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MANAGED COURTS UNDER UNSTABLE  
POLITICAL ENVIRONMENTS:  
RECRUITMENTS AND RESIGNATIONS  
IN THE 1990s JAPANESE JUDICIARY

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## **Managed Courts under Unstable Political Environments: Recruitments and Resignations in the 1990s Japanese Judiciary**

BY J. MARK RAMSEYER & ERIC B. RASMUSEN\*

### **ABSTRACT**

*Because of the risk of political interference, in countries with managed courts jurists who share ruling-party preferences disproportionately self-select into judicial careers. During political turmoil, such jurists will find judicial careers less attractive. Orthodox potential jurists will disproportionately shun the courts, and orthodox incumbent judges will disproportionately resign. Unorthodox potential jurists, on the other hand, might find the judiciary more attractive. Combining data on a random sample of 1,605 Japanese lawyers and all 2,502 judges hired between 1971 and 2001, we locate evidence consistent with these hypotheses: after the political crisis of 1993, the recruitment of young lawyers from elite universities lagged, while the number of early resignations increased.*

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## **Managed Courts under Unstable Political Environments: Recruitments and Resignations in the 1990s Japanese Judiciary**

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In “managed judiciaries,” a personnel office rewards and punishes judges by their performance. Decide cases promptly and predictably, and a judge enjoys a tidy career. Delay adjudication or write bizarre opinions, and he finds his career derailed. Because ruling politicians can appoint political loyalists to senior positions, he may find a political bias to this career metric as well.

Given this risk of political interference, jurists in countries with managed courts who share the preferences of the ruling party disproportionately self-select into judicial careers. During political turmoil, they may find such careers less attractive. To be sure, their more heterodox peers might otherwise now choose such careers. Yet even the heterodox may fear resurgent control if the formerly dominant party returns to office, and the personnel office (staffed still by judges hired by the formerly dominant party) may block them anyway. If either occurs, recruitment during turmoil will suffer. And if incumbent judges find their careers less attractive with their appointing party out of office, resignations will increase.

For decades, Japan has maintained a managed judiciary, and in the years after 1993 found the long-time ruling party thrown from office. To test whether recruitment lagged and resignations increased after 1993, we combine career data on a random sample of 1,605 lawyers with data on all 2502 judges hired between 1971 and 2001. We find evidence consistent with both propositions.

### 1. Managed Courts in Japan

#### 1.1. Managed judiciaries and political turmoil. --

a. The case for managed courts.<sup>1</sup> The U.S. federal courts recruit prominent lawyers in mid-career, and then divorce them from any institutional incentives. Whether they work hard or shirk, write logical opinions or no, they earn the same pay, sit in the same city, and decide the same cases. Once appointed, politics affects their careers no more than does effort or intelligence. Whether they follow or flout their patron’s political preferences, they enjoy the same career.

Because most lawyers make clear their political preferences by mid-career, an U.S. President can politicize the courts by politicizing appointments. By naming men and women who share his preferences, he can impose his preferences without demanding

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<sup>1</sup> We develop these themes in Ramseyer & Rasmusen (2003: ch. 7) and Ramseyer & Ramseyer (2006).

politically skewed incentive structures. Exceptions abound, of course. Some judges radically change their political preferences, and some implement policies anathema to the Presidents who appointed them. Yet most people do maintain relatively stable preferences over the second half of their lives. By appointing judges in mid-career, the President can appoint people who will largely enforce the policies he wants enforced. They enforce them because those are the policies they themselves want enforced.

Other judiciaries -- call them "managed courts" -- appoint judges at the outset of their careers, but maintain elaborate incentive structures to ensure they perform. The implications run not only (or even primarily) to the political. Instead, they run to efficiency. After all, over the vast majority of legal disputes, politicians have no politically driven preferences. Whether left or right, most want drivers to face incentives to drive safely. They want thieves to have incentives to desist. And because contracting parties can negotiate around most bad legal rules *ex ante*, about most contract disputes politicians could not care less.

From the judges they appoint, most politicians instead want routinized adjudication. They want it for the same reason they want efficient bureaucracies -- because voters want it. If the parties to a dispute know what a judge will likely do, they need not bother to ask him. Instead, they can settle their dispute out of court by the expected litigated outcome. They then pocket the fees they would otherwise pay their lawyers. And all else equal, voters prefer politicians who provide efficient governments to less (Becker, 1983).

Because U.S. courts insulate judges from incentives to perform, they rarely provide this routinization; managed courts do. Regularly, a judicial personnel office reviews how its sitting judges perform. Those who work hard and apply predictable rules it promotes quickly. It appoints them to the important posts, moves them to the most desirable cities, and pays them the highest salaries. Those judges who randomize their decisions it names to the trivial posts, keeps in the most unpleasant cities, and never moves up the pay scale.

Although managed courts can improve judicial performance dramatically, they do so by lowering the insulation of the courts from the political branch. Because a personnel office monitors and rewards judges, those who control the personnel office can influence the political complexion of case outcomes. Because politicians can appoint loyal agents to the apex of the judicial hierarchy and those agents then monitor the personnel office, the improved efficiency thus comes at a cost in political independence.

b. The effect of political instability. Political instability introduces uncertainty into these carefully managed courts. Suppose Party D has controlled the government for several decades but now loses power to Party R. If incumbent judges expect D to return to power soon, within the courts little will change. If they expect that R may run the country indefinitely, however, some sitting judges may begin anticipatorily to implement R preferences. Others may adamantly refuse to enforce R policies. To the extent that R

stays in power and judges reject R policies, R politicians may begin overtly to intervene in the courts.

What is more, when party R takes control of legislation it necessarily constrains the ability of the D-appointed judges to make policy. After all, even if the government does not intervene in judicial careers, R politicians can now repeal judicial policy by statute. Even if D-appointed judges need not fear career penalties for opposing R preferences, they will find themselves unable to make long-term policy.

Parliamentary government magnifies these limits to judicial independence. The judicial discretion that divided government under a federal system augments, the parliamentary system constrains.<sup>2</sup> Necessarily, divided government impedes the ability of politicians to repeal judicial policy by statute. Depending on the institutional structure of the courts, it could also cabin their ability to manipulate judicial careers. Parliamentary government presents far fewer checks. The “power of the judiciary,” Iaryczower, Spiller & Tommasi (2006: 699) explains, “is limited in parliamentary systems like those of Japan or Europe, where cabinet’s control over the legislature limits the ability of the courts to innovate.”

Potentially, all this affects recruitment. Suppose, again, that party R replaces formerly dominant party D. Obviously, D-leaning potential jurists will find a judicial career less attractive than before. Although those with R preferences now might opt for such careers if they expected R to hold power long-term, the incumbent D appointees in the court’s personnel office might bar them. Even absent such interference, R-leaning jurists might see R’s long-term prospects as far from certain. If so, then those who joined the courts would be opting for a career potentially under a resurgent D party. What is more, if some R jurists did join the courts, then no matter which party controlled the government it would find itself saddled with noncompliant judges. To control them, it might then begin to intervene in judicial administration. Hence the conclusion: under political instability, judicial careers will become less attractive, fewer potential jurists will choose the courts, and (all else equal) the jurists with the highest opportunity costs (generally the most talented) will avoid the courts.

All this also affects resignations from the judiciary. After all, the incumbent judges joined the courts under the D party. To the extent R stays in power, disproportionately they will serve under a government whose preferences they do not share. To the extent that R does not stay in power, they will potentially serve under unstable governments that (given the presence of noncompliant judges) intervene more overtly in the courts. Again, judicial careers will become less attractive than before, more judges will choose to resign, and (all else equal) the judges with the highest opportunity costs will be the first to resign.

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<sup>2</sup> See generally Iaryczower, Spiller & Tommasi (2006); Bergara, Richman & Spiller (2003); Eskridge (1991); Gely & Spiller (2000); Spiller & Gely (1992).

1.2. The Japanese managed judiciary. -- For most of the post-war decades, Japan maintained just such a managed judiciary.<sup>3</sup> It recruited young jurists immediately after graduation from the national law school, the Legal Research & Training Institute (LRTI). Once appointed, those judges served a series of renewable ten year terms. At two- to three-year intervals, they rotated through a series of jobs: from city to city, from lower courts to the appellate bench, and from the bench to the administrative hierarchy. They faced mandatory retirement at age 65.

Judges in the national personnel office, the Secretariat, determined job placements. They decided which judges would work in the most attractive cities, and which would hold the most prestigious posts. They decided which new LRTI graduates to hire, and which would climb the pay scale most quickly.

These judges in the Secretariat answered to the Supreme Court Chief Justice, and the Prime Minister named the Chief Justice. Generally he appointed Supreme Court justices in their early 60s. All such justices faced mandatory retirement at 70.

Generally, the Secretariat used its control over judicial careers to reward efficient performance -- to reward judges who decided cases expeditiously and predictably. Occasionally, however, it also used it to induce judges to implement the political preferences of the ruling Liberal Democratic Party (LDP). In the occasional politically charged case, if a judge tried to implement the preferences of out-of-power parties the Secretariat sometimes derailed his career. More generally, it favored the careers of right-leaning judges over the leftist. During the 1960s a large number of jurists associated with the communist-affiliated Young Jurist League joined the courts. Over the next few decades, the Secretariat imposed on them significant career penalties.

1.3. Japan in 1993. -- After taking power in 1955, the LDP ruled continuously until 1993.<sup>4</sup> By then, it faced a formidable range of problems. Having championed enormously expensive public-works for years, it had imposed a national sales tax that alienated a broad range of voters. Under pressure from the U.S., it had instituted trade and investment reforms that threatened key party constituents. With the end of the Cold War, its broadly capitalist constituency had begun to unravel. As rural Japanese continued to migrate to the cities, its agricultural base atrophied. When the public discovered several egregious cases of bribery, it lost several prominent leaders. And after a collapse in real estate and stock prices in 1990, the economy spiraled into recession.

Facing these crises, old enemies within the party decided to settle scores. One-time Prime Ministerial candidate Ichiro Ozawa engineered a no-confidence vote, quit the LDP, and created a new party. In the ensuing election, all of his allies won re-election, but the LDP itself captured only a minority of seats. Several non-LDP parties then

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<sup>3</sup> We detailed this system in a series of articles, which we then collected into Ramseyer & Rasmusen (2003).

<sup>4</sup> We describe this state of affairs in Ramseyer & Rasmusen (2006).

regrouped. They coalesced around renegade-LDP politician Morihiro Hosokawa, and threw the LDP out of power. Yet Hosokawa could not break free of Ozawa's influence, and the coalition that had thrust him into power soon unraveled. He himself governed only eight months, and his successor (another ex-LDP politician) lasted barely two.

Maneuvering through the chaos, the Socialists struck a deal with the LDP that catapulted their leader, Tomiichi Murayama, into the Prime Minister's office. It was their first return to power since the short-lived Socialist government of 1947. The return did not last. Like Hosokawa, Murayama labored under behind-the-scenes control from experienced LDP politicians. He implemented little if any of the traditional Socialist agenda, and in the 1996 election the LDP regained sole control of the government. It has held the Cabinet ever since, albeit with small coalition partners.

The turmoil had mixed effects on the courts. In Ramseyer & Rasmusen (2006), we find that even after 1993 the Secretariat continued to enforce the same (sometimes politically skewed) career incentives. Elsewhere, some observers do claim that the Secretariat blocked the appointment of leftist jurists (Nihon minshu, 2004: 10). Did recruitment suffer? Did resignations increase?

## 2. Data and Theory

To examine whether Japanese courts found it harder to recruit and retain talented jurists after 1993, we compile data on both lawyers and judges. To hold constant the attractiveness of the two career tracks, we add as control variables the starting salary of judges and Japan's total trade (the most prestigious legal jobs, most likely to attract talented graduates, are with Tokyo's "international" law firms). As of 2004, the Japanese bar included about 21,000 lawyers. From the bar association directory (Horitsu, 2005), we randomly sample every 8<sup>th</sup> member.<sup>5</sup> This generates a dataset of 1,605 lawyers who began their careers between 1971 and 2001. From the judicial directory,<sup>6</sup> we take every judge (not a sample) hired directly into the courts between 1971 and 2001. This yields a population of 2,502 judges.

Because virtually all judges take their job immediately after the LRTI, we focus on the career choice that a jurist makes at the time he graduates from the Institute. The directory from which we obtain our judicial data stops collecting university affiliation with the LRTI class of 1997 and date of birth (from which we calculate the number of times a judge failed the LRTI exam) in 2001 -- hence the 2001 limit to the dataset (Nihon minshu, 2004). We augment the university data with an alumni directory, but even that coverage stops with the LRTI class of 1999 (Gakushikai, 1998). Thus, our data on the universities judges attended extends through 1999; our data on the number of times they

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<sup>5</sup> Because of lingering differences resulting from the U.S. occupation, we exclude lawyers in Okinawa from our sample. Also note that judges and other government lawyers are not listed in the directory.

<sup>6</sup> Nihon Minshu (2004). Although not an authorized publication, this is far more useful than any judicial directory published by the government. We describe and use earlier versions of this directory in Ramseyer & Rasmusen (2003).

failed the LRTI exam extends through 2001. The sizes of the datasets in the various regressions reflect these various constraints.<sup>7</sup>

With this data, we construct the following variables, summary statistics for which appear in Table 1.

[Insert Table 1 about here.]

**U Tokyo:** 1 if the jurist graduated from the University of Tokyo; 0 otherwise. The University of Tokyo has long maintained the premier law faculty. According to Nakazato, Ramseyer & Rasmusen (2006), University of Tokyo lawyers earn incomes at or near the very top of the bar; according to Ramseyer & Rasmusen (2003), University of Tokyo judges are among the most successful in the courts.

**U Kyoto:** 1 if the jurist graduated from the University of Kyoto; 0 otherwise. The University of Kyoto has traditionally maintained the second-most prestigious (but much smaller) law faculty.

**Flunks:** the number of times a jurist failed the bar-exam-equivalent. Whether to become a lawyer or to become a judge, one must attend the LRTI, but until 1991 the government limited admission to 500 a year. By 1998 it had increased the admits to 812 (the class would join the bar in 2000). Because of the brutal entrance examination, lawyers and judges typically failed the exam several times before passing. **Flunks** gives the number of times a lawyer or judge failed the exam, estimated (in most cases) from his birth year. According to Nakazato, Ramseyer & Rasmusen (2006), **Flunks** is strongly and negatively correlated with a lawyer's income; according to Ramseyer & Rasmusen (2003), it is also strongly and negatively correlated with a judge's performance in the courts.

**Year:** the year the lawyer or judge entered the profession.

**New Judges:** the total number of judges hired in a given year.

**Judiciary Size:** the total number of authorized judges.

**Post93:** in the recruitment regressions of Tables 2-4, this takes the value of 1 if the lawyer or judge entered the profession on or after 1993, and 0 otherwise; in the resignation regressions of Table 5, it takes the value of 1 if the judge retired from the courts on or after 1993.

**Judge Salary:** The monthly starting salary for a judge, in constant 10,000 yen, from the statutes passed near the end of most years.

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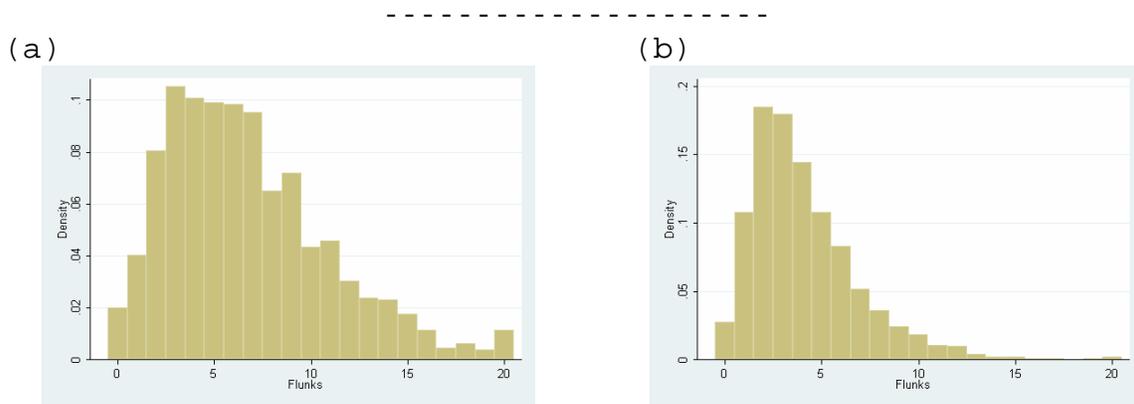
<sup>7</sup> Like the courts, the prosecutor's office also recruits LRTI graduates into its ranks. Unfortunately, we were unable to obtain data on those recruits.

**Total Trade:** Total imports and exports, in constant 10 trillion yen, from Somucho.

**Unemployment:** The national unemployment rate, in percent, from Somucho (various years).

**Per cap GDE:** Per capita gross domestic expenditure, in constant 10 billion yen, from Somucho (various years).

**Litigation/Attorneys:** The number of new civil and administrative litigated case filings (including appeals; /1000), divided by the number of attorneys (missing years interpolated). We take the litigation figures from the *Annual Report of Judicial Statistics* (at [www.courts.go.jp](http://www.courts.go.jp)).



**Figure 1: Flunk Distributions for (a) Lawyers and (b) Judges**

Our theory is simple. Each graduate from LRTI could have utility from working as a judge, from working as a standard private attorney, or from working as an attorney at one of the prestigious Tokyo “international” law firms (international in the sense of working with foreign corporations as clients or clients’ trade partners, not in the sense of being foreign-owned). This utility depends in part on the salary from serving as a judge, while the attractiveness of becoming an international law firm attorney depends on the demand for the services of international law firms. The utility of becoming a judge also depends on the individual tastes of the LRTI graduate, of course. In turn, these tastes include his political preferences -- whether he prefers the conservative and stable courts of pre-1993 or the instability of the courts post-1993, with their potential for increased judicial influence and their increased scope for liberal decisions. It is this last component of utility whose existence we will test for.

Those graduates whose relative utility is highest from becoming a judge still have to be hired as judges. The judiciary will pick the best applicants from the pool. Thus, if

fewer graduates wish to become judges, whether because of judicial salary levels, the attractiveness of the international law firms, or the political situation, the judiciary will have to pick less attractive applicants. We will observe this as fewer judges with Kyoto or Tokyo undergraduate degrees and with higher **Flunks** levels on the LRTI exam.

In our recruitment regressions (Tables 2, 3 and 4), we will use probit, and a dependent variable equal to 1 if an LRTI graduate became a judge. For our independent variables, we use indices of his ability (as measured by college and **Flunks**), indices of the attractiveness of a job (measured by a judge's starting salary, and the amount of international law business proxied by the amount of international trade), and a dummy variable for whether the period is before 1993 or after.

In our early resignation regressions, we test the effect of these considerations on incumbent judges. To the extent that a judge has utility from making policy, he will have the highest policy-related utility under the party whose preferences he shares. Disproportionately (albeit not exclusively), the judges hired before 1993 shared LDP preferences. Necessarily, their job would have generated lower levels of utility after 1993. If so, then after 1993 they should have resigned in larger numbers. Accordingly, in our regressions explaining resignations (Table 5), we use a Cox proportional hazard model, and define the "failure" event as a judge's resignation or retirement. As explanatory variables, we use measures analogous to those in our recruitment regressions.

### 3. Results

3.1. Basic recruitment results. -- By most anecdotal accounts, the courts disproportionately attract the most talented potential jurists. New judges -- as Figure 1 shows -- have failed the LRTI exam fewer times than lawyers. From 1971 to 2001, judges failed the exam a mean 4.14 times; lawyers failed it 6.7 times. From 1971 to 1999, 25 percent of the judges came from the preeminent University of Tokyo; 19 percent of the lawyers came from there.

Yet the summary statistics in Table 1 suggest that the courts may have found it increasingly hard to recruit the best graduates after 1993. Before that year, about 26 percent of the court's recruits came from the University of Tokyo, and 9 percent from the second-ranked University of Kyoto. Thereafter, only 21 percent came from Tokyo and less than 1 percent from Kyoto. Concomitantly, the percent of Tokyo graduates among those who joined the bar as lawyers rose from 17 percent to 24 percent, and Kyoto graduates from 8 percent to 9. The change in LRTI passage is more ambiguous: before 1993 new judges failed the LRTI exam a mean 4.2 times but thereafter 4.03 times; before 1993 new lawyers failed it 6.93 times but thereafter 6.18.<sup>8</sup>

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<sup>8</sup> The average size of the class of new judges did rise -- from 69 judges/class before 1993 to 88 thereafter. Note that the LRTI graduated two classes in 2000.

The probit regressions in Table 2 similarly suggest that the courts found it harder to recruit talented new judges after 1993. Consider several implications of specification (a), the basic regression on which we will focus. First, before 1993 the courts consistently attracted University of Tokyo graduates more strongly than did the bar. The marginal effect on **U Tokyo** is significantly positive. Attending the University of Tokyo increased the probability that a judge would join the courts by 3.3 percent. This is not a small effect compared to the mean probability; of our population of some 12,840 lawyers and 2,502 judges, only 16.3 percent became judges.

Second, before 1993, the courts attracted men and women who had passed the bar quickly. The marginal effect of **Flunks** is significantly (indeed, overwhelmingly) negative. Each year a potential jurist failed the exam reduced the probability that he would become a judge by 2.8 percent. Recall that we estimate the number of times a person failed the exam by calculating the age at which he passed. In fact, however, some people take the LRTI exam in order to switch careers in mid-life. We suspect that the observations with the highest Flunks scores often represent such mid-life career changers rather than multiple exam failures. Accordingly, we omit the 62 observations with Flunks scores above 15.

Third, after 1993 the courts seem to have found it harder to recruit their preferred candidates. Neither University of Tokyo nor University of Kyoto graduates found the courts as attractive as before: the marginal effects on both interacted terms are significantly negative. In fact, the change was so great that after 1993 University of Tokyo graduates actually had 3.7 percent (3.3 minus 7.0 percent) lower probability of becoming a judge, other things equal. Attending the University of Kyoto, which had had no effect earlier, now lowered the probability of becoming a judge by 13.3 percent (though Table 1's summary statistics hint that the effect may have started before 1993).

Plausibly, some of the court's difficulty in recruiting the most talented jurists after 1993 stem from the larger cohorts it apparently hoped to hire. Where the courts hired a mean 70 judges per year before 1993, thereafter it hired an average of 113 judges per year. Unless the talent pool increased, that would entail hiring judges who would not have been hired in earlier years. Specification (a) does include the variable **New Judges**, the size of the cohort of new judges. It is insignificant, telling us that the increase in size was too small to affect clearly the probability of becoming a judge. What about the average quality? In specification (b) we interact the number of judges the court hired in a given year with the proxies for judicial quality. The marginal effects on the university variables are insignificant. The marginal effect of the interaction term on **Flunks**, however, is significantly positive: the more judges the courts tried to hire, the higher the **Flunks** score they were forced to accept.

Specification (c) takes a different approach to the problem of varying cohort size. Here we include all lawyers in the sample, but only those 50 judges with the lowest **Flunks** scores for each year. These are judges who presumably would have been hired in any of the various sizes of cohorts we observe; we are essentially throwing out the bottom of the larger classes in the 90's. Under this data constraint, the marginal effect on

**Post93** interacted with University of Tokyo falls by over 50 percent. Much of the apparent difficulty the court faced in hiring the brightest judges seems to have resulted not from increased reluctance of the best young lawyers to join the courts, but from the desire to increase the number of judges hired each year

[Insert Table 2 about here.]

Our economic control variables take the directions expected. All else equal, we would expect young jurists to opt for judicial careers when judicial pay is highest, and so we find: the marginal effect of **Judge Salary** is positive and significant, with an elasticity of 0.86 ( $=.025*21/.61$ ) computed at the mean levels of the variables. Note, however, that if the government raises salaries in response to unmodelled taste changes that reduce the probability of becoming a judge, this control variable is endogenous. Because the most talented Tokyo lawyers work at the firms specializing in international trade, the courts should find it easiest to recruit talented jurists when international trade flows are low, and that is what we find: the marginal effect on **Total Trade** is negative and significant, with an elasticity of 0.12 ( $=-.010*7.47/.61$ ).

3.2 Robustness checks. -- In Tables 3 and 4 we report a variety of robustness checks. These results largely confirm our findings above -- though, as we will see shortly, they do introduce some ambiguity. To facilitate comparison, we reproduce the basic Regression (a) from Table 2 as Regression (a) in each table.

Table 3 looks at different ways of handling the **Flunks** variable, at the effect of adding additional but incomplete data, and at the effect of additional economic control variables.

In Table 2, we dropped those jurists with **Flunks** scores above 15. Recall that we estimated this score from year of birth or college graduation. We doubt that many with scores above 10 actually failed the exam that often. Instead, most probably switched to the LRTI in mid-career to switch professions. In Reg. (b) of Table 3, we include these high **Flunks** observations. Almost nothing changes as a result.

[Insert Table 3 about here.]

In Reg. (c) of Table 3 we also include the high **Flunks** observations, but we break the **Flunks** variable into four levels to check for nonlinear effects (the omitted variable is  $15 \leq \text{Flunks} \leq 20$ ). Before 1993, the marginal effect of **Flunks** fell from level to level -- as the strongly negative marginal effect on **Flunks** implies. The interaction terms between the various **Flunks** levels and a dummy for the judge's entering year being after 1993 are all insignificant, showing that the political events of 1993 did not have differential effects on the tendency of lawyers with different **Flunks** levels to become judges.

Our data on the **Flunks** score for judges continues to 2001 while data on their university affiliation stops at 1999. Because university affiliation was unavailable, our earlier regressions used data only through 1999. To exploit our data through 2001, in Reg. (d) we drop the university variables while adding the 383 post-93 observations. The uninteracted **Flunks** variable remains strongly negative, with a coefficient almost identical to before, but now there is a significant positive effect after 1993. In an unreported regression that dropped the university variables but only included data up through 1999, that same post-93 effect appears. This suggests that the increase in the marginal effect of the interacted **Flunks** in Reg. (d) derives from our dropping the university variables rather than from adding the classes of 2000 and 2001.

Finally, regression (e) of Table 3 adds two new macroeconomic variables to our basic specification: **Gross Domestic Expenditure per Capita** and the **Unemployment** rate, as measures of economic well-being and the business cycle. It is not clear what effect we might expect these to have on someone's tendency to become a judge, but we include them to check on the robustness of our earlier results. Both new variables come in significant, but they have very little effect on the results from our basic regression; the same variables are statistically significant as before, and the marginal effects remain almost the same.

[Insert Table 4 about here.]

Table 4 tests another dimension of robustness: the effect of time. The effect of time is, of course, the central question of this paper, so we wish to explore its effect carefully. As before, regression (a) is the same as our basic regression (a) of Table 2.

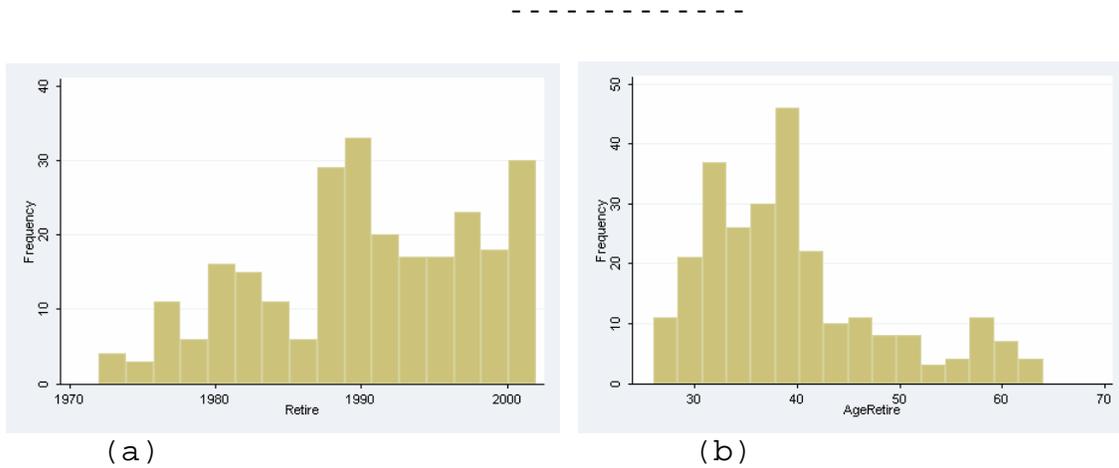
In Reg. (b) of Table 4 we substitute year dummies (coefficients unreported) for the **Post-93** dummy used in the other regressions. The effect of **Judicial Salary** increases, but otherwise the coefficients remain unchanged, except that **New Judges** must be dropped because of collinearity.

In Reg. (c), we use a time trend (the year the jurist left LRTI) instead of time dummies and interact the university variables and **Flunks** with it. The results on the uninteracted variables once again indicate that the University of Tokyo and Kyoto graduates were more reluctant to become judges after 1993. The time trend interacted with **UTokyo** is insignificant, but it is significant and negative for **UKyoto**, further evidence that more than political change was causing Kyoto graduates to disfavor the judiciary over time.

In Regressions (d) and (e) of Table 4 we experiment with alternatives to 1993 as cutoff years. Because the LDP lost power in 1993, in Table 2 we set the break at that year. Using 1991 or 1995 makes little difference, though this is not very informative, since in all three cases most of the estimation in the regression is coming from a comparison between years before 1991 and years after 1995.

4. Retirements

Thus far we have concentrated on whether fewer talented young graduates of LRTI chose to become judges after the political events of 1993. For those who had joined in the 1980's and earlier, however, it was not too late to switch. As Figure 2 shows, some judges did resign before the mandatory retirement age of 65. In fact, most of those who resigned were quite young, in their 30's (judges hired in the early 1970s were only starting to reach retirement age in the late 1990s). Moreover, it seems that early resignation started not in 1993, but shortly before, in the late 1980's.



**Figure 2: Retirements before Age 65  
by (a) Year and (b) Age**

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Does the fact that the early resignations started some five years before 1993 indicate that these young judges were particularly prescient about political events? That is highly unlikely; the LDP's 1993 loss was unforeseen, as was the real estate and stock market crash that initiated the 90's slump that contributed so much to the LDP's fall. Rather, this is a clear case where multiple regression could be helpful. During the late 80's boom, resigning and becoming a private lawyer was financially more attractive than ever before. In the succeeding bust it was not, but political changes may have made resignation attractive for some judges. The fact that resignations continued well after the boom was over suggests that the post-1993 political instability indeed made a difference.

To try to untangle these effects, we use the estimation method of Cox's proportional hazard model. Ordinary regression methods are unsuitable because we would not expect time till resignation necessarily to be normally distributed -- both because many judges do not resign early, and because our data is censored (our sample ends before most of the judges have resigned either early or at the normal age). Cox's method is to estimate the hazard rate instead: the probability that a judge resigns in a

year given that he has stayed in the judiciary that long already. We assume that a judge's hazard rate equals a baseline hazard rate, similar to a constant, times an exponential term that measures the effect of variables such as his **Flunks** score and whether the year is after 1993.

$$\text{Hazard rate}(\text{judge } i \text{ in year } t) = (\text{Baseline hazard rate})\exp(b_1*\text{Flunks} + b_2*\text{Post93}+\dots)$$

This equation can be rewritten as

$$\text{Log}(\text{HR}(\text{judge } i \text{ in year } t) / (\text{Baseline HR})) = b_1*\text{Flunks} + b_2*\text{Post93}+\dots$$

This log ratio can straightforwardly be regressed on the x variables. The unit of observation is one year for one judge who has not yet resigned, and what we are estimating is how his probability of resigning in that year depends on the various independent variables shown in Table 5. A hazard rate greater than 1 is analogous to a positive regression coefficient; a rate less than 1 is analogous to a negative coefficient.

[Insert Table 5 about here.]

Regression (a) is included to illustrate the danger of comparing resignations before 1993 to those after 1993 without including control variables. It seems to show that resignations fell significantly after 1993. So they did, we know from Table 2, and the result is not unpalatable in theory; we could explain that judges like political instability because of the opportunity it gives them to make policy instead of elected officials.

But regression (a) fails to include two key variables, **Judiciary Size** and **Cases per Attorney**. **Judiciary Size**, is a measure of the opportunities for promotion available in the judiciary. If there are more judges, there are more attractive positions for senior judges. And if the courts have grown since they joined, incumbent judges should find it more attractive to stay. Regression (b) in Table 5 adds **Judiciary Size**, which has a highly significant negative effect on resignations. Moreover, **Post93** remains highly significant but switches directions — conditioning on the increase in the judiciary in the 90's, resignations rise rather than fall.

Regression (c) of Table 5 is the most complete specification, adding **Cases per Attorney** as a measure of the demand for private attorneys. This variable has a significant positive effect on resignations, as we might expect. The effect of **Judiciary Size** is almost identical to what it was in regression (b), and **Post93** remains positive, sizeable (a hazard ratio of 2.96, though this magnitude is hard to evaluate without taking the interaction variables into account as well) and highly significant. Judges were more likely to resign after 1993, other things equal. We cannot reject the null hypothesis that talented and untalented judges resigned in equal proportions, however, since the hazard ratios for the talent variables interacted with **Post93** are uniformly insignificant.

Note too that **Flunks** is highly significant, with a high hazard ratio. The hazard ratio for **Flunks** of 5.652 (=exp(b1) in the equation above) means that an increase of one in **Flunks** multiplies the baseline hazard rate by 5.652. Independent of time, less talented

judges are more likely to bail out early.<sup>9</sup> The regression also confirms what the figures told us: that it is the youngish judges who are most likely to resign.<sup>10</sup>

## 5. Conclusions

Because of the potential for political bias, jurists sympathetic to the ruling party should disproportionately self-select into judicial careers. During political turmoil, they will find such careers less attractive, and incumbent judges may block the courts from hiring heterodox jurists who might otherwise choose to join the courts. As a result, recruitment into managed courts should suffer.

Japan has long maintained a managed judiciary, and during the years after 1993 experienced prolonged political turmoil. We assembled data on lawyers and judges who entered the profession before and after 1993 to test whether the Japanese courts experienced problems in recruiting and resignations after that year. We find evidence mildly supporting the proposition that they did.

The picture displayed by resignations is somewhat different from what we saw with new hires, however. Resignations clearly increase after 1993, but the rate of resignation is independent of talent. In the case of new hires, on the other hand, we found that the judiciary had become less attractive to graduates of elite universities.

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<sup>9</sup> From other research, we know that these are the judges who will have the least successful careers in the courts. See Ramseyer & Rasmusen (2003). We also know that -- outside of Tokyo -- attorneys with high Flunks scores are not significantly less successful than those with lower scores. See Nakazato, Ramseyer & Rasmusen (2006).

<sup>10</sup> Anecdotally, some observers suggest that judges who have spent long decades in the highly bureaucratic judiciary find it harder to succeed in the entrepreneurial world of private practice.

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**Table 1**  
**Selected Summary Statistics**

A. Mean university and flunk figures, change over time:

	1971-75	1976-80	1981-85	1986-90	1991-92	1993-97	1998-99
1. Judges -- means, followed by n:							
U Tokyo	.258 (365)	.231 (359)	.326 (291)	.260 (346)	.234 (162)	.215 (506)	.204(191)
U Kyoto	.110 (365)	.131 (359)	.079 (291)	.054 (346)	.006 (162)	.004 (506)	.000(191)
Flunks	3.40 (361)	4.02 (359)	3.99 (290)	4.72 (340)	5.75 (152)	4.54 (495)	3.48(454)*
2. Lawyers -- means, followed by n:							
U Tokyo	.177 (226)	.162 (234)	.148 (271)	.169 (172)	.309 (55)	.182 (154)	.270(74)
U Kyoto	.062 (226)	.085 (234)	.089 (271)	.093 (172)	.055 (55)	.084 (154)	.081(74)
Flunks	5.97 (226)	6.40 (234)	7.30 (271)	8.30 (171)	6.98 (52)	7.27 (155)	5.25 (181)*

B. Probability of becoming a judge, by Flunk score:

Flunks	1971-92	1993-2001
0-5	.26	.34
6-10	.09	.20
11-15	.04	.06
16-20	.03	.02

N.B.: To estimate the total number of lawyers in each cell, we multiply the sampled number by 8. Observations with missing Flunks score were dropped.

C. Other variables:

	1971	1976	1981	1986	1991	1996
Judge Salary**	17.32	18.93	19.19	20.45	21.79	23.14
Total Trade**	4.514	6.560	8.240	6.556	7.809	8.390
Per Cap GDP**	1.870	2.156	2.711	3.092	3.839	3.947
Unemployment (%)	1.2	2.0	2.2	2.8	2.1	3.4
New Judges	69	79	62	70	97	100
Early Retirements	--	5	12	6	14	4

Notes: \* Covers through 2001. \*\* Adjusted to Constant prices.

Sources: Horitsu shimbunsha, ed., Zenkoku bengoshi taikan [National Survey of Lawyers] (Tokyo: Horitsu shimbun sha, 2005); Nihon minshu horitsuka kyokai, ed., Zen saibankan keireki soran dai 4 ban [Overview of Careers of All Japanese Judges, 4th ed. (Tokyo: Konin sha, 2004); Somucho, ed., Nihon tokei nempo [Japan Statistical Yearbook] (Tokyo: Nihon tokei kyokai, various years); Gakushikai, ed., Kaiin shimei roku [Names of Members] (Tokyo: Gakushi kai, 1998).

**Table 2**  
**Probit Estimates of the**  
**Probability of Becoming a Judge**

	(a)	(b)	(c)
UTokyo	0.033 (2.21)**	0.026 (0.28)	0.005 (0.78)
UKyoto	-0.005 (0.28)	-0.092 (0.98)	-0.002 (0.23)
Flunks	-0.028 (14.97)***	-0.049 (4.28)***	-0.021 (17.42)***
Post93	-0.028 (0.83)	0.021 (0.44)	-0.005 (0.30)
Post93UT	-0.070 (2.92)***	-0.070 (1.83)*	-0.019 (1.93)*
Post93UK	-0.133 (5.47)***	-0.133 (4.98)***	
Post93*Flunks	0.004 (1.08)	-0.005 (0.87)	-0.004 (1.17)
Judge Salary	0.025 (4.65)***	0.024 (4.56)***	0.011 (4.96)***
NewJudges/100	0.092 (1.62)	-0.074 (0.65)	-0.050 (2.09)**
Total Trade	-0.010 (1.86)*	-0.009 (1.77)*	-0.002 (0.89)
NewJudges*Flunks/1000		0.313 (1.91)*	
NewJudges*UT/1000		0.078 (0.06)	
NewJudges*UK/1000		1.658 (0.93)	
Observations	3301	3301	2611
Flunk treatment	<u>≤15</u>	<u>≤15</u>	Top 50 judges

Notes: The table gives the marginal effects of the probit regression with the dependent variable equal to 1 if a jurist became a judge. The absolute value of the robust z-statistics are in parentheses, with (\*, \*\*, \*\*\*) for significance at the (10%, 5%, 1%) levels. Regressions are weighted by the procedure of Manski & Lerman (1977) for choice-based stratified samples: .16/.66 for the judges, and .83/.34 for the lawyers.

Observations without **Flunks** scores are dropped. Only the years 1971-1999 are included.

Sources: See Table 1.

**Table 3**  
**Flunks and Macroeconomic Variables as Determinants of**  
**Becoming a Judge**

	(a)	(b)	(c)	(d)	(e)
UTokyo	0.033 (2.21)**	0.033 (2.35)**	0.035 (2.44)**		0.033 (2.17)**
UKyoto	-0.005 (0.28)	-0.003 (0.18)	-0.007 (0.37)		-0.002 (0.09)
Flunks	-0.028 (14.97)**	-0.024 (14.49)**		-0.026 (15.00)***	-0.028 (14.98)***
Post93	-0.028 (0.83)	-0.014 (0.43)	0.096 (0.56)	-0.060 (2.24)**	0.011 (0.26)
Post93*UT	-0.070 (2.92)***	-0.067 (2.94)**	-0.066 (2.76)***		-0.073 (3.09)***
Post93*UK	-0.133 (5.47)***	-0.127 (5.50)***	-0.131 (5.43)***		-0.132 (5.49)***
Post93*Flunks	0.004 (1.08)	0.001 (0.41)	-0.013 (1.54)	0.006 (2.26)**	0.002 (0.64)
Judge Salary	0.025 (4.65)***	0.022 (4.55)**	0.023 (4.33)***	0.028 (5.66)***	0.005 (0.51)
NewJudges/100	0.092 (1.62)	0.095 (1.76)*	0.092 (1.67)*	-0.006 (0.23)	0.097 (1.30)
Total Trade	-0.010 (1.86)*	-0.010 (2.08)**	-0.011 (2.08)**	-0.017 (3.75)***	-0.012 (2.11)**
PerCapGDE					0.073 (3.64)***
Unemployment Rate					-0.030 (1.98)**
Flunks 0-4			0.297 (9.12)***		
Flunks 5-9			0.132 (4.52)***		
Flunks 10-14			0.056 (1.72)*		
Post93*Flunks0-4			-0.056 (0.49)		
Post93*Flunks5-9			0.033 (0.30)		
Post93*Flunks10-14			0.012 (0.15)		
Observations	3301	3361	3361	3744	3301
Flunks	<15	all	all	<15	<15
End Year	1999	1999	1999	2001	1999

Notes: Table 3 gives the marginal effects of the probit regression with the dependent variable equal to 1 if a jurist became a judge. The absolute value of the robust z-statistics are in parentheses, with (\*,\*\*,\*\*\*) for significance at the (10%,5%,1%) levels. Regressions are weighted by the procedure given in Manski & Lerman (1977): .163/609 for the judges and .838/.391 for the lawyers. Observations with **Flunks** greater than 15 are dropped. Years 1971-1999 only. Regression (a) is the same as Table 2, Reg (a).

Sources: See Table 1.

**Table 4: Time Cutoffs and Trends  
as Determinants of Becoming a Judge**

	(a)	(b)	(c)	(d)	(e)
	Judge	Judge	Judge	Judge	Judge
UTokyo	0.033 (2.21)**	0.033 (2.19)**	0.292 (0.05)	0.043 (2.72)***	0.022 (1.54)
UKyoto	-0.005 (0.28)	-0.004 (0.21)	1.000 (3.01)***	0.002 (0.10)	-0.013 (0.74)
Flunks	-0.028 (14.97)***	-0.028 (15.07)***	-0.929 (1.54)	-0.030 (15.07)***	-0.026 (14.99)***
Post93	-0.028 (0.83)		-0.030 (0.67)		
Post93*UT	-0.070 (2.92)***	-0.073 (3.04)***	-0.068 (1.88)*		
Post93*UK	-0.133 (5.47)***	-0.131 (5.45)***	-0.114 (2.40)**		
Post93*Flunks	0.004 (1.08)	0.002 (0.61)	-0.002 (0.40)		
Judge Salary	0.025 (4.65)***	0.036 (3.38)***	-0.010 (0.94)	0.020 (3.42)***	0.023 (4.62)***
NewJudges/100	0.092 (1.62)		0.206 (3.03)***	0.036 (0.68)	0.080 (1.55)
Total Trade	-0.010 (1.86)*	-0.018 (1.78)*	-0.016 (2.83)***	-0.009 (1.75)*	-0.007 (1.36)
Year			0.007 (2.22)**		
Year*UT			-0.093 (0.04)		
Year*UK			-10.270 (3.01)***		
Year*Flunks			0.455 (1.49)		
Post95					0.002 (0.05)
Post95*UT					-0.069 (2.39)**
Post95*UK					-0.133 (4.94)***
Post95*Flunks					-0.003 (0.67)
Post91				-0.015 (0.45)	
Post91*UT				-0.073 (3.40)***	
Post91*UK				-0.130 (5.79)***	
Post91*Flunks				0.010 (3.04)***	
Year Dummies	no	yes	no	no	no

Notes: n = 3,301. Table 4 gives the marginal effects of the probit regression with the dependent variable equal to 1 if a jurist became a judge. The absolute value of the robust z-statistics are in parentheses, with (\*, \*\*, \*\*\*) for significance at the (10%, 5%, 1%) levels. Regression (a) is the same as Table 2, Reg (a). Newjudges/100 was dropped in regression (b) because of collinearity. Regressions are weighted by the procedure given in Manski & Lerman (1977): .163/609 for the judges, and .838/.391 for the lawyers. Observations without **Flunks** scores or with **Flunks** greater than 15 are dropped. are dropped.

Sources: See Table 1.

**Table 5:  
Cox Regressions for Retirement**

	(a)	(b)	(c)
Flunks	5.090 (15.05)***	5.561 (14.66)***	5.652 (14.71)***
UTokyo	1.001 (0.01)	1.388 (1.60)	1.222 (0.56)
UKyoto	0.379 (2.88)***	0.895 (0.37)	0.878 (1.76)*
Post93	0.038 (10.88)***	3.905 (3.73)***	2.962 (2.78)***
Post93*Flunks	1.077 (1.56)	1.044 (0.89)	1.043 (1.12)
Post93UT	1.073 (0.24)	0.673 (1.32)	0.770 (0.68)
Post93UK	2.046 (1.31)	0.584 (1.00)	0.602 (0.19)
Age	0.155 (13.55)***	0.162 (12.58)***	0.159 (12.44)***
Age Squared	1.00 (2.65)***	1.001 (1.43)	1.001 (1.12)
Judiciary Size		0.980 (17.49)***	0.980 (16.31)***
Litigation/Attorneys			1.042 (1.99)**

Notes: n = 2,172. Table 5 gives the hazard ratios (exp(beta)) from Cox regressions where the "failure" event is resignation or retirement. Hazard ratios less than 1 indicate a negative effect. The absolute value of the robust z-statistics are in parentheses, with (\*, \*\*, \*\*\*) for significance at the (10%, 5%, 1%) levels.

Sources: See Table 1.