

JUDICIAL INDEPENDENCE IN CIVIL LAW REGIMES:
ECONOMETRICS FROM JAPAN (Forthcoming, *Journal of Law,
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By J. Mark Ramseyer & Eric B. Rasmusen

Abstract

Because civil-law systems hire unproven jurists into career judiciaries, many maintain elaborate incentive structures to motivate their judges. We use personnel data on 276 Japanese judges to explore the determinants of career success and to test whether politicians manipulate those judicial career incentives for political ends. We find strong evidence that the system rewards the smartest and most productive judges, little evidence of on-going school cliques, and no evidence that the system favors judges who mediate over those who write opinions. We locate several politically driven phenomena. First, even as late as the 1980s, those judges who joined a prominent leftist organization in the 1960s were still receiving less attractive jobs. Second, judges who decided a case against the government incurred the risk that the government would soon punish them with less attractive posts. Third, judges who declared unconstitutional a crucial section of the electoral law advantageous to the majority party received less attractive posts than those who held it constitutional.

Ramseyer: University of Chicago Law School, Chicago, Illinois 60637.

Email: Mark_Ramseyer@law.uchicago.edu .

Rasmusen: Indiana University School of Business, Rm. 456, 1309 E 10th Street, Bloomington, Indiana, 47405-1701. Secretary, 812-855-9219, direct 812-855-3356 Fax: 812-855-3354. Email: Erasmuse@indiana.edu. Web: Php.indiana.edu/~erasmuse.

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A Japanese description of some of these results, translated by Professor Masaru Kohno, will appear as “Nihon ni okeru shiho no dokuritsu o kensho suru” [Examining Judicial Independence in Japan].¹

¹xxx [probably in the political science journal *Rebaisan*, though nothing formally decided yet].

1. INTRODUCTION

Because civil law courts often hire unproven jurists into career judiciaries, many use elaborate incentive structures to prevent their judges from shirking. In Japan, the courts maintain an administrative office called the Secretariat. The office regularly monitors and evaluates each judge, and uses that information to assign judges to new posts every three years. Because not all posts are created equal, it can use the system to induce judges to work hard. If it was willing, it could also use that power to induce judges to follow political orthodoxy.

In the article that follows, we use data from Japan to discover both the general determinants of judicial career success and the extent of political influence. Toward that end, we assemble career data on all 276 judges hired from 1961 to 1965. Within this data base, we find strong evidence that the Secretariat rewards the smartest and most productive judges. We find little evidence of on-going school cliques, and no evidence of rewards for judges who mediate rather than adjudicate.

More controversially, we also find 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, if a judge decided a case against the government, he incurred a significant risk that the Secretariat would punish him with a less attractive post. Third, those judges who invalidated the statutory ban on door-to-door canvassing, a ban advantageous to the majority party, were more likely to receive unattractive posts than those who upheld it.

We do not argue that Japanese politicians overtly intervened in the courts. Rather, we show that Japanese judges faced incentives that were systematically biased in favor of the Liberal Democratic Party (the LDP, in power continuously from 1955 to 1993). By using a systematically biased incentive structure, the LDP was able to obtain the results it wanted without overtly intervening. The logic behind this is basic to the Positive Political Theory developed so elegantly in the pages of this journal, and it is basic to understanding the Japanese judiciary: given the right institutional structure, visible intervention will be an out-of-equilibrium phenomenon.

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 ordered probit, we first explore the determinants of a judge's initial job posting (Section 5.1), and then his posts later in his career (Section 5.2). With more extensive data on the class of 1965, we 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 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. 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, and never earns a raise except in lockstep with every other federal judge.² Judge Harold Baer pushed his luck with Bill Clinton when he excluded 80 pounds of cocaine as evidence, but even then, Clinton could do nothing but fulminate.³ 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 may 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 hard or how fast they work. Instead, it will need to make do with proxies like exam performance.

Because of this limited information, in civil law jurisdictions the government will set up elaborate monitoring and incentive systems to induce its judges to work hard.⁴ It will maintain a judicial administrative office. Through that office, it will grade a judge's work and dispense rewards. When it reassigns a judge, much of the time it will do so randomly simply to prevent judges from developing ties to the local mob. Sometimes, though, it will also use the assignments to reward and punish: the better the work, the more attractive the job it will give a judge and the more money it will pay him.⁵

²State 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 appellate court to traffic court.

³President Clinton called on Judge Baer to resign, which he did, but the president's influence was purely moral suasion, and even for expressing his opinion about Judge Baer he was heavily criticized. "Baiting the Baer," *Newsweek*, Apr. 1, 1996, at 64.

⁴In 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.

⁵E.g., Merryman (1985: ch. 6); Clark (1988: 1840). Note, however, that one can overstate the contrast. Even in common-law systems, judges have some incentives to restrain their own idiosyncracies. Easterbrook (1982: 817); Rasmusen (1994); Spiller & Gely (1994).

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 with 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. The question at stake is whether a judge faces politically skewed incentives in those relatively unusual but sometimes vitally important cases.

2.2. Theory. Whether (or when) rational politicians will manipulate judicial incentives turns on a variety of questions external to the courts.⁶ On the one hand, independent courts potentially solve several vexing political problems, which is perhaps why they remain perennially popular with voters, statesmen, and law reviews. First, as McCubbins & Schwartz (1984) prominently note, independent courts help the ruling party police the bureaucracy. By giving disaffected citizens the right to sue before an impartial tribunal, the ruling party can potentially obtain access to information about how well its bureaucrats perform. Armed with that information, it can improve bureaucratic performance, and, crucially, improve its electoral advantage.

Second, independent courts add 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 ruling party will want to make its commitments credible. Subjecting its promises to the judgment of an independent court can do just that.

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 that politicians and their supporters expect to be out of power, they may rationally prefer courts whose independence reduces 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 such policies. Independent judiciaries can obstruct that delivery.⁷ Moreover, many politicians rationally take short-term perspectives. They care less about long-term

⁶Whether 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.

⁷Though 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).

credibility than about the next election. 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 toe the party line.

Ultimately, therefore, how independent judges will be of politicians depends 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 are independent, however, any control will necessarily be indirect. Any test will necessarily involve an analysis of outcomes, not words.

2.3. Empirical studies. Existing empirical studies do not tell us whether politicians in civil law systems do keep their courts independent. Although several scholars have begun to publish sophisticated empirical analyses of judicial independence, most have studied 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 & Gely (1992: 489) 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 multivariate 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, the debate remains open (compare Haley [1995] and Miyazawa [1994]). On the one hand, the ruling LDP almost never involved itself overtly in the courts. Yet the absence of overt intervention does not mean the LDP gave judges politically unbiased incentives. Critics of the Japanese judiciary have marshalled a variety of anecdotes that suggest just such bias.⁸ The most common stories are about the ban in §138 of the Public Offices Elections Act on door-to-door canvassing.⁹ Because incumbents obtain free media coverage while challengers do not, the ban disproportionately benefits incumbents. Because the LDP had the most incumbents during this period, the ban disproportionately benefited the LDP.

⁸For 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).

⁹Koshoku senkyo ho [Public Offices Elections Act], Law No. 100 of 1950, §138.

From time to time, a few lower court judges insisted that the canvassing ban violated the constitutional guarantee of freedom of expression. According to anecdotal accounts, they suffered when they did (e.g., Ramseyer & Rosenbluth, 1993: ch. 9). Take Haruhiko Abe. He held §138 unconstitutional in 1968, and by 1990 had spent 11 years in branch offices, far more years than normal, as can be seen from Table 2. Or take Masato Hirayu. He held §138 unconstitutional in 1979, and by 1990 had spent 8 years in branch offices. Among the judges who started in the same year as he did, he was now in the bottom 8 percent (Ramseyer & Rosenbluth [1993]: 171-172). In the article that follows, we ask whether a systemic multivariate approach will confirm the bias these anecdotes suggest.

3. THE JAPANESE COURTS

3.1. Lower Court Appointment and Reappointment. As in most civil law systems, all lower-court (i.e., *High*, *District*, and *Family* court) judges in Japan begin their judicial careers immediately upon finishing their legal education. They then stay judges for most of their working lives. By the early 1990s, the courts employed some 2,800 judges (see Table 1). Formally, they work a series of 10-year terms; in substance, they almost always found their terms renewed. Legally, the Prime Minister had the power to determine both initial appointments and later reappointments; in fact, he usually deferred to the Secretariat. Generally, he reappointed all sitting judges until they either resigned or reached mandatory retirement at age 65.

Lower-court judges handle some cases alone, but decide the more important cases as three-judge panels (a point that fogs but does not bias our data). Dissenting judges do not publish their opinions. No lower court judge uses law clerks. The rules by which cases are assigned to specific judges are set by the local court, and are generally non-discretionary.

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 U	.16	0	1
Kyoto U	.19	0	1
Chuo U	.14	0	1
No University	.43	0	1
Opinions/Year	2.02	.04	10.42
1st Post	.91	0	3
1980s Post	1.83	0	3
1st Location	1.00	0	3
1980s Location	1.06	0	3
YJL	.35	0	1

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
<i>Sokatsu</i>	353		

3.2. Lower-Court Postings. During their careers of thirty-odd years, lower-court judges in Japan move through a variety of jobs. These jobs vary along several dimensions.

First, they vary by geography. The Secretariat can—and does—routinely move judges from city to city. Often it does so simply to keep judicial quality uniform and to prevent corruption.

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, divorcees, juveniles, guardians, etc.), to the branch offices of the District and Family Courts. It routinely sends judges to the less prestigious postings, and such an assignment does not necessarily signal disgrace, as can be seen from Table 2.¹⁰ 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 *sokatsu* 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. Visibility and influence do not completely overlap; a staff position within the Secretariat can be highly influential, even if less visible than a seat on a High Court.

As a result, a judge who moves to a worse posting may have – but has not necessarily – been identified for special treatment. This can be seen from the following two examples:

Katsuya Onishi, an exceptional judge. Born in 1928. Kyoto University graduate. 1953: Kyoto District Court (DC). 1958: Secretariat. 1961: Hakodate DC. 1964: Secretariat. 1968: Tokyo DC. 1970 Osaka High Court (HC). 1971: Osaka DC, sokatsu. 1974: Tokyo DC. 1974: Tokyo DC, sokatsu. 1975: Secretariat. 1985: Kofu DC (Chief Judge). 1986: Tokyo HC sokatsu. 1986: Secretariat (Secretary General). 1989: Tokyo HC (President). 1991: Supreme Court.

Masakazu Kuwamori, a typical judge. Born in 1926. No university information. 1953: Yamaguchi DC Branch Office. 1957: Osaka DC. 1960: Sendai HC. 1962: Sendai DC. 1963: Nagasaki Family Court. 1968: Osaka Family Court. 1970: Osaka DC sokatsu. 1971: Fukuoka DC sokatsu. 1974: Fukuoka HC. 1977: Osaka HC. 1979: Fukuoka DC Branch Office (Chief). 1985: Tokushima DC (Chief Judge). 1986: retired (notary public).

¹⁰Note that the custom of rotating the high-quality assignments necessarily means that any formula trying to explain who gets such assignments will have relatively low predictive power— low “ R^2 ” if a linear regression were used.

TABLE 2:
EXCEPTIONAL JUDGES AND THE CLASS OF 1965

	Exceptional Judges			Class of 1965		
	Mean	Minimum	Maximum	Mean	Minimum	Maximum
<i>Personal data:</i>						
Starting Age	26.84	23	32	29.85	24	38
Sex	.96	0	1	.91	0	1
Tokyo U	.76	0	1	.2	0	1
Kyoto U	.12	0	1	.036	0	1
Chuo U	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
<i>Percentage of career in various posts:</i>						
Tokyo	.52	0	.89	.23	0	.88
Osaka	.14	0	.73	.068	0	.56
<i>Sokatsu</i>	.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 post-war 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.

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 cannot explore this issue.

3.3. Judicial preferences. To determine the relative attractiveness of the various lower-court 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.

Although some American readers balk at the notion that judges would prefer Tokyo assignments, within Japan the point is uncontroversial. Tokyo is an upscale Manhattan, the Japanese cultural Mecca, to be sure, but it is much more besides. For aspiring professionals, it is the quintessential information hub— the seat not just of the national government but of the political opposition, most media organizations, and most major corporate headquarters. At least as important for upper middle class families, it is home to the University of Tokyo and two other first-tier national universities, to most of the best private universities, and to most of the best preparatory schools. For any parent with ambitions for his child, no city beats Tokyo. And for just that reason, many judges assigned to the provinces will (like their peers in business) move there alone and leave their families in Tokyo.¹¹

In Table 2 we display the percentage of 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 or 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 *sokatsu* than other judges, but this is because they more quickly move to higher administrative roles like chief judgeships.

3.4. The Supreme Court and the Secretariat. The Japanese Supreme Court is structured along entirely different lines from the lower courts. Fifteen justices comprise the Court. They are appointed by the Prime Minister, and serve until mandatory retirement at age 70. They do not rotate among different judicial posts. They hear most cases on five-judge panels, but the most important *en banc*. Unlike lower-court judges, they use law clerks and sometimes publish dissents. Among the last 20 justices appointed through 1990, the mean age at appointment was 64. Six were promoted from the lower-courts, eight had been practising lawyers, three had been prosecutors, two had been in the foreign service, and one had been a professor. Of the justices, the Chief Justice supervises the Secretary General, the head of the Secretariat. Generally, at least one Supreme Court justice is himself a former

¹¹So long as most judges prefer metropolitan centers, the hypothetical chance that a dissident judge prefers a provincial post does not eliminate the general deterrent effect of using provincial posts as punishment— any more than the occasional criminal who likes to spend his winters in jail eliminates the deterrent effect of jail time.

Secretary General.

Because the LDP appointed Supreme Court Justices late in life, for most of the post-war years the 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 who promotes his new agenda over that of his benefactor. In contrast to U.S. Presidents, the LDP could safely appoint Supreme Court Justices who would soon retire because it faced such high odds (though less than 1, as it turned out) of staying in power.

By appointing older Supreme Court Justices, the LDP also increased its stock of patronage capital. Suppose (as seems likely) the marginal utility to a judge of a year of Supreme Court appointment declines with the number of years. If so, then the LDP necessarily increased its patronage capital by appointing more judges for shorter periods. Given the extent to which even the miniscule probability of a Supreme Court appointment seems to motivate some American judges, this carrot may have been quite useful.

Note two further points. First, because the Supreme Court Chief Justice supervised the Secretariat, the Supreme Court potentially controlled lower-court appointments. Through his control over the Secretariat, in other words, the Chief Justice and his colleagues on the Supreme Court had the power to reward lower-court judges who performed as they wished and punish those who did anything else. Second, because the Supreme Court included at least one justice who recently had headed the Secretariat, it also had the information necessary to use that potential control over the lower courts effectively. These were not supervisors paralyzed by an inability to understand the large bureaucracy they headed. They were supervisors who knew where the bodies were buried— and indeed, to embroider the metaphor a bit, had actually buried some of them. Indirectly 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 its potential control over the lower courts. Because posts vary in quality and rotations were normal practice, by controlling the Secretariat the LDP could 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, the U.S. Constitution leaves them 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 political costs— both because of the time involved and because of the effect such a public action 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 one leftist judge was not reappointed (Ramseyer & Rosenbluth [1993] p. 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.

We test for a more subtle and indirect political strategy: maintaining a politically skewed incentive structure. In effect, we argue that the Prime Minister appointed LDP loyalists to the Supreme Court, and that those loyalists used their control over the lower courts to ensure that ordinary judges faced incentives to toe the political party line. Granted, the Secretariat generally suggested the Supreme Court nominees to the Prime Minister, and the Prime Minister generally rubber stamped those suggestions. But that is irrelevant. For if we are right, because the Secretariat knew the Prime Minister had the power and inclination to reject the politically heterodox, it nominated only LDP loyalists. The Prime Minister then approved its nominees— but only because he knew it had every incentive to suggest appointees he could trust.

4. THE DATA

4.1. Sources. We collected data from several sources. First, for information on judicial careers, we used the *Zen saibankan keireki soran* (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.

Third, we obtained the membership roster for the leftist Young Jurists League (YJL) from Osorubeki saiban. The authors of that book took the roster— current as of mid-1969— from the League's own newsletter.

Last, to investigate whether family status affected career success, we checked the Japanese cross between the *Who's Who* and the *Social Register* (the *Nippon Shinshi Roku*) for 1969. Because none of the judges from the classes of 1961 to 1965 appeared in the book, we did not construct a family status variable. This is actually a useful negative finding— the judiciary appears *not* to be a career for those well connected by birth, despite the large number of judges who attended prestigious universities.

4.2. *Datasets.* From this material, we produced four datasets. (a) *Exceptional judges.* We collected data on the most successful of the post-war 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 1990.¹² Some critics accuse the Secretariat of pressing left-leaning judges into early retirement. To the extent that this happened, our findings understate the true scope of any political discrimination.

Because Supreme Court justices have a large body of professional judges at their disposal to work as law clerks, elsewhere (e.g., for purposes of calculating OPINIONS/YEAR for Table 2) we treated elevation to the Supreme Court as retirement. Although in other circumstances this might have biased our data, it did not do so here for a simple reason. 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 they 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 “anti-government” 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 involve distinctly political issues. For a wide variety of reasons, moreover, the government may not want to win 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 & Posner identified is real, any overt control over the judiciary would reduce its rent-

¹²Of the cohort of 394 judges who finished their legal training in 1961 to 1965, 3 began their careers in private practice; 12 died in judicial office; 4 retired at mandatory retirement age; 1 was fired; 97 resigned; and 3 were dropped for other reasons. Of the 97 resignations, 31 (32 percent) were YJL members. This is comparable to the judicial population as a whole—according to Table 1, 35 percent of the 1961-65 who did not resign were YJL members.

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 pro-government 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. To that straightforward and forthrightly simplistic extent, a decision that goes against the government is an “anti-government” 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 3th, 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 that 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 U: 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. In addition, many critics argue that Tokyo University alumni form a clique within the courts and help each other in their careers, independent of ability.

KYOTO U: 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 U: 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 mean that he either attended the LRTI without graduating from a university or he chose not to disclose his educational background, a choice that suggests he graduated from an unprestigious school.

OPINIONS/YEAR: 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, both official and unofficial, 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 hard, leading to a simultaneity problem. Suppose OPINIONS/YEAR 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 check for 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/YEAR was .98, indicating that adjusting for poor assignments would make little difference.¹³

We will examine four variables that measure job quality for a judge. The numerical values in all of them are ordinal, not cardinal, measures. A value of 3 in 1ST POST does not mean that this judge's first job was 3 times better than that of his classmate who received a value of 1, only that it is a better job. The regressions will use the job quality variables as dependent variables in ordered probit, a procedure which uses only the ordinality of the dependent variables.

1ST POST: the prestige of the first assignment a judge receives. The variable is 3 if it involves an administrative assignment, 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 1.¹⁴

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 *sokatsu* 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,

¹³Table 8 provides further support for this. The prior opinions variable takes values of 1.12 and 1.00 for the two groups in that table, despite large differences in job quality in the other dimensions shown.

¹⁴We do not let this variable take a value of 2 because we judge there are fewer gradations of quality of first post than in later posts or location. In the next variable, 1980s POST, a value of 2 will be used to indicate *sokatsu* posts, but beginning judges never receive that type of post.

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 *sokatsu* status.

1ST 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 in Osaka, 1 if 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 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 ANTI-GOVT: the number of anti-government decisions (defined at Section 4.2(d)) that a judge issued during 1965-74.

LATE ANTI-GOVT: the number of anti-government decisions that a judge issued during 1975-84.

ANY EARLY ANTI-GOV: 1 if a judge issued any anti-government decisions during 1965-74, and 0 otherwise.

ANY LATE ANTI-GOV: 1 if a judge issued any anti-government 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. The best jobs, our regressions suggest, go to the smartest and hardest working judges. Table 3 reports the results of an ordered probit regression of the characteristics of a judge on two measures of the attractiveness of his first job. Consider each column separately.

Column A: Recall that 1ST 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 only a very few judges (3 in our sample) begin their careers 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 suggests, go to the least smart and least hardworking judges.

Column B: The regression on 1ST 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.

TABLE 3:
DETERMINANTS OF FIRST ASSIGNMENT

	A			B		
	1st Post			1st Location		
Sex	-.56	(.97)	[.33]	.35	(.98)	[.33]
Starting Age	-.12	(3.16)	[.00]	-.14	(4.87)	[.00]
Tokyo U	-.19	(.39)	[.70]	1.36	(4.39)	[.00]
Kyoto U	-.73	(1.60)	[.11]	.71	(2.43)	[.02]
Chuo U	-.54	(1.17)	[.24]	.11	(.32)	[.75]
No University	-.49	(1.15)	[.25]	.18	(.63)	[.53]
YJL	-.07	(.32)	[.75]	-.01	(.09)	[.93]
Observations:	276			276		

Notes: Coefficients, followed by t-statistics in parenthesis, and confidence levels in brackets.

Program: STATA, running ordered probit.

The discussion above focussed on which variables were statistically significant, rather than on the values of the coefficients. Interpreting probit coefficients requires some care. To find the predicted post or location for a judge, one combines the estimated Table 3 coefficients and each judge's variable values, as with linear regression. We then use these terms to generate a "score" for each judge. As ordered probit also generates estimated "cutoff scores", we match each judge's "score" to the cut-offs in order to generate a predicted posting for each judge.

To explore how this works, consider Takeo Wada, the very last judge in our sample, who can fairly be called a typical judge based on observable measures. For the regression in Table 3.B., the cutoff scores were -3.25, -2.57, and -2.05. Table 4 shows that Judge Wada's location score is -3.52. Because -3.52 falls below the bottom cutoff of -3.25, the modal value of his predicted location is 0. If his score were -2.90 instead, he would fall in the -3.25 to -2.57 range and have a predicted modal location of 1.

TABLE 4:
INTERPRETING FIRST JOB COEFFICIENTS

Variable	Mean in Population (a)	Value for Judge Wada (b)	Posting Coefficient (c)	Contribution for Wada (b)*(c)	Location Coefficient (d)	Contribution for Wada (b)*(d)
Sex	.91	1	-.56	-.56	.35	.35
Starting Age	29.85	29	-.12*	-3.48	-.14*	-4.06
Tokyo U.	.20	0	-1.8	0	1.37*	0
Kyoto U.	.04	0	-.72	0	.71*	0
Chuo U.	.20	1	-.54	-.54	.11	.11
No University	.47	0	-.49	0	.18	0
YJL	.27	0	-.07	0	-.01	0
Total Score:	—	—	—	-4.46	—	-3.52

Note: Coefficients statistically significant at the .10 level are starred.

The cut points for regression 3A are -5.69 and 1.19.

The cut points for regression 3B are -3.25, -2.57, and -2.05.

Because for each judge we have only an estimated score, our predictions do not take straightforward integer values. If we knew with certainty that Judge Wada's true location score were -3.52, we could predict with certainty a posting of 0. Because -3.52 is just an estimate, however, his true score might be higher or lower. With positive probability, his score might even be 0.5, in which case his predicted posting would be 3 rather than 0. Accordingly, if Judge Wada's score is -3.52, 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 $[-\infty, -3.25]$, $[-3.25, -2.57]$, $[-2.57, -2.05]$, and $[-2.05, +\infty]$, found by using the standard error of the estimate. Our predicted career quality is the resulting weighted average.

For Judge Wada, with a score of -3.52, the expected value of his first location is .64. (The actual value for his first location was 0, so the residual is -.64.) If the score rose from -3.52 to -3.10 because the judge began at 3 years younger, the expected location would improve to 1.05, and the modal location would jump to 1. Thus, the coefficient on age is not only statistically significant, but is large enough that a

reasonable change in its value leads to a real change in the predicted job location.

Table 4 can also be used to see the relative importance of the different variables. If the judge were three years older, his score would fall by .42, as just discussed. If the judge were female, her score would fall by .35. Both of these are realistic magnitudes of change within the population, implying that sex and age are of roughly the same importance.

Interpreting the posting regression is similar, but has less interesting results. Judge Wada has an expected first posting of .90 and a modal location of 1. (His actual posting value was 3.) If the score were to rise from -4.46 to -4.10 because the judge began 3 years younger, the expected location would improve to .98, and the modal location would remain at 1. As this shows, the posting regression has less predictive power than the location regression. It takes a much bigger improvement in characteristics to get a sizeable improvement in predicted career. Not only are the coefficients in the first posting regression mostly statistically insignificant; they are also “economically insignificant”.

5.2. Late assignments. Turn now to Table 5, 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 1ST LOCATION) suggests that critics may 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, whereas 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, 1ST LOCATION correlates with a judge’s later assignments. Although “1ST LOCATION = 2” cannot be significantly distinguished from the other levels, “1ST LOCATION = 1” and “1ST LOCATION = 3” are both significantly better than “1ST LOCATION = 0”, the dummy left out of the regression, and “1ST LOCATION = 3” has the bigger coefficient, as one would expect. 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.

Third, OPINIONS/YEAR 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 significance goes further. From time to time, observers suggest that Japanese society may reward judges who settle cases rather than decide them.¹⁵ 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 tended not to give them the highest positions within the judicial hierarchy.

The correlation between YJL and STARTING AGE is -.31. Disproportionately, it seems, the most able members of the judiciary joined the group. The point 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. Given their superior talent, the YJL members ought to have done better than average. Doing just as well as their classmates would therefore be a sign of failure.

¹⁵The classic account tying low levels of litigation in Japan to a cultural aversion to clear-cut court outcomes is Takeyoshi Kawashima (1963).

TABLE 5:
DETERMINANTS OF CAREER SUCCESS

	A			B		
	1980s Post			1980s Location		
Sex	.33	(.97)	[.33]	.22	(.57)	[.57]
Starting Age	-.03	(1.05)	[.29]	-.07	(2.35)	[.02]
Tokyo U	.03	(.11)	[.91]	.02	(.05)	[.96]
Kyoto U	.20	(.67)	[.50]	.15	(.46)	[.65]
Chuo U	.19	(.61)	[.54]	.48	(1.40)	[.16]
No University	-.07	(.27)	[.78]	-.04	(.14)	[.89]
1st location =1	.33	(1.84)	[.07]	.36	(1.85)	[.07]
1st location =2	-.14	(.63)	[.53]	.30	(1.27)	[.20]
1st location =3	.79	(3.42)	[.00]	1.13	(4.53)	[.00]
Opinions/Year	.19	(4.32)	[.00]	.20	(4.27)	[.00]
YJL	-.28	(1.94)	[.05]	.16	(.32)	[.15]
Observations:	276			276		

Notes: Coefficients, followed by t-statistics in parenthesis, and confidence levels in brackets.

Program: STATA, running ordered probit.

Interpreting these regression coefficients requires the same procedure as with the regressions for the first job. Again we will use Judge Wada, in Table 6. As with Table 4, we ask how a realistic change in the value of a variable would affect the score. Converting from male to female, the score would fall by .22. If the judge were three years older, his score would fall by .21. Both of these are realistic magnitudes of change within the population, and so one might conclude that sex and age are of approximately equal importance. The location score gives the judge an expected location of .94 and a modal location of 0. (The actual value for this judge was 1.) If we add .22 to increase the score from -1.11 to -.89, the expected location would improve to 1.17 and the modal location would jump to 1.

TABLE 6:
INTERPRETING CAREER COEFFICIENTS

Variable	Population Mean (a)	Value for Judge Wada (b)	Posting Coefficient (c)	Contribution for Wada (b)*(c)	Location Coefficient (d)	Contribution for Wada (b)*(d)
Sex	.91	1	.33	.33	.22	.22
Starting Age	29.85	29	-.03	-.87	-.073*	-2.12
Tokyo U.	.20	0	.03	0	0.18	0
Kyoto U.	.04	0	.20	0	.15	0
Chuo U.	.20	1	.19	.19	.48	.48
No University	.47	0	-.07	0	-.04	0
1st location =1	.22	0	.33*	0	.36*	0
1st location =2	.13	0	-.14	0	.30	0
1st location =3	.18	0	.79*	0	1.13*	0
Opinions/Year	1.75	1.55	.19*	.29	.20*	.31
YJL	.27	0	-.28*	0	.16	0
Total Score:	—	—	—	-.03	—	-1.11

Note: Coefficients statistically significant at the .10 level are starred.

The cut points for regression 5A are -1.29, -.77, and .92.

The cut points for regression 5B are -.94, -.61, and -.25.

Indeed, it seems that in the location regression the variables which are statistically significant all have coefficients that are also of meaningful size, though some other variables (like sex) that are statistically insignificant also have large coefficients.¹⁶ Most interesting, perhaps, is the very large coefficient of 1.13 on “1st location =3”. It contributes 1.13 to the judge’s score if his first job was in Tokyo, a very strong effect in terms of getting later Tokyo jobs. Note that this is not just the effect of inertia. The location variable here is based on number of years spent in a location, but almost all judges rotate out of Tokyo at some point even if they begin there.

The posting regression’s coefficients for the variables which were also statistically significant in the location regression— “1st Location=1”, “1st Location=3”, and Opinions/Year—are of the same order of magnitude. Starting Age has a coefficient size about half that in the location regression, so its size is reasonably large, but the error is greater, and so it is not statistically significant in the posting regression. YJL has the reasonably large coefficient of $-.28$ and is statistically significant, but the error distribution in the posting regression is flat enough that the coefficient of .28 does not have strong effect on the predicted posting. The posting score of $-.03$ gives

¹⁶Note the big coefficient in Table 4 for Chuo University, which also comes up to at least the 15 percent significance level. Perhaps the place to look for an Old Boy Effect is Chuo, not Tokyo.

our benchmark, Judge Wada, an expected location of 1.84 and a modal posting of 2. (His actual value was 1.) If we convert him to a YJL member and subtract .28, his score falls from -.03 to -.31, the expected posting would fall to 1.63, and the modal posting remains at 2. For the modal posting to fall to 1 would require a decrease of the score to -.77, while to increase to 3 would require an increase to .92. What this tells us is that although the posting regression may be able to tell us what variables are statistically significant, its overall performance in predicting posting is poor—factors not in our regression equation are relatively more important than in the location regression.

6. THE EFFECT OF ANTI-GOVERNMENT 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.

Our first political variable was YJL membership, which was cleanly defined and relatively easy to collect for the entire cohort of 276 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, theory, and econometrics. We have already discussed some of the measurement issues (Section 4.2(d)).

The theoretical issue arises because of the Priest-Klein (1984) selection effect. Litigated cases are not a random sample of all disputes, and who wins the cases that go to trial may say more about which cases go to trial than about how the judge views the two sides. In order to avoid trial costs, most disputants settle disputes when 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 pro-plaintiff does not necessarily mean he is pro-plaintiff: he may only be pro-plaintiff in the most complicated cases, where 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 pro-government, 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 might be the most pro-government judges.

Two further complications are (a) under Japanese public law, the government will find it hard to settle many types of cases out of court (e.g., tax disputes, as Kaneko (1992, p. 78) explains), and (b) some observers claim that many Japanese plaintiffs litigate public law disputes for their publicity effect rather than to win.

To the extent that either phenomenon occurs, the selection bias will be less and the percentage of government victories will convey more information about a judge's political preferences. A potential problem nonetheless remains.

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, the selection bias should be strongest among judges at the end of their careers, and weakest at the start.

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 54 judges in the Class of 1965 who stayed in the judiciary through 1985. We now must combine our 54 observations on judicial opinions with our 276 observations on all the other variables relevant to a judicial career. If we were willing to drop 222 observations, the econometrics would be simple: we would repeat the probit regressions in Table 5, 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, we take a different approach. We begin with the regressions of Table 5, which use all 276 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 5. 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 5 regressions to generate a "residual" for each judge: his predicted career quality minus his actual posting of 0, 1, 2, or 3. This residual is 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. We then 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, mapping the raw residual u to $\log[(u + 3)/(3 - u)]$. This allows us to use a formal test of a model in which we test the null hypothesis that this transformed residual is just a normally distributed disturbance against the alternative that it also depends on political variables. If a judge'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. Results. According to Table 7, judges who decide cases against the government receive less attractive jobs. In Part A of Table 7, the number of anti-government 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 anti-government 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 a judge decides cases against the government, the expected value of his 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 even identify dissenters, though the Secretariat probably can find out if it cares to.) Notwithstanding these caveats, according to Table 7, anti-government opinions translate directly into less attractive posts in the near future.

We find the haphazard confidence levels a puzzle. In Part 7A, only the location residual is significant, and in 7B 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. Speculatively, however, we might suggest that for the sensitive administrative posts in part B, having any anti-government opinions at all is a black mark, whereas for location, more closely measuring the value of the job to the judge himself, rewards and punishments take a more gradual form. This would accord with the negative effect on post, but not location, of YJL membership found earlier in this article. It is most important to the government to have appropriate judges in the right posts, even though it may be more important to a judge to have the right location. At any rate, it should also be noted that despite the often large standard errors, the signs for the late opinions are negative in all four regressions.

TABLE 7:
UNEXPLAINED CAREER SUCCESS AND
ANTI-GOVERNMENT DECISIONS

A. Number of Anti-Government Decisions:

	1. Post Residual			2. Location Residual		
Early Anti-Govt	.030	(.92)	[.36]	.021	(.66)	[.51]
Late Anti-Govt	-.11	(1.32)	[.19]	-.21	(2.60)	[.01]
R ² :	.04			.12		

B. Any Anti-Government Decisions:

	1. Post Residual			2. Location Residual		
Any Early Anti-Govt	.18	(1.21)	[.23]	-.21	(1.34)	[.19]
Any Late Anti-Govt	-.35	(1.90)	[.06]	-.05	(.28)	[.78]
R ² :	.07			.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.

7. ELECTORAL LAW DECISIONS

Notwithstanding this evidence, several readers of earlier drafts of this article noted that Section 6 does not show distinctly *political* control. It suggests that the courts favor the government, but not whether they favor one political party over another. Indeed, it could simply imply that judges answered to the prosecutors in the Ministry of Justice. To explore this issue, we chose an issue on which the LDP and much of the opposition took flatly opposing views: the constitutionality of the ban in §138 of the Elections Act on door-to-door canvassing. As discussed earlier, because incumbents have greater access to the media than challengers and the LDP had more incumbents than any other party, LDP leaders favored the ban. For precisely the same reasons, many opposition leaders opposed it.

Compare, therefore, the posts received by judges who held the ban constitutional with posts received by those who held it unconstitutional. Among the lower court

judges, we located 37 who held the ban constitutional and 9 who held it unconstitutional. Using data on these 46 judges, we test whether a judge's decision on the issue affected his assignments. To this end, let us introduce several new variables, the average values of which are shown in Table 8 separately for the two groups of judges.

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 *sokatsu* 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 otherwise.

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

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 otherwise.

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

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

LATER *Sokatsu*: the equivalent to PRIOR *sokatsu* for the 10 years after the §138 decision.

PRIOR OPINIONS/YEAR: 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.

The summary statistics in Table 8 suggest that the judges who held the ban unconstitutional were already in worse jobs before they heard the controversial case. The average prior post is .44 for them, compared to 1.41 for the other 37 judges,

prior location is .00 compared to 1.05, prior sokatsu is .00, while prior time in branch offices is .50 compared to .10. The percentage YJL membership was .56 compared to .14, so the politics of these judges may have been well known long in advance of their anti-government decisions.

TABLE 8:
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 <i>Sokatsu</i>	.12	.00
Later <i>Sokatsu</i>	.28	.03
Prior Opinions	1.12	1.28
Sex	1.00	1.00
YJL	.14	.56
Observations	37	9

A glance at Table 8 seems to show that the careers of the unfortunate 9 judges actually improved after their §138 decisions. Ordere probit regressions of Later Posts and Later Location on earlier career variables and the §138 decision failed to show significant relationships, however, and the improvement may just be regression to the mean. When careers are bad enough (e.g., an average location of 0.00), there is nowhere to go but up. Ordered probit, however, is undependable in this context, since there are only 46 observations.

Two other career variables, however, show a distinct punishment effect. Table 9 shows the results of two tobit regressions using time in branch offices and time as *sokatsu* as the dependent variables. Tobit is appropriate rather than ordinary least squares because the dependent variables, being number of years, are bounded below by zero. These variables are narrower than the career variables used in the previous regressions in this paper, however, and this may aid in finding significant results with a small sample. Tobit, while a nonlinear regression technique, is much closer to ordinary least squares than ordered probit, imposing more structure on the regression.

The results in Table 9 are in accord with 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 *sokatsu* duties. The coefficients for the §138 decision variable are large, and significant at the 1 percent and 8 percent level for both Later Branch and Later *Sokatsu*, and this is true even conditioning on the judge's previous job quality. 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) he significantly decreases the amount of time he will spend with *sokatsu* responsibilities.

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

	Later Branch			Later <i>Sokatsu</i>		
Constant	-.67	(2.46)	[.02]	.24	1.65	[.11]
Prior Branch	.97	(1.36)	[.18]	–	–	–
Prior <i>Sokatsu</i>	–	–	–	.88	(2.05)	[.05]
Prior Opinions	.06	(.96)	[.34]	-.22	(1.55)	[.12]
§138 Decision	.80	(2.67)	[.01]	-.47	(1.79)	[.08]
Observations:	46			46		

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

Program: STATA, running tobit.

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 used career data from the Japanese courts 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. 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. Third, those judges who held the ban on door-to-door canvassing unconstitutional, contrary to the hopes of the LDP, spent more time in branch offices and less as *sokatsu* than their peers who held it constitutional. All told, we conclude that LDP appointees created and maintained an incentive structure with a distinct political bias.

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