Soft Capture: Career Linkages and Appellate Oversight

Jintao Zhu*

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Abstract

Formal separation of authority is commonly assumed to be sufficient for effective oversight. This paper shows it is not. When personnel policies create career linkages across institutions designed to check one another, monitors face rational incentives to soften their scrutiny even when formal rules remain unchanged. I call this *soft capture*: the informal erosion of monitor independence through career linkages. Unlike regulatory capture, it requires no external actor seeking favorable treatment; unlike collusion, it requires no agreement or side payment. Most distinctively, it produces no identifiable misconduct– each actor responds rationally within the rules, the decisions it distorts are genuinely subjective and formally unimpeachable, yet the system generates persistent welfare losses that no formal accountability mechanism can reach. Soft capture is structurally present wherever vertical monitoring and internal career governance are institutionally coupled, and it cannot be solved by selecting better people. Using a newly constructed dataset linking 133 million ruling verdicts to judicial career records across China's court hierarchy, I exploit quasi-random variation in the timing of downward judicial reallocations to show that appellate intervention falls by 11 percent relative to baseline, persists for four years, and extends court-wide. Robustness and mechanism tests confirm the decline reflects appellate softening rather than improvements in trial-level adjudication, and boundary condition tests confirm the mechanism operates only where the institutional conditions for soft capture are present. De facto separation of career incentives, not merely de jure separation of authority, is necessary for oversight to function.

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*University of Chicago. Email: jintao@uchicago.edu. Generous financial support from the Malyi Centre at the University of Chicago, and the University of Chicago Political Economy Program Research Fund are gratefully acknowledged.

1 Introduction

Effective oversight requires that monitors be willing to intervene against those they oversee. That willingness is typically secured through institutional design: separation of authority, procedural independence, formal insulation from political pressure (Madison, 1788; Persson et al., 1997; North, 1990). But formal safeguards leave one vulnerability unaddressed. When personnel policies create career linkages across institutions designed to check one another–shared promotion pipelines, common evaluation metrics, overlapping organizational futures–monitors face rational incentives to soften their scrutiny of the monitored, even when legal rules and formal authority remain entirely unchanged. This paper identifies, theorizes, and empirically tests this mechanism, which I call *soft capture*: the informal erosion of monitor independence through career linkages. Unlike regulatory capture (Stigler, 1971), it requires no external actor seeking favorable treatment; unlike collusion (Tirole, 1986), it requires no agreement or side payment. Most distinctively, it produces no identifiable misconduct–each actor responds rationally within the rules, the decisions it distorts are genuinely subjective and formally unimpeachable, yet the system generates persistent welfare losses that no formal accountability mechanism can reach. The erosion of oversight is not engineered. It emerges–not as a pathology of bad actors or dysfunctional institutions, but as an equilibrium response to institutional design. Soft capture is structurally present wherever hierarchical organizations combine vertical monitoring with internal career governance, and it cannot be solved by selecting better people. Formal separation of authority is a necessary but not sufficient condition for effective oversight. De facto separation of career incentives is what makes oversight function in practice.

I study this mechanism in China's judicial appellate system, where the independence requirement is most precisely defined and its violation most directly measurable (Shavell, 1995; Kornhauser, 1995; Cameron and Kornhauser, 2017). As part of the Party-state's cadre management system, judges are periodically reassigned downward from intermediate appellate courts to directly affiliated basic trial courts. These reallocations function as a leadership cultivation device: reassigned judges are widely regarded as high-potential candidates undergoing final-stage evaluation before senior promotion, and among those with traceable post-reallocation records, the large majority subsequently advanced into senior roles within or beyond the judiciary (Appendix Figure A12; Section 2). The practice creates a sharp, identifiable shift in the career relationship between a trial court and its supervising appellate court, while leaving legal rules, case characteristics, and formal review standards unchanged. Exploiting variation in the timing of these reallocations across court pairs in a staggered difference-in-differences design following Sun and Abraham (2021), and drawing on the universe of publicly available court decisions–approximately 133 million judgments–I show that appellate courts become significantly less likely to reverse or remand trial-court decisions following reallocation. Appellate intervention falls by 11 percent relative to baseline, the decline emerges immediately, persists for four years, and extends across the entire trial court rather than being confined to the reassigned judge's own caseload.

This paper connects three literatures. The organizational economics literature recognizes that career concerns distort agent behavior (Holmström, 1999; Dewatripont et al., 1999; Gibbons and Murphy, 1992) and that influence activities reshape organizational decisions (Milgrom and Roberts, 1988; Tirole, 1986), but has not established empirically whether these forces distort the behavior of monitors at scale–the "upward contamination" direction.[1] The judicial politics literature documents that career structures shape judges' own decisions (Ramseyer and Rasmusen, 1997, 2003; Garoupa and Ginsburg, 2015; Epstein et al., 2013) but has treated the intensity of appellate oversight as independent of the career circumstances of those being reviewed (Shavell, 1995; Kornhauser, 1995; Cameron and Kornhauser, 2017).[2] The China governance literature has examined the intended benefits of cadre rotation (Landry, 2008; Xu, 2011; Tsai and Liao, 2022; Guo, 2009) and external threats to judicial independence (Wang, 2018; Ng and He, 2017; Stern and Hassid,

[1] Tirole (1986) theorizes collusion between supervisors and agents within hierarchies, and Milgrom and Roberts (1988) model how influence activities distort organizational decisions. Neither paper provides empirical evidence that these forces operate at the level of an entire institutional hierarchy, nor do they examine the specific channel through which the career characteristics of the monitored reshape monitor behavior.

[2] A related literature examines political influence on judicial behavior (Maskin and Tirole, 2004; Posner, 2008) and local government capture of courts in China (Wang, 2018; Ng and He, 2017). These papers study how external pressure shapes judicial decisions, whereas this paper studies how internal personnel governance reshapes the willingness of monitors to intervene—a distinct mechanism that operates even absent any external pressure or political interference.

2012) but has not studied whether personnel rotation generates unintended costs for institutional oversight (Liebman, 2014; Minzner, 2015; Zhang and Ginsburg, 2019). This paper addresses all three gaps with a unified theoretical framework and system-wide empirical evidence.

The paper makes four contributions. First, it provides the first large-scale empirical evidence of soft capture: the informal erosion of monitor independence through career linkages, operating without changes in formal rules, legal standards, or organizational authority. The mechanism is not specific to courts or to China—it is structurally present wherever hierarchical organizations combine vertical monitoring with internal career governance, including regulatory agencies, auditing bodies, and bureaucratic oversight institutions (Persson et al., 1997; North, 1990; Feld and Voigt, 2003). Second, it documents the monitor-side analog to the career-concerns literature. Existing empirical work in that tradition focuses on how career incentives distort the behavior of *agents* (Holmström, 1999; Dewatripont et al., 1999; Gibbons and Murphy, 1992); this paper shows they also distort the behavior of the *monitors* who oversee those agents–a direction the theoretical literature has noted (Tirole, 1986; Milgrom and Roberts, 1988) but that has not been empirically documented at scale. Third, it contributes to the political economy of judicial institutions by identifying a new channel through which career governance affects adjudication–not by shaping the decisions of judges being evaluated, but by reshaping the willingness of other judges to scrutinize those decisions (Ramseyer and Rasmusen, 1997; Garoupa and Ginsburg, 2011; Damaška, 1986). Fourth, it documents an unintended consequence of China's widespread use of downward personnel rotation, contributing to a reorientation of the rule-of-law literature from external threats to judicial independence toward internal institutional design (Liebman, 2014; Zhang and Ginsburg, 2019). As China's judiciary professionalizes, the binding constraints on judicial independence increasingly come from seemingly innocuous personnel mechanisms rather than overt political pressure (Minzner, 2015).

The remainder of the paper is organized as follows. Section 2 describes the institutional setting. Section 3 develops the theoretical framework. Section 4 presents the empirical strategy and data. Section 5 reports results, robustness checks, boundary conditions, and mechanism tests. Section 6 discusses implications.

2 Institutional Background: Courts and Personnel Governance

2.1 Judicial Hierarchy and Appellate Review

China operates a centralized four-level court system aligned with administrative divisions: the Supreme People's Court at the national level, 32 High People's Courts at the provincial level, 404 Intermediate People's Courts at the prefectural level, and 3,111 Basic People's Courts at the county level (Wang, 2018).[3] Most civil and criminal cases are adjudicated at the basic or intermediate level in the first instance, with higher-level courts handling cases deemed more significant by social impact or monetary stakes. China's appeal system follows a "second instance is the last instance" model: a dissatisfied litigant may appeal once to the next higher court, which has full discretion to uphold, reverse, or remand the trial court's judgment. If the appellate court remands the case, it returns for retrial; a second appeal is permitted after retrial, but the appellate court must then issue a final ruling and may no longer remand.[4] Figure 1 summarizes this structure.

2.2 Judicial Personnel Governance

Judicial personnel governance in China is embedded within the broader Party-state cadre management framework. A judge simultaneously occupies three analytically distinct but institutionally connected roles:

[3]Specialized courts—covering transportation, military, mining, and maritime law—sit alongside the general hierarchy but are not the focus of this study.

[4]Two structural features distinguish China's appellate system from the US model. First, intermediate and high courts adjudicate both first-instance cases of greater importance and second-instance appeals from lower courts—there is no strict separation of trial and appellate jurisdiction. Second, China applies comprehensive review: appellate courts may reexamine both factual findings and legal determinations, unlike the US appellate model, which generally confines review to questions of law. These features make appellate intervention broader in scope and more discretionary, providing an institutional context in which changes in appellate behavior are readily detectable.

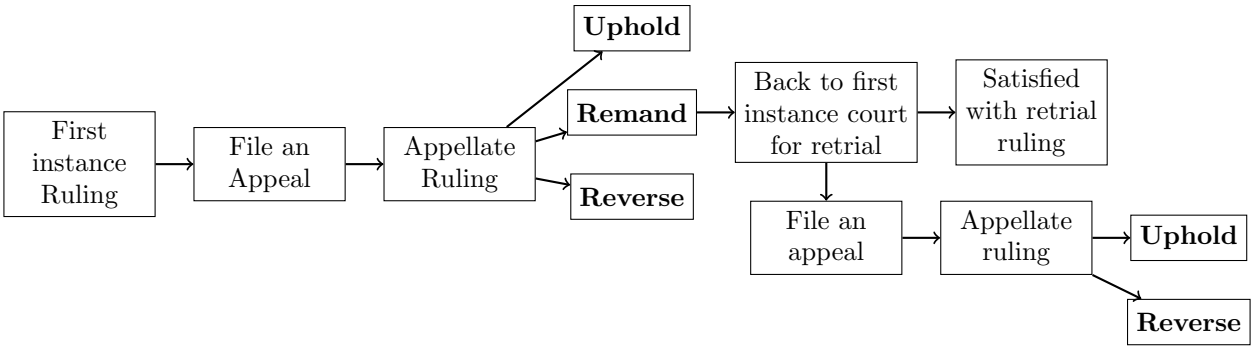


Figure 1: Judicial Appeal Process

an office-holder defined by judicial position, a professional defined by judicial rank, and a cadre defined by administrative grade. Judicial positions—court president, vice president, adjudication committee member, division head, ordinary judge—define daily responsibilities and organizational authority. Judicial rank is a four-grade, twelve-level hierarchy intended to reflect experience and evaluated competence. Administrative grade places judges within the general bureaucratic hierarchy governing all Party-state officials.

The three layers are coupled through predictable correspondences that give rank direct material consequences. Table 1 summarizes the mapping from judicial rank to administrative grade: rank advancement elevates grade, which in turn determines compensation scales, welfare entitlements, and retirement benefits. Appendix Table A2 documents the typical association between court positions and rank ranges across court levels, reflecting institutional matching rather than formal prerequisites. Because formal dismissal is rare absent serious disciplinary violations, the personnel system governs judges primarily through career rewards and penalties: advancement in rank, stalled promotion, and—in principle—rank degradation for judges assessed as incompetent in periodic evaluations. The dominant career incentive is therefore rank advancement, which simultaneously elevates grade, compensation, and eligibility for senior positions.

Appellate outcomes are the central metric through which this career incentive operates. As summarized in Table 2, judicial performance evaluation operates through an index-based system that translates adjudicatory outcomes into career-relevant signals. Within the *achievement* dimension—the core career-relevant component—the quality sub-dimension places primary weight on the number of cases *modified or remanded on review*. Appellate intervention therefore functions not only as a mechanism of legal correction, but as a formal performance signal incorporated into evaluation and linked directly to rank advancement. A judge whose cases are frequently reversed or remanded accumulates negative evaluation signals that delay promotion and reduce eligibility for leadership roles. This direct mapping from appellate outcomes to career trajectories is the institutional foundation on which soft capture operates.

2.3 Personnel Policy: Downward Reallocation of Judges

Within the personnel system described above, downward reallocation refers to the administrative reassignment of judges from intermediate courts to directly affiliated basic courts. Such reassignments are implemented through personnel decisions rather than judicial review or disciplinary procedures and are a routine component of cadre management within the Party-state.

Downward reallocation does not signal professional failure. On the contrary, it is widely understood within the system as a strong positive career signal: reassigned judges are typically viewed as high-potential candidates for further promotion who require additional practical experience or observation before assuming more senior roles. The institutional logic has two components. First, cadre governance has long emphasized grassroots experience as both training and screening for future leaders: service at lower-level courts allows judges to manage heavier and more varied caseloads and demonstrate leadership ability. Second, reallocation provides an opportunity for final-stage evaluation—by observing performance in a lower-level setting, higher authorities can assess managerial competence and professional judgment before making promotion decisions. Among judges with traceable post-reallocation career records, the large majority subsequently advanced into senior roles within or beyond the judiciary: leadership positions at other basic courts, committee or

Table 1: Judicial Ranks and Corresponding Administrative Ranks

Judicial rank (4 grades, 12 levels)	**Corresponding administrative rank**
Chief Justice	Vice-national level
Class I Justice	Ministerial level (full)
Class II Justice	Ministerial level (deputy)
Class I Senior Judge	Departmental level (full)
Class II Senior Judge	Departmental level (deputy)
Class III Senior Judge	Division level (full)
Class IV Senior Judge	Division level (deputy)
Class I Judge	Section level (full)
Class II Judge	Section level (deputy)
Class III Judge	Staff level
Class IV Judge	Staff level
Class V Judge	Staff level

Notes: Judicial ranks correspond to administrative ranks within the Party-state cadre system. Rank advancement affects compensation, authority, and eligibility for leadership positions.
Source: Author compilation based on official judicial rank tables.

Table 2: Judicial Performance Evaluation Matrix

Dimension	**Focus (Selective)**
Morality	Political caliber and moral character, including: adherence to the Party's absolute leadership over the judiciary; adherence to the path of Socialist Rule of Law with Chinese Characteristics; practice of Core Socialist Values.
Ability	Legal professional competence and work capability, including: ability to apply laws and policies; quality of trial operations; level of legal theory and practical judicial research; ability to prevent and control risks; capacity to handle heavy caseloads; ability to apply judicial technology; ability to guide public opinion.
Diligence	Work attitude and responsibility, including: devotion to duty; completion of assigned tasks; willingness to take responsibility.
Achievement	Case-handling performance and other work outcomes, including the quantity, quality, efficiency and efficacy of cases handled.
Integrity	Judicial integrity and self-discipline, including: compliance with integrity and discipline requirements; implementation of the Central Committee's Eight Provisions and related regulations.

Source: Guiding Opinions on Strengthening and Improving Efforts on the Evaluation of Judges (November 2021).

division roles at intermediate courts, appointments at high courts, or posts in the procuratorate and other Party-state organs (Appendix Figure A12).

Consistent with this logic, reallocation is typically targeted rather than random in its institutional design. Judges are most often reassigned to directly affiliated lower courts—courts within the administrative and supervisory jurisdiction of the sending higher court—and are frequently placed into leadership or quasi-leadership positions such as court president or vice president, rather than serving as ordinary adjudicators. This study focuses on the *first* detected movement from an intermediate court to a directly affiliated basic court for each court pair. This moment—the initial transition from an ordinary judicial role to a leadership position at a lower-level court—represents the institutionally significant step at which the career signal is clearest and the evaluation stakes are highest. Judges who are reassigned downward typically retain their existing judicial rank and administrative grade at the time of reassignment, underscoring that the move is not punitive.[5]

Taken together, downward reallocation combines three features that are central to this study: it is a positive career signal, it is embedded in hierarchical supervision through affiliated courts, and it repositions judges into leadership roles at lower levels without immediately altering formal rank or grade. These features make downward reallocation a particularly informative institutional setting for studying soft capture—the next section develops the theoretical framework that explains why these conditions generate reduced appellate scrutiny.

3 Theory: Soft Capture and Appellate Review

The institutional features documented in Section 2 combine to create a career-relevant relational linkage between trial and appellate courts. When a judge is reassigned from an intermediate court to a directly affiliated basic court, she enters the supervisory domain of her former appellate court while remaining under active evaluation for future promotion. Because judicial performance evaluation explicitly incorporates reversal and remand rates as the core case-quality metric, appellate intervention directly shapes the career trajectory of the very judges whose cases are being reviewed.

This configuration generates a rational incentive for appellate judges to soften their scrutiny–not through explicit coordination, personal exchange, or improper influence, but through the ordinary interaction of hierarchical career governance and evaluation design. An appellate judge who reverses or remands a case decided by a high-potential reassigned judge generates a negative career signal about an individual whose advancement reflects on prior personnel judgments by the higher court. When the expected cost of that signal exceeds the expected benefit of accurate review, the appellate judge rationally reduces scrutiny. I call this mechanism *soft capture*: the informal erosion of monitor independence through career linkages. Unlike regulatory capture (Stigler, 1971), it requires no external actor seeking favorable treatment; unlike collusion (Tirole, 1986), it requires no agreement or side payment. Most distinctively, it produces no identifiable misconduct–each actor responds rationally within the rules, the decisions it distorts are genuinely subjective and formally unimpeachable, yet the system generates persistent welfare losses that no formal accountability mechanism can reach.

Because the mechanism operates at the review stage, it does not require any change in the reassigned judge's own behavior. The adjustment is entirely on the appellate side: monitors exercise less scrutiny, intervene less frequently, and provide less elaborated justification when they do uphold. Appendix Appendix A develops a formal model that disciplines this intuition and characterizes the equilibrium conditions under which soft capture arises. The central result is that reduced appellate scrutiny emerges in equilibrium when the expected career value of leniency is sufficiently large relative to the risk of adverse consequences–a condition most readily satisfied when hierarchical affiliation is close and the reassigned judge is widely recognized as a future leader. Three testable predictions follow directly.

Prediction 1 (Main effect). Appellate reversal and remand rates decline following downward reallocation within hierarchically affiliated court pairs. The effect operates at the level of the trial court as a whole,

[5]Downward reallocation is institutionally distinct from judicial rank adjustment. Rank promotion or degradation is governed by evaluation outcomes and formal procedures, whereas reallocation is an administrative deployment decision. Subsequent rank promotion, stagnation, or rank degradation depends on later evaluations and is not mechanically tied to the act of reallocation itself.

not only in cases handled by the reassigned judge herself, because the mechanism runs through the appellate court's response to the reallocation event rather than through any change in individual case quality.

Prediction 2 (Boundary conditions). The effect is present only when the institutional conditions for soft capture are satisfied. It should be absent–or substantially attenuated–when the reallocation does not carry a recognizable leadership career signal, when hierarchical affiliation between courts is absent, and when the career evaluation channel does not run through the domain of adjudication being measured.

Prediction 3 (Mechanism). The decline in reversal and remand rates reflects appellate softening rather than improvements in trial-level adjudication. Measurable indicators of trial-court quality should show no systematic improvement following reallocation, while appellate opinions should exhibit signs of reduced scrutiny at the review stage.

4 Empirical Strategy and Data

4.1 Treatment, Comparison Group, and Identifying Variation

The treatment of interest is downward judicial reallocation: the reassignment of a judge from an intermediate court to a directly affiliated basic court within the same administrative hierarchy. As established in Section 2, such reassignments are part of a leadership cultivation pipeline rather than a response to adjudicatory performance in specific cases. Reassigned judges typically retain their professional rank and administrative grade at the time of reassignment, are placed into leadership or quasi-leadership roles in the receiving court, and are widely regarded as high-potential candidates under active evaluation for future promotion. These features make the treatment a clean shifter of the career relationship between a trial court and its supervising appellate court, while leaving legal rules, case characteristics, and formal review standards unchanged. We focus on the *first* detected reallocation for each intermediate–basic court pair during the observation window; subsequent movements involving the same pair are excluded to avoid contamination of the post-treatment window.

The comparison group consists of trial courts that do not experience inward reallocation from their directly superior intermediate court at any point during the observation window (2014–2020). We restrict the analysis to directly affiliated basic–intermediate court pairs throughout, ensuring that treated trial courts fall squarely within the supervisory domain of the appellate court responsible for review. Courts outside that supervisory relationship do not satisfy the institutional conditions for soft capture and do not provide credible counterfactuals (Section 5.3).

The key source of identifying variation is the timing of reallocation across court pairs. Reallocation decisions are made at higher administrative levels and are not triggered by contemporaneous case outcomes in the receiving court. The destination of each reallocation—which specific basic court receives the judge—is determined primarily by vacancy availability: a position becomes open when an incumbent departs, and the match between a judge and a receiving court therefore depends largely on the timing of unrelated departures elsewhere in the hierarchy. Administrative records of these vacancy-driven assignment decisions are not publicly accessible, limiting the scope for strategic manipulation. Timing varies across court pairs within provinces and across years, further reducing the scope for coincident shocks. The event-study design allows direct inspection of pre-treatment trends, providing a direct test of the parallel trends assumption; we find no evidence of differential pre-trends across any specification.

4.2 Data and Outcome Measures

Court decisions. Our analysis draws on China Judgments Online (CJO), the official platform for publishing court decisions in China, which contains the universe of publicly available civil and criminal judgments across China's judicial hierarchy. We focus on the period 2014–2020, during which publication practices are relatively stable and judicial reallocation is well documented. We restrict the main analysis to civil cases: civil adjudication affords judges substantially greater discretion than criminal sentencing, where statutory constraints and standardized guidelines limit variation in outcomes, making civil cases the more appropriate setting for detecting changes in appellate review behavior.[6]

[6]We examine criminal cases as a robustness check in Section 5.2. The estimated effects are smaller in magnitude and less precisely estimated, consistent with the greater role of mandatory guidelines in criminal sentencing limiting the scope for

To assess the representativeness of the CJO data, we compare aggregate trends in case volume and appellate outcomes with official judicial statistics. Case volumes and the distribution of appellate outcomes in CJO closely track official aggregates over time (Appendix Figure A13). Reversal and remand rates computed from CJO are somewhat higher than official statistics, reflecting selective publication of more complex cases at the appellate level. Because publication decisions are made by appellate courts rather than trial courts, such selection does not undermine the identification strategy and is absorbed by court fixed effects.

Data construction. The analysis requires linking three independently constructed datasets. Court decisions are drawn from the universe of publicly available judgments on China Judgments Online, covering approximately 133 million civil and criminal decisions over 2014–2020. From these records, we extract court identifiers, presiding judge names, and case outcomes for each judgment, and aggregate them to the court–half-year level to construct the outcome measures and text-based quality proxies described above. Judicial reallocation is identified by tracking individual judges' adjudicatory activity across courts—a judge is classified as reallocated when her case records cease in one court and subsequently appear in another within the same administrative hierarchy. Leadership identification is constructed by manually searching court appointment records for each of the 1,154 detected movements, verifying role assignment for 747 judges (64.7 percent of the sample). All three datasets are constructed by the author from primary sources without reliance on pre-existing administrative databases.

Outcome measures. Our primary outcome is the reversal-or-remand rate: the share of appellate decisions that either reverse or remand the trial court's judgment. This measure directly captures the intensity of appellate intervention and corresponds to the core performance metric in judicial evaluation (Section 2.2, Table 2). We construct auxiliary outcomes at both the trial and appellate stages to distinguish review-stage mechanisms from changes in trial-level behavior: indicators of trial-court reasoning quality, measures of appellate opinion characteristics, and the appeal rate itself. These are introduced when presenting the mechanism tests in Section 5.4.

Sample. The main analysis sample consists of 43,470 court–half-year observations spanning 2014–2020, of which 12,477 are treated observations. Standard errors are clustered at the appellate court level (380 clusters), reflecting that the treatment mechanism operates through appellate review behavior common to all trial courts within the same appellate jurisdiction.

4.3 Measurement of Judicial Reallocation

Judicial reallocation is identified by tracking judges' adjudicatory activity across courts. Each judgment record includes the court identifier and the names of presiding judges. We observe a judge's first and last appearances in a given court and classify a downward reallocation when a judge's case records cease in an intermediate court and subsequently appear in a directly affiliated basic court.

The reallocation date is approximated as the midpoint between the judge's last case in the sending court and first case in the receiving court. The median transition gap is 135 days, all values are positive, and no judge exhibits a negative gap, supporting the validity of the midpoint approximation (Appendix Table A3). Robustness to alternative timing definitions is reported in Section 5.2 and yields nearly identical results. Table 3 summarizes observed movements by direction and role across the full detected sample; the annual distribution of reallocation events is reported in Appendix Table A4.

Two measurement issues warrant discussion. First, judges with identical names could generate spurious matches. This concern is mitigated by restricting matches within each province, substantially reducing the likelihood of false positives. Second, we may miss intermediate administrative postings in which judges temporarily serve in non-adjudicative roles. To address this, we exclude movements with unusually long gaps between adjudicatory activity in the two courts. Remaining measurement error is unlikely to bias estimates systematically and is further mitigated by the difference-in-differences design.

Table A5 characterizes the adjudication domain composition of the downward-reallocated judges in the main analysis sample. This characterization is the basis for the civil channel placebo test in Section 5.3: soft capture operates through the civil adjudication domain and should be absent for judges whose caseload consisted exclusively of criminal cases (Appendix Table A5).

appellate softening.

Table 3: Judicial Personnel Movements: Main Analysis Sample

Panel A. All detected movements (full CJO sample)

Direction of Movement	Judges	Judicial Assistants
Downward	3,319	383
Lateral	9,714	1,314
Upward	3,106	454
Total	16,139	2,151

Panel B. Downward movements: main analysis sample

Role at receiving basic court	*N*	Share (%)
Court president (*yuanzhang*)	561	48.6
Vice president (*fu yuanzhang*)	186	16.1
Other leadership role	1	0.1
Subtotal: identified leaders	747	64.7
Unidentified (role not traceable online)	407	35.3
Total	1,154	100.0

Notes: Panel A reports counts of all detected personnel movements across the full China Judgments Online (CJO) sample, based on observed changes in adjudicatory activity across courts. Panel B characterizes the 1,154 downward judge movements constituting the main analysis sample—reassignments from intermediate courts to directly affiliated basic courts meeting the institutional criteria described in Section 4.3. Only the first detected movement per intermediate–basic court pair is included. Role identification is based on manual search of court appointment records; "unidentified" denotes movements for which the role at the receiving court could not be confirmed online, which may reflect either non-leadership transfers or leadership appointments with limited online coverage. The identified leadership share (64.7 percent) is therefore a lower bound on the true fraction of leadership-motivated reallocations in the sample.

4.4 Empirical Specification

We estimate the effect of downward judicial reallocation on appellate review behavior using an event-study specification implemented in a staggered difference-in-differences framework:

$$Y_{ct} = \sum_{k \neq -1} \beta_k \cdot \text{Event}_{k,ct} + \gamma_c + \delta_t + \varepsilon_{ct}, \tag{1}$$

where Y_{ct} is the reversal-or-remand rate for cases originating from trial court c in half-year t. The indicators $\text{Event}_{k,ct}$ equal one if period t is k half-years relative to the reallocation event for court c, and zero otherwise. Trial courts that never experience downward reallocation during the observation window serve as the control group. The parameters γ_c and δ_t denote trial court and time fixed effects, respectively: trial court fixed effects absorb time-invariant heterogeneity across courts, while time fixed effects control for shocks common to all courts in a given period.

The coefficients β_k trace the dynamic response of appellate intervention to reallocation. Under the parallel trends assumption, pre-treatment coefficients ($k < -1$) identify pre-existing differential trends—which we test and find to be absent—while post-treatment coefficients capture the causal effect of reallocation on appellate review behavior relative to never-treated courts.

Because treatment timing varies across courts, we estimate equation (1) using the interaction-weighted estimator of Sun and Abraham (2021), which avoids the negative-weight bias arising from heterogeneous treatment effects under staggered adoption (Callaway and Sant'Anna, 2021; Borusyak et al., 2024).[7] Standard errors are clustered at the appellate court level to account for within-court correlation in review behavior arising from the fact that the treatment mechanism operates at the appellate court level.

[7] Callaway and Sant'Anna (2021) and Borusyak et al. (2024) develop alternative estimators that address the same problem. We report results using the Borusyak et al. (2024) imputation estimator as a robustness check in Section 5.2; the results are nearly identical to the Sun–Abraham estimates.

4.5 Summary Statistics

Table 4 reports pre-treatment summary statistics separately for treated and never-treated trial courts. Pre-treatment statistics for treated courts are computed over all periods prior to the reallocation event; never-treated courts use all available periods. The two groups are closely balanced on the primary outcome: the pre-treatment reversal-or-remand rate is 26.5 percent for never-treated courts and 26.4 percent for treated courts, a difference of less than 0.1 percentage points relative to a within-group standard deviation of approximately 12.7 percentage points. Appellate case volume, reasoning length, and the number of cited legal provisions are similarly balanced. One noteworthy difference is that treated courts have a somewhat higher pre-treatment remand rate (8.57 percent) than never-treated courts (7.96 percent). This difference, which is absorbed by court fixed effects in the event-study specification, if anything strengthens the identification: treated courts enter the post-reallocation period with a higher baseline rate of remand, making the subsequent decline more striking rather than an artifact of mean reversion.

Table 4: Pre-Treatment Summary Statistics

	Never-treated courts			Treated courts		
	Mean	SD	Median	Mean	SD	Median
Panel A. Outcome variables						
Reversal-or-remand rate	0.265	0.127	0.253	0.264	0.127	0.255
of which: remand rate	0.080	0.080	0.061	0.086	0.083	0.066
of which: reversal rate	0.186	0.106	0.171	0.179	0.100	0.167
Panel B. Case characteristics						
Appellate case volume	126.1	190.0	69.0	129.5	195.1	74.0
Reasoning length (characters)	497.1	173.0	468.6	491.5	175.4	464.5
No. of cited legal provisions	2.05	1.09	1.76	2.21	1.22	1.89
Panel C. Sample counts						
Courts		2,173			756	
Observations		31,046			6,660	

Notes: Pre-treatment statistics for treated courts are computed over periods prior to the reallocation event for each court ($k < 0$). Never-treated courts use all available periods. The unit of observation is the court–half-year. Reversal-or-remand rate is the share of civil appellate decisions that reverse or remand the trial court's judgment; remand and reversal rates are its two components. Appellate case volume is the number of civil appellate cases per court per half-year. Reasoning length is the average length of appellate opinions (characters). No. of cited legal provisions is the average number of legal articles cited per appellate decision. All variables are measured at the court–half-year level from China Judgments Online (CJO), 2014–2020. The treated court count of 756 reflects unique basic courts with at least one detected reallocation in the pre-reallocation window. The pre-treatment remand rate is somewhat higher for treated than never-treated courts (8.57 vs. 7.96 percent); this difference is absorbed by court fixed effects in the event-study specification.

5 Results

5.1 Main Effect: Downward Reallocation and Appellate Intervention

Figure 2 plots the estimated coefficients from the staggered difference-in-differences event study. The horizontal axis measures half-years relative to the reallocation event, with $k = 0$ denoting the half-year in which the judge from the directly affiliated intermediate court arrives at the basic court. The vertical axis reports changes in the reversal-or-remand rate relative to never-treated courts. Two patterns are immediately apparent.

Pre-treatment balance. All seven pre-treatment coefficients are small in magnitude and statistically indistinguishable from zero throughout the pre-reallocation window ($k = -8$ to $k = -2$; the omitted period is $k = -1$). Point estimates range from -0.59 to $+0.35$ percentage points, and no pre-treatment coefficient is statistically significant, with p-values ranging from 0.44 to 0.96. There is no evidence of differential trends

between treated and control trial courts prior to reallocation, supporting the parallel-trends assumption underlying the identification strategy.

Post-reallocation decline. Appellate intervention falls immediately and persistently following reallocation. The coefficient at $k = 0$—the half-year of arrival—is -0.30 percentage points ($p = 0.527$), directionally consistent with the subsequent pattern but imprecise, reflecting classical measurement error in event time from the midpoint approximation of the reallocation date.[8] From $k = 1$ onward, the effect is statistically significant at the 5 percent level through $k = 5$, with point estimates in the range of -1.15 to -1.43 percentage points across this window. The coefficient at $k = 8$ is -1.05 percentage points ($p = 0.065$), consistent with a persistent effect over the typical 4–5 year term of a court leader. The sustained nature of the decline across eight observed post-reallocation periods indicates a durable shift in appellate review behavior rather than a transitory response.

The average post-reallocation treatment effect is -1.03 percentage points per half-year (SE = 0.34 pp, $p = 0.003$), estimated on the 12,477 treated observations.[9] The pre-treatment mean reversal-or-remand rate in the sample is 26.5 percent (Table 4), so the estimated decline of 1.03 percentage points corresponds to a reduction of approximately 3.9 percent relative to baseline per half-year. The effect is both statistically and substantively significant: five consecutive post-period coefficients are significant at the 5 percent level, and the estimated magnitude represents a meaningful compression of appellate oversight sustained across the full rotation window.

These estimates establish the existence, timing, and magnitude of soft capture: a systematic decline in appellate intervention following downward reallocation within hierarchically affiliated court pairs. The following sections assess robustness, examine the institutional boundaries under which the effect arises, and investigate whether the decline reflects appellate softening or improvements in trial-level adjudication.

[8]The robustness specifications using the earliest and latest timing definitions—which bound the true reallocation date—yield qualitatively identical patterns, confirming that the attenuation at $k = 0$ reflects timing measurement noise rather than a delayed onset of the effect (Section 5.2).

[9]The aggregate ATT is computed as the inverse-variance weighted average of post-event coefficients following Sun and Abraham (2021). The full sample comprises 43,470 court–half-year observations across 1,154 judge-movement events and 2,173 never-treated courts.

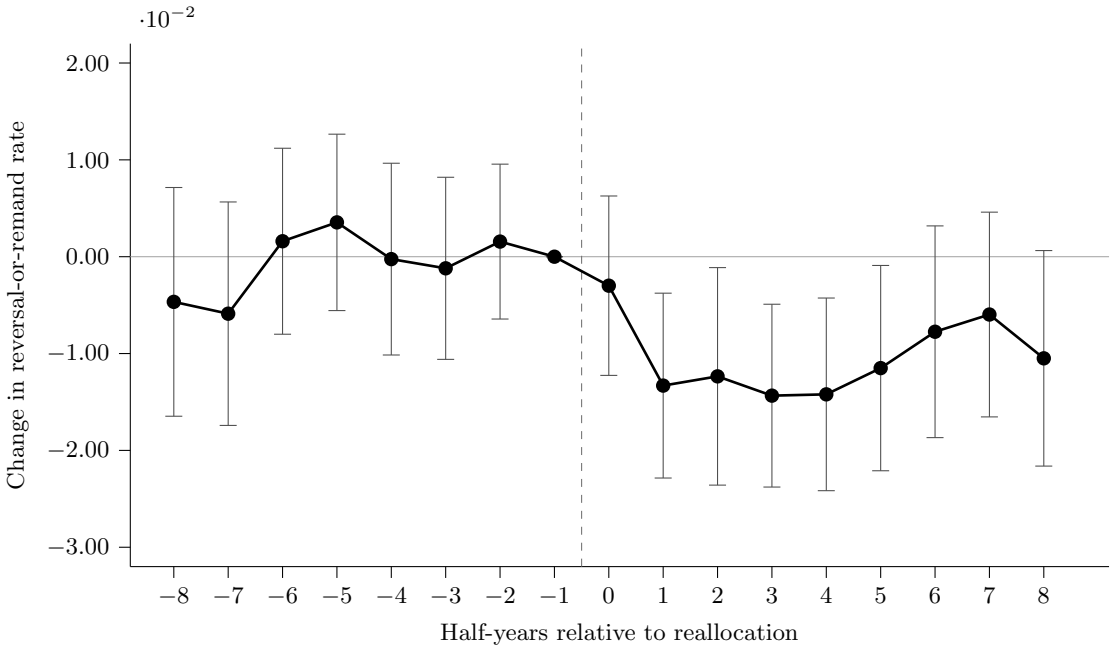


Figure 2: **Effect of downward judicial reallocation on appellate intervention.** Points represent Sun–Abraham interaction-weighted estimates of $\hat{\beta}_k$ from equation (1); vertical bars denote 95% confidence intervals. The dashed vertical line separates pre- and post-reallocation periods. The omitted period is $k = -1$. Standard errors are clustered at the appellate court level (380 clusters). $N = 43{,}470$ court–half-year observations; $N_{\text{treated}} = 12{,}477$ across 1,154 judge-movement events.

5.2 Robustness

Figure 3 summarizes the average post-reallocation effect across five alternative specifications. Each point reports the inverse-variance weighted average of post-event coefficients; whiskers denote 95% confidence intervals. Across all specifications, the estimated effects remain negative and statistically significant, with point estimates ranging from −0.66 to −1.03 percentage points per half-year.

Timing definitions. The baseline approximates the reallocation date as the midpoint between the judge's last ruling in the sending court and first ruling in the receiving court. The *earliest* definition uses the last ruling in the sending court; the *latest* definition uses the first ruling in the receiving court. Both yield negative and significant aggregate effects (−0.81 pp, $p = 0.012$; −0.98 pp, $p = 0.002$). The latest timing produces the strongest result, consistent with it most accurately capturing the date at which the judge begins adjudicating at the receiving court and the career linkage becomes operationally salient. Pre-treatment coefficients are clean under both timing definitions, confirming that the results are not sensitive to the approximation of the reallocation date.

Sample restrictions. The baseline imposes no minimum caseload requirement, recovering judges who assume leadership roles but adjudicate rarely. Imposing a conservative minimum of five adjudicated cases per court—the sample used in conventional specifications in this literature—yields an aggregate ATT of −0.96 pp ($p = 0.012$), statistically indistinguishable from the baseline. This confirms that the main result is not driven by barely adjudicating judges and holds under more restrictive sample criteria.[10]

Removing the upper bound on the transition gap between courts expands the treated sample to 18,166 observations and yields ATT = −0.66 pp ($p = 0.017$). The attenuation relative to the baseline reflects additional measurement noise from including judges with very long adjudicatory gaps, whose reallocation

[10]The no-minimum-case specification is preferred as the baseline on theoretical grounds: the mechanism operates through the career signal generated by the reallocation, not through individual adjudicatory output. Section **??** develops this argument in detail.

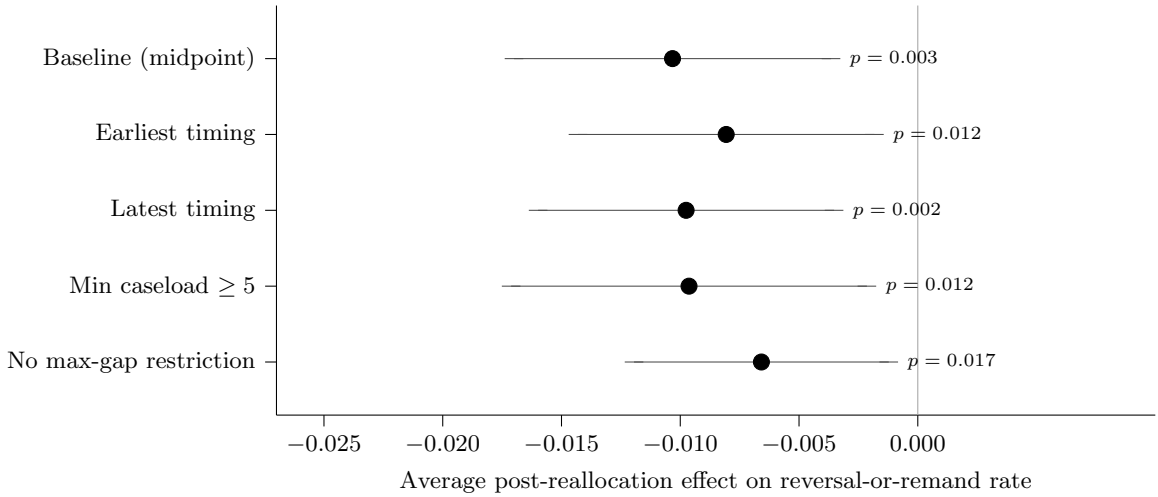


Figure 3: **Robustness across alternative timing definitions and sample restrictions.** Each point reports the inverse-variance weighted average post-reallocation effect on the reversal-or-remand rate; whiskers denote 95% confidence intervals. All specifications use the Sun–Abraham interaction-weighted estimator with court and half-year fixed effects, and standard errors clustered at the appellate court level (380 clusters). p-values for the aggregate ATT are reported to the right of each interval. Full event-study plots and coefficient tables are in Appendix Appendix B.

date is harder to pin down precisely, as discussed in Section 4.3.[11]

Taken together, Figure 3 and Appendix Table A1 demonstrate that the main result—a sustained decline in appellate intervention following downward judicial reallocation—is robust to a wide range of alternative specifications. These findings strengthen the interpretation of the effect as a systematic response to personnel reallocation rather than an artifact of modeling choices.

5.3 Boundary Conditions

Soft capture requires a specific institutional configuration: a recognizable leadership career signal, hierarchical affiliation between sending and receiving courts, and a career evaluation channel that runs through the adjudication domain being measured. Figure 4 reports the average post-reallocation effect across six specifications that vary each condition. Only confirmed leadership reallocation within affiliated hierarchies through the civil adjudication channel produces a significant effect; all other specifications yield estimates indistinguishable from zero.

Career signal. Of the 1,154 judge movements in the baseline sample, 747 have been confirmed as leadership appointments at the receiving basic court (court president or vice president); the remaining 407—unconfirmed movements—could not be verified as such, either because no appointment record was found online or because no leadership role resulted. Confirmed leaders yield ATT $= -1.04$ pp ($p = 0.008$), nearly identical to the baseline. Unconfirmed movements yield ATT $= -0.76$ pp ($p = 0.236$), statistically indistinguishable from zero. Where the career signal is unambiguous, appellate courts respond; where it is absent or unclear, they do not. Judicial assistants provide a complementary placebo: their downward movements place individuals on the pathway to becoming ordinary judges rather than into the leadership pipeline, and their aggregate ATT is $+0.42$ pp ($p = 0.797$).

Hierarchical affiliation. Two features make this setting informative. First, such movements are institutionally anomalous: downward reallocation is designed to occur within directly affiliated hierarchies,

[11] The no-maximum-gap specification exhibits a marginally significant pre-period coefficient at $k = -5$ ($p = 0.033$). This reflects measurement noise from the long-gap sample rather than a genuine pre-trend: when the transition gap is very long, some observations classified as pre-period may already correspond to the post-reallocation window, producing a spurious negative coefficient. The remaining pre-period coefficients are statistically indistinguishable from zero, and the aggregate post-period ATT is negative and significant, confirming the robustness of the main finding.

and a non-affiliated transfer reflects atypical administrative circumstances rather than deliberate leadership cultivation. Second, the pre-trend violation in this specification—several pre-reallocation coefficients are large and statistically significant—corroborates that these movements lie outside the normal reallocation channel and do not satisfy the parallel trends assumption; the estimate is therefore not causally identified. The aggregate ATT is -0.29 pp ($p = 0.585$), indistinguishable from zero.

Direction and domain. For upward reallocation—judges moving from basic to intermediate courts—the aggregate ATT is -0.33 pp ($p = 0.212$). Upward movements do not generate the career-relevant linkage that triggers soft capture: newly promoted judges are not immediate leadership candidates at the intermediate court, and the court has no incentive to adjust its review of their former basic courts. For the civil channel test, we restrict the treatment sample to movements in which the reassigned judge handled only criminal cases at the sending intermediate court. Her career evaluation is then linked to criminal adjudication outcomes, not civil ones, and appellate accommodation through reduced civil reversal and remand rates would carry no career benefit. The aggregate ATT is $+0.73$ pp ($p = 0.442$).

The six tests jointly delineate the scope of the mechanism. The effect requires a recognizable leadership signal, hierarchical affiliation, and alignment between the evaluation domain and the outcome being measured. Remove any one condition and the effect disappears.

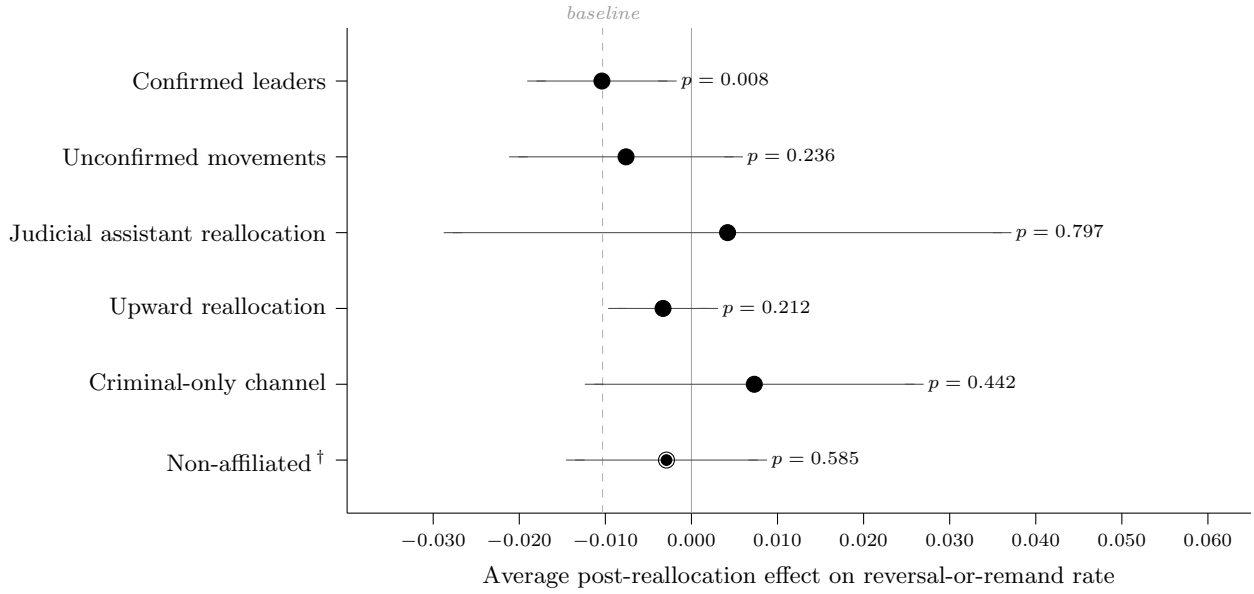


Figure 4: **Boundary conditions of soft capture.** Each point reports the inverse-variance weighted average post-reallocation ATT on the reversal-or-remand rate; whiskers denote 95% confidence intervals. The dashed vertical line marks the baseline ATT (-1.03 pp). *Confirmed leaders*: 747 judge movements verified as court president or vice president appointments. *Unconfirmed movements*: 407 movements for which no leadership appointment could be verified. *Judicial assistant reallocation*: downward movements of judicial assistants. *Upward reallocation*: movements from basic to intermediate courts. *Criminal-only channel*: reassigned judge handled only criminal cases at the receiving court. *Non-affiliated*†: judge originates from a non-directly-affiliated intermediate court; pre-trend violation indicates atypical transfers outside the normal reallocation channel. Standard errors clustered at the appellate court level. Full event-study plots in Appendix B.2.

5.4 Mechanism: Appellate Softening versus Trial-Level Improvement

The decline in the reversal-or-remand rate documented in Section 5.1 is consistent with two competing interpretations that carry sharply different welfare implications. Under the first, the reassigned judge improves trial-level adjudication—transmitting legal knowledge and administrative experience to colleagues in the receiving court—so that appellate courts intervene less because fewer decisions contain correctable error.

Under the second, appellate courts soften their scrutiny of cases originating from courts led by reassigned judges, reducing intervention without any underlying improvement in decision quality. This section distinguishes between these interpretations using two bodies of evidence: the internal structure of the decline and characteristics of surviving remand opinions (Section 5.4.1), and measures of trial-level adjudication quality (Section 5.4.2).

5.4.1 Appellate Softening: Outcome Structure and Opinion Evidence

The aggregate reversal-or-remand rate combines two institutionally distinct forms of appellate intervention that differ fundamentally in what they assert about the trial court. The statutory framework is explicit. Under Article 177 of the Civil Procedure Law, the appellate court must reverse when trial-court error is unambiguous—when legal application is clearly wrong, or when the facts are already established and the conclusion is demonstrably incorrect. It must remand when the facts are "unclear or the evidence insufficient," meaning the trial court's analysis is contestable but not definitively wrong.[12] The critical distinction is therefore not between factual and legal error, but between *certain* error and *uncertain* error: reversal declares that the trial court was unambiguously wrong, while remand signals only that something is contestable and warrants further examination. Because both outcomes enter the official performance metric for trial judges, knowledge diffusion—if it operates—should reduce detectable error on both dimensions. The knowledge diffusion interpretation therefore predicts that reversal and remand rates decline in tandem following reallocation.

Figure 5 reports the estimated event-study coefficients for each outcome separately. The two panels share the same vertical scale to facilitate direct comparison, and the asymmetry is stark. For the remand rate, the pre-treatment coefficients are small and statistically indistinguishable from zero throughout the pre-reallocation window. Following reallocation, the remand rate falls immediately and persistently, with coefficients at $k = 0$ through $k = 5$ all statistically significant at conventional levels. The aggregate post-reallocation effect is -0.94 percentage points (SE $= 0.21$, $p < 0.001$), accounting for 91 percent of the main result of -1.03 percentage points for the combined outcome.[13] For the reversal rate, by contrast, the post-reallocation coefficients are scattered symmetrically around zero across all eight periods, displaying no systematic direction and no episode of sustained significance. The aggregate ATT is -0.09 percentage points (SE $= 0.27$, $p = 0.739$), with a confidence interval spanning nearly one percentage point in each direction, firmly ruling out any economically meaningful decline.[14] The decline in the reversal-or-remand rate is, in its entirety, a remand-rate phenomenon.

This asymmetry directly falsifies the knowledge diffusion interpretation. If the reassigned judge were improving trial-level adjudication—whether through her own caseload, mentoring, or diffusion of practice norms—both reversal and remand rates should decline in tandem, because neither type of error has an institutional reason to respond selectively to improvements in judicial capacity. The null result for reversal, sustained across eight post-reallocation periods and statistically distinguishable from the remand effect, is incompatible with any mechanism that operates through improvements in first-instance decision quality.

The asymmetry is, however, precisely what soft capture predicts. The institutional logic follows directly from the statutory distinction. Soft capture operates at the margin of uncertainty—where the appellate court retains genuine discretion and where the accountability cost of accommodation is lowest. An appellate court that maintains a judgment it would otherwise have reversed is knowingly preserving an unambiguous mistake—one the losing party can appeal further, that supervising courts can identify on review, and that China's lifelong judicial accountability system (*sifa zeren zhongshen zhi*), under which judges bear permanent

[12] *Zhonghua Renmin Gongheguo Minsupingsong Fa* [Civil Procedure Law of the People's Republic of China] (2021 revision), art. 177, paras. 2–3. Note that the relevant provision appeared as Article 153 in the 2007 revision; the substantive standard is unchanged.

[13] The two components sum arithmetically: $-0.94 + (-0.09) = -1.03$ percentage points, exactly replicating the main ATT.

[14] One exception warrants note: the coefficient at $k = 0$ for the reversal rate is positive and marginally significant ($+0.73$ pp, $p = 0.059$), suggesting a transient increase in detectable trial-court error around the time of arrival. One possible interpretation, consistent with the cadre management literature on leadership transition effects (Guo, 2009), is that the transition period between departing and arriving court leadership creates a temporary accountability vacuum: ordinary judges may exploit the gap—after the previous leader has left but before the new one has established oversight—to process cases with less scrutiny. This remains speculative, as we cannot directly test this mechanism with our data. What is clear is that if anything, a temporary decline in trial-level quality at $k = 0$ makes the subsequent sustained fall in appellate intervention more striking rather than less: appellate intervention declines persistently from a baseline that was, if anything, elevated.

responsibility for the decisions they render,[15] can trace directly back to the appellate judge's decision. The accountability cost of softening at the reversal margin is therefore high and verifiable: the error is certain, the record is traceable, and the responsibility is attributable.

At the remand margin, the calculus is fundamentally different. The appellate court that declines to remand a contestable case instead upholds it on the ground that the facts were "sufficiently established"—a judgment that is inherently subjective and practically unreviewable after the fact. No auditable record of error is generated, because the question of whether the evidence was adequate admits no objective answer. The accountability cost of softening at the remand margin is therefore low and practically unenforceable: the error is uncertain, the standard is subjective, and the responsibility is diffuse.

This maps directly onto the model's central parameter. Proposition 1 (Appendix A) establishes that soft capture arises in equilibrium when $k/(1-k)B < 1$—when the probability of reputational or disciplinary cost k is sufficiently low relative to the career value B of maintaining favorable relations with a high-potential future leader. The institutional analysis above implies that k differs systematically across the two intervention margins: high at the reversal margin, where error is certain and accountability is traceable, and low at the remand margin, where the triggering standard is subjective and the decision is effectively unreviewable. The condition for soft capture is satisfied at the remand margin but not at the reversal margin. The data confirm this prediction exactly.

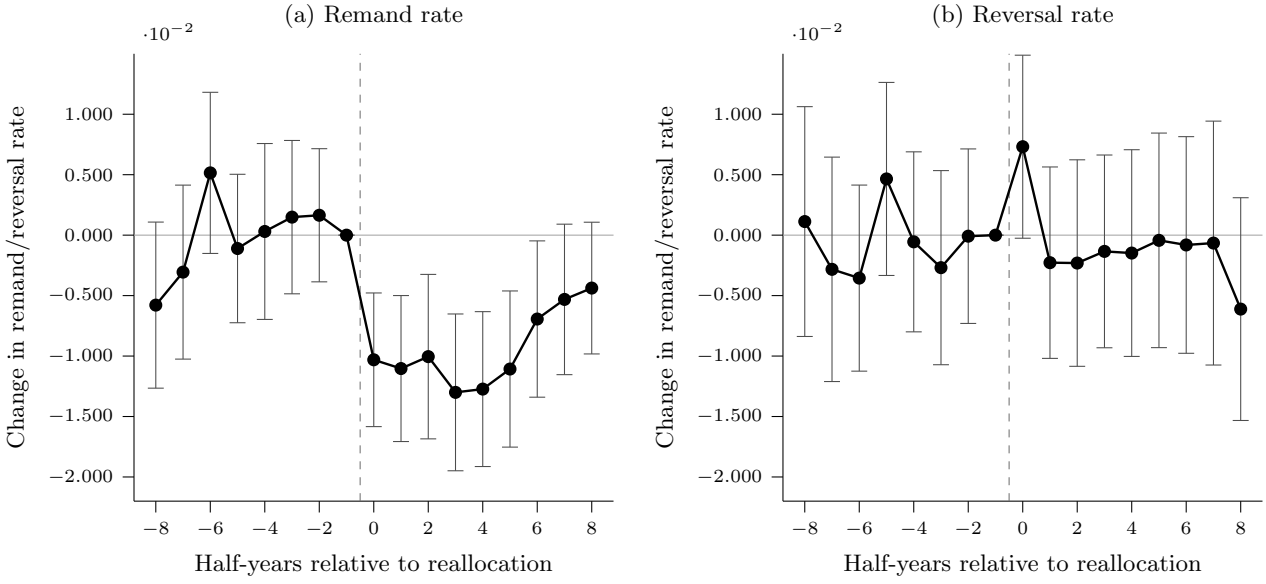


Figure 5: **Decomposition of the main effect: remand rate and reversal rate.** Each panel plots Sun–Abraham interaction-weighted estimates of $\hat{\beta}_k$ from equation (1), estimated separately for the remand rate (panel a) and the reversal rate (panel b). Both panels share the same vertical scale to facilitate direct comparison. Vertical bars denote 95% confidence intervals. The dashed vertical line separates pre- and post-reallocation periods; the omitted period is $k = -1$. The aggregate post-reallocation ATT is -0.94 pp (SE $= 0.21$, $p < 0.001$) for the remand rate and -0.09 pp (SE $= 0.27$, $p = 0.739$) for the reversal rate. Standard errors clustered at the appellate court level (380 clusters). $N = 43{,}470$ court–half-year observations.

The outcome decomposition establishes where soft capture operates. We next ask whether the written opinions in surviving remand cases show corresponding signs of behavioral adjustment. The evidence here is corroborating rather than independently identifying: the conditional remand sample limits causal inference, and the primary identification rests on the outcome decomposition above.

Appendix Figure A11 reports standardized average post-reallocation effects for two opinion-level mea-

[15]The Supreme People's Court formalized this framework in September 2015. See Supreme People's Court, *Opinions on Improving the Judicial Accountability System in the People's Courts* (*Guanyu Wanshan Renmin Fayuan Sifa Zerenzhi de Ruogan Yijian*), September 2015.

sures estimated on the remand subsample.[16] References to the first-instance judgment in the factual section of remand opinions increase significantly following reallocation (ATT = +0.084 references, SE = 0.040, $p = 0.038$), with the effect concentrated in the periods immediately following reallocation and pre-treatment coefficients clean throughout. The length of the factual section also increases, with an average post-reallocation effect of +27.5 characters (SE = 14.1, $p = 0.051$), driven primarily by a large and significant coefficient at $k = 1$ (+71.9 characters, $p = 0.025$) that attenuates over subsequent periods.

Under China's judicial evaluation framework, courts are expected to balance legal, social, and political effects in their decisions—the so-called "three-effect standard" (*sange xiaoguo*).[17] When remand is issued in contested cases, appellate courts face institutional pressure to ground the decision in detailed factual recitation and explicit engagement with the first-instance record. As soft capture raises the threshold for remand, the cases that survive are those with more severe factual disputes, more active litigation by parties, and more complex evidentiary records—precisely the cases that require more extensive factual documentation and closer engagement with the first-instance judgment to justify the remand decision. The increase in factual length and first-instance citation is therefore consistent with both the changed composition of the remand pool and the institutional demands placed on appellate courts when they do intervene in harder cases. Either interpretation points in the same direction: the behavioral pattern in surviving remand opinions is a downstream consequence of soft capture compressing the pool of remanded cases toward those with more severe and complex factual disputes.

5.4.2 Trial-Level Adjudication Quality

If the decline in appellate intervention reflects improvements in trial-level adjudication rather than reduced appellate scrutiny, then the quality of first-instance decisions should improve following reallocation. To evaluate this, we examine a set of text-based measures derived from trial-court judgments, constructed to capture two conceptually distinct dimensions along which knowledge diffusion, if it operates, should produce measurable gains.

The first dimension concerns the depth of independent legal reasoning. We measure this through the ratio of reasoning to factual content in written judgments (*reason intensity*), the share of reasoning text that consists of the judge's own analysis after removing statutory citations, dates, and numerical content (*subjective reasoning ratio*), and the density of logical connectives that structure the argumentative flow of the opinion (*logic word count*). Together, these measures ask whether trial-court judges are investing more analytical effort in their written decisions—the most direct channel through which transferred expertise would manifest.

The second dimension concerns the precision of legal citation. We measure this through the number of statutory provisions cited per judgment (*law reference count*), the share of citations directed at specific operative provisions rather than general or discretionary clauses (*article specificity ratio*), and conversely, the share of citations drawing on abstract or catch-all provisions (*abstract law citation rate*), for which lower values indicate more precise legal reasoning. These measures capture whether trial-court judges are applying the law with greater accuracy and specificity—a second channel through which knowledge diffusion should improve decision quality.

Figure 6 reports the standardized average post-reallocation effects for all six measures.[18] Across both dimensions and all six measures, the estimated post-reallocation effects are small, statistically indistinguishable from zero, and show no consistent direction. Point estimates range from −0.028 to +0.033 standard deviations, and no individual coefficient approaches conventional significance levels. Pre-treatment coefficients are clean throughout, confirming that the null results reflect genuine absence of post-reallocation change rather than pre-existing differences between treated and control courts.

The consistency of the null pattern across qualitatively distinct measures is informative. Reasoning depth

[16] Standardized effects are computed as ATT divided by the residual standard deviation (RMSE) from the event-study regression, enabling comparison across measures with different units. Full event-study plots are reported in Appendix B.3.

[17] Under this standard, courts are expected to evaluate rulings not only for legal correctness but also for their broader social and political consequences. Liebman (2014) documents how the stability imperative shapes judicial decision-making across Chinese courts, and notes that appellate outcomes in contested cases carry institutional significance beyond their legal content; see also Minzner (2015) on the politicization of judicial performance metrics under this framework.

[18] Standardized effects are computed as ATT divided by the residual standard deviation (RMSE) from the event-study regression, enabling comparison across measures with different units.

and citation precision capture different aspects of adjudicatory quality and would be expected to respond to different types of expertise transfer. Their simultaneous absence of movement is difficult to reconcile with knowledge diffusion operating through any plausible channel—whether direct mentorship, exposure to higher-level judicial practice, or administrative guidance from a more experienced court leader. Taken together with the outcome decomposition in Section 5.4.1, the evidence consistently points to an adjustment at the appellate stage rather than an improvement at the trial stage.

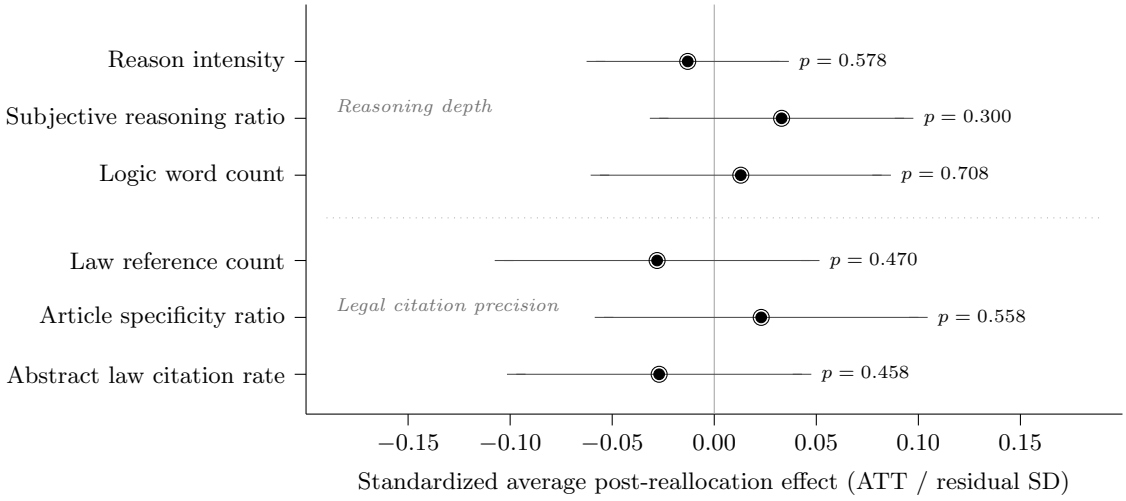


Figure 6: **Trial-level adjudication quality: no improvement following reallocation.** Each point reports the standardized average post-reallocation effect (ATT divided by residual standard deviation) for the indicated measure of first-instance decision quality, estimated using the Sun–Abraham interaction-weighted estimator. Whiskers denote 95% confidence intervals. Measures are grouped into two conceptual dimensions: reasoning depth (top panel) and legal citation precision (bottom panel). The zero line marks no change relative to never-treated courts. Pre-treatment coefficients are statistically indistinguishable from zero for all six measures. Standard errors clustered at the appellate court level (380 clusters). $N = 45{,}031$ court–half-year observations; $N_{\text{treated}} = 12{,}764$.

6 Implications

This paper documents a robust and persistent decline in the appellate reversal-or-remand rate following downward judicial reallocation within hierarchically affiliated court pairs. The estimated average treatment effect of -1.03 percentage points, sustained across five consecutive periods at the 5 percent significance level, represents a reduction of approximately 3.9 percent relative to the pre-treatment baseline. The decline is concentrated entirely at the remand margin—where appellate discretion is widest and accountability for leniency is lowest—while reversal rates are unaffected, directly implicating appellate softening rather than improvements in trial-level adjudication. The effect is tightly bounded by the conditions the theory identifies: career-relevant reallocation, hierarchical affiliation, and alignment between the career evaluation channel and the adjudication domain being measured. This section draws out the implications of these findings for institutional theory, welfare analysis, and institutional design.

6.1 Soft Capture: De Facto Independence and the Limits of Formal Oversight

Soft capture does not require a corrupt actor or a dysfunctional institution. Each actor responds rationally within the rules: the appellate judge exercises discretion within a genuinely subjective standard, her judgment cannot be proven wrong, and no formal record of error is generated. There is no identifiable misconduct—and that is precisely the point. Because there is no bad actor to remove and no rule violation to sanction,

soft capture cannot be solved by selecting better people or tightening procedural rules. It requires changing the institutional conditions under which monitoring occurs.

The empirical evidence in this paper shows how completely formal authority can remain intact while substantive oversight erodes. Throughout the study period, appellate courts retained full legal authority to reverse or remand trial-court decisions. No standards changed, no jurisdictional boundaries were altered, no procedures were modified. What changed was the career relationship between appellate judges and the trial courts they reviewed—and that change alone was sufficient to generate a persistent and institutionally bounded decline in oversight. The decomposition in Section 5.4.1 is particularly revealing: soft capture operates precisely at the remand margin, where the triggering standard is subjective and accountability is low, while the reversal margin—where error is unambiguous and the record is traceable—is left undisturbed. Appellate courts did not abandon oversight wholesale; they adjusted it precisely where formal authority offered the most cover. The opinion evidence reinforces this picture: factual sections grew longer and citations to first-instance reasoning increased, while independent legal analysis remained unchanged. The formal architecture of oversight was preserved; its substantive content was quietly redirected.

This pattern is not easily addressed by the standard toolkit of institutional safeguards—judicial tenure, salary protection, procedural independence. Each of these instruments secures formal independence: insulation from external pressure, protection from removal, procedural barriers to political interference. None of them touches the internal career linkages through which soft capture operates. The existing scholarship on institutional independence has focused on external threats—political pressure, capture by regulated industries, ideological alignment (Feld and Voigt, 2003)—and the standard reforms target those threats. The present results show that internal personnel governance can generate comparable and arguably more insidious effects, because soft capture is harder to detect, harder to attribute, and not visible in the formal record. Formal independence is a necessary but not sufficient condition for effective oversight. *De facto* separation of career incentives is what makes oversight function in practice.

6.2 Welfare Implications

The welfare implications of soft capture depend on the source of the observed decline in appellate intervention. If reduced intervention reflects improvements in trial-level adjudication, the welfare implications would be largely positive: fewer corrections would signal higher-quality first-instance decisions, lower litigation costs, and more efficient use of appellate resources. The evidence in Section 5.4 rules out this interpretation. Trial-level quality measures show no systematic improvement following reallocation, and the decline in appellate intervention is concentrated entirely at the remand margin—precisely where appellate discretion is widest and where accountability for leniency is lowest. The welfare loss from soft capture therefore operates through two distinct channels.

The first and more direct channel is the suppression of error correction. Under China's "second instance is the last instance" system, litigants have exactly one opportunity to appeal a first-instance judgment. The appellate court's decision is final and cannot be further appealed; the only remaining avenue for relief is an application for retrial under the trial supervision procedure, which carries a high threshold and low success rate. Within this institutional framework, remand decisions occupy a uniquely important position: they are the sole mechanism through which a case with unresolved evidentiary disputes can be returned for more careful examination. When soft capture compresses the remand margin, these cases are instead upheld, permanently foreclosing the opportunity for correction.

To illustrate the scale of this effect, we apply the estimated post-reallocation decline of 0.94 percentage points in the remand rate to the pre-treatment average of 146 appellate cases per court per half-year. The pre-treatment remand rate among treated courts is 8.57 percent, so the estimated decline corresponds to a relative reduction of approximately 11 percent—a compression that is concentrated entirely in the most consequential form of appellate relief available to litigants. In absolute terms, each treated court issues approximately 1.4 fewer remand decisions per half-year relative to the counterfactual. Sustained over the typical four-year rotation window, this amounts to roughly 11 fewer remands per affected court. Aggregated across the 756 unique basic courts in our sample that experienced at least one downward reallocation, the cumulative reduction amounts to approximately 8,300 remand decisions over the post-reallocation window—roughly 3.2 percent of the national annual remand volume reported in official court statistics for the same period. This figure is bounded to the courts directly affected by reallocation in our sample, but within those

courts the effect is operationally significant: each of these cases represents a litigant who permanently lost the opportunity for appellate correction that the legal system nominally provides.

The suppression of error correction documented above represents a welfare loss that is both direct and cumulative, and that requires no improper intent or observable misconduct on either side.[19] The welfare cost follows directly from the structural interaction of hierarchical career governance and appellate evaluation design. This has an important institutional implication: it cannot be addressed by disciplining individual judges or tightening procedural rules. It requires changing the conditions under which monitoring occurs, a point we develop in the following section.

6.3 Implications for Institutional Design

The boundary conditions documented in Section 5.3 directly reveal the institutional architecture through which soft capture operates. The effect requires three things simultaneously: a recognizable career signal, hierarchical affiliation between sending and receiving courts, and an evaluation system that links appellate outcomes to the career prospects of the judge under review. Removing any one of these conditions eliminates the effect. This conditionality implies that attenuation requires disaggregating this configuration—separating career governance from the oversight function with which it is currently coupled.

Three reform levers follow directly, though each carries a structural cost. Broadening the reallocation pool—making downward movement a routine deployment rather than a concentrated leadership signal—would dilute the organizational cue that triggers accommodation, but would simultaneously destroy the screening function the practice is designed to serve: downward reallocation to a leadership position is a scarce resource, reserved for the critical first transition from ordinary judge to court leader, and extending it indiscriminately would undermine its training and evaluation purpose. Cross-jurisdictional rotation—placing judges outside the direct supervisory hierarchy of the sending court—would sever the organizational linkage that gives appellate courts both the capacity and the incentive to accommodate, but would also destroy the supervisory continuity through which higher courts observe performance and make promotion decisions. Decoupling appellate outcomes from the performance evaluation of judges under active leadership consideration would remove the career rationale for accommodation, but appellate outcomes are simultaneously the primary metric through which trial-court quality is monitored system-wide, and removing them from evaluation would weaken accountability incentives for the broader judicial corps.

The pattern across all three levers points to the same conclusion. Personnel governance and appellate oversight currently share the same institutional machinery—the same hierarchy, the same evaluation metrics, the same career pipeline. Soft capture is an equilibrium consequence of this coupling, not a policy mistake that can be corrected by adjusting individual parameters. The only durable solution is structural separation: designing career governance and oversight as institutionally distinct systems with separate evaluation criteria, separate supervisory relationships, and separate career horizons.

This logic extends beyond courts and beyond China. The structural condition for soft capture—vertical monitoring combined with integrated career governance—is present wherever hierarchical organizations use the same personnel system to govern the trajectories of both monitors and monitored. The design implication is general: de facto career independence cannot be assumed from formal separation of authority; it must be explicitly built into institutional architecture as a first-order design criterion.

[19] The model identifies a second welfare channel through Lemma 2: when trial judges anticipate relaxed appellate scrutiny, their incentives to exert effort in adjudication are weakened. This anticipatory effort distortion does not require any communication between trial and appellate judges—it arises whenever the expected level of scrutiny shifts in a predictable direction. We cannot directly measure this second-order effect, as our trial-level quality measures capture average court-level behavior rather than the effort choices of individual judges facing changed incentives. Its theoretical presence implies that the welfare cost of soft capture may extend beyond the cases directly affected by reduced remand to the broader stock of trial-court decisions produced under weakened review expectations.

Appendix A Formal Model

This appendix develops the formal model summarized in Section 3. The model is intended as an illustrative abstraction that disciplines the soft capture mechanism and yields sharp testable predictions. All proofs follow by backward induction.

A.1 Setup

Players. A reassigned judge (RJ) chooses effort level $e \geq 0$, capturing the intensity of case handling and performance-relevant activities. An appellate judge (AJ) subsequently chooses the level of scrutiny applied in review.

Reassigned judge. The RJ maximizes the probability of future promotion net of effort costs:

$$\max_{e} \; V_{\mathrm{RJ}}(e, F) = \pi(e, F) - e, \tag{2}$$

where $\pi(e, F)$ is the promotion probability. I specify

$$\pi(e, F) = p(e) + [1 - p(e)]F, \tag{3}$$

with $p(e)$ increasing and concave in e, and $F \in \{0, f\}$ where $F = 0$ denotes standard scrutiny and $F = f > 0$ denotes reduced scrutiny. The term $[1-p(e)]F$ captures the marginal effect of appellate leniency on promotion prospects.

Appellate judge. The AJ chooses F to maximize expected utility:

$$\max_{F} \; U_{\mathrm{AJ}} = \begin{cases} 1, & \text{if } F = 0, \\ (1-k)\big[1 + \pi(e, f)B\big], & \text{if } F = f, \end{cases} \tag{4}$$

where $k \in (0, 1)$ is the probability of reputational or disciplinary cost from applying reduced scrutiny, and $B > 0$ is the expected career value of maintaining favorable relations with a judge widely regarded as a future leader. Standard scrutiny ($F = 0$) yields utility normalized to one.

Timing. The RJ selects effort e; the AJ then observes case outcomes and chooses scrutiny level F.

A.2 Analysis and Equilibrium

Step 1: AJ's scrutiny choice

The AJ applies reduced scrutiny ($F = f$) whenever $U_{\mathrm{AJ}}^{F=f} \geq U_{\mathrm{AJ}}^{F=0}$, which requires:

$$\pi(e, f) \geq \frac{k}{(1-k)B}. \tag{5}$$

Because $\pi(e, f) \in [0, 1]$, a necessary condition is $k/(1-k)B \leq 1$. Substituting $\pi(e, f) = p(e) + [1 - p(e)]f$ and rearranging yields

$$p(e) \geq \frac{k/(1-k)B - f}{1 - f}.$$

Because $p(e)$ is strictly increasing in e, this defines a unique threshold

$$\hat{e} = p^{-1}\left(\frac{k/(1-k)B - f}{1 - f}\right),$$

such that the AJ applies reduced scrutiny if and only if $e \geq \hat{e}$.

Step 2: RJ's effort choice

Under standard scrutiny ($F = 0$): The RJ solves $\max_e \; p(e) - e$. The first-order condition yields $p'(e_0) = 1$.

Under reduced scrutiny ($F = f$): The RJ solves $\max_e \; (1-f)p(e) + f - e$. The first-order condition yields $p'(e_f) = 1/(1-f)$. Since $1/(1-f) > 1$ and $p'' < 0$, it follows that $e_f < e_0$: anticipated leniency weakly lowers the RJ's effort.

Step 3: Equilibrium characterization

Since $e_f < e_0$ always holds, three cases arise:

Case 1: $\hat{e} < e_f < e_0$**.** The AJ applies reduced scrutiny for any effort the RJ might choose; the RJ chooses $e = e_f$. Equilibrium: (e_f, f).

Case 2: $e_f < e_0 < \hat{e}$**.** The AJ applies standard scrutiny; the RJ chooses $e = e_0$. Equilibrium: $(e_0, 0)$.

Case 3: $e_f < \hat{e} < e_0$**.** No pure-strategy equilibrium exists at e_f or e_0. For $e < \hat{e}$ the AJ applies standard scrutiny and $\partial V_{\mathrm{RJ}}/\partial e > 0$; for $e > \hat{e}$ the AJ applies reduced scrutiny and $\partial V_{\mathrm{RJ}}/\partial e < 0$. The unique equilibrium has the RJ choose $e = \hat{e}$, at which the AJ is indifferent and applies $F = f$.

Proposition 1 (Soft Capture in Equilibrium)**.** *Reduced appellate scrutiny arises in equilibrium (Cases 1 and 3) when*

$$\frac{k}{(1-k)B} < 1.$$

In all equilibria with $F = f$, the reversal-or-remand rate declines without any improvement in trial-level adjudication quality: the RJ's effort is weakly lower than under standard scrutiny, and the reduction in observed reversals and remands reflects the AJ's discretionary choice rather than the RJ's performance. □

The condition $k/(1-k)B < 1$ is most readily satisfied when monitoring risk k is low—as within hierarchically affiliated courts where the appellate judge shares organizational futures with the reassigned judge—and when the career value B is large—as when the reassigned judge is widely recognized as a future leader. This delivers the boundary conditions tested in Section 5.3: removing hierarchical affiliation raises k; the absence of a recognizable leadership signal reduces B; both attenuate or eliminate soft capture.

Appendix B Additional Results and Robustness

B.1 Event-Study Estimates under Alternative Specifications

Table A1: Event-Study Estimates under Alternative Specifications

	Dependent variable: Reversal-or-remand rate				
Event time	Baseline	Earliest timing	Latest timing	Min caseload ≥ 5	No max-gap restriction
Pre-event					
$k = -3$	−0.0012	0.0028	0.0005	0.0017	0.0033
	(0.0048)	(0.0049)	(0.0045)	(0.0055)	(0.0041)
Event period					
$k = 0$	−0.0030	0.0068*	−0.0023	−0.0087	0.0001
	(0.0047)	(0.0041)	(0.0043)	(0.0054)	(0.0039)
Post-event					
$k = 1$	−0.0133***	−0.0076	−0.0079*	−0.0128**	−0.0093**
	(0.0049)	(0.0051)	(0.0047)	(0.0057)	(0.0039)
$k = 3$	−0.0143***	−0.0152***	−0.0125**	−0.0129**	−0.0077*
	(0.0048)	(0.0049)	(0.0054)	(0.0056)	(0.0041)
Aggregate ATT	−0.0103***	−0.0081**	−0.0098***	−0.0096**	−0.0066**
	(0.0034)	(0.0032)	(0.0032)	(0.0038)	(0.0027)
Treated obs.	12,477	12,477	12,477	9,105	18,166
Total obs.	43,470	43,470	43,470	43,470	43,470

Notes: Each column reports coefficients from a staggered difference-in-differences event-study specification using the Sun–Abraham (2021) interaction-weighted estimator. Robust standard errors in parentheses, clustered at the appellate court level (380 clusters). Aggregate ATT is the inverse-variance weighted average of post-event coefficients. Event time k is in half-years relative to reallocation; $k = -1$ is the omitted period. The earliest timing definition uses the last ruling in the sending court as event date; the latest definition uses the first ruling in the receiving court. The min-caseload ≥ 5 specification imposes a conservative minimum of five adjudicated cases per court. The no-max-gap specification removes the upper bound on the transition gap. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

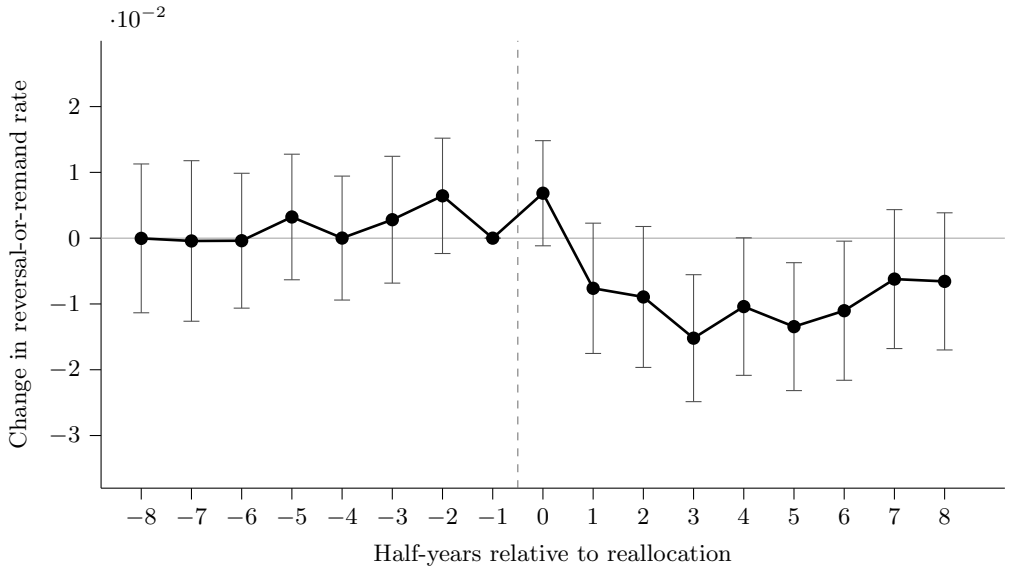


Figure A1: **Earliest timing definition.** Pre-trends are clean throughout ($k = -8$ to $k = -2$). The coefficient at $k = 0$ is positive and marginally significant ($p = 0.095$), reflecting that some judges still have unfinished rulings in the sending court when the earliest timing date is assigned. Aggregate ATT $= -0.81$ pp ($p = 0.012$).

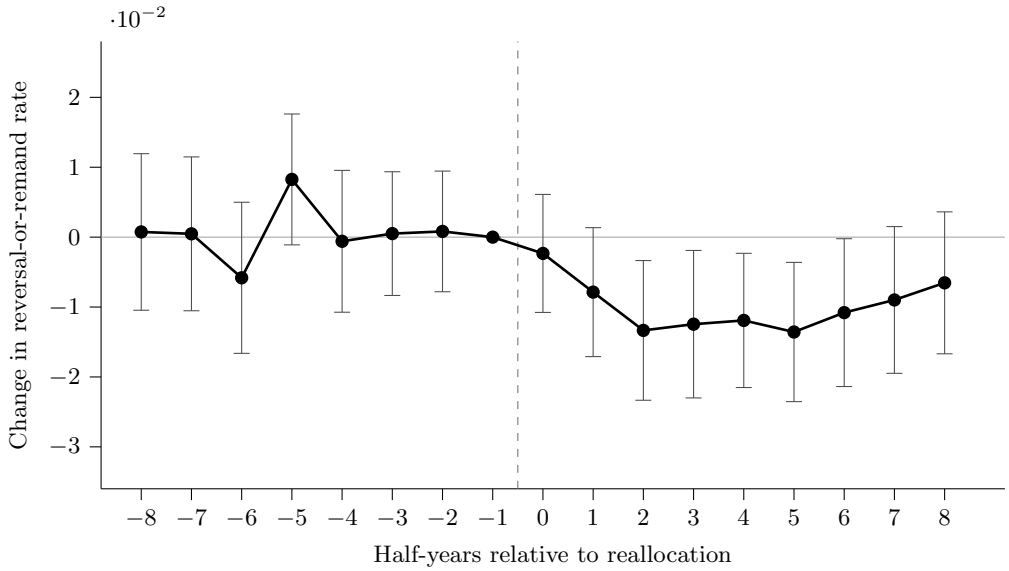


Figure A2: **Latest timing definition.** Pre-trends are clean throughout. The effect is more precisely estimated and persistent relative to the baseline, consistent with the latest timing definition most accurately capturing the date at which the career linkage becomes operationally salient. Aggregate ATT $= -0.98$ pp ($p = 0.002$).

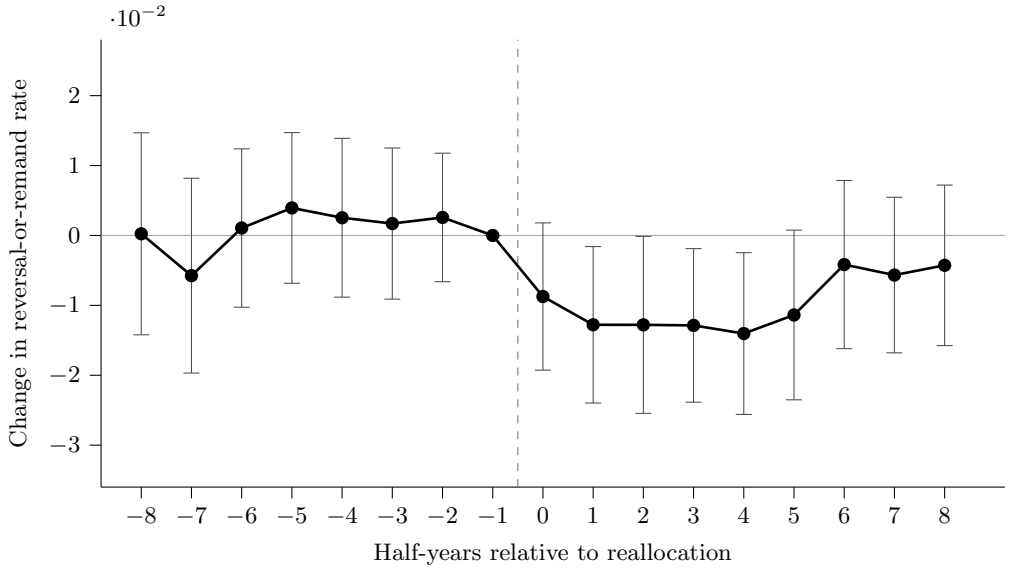


Figure A3: **Minimum caseload restriction (≥ 5 cases).** Pre-trends are clean. Results are nearly identical to the baseline in both magnitude and timing. Aggregate ATT $= -0.96$ pp ($p = 0.012$). $N_{\text{treated}} = 9{,}105$.

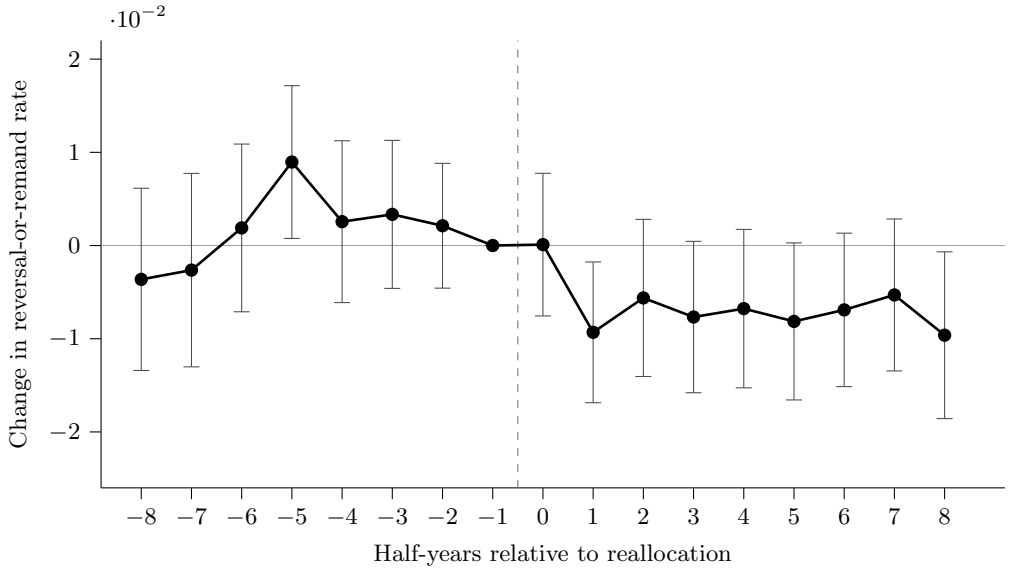


Figure A4: **No maximum transition gap restriction.** The coefficient at $k = -5$ is positive and significant ($p = 0.033$), reflecting measurement noise from judges with very long adjudicatory gaps. The remaining pre-period coefficients are indistinguishable from zero, and the aggregate post-period ATT is negative and significant. Aggregate ATT $= -0.66$ pp ($p = 0.017$). $N_{\text{treated}} = 18{,}166$.

B.2 Event-Study Figures: Boundary Condition Tests

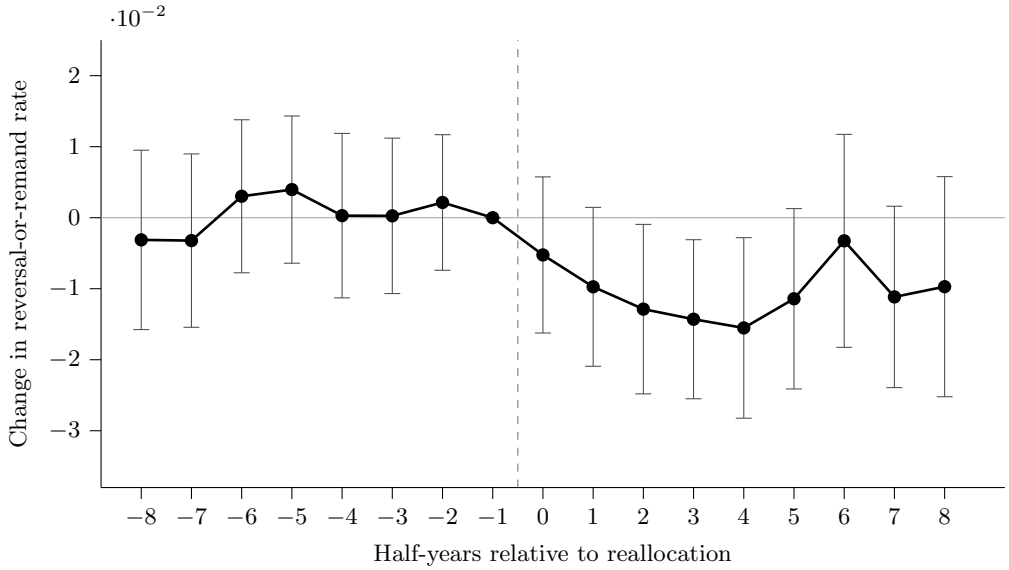


Figure A5: **Confirmed leaders.** Event-study estimates for the 747 judge movements verified as court president or vice president appointments. Pre-trends are clean throughout. Aggregate ATT = −1.04 pp ($p = 0.008$). Standard errors clustered at the appellate court level (378 clusters). $N_{\text{treated}} = 8{,}681$.

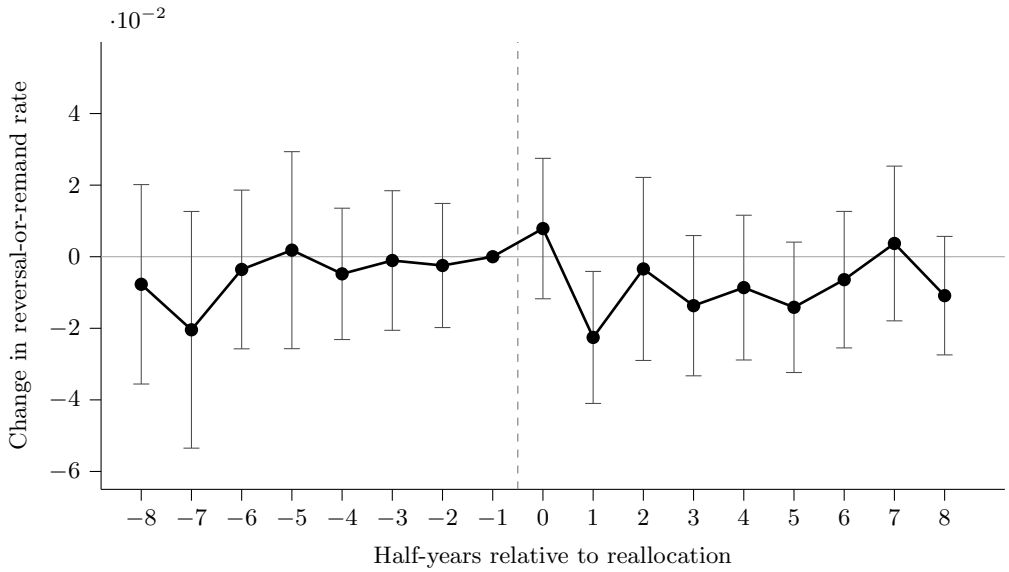


Figure A6: **Unconfirmed movements.** Event-study estimates for the 407 movements for which no leadership appointment could be verified. Pre-trends are clean. Post-reallocation coefficients display no consistent direction. Aggregate ATT = −0.76 pp ($p = 0.236$). Standard errors clustered at the appellate court level (374 clusters). $N_{\text{treated}} = 2{,}743$.

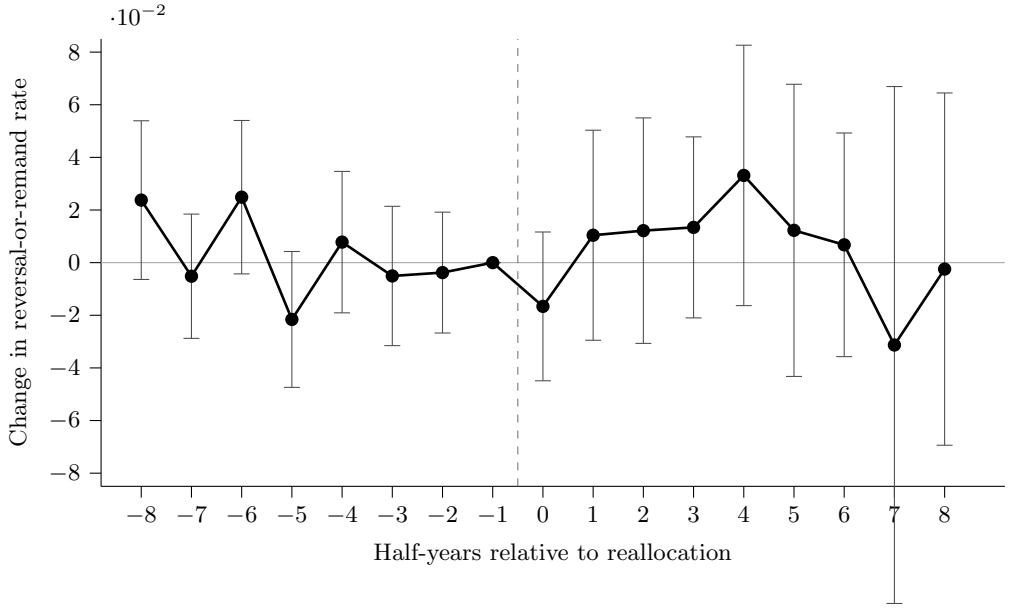


Figure A7: **Judicial assistant reallocation as a placebo.** Event-study estimates for downward reallocation of judicial assistants from intermediate to basic courts. These movements do not signal entry into the leadership cultivation pipeline. Aggregate ATT = +0.42 pp ($p = 0.797$). Standard errors clustered at the appellate court level (369 clusters). $N_{\text{treated}} = 786$.

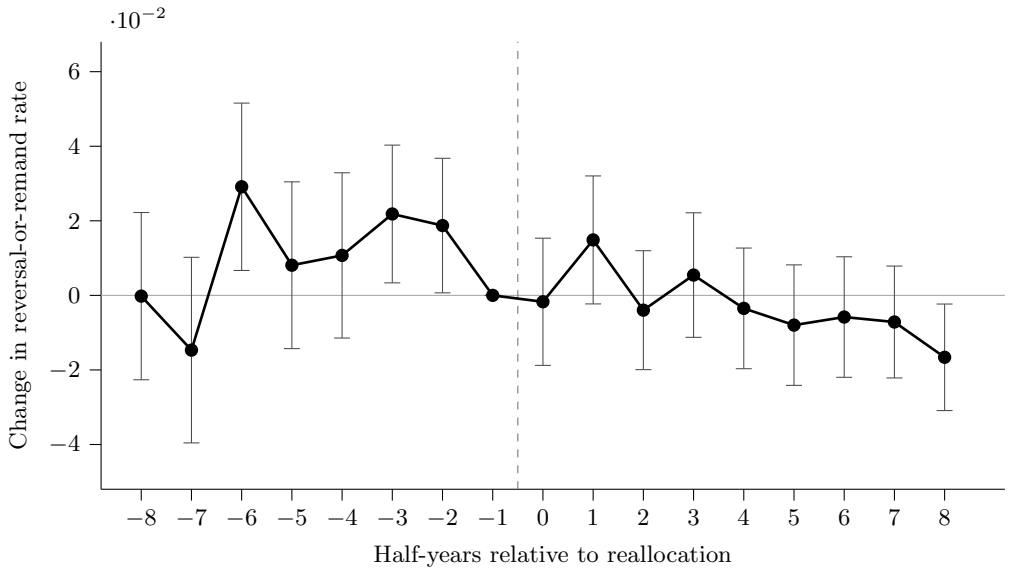


Figure A8: **Non-affiliated court movements.** Event-study estimates for downward movements in which the reassigned judge originates from an intermediate court that is not the direct administrative superior of the receiving basic court. Several pre-reallocation coefficients are large and statistically significant ($k = -6$, -3, -2; all $p < 0.05$), indicating that this sample fails the parallel-trends assumption and reflects systematically atypical personnel decisions. Aggregate ATT $= -0.29$ pp ($p = 0.585$). Standard errors clustered at the appellate court level (369 clusters). $N_{\text{treated}} = 3{,}588$.

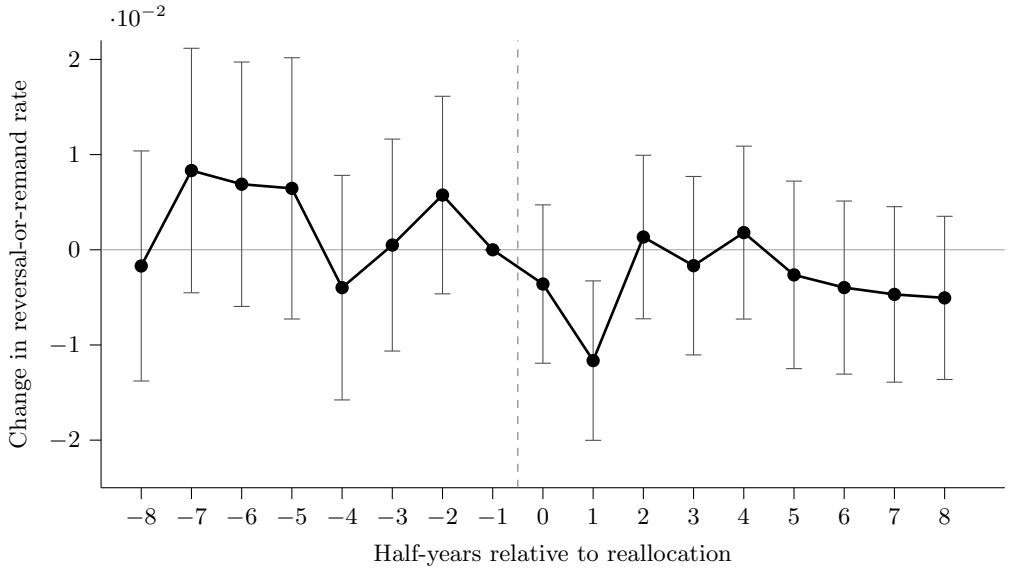


Figure A9: **Upward reallocation.** Event-study estimates for judges moving from basic to intermediate courts. Pre-trends are clean. Post-reallocation coefficients show no systematic direction. Aggregate ATT $= -0.33$ pp ($p = 0.212$). Standard errors clustered at the appellate court level (380 clusters). $N_{\text{treated}} = 12{,}069$.

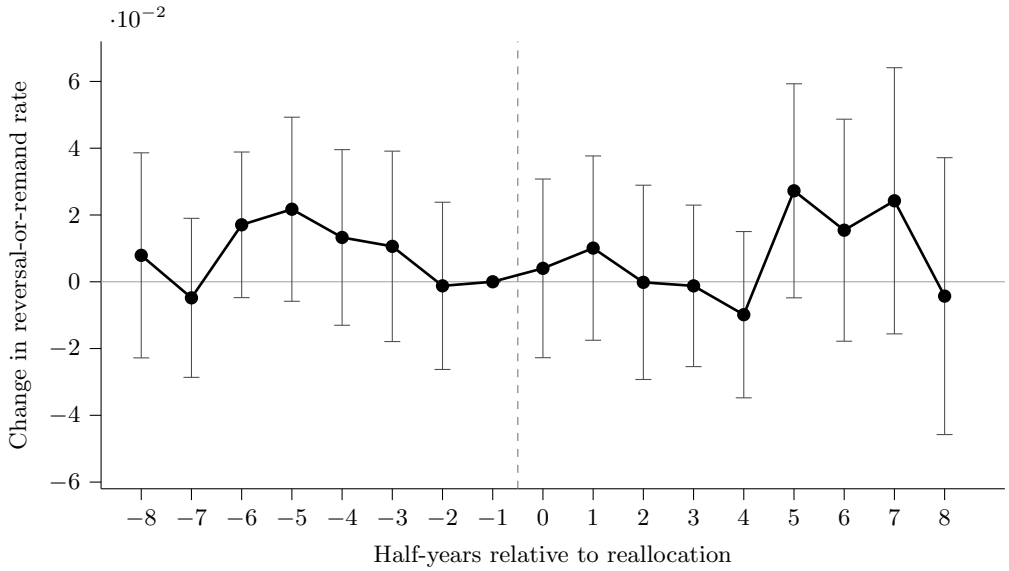


Figure A10: **Criminal-only channel.** Event-study estimates restricting the treatment sample to movements in which the reassigned judge handled only criminal cases at the sending intermediate court. Career evaluation does not operate through the civil adjudication domain in this setting. Aggregate ATT = +0.73 pp ($p = 0.442$). Standard errors clustered at the appellate court level (373 clusters). $N_{\text{treated}} = 1{,}635$.

B.3 Remand Case Opinion Characteristics

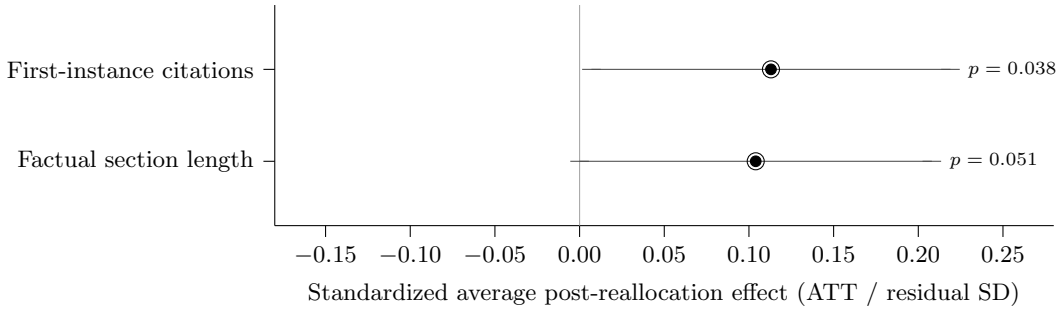


Figure A11: **Appellate opinion characteristics in remand cases.** Each point reports the standardized average post-reallocation effect (ATT divided by residual standard deviation) for the indicated opinion measure, estimated on the remand subsample using the Sun–Abraham interaction-weighted estimator. Whiskers denote 95% confidence intervals. Standard errors clustered at the appellate court level (340 clusters). $N = 18{,}726$ court–half-year observations; $N_{\text{treated}} = 5{,}933$.

B.4 Data Tables

Table A2: Judicial Position and Typical Judicial Ranks by Court Level

Court level	**Judicial position**	**Typical judicial ranks**
Supreme People's Court	President	Chief Justice
	Vice President	Class I Justice; Class II Justice
	Adjudication Committee	Class II Justice; Class I Senior Judge; Class II Senior Judge
	Division Head	Class II Justice; Class I Senior Judge; Class II Senior Judge
	Judge	Class I Senior Judge; Class II Senior Judge; Class III Senior Judge
High People's Court	President	Class I Justice; Class II Justice
	Vice President	Class I Senior Judge; Class II Senior Judge
	Adjudication Committee	Class II Senior Judge; Class III Senior Judge
	Division Head	Class II Senior Judge; Class III Senior Judge; Class IV Senior Judge
	Judge	Class III Senior Judge; Class IV Senior Judge
Intermediate People's Court	President	Class I Senior Judge; Class II Senior Judge
	Vice President	Class II Senior Judge; Class III Senior Judge
	Adjudication Committee	Class III Senior Judge; Class IV Senior Judge
	Division Head	Class III Senior Judge; Class IV Senior Judge; Class I Judge
	Judge	Class III Senior Judge; Class IV Senior Judge; Class I Judge; Class II Judge
Basic People's Court	President	Class III Senior Judge; Class IV Senior Judge
	Vice President	Class III Senior Judge; Class IV Senior Judge; Class I Judge
	Adjudication Committee	Class III Senior Judge; Class IV Senior Judge; Class I Judge; Class II Judge
	Division Head	Class IV Senior Judge; Class I Judge; Class II Judge; Class III Judge
	Judge	Class IV Senior Judge; Class I Judge; Class II Judge; Class III Judge; Class IV Judge; Class V Judge

Table A3: Distribution of Transition Gap between Courts

	N	Mean	SD	Min	p_{10}	p_{25}	Median	p_{75}	p_{90}	Max
Days	1,154	153.5	96.1	1	29	73	142	228	294	360

Notes: The transition gap is the number of calendar days between the judge's last recorded ruling in the sending intermediate court and the first recorded ruling in the receiving basic court. The reallocation date is approximated as the midpoint of this interval in the baseline specification. All values are positive and no judge exhibits a negative gap. Sample: 1,154 judge-movement events.

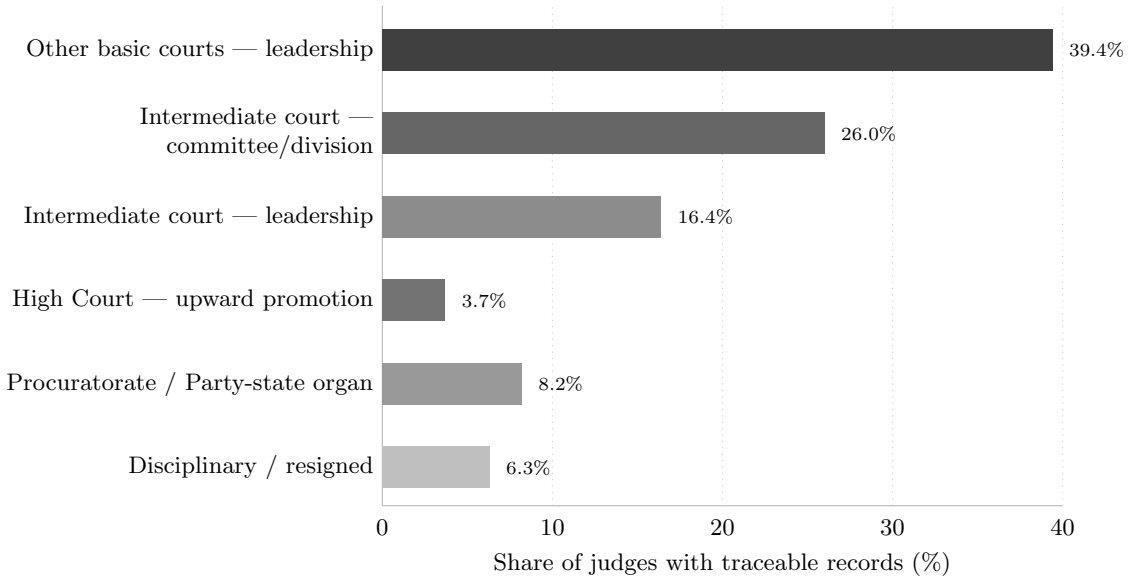


Figure A12: **Career trajectories of downward-reallocated judges.** Distribution of 269 observed post-reallocation positions among judges with traceable career records. Among those for whom subsequent appointments could be identified, the large majority assumed leadership or senior roles within the judiciary or Party-state apparatus. *Committee/division* includes adjudication committee members, division heads, and equivalent roles at intermediate courts.

Table A4: Annual Distribution of Judicial Reallocation Events

Year	N	Share (%)
2014	84	7.3
2015	96	8.3
2016	171	14.8
2017	165	14.3
2018	70	6.1
2019	117	10.1
2020	155	13.4
2021	233	20.2
2022	63	5.5
Total	1,154	100.0

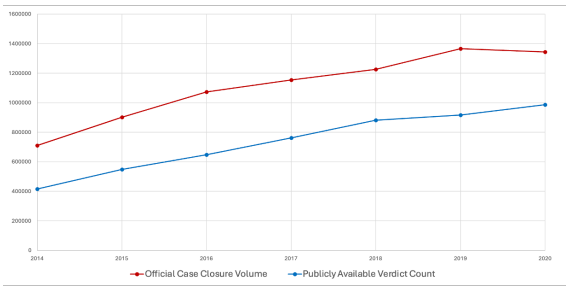
Notes: Year refers to the calendar year in which the reallocation is estimated to have occurred. The distribution shows two waves of increased reallocation activity: 2016–2017 and 2019–2021, broadly coinciding with cadre management reform cycles. The low count in 2022 reflects right-censoring of the observation window. Sample: 1,154 judge-movement events.

Table A5: Adjudication Domain Composition of Downward-Reallocated Judges

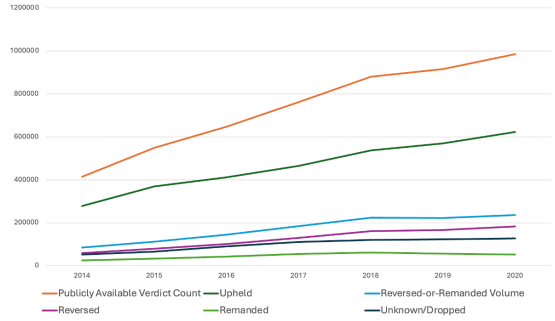
	N	Share (%)
Panel A. Sending intermediate court		
Civil cases only	527	45.7
Both civil and criminal	409	35.4
Criminal cases only	218	18.9
Total	1,154	100.0
Panel B. Receiving basic court		
Civil cases only	576	49.9
Both civil and criminal	409	35.4
Criminal cases only	169	14.6
Total	1,154	100.0

Notes: Unit of observation is the judge-movement event. Adjudication domain is defined by whether the judge's recorded caseload consists of civil cases only, criminal cases only, or both. Panel A characterizes activity in the sending intermediate court prior to reallocation; Panel B characterizes activity in the receiving basic court following reallocation. Sample: 1,154 judge-movement events.

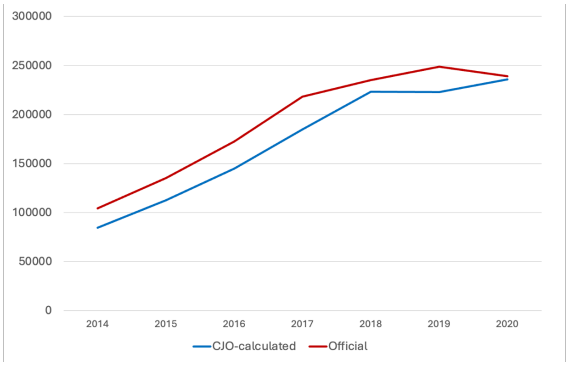
B.5 CJO Data Validation



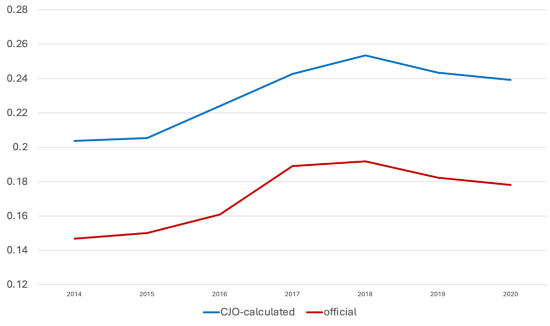
(a) Civil Case Volume (Official vs CJO)



(b) Appeal Case Outcome Distribution



(c) Reverse-or-Remand Volume (Official vs CJO)



(d) Reverse-or-Remand Rate (Official vs CJO)

Figure A13: **CJO data validation.** Case volumes and appellate outcome distributions in China Judgments Online (CJO) closely track official judicial statistics over the 2014–2020 study period. Reversal and remand rates in CJO are somewhat higher than official statistics, reflecting selective publication of more complex cases; this selection is absorbed by court fixed effects in the empirical specification.

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