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Judicial Independence in Civil Law Regimes: Econometrics from Japan

J. Mark Ramseyer and Eric B. Rasmusen[*]

I. Introduction

Because civil law courts hire unproven jurists into career judiciaries, many use elaborate incentive structures to prevent their judges from shirking. In Japan, for example, the courts maintain an administrative office called the Secretariat that regularly monitors and evaluates each judge. On the basis of those evaluations, the Secretariat assigns judges to new posts every three years. Because not all posts are created equal, at least hypothetically it could use this rotation system to influence judges toward a variety of ends: it could use it to induce judges to work hard, for example, or use it to induce them to follow a political orthodoxy.

In the article that follows, we use data from Japan to discover both the general determinants of judicial career success, and the actual extent of political manipulation. Toward that end, we assemble career data on all 274 judges hired from 1961 to 1965. Within this data base, we find strong evidence that the Secretariat rewards the smartest and most productive judges. Contrary to some observers, we find no evidence of ongoing school cliques, and no evidence that it favors judges who mediate over those who adjudicate.

More controversially, we also find three signs that political considerations influence the careers of sitting judges. First, even as late as the 1980s, those judges who joined a prominent leftist organization in the 1960s were receiving less attractive jobs. Second, whenever a judge decided a case against the government, he incurred a significant risk that the Secretariat would soon punish him with a less attractive post. Finally, those judges who decided a high percent-

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age of cases against the government early in their careers received less attractive jobs than their peers in 1980s.

We begin by outlining the theoretical and empirical literature on judicial independence (Section 2). We then explain the structure of Japanese courts (Section 3) and the nature of our data base (Section 4). Using an ordered probit model, we first explore the determinants of a judge's initial job posting (Section 5.1), and of his posts later in his career (Section 5.2). With more extensive data on the class of 1965, we then test whether a judge who decides cases against the government receives less attractive posts (Section 6). Finally, we turn to the most common anecdotes of a link between the political content of a judge's decisions and his career success: cases involving the constitutionality of the ban on door-to-door canvassing (Section 7).

2. CIVIL-LAW SYSTEMS AND JUDICIAL INDEPENDENCE

2.1. Manipulability. To understand the potential manipulability of civil law courts, consider first—by way of contrast—the U.S. federal courts. To them, the president generally appoints only prominent middle-aged lawyers. Most have proven themselves both politically loyal and congenitally workaholic. He appoints these men and women to particular posts in particular towns. There, they hear cases for the rest of their working lives.

Sometimes, a federal judge will move from the district court to the court of appeals. Sometimes, a Stephen Breyer will move from the court of appeals to the Supreme Court. A William Webster will move from the courts to the FBI. Otherwise, a typical judge never moves out of town, never changes jobs, never earns a raise except in lockstep with every other federal judge.[1] For most federal judges, how they do their job will have little effect on tenure, advancement, or compensation.

Not so in many civil-law regimes. There, judges face just such threats. Often, they join the courts immediately upon passing the bar. Because they are young and unproven, the government has relatively little information about them. It will seldom know their political preferences. Perhaps more basic, neither will it know how

[1] State court judges may face greater mobility. Yet even they seldom worry that an administrator might move them from Los Angeles to Fresno, or demote them from an appellate court to traffic court.

hard or how fast they work. Instead, it will need to make do with proxies like exam performance.

Because of this limited information, in many civil law jurisdictions the government will set up elaborate monitoring and incentive systems to induce its judges not to shirk.[2] Toward that end, it will maintain a judicial administrative office. Through that office, it will regularly grade a judge's work and dispense rewards. The better the work, the more attractive a job it will give a judge and the more money it will pay him.[3]

Incentive structures, however, are manipulable. A government may introduce an institutional structure to induce effort but use it to enforce political loyalty. At least in theory, in many civil-law systems it could use the structure to reward judges by the political complexion of the judgments they issue and the opinions they write. The loyal it could ply with prestigious posts in attractive cities and a quick climb up the pay scale. The heterodox it could let languish at low pay in branch offices in the outback. In most cases, politics will not matter, for most cases involve no political issues of moment. In a few cases it will—and the question at stake is whether the government will manipulate incentives to shape decisions in those relatively unusual but sometimes vitally important cases.

2.2. Theory. Whether (or when) rational politicians will manipulate judicial incentives is an issue on which theory can go either way.[4] On the one hand, independent courts potentially solve several vexing political problems—and perhaps for those reasons remain perennially popular in ballot boxes and law reviews. First, they add

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[2] In common-law systems, responsible judging will be one equilibrium—but not the only one (Rasmusen, 1994). Presumably, where judges are appointed without track records, the risk of irresponsible judging is more severe.

[3] E.g., Merryman (1985: ch. 6); Clark (1988: 1840). Note, however, that one can overstate the contrast. Even in common-law systems, judges may have incentives to restrain their own idiosyncrasies. Rasmusen (1994); Easterbrook (1982: 817); Spiller & Gely (1994).

[4] Whether politicians are self-interested or public-interested is not a central issue here. It is by no means clear that an unselfish politician would necessarily prefer judicial independence; he does not want the judiciary to thwart the policies he proposes for the public good, whether because the judge disagrees or because that beneficial policy is truly unconstitutional.

credibility to governmental promises. Whether to maximize the rents it extracts (Landes & Posner, 1975) or to lower the cost of its debt (North & Weingast, 1989), a government will want to make its commitments credible. Subjecting its promises to the judgment of an independent court sometimes does just that.

Second, independent courts help police the bureaucracy (McCubbins & Schwartz, 1984). By giving disaffected citizens the right to sue a bureaucracy, a government can potentially obtain access to information about how well its bureaucrats perform. Armed with that information, it can potentially improve bureaucratic performance and its party's electoral odds.

Third, independent courts minimize a party's losses while out of power. To the extent judges are independent, they do not necessarily serve the party in control. That they do not, in turn, will often comfort out-of-power politicians and their electoral sympathizers (Ramseyer, 1994). To the extent politicians and their supporters expect sometimes to be out of power, therefore, they may rationally prefer courts that (by being independent) reduce the stakes to controlling the government.

On the other hand, independent courts introduce political problems of their own. Politicians do not maximize votes by promising desired policies, but rather by delivering desired policies. Independent judiciaries can obstruct that delivery.[5] Moreover, many politicians rationally take short-term perspectives. They care less about long-term credibility than about the next election. And the ruling party will always have a temptation to cheat on judicial independence in small ways. For the party in power, the ideal judges are those with a reputation for independence (thus making its promises credible and cowing the bureaucracy) who actually do as it says.

Ultimately, therefore, the extent to which politicians keep judges independent will depend on factors external to the courts. The more readily politicians can make their promises credible, the more cheaply they can monitor their bureaucrats, and the less likely they are to revert to minority status, the smaller their incentive to keep judges independent. Because they will always wish to pretend that the judges

[5] Though even independent judges may sometimes find that restraining their own behavior earns them returns of various sorts. See Rasmusen (1994); Spiller & Gely (1994); Spiller & Spitzer (1992).

are independent, however, any analysis of the actual outcome must depend on observing behavior rather than words.

2.3. Empirical Studies. Existing empirical studies do not tell us whether politicians in civil law systems keep their courts independent. Although several scholars have begun to publish sophisticated empirical analyses of judicial independence, they have studied only the comparatively hard-to-manipulate common-law systems (e.g., Spiller & Gely, 1992; Toma, 1991; Anderson, Shughart & Tollison, 1989; Cohen, 1991). Generally, they find some evidence, relatively weak, either that judicial institutional structures affect the political cast of what judges do, or that they respond—as Spiller and Gelly (1992: 489) nicely put it—"albeit quite indirectly, to interest-group and voter pressures."

Although civil law systems would seem to give more opportunities for political intervention, we know of no systematic econometric study of judicial independence in a civil-law environment. A few scholars have considered the relationship of civil-law judges to politicians. To date, though, they have used historical rather than quantitative approaches and emphasized crises rather than routine situations (e.g., Muller, 1991, on German judges under Nazi rule).

Even in Japan, where the debate has taken an aggressively political tone, most scholars have produced only anecdotal studies.[6] The most common accounts involve the ban in § 138 of the Public Offices Elections Act on door-to-door canvassing.[7] Because incumbents obtain free media coverage but challengers do not, the ban disproportionately benefits incumbents. During the postwar years, the Liberal Democratic Party (the LDP) had the most incumbents, so the ban disproportionately benefited the LDP.

From time to time, a few lower court judges insisted that the canvassing ban violated the constitutional guarantee of freedom of expression. According to the anecdotal accounts, they incurred penalties when they did (e.g., Ramseyer & Rosenbluth, 1993: ch. 9). Take Haruhiko Abe. He held § 138 unconstitutional in 1968, and by

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[6] For studies in English, see Hayakawa (1971); Miyazawa (1994); Ramseyer (1994); Ramseyer & Rosenbluth (1993: chs. 8-9). The best original empirical research in Japan is probably Sakaguchi (1988) and Tsukahara (1991).

[7] Köshoku senkyo hö [Public Offices Elections Act], Law No. 100 of 1950, § 138

1990 had spent 11 years in branch offices, far more than normal (see Table 1). Or take Masato Hirayu. He held § 138 unconstitutional in 1979, and by 1990 had spent 8 years in branch offices.

Yet anecdotal accounts are always subject to the criticism that they reflect special situations and not the norm, and different anecdotes suggest different conclusions. John Haley, dean of Japanese law scholars in the U.S., examined the evidence carefully in 1995 and concluded that Japanese courts remained fundamentally autonomous. Premier Japanese law and society scholar Setsuo Miyazawa did the same in 1994 and concluded the opposite. The question thus remains open.

3. THE JAPANESE COURTS

3.1. Appointment and Reappointment. As in most civil law systems, Japanese judges begin their judicial careers immediately upon passing the bar. They then stay judges for most of their working lives. By the early 1990s, the court system had 2,800-2,900 judges (see Table 1). These judges decided some cases alone, but most as 3-judge panels.

Formally, lower-court judges work a series of 10-year terms. The prime minister, who from 1955 to 1993 was from the modestly conservative LDP, legally has the power to determine both initial appointments and later reappointments. In fact, he usually defers on these matters to the Secretariat. Generally, he reappoints all sitting judges until they either resign or reach retirement age.

3.2. Postings. During their careers of thirty-odd years, Japanese judges move through a variety of posts. These posts vary along several dimensions. First, they vary by geography. The Secretariat can—and does—routinely move judges from city to city.

Second, the posts vary along the judicial hierarchy. The Secretariat can—and again does—bounce judges up and down the hierarchy from the high courts (the courts of appeals) to the district courts to the family courts (hearing cases involving, divorces, juveniles, guardians, etc.), to the branch offices of the district and family courts. Note that the less prestigious postings are not extraordinary occurrences, and do not necessarily signal disgrace (see Table 2). As a result, a judge who moves to a worse posting may have—but has not necessarily—been identified for special treatment.

TABLE 1
Background Statistics

A. Selected Summary Statistics, Classes of 1961-65

	Mean	Minimum	Maximum
Starting Age	28.73	24	38
Sex	.96	0	1
Tokyo University	.16	0	1
Kyoto University	.19	0	1
Chuo University	.14	0	1
No University	.43	0	1
Opinions	2.02	.04	10.42
First Post	.91	0	3
1980s Post	1.83	0	3
First Location	1.00	0	3
1980s Location	1.06	0	3
YJL	.35	0	1
Percent Early Antigovt.*	9.64	0	100
Percent Late Antigovt.*	2.56	0	100

Observations: 275 for all except asterisked items, which are for the Class of 1965 only (55 observations).

Note: Variables are as defined in Section 4.3.

B. Aggregate postings, as of 1990

By Court Hierarchy		By Geography	
Secretariat	45	Tokyo	537
Other Non-judicial	96	Osaka	231
High Court	295	Other Metropolitan	416
District Court	1101	Non-metropolitan	1742
Family Court	541		
Branch Offices	848		
Sōkatsu	353		

Third, some posts involve prestigious administrative duties. The most successful judges become one of eight high court presidents. Modestly successful judges become district or family court chief judges. Almost all judges spend some time as a district judge with internal personnel responsibilities (a sökatsu assignment). And a few judges work several years in the Secretariat or at the Ministry of Justice. The Secretariat itself selects the judges who staff the Secretariat; it apparently negotiates the Ministry of Justice postings with the Ministry's own personnel office. Note too that visibility and influence do not completely overlap. A staff position within the Secretariat can be highly influential, even if not as visible as a seat on the high court.

The Secretariat can also promote judges along the pay scale at different speeds. By the constitution, it cannot cut a judge's pay. It can vary the rate of promotion, however, and critics have accused it of penalizing the politically heterodox by doing just that. Unfortunately, we lack judicial pay data and thus do not explore this issue.

3.3. Judicial Preferences. To determine the relative attractiveness of the various judicial appointments, we talked with a wide range of Japanese observers and looked at the careers of the most successful judges. Idiosyncratic preferences aside, most judges seem to prefer Tokyo posts to all others, and to prefer Osaka if they cannot be in Tokyo. They mildly prefer high and district court posts to family court posts, and strongly prefer all such posts to lower court branch offices. Most aspire to some administrative responsibilities.

In Table 2 we display the percentage of a judge's career spent in various assignments for two groups of judges: (a) the most successful judges (they eventually became Supreme Court justices or high court presidents), and (b) all those in the cohort of judges who began their careers in 1965, whether successes and failures, who had not retired or left the judiciary early (before 1990). Note that the most successful judges spend more time in Tokyo and Osaka, more time in the Secretariat and other non-judicial posts (e.g., the Ministry of Justice), and less time in branch offices. They do not spend much more time as sökatsu than other judges, but this is because they more quickly move to higher administrative roles like chief judgeships.

TABLE 2:
EXCEPTIONAL JUDGES AND THE CLASS OF 1965

	Exceptional Judges			Class of 1965		
	Mean	Min.	Max.	Mean	Min.	Max.
Personal data:						
Starting Age	26.84	23	32	29.85	24	38
Sex	.96	0	1	.91	0	1
Tokyo University	.76	0	1	.2	0	1
Kyoto University	.12	0	1	.036	0	1
Chuo University	0.00	0	0	.2	0	1
No University	.04	0	1	.47	0	1
Opinions	4.02	0	16.5	1.75	.16	7.82
YJL	.08	0	1	.27	0	1
Percentage of career in various posts:						
Tokyo	.52	0	.89	.23	0	.88
Osaka	.14	0	.73	.068	0	.56
Sökatsu	.14	0	.33	.10	0	.4
Secretariat	.17	0	.58	.007	0	.12
Other Non-judicial	.12	0	.78	.083	0	.56
Branch Offices	.043	0	.19	.15	0	.58
Observations:	25			55		

Notes: The variables are as defined in Section 4.3. "Exceptional judges" are those judges who were named eventually either to the Supreme Court or to the presidency of a high court, and whose career records appear in the ZSKS. This rules out those appointed to these positions early in the postwar era, as they would have begun their careers prior to the 1948 and thus would not appear in the ZSKS. For purposes of deriving these figures, the time of appointment to the Supreme Court is treated as the time of retirement. The percentage postings figures give the percentage of career, as of 1990, spent in the various positions.

3.4. The Supreme Court and the Secretariat. The Japanese Supreme Court consists of fifteen justices who are appointed by the prime minister and serve until mandatory retirement at age 70. Among the last 20 justices, the mean age at appointment has been

64. The chief justice supervises the secretary general, the head of the Secretariat. Generally, at least one Supreme Court justice is himself a former secretary general.

Because the LDP appointed justices late in life, for most of the postwar years the Supreme Court included only recent appointees. By appointing them at age 64 with mandatory retirement at 70, the LDP effectively mitigated the "Harry Blackmun problem": the risk that a politically loyal appointee would evolve over time into a very different beast, and one who promoted his new agenda over that of his benefactor. In contrast to U.S. presidents, the LDP could safely appoint judges who would soon retire because it faced high odds (though less than 1, as it turned out) of staying in power.

By appointing older justices, the LDP probably also increased its stock of patronage capital. Suppose (as seems likely) that the marginal utility to a judge of a Supreme Court appointment declines over time. If so, then the LDP necessarily increased its patronage capital by appointing more judges to the Court for shorter periods. Given the extent to which even the minuscule probability of a Supreme Court appointment can affect some American judges, this carrot may have been quite useful.

Note two further political consequences. First, because the chief justice supervised the Secretariat, the Supreme Court potentially controlled appointments. Second, because the Supreme Court included at least one justice who recently had headed the Secretariat, the Supreme Court had the information necessary to use that potential control. Indirectly but necessarily, by controlling Supreme Court appointments the LDP potentially controlled lower-court judicial careers as well.

3.5. This Project. In this study, we test whether the LDP exercised that potential control over the lower courts. Because posts vary in quality and rotations were normal practice, by controlling the Secretariat the LDP could potentially control judicial careers without visibly intervening. Invisibility is important, because constitutions seldom prevent politicians from intervening if they are willing to be heavy-handed enough. If American senators dislike a judge's decisions, for example, they are free to impeach him on trumped-up charges. Hypothetically, they might even be able to transfer him to an undesirable city by changing the statutory structure of the courts (Ramseyer, 1994). By doing so, however, they would incur high po-

litical costs—both because of the time involved and because of the effect it would have on the appearance of judicial independence.

The Japanese prime minister has analogous high-stakes options. For instance, he can refuse on political grounds to reappoint a sitting judge. He will incur high political costs if he does, however, as the government discovered in 1971 when a leftist judge was not reappointed (Ramseyer & Rosenbluth, 1993: 165). Perhaps the prime minister could even intervene directly in the Secretariat to manipulate postings. Because the statutory structure of the courts does not formally allow such direct intervention, though, we doubt that he could intervene consistently for forty years in such a direct manner and still keep it quiet.[8]

We test for a more subtle and indirect political strategy: that the prime minister stacked the most powerful positions in the courts with reliable judges who then used their power to ensure that ordinary judges had an incentive to toe the political party line. Necessarily, our test is indirect: we look for correlation between political indices (a judge's membership in leftist groups, or the content of his reported decisions) and the desirability of the posts he receives.

4. THE DATA.

4.1. Sources. We collected data from three sources. First, for information on judicial careers, we used the Zen saibankan keireki söran (ZSKS): a list of all postings for every judge hired after 1948.

Second, for data on judicial opinions, we used the TDK LEX/DB data base of judicial opinions. Available on eight CD-ROM disks, the data base works much like the Lexis and Westlaw systems. Unfortunately, the collection is still slightly incomplete. TDK began compiling the opinions only a few years ago and had nothing like the West national reporter system from which to work. Nonetheless, we have checked the compilation scheme, and have no reason to think the coverage is biased in any way relevant here.

Last, we obtained the membership roster for the leftist Young Jurists League (YJL) from Osorubeki saiban. The authors of that

[8] Haley (1995), for example, makes much of the absence of this formal intervention.

book took the roster—current as of mid-1969—from the League's own newsletter.

4.2. Datasets. From this material, we produced four datasets.

(a) Exceptional judges. We collected data on the most successful of the postwar judges: all judges in the ZSKS who eventually obtained postings to either the Supreme Court or the presidency of a high court. As discussed above, we used this data to learn which posts constitute advantageous assignments (see Table 2).

(b) Judges who ruled on Section 138 cases. To explore whether judges who decided politically sensitive cases in ways contrary to LDP interests received unfavorable assignments, we investigated all judges who published opinions on the issue most commonly cited in this context: the constitutionality of the ban on door-to-door canvassing under § 138 of the Elections Act.

(c) Judges from the classes of 1961 to 1965. We compiled career data on all judges, not just a sample, who entered the courts during 1961 to 1965. In order to compare careers of equal length, we then dropped those judges who had left the judiciary by April 1990. Some critics accuse the Secretariat of pressing left-leaning judges into early retirement. To the extent that this happened, our findings will understate the true scope of any political discrimination. Because Supreme Court justices have a large corps of professional judges at their disposal to work as law clerks, elsewhere (e.g., for purposes of calculating OPINIONS for Table 2) we treated elevation to the Supreme Court as retirement. Although in other circumstances this might have biased our data the other way, for a simple reason it did not do so here—as of 1990, none of the judges in the classes of 1961-65 had been named to the Supreme Court.

(d) Judges from the class of 1965. For judges in the class of 1965 (a subset of dataset (c)), we investigated every decision the judge published that involved the government as litigant in one of four fields: labor, administrative, tax, and criminal law. We included all opinions, whether written alone or by a three-judge panel. We coded an opinion as "antigovernment" if the party fighting the government won a full or partial victory.

Thoughtful readers will note the imprecision of this test. Many of these opinions, for example, do not not involve distinctly political issues. For a wide variety of reasons, moreover, the government may not want to win even every suit. If it used biased judges to win every

case, its litigators would have less incentive to work hard. If those litigators did not always represent government interests (whether because they were heterodox or lazy), it would find some victories hollow. If the commitment problem Landes and Posner identified is real, any overt control over the judiciary would reduce its rent-extractive potential. And if it perceived its judges as biased in its favor, it might simply take more egregious positions—to the point where even its progovernment judges would balk (this selection effect reappears in Section 6.3(a) below).

Despite these objections, we use our coding scheme for two basic reasons. First, our scheme is simple and objective. We considered coding opinions according to our subjective sense of whether they furthered LDP interests, but concluded that doing so would invite charges that we "cooked" the data. To minimize the chance of conscious or unconscious bias on our part, we opted for a less precise but more objective test instead. Importantly, given the politically sensitive nature of our findings within Japan, this objectivity insures the replicability of our results.

Second, caveats about incentive effects, promissory credibility, and agency slack notwithstanding, governments generally litigate disputes because they want to win them (even if not quite every time). To that straightforward and forthrightly simplistic extent, a decision that a government loses is an "antigovernment" decision that will generally disappoint the men in power.

4.3. The Variables. We construct the following variables.

STARTING AGE: the age at which a judge joined the judiciary. To become a judge (or lawyer or prosecutor) in Japan, one must graduate from the government-run two-year Legal Research & Training Institute (the LRTI). During most of the years at stake, the pass-rate on the entrance exam to this Institute ranged from 1 to 4 percent. Would-be lawyers, prosecutors, and judges typically passed it only on their 3d, 4th or 5th try. We hypothesize, therefore, that the lower the age at which a person graduates from the Institute, the higher his cognitive ability and the stronger his determination to succeed. To the extent career success depends on intelligence and drive, STARTING AGE should inversely correlate with career success.

SEX: 1 if a judge is male and 0 if female.

TOKYO UNIVERSITY: 1 if a judge went to Tokyo University, and 0 otherwise. Because observers widely consider the Tokyo University Law Department the most selective, graduation there should positively correlate with intelligence and drive. Note, however, that many critics argue that Tokyo University alumni form a clique within the courts and help each other in their careers, independent of ability.

KYOTO UNIVERSITY: 1 if a judge went to Kyoto University, and 0 otherwise. Traditionally, observers have considered the Kyoto University Law Department second only to Tokyo University. Critics have accused Kyoto University alumni of running a clique as well.

CHUO UNIVERSITY: 1 if a judge went to Chuo University, and 0 otherwise. Chuo University operates a large and respectable but not first-tier law department. We include the variable because so many judges attended the school.

NO UNIVERSITY: 1 if the ZSKS lists no university for a judge, and 0 otherwise. A 1 could mean either that he attended the LRTI without graduating from a university, or that he chose not to disclose his educational background.

OPINIONS: the number of recorded decisions a judge published up to 1990, divided by the number of years he spent on the bench. We exclude those years during which he handled only administrative work.

Note a potential problem here. The law reporters (there are both official and unofficial ones) do not publish all opinions. Instead, they publish an opinion only if the editors find it interesting or important. If a branch office judge hears less important cases, this could mean that he will not publish as much even if he works as hard. That, in turn, presents a potential simultaneity problem. Suppose OPINIONS is positively correlated with career success. That fact could mean either that judges receive inferior assignments because they publish less, or that they publish less because they receive inferior assignments. To resolve this problem, we used our Class of 1965 data to create another variable: productivity for all years in courts other than lower court branch offices or summary courts. Fortunately for our purposes, the correlation between that new variable and OPINIONS was .98, indicating that adjusting for poor assignments would make little difference.

FIRST POST: the prestige of the first assignment a judge receives. The variable is 3 if it involves an administrative assignment, 2 if it involves sōkatsu duties, 1 if it is on a district or family court, and 0 if it involves a lower court branch office or summary court. For the vast majority of judges, the value was 1.

1980s POST: the prestige of a judge's assignments during the 1980s. If he spent at least 3 years in an administrative assignment, it is 3; if he spent at least 3 years in either an administrative assignment or a sōkatsu post (but not 3 years in an administrative assignment), it is 2; if he does not qualify for the categories above and spent at least 3 years in a lower court branch office or summary court, it is 0; otherwise, it is 1. For this and the other variables, we count time in the branch office only if the judge was not the official head of the branch office, and did not have sōkatsu status.

FIRST LOCATION: the location of a judge's initial assignment. This is 3 if the judge's first assignment was in Tokyo (including Hachioji), 2 if it was in Osaka, 1 if it was in another large metropolitan area (Yokohama, Nagoya, Sapporo, Kobe, Kyoto, Fukuoka, Kawasaki, Hiroshima, or Kitakyushu), and 0 if otherwise.

1980s LOCATION: a judge's location during the 1980s. It is 3 if he or she spent at least 5 years in Tokyo, 2 if at least 5 years in Osaka or Tokyo (but not 5 years in Tokyo), 1 if at least 5 years in a major metropolitan area (but not 5 years in Tokyo or Osaka), and 0 if otherwise.

YJL: membership in the Young Jurists League (YJL). The YJL is an organization of lawyers, law professors, and judges that generally supports leftist causes and which its detractors consider a Japan Communist Party affiliate. The variable is 1 if the judge was a member in 1969, and 0 otherwise.

EARLY ANTIGOVT: the number of antigovernment decisions (defined at Section 4.2(d)) that a judge issued during 1965-74.

LATE ANTIGOVT: the number of antigovernment decisions that a judge issued during 1975-84.

PERCENT EARLY ANTIGOVT: the percentage of antigovernment decisions that a judge issued during 1965-74.

PERCENT LATE ANTIGOVT: the percentage of antigovernment decisions a judge issued during 1975-84.

ANY EARLY ANTIGOVT: 1 if a judge issued any antigovernment decisions during 1965-74, and 0 otherwise.

ANY LATE ANTIGOVT: 1 if a judge issued any antigovernment decisions during 1975-84, and 0 otherwise.

5. THE RESULTS.

5.1. First assignments. We begin by investigating the factors that determine a judge's initial assignment (Table 3). The best jobs, our regressions suggest, go to the smartest and hardest working judges. Table 3 reports the results: an ordered probit regression of the characteristics of a judge on two measures of the attractiveness of his first job. Consider each column separately.

TABLE 3
DETERMINANTS OF FIRST ASSIGNMENT

	A. First Post	B. First Location
Sex	-.56 (.97) [.33]	.33 (.92) [.36]
Starting Age	-.12 (3.17) [.00]	-.14 (4.81) [.00]
Tokyo University	-.19 (.40) [.69]	1.28 (4.15) [.00]
Kyoto University	-.73 (1.60) [.11]	.69 (2.34) [.02]
Chuo University	-.54 (1.17) [.24]	.10 (.30) [.76]
No University	-.49 (1.16) [.25]	.18 (.64) [.53]
YJL	-.07 (.34) [.74]	-.03 (.17) [.86]
Pseudo R^2:	.08	.11
Observations:	274	274

Notes: Coefficients, followed by t-statistics in parenthesis, and confidence levels in brackets.
Program: STATA, running ordered probit.

Column A: Recall that FIRST POST measures whether a judge receives administrative responsibilities, receives a routine district or family court assignment, or is stationed to a branch office or summary court. Because no judge begins his career with administrative responsibilities, Column A effectively shows only that the worst jobs (primarily the branch office assignments) go to the oldest novice judges. Because age at appointment roughly correlates with

the number of times the judge failed the LRTI exam, it inversely correlates with intelligence and drive. The worst initial jobs, the regression thus suggests, go to the least smart and least hardworking judges.

Column B: The regression on FIRST LOCATION asks who receives the prized Tokyo and Osaka assignments. According to the results, those jobs go to the judges (i) who are youngest, and (ii) who attended the most selective universities. Once more, the regression suggests that the best jobs go to the smartest and hardest working judges.

The coefficient for YJL is insignificant in both regressions. Because the League's membership rolls did not become public until 1969, the Secretariat probably would not have known who was a member. Nonetheless, if the coefficient had been significant, it would have suggested that the Secretariat both had access to other information about a judge's political beliefs, correlated with YJL membership, and used that information to discriminate by ideology. In fact, it seems not to have done so.

5.2. Late assignments. Turn now to Table 4, the determinants of late-career success. In this set of regressions, we ask which judges received the prized jobs in the 1980s, some 20 years after they joined the courts.

First, in the location regression STARTING AGE is significant, but in both regressions university affiliation is not. That STARTING AGE continues to be important decades later suggests that intelligence and drive matter, and in ways beyond their effect on the judge's first job. That university affiliation loses significance (other than through its effect on FIRST LOCATION) tentatively suggests that critics exaggerate the importance of university cliques. If cliques mattered, university affiliation should affect later assignments, perhaps even more than the initial assignment, since over the course of time a judge's university classmates would rise to power in the judicial establishment. That a judge's university matters only through the initial assignment (while STARTING AGE has an independent continuing effect) implies that the Secretariat uses it as a proxy for ability in determining a judge's initial assignment, but finds that it becomes less useful as a proxy once the judge has developed a track record.

Second, FIRST LOCATION correlates with a judge's later assignments. This corroborates those accounts suggesting that the Secretariat places new judges on fast and slow tracks, and that an initial assignment to the Tokyo District Court predicts later success.

TABLE 4
DETERMINANTS OF CAREER SUCCESS

	A. 1980s Post	B. 1980s Location
Sex	.31 (.92) [.36]	.21 (.57) [.57]
Starting Age	-.029 (1.07) [.29]	-.070 (2.26) [.03]
Tokyo University	.092 (.30) [.77]	.076 (.22) [.82]
Kyoto University	.058 (.20) [.84]	.0089(.03) [.98]
Chuo University	.10 (.34) [.73]	.40 (1.20) [.23]
No University	-.15 (.56) [.58]	-.11 (.38) [.70]
1st location	.19 (2.67) [.01]	.31 (4.05) [.00]
Opinions	.18 (4.11) [.00]	.20 (4.35) [.00]
YJL	-.28 (1.94) [.05]	.13 (.83) [.41]
Pseudo R^2:	.08	.13
Observations:	274	274

Notes: Coefficients, followed by t-statistics in parenthesis, and confidence levels in brackets.
Program: STATA, running ordered probit.

Third, OPINIONS matters: judges who write many publishable opinions do better than those who write few. Although this restates the importance of intelligence and hard work, its potential significance goes further. From time to time, observers suggest that Japanese society may reward judges who settle cases rather than decide them. Because of a cultural preference for negotiated settlements, they argue, Japanese encourage their judges to settle cases when they can. Because settlements do not appear in our data, we do not know whether the most successful judges settle the lowest percentage of their disputes. We do know that the most successful judges are the most prolific in writing published opinions for the cases that failed to settle.

Last, independent of intelligence and hard work, political preferences matter: whether a judge was a YJL member inversely correlates with whether he received prestigious administrative responsibilities in the 1980s. Those judges named as part of the Marxist group in 1969 were still receiving less attractive jobs 10 to 20 years later. Curiously, YJL membership did not affect the location where the judge worked. Perhaps the Secretariat was willing to assign leftists to the cities. Crucially, however, it did not trust them with the highest positions within the judicial hierarchy.

Note that the correlation between YJL and STARTING AGE is -.31. Disproportionately, it seems, the most able members of the judiciary joined the group. The point both explains why some analyses that simply compare the jobs of League members and non-members find no discrimination (e.g., Ramseyer & Rosenbluth, 1993: ch. 9), and underscores the importance of multivariate analysis.

6. THE EFFECT OF ANTIGOVERNMENT OPINIONS.

6.1. The Method. We now turn to a more complex inquiry: whether the way a judge decides a case influences the jobs he obtains. More specifically, we ask whether any tendency to decide cases against the government hurts his career. We find that it does. The phenomenon has two sides: (a) a punishment effect, where antigovernment opinions translate immediately into less attractive job assignments in the short-term, and (b) an information effect, where antigovernment preferences revealed in a judge's decisions translate into a career disadvantage in the long-term.

Our first political variable was YJL membership, which was cleanly defined and relatively easy to collect for the entire cohort of 274 judges. We now wish to look at a more complicated characteristic: whether a judge rules for or against the government when the government comes to court. This introduces problems in measurement, econometrics, and theory. We have already discussed some of the measurement issues (Section 4.2(d)). We shall turn to the theoretical issues in Section 6.3.

The econometric issues result from the enormous amount of time necessary to collect this data. Because of this problem, we examined the opinions only of the 55 judges in the Class of 1965, a smaller number of observations than for all the classes between 1961

and 1965. We now must combine our 55 observations on judicial opinions with our 274 observations on all the other variables relevant to a judicial career. If we were willing to drop 219 observations, the econometrics would be simpler: we would repeat the probit regressions in Table 4, but with opinion variables added to the right-hand-side. This not only discards information, however, but raises doubts about the validity of the estimates and the standard errors, since probit is a nonlinear, asymptotic technique for which having a large sample is especially important.

Instead, therefore, we take a different approach. We begin with the regressions of Table 4, which use all 274 observations to predict career success. These regressions do not explain all the variance in the data, and generate an unexplained residual for each judge. If we can explain this residual using judicial opinion variables, we will have shown that a judge's opinions matter, and ought to have been in the regressions in Table 4. Moreover, because the residual is a continuous variable, we can use ordinary least squares, which does not rely on asymptotics for its validity.

More specifically, we first turn to our Class of 1965 dataset and use our Table 4 regressions to generate a "residual" for each judge. For this process, we take our estimated Table 4 coefficients and each judge's variable values. We then use these terms to generate a "score" for each judge. As ordered probit also generates estimated cutoff ranges, we match each judge's "score" to the cut-off ranges in order to generate a predicted posting for each judge (an integer from 0 to 3).

To explore how this works, take a hypothetical example. We did not report the estimated cutoff scores above, but for the regression in Table 4.A., they were -1.37, -.90, and .78. Suppose judge *X* has a "score" of -3.3. Because it falls below the bottom cutoff of -1.37, his predicted posting is 0. If his score were -1.20, he would fall in the -1.37 to -.90 range and have a predicted posting of 1.

Because for each judge we have only an estimated score, our predictions do not take straightforward integer values. If we knew with certainty that *X*'s score were -3.3, we could predict with certainty a posting of 0. Because -3.3 is just an estimate, however, his true score might be higher or lower. With positive probability, it might even be 0.5, and if so his predicted posting would be 3 rather than 0.

Accordingly, if *X*'s "score" is -3.3, our best prediction is not a posting of 0 but a weighted average of 0, 1, 2, and 3. Those weights will be our estimated probabilities of the true score lying in the four intervals of [-infinity, -1.37], [-1.37, -.90], [-.90, 0.78], and [0.78, +infinity]. We find the probabilities by using the standard error of our estimate. Our predicted career quality is the resulting weighted average. By now comparing this predicted career quality with a judge's actual posting (of 0, 1, 2, or 3), we calculate a residual—a continuous variable that measures judge *X*'s unexplained career quality. If positive, it indicates that he did better than our regression predicted; if negative, it indicates he did worse. Finally, we used a logit transformation to map the value of the residual, which lies between -3 and +3, to the entire real line between positive and negative infinity. This transformation maps the raw residual u to $\log[(u + 3)/(3 - u)]$.

Having now constructed a variable representing the unexplained success of a judge's career, we ask whether any tendency to decide public-law cases against the government has a negative correlation with that variable. If judge *X*'s decisions had no impact on his career, then regressing his residual on a variable summarizing his decisions would yield an insignificant coefficient. If they did have an impact, then—crucial to the analysis here—the coefficient might be significant.

6.2. The Punishment Effect. According to Table 5, judges who decide cases against the government soon receive less attractive jobs. In Part A of Table 5, the absolute number of antigovernment opinions that a judge writes in 1975-84 inversely correlates with the odds of receiving a post in an attractive city in the 1980s. In Part B, whether a judge decides any antigovernment opinions (a 0-1 variable) in 1975-84 inversely correlates with receiving high-status posts in the judicial hierarchy in the 1980s.

The simplest explanation for this phenomena is that it represents a straightforward punishment strategy: if you decide cases against the government, the expected value of your next several jobs falls. The probability of punishment may well be less than 1. After all, the government will not care equally about all its cases; it will not want to win every case (for the reasons discussed in Section 4.2(d)); and it will not necessarily punish every judge on a 3-judge panel (the opinions do not identify dissenters, but the Secretariat probably has

access to that information). Notwithstanding these caveats, according to Table 5, antigovernment opinions translate directly into less attractive posts in the near future.

TABLE 5
THE PUNISHMENT EFFECT

A. Number of Antigovernment Decisions:

	Post Residual	Location Residual
Early Antigovt	.025 (.77) [.45]	.015 (.48) [.63]
Late Antigovt	-.097 (l.18) [.25]	-.20 (2.51) [.02]
R^2:	.03	.11

B. Any Antigovernment Decisions:

	Post Residual	Location Residual
Any Early Antigovt	.17 (1.13) [.26]	-.21 (1.40) [.17]
Any Late Antigovt	-.32 (1.75) [.09]	-.036 (.20) [.84]
R^2:	.06	.06
Observations:	54	54

Notes: Coefficients, followed by t-statistics in parentheses, and confidence levels in brackets.
Program: STATA, running ordinary least squares on a logistic conversion of the residual from the career regression.

We find the haphazard confidence levels a puzzle. In Part A, only the location residual is significant, and in B only the post residual. We suspect that this reflects the noise in the data (discussed in the immediately preceding paragraph) and the relatively small sample size. Despite the large standard errors, however, the signs for the late opinions are negative in all four regressions.

6.3. The Information Effect. Potentially, a judge's opinions convey information about his political preferences. If litigated cases were a random sample of all disputes, for example, then the percentage of

cases that the government won before a judge would say something about his biases. The Secretariat could use that information to decide where to post its judges.

a. The selection bias. Litigated cases are not a random sample of all disputes, however, and the percentage of cases won says as much about the types of cases that go to trial as about how the judge views disputes in general.[9] In order to avoid the costs of trial, most disputants settle disputes whenever they agree about the likely litigated outcome. As a result, cases do not go to trial randomly. Instead, they go to trial when the judge's expected decision is unclear. Just because 80 percent of judge *Y*'s decisions are proplaintiff does not necessarily mean he is proplaintiff: he may only be proplaintiff in the most complicated cases, when the litigants found his decisions hardest to predict. For the purposes of this analysis, settlement could have an even more bizarre effect: it may be that the government goes to trial with its most outrageous cases only when it knows the judge is particularly progovernment, and is only moderately displeased when the government's arguments are too weak even for that judge to swallow. Thus, the judges who rule against the government most often might be the progovernment judges.

Settlement will be most common where the parties know a judge's style and biases most precisely. If they know nothing about a judge, he will hear cases that are randomly selected. Given that randomness, his verdict rate will indeed tend to disclose his biases. A judge with a shorter track record is one about whom litigants will have less information. Accordingly, we hypothesize that the selection bias will be strongest among judges at the end of their careers, and weakest at the start.

Our data contain separate variables for early and late antigovernment decisions. Lack of correlation between these two variables could be a sign either that no judges are biased, or that the selection

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[9] Priest & Klein (1984). Two further complications: under Japanese public law, the government will find it hard to settle many categories of cases (e.g., tax disputes—see Kaneko, 1992: 78). Second, some observers claim that many Japanese plaintiffs litigate public law disputes for their publicity effect rather than because they expect to win the disputes. To the extant that either of these phenomena occurs, the selection bias will be less and the percentage of government victories among a judge's opinions will convey more information about his political preferences.

effect means that once a judge acquires a track record, he tries only a selected group of cases and his antigovernment percentage says nothing about his biases. The correlation between PERCENT EARLY ANTIGOVT and PERCENT LATE ANTIGOVT is .12. Whether the size of this correlation is evidence for or against selection bias is unclear, but it is statistically insignificant (t=.28, significance level 78 percent). Second, the correlation between YJL and PERCENT EARLY ANTIGOVT is .23, and between YJL and PERCENT LATE ANTIGOVT is .06. If membership in the YJL provided information about whether a judge would decide cases against the government, we would worry more about a selection bias even for younger judges. It does not seem to provide such information, however, for the t-statistic for the regression of PERCENT EARLY ANTIGOVT on YJL and a constant is only 1.16 (a significance level of 25 percent).

b. The results. According to Table 6, a judge who decides a high percentage of cases against the government early in his career does indeed receive geographically less attractive jobs later in his career. The percentage of antigovernment opinions early in his career matters because it reveals information about his true biases. The later career percentage does not, though the absolute number matters to the government's short-term punishment strategy (Table 5), because it conveys no meaningful information.

TABLE 6
THE INFORMATION EFFECT

	Post Residual	Location Residual
Percent Early Antigovt	-.15 (.48) [.63]	-.52 (1.67) [.10]
Percent Late Antigovt	.27 (.62) [.54]	.46 (1.05) [.30]
R^2:	.01	.06
Observations:	44	44

Notes: Coefficients, followed by t-statistics in parentheses, and confidence levels in brackets.
Program: STATA, running ordinary least squares on a logistic conversion of the residual from the career regression. Only 44 of the judges are used,

because the others had no published opinions during the early time period and their percentage of antigovernment opinions is thus undefined.

7. ELECTORAL LAW DECISIONS.

Consider now the most common anecdotal accounts of political bias: the alleged discrimination against judges who held the § 138 canvassing ban unconstitutional. Among lower court judges, we located 37 who held the ban constitutional and 9 who held it unconstitutional. Using the data on these 46 judges, we test whether a judge's decision on the issue affected the assignments he received. Toward this end, we introduce several new variables:

PRIOR POSTS: the prestige of a judge's assignment before the § 138 decision. The variable equals 3 if he spent at least 3 years in an administrative job during the 10 years before the decision; 2 if he did not meet that requirement but spent at least 3 years in an administrative or sökatsu capacity; 0 if he did not meet either of those requirements but spent at least 3 years in a lower court branch office or Summary Court; and 1 if otherwise.

LATER POSTS: the equivalent to PRIOR POSTS for the 10 years after the decision. It takes the values 3, 2, 1, or 0.

PRIOR LOCATION: the desirability of the judge's location before the § 138 decision. The variable is 3 if the judge spent at least 5 of the previous 10 years in Tokyo; 2 if at least 5 years in Tokyo or Osaka (but not 5 in Tokyo); 1 if at least 5 years in metropolitan areas generally; and 0 if otherwise.

LATER LOCATION: the equivalent to PRIOR LOCATION for the 10 years after the decision. It takes the values 3, 2, 1, or 0.

PRIOR OPINIONS: the judge's productivity (published opinions per year on bench) for the 10 years before the § 138 decision.

138 DECISION: 0 if the judge held the canvassing ban constitutional and 1 if otherwise.

Surprisingly perhaps, Table 8 shows no statistically significant results.[10] Critics of the court have consistently told stories of judges

[10] We have omitted from these regressions the other variables used in this study (e.g., STARTING AGE, TOKYO UNIVERSITY) because there is no reason to expect them to be relevant to the measure tested here: the *change* in posting caused by the decision.

punished for writing heterodox opinions on § 138. Notwithstanding, the initial results in Table 8 present no evidence of the phenomena.

TABLE 7
SUMMARY § 138 STATISTICS

	Constitutional Mean	Unconstitutional Mean
Prior Posts	1.41	.44
Later Posts	1.70	1.56
Prior Location	1.05	.00
Later Location	1.14	.44
Prior Branch	.14	.24
Later Branch	.10	.50
Prior Sökatsu	.12	.00
Later Sökatsu	.28	.03
Prior Opinions	1.12	1.28
Sex	1.00	1.00
YJL	.14	.56
Observations	37	9

TABLE 8
EFFECT OF § 138 OPINIONS ON CAREERS

	Later Posts	Later Location
Prior Posts	.06 (.29) [.78]	
Prior Location		.40 (2.61) [.01]
Prior Opinions	-.18 (1.73) [.09]	.17 (1.21) [.24]
138 Decision	-.13 (.28) [.78]	-.25 (.47) [.64]
Pseudo R^2:	.03	.10
Observations:	46	46

Notes: Coefficients, followed by t-statistics in parentheses, and confidence levels in brackets.
Program: STATA, running ordered probit.

Recall, however, that the dependent postings and locational variables we use in Table 8 are artificial composites of a variety of different indices about the attractiveness of various types of posts. To look more closely at what happened to the two different groups of judges, we examine two key components of the postings variable: time in branch offices, and time as sökatsu.Consider several additional variables:

PRIOR BRANCH: the percentage of years a judge spent in branch offices during the 10 years (adjusted appropriately, if fewer years on the bench) before the § 138 decision.

LATER BRANCH: the equivalent to PRIOR BRANCH for the 10 years after the § 138 decision.

PRIOR SÖKATSU: the percentage of years a judge spent in sökatsu assignments for the 10 years (adjusted appropriately, if fewer years on the bench) before the § 138 decision.

LATER SÖKATSU: the equivalent to PRIOR SÖKATSU for the 10 years after the § 138 decision.

TABLE 9
EFFECT OF § 138 OPINIONS ON BRANCH OFFICE AND SÖKATSU POSTINGS

	Later Branch	Later *Sökatsu*
Prior Branch	.97 (1.36) [.18]	
Prior Sökatsu		.88 (2.05) [.05]
Prior Opinions	.06 (.96) [.34]	-.22 (1.55) [.12]
138 Decision	.80 (2.67) [.01]	-.47 (1.79) [.08]
Pseudo R^2:	.17	.24
Observations:	46	46

Notes: Coefficients, followed by t-statistics in parentheses, and confidence levels in brackets.
Program: STATA, running tobit.

The result confirms the popular accounts of the § 138 controversy: the position a judge takes on the constitutionality of § 138 ban

significantly affects both the time he spends in branch offices and the time he spends with sökatsu duties. Should he hold § 138 ban unconstitutional (i) he significantly increases the amount of time he will likely spend in branch offices over the next 10 years, and (ii) significantly decreases the amount of time he will spend with sökatsu responsibilities.

8. CONCLUSIONS.

Because civil-law systems hire unproven jurists into career judiciaries, many maintain elaborate incentive structures to prevent their judges from shirking. In this article, we use career data from the Japanese courts both to explore the general determinants of career success, and to test how extensively the government manipulates those incentives toward political ends.

We find considerable evidence that the government rewards the smartest and hardest working judges. Contrary to some observers, we find little evidence of ongoing school cliques (more precisely, no evidence beyond the school advantage in the initial job assignment). We also find no evidence that the Japanese system rewards judges who mediate over those who adjudicate. Rather, the judges who do best are those who publish the most opinions.

More controversially, we locate several politically driven phenomena. First, those judges who joined a prominent leftist organization in the 1960s were still receiving less attractive jobs than their peers in the 1980s. Second, those judges who decided cases against the government faced a straightforward short-term penalty: on average, they received less attractive assignments over the next several years. Last, those judges who decided a high percentage of cases against the government early in their careers were still receiving less attractive jobs than their peers in the 1980s.

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