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Suing the government under weak rule of law: Evidence from administrative litigation reform in China ☆

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ABSTRACT

There is a long-standing debate in the literature about the effectiveness of strengthening judicial independence in developing countries with weak rule of law. This paper exploits a recent Chinese judicial reform in administrative litigation, which changed the jurisdiction rule from intra-regional to trans-regional, to estimate the effects of improved judicial independence on protecting private entities against potential abuses of public authority. We find a significant increase in the probability of successfully suing local governments after the reform, especially when the defendants are more powerful government departments and when the plaintiffs are individual citizens with fewer legal resources than firms. But this effect is more limited for higher-level governments. The reform also results in increased case filings, prolonged trial time, and enhanced judicial quality. In addition, it raises the awareness of both governments and citizens about the rule of law, increases firm entry, and worsens general public attitudes toward local governments, at least in the short term. Our study highlights trans-regional jurisdiction as a new source of judicial independence in a party state and its potential limitations.

1. Introduction

Many developing countries, especially those ruled by non-democratic regimes, have long suffered a lack of judicial independence from political intervention, largely due to the limited constraints on executive power. While a conventional view holds that authoritarian governments are primarily motivated to protect their discretionary power and have no incentives to respect the rule of law, a burgeoning strand of the literature maintains that authoritarian leaders *do* have incentives to strengthen the independence of the judicial system, at least for pragmatic purposes.[1] Some studies have demonstrated that a more effective legal system helps authoritarian countries attract private and foreign investment (Moustafa, 2007). More importantly, authoritarian leaders expect to leverage the legal system as a powerful vehicle for the "rule *by* law" to mitigate the principal–agent problem (Ginsburg and Moustafa, 2008), such as by strengthening political control over lower-level officials (Albertus and Menaldo, 2012), monitoring social discontent (Ríos-Figueroa and Aguilar, 2018), and reducing internal conflicts (Sievert, 2018).

Therefore, the real challenge for many developing countries is not simply whether there is an incentive to establish judicial independence, but also how to achieve it effectively when the government ultimately retains discretionary control over civil society. Most prior studies in this area have relied on case studies or anecdotal evidence (e.g., Hendley, 1996; Moustafa, 2007; Liu and Weingast, 2020); very few have attempted to provide causal evidence of effective ways to improve judicial independence in developing countries. Mehmood (2022) and Chemin (2021) are two notable exceptions. Mehmood (2022) presents evidence that a constitutional amendment that changed the judicial selection procedure from presidential appointment to peer appointment in

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[1] See Helmke and Rosenbluth (2009) and Moustafa (2014) for reviews.

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Pakistan significantly reduced the number of pro-government judgments. Chemin (2021) exploits judicial reforms in a set of African countries – which international organizations funded to enhance the access, speed, and quality of their judiciaries – and finds that societal groups that lacked power and experienced discrimination benefited disproportionately from the reforms.

We investigate a new source of judicial independence by exploiting the recent Trans-Regional Jurisdiction (TRJ) reform in China, a party state with weak rule of law, which changed the jurisdiction rule of administrative litigation (i.e., lawsuits against governments) from intra-regional to trans-regional. Prior to the reform, such cases were handled under the rule of *intra*-regional jurisdiction: if a citizen sued the government of county *A*, the court of county *A* would hear the case. Yet since the court of county *A* is under the direct control of county government *A*, it was very hard for the citizen to win the case. After the reform, which introduced the new principle of *trans*-regional jurisdiction, the case would be transferred to (and handled by) a court in another county (say, *B*) in the same prefecture.[2] The court of county *B* is outside the control of county government *A*; thus the latter would find it more difficult to influence the judgments of court *B* than those of court *A*. This suggests that the reform might make citzens more likely to win their cases. Yet some may argue that county government *A* retains the capacity to influence the decisions of court *B* through informal connections with either county government *B* or their common superior prefectural government. This raises an interesting empirical question: can the TRJ reform significantly improve Chinese citizens' legal protection against local governments?

The reform's staggered roll-out facilitates a difference-in-differences (DID) estimation and empirical analysis based on a large number of legal documents made available by local Chinese courts. We find that the TRJ reform makes government authorities 3.9 percentage points more likely to lose in administrative litigation cases than before the reform, which is both statistically and economically significant. A classic event study reveals that counties that implemented the reform exhibit a similar trend in governments' probability of losing as counties that have not. Moreover, following recent work by Goodman-Bacon (2021) on the potential biases of the two-way fixed-effect (TWFE) DID estimator with the staggered adoption of a policy treatment (like the one we analyze here), we find that our key results are mainly driven by comparisons between timing groups and never-treated groups, which suffer from few biases even when the treatment effect is dynamic and heterogeneous. Our key results are also robust to alternative methods of dealing with the pitfalls of staggered TWFE-DID estimations. Additionally, we conduct a full battery of robustness checks, including switching cluster units from counties to prefectures, restricting the analysis to treatment group counties only or a matched sample, redefining the treatment variable to tackle potential measurement errors, controlling for judge fixed effects or county-level judicial endowment, and taking the impact of other judicial reforms into consideration. Our baseline results are robust to all these checks.

To strengthen the causal interpretation of our estimation, we exploit specific institutional details of the reform. Prefectures sometimes designate a particular county court as the "centralized court" that handles all administrative cases within the prefecture, including its own cases as well as those transferred from other counties. So, for cases against the government of the county where the centralized court is located, there is no genuine separation between the origin and adjudication (through which the TRJ reform would take effect) – what we call a "pseudo reform." In pseudo-reform counties we expect to see no significant effect on governments' probability of losing, and indeed our empirical analysis presents evidence that is consistent with this expectation. This placebo test not only highlights that trying cases in a different county is an important source of judicial independence; it also helps preclude other potential confounding factors associated with the TRJ reform, such as local governments becoming more hesitant to interfere with the judicial process.

We find evidence that the reform's effects are heterogeneous across different types of plaintiffs and defendants: after the reform, more powerful departments of county governments (e.g., public security departments) are more likely to lose their cases, while individual citizens, who possess fewer legal resources than firms, are more likely to win their cases against governments. Yet the reform's impact is less significant when county-level governments are sued as a whole (which are at a higher administrative level and are thus more likely to interfere with courts than their agencies or sub-county governments), indicating its limitations.

Our additional analyses reveal that the reform significantly increases the number of cases against local governments and makes litigants more likely to accept court judgments. However, these benefits are partly offset by the longer time needed for court trials due to the increased workload. We also find evidence that the reform heightens awareness of the rule of law among both governments and citizens, and encourages firm entry. Somewhat surprisingly, while the reform makes citizens more likely to win their cases against governments, it decreases their satisfaction with local governments. This finding is likely attributable to the relatively short sample period: local citizens' perceptions of their local governments were overwhelmed by the sudden increase in the number of lawsuits against local governments, which may have revealed that government authorities engage in serious abuses or misbehaviors.

This paper advances inter-disciplinary research on the effectiveness of strengthening judicial independence in developing countries governed by authoritarian or other regimes with weak rule of law. While some studies emphasize that ruling parties in non-democracies have incentives to improve the judicial system and grant a degree of genuine independence to courts (Barros, 2002; Moustafa, 2007; Ginsburg and Moustafa, 2008; Albertus and Menaldo, 2012; Ríos-Figueroa and Aguilar, 2018; Sievert, 2018), many others question whether such measures can increase judicial independence, since non-democratic governments would like to informally pressure judges through social ties or even physical attacks to influence judicial outcomes, especially in high-stakes cases (Solomon, 2010; Ledeneva, 2008; Llanos et al., 2016). Our analysis contributes to this debate by offering strong empirical evidence that administrative litigation with appropriate institutional arrangements, such as separation between the origin and adjudication of cases, could significantly constrain government power and protect civil rights, even in a party state like China.[3] Part of the feasibility of the TRJ reform is derived from the fact that it improves the judicial system by constraining lower-level government officials' abuse of power without threatening the overall political regime. In this sense, it echoes the notion of "rule *by* law," which some scholars maintain is a pragmatic way of promoting judicial independence in authoritarian countries (Moustafa, 2014).

[2] A prefecture in China typically has about eight counties under its jurisdiction. The TRJ reform currently applies to counties in the same prefectures. It has not yet extended beyond the prefecture level.

[3] Among the extant studies, Chang et al. (2019) is the closest to ours. They focus on the effect of the TRJ reform in a single prefectural city (Jiangmen) in Guangdong Province. We analyze a comprehensive and nationally representative dataset covering nearly 300 prefectures, which allows us to exploit rich regional variations and institutional details of the reform to generate rigorous causal inferences (see Section 2.3 for a thorough introduction). We also go beyond judicial outcomes in administrative litigation to explore the reform's broader social influences (see Section 6.3).

Our study also complements prior research on the determinants of judicial independence by highlighting trans-regional jurisdiction as a new way to improve judicial independence. Previous studies have focused on multiple factors that affect the independence of judicial systems, such as political competition across parties (Hanssen, 2004), power relations between the presidency and congress (Gardner and Thrower, 2023; Iaryczower et al., 2002), judge appointment procedures (Mehmood, 2022; Ash and MacLeod, 2021), and judges' tenures (Klerman and Mahoney, 2005; Porta et al., 2004). By contrast, we utilize the TRJ reform to demonstrate that trans-regional jurisdiction – which does not substantively change the judicial system or completely release the judiciary from political control – could also significantly improve judicial independence. Our analysis suggests that judicial independence can be improved in the common setting of intra-regional jurisdiction for administrative litigation in many developing countries (e.g., Brazil and South Africa), where the influence of political power on judicial outcomes is also salient (Poblete-Cazenave, Forthcoming; Lambais and Sigstad, 2023). Moreover, the reform insulates the adjudication process from the influence of local politicians, and thus plays a role similar to circuit courts in developed countries.[4] These implications allow our research to join the policy debate about judicial independence in a broader context that is not restricted to developing countries.

Finally, we empirically analyze a unique digitized dataset that contains nearly all judicial documents in China. Because it is generally difficult to access a country's complete judicial records, previous studies on judicial independence have selected their samples based on regions (Assumpcao and Trecenti, 2020), categories of defendants (Lambais and Sigstad, 2023; Sanchez-Martinez, 2017), or types of courts (Franck, 2009). The compulsory, real-time disclosure of digitized judgment documents in China since 2014, which will be detailed in Section 3.1, provides a unique opportunity to conduct empirical analyses based on complete judicial records. Our study is one of the first to employ a sample of data close to the universe of administrative litigation cases in China to analyze the effects of the TRJ reform.[5]

The remainder of the paper is organized as follows. Section 2 introduces the institutional background of this research. Section 3 describes the data, variables, and sample construction. Section 4 presents the identification strategy. Section 5 reports the main empirical findings, while Section 6 discusses heterogeneity, the effects on other judicial outcomes, and the reform's broader influences. Section 7 concludes.

2. Institutional background

2.1. China's court system

Mainland China has five hierarchical levels of territory administration. In descending order, these are the center, provinces, prefectures, counties, and townships. The court system parallels these levels: the national-level central court (the Supreme People's Court, or SPC) and three levels of local courts – High People's Courts at the provincial level, Intermediate People's Courts at the prefectural level, and Basic People's Courts at the county level.[6] Each province, prefecture, and county has only one local court. As of 2013, there were 31 provinces (High People's Courts), 333 prefectures (Intermediate People's Courts), and 2,853 counties (Basic People's Courts) in mainland China.[7]

The political regime's institutional arrangements ensure that the administrative authorities exercise more power than the court system. The local standing committee of the Communist Party of China is the highest political authority in a given sub-national region (e.g., a county). As shown in Fig. 1, the committee secretary (also known as the party secretary) handles the party's overall work and manages the region in general. The local government head serves as the vice secretary, who is in charge of administrative affairs. The secretary of the Political and Legal Affairs Committee (*Zheng Fa Wei*) – the party department that leads the local public security bureau, procuracy, and court – is usually also a member of the local standing committee. Thus, the president of the court ranks far below the party secretary and government head of the region, which makes it almost impossible for the judicial system to effectively restrict administrative power.

2.2. Administrative Litigation in China

The Organic Law of the People's Courts classifies the lawsuits handled by Chinese courts into three categories based on the type of litigants and causes of cases: criminal, civil, and administrative litigation. We focus here on administrative litigation, in which individuals or firms sue governments for inappropriate administrative actions (or inactions).[8] The defendants in administrative litigation cases can either be entire local governments (e.g., the government of county *A* or township *B*) or local government departments (e.g., the Bureau of Public Finance of county *A*). The target of administrative litigation depends on which government agency takes the inappropriate administrative action (or fails to undertake the due obligation), which could be further traced to legal provisions about the responsibilities of administrative organs. Since county governments are legally prohibited from delegating some functions to subordinate departments (e.g., issuance of licenses, administrative arbitration), citizens who want to file a lawsuit concerning such issues must sue the entire county-level government. An administrative lawsuit against the local government should be submitted to the local court at the same level of territorial administration: lawsuits against prefectural governments should be filed to prefectural courts, and those targeting county or sub-county governments should be submitted to county courts (since there are no township- or village-level courts).

The passage of the Administrative Litigation Law in 1989 (hereafter, the 1989 Law) signaled the establishment of an administrative litigation system in China, which came into effect in October 1990. This legislation established a judicial channel through which Chinese individuals and firms can protect their interests against potential infringement by local governments at all levels. Given China's long tradition of centralized government power, it was a significant move toward establishing the rule of law to allow judicial institutions, rather than superior governments or intra-party

[4] However, circuit courts usually handle appeals or special types of cases (e.g., intellectual property rights), while the trans-regional jurisdiction in our analysis covers all administrative litigation cases of first instances. See Lerner (2009),Galasso and Schankerman (2010), and Atkinson et al. (2009) for comprehensive introductions of the Court of Appeals for the Federal Circuit in the U.S. as an example.

[5] Kahn and Li (2020) also use the sample of nearly all judicial documents released since 2014 to examine the effect of daily temperature on court judges' productivity in handling cases. Besides, some recent studies rely on randomized or selective samples of judicial documents to examine judicial independence in China (e.g., Chang et al., 2019; Zhou et al., 2021).

[6] There are no separate courts for townships; the courts of superior counties handle lawsuits involving township-level governments. Our sample excludes special courts responsible for handling cases in particular fields such as maritime affairs or intellectual property rights issues because they are small in number and are rarely involved in administrative litigation.

[7] Counties in China vary greatly in population size, economic conditions, and geographic area. According to *China County Statistical Yearbook*, a county on average had a population of 480,000, a GDP of 16 billion RMB, and an area of 4,300 square kilometers as of 2013.

[8] Rigorously speaking, non-enterprise entities like civil organizations could also launch administrative litigation as plaintiffs, while such cases are quite rare in our sample. So throughout the paper, for simplicity, we mainly use "individuals and firms" to refer to plaintiffs in administrative cases.

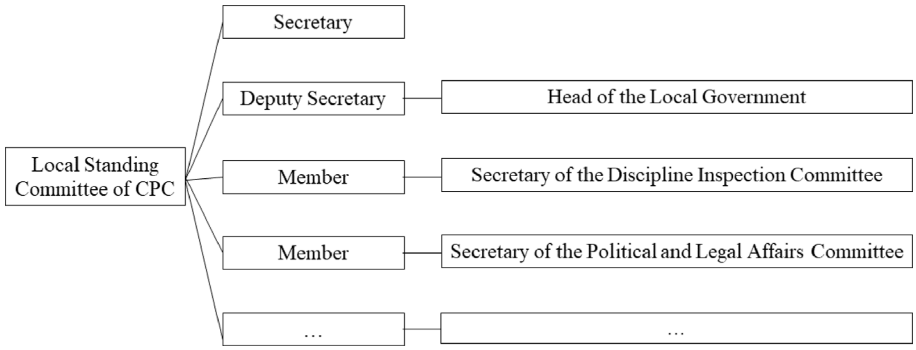

Fig. 1. Organization and positions in the local CPC Standing Committee. *Note*: This figure depicts an example of the composition and division of the CPC local standing committee. The last column displays the additional posts held by members of the standing committee.

organs, to review administrative agencies' decisions (He, 2018). Since the government rarely approves applications for permits to demonstrate or protest, both scholars and the public expected the 1989 Law to constrain the abusive use of administrative power (Li, 2013; Cui, 2017).[9]

Despite such high expectations, the enforcement of the 1989 Law has been far from satisfactory. Plaintiffs face three major problems. First, it is not easy to register administrative cases in courts. Second, it is difficult to get cases adjudicated efficiently and fairly. Third, even with favorable judgments, the execution process is still beset with difficulties. (He, 2018; Administrative Division of the Supreme People's Court, 2018). In addition to the weak position of the court system outlined above, the poor enforcement of the 1989 Law has been exacerbated by the principle of "intra-regional jurisdiction," which stipulates that "[a]n administrative case shall be under the jurisdiction of the people's court at the place where the administrative agency taking the original administrative action is located." For instance, if a farmer wanted to sue the government of county *A* for under-compensating him for land it requisitioned, the bill of complaint must be filed to the court of county *A*. Given that the local judicial system ranks below the local government politically, it is understandably hard for local judges to adjudicate administrative cases independently.

2.3. The trans-regional jurisdiction reform

While the SPC has taken multiple measures to address these widespread complaints about the enforcement of the 1989 Law, it has achieved only limited success. In 2007, it issued a judicial interpretation which stipulated that the superior prefectural court could either adjudicate administrative cases originally registered at the county level on its own (referred to as "higher-level jurisdiction") or assign them to another subordinate county court in the same prefecture (called "assigned jurisdiction"). These two optional arrangements were designed to decouple local courts from governments in administrative litigation, and they applied only under special circumstances, such as cases causing a profound social impact or attracting extensive attention. The overall administrative litigation system was left virtually unchanged.

The turning point came in 2014, when the Standing Committee of the National People's Congress passed the Amendment to the Administrative Litigation Law (the 2014 Amendment, hereafter). This amendment constituted a major revision of the 1989 Law: the revised law was composed of 103 articles, 33 of which were new; 45 were substantially revised, and only 25 were preserved from the original 1989 Law. One of the most important changes was the new rule for assigning courts' jurisdiction over cases. This rule, codified in Article 18, shifted from the original principle of intra-regional jurisdiction to trans-regional jurisdiction in an effort to curb administrative interference from local governments. Under this new procedure, county courts could handle administrative lawsuits against governments of other counties in the same prefecture. This separation between the origin and adjudication of cases was known as the TRJ reform.

The TRJ reform was implemented in three steps. First, provincial courts selected some subordinate prefectures as pilot regions and authorized the corresponding prefectural courts to draft reform proposals. Prefectural courts then designed reform schemes and submitted them to provincial courts for ratification. After being approved by provincial courts, prefectural courts formally issued a detailed reform plan and instructed subordinate county courts to implement it. County courts are thus the ultimate executors of the reform; therefore our analysis is restricted to cases handled by county courts.

In the second step, prefectural courts had stipulated the correspondence between the origin counties of lawsuits and jurisdictional courts (i.e., which county court handles cases against which county government) since the start of the TRJ reform.[10] In other words, the reform pre-specified a mapping relationship between defendant governments and jurisdictional courts at the county level. This makes it impossible for prefectural politicians to endogenously assign different cases to different courts according to case-level characteristics. The pre-specified correspondence has remained largely unchanged since the reform was initiated. Even where there was a change in defendant-to-court mapping relations in some prefectures, it was simply a one-time adjustment at the county level and remained independent of case-level characteristics.[11]

The modes of implementing the TRJ reform varied across regions (see Fig. 2). We assume a prefecture composed of four counties (*A* to *D*) and arrows denote rights of jurisdiction. For example, an arrow from *B* to *A* indicates that a case against county government *B* would be handled by the court of county *A*. Fig. 2a displays the pre-reform arrangement, in which all arrows are

[9] Though litigation costs may sometimes discourage plaintiffs from filing lawsuits, this is not the case for administrative litigation in China. According to the Regulation on the Payment of Legal Fares legislated by China's State Council in 2007 (accessible at http://www.gov.cn/ziliao/flfg/2006-12/29/content_483682.htm), the legal fees associated with administrative cases are either: (i) 100 RMB (about US$14) for cases related to trademarks, patents, and maritime affairs or (ii) 50 RMB for other administrative cases. The TRJ reform did not change these fees.

[10] See the Notice on Carrying Out the Pilot Work of the Relatively Centralized Jurisdiction of Administrative Cases issued by the SPC in 2013, accessible at https://www.court.gov.cn/zixun-xiangqing-5012.html.

[11] Such changes were generally made for practical reasons unrelated to administrative litigation. For example, a county court may have been chosen to serve as a special court for environmental cases and thus no longer handles administrative cases. Then the prefectural court has to partly modify the defendant-to-court relations at the county level. Our baseline results remain highly robust if we remove counties that experienced such a change from the regression sample, as reported in Appendix Table B2.

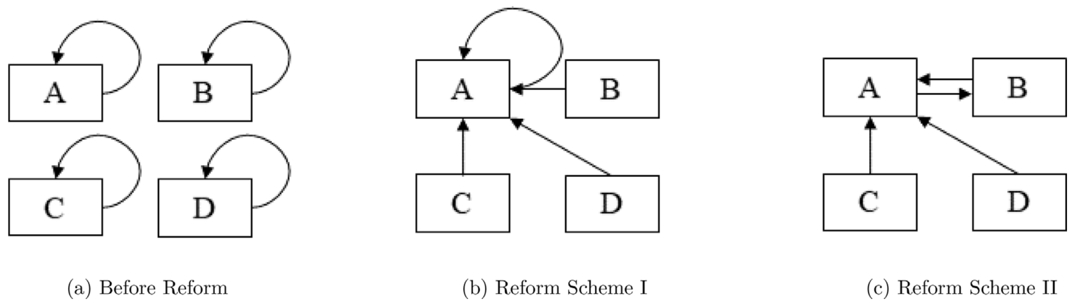

Fig. 2. Graphic illustration of typical reform schemes. *Note*: This figure illustrates typical schemes of the TRJ reform. This example depicts a prefecture consisting of four counties, *A-D*. The arrows denote the right of jurisdiction. For instance, an arrow from *B* to *A* means that a case against the government of county *B* would be heard by the court of county *A*. Fig. 2a displays the pre-reform arrangement–the default rule of intra-regional jurisdiction. Figs. 2b and 2c demonstrate two typical reform arrangements, in which trans-regional jurisdiction is exemplified by the straight arrows connecting different boxes.

self-tapped and stand for the default state of intra-regional jurisdiction. Figs. 2b and 2c demonstrate two typical arrangements after the TRJ reform. In Fig. 2b, all cases against the governments of counties *A*–*D* would be handled by the court of county *A*, which is referred to as the "centralized court." In Fig. 2c, cases against the governments of counties *B*, *C*, and *D* would be transferred to the centralized court of county *A*, and cases against county government *A* would be transferred to the court of county *B*.

The institutional features embedded in specific reform arrangements allow us to conduct two sets of empirical analyses. First, since the TRJ reform was only one of multiple reform components of the 2014 Amendment, the reform scheme exemplified by Fig. 2b helps us distinguish the impact of the TRJ reform from that of other components of the amendment. Fig. 2b depicts two types of defendant governments under such a situation: (i) "genuine-reform" counties experiencing the separation between origin and adjudication (counties *B*, *C*, and *D*) and (ii) "pseudo-reform" counties that have centralized courts (county *A*). We refer to the latter type as "pseudo-reform" counties since they were subject to the entire judicial reform introduced in the 2014 Amendment except for the genuine separation through which the TRJ reform would take effect. This difference helps us construct a placebo test in Section 5.4. Second, some reform schemes may unevenly distribute county courts' workload. In Fig. 2c, the number of cases handled by the court of county *B* should remain relatively stable (one jurisdictional court for one defendant government). By contrast, all administrative cases from the other three counties are assigned to the centralized court in county *A*, which would naturally increase the workload (one jurisdictional court for three defendant governments). The judicial manpower in each county court could not expand at the same speed because there is an explicit quota of officials for all public sectors in China. We utilize this difference to further analyze potential side effects of the reform in Section 6.2.

3. Data and sample construction

3.1. Judgment documents in China

The SPC of China has required all local courts to upload judicial decisions to a publicly accessible website (China Judgments Online) since January 2014, which greatly enhanced our ability to research judicial independence.[12] The documents available on the website provide detailed information of plaintiffs, defendants, judges, courts, facts of cases, judgment decisions, as well as dates of prosecution, judgment, and online publication. As of November 2021, 125 million judgment documents had been made publicly available.

We obtained all available documents on administrative litigation as of September 2018, and refined the sample in four steps. First, as introduced in Section 2.3, we restricted the sample to documents uploaded by county courts, since they are the final executors of the TRJ reform. Thus, we excluded cases in which the defendants are governments above the county level because they are not handled by county courts. Second, following prior studies that analyze this dataset (e.g., Kahn and Li, 2020), we retained only first-instance cases because there are extra and complex stipulations for second instances or retrials, which may introduce unnecessary complications.[13] Third, we dropped counties in two provinces (Shandong and Gansu) and five prefectures (Longyan in Fujian, Guiyang in Guizhou, Ningbo, Taizhou, and Lishui in Zhejiang) because these regions adopted special reform schemes and are thus not comparable to other counties.[14] Finally, judicial documents uploaded by local courts can be roughly categorized into two types: *judgment documents*, which contain judicial analyses of cases and trial decisions on plaintiffs' claims, and *ruling documents*, which report judges' decisions on procedural issues without substantively examining cases. The majority of ruling documents are notices regarding problems in the case filing (e.g., the plaintiff did not provide enough information about the defendant, or the charge was not properly stated). We focus on the former type to investigate the effects of the TRJ reform on substantial judgments.

We generate three sets of variables of interest from the raw judgment documents. First, we extract basic information on each case, including the names of plaintiff(s), defendant(s), and judge (s); the jurisdictional court; and the dates of prosecution, judgment, and publication. Second, we utilize textual analysis tools to capture the sentences declaring whether the plaintiff or defendant is responsible for paying the legal fee to identify the losing party of the lawsuit.[15] Third, we compute the total number of words in each document, a commonly used measure in the literature to gauge

[12] See the Provisions on the Issuance of Judgments on the Internet by the People's Courts published by the SPC in 2014, accessible at https://wenshu.court.gov.cn.

[13] Although our regression sample does not include second-instance cases or retrials, we use those judgment documents to determine whether a first-instance judgment was appealed and whether the original judgment was reversed (for appeals). See Section 6.2 for more details.

[14] For instance, the reform arrangement in Shandong Province did not explicitly define the defendant-to-court relations after the reform. Instead, plaintiffs could choose to file the case in either intra-regional or trans-regional courts, which makes it difficult to determine the treatment status.

[15] According to the Regulation on the Payment of Legal Fees, the legal fees should be paid by the losing party unless the winning party volunteers to bear the cost. The plaintiff and defendant shared the legal fees in 319 cases, which indicates a hybrid judgment that partly supported the plaintiff's claims. In the baseline setting, we adopt a strict criterion and consider the government to be the losing party only if it is required to pay all legal fees. We also experiment with (i) dropping the 319 cases with fee sharing and (ii) redefining the government as the losing party if it is required to pay all or some portion of the legal fees. Both alternative definitions generate highly similar results, given the tiny sample size of the fee-sharing cases. See Appendix Table A2 for details.

readability and complexity.[16] Appendix A describes the processing of judgment documents and variable definitions in more detail.

We use the first set of information to further classify plaintiffs as individuals or firms based on their names. We also categorize defendants into local governments as a whole (e.g., the government of county *A* or township *B*) or departments of local governments (e.g., the Bureau of Public Finance of county *A*). We then use the case-level information to construct aggregated measures of administrative litigation activities at the county-year and court-year levels, such as the total number of cases (in logarithm), the proportion of cases that governments lose, and the disclosure rate of judgment documents.

3.2. County-specific timing of the reform

In 2018, the SPC released a book entitled *Practice and Exploration of Administrative Litigation Reform on Trans-Regional Jurisdiction* to introduce and summarize the general design, practice, and shortcomings of the reform. For most counties, this book documents the specific calendar dates of the TRJ reform as well as which county court handles cases against which county government. We digitized all of these records to identify county-specific timings of the reform and the mapping relations between defendant governments and jurisdictional courts. We also collected information from official documents disclosed by local courts and media reports in local newspapers, which we employed to cross-validate the data from the review book and supplement missing information. Thus, for each county c and year t, we know exactly which court has jurisdiction over administrative cases against the government of county c (including its subordinates and departments). Based on this information, we assign the status of county c in year t as treated if and only if cases against the government of county c would be handled by the court of another county in the same prefecture.

Fig. 3 displays the progress of the reform implementation. The bars denote the number of counties that started to reform in corresponding periods, and the dashed line represents the ratio of all counties that have already completed the reform. We find that pioneer counties began to reform as early as late 2013, and the period of 2015–2016 exhibited rapid progress. As of 2018, over 40% of the counties in our sample had implemented the TRJ reform.

3.3. County-level attributes

To control for potentially heterogeneous trends exhibited by different counties, we collected data on three sets of cross-sectional attributes, which are time-invariant and predetermined to our sample period to avoid the "bad control" problem (Angrist and Pischke, 2009).

First, regional economic development and population size may have a substantial influence on judicial activities. Inspired by recent progress in the utilization of remotely sensed data (Henderson et al., 2012; Li et al., 2016), we employed raster data of nightlight intensity in 2012 and population density in 2010 to compute mean values for each county, which constitute imperfect but informative measures of local economic activities and population size. The nighttime light intensity data is released by the US Air Force Defense Meteorological Satellite Program's Operational Linescan System (DMSP-OLS), and population density data is retrieved from the Worldpop project.[17] We combined these data with county boundaries to compute county-level averages.

The second set of attributes, as suggested by previous studies (Arnold et al., 2018; Kleinberg et al., 2018), relates to transportation costs, which may have a profound impact on citizens' willingness to file cases. Since regional transportation costs largely depend on geographic topography, we combined county boundaries with the Digital Elevation Model from the 1:1 Million Scale National Basic Geographic Database,[18] and utilized ArcGIS software to compute county-level average altitude, average slope, and the distance between the centroids of each county and its superior prefecture.

Third, we gathered data on county governments' administrative characteristics. Counties in China are classified into multiple types, such as ordinary counties, districts of prefectural cities (DPCs, i.e., *Shixiaqu*), and county-level cities (CLC, i.e., *Xianjishi*). This is highly relevant for our analysis since the governments of different types of counties may act differently (Li et al., 2016). For instance, administrative agencies in CLCs are generally more autonomous and more powerful than their counterparts in ordinary counties and DPCs.[19] We obtained this information from the Ministry of Civil Affairs[20] and constructed two dummy variables denoting governments of DPCs and CLCs.

In summary, we constructed a unique and comprehensive dataset consisting of 62,392 administrative litigation cases against governments in 2,000 counties during 2013–2018.[21] Table 1 reports the descriptive statistics of the main variables, and Appendix Table A1 summarizes the granularity, sample periods, and data sources of all variables used in this paper.[22]

4. Empirical strategy

4.1. Estimation framework

To identify the effects of the TRJ reform, we exploit its staggered implementation to construct a DID model. The benchmark specification is as follows:

$$Y_{ict} = \beta PostReform_{ct} + \mathbf{Controls_i} + \alpha_c + \gamma_t + \varepsilon_{ict}, \qquad (1)$$

where i, c, and t indicate case, county, and year, respectively. For each case i adjudicated in year t that is filed against the government of county c (including departments or subordinate townships), the dummy variable Y_{ict} denotes whether the government loses. The dummy variable $PostReform_{ct}$ indicates whether the jurisdictional court locates in a county different from c, according to the adjusted jurisdiction rule. $\mathbf{Controls_i}$ represents case-level control variables, including the logarithmic total number of words, and dummies

[16] Compared with alternative measures based on the pronunciation or uncommonness of words, the total length of documents is quite easy to construct and is robust to different language settings. Therefore it is commonly used in some fast-growing strands of literature, such as studies on the readability of financial reports (e.g., Li, 2008; Guay et al., 2016).

[17] https://www.worldpop.org/geodata/summary?id=4034.

[18] https://www.webmap.cn/commres.do?method=result100W.

[19] Our paper differs from most county-level studies of China in the sample composition. We included DPCs in the analysis because they are an indispensable part of the TRJ reform. However, previous studies have dropped DPCs because they are not covered in the statistical yearbooks published by the official bureaus of statistics. The cost of expanding to DPCs is that we could not utilize the commonly used county attributes extracted from published yearbooks and have to construct those measures ourselves.

[20] http://www.mca.gov.cn/article/sj/xzqh/1980.

[21] Our study period refers to the time span of judgment dates of cases in our baseline sample. Since the website was launched in 2014, the timing of the document disclosure should be no earlier than 2014. However, there may be time lags between the judgment and disclosure, so the cases disclosed in 2014 may have been adjudicated in 2013. Therefore, the start of our sample period is slightly earlier than the website's launch.

[22] As introduced in Section 6.3, we also constructed several sets of auxiliary variables. Appendix Table A3 reports their summary statistics.

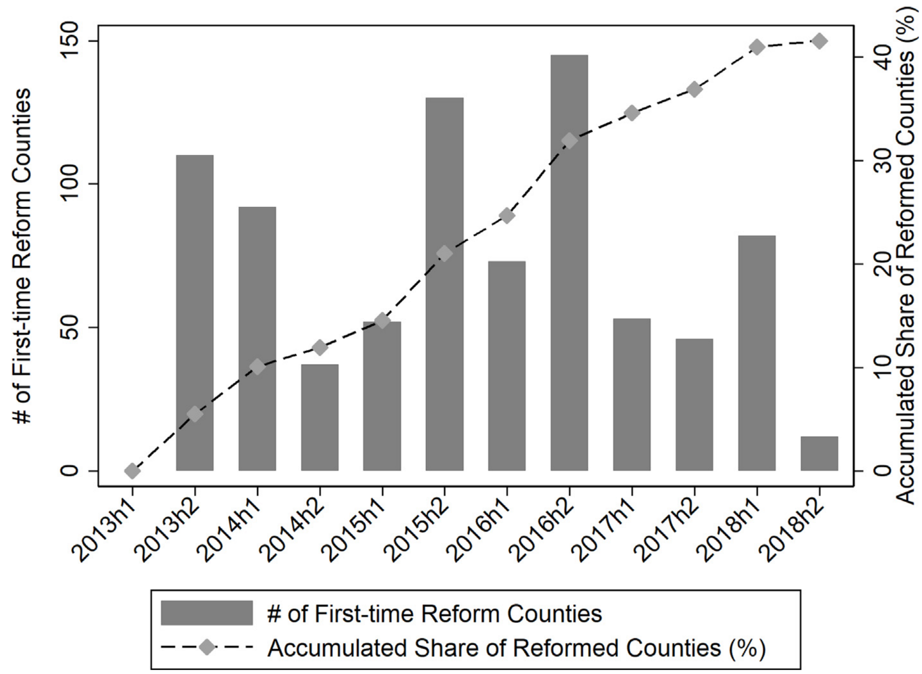

Fig. 3. Implementation of the TRJ Reform. *Note*: This figure reports the implementation process of the TRJ reform. The left *y*-axis represents the incremental number of counties that started to undertake the TRJ reform, while the right *y*-axis represents the accumulated share of counties that had ever undertaken the reform. The *x*-axis represents the timeline, where *h*1 and *h*2 stand for the first and second halves of the year, respectively.

for types of plaintiffs and defendants.[23] α_c and γ_t are county and year fixed effects. ε_{ict} is the error term.

β is the coefficient of interest, which reflects whether (and how) the reform has affected local governments' probability of losing (or equivalently, the plaintiffs' probability of winning) in administrative litigation. To address concerns about potential serial correlation and heteroskedasticity, we cluster standard errors at the county level. We refer to counties that implemented the reform during the study period as the treatment group, and others as the control group.

4.2. Identifying assumption

The DID estimator of β could be interpreted as the causal effect of the TRJ reform if necessary identifying assumptions hold, the most important of which is the parallel trend condition. Our baseline analysis requires that the difference in county governments' probability of losing between the treatment and control groups should be constant over time in the absence of the reform.[24]

A primary threat to this identifying assumption is that the treatment counties were not chosen randomly: the provincial courts selected the pilot prefectures, and the arrangements of county courts within pilot prefectures were also purposefully made. To address this concern, we perform a balance check on pre-existing differences between the treatment and control groups before the reform, following the common practice in the literature (Gentzkow, 2006; Agarwal and Qian, 2014; Li et al., 2016). Panel A of Table 2 reports the comparison of county-level socioeconomic and geographic attributes, including light intensity, population density, average altitude, average slope, distance to the centroid of the prefecture, and administration type. It is clear that the treatment and control groups are not identical in many dimensions: counties in the treatment group generally have significantly higher light intensity, larger population density, and greater average slope, and are less likely to be CLCs.

Despite the differences between the treatment and control groups, however, we find no evidence that administrative litigation exhibited systematic differences in terms of case volume or governments' probability of losing, as shown in Column 3 of Panel B.[25] Taken together, although some pre-existing characteristics seem to have predictive power over the treatment status, the differences may not necessarily lead to systematic discrepancies in potential outcomes between the treatment and control groups.[26]

Nonetheless, we propose an augmented specification to rigorously control for the endogenous chronological evolution of the

[23] To address the possibility that judgment length could also be considered an outcome variable since the TRJ reform may have affected how judges write their judgments, we have used judgment length as the outcome variable in the baseline setting specified below, but failed to find a significant effect (see Column 3 of Appendix Table A2). A possible explanation is that because administrative cases generally involve disputes over administrative procedures and are usually less complicated than civil or criminal cases, there is limited room for judges' discretion when drafting judgment documents.

[24] Recent advances in econometrics indicate that the TWFE-DID estimator could still be biased even if the classic identifying assumptions hold (Baker et al., 2022; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). To address this concern, we conduct several relevant checks on the robustness of our DID estimates in Section 5.3 and discuss the issue in detail in Appendix C.

[25] Since the sample period is 2013–2018 and the reform started in 2014, we only have data for 1 year (2013) to compute pre-treatment measures, which is vulnerable to annual shocks or noises. Alternatively, we average the numbers for 2013 and 2014 to smooth potential annual fluctuation at the cost of bringing in some post-treatment values. However, as indicated in Fig. 3, few counties started implementing the reform in 2014, which should have little impact on the balance test here.

[26] We have also conducted a balance test of initial values of outcome variables regarding the broader social impact (see Section 6.3). As shown in Appendix Table A4, after controlling for predetermined regional characteristics, those variables exhibit few differences as well.

Table 1
Summary statistics.

Panel A: Types of defendants and plaintiffs	Pre-reform		Post-reform		Overall	
	Num.	Pct.	Num.	Pct.	Num.	Pct.
Defendants						
Governments (county)	5,413	12.49	1,804	9.48	7,217	11.57
Governments (sub-county)	5,807	13.39	3,071	16.13	8,878	14.23
Police and public security	8,153	18.81	3,007	15.80	11,160	17.89
Health and birth control	354	0.82	104	0.55	458	0.73
Land and real estate	6,543	15.09	3,162	16.61	9,705	15.55
Market and environment supervision	2,554	5.89	1,260	6.62	3,814	6.11
Medical insurance and pension	6,648	15.33	3,194	16.78	9,842	15.77
Transportation	1,042	2.40	450	2.36	1,492	2.39
Multiple defendants in different types	3,398	7.84	1,599	8.40	4,997	8.01
Others	3,443	7.94	1,386	7.28	4,829	7.74
Total	43,355	100.00	19,037	100.00	62,392	100.00
Plaintiffs						
Individual	36,538	84.28	15,676	82.34	52,214	83.69
Enterprise	6,260	14.44	3,168	16.64	9,428	15.11
Multiple plaintiffs in different types	120	0.28	24	0.13	144	0.23
Others	437	1.01	169	0.89	606	0.97
Total	43,355	100.00	19,037	100.00	62,392	100.00
Panel B: Other variables						
	Mean	S.D.	Median	Min	Max	Obs.
Outcome variables						
Government losing dummy	0.41	0.49	0.00	0.00	1.00	62,392
Case appealed dummy	0.32	0.47	0.00	0.00	1.00	62,392
Judgment reversed on appeal dummy	0.11	0.32	0.00	0.00	1.00	62,392
Trial time (# of days, in log)	4.56	0.55	4.52	3.04	6.78	62,392
Disclosure rate	0.52	0.26	0.51	0.08	1.00	5,740
Case volume (# of cases, in log)	1.49	1.01	1.39	0.00	6.10	9,460
Treatment variables						
Post-reform dummy	0.31	0.46	0.00	0.00	1.00	62,392
Post-pseudo-reform dummy	0.02	0.13	0.00	0.00	1.00	62,392
Centralized court dummy	0.25	0.44	0.00	0.00	1.00	62,392
Control variables						
Document length (# of characters, in log)	8.34	0.47	8.34	6.46	9.89	62,392
Light intensity (in log)	2.18	1.11	2.13	0.00	4.16	62,392
Population density (100 persons/km^2, in log)	1.63	1.01	1.49	0.00	5.45	62,392
Average altitude (meters, in log)	4.86	1.68	5.11	0.09	8.48	62,392
Average slope (degrees)	9.51	6.12	8.07	0.85	33.78	62,392
Distance to prefecture centroid (kilometers, in log)	3.57	0.70	3.67	0.00	5.98	62,392
County-level city dummy	0.22	0.42	0.00	0.00	1.00	62,392
District of prefectural cities dummy	0.33	0.47	0.00	0.00	1.00	62,392
Sub-provincial city dummy	0.09	0.29	0.00	0.00	1.00	2,248
Prefecture-level city dummy	0.88	0.33	1.00	0.00	1.00	2,248
GDP per capita (RMB, in log)	10.44	0.57	10.38	8.95	12.10	2,248

Note: This table reports summary statistics of the main variables used in the paper. In Panel A, cases are classified by the type of defendants/plaintiffs, for before/after reform as well as the entire sample period, respectively. In administrative litigation, defendants include county governments and their subordinates or departments, while plaintiffs are mainly individuals and firms. Note that "Multiple defendants (plaintiffs) in different types" denotes cases with more than one type of defendants (plaintiffs), and "Others" denotes defendants (plaintiffs) that cannot be classified into the listed types. Panel B summarizes the outcome, treatment, and control variables, respectively. See Appendix A for detailed definitions and an explanation of the data cleaning process.

outcome variable that could potentially be caused by these pre-existing differences:

$$Y_{ict} = \beta PostReform_{ct} + \psi Treat_c \cdot \tau_t + \mathbf{S_c} \times \gamma_t + \mathbf{Controls_i} + \alpha_c + \gamma_t + \varepsilon_{ict}. \quad (2)$$

Eq. (2) makes two modifications to Eq. (1). First, $Treat_c$ is a dummy indicating that county c belongs to the treatment group, and $\tau = t - t_0$ is a linear time trend indicating the number of relative years between year t and the first year of our sample period ($t_0 = 2013$). Thus, the interaction term $Treat_c \cdot \tau$ captures the linear trend specific to counties in the treatment group. Second, $\mathbf{S_c}$ denotes the predetermined geographic and socioeconomic characteristics of county c reported in Panel A of Table 2. We include $\mathbf{S_c} \times \gamma_t$ to control for time-variant shocks that are heterogeneous to those predetermined attributes. With these control terms added, the key identifying assumption is further relaxed to the *conditional* parallel trend, which is more likely to be fulfilled in our empirical setting (Abadie, 2005; Heckman et al., 1997).

Finally, we propose the specification of event study based on Eq. (2), which could serve as a preliminary test of the conditional parallel trend assumption:

$$Y_{ict} = \sum_{k=-6}^{18} \beta_k \cdot D_{ick} + \psi Treat_c \cdot \tau_t + \mathbf{S_c} \times \gamma_t + \mathbf{Controls_i} + \alpha_c + \gamma_t + \varepsilon_{ict}, \quad (3)$$

where D_{ick} denotes a set of dummies indicating whether the judgment date of case i is in the kth period relative to the reform date of county c, where each period lasts for 60 days and $k = -1$ is omitted as the benchmark to guarantee feasible estimation. Estimates of $\{\beta_k\}_{k<0}$ allow us to detect whether the outcome variable of treatment counties varies similarly to that of control counties in the pre-treatment period as early as 1 year ($60 \times 6 = 360$ days) before

Table 2
Balance checks on predetermined county-level attributes.

Variable	Control	Treatment	Difference
	(1)	(2)	(3)
Panel A: Geographical and sociological status			
Light intensity (in log)	1.827	1.966	0.139***
	(1.077)	(1.115)	(0.050)
Population density (100 persons/km^2, in log)	1.340	1.437	0.096**
	(0.951)	(1.015)	(0.044)
Average altitude (meters, in log)	5.377	5.458	0.081
	(1.525)	(1.634)	(0.071)
Average slope (degrees)	10.151	11.478	1.327***
	(6.562)	(6.293)	(0.292)
Distance to prefecture centroid (kilometers, in log)	3.716	3.664	-0.053
	(0.776)	(0.724)	(0.034)
County-level city dummy	0.165	0.129	-0.036**
	(0.371)	(0.335)	(0.016)
District of prefectural cities dummy	0.313	0.318	0.005
	(0.464)	(0.466)	(0.021)
Panel B: Administrative litigations in initial years (2013–2014)			
Initial case volume (# of cases, in log)	1.396	1.436	0.040
	(1.064)	(1.108)	(0.049)
Initial proportion of government losing cases	0.422	0.405	-0.017
	(0.361)	(0.350)	(0.018)
Observations	1,163	837	2,000

Note: This table reports the results of the balance checks on predetermined county-level attributes between the treatment and control groups. These traits could be divided into two categories: one is geographic and socioeconomic status ($\mathbf{S_c}$), and the other is administrative litigation activities in initial years. They are reported in Panels A and B, respectively. Columns 1 and 2 show the means and standard deviations (in parentheses) of the corresponding variables. Column 3 displays the differences between the treatment and control groups, with standard errors in parentheses. *** denotes significance at 1%, ** at 5%, and * at 10%.

the reform, and estimates of $\{\beta_k\}_{k \geqslant 0}$ further depict the dynamic effect in the 3 years ($60 \times 19 = 1,140$ days) after the reform.

5. Empirical results

In this section, we begin by probing the impact of the TRJ reform on government losing rates in administrative cases, which reflects whether improved judicial independence helps protect the legal rights of both individuals and firms. Next, we conduct an event study to empirically test the conditional parallel trend assumption. Then a full battery of robustness checks and placebo tests on the baseline results will be performed. The section concludes with a detailed discussion of the selective disclosure of judgment documents.

5.1. Baseline results

Table 3 reports the baseline results. In Column 1, we begin with the simplest and most straightforward specification shown in Eq. (1). Column 2 further includes a linear trend for counties in the treatment group, and thus controls for heterogeneous trends of counties with different reform statuses. To account for differences between the treatment and control groups that are observed in the balance checks, we add the interactions between county attributes and year dummies in Column 3. The results in the first three columns of Table 3 consistently demonstrate that the TRJ reform has made governments more likely to lose administrative cases. The estimates of the coefficient β are relatively stable, ranging from 0.035 to 0.042, and are all statistically significant at the 5% level. The preferred specification in Column 3, which includes a full set of controls, implies that the reform has increased governments' probability of losing by about 3.9 percentage points. Given that governments lose in 41% cases in the full sample, this estimate means that the TRJ reform gives plaintiffs a roughly 9.5% (≈3.9/41) greater chance of winning, which reflects the economic significance of the reform effect.

We have also experimented with three alternative econometric settings. First, although county courts are the final executors of the TRJ reform, the overall plan was designed and coordinated at the prefecture level. To account for possible correlations among the error terms of counties in the same prefecture, we switch the clustering units of standard errors from counties to prefectures in Column 4. The estimate is still statistically significant at the 10% level. Second, as discussed in Section 4.2, some county-level attributes seem to affect which counties were selected to carry out the reform. To alleviate concerns about the comparability between different counties, we restrict the sample to counties that ultimately implemented the reform and re-estimate Eq. (2). This estimation narrows the DID model to a pure before/after comparison within the treatment group, which is immune to potential discrepancies between counties caused by the selection process. Column 5 of Table 3 shows that, although the number of observations shrinks by almost half, the estimate of β remains stable in both statistical significance and magnitude. Third, we estimate Eq. (2) after performing propensity score matching based on predetermined characteristics $\mathbf{S_c}$. The corresponding result reported in Column 6 also remains robust.

5.2. Event study

Fig. 4 depicts the three main results of the event study as specified in Eq. (3). First, the estimates of $\{\beta_k\}$ with k ranging from -6 to -2 are all statistically insignificant and exhibit no obvious trend. Recall that each bin represents 60 days. Thus the graph suggests that in a 1-year time window ($60 \times 6 = 360$ days), counties in the treatment and control groups exhibit a similar pattern in governments' probability of losing, which is critical for the credibility of our DID strategy.[27] Second, we observe no significant effect until 8 months ($60 \times 4 = 240$ days) after the reform, which implies that it takes time to implement such a comprehensive reform and produce a profound impact. Third, the reform's effect remains significant even 3 years later ($60 \times 19 = 1,140$ days). This persistence reflects the effectiveness of the institutional changes: rearranging defendant-to-court relations from intra-regional to trans-regional has significantly alleviated governments' interference in administrative litigation.

5.3. Robustness checks

In this subsection, we conduct five robustness checks of the DID strategy. Table 4 and Appendix Table C1 report the estimation results.

First, we explore the potential impact of measurement errors derived from cases adjudicated during the transitional period of the TRJ reform. In the baseline analysis, the treatment variable was defined by comparing the timing of case judgment and reform implementation: *PostReform* takes a value of 1 if a case is judged after the reform. As shown in Fig. 5, *PostReform* = 1 holds for both cases 2 and 3. The concern comes from the potential ambiguity

[27] We have also plotted the trend of raw data to double-check whether treatment and control counties exhibit different trends before the TRJ reform. We restrict the sample to cases adjudicated during 2013–2014, the initial years of our study period when few counties had implemented the TRJ reform, and further drop the cases that were adjudicated after the reform in a few pilot counties. Then we calculate the mean and standard deviation of government losing rates in treatment and control counties on a quarterly basis, respectively. As shown in Appendix Fig. C1, government losing rates exhibit parallel trends for the treatment and control groups in the initial years of our sample period.

Table 3
Effect on government losing probability.

Dep. Var.	Government losing dummy					
	(1)	(2)	(3)	(4)	(5)	(6)
PostReform	0.035**	0.042**	0.039**	0.039*	0.041**	0.043**
	(0.017)	(0.020)	(0.020)	(0.023)	(0.020)	(0.021)
County fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Case-level controls	YES	YES	YES	YES	YES	YES
Treatment linear trend		YES	YES	YES	YES	YES
County traits×Year dummies			YES	YES	YES	YES
Clustering at prefectures				YES		
Only treatment counties					YES	
P-score matching						YES
Adjusted R^2	0.252	0.252	0.252	0.252	0.273	0.260
Number of clusters	2,000	2,000	2,000	285	824	1,342
Number of observations	62,392	62,392	62,392	62,392	27,365	52,698

Note: This table reports the baseline results. The dependent variable is a dummy indicating whether the government loses in a case. All regressions control for county and year fixed effects, as well as case-level controls including document length (# of Chinese characters in logarithm) and dummies for types of plaintiffs and defendants. In Column 2, treatment-specific linear time trends are included to control for the differences in time trends between the treatment and control groups. In Column 3, we include interactions of predetermined county-level traits $\mathbf{S_c}$ with year dummies to absorb potential discrepancies in the outcome due to county endowments. In Columns 4–6, the regression specification and estimation procedure are further adjusted to demonstrate robustness, including changing the clustering level from county to prefecture, keeping only treatment group counties, and performing a propensity score matching before the regression, respectively. Standard errors are reported in parentheses and clustered at the county level except for Column 4. *** denotes significance at 1%, ** at 5%, and * at 10%.

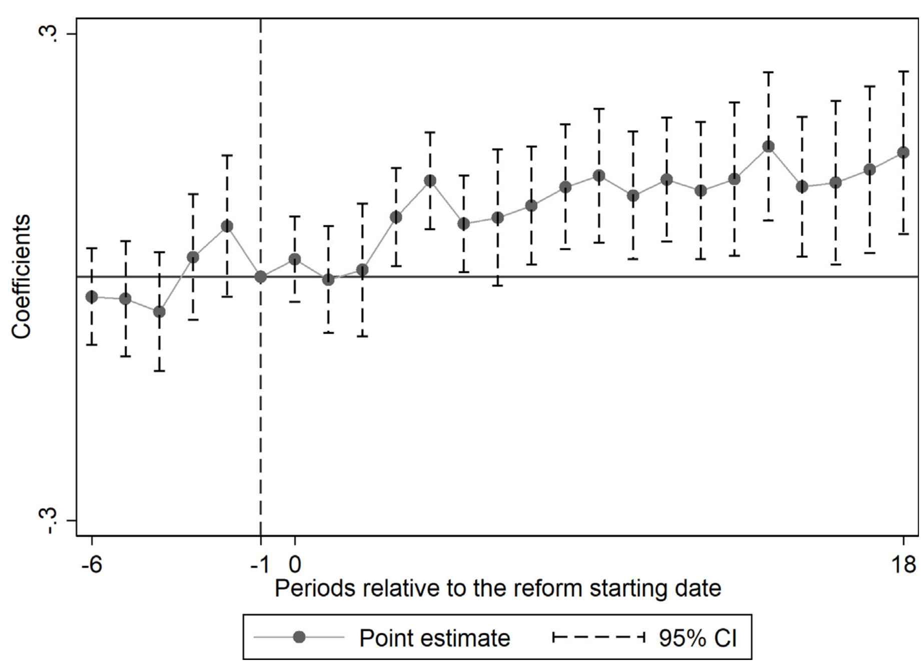

Fig. 4. Dynamic effect of the TRJ Reform. *Note*: This figure portrays estimates from the event study as specified in Eq. (3), with a time window ranging from 1 year before to 3 years after the reform. The x-axis represents the time period relative to the reform date and each bin contains 60 days. $k = -1$ is set as the benchmark period with $\beta_{-1} \equiv 0$.

during the transitional period as exemplified by case 2, which was filed before the reform but adjudicated afterward. To empirically test whether our baseline results are driven by measurement errors derived from the transitional period, we restrict to the subsample for which we have prosecution dates and then drop cases adjudicated during the transitional period (1,100 out of 42,192). The estimation based on this sample, as reported in Column 1 of Table 4, shows a larger and more significant impact than the baseline estimation.

Second, the county-level judicial endowment (e.g., judges' staff sizes) may also be a relevant attribute. To account for this possibility, we have collected lists of judges for all county-level courts from the China Judicial Process Information Online (https://splcgk.court.gov.cn/). For each county, we compute the total number of judges and then standardize it by total population to proxy for the county-level judges' strength. After adding this new variable to the list of predetermined county traits, which are further interacted with year fixed effects in Eq. (2) to account for the yearly differential impact, the result is displayed in Column 2 of Table 4. It reveals that our baseline estimate remains stable in magnitude after taking the judges' strength into consideration. Its statistical significance decreases somewhat, partly due to the missing data of over 500 counties (the size of clusters drops from 2,000 to 1,465). For this reason, we only add this new variable as a county attribute in robustness checks instead of our baseline regressions to avoid a significant loss in sample size.

Third, many empirical legal studies suggest that since judicial decisions are subjectively made by judges instead of robots or algo-

Table 4
Excluding potential confounding factors.

Dep. Var.	Government losing dummy							Misused public funds
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PostReform	0.061***	0.041*	0.048**	0.036*	0.038*	0.034*	0.038*	0.097
	(0.023)	(0.023)	(0.019)	(0.020)	(0.020)	(0.020)	(0.020)	(0.114)
Tenured				0.017		0.014		
				(0.012)		(0.012)		
Consolidated					0.021	0.019		
					(0.014)	(0.015)		
PostPseudoReform							−0.076	
							(0.057)	
County fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Case-level controls	YES	YES	YES	YES	YES	YES	YES	YES
Treatment linear trend	YES	YES	YES	YES	YES	YES	YES	YES
County traits×Year dummies	YES	YES	YES	YES	YES	YES	YES	YES
Defining by prosecution dates	YES							
Controlling county judge number		YES						
Judge fixed effects			YES					
P-value of Coef. Diff.							0.0494	
Mean of Dep. Var.	0.411	0.411	0.413	0.411	0.414	0.411	0.414	8.433
Adjusted R^2	0.271	0.251	0.309	0.254	0.252	0.254	0.252	0.534
Number of clusters	1,806	1,448	1,942	1,980	1,997	1,977	2,000	1,995
Number of observations	41,092	45,759	60,595	59,497	62,367	59,474	62,392	15,815

Note: This table reports the results of empirical exercises designed to exclude potential confounding factors. In Column 1, *PostReform* is redefined according to whether the prosecution date (rather than the judgment date) of the case is later than the reform date, and cases that are filed before but adjudicated after the TRJ reform are removed from the sample to avoid ambiguity. In Column 2, the number of judges in each county is further added to the predetermined traits $\mathbf{S_c}$. Column 3 controls for judge fixed effects. In Columns 4–6, two other reforms that were introduced during the sample period are controlled for, respectively as well as simultaneously. The dummy *Tenured* denotes the personnel reform that provides tenured positions for judges, while the dummy *Consolidated* denotes the consolidation of financial and personnel controls over local courts at the provincial level. In Column 7, we conduct a placebo test by estimating the effect of *pseudo* reform separately. In Column 8, the dependent variable is misused public funds of county governments in logarithmic value during 2010–2017 and we include all controls in the baseline specification except for case-level covariates. Standard errors are reported in parentheses and clustered at the county level in all columns. *** denotes significance at 1%, ** at 5%, and * at 10%.

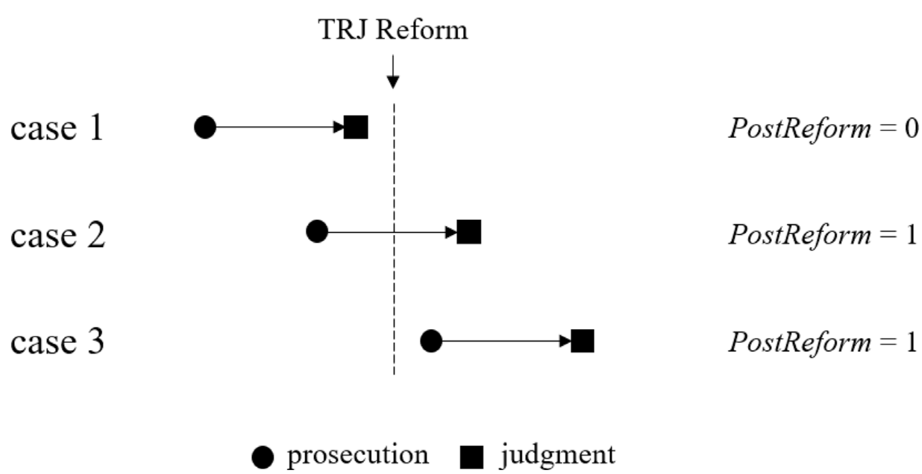

Fig. 5. Graphic illustration of possible timelines around the TRJ Reform. *Note*: This figure illustrates three examples of possible timelines for cases around the TRJ reform taking effect: (i) the case was filed and adjudicated before the reform; (ii) the case was filed before the reform but adjudicated after the reform; and (iii) the case was filed and adjudicated after the reform.

rithms, individual effects are non-negligible (Kleinberg et al., 2018; Eren and Mocan, 2018; Philippe and Ouss, 2018). To address this concern, we control for judge fixed effects to account for time-invariant judge characteristics such as demographics, political views, and work efficiency.[28] As shown in Column 3, the estimate remains significant at the 5% level and its magnitude increases (from 0.039 to 0.048), which enhances our confidence in the baseline results.

[28] Since different judges may share the same name, and the same judge may rotate among different courts, we use both prefecture *and* name to try to uniquely identify different judges.

Fourth, two other judicial reforms occurred in parallel with the TRJ reform that might have enhanced judicial independence and thus confounded our estimation. The first relates to the appointment and removal of judges. In 2014, the SPC introduced personnel ration into the judge management system, which capped judges' quotas and launched the selective retention of sitting judges. The reappointed judges enjoyed tenured positions and could not be removed from their posts unless they voluntarily retired or were found guilty of violating disciplinary standards. Prior research (e.g., Mehmood, 2022) has found that whether judges hold tenured positions is a key indicator of judicial independence. Another related reform starting in 2014 was the consolidation of financial and personnel controls over local courts at the provincial level. It was designed to strengthen the independence of local courts from the influence of same-level sub-provincial governments.[29] Both reforms were implemented in a staggered fashion.

To empirically test whether reforms related to the appointment/removal of judges and courts' fiscal independence are driving our baseline results, we hand-collected related information and constructed two dummy variables, $Tenured_{ct}$ and $Consolidated_{ct}$, indicating whether county c has been engaged in the corresponding reform at time t, respectively. The empirical results reported in Columns 4–6 of Table 4 demonstrate that the coefficients of *PostReform* remain quite stable in magnitude, though decrease slightly in statistical significance, which indicates our baseline results are robust to potentially confounding judicial reforms. By contrast, the estimates on *Tenured* and *Consolidated* are statistically insignificant regardless of whether they are controlled for independently or collectively. This analysis, though not exhaustive, provides additional evidence that our baseline results are mainly driven by the TRJ reform.

Finally, as recent advances in econometric theory have highlighted, the classic DID estimator might be biased if the treatment is staggered and the treatment effect is dynamic (Baker et al., 2022; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). In response to this potential challenge, we conduct robustness checks on the baseline estimation using newly proposed methods. Borrowing the terminologies used in Goodman-Bacon (2021), we classify all units into three groups: the "always-treated" group (units that were treated before the first period), the "timing" group (units that were treated during the sample period), and the "never-treated" group (units that received the treatment). Then the TWFE-DID estimator can be expressed as a weighted average of DID estimators derived from all two-group/two-period (2×2) comparisons including timing group vs. never-treated group, timing group vs. always-treated group, and pre- vs. post-treatment within timing groups. When the assumptions of parallel trend and constant treatment effect do not hold, the latter two types of estimators will be biased, which is sometimes expressed as the "negative weighting problem" (de Chaisemartin and D'Haultfuille, 2020; Borusyak et al., 2021).

To address this concern, we first follow Goodman-Bacon (2021) and decompose the baseline estimator into these 2×2 DID estimators, and find that the comparison between the timing group and never-treated groups yields an average treatment on the treated (ATT) estimate of 0.042 (similar to the baseline estimate, 0.039) and accounts for 79.2% of the aggregated TWFE-DID estimate, as reported in Column 1 of Appendix Table C1. That is to say, our baseline TWFE-DID result is mainly driven by the comparison that is immune to potential violations of assumptions. We also adopt alternative estimation procedures proposed by Callaway and Sant'Anna (2021) and de Chaisemartin and D'Haultfuille (2020), based on either an aggregated county-year panel or cross-sectional case-level data. As reported in Columns 2–5 of Appendix Table C1, the corresponding estimates range from 0.044 to 0.067, all significantly different from zero and larger than the baseline estimate (0.039). Taken together, our baseline result seems unlikely to be seriously biased by the negative weighting problem. Appendix C contains a more detailed discussion of the above checks on the TWFE-DID estimator.

5.4. Placebo test

As elaborated in Section 2.3, some counties experienced the judicial reform induced by the 2014 Amendment but did not separate the origin and adjudication of lawsuits against governments, as exemplified by county *A* in Fig. 2b. This scenario allows us to examine whether the TRJ reform, rather than other components of the 2014 Amendment, is driving our baseline results.

Under the reform scheme specified in Fig. 2b, the court of county *A* is chosen to be the centralized court and is made responsible for handling cases against the governments of counties *A–D* after the reform.[30] As a result, lawsuits against the governments of counties B, C, and D are adjudicated under the rule of trans-regional jurisdiction after the reform, while those against county government *A* experience no change in jurisdiction rule. We refer to the latter situation as "pseudo reform," which might be influenced by all other confounding factors correlated with the 2014 Amendment (e.g., greater attention from superior courts to protecting non-government entities or more respect for legality from government officials after the reform) except for the change in jurisdiction rule.

Accordingly, we define a dummy variable $PostPseudoReform_{ct}$, which indicates that the court of county c in year t serves as a centralized court in the TRJ reform and also handles cases against the government of county c. In this way, some counties that originally belonged to the control group are reassigned to the pseudo-reform group. This new dummy is added to the baseline specification as an explanatory variable and Column 7 of Table 4 reports the corresponding results. The estimated coefficient of *PostReform* remains significantly positive, while the coefficient of *PostPseudoReform* is quite insignificant. This result implies that the baseline results are driven by the separation between jurisdictional courts and defendant governments rather than other components of the reform package in 2014 Amendment.[31]

Additionally, we design another placebo test to shed light on the uniqueness of the TRJ reform compared to other efforts in strengthening intra-government supervision such as anti-corruption campaigns. Concretely speaking, the essence of administrative litigation is that private entities are allowed to protect their own interests by legal means if they feel violated, rather than to supervise or monitor the functioning of administrative authorities *internally*. In this regard, the TRJ reform could hardly impact inappropriate behavior within the government system, which is invisible to most citizens and does not directly infringe on personal interests (e.g., improper usage of public funds or irregular promotion of officials).

To empirically test this conjecture, we gathered information on misused public funds for counties from 2011–2017 from *Chinese Audit Yearbooks* to measure intra-government wrongdoings in public finance (Li et al., 2019), which should not be affected by the TRJ reform. Then we regress the logarithmic size of misused public funds on the *PostReform* dummy under the same specification as Column 3 of Table 3 (removing case-level controls). The estimate in the last column of Table 4 is neither statistically nor economically significant, which corroborates that the baseline results are driven by the improved judicial independence that effectively constrains administrative power rather than the enhanced internal governance induced by measures like anti-corruption campaigns.

5.5. Selective disclosure

The documents disclosed on China Judgments Online do not represent the universe of all documents issued by courts in China (Wu et al., 2022; Liu et al., 2022b). For example, the SPC allows local courts to withhold documents expected to have "corrosive

[29] Some recent studies, such as Li (2021) and Liu et al. (2022a), focus on evaluating this reform.

[30] The potential endogeneity in the choice of centralized courts could largely undermine our analysis based on this design. Appendix B addresses this concern.

[31] We have also experimented with directly dropping the pseudo-reform counties and obtained a similar result, which is reported in Appendix Table B2 Column 1.

social impacts." Since the potential sample selection bias that could be generated via this process would inevitably threaten the validity of our empirical design, we must take it seriously.

Wu et al. (2022) proposed a novel method using the German Tank Model to detect the potential sample selection of the disclosed documents. This model offers a statistical method of estimating the maximum number N in the population of a consecutive series $1, 2, \ldots, N$, based on a randomly-drawn sample of k limited observations with the largest number m. Goodman (1952) described a minimum-variance unbiased estimator for N, given by:

$$\widehat{N} = m\left(1 + k^{-1}\right) - 1. \tag{4}$$

Inspired by Wu et al. (2022), we employ the German Tank Model to construct a measure of disclosure rate at the court-year level. For each court in each year, every judicial document written by judges would be indexed with a unique serial number, which is an integer and assigned consecutively. Yet since every document is not disclosed, the document indexes in our sample are nonconsecutive. Using these nonconsecutive serial numbers, we first utilize the German Tank Model to infer the total number of documents for each court-year observation, and then use it as the denominator to compute the court-year disclosure rate. Since judicial documents are indexed consecutively within each court, we could only apply the German Tank Model on a court-year basis.

We use this imputed disclosure rate to implement a battery of additional analyses. First, we use it as the outcome variable and re-estimate Eq. (2). The reform does not seem to have impacted local courts' intention to publicize documents in administrative litigation, as reflected in Column 1 of Table 5. Next, we restrict the sample to court-year units with disclosure rates above 50%, since these should suffer less from the selection bias in judicial documents. The estimate in Column 2 remains significant at the 10% level and even increases in magnitude. We also try to directly control for the disclosure rate in the regression and further include its interaction with *PostReform*. Columns 3 and 4 jointly confirm that the estimated coefficients of *PostReform* are similar to the benchmark result, although the disclosure rate is negatively correlated with governments' probability of losing. Overall, Table 5 suggests that the selective disclosure of judicial documents is not the driving force behind our empirical findings.

In addition to using the German Tank Model to infer disclosure rates, we also tried to collect information on government losing rates from another data source and cross-validated them with numbers computed from our data. Specifically, local Chinese courts are required to report their work to local people's congresses on a yearly basis, which might include governments' yearly losing rates in administrative litigation. Due to multiple challenges associated with this data collection effort, we ended up with a small sample of 197 observations. We first compute the difference between the aggregated losing rate based on case-level data and the disclosed losing rate in the court work reports. This "excessive government losing rate" measures the gap between the two data sources. As shown in Appendix Fig. D1, the probability distribution of this gap is centered and intensively distributed around zero, which indicates that the difference between government losing rates from the two sources resembles "white noise" and represents no systematic deviation. Second, if the observed effect of the TRJ reform is solely driven by selective disclosure (i.e., treatment counties tend to release more cases in which governments lose after the reform), we should observe a positive correlation between the excessive government losing rate and the TRJ reform. We simply regress the excessive government losing rate on the *PostReform* dummy, controlling for county and year fixed effects, and find that this is not the case, since the coefficient is negative (Column 1 of Appendix Table D1). Third, we use the government losing rate reported by local courts as the outcome variable and re-estimate the impact of the TRJ reform. As shown in Column 2 of Appendix Table D1, though the estimate is not statistically significant (probably due to the tiny sample size), the magnitude (0.041) is almost the same as the baseline result (0.039). Although they are subject to serious data limitations, this preliminary evidence helps alleviate concerns about the incomplete disclosure of judgment documents. See Appendix D.1 for more details.

6. Further analysis

6.1. Heterogeneity of defendants and plaintiffs

As introduced in Section 2.2, both defendants and plaintiffs in administrative litigation exhibit considerable heterogeneity in relation to the judicial system. On the one hand, governments differ in administrative ranks (e.g., county governments vs. their subordinate townships), and their influences on judicial decisions may vary. On the other hand, the reform may benefit plaintiffs (i.e., individuals and firms) differently since they have different degrees of access to legal resources such as assistance from professional lawyers and connections to judges. In this subsection, we examine the reform's potential heterogeneous effects on different types of defendants and plaintiffs.[32]

First, we explore the effect on defendants in administrative litigation, which can either be entire local governments (county-level or sub-county-level (e.g., township)) or departments of county governments. County government departments can be further divided into subcategories based on their function. We decompose *PostReform* from Eq. (2) into 10 dummies according to the type of defendant in each case. The regression result is reported in Column 1 of Table 6.

As shown in the first two rows of Column 1 in Table 6, the reform has a negligible effect on county-level governments in terms of its economic magnitude and statistical significance. By contrast, it increases the probability of losing for sub-county governments by 4.1%, which is marginally significant (and significant at the 10% level if we use subsample regression instead of dummy decomposition, as shown in Column 2 of Panel A in Appendix Table E1). This result may reveal the difference in political power enjoyed by governments at different levels. Since county leaders rank higher in the administrative hierarchy than township leaders, they may leverage their political power to influence court judgments in a neighboring county even after the reform. However, it is very difficult for township leaders to do the same.

We then compare the impacts of the reform on government departments with different functions and report the results in the remaining rows of Column 1 in Table 6. We observe the reform's most significant effects in cases filed against government departments in charge of the police and public security, health and birth control, and land and real estate; it has no significant effect on the losing probability of other departments. It is not a surprise to see that the "powerful" departments experience the largest increase in the probability of losing. These departments are responsible for handling local governments' most important and toughest affairs in relation with citizens and firms: maintaining local social stability is the fundamental responsibility of local bureaucrats (Wang, 2015); the enforcement of the birth control policy was a top priority for local officials until its recent relaxation (Suárez Serrato et al., 2019); and land expropriation has been a

[32] In the discussion below, we mainly focus on the results of the heterogeneity analysis conducted with a decomposition of *PostReform*. We have also implemented heterogeneity analysis with subsample regressions, which yields similar results. To save space, we relegate these analyses to Appendix E.

Table 5
Selective reporting of judgment documents.

Dep. Var.	Disclosure rate	Government losing dummy		
	(1)	(2)	(3)	(4)
PostReform	−0.034	0.059*	0.040*	0.040*
	(0.022)	(0.033)	(0.022)	(0.022)
DisclosureRate			−0.065***	−0.067***
			(0.021)	(0.025)
PostReform × DisclosureRate				0.008
				(0.062)
County fixed effects	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES
Case-level controls		YES	YES	YES
Treatment linear trend	YES	YES	YES	YES
County traits×Year dummies	YES	YES	YES	YES
High disclosure rate (>50%) subsample		YES		
Mean of Dep. Var.	0.521	0.395	0.405	0.405
Adjusted R^2	0.371	0.290	0.257	0.257
Number of clusters	1,624	1,376	1,889	1,889
Number of observations	5,740	33,139	55,661	55,661

Note: This table investigates the selective disclosure of judgment documents. The court-year disclosure rate is the ratio of the disclosed number of judgment documents to the total number of judgment documents imputed by the German Tank Model. In Column 1, the disclosure rate is regressed on the *PostReform* dummy defined at the court-year level, which denotes whether the court functions in a trans-regional way, controlling for county and year fixed effects. Note that since the disclosure rate is computed at the court level, the "county fixed effects" in Column 1 are specified according to the location of jurisdictional courts that handle lawsuits, rather than the location of defendant governments. In Column 2, we restrict the sample to cases adjudicated by courts with a disclosure rate above 50% and re-estimate the Eq. (2). In Column 3, we add the disclosure rate as an additional control. In Column 4, we further include its interaction with *PostReform*. Standard errors are reported in parentheses and clustered at the county level. *** denotes significance at 1%, ** at 5%, and * at 10%.

Table 6
Heterogeneity by types of defendants and plaintiffs.

Dep. Var.	Government losing dummy	
	(1)	(2)
PostReform × Defendant types		
Governments (county)	0.005	
	(0.031)	
Governments (sub-county)	0.041	
	(0.032)	
Police and public security	0.104***	
	(0.024)	
Health and birth control	0.132**	
	(0.064)	
Land and real estate	0.053*	
	(0.030)	
Market and environment supervision	0.041	
	(0.033)	
Medical insurance and pension	0.015	
	(0.025)	
Transportation	−0.038	
	(0.062)	
Multiple defendants in different types	0.052*	
	(0.028)	
Others	0.014	
	(0.031)	
PostReform × Plaintiff types		
Individual		0.047**
		(0.021)
Enterprise		0.012
		(0.023)
Multiple plaintiffs in different types		−0.078
		(0.156)
Others		0.036
		(0.044)
County fixed effects	YES	YES
Year fixed effects	YES	YES
Treatment linear trend	YES	YES
County traits×Year dummies	YES	YES
Case-level controls	YES	YES
Adjusted R^2	0.253	0.252
Number of clusters	2,000	2,000
Number of observations	62,392	62,392

Note: This table reports the heterogeneous effects of the TRJ reform according to defendants' and plaintiffs' types. Standard errors are reported in parentheses and clustered at the county level. *** denotes significance at 1%, ** at 5%, and * at 10%.

leading cause of conflicts of interest between farmers and local governments as land leasing revenues have constituted a major source of local fiscal revenue in the past two decades (Chen and Kung, 2016). Before the reform, these departments received strong support from local government leaders and were thus able to interfere in judicial judgments. Our results show that the reform has made plaintiffs better able to successfully sue these powerful bureaus.[33]

Second, we study the heterogeneous effects of the reform on different types of plaintiffs. Both individuals and firms can launch administrative litigation against governments as plaintiffs. Yet they have different endowments of legal and financial resources, which could substantially influence the outcomes of the lawsuits. Prior research has found that entities with more resources can better use the judicial system to defend their claims (Galanter, 1974). Thus it is reasonable to expect that individuals, who had fewer advantages before the reform, would benefit more than firms from the TRJ reform. To empirically test this conjecture, we decompose *PostReform* into four dummies according to the type of plaintiff: individuals, firms, multiple plaintiffs with hybrid types, and others. Column 2 in Table 6 reports the results. We find that the reform has no significant effect on cases with firms as plaintiffs, but has a significantly positive effect on those initiated by individuals. Our findings imply that the reform helps empower those who lacked legal resources before the reform.

The heterogeneity analysis above enriches our understanding of the TRJ reform and suggests a two-sided explanation of what it has achieved so far. On the one hand, the reform helps level the playing field to allow individual citizens to defend themselves against powerful government departments, which have caused wide-

[33] The results also help exclude an alternative interpretation of our baseline results. That is, the positive effects of the TRJ reform may be driven mainly by a significant increase in the probability of losing trivial or low-stakes lawsuits. By contrast, more powerful departments may experience no change (or even a decrease) in the probability of losing. If that is true, the overall increase in governments' probability of losing is cheap and benefits plaintiffs little. The results of the heterogeneity analysis of different government departments directly contradict this alternative story.

spread social complaints in recent decades. On the other hand, there is evidence suggesting the reform is only effective at tying the hands of lower-level governments or government departments, but less effective for those ranked higher in the political hierarchy (i.e., county-level governments).

6.2. Effects on other judicial outcomes

Next we investigate the reform's impact on other judicial outcomes such as judicial quality, caseload, and efficiency, which are closely related to the TRJ reform and have received attention in previous studies on judicial activities (e.g., Kahn and Li, 2020).

First, one may be concerned that the observed effect of the reform comes at the expense of judicial quality. For example, to meet the reform targets, court judges may start to favor plaintiffs even when their claims are unreasonable, which would make innocent governments lose. Though it is hard to determine the quality of judges' decisions only by reading the written judgments, we address this challenge by examining the acceptance and reversal of judgments, since plaintiffs and defendants would appeal unsatisfactory first-instance decisions.

We create a dummy indicating whether a first-instance decision was appealed and use it as the outcome variable to re-estimate Eq. (2). As shown in Column 1 of Table 7, the estimated coefficient on the TRJ reform is negative, which suggests that first-instance judgments are less likely to be appealed after the reform.[34] We further adopt a two-step selection model in the spirit of Heckman (1976) to check whether the reform affects the probability of reversal if litigants have already filed an appeal. In the first step, we predict whether a judgment will be appealed using a probit model that controls for all covariates in the baseline specification as well as the dummy for government losing. We then calculate the inverse Mills ratio based on the prediction from this probit model. In the second step, we add the inverse Mills ratio to Eq. (2), replace the dependent variable with a dummy indicating whether the appellate court overrules a judgment, and restrict the sample to judgments that are appealed. Column 2 of Table 7 reports the estimated coefficient in this second step, which suggests the reform has significantly reduced the possibility of reversal conditional on the lodged appeal. These results demonstrate that the reform improved the quality of the judgments.

Second, given the reform's non-negligible success in eradicating roadblocks to protecting the legitimate rights and interests of individuals and firms, we expect the demand for administrative litigation to increase after its implementation. We test this hypothesis by regressing the total volume of cases against county c in year t on $PostReform_{ct}$ together with other non-case-level controls in the baseline regression. The result, which is reported in Column 3 of Table 7, demonstrates that the TRJ reform increased the administrative litigation case volume by 11%. This finding has two implications. First, the reform has promoted judicial justice for private entities not only at the intensive margin (by increasing the government's probability of losing), but also at the extensive margin (by encouraging more entities to file administrative lawsuits). Second, the reform has increased judges' workload, given the rapid increase in the volume of cases and the relatively stable size of judicial manpower in each county court. Moreover, the increased caseload is not uniformly distributed among courts: centralized courts (such as the court of county *A* in Figs. 2b and 2c) that have jurisdiction over cases against the governments of multiple counties should receive disproportionately more cases as a result of the reform.[35]

Third, following the discussion above, we investigate whether the sharp rise in the volume of cases and the uneven distribution would increase the burden of the judicial system and delay its judgment process, which the SPC claimed was one of the main challenges caused by the TRJ reform (Administrative Division of the Supreme People's Court, 2018). We follow Kahn and Li (2020) and calculate the time needed for case trials (i.e., the number of days from prosecution to judgment) to proxy for the efficiency of the judicial system and use it as the outcome variable to re-estimate Eq. (2).[36] In Column 4 of Table 7, the case-level regression shows that the reform has greatly increased the trial time per case. Column 5 further illustrates that these delays are more conspicuous in centralized courts, which handle cases from more than one county and thus experience larger increases in workload. These findings suggest that the aforementioned benefits of the TRJ reform come at a cost of decreased judicial efficiency.

6.3. The reform's broader influences

The TRJ reform has also had wide-ranging social ramifications and affected the behavior of governments, citizens, and enterprises in relation to the functioning of the judicial system. This subsection explores the broader consequences of the reform.

First, we look at how the reform has affected the legal awareness of both governments and citizens. On the government side, given the significant changes in administrative litigation, local governments may pay more attention to the legitimacy of their actions now because citizens and enterprises are more likely to protect their rights by legal means. Inspired by prior studies that interpret expressions in annual government work reports (GWRs) as policy intentions (Chen et al., 2018a; Chen et al., 2018b),[37] we digitized the annual GWRs of 270 prefectures from 2011 to 2018. For 118 of them, at least one subordinate county implemented the TRJ reform by this time, which we consider as belonging to the treatment group at the prefecture level. We further identify sentences containing keywords related to the rule of law,[38] and then calculate the ratio of the length of those sentences to the total number of characters in each report. A higher ratio indicates that local governments pay more attention to the rule of law. Using this ratio as the outcome variable, the prefecture-year level regression in Column 1 of Table 8 reveals that the rule-of-law content in GWRs increases by about 0.7% after

[34] If we further distinguish between different appellants, the regression results reveal that only appeals filed by plaintiffs have decreased; those filed by defendants have not been affected. These results are available upon request.

[35] Another concern is whether centralized courts might have become stricter about registering administrative lawsuits after the TRJ reform due to the expected increase in workload. The SPC launched the Case Registration and Filing System Reform on May 1, 2015, which stated that "all cases should be accepted and all litigation should be handled." The new system greatly reduced courts' ability to prevent plaintiffs from suing the government. For the 42,192 cases in our baseline regression sample that have accurate records of prosecution dates, 28,578 were filed after May 1, 2015. Thus most of the cases (nearly 70%) were registered under the new filing system, which suggests that the potential selection during the case filing process should not be a serious concern. See the Provisions on Several Issues concerning the Registration and Docketing of Cases by People's Courts issued by the SPC in 2015, accessible at http://gongbao.court.gov.cn/Details/afc1fd6a2b622b4001326039961d82.html?sw=.

[36] For cases without an explicit record of prosecution timing information, we simply set the trial time as the sample median of the non-missing group to avoid a large drop in the sample size. We also add an auxiliary dummy denoting whether the trial time is imputed or not into the regressions, to absorb any potential noises created by this procedure.

[37] GWRs are among the most important official documents at all levels of government. They comprehensively review the social and economic achievements over the past year and propose development targets and work plans for the upcoming year. These reports are delivered by the government heads at the corresponding levels of people's congresses, usually in the first quarter of every year. Their content usually attracts a lot of attention from the media and the public, as they reveal critical information about governments' policy initiatives and priorities.

[38] The keywords include "constitution" (*Xianfa*), "law" (*Falv*), "rule of law" (*Fazhi*), "by law" (*Yifa*), and "law enforcement" (*Zhifa*).

Table 7
Influences on other judicial outcomes.

Dep. Var.	Appealed	Reversed	Case volume	Trial time	
	(1)	(2)	(3)	(4)	(5)
PostReform	−0.033*	−0.108***	0.119***	0.119***	
	(0.018)	(0.017)	(0.041)	(0.031)	
PostReform × CentralizedCourt = 0					0.054
					(0.042)
PostReform × CentralizedCourt = 1					0.126***
					(0.032)
County fixed effects	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES
Treatment linear trend	YES	YES	YES	YES	YES
County traits×Year dummies	YES	YES	YES	YES	YES
Case-level controls	YES	YES		YES	YES
P-value of Coef. Diff.					0.0988
Mean of Dep. Var.	0.324	0.345	1.133	4.557	4.557
Adjusted R^2	0.117	0.581	0.578	0.224	0.225
Number of clusters	2,000	1,474	2000	2,000	2,000
Number of observations	62,392	19,907	12000	62,392	62,392

Note: This table reports the effect of the TRJ reform on other judicial outcomes. In Column 1, we replace the dependent variable in Eq. (2) with a dummy indicating whether the first-instance judgment was appealed. In Column 2, we report the second step of a "Heckman two-step" analysis of whether the TRJ reform would affect the probability of reversal upon appeal. In Column 3, we compute the logarithmic total number of cases at the county-year level and regress it on the *PostReform* dummy. In Column 4, we focus on the TRJ reform's effect on the trial time of cases, defined as the logarithmic number of days from the prosecution date to the judgment date. In Column 5, we further decompose the *PostReform* dummy according to whether the jurisdictional court is a centralized one. Standard errors are reported in parentheses and clustered at the county level. *** denotes significance at 1%, ** at 5%, and * at 10%.

the reform,[39] which is equivalent to 15.8% of its average value. This finding indicates a heightened government emphasis on the principle of the rule of law after the reform.

The reform could also change citizens' legal awareness. We obtained the annual *Baidu* search indexes for specific keywords for 281 prefectures in China since 2011, 120 of which implemented the TRJ reform in at least one subordinate county.[40][41] Columns 2 and 3 in Table 8 report estimates from regressions in which the outcome variables are logarithmic search indexes of "judicial reform" (*Sifa Gaige*) and "administrative litigation" (*Xingzheng Susong*), respectively. The results show that more citizens search for keywords related to administrative litigation after the reform, which implies an increase in their legal awareness. However, when the outcomes are replaced by search indexes of "criminal litigation" (*Xingshi Susong*) and "civil litigation" (*Minshi Susong*) in Columns 4 and 5, respectively, the significance of the coefficients fades away. The results in Columns 4 and 5 serve as perfect placebo tests because the latter two words are important judicial concepts that are not directly related to the TRJ reform.

In the analysis above, the prefecture-level treatment dummy takes a value of 1 if at least one subordinate county had implemented the TRJ reform by the time of the study period. As mentioned in Section 2.3, counties in which centralized courts are located are also involved in the TRJ reform in a general sense even though they have not experienced the separation between origin and adjudication. To consider those effects as a whole, we constructed a continuous variable that is equal to the proportion of counties generally involved in the TRJ reform (scored between 0 and 1) and re-implemented the prefecture-level analysis. The new estimates reported in Panel B of Table 8 exhibit few changes in either statistical significance or economic magnitude.

Second, we examine citizens' political attitudes and firms' reactions to the reform. Given the broader awareness and far-reaching benefits of the reform documented above, it is natural to expect citizens to feel more satisfied with a better-disciplined government. However, we find that this is not the case. Using China Family Panel Studies (CFPS) data from 2010 to 2018,[42] we measure citizens' attitudes toward local governments with a standardized score ranging from 0 to 4.[43] Higher scores indicate a better impression of local government performance in the last year. Using this score as the outcome variable, the regression in Column 1 of Table 9 shows that citizens felt less satisfied with local governments after the reform, contrary to the intuitive expectations.

We propose one plausible explanation for this somewhat surprising finding. Since the reform has made governments more likely to lose administrative cases, more people who suffer from the abuse of public power are encouraged to settle their disputes by legal means. The rising activities in administrative litigation may make more ordinary people aware of inappropriate actions by administrative authorities, which then impacts public perceptions negatively. Wang and Dickson (2022) have documented a similar phenomenon. They empirically find that China's anti-corruption campaign deteriorated citizens' beliefs about public officials and led them to be disenchanted with the government.

The firm-side effects seem to be different. We use the State Administration of Industry and Commerce firm registration database to compute the logarithmic numbers of newly registered firms. As shown in Column 2 of Table 9, assessing the same sample period as the CFPS (i.e., 2010–2018) indicates the TRJ reform has brought about an increase in new firm entries; if we extend the

[39] Note that the $PostRoform_{pt}$ dummy at the prefecture-year level is defined according to whether at least one county in prefecture p implemented the reform in year t. In this regression, we also control for prefecture and year fixed effects, treatment-specific linear time trends, and interactions of predetermined prefecture traits (dummies indicating administrative types, log GDP per capita, and log population density in 2012) with year dummies.

[40] Analogous to Google Trends, these search indexes representing the frequency of active search behaviors by users are published by *Baidu*, the biggest Chinese search engine, which accounted for 65.4% of the market share in China in 2012.

[41] https://gs.statcounter.com/search-engine-market-share/all/china/. Academic researchers widely use the *Baidu* search index, especially in the context of China (e.g., Fisman et al., 2021; Chu et al., 2021). For a more detailed introduction and description, see https://index.baidu.com/v2/main/index.html#/help.

[42] The design of the CFPS is similar to the Panel Study of Income Dynamics in the U. S. It is jointly conducted by Peking University and the University of Michigan. It collects information on the demographics, education, health, socioeconomic conditions, and subjective attitudes and perceptions of 14,960 families in over 100 representative counties, 50 of which had adopted the TRJ reform by the time of our study period. The survey was initiated in 2010 and families are re-interviewed every 2 years. See http://www.isss.pku.edu.cn/cfps.

[43] The CFPS asks each adult interviewee "[h]ow would you rate the performance of the county/district government in the last year?"

Table 8
Influences on legal awareness of governments and citizens.

Dep. Var.	*Rule-of-law*content in the GWRs	Baidu search indexes			
		Judicial reform	*Administrative litigation*	*Criminal litigation*	*Civil litigation*
	(1)	(2)	(3)	(4)	(5)
Panel A					
PostReform dummy	0.646***	0.183***	0.055*	0.017	0.043
	(0.222)	(0.062)	(0.030)	(0.027)	(0.035)
Adjusted R^2	0.420	0.894	0.941	0.919	0.954
Panel B					
Share of reformed counties	0.679***	0.190***	0.057*	0.013	0.030
	(0.242)	(0.066)	(0.033)	(0.031)	(0.038)
Adjusted R^2	0.420	0.894	0.941	0.919	0.954
Prefecture fixed effects	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES
Treatment linear trend	YES	YES	YES	YES	YES
Prefecture traits×Year dummies	YES	YES	YES	YES	YES
Mean of Dep. Var.	4.069	1.573	1.764	0.883	2.069
Number of clusters	270	281	281	281	281
Number of observations	2,085	2,248	2,248	2,248	2,248

Note: This table examines whether and how the TRJ reform influences the legal awareness of governments and citizens. In Column 1, the dependent variable is the ratio of rule-of-law content in the GWRs for 2011–2018. In Columns 2–5, the dependent variables are *Baidu* search indexes in logarithmic value of *judicial reform*, *administrative litigation*, *criminal litigation*, and *civil litigation* during 2011–2018. In Panel A, the dependent variables are regressed on the prefecture-year $PostReform_{pt}$ dummy, which is defined by whether at least one county in prefecture p had implemented the TRJ reform in year t. In Panel B, the independent variable is replaced with the number of counties generally involved in the TRJ reform as a share of the total number of counties in the prefecture. We also control for prefecture and year fixed effects, treatment-specific linear time trends, and interactions of predetermined prefecture traits (dummies indicating administrative types, log GDP per capita, and log population density in 2012) with year dummies. Standard errors are reported in parentheses and clustered at the prefecture level. *** denotes significance at 1%, ** at 5%, and * at 10%.

Table 9
Influences on citizens' political attitudes and firms' entry decisions.

Dep. Var.	Citizens' satisfaction with local governments	Log number of new entrant firms	
Time period	(2010–2018)	(2010–2018)	(2010–2020)
	(1)	(2)	(3)
PostReform	−0.085**	0.060**	0.075***
	(0.033)	(0.026)	(0.022)
Individual fixed effects	YES		
Survey wave fixed effects	YES		
Individual and family controls	YES		
County fixed effects		YES	YES
Year fixed effects		YES	YES
Treatment linear trend		YES	YES
County traits×Year dummies		YES	YES
Mean of Dep. Var.	2.481	6.268	6.442
Adjusted R^2	0.237	0.808	0.793
Number of clusters	100	1,904	1,959
Number of observations	75,690	16,908	20,832

Note: This table examines whether and how the TRJ reform affects citizens' political attitudes toward governments and enterprises' entry decisions. In Column 1, the dependent variable is the subjective rating of satisfaction with local governments. We also control for person and survey wave fixed effects, individual attributes (years of education, age, age squared, and dummies for whether married, Party member, and employment status), and family-level attributes (log number of family members, log average net income, and the share of government transfer payment in total income). In Columns 2 and 3, the dependent variables are the log number of new entrant firms in each county during 2010–2018 and 2010–2020. We include all controls in the baseline specification except for case-level covariates. Standard errors are reported in parentheses and clustered at the county level. *** denotes significance at 1%, ** at 5%, and * at 10%.

sample period to 2020, the impact becomes even larger and more significant (Column 3). Taken together, the heterogeneous responses of citizens and firms may be because our analysis covers a relatively short period after the reform and it may take more time to observe the impact of improved judicial independence. Though the evidence is far from decisive, it sheds some light on the complexity of judicial reform, which is a promising area for future research.

7. Conclusion

There is a long-standing debate in the literature about the effectiveness of improving judicial independence in developing countries with weak rule of law. In this paper, we have examined China's recent judicial reform in administrative litigation to estimate the extent to which improved judicial independence protects citizens and firms from potential abuses by local governments. We find a significant increase in the probability of successfully suing local governments after the reform, especially when the defendants are powerful government departments, and when the plaintiffs are individual citizens who presumably have a weaker legal capacity than firms or civic organizations. But this positive effect is less salient for cases against higher-level governments, which indicates a limitation of the reform. The reform also results in a growing number of case filings and a lower rate of appeals, as well as longer trial time potentially induced by the heavier workload in the judicial system. Moreover, the reform raises the legal awareness of both governments and citizens and encourages firm entry, but worsens general public attitudes toward local governments, at least in the short term.

In sum, our study highlights how trans-regional jurisdiction can improve judicial independence and help protect the rights of private entities. Moreover, given that the effect of the TRJ reform is much weaker when higher-level governments are the defendants, we suggest it should be extended from the current within-prefecture level to a higher (e.g., provincial) level to further suppress the influence of local administrative power and promote judicial independence. Finally, we acknowledge that the paper's empirical findings are limited by the relatively short time period due to data constraints. Future research should explore the reform's long-term impacts on various judicial and economic outcomes in China.

Data availability

Data will be made available on request.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jpubeco.2023.104895.

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