

Dictators and Their Repressive Agents: Judicial Institution, Information Screening, and State Repression

Abstract. Rulers employ various repressive agents to enforce control. Yet, less is known about judiciaries as agents and how the principal-agent relationship between the ruler and judges impacts repression. We argue that courts are critical information filters to rulers. When the quantity of information overloads the ruler due to increased dissent, judges are often empowered to screen out less regime-threatening cases for the ruler to review. However, it creates a moral hazard problem where judges manipulate cases qualified for review to avoid decision rejection and sanctions, which hurts rulers' control and undermines repression. Using new data on judicial repression in Taiwan, we find that when the president only reviews cases above a severity threshold, judges become less likely to sentence dissidents above that threshold, and this distorted behavior is driven by judges' fear of sanctions. These findings improve our understanding of autocratic judiciaries and different aspects of moral hazard in dictatorships.

Keywords: State repression, autocratic judiciary, information screening, moral hazard, political trials

The top priority of states is to construct order and maintain political control. To achieve this goal, repression is a convenient tool for rulers to suppress political dissent and maintain control. In studying state repression and human rights abuses, scholars typically view repressive violence as rational decisions by the state. Acting as a rational and unitary actor, the state is assumed to strategically use violence against dissidents and for political objectives based on some cost-benefit calculation (Davenport, 2007; H.R. deMeritt, 2016; Ritter and Conrad, 2016). Yet, this rational-unitary-actor assumption underplays the fact that state apparatus is a hierarchy where the principal authorizes agents to exercise repression. More importantly, when executing repression orders from the principal, agents may choose to engage in behavior other than those they are explicitly ordered to undertake. This disobedience by agents can generate irrational and unintended repression outcomes that go against the ruler's interest in maintaining order (Cohen and Nordås, 2015; Carey and Mitchell, 2017; Slough and Fariss, 2021).

A growing body of recent scholarship has challenged the rational-unitary-actor assumption of the state, unpacking the repressive state into its constitute and studying principal-agent problems in repression. However, the attention has been largely on the security sectors, including the military sector (Tyson, 2018; Dragu and Przeworski, 2019; Fruge, 2019), militias (Mitchell, Carey and Butler, 2014; Cohen and Nordås, 2015; Carey and Mitchell, 2017), and the police (Hu and Conrad, 2020; Arriola et al., 2021). The agency problems in repression associated with the judicial institution have received scant attention because courts are generally regarded as little more than window dressing for rulers, and judges are believed to have little discretion on repression decisions (Moustafa, 2014). Hence, we know relatively little about how the principal-agent relationship between the ruler and judges can shape the supply of repression.

We direct research to examine the dynamic between the ruler and judges and how it shapes repression decisions. Specifically, we ask the following questions: Why do judges strategically sentence certain dissidents heavily while showing leniency to others? What

explains the variation in political sentencing when the dictator strives to oversee and control the jurisdiction? What are the conditions under which judges have more discretion on the degree of repression against dissidents? In answering these questions, we theorize the relationship between the principal (dictator) and agents (judges) and discuss mechanisms through which judicial empowerment initiated by the ruler to foster repression can ultimately undermine repression campaigns.

By conceptualizing the state as a hierarchy, we argue that the dynamic between the ruler and agents is not just a simple top-down command and execution relationship but also a bottom-up process where agents collect and screen information before the ruler makes decisions based on the processed information. The degree of information screening represents a trade-off between agent empowerment and control, and the key determinant is the rulers' information-processing capacity. Rulers have the incentive to fully control information on dissent and decide repression independently without delegating power to agents. However, this incentive decreases when political threats loom large and the quantity of dissent information overflows. With limited time and resources, the ruler needs to concentrate on addressing more prominent threats while allowing agents to perform information quality control, i.e., filtering out unimportant cases and only submitting adjudications on more threatening cases for the ruler to review. While controlled information flow enhances the ruler's decision-making efficiency, it also incentivizes career-minded judges to cheat and manipulate the definition of *important* cases so fewer cases are qualified for review. In so doing, the likelihood of sanction, imposed on judges if their decisions are disliked by the ruler during review, is reduced. This distorted agent behavior presents an iconic moral hazard problem where the original intention to increase repression efficiency by the ruler ultimately hurts repression efforts because judges become more lenient in sentencing when a review threshold is imposed. This problem is particularly acute when judicial empowerment in case screening fails to be accompanied by a strong agent monitoring mechanism to prevent agents from misbehaving.

We test this argument using newly declassified data on Taiwanese political victims in the White Terror period (1949-1991) collected by the Taiwanese Transitional Justice Commission (TJC). This dataset provides a rare opportunity for us to empirically study the process of military trials against regime enemies at the individual level when it is difficult to investigate in many authoritarian contexts due to data limitations. Taiwan's authoritarian period also features a highly repressive regime seeking to seize control of its remaining territory after a failed civil war against Mao's communist party, thereby offering a useful context to study how regimes leverage repression to control their societies. Detailing the judicial process of dissent case screening and presidential review, this unique data allows us to dig into the power struggle between the judiciary and the ruler and how judges maneuver to escape sanction and increase autonomy.

Empirical results affirm the notion of moral hazard between the ruler and judges in the delegation of information screening power. In response to the growing pressure from dissent and information overload, President Chiang announced a new law in 1956 allowing military judges to adjudicate unimportant threats on his behalf without review when he focused on reviewing the most threatening cases only (longer than fifteen years imprisonment). Using a regression discontinuity design, we find that judges became significantly less likely to sentence criminals to longer than fifteen years imprisonment (hence no review needed) following the enforcement of the law. The unintended consequences of agent empowerment go against the purpose of delegation and also undermine repression effects. We also find evidence that this distorted behavior is driven by judges' fear of sanctions, which are imposed when the president frequently rejected their judgment in review.

These findings contribute to the burgeoning literature on state repression, human rights, and autocratic judiciaries in several important ways. First, we focus on previously understudied judges under the principal-agent framework in the repression literature and present novel data on the judicial process to examine how judges manipulate political sentencing and impact repression outputs. Secondly, unlike existing studies emphasizing

the delegation of military power and control between the ruler and the security sectors primarily, our argument highlights the delegation of information power and the trade-off between information quantity and quality for regimes, providing a new information-based moral hazard theory that explains the power struggle in many dictatorships. Additionally, our findings challenge the existing understanding of judiciaries and human rights. Human rights literature typically views courts and judges as protectors of citizens' rights from government encroachment and abuse. Instead, we direct research to consider judges as political actors and show that judges can manipulate sentencing to advance their own career interests. Lastly, our findings bridge two largely disconnected literature on state repression and authoritarian judiciary, showing how judicial autonomy can grow under authoritarian rule and its implications on repression efforts and autocratic control.

Rethinking Judiciaries and State Repression

One key question that attracts many intellectual inquires is how we can protect human rights against state oppression and violence. Answering this question inevitably requires us to understand how the state violates human rights and who the perpetrators are to exercise repression on behalf of the state. This endeavor leads us to unpack the repressive regime into its constituents. A growing body of literature has begun investigating the role of repressive agents, i.e., the police, the military, militias and mercenaries, and how they facilitate the act of repression by the regime. Courts and the judiciary, however, rarely appear on the list of repressive agents to investigate because judiciaries are considered as an obstacle to rather than a facilitator of state repression.¹ Repression and human rights scholars tend to think that the importance of judiciaries lies in their ability to protect human rights and thus show more interest in studying independent courts that help citizens resist rights abuses (Hill and Jones, 2014; Keith, 2012; Conrad and Ritter, 2019; Hu and Conrad, 2020). Less scholarly effort has been made to discuss state-controlled courts and

¹Slough and Fariss (2021) make advance in discussing how the judicial system, state prosecutors more specifically, can obstruct human rights by looking at a special form of abuses (unintentional delayed in prosecution).

semi-independent judiciaries, even though they can play a critical role in shaping repression decisions and intensity.

By contrast, the scholarship on autocratic judiciaries and authoritarian institutions is more interested in the role of semi-independent judiciaries and how regimes use them to expand power. Authoritarian courts are found to have important functions that advance administrative discipline within state institutions, maintain cohesion among domestic factions, facilitate market transitions, and bolster regime legitimacy (Moustafa, 2007). Recent literature also shows that political trials against the elite inside the ruling coalition can be used to mobilize insider's support for the ruler (Shen-Bayh, 2018). This line of work heavily emphasizes institutional designs that help rulers exercise top-down control over the judiciary to advance their power, including control over judges' appointments and promotions, limited jurisdiction, ideology, and fragmentation (Pereira, 2005; Solomon, 2007; Ginsburg and Moustafa, 2008; Moustafa, 2014). Yet, what is understudied is the bottom-up dissent information transmission and filtering from judges to the ruler and the agency problem in the judicial hierarchy, which can impact repression outcomes.

If the state is a conglomerate of specialized agents working under the command of the ruler, then it is important to study organizational behavior and compliance issues between subordinates and the leader. Of great relevance to our research is the work on the agency problems in state repression. Scholars analyze repressive behavior from the view of contestation and compliance between the ruler and repressive agents. For example, the seminal work by Svoboda (2012) explains the tension between the ruler and the military, the conditions under which the latter decides to repress dissidents out of their own interest, and the likelihood of military coups. Others follow a similar path to study related agency problems that influence the supply of repression and state violence (Tyson, 2018; Dragu and Lupu, 2018; Dragu and Przeworski, 2019; Carey, Colaresi and Mitchell, 2015). While important in improving our understanding of agents' role in repression, this line of research only examines the delegation of "military power", discussing when the military force will comply and repress dissidents and when they will defect against the ruler.

An essential aspect of the principal-agent relationship missed in the scholarship is the delegation of “information power.” Repressive agents may be empowered by the ruler to enhance the regime’s information quality on dissent, and agents can advance their own interest at the expense of the ruler’s in the delegation. In the following section, we develop a theoretical argument about information filtering and the moral hazard problem in the principal-agent relationship, showing that judiciary empowerment aiming to enhance state repression results in judges’ misbehavior to the point of undermining the goal of repression.

Moral Hazards in State Repression: Information Screening and Sentencing Manipulation in Authoritarian Courts

We conceptualize repressive state apparatus as a hierarchical entity where the ruler delegates power to specialized agents who exercise coercion and political control. In the process of ordering repression, the ruler also digests information submitted by agents from the bottom up so that he can make informed decisions on repression. One key function of the coercive apparatus is to collect, organize, and screen information for the principal’s consumption. For example, the intelligence agency is usually tasked to collect dissent information that helps the police capture individual dissidents. We argue that judicial investigation is also essential in the information processing. After individuals are arrested and prosecuted, courts organize evidence of crimes, try the criminals based on their actions, and propose punishments for the ruler to review. A thoroughly investigated and well summarized case backed up by clear motives, subversion evidence, networks of accomplices, and proposed penalties provides the ruler with a comprehensive understanding of the current dissent movement dynamics. This final stage of information processing makes dissent information much more accessible to the ruler and helps the ruler assess the effectiveness of current repression efforts.

Power delegation to agents without control is dangerous to dictators. Similar to bureaucrats in democracies, agents commonly have an informational advantage over the

principal (see Gailmard and Patty, 2012). Due to the lack of potential external information providers, such information asymmetry is particularly acute in dictatorships (Wintrobe, 1998). In the process of information refining, judges have better access to a broader set of raw dissent information than the ruler, which makes it necessary for the principal to monitor agents and sanction misbehavior in information processing. Comprehensively reviewing case verdicts written by judges reduces information asymmetry and helps rulers understand how well a case has been investigated (e.g., any unclear relationships needing further investigation or missing subversives pending prosecution before a case can be concluded) and whether criminals are appropriately sentenced (e.g., any under- or over-sentencing). Over-sentencing and excessive repression are harmful for the regime as it can trigger grievances and push citizens to side with dissent (Rozenas and Zhukov, 2019); under-sentencing is equally detrimental as it can encourage opportunist behavior and fuel further resistance. Reviewing narratives and evidence laid out in these verdicts therefore provides convenient access for rulers to evaluate judges' performance in trials. If a judge's decision is disliked by the dictator, it can be rejected and retried. Frequent rejections and retrial requests can result in sanctions against judges to ensure compliance and enforce the principal's preference.

However, time and resources are finite, and the ruler faces the trade-off between thoroughness and efficiency. When political challenges are limited and the quantity of dissent information is manageable, the ruler has every incentive to review as many verdicts as possible to closely monitor agents. But when dissent increases and the quantity of information overflows, the ruler can be quickly overwhelmed in case review. Power delegation becomes preferable. Under such conditions, the ruler has more incentives to authorize agents to perform information quality control, screen out unimportant information, and make sure that only essential information will be sent to his desk. By focusing on reading and reviewing information about the most threatening cases, the ruler can prioritize

attention and resources to prominent regime challengers and eliminate them more efficiently while allowing delegated agents to make judgments on less imminent threats on his behalf.

Ironically, empowering agents to control information flow can create a moral hazard problem where agents are incentivized to misbehave in the system due to misaligned preferences between the principal and agents. While the ruler's priority is to maintain political order through repression, judges care more about job security and avoiding punishment by the ruler. If processed information is considered insufficient and porous, they can be punished for being incompetent and removed from their office once the ruler has reviewed their verdicts. The risk of punishment consistently haunts these agents, motivating them to perform better and make adjudications more closely aligned to the dictator's preference (see Shotts and Wiseman, 2010). However, when the ruler becomes overwhelmed by the quantity of review and authorizes agents to determine the importance of cases (and therefore worthy of being reviewed or not), career-oriented agents can be emboldened to manipulate the mandate, such as strategically reducing cases qualified for review so as to evade accountability. In so doing, the delegation of information filtering power distorts agents' behavior and reduces repression effects, which eventually undermine the original purpose of improving repression efficiency. This distorted behavior tends to be amplified when no additional agent monitoring system is implemented to check whether agents loyally triage information in a way that adheres to the ruler's mandate.

If the ruler has specified the criteria of important cases, however, how would agents be able to manipulate the ruler's mandate and evade review? The answer once again lies in information accessibility. The fundamental feature in the principal-agent relationship is information asymmetry. In the principal-agent relationship where agents act as the gatekeepers of information processing, the ruler doesn't know the true quality of information, and the most intuitive way for him to differentiate important versus unimportant cases is by looking at some severity threshold in sentencing. Capital punishment or long imprisonment means that these cases are more significant, while shorter years of imprisonment or

innocence imply less important threats to the regime that should be filtered out. Since the ruler does not know the true quality of information, agents can manipulate and relax the “definition” of importance so that fewer dissidents will be qualified for severe punishment, resulting in fewer cases to be reviewed by the ruler.

Following the above discussions, we hypothesize that judges will have less incentive to issue severe punishment to dissidents when the ruler only demands more severe cases to be reviewed and approved. Specifically, if cases above a certain severity threshold in sentencing are required to be reviewed, judges tend to be motivated to strategically reduce sentencing so that fewer cases will be qualified for review:

Hypothesis: When the ruler demands to review dissent cases above a severity threshold, judges are less likely to sentence dissidents above that threshold.

Empirical Case: Repression During Taiwan’s White Terror Period (1949-1991)

The period under investigation was an extremely repressive time in Taiwanese history. Historians commonly refer to it as “Taiwan’s White Terror,”² when Taiwan was ruled under a single-party regime of the Republic of China’s Nationalist Party (or the Kuomintang, KMT, 國民黨). Chiang Kai-shek, the leader of the KMT, was defeated by Mao’s communist party and retreated to Taiwan in the winter of 1949. In the same year, the KMT announced the Martial Law Act (臺灣省戒嚴令) and introduced the Betrayers Punishment Act (懲治叛亂條例), aiming to immediately control Taiwanese society and avoid infiltration by mainland communists.

Taiwan’s repressive apparatus was both professional and highly centralized under the rule of Chiang. The security agencies, including the Secrets Bureau (國防部保密局) and Taiwan Garrison Command (臺灣警備總司令部/保安司令部), were tasked to collect intelligence and arrest dissidents. After suspected dissidents were arrested and interrogated, they were sent to the military court (國防部軍事法庭) for trial under the rule of martial

²The term of White Terror is often used to describe episodes of mass violence in history, carried out by insurgents or the state.

law.³ In the tribunal, judges made initial decisions before each case was reviewed and approved by Chiang. The presidential power of trial review and approval implies that the entire security and judicial systems were under the central command where repressive agents collected and assessed dissent information, and the ruler made the final judgement call on the degree of repression and prosecution.

The president-judges relationship experienced a major change in 1956 when a new law of military trial was announced. Hoping to seize control over the island since the retreat in 1949, Chiang's regime intensified the level of repression to encounter the increasing challenges from underground communists. Back then, Taiwan was newly governed by the KMT regime after decades of Japanese colonization; however, the corrupt KMT administration in Taiwan, political exclusion of islanders, and shattered economy frustrated Taiwanese islanders, fueling the sentiment to join Mao's communist China and overthrow Chiang's regime. Official documents show that more than 250 underground branches were established throughout the island by 1949 with over 2000 members joining organizations (Lin, 2009). The subversive activities ranged from armed activities that planned to steal, purchase, and make weapons or explosives, to unarmed activities that focused on expanding membership, educating communist ideology, and training to prepare for the mainland Communist's control over military-industrial facilities when the Red Army marches ashore.

Increased dissent as well as intensified repression resulted in a flood of information and prosecution entering the repressive apparatus which led to administrative overload and fatigue. Government internal documents reveal that the President and his office felt overwhelmed by mounting cases waiting to be reviewed, which causes institutional delays in prosecution. They pointed out that a reform is needed to allow the review to concentrate on coping with the most imminent threats to the country.⁴ After internal discussion

³Unlike other countries where civilian judges can serve in the military tribunal, Taiwanese military tribunal only consisted of military judges who have military status and ranks.

⁴See Appendix Section 1.

between Chiang and the ruling elites, the new law of military tribunal was enacted to reform the presidential review procedure and relax the review requirement, allowing judges to finalize prosecution on minor cases without needing presidential approval.⁵ The president remained a firm grip on subversive cases associated with military personnel as they are highly threatening to the regime. For civilian cases, however, the new law specifies that only those sentenced to fifteen years or more are required to be reviewed by the president, striking a balance between thoroughness and efficiency.⁶

Empirical Strategy

To test the hypothesis, we utilize newly declassified archives and data collected by the Taiwanese TJC. The dataset documents the court process of more than ten thousand victims tried by military courts under the authoritarian rule during 1949–1991 in Taiwan. The list of victims is collected by various non-governmental organizations and the judicial branch. Following several transitional justice initiatives since democratization in the 1990s, these organizations have compiled a large set of official judicial documents of victims who went through military trials. In the dataset, a victim charged for one case is counted as one observation. The dataset covers 13,683 observations in total⁷, which is believed to be a quite comprehensive coverage of political prisoners in the authoritarian era in Taiwan.

The dataset includes details such as names and positions of the involved judges and officials in the trial process, crime descriptions, and court verdicts. In addition, the demographic information of the dissidents are also included. Importantly, the dataset allows us to explore whether and the conditions under which judges' decisions were later reviewed and overruled by the president.

⁵During five years before the reform, the president reviewed 62% of all civilian cases. Among dissidents who were sentenced less than one year, still 40% were reviewed by the president. During five years after the reform, only 20% of all civilian cases were reviewed.

⁶Article 133 of the Military Trial Act stipulates that judges' adjudication needs to be reviewed by the President, which formally legalizes presidential intervention in the new law. The enforcement regulations document of the Military Trial Act, which specifies the scope of review, stipulates that non-military cases should be reviewed when they are sentenced more than fifteen years.

⁷There are 13,273 unique dissidents. Some dissidents were charged more than once.

Following the hypothesis, we expect that after October 1st, 1956, when the threshold of presidential review for cases sentenced to more than fifteen years of imprisonment had been installed, judges became less likely to sentence dissidents above that threshold. To identify effects of the reform, using the entire sample and simply comparing sentencing severity before and after 1956 can be problematic because dissident cases and judicial system in different time periods may differ due to other factors. For example, as the regime furthered control over the society, particularly since the mid 1960s, uprisings became less organized and less threatening; when political liberalization gradually started in the 1980s, judges were likely to have more autonomy compared to in the 1950s. These variations can complicate our inference.

To isolate the causal effect of the reform, we use the regression discontinuity (RD) design. This design allows us to compare the sentences of cases closely before and after the reform. As has been well established in the literature, RD design leverages as-good-as-random variation in treatment and continuity in potential outcomes around the cutoff (e.g. de la Cuesta and Imai, 2016; Calonico, Cattaneo and Titiunik, 2014). The design rests on the assumptions that dissidents and judges are unable to sort around the cutoff, and there are no significant differences between cases before and after the reform except for their treatment status within a narrow window near the cutoff. Given that the precise timing of the enactment of the new military tribunal law was unanticipated by the public, dissidents' actions were orthogonal to the reform. Furthermore, even though judges might foresee the reform coming, Chiang and the ruling elites were aware of the possibility that judges may manipulate the timing of when they trial cases, and therefore formally forbid judges to delay any trial.⁸ Based on these regulations, we believe the model assumptions are held reasonably.

Dependent variable. To test whether the possibility of presidential review has effects on sentencing issued by judges, we utilize the decisions of the first instance courts as the new presidential review requirement was based on first instance court decisions. Sentencing

⁸See Appendix Section 2.

on dissidents ranges from no penalty to death sentencing. Since we focus on investigating whether judges strategically reduce sentencing below fifteen years of imprisonment, we create a binary variable in which 1 represents severe punishments with more than a fifteen-year sentencing (including death and life sentencing) and 0 refers to milder sentencing below the threshold. We only focus on sentences for civilian dissidents because the military tribunal law detailed different regulations about presidential review criteria for cases associated with military personnel, and most military cases were still required to be submitted to the president. Among all cases charged in the 1950s, 70% dissidents were civilians.⁹

RD running variable and the treatment. The RD running variable is the date of the first instance trial for each defendant. The cutoff is October 1st, 1956, when the new military tribunal law took place. The treatment condition is being sentenced in the first instance court since the date.

Covariates. We include several potential confounders. First, features of the committed crimes affect the severity of sentences. For this aspect, we include the logged number of codefendants. It is expected that when a case involves more dissidents and more organized groups, dissidents tend to be harshly charged. We also code whether the dissidents were charged for *committing subversion*, *leaking military intelligence*, or having *weapons* based on crime descriptions by the first instance courts. *Committing subversion* refers to that an individual was found guilty in conducting subversive activities and being substantially involved in treason. *Leaking military intelligence* indicates a charge against individuals who provided sensitive military information to communists and facilitated potential invasions by the People's Republic of China. Having *weapons* means that the defendants used, acquired, sold, or delivered any firearm or ammunition. These charges tended to lead to a death penalty based on the rules of military justice then.

⁹Because files of codefendants were usually submitted to upper levels together, we classified dissidents as civilians if none of the codefendants were military personnel. The classification based on individual occupations also generates consistent results.

Additionally, for the demographics of dissidents, we control for gender, age, and a dummy variable indicating whether they were born in Taiwan (*Islander*) or retreated from mainland China with KMT after 1949. Finally, judges' decisions were likely to be affected by the ruling of the president for previously submitted cases. When the president disapproved judges' decisions and reviewed a submitted case more than once, it manifested that the president was discontented with judges' decisions. Under such conditions, judges may issue harsher sentences for subsequent cases to fulfill the president's preferences. To take this possibility into account, we create an indicator to measure the proportion of the submitted cases that were reviewed by the president more than once in the previous year ($t - 1$). The descriptive statistics of the variables are documented in Tables A.1 and A.2 in the Appendix.

Results

Severity Threshold. Figure 1 displays the probabilities of being sentenced more than fifteen years imprisonment for civilian dissidents around the cutoff date of October 1st, 1956. A fourth-order polynomial regression is fitted separately on each side of the cutoff. There is a clear discontinuity in court decisions between defendants who were sentenced barely before the enactment of the new law and those who were convicted right after it.

In Table 1, we formally estimate the degree of the discontinuity. Following recent recommendations (Cattaneo, Idrobo and Titiunik, 2019), the bandwidths are selected using a data-driven approach that minimizes the mean square error of local regression point estimates and optimize the bias-variance trade-off. In Models 1 and 2, a triangular kernel function is employed to give more weights to court decisions closer to the cutoff and facilitate a comparison between dissidents sentenced right before and after the cutoff date. In Model 3, observations within the bandwidth are uniformly weighted. To avoid overfitting, we use linear or quadratic specifications (Gelman and Imbens, 2019). In the main analyses, standard errors are clustered at the trial case level. We report bias-corrected and robust confidence intervals based on Calonico, Cattaneo and Titiunik (2014).

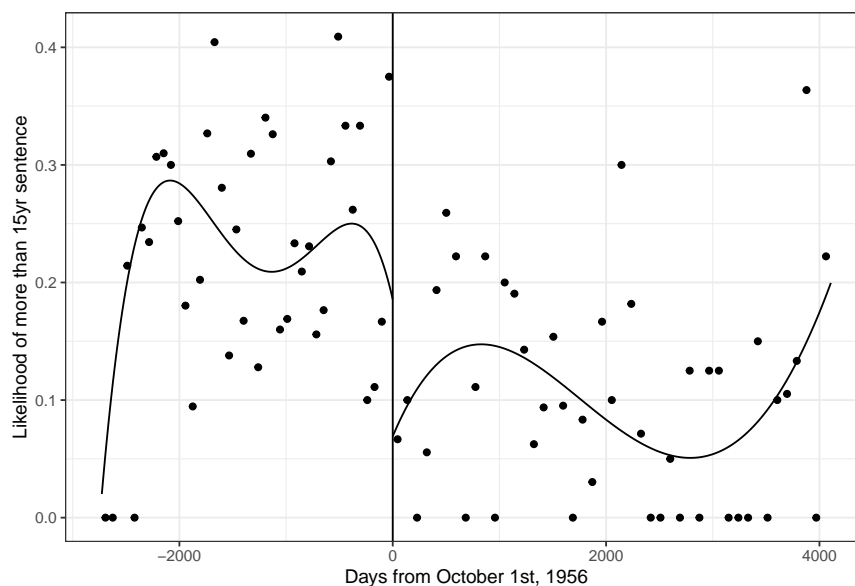


Figure 1. The 1956 reform and the severity of sentences

Note: All observations between 1949 and 1967 are grouped into 85 evenly-spaced bins, with each point representing the unconditional mean of more severe sentences than a fifteen-year sentence. The solid line is the fourth-order polynomial regression fit separately on each side of the cutoff and based on the original data (full results reported in Table A.11).

Models 1–3 show that, across the different specifications, civilian dissidents who were charged after the 1956 reform were less likely to be sentenced for more than fifteen years, compared to those charged before the reform. The magnitude of the effect is about a 29% reduction. Tables A.4 and A.5 in the Appendix include models with the Imbens-Kalyanaraman optimal bandwidths (Imbens and Kalyanaraman, 2012) and standard errors clustered at the running variable as suggested by Lee and Card (2008), and the results are consistent.

We further restrict the analysis to defendants whose penalties were around the boundary of a fifteen-year sentencing. It helps further verify that the discontinuity is mainly driven by the manipulation in the middle range of cases because judges may have less incentive to manipulate sentencing too drastically to the extent that their decisions look suspicious and difficult to justify. In Model 1 of Table A.6 in the Appendix, we only include defendants sentenced more than seven years in prison and exclude those who were charged with subversion or treason, since these charges tended to lead to a death penalty.

Table 1. Regression discontinuity of the 1956 reform on the severity of sentences

DV	More than 15yr sentence		
	(1)	(2)	(3)
New Law of Military Trial	-0.287***	-0.197**	-0.261***
Bias-corrected 95% CI	[-0.450, -0.123]	[-0.354, -0.040]	[-0.401, -0.121]
Robust 95% CI	[-0.480, -0.092]	[-0.380, -0.014]	[-0.419, -0.102]
Bandwidth (days)	721	1655	727
Effective N	435	2016	454
Polynomial order	Linear	Quadratic	Linear
Weight	Triangular	Triangular	Uniform

Note: Confidence interval (CI) clustered at the case level. Mean square error optimal bandwidth. Covariates of age, male, islander, (log) number of codefendants, weapon, committing subversion, leaking military intelligence, and president's review rate in $t - 1$ are included. Full regression results are reported in Table A.12. ** $p < 0.05$; *** $p < 0.01$.

In Model 2 of Table A.6, we utilize an alternative way to identify middle range cases. We fit a random forest model using the pre-1956 data as the training set, and rely on all the covariates to predict the three categories of judicial decisions: a death penalty, a seven-year sentence to life imprisonment, and less than a seven-year sentence. We then make predictions of judicial decisions for the post-1956 cases based on the random forest algorithm trained with the pre-1956 data. Model 2 includes pre-1956 defendants actually charged with a seven-year sentence to life imprisonment and post-1956 defendants predicted as this category of charges. Using either method to identify middle range cases, the negative local average treatment effects are consistent with those shown in Table 1 with full sample, and the degree of the discontinuity is more substantial. Model 3 of Table A.6 excludes dissidents tried more than once, and the results are also consistent.

To verify the stability of our results, Figure A.1 in the Appendix plots the estimates for alternative bandwidths ranging from 200 to 1500 days. The negative effects are in general robust to bandwidth choice. The plot shows that the estimates with bandwidths of 500 and greater are stable. With various bandwidth specifications, we similarly find a significant and substantial reduction in the likelihood of more severe sentences than fifteen years imprisonment after the cutoff. Figure A.2 displays estimates with placebo cutoffs to verify that there is no similar discontinuity at other time points. The figure includes results

for false cutoffs at an interval of half year between 1950 and 1963, and no significant negative treatment effects can be detected for these cutoffs. Further validation tests on the possibility of sorting are reported in the robustness section.

Testing the Mechanism: Sanction of Judges. Since we argue that judges manipulate sentencing in fear of being sanctioned by the ruler if he dislikes their decisions, we should expect that judges whose decisions were frequently disapproved by the president to be actually punished. To test this mechanism at play, we employ the number of times individual judges serve at military courts as a proxy of whether the judges were sanctioned. When judges were punished, it is likely that they were prevented from continuously serving at courts, which tends to significantly impact their future career. Although the TJC dataset does not comprehensively cover the career trajectories of military judges in Taiwan's authoritarian period, we believe that the number of court services provides a consistent and reliable measure across judges of different ranks and units.

For all judges who heard cases of dissidents at military courts and are documented in the TJC dataset for at least consecutive two years, we calculate the number of all their services each year in the dataset. In total, we have the data of 466 judges and on average 2.4 years of records for each of them (with the maximum 20 years). The unit of analysis is judge-year.

Dependent variable. The dependent variable is the annual change in the number of judge i 's appearance at all courts from year $t - 1$ to t .

Independent variables. The independent variables are the number of judge i 's decisions on civilian defendants at first instance courts that were latter vetoed or reviewed more than once by the president in year $t - 1$. Since these indicators are strongly right skewed, in Table 2, they are plus one log-transformed, while in Table A.7 in the Appendix, we use dummies to indicate whether judge i 's decisions were ever vetoed or reviewed more than once by the president in year $t - 1$, where 1 refers to "yes."

Covariates. Although we do not have information about the demographics of judges, such as their age and education, we control for fixed effects of judges. To take the overall level of social unrest and judges' workload into account, we include the number of all accused dissidents in year t . Additionally, yearly fixed effects are included for other contemporaneous factors. The descriptive statistics of these variables are reported in Table A.3.

Results. The results are shown in Tables 2 and A.7. In these two tables, Models 1 and 2 cover the full sample, Models 3 and 4 include judges' services before the new military tribunal law, while Models 5 and 6 include those after the reform. Fixed effects of judges and years are included in all models. Standard error are clustered at the level of judges.

The results of Models 1 to 4 are consistent with our expectation: if judge i 's decisions on civilian defendants had been disapproved by the president or reviewed by the president more than once in year $t - 1$, the number of times that judge i served at military courts in the following year was significantly reduced. Based on Model 1 in Table 2, if the number of a judge's decisions that had been vetoed by the president increases by 10% in year $t - 1$, the number of the judge's service in year t was on average decreased by more than five times.

As shown in Models 5 and 6, these negative effects become less pronounced after the reform. This is mainly due to the fact that since judges were able to avoid being reviewed by lowering sentencing, the number of cases that were later vetoed by the president was largely reduced. During 1957 to 1966 for example, the president did not veto any submitted cases.

Table 2. Regression estimates of the effect of presidential disapproval on judges' appearance

<i>DV</i>	ΔN of services at all military courts					
	(1)	(2)	(3)	(4)	(5)	(6)
	All		Before 1956		After 1956	
<i>ln</i> N of president disapproval _{<i>t</i>-1}	-56.09*** (16.05)		-68.25*** (21.09)		-7.979 (6.989)	
<i>ln</i> N of president review again _{<i>t</i>-1}		-41.06*** (12.80)		-45.85*** (17.24)		-15.72** (7.203)
N of all defendants	2.217*** (0.507)	2.216*** (0.506)	-0.349 (0.224)	-0.339 (0.224)	0.772*** (0.144)	0.777*** (0.146)
constant	-88.41*** (15.42)	-92.39*** (15.20)	-10.45 (29.48)	-16.38 (31.96)	-68.05*** (13.09)	-68.68*** (13.35)
N	1115	1115	473	473	642	642
N of judges	466	466	231	231	292	292
Judge FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Robust standard error clustered at the level of judges in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Robustness Checks and Alternative Explanations

We conduct several robustness checks to verify the main result: the probabilities of being sentenced to more than fifteen years imprisonment were largely reduced since the new law of presidential review was introduced in 1956. One identification assumption of the RD design is that dissidents and judges were not able to manipulate the timing of their cases relative to the cutoff. It means that introducing the law and sentencing threshold would not impact dissent behavior (e.g., no drastic increase or decrease in cases) and judge's behavior (e.g., strategically postponing case trials). Figure 2 plot the monthly number of civilian dissident cases charged in military courts around the cutoff date of October 1st, 1956. The figure shows that the distribution is roughly equivalent before and after the cutoff date, and the number does not sharply decline or increase in this local area. To formally verify the assumption of no sorting, we conduct the density test introduced by McCrary (2008). The estimate is 0.31 with a p-value of 0.75, suggesting that there is no significant discontinuity in the density of observations around the cutoff.

Furthermore, if there is no sorting and observations are assigned as-if randomly to both side of the cutoff, cases before and after the date should be similar with regard to other

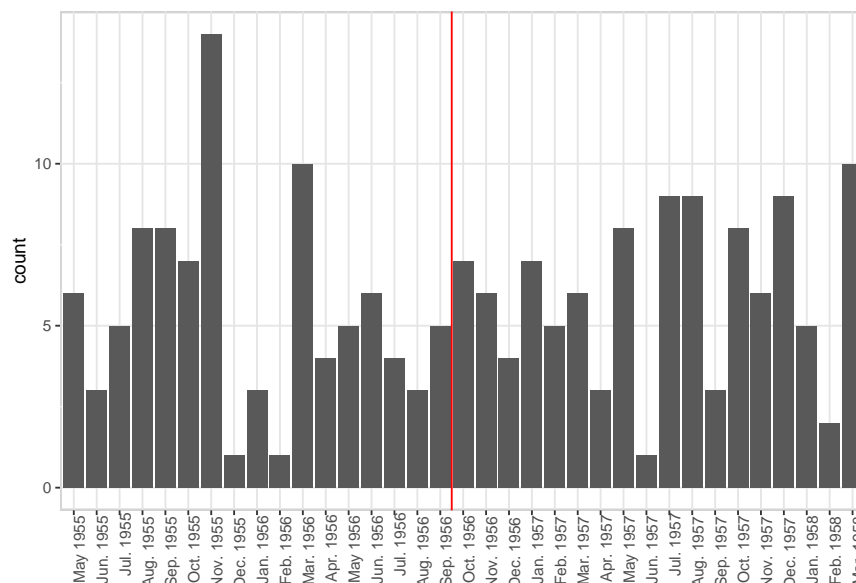


Figure 2. Frequency of civilian dissident cases around Oct. 1st, 1956

characteristics (de la Cuesta and Imai, 2016). Table A.8 presents separate RD estimates for the covariates for civilian dissidents and demonstrates that there are no large imbalances at the cutoff across these covariates, including the demographics of defendants and features of the committed crimes. These estimates show that there is no sharp decline in the number of charged cases after the cutoff, and observations before and after the cutoff are largely similar.

The new military tribunal law was passed by the legislature on July 1st, 1956 and implemented on October 1st in the same year. Although judges were ordered not to delay trials, one may be concerned that during the three-month window, judges would manipulate their sentencing decisions. To alleviate this concern, we exclude observations closest to the cutoff and conduct the same RD analyses (donut-hole approach). The results are shown in Table A.9. Either excluding observations within one month or three months from the cutoff date, the patterns are consistent with our main finding: the local treatment effect is significantly negative.

Finally, our arguments depend on that the reform was indeed put into effect since the cutoff date. That is, court decisions milder than fifteen years imprisonment were truly

less likely to be submitted to the president since the reform. We conduct separate RD analyses for civilian dissidents who received more severe punishments and those with milder punishments. The dependent variable is presidential review, a binary indicator taking 1 if the file of a defendant was reviewed by the president later on, and 0 if the file was never reviewed by the president. The results are shown in Table A.10. Consistent with the expectation, for cases with milder court decisions, the likelihood of being reviewed later by the president is substantially reduced since October 1st, 1956; while for more threatening cases imposed with sentences above the threshold, the likelihood does not change significantly.

Does sentencing manipulation undermine the repression efforts?

If imposing a review threshold incentivizes distorted behavior in strategic leniency by judges, it is logical to ask to what extent sentencing manipulation would undermine the repression campaign. Since manipulation tends to be around the threshold, mild reduction in sentencing may not drastically impact political control and social stability. However, lighter sentencing for people who commit heavy crimes (e.g., twenty years reduced to ten years imprisonment) allows them to rejoin dissent movements when they are still young, which causes indirect costs in repression. By examining the released (after jailed a couple of years) and recaptured dissidents¹⁰ after the new law in 1956, we find that the average number of codefendants is six times higher for the recaptured (1.5) than non-recaptured (0.26), suggesting that they can bring in more opposition and disturbance after released. While we need more data to thoroughly analyze the impact of sentencing reduction on dissent movements which is beyond the scope of this paper, it provides preliminary indication that leniency, even a mild one, can still increase cost in repression in the long run.

¹⁰The average time in prison of these individuals is 9 years when they were first tried.

Conclusion

This study seeks to understand the role of judicial institutions in shaping repression decisions in dictatorships. We conceptualize the state as a hierarchy and argue that the judiciary serves as a critical information processor that controls what information flows upwards to the ruler. When dissent challenge is mild, the ruler has the capability to comprehensively review dissent cases tried by judges to ensure that investigation and sentencing are thorough and appropriate. However, when dissent pressure increases and the quantity of information mounts up, the ruler often delegates more power to judges in adjudicating unimportant cases on his behalf without review and only investigates more threatening cases to increase the decision-making efficiency. This empowerment creates a moral hazard problem where judges can cheat by reducing cases qualified for review to avoid decision rejection and sanctions by rulers, ultimately hurting rulers' control over the judiciary and undermining repression.

To test this argument, we use newly declassified military trial data in the authoritarian period of Taiwan. We show that mounting dissent in the island after Chiang's regime moved to Taiwan forced a change in the way that the regime processed dissent information and decided how much violence to use. We find that after Chiang implemented a new military tribunal law that required judges to only send important cases above a severity threshold (fifteen years of imprisonment) for president's review, judges became significantly less likely to sentence dissidents above that threshold. We also find evidence that this distorted behavior is driven by judges' fear of sanctions when the president rejects their decisions and subsequently punishes them.

These findings raise some important implications for future research. First, it is important to analyze human rights abuses in repressive regimes by considering the state as a hierarchy, not a unitary actor, where the decision of repression is made dynamically between the ruler and the repressive agents. Secondly, dissent rarely impacts repression decisions in a straight line. How dissent shapes the use of violence can be channeled

through other mechanisms, such as creating internal pressure for the state to process dissent information and generating compliance issues within the coercive institution. Lastly, so far when we discuss the principal-agent relationships in political control, the scholarship only examines the moral hazard problem between the ruler and the military, but we need to look beyond the dilemma in military power delegation and start thinking about other types of delegation issues, i.e., information power, that exist in different coercive agencies, which will help us understand a wider condition of dysfunctioning in authoritarian governance.

Turning to policy implications, analyzing agency problems in repression helps us reconsider transitional justice in post-authoritarian or post-repression societies. In seeking justice for political victims, the aim is typically to identify the perpetrators and hold them accountable. But when we start looking at human rights abuses that are dynamically determined by the leader and delegated agents, the institutional problems will emerge, which then prompts us to think about institutional designs (e.g., ways to increase judicial autonomy) that can better curb state encroachment of rights not just in autocracies but also democracies.

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