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### The Selection of Litigation against Government Agencies: Evidence from China

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# **The Selection of Litigation against Government Agencies: Evidence from China**

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**Abstract:** We test the relevance of the selection theory of litigation in a contemporary, civil law setting, using Chinese judicial data that span 25 years regarding lawsuits against government agencies. Civil law systems may be characterized by lower costs of litigation and lower rates of settlement than the U.S. legal system, and therefore the presence of selection effects cannot be assumed. We show that selection effects are indeed manifest in Chinese administrative litigation, and suggest that this may be explained by hidden or intangible litigation costs. Our test for selection effects builds on the approach of previous U.S. studies and potentially allows the identification of selection effects to help improve inferences from decided cases. Finally, we examine patterns of settlement and plaintiff wins in pre-litigation administrative appeals in China, and do not find sufficient evidence for selection effects in this process. This could potentially be explained if most appellees pursuing administrative appeals do not intend to litigate.

**Key words:** selection theory, Priest and Klein, administrative litigation, civil law v. common law, Chinese law, authoritarian legal systems

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# The Selection of Litigation against Government Agencies: Evidence from China

## Introduction

The selection theory of litigation, pioneered by Priest and Klein (1984), is considered one of the most influential theories in the economic analysis of civil procedure and in fact in the economic analysis of law in general.<sup>1</sup> Yet the impact of the theory has been limited so far in at least two significant respects. First, the main lesson of the theory is generally taken as urging caution against making inferences about legal standards from judicial decisions. The theory offers potentially powerful critiques of bad empiricism, but is less known for offering positive suggestions about how to fashion better judgments about legal systems on the basis of decided cases. Second, there in fact have been relatively few empirical tests of some of the core claims of the selection theory itself,<sup>2</sup> and, at least judging from the literature in English, few studies have been conducted outside the theory's native home—the American legal system.

This article presents a study that offers evidence for the relevance of the selection theory in the context of China, using judicial data regarding lawsuits against government agencies that span 25 years. To our knowledge, this is the first empirical test of selection effects in litigation in a contemporary, non-U.S., and especially non-common-law context. We find robust evidence for selection effects in Chinese administrative litigation—the evidence seems stronger, indeed, than what has been produced for litigation in U.S. federal courts. This is surprising, because the financial cost for such litigation in China is quite low, thanks to a package of government subsidies for judicial tribunals that adjudicate lawsuits against government agencies. *A priori*, one might not expect selection effects. Our finding thus underscores the importance of empirically testing the relevance of the selection theory.<sup>3</sup>

Our study of the applicability of the selection theory to China begins with the premise that, even if one subscribes to the selection theory, selection effects may be present—and therefore the selection theory is relevant—only in some contexts but not others. Consequently, using the selection theory to explain observed judicial outcomes in a particular context will be more persuasive if the relevance of the theory can be independently demonstrated. By contrast, much of the existing literature—in both the U.S. and non-U.S. contexts—tends simply to assume the theory's applicability or non-applicability. How, though, does one establish the relevance of the selection theory for any given context? Some tests possess greater utility and feasibility than others. For example, selection effects can be demonstrated by comparing adjudicated cases with settled cases. But if one already has access to the features of the broader population of disputes, one presumably would have no need to make inferences only on the basis of adjudicated cases. This way of testing the selection theory would thus not help improve inferences from decided cases when only the latter are available. Instead, we follow the approach in Waldfogel 1995 by using correlations between the rate of adjudication and the plaintiff win rate as evidence for selection effects. Such a test is feasible even when we have no access to the underlying population of cases and even when many other features of disputes are unobservable. After establishing

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<sup>1</sup> See Klerman and Lee 2014 for a recent discussion of the influence of Priest and Klein's theory.

<sup>2</sup> We discuss the limitations of the existing literature in Section 1 below.

<sup>3</sup> We interpret the presence of selection effects in terms of the possibility of substantial non-financial costs to administrative litigation in an authoritarian regime.

the presence of selection effects, we discuss how the selection theory can help improve inferences from decided cases in Chinese administrative litigation.

Using Chinese data, we also try to extend the scope of empirical tests of the selection theory by examining the pattern of pre-litigation settlements and their impact on judicial outcomes. Under the selection theory, settlements reached prior to the filing of lawsuits should affect judicial outcomes just as settlements reached after filing suits. However, pre-litigation settlements are generally difficult to observe. Studying administrative litigation potentially allows one to circumvent this difficulty. For disputes with government agencies, the exhaustion of administrative remedies is a frequently-imposed requirement in many countries; and administrative procedures are likely to require record-keeping for settlements reached during administrative appeals. We show that overall, litigated cases that were preceded by administrative appeals in China are much less likely to settle than cases that were brought directly to courts. However, settlement patterns seem to have little impact on plaintiff win rates. Overall, therefore, the evidence for the selection effect through administrative appeals is weak.

While our main focus is on empirically testing the applicability of the selection theory in a civil law setting and in administrative appeals procedures, the article also contributes to the study of legal processes in China and in authoritarian regimes in general. Not only may the selection theory directly provide novel insights into Chinese judicial outcomes, it also generates a critique of a popular scholarly discourse regarding administrative litigation in authoritarian regimes. This discourse over-emphasizes administrative litigation as a way for the judiciary to constrain the executive branch, and ignores the indispensable role of the plaintiff in activating this form of constraint. Because the selection theory is developed from assumptions about litigants' rational choices, it offers a healthy antidote to the depiction of administrative litigation as being mainly about the power balance between the judicial and executive branches.

The article proceeds as follows. Section 1 sets out our view of what empirically testing the selection theory requires: in particular, we distinguish, in terms of both methodologies and practical importance, between two tasks in such a test, i.e. identifying the presence of selection effects v. identifying the mechanisms that explain selection effects. Section 2 sets out the aspects of Chinese administrative litigation that are of relevance for testing selection effects. Section 3 describes the Chinese judicial data we use. Section 4 presents the results of two sets of empirical analysis: evidence for selection effects produced through both panel and times series data; and cross-agency comparisons of litigation outcomes interpreted in light of the selection theory. Section 5 then introduces additional data regarding administrative appeals in China, and discusses both the mixed evidence for selection and how such evidence might be interpreted. A Conclusion follows.

## 1. Empirically Testing the Selection Theory

Hylton and Lin (2012, at 487) defines the “trial selection theory” as “models that attempt to explain or predict the characteristics that distinguish cases that are litigated to judgment from those that settle, and the implications of those characteristics for the development of legal doctrine and for important trial outcome parameters, such as the plaintiff win rate.” Two aspects of this helpful definition may be noted. First, it postulates that adjudicated cases may be systematically different from cases that are settled without judgment. Consequently, decided cases may not be representative of the underlying population of disputes, and inferences from decided cases to the larger population of disputes may be unreliable. Second, the selection theory aims to explain systematic differences between

settled and adjudicated cases, which explanations can improve the interpretation of trial outcomes and legal doctrines.

Corresponding to these two aspects of Hylton and Lin's definition, one can divide the task of empirically testing the selection theory into two sub-tasks: (1) demonstrating the presence of selection effects; and (2) identifying the precise causal mechanisms responsible for any selection effect observed. Both tasks face certain basic challenges that arguably have not been fully recognized in the prior literature.<sup>4</sup>

### 1.1 Are selection effects present?

A threshold question for empirically testing the selection theory is whether, for any given judicial venue or legal system, adjudicated cases and settled cases are in fact systematically different. Where settlement decisions do not create systematic biases in the observed characteristics of decided cases, one might say that there is no "selection effect". In such circumstances, it would obviously be mistaken to insist on using the selection theory to guide the interpretation of trial outcomes and judicial doctrine.<sup>5</sup> The selection theory would simply be irrelevant. What is more, whether selection effects are present for a given judicial venue or legal system is a genuine question; there are both theoretical and empirical reasons why its answer should not simply be assumed.

Theoretically, even within the terms of the orthodox selection theory framework, some factors may diminish the theory's relevance. One example is low litigation costs. Litigation costs clearly affect the strength of selection effects: the impact of legal uncertainty, information asymmetry, asymmetry in stakes, etc. on litigants' settlement decisions would all diminish if the cost of litigation is truly low, e.g. virtually indistinguishable from settlement costs. Therefore, unless litigation is always costly, in any legal system, the selection theory will not be universally applicable in predicting litigation outcomes. This point has been implicitly acknowledged even by scholars in the U.S. context. For instance, in attempting to evaluate empirically between the divergent expectation and the asymmetrical information models of the selection theory, Waldfogel 1998 excludes all prisoner litigation cases because of their low litigation costs. Presumably, this is because Waldfogel assumes that low litigation cost would nullify any selection effect.<sup>6</sup> More pertinently for our study, civil law judicial systems, even if they are not always cheaper for litigants than their common law counterparts, may precisely offer low-cost dispute resolution mechanisms with greater frequency. This may be the result of case law's lack of precedential value, non-adversarial procedures and simpler evidentiary rules, and a number of other factors. Consequently, settlement rates in civil law systems may on average be much lower than in common law systems. Although we are aware of no general study of comparative settlement rates, it has been reported that the overall settlement rate in France for civil litigation in 1995 was only 22.9% (Doriat-Duban 2001, at

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<sup>4</sup> Consistently with much of the empirical literature on the selection theory since the early 1990s, we do not view the primary empirical task as testing Priest and Klein's famous "50% hypothesis", since that hypothesis (i) describes only the limiting case where the adjudication rate approaches zero, and (ii) assumes symmetrical stakes and information, whereas the implications of asymmetrical stakes and information (which are common phenomena in litigation) are of high interest to selection theorists.

<sup>5</sup> See De Figueiredo 2005 (using phases of federal telecommunications litigation in which selection effects are suspended as an identification strategy for testing the attitudinal model of judicial behavior).

<sup>6</sup> It is also significant that most sophisticated empirical tests of the selection theory in the U.S. consider litigation patterns in federal courts, where the costs of litigation are presumably higher than litigation in most (especially low-level) state courts.

187). In the Chinese judicial data discussed in Section 3, settlement rates also reach comparable low levels at times.

Ultimately, however, whether selection effects are present in any particular context is an empirical question. For example, lower litigation costs in civil law systems (which tends to reduce settlement) may be matched by lower legal uncertainty (which tends to increase settlement), allowing selection effects to manifest even in such systems. We later provide evidence for selection effects in the context of Chinese administrative litigation, which is characterized by very low financial costs. We conjecture that this may best be explained by non-financial litigation costs.<sup>7</sup> Conversely, Waldfogel's study of the U.S. federal court in the Southern District of New York (SDNY) fails to find evidence for selection effects in contract and intellectual property cases (Waldfogel 1995). Such mixed evidence for selection effects strongly suggests that before invoking the selection theory in any context to explain any observed phenomenon (e.g. a particular pattern in plaintiff win rates), it is useful independently to demonstrate the presence of selection effects.<sup>8</sup> Yet to our knowledge, no previous scholar has directly tested for the presence of selection effects in a civil law setting. Although many studies carried out in the context of civil law systems invoke Priest and Klein's explanatory framework, the presence or absence of selection effects is always simply assumed, instead of empirically established.<sup>9</sup>

Yet attempts to demonstrate the presence of selection effects face a basic predicament. The most direct way of showing that adjudicated cases and settled cases are systematically different is to compare them. But it is rare for scholars to have access to the features of settled cases. One study based on such access is Klerman 2012, which compares non-settled, adjudicated criminal cases in 13<sup>th</sup>-century England with a set of settled cases that, possibly to the surprise of the litigants, nonetheless ended up being adjudicated by judges. Klerman shows that settled and non-settled cases in his sample of adjudicated cases differ in ways arguably predicted by the selection theory. Even if one accepts such evidence,<sup>10</sup> however, this direct method of demonstrating selection effects is of limited utility as a general matter: when one can observe features of the underlying population of disputes (including settled cases), there presumably is no longer any need to make inferences only from adjudicated cases. Instead, the selection theory is of the most value when only features of litigated cases can be observed. The challenge is demonstrating the presence of selection effects within this basic empirical constraint.

A resolution of this predicament is offered in Waldfogel 1995's important study of the SDNY court. Waldfogel points out that any statistically significant correlation between the adjudication rate (i.e. the proportion of all lawsuits that are brought to courts that are adjudicated) and the plaintiff win

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<sup>7</sup> But such non-financial litigation costs are not easily observable, and the identification of selection effects is actually one way of verifying their existence.

<sup>8</sup> Siegelman and Waldfogel 1999 assert (at 103) that "[while] the exact nature of the selection mechanism has not been resolved, few now question that selection of some sort does occur. The real debate, then, is about what kind of selection is taking place and why." We believe that such assertion is unwarranted generally and especially in civil law systems, and is belied by the mixed evidence for selection effects even in the U.S..

<sup>9</sup> See, e.g. Bøhren & Krosvik 2013 (using Priest & Klein to speculate about why minorities might litigate less regarding stock matters in Norway); Peyer 2012 (citing Priest and Klien to explain when parties are likely to settle in German private anti-trust lawsuits); Huang 2008 (speculating that selection effects explain higher win rate for represented defendants in Taiwan, not representation per se); Ramseyer and Rasmusen 1999 (speculating that the selection theory may explain observed high government win rates in Japanese tax litigation).

<sup>10</sup> Klerman's sample does not include a substantial number of cases that are settled and not adjudicated; he argues that such cases should not differ dramatically from cases that are settled but nonetheless adjudicated.

rate in adjudicated cases would constitute evidence for selection effects. The logic is that if the plaintiff win rate depends on the percentage of cases adjudicated, then it would clearly not be the case that adjudicated cases are random, representative samples of the underlying population of lawsuits. A significant correlation between the adjudication rate and the plaintiff win rate would thus constitute sufficient evidence for selection effects. Importantly, both rates are often observable from court statistics, without requiring one to access the characteristics of settled cases.

Despite the compelling argument for this approach to testing selection effects, it is, perhaps surprisingly, rarely adopted. As Siegelman and Waldfogel 1999 point out, while a number of empirical studies purport to support the selection theory, what they really do is to use variables generally assumed to be relevant to settlement decisions to predict observed plaintiff win rates.<sup>11</sup> Most such studies neither present evidence about settled cases nor examine the relation between trial rate and plaintiff win rate. In other words, the basic claim of the selection theory, that adjudicated and settled cases are different, has remarkably gone largely untested.

## 1.2 What drives the selection effect?

While demonstrations of even the bare existence of selection effect are less common than is generally acknowledged, such demonstration is of course not sufficient to support the selection theory. Instead, the theory claims that a small set of factors —decision standards, the degree of legal uncertainty, costs of litigation, expected damages (the last three of which may be asymmetrically distributed), and possibly the bargaining process—will determine both settlement decisions and litigation outcomes through the rational calculus of dollar-maximizing actors. The theory can thus be said to be truly empirically verified only if the influence of each of these factors, through the mechanism of settlement, can be demonstrated,<sup>12</sup> and alternative explanations of the presence of selection effects ruled out. However, any attempt to empirically identify the impact of these factors on litigation outcomes faces serious challenges.

A first challenge is that, even if one had access to settled cases, many of the parameters figuring in the selection theory are unobservable. Decision standards (which reflect both the law and the biases of individual judges), the degree of legal uncertainty (and information asymmetry), and asymmetrical stakes are all difficult to measure. If one uses the selection theory to explain litigation outcomes merely by speculating about these parameters, the theory risks becoming “unfalsifiable” (Siegelman and Waldfogel 1999). To address this concern, Siegelman and Waldfogel pursue a strategy where they first collect “independent information about the underlying parameters for different types of cases,” and then examine whether the values of decision standards, uncertainty, and stake asymmetry “estimated from the trial and win-rate data are consistent with the independent measures of these parameters”.<sup>13</sup>

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<sup>11</sup> They cite Kessler et al 1996 and Eisenberg & Farber 1997. Siegelman and Donohue 1995 examine the determinants of settlement and win rates simultaneously, without emphasizing the correlation between the two. Klerman 2012 also notes the dearth of studies directly demonstrating selection effects.

<sup>12</sup> Similarly, simply knowing that decided cases are unrepresentative does not itself yield useful interpretations. It is only by identifying factors that create systematic biases that the selection theory can enhance the interpretation of litigation outcomes.

<sup>13</sup> They use (i) the ratio of non-individual plaintiffs to non-individual defendants and the ratio of repeat players for a given type of case as proxies of stakes asymmetry; (ii) jury v. judge trials and the publication rate of cases as proxies for legal uncertainty; and (iii) the proportion of pro se plaintiffs and the rate of early termination of a

However, whether the independent measures they construct are adequate proxies for the unobservable parameters used in the selection theory is inevitably controversial. There appears to have been no other attempt since Siegelman and Waldfogel 1999 to independently measure the parameters of the selection theory.<sup>14</sup>

Even if one cannot precisely measure the parameters employed in the selection theory, it is possible to test the theory if one could randomly assign cases to a treatment that plausibly produces a change in some parameter (or adopt other techniques that approximate random assignments). We are not aware of any study using such research design. In fact, a second challenge for testing the selection theory is linking variations in the parameters identified by the theory with variations in adjudication and plaintiff win rates. This difficulty is illustrated by Waldfogel 1995. Waldfogel's strategy is to find correlations between the trial rate and plaintiff win rate—which he labels T and P, respectively—based on variations across (a) 34 case types, and (b) 21 judges. For variation type (a), Waldfogel finds a statistically significant, non-linear correlation between T and P. However, he acknowledges (at 245) “that we do not know the source of variation of P and T across case types.” For variation type (b), Waldfogel finds a statistically significant linear correlation between T and P for all cases in the aggregate. But he is again agnostic about the sources of variation across judges.

Waldfogel argues that for purposes of testing “the selection hypothesis”, it is enough that there is sufficient variance in T and P, and there is no need to identify the causes of such variance. It is likely that he here trades on two possible meanings of the term the “selection hypothesis”, which may refer either to the existence of selection effects, or to Priest-Klein-style rational choice theories explaining selection effects. A significant correlation between T and P should be viewed as sufficient evidence only for the former but not the latter. The distinction we make above between the two subtasks in testing the selection theory is thus important for determining what constitutes sufficient empirical evidence. There is also a converse reason for maintaining the distinction. Suppose that for a given population of cases, the litigants with high stakes also tend to be the parties that are informationally disadvantaged. Then, even if the selection theory is correct about what factors and mechanisms determine litigation outcomes, the effect of asymmetrical stakes may cancel out the effect of asymmetrical information in this particular population of cases. If the other parameters considered by the selection theory also cancel out one another,<sup>15</sup> one could have a situation where the selection theory, while true, is irrelevant for interpreting litigation outcomes, because of the absence of selection effects.

We believe that because of the challenges described above, the empirical literature following Priest and Klein has not come close to providing robust tests of the causal mechanisms postulated by the selection theory. However, this may be a concern mainly for theorists: unless alternative theories explaining litigation outcomes (especially observed selection effects) are being considered, one might be content to rely on the selection theory as the default law and economics theory of litigation. By contrast, the establishment of the presence of selection effects is more important for practical purposes. This is

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lawsuit to proxy for information asymmetry. Their results from comparing these independent measures with structural estimations are mixed and not always inconsistent with the selection theory's predictions.

<sup>14</sup> Klerman 2012, for example, offers a narrative about stakes asymmetry in the sample of cases he examines, without attempting to measure such asymmetry.

<sup>15</sup> For example, suppose in a study of T-P correlations across case types or across individual judges (as in Waldfogel 1995), both the legal standard (e.g. degree of perceived judge bias) and the level of uncertainty vary. One then might not be able to trace the kind of T-P correlation predicted by simple comparative statics.



because even for those who subscribe to the selection theory, the theory would not help interpret litigation outcomes if selection effects are absent. In other words, the *relevance* of the selection theory is more important for legal scholars than its merits relative to alternative theories.

Against this background, our study reported below attempts mainly to contribute to the literature by offering empirical evidence for the presence of selection effects. As discussed above, such a test is valuable for at least three reasons: (1) there have been (surprisingly) few direct tests for selection effects even in the U.S., and none for civil law systems; (2) the demonstration of selection effects is arguably a pre-condition for applying the selection theory to interpret litigation outcomes; and (3) we implement the approach used in Waldfogel 1995, which allows the identification of selection effects even without access to settled cases (i.e. where inferences from decided cases is most useful), and which has rarely been replicated. However, we do not purport to provide causal identification of selection mechanisms.

## 2. Background on Chinese Administrative Litigation

In civil law systems, judicial decisions have limited precedential value. Correspondingly, there tends to be less of a professional and intellectual tradition of inferring legal standards from court decisions than in common law jurisdictions. In fact, given most case law's lack of precedential value, substantial bodies of judicial decisions may not be intended to be informative as to the content of legal standards and may also never be published. The most important implication of the selection theory, that inferences from judicial decisions are unreliable, thus packs a weaker punch. Nonetheless, even in a civil law system, policymakers, scholars, and potential litigants make extensive inferences from observed litigation outcomes. The applicability of the selection theory thus is still of high interest.

Because of China's authoritarian government and weak rule of law,<sup>16</sup> administrative litigation in China—lawsuits filed by private parties against government agencies and government-affiliated entities—has long been held as an emblem of instruments for constraining government. Ever since the 1980s, Chinese and foreign scholars have resorted to the study of administrative litigation as a way of assessing the development of China's legal system.<sup>17</sup> Various authors have also proffered putative evidence of low case volumes and low plaintiff win rates as confirming the lack of independence on the part of the Chinese judiciary. Even the Chinese government itself regards these litigation statistics as relevant for evaluating court performance. One recent report published by the Supreme People's Court (SPC), for example, states:

"The losing rate for administrative agencies in administrative litigation cases ...started to rise in the recent years. This indicates that the People's Courts at all levels are continuously reinforcing the awareness of the legal effects of administrative litigation, promptly correcting clearly illegal administrative acts, and practically implementing the judicial functions of the courts granted by the Constitution and the law."<sup>18</sup>

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<sup>16</sup> See, e.g. Peerenboom 2002 and Liebman 2007.

<sup>17</sup> See, e.g., Potter 1994, Pei 1997, and Clarke et al 2008.

<sup>18</sup> Liu 2015.

To consider whether the selection theory sheds light on these inferences, one should start by noting that the Chinese regime for administrative litigation is, at a first glance, characterized by very low cost for litigants. First, court fees for filing an administrative lawsuit are intentionally set to be only nominal, and are significantly lower than civil litigation in general.<sup>19</sup> Second, almost all Chinese courts—from basic courts at the county level, to intermediate and high courts in large cities and provinces, and to the SPC—have specialized administrative tribunals. There are thus about 3,500 administrative tribunals situated within courts throughout the country, with each tribunal staffed with several judges and dedicated to adjudicating lawsuits involving government parties. Yet up to 2013, the highest number of first-instance administrative lawsuits brought in one year is 136,353 (see Figure 1). This implies an average case load of less than 40 (first-instance) cases per year per tribunal. In other words, most administrative tribunals are over-staffed. Both the financial cost and the risk of court delays are thus generally low for potential litigants. Finally, although this reflects more the weak demand on the part of potential litigants rather than government subsidy, lawyer fees are also believed not to be high for administrative litigation.

Trial selection theory, as developed in the U.S. context, has tended to conceive of litigation costs as being mostly financial. If this conception is adopted, one would expect to find at most limited selection effects in Chinese administrative litigation. However, hidden or intangible costs, including the costs of deploying social and political capital in litigation, are also litigation costs.<sup>20</sup> For example, if courts are corrupt or non-transparent, or if there are other reasons for litigants to believe in the utility of trying to influence courts outside formal procedures, litigants may consider incurring expenses to exercise such influence. Both plaintiffs and defendants may also have special reasons to be particularly averse to administrative litigation, beyond their normal degrees of aversion to financial gain or losses. For example, one frequently-mentioned reason for plaintiff litigation aversion is the fear of retaliation by the defendant agency. One frequently-mentioned reason for *defendant* litigation aversion is negative bureaucratic evaluations that attach to being sued, sometimes even if the agency prevails in litigation. Evidence of the existence of selection effects, therefore, could vindicate the hypothesis that such hidden or intangible costs are significant.

Finally, one feature of the Chinese administrative state will be relevant to the application of the selection theory. According to the theory, higher-stake parties are more likely to settle, all other things equal. Consequently, they will appear to win more often in adjudicated cases. In the U.S. context, it is common to conceive of government defendants as having relatively high stakes, because a court loss may create difficulties for the future policy implementation. However, in the context of a civil law system, where most first-instance decisions have no precedential value, the relative stake of the government defendant may be lower, and administrative litigation may become a type of lawsuit where an assumption of stake symmetry is more justified.

There is another reason for assuming stake symmetry in interpreting Chinese administrative litigation patterns. The Chinese administrative state is highly decentralized. Very few lawsuits are filed against the national, provincial and even large-municipal governments, for the simple reason that these government entities rarely face citizens. Most lawsuits are against agencies at the county level or below. As mentioned above, the average administrative tribunal within a county-level basic court in China reviews at most 40 lawsuits a year, *against all government agencies*. Thus the average county- or lower-

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<sup>19</sup> The court fee for filing an administrative case is RMB 50 (slightly more than US\$ 7).

<sup>20</sup> Klerman 2012 considers psychological, non-pecuniary benefits in discussing stake asymmetry in criminal cases.

level government department—be it the police, tax collector, or environmental agency—is decidedly not a “repeat player” as far as going to court is involved. This observation applies to all agencies for which the SPC provides separate litigation data. Therefore it is plausible to rule out stake asymmetry as having a major impact on most litigation outcomes.

### 3. Source of data, construction of variables, and descriptive statistics

#### 3.1 Source and construction

Our data covers first-instance administrative litigation in all Chinese courts for the 25 years from 1989 to 2013. The SPC has published such data in the China Law Yearbook since 1990.<sup>21</sup> In the first year for such data (1989), the SPC reported the total numbers of first-instance administrative lawsuits filed during the year, of cases concluded during the same year, and of cases falling in five categories of dispositions. These categories were proceedings where the courts (1) upheld, (2) vacated, or (3) ordered changes to an agency action; (4) where the plaintiff withdrew the lawsuit; or (5) that ended in other manners. The SPC also reported each of the foregoing types of information for (a) public security bureaus (i.e. the police) and land management bureaus, and (b) a residual category of all “other” agencies. Over the years, the SPC began to offer separate reporting for more types of agencies. Since 2002, at least ten types of agency were subject to separate reporting each year,<sup>22</sup> and altogether, 15 agencies have been subject to separate reporting at some point. Since there are between 60 and 100 functional types of government agencies in China at any time, lawsuits against most agencies are included under the residual “Other” category. In some recent years (namely, 2005, 2010, 2012 and 2013), the SPC’s reporting regarding the categories of dispositions also became more detailed. For 2013, for instance, 15 categories of dispositions were included.<sup>23</sup>

While the information included in these statistics is limited, for purposes of testing the selection theory we only need to construct two variables from the data, i.e. the adjudication rate and the plaintiff win rate.<sup>24</sup> For the years for which we have detailed classification of dispositions, both can be determined. We count as adjudication all decisions that involved a court judgment, plus those non-judgement decisions that consist in dismissals of lawsuits.<sup>25</sup> The adjudication rate is the number of cases

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<sup>21</sup> We are not aware of any analogous publication in the United States, nor of similar comprehensive judicial statistics for administrative litigation in other countries.

<sup>22</sup> There are changes in the identities of agencies included in the separate reporting list; some agencies were subject to such reporting for as few as 4 years.

<sup>23</sup> These were: (1) uphold, (2) vacate, (3) change, (4) specific performance ordered, (5) affirming legality or validity, (6) confirming illegality or invalidity, (7) rejecting plaintiff’s request, (8) ordering compensation, (9) sustaining a no-compensation decision, (10) dismissal of suit, (11) withdrawals, (12) transfers, (13) termination, (14) other, and (15) mediation of compensation. The SPC classifies dispositions (1) to (9) as judgments, whereas dispositions (10)-(15) are decisions that do not require judgments.

<sup>24</sup> The key information used in Waldfoegel 1995, for example, involves (a) how many cases were adjudicated and how many were settled before adjudication; (b) how often the plaintiff won; (c) case type (i.e. to which of a large assortment of civil dispute categories a case belongs); and (d) the identity of the judges. Waldfoegel’s data thus incorporates no more case-level information than ours.

<sup>25</sup> In other words, adjudication consists in disposition categories (1) to (10) listed in the previous note. When a lawsuit against a government agency is dismissed by a court, there is no settlement and the plaintiff clearly loses. Compare Siegelman and Waldfoegel 1998, at 106-7 (pretrial motions in favor of the defendant counted as “trials”).

disposed in these ways divided by the number of cases closed during the year. We then take the sum of those dispositions in the plaintiff win categories—decisions that “vacate” or “change” agency actions and that order agencies to carry out specific performance or to provide compensation to the plaintiff—and then obtain the win rate by dividing the sum into the total number of cases adjudicated.<sup>26</sup> Both procedures are consistent with the approach adopted in empirical studies done in the U.S., and the information we derive is also comparable.

The main data challenge we encounter is the SPC’s category of “Other” dispositions. We treat cases withdrawn as settled/non-adjudicated cases, and this is generally the largest category of non-adjudicated cases. However, even in a recent year with a refined breakdown of disposition types such as 2013, the category of “Other” dispositions still represents over 13% of all administrative lawsuits closed. In other years with less detailed breakdowns of disposition types, the “Other” category can represent up to 30% of all cases closed, and thus is comparable in significance to the “withdrawal” category (see Figure A.1 in the Appendix). The nature of this category is unclear. An important question is then whether the category should be assumed to involve (mostly) adjudication or non-adjudication.<sup>27</sup> Counting “Other” as adjudication (non-adjudication) would raise (lower) the rate of adjudication for each year and each agency, and lower (raise) the rate of plaintiff wins relative to the alternative. While neither possibility can be ruled out, the approach we favor is to treat the “Other” category as comprising non-adjudicated cases. This is because for all cases for which Chinese courts have entered judgments, it is more likely that the court clerical systems would record them accurately (even if the SPC’s survey in any given year may not gather such information from lower courts). In other words, to the extent that cases closed are included in the “Other” category because of unclear classification or poor record keeping, this is more likely to happen to non-adjudicated than adjudicated cases.<sup>28</sup> Therefore, for each of our empirical tests of the selection hypothesis, the baseline approach we take excludes “Other” from the category of adjudicated cases. However, we also perform robustness checks using variables constructed in two other ways: (1) treating all “Other” dispositions as adjudications, and (2) treating all “Other” disposition as adjudications except during the four years when detailed classifications are available (where “Other” dispositions are treated as non-adjudicated).<sup>29</sup>

One other imperfection in the SPC data relates to 1997, when the SPC failed to report the numbers of cases withdrawn. Because withdrawal is the crucial selection phenomenon we study, we drop 1997 from our analysis. Therefore, from 25 years of SPC data, 24 years are usable.

### 3.2 Descriptive statistics: trends over time

As can be seen in Figure 1, the aggregate adjudication rate in Chinese administrative litigation experienced substantial variation over time. The path of evolution can be broken into three phases. The

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<sup>26</sup> Chinese scholars are accustomed to calculating the plaintiff win rate by using total number of cases closed as the denominator. The plaintiff win rates we calculate thus appear to be higher, especially when “Other” dispositions are not counted as adjudications.

<sup>27</sup> That is, “Other” may conceivably include court judgments of categories that are not specifically surveyed for a given year.

<sup>28</sup> We also note that the relative size of the “Other” category was also especially large from 2005 to 2011, during a period when Chinese courts were believed to have engaged in more mediation to secure “social harmony”.

<sup>29</sup> The correlations of the baseline construction of the adjudication rate with alternative constructions (1) and (2) are 0.60 and 0.625, respectively. The correlations of the baseline construction of the plaintiff win rate with alternative constructions (1) and (2) are 0.849 and 0.816, respectively.

first phase, from the late 1980s to 1997, saw a large rise in the rate of withdrawals of lawsuits. This drove down the adjudication rate from above 60% in the late 1980s to the low 30% range in 1996.<sup>30</sup> The second phase lasted from 1997 until around 2005, during which the aggregate rate of withdrawals declined steadily. Adjudication rate thus returned to levels above 50% (or higher if “Other” dispositions are treated as adjudicated). Since 2005, the adjudication rate again came down as a result of growth in the withdrawal rate.

What explains the fluctuation of adjudication rates overall? Empirical studies of Chinese administrative litigation are still scant, and few scholars have tried to answer these questions. However, one scholar has suggested that courts were concerned with “overly-high” withdrawal rates in the early and mid-1990s, and thus the decline in the withdrawal rate from 1997 to 2005 was the result of judicial interventions.<sup>31</sup> More Chinese scholars have claimed that the rise in settlement rates since 2006 is attributable to the Chinese government’s policy of promoting social harmony,<sup>32</sup> presumably again through court interventions. If Chinese courts did actively try to discourage settlements between 1997 and 2005, and subsequently to encourage settlements, this presumably would have been reflected in changes in the cost of litigation relative to settlement (i.e. the cost differential between litigation and settlement was raised between 1997 and 2005 and lowered after 2005). Holding other things (in particular, the locations of judicial standards, stake asymmetry, and the degree of uncertainty) equal, the selection theory would predict that changes in litigation costs would result in changes in adjudication and therefore win rates. This suggests one way of interpreting the panel and time series analyses below: variations in adjudication rates and plaintiff win rates over time are partially driven by changing litigation costs.<sup>33</sup>

Where there does seem to be a clear declining trend for is plaintiff win rates: in the aggregate, this rate has been coming down since the late 1990s (see Figure 1). The general low level of plaintiff win rates is notable. Because we exclude “Other” dispositions as adjudications in the baseline construction of variables, the plaintiff win rate is much higher than under alternative constructions. Nonetheless, the aggregate plaintiff win rate has stayed below 40% since 1998 and has been in the low 20% range in recent years.<sup>34</sup> We are not aware of any general explanation offered regarding why Chinese plaintiffs are less likely to win in a lawsuit against government agencies on average now than they did in the early 1990s. In light of this trend, however, we include a time counter in our times series analysis of the relation between adjudication and plaintiff win rates.

Finally, after decomposing the aggregate data by agency, one can see that, for each defendant agency type, the evolution of adjudication rate over time is often less smooth (see Figures 2a-2c). Variations over time in plaintiff win rates for different case types are shown in Figures 3a to 3c. The

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<sup>30</sup> If the “other” category of dispositions is treated as adjudicated cases, then these percentages would be higher, and are 70% in 1989 and 46% in 1996, respectively.

<sup>31</sup> He 2012, at 89.

<sup>32</sup> He 2012, id.

<sup>33</sup> Another factor that could have affected the evolution of the adjudication rates over time is the degree of legal uncertainty. As law becomes more certain over time, litigants may be expected to have less divergent expectations about judicial outcomes, and therefore to settle more often. Our data, however, does not show any strong trend (yet) of a decline in the adjudication rate.

<sup>34</sup> Figures A.2.a and b show the evolution of aggregate plaintiff win rates under the two alternative approaches to variable construction.

greater year-to-year variation at the agency level recommends testing the correlation between adjudication and win rates at this level in addition to at the aggregate level.

## 4. Empirical Results and Interpretations

Our empirical strategy follows that of Waldfogel 1995, who observes correlations between adjudication rates (labeled “T”) and plaintiff win rates (labeled “P”), based on variations in T and P across case types and individual judges on the SNDY court. In our study, the variations of T and P also occur across two dimensions: 24 years and 16 agency types.<sup>35</sup> Case types and agency types seem similar. But longitudinal variations in T and P potentially may have clearer sources than variations across judges.<sup>36</sup> On the one hand, although judgment size, information asymmetry, legal standards within particular areas of dispute may all vary over time, it is also just as plausible to assume that they do not.<sup>37</sup> On the other hand, as discussed in the last section, there are reasons to believe that litigation costs changed over time relative to settlement costs in Chinese administrative litigation.<sup>38</sup> Therefore, it is plausible to believe that these are the driving sources of variation. That the SPC data is nation-wide also reduces the relevance of omitted variables specific to local institutions. For comparisons across agencies, by contrast, it is plausible to assume that legal standards, information asymmetry and judgment size differ, whereas litigation costs are more or less uniform.

### 4.1 Intra-agency correlation between adjudication and plaintiff win rates

We begin by testing for correlations between adjudication rates and plaintiff win rates within agencies. We do this in two ways. The first uses full panel data on Chinese administrative litigation that include information for all agencies and all years. This corresponds to Waldfogel’s test for the T-P correlation across judges for all case types in the aggregate, except that we have 10 times the number of observations (233 as compared to Waldfogel’s 21) and apply year and agency-type fixed effects at the same time. The second uses time series data for each of the agencies for which SPC separate reporting is available. This second approach, corresponding to Waldfogel’s examination of the T-P correlation for particular case types, allows us to identify those agencies for which the selection effect in litigation is most pronounced. However, we are able to examine 11 agency types, whereas Waldfogel examines only 3 case types.

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<sup>35</sup> The sample size of our regression analyses is thus comparable to Waldfogel 1995’s: corresponding to his 23 judges, we have 24 years; corresponding to his 34 case types, we have 16 agency types.

<sup>36</sup> Waldfogel assumes random case assignment to judges. This seems to imply that all independent variables determining settlement are randomly distributed across judges, except for perhaps judge-specific decision standards and legal uncertainty. But since variations in legal standards should not result in variations in settlement rate under the selection theory, the only source of variation would be judge-specific legal uncertainty. However, Waldfogel does not subscribe to this implication.

<sup>37</sup> That is, just as Waldfogel assumes that different judges have similar case compositions, we may assume that both for a given agency, and for all agencies in the aggregate, the nature of the cases (in terms of legal standards and information asymmetries) displays substantial similarity from year to year. Moreover, we argued that it is plausible to treat most of administrative litigation in China as involving parties with symmetric stakes. This pattern has not changed over time.

<sup>38</sup> We use additional techniques (a time counter and the year fixed effect) to control for time trends.

Table 1 shows the result of the simple panel OLS regression of plaintiff win rate on adjudication rate using agency and year fixed effects. The coefficient is statistically significant at a 90% confidence level: when adjudication rate increases by 10%, the plaintiff win rate decreases by 1.3%. The model explains 74.2% of the variation in the sample (likely due to the fixed effect variables). In Tables A.1a and A.1b in the Appendix, we present robustness checks on this result, using the two alternative constructions of the two relevant variables. The regression result improves in statistical significance under construction (2) but loses statistical significance under construction (1). In all three tests, however, the coefficient has a negative sign.

An agency-by-agency analysis provides a better sense of where the selection effect manifests itself. In testing the T-P correlation using individual agency time series, we control for time trends in the two variables using a year counter.<sup>39</sup> Table 2 displays the results of OLS regressions for 11 agency categories for which we have observations for more than 10 years. Among these, 6 display statistically significant negative correlations between T and P: lawsuits against bureaus of industry and commerce (the agency primarily in charge of business registration), the police, tax collectors, township governments, urban construction bureaus (in charge of zoning and construction permits), and the generic category of “other” agencies, display selection effects. The selection effect also tends to be strong: for public security bureaus, for example, a 10% increase in the adjudication rate reduces the plaintiff win rate by 7.8%.

Tables A.2a and A.2b in the Appendix present robustness checks on the results for agency-by-agency regressions, using the two alternative constructions of the relevant variables. The negative correlation between T and P retains statistical significance for tax, public security, and the “other” category under both alternative constructions.<sup>40</sup> Under the construction where all “Other” dispositions are treated as adjudications, 4 out of 11 agency categories display selection effects. Under the other alternative construction, 10 out of the 11 agencies display selection effects. In each of these alternative constructions, selection effects seem more prevalent than Waldfogel was able to discover for the SDNY (i.e. 1 out of 3 case types).

The pervasiveness of the selection effects we demonstrate can be appreciated in two additional ways. First, even under the variable construction approach where the fewest categories of agencies display selection effects, the agencies involved (family planning, policing, tax collection, and “Other”) have on average accounted for over 54% (ranging from 37% to 71%) of the total number of administrative lawsuits in China. Second, these agencies also represent some of the largest branches of the Chinese administrative state, and account for the bulk of the Chinese government in terms of staff size. The significant negative correlation between T and P found in connection with these agency categories, therefore, strongly vindicate the selection hypothesis in Chinese administrative litigation.

## 4.2 Inter-agency comparisons

The presence of selection effects in Chinese administrative litigation strongly suggests the utility the selection theory for interpreting litigation outcomes. For example, all statistically significant correlations between T and P in our regressions are negative in sign. In other words, as low as Chinese

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<sup>39</sup> Inclusion of the time counter substantially improves the R-squared of the regression for most of the agencies.

<sup>40</sup> Under these alternative constructions, some other agencies (e.g. family planning) that did not display significant correlations between T and P now display significant (negative) correlations.

plaintiffs' chances of winning seem to be in decided cases, the proportion of plaintiff wins would be even lower if more cases were adjudicated. According to the selection theory, this suggests adverse legal standards against plaintiffs across regulatory areas.

Testing the T-P correlation across agencies for a given year is easy to implement,<sup>41</sup> but challenging to interpret. Given the limitations of the SPC data, such a cross-sectional regression would have at most 12 observations. Moreover, the sources of variation in T and P across agencies may be multiple. Therefore instead of estimating linear or non-linear T-P correlation on cross-sections, we compare difference among the agencies visually and using the ANOVA test.

Figure 4 contrasts the variations and medians in both T and P for all agencies. We suspect that the difference in median across agencies is not by chance. The results of an ANOVA test show that the medians in both the plaintiff win rate and the adjudication rate are not equal ( $p=0.0001$ ).<sup>42</sup> However, because we can reject the hypothesis of equal variance for the plaintiff win rate ( $p=0.0001$ ), which violates the equal-variance assumption of ANOVA, we are cautious about the finding on plaintiff win rate.<sup>43</sup> For the adjudication rate, we cannot reject the hypothesis of equal variance ( $p=0.344$ ), and therefore are more confident in the statistically significant finding from ANOVA.

These inter-agency differences are suggestive. First, the rate of adjudication varies among agencies as they do over the years. Factors that potentially explain this variation include the size of judgments relative to litigation costs, and the degree of legal certainty. For example, it may be that the size of judgments in lawsuits against land management bureaus and urban construction bureaus tend to be higher (as they involve compensation for takings or valuable permits) when compared to judgments in lawsuits against agencies that impose penalties (e.g. environment, family planning, and public health). Given similar litigation costs, the former types of litigations would tend to be adjudicated more often. However, how agencies differ with respect to legal uncertainty is not obvious, and our data does not permit the disentangling the effects of these different parameters.

Second, there are variations in plaintiff win rates that may correspond to differences in both legal standards and information asymmetry. In terms of legal standards, one might think that tax, family planning, and public security, being the most coercive branches of the Chinese state, have legal standards that are most adverse to plaintiffs. This could explain the lower plaintiff win rate as compared to other agencies. More interestingly, it is useful to think about systematic asymmetries in information. In litigation against the government, an important type of information asymmetry may pertain not to facts but to law. The government party may be conceived of as having on average superior information about the law: tax, family planning, and public health agencies are paradigmatic examples of enforcers of law. By contrast, land management and urban construction agencies are primarily licensing agencies,

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<sup>41</sup> The panel fixed effects regression discussed in IV.1 above can be interpreted as average strength of correlation between T and P, as varied across agencies, for all years.

<sup>42</sup> We performed tests on the independence and normality assumptions of ANOVA and found both to be satisfied. In particular, to test the normality assumption, we perform the Shapiro-Wilk test. The results show that we cannot reject the null hypothesis that the data of each agency for adjudication rate and plaintiff win rate is normally distributed. Despite this, since the sample size is smaller than 30 for all agencies for both types of rates, we conduct the Kruskal-Wallis H test or one-way ANOVA on ranks by comparing medians, which requires no assumption of normality. The results are almost identical if we run an ordinary one-way ANOVA by comparing means.

<sup>43</sup> The results of ANOVA tests for the alternative variable constructions are reported in the Appendix.



where equal knowledge between plaintiff and defendant about the law is more plausible. The example that supports this argument most is township governments: township governments are not specialized agencies. They do many different kinds of things—and can be sued for any of them. It is the type of government entity that has the least expertise in the law, which is consistent with the relatively high plaintiff win rates in this field.

Of course, the selection theory cautions against making inferences about legal standards from plaintiff win rates, and the foregoing conjectures about differences among agencies are largely speculative. Nonetheless, they suggest a much wider range of factors for explaining litigation outcomes than is normally considered in current academic commentaries on Chinese administrative litigation. Such commentaries tend to view litigation outcomes as primarily determined by the power balance between courts and executive branch agencies. It is often argued that plaintiffs lose (or choose not to sue at all) because courts (can be expected to) side with the government. However, the interagency variations in plaintiff win rates displayed in Figure 4 clearly do not lend themselves to such simplistic explanations. In terms of resources and political clout, few (at least based on anecdotal knowledge) would surmise that township governments, land management bureaus, or urban planning agencies are “weak” agencies, and that courts would be less deferential to them than to public health and family planning agencies. The selection theory thus offers distinctive insights into Chinese administrative litigation outcomes, by suggesting the potential relevance of asymmetrical information and (non-judicial) legal standards, even if our data is incapable of demonstrating their causal impact.

## 5. Selection through the exhaustion of administrative remedies

In many countries, the appeal against the action of an executive branch agency before a dispute is brought to courts is either a requirement or a right. This requirement or option to “exhaust administrative remedies” prior to litigation distinguishes administrative litigation from other types of civil litigation. It implies the existence of an institutionalized, pre-judicial procedure for resolving disputes between private parties and the government. Just as settlements during litigation reduce the number of cases tried by judges and potentially affect observed plaintiff win rates, settlements reached during or after appeals within the executive branch could have the same effect. Moreover, administrative appeals, as a public institution, may require the government to record the quantity of disputes between it and private parties and the manners in which they are resolved. In other words, pre-litigation settlements may be more observable here than in other areas.

In China, the administrative remedy that is available to private parties as either an alternative or a prerequisite to seeking judicial remedies is called “administrative reconsideration” (AR). In contrast to the U.S., the exhaustion of administrative remedy requirement is imposed in relatively few areas (tax assessments being perhaps the most important of those few), and only around 10% of all administrative litigation is preceded by AR (see Table 4 below). However, examining the minority of disputes that do go through the AR process is also relevant to testing the selection theory.

The Chinese government has continuously published annual national statistics on AR proceedings since 2004.<sup>44</sup> Such data provides the total number of AR proceedings handled by all

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<sup>44</sup> The data is released in the same annual China Law Yearbook from which our SPC data on administrative litigation is drawn.

government agencies in the country, as well as a breakdown of these total numbers into several categories of dispositions.<sup>45</sup> From this breakdown it is possible to calculate the proportion of cases settled before a verdict is rendered in an AR proceeding, as well as the win rate of appellants. Unlike SPC administrative litigation statistics, the AR data provides no information about the quantity or manners of disposition of AR proceedings against separate types of agencies. This means not only that one cannot perform cross- or within-agency analyses of dispute patterns (as we did with SPC data), but also that there is no way of knowing directly whether the disputes brought through AR processes are similar to the disputes brought to courts.

Table 3 shows the patterns of dispositions for AR proceedings for all agencies in the aggregate from 2003 to 2012. Column 1 indicates that the absolute level of AR proceedings has been consistently subdued: in all years between 2003 and 2009, AR proceedings occurred far less frequently than first-instance administrative lawsuits. Column 14 shows that among AR proceedings, settlements reached before a ruling is rendered consistently represented between 22% and 26% of all cases filed. Finally, stunningly, among all AR proceedings that were not settled, agency actions were sustained consistently 70%-80% of the time (column 15). Appellants' rate of success at obtaining a favorable ruling or an agency reversal that leads to a settlement thus ranged only between 15% and 28%.<sup>46</sup>

What impact do AR proceedings have on subsequent litigation? Despite the fact that appellants of agency actions lose more than 60% of the time in all AR proceedings and 70-80% of the time when an AR ruling is rendered, Table 4 shows that, consistently between 2003 and 2009, only 16-17% of all AR proceedings (or between 20% and 24% non-settled AR proceeding) go on to be litigated.<sup>47</sup> The failure of appellants to file lawsuits after receiving adverse AR rulings thus constitutes the main way in which AR proceedings reduce the volume of subsequent litigation. Table 4 also displays two other notable facts. First, the rate of withdrawal for lawsuits that were preceded by AR (column 7) is consistently and substantially lower than such rate for administrative litigation in general (column 6). Second, judging by the frequency with which courts sustained agency actions, the rate of plaintiff wins for lawsuits that were preceded by AR is dramatically lower than such rate for administrative litigation in general. In post-AR litigation, courts sustained agency actions in as high as 82% of the time (in 2006), as compared to 43.7% for all administrative litigation for the same year. However, the rate of defendant wins in post-AR litigation is comparable to the rate of appellee wins (i.e. agency action being sustained) during AR proceedings.

The implications of these patterns for the selection theory are complex. At first, the substantial discrepancy between the settlement rates in post-AR litigation and administrative litigation in general might seem to vindicate the selection theory. Cases that have gone through prior "screening" through

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<sup>45</sup> These include (1) rejecting the application for AR, (2) sustaining an agency action, (3) ruling the agency action to be illegal, (4) voiding the agency action, (5) changing the agency action, (6) ordering an agency to perform, (7) conciliation (tiaojie), (8-11) four types of terminations (a settlement agreement (hejie xieyi), voluntary withdrawal of the application, withdrawal after appellee agency alters action, and other termination), and (12) other dispositions.

<sup>46</sup> Appellants may also lose by having their AR applications rejected.

<sup>47</sup> Chinese plaintiffs have 60 days after receiving an adverse AR verdict to appeal to a court. Given this limited time window, the fact that administrative litigation is usually disposed of in a matter of months, and that the levels of both AR concluded and post-AR litigation are relatively steady, we simply divide the volume of post-AR litigation by the total volume of AR proceedings in the same year to calculate the proportion of AR proceedings that go on to litigation.

settlement are different, in terms of probability of subsequent settlement, from cases that have not gone through such screening. Some scholars have previously taken this as supporting evidence for the selection theory.<sup>48</sup> However, the evidence is weak for two reasons. First, even without a rational choice theory of settlement and trial selection, one might predict a substitutive effect between earlier and later settlements, because the disputes that are more amenable to settlement are likely to have been settled earlier. Second, for disputes that go through the AR process in China, it is clear that the rate of settlement does not change monotonically: the settlement rate after the application for AR but before an AR verdict is rendered is between 22% and 26%; the settlement rate after receiving an AR verdict and before launching a lawsuit is in the 70%-80% range; and the settlement rate after the launching of a lawsuit is below 20%. This suggests that disputants choose strategically when to settle, rather than the timing of settlement being the mere product of other rational choices.<sup>49</sup> Thus even though prior opportunities to settle may influence settlement rates during litigation, this should not be accepted as in itself relevant evidence for the selection theory.<sup>50</sup>

Furthermore, the fact that the appellee/defendant win rates remain very high—and roughly constant—between the AR and litigation stages is inconsistent with selection. It is clear that the sequence of procedures allows “two-sided” selections of disputes:<sup>51</sup> appellants/plaintiffs can decide whether to continue to pursue a dispute at numerous points, as can appellees/defendants.<sup>52</sup> According to the Priest and Klein divergent expectation model, as the number of cases adjudicated goes down, the plaintiff win rate should rise (given its low starting point) to 50%. This, unfortunately, is not observed for post-AR litigation. Nor is there evidence for one-sided selection, where the decisions of the party with the information advantage drive plaintiff win rates to either 0 or 1.<sup>53</sup>

One possible explanation of the Chinese pattern is the following. As noted above, the exhaustion of administrative remedies is not required for most disputes against Chinese government agencies, and most litigants bring lawsuits against the government directly, without first resorting to administrative appeal. Conversely, most appellees in AR proceedings do not go on to litigate. It is possible that, with the exception of those few areas of disputes (e.g. regarding tax assessments) where the exhaustion of administrative remedies is required, potential disputants against government agencies view AR and litigation as substitutes.<sup>54</sup> In that case, the massive rate at which disputes are dropped after

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<sup>48</sup> See, e.g. Lederman (1999), who finds that cases brought to U.S. tax courts that are preceded by IRS internal appeals are less likely to settle than those that have not gone through internal appeals.

<sup>49</sup> Waldfogel 1998 shows a similar non-monotonic sequence of settlement rates for disputes going through the SDNY.

<sup>50</sup> This does not prevent lessons from being drawn about the unrepresentativeness of published cases: The lower settlement rates for lawsuits preceded by AR implies that disputes preceded by AR may be over-represented in published court verdicts, since only cases that are not settled receive judicial verdicts.

<sup>51</sup> See Waldfogel 1998 for the distinction between one-sided and two-sided selections.

<sup>52</sup> Appellants/plaintiffs can make such decisions (a) before the filing of an AR appeal, (b) before an AR verdict is rendered, (c) after an AR verdict and before launching a lawsuit, and (d) after filing a lawsuit but before a court renders a judgement. Appellees/defendants can make similar decisions at least at junctures (a), (b) and (d), and potentially at juncture (c) as well (i.e. even after winning during the AR proceeding).

<sup>53</sup> In administrative litigation, the information-advantaged party is often presumably the government defendant.

<sup>54</sup> He 2014 asserts that cases handled by the AR and administrative litigation processes in China have different compositions of defendant/appellee agency types. He examines the same published data as we do, and we find no direct evidence for his assertion (since no detailed agency breakdowns are given for AR proceedings). However, it remains possible that cases handled by the AR and administrative litigation processes in China differ in other ways.

the rendering of (mostly appellee-unfavorable) AR verdicts may reflect choices made not during a process towards litigation, but processes that were not going to end in litigation anyway. Since the settlement and appellee win rates for AR are summed over cases brought by both appellees who would consider litigation and those who do not, the selection effect for those who intend to litigate against adverse outcomes may be masked.

This explanation, if valid, implies that studying the administrative appeal process in China will not advance the understanding of the selection theory in the way suggested at the beginning of this Section. Instead, studying the exhaustion of administrative remedies where it is required may be more appropriate. Nonetheless, our review of AR and post-AR litigation data in this Section informs the understanding of the selection theory in three ways. First, Ramseyer and Rasmussen 1999, in using the selection theory to explain Japanese tax litigation outcomes, suggests that the high (i.e. greater than 90%) observed government win rates may be explained by case selection through a prior appeals process: the idea is that the government will litigate only strong cases and will settle all other cases during administrative appeal. However, our study of Chinese AR proceedings offers a reminder that appellees can also drop disputes after an unfavorable outcome from administrative appeals. What goes on to be litigated is not decided by the government alone. This makes the low plaintiff win rate in litigation found by Ramseyer and Rasmussen harder to explain by selection theory. Second, we have noted that different settlement rates for cases that have and have not been exposed to prior settlement opportunities do not themselves support the selection theory. Finally, as we emphasized in Section 1, the presence of selection effects should not be taken for granted. Confirming (and understanding) its presence or absence, instead of simply making assumptions about such matters, is a necessary step in understanding the relevant legal processes. The patterns of post-AR litigation in China reinforce our belief in this regard.

## Conclusion

An important attraction of the selection theory is its abstraction from most institutional details. Thus within the American context, scholars have applied the theory across many types of litigation, regardless of the particular subject matter or the institutional features of the particular judicial fora. While many scholars writing about litigation patterns in civil law courts have been willing to assume the relevance of the selection theory, none has tested empirically the theory's applicability. Providing such evidence seems an important step in extending the economic analysis of dispute resolution to civil law systems (Kornhauser 2012).

We designed such a test using Chinese administrative litigation data, and obtained strong evidence for selection effects. Our test follows the approach in Waldfogel (1995 and 1998a) by focusing on correlations between adjudication and plaintiff win rates, two observable characteristics of many judicial systems. Such a test for selection effects could potentially aid inferences about unobservable case characteristics, whereas tests of the selection theory that deploy more robust information—such as access to the underlying population of disputes—would simultaneously reduce the utility of knowing that the selection theory applies to a particular context. Our empirical evidence for selection effects is more robust than what Waldfogel 1995 finds for a U.S. federal district court, and we provide an interpretation for drivers of variations in settlement that is absent from Waldfogel's study.

The applicability of the selection theory to Chinese administrative litigation is surprising, but it could help to explain more of the variations in litigation patterns than the factor that the academic

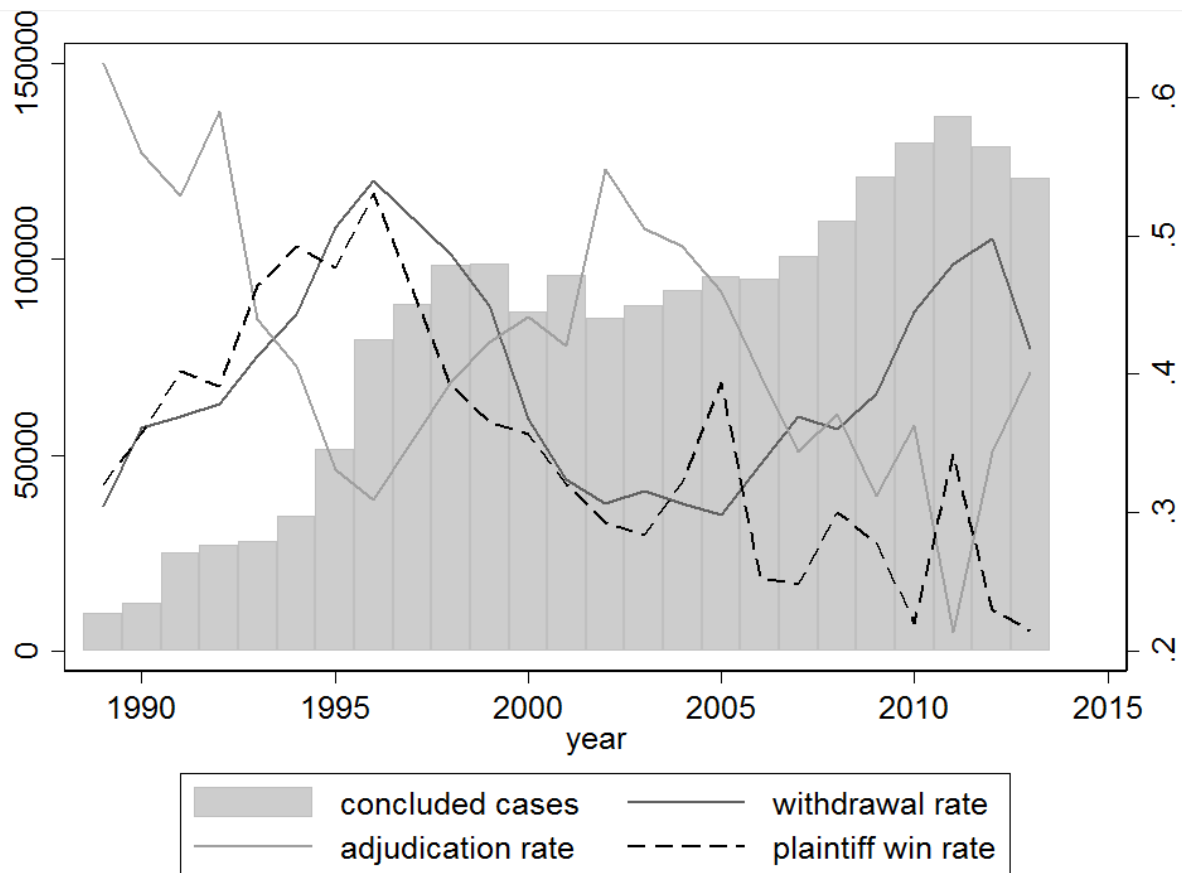
literature on Chinese litigation has so far focused on. While our examination of the data from Chinese administrative reconsideration proceedings does not detect selection effects, it potentially sheds light on the differences between AR and litigation proceedings. Clearly, much work remains to be done for the selection theory to explain real world legal processes.

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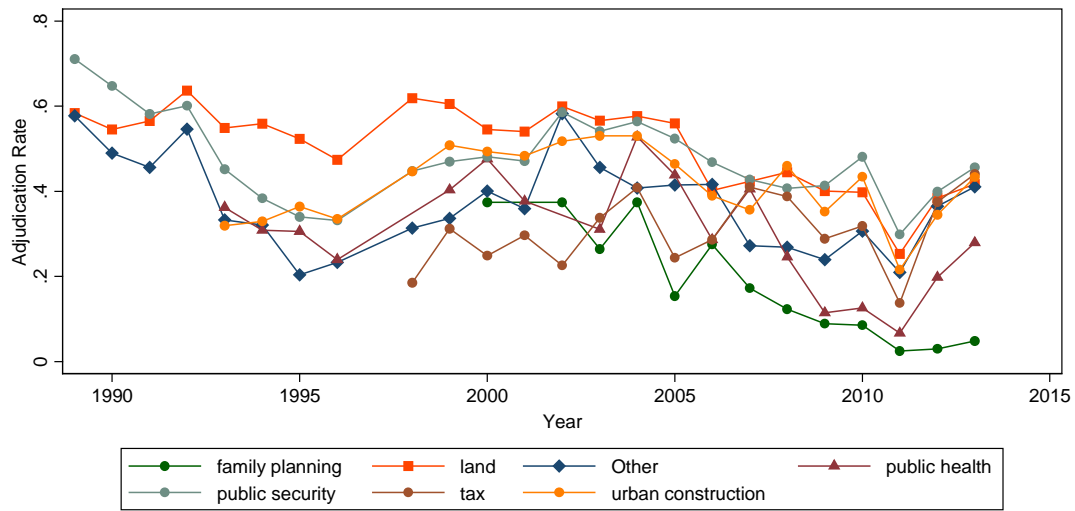
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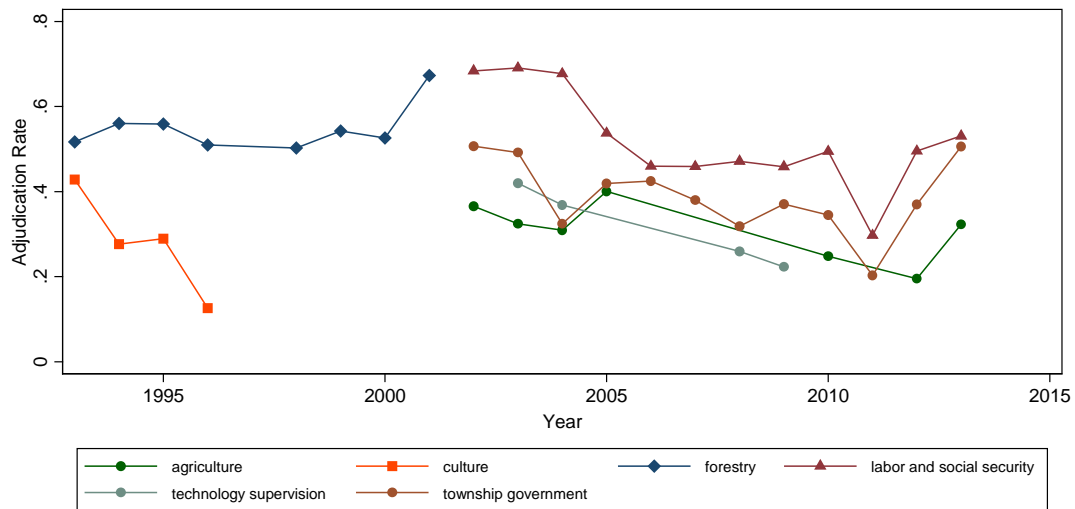
**Figure 1 Evolution of Chinese Administrative Litigation: Aggregate Patterns**



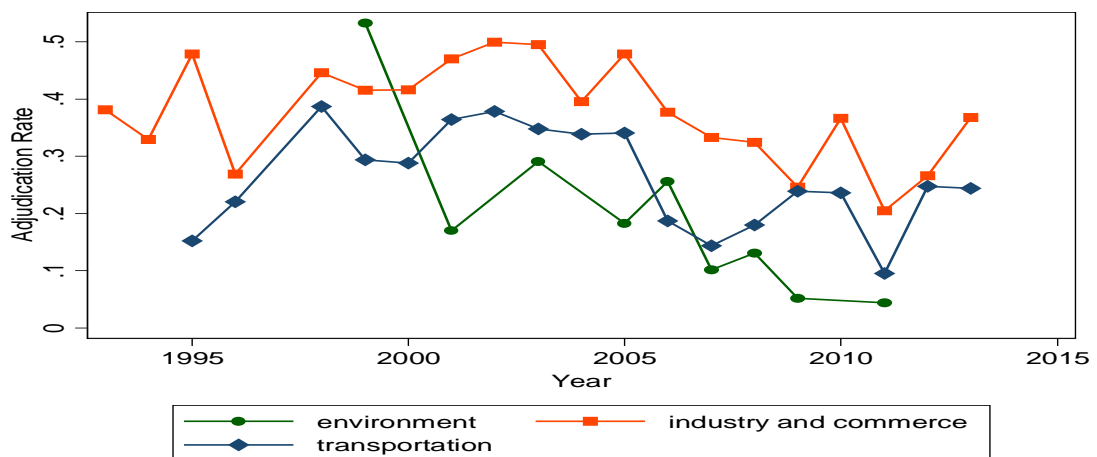
**Figure 2a The Evolution of Adjudication Rates for Select Agencies (I)**



**Figure 2b The Evolution of Adjudication Rates for Select Agencies (II)**

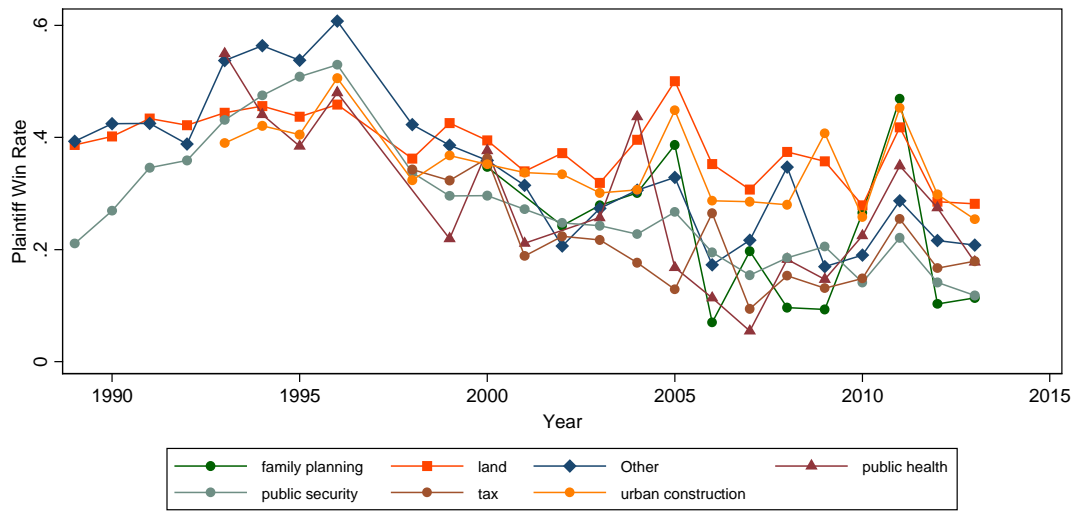


**Figure 2c The Evolution of Adjudication Rates for Select Agencies (III)**

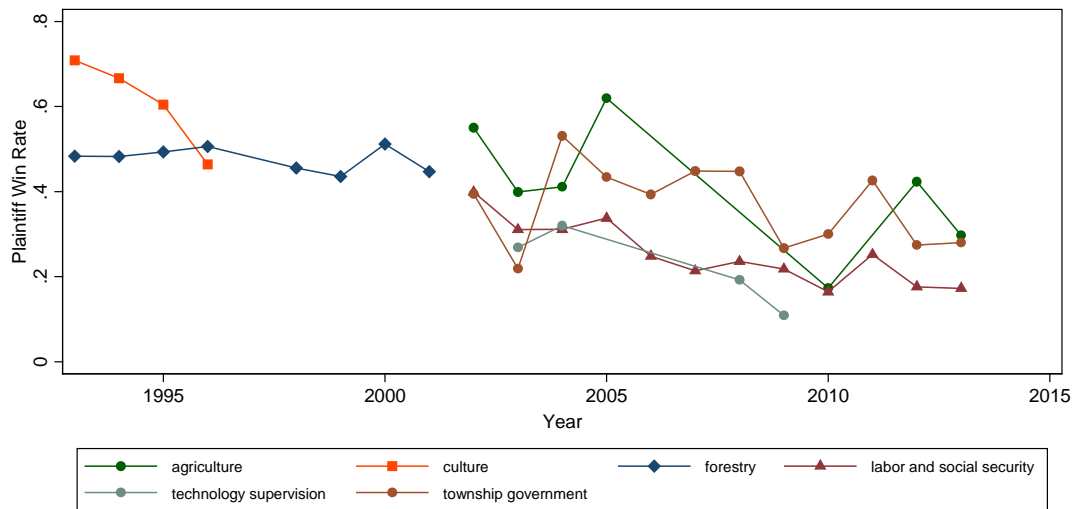




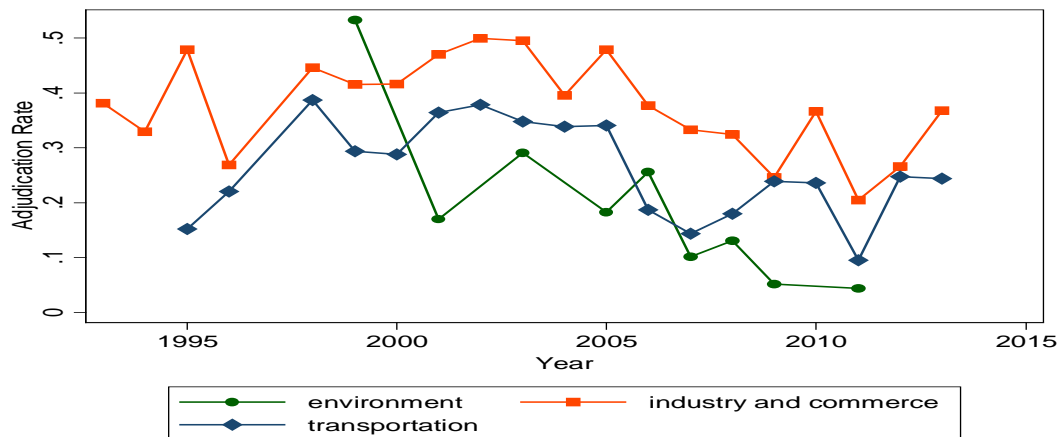
**Figure 3a The Evolution of Plaintiff Win Rates for Select Agencies (I)**



**Figure 3b The Evolution of Plaintiff Win Rates for Select Agencies (II)**



**Figure 3b The Evolution of Plaintiff Win Rates for Select Agencies (II)**



**Table 1 Correlation between Adjudication and Plaintiff Win Rates: All Cases, 1989-2013**  
**(OLS with Fixed Effects)**

	(1)
D.V.=Plaintiff Win Rate	
Adjudication Rate	-0.130*
	(0.077)
Constant	0.398***
	(0.060)
Observations	233
R-squared	0.742
Agency Type FE	yes
Year FE	yes

Standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 2 Cross-Agency Comparison (Excluding Other)**

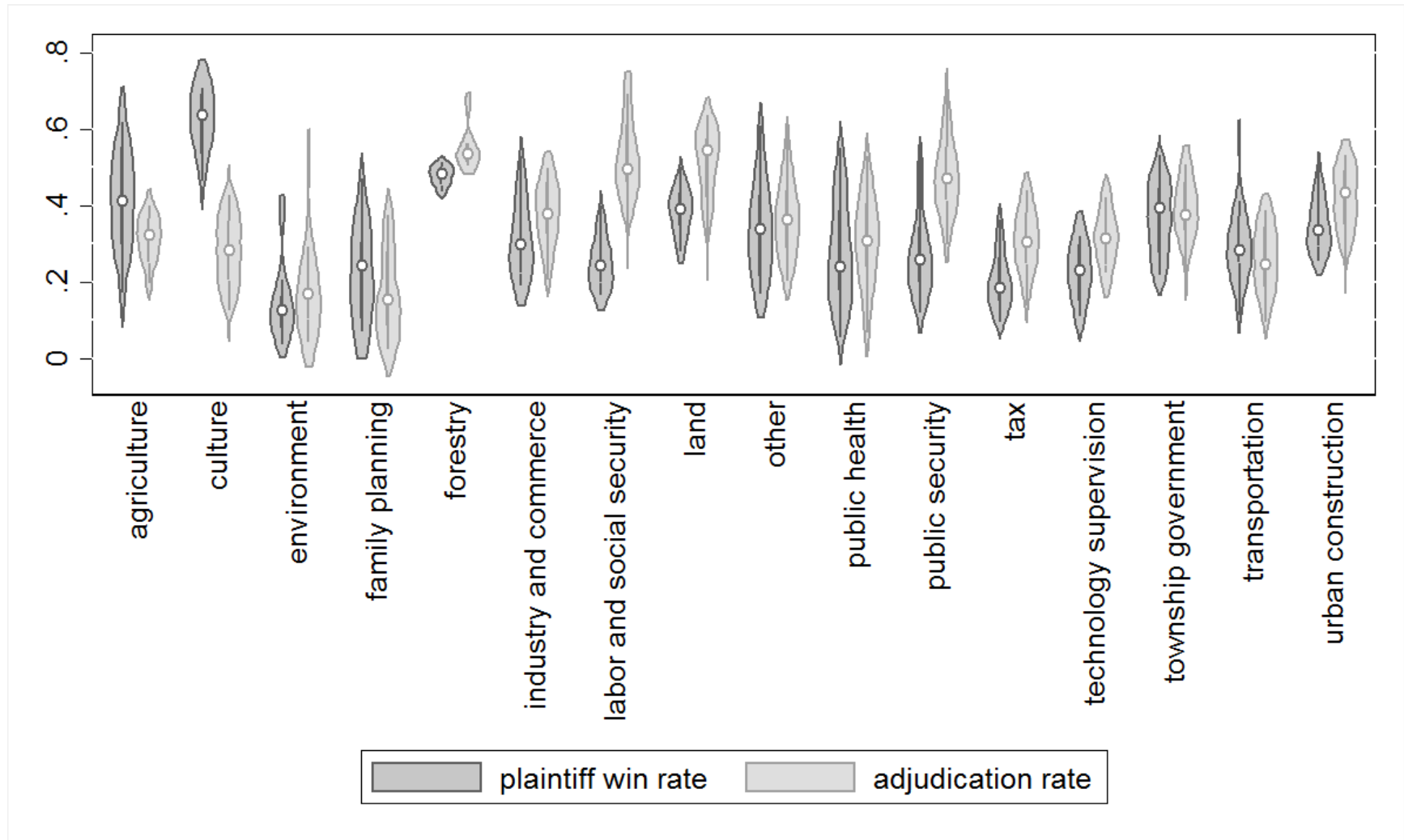
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
D.V.=Plaintiff	Win	family	industry	labor	land	other	public	public	tax	township	transportation	urban
Rate		planning	and	and			health	security		government		construction
			commerce	social								
				security								
Adjudication Rate		-0.863	-0.513***	-0.029	-0.046	-0.603***	-0.237	-0.780***	-0.337*	-0.692**	0.115	-0.404**
		(0.693)	(0.148)	(0.160)	(0.208)	(0.111)	(0.313)	(0.123)	(0.167)	(0.267)	(0.340)	(0.143)
Year Counter		-0.037*	-0.017***	-0.018***	-0.005**	-0.017***	-0.016***	-0.016***	-0.008**	-0.015*	-0.009**	-0.006***
		(0.017)	(0.001)	(0.004)	(0.002)	(0.002)	(0.004)	(0.001)	(0.003)	(0.007)	(0.003)	(0.002)
Constant		1.093**	0.775***	0.625***	0.477***	0.787***	0.608***	0.856***	0.459***	0.930***	0.417***	0.606***
		(0.451)	(0.060)	(0.149)	(0.130)	(0.059)	(0.132)	(0.062)	(0.076)	(0.204)	(0.084)	(0.071)
Observations		13	20	12	24	24	18	24	16	12	18	20
R-squared		0.236	0.817	0.763	0.363	0.819	0.467	0.864	0.497	0.463	0.273	0.467

Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Note: Agencies of agriculture, culture, environment, forestry and technology supervision are excluded due to data scarcity (n < 10).

Figure 4 Violin Plot of Plaintiff Win Rate and Adjudication Rate



**Table 3 Disposition of Disputes in Administrative Reconsideration (AR), 2003-2012**

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Year	AR proceed ings total	Rejectio n of appeal	Sustain	Agency action illegal	Vacate	Change	performan ce ordered	Concili ation	Settleme nt Agmt	Volunt ary withdr awal	Withdrawal after appellant modificatio n	Other terminati ons	Other	settlement rate (sum of columns 8-10 and 12-13 divided by column 1)	Sustain rate (column 3 divided by (1-column 14))
2012	91866	0.0550	0.5766	0.0122	0.0576	0.0038	0.0139	0.0260	0.0149	0.1542	0.0250	0.0213	0.0395	0.2559	0.7749
2011	84387	0.0604	0.5918	0.0106	0.0582	0.0054	0.0109	0.0201	0.0150	0.1511	0.0259	0.0130	0.0376	0.2368	0.7754
2010	77606	0.0484	0.5878	0.0059	0.0644	0.0049	0.0082	0.0248	0.0182	0.1656	0.0281	0.0084	0.0353	0.2523	0.7861
2009	64668	0.0365	0.6222	0.0088	0.0681	0.0064	0.0080	0.0226	0.0134	0.1569	0.0298	0.0124	0.0148	0.2201	0.7978
2008	66479	0.0289	0.6171	0.0058	0.0745	0.0077	0.0057	0.0189	0.0213	0.1622	0.0289	0.0101	0.0190	0.2315	0.8030
2007	72747	0.0162	0.6054	0.0116	0.0790	0.0155	0.0051	0.0132	0.0117	0.1473	0.0462	0.0103	0.0385	0.2210	0.7772
2006	72029		0.6059	0.0078	0.1113	0.0185	0.0091			0.1892			0.0582	0.2474	0.8051
2005	70095		0.5954	0.0075	0.1299	0.0229	0.0100			0.1857			0.0486	0.2343	0.7776
2004	64953		0.5808	0.0063	0.1467	0.0268	0.0086			0.1809			0.0499	0.2308	0.7551
2003	62189		0.5571	0.0065	0.1601	0.0367	0.0106			0.1914			0.0499	0.2413	0.7343

Data source: China Law Yearbook

Notes:

1. See footnote 45 for the categories of dispositions.
2. Fractions in columns 3-14 are derived using total AR proceedings (column 1) as the denominator.

**Table 4 Dispositions of Post-AR Administrative Litigation, 2003-2009**

1	2	3	4	5	6	7	8	9
Year	AR proceedings concluded	Admin litigation cases concluded	Post-AR litigation cases concluded	Post-AR litigation rate	Rate of withdrawal of all admin litigation	Rate of withdrawal of post-AR litigation	Rate of agency action sustained all admin litigation	Rate of agency action sustained post-AR litigation
2003	62189	88050	10185	0.164	0.316	0.178	0.352	0.766
2004	64953	92192	10181	0.157	0.306	0.189	0.338	0.757
2005	70095	95707	12518	0.179	0.298	0.183	0.358	0.802
2006	72029	95052	11576	0.161	0.335	0.185	0.437	0.821
2007	72747	100683	11718	0.161	0.370	0.148	0.459	0.775
2008	66479	109085	10645	0.160	0.359	0.151	0.483	0.735
2009	64668	120530	10933	0.169	0.384	0.153	0.415	0.738

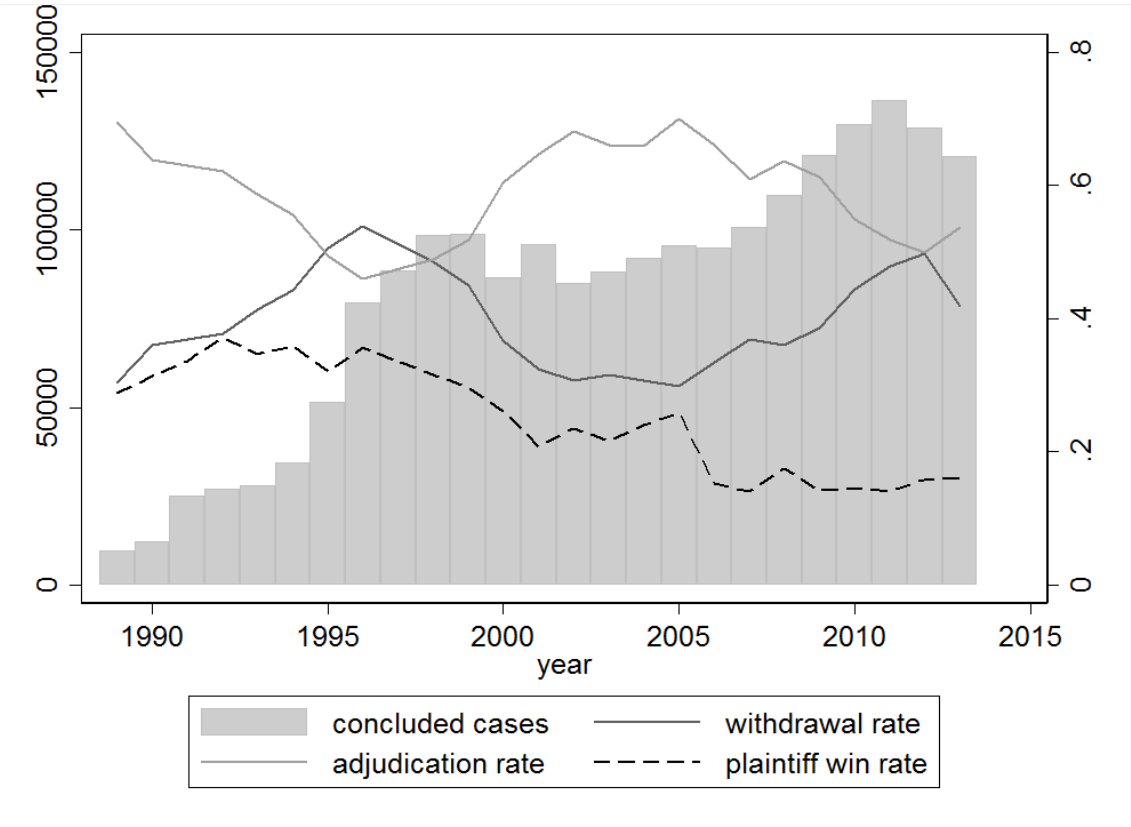
Data source: China Law Yearbook

## Appendix

Figure A.1 The Ratio of Disposition "Other" over Total Concluded Cases



Figure A.2a Evolution of Chinese Administrative Litigation: Aggregate Patterns (Including Other)





**Figure A.2b Evolution of Chinese Administrative Litigation: Aggregate Patterns (Excluding Other for Select Years)**

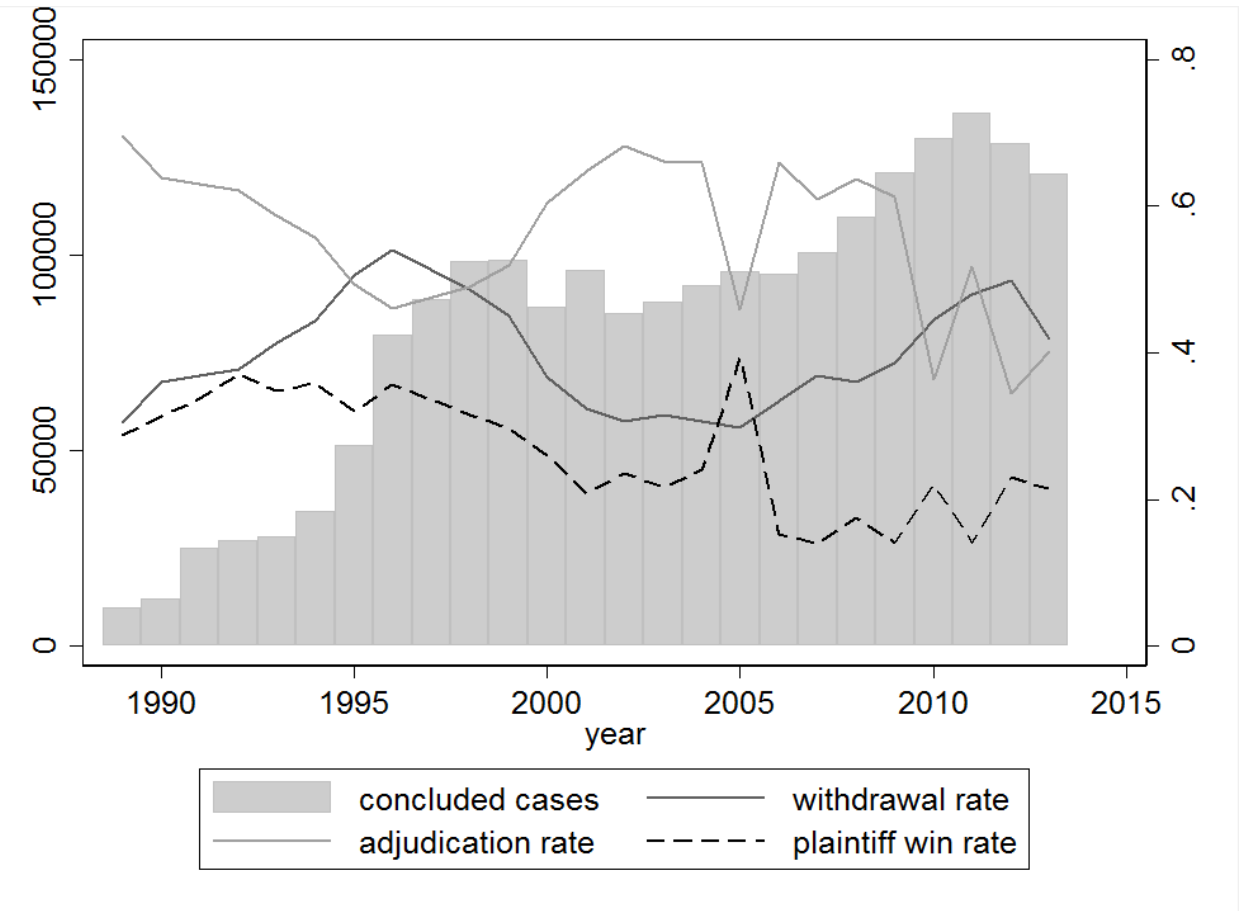


Table A.1a Correlation between Adjudication and Plaintiff Win Rates: Cases Including Other, 1989-2013  
(OLS with Fixed Effects)

D.V.=Plaintiff Win Rate	(1) Fixed Effects
Adjudication Rate	-0.068 (0.044)
Constant	0.323*** (0.041)
Observations	233
R-squared	0.828
Case FE	yes
Year FE	yes

Standard errors in parentheses  
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table A.1b Correlation between Adjudication and Plaintiff Win Rates: Cases Excluding Other for Select  
Years, 1989-2013 (OLS with Fixed Effects)

D.V.=Plaintiff Win Rate	(1) Fixed Effects
Adjudication Rate	-0.110** (0.046)
Constant	0.351*** (0.044)
Observations	233
R-squared	0.792
Case FE	yes
Year FE	yes

Standard errors in parentheses  
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table A.2a Correlation between Adjudication and Plaintiff Win Rates cross-agencies: Cases Including Other, 1989-2013**  
(OLS with Fixed Effects)

D.V.=Plaintiff Win Rate	(1) family planning	(2) industry and commerce	(3) labor and social security	(4) land	(5) other	(6) public health	(7) public security	(8) tax	(9) township government	(10) transportation	(11) urban construction
Adjudication Rate	-0.205*** (0.040)	-0.211 (0.188)	0.193 (0.336)	0.156 (0.197)	-0.298*** (0.086)	0.025 (0.301)	-0.414*** (0.125)	-0.272*** (0.085)	-0.115 (0.160)	-0.032 (0.103)	-0.127 (0.135)
Year Counter	-0.021***	-0.012***	-0.013	-	-0.011***	-	-0.011***	-0.006**	-0.005	-0.009*	-0.005**
Constant	(0.002) 0.590*** (0.052)	(0.002) 0.522*** (0.124)	(0.011) 0.326 (0.424)	(0.001) 0.311** (0.134)	(0.001) 0.543*** (0.054)	(0.003) 0.354** (0.147)	(0.001) 0.612*** (0.070)	(0.003) 0.378*** (0.062)	(0.006) 0.425** (0.185)	(0.005) 0.344** (0.119)	(0.002) 0.394*** (0.065)
Observations	13	20	12	24	24	18	24	16	12	18	20
R-squared	0.892	0.767	0.723	0.722	0.900	0.645	0.840	0.586	0.081	0.267	0.430

Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Note: Agencies of agriculture, culture, environment, forestry and technology supervision are excluded due to data scarcity (n < 10).

**Table A.2b Correlation between Adjudication and Plaintiff Win Rates cross-agencies: Cases Excluding Other for Select Years, 1989-2013**  
(OLS with Fixed Effects)

D.V.=Plaintiff Win Rate	(1) family planning	(2) industry and commerce	(3) labor and social security	(4) Land	(5) other	(6) public health	(7) public security	(8) tax	(9) township government	(10) transportation	(11) urban construction
Adjudication Rate	-0.409*** (0.077)	-0.333** (0.124)	-0.410** (0.148)	-0.268*** (0.094)	-0.401*** (0.077)	-0.307* (0.157)	-0.457*** (0.108)	-0.288*** (0.077)	-0.372* (0.199)	-0.143 (0.129)	-0.402*** (0.110)
Year Counter	-0.028*** (0.005)	-0.012*** (0.002)	-0.027*** (0.006)	-0.008*** (0.001)	-0.011*** (0.001)	-0.011*** (0.003)	-0.011*** (0.001)	-0.006* (0.003)	-0.010 (0.008)	-0.008 (0.005)	-0.004** (0.002)
Constant	0.843*** (0.132)	0.593*** (0.092)	0.989*** (0.192)	0.589*** (0.071)	0.602*** (0.052)	0.515*** (0.094)	0.642*** (0.061)	0.381*** (0.062)	0.672** (0.253)	0.393** (0.135)	0.547*** (0.073)
Observations	13	20	12	24	24	18	24	16	12	18	20
R-squared	0.822	0.699	0.709	0.473	0.864	0.502	0.831	0.596	0.275	0.177	0.414

Robust standard errors in parentheses

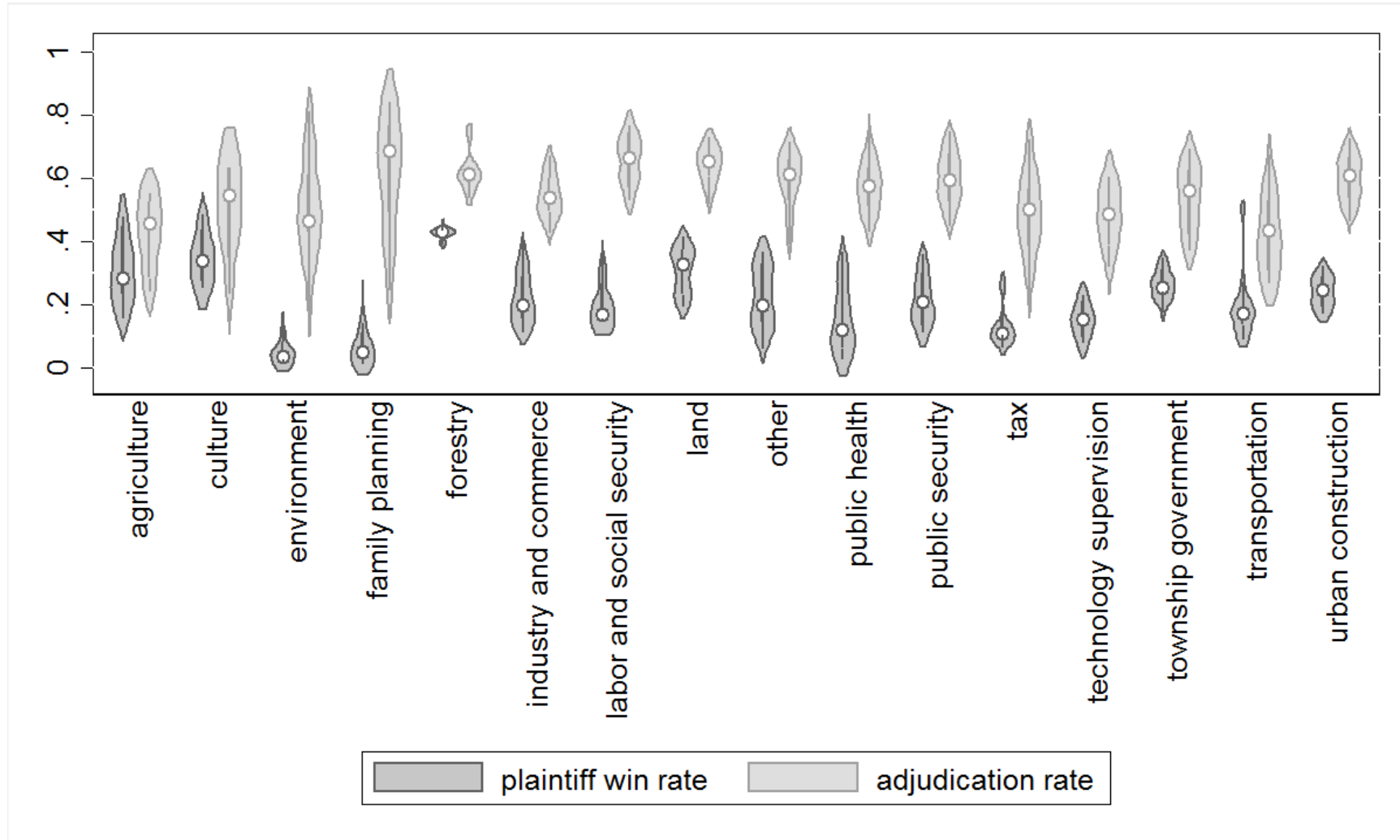
\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Note: Agencies of agriculture, culture, environment, forestry and technology supervision are excluded due to data scarcity (n < 10).

### ANOVA Analysis I (Including Other)

Finally, we perform an ANOVA test on plaintiff win rate and adjudication rate, respectively when we construct two variables by including others. The results show that the means in both the plaintiff win rate and adjudication rate are not equal ( $p=0.000$ ). However, because we can reject the hypothesis of equal variance for both rates ( $p=0.002$  for the plaintiff win rate, and  $p=0.000$  for the adjudication rate), which violates the equal-variance assumption of ANOVA, we are cautious about the findings.

Figure A.3a Violin Plot of Plaintiff Win Rate and Adjudication Rate (Including Other)



### ANOVA Analysis II (Excluding Other for Select Years)

We also perform an ANOVA test on plaintiff win rate and adjudication rate, respectively when we construct two variables by excluding others for select years. The results show that the means in both the plaintiff win rate and adjudication rate are not equal ( $p=0.000$ ). However, because we can reject the hypothesis of equal variance for both rates ( $p=0.005$  for the plaintiff win rate, and  $p=0.000$  for the adjudication rate), which violates the equal-variance assumption of ANOVA, we are cautious about the findings.

**Figure A.3b Violin Plot of Plaintiff Win Rate and Adjudication Rate (Excluding Other for Select Years)**

