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Why the Japanese Taxpayer Always Loses

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Abstract: The tax office wins most cases in Japan, and we ask why that might be. The well-known Priest-Klein hypothesis suggests that the high government win-rate should have nothing to do with any bias in courts. Rather than rest with theory, however, we ask empirically whether the Japanese government uses its ability to manipulate judicial careers to enforce a pro-government bias in tax cases. Using career data on over 300 judges, we explore the impact of how a judge decides a tax case on the judge's career. We find no evidence that judges who favor taxpayers suffer. Crucially, however, if a case is reversed on appeal, the trial judge's career does take a turn for the worse. On the other hand, the career of a judge who rules for the taxpayer and has the ruling affirmed on appeal is less likely than average to go bad. Apparently, the government rewards accurate -- not pro-government -- adjudication.

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Drafts of this paper can be found in ascii (*.txt) and Adobe Acrobat (*.pdf) formats on the Web at WWw.bus.indiana.edu/~erasmuse/@Articles /Unpublished/japtax.txt and Www.bus.indiana.edu/~erasmuse/@Articles/ Unpublished/japtax.pdf. Footnotes starting with xxx are notes to ourselves.

When sued, the Japanese government always wins. At least, almost always. Year in and year out, roughly 75-95 percent of the time it wins. The question is why. By the occasional word on the street in Japan, it wins because it cheats -- because it manipulates the judicial apparatus to obtain decisions biased in its favor.

Crucial to this claim, judges in the lower courts in Japan, like judges in most countries outside the United States, work as career civil servants. They begin their careers as judges, remain so for most of their working lives, and retire as judges. During that time, they work under the supervision of the administrative office of the lower courts, the Secretariat. The Secretariat answers to the Supreme Court justices, and the justices are appointed by the Cabinet. At the behest of the Secretariat, the lower-court judges then move up and down the judicial hierarchy and all around the country. Through this indirect influence over judicial careers, the Japanese Cabinet thus has the power to reward and punish judges by the complexion of the opinions they write. The question is whether -- or when -- it uses it.

Even if the government did manipulate judicial careers, the connection to verdict rates would remain problematic. According

to the well-known Priest-Klein hypothesis,[1] judicial bias should have nothing to do with such rates. If Priest and Klein are right (as we believe they are) the word on the street is wrong. Put simply, Priest and Klein point out that if a judge is biased in favor of the government, the government may be emboldended to bring shakier cases. If so, then ultimately its verdict rate may look no better than if it faced unbiased judges. The Secretariat might or might not be manipulating judicial careers, but from verdict rates we would not be able to tell.

Rather than rest on this logic, we use data from tax litigation to test directly whether the Secretariat punishes judges for deciding against the government. More specifically, we combine data on the careers of individual judges with data on the opinions they write. We then ask whether (holding constant a variety of other factors) judges who decide cases in favor of the government do better than those who favor taxpayers.

We have used this technique elsewhere, and found that it captures a wide array of influences on judicial careers in Japan. For example,

 i. Judges from elite schools have more successful careers than others.[2]

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[1] George Priest & Benjamin Klein, The Selection of Disputes for Litigation, 13 **J. Legal Stud.** 1 (1984).

[2] J. Mark Ramseyer & Eric B. Rasmusen, Judicial Independence in a Civil Law Regime: The Evidence from Japan, 13 **J. Law, Econ. & Org.**259, 274 tab. 3 (1997); J. Mark Ramseyer & Eric B. Rasmusen, Why the Japanese Conviction Rate Is so High (Working Paper, Kelley School of Business, Indiana University; Working Paper, John M. Olin Center for Law, Economics and Business, Harvard Law school, 1998).

ii.   Judges who flunked the Japanese bar-exam-equivalent (the pass rates hovers between 1 and 4 percent; this is the LRTI entrance exam, described below) fewer times have more successful careers than those who flunked it more often.[3]

iii. Judges who in the 1960s joined a leftist bar group (the YJL, described below) had less successful careers than those who did not.[4]

iv.   Judges who acquit defendants do worse than those who always convict.[5]

 v.   Judges who held unconstitutional a section of the electoral law favorable to the ruling party did worse than those who held it constitutional.[6]

Here, we ask a similar question: do judges who favor taxpayers have less successful careers than those who always favor the government?

    Although we find that tax opinions affect a judge's career, the Secretariat does not punish judges for writing pro-taxpayer opinions. In fact, judges who write pro-taxpayer opinions do no worse in their careers than those who favor the government. Rather, the Secretariat punishes judges for writing wrong opinions. On average, a judge who finds a tax opinion reversed on appeal will spend more time in provincial branch offices and less time with prestigious administrative responsibilities. Even stronger, a judge who rules for the taxpayer and has the

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[3] Ramseyer & Rasmusen, supra note 2 (1997), at 274 tab. 3, 277 tab. 5.

[4] Id., at 277 tab. 5.

[5] Ramseyer & Rasmusen, supra note 2 (1998).

ruling affirmed on appeal is <u>less</u> likely than average to end up in a branch office. Simply being pro-taxpayer is not the judge's problem; getting reversed is.

This leaves two questions: (i) if the Secretariat does not punish judges for favoring taxpayers, why is the verdict rate so high?, and (ii) if the government could manipulate judicial careers to win cases, why does it not do so? On the first question, one possibility is clear: the high verdict rate may reflect a rational case selection strategy by the government. As a repeat player in the courts, perhaps the government disproportionately chooses to litigate those disputes most likely to move precedent in an advantageous direction.

The second question is harder. If the government could manipulate judicial careers to win, why does it not do so? Given the additional tax revenues it could earn by subtly altering judicial incentives, is it leaving money on the table? We conclude this article by explaining how the reason for the dogged independence of the courts on this score may lie in the political economy of Japanese electoral competition.

We begin by detailing the verdict rates in Japan, explaining the court structure, outlining the common reaction to the phenomenon, and noting the implications of Priest-Klein (Section I). We then use data from reported opinions and judicial careers to test whether the Secretariat uses its control over judicial

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[6] Ramseyer & Rasmusen, <u>supra</u> note 2 (1997), at 285 tab. 9.

appointments to reward pro-government opinions (Section II). We conclude by exploring the political economy of judicial manipulation and electoral competition (Section III).

I.   The Problem

A.   The Rates:

In 1994, Japanese district courts decided 154 civil disputes between taxpayers and the government. Of these, the government won 94 percent. The government also litigated another 622 non-tax administrative cases. Of those, it won 93 percent.[7] This is not unusual. Year after year, the government wins by similar odds.[8]

Because the Japanese government publishes a relatively high proportion of the tax opinions, the verdict rate among published opinions tends to track the total. Given that the case reporters had apparently not yet finished publishing 1994 opinions at the time of our writing, take the 1989 opinions. That year, the government litigated 182 tax cases and won 87 percent. It litigated another 355 non-tax administrative cases and won 90

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[7] **Saiko saiban sho, Shiho tokei nempo [Annual Report of Judicial Statistics]** tab. 80 (Tokyo: Hoso kai, 1994).

[8] J. Mark Ramseyer, Kokuzeicho wa naze katsuka: "Ho to keizaigaku" kara mita shoso ritsu [Why the National Tax Office Wins: Verdict Rates from a "Law & Economics" Perspective], 934 **Jurisuto** 130 (1989); **J. Mark Ramseyer, Ho to keizaigaku: Nihon ho no keizai bunseki [Law & Economics: An Economic Analysis of Japanese Law]** ch. 3 (Tokyo: Kobundo, 1990).

percent.[9]   Of the tax cases, 92 were published (51 percent,

including both civil and criminal tax cases).   Of those published

cases, the government won 92 percent.   Thus, the win rates on

published and unpublished tax cases are almost identical.

B.   Japanese Courts:

The puzzle is what to make of these high government win

rates.   For its critics, the government wins because it

manipulates the courts to bias them in its favor.   It can do so,

they explain, because of the career structure of the judiciary.

Because it hires young and unproven jurists into the court, it

manipulates career paths to induce them to work carefully and

hard.   Argue the critics, it also uses that career path to induce

them to favor the government.[10]

To understand how the government can intervene in the

courts, consider the structure of the Japanese judiciary.[11]

During most of the post-war decades, the conservative Liberal

Democratic Party (LDP) controlled the Japanese Parliament.   As

majority party, it also controlled appointments to the 15-member

Supreme Court.   As a moderately conservative party facing a

socialist and communist opposition, it primarily appointed

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[9] **Saiko**, supra note 7, at tab. 80 (1989).

[10] An argument we ourselves have made. See Ramseyer & Rasmusen, supra note 2 (1997), at 280-82.

moderately conservative justices. To prevent justices from shifting their ideology during their tenure, it appointed them late enough in life that they did not have time to shift -- generally in their early 60s, shortly before the mandatory retirement age of 70. The LDP felt safe in doing so because it had a secure hold on Parliament for most of this period; the suspense in Japanese politics was in which faction of the LDP would hold power, not in whether the party itself would appoint the next judge.

Typically, the Cabinet names a majority of Supreme Court justices from the lower courts. Generally, it has kept on the bench at least one justice who earlier ran the Secretariat and knows the intricacies of administering judicial careers. In turn, these Supreme Court justices supervise the Secretariat.

Cruial to the discussion here, Japanese lower court judges do not sit in one court for most of their careers. Instead, they join the courts straight out of law school. They then move around the country at 3-year intervals. At the behest of the Secretariat, they move from court to court -- from the desireable metropolitan courts to rural branch offices, from courts of appeal to family courts, from jobs with prestigious administrative responsibilities to jobs without.

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[11] See generally Ramseyer & Rasmusen, supra note 2 (1997); **J. Mark Ramseyer & Frances McCall Rosenbluth, Japan's Political Marketplace** chs. 8-9 (Cambridge: Harvard University Press, 1993).

Nominally, all judges are created equal. In fact, some are noticeably more equal than others. Some judges spend many years in the coveted metropolitan courts, spend few years in the widely despised branch offices, and carry prestigious administrative responsibilities. Others toil long years in small-town branch offices, with rarely a stint in the cities or on non-judicial work. The Secretariat promotes judges at different rates because it hires them before it has good information about their abilities and work habits. Because not all are congenital workaholics, it tries to reward care and effort. Because not all are brilliant — though remember that the LRTI examination is one of the toughest in any profession in any country -- it tries to give the brightest the most responsible jobs.

C. Explanations:

For high government verdict rates, there seems a straightforward explanation: the government rewards pro-government judges. Yet more than a decade after the Priest-Klein hypothesis, one should wonder. Even if the courts relentlessly favored the government, rational taxpayers and bureaucrats would take that bias into account when they bargained. If they did, the bias would shape the terms of their out-of-court settlements. It would not, however, affect the government's rate of victory among the few cases that proceeded to litigation.

By the original Priest-Klein hypothesis, legal bias or no observed verdict rates should hover around 50 percent. Researchers since have failed to confirm this 50 percent hypothesis.[12] They have, however, left intact the intuition that legal bias will not correlate with verdict rates. If so, the Japanese tax office may win consistently -- but that verdict rate is no evidence of biased judicial incentives.

Instead, one of the more straightforward reasons for the high government win rates in Japan is a repeat-player strategy.[13] Suppose one party faces repeated disputes over similar issues. Suppose further that judges generally follow precedent, and change precedent only reluctantly. If so, then repeat players will disproportionately select for litigation those cases where they see a good chance of shifting the law in their favor. As Priest and Klein observed, a "systematic difference in stakes to the parties" will cause the observed verdict to differ from 50 percent.[14] The Japanese tax office is exactly such a repeat player. Presumably, it adopts exactly such a litigation strategy.

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[12] E.g., D. Kessler, T. Meites & G. Miller, Explaining Deviations from the Fifty-Percent Rule: A Multimodal Approach to the Selection of Cases for Litigation, 25 **J. Legal Stud.** 233 (1996); Joel Waldfogel, The Selection Hypothesis and the Relationship between Trial and Plaintiff Victory, 103 **J. Pol. Econ.** 229 (1995).

[13] Ramseyer, supra note; **Ramseyer**, supra note, at ch 3.

[14] Priest & Klein, supra note 1, at 40.

As data consistent with this hypothesis (inconclusive to be sure), compare the verdict rates of national and municipal governments.[15] As Table 1 shows, the national government tends to win at a higher rate than local governments.[16] Given the different incentives that national and local governments face, the phenomenon is consistent with rational levels of investment in precedent. Unlike the national government, local governments face a collective action problem: should any one government invest in the litigation that produces favorable precedent, much of the gain will accrue to other local governments. As a result, one would expect the national government to invest more heavily in precedent -- and higher national verdict rates would result.

The government also maintains a case publication policy that suggests an interest in tax precedent. In tax but not in most fields, it publishes a high percentage of district court opinions. In most civil litigation, the government publishes officially only a small percentage of the lower court opinions. In tax, however, it maintains an official reporter devoted

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[15] The data are ultimately inconclusive because (i) the Japanese government faces some limits (often not binding) on its ability to settle disputes, and (ii) the courts tend to give broader discretion to national than to local bureaucrats. On the legality of settlements by the government, compare K.K. Nishizawa v. Nagasaki kenshiji, 12 Gyösai reishü 2505 (Nagasaki D. Ct. Feb. 3, 1961) (may settle) with Sasakawa takushoku ringyö, K.K., 7 Kakyü minshü 1895 (Tokyo D. Ct. July 14, 1956) (may not settle); for a contrary interpretation of the data in Table 1, see Ramseyer & Nakazato (1998: ch. 8).

[16] Xxx We'll want to do a formal statistical test for equality of tw binomial probabilities, which will probabl confirm this conclusion, since the sample size is so big. .

exclusively to the field. Of the 182 non-criminal tax cases litigated in 1989, xxx appeared in an official reporter.[17]

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[17] To be sure, many of these cases involved primarily factual cases, necessarily disputes with less precedential value.

Table 1:
Verdict Rates, by Government Sued

	A. National Government	B. Local Government	B/A
1986	9.16 (251)	6.25 (64)	.68
1987	10.43 (211)	14.00 (50)	1.34
1988	7.20 (250)	19.23 (52)	2.67
1989	9.16 (273)	12.20 (82)	1.33
1990	7.93 (353)	6.10 (82)	.77
1991	9.46 (296)	9.46 (74)	1.00
1992	11.01 (318)	9.68 (93)	.88
1993	7.41 (432)	7.56 (119)	1.02
1994	6.30 (492)	13.08 (130)	2.08
1995	10.30 (369)	14.73 (129)	1.43
Total:	8.63 (3245)	11.09 (875)	1.29

Notes: The percentage of petitioner wins in suits resulting in an opinion (hanketsu) is followed by the total number of suits in parenthesis.

Suits against the local government are those listed as chihö jichi (regional self-government) suits in the national data. Suits against the national government are all other non-tax suits.

Source: **Saikö saibansho jimu sökyoku (ed.), Shihö tökei nempö: minji, gyösei hen [Court Statistics Annual: Civil and Administrative]** tab. 80 (Tokyo: Hösö kai, various years).

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II. The Test

A. Introduction:

On whether judges who publish opinions favoring taxpayers do indeed do worse than others, consider three independent hypotheses. First, if the word-on-the-street in Japan is right, then a judge who writes pro-taxpayer opinions incurs a non-

trivial risk of damaging his career.[18]   This may not show up in every career, but disproportionately, such judges should receive worse assignments than those who favor the government.

Second, the government could have a high win rate simply because it avoids risking unfavorable precedents. A judge who favors taxpayers should then suffer no career damage.

Third, at least hypothetically, a judge might be <u>rewarded</u> for ruling against the government -- perhaps because powerful taxpayers pressure the government to punish judges who rule against them.[19]

Finally, suppose the Secretariat tries to reward judicial accuracy among lower-court judges. Should a judge write an opinion that is wrong, he will receive worse assignments, whether he be pro-government or pro-taxpayer. This hypothesis is independent of the first three, which are themselves mutually exclusive.

To test these various hypotheses we estimate the quality of a judge's post-tax-opinion job postings y through the regression equation:

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[18] A suggestion we ourselves have made. <u>See</u> Ramseyer & Rasmusen, <u>supra</u> note 2 (1997), at 280-82.

[19] Obviously, this is not an explanation for the high pro-government verdict rate. Moreover, we do not know of anyone who has suggested that this is the case in Japan. It is, however, a plausible scenario for the United States, where the Internal Revenue Service is frequently under attack from a Congress lobbied by influential constituents who would like to pay less taxes. See, for example, Associated Press, <u>IRS Chief Tells Senate Panel He will Probe "Every Allegation</u>," **Buffalo News**, May 1, 1998, Pg. 6A.

$$y = a + ß_1X_1 + ß_2X_2 + e.$$

Here, X_1 is a vector of variables relating to the judge's tax opinions, including whether he favors the government (to distinguish among our first three hypotheses) and whether he is reversed (to test the fourth hypothesis). X_2 is a vector of control variables related to the judge's seniority, political inclinations, intelligence, and effort. We would expect these to matter under any of the explanations but we control for them lest we confuse the effect of tax dispute variables with that of coincidental talent and political bias.

B.   The Data:

We code a tax case according to whether the taxpayer or the government won, whether the case was appealed, and whether it was reversed on appeal.[20] For each judge involved, we also collect data about the jobs he held for the ten years before and after the year of the decision. We add to this other potentially relevant variables, primarily proxies for intelligence and effort.

For our database, we examine all published cases (whether civil or criminal) that construe either the Income Tax Act (for

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[20] We treat a case as a taxpayer victory if the court adopted any or all of the taxpayer's position.

individual taxpayers) or the Corporate Tax Act (for firms).[21] We
locate these cases on the Hanrei takei data base, available on
nine or ten CD-ROM diskettes.[22] Analogous to Lexis and Westlaw,
the dataset includes virtually all pubished opinions. For data
on judicial careers, we used the Zensaibankan keireki soran.[23]
The book covers all job postings for all judges educated since
the war. For membership in the YJL, we examined Osorubeki
saiban.[24] It includes the YJL roster for 1969, taken from
League's own roster.

    We use this material to assemble two samples. These can be
used in combination for some purposes but must be used separately
for others. First, for the "Tax Trials" dataset, we examine all
district court tax cases published in either 1976 or 1979. We
located 113 tax opinions for 1976 and 116 for 1979. Because some
judges wrote several tax opinions, this produced a set of 179
judges who wrote at least one tax opinion in either of the 2

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[21] We include criminal tax cases (there were 33 cases, 2 with
acquittals) because we believe the case selection dynamic in tax fraud
cases is often close to that of civil tax cases. We dropped judges not
in the data souce (primarily judges educated before the war and
prosecutors seconded to the courts) and judges who joined the bench less
than a year before the year of the decision or who quit less than 2
years after the year of the decision. Where a judge who opinions in
both 1976 and 1979, we coded the career data based on the year in which
he decided a pro-taxpayer decision. If he wrote a pro-taxpayer decision
in both or neither of the two years, we based the career data on 1979.

[22] **Daiichi hoki, ed., Hanrei taikei CD-ROM [A Systematic Case law:
CD-ROM]** (Tokyo: Daiichi hoki, biannually updated).

[23] ZSKS, supra note.

[24] **Shiso undo kenkyu sho, ed., Osorubeki saiban [Fearsome Trials]**
(Tokyo: Zenbo sha, 1969).

years. We chose years in the late 1970s because (at the time we began collecting the material) our data on judicial careers expired in 1990 and we need ten years of post-opinion career data.[25]

We chose two years that were three years apart because doing so yielded two cohorts with little overlap, given that most judges are reassigned every three years. Note that some judges specialize by subject matter during a 3-year assignment -- including tax. Rarely, however, do judges write many tax opinions for more than three years in a row. Note further that most tax cases are heard by three-judge panels, even in the lower courts, that all Japanese trials are bench trials, and that lower courts never publish dissents.

Second, in the "Tax Appeals" sample, we collected data on those trial court decisions that were appealed, but over a longer time horizon. We included all the appealed cases in 1976 and 1979, since we already had them from the Tax Trials sample. We added to that all cases reported for the other years between 1975 and 1984 in which either (a) the trial judge was reversed on appeal or (b) a pro-taxpayer opinion was affirmed (a total of 78 cases). Finally, we collected a random sample of 78 pro-government opinions written during those years and affirmed on appeal. This is by far the most common kind of appeals case.

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[25] **Nihon minshu horitsuka kyokai, ed., Zen saibankan keireki soran: kaitei shinban [Biographical Information on All Judges: New**

Because of the high cost of collecting this data, we used this simple form of stratified sampling to concentrate on the most interesting data.[26]

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[26] We used the following sampling procedure. Ramseyer determined how many cases in a given year had TP_AFF, TP_REV, or J_REV equal to 1, and all of those went into the sample—9 cases for 1975, for example. He then numbered the cases that year with J_AFF equal to 1—37 of them for 1975. Rasmusen then used STATA to generate 9 different random numbers from 1 to 37, and Ramseyer used those J_AFF cases for our sample. Thus, we end up sampling the same numbers of J_AFF cases as all other cases for each year except for 1976 and 1979, for which our sample was the entire population of tax cases.

Table 2a: Summary Statistics for the Tax Trial Sample

	Min	Median	Mean	Max
ANYPROTP	0	0	.34	1
ANYREV	0	0	.09	1
PREGOODJOB	0	0	.20	1
POSTGOODJOB	0	.27	.34	1
PREBADJOB	0	0	.16	.73
POSTBADJOB	0	.20	.22	.85
SENIORITY	2	13	13.52	29
FLUNKS	0	3	4.03	17
ELITE_UNIV	0	0	.39	1
1ST_TOKYO	0	0	.10	1
OPINIONS/YR	0	1.85	2.88	20.6
YJL	0	0	.07	1

Number of judges: 179

Table 2b:
Summary Statistics for the Tax Appeal Sample

	Min	Median	Mean	Max
TP_AFF	0	0	.11 (.19)	1
TP_REV	0	0	.15 (.26)	1
J_AFF	0	1	.70 (.45)	1
J_REV	0	0	.12 (.20)	1
PREGOODJOB	0	0	.17 (.17)	1
POSTGOODJOB	0	.19	.33 (.33)	1
PREBADJOB	0	0	.18 (.11)	.81
POSTBADJOB	0	.20	.24 (.24)	.97
SENIORITY	2	12	12.93 (12.93)	31
FLUNKS	0	3	4.01 (4.01)	17
ELITE_UNIV	0	0	.36 (.36)	1
1ST_TOKYO	0	0	.12 (.12)	1
OPINIONS/YR	0	1.90	2.91 (2.91)	29.02
YJL	0	0	.06 (.06)	1

Notes:
Number of judges: 284.
This is a stratified sample. The values weighted to estimate the population values are followed in parentheses by the unweighted, sample values. For the median, weighting happens not to alter the values. For the Min and Max, weighting makes no difference, and population values are not estimated.

C. The Variables:

Using this data, we constructed the following variables. Where relevant, we note the predicted effect that these variables will have on judicial careers.[27] Summary statistics appear in Table 2.

ANYPROTP: This equals 1 if a judge published a tax opinion (in 1976 or 1979, depending on the judicial cohort) in which the government lost on any count; 0 otherwise.

ANYREV: This equals 1 if a judge published a tax opinion in a reference year (1976 or 1979) that was reversed on any issue; 0 otherwise.

PREGOODJOB: The percentage of the 10 years before the reference year in which the judges either was chief judge, had sokatsu responsibilities (a modestly prestigious administrative appointment), or was in another administrative post.

POSTGOODJOB: Equivalent to PREGOODJOB for the 10 years after the reference year.

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[27] Based on earlier research. See Ramseyer & Rasmusen, supra note 2 (1997); Ramseyer & Rasmusen, supra note 2 (1998).

PREBADJOB: The percentage of the 10 years before the reference year in which the judge was in a branch office.

POSTBADJOB: Equivalent to PREBADJOB for the 10 years after the reference year.

SENIORITY: The number of years between the reference year and the year of the judge's graduating class from the national law school, the Legal Research & Training Institute (LRTI). Class 10 would have graduated in 1958, Class 20 in 1968, and so forth. All else equal, as judges rise in seniority they obtain better jobs. As a result, SENIORITY should correlate positively with POSTGOODJOB and negatively with POSTBADJOB.

FLUNKS: This is the estimated number of years between a judge's reaching age 22 and his entrance to the LRTI. To become a lawyer, judge or prosecutor, a college graduate must first graduate from the LRTI. During the years under study here, the LRTI maintained an entrance exam with a pass rate that varied between about 1 and 4 percent. As a result, most applicants never passed, and most who did passed only after failing several years first.

In effect, FLUNKS approximates the number of times the judge failed the LRTI entrance exam. Hence, it proxies for a combination of IQ and hard work. All else equal, the smarter and harder working judges (the judges with the lowest FLUNKS scores) will tend to obtain the better job postings. Thus, FLUNKS should

correlate positively with POSTBADJOB and negatively with POSTGOODJOB.

ELITE_UNIV: This equals 1 if a judge graduated from either of the two top-ranked universities, the University of Tokyo or the University of Kyoto. All else equal, graduation from these schools correlates with initial appointment to the best jobs in the courts.

OPINIONS/YR: The number of opinions authored or co-authored by the judge during the 10 years before the reference year, divided by the number of years he was on the bench during that time. All else equal, OPINIONS/YR should correlate positively with time in the better jobs.

YJL: This equals 1 if a judge was a member of the YJL in 1969; 0 otherwise. The YJL was a leftist organization among lawyers, judges and law professors -- consider it the Japanese National-Lawyers-Guild-equivalent. All else equal, membership in the YJL in 1969 correlates with less prestigious postings.

1ST_TOKYO: This equals 1 if a judge's first job was in the Tokyo District Court; 0 otherwise. When it hires a new cohort of judges, the Secretariat distinguishes among them on the basis of observed industry and intelligence. It then places the most able on a distinct fast track. The clearest signal of that fast-track status is appointment to the Tokyo District Court as a judge's first job.

D. The Results:

1. Preliminary considerations: We performed three sets of regressions: (1) preliminary regressions illustrating the determinants of early or intermediate judicial careers (on both samples combined), (2) regressions examining the effects of pro-taxpayer opinions and of reversals (on the Tax Trials sample), and (3) regressions testing for differences between pro-government and pro-taxpayer reversals (on the Tax Appeals sample).[28]

In the first set, we examine the determinants of a judge's early or intermediate career. As we are unaware of any relevant bias that would result from merging the two data sets, we combine them here. Eliminating dublicate entries results in a data set with xx judges. Regressions 3.1, 3.2, and 3.3[29] in Table 3 give the results; the dependent variables are 1ST-TOKYO, PREBADJOB, and PREGOODJOB.

By the word on the street, the Secretariat identifies the most promising new recruits and assigns them to the Tokyo District Court for their first job. Only three of our control variables are relevant to this initial appointment, and the

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[28] Note that the Trials sample has 179 observations, the Appeals sample has 329 observations, and the combined sample has 335 observations. The number of observations in the first two samples adds up to more than 335 because there is some overlap between the Trials and Appeals samples.

[29] The regressions in Table 3 are not weighted by sampling probability.

regression shows that having gone to an elite university and having graduated from LRTI at a young age (and hence having likely failed the entrance examination fewer times) both confer significant advantage. Having been a member of the YJL, on the other hand, is not statistically significant.[30]

Regressions (3.2) and (3.3) illustrate some of the factors that determine the quality of jobs a judge receives relatively early in his career.[31] According to (3.2), writing few opinions (low OPINIONS/YR), having flunked the LRTI exam more often (high FLUNKS), and having joined the YJL all lead to longer stints in branch offices. Just as interestingly, having gotten a very good first job (1ST_TOKYO) and gone to a top undergraduate college (ELITE_U) are insignificant. Starting high does not mean a judge avoids branch office time entirely.

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[30] The coefficient on YJL is insignificant for a simple reason. A conservative group first located and widely disseminated the membership roster of judges in 1969. Unless the Secretariat had independent evidence of a judge's political inclinations before it hired him, it could not have discriminated against judges in their first posting. Because the League kept the membership roster secret after 1969, we do not have the names of any League members who joined the courts after that date. Accordingly, our regressions could not disclose any discrimination against them.

[31] Obviously, some judges are more senior than others during this period. We say "relatively early" only for the simple reason that for each judge, it is the decade earlier than in our next set of regressions. Note that OPINIONS/YR cannot technically be a determinant of either PREGOODJOB or PREBADJOB, since it is calculated over the same period. We use it here on the theory that it proxies for the judge's general rate of published productivity.

Curiously, the coefficient on SENIORITY in significant and positive: in the PREBADJOB regression, more senior judges spent more time in branch offices during the decade before the tax opinions. Note that this contradicts the more general result -- seen in the Table 4 regressions -- that length of time in branch office generally declines with seniority. What Regression (3.2) reflects, however, is the non-linearity in this relationship: branch office is generally a mid-career phenomenon. The very youngest judges avoid branch office time because they are too inexperienced to be left alone in a small office. Only after judges have worked several years (often a decade) at the courts do they start to be sent to branch offices -- and it is at that point that branch office time starts to function as a potential punishment.

Because the Regression (3.2) uses PREBADJOB as the dependent variable, the coefficient on SENIORITY pimarily reflects the early-to-mid-career transition -- a point at which branch office time increases. To capture the different effect that SENIORITY has on junior and senior judges, we transformed SENIORITY into exponential form. Estimating that the break occurs shortly after a judge finishes his first 10-year term, we constructed a variable equal to (SENIORITY - 13) squared, and reran regression (3.2) (on PREBADJOB) and regression (4.3a) (on POSTBADJOB). In

both cases, the coefficient on the transformed variable was now negative and statistically significant.[32]

Regression (3.3) illustrates the determinants of the most prestigious mid-career jobs.[33] We see that writing many opinions (OPINIONS/YR), starting in the Tokyo District Court (1ST_TOKYO), and having considerable experience (SENIORITY) help, while having attended an elite college (ELITE_UNIV), passed the LRTI quickly (FLUNKS), or joined the YJL are insignificant for the prestigious appointments. In fact, however, a judge who graduated from an elite university or passed the LRTI exam early <u>is</u> more likely eventually to obtain prestigious appointments. The effect is simply later and indirect. We know from Regression (3.1) that university affiliation and LRTI early passage help one obtain an initial posting to the Tokyo District Court. As Regression (4.3b) shows, that initial posting will in turn lead to prestigious appointments later. Indeed, at that point ELITE_UNIV and FLUNKS reappear with independent statistical significance even holding constant the effect of 1ST_TOKYO.

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[32] Xxx Done in OLS, check in Tobit.

[33] We would not necessarily expect this to be symmetric to the results in (3.2), since the qualities that prevent one from being posted to the very worst jobs are not necessarily those which make one fit for the very best. That a judge is clearly not destined for the Supreme Court does not mean he is good for nothing more than traffic cases.

Table 3:
Early and Intermediate Judicial Careers

Dependent Variable	(3.1) 1ST_TOKYO	(3.2) PREBADJOB	(3.3) PREGOODJOB
SENIORITY	XXXXXX	**.019**** **(5.46)**	**.046**** **(12.90)**
FLUNKS	**-.16**** **(2.16)**	**.024**** **(2.93)**	-.007 (0.87)
ELITE_UNIV	**.89**** **(2.67)**	-.074 (1.40)	.077 (1.61)
1ST TOKYO	XXXXXX	.010 (0.13)	**.22**** **(3.02)**
OPINIONS/YR	XXXX .	**-.072**** **(5.56)**	**.016**** **(2.44)**
YJL	-.45 (0.77)	**.20**** **(2.22)**	-.11 (1.27)
Intercept	-1.73** (4.99)	-.21** (2.95)	-.77** (9.30) .
Pseudo R^2:	.05	.20	.58
Standard Error:	XXXXX	.37	.32
Censoring (y<0, unc.,y>1)	XXXXX	(211,124,0)	(202,129,4) .

Notes:
Coefficients, followed by t-statistics in parentheses below.
** Significant at the 5 percent level for a two-sided test.
* Significant at the 10 percent level for a two-sided test.
n = 335.
Regressions (3.2) and (3.3) use tobit; Regression (3.1) uses logit.[34]

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[34] XxxxReplace the logit regression with probit for nomral distirubiton and better comparability with the tobits.

Table 4:
The Effect of Tax Opinions on Judicial Careers

Dependent Variable:	(4.1a) POST BADJOB	(4.1b) POST GOODJOB	(4.2a) POST BADJOB	(4.2b) POST GOODJOB	(4.3a) POST BADJOB	(4.3b) POST GOODJOB
ANYPROTP	.037 (0.64)	−.011 (0.13)	XXXXX	XXXXX	**−.003 (0.05)**	**.043 (0.48)**
ANYREV	XXXX	XXXXX	.20** (2.24)	−.28* (1.96)	**.20** (2.14)**	**−.30** (2.02)**
PREGOODJOB	XXXX	−.078 (0.42)	XXXXX	−.070 (0.39)	XXXXX	−.076 (0.43)
PREBADJOB	.051 (0.72)	XXXXX	.019 (.14)	XXXXX	.018 (.13)	XXXXX
SENIORITY	−.015** (4.32)	.047** (7.57)	−.016** (4.66)	.048** (7.84)	**−.016** (4.63)**	**.049** (7.85)**
FLUNKS	.016* (1.66)	−.012* (1.70)	.016* (1.72)	−.024 (1.62)	**.016* (1.71)**	**−.024* (1.66)**
ELITE_UNIV	−.050 (0.90)	.072 (1.55)	−.060 (1.09)	.14* (1.79)	−.060 (1.09)	**.14* (1.75)**
1ST_TOKYO	−.25** (2.45)	.107* (1.73)	−.27** (2.61)	.24 (1.65)	**−.27** (2.61)**	**.24* (1.85)**
OPINIONS/YR	−.014 (1.27)	.014** (1.35)	−.011 (0.98)	.014 (1.08)	−.010 (0.93)	.012 (0.87)
YJL	.13** (1.23)	−.073 (−.84)	.13 (1.27)	−.13 (0.88)	.13 (1.27)	−.13 (0.88)
Intercept	.30** (4.26)	−.43** (3.68)	.30** (4.47)	−.43** (3.74)	.31** (4.43)	−.44** (3.77)

Pseudo R^2:	.21	.33	.23	.34	.23	.34
Standard Errors:	.32	.45	.31	.45	.31	.44
Censoring (y<0, y>1)	76,0	76,18	76,0	76,18	76,0	76,18.

Notes:
Coefficients, followed by t-statistics in parentheses below.
** Significant at the 5 percent level for a two-sided test.
* Significant at the 10 percent level for a two-sided test.
n = 179.
The regressions use tobit.

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2.   Tax cases. -- a.   Introduction.   Turn, now to the
question central to this article: whether a judge's tax
decisions affect his career.   Table 4 shows the regression
results for the Tax Trials sample.   Regressions (4.1a) and (4.1b)
examine the effect of writing any pro-taxpayer opinions
(ANYPROTP).   Regressions (4.2a) and (4.2b) examine the effect of
writing opinions that are reversed (ANYREV).   Regressions (4.3a)
and (4.3b) repeat the regressions but include both ANYPROTP and
ANYREV.   We focus here on the (4.3) regressions.

b.   Control variables.   As discussed earlier, the best
positions go to the most senior judges: SENIORITY is positively
and significantly correlated with POSTBADJOB, and negatively and
significantly correlated with POSTGOODJOB.

In a variety of ways, the regressions once more confirm the
importance of intelligence and hard work.   Judges placed

initially on the fast-track disproportionately obtain the best jobs: 1ST_TOKYO is negatively and significantly correlated with POSTBADJOB and positively and significantly correlated with POSTGOODJOB. Judges who failed the LRTI entrance exam the fewest times also obtain good jobs: FLUNKS is positively and significantly correlated with POSTBADJOB and negatively and significantly correlated with POSTGOODJOB.

One variable also illustrates that what produces an exceptionally good career may not be simply the opposite of what produces an exceptionally bad career. Judges from the schools with the most brutally competitive entrance examinations hold the most administrative responsibilities but do not avoid branch office time: ELITE_UNIV is positively and significantly correlated with POSTGOODJOB, but not significantly negatively correlated with POSTBADJOB.[35]

Notably, FLUNKS and ELITE_UNIV maintain a continuing significance in regression (4.3b) wholly apart from the separate effect of 1ST_TOKYO. Given that the Secretariat decided who to put on the fast track on the basis of observable proxies for intelligence and effort, one might have thought that the presence

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[35] The perceptive reader will notice that PREGOODJOB did **not** have a significant correlation with ELITE_UNIV, unlike POSTGOODJOB. This suggests an interaction between ELITE_UNIV and seniority—the usefulness of going to a top college may be greatest at the start of a career (as shown in Regression 3.1) and at the end (from 4.3b). Note, of course, that even PREGOODJOB depends on 1ST-TOKYO, and 1ST-TOKYO disproportionately includes judges from elite schools.

of 1ST_TOKYO in the regressions would swamp FLUNKS and
ELITE_UNIV. Despite the modest collinearity (1ST_TOKYO's
correlation in the Tax Trials sample with ELITE_UNIV is .09 and
with FLUNKS is -.12; the correlation between the latter two is -
.15), all three remain independently significant.[36]

   We predicted that judges who were members of the leftist YJL
in 1969 would still work disproportionately in branch offices in
the 1980s. YJL is positively correlated with POSTBADJOB and
negatively with POSGOODJOB, but the effect is not statistically
significant.

   b. ANYPROTP. Finally, we come to the variables of greatest
importance to our hypotheses explaining the high government win
rates. In short: ANYPROTP has no significant effect in the
regressions, while ANYREV does. Whether a judge writes a pro-
taxpayer opinion has no significant effect on the jobs he obtains
in the succeeding years. Even the signs are not consistently in
the directions predicted. Given the possibility that judges who
write pro-taxpayer opinions write them regularly, and that
PREGOODJOB and PREBADJOB would then incorporate the punishment
imposed for past pro-taxpayer opinions -- given this possibility,
we reran the (4.3) regressions with PREBADJOB and PREGOODJOB

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[36] That the effect of elite school graduation would continue
beyond the initial appointment contradicts our conclusion in Ramseyer &
Rasmusen, supra note 2 (1997), at 276-77.

omitted. Notwithstanding the change, ANYPROTP remained insignificant.[37]

Whether a higher court reverses a judge's opinion does affect his career: the coefficient on ANYREV is negative and significant in the POSTGOODJOB regression; and positive and significant in the POSTBADJOB regression.[38] Thus, it does not matter whether a judge decides for the taxpayer or for the government. It does matter whether he decides correctly.[39]

Hypothetically, the "no observed punishment" on ANYPROTP result is equally consistent with an equilibrium-with-punishment story. In this story, the Secretariat would punish judges for anything more than a trivially pro-taxpayer cases. Because all judges anticipate the punishment, however, no one writes

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[37] Xx Done in OLS; please check in Tobit.

Perhaps the mot serious missing variable problem concerns docket-clearance rates. We have no data on the ability of judges to clear the docket -- yet suspect that it does significantly affect careers. Here, however, the effect works in our advantage. In general, we suspect that judges who clear the docket fastest are those most inclined to rubber-stamp the government. If so, then the ANYPROTP judges will tend to be judges who clear the docket more slowly. If so, then including the hypothetical variable on docket clearance would lessen any punishment incoporated in the ANYPROTP coefficient.

[38] The correlation between ANYREV and ANYPROTP is .34.

[39] By the logic behind Priest-Klein, if both parties agree that a decision issued by a trial court is wrong, they will not appeal. Instead, they will settle out of court by reference to the expected reversal on appeal. As a result, the lack of an appeal is no evidence that a trial judge decided correctly. An actual reversal on appeal, however, is relatively clear-cut evidence that other judges believe that the trial court was wrong.

seriously pro-taxpayer opinions and no one is punished.
Hypothetically -- but we think not realistically.

We think the equilibrium unlikely, for the simple reason
that the rest of the judicial system seems so wildly out of
equilibrium. Recall the results we summarized at the outset:
judges who join leftist groups are visibly punished, judges who
decide electoral cases against the ruling party are punished,
judges who acquit criminal defendants are punished -- indeed,
according to the data here, even judges who find their opinions
reversed are punished. Given that some opinions themselves seem
more than trivially pro-taxpayer, and given that in other corners
of the judiciary the Secretariat does punish judges -- given all
that, we doubt that the threatened-punishment equilibrium
explains the no-punishment-on-ANYPROTP results.

c. Appeals. We are not done, though. Might it be that
what matters for a judge is not being reversed, but being
reversed when he favors the taxpayer? Maybe a judge's mistakes
are not penalized when those mistakes favor the government.
Maybe he suffers only when he mistakenly favors taxpayers.
Because our Tax Trials data base has too few cases appealed to
test this proposition, we turn to our augmented Tax Appeals
sample.

The Tax Appeals data base includes only judges whose cases
were appealed to a higher court. Of those, the great majority

were pro-government opinions that were upheld by the higher
court, so we will use that as our base situation and omit the
variable J_AFF from our regressions. We are interested, however,
in whether the other variables -- J_REV, TP_AFF, and TP_REV --
have a significant effect. Table 5 shows the results.

The results in Table 5 surprised us. Only one of our new
variables is significantly different from the J_AFF base:
TP_AFF, which has the wrong sign! It seems that a judge who
rules in favor of the taxpayer, has the case appealed, but is
sustained on appeal actually reduces his chances of going to a
branch office. Perhaps we should not be surprised, however. For
a judge to rule against the government, particularly in a case
controversial enough to be appealed, requires a measure of self-
confidence and initiative. The safest course for a judge is to
always rule for the government, the typical correct decision. If
a judge rules for the taxpayer and his reasoning is affirmed by a
higher court, this probably reflects considerable intellectual
ability. Consistent with this, compare the correlation
coefficients of J_AFF and TP_AFF with several proxies for ability
(along with politics and experience):

	J_AFF	TP_AFF
OPINIONS/YR	-.21	.32
1ST_TOKYO	-.12	.05
ELITE_UNIV	-.11	.16
SENIORITY	-.07	.07
FLUNK	.09	-.05
YJL	.02	-.06
PREGOODJOB	-.13	.12

For each of the variables, the judges writing TP_AFF opinions
seem a more talented (also more experienced and politically
conservative) group. Again, the Secretariat seems not to be
trying just to help the government win; it seems simply to be
rewarding talent.

Table 5:
The Effect of Tax Opinions on Judicial Careers

Dependent Variable:	(5.1a) POSTBADJOB	(5.1b) POSTGOODJOB
TP_AFF	-.15* (1.89)	.056 (0.54)
TP_REV	-.087 (1.31)	.12 (1.37)
J_REV	.020 (0.28)	-.12 (1.31)
PREGOODJOB	XXXX	.09 (0.48)
PREBADJOB	.40** (3.07)	XXXXXXXX
SENIORITY	-.015** (5.37)	.043** (7.90)
FLUNKS	.0061 (0.81)	**-.020*** **(1.76)**
ELITE_UNIV	-.016 (0.33)	.050 (0.76)
1ST_TOKYO	**-.32**** **(4.03)**	**.25**** **(2.68)**
OPINIONS/YR	-.0028 (0.29)	**.021*** **(1.92)**
YJL	**-.19*** **(1.88)**	.068 (0.57)
Intercept	.32** (5.63)	-.43** (4.80)

Pseudo R^2:	.20	.34
Standard Error:	.33	.45
Censoring (y<0,unc. y>1)	(121.0, 163.0, 0)	(118.3, 132.8, 32.9)

> Notes:
> Coefficients, followed by t-statistics in parentheses below.
> ** Significant at the 5 percent level for a two-sided test.
> * Significant at the 10 percent level for a two-sided test.
> n = 284. The regressions use tobit, with observations weighted by the inverse of the sampling probability.

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III. Political Economy

Although the Japanese government consistently wins in court, it does not win because it manipulates judges' career paths. Simply put, the Secretariat does not punish judges for writing pro-taxpayer opinions; instead, it punishes them for writing opinions that are wrong. Given the institutional structure of the courts, the LDP could have manipulated incentives to increase government revenue. According to the data from tax opinions, it does not.

Given the apparent gains from skewing judicial outcomes, perhaps the puzzle is why the government chose not to manipulate the courts. After all, as we show elsewhere it does reward judges who convict criminal defendants, judges who avoid left-wing entanglements, or judges who favor the government in some constitutional cases. The answer, we suggest, lies partly in the

LDP's long-term dominance and partly in the LDP's internal
structure.

First, as what appeared to be the long-term majority party
in a parliamentary regime, the LDP could readily change laws it
did not like.[40]   If it wanted the tax office to raise more
revenue, it could straightforwardly accomplish that result by
statute. It could either impose by statute the desired judicial
interpretation, or simply raise tax rates. It had no need to
rely on aggressive judicial interpretation

Given the ease of making major changes by statute, the
government had no need to intervene in the courts to raise its
take. What it needed was the ability to make minor context-
specific changes in the law as applied. Broad changes in revenue
it could engineer through legislation. For more nuanced changes,
it could rely on regulations and precedent: where precision
mattered, it presumably turned to regulations and internal
circulars; where factual context mattered, it selected the
appropriate cases for litigation and relied on the courts.

Second, the LDP maintained an internal structure that
enabled it to commit to long-term strategies. Like voters
elsewhere, all else equal[41] voters in Japan prefer unbiased

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[40] In what was a dramatic surprise to most observers, the LDP did
lose power in 1993. On the internal party dynamics that led to this
perhaps-temporary fall from power, see **J.M. Ramseyer & F.M. Rosenbluth**,
<u>supra</u> note. 11

[41] All else is not always equal -- as, for example, if given
objective probabilities an acquital in a criminal case would result in a

courts.   Yet given how long it can take to verify whether politicians intervene in courts, a political party unable to commit credibly to long-term strategies might well intervene anyway.   The move would generate for it a large long-term net cost (from voter disaffection) but a modest short-term advantage (from more government revenue).

Crucially, the internal structure of the LDP facilitated commitment to long-term strategies.[42] The party drew its leaders from legislators who showed that they could win elections repeatedly.   In exchange for their running the party, the party paid them lavish amounts of cash -- some of it legal, some of it not.   In effect, it delegated power to those members who most clearly played an indefinitely repeated game; then, to ensure that they not defect from strategies that maximized the gains to the party as a whole, it paid them efficiency wages.[43]

IV. Conclusion

Consistently, the Japanese government wins in court.   It does not win, we conclude, because it manipulates the career judiciary to produce biased courts.   Japanese judges do not enjoy

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guilty defendant walking free.   See Ramseyer & Rasmusen, supra note 2 (1998).

[42] Ramseyer & Rosenbluth, supra note 11, at ch. 5.

[43] On efficiency wages, see George Akerlof and Janet Yellen, eds., Efficiency Wages Models of the Labor Market, or the more modest, and perhaps falsified, contribution by one of us explaining why paying high wages to politicians discourages stealing (Eric Rasmusen, An Income Satiation Model of Efficiency Wages, 30 Economic Inquiry 467 [1992]).

better careers if they favor the government. Instead, it apparently wins because, as a rational repeat player, it disproportionately selects for litigation those cases that will shift precedent in an advantageous direction. At least in the context of tax litigation, the system favors accurate judges rather than biased judges. Systematically, those judges who find their opinions reversed on appeal do worse than their peers.

As majority party in the Japanese government, the LDP probably left the courts alone in this field because doing so earned it a long-term advantage. After all, if it needed to raise revenue, it could simply change the statute. By delegating leadership to a corps of well-paid politicians who played an indefinitely repeated game, the party was able to avoid strategies (like judicial intervention) with a short-term advantage but a possible net long-term cost. Although it used a career structure in the courts that would have facilitated intervention to its short-term advantage, in this context it used an internal party structure that facilitated commitment to safer long-term strategies instead.