyer and Rosenbluth 1997, 7). Necessarily, these are players with incentives to maintain their reputation. Necessarily, when these leaders promise policy their promises are credible.

Second, through its local organizations the LDP maintains its own fire alarms for detecting bureaucratic misbehavior. For decades, Japanese voters elected their politicians from multimember districts under a single nontransferable vote system. As a result, to capture a majority of the Diet, a majority party needed to elect multiple representatives from most districts. That in turn required it to divide its supporters among several candidates. Rather than do this by ideology, the LDP used candidate-specific support groups that dispensed pork and provided ombudsman services. In part, therefore, candidates gave voters some bureaucratic interventionist services directly (Ramseyer and Rosenbluth 1997, chap. 2, 113).

This system left the LDP with little reason to encourage citizens to use the courts to complain about bureaucrats in any dispute of moment. Indeed, because voters from LDP districts often could obtain the help they needed from their representatives, those who sued the government over substantial issues may have hailed disproportionately from non-LDP districts. By disabling the courts as a means of controlling bureaucrats in cases that raise significant policy issues, the LDP may even have not decreased but increased voters' gains to returning its candidates to office.

<sup>4</sup> Compare, e.g., Higgins and Rubin 1980 (potential promotions do not affect judicial behavior) with Anderson, Shughart, and Tollison 1989 (a positive relation between state supreme court justice salary and the tendency to overturn statutes), Cohen 1991 (potential promotions do affect judicial behavior), Rosenberg 1992 (U.S. Supreme Court responds to a wide variety of threats from Congress), and Toma 1991 (U.S. Supreme Court budget affects opinions).

Third, the LDP could rationally expect to stay in power. The probability was less than 1, to be sure, as it discovered in 1993. But that loss was a surprise, the result of brinkmanship by party factions over how to reposition the party (Ramseyer and Rosenbluth 1997, Preface, chaps. 2, 5). From 1955 to 1993, the LDP maintained steady control over the Diet and could rationally expect that situation to continue.

## THE STRUCTURE OF THE JAPANESE COURTS

### The Supreme Court

Even the U.S. Supreme Court does not invalidate legislation as a matter of course, but the Japanese Supreme Court is deferential in the extreme. As of 1993, it had held legislation unconstitutional only about a half-dozen times in its entire history (Haley 1998, 179–80; Okudaira 1993, 20). The reason is straightforward. Almost all the justices were recent LDP appointees, and the party passing the legislation was the LDP.

Given the frequent political turnover in America, U.S. presidents try to stack the Supreme Court with relatively young justices to take advantage of lifetime tenure. This produces the motley ideological array that Americans take for granted: The Court includes both Democrats and Republicans as well as justices (because they often serve 20 years or more) who dramatically change their political preference since their appointment.

LDP leaders faced a different political environment. During most of the postwar period, they tightly controlled the party, which controlled the Diet, and no opposition party had a significant chance of coming to power. Virtually all justices except a few carryovers from Katayama's short-lived Socialist cabinet of 1947–48 were conservative appointees. Because the LDP expected to stay in power, its leaders could afford to appoint justices old enough (generally in their early 60s) not to change their views before mandatory retirement at age 70 (Ramseyer and Rosenbluth 1997, chap. 8).

Although the prime minister largely rubber-stamps Supreme Court nominees selected by a group within the career judiciary, that is irrelevant. The group only nominated people they knew the prime minister would approve. Many postwar justices came from the bar and the universities, but they were hardly a random sample of talented lawyers. During much of the period, the bar and especially the universities in Japan—as in other wealthy democracies—were disproportionately left of center. Had the nominating judges looked only to raw talent, they would regularly have proposed leftists, but they never tried. The prime minister could safely rubber-stamp nominees because the nominators knew he could just as easily reject them.

### The Lower Courts

The real puzzle is not the conservatism of the Japanese Supreme Court but of the more than 2,000 judges in

the district courts, high courts, and family courts. Unlike U.S. federal judges, Japanese lower court judges are not appointed with verifiable political histories. Instead, they are ordinarily named in their late 20s, straight out of the national law school, the Legal Research and Training Institute (LRTI). It is hard to predict what a young appointee's political beliefs will be decades later.

From time to time, observers have tried to explain lower court deference to government by the prewar autocratic legacy or the purportedly greater deference in civil-law judiciaries generally. Neither explanation works. Japanese politics and government differ radically today from prewar years. The legacy argument cannot explain why this aspect of modern government is affected but not others. Some modern European courts defer to the government less than do Japanese courts, notwithstanding their shared civil-law tradition.

### Appointment and Reappointment

The reason for Japanese lower court deference lies in the internal structure of the courts. American federal trial judges have a job for life. Absent egregiously bad behavior or senility—and perhaps even then—they can work as long as they want. Unless they quit by choice or are promoted to the Court of Appeals, they will sit in the same court in the same city for their entire judicial career and collect the same salary as all other federal trial judges.

By contrast, after their initial appointment Japanese judges are reassigned every few (generally three) years.<sup>5</sup> A hypothetical judge, Ichiro Tanaka, illustrates the typical pattern. During his first three years he works as a trial judge in the Osaka District Court. He then is transferred, first to the Sendai High Court, then to the family court branch office in Miyazaki, and from there to the Ministry of Justice in Tokyo. He may spend a stint teaching at the LRTI or working in the Secretariat, the administrative offices of the court system.<sup>6</sup> So long as Tanaka is reappointed every ten years, as are virtually all judges, he will have a job in the judiciary until age 65. Yet, the quality of this job will depend crucially on how the judges in the Secretariat rate his work. Even his pay may hinge on their evaluation. They cannot constitutionally cut his salary, but they have no obligation to give him prestigious jobs or move him up the pay scale at the same rate as everyone else.

By all accounts, most Japanese judges find administrative duties prestigious and branch office assignments embarrassing. Like the vast majority of professionals, they want to live in Tokyo if possible and in Osaka if not. Like the vast majority of humans, they prefer higher pay to lower. The fact that prestige, geography,

and pay depend on performance should induce them to work hard and carefully, and by all accounts they usually do.

At least indirectly, however, this system presents the potential for political manipulation. LDP prime ministers appoint moderately conservative justices to the Supreme Court; they give them the job of supervising the Secretariat; and they usually keep on the Court at least one justice who previously headed the Secretariat and knows its workings intimately. The Secretariat, in turn, decides which judges will go to what cities, who will hold which prestigious administrative jobs, who will spend how many years in branch offices, and who will climb the pay scale at what rate.<sup>7</sup> The question is whether LDP leaders use this potential political influence.

Note three additional features of the courts. First, Japanese courts do not use juries. All trials are bench trials, with the judge deciding questions of fact as well as law. Second, most trials are conducted by three-judge panels, routine nonserious criminal trials being the exception. Third, lower court opinions are signed by the entire panel. Even if a judge dissents, that fact is not publicly disclosed.

## SAMPLES

### The Project

A systematic examination of how public law opinions affect judicial careers requires a study of disputes that involve large numbers of judges. A dispute in which only one or two lower court panels are engaged provides anecdotal evidence of political influence, and scholars have detailed these anecdotes in both Japanese and English (e.g., Kashimura 1991; Miyazawa 1991; Ramseyer and Rosenbluth 1997, chaps. 8–9; Sakaguchi 1988; Tsukahara 1991). Although the anecdotes suggest that judges who flout the political preferences of the LDP receive worse assignments, such evidence is inconclusive. Too many judicial transfers have nothing to do with politics. Many are simply random. The Secretariat worries about corruption of judges by organized crime, and the easiest way to reduce the likelihood is to move judges regularly. Other transfers are incentives for effort. The Secretariat cares whether judges work or shirk, and assignments can be used to reward or punish. Even the most pro-LDP judges may spend time in branch offices and provincial cities if they shirk.

To determine whether the Secretariat uses job assignments to punish and reward judges for the opinions they write, we need a systematic multivariate approach. Accordingly, we focus on disputes for which a relatively large number of opinions were written. We then code these by a political metric and whether they were

<sup>5</sup> For more detail, see Ramseyer and Rosenbluth 1997, chaps. 8–9.

<sup>6</sup> The Secretariat is staffed by career judges. It is headed by the secretary general (also a career judge, but one on a fast track; unless he makes serious mistakes he has high odds of being named to the Supreme Court soon), who answers only to the Supreme Court. The LDP has potential indirect control over the courts, through its control over the cabinet, which names Supreme Court justices. The LDP has no direct control.

<sup>7</sup> No legislative body in Japan plays the oversight role of the U.S. Senate Judiciary Committee. Because of the LDP's hold over the Diet, oversight is primarily played out within the party and executed indirectly through the appointment of party loyalists to the Supreme Court.

reversed, and we ask whether judges' decisions help explain the appointments they later received. More precisely, we estimate the quality of a judge's post-public-law opinion job postings, denoted *Job*, through a regression equation:

$$Job = a + B_1Opinion + B_2Controls + e.$$

*Opinion* is a vector of variables that describe the judge's public law opinion—for example, whether it followed LDP policy or was reversed on appeal. *Controls* is a vector of control variables that proxy for the judge's seniority, ideology, effort, and intelligence.

Why would opinion ever take an antigovernment value if judges know that job would be affected? If we are correct that such behavior hurts careers, then it should not happen in equilibrium. Opinion—our independent variable—should not be totally independent but should depend on the size of expected punishment. This is not a serious concern here for two reasons. First, because most of the heterodox opinions we study date from the 1960s and 1970s, a period when the LDP was still institutionalizing its career structure, judges could not yet be certain whether they would be punished. Although judicial decisions today may indeed be endogenous to the expected punishment, that is less a problem the earlier the decision.

Second, some judges simply will not sacrifice principle for career in even the small number of politically sensitive cases that come before them. Their behavior is effectively independent of career concerns (those observations in our data sets provide the variance for our statistical tests).

We will investigate three sets of politically charged disputes for our opinion variables: the constitutionality of the military, malapportionment, and injunctions against the government. If we find that opinions in only one category of dispute affect job quality, then we might conclude either that it involves a particularly sensitive area for politicians or that the result is an accident of the data. If we find that opinions in each set of disputes consistently affect job quality, then we can safely conclude that judges face politically biased incentives in politically charged cases. As a supplementary test, we will investigate the effect of a judge's membership in a leftist bar organization on the speed of promotion, which indirectly tests the effect of political affiliation on pay.

## The Data Sets

**Sources.** We assemble data from several sources. For judicial opinions, we rely on the *Hanrei taikai* (Dai-ichi various years), which resembles the American Westlaw and Lexis and includes virtually all post-World War II published opinions on CD-ROMs. For judicial careers, we use the *Zen saibankan keireki soran* (Nihon minshu 1998), a book that details all job postings for judges educated after World War II. For membership in the communist-leaning Young Jurists League (YJL) as of 1969, we use *Osorubeki saiban*, which copied the list

from the league's own newsletter (Shiso 1969). We present selected summary statistics in Appendix A.<sup>8</sup>

**Constitutionality of the Military.** Our first set of cases involves the constitutionality of the Japanese military. Article 9 of the Constitution proclaims that "land, sea, and air forces, as well as other war potential, will never be maintained." By any but the most tortured interpretation, this bans the SDF. Consistently, the LDP has claimed it does not. By no stretch of the imagination could Douglas MacArthur, godfather of the clause, have thought it banned U.S. bases. Occasionally, the opposition has said it does.

Each time the Supreme Court faced a challenge to the SDF or American bases it refused to hold either unconstitutional (Beer 1996). From time to time, however, lower court judges did. We found 25 district court opinions that addressed Article 9, three of which held either the SDF or the bases unconstitutional (the source of our key independent variable). As we cannot code opinions and careers about which we have insufficient data, here and elsewhere we drop (1) unsigned opinions, (2) judges who do not appear in Nihon minshu (1998) (generally judges educated before the war), and (3) judges with less than eighteen months of experience before the opinion or less than 2.5 years' experience afterward. Through this process, we obtained a set of 47 judges who wrote opinions on Article 9.<sup>9</sup>

**Malapportionment.** Our second data set concerns electoral apportionment, which is a chronic issue in Japanese courts. Through the 1960s and into the 1970s, the LDP relied heavily on the rural vote, but farm families were steadily migrating to metropolitan centers. As a result, the LDP gained by stalling reapportionment. By keeping the old rules, it maximized the number of representatives from heavily LDP rural districts.

Increasingly, LDP leaders recognized that delaying reapportionment was a bad strategy for the long term. Sooner or later, the LDP would have to create a new identity as a party for urban consumers. Many in the rank-and-file, particularly Diet members from the rural districts, fought this change. Into the 1980s there was internal turmoil between the leaders, who eventually would lose their power if the party did not reposition itself, and the rank-and-filers who would immediately lose their jobs if it did (Ramseyer and Rosenbluth 1997, chap. 3).

Faced with challenges to the existing apportionment schemes, the Supreme Court wrote opinions that generally tracked the positions of LDP leaders. During the first period, it rejected challenges to the rural overrepresentation. In the 1979 case of *Kurokawa v. Chiba*,<sup>10</sup> however, it switched sides. By this point, the LDP

<sup>8</sup> The data and STATA programs for the regressions are available at Ramseyer and Rasmusen 2001a and at the *APSR* web site, linked to our abstract.

<sup>9</sup> Three of them wrote two opinions each. To avoid improperly weighting judge-specific effects for these judges in our regressions, we include dummy right-hand-side variables for each.

<sup>10</sup> *Kurokawa v. Chiba ken senkyo kanri iinkai*, 808 Hanrei jihu 24

leaders were pushing the party to jettison the agricultural vote. The plaintiffs in *Kurokawa* claimed that some votes counted five times as heavily as others, and the Court held the apportionment unconstitutional. In the process, it helped the LDP leaders who otherwise would have found it harder to force LDP Diet members to redistrict themselves potentially out of a job. In 1985, in *Kanao v. Hiroshima*, the Court reiterated the point: Rural overrepresentation was unconstitutional.<sup>11</sup> By then, LDP leaders were solidifying the party's position as an urban party and abandoning the farmers to the socialists and communists. Again, the Supreme Court strengthened their hand.

Given this shift in the position of the LDP leadership and the Supreme Court, one would not expect the Secretariat always to punish judges for holding apportionment rules unconstitutional.<sup>12</sup> Instead, one would expect judges to be punished only if they either (1) held an apportionment scheme unconstitutional before the lower court opinions in *Kurokawa* (1974) and *Kanao* (1984) or (2) held an apportionment scheme unconstitutional and found that opinion reversed on appeal.

To test these hypotheses, we begin with the 69 lower court opinions that raised the propriety of electoral apportionment schemes, whether on constitutional or statutory grounds.<sup>13</sup> By law, most electoral challenges begin at the intermediate appellate level, so the judges in this data set were already in somewhat prestigious positions at the time of their decisions. Among the cases, 54 involved challenges to national elections and 15 to local elections. We coded the cases according to whether they invalidated the apportionment scheme, antedated *Kurokawa* (1974) or *Kanao* (1984), involved the local or national government, and were reversed on appeal.<sup>14</sup>

**Injunctions against the Government.** In administrative litigation, a plaintiff who can show the potential for irreparable harm can obtain a preliminary injunction

that stops the government from doing the harmful action at least until the underlying lawsuit is finally decided.<sup>15</sup> Do judges risk their careers in granting such injunctions?

One would not expect careers to be jeopardized simply for deciding routine administrative cases against the government. As noted earlier, to stay in office the LDP not only must enact the policies voters want but also must deliver them, and for that it needs dependable bureaucrats. Yet, bureaucrats can shirk their job and ignore instructions. Although LDP politicians maintain staffs that provide some bureaucratic interventionist services directly, they do not necessarily want to intervene in every tax audit and taxi license revocation. To discipline bureaucrats in these more mundane—or politically delicate—disputes, they allow citizens to sue them (McCubbins and Schwartz 1984). For that mechanism to work, in turn, they need a cohort of relatively unbiased judges.

In several crucial ways, the LDP facilitates legal challenges in mundane administrative cases. First, Japanese “standing” rules generally permit challenges to bureaucratic decisions that are too minor to warrant direct intervention by the LDP or the local Diet representative. Second, to ensure an impartial forum in these ordinary administrative disputes, the Secretariat does not punish judges simply for favoring plaintiffs who challenge the government (Ramseyer and Rasmusen 1999b). Third, because accuracy matters if the courts are to monitor the bureaucracy, the Secretariat punishes judges whose decisions are reversed on appeal by the higher courts (Ramseyer and Rasmusen 1999b).

Injunctions against the government, however, can be decidedly nonroutine. It is one thing to hold that a taxpayer owes only  $X$  in back taxes rather than the  $2X$  dishonestly claimed by a bureaucrat trying to fill a quota. It is quite another to block government policy. Because national bureaucrats answer to the cabinet, if LDP leaders want a national agency to stop doing action  $Y$ , they can simply tell it to stop and fire the agency head if it does not. In cases important enough to prompt politicians to intervene, a court that orders an agency to desist from doing  $Y$  thus directly jeopardizes LDP-mandated policy, since the judge cannot be fired. Therefore, one might plausibly suspect that judges who readily enjoin the national government jeopardize their career.

By the same logic, a judge would not face this threat for enjoining local governments. During the 1960s, the LDP increasingly lost control at this level. By 1975, only 12.5% of mayors ran on an exclusively LDP ticket.<sup>16</sup> As a result, even if the Secretariat punished judges for enjoining LDP policy, we should not observe the punishment among judges who enjoined local governments.

To test these hypotheses, we coded all published

(Sup. Ct. Apr. 14, 1979) (en banc), *rev'g* 30 Saihan minshu 288 (Tokyo High Ct. April 30, 1974); see Haley 1998, 179–80.

<sup>11</sup> *Kanao v. Hiroshima ken senkyo kanri iinkai*, 1163 Hanrei jiho 3 (Sup. Ct. July 17, 1985 (en banc), *aff'g* 1134 Hanrei jiho 27 (Hiroshima High Ct. September 28, 1984). The Court needed more than one opinion to make the point forcefully because of the fact-specific nature of the problem. The Court did not require that every vote have exactly the same effect, so several opinions were needed to clarify just how much variation in electoral power it would allow.

<sup>12</sup> And it did not. When we created a general variable equaling 1 if a judge held any apportionment scheme improper and ran a regression with the same control variables as in Table 4, we obtained coefficients and standard errors of  $-.040$  (0.17) (for the bad jobs afterward regression) and  $.018$  (0.09) (for the good jobs afterward regression); the effect on careers of striking down rural overrepresentation, averaged over the entire period, is nil.

<sup>13</sup> This data set includes 89 judges. Eight of them wrote two opinions. As we do elsewhere for judges who wrote multiple controversial opinions, we use judge-specific dummy variables.

<sup>14</sup> Among the observations in our sample, 2% are invalidations before 1974 and 31% after, with 67% being opinions that uphold the apportionment. Seven percent are invalidations before 1984 and 26% after. Four percent are invalidations that were reversed, and 1% are validations that were reversed. This sample is composed of more senior judges than those in the Article 9 sample (a mean of 23.01 years compared to 13.12 years of seniority), who have commensurately better initial jobs.

<sup>15</sup> *Gyosei jiken soshu ho*, Law No. 139 of 1962, § 25.

<sup>16</sup> Ramseyer and Rosenbluth 1997, 48, Table 3.3. This was a factor that led LDP leaders to reposition the party away from the overrepresented rural districts.

administrative cases from 1961 to 1970 in which a petitioner demanded a preliminary injunction. We restricted observations to one decade to limit the potential length of time between multiple injunctions issued by any one judge (the career effect of two injunctions over thirty years should be quite different from two over ten years) and because injunctions were common enough to provide a good-sized sample even over one decade. We used cases from the 1960s because judges issued fewer preliminary injunctions thereafter, which was a reasonable response to what we will show below.

This process generated a data set of 130 judges.<sup>17</sup> If a judge handled several injunctive petitions, we coded his career by the most recent year in which he granted an injunction. We located only five opinions in which a higher court reversed the grant of an injunction, so we did not include a reversals variable. For each judge, we counted the number of national and local injunctions granted and the number denied (our key independent variables).

**Political Affiliation and Pay.** Does the punishment against political nonconformists extend to salaries? During their first ten years on the bench, Japanese judges climb through the twelve steps of the assistant judge pay scale, which ranged as of 1989 from 190,600 to 405,600 yen per month. During the rest of their career, they move through another nine steps—from 494,000 yen (step 8), to 912,000 (step 3), to 1,115,000 (step 0). Although the Constitution protects them from explicit pay cuts, the Secretariat need not promote all judges at the same rate. If it is unhappy with a judge's work, it need not grant any promotion whatsoever.

Although a judge's salary is confidential, it correlates with certain observable indices. Most important, according to some observers a judge can serve as *sokatsu* (an administrative post with some personnel responsibilities) only after reaching step 3 (Netto 1995, 204). If so, then the time from initial appointment to this assignment will reflect, however imperfectly, the amount of time taken to reach step 3 in pay.

In our data set are all judges hired between 1959 and 1968. To control for unobservable differences among the cohorts, we added dummy variables indicating the year in which a judge finished education at the LRTI. As the dependent variable, we used the time it takes a judge to reach first *sokatsu* appointment. See Appendix B.

## THE VARIABLES

### Dependent Variables

*Good Jobs Afterward* is the percentage of the decade after a potentially controversial opinion that a judge spends in prestigious appointments (as chief judge, with *sokatsu* responsibilities, or in another administrative post). *Bad Jobs Afterward* is the percentage of the

decade after the opinion that a judge spends in a branch office (other than the relatively desirable Hachioji office in suburban Tokyo).

Because these dependent variables are censored, only taking values between 0 and 100, we use tobit rather than ordinary least squares (OLS) in our regressions (using the program Stata 5). We use one-tailed tests throughout, since we hypothesize that antigovernment behavior hurts careers. We use linear specifications, but as diagnostics for robustness we report for each regression whether the significance of any of the opinion variables changes if (1) the observations with the three largest residuals in the reported regression are dropped, (2) a log-linear specification is used, or (3) a log-log specification is used (where  $\log(1+x)$  rather than  $\log(x)$  is used because of the many zeroes in our data).

Note that the variables good jobs afterward plus bad jobs afterward will not sum to 100 for an individual judge, because not all jobs are good or bad. Most are mediocre. Our interest is in carrots and sticks, not benign neglect.

*Time to Sokatsu* is the number of years after a judge's graduation from the LRTI to first appointment as *sokatsu*. Because this dependent variable is uncensored, we use OLS for the relevant regression.

### Opinion Variables

**The Constitutionality of the Military.** *SDF Unconstitutional* is coded 1 if a judge held either the SDF or U.S. bases unconstitutional, 0 otherwise.

**Malapportionment.** *Invalidation before 1974* is coded 1 if a judge held a national apportionment scheme illegal before the 1974 trial court opinion in *Kurokawa*, 0 otherwise.<sup>18</sup> *Invalidation after 1974* is coded 1 if a judge held a national apportionment scheme illegal in or after the 1974 trial court opinion in *Kurokawa*, 0 otherwise. *Invalidation before 1984* is coded 1 if a judge held a national apportionment scheme illegal before the 1984 trial court opinion in *Kanao*, 0 otherwise. *Invalidation after 1984* is coded 1 if a judge held a national apportionment scheme illegal in or after the 1984 trial court opinion in *Kanao*, 0 otherwise. *Invalidation Reversed* is coded 1 if the Supreme Court reversed a judge's opinion that held a national apportionment scheme illegal, 0 otherwise. *Validation Reversed* is coded 1 if the Supreme Court reversed a judge's opinion that held a national apportionment scheme legal, 0 otherwise.

<sup>17</sup> By contrast, from 1971 through 1980, there were only 20 reported district court opinions that granted preliminary injunctions; from 1981 through 1997, there were 17.

<sup>18</sup> We also ran the first four regressions in Table 2 using a variable that combined judges involved in all apportionment challenges, whether local or national. Because many local electoral schemes benefited parties other than the LDP, one would expect the punishment effect to be less pronounced. The coefficients and standard errors on a variable equal to 1 if the judge held improper any (national or local) pre-*Kurokawa* apportionment scheme were 1.32 (0.47) (bad jobs afterward) and  $-.689$  (0.33) (good jobs afterward); for pre-*Kanao* apportionment schemes, they were .453 (0.24) (bad jobs afterward) and  $-.046$  (0.15) (good jobs afterward).

**Injunctions against the Government.** *National Injunctions Granted* is the number of injunctions against the national government granted by a judge during 1961–70. *National Injunctions Denied* is the number of injunctions against the national government denied by a judge during 1961–70. *Local Injunctions Granted* is the number of injunctions against a local government granted by a judge during 1961–70. *Local Injunctions Denied* is the number of injunctions against a local government granted by a judge during 1961–70.

**Control Variables**

*Good Jobs Before* is like the variable good jobs afterward, but for the decade before the opinion (this captures various otherwise unobserved information about the judge). *Bad Jobs Before* is like the variable bad jobs afterward, but for the decade before the opinion. *Seniority* is the number of years between the opinion and the year a judge graduated from the LRTI. *Flunks* is the number of times a judge failed the LRTI entrance exam (the pass rate varied between 1% and 4%), which is an inverse proxy for intelligence and work habits. *Elite College* is coded 1 if a judge graduated from either of the two most prestigious universities (Tokyo or Kyoto), 0 otherwise. This is a proxy for intelligence and work habits that also captures any old-school ties. *Opinions per Year* is a judge’s average productivity for the decade before the opinion, as measured in published opinions per year on the bench. This also is a proxy for intelligence and work habits. *YJL* is coded 1 if a judge was a member of the YJL as of 1969, 0 otherwise. *Tokyo Start* is coded 1 if a judge began his career at the Tokyo District Court (a mark of fast-track status), 0 otherwise.

**RESULTS**

**Control Variables**

In all regressions below, when the control variables are significant, they have the predicted signs. Consistently, they reflect both the meritocratic organization of the Japanese courts, and the use of career incentives to reduce judicial shirking. The regressions are presented in tables 1–4. The variable good jobs before (which captures otherwise unobserved information about a judge’s status) has a positive effect on the variable good jobs afterward. The more administrative responsibilities a judge had before deciding a controversial case, the more he had afterward (Table 1, regression 1.2, and Table 3, regression 3.2).

Seniority has a positive effect on the variable good jobs afterward and a negative effect on the variable bad jobs afterward. Administrative responsibilities tend to go to the more senior judges, and branch office assignments go to the younger judges (Table 1, regression 1.2; Table 2, regressions 2.2, 2.4, 2.6; Table 3, regressions 3.1, 3.2).

The variable flunks is negatively correlated with the variable good jobs afterward. Judges who fail the LRTI exam the fewest times (the smartest and the hardest

**TABLE 1. Job Quality by Article 9 (Military) Opinions and Control Variables (Tobit Regressions)**

Independent Variable	Dependent Variable	
	(1.1) Bad Jobs Afterward	(1.2) Good Jobs Afterward
Unconstitutional	-.028 (.208)	-.397 (.223)*
Good Jobs Before		.729 (.344)*
Bad Jobs Before	-.073 (.299)	
Seniority	.011 (.009)	.019 (.011)*
Flunks	.026 (.018)	-.018 (.020)
Elite College	-.256 (.119)*	.201 (.113)*
Tokyo Start	.071 (.157)	.231 (.143)
Opinions per Year	-.100 (.042)*	-.023 (.015)
YJL	.140 (.253)	.099 (.289)
Intercept	.177 (.179)	.061 (.196)
Pseudo R <sup>2</sup>	0.46	0.55
Standard error	0.29	0.31
Diagnostics (outliers, log-lin, log-log)	(c,c,c)	(c,s,c)
Censoring ( $y < 0$ , unc., $y > 1$ )	(26,24,0)	(11,34,5)

Note: N = 50. Coefficients are followed by standard errors in parentheses. \*p < .05; one-tailed tests. These regressions include dummies for the three judges with multiple opinions, but the coefficients are not reported. Diagnostics show whether the significance of unconstitutional is confirmed (c) or switched (s). See page 337 for details.

working) have the most administrative responsibilities, even beyond the effect this has on their career before the controversial case (Table 3, regression 3.2).

The variable elite college has a positive effect on the variable good jobs afterward and a negative effect on the variable bad jobs afterward. Judges from the prestigious universities of Tokyo and Kyoto spend the most time in administrative roles and the least time in branch offices—again, beyond the effect their education has on their career before the decision (Table 1, regressions 1.1, 1.2; Table 2, regressions 2.1, 2.3, 2.5; Table 3, regressions 3.1, 3.2).

The variable Tokyo start has a negative effect on the variable time to sokatsu. Judges identified as the most promising at the outset spend the most time at administrative jobs and climb the pay scale the most rapidly, as shown in Table 4.

The variable opinions per year has a positive effect on the variable good jobs afterward and a negative effect on the variable bad jobs afterward. Judges who publish the most opinions spend the most time in administrative jobs



**TABLE 2. Job Quality by Malapportionment Opinions and Control Variables (Tobit Regressions)**

Independent Variable	Dependent Variable					
	(2.1) Bad Jobs Afterward	(2.2) Good Jobs Afterward	(2.3) Bad Jobs Afterward	(2.4) Good Jobs Afterward	(2.5) Bad Jobs Afterward	(2.6) Good Jobs Afterward
Invalidation before 1974	1.36 (.470)**	-.710 (.330)*				
Invalidation after 1974	-.018 (.163)	.018 (.094)				
Invalidation before 1984			.607 (.271)*	-.243 (.173)		
Invalidation after 1984			-.173 (.204)	.036 (.104)		
Invalidation Reversed					.980 (.368)**	-.380 (.224)*
Validation Reversed					.448 (.505)	-.199 (.389)
Good Jobs Before		-.053 (.212)		-.016 (.214)		-.026 (.212)
Bad Jobs Before	-.129 (.363)		-.183 (.392)		-.258 (.386)	
Seniority	.010 (.011)	.020 (.010)*	.015 (.012)	.020 (.010)*	.013 (.012)	.020 (.010)*
Flunks	.001 (.029)	-.017 (.020)	-.010 (.033)	-.016 (.020)	-.014 (.033)	-.016 (.020)
Elite College	-.389 (.176)*	.135 (.100)	-.276 (.179)	.111 (.102)	-.339 (.178)*	.120 (.101)
Tokyo Start	.191 (.207)	-.107 (.127)	.218 (.222)	-.111 (.129)	.241 (.212)	-.108 (.014)
Opinions per Year	-.084 (.033)**	.031 (.014)*	-.096 (.040)*	.027 (.014)*	-.100 (.040)*	.029 (.014)*
YJL	-.174 (.272)	.030 (.142)	-.116 (.296)	.029 (.146)	-.131 (.285)	.031 (.144)
Intercept	.003 (.282)	-.036 (.207)	-.069 (.302)	-.043 (.211)	.006 (.296)	.047 (.204)
Pseudo $R^2$	0.33	0.30	0.31	0.29	0.33	0.29
Standard error:	0.45	0.36	0.48	0.37	0.47	0.37
Diagnostics (outliers, log-lin, log-log)	(c,c,c)	(c,c,c)	(c,c,c)	(c,c,c)	(c,c,c)	(c,s,s)
Censoring ( $y < 0$ , unc., $y > 1$ )	(71,24,1)	(13,64,19)	(71,24,1)	(13,64,19)	(71,24,1)	(13,64,19)

Note:  $N = 97$ . Coefficients are followed by standard errors in parentheses. \* $p < .05$ , \*\* $p < .01$ ; one-tailed tests. These regressions include dummies for the 8 judges with multiple opinions, but the coefficients are not reported. Diagnostics show whether the significance of the opinion variables is confirmed (c) or at least one switched (s). See page 337 for details.

and the least time in branch offices (Table 1, regression 1.1; Table 2, regressions 2.1–2.6).<sup>19</sup>

### Article 9: The SDF

The best-known constitutional dispute in Japan is the argument over Article 9 of the Constitution. The regressions in Table 1 measure the career effects of a judge's decision on that issue. Crucially, the coefficient on the variable unconstitutional in the good jobs afterward regression (1.2) is negative and significant at  $-.397$ . Compared to judges who ruled the SDF or U.S. bases constitutional (or who ducked the issue), those who held either of them unconstitutional received fewer prestigious administrative duties in the decade after the opinion.

The effect of the variable unconstitutional coefficient on the variable bad jobs afterward is not statistically significant. To punish a judge, the Secretariat can use

either longer assignments to branch offices or shorter administrative appointments, and we have no theory about when it uses one or the other. Here, it seems to have cared more about keeping erring judges out of administrative positions.

### Malapportionment

Table 2 confirms our hypothesis about the malapportionment cases. Before the switch in position of the LDP leadership (and the Supreme Court) on the issue, judges who upheld the constitutionality of national apportionment rules did better than those who struck them down; after that switch, the effect disappeared. Judges who held a national apportionment scheme improper during the years when the LDP depended on the rural vote were punished: The variable invalidation before 1984 in regression 2.3 is significant and has a positive effect on the variable bad jobs afterward.<sup>20</sup>

<sup>19</sup> Ramseyer and Rasmusen (1997, 272) explain why a low opinions-per-year score is more properly a cause of inferior assignments than a result.

<sup>20</sup> The effect is even stronger for the very earliest opinions (invalidation before 1974), but we do not base our analysis on that because of the small number of observations.

**TABLE 3. Job Quality by Injunctions and Control Variables (Tobit Regressions)**

Independent Variable	Dependent Variable	
	(3.1) Bad Jobs Afterward	(3.2) Good Jobs Afterward
National Injunctions Granted	-.038 (.073)	-.119 (.070)*
National Injunctions Denied	-.132 (.065)*	.076 (.040)*
Local Injunctions Granted	.051 (.057)	-.003 (.050)
Local Injunctions Denied	.075 (.073)	.046 (.067)
Good Jobs Before		1.074 (.257)**
Bad Jobs Before	.207 (.220)	
Seniority	-.035 (.008)**	.045 (.008)**
Flunks	.009 (.013)	-.039 (.013)**
Elite College	-.158 (.081)*	.127 (.071)*
Tokyo Start	-.114 (.145)	.056 (.127)
Opinions per Year	.021 (.027)	-.026 (.025)
YJL	.031 (.112)	-.116 (.127)
Intercept	.371 (.115)**	-.223 (.110)*
Pseudo R <sup>2</sup>	0.22	0.44
Standard error	0.37	0.34
Diagnostics (outliers, log-lin, log-log)	(c,c,c)	(c,s,s)
Censoring (y < 0, unc., y > 1)	(64,66,0)	(61,65,4)

Note: N = 130. Coefficients are followed by standard errors in parentheses. \*p < .05, \*\*p < .01; one-tailed tests. Diagnostics show whether the significance of the opinion variables is confirmed (c) or at least one switched (s). See page 337 for details.

Unlike the Article 9 cases, here the punishment seems to have operated mainly through more branch office time rather than less time in prestigious postings. As expected, for the years after 1984, when the LDP leadership decided to jettison its rural base, we find no evidence of any punishment against judges who held the schemes improper: The variable invalidation after 1984 is insignificant.

Table 2 also illustrates a related phenomenon. If the Supreme Court reversed an opinion that invalidated an apportionment scheme, the judge spent more time in branch offices and less time with administrative responsibilities.<sup>21</sup> The variable invalidation reversed in regressions 2.5 and 2.6 has a significant positive effect on the variable bad jobs afterward and a negative effect on the variable good jobs afterward. Note that a judge suf-

<sup>21</sup> On why the punishment is not just a bureaucratic response to those who went against the Supreme Court, see the discussion in Ramseyer and Rasmusen 1999a.

ferred no significant penalty for having the validation of an apportionment scheme reversed.

These regressions help resolve an issue that the Article 9 regressions could not address. A possible explanation for the Article 9 regressions is that the Secretariat was simply run by judges who were conservative both in politics and judicial temperament. As a result, they hesitated to use Article 9 to weaken national defense, even though they were completely independent from LDP pressure. That disposition, however, would not explain a Secretariat indifferent to rural overrepresentation until the mid-1970s or 1980s but bothered by it thereafter. The timing suggests that the courts were following the LDP leadership.

**Preliminary Injunctions**

Tale 3 confirms that judges who enjoin the national government jeopardize their career. The coefficient on the variable national injunctions granted in regression 3.2 is negative and significant: Judges who enjoined the national government received fewer administrative responsibilities over the ensuing decade. This contrasts sharply with the fate of judges who denied injunctions. The coefficient on the variable national injunctions denied is positive in regression 3.2 and negative in regression 3.1; judges who refused to issue injunctions against the national government received additional administrative assignments and spent less time in branch offices. The results also confirm the predicted distinction between national and local bureaucrats. In both regressions 3.1 and 3.2, the granting or denial of injunctions against local governments had no significant effect on a judge's career.

These regressions also help distinguish among different explanations for judicial behavior. First, the Secretariat is not judicially conservative, unwilling to thwart the actions of elected officials or to intervene in political issues. Rather, it seems to be conservative with respect to the national government and neutral with

**TABLE 4. 1959–68 Judges: Time to Sokatsu Promotion by YJL Membership and Control Variables (OLS)**

Independent Variable	Dependent Variable: Time to Sokatsu
YJL	.919 (.538)*
Tokyo Start	-1.383 (.804)*
Flunks	.014 (.071)
Elite College	.086 (.516)
Intercept	22.905 (.785)**
Diagnostics (outliers, log-lin, log-log)	(c,s,s)
R <sup>2</sup>	.11

Note: N = 501. Coefficients are followed by standard errors in parentheses. \*p < .05, \*\*p < .01; one-tailed tests. Dummies for the year the judges were hired were included but are not reported. Time to Sokatsu is the number of years to the judge's first sokatsu appointment from the time he graduated from the LRTI. Diagnostics show whether the significance of YJL is confirmed (c) or switched (s). See page 337 for details.

respect to the local government—just as the LDP would have wanted. Second, the explanation for punishment cannot lie exclusively in whether judges' decisions were reversed. Some of those punished in the Article 9 and malapportionment samples were indeed reversed on appeal, and we know from other studies that reversals can hurt careers. That is not, however, what occurred in these injunction cases. Instead, the explanation again lies in politics.

### Political Affiliation and Pay

Table 4 reports the results of our investigation into the effect of political affiliation on pay. Notably, the coefficient on the variable YJL is positive and significant. Judges who joined the league in the 1960s received their first sokatsu assignment a year later than their peers. If, as observers claim, the appointment signals promotion to step 3 on the pay scale, then YJL members did indeed climb the pay scale more slowly than their peers.

One caution is that the variable YJL becomes insignificant if logarithmic specifications are used. This suggests that perhaps the significance in the basic regression (which is confirmed in the diagnostic regression with outliers dropped) is due to a long delay in sokatsu for a relatively small number of judges in this large sample, an effect that is larger in a linear specification than a logarithmic one.

### CONCLUSION

To test whether Japanese politicians indirectly influence judges, we used newly assembled data and a multivariate approach. We asked whether judges who flout the ruling party in politically volatile cases pay a career penalty.<sup>22</sup> They do. In the case of SDF constitutionality, judges who sided with the LDP received better assignments than those who did not. In the

apportionment debate, those who held existing schemes valid during the time when the LDP relied on overrepresented rural districts did better than those who did not. Judges who granted injunctions against a national agency did worse than those who denied them. The politically biased penalties even extended to money: Judges who joined the leftist YJL climbed the pay scale more slowly than their more conservative peers.

Granted, very few judges flouted the LDP. Of the 47 judges who ruled on Article 9, only five held the military unconstitutional. Among the 89 judges who addressed electoral districting, only 7 struck down the rules before the mid-1980s. The small number of judges who flouted LDP policies raises the question of whether those who did shared some characteristic correlated with unsuccessful careers. In fact, however, this problem is less severe than it might appear. In our controls, we use variables (good jobs before and bad jobs before) that largely capture the effect of any omitted variables correlated with career success. Because we look not for bad careers but for careers that become worse after a decision, omitted variables would explain our results only if they were correlated with events that occurred the same year as the decision in question.

Moreover, precisely because of the small number of heterodox judges in some of our samples, we repeated the tests on a variety of independent data sets. In all politically sensitive sets of cases, we obtained the results we predict. In related studies, we find significant punishment of judges who held unconstitutional the ban on door-to-door canvassing, who acquitted criminal defendants on formalistic grounds, or whose tax opinions were reversed on appeal (Ramseyer and Rasmusen 1997, 1999b, 2001b). Indeed, not all our tests involve small numbers of judges anyway. Among the 130 who ruled on petitions for preliminary injunctions, 42 enjoined the national government. Among the 500 judges hired between 1959 and 1968, 140 had joined the leftist bar association. To us, this uniformity across a wide variety of data sets suggests that we capture far more than a statistical anomaly. We capture indirect political manipulation.

<sup>22</sup> It goes without saying that the vast majority of cases in any legal system have nothing to do with electoral politics. The indirect political manipulation involved in these politically charged cases says nothing about potential bias in private law cases or even (see Ramseyer and Rasmusen 1999b) routine cases involving the government.

**APPENDIX A: SUMMARY STATISTICS**

Variable	Minimum	Median	Mean	Maximum
<b>A. Article 9 (Military) Data Set (N = 50)</b>				
Good Jobs Afterward	0	0.39	0.44	1
Bad Jobs Afterward	0	0	0.17	0.92
Unconstitutional	0	0	0.12	1
Good Jobs Before	0	0	0.22	0.93
Bad Jobs Before	0	0	0.09	0.8
Seniority	2	11	13.12	29
Flunks	0	3	3.84	12
Elite College	0	1	0.56	1
Tokyo Start	0	0	0.24	1
Opinions per Year	0	2.23	3.36	29.75
YJL	0	0	0.12	1
<b>B. Malapportionment Data Set (N = 97)</b>				
Good Jobs Afterward	0	0.64	0.60	1
Bad Jobs Afterward	0	0	0.10	1
Invalidation before 1974	0	0	0.02	1
Invalidation after 1974	0	0	0.31	1
Invalidation before 1984	0	0	0.07	1
Invalidation after 1984	0	0	0.26	1
Invalidation Reversed	0	0	0.04	1
Validation Reversed	0	0	0.01	1
Good Jobs Before	0	0.31	0.39	1
Bad Jobs Before	0	0	0.12	0.73
Seniority	5	24	23.01	39
Flunks	0	2	3.00	9
Elite College	0	0	0.46	1
Tokyo Start	0	0	0.20	1
Opinions per Year	0	4.85	5.88	22.22
YJL	0	0	0.12	1
<b>C. Preliminary Injunctions Data Set (N = 130)</b>				
Good Jobs Afterward	0	0.07	0.26	1
Bad Jobs Afterward	0	0.09	0.21	0.9
National Injunctions Granted	0	0	0.37	3
National Injunctions Denied	0	0	0.50	7
Local Injunctions Granted	0	0	0.40	4
Local Injunctions Denied	0	0	0.40	2
Good Jobs Before	0	0	0.06	0.75
Bad Jobs Before	0	0	0.11	0.81
Seniority	2	8	9.42	22
Flunks	0	4	4.32	16
Elite College	0	0	0.46	1
Tokyo Start	0	0	0.09	1
Opinions per Year	0	1.49	2.07	10.67
YJL	0	0	0.12	1
<b>D. Sokatsu Data Set (N = 501)</b>				
Time to Sokatsu	11	21	21.71	37
YJL	0	0	0.28	1
Tokyo Start	0	0	0.10	1
Flunks	0	4	4.83	18
Elite College	0	0	0.32	1

## APPENDIX B

For our Table 4 regression, we made several further adjustments. First, in those rare cases in which a judge served as chief judge before serving as sokatsu, we treated appointment to the chief judgeship as the sokatsu posting. Such an appointment is unambiguously higher than a sokatsu post. Second, we dropped those judges who held nonjudicial postings (generally regarded as prestigious) within two years before their first sokatsu posting. A judge in such a post could not have held sokatsu duties but might well have been at step 3 already, and including the time to first sokatsu in the database would exaggerate the time required for his promotion.

Of course, by dropping such judges entirely we bias our data. Since the judges were among the more successful in their cohort, removing them from the sample exaggerates our estimate of the group's true time to sokatsu. Crucially, however, Ramseyer and Rasmusen (1997) find that YJL members did not receive these prestigious administrative responsibilities as often as their peers. As a result, dropping these star judges disproportionately drops non-YJL judges. In the process, we understate the success of the non-YJL judges and concomitantly bias our data against finding anti-YJL discrimination.

Third, we dropped judges who never obtained a sokatsu appointment but who quit or died before the mean time to first sokatsu for the rest of the group (20.41 years). This omitted another 164 judges. Of these, 23.8% were YJL members.

Fourth, judges who quit or died after 20 years without a sokatsu appointment were kept in the database, but the death or resignation was treated as the first sokatsu appointment. We applied this procedure to 83 judges, 33.7% of whom were YJL members.

Although this last adjustment also biases the data, it again biases it against finding anti-YJL discrimination. The fact of quitting during the first 20 years of a career tells us little about how well a judge was doing. By contrast, those who quit at a time when many of their peers are serving as sokatsu but they are not can be considered relatively unsuccessful. Yet, by treating their resignation as equivalent to a sokatsu appointment, we overstate their professional success. Because they never received a sokatsu appointment, the length of their career will necessarily be shorter (or equal to) the length of time it would have taken them to reach sokatsu had they not quit. Crucially, this group of unsuccessful judges included a disproportionately high fraction of YJL members (33.7%). By making this adjustment, we therefore underestimate the true time to first sokatsu for YJL members.

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