The Shadow Cost of State Violence: Evidence from Bureaucratic Purges in China

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Abstract

State violence inflicts obvious direct costs on its victims, but many of its significant consequences may be indirect. Building on the literature on agency problems in authoritarian regimes, we argue that coercion against bureaucrats motivates them to pursue over-zealous goals at the expense of wider social costs. We test this argument by studying how bureaucratic purges in China under Mao impacted the behavior of local bureaucrats during the Great Leap Forward, a campaign that caused over 30 million deaths from mass starvation. Exploiting variations in purge intensity across about 1,400 counties, we find the purge intimidated local bureaucrats into inflating agricultural production and extracting excessive amounts of grain from farmers, which resulted in significantly higher famine mortality. The results highlight the perils of "accountability by violence" in authoritarian regimes.

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1 Introduction

Principal-agent problems between politicians and bureaucrats are central to all political relationships. Along with the well-known literature on political-bureaucratic relationships in democracies, recent scholarship has investigated similar agency problems in non-democratic regimes (Egorov, Guriev and Sonin 2009; Egorov and Sonin 2011; Gehlbach and Simpser 2015; Rundlett and Svolik 2016; Tyson 2018). Unlike in democracies where bureaucrats are mostly motivated by salaries and promotional opportunities, autocrats have the option to use violence against bureaucrats (Montagnes and Wolton 2019; Wintrobe 1998). In theory, the threat of violence can produce high-powered incentives among bureaucrats. Since bureaucrats are responsible for implementing state policies with broad social impacts, changes in bureaucratic behavior due to coercion may have a significant impact on wider society. How does the threat of violence motivate bureaucratic behavior? What are the wider social consequences of an administrative system grounded in coercive incentives?

We propose a theoretical framework on the shadow cost of "accountability by violence" in autocracies, which combines two streams of literature. The literature on authoritarian repression suggests that state violence can motivate citizens to behave loyally toward the regime even if they do not intrinsically prefer to do so (Kuran 1995; Rozenas and Zhukov 2019; Wintrobe 1998). Another stream of literature, which focuses on bureaucratic incentives, shows high-powered incentives can induce perverse bureaucratic behavior that can generate negative social consequences (Acemoglu et al. 2020; Breton and Wintrobe 1986; Fisman and Wang 2017). Bringing them together, we hypothesize that politically-motivated bureaucratic purges incentivize surviving bureaucrats to demonstrate excessive compliance when implementing state policies to avoid persecution. In particular, purges create perverse incentives in the bureaucracy where political consideration dominates the administrative process and bureaucrats attempt to signal their loyalty by "over-performing" on their duties. When the policy in question primarily concerns revenue extraction from society, the bureaucracy's over-performance can result in exces-

sive extraction, which can lead to grave social consequences.

We test this intuition by studying the Chinese Communist Party's (CCP) Anti-rightist Campaign in the 1950s, which persecuted a large number of local bureaucrats. Following the purge, the CCP leader, Mao Zedong, initiated the so-called Great Leap Forward (GLF) with the goal of rapidly increasing China's agricultural output. Local officials were pressured to boost grain production and increase grain extraction from rural communities. Under the shadow of purges, they displayed exceptional enthusiasm in carrying out grain extraction orders from above. The GLF was characterized by wildly exaggerated production statistics provided by local officials, as well as excessive extraction of grain from rural communities. The GLF was followed by a nationwide famine from 1959 to 1961, in which more than thirty million people perished (Ashton et al. 1984; Cao 2005).

We argue that the purge of local bureaucrats prompted their surviving peers to demonstrate support for the GLF in an excessive manner. The Anti-rightist Campaign, which aimed to target ideologically incongruent bureaucrats, created incentives for surviving bureaucrats to exaggerate local grain output as a way to signal their ideological congruence with Mao. However, due to the exaggeration of grain output, local grain extraction quotas were accordingly expanded, which compelled the bureaucrats to extract excessive amounts of grain from the rural population at the cost of their food rations. Excessive grain extraction by state bureaucrats is recognized as one of the main causes of the Great Chinese Famine of 1959-1961 (Kasahara and Li 2020; Kung and Lin 2003; Li and Yang 2005; Meng, Qian and Yared 2015). We argue that they had a deeper root in state violence against bureaucrats even before the GLF was set in motion. We hypothesize that the anti-rightist purge had a causal effect on both excessive grain extraction and famine mortality.

We estimate the effect of anti-rightist purges on excessive grain extraction and famine mortality using new county-level data. Leveraging the number and the density of purge victims in each county, we show that higher purge intensity was associated with more excessive grain extraction from agricultural collectives and lower food availability during the GLF. We also find a robust positive correlation between purge intensity and excess mortality during the famine. We show that the estimated effect of purges was unlikely

driven by omitted variables that could affect both purge intensity and the GLF outcomes. To further address causality concerns, we supplement the baseline results with a regression discontinuity design (RDD). The RDD exploits discontinuous changes in purge intensity induced by temporal variations in the CCP's military occupation of different counties during the Chinese Civil War. Our results show that counties just happened to under later CCP control had a significantly higher number of purge victims, which resulted in higher famine mortality.

We further show that the effect of purges likely operated through shaping loyalty signaling incentives of the bureaucrats. We find that the officials were more likely to report exaggerated production figures in counties where more people had been previously purged. The magnitude of their exaggerations was also positively correlated with purge intensity. On the other hand, descriptive and qualitative evidence suggests that the selection effect of the anti-rightist purge was limited. We find no evidence that the purge led to increasing turnovers of county party secretaries or the appointment of party secretaries with certain origins.

This study makes three principal contributions to the literature, specifically in the areas of bureaucratic politics, state violence, and authoritarian rule. A body of research has investigated the origins of perilous bureaucratic incentives in different contexts (Acemoglu et al. 2020; Breton and Wintrobe 1986; Greitens 2016), focusing on incentive mechanisms such as salaries and promotion opportunities. This study takes a different angle by investigating how politically motivated and ideologically driven violence impacts bureaucratic incentives. Our focus on political purges highlights an important yet understudied feature of the bureaucracy in authoritarian states: instead of being isolated from politics, bureaucratic behavior is heavily influenced by political dynamics (e.g., state repression). Our finding thus sheds light on the intertwined relationship between politics and administration in authoritarian states and expands our understanding of how politicized bureaucratic control shapes policy outcomes.

This study also expands our understanding of state violence in authoritarian states. A conventional wisdom about state violence – whether directed at civilians, bureaucrats, or political elites – views it as a tool for inducing obedience and compliance. This paper

highlights the agency of those subjected to violence, whose response to coercion may extend beyond merely following state orders. One of the responses from state bureaucrats, as we show in this paper, could be over-performance in carrying out state projects. Our finding also complements the literature that shows state violence has lasting impacts on political behavior (Lupu and Peisakhin 2017; Rozenas, Schutte and Zhukov 2017; Wang 2021). In particular, past violence can be perceived as a signal for future repression in authoritarian regimes, which compels individuals to constantly demonstrate loyalty to the regime (Rozenas and Zhukov 2019). Our finding, based on the anti-rightist purge under Mao, may also shed light on the social consequences of political purges in other countries, such as Stalin's Great Purge in the Soviet Union, the purges following the Iranian Revolution, and Pinochet's purge in Chile.

By investigating the bureaucratic politics of famine in China, our paper complements Sen's theory stressing the role of democratic institutions in creating governmental incentives to prevent social disasters (Sen 1981, 1999). We also make several contributions to the literature on the causes of the Great Chinese Famine. Building on historical research that suggested a potential influence of the Anti-rightist Campaign during the GLF (Bernstein 2006; Joseph 1986; Walder 2015), this study presents the first systematic analysis of this causal effect, clarifying the underlying mechanisms based on micro-level data. By focusing on local bureaucrats and their agency in implementing state grain extraction orders, this paper complements the qualitative literature that focuses on political dynamics at the central level (Bernstein 2006; Lieberthal 1987; Yang 1996). Our finding also extends the quantitative literature that highlights the institutional and social origins of the famine by looking into local variations (Cao, Xu and Zhang 2022; Kasahara and Li 2020; Kung and Lin 2003; Meng, Qian and Yared 2015). In particular, our finding complements the research that highlights promotion incentives (e.g., Kung and Chen 2011), providing a different angle to understand the origins of bureaucratic radicalism during the GLF and beyond.

2 The Shadow Cost of Purges

Coercion is widely used by authoritarian states to maintain their hold on power. It not only shapes the relationship between authoritarian states and their citizens (Miller and Smith 2015; Truex 2019; Xu 2021). Coercion is also pervasive in the political-bureaucratic relationship in autocracies (Englehart 2007; Li and Manion 2022; Wang 2022; Wintrobe 1998). Autocrats often resolve principal-agent problems through purges, denying bureaucrats of their jobs, freedom, and even lives.

How do purges shape policy outcomes? We build on the observation by Montagnes and Wolton (2019) that purges have two immediate consequences: they change the composition of the bureaucracy through removal and replacement; purges also change performance incentives among surviving bureaucrats.

2.1 Purges and Bureaucratic Selection

Purges may also affect policy outcomes through bureaucratic selection. The removal and replacement process may result in changes in the composition of the bureaucracy. When autocrats purge ideologically incongruent agents and replace them with congruent ones, bureaucratic performance is expected to increase. For ideologically congruent bureaucrats, it is less costly to exert effort in executing the autocrat's preferred policies compared with the incongruent ones (Montagnes and Wolton 2019). As such, the changes in policy implementation outcomes following bureaucratic purges are likely the results of both incentive changes and bureaucratic selection. However, the relative influence of the selection mechanism versus bureaucratic incentives is constrained by certain factors.

The selection effect is constrained by the breadth of a purge. Intuitively, a purge is likely to induce greater changes in the bureaucracy's composition when a larger proportion of bureaucrats is replaced. However, extensive purges can be costly for autocrats, which leads them to restrict the breadth of purges. Another factor that influences the

selection effect is the autocrat's ability to identify loyal versus disloyal bureaucrats in a purge. When the autocrat has accurate information about the bureaucrats' loyalty, a purge can effective increase the overall distribution of loyalty within the bureaucracy by specifically removing the least loyal ones. However, it is often difficult for autocrats to distinguish disloyal bureaucrats, which further limits the selection effect.

2.2 Purges and Bureaucratic Incentives

Past purges are perceived as a signal for future purges. Violence changes behavior not momentarily; it persistently shapes how individuals perceive the state's intention of using violence. When violence is used to punish certain types of behavior and this signal of punishment is effectively communicated, individuals who observe the signal will be compelled to avoid the behavior perceived as punishable in the future (Kalyvas 2006; Lichbach 1987). As such, momentary use of violence can generate a lasting threat perceived by the population and results in persistent behavior patterns in alignment with the state's preferences (Rozenas and Zhukov 2019). Variations in this perceived threat result from both the breadth of violence and the intensity of violence (Montagnes and Wolton 2019). Bureaucrats are less likely to associate a purge to their own safety when it only victimizes few people. However, as the share of purged individuals increases, other bureaucrats will perceive a greater threat of purges in the following periods. Hence, the coercive effect of purges on future bureaucratic behavior increases as the breadth of purges expands.

Bureaucrats survived from past purges persistently signal loyalty to keep themselves safe from future purges. Purges are fundamentally rooted in enforcing loyalty, regardless of whether they target elites, bureaucrats, or the masses. This logic is evident in notable purges in modern history, including the Soviet Union under Stalin, China under Mao, Iraq under Saddam Hussein, Turkey under Erdoğan, and the United States during McCarthyism. A credible threat of purges can extract loyalty, but its manifestation might differ between the general populace and state bureaucrats. Citizens demonstrate loyalty by avoiding behavior that could be perceived as challenging the regime, a more passive form of loyalty

signaling. In contrast, the threat of purges within the bureaucracy may create "high-powered" incentives that compel bureaucrats to proactively demonstrate loyalty through their performance in policy implementation.

Bureaucrats signal loyalty by over-performing on the dictator's project. Autocrats often interpret bureaucrats' loyalty from their performance in executing orders. If we consider loyalty as private types reflecting a bureaucrat's ideological congruence with the autocrat, the autocrat who seeks to maximize the proportion of congruent agents only purges agents with bad performance, because congruent subordinates tend to exert more effort than incongruent ones (Montagnes and Wolton 2019, 1046). Moreover, bureaucrats possess agency beyond simply following orders. They often operate in environments that provide them with both incentives and opportunities to over-perform on state-assigned tasks (Breton and Wintrobe 1986; Greitens 2016; Kung and Chen 2011). In autocracies, a primary source of such incentives is the threat of purges. During Stalin's Great Terror, for example, local Soviet officials vied to expand their repression quotas as a display of loyalty, driven by fear for their own survival (Gregory 2009).

Bureaucrats' over-performance can lead to negative social outcomes. Unlike democracies where bureaucratic behavior is largely constrained by citizen preferences, bureaucrats in autocracies face limited incentives to prioritize citizen welfare when implementing state policies. Autocrats often direct bureaucrats to implement tasks that primarily serve the regime's interests, such as suppressing dissent, manipulating elections, and engaging in corruption, often at significant social costs. Even in the context of economic development projects, a simple positive correlation between bureaucratic performance on state-defined goals and social welfare does not always exist (Scott 1998). Consider taxation in autocracies. A moderate level of taxation, when devoted for public goods, increases citizen welfare. However, autocrats often impose higher tax rates than needed for public goods. Beyond a certain level of taxation, the society's immediate income and future incentives for production both diminish (Olson 1993). This negative social outcome is particularly likely when bureaucrats, driven by the need to meet demanding targets set by the autocrat, prioritize revenue collection over the economic well-being of citizens.

3 Historical Context

In June 1957, the CCP launched a nationwide campaign to purge alleged "rightists" within the CCP and abroad. During the campaign, hundreds of thousands of people were labeled as "rightists" and condemned as regime enemies. The purge specifically targeted ideologically disaffected individuals in the party and among the general populace, particularly those who had expressed criticism of the CCP's policies. Shortly before the campaign began, Mao proposed the "hundred flowers" policy encouraging free airing of criticism to the party. The policy signaled a relaxation of repression and received enthusiastic responses from educated individuals. These people actively expressed dissatisfaction with the lack of democracy, repressive policies, and hasty socialist reforms imposed by the communist party. Mao was soon overwhelmed by this unexpected outburst of resentment; he reacted by persecuting the critics using the Anti-rightist Campaign. By the time the campaign concluded in 1958, nearly half a million people were branded as "rightists" and suffered severe punishment (Shen 2008, 662). Alleged rightists were deprived of jobs and freedom and sent to labor camps and collective farms. A large number of them died in later political campaigns when they suffered heightened violence.

In addition to targeting members of the so-called democratic parties, intellectuals, journalists, and college students, the purge also victimized a large number of CCP members and young cadres who had responded to Mao's call to correct the party's flaws through open criticism (Walder 2015, 152). A significant proportion of purge victims at the county level were employees of public institutes, as documented by county gazetteers. According to available data from eighty-three county gazetteers, government officials comprised 60% of the rightists, on average. An additional dataset, consisting of about 10,000 persecuted individuals, reveals that between 37% and 48% of them were government employees.

The anti-rightist purge was followed by the GLF, a radical economic campaign launched by Mao with the goal of swiftly increasing China's agricultural and industrial output. In the agricultural sector, the campaign aimed to achieve a rapid breakthrough in grain sup-

plies through political mobilization. Mao bypassed central planners and compelled local officials to quickly increase grain production. While local officials displayed great zeal for Mao's initiative, their efforts to increase grain output turned out to be a gross failure. Local officials experimented with a variety of production measures that proved counter-productive and caused a significant decline in grain output across the nation.

The consequences of the GLF were disastrous. The campaign was followed by a nationwide famine that caused about 30 million deaths (Ashton et al. 1984; Cao 2005). Five percent of the Chinese population perished within three years, making it the deadliest famine in human history. The GLF famine was caused primarily by over-zealous bureaucratic behavior (Walder 2015, 159-162). Misguided production measures promoted by local officials led to massive crop failures. Despite production declines, local officials increased grain extraction from agricultural collectives, leaving the rural population with inadequate food for subsistence. The amount of grain taken by the state increased by 40% from 1957 to 1959 when output fell by 13% (Li and Yang 2005, 846). Grain retention by farmers consequently dropped below subsistence level, condemning tens of millions to death.

During the GLF, local officials were involved in a race to produce wildly exaggerated crop yield figures. This was also known as announcing high-yield satellites – a metaphor for astronomical achievement in crop yields – that emerged as a response to Mao's call for "leaping forward." To show their enthusiasm for Mao's plan, local officials competed to fabricate extravagant agricultural successes (Xu and Tian 2020). Despite their fraudulent nature, those high-yield satellites received accolades from Mao, who needed them to silence colleagues opposed to his rash plan (Bernstein 2006, 441-442). The CCP's supreme political media, *People's Daily*, published hundreds of articles boasting about and celebrating the new records of crop yields reported by various localities. The propaganda was accompanied by inflated statistics of grain output at all administrative levels. As an aggregation of local statistics, reported national grain output was inflated by 88% in 1958 and 59% in 1959 (Walder 2015, 170-171).

Local officials' fabrication of agricultural production was not merely cheap talk. As local officials falsely reported massive increases in grain output, their grain delivery quotas

from agricultural collectives to the state were raised accordingly. The central government mandated local officials to "procure" a vast majority (80 to 90 percent) of the surplus in grain output from agricultural collectives (Bernstein 1984, 352). Since the procurement quotas covered almost all surplus grain, the amount of grain procurement hinged on reported output. Officials who had launched grain satellites were trapped by their earlier exaggerations, which forced them to increase grain procurement (Walder 2015, 159).

How did local officials find more grain for extraction in the face of declining production? They blamed farmers and accused them of hiding "surplus" grain from the state. A campaign was waged against alleged grain hoarding: homes were ransacked, heads of households were detained and interrogated, and grain rations were denied (Walder 2015, 165). Local officials' frenetic efforts to squeeze the last portion of food from farmers played a central role in precipitating mass starvation.

Qualitative evidence suggests that the over-zealous bureaucratic behavior during the GLF was partly due to the Anti-rightist Campaign. During the campaign, any dissent over Mao's agricultural policies would be turned into political accusations. Criticizing official policies, especially the grain procurement policy, was the main criteria to establish someone as a rightist (Cao and Li 2010; Song 2015). The distinctive political nature of the anti-rightist purge differentiated it from other bureaucratic purges. Through the anti-rightist purge, Mao signaled that any disagreement with his policies would be treated as disloyalty and result in dire misfortune. This incentivized officials to publicly display support for his "leaping forward" by executing the campaign with excessive zeal (Walder 2015, 152–169).

4 Data and Variables

Our analysis exploits the variations at the county level. The anti-rightist purge persecuted a large number of county bureaucrats, who played a significant role in implementing the grain procurement policy, especially in determining the amount of grain to be extracted

from rural collectives (Oi 1989, 41).[1] Our key variables—purges, grain extraction, and famine mortality—all exhibit rich variations at the county level.

We assemble a data set covering 2,062 county-level divisions. Our key independent variable is a cross-sectional measure for purge intensity during the anti-rightist purge. The outcome variables are panel data for grain extraction and mortality rates, both measured between 1954 and 1961.

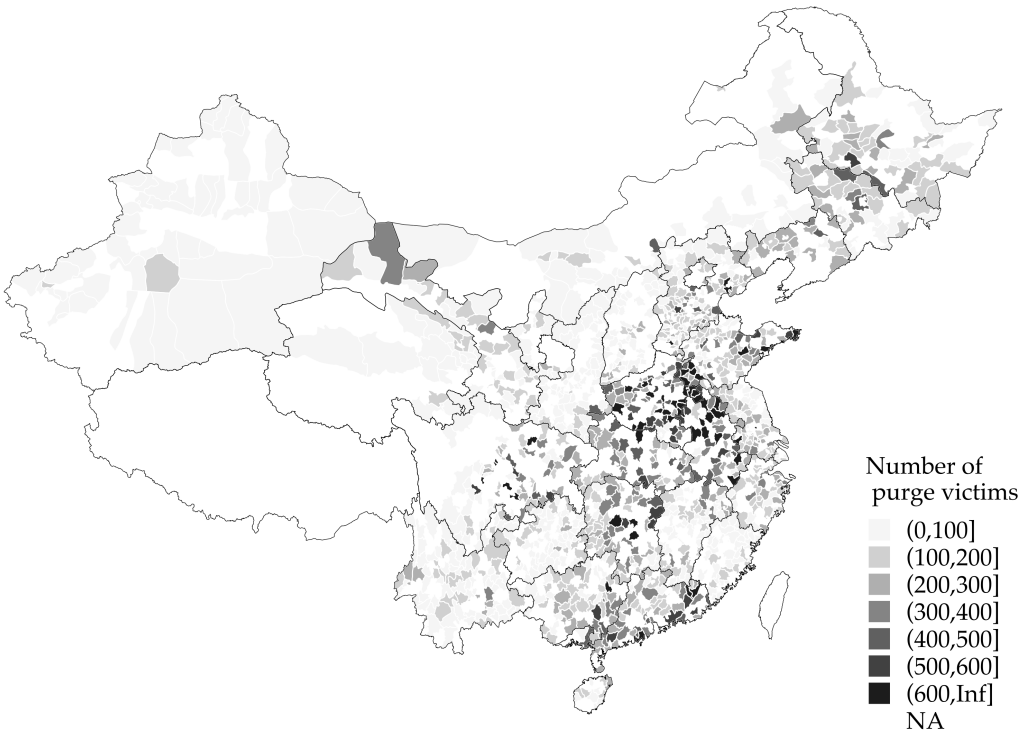
Purge Intensity The data are drawn from county gazetteers under the chapter, "Chronicle of Events" (*dashiji*). We searched the gazetteers for all counties in our data set and found records for the number of people purged during the Anti-rightist Campaign in 1,389 counties. We construct two cross-sectional measures for purge intensity: (1) *purge victims*—the number of people purged in a county, and (2) *victim density*—purge victims per thousand population. Figure 1 shows the variation of purge victims across counties. The average number of people purged was 180 with a standard deviation of 220.

Concerns may arise about data quality. One concern is about selective coverage of state repression by county gazetteers. Statistics related to the number of state violence victims are often censored from county gazetteers (Yan and Xiao 2023). The fact that we only found purge victim data for about 1,400 counties confirms the presence of censorship. To assess the extent to which the censorship was selective, we examine whether purge data availability is correlated with observable county characteristics. We do not find any significant correlations, alleviating concerns about potential bias due to sample selection. Moreover, purge data availability does not correlate with county death rates during the GLF, mitigating concerns about selecting observations based on the dependent variable (see Appendix A1).

To understand the sources of variations in purge intensity, we analyze the relationship between purge intensity and various county characteristics. There was no significant relationship between purge intensity and the characteristics of county party secretaries. In particular, we do not find evidence suggesting that native county party secretaries – those

[1]Despite decentralization during the GLF, county officials still played a vital role in setting grain procurement quotas and overseeing the implementation of the quotas at the grassroots level (Oi 1989, 55–62, 84-103).

Figure 1: Purge Victims Across Counties



who administered a county within their home province – persecuted fewer people during the Anti-rightist Campaign. Furthermore, the correlations between purge intensity and the intensity of previous political campaigns were mostly insignificant. This suggests that purge intensity was unrelated to the factors that drove the intensity of earlier campaigns. On the other hand, counties with a larger population size and higher grain productivity had higher purge victim densities (see Appendix A2).

Grain Extraction We compile a county-year panel of grain output and state procurement for the period from 1954 to 1961. The data are collected from three major sources. First, we searched the gazetteer for all counties in our county list and digitized all available grain production and procurement data for the period 1954-1961. The second source is provincial or prefectural statistical materials on agriculture and economy. We digitized all available county-level data on grain production and state procurement covered by these

materials. Appendix A3 provides the names of the statistical materials. The third source is grain production data from the Ministry of Agriculture, which cover 17 provinces with data available for around 20 counties in each province (Kasahara and Li 2020).

We measure excessive grain extraction based on grain retention levels, which reflected the quantity of grain left in rural communities after state procurement. We calculate grain retention per capita using the difference between total production and procurement then divided by county population.[2] To measure excessive grain extraction, we create a dummy variable that equals one if per capita grain retention fell below 165 kilograms per year after state procurement, a threshold based on official grain ration scales. Specifically, the central government stipulated a minimum grain ration of 27.5 jin per month, or 165 kilograms per year, for urban adults with minimum labor activities.[3] This represents a food ration around the subsistence level, which is barely enough to prevent a person from going hungry.

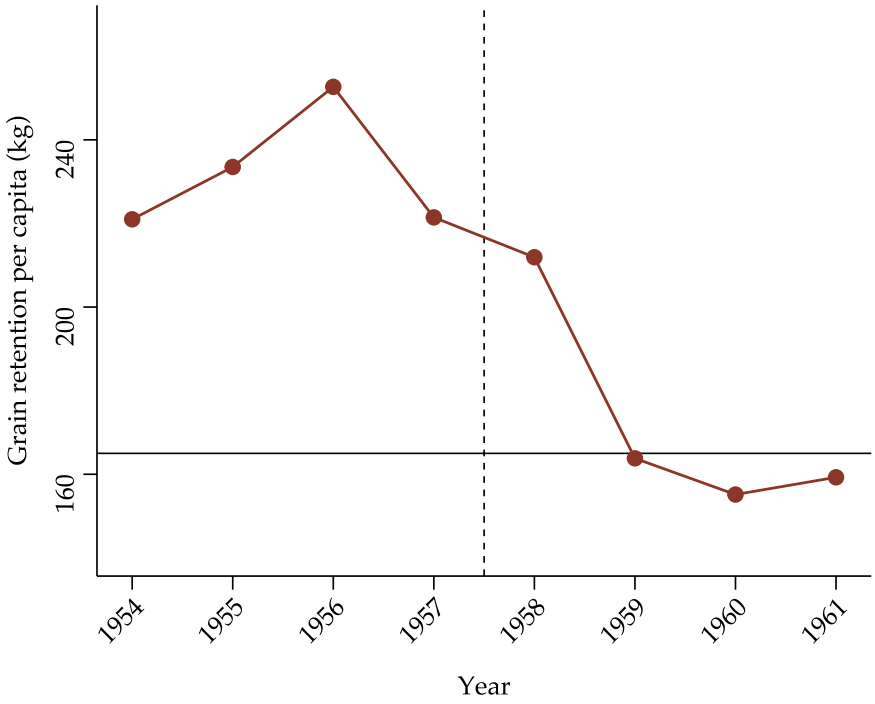
Figure 2 illustrates how county grain retention levels changed over time. There was a significant decline in grain retention across counties since 1959. During the three-year period of the famine, the average annual retention fell below the minimum quota of 165 kilograms. In addition to temporal variations, the intensity of grain extraction also displayed large variations across counties during the GLF. The standard deviation of grain retention per capita was between 77 and 93 kilograms during 1958-1961.

A concern about our data is that the local statistics of grain output could have been inflated. The county gazetteers and statistical materials from which our data were digitized were compiled in the post-Mao era, a period when the political imperative to exaggerate grain production had diminished. Existing research shows that economic statistics from county gazetteers are less susceptible to misreporting (Almond, Li and Zhang 2019, 565). Additionally, at the time of compilation, those statistics were presumably corrected to rectify reporting errors from the Mao era, as our data reveal significant decreases in grain output during the GLF, which was aligned with the actual situation. For instance, using

[2]We use the total population in a county as a substitute for the rural population, since the latter contains a large proportion of missing values. For counties in our sample, the proportions of the rural population were sufficiently high (mostly larger than 90%) with small variations.

[3]Because similar regulations were absent for the rural population, we use the scale for the urban population instead. See Policy documents (1979) for details about the official food ration.

Figure 2: Temporal Variations in Grain Extraction



Notes: County average values of annual grain retention per capita in kilograms. The horizontal solid line represents the minimum annual food quota of 165 kilograms per person according to official documents.

county grain output in 1957 as a benchmark, our data show that approximately 79% of the counties with available data (N=1,504) experienced a decline in grain output in 1960. This provides further assurance to the quality of the grain output data used for our analysis.

Mortality Rates We compile a county-year panel of death rates between 1954 and 1961. The data are mainly drawn from provincial statistical materials on population. We found the materials for most provinces, from which we digitized all county-level data on total population and death rates. When corresponding material for a province was not available or consisted of many missing values, we further searched county gazetteers for the data.

For each county-year observation, the death rate is defined as the number of deaths per thousand population. In the years before 1958 or after 1961, the average death rate was between 10 and 13 per thousand population. During 1959-1961, it reached 18, 28, and

16 per thousand population, respectively. The largest number of deaths was observed in 1960 and amounted to 46% of all deaths during this three-year period. Death rates during the famine varied substantially across counties, with a standard deviation of 26 per thousand population in 1960. The average death rate in provinces like Shanxi and Jiangxi changed modestly during the famine. By contrast, in Anhui and Sichuan the average death rate increased by 30 per thousand population. A majority of the counties were affected by the famine with 77% experiencing higher than usual death rates.

Covariates In the difference-in-differences estimations that follow, we control for a number of cross-sectional covariates.

- We control for county population in 1957, which can be correlated with both purge intensity and famine mortality. The data are drawn from the same sources as the mortality rate data.
- We control for weather conditions in the pre-famine period using rainfall deviation. We draw on precipitation data from *China Precipitation Materials*, which provides the precipitation level at over 450 locations across the country in each year between 1951 and 1970. We map the data to the county level using spatial interpolation. For each county-year observation, we calculate the degree that the precipitation deviated from the county's long-term average between 1951 and 1970. We use aggregated rainfall deviations between 1954 and 1957 as the covariate.
- We control for county grain productivity in 1957 using per capita grain production, which has been found to be positively correlated with famine mortality (Meng, Qian and Yared 2015). The data are digitized from *Compilation of County Statistical Materials on Sown Area and Crop Yields in Each Province, Municipality, and Region for 1957*.
- Because local school teachers were one of the primary targets of the anti-rightist purge (Shen 2008), we control for the number of middle schools in a county in 1947. The data are digitized from *The Second Chinese Education Yearbook*.

- Local clan strength has been found to be negatively correlated with famine mortality (Cao, Xu and Zhang 2022). We control for county clan strength using the number of genealogy books compiled before 1950 as a proxy. The data are from Dincecco and Wang (2021).

- Moreover, we control for the influence of central elites using the number of native members in the CCP's Central Committee, which has found to be negatively correlated with famine mortality (Kung and Zhou 2021). We focus on the Eighth Central Committee, which consisted of 195 members appointed in 1956. The data are digitized from *The Dictionary of the CCP Central Committee Members*.

- We control for the densities of CCP members and KMT members at the county level, which reflect the number of party members per thousand population. We digitize the number of party members in each county from county gazetteers and then divide it by county population. For CCP membership, we use the average density between 1954 and 1957, with missing values estimated using spatial interpolation. We digitize cross-sectional data on KMT membership in the 1940s and calculate the density based on county population in 1953. For counties without density observations, we estimate the values using spatial interpolation.

- Following previous research (Kung and Chen 2011), we control for a vector of characteristics of the First Party Secretary of each province in 1957 (PSC covariates). These characteristics capture factors at the provincial level that might have affected both purges and famine mortality.

Table 1 shows descriptive statistics of the key variables and the covariates.

5 Estimating the Effects of Purges

We estimate the effects of purges on grain extraction and famine mortality using the county-year panel. Our estimation strategy exploits the timing of the GLF and cross-

Table 1: Descriptive Statistics

	N	Mean	SD	Min	Max
Explanatory variables					
Log purge victims	1389	4.70	1.06	0.00	8.26
Log victim density	1337	-0.59	0.78	-3.90	2.71
Outcome variables					
Over-extraction	8804	0.35	0.48	0.00	1.00
Log mortality rate	13197	2.58	0.52	-2.14	5.95
County-level covariates					
Population, 1957	1923	0.28	0.22	0.00	3.65
Rainfall deviation	1960	3.26	1.04	0.44	6.33
Grain productivity	1874	0.33	0.32	0.00	6.24
Middle schools	2062	0.84	1.56	0.00	24.00
Log genealogies	1960	0.93	1.33	0.00	6.88
Central elites	2062	0.09	0.41	0.00	8.00
Log CCP density	2044	2.46	0.62	-2.66	4.92
Log KMT density	1960	1.32	1.25	-3.94	5.63

Notes: Descriptive statistics of the key independent variables and the main outcome variables, as well as the county-level covariates. The outcome variables are in a panel structure. All independent variables are cross-sectional.

sectional variations in purge intensity:

$$y_{ct} = \beta \times (purge_c \times GLF_t) + \sum \delta_j \times (X_{cj} \times GLF_t) + \theta_c + \gamma_t + \varepsilon_{ct} \quad (1)$$

where y_{ct} represents our outcome variables including a binary indicator for over-extraction and mortality rates. $purge_c$ represents purge intensity in county c, which is interacted with a time dummy GLF_t that equals one for the years from 1958 to 1961. X_{cj} is a vector of county- and provincial-level covariates detailed in the data section. All covariates are interacted with the GLF dummy. θ_c is county fixed effects. γ_t is year fixed effects. The coefficient of interest β captures how the increases in grain extraction and mortality during the GLF were correlated with purge intensity. We expect to obtain a positive estimate of β for both outcome variables, which indicates that purges increased the likelihood of over-extraction and mortality rates during the GLF.

5.1 Empirical Results

Table 2 shows the estimated effects of purges. The positive and significant estimates of β in Columns (1) and (2) indicate an increase in the likelihood of excessive grain extraction after purges. The estimated coefficient in Column (1) suggests that a one-unit increase in log purge victims will increase the probability of over-extraction by 0.08. To put this marginal effect into perspective, consider an increase in the number of purge victims by an interquartile range, from the 25th percentile (55 victims) to the 75th percentile (226 victims). Such an increase is expected to increase over-extraction probability by 0.11, which is equivalent to 22% of the average over-extraction probability during the GLF. This quantitative evidence provides support to multiple historical accounts that link over-zealous bureaucratic behavior during the GLF to the Anti-Rightist Campaign (Bernstein 2006, 423; Joseph 1986, 427-428; Walder 2015, 152).

Excessive grain extraction is widely considered to be one of the main causes of the Great Chinese Famine. A recent study by Kasahara and Li (2020) shows that excess deaths in 1960 would have been lower by 27% if the grain procurement policy had been the same as in 1957. Since the likelihood of excessive grain extraction was increased by previous purges, we may expect that previous purges further led to increases in famine mortality. The positive and significant estimates of β in Columns (3) and (4) of Table 2 support this hypothesis. The estimated coefficient 0.054 in Column (3) indicates that a one-unit increase in log purge victims was associated with a 5.4% increase in log mortality rates during the GLF. Based on this marginal effect, increasing the number of purge victims by an interquartile range is expected to increase famine mortality rates by 8%. According to the regression model's prediction, the same increase in purge victims will lead to an increase in the average mortality rate during a famine year from 26 to 28 per thousand people. This is likely a conservative estimate when we consider potential downward bias, which we discuss in more detail in later sections.

Figure 3 illustrates the effects of various repression campaigns on the GLF outcomes in event studies. Panel (a) shows a significant change in the relationship between purge

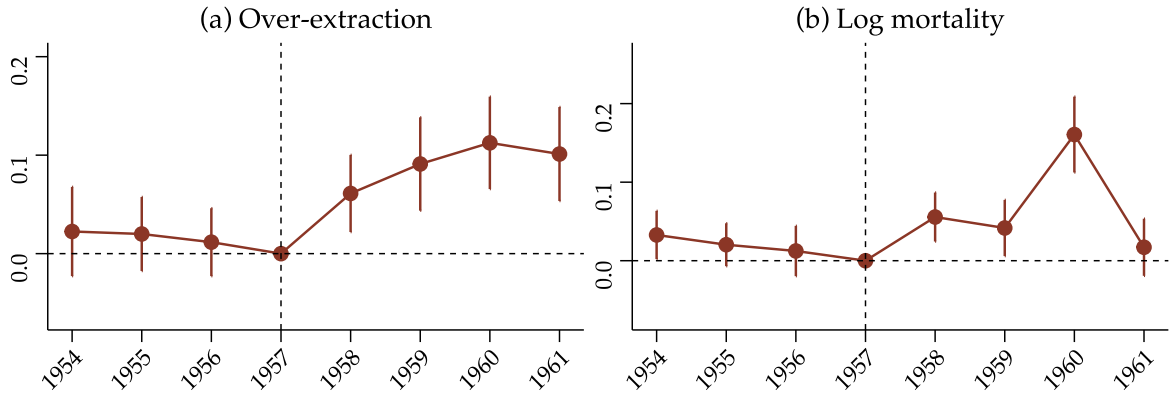
Table 2: Effects of Purges on Grain Extraction and Famine Mortality

	Over-extraction		Log mortality rate	
	(1)	(2)	(3)	(4)
Log purge victims × GLF	0.080***		0.054***	
	(0.016)		(0.016)	
Log victim density × GLF		0.068***		0.043***
		(0.018)		(0.017)
Population, 1957 × GLF	-0.088	0.183**	0.153*	0.323***
	(0.087)	(0.073)	(0.083)	(0.068)
Rainfall deviation × GLF	-0.018	-0.015	0.013	0.015
	(0.014)	(0.014)	(0.012)	(0.012)
Grain productivity × GLF	-0.088**	-0.103***	0.053	0.047
	(0.035)	(0.038)	(0.038)	(0.041)
Middle schools × GLF	0.012*	0.012*	0.029***	0.028***
	(0.006)	(0.006)	(0.008)	(0.008)
Log genealogies × GLF	-0.025**	-0.022**	-0.041***	-0.039***
	(0.011)	(0.011)	(0.008)	(0.008)
Central elites × GLF	-0.027	-0.028	-0.028	-0.029
	(0.038)	(0.039)	(0.019)	(0.018)
Log CCP density × GLF	0.017	0.006	-0.066***	-0.071***
	(0.025)	(0.025)	(0.023)	(0.024)
Log KMT density × GLF	0.005	0.000	0.057***	0.054***
	(0.011)	(0.011)	(0.010)	(0.010)
PSC covariates × GLF	Yes	Yes	Yes	Yes
Observations	6,237	6,237	8,765	8,765
Adjusted R^2	0.48	0.48	0.45	0.45
County FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes

Notes: Standard errors clustered at the county level in parentheses. ***p $<$ 0.01; **p $<$ 0.05; *p $<$ 0.1.

intensity and our main outcome variables during the GLF, when the correlations became significantly higher compared with periods before the GLF. Moreover, there was no discernible divergence in the outcomes prior to 1957 across counties with different purge intensities, which indicates that the estimated effects are not driven by time-varying confounding variables.

Figure 3: Effects of Purges on Grain Extraction and Famine Mortality



Notes:

5.2 Robustness Tests

To evaluate potential bias due to missing observations on purge intensity, we interpolate the missing values based on spatial correlations within the data. We regress the outcome variables on interpolated purge victims, while adjusting standard errors to address increasing spatial correlations after interpolation. The estimated effects of purges remain statistically significant ($p < 0.01$). After interpolation, the estimated coefficient increases by about 58% for excessive grain extraction and by about 144% for log mortality rate, which indicates potential underestimation of the effects with the presence of missing observations (see Appendix A4).

We evaluate the sensitivity of our results to potential measurement errors following (Gallop and Weschle 2019). We focus on potential bias in our estimation when measurement errors in purge victims are correlated with the outcome variable, famine mortality. With the assumption that the number of purge victims was systematically over-reported or under-reported for counties that experienced famine afterward, we adjust purge victims to various hypothetical numbers and re-estimate the effect on famine mortality.[4] The results suggest that our estimate is subject to a downward bias when the number of purge victims was under-reported for famine counties. In contrast, if the number of victims was

[4]We define famine counties as counties where the average death rate during 1958-1961 was higher than the average death rate during 1954-1957.

over-reported for famine counties, the effect of purges could have been over-estimated. Qualitative evidence suggests that county gazetteers were more likely to under-report the number of victims of state violence (Yan and Xiao 2023), which alleviates the concern about over-estimation due to measurement errors. Moreover, the sensitivity analysis suggests that the number of purge victims in famine counties has to be exaggerated by over 18% to invalidate our results (see Appendix A5).

We further test the robustness of our results using an alternative measure for famine severity. We measure famine severity using census data, which are less susceptible to manipulation than reported death rates. Following previous research (Meng, Qian and Yared 2015), we estimate county-specific population sizes for each birth cohort using a one-percent random sample of the 2000 Census. Famines can reduce fertility and increase infant mortality, resulting in lower population sizes for cohorts born during famine years (famine cohorts). According to our data, the average population size of the famine cohorts was 26% lower than the pre-famine cohorts. Thus, variations in famine cohort sizes can reflect famine severity across counties. We replicate the baseline regression, this time using the log number of people born in county c and year t as the outcome variable. We find that counties with more purge victims in 1957 suffered greater population losses during the famine, as captured by the significant decrease in birth cohort sizes (see Appendix A6).

5.3 Falsification Tests

The anti-rightist purge had two attributes that were instrumental for understanding its negative consequences during the GLF. The purge widely victimized local bureaucrats who were responsible for implementing state grain extraction policies. Moreover, the purge itself was a coercive response to policy disagreement within the bureaucracy. By persecuting bureaucrats who questioned the state's grain procurement policies, the purge politicized those policies and significantly affected their implementation in subsequent periods (Walder 2015). These two features together set the anti-rightist purge apart from

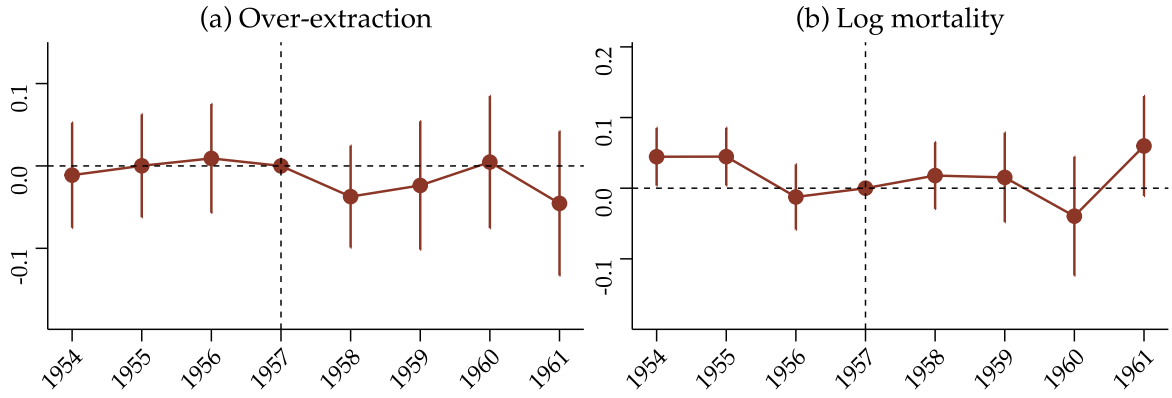
other repression campaigns that took place prior to the GLF, which either targeted different groups or were not directly linked with the policies of interest. Due to these distinctions, we would not expect other repression campaigns to have the same effect as the anti-rightist purge had during the GLF. However, if our estimated effects of purges were driven by unobservable county characteristics or measurement errors that shaped the variations of all repression campaigns, we may find significant correlations between other repression campaigns and the GLF outcomes as well.

We conduct falsification tests by substituting purge intensity with the intensities of other repression campaigns and estimate their effects on grain extraction and famine mortality. We focus on two repression campaigns. One is the Campaign to Suppress Counter-revolutionaries between 1950 and 1953, which persecuted individuals with connections with the KMT regime, secret societies, criminal gangs, and religious sects. To measure the intensity of this campaign, we digitize the total number of people killed in each county from county gazetteers. The other campaign is the *San Fan Wu Fan* in 1951 and 1952, characterized by violent expropriations of private assets by the state under the name of anti-corruption, with CCP members, bureaucrats, and business-owners as the primary targets. We measure its intensity using the number of people prosecuted in a county drawn from county gazetteers. While the Suppression Campaign and the Anti-corruption Campaign also involved extensive persecution of alleged regime enemies, neither of them were directly linked with state grain procurement policies, which were officially introduced in 1953 after these campaigns had concluded.

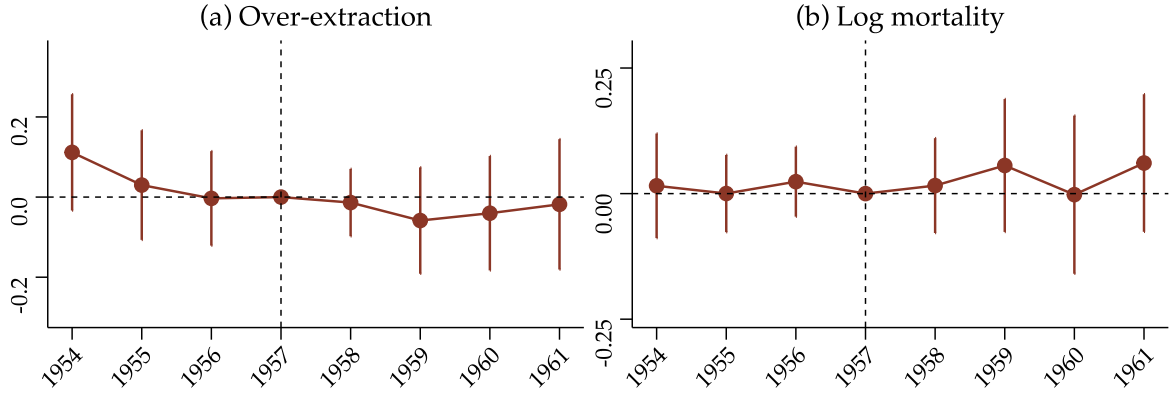
Figure 4 shows the estimated effects of the Suppression Campaign and the Anti-corruption Campaign, respectively. Unlike the estimates for the anti-rightist purge, no significant correlation is detected between these campaigns and the GLF outcomes. This aligns with our assumption about the key causal mechanisms underlying the effect of anti-rightist purges: politicization of grain procurement policies by persecuting bureaucrats. Furthermore, the absence of significant correlations in these falsification tests alleviates concerns about potential omitted variables and measurement errors.

Figure 4: Campaigns of Repression and Famine Mortality

Panel (a): *Effects of the Anti-Corruption Campaign*



Panel (b): *Effects of the Suppression of Counterrevolutionaries*



Notes: Estimated effects of two other repressive campaigns on death rates. All regressions control for the baseline covariates interacted with year dummies, county fixed effects, and year fixed effects. 95% confidence intervals based on standard errors clustered at the county level.

5.4 Potential Omitted Variables

Purge intensity can be correlated with unobservable county characteristics that also had an impact on famine mortality. We consider potential bias in our estimation when the variations in purge victims were shaped by the variations in bureaucratic loyalty (or ideological alignment) across counties. Counties with a higher proportion of misaligned bureaucrats were likely to see more purges, which can lead to bureaucrats' over-performance during the GLF. It is also noteworthy that misalignment may have a direct effect on bu-

reaucratic performance, which is expected to be negative (Montagnes and Wolton 2019). When misalignment is an omitted variable, the estimated coefficient of purges may pick up the omitted variable's negative direct effect on over-extraction and famine mortality, resulting in a downward bias in our estimation. Evidence based on simulation supports this intuition: The effect of purges is underestimated when misalignment is omitted in the regression. The degree of the underestimation depends on the omitted variable's influence on purges and its direct effect on over-extraction (see Appendix A7).

Another concern is that purge intensity may reflect a county party leader's loyalty to the regime and a loyal county leader may execute both purges and economic leaps with exceptional zeal. To address this concern, we replicate the baseline estimation with a subsample of counties that experienced a turnover of the party secretary after the purge. In those counties, the two campaigns were overseen by different party secretaries. This subsample design allows us to hold constant the leader fixed effects, which might confound the relationship between purges and the GLF outcomes. The estimated effect of purges from this subsample analysis remains significant ($p < 0.01$), suggesting that the effect of purges was relatively independent of county party leader loyalty (see Appendix A8).

To further address potential bias due to omitted variables, we conduct a sensitivity analysis following Cinelli and Hazlett (2020). To evaluate the sensitivity of our point estimate to omitted variables, we use *clan strength* as a benchmark, which has been found to be significantly correlated with famine mortality (Cao, Xu and Zhang 2022). The sensitivity analysis suggests that the omitted variable would need to be six times as influential as clan strength to completely account for the estimated effect of purges (see Appendix A9). Thus, it is unlikely that our baseline results are mostly driven by omitted variables.

6 Regression Discontinuity Design

6.1 Loyalty, Purges, and Grain Extraction

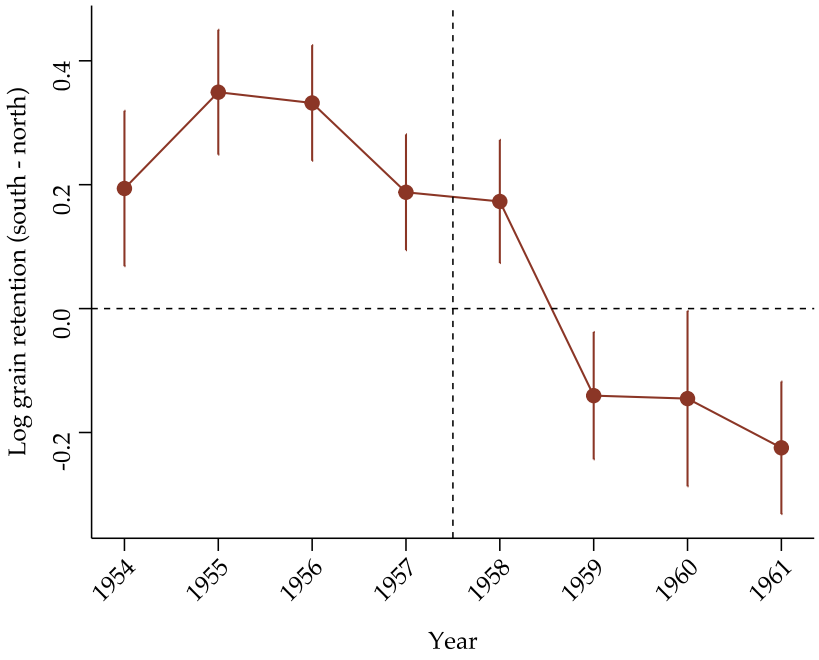
We further use a regression discontinuity design (RDD) to estimate the effect of the anti-rightist purges. In the RDD, we exploit plausibly exogenous variations in purge intensity induced by temporal variations in the CCP's control of different counties during the civil war between 1946 and 1949. The CCP's military victory allowed it to take control over most of the Chinese territories in 1949. Before that, the CCP only controlled the north; the southern territories of China was under the control of the KMT government. This temporal variation in the CCP's military control shaped geographical variations in both purges and grain extraction after the CCP took national power, which exhibited significant differences across the wartime military border.

During its occupation of the north, the CCP launched waves of purges in so-called "old liberated areas" (OLA) (Gao 2018; Opper 2020, 142). However, in the "new liberated areas" (NLA), purging regime enemies did not begin until the 1950s. Due to the temporal variation in the CCP's occupation, there was a remarkable difference in the intensity of the anti-rightist purge between the OLA and the NLA counties. According to our data, purge intensity in the NLA was 23% higher than in the OLA.

In addition to purge intensity, grain extraction under the CCP also exhibited significant differences around the civil war military border. Figure 5 shows the differences in grain retention between counties occupied by the CCP during the civil war and those occupied only by the end of the civil war, both located within a distance of 100 kilometers from the wartime military border. In the years before the anti-rightist purge, grain retention levels were significantly higher in counties located south of the border, which had been under the KMT's control during the war. Not long after the purge, this difference reversed: rural communities in those previous KMT-controlled counties now had lower grain retention levels.

The descriptive evidence above has important implications for estimating the effect

Figure 5: Differences in Grain Retention Around the Military Border

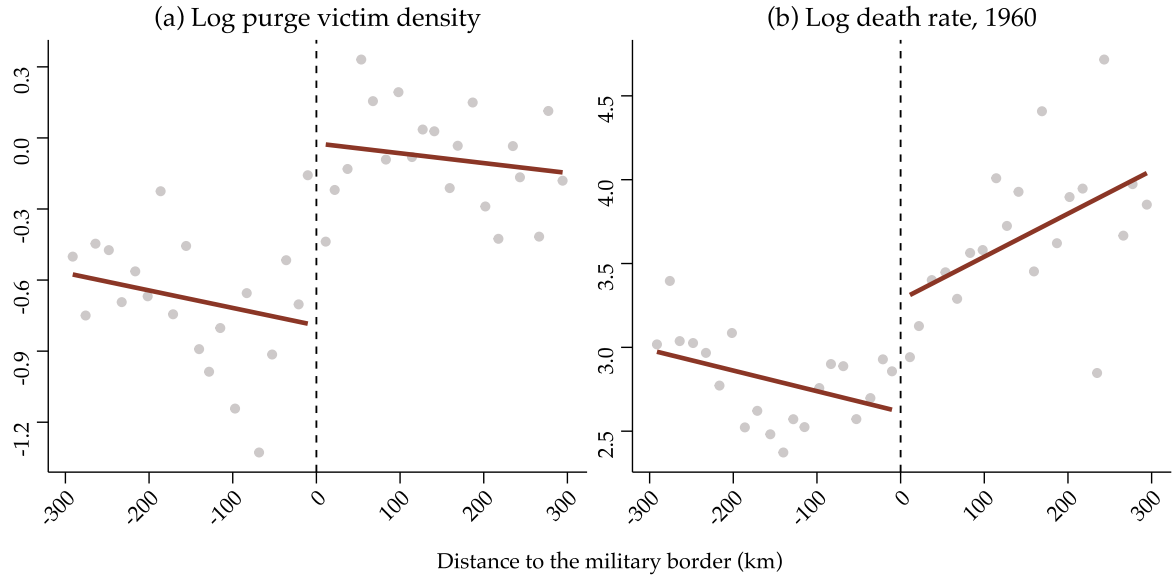


of purges. The officials in previous KMT counties were less enthusiastic for the state's objective of maximizing grain extraction before the purge, possibly due to the presence of more former KMT bureaucrats in those counties. The same counties, after relatively intense purges in 1957-1958, acted even more zealously than their northern counterparts in extracting grain from rural communities. In other words, variations in the composition of the county bureaucracy shaped purge intensity, which could have an independent effect on bureaucratic performance in the next period. This motivates us to use a regression discontinuity design to identify the effect of purges by exploiting potentially discontinuous variations across the civil war military border.

6.2 RDD Estimation and Results

To determine the military border, we use the time of the CCP's land reform as a proxy for military occupation. A county that experienced land reform before 1950 is labeled as belonging to the OLA, while those experiencing land reform in 1950 or after are la-

Figure 6: Discontinuous Changes in Purge Intensity and Famine Mortality



Notes: The points represent the binned average of purge intensity and mortality rates, which is a function of the distances from the military border, as represented by the lines. Negative distance values represent counties located north of the border (CCP-controlled before 1950), while positive distance values represent counties located south of the border (KMT-controlled until 1949).

beled as NLA counties. In the RDD analysis, we restrict the data to counties within seven provinces that enclosed the military border (see Appendix A10).

Figure 6 illustrates how purge intensity and famine mortality varied across the military border. A discontinuous increase in purge victims was observed along the military border and was significantly higher on the southern side. A similar pattern existed for famine death rates: the southern side of the border saw significantly higher mortality rates in 1960 than the northern side.

The spatial RDD estimate is constructed from a two-stage least squares (2SLS) regression in the following form:

$$\text{purge}_i = \lambda + \pi \times \text{NLA}_i + f(d_i) + \theta_p + g(lon_i, lat_i) + \epsilon_i \tag{2}$$

$$\text{mortality}_i = \alpha + \beta \times \widehat{\text{purge}}_i + f(d_i) + \theta_p + g(lon_i, lat_i) + \varepsilon_i \tag{3}$$

where the outcome variable $mortality_i$ is the log number of deaths per thousand popula-

tion in county i in 1960. It is a function of log purge victims predicted by the NLA dummy capturing later CCP control. The equations control for a linear function of Euclidean distance from county i to the nearest point on the military border $f(d_i)$, while allowing the slope to vary on either side of the border. Additionally, we control for province fixed effects θ_p and the fixed effects for longitude and latitude intervals $g(lon_i, lat_i)$. Counties are weighted by the inverse of the running variable, which assigns counties closer to the military border more influence on the RDD estimate.

Regression discontinuity estimates can be interpreted causally if county characteristics varied smoothly around the military border. Because the battlefront shifted back and forth due to varied combat outcomes, the military border's geographical location was determined as if randomly. We further test the randomness of the military border by comparing counties on either side of the border across a number of county characteristics. Table 3 presents the mean values of the county characteristics along with their differences across the military border. In a simple comparison of the mean values between all counties on either side of the border (simple OLS), we observe significant differences on certain county characteristics. Thus, a simple comparison between the old liberated areas and the new liberated areas on famine mortality is likely to yield selection bias. We further conduct the comparison with the RDD estimator to detect potential discontinuities at the cutoff, following the recommendation by Cattaneo, Idrobo and Titiunik (2019, 79-80). We restrict the comparison to counties within a distance of 100 kilometers from the military border, while conditioning on linear functions of the distance and the fixed effects. The RDD estimates in Table 3 indicate that counties in the vicinity of the military border were much more comparable, with no major discontinuities on various county characteristics after conditioning on the covariates.

Given the specific geographical conditions of the broad CCP-controlled areas, comparison between all counties on either side of the military is likely to be confounded by differences other than purge intensity, as indicated by the simple OLS results in Table 3. To obtain a valid counterfactual, the RDD instead compares only counties in close proximity to the military border, based on the fact that the political, socioeconomic, and geographical characteristics are smooth around the border. Comparing counties that just

Table 3: Balance of County Characteristics Across the RDD Border

Variable	Mean	Coef.	p-value	N
Political characteristics				
Log CCP density	2.50	-0.04	0.74	191
Log KMT density	0.60	0.31	0.14	219
Central elites	0.07	-0.04	0.20	192
Socioeconomic characteristics				
Death rate, 1957	11.36	0.77	0.21	186
Population, 1957	0.43	0.06	0.38	183
Grain productivity	0.29	0.00	0.97	183
Middle schools	0.62	0.58	0.11	192
Log genealogies	1.48	0.31	0.36	192
Distance to railroads	39.94	0.12	0.99	206
Geographical characteristics				
Rice suitability	9.10	1.26	0.58	192
Terrain ruggedness	1.23	0.57	0.05	192
Precipitation	8.99	0.05	0.70	219
Temperature	13.71	-0.10	0.28	219

Notes: Sharp RDD estimates for the difference in observable county characteristics around the military border with a bandwidth of 100 kilometers. The p-values correspond to standard errors clustered within 1×1 degree grid cells.

happened to be within the control of the CCP with those marginally fell outside due to military contingencies thus helps isolate the causal effect of purges shaped by the border. As such, the fuzzy RDD estimator calculates the local average treatment effect of higher purge intensities due to later CCP control for counties just around the border of control. In selecting counties for the RDD estimation, we use the MSE-optimal bandwidth following Calonico, Cattaneo and Titiunik (2014).

Table 4 displays the spatial RDD estimates. In the first-stage estimation, we regress purge victims on later CCP control. The estimated coefficient indicates significantly higher numbers of purge victims in counties under later CCP control. The reduced-form regression further shows significantly higher mortality rates in 1960 for counties under later CCP control. The fuzzy RDD estimate based on the two-stage regression suggests this reduced-form relationship is driven by purges.

The RDD estimates in Table 4 are based on linear functions of distance and MSE-

Table 4: Spatial RDD Estimates

Regression	Outcome variable	Coefficient	Std. Error	Bandwidth	N
First-stage	Log purge victims	0.67	0.17	90	129
Reduced-form	Log mortality, 1960	0.20	0.08	129	207
Two-stage	Log mortality, 1960	0.47	0.18	111	135

Notes: The effect of purges on famine mortality estimated by spatial RDD. Standard errors adjusted for spatial correlations within 1×1 degree grid cells.

optimal bandwidths. We check the robustness of the fuzzy RDD estimate to other combinations of polynomials and bandwidths. We obtain estimates using different bandwidth values between 80-220 kilometers in 10-kilometer increments. The 95% confidence interval covers only positive values for all combinations of bandwidths and polynomial functions (Appendix A11).

7 Mechanisms

7.1 Loyalty Signaling Incentives

To measure local bureaucrats' behavior of exaggerating agricultural production, we use news reports on high crop yields published in the *People's Daily* (Xu and Tian 2020). We searched for articles that were published between 1954 and 1961 and included the keywords "county" (*xian*) and "yields per mu" (*muchan*). We then read all the articles retrieved by the search engine and matched the crop yield figures mentioned in the articles with the counties in our dataset, based on the location where a high-yield figure was announced. Our data cover a total number of 1,888 high-yield figures from 865 counties. As expected, the majority of those high-yield figures were announced between 1958 and 1960. Seventy-three percent of the crops mentioned were grains, including rice, wheat, maize, and other less common species. We match those high-yield figures to the county-year panel and create dummy variables that equal one if a high-yield figure was

Table 5: Effects of Purges on Production Exaggeration

	All satellites		Grain satellites	
	(1)	(2)	(3)	(4)
Log purge victims × GLF	0.016*		0.016*	
	(0.009)		(0.008)	
Log victim density × GLF		0.020**		0.021**
		(0.009)		(0.009)
Covariates × GLF	Yes	Yes	Yes	Yes
Observations	10,248	10,248	10,248	10,248
Adjusted R^2	0.34	0.34	0.32	0.32
County FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes

Notes: Standard errors clustered at the county level in parentheses. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

announced in a county and year.

We estimate the effect purges on production exaggeration with the baseline model specification, while the outcome variable now is the dummy variable for high-yield satellites. Because the high-yields satellites were primarily concentrated in the early period of the GLF, we use weighted regressions that assign higher weights to years with more high-yield satellites. Table 5 presents the regression results. The positive and significantly estimates indicate that the purge increased the likelihood of announcing high yields in the *People's Daily* by county officials. This supports our hypothesis that the purge affected the GLF outcomes by increasing loyalty signaling incentives among local officials.

7.2 Bureaucratic Replacement

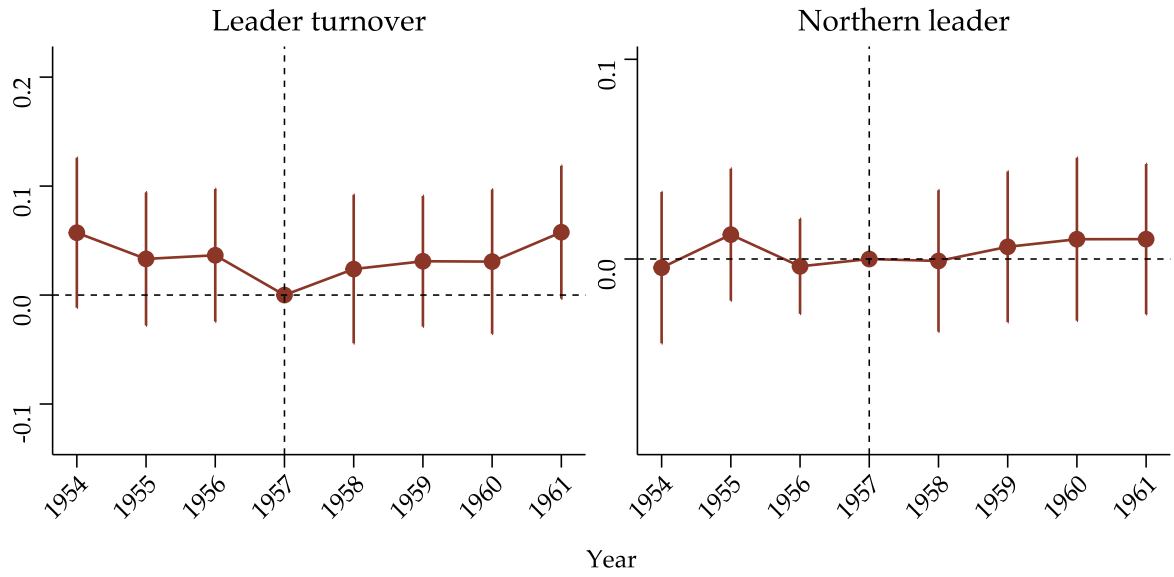
In addition to the incentive effect on surviving bureaucrats, the anti-rightist purge can also affect grain extraction through the replacement of bureaucrats. The campaign might have resulted in the replacement of purged bureaucrats with ideologically more congruent ones, who would have behaved zealously during the GLF even without being coerced.

While an empirical evaluation of the relative impact of these two mechanisms—incentive change and personnel selection—can better inform our understanding of the anti-rightist purge, the available data cannot afford such a test to quantify these channels.

The relative impact of selection versus incentives depends on the breadth of purges and the accuracy with which they identify and remove disloyal bureaucrats. The breadth of the anti-rightist purge was limited: The average purge ratio was less than 0.1% when evaluated against the population. The ratio was less than 8% if evaluated against the number of CCP members in a county. Moreover, qualitative evidence suggests that the purge was not effective at identifying disloyal bureaucrats. The selection of targets was often arbitrary, with many bureaucrats purged only to meet quotas (Cheng 2024; Shen 2008). Thus, it is unlikely that the purged bureaucrats and their replacements differed significantly in terms of loyalty. Given the limited breadth and effectiveness of bureaucratic selection in the anti-rightist purge, this mechanism likely explained less significant variations in over-zealous bureaucratic behavior during the GLF when compared with the incentive mechanism.

Using available data, we examine whether certain changes in the composition of county bureaucracies were correlated with purge intensity. We digitized a data set of county party secretaries who were in the office between 1954 and 1961. Using this data set, we examine whether the turnover and the origin of county party secretaries were affected by the anti-rightist purge. Figure 7 shows the estimated effects of purges on bureaucratic selection in event studies. The left panel illustrates the temporal changes in the correlation between purges and county leader turnovers. It shows that the purge did not result in increasing turnovers of county party secretaries. The right panel shows that the purge did not result in the appointment of more county party secretaries from the north, who could have acted more zealously during the GLF. These results further indicate that the selection effect of purges was likely limited.

Figure 7: Purges and Bureaucratic Selection



Notes: Estimates of the interaction between log purge victims and year dummies. 95% confidence intervals based on standard errors clustered at the county level. All baseline covariates are included in the regressions.

8 Conclusion

This paper analyzed the shadow cost of bureaucratic purges in China under Mao. We found that the purge played a significant role in driving over-zealous bureaucratic behavior during the Great Leap Forward, a catastrophic economic campaign that resulted in the loss of tens of millions of lives. The purge incentivized local officials to display enthusiasm for increasing agricultural output by fabricating statistics about local grain production. The widespread lying about production was accompanied by excessive grain extraction from rural communities, which ultimately caused mass starvation.

This finding has implications for our understanding of how principal-agent problems shape the well-being of citizens in authoritarian states. The agency problem between dictators and bureaucrats is well recognized by scholars of authoritarian politics, specifically in the analysis of state repression, electoral fraud, and other actions to consolidate the dictator's hold onto power (Gehlbach and Simpser 2015; Hassan 2020; Rundlett and Svolik 2016; Tyson 2018). Yet there has been limited research that investigates the societal

consequences of this agency problem. We fill this gap by asking how bureaucratic purges precipitate policy failure. Our finding shows that politically motivated coercion against bureaucrats, which is used to solve the many-to-one accountability problem, can lead to over-zealous behavior in policy implementation, resulting in policy outcomes with negative impact citizens' welfare.

More broadly, our finding highlights the cost of loyalty signaling in authoritarian states. In contrast to the Weberian bureaucracy, bureaucrats in authoritarian states are frequently compelled to show their loyalty to the ruling elite. Bureaucratic behavior in carrying out policies is often interpreted in political terms by the principal. Under the threat of violence, loyalty signaling can dominate bureaucratic choices in policy implementation. The politicization of the administrative process, as this paper shows, is costly to the wider society.

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Supplemental Materials

A1 Whether Purge Data Were Missing on Observables?

Variable	Coefficient	Std. Error	p-value	N
Famine mortality				
Log mortality rate, 1958	0.053	0.033	0.106	1622
Log mortality rate, 1959	0.046	0.029	0.115	1619
Log mortality rate, 1960	0.026	0.022	0.227	1573
Log mortality rate, 1961	0.023	0.030	0.437	1695
Baseline covariates				
Population, 1957	0.024	0.072	0.744	1860
Rainfall deviation	-0.002	0.013	0.870	1960
Grain productivity	-0.056	0.029	0.057	1815
Middle schools	-0.002	0.009	0.789	1960
Log genealogies	-0.011	0.009	0.208	1960
Central elites	-0.014	0.026	0.589	1960
Log CCP density	-0.068	0.026	**0.009**	1948
Log KMT density	0.009	0.009	0.313	1960

Notes: Regressions of purge data availability on county characteristics. The dependent variable is a binary indicator that equals 1 if the observation is missing on purge intensity. The independent variable of each regression is shown. Each coefficient is estimated independently. Standard errors with Conley adjustment for spatial correlations with a 150 kilometers cutoff.

A2 Covariates of Purge Intensity

	Outcome: log purge victim density			
Covariates	Coefficient	Std. Error	p-value	N
Baseline				
Population, 1957	-0.680	0.235	**0.004**	1299
Rainfall deviation	0.035	0.045	0.440	1299
Grain productivity	0.340	0.079	**0.000**	1283
Middle schools	-0.010	0.026	0.720	1299
Log genealogies	0.001	0.025	0.977	1299
Central elites	-0.058	0.078	0.458	1299
Log CCP density	0.007	0.054	0.894	1297
Log KMT density	0.034	0.030	0.255	1299
Additional				
Death rate, 1957	-0.016	0.009	0.073	1191
Native party secretary	-0.152	0.093	0.102	883
Northern party secretary	0.104	0.153	0.498	884
Log suppression victims	-0.004	0.017	0.789	625
Log anticorruption victims	-0.036	0.048	0.461	260

Notes: Regressions of log purge victim density on county-level covariates. The independent variable of each regression is shown in the first column. Each coefficient is estimated independently. Standard errors with Conley adjustment for spatial correlations with a 150 kilometers cutoff.

A3 List of Statistical Materials

Province	Name of statistical materials
Grain data	
Hebei	Chengde wushi nian; Handan 60 nian
Shanxi	Shanxi nongcun tongji ziliao gaiyao (1949-1990)
Neimenggu	Nongmuye shengchan tongji ziliao (1947-1978)
Liaoning	Liaoning nongcun liangshi chan gong xiao tongji shiliao (1949-1990)
Jilin	Jilin nongye 60 nian fazhan shuju huibian (1949-2009)
Jiangsu	Jiangsusheng nongye tongji ziliao (1949-1980)
Zhejiang	Quzhou tongji nianjian 2007; Shaoxing tongji nianjian 1995
Jiangxi	Jiangxisheng fen xianshi nongye tongji ziliao huibian (1949-1965)
Shandong	Jinan jingji he shehui fazhan (1949-1989)
Henan	Jianguo sanshi nian henansheng nongye tongji ziliao (1949-1979)
Hubei	Hubeisheng nongye tongji ziliao (1949-1978)
Guangxi	Guangxi nongye sishiwu nian
Gansu	Xinzhongguo liushi nian: gansu
Qinghai	Qinghaisheng nongye tongji ziliao (1949-1985)
Ningxia	Ningxia huizu zizhiqu nongye tongji lishi ziliao (1949-1988)
Mortality data	
Hebei	Hebeisheng renkou tongji ziliao (1949-1984)
Shanxi	Shanxi renkou sishinian (1949-1990)
Liaoning	Liaoningsheng renkou tongji ziliao (1949-1984)
Jilin	Jilin nongye 60 nian fazhan shuju huibian (1949-2009)
Heilongjiang	Heilongjiang renkou tongji nianjian 1989
Jiangsu	Jiangsusheng renkou tongji ziliao huibian
Zhejiang	Zhejiangsheng renkou tongji ziliao huibian (1949-1985)
Anhui	Anhui shengzhi renkou zhi
Fujian	Fujiansheng renkou tongji ziliao huibian (1949-1988)
Jiangxi	Jiangxisheng renkou tongji ziliao huibian (1949-1985)
Shandong	Shandongsheng renkou tongji ziliao huibian (1949-1984)
Henan	Jianguo sishinian henansheng renkou tongji ziliao huibian (1949-1988)
Hubei	Hubeisheng renkou tongji ziliao (1949-1978)
Hunan	Hunansheng renkou tongji ziliao (1949-1991)
Guangdong	Guangdongsheng renkou tongji ziliao huibian (1949-1985)
Guangxi	Guangxi zhuangzu zizhiqu renkou tongji ziliao huibian (1949-1985)
Hainan	Guangdongsheng renkou tongji ziliao huibian (1949-1985)
Chongqing	Sichuan renkou nianjian (1950-1987)
Sichuan	Sichuan renkou nianjian (1950-1987)
Guizhou	Guizhousheng renkou tongji ziliao huibian (1949-1984)
Yunnan	Yunnansheng renkou tongji ziliao huibian (1949-1988)
Shaanxi	Shaanxisheng renkou tongji ziliao huibian (1949-1990)
Gansu	Gansusheng renkou tongji ziliao huibian (1949-1987)
Ningxia	Ningxia huizu zizhiqu renkou tongji ziliao huibian (1949-1985)

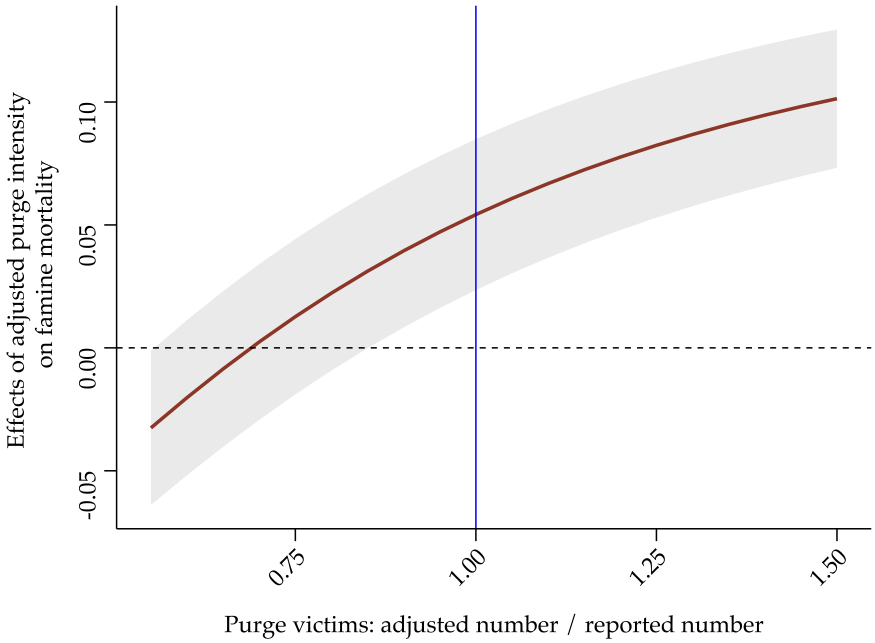
Notes: Statistical materials that provide data on grain production, grain procurement, or mortality rates at the county level. For provinces without statistical materials, we use other data sources (e.g., county gazetteers).

A4 Interpolation for Missing Values of Purges

	Over-extraction		Log mortality rate	
	(1)	(2)	(3)	(4)
Log purge victims × GLF	0.080***		0.054***	
	(0.015)		(0.019)	
Log purge victims (interpolated) × GLF		0.126***		0.132***
		(0.027)		(0.032)
Covariates × GLF	Yes	Yes	Yes	Yes
Observations	6,237	8,525	8,765	12,389
Adjusted R^2	0.48	0.48	0.45	0.47
County FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes

Notes: Standard errors with adjustment for spatial correlations following Conley (1999) with a 150-kilometers cutoff. ***p < 0.01; **p < 0.05; *p < 0.1.

A5 Sensitivity to Measurement Errors



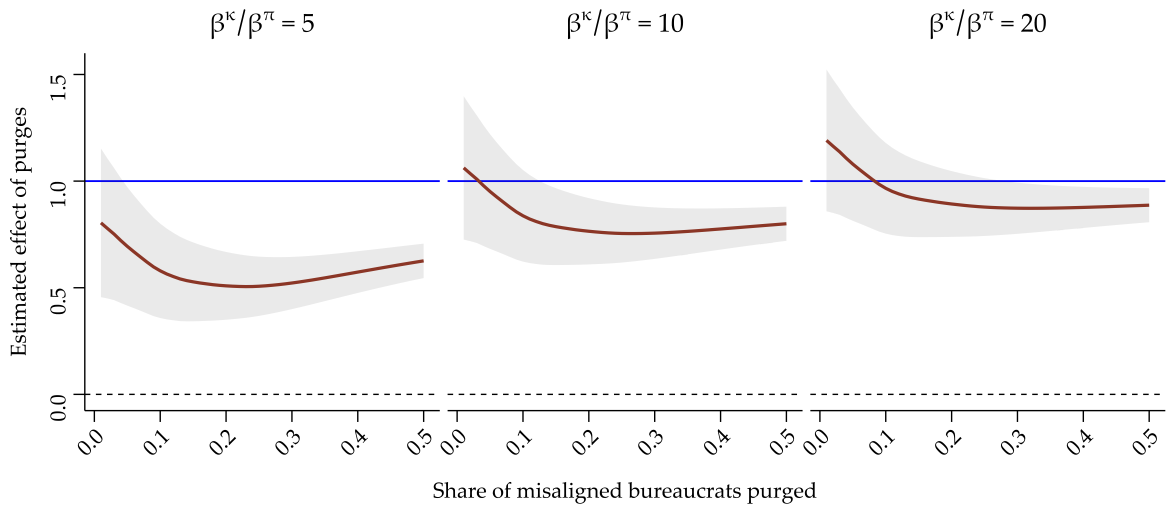
Notes: The estimated effect of purges on famine mortality. Purge intensity is adjusted with different assumptions about potential measurement errors correlated with famine mortality.

A6 Famine Severity Using Census Data

	Log birth cohort sizes	
	(1)	(2)
Log purge victims × GLF	-0.038***	
	(0.011)	
Log victim density × GLF		-0.048***
		(0.012)
Covariates × GLF	Yes	Yes
Observations	9,431	9,431
Adjusted R^2	0.92	0.92
County FEs	Yes	Yes
Year FEs	Yes	Yes

Notes: The effect of purges on famine severity measured by log birth cohort sizes. All baseline covariates are included in the regressions. Standard errors clustered at the county level in parentheses. ***p $<$ 0.01; **p $<$ 0.05; *p $<$ 0.1.

A7 Simulation of Omitted Variable Bias



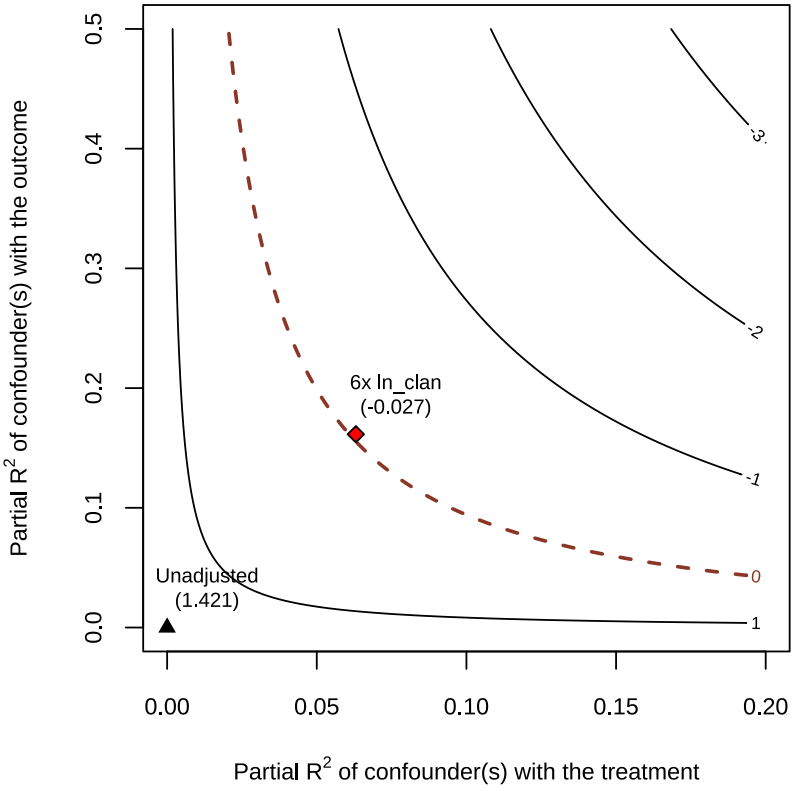
Notes: Following the notations by Montagnes and Wolton (2019), π represents the proportion of congruent agents and κ represents the proportion of bureaucrats purged. β_κ/β_π thus represents the relative effect of purges versus the effect of loyalty (both β_κ and β_π are expected to be positive). The blue horizontal line represents the true effect of purges, while the red line represents the estimated effect of purges with loyalty as the omitted variable. The results suggest a potential downward bias in the presence of the omitted variable loyalty. The bias increases when the relative direct effect of loyalty on famine mortality is large and when the breadth of purges is large.

A8 Subsample with County Leader Turnover

	Over-extraction		Log mortality rate	
	(1)	(2)	(3)	(4)
Log purge victims × GLF	0.075***		0.061***	
	(0.022)		(0.019)	
Log victim density × GLF		0.066***		0.059***
		(0.024)		(0.020)
Covariates × GLF	Yes	Yes	Yes	Yes
Observations	3,095	3,095	4,424	4,424
Adjusted R^2	0.47	0.47	0.47	0.47
County FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes

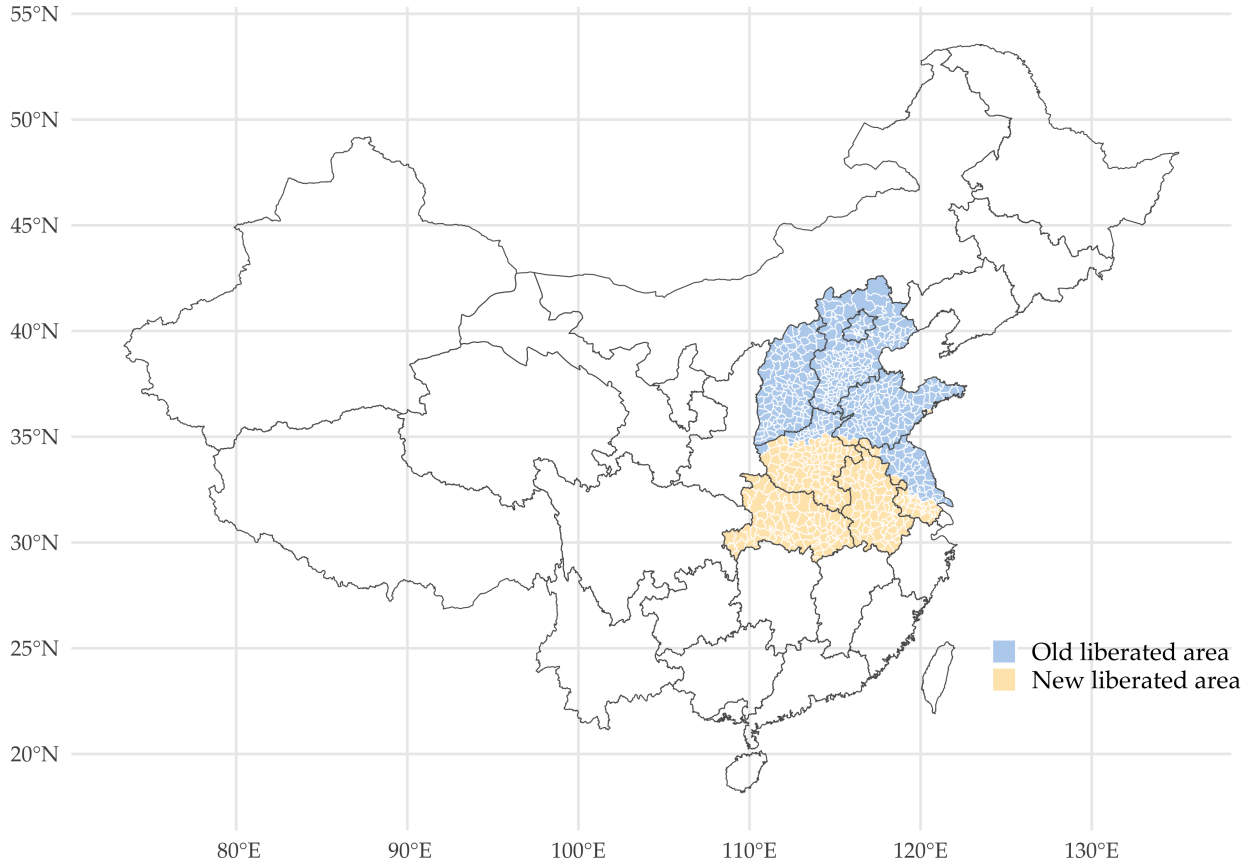
Notes: Regressions based on a subsample of counties where party secretary turnover occurred during 1958-1961. All baseline covariates are included in the regressions. Standard errors clustered at the county level in parentheses. ***p < 0.01; **p < 0.05; *p < 0.1.

A9 Sensitivity to Omitted Variables



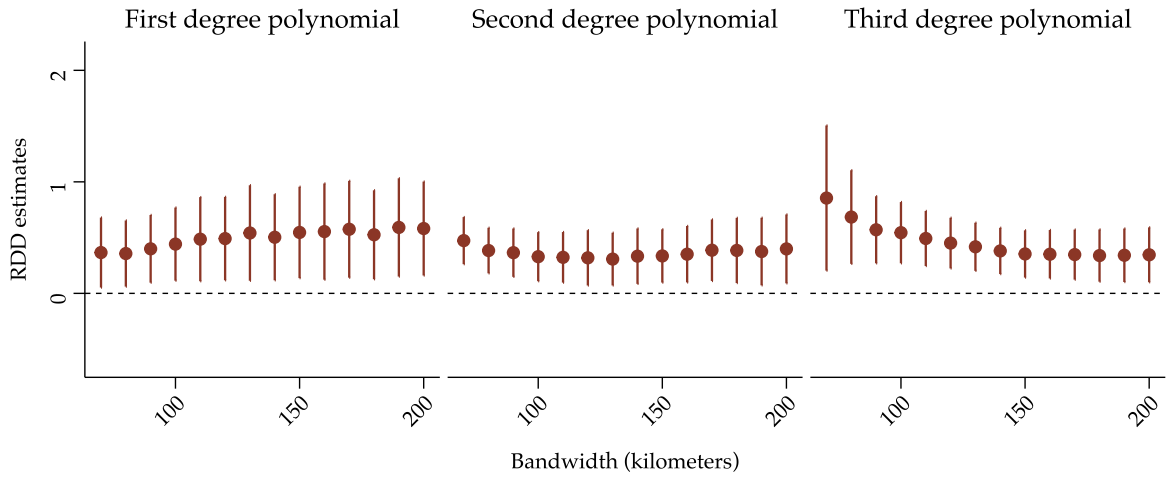
Notes: Sensitivity analysis for effect of purges on excess mortality using clan strength as a benchmark. The sensitivity analysis is performed on a cross-sectional regression where the outcome variable measures the change in the average mortality rate from 1954-1957 to 1958-1961. It is predicted by log purge victims and all baseline covariates. Coefficients of purges are shown in parentheses, which are estimated under different assumptions for the influence of omitted variables. The black triangle represents the coefficient of purges without omitted variable. The red diamond represents the coefficient of purges when omitted variables are six times as influential as log genealogies.

A10 Geographical Area of the Spatial RDD



Notes: The spatial RDD is restricted to counties within seven provinces: Shanxi, Hebei, Shandong, Hubei, Henan, Anhui, and Jiangsu. The map is based on administrative boundaries in 1964.

A11 Robustness of the RDD Estimates



Notes: The effects of purges on famine mortality estimated with fuzzy RDD using different functions of distance and bandwidth values. The outcome variable is county mortality rate in 1960. The independent variable is log purge victims. All estimations control for polynomials of distance to the military border, province fixed effects, and fixed effects for longitude and latitude intervals. The regressions are weighted by the inverse of distance to the border. Coefficients with 95% confidence intervals based on standard errors clustered within 1×1 degree grid cells.

A12 RDD Falsification Tests

Variable	Coefficient	Std. Error	p-value	Bandwidth	N
Political characteristics					
Log CCP density	-6.65	21.46	0.76	157	240
Log KMT density	0.49	0.38	0.19	109	182
Central elites	-18.36	56.72	0.75	139	227
Socioeconomic characteristics					
Death rate, 1957	0.46	0.52	0.37	174	261
Population, 1957	1.73	0.79	0.03	100	167
Grain productivity	17.25	25.27	0.49	156	229
Middle schools	0.19	0.11	0.10	112	183
Log genealogies	0.31	0.16	0.06	112	182
Distance to railroads	0.09	0.18	0.60	135	212
Geographical characteristics					
Rice suitability	0.21	0.30	0.49	163	247
Terrain ruggedness	0.36	0.24	0.13	133	215
Precipitation	6.98	23.67	0.77	119	192
Temperature	-4.13	4.12	0.32	123	200

Notes: Effects of county characteristics on famine mortality estimated with fuzzy RDD. The outcome variable is log mortality rate in 1960. Independent variables are shown in the first column. Their coefficients are estimated independently. The estimation controls for linear distance to the border, province fixed effects, and fixed effects for longitude and latitude intervals. Regressions are weighted by the inverse of distance to the border. Data samples are selected using the optimal bandwidth following Calonico, Cattaneo and Titiunik (2014). Standard errors are clustered within 1×1 degree grid cells.

A13 Estimates with Different Sample Periods

	Log mortality rate		
Sample periods	1954:1961	1958:1965	1954:1965
	(1)	(2)	(3)
Log purge victims × GLF	0.054***	0.099***	0.078***
	(0.016)	(0.014)	(0.014)
Covariates × GLF	Yes	Yes	Yes
Observations	8,765	9,181	13,520
Adjusted R^2	0.45	0.53	0.45
County FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes

Notes: Standard errors clustered at the county level in parentheses. ***p < 0.01; **p < 0.05; *p < 0.1.

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