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THE ECONOMIC COSTS OF COURT DECISIONS CONCERNING DISMISSALS IN JAPAN: IDENTIFICATION BY JUDGE TRANSFERS

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The Economic Costs of Court Decisions Concerning Dismissals in Japan: Identification by Judge Transfers[1※]

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Abstract

Despite its significant influence on the actual enforcement of the law, the economic cost of court discretion has been ignored in the literature on employment protection. This paper exploits a distinctive feature of the Japanese judicial system, periodic judge transfers, to identify court discretion. Because judges move across local labor markets while a single national court system ensures that there are no legal boundaries between regions, it is possible to shut down any confounding relationships between current litigation outcomes and local labor market. A key finding is that an increase in the worker victory ratio in adjustment dismissal litigations in the last 10 years reduces rates of both job creation and destruction. Ignoring the uncertainty inherent in court decisions would lead to misspecification of the actual cost of employment protection, especially in countries with high judicial activism.

Journal of Economic Literature Classification Numbers: J65, K31, K42

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1. Introduction

Courts' discretion can lead to substantial differences in the actual enforcement of the law, especially in countries with high judicial activism. The economic cost of such discretion is often ignored, despite its potentially large influence on behavior. One example is court-enforced employment protection legislation. According to economic theory, an increase in the cost of laying off an employee should unambiguously reduce the numbers of not only layoffs but also new hires due to fears of possible future downsizing (Hopenhayn and Rogerson 1993; Bentorila and Bertola 1990). Among other effects, this disincentive to expand hampers a smooth recovery from an economic crisis.

As such, recent empirical studies have increasingly focused on quantifying the effect of employment protection laws on job flows (Fraisse, Kramarz, and Prost 2015; Millán et al. 2013; Kugler and Pica 2008), employment response to shocks (Adhvaryu, Chari, and Sharma 2012), productivity (Autor, Kerr, and Kugler 2007), and allocative efficiency (Petrin and Sivadasan 2013), rather than on total employment.[2] Nonetheless, few of these studies have considered differences in judicial review. Although Autor, Kerr, and Kugler (2007) examined the impact of U.S. state courts establishing exceptions to at-will doctrine, the relevant legal precedents are still open to a wide range of interpretations, and judges retain considerable discretion. Provided that firms take the enforcement level into account as part of the indirect firing costs that affect the odds of winning a case at trial, ignoring the uncertainty inherent in court discretion will lead to misspecification of the actual cost of employment protection.

[2] There is a large literature estimating the impact on the employment level, with inconclusive results. See, for example Autor, Donohue, and Schwab (2006), Heckman and Pages-Serra (2004), Besley and Burgess (2004), Nickell (1997), and Lazear (1990). Addison and Teixeira (2003) provide a broad survey.

However, estimating the impact of court discretion poses an empirical challenge, because judgments are often confounded by unobserved traits in the local labor market. First, given points of dispute, judge's decisions may be biased by local labor market conditions, leading to reverse causation (Marinescu 2011; Ichino, Polo, and Rettore 2003). Second, cases are selected into trial according to dispute characteristics (Priest and Klein 1984), which can create systematic regional patterns in litigation outcomes. Previous studies have managed the endogeneity issue by considering the firing restriction to be exogenous in certain dimensions. In the U.S., for example, differential timing for the acceptance of exceptions to employment-at-will doctrine generated state-level variations in the extent of firing restrictions; researchers thus used these to identify the impact (Autor, Donohue, and Schwab 2006; Autor 2003). In studies of Italy, a discontinuity in legislative firing costs across firms was used (Leonardi and Pica 2013; Kugler and Pica 2008). While these previous studies have considered differences in established case law or written statute as exogenous policy changes, identifying the impact of enforcement action itself requires additional caution: individual court decisions are made at too local a level to ignore the tight connections between them and unobserved local labor market conditions.

This paper aims to identify the causal impact of dismissal-related court discretion on job flows by exploiting a unique feature of the Japanese legal system: the exogenous allocation of judges to prefectures. Within the Japanese system for managing judicial careers, judges are periodically transferred across prefectures in a way that is uncorrelated to local labor market conditions. Because Japan has a single national court system and there are no legal boundaries between prefectures, these judge transfers should be the main source of

variation generating differences in the enforcement of employment law across prefectures. Since judges move beyond prefecture borders, it is also possible to shut down the effects of any confounding relationships between current litigation outcomes and local labor market characteristics.

Specifically, this paper's analytic approach first estimates the effect specific to each judge based on records for litigation heard by that judge, excluding those within the prefecture to which the judge is currently assigned. Removing the litigation records from the current prefecture allows us to eliminate the direct reverse causation from current labor market conditions; it also prevents finding a systematic trend arising from selection in accordance with local dispute traits. Then, the estimated judge effects are used to instrument for the level of enforcement of firing restrictions within each prefecture, namely the worker victory ratio in adjustment dismissal cases, in order to examine whether pro-worker enforcement decreases employment flows in the local labor market.

Estimation results reveal that court discretion per se has nontrivial impacts. Consistent with theoretical predictions, a 10% increase in the worker victory ratio over the prior 10 years reduces the prefecture job creation rate by 1.2 to 3.7% and the job destruction rate by 0.9 to 2.7%. The reduction in job flows is mainly concentrated among women in the service sector and manufacturing-sector employees of both genders. Interestingly, it is also found that firms and workers consider current court decisions in relation to the outcomes of past litigation—that is, present court decisions have no significant effect per se. The worker victory ratio thus has a significant causal effect on job flow rates only when it is constructed based on litigation records at least 10 years old. This is consistent with restrictive but reasonable assumptions

on how agents form their beliefs regarding the court's enforcement actions: as agents do not know which judge would eventually oversee their litigation in the case of a trial, uncertainty in enforcement action anchors the perceived firing cost at the worst-case scenario within their prefecture.

In addition to answering a long-asked question in labor economics, this paper makes unique contributions to the literature. First, this paper applies the same method employed in a series of studies considering judge effects in the case of employment protection (Dobbie and Song 2015; Aizer and Doyle 2015; French and Song 2014), but it does so while completely excluding the confounding factors, including judicial bias, pointed out in the literature (Marinescu 2011; Ichino, Polo, and Rettore 2003). This is possible here because, unlike in previous studies, Japanese judges move beyond local labor markets, but the same law is still applied nationwide. Second, this paper also differs from a series of within-country studies on employment protection by focusing on the economic consequences of court or judicial discretion per se rather than on written regulations or established case law (Autor, Donohue, and Schwab 2006; Autor 2003; Leonardi and Pica 2013; Kugler and Pica 2008; Schivardi and Torrini 2008). Lastly, this work provides the first evidence on the impact of employment protection in Japan, showing that even in the absence of legal boundaries, administrative boundaries may suffice to generate cross-regional policy variations.[3]

The remainder of the paper proceeds as follows. Section 2 offers an overview of

[3] Ohtake and Fujikawa (2002) and Ohtake (2004) first compiled statistical information on the doctrine of adjustment dismissal established by the courts. JILPT (2006, 2007) followed; however, neither work analyzed the relationship of this doctrine to the labor market. Okudaira (2008, in Japanese), the previous version of this paper, presented preliminary evidence on the impact of judgments regarding adjustment dismissals but did not present the results of the IV estimations, which are of primary interest in the current paper.

Japanese employment protection, while Section 3 presents the estimation methods. Section 4 describes the data, while results are presented and discussed in Section 5. Section 6 concludes.

2. Institutional Background

Japan has a unique legal system. Its labor law is originally based on German law but heavily follows precedents as a result of the influence of common law during the United States' short occupation after the Second World War.[4] This introduction of American law allowed judges to make more liberal and teleological interpretations of existing civil laws (Araki 2002). Examples of such "judge-made law" include employment protection's so-called *doctrine of four prerequisites* (referring to the four prerequisites in the Doctrine of Abusive Adjustment Dismissals). Unlike some European countries, Japan had no written statute requiring just cause for dismissing workers.[5] Instead, by establishing the doctrine of four prerequisites, Japanese courts have de facto regulated employers' firing practices that result from business necessity (adjustment dismissal, or *Seiri Kaiko* in Japanese).[6]

The four prerequisites have severely limited firms' ability to adjust the number of employees in response to business shocks and flexibly achieve optimal production levels.

[4] The power of judicial review is a by-product of the 1947 constitution, which organized the present trial system (Itoh 1991, 195).

[5] The Japanese government codified the Doctrine of Abusive Dismissal in 2003 (Labor Contract Act, Art. 16), although the provision only generalized existing case law and did not stipulate the four prerequisites.

[6] The doctrine of four prerequisites for adjustment dismissal has been derived from the Doctrine of Abusive Dismissal, which was originally a "modification" by the courts to the Civil Code provision. It states that, "if the employment is not for a definite period, either party may make a request to terminate the contract at any time…" (Civil Code, Art. 627, Par. 1). Judges issued the Doctrine to alter this written statute in the face of a serious economic downturn after the Second World War, when the cost to workers of being fired was exceptionally high. See Sugeno (2002, 473–493).

The doctrine requires a firm to satisfy the following four prerequisites in order for an adjustment dismissal to be legitimate:[7]

1) There must be a need to reduce the number of employees.
2) Resorting to adjustment dismissals must be necessary to attain this personnel reduction.
3) The selection of the person or persons to be dismissed must be appropriate.
4) The dismissal procedures followed must be appropriate.

Ohtake (2004) statistically analyzed all published adjustment dismissal litigation records and revealed that Japanese courts have rigorously required defendant firms to demonstrate reduced sales in the prior fiscal term in order for firms to satisfy the first prerequisite. As a result, firms must retain unproductive labor until they meet this standard. Similarly, the second prerequisite is satisfied only if firms have clearly made their best efforts to avoid dismissing workers. For example, they must have suspended hiring mid-career professionals and new graduates, reallocated workers within a company, farmed out workers to related companies, and solicited early retirement (Ohtake 2004; Sugeno 2002). By soliciting early retirement, firms risk forgoing productive labor.

Japan is regarded as having relatively stringent employment protection (OECD 1999, 2004). OECD (2007) also noted that employment protection is particularly restrictive for

[7] Prominent examples include *Shimazaki v. Toyo Oxygen*, 30 Rodo Minshu 1002 (Tokyo High Ct. Oct. 29, 1979).

regular workers in Japan and proposed reforms to its EPL. This proposal was partly motivated by the uncertainty inherent in court-mandated law. Indeed, the four prerequisites are continuously revised, and the doctrine's wording is open to a wide range of interpretations.

Particularly since the late 1990s, the Tokyo District Court has attempted to relax the prerequisites and allow firms to more easily exercise adjustment dismissal (Mori 2001). With regard to the second prerequisite mentioned above, for example, the court has tended to define a relatively narrow scope for a labor contract by requiring firms to reallocate workers only within an affiliated company and not across all related companies (*Saitoh v. Chase Manhattan Bank*, 609 Rodo Hanrei 63, Tokyo D. Ct. Feb. 27, 1992). This leaves employers with considerable freedom to legitimately adjust labor allocation. A series of similarly lenient decisions followed in Tokyo in the late 1990s, although other courts maintained relatively strict applications of the four prerequisites (Sugeno 2010).[8]

To support this paper's analysis, it is useful to provide a brief summary of the Japanese judicial system. First, all litigation involving adjustment dismissals proceeds via bench trials, without juries. Judges decide questions of fact in addition to questions of law (Ramseyer and Rasmusen 2003). Second, Japan employs a three-instance trial system: parties usually have three opportunities to contest a case in court, via prefectural district courts, high courts at each regional block, and the supreme court as a final stage. Third, Japan employs a single national court system but no prefectural (that is, state) court systems. Judgments made in

[8] Judges at Division 10 of Tokyo District Court also had a strong influence on the preliminary debate of the Doctrine of Abusive Dismissal by declaring their opinions in published works (Foote 2006). Judge convocation (*Saiban-kan Kaido*) also fosters a specific judicial climate. Judge convocation is held by the Supreme Court Secretariat, occasionally within the High Court jurisdiction area, in order to provide judges with an opportunity to exchange their opinions and, more importantly, consolidate their views (Nishikawa 2005).

lower courts are applicable to all lower courts but do not necessarily bind future decisions. Only Supreme Court decisions bind lower court judgments. Thus, a court decision in a given prefecture can constitute both enforcement action and precedent. Additionally, as exploited in this paper's estimation strategy, the prefecture for the first trial is exogenously given to both parties: plaintiff workers must bring their case to a lower court in the region where the defendant firm or plant resides (Code of Civil Procedure, Art. 4). Also crucial to this paper's approach is the fact that every year, Japan's Supreme Court Secretariat, which manages judicial human resources, transfers a proportion of judges across prefectures and courts. Judges usually do not stay for very long in a given prefecture or court, especially when they are young: typically, each judge receives a new assignment every three to five years. Practically, judges cannot reject their postings, although they are nominally entitled to do so (Ramseyer and Rasmusen 1997).[9]

3. Empirical Strategy

3.1. Estimation Model

The baseline model used in this paper begins with a simple ordinary least squares (OLS) estimation with fixed effects. Let Y denote the job flow rate in the local labor market and W_{pt}^{m} denote the worker victory ratio in the past *m* years—that is, the share of cases won by the suing worker between years ($t - m$) and t. Let subscript p refer to a prefecture and t

[9] "Nominally, a judge can refuse any posting he dislikes. In fact, he refuses at his peril. By 1969, Judge Shigeharu Hasegawa had worked in Hiroshima for seventeen years. The Secretariat then assigned him to an out-of-town position, formally a promotion. Hasegawa, however, had a sick wife and did not want it. He declined the promotion, and when the time came for his next ten-year appointment he found himself out of work" (Ramseyer and Rasmusen 1997, 156).

refer to a year. The OLS regression model is presented in equation (1):

$$\ln(Y_{pt}) = \alpha + \beta_1 W_{pt-1}^{m} + \varphi_p + \theta_t + \varepsilon_{pt}, \quad (1)$$

Both year dummies (θ_t) and prefecture fixed effects (φ_p) are included. A one-year lag in the worker victory ratio is used to account for the possibility that it takes time for firms and their attorneys to gain new information, use it to update their expected cost of firing, and incorporate this updated cost into their employment decisions. Several different time windows (m) are used to calculate the worker victory ratio in order to examine how long a span of past litigation history the economic agents take into account. Standard errors are clustered at the prefecture level to allow for arbitrary serial correlation within the prefecture (Bertrand, Duflo, and Mullainathan 2004). To support the paper's main hypothesis (Hopenhayn and Rogerson 1993), the estimate of β_1 is expected to take a statistically significant negative value, as pro-worker enforcement of the doctrine creates a wedge between the value of the marginal product of labor and the wage rate, thereby reducing the job turnover rate, given fixed wages.

The direct estimation of equation (1) would likely lead to an inconsistent estimate of β_1: $\mathrm{E}\left[\varepsilon_{pt} | W_{pt-1}^{m}\right] \neq 0$, as the worker victory ratio reflects some unobserved but important confounders. For example, local labor market conditions can influence judgments regarding adjustment dismissals because judges may disproportionately favor workers during recessions (Ichino, Polo, and Rettore 2003). Alternatively, judges could adjudicate in favor of firms during recessions because the four prerequisites are generally more easily satisfied

when economic conditions are worsening. Another source of endogeneity stems from an unobserved dispute climate, which could affect both the litigation selection process and wage rigidity: in prefectures with smaller worker stakes, such as lower bargaining power, the litigation proceedings selected are those in which workers have a lower probability of victory; the lower bargaining power of workers also brings about wage adjustment, which could in turn affect the extent of job flows.

The instrumental variable (IV) analysis in this paper is adapted from Kling (2006) and Aizer and Doyle (2015), who identified the effects of incarceration length on future labor market outcomes using the random assignment of judges to individuals as an instrumental variable.[10] This paper applies a modified version of this approach to analyze the employment protections imposed by local courts in a two-stage least squares (2SLS) estimation framework. Similar to Kling (2006) and Aizer and Doyle (2015), this approach compares groups of otherwise similar prefectures that experience more or fewer worker victories because they were exogenously assigned to judges with different levels of leniency when adjudicating adjustment dismissals. Unlike previous studies, however, the first-stage equation used in this paper must transform judge-level information into a prefecture-level structure, as this paper uses discretion exercised at the judge level to identify the impact of de facto firing restrictions at the prefecture level. To do so, the first-stage equation is modeled

[10] An increasing number of studies is using the random assignment of judges or examiners to identify policy effects. Di Tella and Schargrodsky (2013) tested whether being assigned electronic monitoring matters for recidivism. Doyle (2007, 2008) estimated the impact of foster care placement on long-term outcomes of children. Dobbie and Song (2015) and Chang and Schoar (2013) identified the effects of consumer or firm bankruptcy protections on long-term outcomes. French and Song (2014) and Maestas et al. (2013) quantified the labor supply effects of receiving Social Security Disability Insurance benefits.

with a set of judge effects as instruments for W_{pt}^{m}:

$$W_{pt}^{m} = \alpha_0 + \sum_{j=1}^{J} \lambda^j \left[D(n)_{pt}^{j} \cdot H_{-p}^{j} \right] + \varphi_p + \theta_t + u_{pt}, \qquad (2)$$

$D(n)_{pt}^{j}$ is a judge dummy indicating the assignment of a judge who had made a decision that affected the worker victory ratio within the relevant time period: it equals one if judge *j* made a decision related to abusiveness in adjustment dismissals in prefecture *p* and year *t*, and continues to take a value of one until year (*t* + *n*) to accommodate the fact that the endogenous variable was constructed using past information. In the main analysis, all *n*'s are set to *m*. However, *n* is allowed to take a smaller value, namely *n* = 1, later in the analysis. H_{-p}^{j} is an estimated judge effect for judge *j* when he or she is assigned to prefecture *p*.

The purpose of estimating equation (2) is to remove the variation in W_{pt}^{m} that arises solely from judicial restrictions imposed by judges, not from confounders such as unobserved heterogeneity in local labor markets. Most previous studies estimate judge effects by calculating a leave out mean of judge *j*'s decisions excluding the present judgments made by judge *j* within the same court (Dobbie and Song 2015; Aizer and Doyle 2015; French and Song 2014). However, this method could be problematic here because the accumulated worker victory ratio, W_{pt}^{m}, includes past judgments by definition; it thus reflects information on past local labor market conditions in prefecture *p*. Leaving out present judgments only would be insufficient, also because it can give rise to a systematic trend caused by selection in accordance with specific dispute traits (Priest and Klein 1984) as well as persistent

economic shocks that may trigger judicial bias (Marinescu 2011; Ichino, Polo, and Rettore 2003). The estimated judge effect for judge *j* in prefecture *p* and year *t* should be independent from all past, current, and future litigation records for judge *j* within prefecture *p*.

This paper overcomes this problem by taking advantage of the fact that judges in Japan are regularly transferred across prefectures. Specifically, we first calculate judge *j*'s individual effect while excluding *all* the decisions made by judge *j* in the present prefecture to obtain a leave-out mean for judge *j*. This is possible because, due to the transfers, judges have litigation records in prefectures other than their current prefecture. Then, the average leave-out mean at the same prefecture is subtracted from each judge's leave-out mean to standardize the judge effects to be zero in case of no assignment:

$$H_{-p}^{j} = \left(\frac{1}{N_j - n_p^j}\right) \sum_{l \; not \in \mathbf{L_p}}^{l \in \mathbf{L}} v_l^j - \bar{\mathrm{V}}_{-p}, \qquad (3)$$

where N_j is the total number of litigations that involved judge *j* and n_p^j is the total number of litigations in prefecture *p* that involved judge *j*. **L** is the set of all litigations heard by judge *j* over the sample period, and $\mathbf{L_p}$ is the set of all litigations heard by judge *j* in prefecture *p*. Let v_l^j be a dummy variable indicating worker victory in litigation *l*, which was overseen by judge *j*. $\bar{\mathrm{V}}_{-p}$ is a prefecture-level average of the first term on the right-hand side of equation (3). A positive (negative) value of H_{-p}^{j} indicates that a judge has a more strict (less strict) view towards approving dismissals than do other judges in the same prefecture. While a judge's leniency could be confounded by current local labor conditions, the estimated H_{-p}^{j}

is uncorrelated with ε_{pt} even in a small sample because all litigations in prefecture *p* have been removed. The resulting 2SLS estimate of β_1 should yield a consistent estimate of the effect of an increase in the accumulated worker victory ratio stemming directly from the assignment of judges who are lenient to workers.

The above identification strategy is justified if judges exercise considerable discretion in adjustment dismissal litigations *and* if the assignment of judges to prefectures is uncorrelated with labor market performance. In other words, the instruments must be relevant and valid. The latter condition will be examined in the next section. Regarding the relevance of the instruments, econometric literature emphasizes the importance of checking for weakness of instruments, which can cause serious finite-sample bias as well as size distortions in inference (Bound, Jaeger, and Baker 1994; Stock, Wright, and Yogo 2002; Murray 2006). Moreover, the finite-sample bias in 2SLS becomes more detrimental when the number of instruments is large relative to the number of observations (Hahn and Hausman 2002, 2003). To accommodate these potential problems with instrumenting with many judge effects, results are presented from an alternative estimation method along with conventional statistics for detecting weak instruments (Stock and Yogo 2005). In particular, Fuller (1977)'s modification of the Limited Information Maximum Likelihood (LIML) estimator is used in the IV analysis along with 2SLS, as it is known to have better properties than 2SLS or LIML in the presence of weak instruments.[11]

[11] The Limited Information Maximum Likelihood (LIML) estimator has been proven to possess better finite-sample properties than 2SLS estimators, although LIML and 2SLS share the same asymptotic distribution. On the other hand, LIML has no moments; Fuller's (1977) LIML thus makes this modification. Fuller's parameter is set to one.

3.2. Are Judge Transfers Exogenous?

To obtain consistent estimates for our parameter of interest, it is crucial to confirm that judge assignments, as explained at the end of Section 2, are exogenous to unobserved labor market conditions. In terms of equation (2), we must verify that $\mathrm{E}\left[\varepsilon_{pt}|D(n)_{pt}^{j} \cdot H_{-p}^{j}\right] \neq 0$ for all j. As explained in the last section, each judge effect is estimated while omitting any judgments made by the judge in question within his or her currently assigned prefecture. This ensures that H_{-p}^{j} is uncorrelated with the first equation's error term, ε_{pt}. However, this does not necessarily mean that the assignment itself, which is represented by a set of assignment dummies, is exogenously given.

Appointments are not necessarily exogenous for two reasons. First, the Japanese judicial transfer system has an incentive scheme allowing judges to be promoted at an earlier stage or transferred to "prestigious" courts, such as those in metropolitan areas. Examples of such fast-track positions are those at courts in Tokyo or administrative positions at the Secretariat; unfavorable appointments include positions at branch offices or rural courts (Nishikawa 2005). Ramseyer and Rasmusen (2003) examined judges' detailed career records and revealed that judges are promoted to *Sokatsu*, an administrative position, significantly earlier if their first position was at the Tokyo District Court and significantly later if they were initially assigned to a branch office. Ramseyer and Rasmusen (2003) also presented evidence suggesting that the Secretariat retaliated by assigning judges to positions at branch offices of rural area courts if judges' rulings suggest constitutional illegitimacy or express any anti-government opinions. This works as an incentive for judges to avoid such actions and thus be transferred to "better" courts or metropolitan areas, in which local labor market

performance may systematically differ. Second, since the Secretariat takes judges' location preferences into account but to an unknown degree, a judge's ideology, as expressed through her decisions in litigation proceedings, could select her into a region where the general political background aligns with her own.[12] For instance, pro-worker judges may systematically prefer to be transferred to regions where unions exercise considerable political power. For the sake of our analysis, it is essential that judges' leniency toward dismissals does not predict the type of prefecture to which they are assigned.

To confirm this requires testing for whether a current judgment matters for the "quality" of the next assignment. To do so, prefectures are first ordered by rank on several outcome measures in each year; then, the rank difference is taken between each judge's current prefecture (court) and the next prefecture (court) to which he or she is assigned. Then, the rank difference, $(CurrentRank_t - NextRank_t)$, is regressed on the judge's decisions regarding dismissals of litigation proceedings within the current prefecture. If pro-worker judges are transferred to prefectures with certain labor market characteristics, the coefficient estimate should be significant. (Note that rank differences are used to avoid reflecting the labor market conditions in the current prefecture, which could affect judges' current court decisions).

The sample is limited to those judges who dealt with adjustment dismissal cases at least once in the time covered by the dataset (introduced in the next section). Transfer records

[12] Judge transfers usually involve three steps. First, the Secretariat asks judges to note their preferences on an information sheet including information about family members, such as occupations and health status, as well as how many and what kinds of litigation proceedings the judge has previously managed. In the second step, the Secretariat asks top managers in each court to provide evaluations of each judge. In the final step, the Secretariat assigns a judge to his or her next court by taking all this information into account; this decision process, however, is something of a "black box" (Nishikawa 2005).

are dropped if the judge's next assignment is in a non-court position or if the judge retires. For each judge, the first transfer after he or she offered a dismissal-related decision is considered. The data used consists of 160 judge transfers identified by *Zen Saibankan Keireki Soran* ("Biographical Information on Judges," the ZSKS hereafter) between 1985 and 2000.

Table 1 reports the average worker victory indicators for judges by rank-difference categories. Ranks are taken for the outcome variables analyzed in the analysis and for those that could potentially explain the unobserved labor market traits. At the bottom of each panel are presented coefficients from a simple regression of rank difference on the judgment variables. Table 1 shows that judges with higher worker victory rates are not necessarily transferred to prefectures sharing specific characteristics, at least in terms of the four outcome variables considered. No systematic pattern can be found between rank differences and litigation outcomes, and none of the regression significantly predicts improvements in outcome measures. Thus, although there is an incentive for judges to adjudicate in a way that identifies them for promotion to specific prefectures, judges are at least transferred in a way that is exogenous to potential labor market confounders. Thus, not only the judge effects (H_{-p}^{j}) but also their interaction terms with assignment dummies ($D(n)_{pt}^{j} \cdot H_{-p}^{j}$) are uncorrelated with the second-stage error term, ε_{pt}.

4. Data

Data for the outcome variable (Y) in equation (1) is taken from *Surveys on Employment Trends* annual aggregated files for the years 1985 to 2000. These surveys are conducted semiannually by the Japanese Ministry of Health, Labor, and Welfare by sampling

establishments with at least five regular employees from 16 major industries in Japan. The job creation and destruction rates are calculated by prefecture-year group to measure flows of jobs into and out of the local labor market. The job flow data is based on regular workers (*Joyo Rodosya*) with both fixed-term and permanent contracts and does not include outsourced workers or employment via temporary help agencies. Japanese courts have typically regarded the repetitive renewal of a fixed-term contract as equivalent to a permanent contract and applied the doctrine of four prerequisites; as such, "regular workers" roughly capture the complete set of workers who are subject to the doctrine. Table 2 provides summary statistics.

The worker victory ratio (W_{pt}^{m}) was constructed based on adjustment dismissal cases in which plaintiff workers sued defendant firms for the abusiveness of firing due to business necessities. Let EE_{pt} and ER_{pt} respectively denote the number of worker victories and the number of employer victories in adjustment dismissal litigations observed in prefecture *p* and year *t*.[13] Then, the worker victory ratio for prefecture *p* in year *t* given the precedents of the last *m* years is defined by:

$$W_{pt}^{m} = \frac{\sum_{i=t-m}^{t} EE_{pi}}{\sum_{i=t-m}^{t}(EE_{pi}+ER_{pi})} \qquad \text{if } \sum_{i=t-m}^{t}(EE_{pi}+ER_{pi}) > 0$$

$$= 0.5 \text{ , otherwise.} \qquad (4)$$

When no adjustment dismissal litigations were recorded in the relevant period (that

[13] Cases were considered neutral and double-counted both in *EE* and *ER* if the defendant firm won the case but the plaintiff worker also obtained some compensation.

is, $\sum_{i=t-m}^{t}(EE_{pi} + ER_{pi}) = 0$), 0.5 was assigned. Worker victory ratios were calculated in cases of m = 1, 3, 5, and 10, as well as $m = t$ –1950 (that is, including all litigation records from 1950 until year t, with 1950 used as the starting point because the Allied High Command introduced a set of labor regulations between 1945 and 1947 that formed the benchmark for postwar industrial relations.)[14]

The litigation dataset was drawn from a collection of adjustment dismissal precedents organized by Ohtake (2004) and used in his analysis of the development of the doctrine of four prerequisites.[15] The records were originally taken from the prevalent "Judicial Information System" (JIS) report, provided by *Dai Ichi Hoki Co.* JIS encompasses every *released* litigations in approximately 90 published periodical reports since the Meiji era. While JIS does not include unpublished litigation records, it captures the information set influencing the relevant economic agents, who usually observe only released cases.[16] The Ohtake (2004) dataset was obtained by typing "adjustment dismissal (*Seiri Kaiko*)" into the JIS keyword search to identify reports of cases in which the plaintiff worker(s) accused the defendant firm of wrongful adjustment dismissal. Each litigation record was checked by law school students, and some cases that did not involve dismissals were excluded from the dataset.[17] The sample period ranges from January 1950 to December 2000, encompassing a

[14] These laws were the Trade Union Law in 1945, Labor Relations Adjustment Law in 1946, and Labor Standards Law in 1947. With these laws, Japanese courts established the Doctrine of Abusive Dismissal and later the doctrine of four prerequisites. See Sugeno (2002) for a thorough discussion of the historical development of Japanese labor law.

[15] Fumio Ohtake (Osaka University) kindly provided the dataset. Note that in Ohtake (2004) certain cases were added to the dataset if they were well known and influential but not detected by a JIS keyword search.

[16] It is known that the Supreme Court Secretariat tends to allow "époque-making" or "rare" cases to be published in litigation reports (comments from a seminar participant (judge) at the Legal Research and Training Institute, November 9, 2006, Tokyo).

[17] Some legal scholars indicate that it is difficult to precisely distinguish "adjustment dismissal" cases from

total of 260 adjustment dismissal cases. In constructing the worker victory ratio in equation (4), each case is allocated to all prefectures under the jurisdiction of the court in which the case was heard: a case was allocated to the corresponding prefecture if it was heard in a district court, to all corresponding prefectures in the jurisdiction if it was heard in a high court, and to all 47 prefecture if it was heard in the Supreme Court.

The sample period ended in 2000 since Japan introduced a new system for resolving individual labor disputes in 2001. The new system led many potential litigants to go through simple legal examination processes before bringing a case to court.[18] While this approach became increasingly popular among workers, it also substantially altered the way in which dismissal disputes are selected into courts. As the cost of bringing a case to court has become disproportionately lower for plaintiff workers, this may have shifted the distribution of the worker victory among the cases brought to court (Priest and Klein 1984). To avoid this change impacting the analysis, the sample period was chosen to end in 2000.

Figure 1 graphs the worker victory ratio since 1950 ($m = t - 1950$) for each prefecture. There are three points to be noted. First, Tokyo Prefecture (No. 13) exhibits a persistently low worker victory rate, whereas Osaka Prefecture (No. 27) maintained a high worker victory rate after the 1970s. This is consistent with a series of lenient interpretations of the four prerequisites by judges in Tokyo District Court, as mentioned in Section 2. Second, prefectures under the same High Court (H.C.) jurisdictions share similar patterns; this is a

other types of dismissal litigations, where, for example, workers were fired due to their incompetence (Ogawa et al. 2007). This paper includes those cases and thus broadly defines "adjustment dismissal."

[18] Among about 300,000 civil cases examined at the initial stage in 2013, 27.2% were dismissal-related cases (Reviews on the Execution Status of Individual Labor Disputes Resolution System, the Japanese Ministry of Health, Labor, and Welfare).

result of the variable construction strategy in which High Court judgments are applied to all prefectures in the same jurisdiction. In essence, prefectures under the jurisdiction of Tokyo H.C. [c] and Fukuoka H.C. [h] saw relatively fewer worker victories, while those under Osaka H.C. [e] and Hiroshima H.C. [f] saw relatively more. Third, some common trend can be observed, perhaps partly due to guidelines published by the Secretariat.[19]

The number of adjustment dismissal cases obtained above, 260, is too small relative to the number of involved judges, 595, to allow for estimating the judge effects (H_t^j) in equation (3).[20] To overcome this difficulty, this paper makes use of the fact that judges issue decisions in adjustment dismissal litigations by following a general principle explained in Section 2, the Doctrine of Abusive Dismissal (*Kaikoken Ranyo Hori*), which was originally established by the Supreme Court[21] and later enacted in the Labor Contract Act.[22] Since this doctrine is applied not only to adjustment dismissal litigations but also to other types of dismissal cases, judges are likely to exercise similar discretion when judging abusiveness in all dismissal cases. As such, the adjustment dismissal records described in the previous section are augmented with records on other types of dismissal litigation proceedings for which abusiveness is also the point at issue. In particular, instead of typing "adjustment dismissal

[19] The Supreme Court Secretariat distributed confidential guidelines regarding the doctrine of four prerequisites to all lower courts in January 1986. The guideline contained a rather pro-employer interpretation of the doctrine, which is consistent with the common downward trend shown in Figure 1. The Secretariat later commented that this was not meant as a judicial control but rather an exchange of opinion (*Asahi Newspaper*, evening edition, April 16, 1988).

[20] A panel of judges for each case usually constitutes one to five judges at lower courts. This paper assumes that each judge made decisions independently.

[21] *Ichikawa v. Nihon Shokuen Seizo Co.*, SC., April 25, 1975, 29 Minshu 456.

[22] "A dismissal shall, if it lacks objectively reasonable grounds and is not considered to be appropriate in general social terms, be treated as an abuse of right and be invalid" (Labor Contract Act, Art. 16). The doctrine of four prerequisites for adjustment dismissal has also been derived from the Doctrine of Abusive Dismissal. See Sugeno (2002) for details.

(*Seiri Kaiko*)" into the JIS search system as done in Ohtake (2004), "dismissal (*Kaiko*)" was used to obtain all types of dismissal-related litigations from 1949 to 2000 (including punitive, union-shop, and worker incompetence dismissals in addition to adjustment dismissals). The data is limited to lower-court decisions. This increases the number of observations from 260 to 1727 litigations, which were used to estimate H_{-p}^{j} in equation (3). Judge effects are estimated for judges who were involved in adjustment dismissal litigations and also experienced at least one transfer during the sample period. A total of 164 judge effects are estimated from 1950 to 2000. Among these, 104 judges obtained assignments to the prefectures considered in our main analysis, for which the sample period ranges from 1985 to 2000.

5. Results

5.1. Baseline Estimates

Table 3 reports the results of estimating the baseline model (equation (1)), with *m* taking several different values. Odd-numbered columns control only for year dummies while even-numbered columns control for both prefecture and year fixed effects. The results in the odd-numbered columns, excluding column 11, show that increases in the worker victory ratio are significantly and negatively correlated with reductions in job flows, consistent with theory. Taken at face value, the larger *m*, the greater the estimated impact of a same-unit increase in the worker victory rate. This is consistent with the fact that as *m* increases, a larger number of judgments in which the worker wins are needed in order to obtain the same unit increase in the worker victory ratio.

In contrast, the results in the even-numbered columns indicate that some of these negative estimates are driven by unobserved time-invariant fixed effects across prefectures. For m = 1, 3, and 5, the worker victory ratio is not statistically related to either the job creation rate or the destruction rate, after controlling for prefecture fixed effects. For m = 10, the estimates are significant but smaller in absolute term than those excluding fixed effects. Comparing the even- and odd-numbered columns suggests that simple OLS estimates without prefecture fixed effects suffer from spurious negative bias when m is less than or equal to 10. Provided that prefecture-specific effects do indeed capture persistent differences in local labor markets, this result is consistent with Ichino et al. (2003), who found judges to favor workers in recessions.

Interestingly, the bias arising in the simple OLS estimates reverses and the negative impact gets even larger when the worker victory ratio is constructed for $m = t - 1950$, as shown in columns 10 and 20 of Table 3. This indicates a spurious positive bias in simple OLS estimation without fixed effects. Since the worker victory ratio with m = 10 utilizes litigation records no older than 1975, the spurious positive correlation may arise from a correlation between pre-1975 worker victory levels and unobserved labor market heterogeneity. As the doctrine of four prerequisites arose in the mid-1970s (Ohtake 2004), court decisions issued around this time may have judged the legitimacy of firms firing workers in light of the four prerequisites. In prefectures undergoing economic booms, it is difficult for firms to satisfy the four prerequisites since they are dependent on business conditions, as discussed in Section 2. Thus, local labor market conditions may also have affected the post-1950 worker victory ratio but in a direction opposite to that in previous cases.

To summarize, the effect of the worker victory ratio remains significant and negative only when $m = 10$ and $m = (t - 1950)$. Thus, current fluctuations in worker victory rates matter only when compared to past litigation records from at least ten years prior. This implies that recent litigation outcomes are not seen as effective enforcement action per se, and agents also consider past litigation records.

5.2. IV Estimates

A key to the identification is to find sufficient variations in worker victory ratios via judge allocations in the first stage. To confirm this point, Figure 2 plots first-stage predictions for Tokyo and Osaka Prefectures for the cases where $m = 10$ and $m = t - 1950$. Blue lines indicate first-stage linear predictions without prefecture and year dummies (thus, including only judge effects and a constant). Red dots indicate first-stage linear predictions with prefecture and year dummies along with judge effects. Connected green dots are the actual worker victory ratios. The figures show that judge assignments explain large shares of the trends in worker victory ratios. Despite the fact that judge effects are estimated excluding litigation records in the current prefecture, the blue lines closely track the actual changes in worker victory ratios. Most importantly, predictions with and without prefecture and year dummies vary little. Thus, variation in the first stage stems mainly from judge effects, not from prefecture and year fixed effects, validating the IV approach proposed in equation (2). Appendix Figures 1 and 2 present analogous graphs for all prefectures over all possible sample periods, 1950 to 2000. In both figures, the assignment of judges can be seen to lead to many but not all of the changes in worker victory ratios. In some prefectures, however, the

judge assignments fail to predict changes in the worker victory ratio, especially for those prefectures under the jurisdiction of Nagoya H.C. [d] and Hiroshima H.C. [f].

Consistent with Figure 2, first-stage statistics from the IV estimations confirm that the first-stage estimations have considerable explanatory power in all cases. Table 4 reports IV estimation results along with relevant first-stage statistics, including F-statistics from a joint significance test for all excluded instruments. The partial R^2 ranges from 0.66 to 0.92, suggesting that judge assignments explain at least two-thirds of the variation in worker victory ratios, even after the effects of included instruments are purged out. The column displaying the 2SLS results shows that they are similar to those found in the OLS estimation, which are replicated from Table 3. In particular, an increase in the worker victory ratio is found to significantly reduce job flows when $m = 10$ and $m = (t - 1950)$. The magnitude of the negative impact estimated via the 2SLS approach is larger than that derived from the OLS estimates, suggesting that the fixed-effect OLS estimates still suffer from positive bias. Interestingly, we find no significant estimates when m =1, 3, or 5, even though the partial R^2 suggest equally high explanatory power in the first stage.

While the first-stage F-values are not small, our 2SLS estimation may still suffer from potential finite sample bias and size distortions because the first-stage equation includes many instruments (Hahn and Hausman 2002, 2003). Indeed, Stock and Yogo (2005)'s tables suggest that some of the 2SLS specifications presented here may have large size distortions. For instance, the first-stage partial F-value when $m = (t - 1950)$, as shown in panel A, does not allow one to reject the null hypothesis that the actual size of the 2SLS 5% test exceeds 25% (critical value = 32.16, vs. F value = 29.74). While it does reject the null hypothesis that

the maximal bias of the 2SLS estimator (relative to OLS) exceeds 5%, a conservative reading suggests that the 2SLS estimates shown in Table 4 suffer from weak instruments.

For this reason, the LIML column in Table 4 presents the estimation results from Fuller's modification of LIML along with critical values for testing the weakness of the instruments based on Fuller's estimator with maximal 5% bias. Similar to the OLS results, they show a significant negative effect of worker victory ratios when $m = (t - 1950)$ and $m = 10$, even after accounting for the weak instruments. For instance, a 10% increase in the worker victory rate since 1950 significantly reduces the job creation rate by 8.6% and the job destruction rate by 6.7%. Unlike in the 2SLS case, the F-values are well above the critical values based on LIML bias in all cases, suggesting that finite sample bias is minimal. It should be noted that all cases also pass the 5% size test based on Fuller's estimator. Again, the estimates are insignificant when m is small, even though they all pass the weak instrument tests. Thus, agents consider information on past litigation in relation to current worker victories; present court decisions have no impact per se.

One concern with the above analysis is that judge assignments may be considered a one-time shock to enforcement action, as judges do not stay in the same prefecture forever. If judge assignments themselves matter, it should be sufficient to instrument judge assignments for one year, $n = 1$, rather than for the whole sample period, $n = m$, in equation (2). To test if the results in Table 4 are robust to the length of period for which the judge assignments are instrumented, Table 5 presents identical IV estimation results but with $n = m$ replaced by $n = 1$. Unlike in the previous results, the first-stage explanatory power is low in all cases. The greater the value of m, the lower both the partial R^2 and the F-statistics are.

This is partly because judge effects are instrumented only for one year, so the first-stage estimations do not capture many of the persistent changes in worker victory ratios that arise from older litigation records. Additionally, the first-stage F-statistics are quite small, suggesting large finite sample bias and size distortions in the 2SLS estimation. However, comparisons between the F-values and the critical values suggest that Fuller's modified LIML estimator still performs sufficiently well except when m = (t – 1950). The Fuller's estimates in panel B suggest that an increase in worker victory ratios in the last 10 years significantly decreases job flow in terms of both creation and destruction. The negative effects estimated are larger than those in Table 4. As above, the results are insignificant when *m* is reduced, even though the first-stage diagnostic statistics indicate greater explanatory power. Thus, this robustness test of *n* does not alter the findings in Table 4, but the specification used here no longer provides a credible IV estimate when using the worker victory ratio since 1950.

To summarize the main findings, the conservative estimates suggest that a 10% increase in the worker victory ratio over the last 10 years reduces the job creation rate by 1.2 to 3.7% and the job destruction rate by 0.9 to 2.7%. Differences in impacts on job creation and job destruction rates are not statistically significant, but the estimate is always larger for the creation side than for the destruction side. Thus, no strong evidence is found to support the claim that more worker victories either reduce or increase employment. However, a caveat applies: the empirical strategy employed here does not provide estimates that are precise enough to specify the combined effect of court decisions on the net employment change, as evident from the large standard errors.

The estimates also suggest that the impact of any given court decision is not always high; instead, it depends on past litigation information, such as the total number of litigations or worker victories. To increase the last 10 years' (as of 2000) worker victory ratio by 10% in Tokyo, for instance, would require just three more adjustment dismissal litigations in which the worker won. In Osaka, in contrast, the worker victory rate is already as high as 0.85, and about 24 more litigations with victorious workers would be needed to obtain the same 10% increase. This is reasonable as agents would not weigh a single worker victory very highly if they had already seen many of them. Indeed, a failure to account for this past information leads to insignificant relationship between the worker victory ratio and the outcome variables, as observed for $m = 1$, 3, and 5.

It is interesting to consider why past litigation records matter if judges are periodically transferred, as information on past litigation should no longer have an effect once the involved judge has moved to another prefecture. One possible explanation is that firms and workers do not have complete information about judge assignments; the uncertainty regarding litigation outcomes thus anchors agents' behavior at the worst case within the prefecture. Even if firms and workers know which judges are assigned to their prefecture, they do not know in advance who would rule on their dispute were it brought to trial. Uncertainty, which is different from risk, has distinctive behavioral outcomes: preferences are hard to describe with a single distributional measure when agents are faced with uncertainty (Knight 1921). In such cases, agents' preferences can be represented by the minimum expected utility value, which is calculated as an element of a set of probability measures (Schmeidler 1989; Giloboa and Schmeidler 1989). Thus, agents base their

decisions on the past worst-case scenario available to them, ending up with high firing costs.

5.3. Estimates by Gender and Industry

Finally, Table 6 examines whether the results obtained above are driven by any specific gender or industry groups. Estimates are obtained using Fuller's LIML estimation with the same specification as applied above but with the outcome variable replaced by the job creation or destruction rate for each gender-industry category. The results, shown in Table 6, suggest that the impacts are mainly concentrated among men and women in the manufacturing sector and among women in the service sector. Indeed, almost half of the cases in the JIS sample (129 out of 260) involve the manufacturing sector. Workers in the wholesale and retail sectors do not experience a significant decline in job flows.

6. Conclusion

Despite the potential influence on the de facto stringency of the law, the economic costs of court discretion are often ignored. This paper sought to quantify the economic impact of judicial discretion, using employment protection as an example. Doing so requires an extra caution because individual court decisions are tightly related to local labor market characteristics, which could cause a serious endogeneity issue; this was circumvented by exploiting three distinct features of Japanese courts. First, the prefecture for the initial trial is exogenously given to both parties. Second, judges are periodically transferred across local labor markets, while one national court system ensures no legal boundaries across prefectures. Lastly, the way in which judges are transferred is uncorrelated to local labor market

characteristics. Leveraging these features allowed us to remove any confounding relationship between court discretion and local labor market traits, including the reverse bias pointed out in the literature (Marinescu 2011; Ichino, Polo, and Rettore 2003).

Findings in this paper showed that court discretion per se has a remarkable impact. Consistent with theoretical predictions, a 10% increase in the worker victory ratio over the prior 10 years reduces the job creation rate by 1.2 to 3.7% and the job destruction rate by 0.9 to 2.7%. While the estimates are not precise enough to test the significance of the differential effects on the job creation and destruction rates, the magnitude of the estimate is larger for the job creation rate than for the job destruction rate in most cases. Additionally, the negative effects on job flows are concentrated among all workers in the manufacturing factor and women in the service sector.

Interestingly, we also found that the impact of the worker victory ratio is no longer significant if it is constructed based only on litigation records from within the past five years. Thus, current fluctuations in the worker victory ratio have no impact per se, though they do matter when compared to past litigation records from at least 10 years prior. This result would seem odd if firms and workers possessed complete information about which judges would oversee their litigation proceedings if brought to trial, but this is highly unlikely: as judge assignments are uncertain, firms likely anchor their expectations based on the worst-case scenario they know within their local court jurisdiction.

This paper's analysis offered some important insights regarding Japan's judicial career system. While judge transfers were shown to be exogenous to local labor market characteristics, Appendix Figures 1 and 2 suggested certain specific regional patterns in the

first-stage predictions. For instance, Osaka Prefecture (No. 27) had a persistently high predicted worker victory ratio after the 1970s, while Tokyo Prefecture (No. 13) showed an opposite pattern. Prefectures under the jurisdiction of Fukuoka High Court [h] also had persistently low worker victory ratios. If judge assignments were completely random, the first-stage predictions would show no long-term differences across prefectures, especially if all litigation records since 1950 are used to construct the worker victory ratio. The existence of persistent differences suggests that judge transfers follow specific patterns related to judges' ideologies in dismissal cases, even though the transfers were shown to be exogenous to local labor market traits (Table 1).

It is possible that the transfer pattern is roughly segregated by regional blocks, as the Secretariat takes judges' preferences into account when deciding assignments (see footnote 12). While judges do get assigned to other regions, they tend to return to their hometown or home region regularly, according to judicial career records (Nihon Minshu 2004). Additionally, judge convocation (*Saiban-kan Kaido*), held within high court jurisdictional areas, provides an opportunity for judges to exchange opinions and consolidate viewpoints within a region (Nishikawa 2005). Japan's judicial system may thus have grouped peers exposed to similar opinions into specific regional blocks. Another possibility is that, due to promotion incentives, judges who prefer to be in Tokyo have taken care not to indicate any extreme opinions (Ramseyer and Rasmusen 1997) whereas those wishing to be elsewhere do express rather extreme opinions.[23] Investigating these possibilities in depth is left to further

[23] "In general, judges in Osaka tend to make extreme and at-odd decisions" (interview with a lawyer, November 19, 2011, Okayama city).

research.

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Figure 1. Worker Victory Ratio since 1950 (*m* = *t* - 1950)

Note . Figures present accumulated worker victory ratio since 1950 for each prefecture. [ ] denotes High Court jurisdiction; [a] under Hokkaido (Sapporo) H.C., [b] under Miyagi (Sendai) H.C., [c] under Tokyo H.C., [d] under Aichi (Nagoya) H.C., [e] under Osaka H.C., [f] under Hiroshima H.C., [g] under Kagawa (Takamatsu) H.C., and [h] under Fukuoka H.C. Litigation records are concentrated in Tokyo (No.13) and Osaka (No.27) maninly, consinsting 48 % of the total cases.

Table 1. Worker Victory by Rank-differnce

	Population		Job creation rate	
	(1)	(2)	(3)	(4)
$(CurrentRank_t - NextRank_t)$	Worker victory rate within a current court	Judgment right before a transfer (1= worker victory)	Worker victory rate within a current court	Judgment right before a transfer (1= worker victory)
-30 or less	0.72	0.67	0.67	0.67
-20 to -11	0.33	0.35	0.47	0.44
-10 to -1	0.62	0.64	0.57	0.46
0 to 9	0.55	0.55	0.57	0.59
10 to 19	0.48	0.47	0.44	0.46
20 to 29	0.64	0.60	0.57	0.55
30 or more	0.46	0.36	0.45	0.60
Total	0.53	0.52	0.54	0.53
Regression coefficient	-1.956	-2.620	-0.794	0.658
	(3.034)	(2.686)	(2.877)	(2.552)
	Job destruction rate		Unionization index	
	(5)	(6)	(7)	(8)
$(CurrentRank_t - NextRank_t)$	Worker victory rate within a current court	Judgment right before a transfer (1= worker victory)	Worker victory rate within a current court	Judgment right before a transfer (1= worker victory)
-30 or less	0.38	0.29	0.50	0.45
-20 to -11	0.56	0.55	0.69	0.69
-10 to -1	0.54	0.51	0.49	0.48
0 to 9	0.48	0.45	0.54	0.59
10 to 19	0.65	0.67	0.48	0.44
20 to 29	0.45	0.50	0.47	0.40
30 or more	0.67	0.67	0.56	0.47
Total	0.52	0.50	0.53	0.52
Regression coefficient	1.076	-1.781	-2.250	-3.149
	(2.151)	(2.873)	(3.562)	(3.153)

Note: Rank difference is a difference in prfecture ranks between a judge's current court and his or her next destination. All variables are ranked in descending orders. Regression coefficent stands for the estimate from a simple regression of the rank difference on worker victory rate within a current court or worker vicory indicator in a litigation that a judge handled right before a transfer. Standard errors are given in parentheses. Number of total transfer is 160, and sample transfer period is 1985-2000 in all columns. The dataset for unionization index is constructed from *Surveys on Labour Unions* and *System of Prefecture Account* .

Table 2. Summary Statistics

	Sample period	No. of observations	Standard deviation	Min.	Mean	Max.
100 × log (Job creation/population)	1985-2000	752	24.0	-277.5	-193.0	-111.2
100 × log (Job destration/population)	1985-2000	752	20.7	-254.2	-193.0	-92.6
Ratio of worker victory in the last year	1985-2000	752	0.242	0	0.418	1
Ratio of worker victory in the last 3 years	1985-2000	752	0.337	0	0.356	1
Ratio of worker victory in the last 5 years	1985-2000	752	0.322	0	0.310	1
Ratio of worker victory in the last 10 years	1985-2000	752	0.304	0	0.362	1
Ratio of worker victory for all cases since 1950	1985-2000	752	0.162	0	0.516	1

Table 3. Baseline Results

	Worker victory ratio in the last *m* years									
	m = 1		*m* = 3		*m* = 5		*m* = 10		*m* = *t* - 1950	
Dependent variable (Y)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
100 × ln (Job creation rate)	-10.08*	-3.003	-7.511*	-2.059	-10.41**	-0.426	-20.54***	-9.631*	-25.18**	-59.53***
	(5.794)	(4.057)	(3.830)	(3.187)	(5.152)	(5.064)	(6.109)	(5.545)	(9.583)	(19.53)
Year effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture effect	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R-sq.	0.069	0.086	0.069	0.086	0.072	0.086	0.090	0.090	0.089	0.105
Dependent variable (Y)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
100 × ln (Job destration rate)	-8.033	-2.667	-6.614**	-3.586	-9.811**	-2.366	-19.54***	-7.999*	-27.03***	-35.95***
	(4.979)	(3.948)	(3.271)	(3.010)	(4.224)	(3.747)	(5.270)	(4.142)	(9.162)	(12.19)
Year effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture effect	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R-sq.	0.030	0.043	0.030	0.044	0.034	0.043	0.057	0.047	0.063	0.053

Note: Standard errors, adjusted for clustering at the prefecture level, are given in parentheses. The data are for the fourty-seven prefectures for the period 1985-2000. Number of observations is 752 in all specifications. *** p<0.01, ** p<0.05, * p<0.1

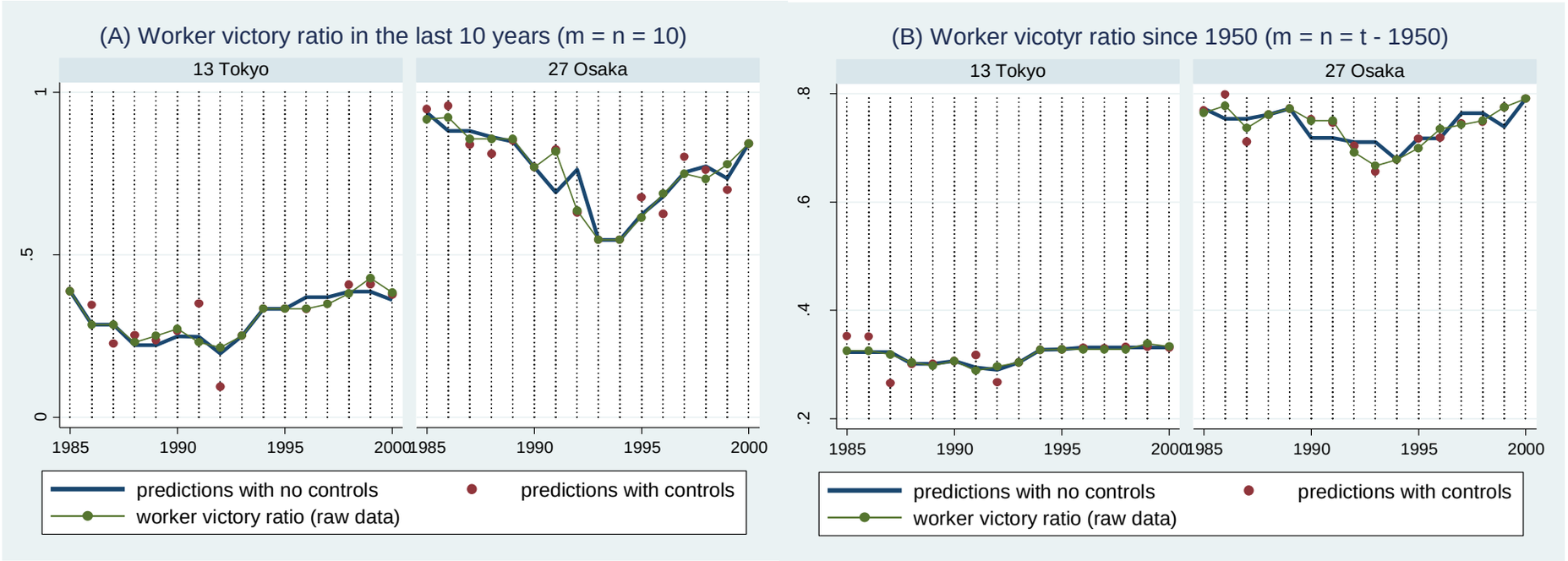


Figure 2. First-stage Predictions

Note . Blue lines indicate first-stage linear predictions without prefecture and year dummies (thus, including only judge effects and a constant). Red dots indicate first-stage linear predictions with prefecture and year dummies along with judge effects. Connected green dots are the actual worker victory ratios. In the main IV analysis, predicted values in red dots are used in the second stage estimation. The data are for the fourty-seven prefectures for the period 1985-2000. Number of observations is 752 in all specifications.

Table 4. IV Estimation Results

	OLS	2SLS	First-stage statistics: Partial R-sq.	First-stage statistics: Cragg-Donald Wald F-stat.	First-stage statistics: Critical value (LIML)	LIML
A. Worker victory ratio since 1950 (*m* = *n* = *t* - 1950)						
100 × ln (Job creation rate)	-59.53***	-85.90***	0.669	29.74	1.97	-86.18***
	(19.53)	(22.79)				(23.57)
100 × ln (Job destration rate)	-35.95***	-66.93***				-67.41**
	(12.19)	(25.81)				(26.64)
B. Worker victory ratio in the last 10 years (*m* = *n* = 10)						
100 × ln (Job creation rate)	-9.631*	-11.74*	0.915	75.32	1.62	-12.03*
	(5.545)	(6.276)				(6.344)
100 × ln (Job destration rate)	-7.999*	-8.982**				-9.088**
	(4.142)	(4.363)				(4.402)
C. Worker victory ratio in the last 5 years (*m* = *n* = 5)						
100 × ln (Job creation rate)	-0.426	-3.884	0.752	28.72	1.75	-4.251
	(5.064)	(5.489)				(5.633)
100 × ln (Job destration rate)	-2.366	-5.241				-5.468
	(3.747)	(4.107)				(4.184)
D. Worker victory ratio in the last 3 years (*m* = *n* = 3)						
100 × ln (Job creation rate)	-2.059	-5.707	0.664	21.91	1.82	-6.245
	(3.187)	(4.621)				(4.863)
100 × ln (Job destration rate)	-3.586	-4.390				-4.456
	(3.010)	(3.833)				(3.941)
E. Worker victory ratio in the last year (*m* = *n* = 1)						
100 × ln (Job creation rate)	-3.003	-0.390	0.685	30.43	1.94	-0.160
	(4.057)	(4.980)				(5.120)
100 × ln (Job destration rate)	-2.667	-3.376				-3.416
	(3.948)	(4.184)				(4.259)

Note: Eech cell represents a coefficient estimate for the endogenous variable from separate estimations. All estimation controls for prefecture dummies and year dummies.Standard error, adjusted for clustering at the prefecture level, is given in parentheses.Critical value (LIML) reports a critical value for a 5% test of the null hypothesis that the instruments are weak, in the sense that the weak-instrument asymptotic relative bias of the Fuller estimator exceeds 5%. The data are for the fourty-seven prefectures for the period 1985-2000. Number of observations is 752 in all specifications. Columns for OLS replicate the estimates from even-numbered columns in Table 3. LIML sets Fuller parameter = 1. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Robustness Test (n = 1)

	OLS	2SLS	First-stage statistics: Partial R-sq.	First-stage statistics: Cragg-Donald Wald F-stat.	First-stage statistics: Critical value (LIML)	LIML
A. Worker victory ratio since 1950 (m = t - 1950, n = 1)						
100 × ln (Job creation rate)	-59.53***	-91.59***	0.105	1.65	1.94	-121.2**
	(19.53)	(34.44)				(60.34)
100 × ln (Job destration rate)	-35.95***	-65.38***				-79.00**
	(12.19)	(22.11)				(32.63)
B. Worker victory ratio in the last 10 years (m = 10, n = 1)						
100 × ln (Job creation rate)	-9.631*	-26.68**	0.133	2.16	1.94	-36.53**
	(5.545)	(11.28)				(17.26)
100 × ln (Job destration rate)	-7.999*	-22.37***				-26.75***
	(4.142)	(7.347)				(9.752)
C. Worker victory ratio in the last 5 years (m = 5, n = 1)						
100 × ln (Job creation rate)	-0.426	-3.568	0.176	2.99	1.94	-5.003
	(5.064)	(9.784)				(13.35)
100 × ln (Job destration rate)	-2.366	-6.888				-8.060
	(3.747)	(6.336)				(7.650)
D. Worker victory ratio in the last 3 years (m = 3, n = 1)						
100 × ln (Job creation rate)	-2.059	-5.859	0.229	4.16	1.94	-7.049
	(3.187)	(6.050)				(7.377)
100 × ln (Job destration rate)	-3.586	-6.655				-7.224
	(3.010)	(4.448)				(5.075)
E. Worker victory ratio in the last year (m = n = 1)						
100 × ln (Job creation rate)	-3.003	-0.390	0.685	30.43	1.94	-0.160
	(4.057)	(4.980)				(5.120)
100 × ln (Job destration rate)	-2.667	-3.376				-3.416
	(3.948)	(4.184)				(4.259)

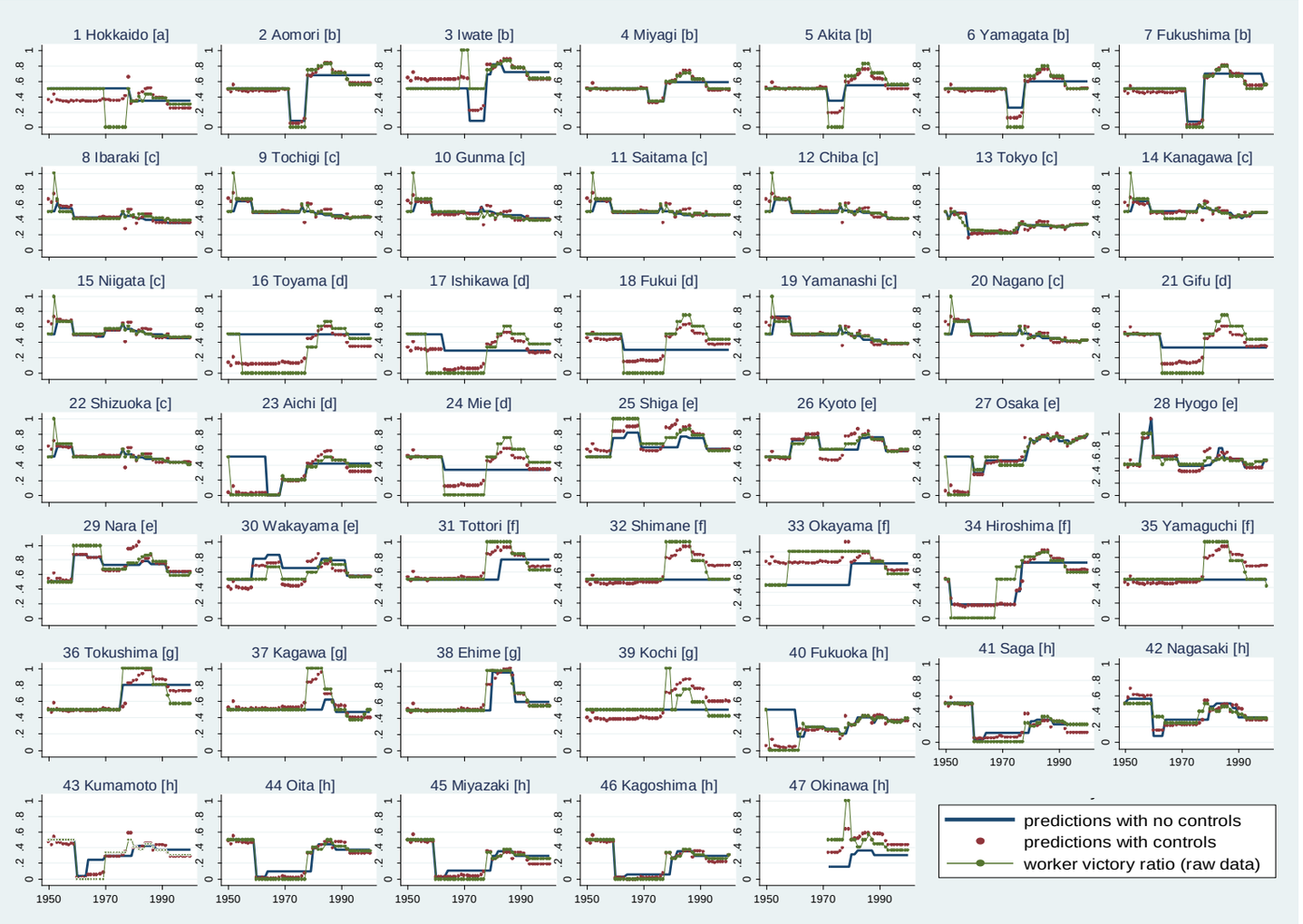
Note: Eech cell represents a coefficient estimate for the endogenous variable from separate estimations. All estimation controls for prefecture dummies and year dummies.Standard error, adjusted for clustering at the prefecture level, is given in parentheses. Critical value (LIML) reports a critical value for a 5% test of the null hypothesis that the instruments are weak, in the sense that the weak-instrument asymptotic relative bias of the Fuller estimator exceeds 5%. The data are for the fourty-seven prefectures for the period 1985-2000. Number of observations is 752 in all specifications. Columns for OLS replicate the estimates from even-numbered columns in Table 3. LIML sets Fuller parameter = 1. *** p<0.01, ** p<0.05, * p<0.1

Table 6. The Impact by Gender and Industry: Fuller's LIML Estimation

	A. Worker victory ratio since 1950 (m = t - 1950)					
	Male			Female		
	(1)	(2)	(3)	(4)	(5)	(6)
	Manufacturing	Wholesale and Retail Trade	Service	Manufacturing	Wholesale and Retail Trade	Service
100 × ln (Job creation rate)	-91.01***	-61.06	-38.85	-74.70**	-78.00	-70.96***
	(19.58)	(55.68)	(29.80)	(30.27)	(48.71)	(23.51)
100 × ln (Job destration rate)	-64.90***	-31.15	-27.17	-90.29***	-77.28*	-53.68**
	(17.11)	(54.77)	(28.33)	(18.27)	(44.69)	(22.30)

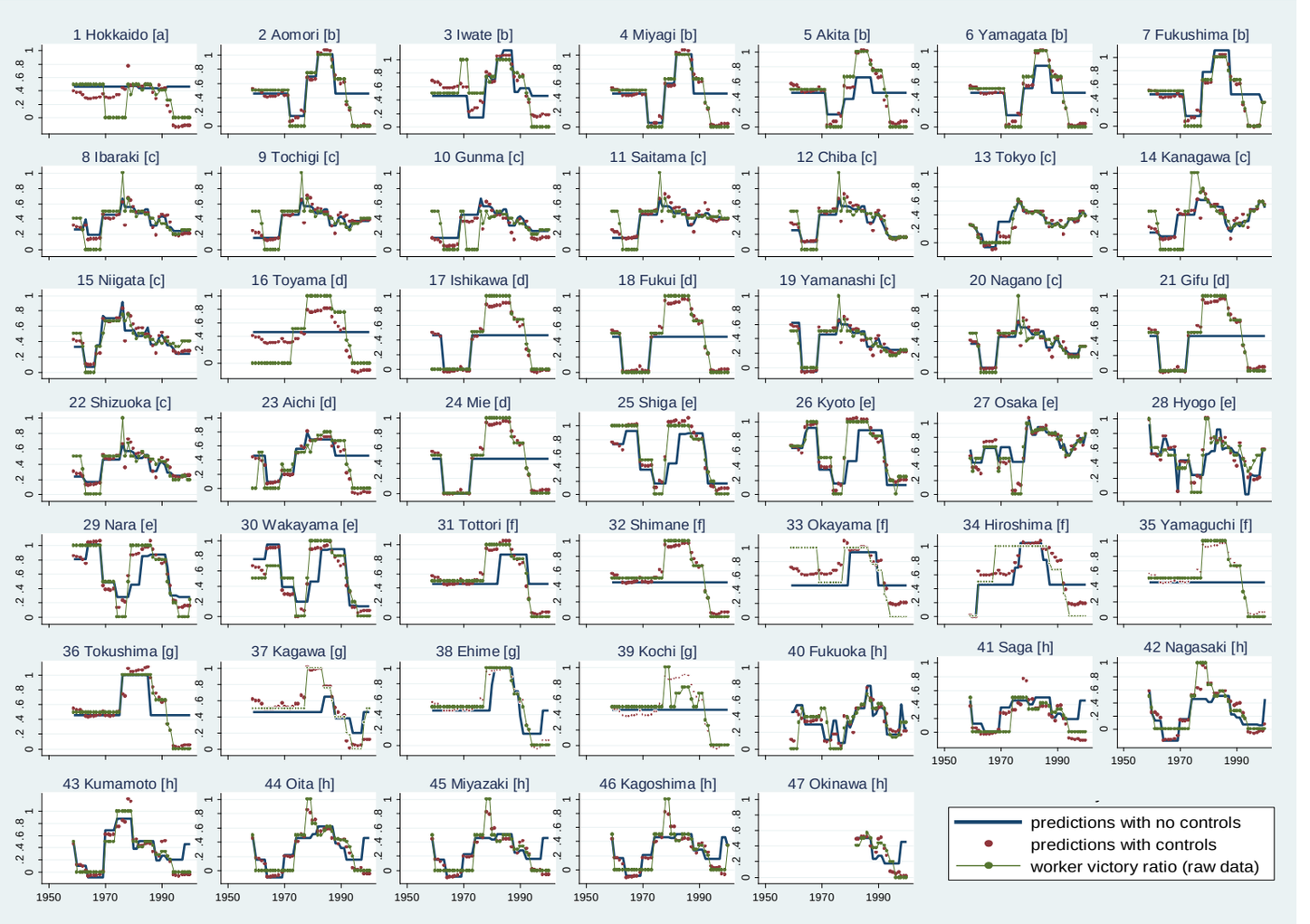
	B. Worker victory ratio in the last 10 years (m = 10)					
	Male			Female		
	(7)	(8)	(9)	(10)	(11)	(12)
	Manufacturing	Wholesale and Retail Trade	Service	Manufacturing	Wholesale and Retail Trade	Service
100 × ln (Job creation rate)	-17.95**	0.962	-16.43	-21.20*	-3.147	-24.78***
	(7.453)	(20.44)	(11.04)	(11.23)	(17.87)	(8.703)
100 × ln (Job destration rate)	-11.90*	2.017	-2.018	-21.95***	-21.20	-15.89*
	(6.438)	(20.07)	(10.48)	(6.911)	(16.36)	(8.267)

Note: All models are estimated by Fuller's LIML. Fuller's parameter is set equal to 1. Standard errors are given in parentheses. The data are for the fourty-seven prefectures for the period 1985-2000. Some observations are dropped due to a lack of data in Surveys on Employment Trends arising from small sample size in a category. All estimation controls for prefecture dummies and year dummies.



Appendix Figure 1. First-stage predictions with and without controls ($m = n = t$ - 1950)

Note . Blue lines indicate first-stage linear predictions without prefecture and year dummies (thus, including only judge effects and a constant). Red dots indicate first-stage linear predictions with prefecture and year dummies along with judge effects. Connected green dots are the actual worker victory ratios. In the main IV analysis, predicted values in red dots are used in the second stage estimation. The data are for the fourty-seven prefectures for the period 1950-2000. Number of observations is 2375. [ ] denotes High Court jurisdiction; [a] under Hokkaido (Sapporo) H.C., [b] under Miyagi (Sendai) H.C., [c] under Tokyo H.C., [d] under Aichi (Nagoya) H.C., [e] under Osaka H.C., [f] under Hiroshima H.C., [g] under Kagawa (Takamatsu) H.C., and [h] under Fukuoka H.C. Litigation records are concentrated in Tokyo (No.13) and Osaka (No.27) maninly, consinsting 48 % of the total cases.



Appendix Figure 2. First-stage predictions with and without controls ($m = n = 10$)

Note . Blue lines indicate first-stage linear predictions without prefecture and year dummies (thus, including only judge effects and a constant). Red dots indicate first-stage linear predictions with prefecture and year dummies along with judge effects. Connected green dots are the actual worker victory ratios. In the main IV analysis, predicted values in red dots are used in the second stage estimation. The data are for the fourty-seven prefectures for the period 1959-2000. Number of observations is 1951. [ ] denotes High Court jurisdiction; [a] under Hokkaido (Sapporo) H.C., [b] under Miyagi (Sendai) H.C., [c] under Tokyo H.C., [d] under Aichi (Nagoya) H.C., [e] under Osaka H.C., [f] under Hiroshima H.C., [g] under Kagawa (Takamatsu) H.C., and [h] under Fukuoka H.C. Litigation records are concentrated in Tokyo (No.13) and Osaka (No.27) maninly, consinsting 48 % of the total cases.