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## ARTICLE

# WHY THE JAPANESE TAXPAYER ALWAYS LOSES

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

Like judges in most countries outside the United States, judges in the lower courts in Japan work as career civil servants. They begin their careers as judges and remain judges for most of their working lives. During that time, they work under the supervision of the Secretariat, the administrative office of the lower courts. 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 move up and down the judicial hierarchy and all around the country. Through this indirect influence over judicial careers, the Japanese Cabinet has the power to reward and punish

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judges by the complexion of the opinions they write. The question is whether—or when—it uses it.

All this has implications for the amount of judicial independence we will likely see in competitive democracies generally. If all else were equal, voters might prefer more independence to less. But all else is rarely equal. Voters do not just want independent judges. They also want policies enforced and programs implemented. And precisely because they are unconstrained, independent judges can obstruct policies and programs as they wish. Rather than give the voters what they want, independent judges can enforce the policies they like personally, and then implement the programs they *wish* legislators had enacted. Because the Japanese government both (i) faces competitive elections and (ii) has the institutional power to intervene in the courts, the patterns by which it does and does not intervene tell us something about when voters will most likely want independent courts.

Even if the Japanese government did manipulate judicial careers, the connection to win rates would remain problematic. According to the well-known Priest-Klein hypothesis,<sup>1</sup> 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 emboldened to bring shakier cases. If so, then ultimately its verdict rate will look no better than if it faced unbiased judges. The Secretariat may or may not be manipulating judicial careers, but we would not be able to tell this from verdict rates.

Both to avoid resting on this logic and to explore broader questions of judicial independence, we use data from tax litigation to test 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. Holding constant a variety of other factors, we then ask whether 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;<sup>2</sup>

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1. See George Priest & Benjamin Klein, *The Selection of Disputes for Litigation*, 13 J. LEGAL STUD. 1 (1984).

2. See J. Mark Ramseyer & Eric B. Rasmusen, *Judicial Independence in a Civil Law Regime: The Evidence from Japan*, 13 J.L. ECON. & ORG. 259, 274 tbl.3 (1997) [hereinafter *Judicial Inde-*

- ii) judges who flunked the Japanese bar-exam equivalent (the entrance exam to the Legal Research & Training Institute [LRTI]; during the period at stake, it had a pass rate that hovers between one and four percent) fewer times had more successful careers than those who flunked it more often;<sup>3</sup>
- iii) judges who in the 1960s joined the leftist Young Jurists League (YJL) had fewer administrative responsibilities and earned lower pay than their more conservative colleagues;<sup>4</sup>
- iv) judges who sometimes acquit defendants do worse than those who always convict;<sup>5</sup>
- v) judges who held unconstitutional the ban on door-to-door canvassing—a campaign tactic used by Communist candidates—did worse than those who held it constitutional;<sup>6</sup>
- vi) judges who held unconstitutional the Japanese military or American bases did worse than those who did not;<sup>7</sup>
- vii) judges who held unconstitutional the heavy rural overrepresentation in the parliament at a time when the Liberal Democratic Party (LDP) relied heavily on the rural vote did worse than those who did not;<sup>8</sup> and
- viii) judges who enjoined the national government in administrative cases the most often did worse than others.<sup>9</sup>

Here, we ask a similar question: Do judges who favor voters in one category of routine administrative cases (tax cases) have less successful careers than those who favor the government?

Although we find that a judge's tax opinions affect his 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. Instead, the Secretariat punishes judges for writing wrong opinions. On average, a judge who finds a tax

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*pendence*]; J. Mark Ramseyer & Eric B. Rasmusen, *Why Is the Japanese Conviction Rate So High?* 19 (Sept. 4, 1998) (unpublished manuscript, on file with the Kelley School of Business, Indiana University, and the John M. Olin Center for Law, Economics and Business, Harvard Law School) [hereinafter *Conviction Rates*].

3. See *Judicial Independence*, *supra* note 2, at 274 tbl.3, 277 tbl.5.

4. See *id.* at 277 tbl.5; J. Mark Ramseyer & Eric B. Rasmusen, *Why Are Japanese Judges So Conservative in Politically Charged Cases?* 21, (Jan. 25, 1999) (unpublished manuscript, on file with the authors) [hereinafter *Conservative*].

5. See *Conviction Rates*, *supra* note 2, at 28.

6. See *Judicial Independence*, *supra* note 2, at 285 tbl.9.

7. See *Conservative*, *supra* note 4, at 11.

8. See *id.* at 13-14.

9. See *id.* at 16.

opinion reversed on appeal will spend more time in provincial branch offices and less time with prestigious administrative responsibilities. Moreover, a judge who rules for the taxpayer and has the ruling affirmed on appeal is less likely on average to end up in a branch office. Simply being pro-taxpayer is not the judge's problem; getting reversed is.

This result presents two questions. First, if the Secretariat does not punish judges for favoring taxpayers, why is the win rate so high? Second, 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. Given the additional tax revenues it could earn by subtly altering judicial incentives, is the government leaving money on the table? We conclude 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.

In Part I of this Article, we detail the win rates in Japan, explain the court structure, outline the common reaction to the phenomenon, and note the implications of the Priest-Klein hypothesis. In Part II, we examine data from reported opinions and judicial careers to test whether the Secretariat uses its control over judicial appointments to reward pro-government opinions. In Part III, we explore the political economy of judicial manipulation and electoral competition.

## 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%. The government also litigated another 622 non-tax administrative cases. Of those, it won 93%.<sup>10</sup> This is not unusual. Year after year, the government wins by similar odds.<sup>11</sup>

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10. See Saiko saiban sho, Shiho tokei nempo [Annual Report of Judicial Statistics] tbl.80 (1994) [hereinafter Saiko].

11. See 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

Because the Japanese government publishes a relatively high proportion of the tax opinions, the win 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, consider the 1989 opinions. In that year, the government litigated 182 tax cases and won 87%. It litigated another 355 non-tax administrative cases and won 90%.<sup>12</sup> Of the tax cases, 92 were published (51%, including both civil and criminal tax cases). Of those published cases, the government won 92%. Thus, the win rates on published and unpublished tax cases are almost identical.

### B. THE 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 in its favor. It can do so, they explain, because of the career structure of the judiciary. Because it hires young and unproven judges into the court, it can manipulate career paths to induce judges to work carefully and to work hard. Further, argue the critics, the government can also induce judges to favor the government.<sup>13</sup>

To understand how the government can intervene in the courts, consider the structure of the Japanese judiciary.<sup>14</sup> During most of the post-war decades, the LDP controlled the Japanese Parliament. As majority party, the LDP also controlled appointments to the fifteen-member Supreme Court. As a moderately conservative party facing a socialist and communist opposition, it primarily appointed 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 sixties, shortly before the mandatory retirement age of seventy. Because the LDP had a secure hold on Parliament during most of the post-war period, it felt safe making these appointments.

Typically, the Cabinet names a majority of Supreme Court justices from the lower courts. Generally, it keeps at least one justice on the bench who ran the Secretariat earlier, and thus knows the intricacies of adminis-

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& ECONOMICS: AN ECONOMIC ANALYSIS OF JAPANESE LAW] ch. 3 (1990) [hereinafter ECONOMIC ANALYSIS].

12. See Saiko, *supra* note 10, at tbl.80.

13. We have also made this argument. See *Judicial Independence*, *supra* note 2, at 280-82.

14. See generally *id.*; J. MARK RAMSEYER & FRANCES MCCALL ROSENBLUTH, JAPAN'S POLITICAL MARKETPLACE chs. 8-9 (1993).

tering judicial careers. In turn, these Supreme Court justices supervise the Secretariat.

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

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 or carrying prestigious administrative responsibilities. Others toil long years in the widely despised small-town branch offices, with rarely a stint in the cities or on nonjudicial work. The Secretariat promotes judges at different rates because it hires them before it has reliable information about their abilities and work habits. Because not all judges are congenital workaholics, the Secretariat tries to reward care and effort. Because not all judges are brilliant, the Secretariat tries to give the brightest judges the jobs with the most responsibility.

### C. EXPLANATIONS

For the high government win 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 account for that bias when they bargained. Consequently, 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 trial.

By the original Priest-Klein hypothesis, legal bias or no, observed win rates should hover around fifty percent. Researchers have since failed to confirm this fifty-percent hypothesis.<sup>15</sup> They have, however, left intact the intuition that legal bias will not correlate with observed verdict rates. If

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15. See, e.g., Daniel Kessler, Thomas Meites & Geoffrey 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).

so, the Japanese tax office might win consistently, but that win rate is no evidence of biased judicial incentives.

A better explanation of the government's high win rate is a repeat-player strategy.<sup>16</sup> For example, suppose that a 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 litigate those cases most likely to shift precedent in their favor. As Priest and Klein observed, a "systematic difference in stakes to the parties" will cause the observed verdict to differ from fifty percent.<sup>17</sup> The Japanese tax office is exactly such a repeat player, and presumably adopts exactly such a litigation strategy.

As data consistent with this hypothesis (inconclusive to be sure), compare the win rates of national and municipal governments.<sup>18</sup> Table 1 illustrates that the national government tends to win at a higher rate than local governments. 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. If any one government invests in 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 win rates would result.

Additionally, the national government 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, it officially publishes only a small percentage of the lower court opinions. In tax cases, however, it maintains an official reporter devoted exclusively to the field.

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16. See ECONOMIC ANALYSIS, *supra* note 11, at ch. 3.

17. Priest & Klein, *supra* note 1, at 40.

18. 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) (the government may settle) with *Sasakawa takushoku ringyō, K.K.*, 7 KAKYŪ MINSHŪ 1895 (Tokyo D. Ct. July 14, 1956) (the government may not settle). For a contrary interpretation of the data in Table 1, see J. MARK RAMSEYER & MINORU NAKAZATO, *JAPANESE LAW: AN ECONOMIC APPROACH* ch. 8 (1998).

TABLE 1. WIN RATES, BY GOVERNMENT SUED<sup>19</sup>

<i>Year</i>	<i>National Government</i>	<i>Local Government</i>	<i>B/A</i>
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 chiho jichi (regional self-government) suits in the national data. Suits against the national government are all other non-tax suits.

## II. THE TEST

## A. INTRODUCTION

Consider four hypotheses about whether judges who publish opinions favoring taxpayers will have worse careers than those who favor the government.

First, if the word on the street in Japan is right, then a judge who writes pro-taxpayer opinions incurs a nontrivial risk of damaging his ca-

19. Shihō tōkei nempō: minji, gyōsei hen [Court Statistics Annual: Civil and Administrative] tbl. 80 (Saikō saibansho jimu sōkyoku ed., various years).

reer.<sup>20</sup> Such judges should disproportionately 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 rewarded for ruling against the government—perhaps because powerful taxpayers pressure the government to punish judges who rule against them.<sup>21</sup>

Finally, suppose the Secretariat tries to reward judicial accuracy among lower-court judges. If so, then a judge who writes an incorrect opinion will receive worse assignments regardless of whether he is pro-government or pro-taxpayer. This hypothesis is independent of the first three, which are themselves mutually exclusive.

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

$$y = \alpha + \beta_1 X_1 + \beta_2 X_2 + \epsilon.$$

Here,  $X_1$  is a vector of variables related 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, and effort. We expect these factors to matter under any of the hypotheses, 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 tax cases according to: (1) whether the taxpayer or the government won; and (2) whether the case was reversed on appeal.<sup>22</sup> For each judge involved, we collect data about the jobs he held for the ten years before and after the year of the decision. Additionally, we consider other

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20. We have also made this argument. See *Judicial Independence*, *supra* note 2, at 280-82.

21. 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 seeking tax advantages. See, e.g., *IRS Chief Tells Senate Panel He Will Probe "Every Allegation,"* BUFFALO NEWS, May 1, 1998, at 6A.

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

potentially relevant variables, primarily as 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 individual taxpayers or the Corporate Tax Act for firms.<sup>23</sup> We locate these cases on the *Hanrei takei* database, which includes virtually all published opinions.<sup>24</sup> For data on judicial careers, we used the *Zensaibankan keireki soran*, which covers all job postings for all judges educated since World War II.<sup>25</sup> For membership in the YJL, we examine *Osorubeki saiban* which includes the YJL roster for 1969 taken from the League's own newsletter.<sup>26</sup>

We used this material to assemble two samples, which can be used in combination for some purposes but must be used separately for others. First, for the "Tax Trials" data set, we examined all district court tax cases published in either 1976 or 1979.<sup>27</sup> We located 113 tax opinions for 1976 and 116 for 1979. Some judges wrote several tax opinions, producing a set of 179 judges who wrote at least one tax opinion in either of the two years.

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 three-year assignment—including tax. Rarely, however, do judges write many tax opinions for more than three years in a row. Further, most tax cases are heard by three-judge panels, even in the lower

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23. We include criminal tax cases (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 source—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 two years after the year of the decision. Where a judge wrote opinions in both 1976 and 1979, we coded his career 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.

24. See HANREI TAIKEI CD-ROM [A SYSTEMATIC CASE LAW: CD-ROM] (Daiichi hoki ed., Mar. 1, 1996).

25. See ZEN SAIBANKAN KEIREKI SORAN: KAITEI SHINBAN [BIOGRAPHICAL INFORMATION ON ALL JUDGES: NEW REVISED EDITION] (Nihon minshu horitsuka kyokai ed., 1990) [hereinafter ZSKS].

26. See OSORUBEKI SAIBAN [FEARSOME TRIALS] (Shiso undo kenkyu sho ed., 1969).

27. 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 needed 10 years of post-opinion career data. See ZSKS, *supra* note 25. The 1998 revision to this book now extends the data to the late 1990s.

TABLE 2A. SUMMARY STATISTICS FOR THE TAX TRIAL SAMPLE

	<i>Min</i>	<i>Median</i>	<i>Mean</i>	<i>Max</i>
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

courts. All Japanese trials are bench trials, and lower courts never publish dissents.

Second, in the “Tax Appeals” sample, we collected data on trial court decisions that were appealed, but over a longer time horizon. Because we used them for the Tax Trials sample, we included all the appealed cases in 1976 and 1979. 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. This amounted to 78 cases. Finally, we collected a random sample of 78 pro-government opinions written during the same years and affirmed on appeal. This is by far the most common kind of appeals case. Because of the high cost of collecting this data, we used a simple form of stratified sampling to concentrate on the most interesting data.<sup>28</sup>

28. We used the following sampling procedure. Ramseyer determined how many cases in a given year had TP\_AFF (pro-taxpayer opinions affirmed), TP\_REV (pro-taxpayer opinions reversed), or J\_REV (pro-Japan opinion reversed) 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 (pro-Japan opinion affirmed) 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 2B. SUMMARY STATISTICS FOR THE TAX APPEAL SAMPLE

	<i>Min</i>	<i>Median</i>	<i>Mean</i>	<i>Max</i>
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

Number of judges: 284. This is a stratified sample. The values weighted to estimate the population values are accompanied 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 would have on judicial careers.<sup>29</sup> Summary statistics appear in Table 2.

29. Based on earlier research. See *Judicial Independence*, *supra* note 2, at 271-73; Conviction Rates, *supra* note 2, at 18-19.

ANYPROTP: This equals 1 if a judge published a tax opinion in which the government lost on any count during 1976 or 1979, depending on the judicial cohort. Otherwise, we assigned 0.

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

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

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

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

POSTBADJOB: Equivalent to PREBADJOB for the ten 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). 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: The estimated number of years between a judge's reaching age twenty-two and his entrance to the LRTI. A college graduate must first graduate from the LRTI to become a lawyer, judge, or prosecutor. During our study years, the LRTI maintained an entrance exam with a pass rate that varied between about one and four percent. As a result, most applicants never passed, and those 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 universities 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 ten 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—similar to the National Lawyers Guild in the United States. 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 the Secretariat hires a new cohort of judges, it distinguishes among them on the basis of observed industry and intelligence. It then places the most able judges on a distinct fast track. The clearest signal of a judge's fast-track status is his appointment to the Tokyo District Court as a first job.

#### D. THE RESULTS

##### 1. *Preliminary Considerations*

We performed three sets of regressions: (1) preliminary regressions to illustrate the determinants of early or intermediate judicial careers (on both samples combined); (2) regressions to examine the effects of pro-taxpayer opinions and of reversals (on the Tax Trials sample); and (3) regressions to test for differences between pro-government and pro-taxpayer reversals (on the Tax Appeals sample).<sup>30</sup>

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 combined the Tax Trials sample and the Tax Appeals sample. Eliminating duplicate entries results in a data set with 335 judges. Regressions 3.1, 3.2, and 3.3 in Table 3 show the results: The dependent variables are 1ST\_TOKYO, PREBADJOB, and PREGOODJOB.<sup>31</sup>

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30. Note that the Trials sample and the Appeals sample together sum to more than 335 because there is some overlap between the two samples.

31. The regressions in Table 3 are not weighted by sampling probability.

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 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.<sup>32</sup>

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.<sup>33</sup> 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. Interestingly, a judge's first job (1ST\_TOKYO) or his attendance at a top undergraduate college (ELITE\_UNIV) are insignificant. Starting high does not mean a judge avoids branch office time entirely.

Curiously, the coefficient on SENIORITY is 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 a branch office generally declines with seniority. However, Regression 3.2 reflects the non-linearity in this relationship: A branch office assignment 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, do they get assigned to branch offices. It is at this point that branch office time starts to function as potential punishment.

Because Regression 3.2 uses PREBADJOB as the dependent variable, the coefficient on SENIORITY primarily reflects the early-to-mid-career

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32. 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.

33. 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.

transition where branch office time increases. To capture the different effect that SENIORITY has on junior and senior judges, we constructed an

TABLE 3. EARLY AND INTERMEDIATE JUDICIAL CAREERS

<i>Dependent Variable</i>	(3.1) <i>1ST_TOKYO</i>	(3.2) <i>PREBADJOB</i>	(3.3) <i>PREGOODJOB</i>
SENIORITY		.019** (5.46)	.046** (12.90)
FLUNKS	-.074** (2.07)	.024** (2.93)	-.007 (0.87)
ELITE_UNIV	.48** (2.64)	-.074 (1.40)	.077 (1.61)
1ST TOKYO		.010 (0.13)	.22** (3.02)
OPINIONS/YR		-.072** (5.56)	.016** (2.44)
YJL	-.28 (0.69)	.20** (2.22)	-.11 (1.27)
Intercept	-1.06** (5.86)	-.21** (2.95)	-.77** (9.30)
Pseudo R <sup>2</sup> :	.05	.20	.58
Standard Error:		.37	.32
Censoring (y<0, unc.,y>1)		(211,124,0)	(202,129,4)

Notes:

Coefficients, with t-statistics in parentheses below.

\*\* Significant at the five percent level for a two-sided test.

\* Significant at the ten percent level for a two-sided test.

n = 335.

Regression 3.1 uses probit; Regressions 3.2 and 3.3 use tobit.

additional variable. Estimating that the break occurs shortly after a judge finishes his first ten-year term, we subtracted 13 from SENIORITY and squared the difference. We then reran Regression 3.2 (PREBADJOB) with this new variable instead of SENIORITY. The coefficient on the transformed variable now became negative and statistically significant.

TABLE 4. THE EFFECT OF TAX OPINIONS ON JUDICIAL CAREERS

Dependent Variable	(4.1a) POST BAD JOB	(4.1b) POST GOOD JOB	(4.2a) POST BAD JOB	(4.2b) POST GOOD JOB	(4.3a) <b>POST BAD JOB</b>	(4.3b) <b>POST GOOD JOB</b>
ANYPROTP	.037 (0.64)	-.011 (0.13)			<b>-.003</b> (0.05)	<b>.043</b> (0.48)
ANYREV			.20** (2.24)	-.28* (1.96)	<b>.20**</b> (2.14)	<b>-.30**</b> (2.02)
PREGOODJOB		-.078 (0.42)		-.070 (0.39)		-.076 (0.43)
PREBADJOB	.051 (0.72)		.019 (.14)		.018 (.13)	
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 <sup>2</sup>	.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, with t-statistics in parentheses below.

\*\* Significant at the five percent level for a two-sided test.

\* Significant at the ten percent level for a two-sided test.

n = 179.

The regressions use tobit.

Regression 3.3 illustrates the determinants of the most prestigious mid-career jobs.<sup>34</sup> 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 is more likely to obtain prestigious appointments. The effect is simply 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, this initial posting will in turn lead to prestigious appointments. Indeed, at that point ELITE\_UNIV and FLUNKS reappear with independent statistical significance even holding constant the effect of 1ST\_TOKYO.

## 2. Tax Cases

We now turn to the central question of 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 whether a judge's writing any pro-taxpayer opinions affects his career (ANYPROTP). Regressions 4.2a and 4.2b examine whether a reversal of a judge's written opinions affects his career (ANYREV). Regressions 4.3a and 4.3b repeat the regressions but include both ANYPROTP and ANYREV. We focus here on the 4.3 regressions.

a. *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.

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34. We would not necessarily expect this to be symmetric to the results in Regression 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.

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.<sup>35</sup>

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 whom to put on the fast track on the basis of observable proxies for intelligence and effort, one might have thought that the presence 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.<sup>36</sup>

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 POSTGOODJOB, but the effect is statistically insignificant.

b. *ANYPROTP*: Finally, we come to the variables of greatest importance to our hypotheses explaining the government's high win rates. In short, ANYPROTP has no significant effect in the regressions, while ANYREV does. A pro-taxpayer opinion has no significant effect on the jobs the judge 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, we reran the 4.3 regressions with PREBADJOB and PREGOODJOB omitted. Notwithstanding this change, ANYPROTP remained insignificant.<sup>37</sup>

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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.

36. That the effect of elite school graduation would continue beyond the initial appointment contradicts our conclusion in a previous study. See *Judicial Independence*, *supra* note 2, at 276-77.

37. Perhaps the most 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 to our advantage. In general, we suspect that judges who

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.<sup>38</sup> A judge safely may decide for the taxpayer or for the government, for—provided he is right—he will not affect his career. If he decides incorrectly, he suffers.<sup>39</sup>

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 trivially pro-taxpayer opinions. If all judges anticipated the punishment, however, no judge would write seriously pro-taxpayer opinions, and consequently, no one would be punished.

We think the equilibrium hypothesis unlikely, simply because 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 politically salient cases against the ruling party are punished, and judges who acquit criminal defendants are punished.<sup>40</sup> Indeed, according to our data, even judges who find their tax opinions reversed are punished. Given that some opinions themselves seem more than trivially pro-taxpayer, and that the Secretariat does punish judges in other corners of the judiciary, we doubt that the threatened-punishment equilibrium explains the no-punishment-on-ANYPROTP results.

c. *Appeals*: Perhaps what matters for a judge is not being reversed, but being reversed when he favors the taxpayer. Could it be that a judge is not penalized when his mistakes favor the government, but is punished when he mistakenly favors taxpayers? Because our Tax Trials database has too few cases appealed to test this proposition, we turn to our augmented Tax Appeals sample.

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clear the docket fastest are those most inclined to rubber-stamp the government. If so, then 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 incorporated in the ANYPROTP coefficient.

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

39. According to the Priest-Klein logic, 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.

40. See *supra* notes 3-5 and accompanying text.

The Tax Appeals database 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. We use this information as our base situation and omit the variable *J\_AFF* (lower-court judgment in favor of the government affirmed on appeal) from our regressions. We are interested, however, in whether the other variables, *J\_REV* (lower-court judgment in favor of the government reversed), *TP\_AFF* (lower-court judgment in favor of the taxpayer affirmed), and *TP\_REV* (lower-court judgment in favor of the taxpayer reversed), have a significant effect. Table 5 shows the surprising results.

Only one of our new variables is significantly different from the *J\_AFF* base: *TP\_AFF*, which has the wrong sign. It appears 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 by this result. For a judge to rule against the government, particularly if the case is controversial enough to be appealed, he must possess a measure of self-confidence and initiative. The safest course for him is always to rule for the government, the typically correct decision. If he rules for the taxpayer and his reasoning is affirmed by a higher court, this probably reflects considerable intellectual ability. Consistent with this suggestion, compare the correlation coefficients of *J\_AFF* and *TP\_AFF* with several proxies for ability, politics, and experience:

	<i>J_AFF</i>	<i>TP_AFF</i>
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 pro-taxpayer opinions affirmed on appeal (*TP\_AFF*) seem more talented, more experienced, and politically conservative. Provided these variables do not fully capture these characteristics, the Secretariat could here simply be rewarding talent and political acumen.

TABLE 5. THE EFFECT OF TAX OPINIONS ON JUDICIAL CAREERS

<i>Dependent Variable:</i>	<i>(5.1a) POSTBADJOB</i>	<i>(5.1b) POSTGOODJOB</i>
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		.09 (0.48)
PREBADJOB	.40** (3.07)	
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 <sub>2</sub> :	.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 five percent level for a two-sided test.

\* Significant at the ten percent level for a two-sided test.

n = 284. The regressions use tobit, with observations weighted by the inverse of the sampling probability.

## 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. Rather, it punishes them for writing opinions that are wrong. Given the institutional structure of the courts, the LDP could manipulate incentives to increase government revenue. According to the data from tax opinions, it does not.

Given the apparent gains from skewing judicial outcomes, why does the LDP choose not to manipulate the courts? After all, it does reward judges who convict criminal defendants, who avoid left-wing entanglements, and who favor the government in some prominent constitutional cases.<sup>41</sup> The answer, we suggest, lies partly in the LDP's long-term dominance and partly in the LDP's internal structure.

First, as the long-term majority party in a parliamentary regime, the LDP could readily have changed laws it did not like.<sup>42</sup> If the LDP had wanted the tax office to raise more revenue, it could either have imposed the desired judicial interpretation by statute or have raised 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. It could engineer broad changes in revenue 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. All else equal (and we speculate in Part IV about when it might not be equal), voters in Japan prefer unbiased courts. Yet given how long it can take to verify whether politicians actually intervene in courts, a political party unable to commit credibly to long-term strategies might well intervene anyway. The move would gen-

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41. See *Judicial Independence*, *supra* note 2, at 285 tbl.9; *Conviction Rates*, *supra* note 2, at 28; *Conservative*, *supra* note 4, at 21.

42. 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 generally RAMSEYER & ROSENBLUTH, *supra* note 14.

erate 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.<sup>43</sup> 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, legal and illegal. In effect, it delegated power to those members who most clearly played an indefinitely repeated game. To ensure that they would not defect from the strategies that maximized the gains to the party as a whole, it paid them efficiency wages.<sup>44</sup>

#### IV. CONCLUSION

Consistently, the Japanese government wins in court. It does not win because it manipulates the careers of judges. Japanese judges do not enjoy better careers if they favor the government. Rather, it apparently wins because, as a rational repeat player, it disproportionately selects for litigation those cases that will shift precedent to its advantage. 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, regardless of whether their opinions were pro-taxpayer or pro-government.

As the majority party in the Japanese government, the LDP probably left the courts alone in this field in order to gain a long-term advantage. After all, if it needed to raise revenue, it could simply change the tax statutes. 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 gain but a 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.

What does any of this tell us about when voters in competitive democracies will want independent courts? Given the risks that independent judges may skew legislated policies and programs away from voters pref-

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43. See RAMSEYER & ROSENBLUTH, *supra* note 14, at ch. 5.

44. On efficiency wages, see generally EFFICIENCY WAGES MODELS OF THE LABOR MARKET (George Akerlof & Janet Yellen eds., 1986), and see the more modest contribution by Rasmusen explaining that paying high wages to politicians may discourage stealing, Eric Rasmusen, *An Income Satiation Model of Efficiency Wages*, 30 ECON. INQUIRY 467 (1992).

erences, why would voters ever want judges in administrative cases to be independent? To rephrase the question, we earlier argued that the LDP earned a net long-term benefit from committing to independent courts—why? The answer, we suspect, lies in two considerations: (i) the forum that courts provide voters for resolving routine disputes, and (ii) the efficiency with which parliamentary governments can reverse courts by statute.

First, bureaucrats anywhere can misbehave, and voters like having a forum in which to complain when they do. Because courts that are independent from the government provide such fora, all else equal they can help majority parties win elections.<sup>45</sup> Second, parliamentary structure mitigates the central problem that independent judges create—the risk that judges will alter the shape of national policy away from the preferences held by a majority of voters. Suppose a judge does decide to indulge his own policy preferences at the expense of voter preferences. In a presidential system, voters can reverse him only if they marshal the president and both houses of congress. In a parliamentary system with a strong lower house, they need only convince the majority party.<sup>46</sup> Precisely because the party in a parliamentary system can pass statutes so easily, it risks less in keeping judges independent in routine administrative disputes—such as tax.

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45. Because crime is overwhelmingly a product of the underclass in most societies (including Japan), we suspect voters have far less interest in encouraging acquittals. *See Conviction Rates, supra* note 2, at 28.

46. The difficulty of legislative repeal of judicial decisions involving constitutional issues explains, we suspect, the political intervention we observed there. *See Judicial Independence, supra* note 2, at 284; *Conservative, supra* note 4, at 18-19.