The Short- and Long-Run Effects of Affirmative Action: Evidence from Imperial China*

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Abstract

We study the short- and long-run effects of an affirmative-action reform in imperial China. In 1712, province-specific quotas tied final-stage civil-service examination passes to each province's candidate pool. Using newly assembled prefecture-by-cohort panels and individual examination and career records, we show that greater reform intensity increased successful candidates without weakening their subsequent careers. Gains were uneven within provinces. After the examination system was abolished in 1905, the relative advantage of provinces that had gained more weakened, though some gains persisted. Reform intensity also remained positively associated with secondary and college attainment nearly three centuries later.

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I Introduction

Affirmative action policies are designed to improve the position of groups that have been underrepresented or disadvantaged.[1] Such policies generally grant some degree of preferential treatment in the allocation of scarce opportunities, including jobs, university places, and government contracts. Programs based on race, gender, ethnicity, class, place of origin, or high school type have been implemented by institutions and governments around the world (Moses and Jenkins, 2014). Their effects on equity and efficiency remain heavily debated.[2]

This paper studies a case of affirmative action in an unlikely historical setting: imperial China. At first glance, one may not associate the Qing bureaucracy with policies designed to promote inclusion or redistribution. Yet in 1712, the Qing state enacted a reform to its examination system that shares key features with modern affirmative action: it revised the rules of final-stage selection to shift admissions toward provinces that had historically produced fewer successful candidates (Elman, 2000, 2013). In effect, the reform granted preferential treatment to candidates from underrepresented provinces in a highly selective process governing access to advancement.

The motivations behind the reform were not ideological in the modern sense, but likely reflected a combination of administrative pragmatism, concerns about regional balance, and broader efforts to maintain legitimacy across a vast and diverse empire. Regardless of the state's intent, the policy changed the opportunity structure in ways that resemble modern affirmative action, particularly place-based systems such as those in France or Australia (Sabbagh, 2002; Farnhill and Thomas, 2017). By raising the probability of final-stage success for candidates from disadvantaged provinces while keeping earlier stages of selection unchanged, the reform altered the expected returns to educational investment and the prospects for upward mobility. These features make the setting valuable for studying how redistributive selection policies affect human capital accumulation and social mobility.

The imperial examination system was a long-standing and highly prestigious means of selecting candidates for scarce, high-status bureaucratic positions. Its appeal, however, extended well beyond government service. Success conferred cultural distinction and elite social standing, and the examination system shaped educational aspirations across much of the male population. Candidates who never entered the bureaucracy often became teachers, clerks, or community elites.

[1] Although typically associated with liberal democracies, affirmative action policies are not exclusive to them. Both the Soviet Union and contemporary China have implemented ethnicity-based affirmative action systems in education and public employment. These cases show that redistributive selection policies can also emerge under autocratic regimes as tools of integration, legitimacy, or control. See Martin et al. (2001) on the Soviet Union and and (1998) on minority preferences in socialist China.

[2] In the United States, the modern federal affirmative action framework emerged during the 1960s, including through Lyndon Johnson's 1965 executive order, following his Howard University commencement address earlier that year. See Arcidiacono and Lovenheim (2016) and Holzer and Neumark (2000) for reviews of the empirical literature.

In the absence of mass schooling or modern credentialing institutions, the examinations became a national institution of aspiration.[3] The system was valued for both its material rewards and its symbolic and cultural significance.

Success rates nevertheless differed sharply across provinces.[4] These differences reflected linguistic barriers, unequal educational resources, and the costs and difficulties of traveling to examination sites (Elman, 1991).[5] In 1712, the Qing state introduced a new policy that increased the probability of final-stage success for candidates from underrepresented provinces. The reform remained in force until the imperial examination system ended in 1905.

The key innovation of the 1712 reform was to introduce province-specific quotas at the metropolitan examination, the final and most prestigious stage of the three-tiered system. Before 1712, final-stage competition and admissions were organized within three broad examination regions: North, South, and Central. The reform replaced this regional allocation with quotas assigned to individual provinces according to the number of candidates they sent to the metropolitan examination. It thereby brought acceptance rates closer together across provinces, conditional on reaching the final stage. The policy effectively lowered the admission threshold for candidates from provinces that had historically produced relatively few successful candidates and raised it for those from provinces that had previously accounted for larger shares.

The 1712 reform provides an opportunity to study both the short- and long-run consequences of affirmative action. We begin by examining its contemporaneous effects on the production and subsequent careers of successful candidates, using a new dataset covering 16,073 individuals who obtained the *jinshi* degree between 1650 and 1840. Combining variation in reform intensity with the timing of the policy, we estimate its effects in a continuous difference-in-differences framework. We then examine how the gains and losses associated with the reform were distributed across prefectures within provinces. Finally, we study whether its effects remained visible in educational and occupational outcomes after the province-specific rule ended.

To measure provincial exposure to the reform, we construct a reform-intensity measure based on the change in a province's share of successful *jinshi* within its historical examination region.[6] We compare each province's share in a pre-reform window with its share in the early post-reform period. Because examination-region boundaries did not perfectly coincide with provincial bor-

[3]Modern evidence from China's *gaokao* similarly shows how a standardized, high-stakes examination can operate as a centralized tournament that shapes household investment in preparation and perceived access to mobility (Jia, Li, and Cousineau, 2025).

[4]Candidates from some regions, particularly in the North, historically performed less well. The Qing system inherited an earlier effort to address regional imbalance through the broad regional quotas introduced in 1454.

[5]In models of uneven tournaments, affirmative action can offset differences across groups in the cost of effort (Schotter and Weigelt, 1992).

[6]As an alternative measure of ex ante reform exposure, we use the pre-1712 gap between a province's share of *juren* and its share of *jinshi* within the same examination region. A larger gap indicates that the province accounted for a relatively large share of candidates eligible for the final stage but a smaller share of final-stage successes. The main findings are similar using this alternative measure.

ders, we harmonize the units over time by constructing time-invariant "artificial provinces" whose boundaries lie entirely within a single examination region.[7] Reform intensity ranges from approximately -0.15 to 0.10. Provinces with lower initial shares generally experienced larger increases, while those with higher initial shares experienced smaller increases or declines, consistent with the reform shifting final-stage admissions toward previously underrepresented provinces.

The reform expanded access to the *jinshi* degree by changing the allocation of successful candidates across provinces. A natural concern is that this expansion may have weakened the average preparedness or subsequent performance of those admitted. The reform lowered the final-stage admission threshold for candidates from provinces that gained more and raised it for candidates from provinces that lost, potentially allowing candidates who would previously have fallen below the threshold to succeed.

We find no evidence of weaker post-admission outcomes among successful candidates from provinces with greater reform intensity. They received higher-ranked first assignments and attained higher peak and career-average ranks. The additional results on career duration, posting characteristics, conflict, and disaster relief likewise show no systematic deterioration. The reform therefore expanded access without reducing the subsequent performance of those selected.

The gains were unevenly distributed within provinces. Prefectures with greater pre-existing human capital, measured by *jinshi* per capita before 1650, experienced more favorable post-reform changes. Within provinces that gained, historically stronger prefectures benefited more. Within provinces that lost, they experienced smaller declines than prefectures with weaker educational foundations. Consistent with these results, the reform reduced inequality between provinces while reinforcing the importance of pre-existing human capital in determining outcomes within them.

Finally, we extend the analysis beyond 1905 to examine whether the effects of the reform persisted after the province-specific rule ended. During the following four decades, prefectures in provinces with greater reform intensity experienced less favorable relative changes in several measures of educational and professional attainment, indicating a partial reversal of the earlier advantage. Yet the effects did not disappear entirely. Reform intensity remained positively associated with secondary and college education in 1982 and with occupational prestige among non-agricultural workers. The evidence therefore points to both a weakening of the relative advantage after the policy ended and longer-lasting educational and occupational effects.

Related Literature Our study contributes first to the literature on the effects of affirmative action. Theoretical work emphasizes that preferential selection can change educational investment by altering expected returns. Lundberg and Startz (1983) shows that equal-opportunity policies can increase human-capital investment, while Schotter and Weigelt (1992) finds experimentally

[7] From 1454 until 1712, metropolitan-stage competition and quota administration were organized within three examination regions: North, South, and Central. These regional boundaries did not perfectly coincide with provincial borders; after 1712, final-stage quotas were assigned at the provincial level.

that affirmative action reduces dropout among disadvantaged participants. Other models highlight role-model effects (Chung, 2000; Fang and Moro, 2011), stereotype formation (Coate and Loury, 1993), and the possibility of mismatch when beneficiaries enter more demanding institutions (Sander, 2004; Arcidiacono et al., 2011; Arcidiacono and Lovenheim, 2016).

The 1712 reform speaks particularly closely to work on affirmative action applied at the final stage of a multi-stage selection process. Such policies alter final admission while leaving earlier investments and screening stages in place. This structure may preserve incentives to prepare while changing the probability of ultimate success (Coate and Loury, 1993; Chan and Eyster, 2003; Schotter and Weigelt, 1992). The Qing reform followed this design: it changed the allocation of metropolitan-examination passes across provinces without changing access to education, examination content, or the lower-tier selection stages. The setting therefore allows us to study how final-stage preferential treatment affects preparation, selection, and outcomes after admission.

Empirically, our analysis relates to studies that use policy changes or admissions rules to estimate the effects of affirmative action on intended beneficiaries. Bleemer (2022) shows that the end of affirmative action in California shifted underrepresented minority students toward lower-quality colleges. Kapor (2024) finds that Texas's Top Ten Percent policy increased enrollment from high-poverty schools and that students admitted under the policy earned higher college grades. In India, Bagde, Epple, and Taylor (2016) finds that caste-based preferences increased college attendance without evidence of mismatch. Bertrand, Hanna, and Mullainathan (2010) documents positive returns to admission for targeted applicants but finds that the gains were concentrated among lower-caste applicants from stronger socioeconomic backgrounds. Our results similarly examine outcomes after selection, but over a much longer period and within a setting in which earlier stages of preparation remained unchanged. Successful candidates from provinces with greater reform intensity did not experience weaker subsequent careers, while the gains within provinces were concentrated in prefectures with stronger pre-existing human capital.

This within-province evidence also contributes to the smaller literature on how affirmative action distributes benefits within targeted groups (Bertrand, Hanna, and Mullainathan, 2010; Akhtari, Bau, and Laliberté, 2024). Policies defined at the group level may disproportionately benefit individuals or localities with greater resources, preparation, or capacity to respond. We find this pattern on both sides of the reform-induced reallocation. Historically stronger prefectures gained more within provinces that benefited and were better insulated within provinces that lost. The policy reduced inequality between provinces while leaving substantial inequality within them.

Our analysis also addresses the durability of affirmative action. Existing work asks whether preferential policies generate effects that survive their withdrawal or whether relative gains weaken

once the policy ends.[8] The duration of the Qing policy allows us to examine both margins. During the four decades after the province-specific rule ended in 1905, higher-intensity provinces experienced less favorable relative changes in several educational and professional outcomes. Yet reform intensity remained positively associated with secondary and college education in 1982 and with occupational prestige among non-agricultural workers. The evidence thus combines a partial reversal in the period immediately following withdrawal with effects that remained visible over a much longer horizon.

Because successful candidates generally entered government service, the paper also relates to research on the selection of public officials. Institutions governing access to public office affect who reaches positions of authority (Besley, 2005), while recruitment and patronage can shape bureaucratic advancement and administrative performance (Xu, 2018). We follow successful candidates from admission into their later careers and examine whether the reform changed initial appointments, promotions, career-average rank, posting characteristics, and outcomes at the places where they served. This allows us to study whether redistributing access to a state credential changed both entry into public office and subsequent performance.

The policy was assigned by province of registration, making it comparable to modern place- or school-based affirmative action. Examples include Sciences Po's Equal Opportunity Program and regional preferences in Australia and Sri Lanka (Sabbagh, 2002; Farnhill and Thomas, 2017). A concern with such policies is that individuals may change schools or locations to obtain preferential treatment (Cullen, Long, and Reback, 2013). This margin was sharply constrained in the Qing setting. Examination eligibility was tied to ancestral registration, and changing one's legally recognized province of origin was difficult. False registration did occur, especially later in the Qing, but it was punishable and appears to have been uncommon during most of our period.[9] The limited scope for strategic relocation makes it possible to study a place-based policy with relatively little treatment arbitrage.

The paper further contributes to research on the persistent effects of historical institutions and interventions. A large literature shows that historical policies can leave long-run effects on development, state capacity, and human-capital accumulation (Acemoglu, Johnson, and Robinson, 2001; Dell, 2010; Acemoglu, Reed, and Robinson, 2014; Dell, Lane, and Querubin, 2017; Dell and Olken, 2020; Lowes and Montero, 2021). More closely related to state organization, Xu (2019) shows that bureaucratic appointment practices can shape the subsequent development of fiscal capacity. We study a different margin: how a long-lasting change in the rules governing access to a selective state credential affected immediate selection, within-group inequality, and

[8]See Holzer and Neumark (2006) and Arcidiacono, Lovenheim, and Zhu (2015) for discussions of the equity and efficiency consequences of affirmative action.

[9]This practice, known as *maoji*, became more visible during the nineteenth century, including in Guangdong and Guangxi. Appendix C.1.1 provides further discussion. For the institutional rules governing residence and examination eligibility, see Von Glahn (2012).

educational outcomes after the policy ended. The within-province results also relate to work on cumulative advantage and the persistence of inequality, in which locations with stronger initial endowments are better able to respond to new opportunities (Durlauf, 1996; Piketty, 2014).

Finally, we contribute to the historical and quantitative literature on China's examination system (Ho, 1962; Campbell and Lee, 2008; Elman, 2013; Bai and Jia, 2016; Shiue, 2017; Yuchtman, 2017; Chen, Kung, and Ma, 2020; Shiue, 2025; Wen, Wang, and Hout, 2024). Historians have long debated how much mobility the system permitted. Ho (1962) emphasizes substantial circulation into elite status, whereas Elman (1991) and Elman (2013) stress its limitations. Recent quantitative work studies the examination system's political and economic effects. Bai and Jia (2016) argues that its abolition displaced prospective elites and increased revolutionary activity where examination opportunities had previously been greater. Yuchtman (2017) examines how classical education affected modernization, while Chen, Kung, and Ma (2020) links historical examination participation to modern human-capital investment. Shiue (2025) studies how changes in the return to examination success affected intergenerational mobility. We add evidence on the 1712 reform, a major intervention in the allocation of final-stage success, and trace its effects from examination outcomes and bureaucratic careers to inequality within provinces and much later educational attainment.

To our knowledge, this is the first study of an affirmative action policy operating in a premodern setting over nearly two centuries. The reform remained in place for 193 years, from 1712 to 1905, and our main data cover all identified candidates who obtained the metropolitan degree between 1650 and 1840. The setting also has several empirical advantages. Selection proceeded through centralized, rule-based written examinations, and the reform altered final-stage allocation without accompanying changes in school access, curriculum, or direct financial support.[10] Linked administrative records then allow us to follow admitted candidates through their initial assignments and later careers. Successful candidates also entered a common bureaucratic career structure rather than dispersing across different employers and labor markets. This limits the scope for differential access to employment after selection and makes subsequent career outcomes more directly comparable across candidates. These features help separate the consequences of preferential final-stage selection from simultaneous changes in educational provision, admissions criteria, or post-selection access to employment.

The remainder of the paper is organized as follows. Section II provides the historical background. Section III describes the data and variable construction. Section IV explains the reform-intensity measure. Section V examines successful candidates' examination and career outcomes.

[10]The 1712 rule set provincial passes in relation to the number of candidates appearing at the metropolitan examination and did not introduce ad hoc province-specific evaluation (Kracke, 1957; Ho, 1962; Elman, 2013). We find no evidence that the proportional rule extended to appointments or promotions, which continued to follow Board of Personnel regulations and subsequent evaluations (Ch'ü, 1962).

Section VI studies the distribution of gains and losses within provinces. Section VII examines the policy reversal in 1905, and Section VIII studies persistence in educational and occupational outcomes. Section IX discusses mechanisms and interpretation. Section X concludes.

II Historical Background

In 1712, a major reform was introduced to China's long-standing imperial examination system, a central component of the country's political and social institutions since the Song dynasty (960–1279). The reform equalized acceptance rates across provinces at the final stage of the examinations. Under the new policy, candidates from provinces that had historically produced fewer successful candidates no longer competed directly with those from more dominant provinces. Instead, the number of passes assigned to each province was set in proportion to the size of its candidate pool. Success in the lower-tier examinations, which remained subject to separate quotas, was still required to reach the final stage.

The imperial examination system, first established in 587 and institutionalized during the Song, followed a three-tier structure: the Prefectural Examination (*yuankao*), the Provincial Examination (*xiangshi*), and the Metropolitan Examination (*huishi*). Degrees were awarded at each stage—*shengyuan*, *juren*, and *jinshi*—and holders of the *jinshi* degree were eligible for high-ranking bureaucratic appointments. Although the *jinshi* was the most prestigious credential, particularly for elite appointments, it was not the only route into official service. During the eighteenth and especially the nineteenth century, a growing share of officials entered through purchased degrees (*juanna*), which the Qing state used to raise revenue.[11] Our analysis focuses on examination-qualified *jinshi*, whose appointments were centrally administered and highly competitive. The provincial and metropolitan examinations generally followed a three-year cycle, as summarized in Figure A.8.[12]

The examinations emphasized disciplined written structure and close textual analysis. In many sittings, examiners also set *cewen* policy questions requiring candidates to develop applied arguments. These tasks cultivated and screened for structured reasoning and concise written communication (Ho, 1962; Miyazaki, 1976; Elman, 2000, 2013). Evidence from modern settings shows that general cognitive and communication skills have broad value across occupations (Autor, Levy, and Murnane, 2003; Heckman, Stixrud, and Urzua, 2006; Deming, 2017).

At the metropolitan stage, the total number of *jinshi* admitted varied across sittings and was finalized after grading and imperial approval; it was not fixed or publicly announced in advance (Elman, 2000, 2013). By contrast, admission at the lower tiers followed established quota rules,

[11] See Zhang (2022) for an account of purchased degrees in Qing China. For a quantitative breakdown of entry routes into the bureaucracy, see Chen et al. (2020).

[12] Appendix C.3 and C.3.3 provide detailed discussions of career trajectories for successful and unsuccessful candidates.

with county- or prefecture-level quotas for *shengyuan* and provincial quotas for *juren* (Ho, 1962). Decadal fluctuations in the total number of *jinshi* therefore reflect variation in the number of sittings and court decisions rather than a constant admissions target.[13]

The centrality of the examination system reflected the broader organization of elite status in imperial China. The hereditary aristocracy that had dominated earlier periods largely disappeared after the Tang–Song transition, and subsequent elites depended more heavily on education, officeholding, land, and family networks. These advantages could persist across generations, but they were also repeatedly disrupted by dynastic collapse, warfare, and institutional change. By the Qing period, there was no broad titled Han nobility with guaranteed access to bureaucratic office. Although wealth, family networks, and education remained important sources of advantage, elite status no longer conferred an automatic right to official appointment.[14] The examination system consequently became the principal formal route to bureaucratic advancement and an important channel of upward mobility. Examination success—and particularly attainment of the *jinshi* degree—therefore became a central axis of status competition, shaping both inequality and the renewal of elites.[15]

This examination-based system encouraged literacy and educational effort in a premodern setting without mass public schooling. Instruction typically came from families, lineage groups, private or community-run schools, and local academies for more advanced students. Successful candidates gained prestige and access to official appointments (Ho, 1962), while many unsuccessful aspirants became village teachers or tutors, generating local spillovers in literacy and basic education. The result was a class of literate men far larger than the bureaucracy itself, sustaining norms of learning and preparation well beyond those who ultimately entered official service.[16]

Eligibility for the examination system was broad by the standards of the period. In principle, most adult men could participate, and there were no formal wealth or income requirements.[17] By the late nineteenth century, approximately three million individuals—about one in fifty adult men—sat for the lowest-level examinations each year (Elman, 2013).

Over time, the system became deeply embedded in popular culture, giving rise to a wide range

[13]In the empirical analysis, decade fixed effects absorb empire-wide fluctuations in the total number of passes.

[14]The one important exception is the Manchu and allied Mongol banner aristocracy. Banner titles were hereditary and, until the late empire, bannermen enjoyed their own examination quotas and privileged access to certain offices; see Elliott (2001, ch. 5) and Xi (2018) and Chen et al. (2023) for details.

[15]See Ma (2012) on elite formation and redistribution across dynasties; Milanovic, Lindert, and Williamson (2011) for comparative inequality trajectories; and Ho (1962) on social mobility through the examination system.

[16]The tournament structure also had important implications for motivation and educational investment, as emphasized in economic models of rank-based competition (e.g., Lazear and Rosen, 1981). Literacy rates varied substantially by region. Evelyn Rawski estimates that national male literacy in the nineteenth century was roughly 20–30%, while reaching 40–50% in parts of the Yangtze Delta (Rawski, 1979). Elman (2000) similarly notes that in some Jiangnan counties, more than half of the adult male population had acquired basic classical literacy.

[17]Some hereditary status groups were excluded. The Tanka people, for example, were barred for much of imperial history and continued to face substantial obstacles even after being formally permitted to participate in the eighteenth century.

of rituals, superstitions, and symbols. Candidates and their families sought divine assistance through temple offerings, divination, and talismans promising success, while examination halls became associated with stories of fortune, failure, and anxiety. Civil-service lotteries, for instance, emerged in nineteenth-century Guangdong, where participants placed bets on the surnames of recent examination passers (Li, 2023). These practices show that the examinations extended far beyond bureaucratic selection: they organized aspirations, inspired popular beliefs, and occupied a central place in the social and cultural life of late imperial China.[18]

Although often idealized for its meritocratic design, the imperial examination system also served important political functions. By selecting officials through a formalized, competitive process, the state bolstered its legitimacy and projected an image of fairness and competence.[19] The examinations incorporated rules intended to promote fairness, including standardized texts, anonymized grading, and formal penalties for cheating. Corruption and favoritism nevertheless persisted, especially at lower levels and in later dynasties.[20]

Despite these formal safeguards, candidates' chances of examination success remained deeply unequal across provinces. Regional differences in examination performance reflected a combination of structural, cultural, and geographic disadvantages, including linguistic barriers and unequal access to education.[21] Candidates from remote provinces often had less exposure to examination culture, while the cost and difficulty of travel limited their ability to seek opportunities elsewhere. Restrictions on mobility further reinforced the disadvantages faced by frontier regions.[22]

These disparities became increasingly visible after 1650 as the Qing consolidated control over a territorially expansive empire. At the same time, nearly half of official posts were reserved for Manchu elites, further limiting the offices available through the examination system (Xi, 2018). Guizhou, which had a large non-Han population and a relatively weak examination tradition, produced only nine successful candidates during the sixty years from 1650 to 1710 and repeatedly recorded examination years with no *jinshi* at all (Table A.17).

To address these imbalances, Emperor Kangxi issued a reform decree in 1712. In his edict, he proclaimed (*Veritable Records of the Kangxi Emperor,* 1985):

> Across the empire, increasing numbers of scholars, including many of modest means, undertake the long journey to the capital to sit for the examinations. Yet however

[18] Appendix C.2.6 provides further examples and illustrations of examination-related rituals and beliefs.

[19] Bol (2008) describes the examination system as foundational to a "scholar-official government," with the emperor playing a central but constrained role within a broader institutional structure.

[20] See Appendix C.2.2 for a discussion of enforcement practices and known limitations.

[21] The curriculum was based on Confucian classics written in literary Chinese, disadvantaging candidates with different mother tongues. See Appendix C.2.3.

[22] See Appendix C.1.1. Related literature on place-based educational inequality includes Ludwig et al. (2013), Chetty and Hendren (2018a), and Chetty and Hendren (2018b).

> diligently they prepare, only a few succeed. This is deeply troubling. Henceforth, due consideration shall be given to each candidate's province of origin. Once the examinees have assembled, the number of successful candidates from each province shall be determined in proportion to the number it presents, so that talented and worthy men are not unjustly excluded.

Kangxi issued the decree following the 1712 metropolitan examination. Officials then translated it into a new allocation rule. In an unusual move, another metropolitan examination was held in 1713, only one year after the previous sitting, and applied the new system of province-specific selection.[23]

The reform targeted the final, metropolitan stage of the examinations. Debates over regional balance at this stage dated back at least to 1066, when Sima Guang supported regional quotas and Ouyang Xiu defended selection based solely on examination performance (Ji, 2005). The Ming later introduced a regional allocation of metropolitan passes: in 1425, 60 percent were assigned to the South and 40 percent to the North; in 1454, this was revised to shares of 55, 35, and 10 percent for the South, North, and Central regions (Kracke, 1957). These broad regional allocations nevertheless left substantial differences in final-stage success across provinces.

The 1712 reform replaced these region-level shares with province-specific pass allocations proportional to each province's pool of examinees, while the lower-tier examinations remained governed by a separate quota system. The allocation rule was formula-based rather than discretionary (Kracke, 1957; Ho, 1962; Elman, 2013). Conditional on the number of examinees from each province and the empire-wide number of passes approved for a sitting, provincial pass shares were set in proportion to examinee shares rather than through ad hoc adjustments. A candidate's probability of success therefore came to depend primarily on relative performance within his province rather than on the strength of the candidate pool across the broader examination region.

Figure A.1 shows how provincial *jinshi* per capita, relative to the empire-wide average, evolved before and after 1712 for provinces with high and low pre-reform within-region *jinshi*-to-*juren* ratios. We calculate each province's pre-1712 share of *jinshi* within its examination region and divide it by its share of *juren* within the same region, then split provinces at the median of this ratio.[24] The figure shows a broad divergence after the reform: several below-median provinces rose relative to the empire-wide average, while most above-median provinces declined.

[23] Metropolitan examinations were typically held every three years; examinations in consecutive years were highly unusual.

[24] A low ratio indicates that a province's share of final-stage *jinshi* lagged behind its share of lower-tier *juren* within the same examination region before 1712. Provinces with zero pre-1712 *juren* share are omitted because the ratio is undefined.

III Data

This section describes the primary data sources and variables underpinning our analysis. Table A.1 presents the summary statistics of the core variables. Additional variables and their construction details are provided in Appendix B.1.

Our sample covers sixteen provinces and 267 prefectures that can be consistently identified across the pre- and post-reform periods. For comparability with the pre-1712 regime, we define provinces so that each falls entirely within one historical exam region (South, North, or Central). Details appear in Section IV.

III.A Successful Candidates in Imperial Exams

Our analysis of successful candidates begins with a comprehensive roster of *jinshi* degree holders between 1371 and 1840, drawn primarily from Zhu and Xie (1980). This backbone source includes over 51,000 individuals—the most complete roster available for the Ming and Qing dynasties[25]—and provides each candidate's exam year, birthplace, and final exam rank. We then link this roster to additional sources to recover career outcomes, candidate characteristics, and family-background measures.

From these records, we construct four key measures:

1. **Jinshi per capita.** For each province (or prefecture) in each ten-year period, we compute *jinshi* per 10,000 residents. The population denominator uses Shuji Cao's reconstructed 1776 *prefecture-level* totals, aggregated to our synthetic provinces. We use 1776 because reliable population figures exist for only a few benchmark years, and 1776 is chronologically central to the panel we analyze (Ge and Cao, 2001).

2. **Exam Rank and Exam Class.** Each candidate's academic performance is measured using his final ordinal placement in the Palace Examination. After passing the Metropolitan Examination, successful candidates proceeded to the Palace Examination, where they were ranked by senior officials and the emperor. This rank determined each candidate's position on the official *jinshi* list and informed subsequent appointments.

   The full ranking was organized into three broad classes, traditionally known as the *san jia*. First Class was the highest and comprised the top three candidates. The remaining candidates were ranked within Second Class and Third Class, with Third Class the lowest. We code these categories as Exam Classes 1, 2, and 3, respectively. Exam Class therefore groups the detailed ordinal ranking into three broader categories.

   We construct a standardized version of the exact ordinal rank:

[25] Zhu and Xie (1980) documents 51,000+ *jinshi* from 201 exam cohorts spanning 1371–904. We end in 1840 for reasons discussed in Section V.

$$\text{Standardized Rank} = 1 - \frac{\text{Raw Rank}}{\text{Total Number of Passes}},$$

so that higher values indicate a higher position among all successful candidates in that examination.

3. **Bureaucratic Rank.** We use two sources to construct measures of bureaucratic rank. The first is Gong (2019), which contains biographical entries for a large number of *jinshi*, including mentions of bureaucratic appointments for 4,314 individuals (approximately 27% of all *jinshi*). To supplement this, we incorporate information from the China Government Employee Database-Qing (CGED-Q) (Ren et al., 2016; Campbell et al., 2019; Lee-Campbell Group), which is organized around official posts. The database records, post by post and quarter by quarter, the individuals who held a given office based on archival appointment rosters. While CGED-Q is still under development, three periods of data have been released: 1760–1798, 1850–1864, and 1900–1912. From these, we identify 527 additional *jinshi* who assumed office during one of these periods. When individuals appear in both sources, we prioritize Gong, as the CGED-Q snapshots—due to the short coverage windows—are unlikely to capture an official's entire career.[26]

   Official posts were organized into nine principal ranks, each divided into two sub-levels, producing 18 total tiers. We assign a numeric scale from 0.5 (lowest) to 9 (highest). For instance, county-level officials typically occupy tiers around 2–3, while ministerial posts exceed 8. As above, we average bureaucratic rank by decade and region, assigning a value of zero to locations with no recorded officials.

4. **Candidate Characteristics and Family Background.** For candidate-level attributes around exam success, we combine several sources. Age at exam success and recorded relatives are drawn primarily from the China Government Employee Database–Qing Extended Records (CGED-Q ER), which transcribes *tongnian chilu* and related examination rosters for degree holders and their kin; CBDB (Harvard University, Academia Sinica, and Peking University, 2024) contributes additional biographical matches and checks. For a linked subset of successful candidates, we also use FamilySearch genealogies to construct a lineage-based family measure: the number of same-lineage relatives known to be alive at the exam year. Appendix B.4 describes the construction of this measure and reports the results.

[26] While Gong (2019) includes many *jinshi*, only a subset contain references to bureaucratic appointments. CGED-Q supplements this by linking individuals to specific posts based on archival records, enabling us to identify additional officeholders not listed in Gong's database. However, because each CGED-Q release covers only about a decade or two, it likely reflects only a portion of an individual's full career trajectory.

III.B Educational and Professional Elites in Republican China

We assemble several measures of educational and professional attainment in Republican China, covering approximately 1912–1949. The records identify university graduates, overseas returnees, and prominent figures across a range of professions. We geocode individuals by place of origin and aggregate the records to the prefecture level. The main sources are:

1. **Notable Figures.** We use the biographical compendium compiled by Xu (2007), which records more than 17,000 individuals active during the Republican era. The volume defines its coverage period as extending from 1905, the year in which the Tongmenghui was founded, through September 30, 1949, while allowing individual biographies to extend earlier or later when needed to describe life histories. It applies explicit inclusion criteria across a broad range of fields, including senior central and provincial officials and national representatives; high-ranking military officers, such as army division commanders or above; prominent entrepreneurs and financial-sector leaders; leading scientists, engineers, physicians, and nurses; scholars, university professors, researchers, and school principals; journalists, publishers, lawyers, athletes, coaches, religious leaders, and other widely recognized public figures. The compendium draws on government gazettes, archival materials, published biographies, local gazetteers, newspapers and periodicals, and provincial and county investigations or solicited submissions. For each individual, it reports a place of origin, which we geocode to the prefecture level and use to construct prefecture-level measures of notable figures.

2. **University Graduates and Overseas Returnees.** Official alumni directories from Peking University and Tsinghua University identify more than 16,000 graduates. The records report each graduate's home prefecture and provide birth or enrollment years that allow us to assign individuals to cohorts covering approximately 1910–1950. We aggregate these records to construct prefecture-by-cohort measures of Peking University and Tsinghua University graduates. Separately, we use data on Chinese students who studied in Japan and subsequently returned to China. Many qualified for overseas study through competitive screening examinations before departure, so this series disproportionately captures individuals with strong academic preparation.

III.C 1982 Population Census

To measure educational and occupational outcomes in the late twentieth century, we use the 1% public-use micro-sample of the 1982 Chinese census (King et al., 2017). We assign each respondent's location to 1911 prefectural boundaries and calculate, for each prefecture, the shares of individuals who completed primary, secondary, and college education. We also map respondents' occupations into two standardized measures of occupational standing: the International Socio-Economic Index (ISEI) and the Standard International Occupational Prestige Scale (SIOPS).

These variables provide prefecture-level measures of educational attainment and occupational status nearly three centuries after the reform.

IV Constructing Reform Exposure

We measure a province's exposure to the 1712 reform using the change in its share of *jinshi* degree holders within its historical examination region. Before the reform, provinces competed within three regional pools: South, North, and Central. We therefore calculate each province's share within its region, both before and after the reform, and define

$$\Delta Share^{1712} \;=\; Share^{1710\text{–}1730} \;-\; Share^{1680\text{–}1700}, \tag{1}$$

Here, $Share^{1680\text{–}1700}$ is the province's share of all *jinshi* degree holders from its examination region in the pre-reform window, and $Share^{1710\text{–}1730}$ is the corresponding share in the early post-reform window. In the code, the post-reform window excludes the 1712 cohort from the 1710s decade total. A positive value indicates that the province gained representation relative to other provinces in the same historical examination region, while a negative value indicates a relative loss. We refer to this measure as $ReformIntensity_{prov}$ throughout the paper.

We use the early post-reform period because changes in provincial representation during these years are likely to reflect primarily the immediate reallocation produced by province-specific quotas. Educational investments that affected success in the metropolitan examination generally began long before candidates reached the final stage. Preparation for the lowest-level examinations commonly began in early childhood, at approximately age five (Elman, 2000), while the average successful *jinshi* in our data was 33 years old. The full interval between beginning preparation and obtaining the degree was therefore approximately 28 years for the average successful candidate.

This calculation does not rule out all behavioral responses within the 1710–1730 post-reform window. Older children and candidates already on the examination track could have adjusted their preparation, persistence, or participation after the reform, and the youngest affected cohorts could begin to appear near the end of the window. It does suggest, however, that much of the observed change during the early post-reform decades came from the direct change in provincial access at the metropolitan stage, before the reform could fully affect the educational preparation of newly entering cohorts.

The length of the window also reflects a precision constraint. The metropolitan examination was generally held once every three years, and the number of successful candidates from any one province in a single sitting was small. Very short windows therefore produce sparse and unstable province-level shares. Pooling the examinations in 1680–1700 and in 1710–1730, while excluding the 1712 cohort from the post-reform side, provides enough observations to construct regional

shares while retaining a period in which the direct quota effect is likely to remain an important component of measured exposure.[27]

In the panel specifications below, we omit the 1710s because that decade contains observations from both sides of the 1712 reform. This sample restriction is separate from the year-based windows used to construct $\Delta Share^{1712}$.

We first calculate each province's share of *jinshi* in 1680–1700, $Share^{1680–1700}$, and in 1710–1730, $Share^{1710–1730}$. In the code, the post-reform total excludes the 1712 cohort from the 1710s decade. Before 1712, examination competition was organized within the South, North, and Central exam regions, established by the Ming government in 1454 (Kracke, 1957). Provincial quotas replaced regional competition in 1712. To make the pre- and post-reform shares comparable, we construct sixteen time-invariant province units, each lying entirely within one historical exam region.[28] *Jiangbei* illustrates the logic of this reconstruction. Although northern *Jiangsu* and northern *Anhui* were later incorporated into provinces that also included parts of *Jiangnan*, the 1454 quota system assigned *Jiangbei* to the Central Region and *Jiangnan* to the South. We therefore treat them as separate units, preserving the geographic structure of pre-1712 examination competition.

Ideally, reform exposure would be measured using changes in admission rates. Historical population estimates, including model-based reconstructions such as HYDE (Klein Goldewijk et al., 2017), can provide useful information about relative population differences across prefectures at a given point in time, even when population levels are measured imprecisely. They are much less suited to recovering changes in prefecture-level population over the relatively short windows used here, particularly during the early Qing. The uncertainty is substantial even for China's total population.[29] Constructing comparable, time-varying prefecture-level denominators for 1680–1700 and 1710–1730 is therefore beyond what the available evidence can support. We instead use each province's share of *jinshi* within its historical exam region. In the panel regressions, exam-region-by-decade fixed effects absorb decade-specific changes shared by prefectures in the same historical exam region, including broad shifts in regional population and examination conditions.[30]

[27]Suppose region r contributes N_r successful *jinshi* during a given window and province (p) has population share s_{pr} among those degree holders. Under a binomial approximation, $\text{Var}(\hat{s}pr) \approx spr(1 - s_{pr})/N_r$. Shorter windows reduce N_r and increase the sampling variance of the estimated provincial shares. Disruptions to the usual triennial schedule can further reduce the number of observations.

[28]The reconstructed units follow contemporaneous examination administration. They include *Jiangnan* in the South, *Jiangbei* in the Central Region, *Hunan-Hubei* in the South, and *Shaan-Gansu* in the North. The remaining units correspond closely to historical provinces contained within a single region, including *Fujian*, *Guangdong*, *Guangxi*, *Guizhou*, *Henan*, *Jiangxi*, *Shanxi*, *Shandong*, *Sichuan*, *Yunnan*, *Zhejiang*, and *Zhili/Hebei*. Appendix B.2.2 and Table A.8 provide the construction details and crosswalk.

[29]Estimates range from roughly 80–100 million around 1644 to 150–200 million by 1740 (Ho, 1959; Perkins et al., 1969; Liang, 1980).

[30]Equation (3) includes exam-region-by-decade fixed effects, $\rho_{r \cdot t}$, which absorb all time-varying factors common to prefectures within an exam region in a given decade.

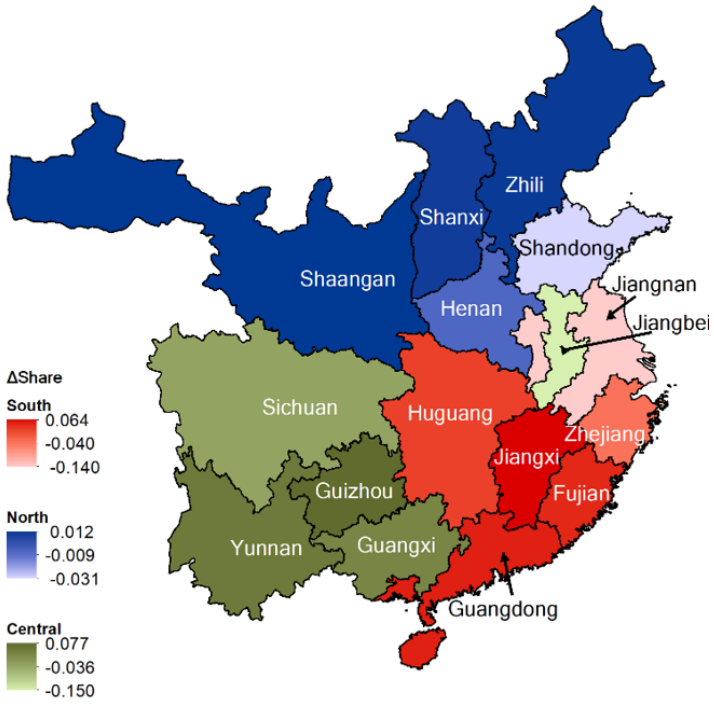


Figure I: Who Benefited From the Reform?

Notes: This map illustrates the difference between the share of jinshi within the region in 1710–1730 and the share of jinshi within the region in 1680–1700, with the 1712 cohort excluded from the post-reform window. Darker shades correspond with a larger, positive change. The light green province in the middle of the Central Region is *Jiangbei* (northern *Jiangsu* and *Anhui*), which, following the Ming government's 1454 classification of exam regions, was placed in the Central Region despite its proximity to the prosperous *Jiangnan* area.

By subtracting $Share^{1680–1700}$ from $Share^{1710–1730}$, with the 1712 cohort excluded from the post-reform side, we obtain a measure of how much each province's share of successful *jinshi* rose or fell. Figure I maps the resulting $\Delta Share^{1712}$ values: lighter shading indicates losses, while darker shading denotes gains. Although the reform did not explicitly target particular social or cultural groups, the provinces that gained tend to overlap geographically with historically peripheral or ethnic regions, including those classified as frontier provinces. Comparing Figure I with Figure A.5 shows considerable overlap between reform-beneficiary areas and frontier or minority-populated provinces.

Figure S.1 plots $\Delta Share^{1712}$ against $Share^{1680–1700}$. Provinces with lower pre-reform regional *jinshi* shares generally experienced larger gains after 1712, while provinces with higher initial shares experienced smaller gains or declines. The figure therefore shows that the reform shifted metropolitan-stage success toward provinces that had previously accounted for a smaller share of successful candidates within their historical examination regions. In this sense, $\Delta Share^{1712}$ captures the extent of the redistribution across provinces following the reform. Appendix B.2.3 reports versions based on national *jinshi* shares and on the pre-1712 within-region gap between

juren and *jinshi* shares.

V Access and Performance Under the 1712 Reform, 1650–1840

Models of affirmative action emphasize its potentially opposing effects on investment incentives.[31] Preferential treatment can weaken effort by lowering the effective standard faced by beneficiaries. It can also increase investment by raising the expected return to preparation for groups that previously faced limited prospects of success. These competing forces are especially relevant when affirmative action is introduced only at the final stage of a multistage competition. Because candidates must still clear the earlier stages under unchanged rules, such a policy leaves much of the existing incentive structure intact while increasing the expected payoff from reaching the final round.

The 1712 reform provides a useful historical setting for examining these competing forces. It introduced province-specific quotas only at the metropolitan examination, while leaving the prefectural and provincial examinations unchanged. Candidates from beneficiary provinces therefore still had to satisfy the same lower-stage requirements, but the expected return to reaching the final round increased. The observed effects may consequently reflect both the direct reallocation of final-stage slots and indirect responses in preparation and persistence along the examination track.

In this section, we first document how admissions shifted after 1712 by examining changes in *jinshi* per capita. We then study post-admission outcomes, asking whether candidates from provinces that benefited more from the reform performed differently once they entered the bureaucracy. These outcomes allow us to examine whether expanded access was accompanied by weaker or stronger subsequent performance. We also report supplementary evidence on candidates' age and family background at the time of *jinshi* success, which provides a limited view of whether the composition of admitted cohorts varied with reform intensity.

For the main panel analysis, we use data spanning 1650 to 1840, allowing us to compare outcomes before and after the reform. We end the sample in 1840 to focus on the pre-crisis Qing period and exclude later disruptions that complicate interpretation, including the Taiping Rebellion (1850–1864) and the nineteenth-century expansion of office purchase (*juanna*), which created additional non-examination routes into government service.[32]

V.A Documenting the Admissions Response

The 1712 reform reshaped the geographic allocation of *jinshi* degrees by introducing province-specific quotas at the final (metropolitan) stage of the civil service examination. We begin by doc-

[31] See Schotter and Weigelt (1992), Coate and Loury (1993), and Chan and Eyster (2003).

[32] See Zhang (2022).

umenting the admissions response, asking whether *jinshi* production shifted differentially across provinces after 1712 in the direction implied by the reform's quota reallocation.

Our outcome is $JinshiPerCapita_{i,prov,t}$, the number of successful metropolitan exam candidates per 10,000 population from unit i (prefecture or county) in province $prov$ during decade t. The key regressor is $ReformIntensity_{prov} \times Post_t$, where $ReformIntensity_{prov}$ is the post-minus-pre change in a province's *share of jinshi within its historical exam region*, as defined in Section IV, equation (1). For convenience, we reproduce the definition here:

$$\Delta Share^{1712} \;=\; Share^{1710\text{–}1730} \;-\; Share^{1680\text{–}1700}.$$

We estimate the following difference-in-differences specification:

$$JinshiPerCapita_{i,prov,t} = \beta_0 + \beta_1 ReformIntensity_{prov} \cdot \mathrm{Post}_t + \gamma X_i \cdot \eta_t + \delta_i + \theta_r \cdot \eta_t + \varepsilon_{i,prov,t}. \qquad (2)$$

To ensure a clean separation between pre- and post-reform cohorts, we exclude candidates who obtained the degree during the 1710s, the implementation decade in which the reform took effect mid-period. We adopt this treatment of the 1710s consistently across the paper.

Because the 1712 reform reallocates final-stage access at the province level, our treatment measure $ReformIntensity_{prov} \times Post_t$ varies at the province-by-decade level, while outcomes are measured for prefectures. We use prefecture-level outcomes to study within-province patterns and heterogeneity, but the identifying variation remains at the province-by-decade level. Using prefecture-level outcomes is useful because treatment varies at a higher administrative level than the unit of observation, reducing concerns that prefecture-specific shocks mechanically drive both the outcome and measured exposure. At the same time, the subprovincial data allow us to study heterogeneity in the admissions response within provinces. Accordingly, we cluster standard errors at the province level. This allows arbitrary correlation across localities within a province and avoids overstating precision when many subprovincial observations share the same province-level treatment variation (Moulton, 1990; Cameron and Miller, 2015).[33]

The specification includes unit fixed effects (δ_i) and region-by-decade fixed effects ($\theta_r \cdot \eta_t$). Unit fixed effects absorb time-invariant local determinants of exam success, including geography, long-run educational traditions, and persistent differences in prosperity. Region-by-decade fixed effects allow broad macroregions to follow distinct time paths and absorb common changes associated with the pre-1712 regional quota regime and its dismantling. In addition, X_i includes time-invariant unit characteristics—pre-reform population density, log distance to Beijing, caloric suitability, ruggedness, access to navigable rivers, and predetermined quotas for lower-tier exams—each interacted with decade fixed effects to allow their influence to vary over time.

[33] As a robustness check for inference with a limited number of provinces, we also compute wild cluster bootstrap p-values using province-level resampling with Webb weights. In tables reporting this procedure, coefficient stars are based on the bootstrap p-values, while the parentheses report conventional province-clustered standard errors.

Table I: Admissions Response to the 1712 Reform: Jinshi Per Capita

	Dependent Variable: Jinshi Per Capita							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1712 Reform Intensity × Post	0.0971***	0.0856**	0.0887**	0.0875**	0.0818*	0.0848*	0.0854*	0.0854*
	(0.032)	(0.040)	(0.042)	(0.040)	(0.043)	(0.042)	(0.042)	(0.042)
Decade FE	✓	✓	✓	✓	✓	✓	✓	✓
Decade FE × Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓	✓	✓
Decade FE × Controls	None	Pop. density	Distance	Quota	CSI	Ruggedness	Full	Full
Mean of Dep. Var.	0.024	0.024	0.024	0.024	0.024	0.024	0.024	0.024
Observations	4806	4806	4806	4806	4806	4806	4806	4806
R-squared	0.564	0.570	0.569	0.605	0.607	0.606	0.606	0.606

Notes: This table estimates the effect of the 1712 reform on prefecture-level *jinshi* admissions. Each observation is a prefecture–decade from 1650 to 1830, excluding 1710. The dependent variable is the number of *jinshi* per 10,000 population. "Reform intensity" is defined as the change in a province's share of *jinshi* within its historical exam region between 1680–1700 and 1710–1730, with the 1712 cohort excluded from the post-reform side. Columns 2–7 add controls sequentially. Standard errors, clustered at the province level, are reported in parentheses. In column 8, stars use wild cluster bootstrap inference with Webb weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table I reports the results using prefecture–decade observations. *Jinshi* per capita rises more after 1712 in provinces with higher reform intensity. The pattern is robust to including time-varying effects of local characteristics in column 2. In column 8, which uses wild cluster bootstrap inference, the p-value for the reform-intensity coefficient is 0.084. Appendix B.2.3 shows that the admissions results remain positive when reform exposure is measured using national *jinshi* shares or using the pre-1712 within-region gap between *juren* and *jinshi* shares. The estimates are less precise in some specifications using the national-share measure, but the overall pattern remains consistent with the baseline results.

Dynamic Path. Figure A.2 plots decade-by-decade coefficients from a version of Equation (2) that replaces the single post-reform indicator with interactions between *ReformIntensity* and decade dummies, while retaining the same fixed effects and controls. The coefficients are close to zero before 1712 and turn positive afterward, with the largest increases in the 1720s and 1730s. They attenuate during the 1760s–1790s, when the empire-wide number of *jinshi* was unusually low (Figure A.1), and are generally positive again in subsequent decades. Overall, the event-study profile is consistent with a persistent, though uneven, admissions response following the reform.

V.B Post-Admission Outcomes: Career Performance in the Bureaucracy

A central question in the affirmative action literature is whether expanded access is accompanied by weaker performance after admission. Related work examines academic achievement, field choice, and graduation among students affected by affirmative-action policies (Arcidiacono, Aucejo, and Spenner, 2012; Arcidiacono, Hotz, and Aucejo, 2016). In our setting, the 1712 reform increased the probability that candidates from previously underrepresented provinces would

pass the metropolitan examination and obtain the highest examination degree. If the reform admitted candidates who were less prepared, their subsequent performance may have been weaker. If, instead, it encouraged greater preparation or enabled capable candidates who had previously faced unfavorable admission prospects to succeed, post-admission performance need not have deteriorated. We examine this question using candidates' subsequent career outcomes.[34]

We therefore examine whether the post-admission career outcomes of *jinshi* changed differentially after 1712 in provinces that benefited more from the reform.[35]

We estimate the following difference-in-differences model:

$$Y_{pref,prov,t} = \beta_0 + \beta_1 ReformIntensity_{prov} \cdot Post_t + \gamma X_{pref} \cdot \eta_t + \delta_{pref} + \theta_r \cdot \eta_t + \varepsilon_{pref,prov,t}, \qquad (3)$$

where $pref$ indexes prefectures, $prov$ provinces, and t decades. The dependent variable $Y_{pref,prov,t}$ is one of three prefecture–decade career outcomes for *jinshi* originating from prefecture $pref$: the rank of the first assignment, the highest rank attained, or the average rank across all recorded positions. Official ranks are coded on a scale from 0.5 to 9, with higher values corresponding to more senior positions. Prefecture–decades with no recorded position holder are assigned a value of zero.

The coefficient β_1 captures whether post-1712 changes in career outcomes were larger in provinces with higher reform intensity. The set of controls mirrors those used in Equation 2. Specifically, X_{pref} includes time-invariant prefecture-level characteristics—pre-reform population density, log distance to Beijing, caloric suitability, ruggedness, access to navigable rivers, and predetermined quotas for lower-tier examinations—each interacted with decade fixed effects. The specification also includes prefecture fixed effects, δ_{pref}, and region-by-decade fixed effects, $\theta_r \cdot \eta_t$. Together, these terms account for persistent geographic differences and differential changes over time across examination regions.

Interpreting bureaucratic rank. First assignment partly reflects metropolitan-exam rank and assessments made during the appointment process, and therefore captures initial placement rather than performance after entry alone. Highest position attained and career-average position incorporate subsequent promotions, evaluations, demotions, and removals over the course of a candidate's career. Although the Qing personnel system was not perfectly meritocratic, and connections and office purchase existed, particularly in the nineteenth century, advancement was structured by recurrent evaluations conducted under Board of Personnel regulations and censorate oversight. Triennial reviews in the capital and the provinces governed promotions, demotions, and dismissals, and weak evaluations could stall or end a career (Ch'ü, 1962). Highest and career-

[34] Our difference-in-differences estimates identify relative changes across provinces exposed to different reform intensities. They do not recover aggregate general-equilibrium effects on the examination system as a whole.

[35] For institutional details on postings, evaluations, and promotion rules, see Appendix C.3.2.

Table II: Post-Admission Outcomes: Bureaucratic Rank

	First Assignment		Highest Position		Career-Average Position	
	(1)	(2)	(3)	(4)	(5)	(6)
1712 Reform Intensity × Post	2.271**	2.025**	4.523*	3.279*	3.391**	2.811**
	(0.884)	(0.967)	(1.583)	(1.757)	(1.328)	(1.402)
Decade FE	✓	✓	✓	✓	✓	✓
Decade FE × Region FE	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓
Decade FE × Controls		✓		✓		✓
Mean of Dep. Var.	1.021	1.021	1.701	1.701	1.497	1.497
Observations	4806	4806	4806	4806	4806	4806
R-squared	0.444	0.445	0.412	0.412	0.430	0.429

Notes: This table reports difference-in-differences estimates of changes in the career outcomes of successful metropolitan-exam candidates around the 1712 reform. Each observation is a prefecture–decade from 1650 to 1830, excluding the 1710s. Columns 1 and 2 report the rank of the first assignment, columns 3 and 4 the highest rank attained, and columns 5 and 6 the average rank across all recorded positions. Official ranks are coded from 0.5 (lowest) to 9 (highest). Each outcome is averaged across all successful candidates originating from prefecture p in decade t. A value of zero is assigned if no candidate from a prefecture–decade held a recorded position. Columns 2, 4, and 6 include pre-reform population density, log distance to Beijing, agricultural suitability, predetermined quotas for lower-tier examinations, ruggedness, and access to navigable rivers, all interacted with decade fixed effects. All specifications include decade fixed effects, prefecture fixed effects, and region-by-decade fixed effects. Standard errors, clustered at the province level, are reported in parentheses. Stars use wild cluster bootstrap inference with Webb weights. $p < 0.10$, $p < 0.05$, $p < 0.01$.

average rank therefore provide informative measures of post-admission performance accumulated across multiple evaluation cycles. This interpretation is also consistent with the examination-based selection and promotion system described by Ho (1962) and Elman (2013). We therefore interpret stable or improving career ranks in higher-intensity provinces as evidence against weaker post-admission performance among admitted *jinshi*.

Table II presents the baseline results. Across all three measures—first assignment, highest position attained, and career-average position—the coefficient on $ReformIntensity \times Post$ is positive and statistically significant. The career outcomes of candidates from higher-intensity provinces therefore improved more after 1712 than those of candidates from lower-intensity provinces. With one standard deviation in reform intensity equal to 0.046, the estimates for highest position in columns 3 and 4 imply gains of approximately 0.15–0.21 points on the 0.5–9 scale, or roughly one-third to two-fifths of a half-rank tier.

Appendix Table A.2 shows that the results for first assignment, highest position, and career-average position remain similar when the corresponding prefecture–decade missing-rank shares are included as additional controls.

Dynamic Path. Figure A.3 plots event-time coefficients from the specification corresponding to column 4 of Table II, replacing the single post-reform indicator with interactions between *ReformIntensity* and decade dummies while retaining the same fixed effects, controls, and clustering.

The pre-1712 estimates do not suggest a clear differential trend. After 1712, the coefficients are generally positive across several cohorts and show no pattern of deterioration, consistent with the average gains reported in Table II.[36]

Taken together, the estimates suggest that the reform's effects extended beyond admission into the *jinshi* pool. Candidates from provinces that benefited more from the reform did not experience weaker post-admission trajectories; if anything, their career ranks improved on average after 1712. This pattern is consistent with the possibility that last-stage quotas increased expected returns before entry and thereby encouraged greater preparation or altered selection into the examination track among candidates from previously underrepresented provinces.

Quality Dilution Within Cohorts: Heterogeneity by Exam Rank To probe quality dilution more directly, we ask whether the reform's post-admission effects were systematically worse for candidates who entered at the lower end of the admitted exam-rank distribution. If the reform mainly expanded access by admitting weaker marginal candidates, any deterioration in later bureaucratic outcomes should be concentrated in the lower rank bins. We therefore partition successful candidates by standardized metropolitan exam rank and interact those bins with reform intensity in the post-1712 period.

$$\begin{aligned} Y_{i,pref,prov,t} = \beta\, ReformIntensity_{prov} \cdot Post_t \sum_{b \neq \text{Top}} \gamma_b\, \mathbf{1}\{RankBin_i = b\} \\ + \sum_{b \neq \text{Top}} \delta_b\, \mathbf{1}\{RankBin_i = b\} \cdot ReformIntensity_{prov} \cdot Post_t \lambda X_{pref} \cdot \eta_t \\ + \alpha_{pref} + \eta_t + \theta_r \cdot \eta_t + \varepsilon_{i,pref,prov,t}. \end{aligned} \tag{4}$$

Here, $Y_{i,pref,prov,t}$ denotes the bureaucratic outcome of candidate i, and $RankBin_i$ indexes bins of standardized metropolitan exam rank. The omitted category is the top rank bin (0.8–1.0). The coefficients δ_b measure whether the reform effect differs for candidates in bin b relative to top-ranked candidates. If quality dilution were important at the margin, we would expect these coefficients to become more negative as we move toward the lower rank bins.

Table A.3 provides little evidence of such a pattern. The estimates do not show a systematic worsening of career outcomes concentrated among lower-ranked admitted candidates.

Controlling for Exam Performance Successful candidates entered the bureaucracy at different levels partly on the basis of their metropolitan-exam rank, together with other assessments made during the appointment process (see Appendix C.2.4). Figure A.6 and Table A.11 show

[36]The estimates are smaller in the mid-to-late eighteenth century, especially between approximately 1760 and 1790. This attenuation is consistent with the heterogeneity results in Table A.12, which show stronger estimated reform effects in decades when the nationwide number of *jinshi* awarded was higher. If aggregate exam supply was relatively low during 1760–1790, the interaction estimates imply a correspondingly smaller reduced-form effect of reform exposure in those decades.

that exam rank is strongly associated with subsequent career outcomes, especially the highest bureaucratic position attained. Exam rank is therefore useful for interpreting the career results because it reflects accumulated academic preparation and provides a proxy for candidate quality.[37] It is not, however, well suited as a primary outcome. Exam rank is observed only for candidates who passed the metropolitan examination, and the reform changed the composition of this selected group.[38]

Exam rank is also not a predetermined control. The 1712 reform changed which candidates entered the ranked pool, since province-specific quotas excluded some candidates, particularly from provinces that lost quota share, from the final ranking process altogether. Conditioning on exam rank therefore changes the estimand and should be interpreted as a descriptive decomposition rather than as part of the baseline specification. With this caveat, column 1 of Table A.4 adds exam rank to the specification for highest position attained. The coefficient on $ReformIntensity \times Post$ remains positive, indicating that the baseline career differences are not explained solely by candidates' relative positions in the metropolitan-exam ranking.

Accounting for First Assignment First assignment was closely related to exam rank, and early placement could shape later promotions and career trajectories (Oyer, 2006). First assignment is itself a post-admission outcome, however, and may have been affected by the reform. Conditioning on it therefore changes the estimand and is best interpreted as a decomposition of the career results. With this caveat, column 2 of Table A.4 re-estimates the reform effect on *highest position* while controlling for first assignment. The reform coefficient attenuates, consistent with differences in initial placement accounting for part of the baseline difference in the highest position attained.

As a complementary exercise, column 3 uses the difference between highest-position rank and first-assignment rank as the dependent variable. This "promotion gap" measures advancement relative to a candidate's initial placement. The positive estimate provides no evidence that greater reform exposure was associated with weaker subsequent progression through the bureaucracy.

Beyond Rank: Non-Rank Attributes of Bureaucratic Appointments Beyond formal bureaucratic rank, we examine whether successful candidates from provinces with greater reform intensity were assigned to different types of locations. Candidates could attain comparable ranks while still receiving postings that differed in remoteness, political importance, or local fiscal and

[37] This echoes evidence from modern admissions settings in which standardized academic credentials strongly predict later outcomes; see Chetty, Deming, and Friedman (2026).

[38] Exam rank is also ordinal rather than cardinal: it records a candidate's relative position within a particular cohort of successful candidates rather than performance on a common scale across examination cohorts. Normalizing rank to lie between zero and one does not remove this limitation, so changes in rank across cohorts cannot be interpreted straightforwardly as comparable changes in academic performance.

urban conditions.[39]

Table A.5 examines the characteristics of each *jinshi*'s highest recorded appointment, matching posting locations to county-level data. Columns 1 and 2 consider distance to the border and Beijing. Columns 3 and 4 examine whether the appointment was in the central government or classified as institutionally important. Columns 5 and 6 consider Ming tax per capita and Ming urban population at the posting location.

The clearest difference is in distance to the border. The estimate in column 1 implies that a one-standard-deviation increase in reform intensity, equal to 0.046, was associated with an appointment about 33 kilometers closer to the border ($0.046 \times 708.7 \approx 33$), or roughly 4% of the mean distance. The remaining estimates are imprecise. Their implied magnitudes are also generally small: the estimates for appointments in the capital, institutional importance, Ming tax per capita, and Ming urban population correspond to changes of only a few percent or less relative to their respective means. Overall, the results suggest that candidates from higher-intensity provinces were posted somewhat closer to the border, but they do not indicate broader systematic differences across the other posting-location characteristics examined.

Governance Outcomes at Jinshi Office Locations: Conflict Bureaucratic rank captures formal advancement within the imperial hierarchy, but it does not directly measure how effectively an official governed. We therefore examine recorded conflict at the locations where *jinshi* served as a complementary, though necessarily coarse, indicator of administrative performance. If the reform admitted less capable candidates, or if its favorable career effects reflected assignment to systematically easier posts, candidates from more reform-exposed provinces might have served in locations with different subsequent conflict patterns.

Table A.6 relates reform intensity to conflict at *jinshi* posting locations. The dependent variable is an indicator for whether any local conflict was recorded during a given ten-year window relative to the year in which a candidate obtained the *jinshi* degree. Panel A uses the location of the candidate's highest-ranked appointment, while Panel B uses the location of the first assignment. The columns span the period from 40–30 years before to 30–40 years after exam success, allowing us to compare conditions before and after the candidate entered the bureaucracy.

These outcomes should not be interpreted as measures of conflict occurring precisely during an official's tenure. Appointments generally lasted only three to six years, whereas the outcomes are measured over ten-year windows and linked to the exam year rather than the exact dates of service. The timing is especially important in Panel A because candidates typically reached their highest position around two decades after obtaining the *jinshi* degree (see Appendix C.3.2).

[39] Assignments obeyed native-place "avoidance" rules that generally barred officials from serving near their place of origin. In 1703, the Kangxi court standardized a 500 *li* radius, later extending the rule to places of temporary residence, with limited exceptions for capital appointments, particularly outside finance and the Censorate (Man-Cheong, 2004).

Historical evidence indicates that successful candidates were generally in their mid-thirties at exam success and that senior officials were commonly in their late forties or fifties (Chu and Saywell, 1981; Chen et al., 2020). The 20–30 and 30–40 years-after windows therefore correspond most closely to conditions during or following service in the highest-ranked appointment. We exclude capital and border postings, where conflict is either difficult to localize or unlikely to provide a comparable measure of local administration.

The estimates do not reveal a systematic relationship between reform exposure and conflict. In Panel A, the coefficient is negative for the 20–10 years-before window and positive for the 0–10 years-after window, but the later post-exam estimates that correspond more closely to service in the highest position are small and statistically insignificant. In Panel B, the coefficients also vary in sign across the pre- and post-exam windows, with no consistent increase in conflict following exam success. Conflict depends on many local conditions beyond an individual official's control, and the timing does not permit it to be attributed cleanly to the officeholder. Subject to these limitations, the table provides no systematic evidence that candidates from more reform-exposed provinces were associated with greater instability at the locations where they served.

Governance Outcomes at Jinshi Office Locations: Disaster Relief We next examine disaster-relief activity at *jinshi* posting locations as a second governance outcome. Recent work treats relief provision in imperial China as an indicator of administrative responsiveness and competence, reflecting officials' role in organizing responses to local crises (Liu and Zhang, 2025). Unlike the occurrence of natural disasters themselves, relief activity may therefore capture a dimension of government performance. To separate relief responses from differences in exposure to natural shocks, the regressions control for contemporaneous weather conditions at each location.

The empirical design mirrors that in Table A.6. The dependent variable equals one if any disaster-relief activity is recorded at the prefecture or county where a *jinshi* served, measured in ten-year intervals relative to the year of exam success. Panel A uses the location of the candidate's highest-ranked appointment, while Panel B uses the location of the first assignment. As before, the decade windows should be interpreted as conditions around the relevant career stage rather than as outcomes occurring precisely during the candidate's tenure. In Panel A, the 20–30 and 30–40 years-after windows correspond most closely to service in or shortly after the highest-ranked appointment. We exclude capital appointments and focus on postings where relief provision can be meaningfully linked to local administration.

Table A.7 reports no systematic relationship between reform exposure and disaster-relief activity. In Panel A, the coefficients vary in sign and are imprecisely estimated across all event windows. In Panel B, the estimates are positive in the 20–10 years-before window and negative in the 10–20 years-after window, but both are only weakly distinguishable from zero at the 15 percent level. The remaining estimates are also imprecise and do not form a consistent temporal

pattern. Because the regressions control for contemporaneous weather conditions, the results capture differences in relief activity conditional on exposure to natural shocks. Taken together with the conflict results, the evidence provides little support for the view that candidates from more reform-exposed provinces were systematically less effective in the locations where they served.

V.C Characteristics of Admitted Candidates

Table III uses linked biographical and career records to examine candidate-level outcomes that are not available in the prefecture–decade panel. We consider age at *jinshi* success, the number of recorded relatives, and career duration. Age at exam success and recorded relatives are drawn primarily from the China Government Employee Database–Qing Extended Records (CGED-Q ER), which transcribes *tongnian chilu* and related examination rosters for degree holders and their kin. CBDB contributes additional biographical matches and checks (Harvard University, Academia Sinica, and Peking University, 2024). Because these sources contain limited information on candidates before 1712, we cannot estimate the difference-in-differences specification used in the main analysis. The results in this subsection are therefore cross-sectional associations among post-reform candidates.

Columns 1 and 4 show that candidates from higher-intensity provinces obtained the *jinshi* degree at younger ages. Given that one standard deviation in reform intensity equals 0.046, the estimates imply success about 0.58 years earlier in the 1713–1840 sample and 0.67 years earlier in the 1713–1905 sample. The most direct explanation is that the reform increased the probability of passing at the metropolitan stage, allowing candidates to obtain the degree after fewer attempts. The result therefore suggests faster progression through the examination system in higher-intensity provinces.

Columns 2 and 5 examine the number of relatives recorded for each candidate. This measure captures the extent of a candidate's observed family network and may also proxy for family advantage, since more prominent lineages were more likely to leave detailed biographical records and to sustain broader documented kin networks (Campbell and Lee, 2008). The coefficients are negative in both samples but statistically insignificant. The results therefore provide no evidence that candidates from higher-intensity provinces came from more advantaged or more extensively recorded family networks.

Columns 3 and 6 report results for career duration, measured in years in the bureaucracy. The estimates are statistically insignificant in both samples and differ in sign across the two periods: positive in the 1713–1840 sample and close to zero in the 1713–1905 sample. Reform intensity therefore does not appear to have systematically affected how long successful candidates remained in office. Chen et al. (2020) use duration in office as an indicator of bureaucratic performance. Under that interpretation, these results reinforce the broader finding that expanded access was not accompanied by weaker subsequent performance.

Table III: Age, Family Background, and Duration in Bureaucracy

	1713–1840			1713–1905		
	Age	Num. Relatives	Duration	Age	Num. Relatives	Duration
	(1)	(2)	(3)	(4)	(5)	(6)
1712 Reform Intensity	-12.62***	-99.83	1.701	-14.54***	-68.38	-0.179
	(4.603)	(213.659)	(3.745)	(3.772)	(88.959)	(3.961)
Population Density 1776	0.000427	0.159	-0.000948	-0.00117	-0.0470*	0.0000865
	(0.002)	(0.113)	(0.001)	(0.001)	(0.023)	(0.001)
Population Density 1820	-0.00181	-0.300+	0.0000969	-0.00293*	-0.0364	-0.000290
	(0.002)	(0.171)	(0.002)	(0.002)	(0.044)	(0.002)
Avg. Quota	0.212	3.214	0.511***	-0.219	-6.387**	0.349+
	(0.437)	(6.434)	(0.120)	(0.217)	(2.700)	(0.222)
Caloric Suitability Index	0.000622	0.0576+	0.000657	0.0000889	0.0284**	0.000198
	(0.001)	(0.037)	(0.000)	(0.000)	(0.013)	(0.000)
Log Ruggedness	-0.0997	-21.45+	0.183	-0.216*	-3.848	-0.0558
	(0.204)	(13.218)	(0.141)	(0.118)	(7.401)	(0.139)
River Access	-1.609**	-41.43	0.779*	-1.598***	3.685	0.922**
	(0.583)	(37.131)	(0.421)	(0.325)	(10.327)	(0.328)
Region FE	✓	✓	✓	✓	✓	✓
Decade FE	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	33.859	76.095	8.097	34.079	82.516	9.820
Observations	771	84	2036	2298	1193	3490
R-squared	0.042	0.070	0.256	0.026	0.043	0.216

Notes: This table reports the 1712 reform's association with age, family background, and bureaucratic duration of *jinshi* degree holders. Each observation is a successful *jinshi* candidate. Dependent variables include age, number of relatives, and duration in bureaucracy (years). Columns 1–3 restrict the sample to successful candidates before 1840, while columns 4–6 extend the sample to 1905. Coefficients are reported with standard errors clustered at the province level in parentheses. For the 1712 reform-intensity coefficient in columns 1–6, stars are based on wild cluster bootstrap p-values with Webb weights. All other coefficients shown use conventional province-clustered inference. * $p < 0.10$, $p < 0.05$, *** $p < 0.01$.

Appendix B.4 examines age and family background using a separate FamilySearch genealogy linkage that includes candidates from before and after 1712 and therefore permits a candidate-level difference-in-differences exercise. In that sample, family background is measured by the number of same-lineage relatives known to have been alive in the candidate's exam year. Although the linked sample is small, it provides a complementary test using a different source and a different measure of family background.

Summary and interpretation. Taken together, the evidence provides no indication that the 1712 reform weakened candidates' post-admission performance. *Jinshi* from provinces with greater reform intensity received higher first assignments, attained higher peak ranks, and held higher career-average ranks. The attenuation of the highest-position estimate after controlling for exam rank or first assignment indicates that observed exam performance and initial placement account for much of this difference. At the same time, the promotion-gap estimates provide no evidence of weaker subsequent advancement. The rank-bin analysis likewise shows no deterioration con-

centrated among candidates admitted from the lower end of the exam-rank distribution. Career duration did not decline, and the posting-location, conflict, and disaster-relief results reveal no systematic offsetting disadvantages.

Candidates from higher-intensity provinces also obtained the *jinshi* degree at younger ages. This pattern is consistent with the reform allowing candidates to pass the metropolitan examination after fewer attempts and progress through the examination system more quickly.

The results are consistent with the reform affecting both effort and selection. By raising the expected payoff to reaching the final stage, the reform strengthened incentives for preparation and persistence and may have encouraged capable candidates to remain on the examination track. Because it operated only at the metropolitan stage, candidates still had to pass the unchanged lower-tier examinations. The reform could therefore broaden final-stage access while preserving the screening and effort incentives embedded in the earlier stages of the system.

VI Distributional Consequences within Provinces

VI.A Heterogeneous Effects Within Provinces

A central question in the affirmative action literature is whether policies designed to reduce inequality between groups inadvertently widen disparities within the beneficiary group. New opportunities may be captured disproportionately by individuals or locations with greater pre-existing capacity to respond (Bertrand, Hanna, and Mullainathan, 2010; Akhtari, Bau, and Laliberté, 2024).

We examine this possibility by studying heterogeneity across prefectures within the same province. Our measure of pre-existing human capital, $Pre1650HC_{pref}$, is the number of *jinshi* per capita produced by a prefecture before 1650. It captures long-standing differences in educational preparation and local capacity well before the 1712 reform. Figure S.2 shows substantial variation in this measure both across and within provinces.

Unlike the preceding analysis, which uses the signed, continuous measure of reform intensity, we separate its positive and negative components. We define $Gainprov = \max ReformIntensityprov, 0$ and $Lossprov = \max -ReformIntensityprov, 0$. Reform intensity is the post-minus-pre change in a province's share of *jinshi* within its historical examination region. Thus, *Gainprov* measures the increase in this share for provinces that gained after the reform, while *Lossprov* measures the absolute decline for provinces that lost. Separating the two components allows the relationship between pre-existing human capital and post-reform outcomes to differ between gaining and losing provinces.

To assess how this variation shaped the reform's consequences, we estimate:

$$
\begin{aligned}
Jinshi_{pref,prov,t} = \beta_0 &+ \beta_1 Gain_{prov} \cdot Pre1650HC_{pref} \cdot Post_t + \beta_2 Loss_{prov} \cdot Pre1650HC_{pref} \cdot Post_t \\
&+ \beta_3 Gain_{prov} \cdot Pre1650HC_{pref} + \beta_4 Loss_{prov} \cdot Pre1650HC_{pref} + \beta_5 Pre1650HC_{pref} \cdot Post_t \\
&+ \beta_6 Gain_{prov} \cdot Post_t + \beta_7 Loss_{prov} \cdot Post_t \\
&+ \gamma X_{pref} \cdot \eta_t + \delta_{prov} \cdot \eta_t + \theta_{pref} + \eta_t + \varepsilon_{pref,prov,t} \qquad (5)
\end{aligned}
$$

where $Jinshi_{pref,prov,t}$ is the number of successful candidates per capita from prefecture $pref$, province $prov$, and decade t. The coefficients of interest are β_1 and β_2. The first captures whether prefectures with greater pre-1650 human capital experienced larger post-reform gains within provinces whose share of successful candidates increased. The second captures whether pre-existing human capital moderated the adverse consequences within provinces whose share declined.

The model includes prefecture fixed effects θ_{pref}, decade fixed effects η_t, and province-by-decade fixed effects $\delta_{prov} \cdot \eta_t$. The province-by-decade fixed effects absorb all changes common to prefectures in the same province and decade, including the average consequences of provincial gains or losses. Identification therefore comes from changes across prefectures with different levels of pre-existing human capital within the same province. We present the full set of lower-order interactions involving $Pre1650HCpref$, $Gainprov$, and $Loss_{prov}$; terms collinear with the fixed effects are absorbed in estimation.

The vector of time-invariant prefecture characteristics, X_{pref}, includes pre-reform population density, log distance to Beijing, caloric suitability, ruggedness, access to navigable rivers, and lower-tier quota assignments. Each is interacted with decade fixed effects.

Table IV reports the results using prefecture–decade observations. Column (1) includes the fixed effects and lower-order interactions. Column (2) adds decade interactions with pre-reform population density, and the subsequent columns add the remaining baseline controls sequentially.

Across specifications, the coefficient on $Gainprov \times Pre1650HCpref \times Post_t$ is positive and statistically significant. Within provinces whose share of successful candidates increased after the reform, prefectures with stronger pre-existing human capital experienced larger increases in *jinshi* per capita than other prefectures in the same province.

The coefficient on $Lossprov \times Pre1650HCpref \times Post_t$ is also positive. Within provinces whose share of successful candidates declined, prefectures with greater pre-1650 human capital were relatively less adversely affected. On both sides of the reform-induced change, historically stronger prefectures performed better than historically weaker prefectures in the same province.

To assess whether this pattern reflects long-run economic potential rather than human capital, Appendix B.5.1 allows caloric suitability to interact separately with provincial gains, provincial losses, and the post-reform period. The estimated interactions with pre-1650 human capital remain similar.

Table IV: The Distributional Consequences of the 1712 Reform

	Dependent Variable: Jinshi Per Capita							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gain × Pre1650HC × Post	0.112**	0.0944**	0.104**	0.0973**	0.0880**	0.0930**	0.0932**	0.0932**
	(0.040)	(0.041)	(0.043)	(0.041)	(0.036)	(0.037)	(0.037)	(0.037)
Loss × Pre1650HC × Post	0.0600***	0.0578***	0.0634***	0.0577***	0.0528***	0.0561***	0.0550***	0.0550***
	(0.017)	(0.017)	(0.018)	(0.017)	(0.014)	(0.015)	(0.016)	(0.016)
Decade FE	✓	✓	✓	✓	✓	✓	✓	✓
Decade FE × Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓	✓	✓
Decade FE × Controls	None	Pop. density	Distance	Quota	CSI	Ruggedness	Full	Full
Mean of Dep. Var.	0.024	0.024	0.024	0.024	0.024	0.024	0.024	0.024
Observations	4806	4806	4806	4806	4806	4806	4806	4806
R-squared	0.572	0.576	0.576	0.612	0.613	0.612	0.612	0.612

Notes: This table estimates whether the 1712 reform changed the distribution of *jinshi* within provinces. Each observation is a prefecture–decade from 1650 to 1830, excluding 1710. Pre1650HC is *jinshi* per capita before 1650, normalized using population around 1600. All specifications include prefecture fixed effects, decade fixed effects, province-by-decade fixed effects, and lower-order terms. Columns 2–7 add controls sequentially; column 8 uses wild cluster bootstrap p-values with Webb weights for significance. Standard errors, clustered at the province level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The magnitudes are substantial. Using column (2), a one-standard-deviation difference in pre-reform human capital, equal to 2.101 *jinshi* per 10,000 people, implies a differential increase of approximately 0.002 *jinshi* per 10,000 people per decade for each one-percentage-point increase in $Gain_{prov}$. This is about 8% of the sample mean.[40]

Among gaining provinces, moving from the 25th to the 75th percentile of *Gainprov* implies that a prefecture with one-standard-deviation greater pre-reform human capital produced about 0.008 additional *jinshi* per 10,000 people per decade relative to another prefecture in the same province. Among losing provinces, the corresponding interquartile change in *Lossprov* implies a differential of approximately 0.016 *jinshi* per 10,000 people.[41]

The reform therefore reinforced pre-existing educational advantages within provinces. In gaining provinces, historically stronger prefectures captured more of the post-reform increase in successful candidates. In losing provinces, they were better able to withstand the decline. A reform that reduced disparities across provinces thus widened disparities among prefectures within them.

These results are consistent with models in which initial human-capital advantages compound because better-positioned locations can respond more strongly to changes in educational incentives (Durlauf, 1996). They also reflect the broader idea that inequalities in resources and opportunity can reproduce themselves in the absence of countervailing forces (Piketty, 2014).

Dynamic Path. Figure A.4 traces the differential effects by decade. In gaining provinces, the rel-

[40] $2.101 \times 0.01 \times 0.0944/0.024 \approx 8\%$.

[41] The corresponding calculations are $2.101 \times (0.0509 - 0.0124) \times 0.0944 \approx 0.008$, or about 32% of the sample mean, and $2.101 \times (0.1402 - 0.0126) \times 0.0578 \approx 0.016$, or about 65% of the sample mean.

ative advantage of high-human-capital prefectures emerges after the reform. In losing provinces, the divergence becomes larger over time, indicating that historically stronger prefectures were increasingly better able to absorb the adverse change. The dynamic results reinforce the conclusion that the reform's consequences within provinces depended strongly on pre-existing educational capacity.

VI.B Trends in Inter- and Intra-Provincial Inequality

To examine how the geographic distribution of *jinshi* changed around the 1712 reform, we calculate inequality in *jinshi* per capita across prefectures over the period 1650–1840. We report both the Gini coefficient and the Theil-T index. For the Gini coefficient, we compare overall inequality across prefectures with inequality between province-level averages. The Theil-T index permits an additive decomposition of overall inequality into between-province and within-province components.

Figures S.3a and S.3b show that overall inequality declined after the reform, with a more pronounced decline in inequality between provinces. The between-province Gini fell sharply in the period spanning the reform and remained below its pre-reform level thereafter, while the overall Gini declined more gradually. The Theil decomposition shows a similar pattern: the between-province component fell after the reform, whereas the within-province component changed relatively little. Consequently, differences among prefectures within the same province accounted for a larger share of the inequality that remained.

This pattern complements the heterogeneous-effects results in Section VI.A. Within provinces whose share of successful candidates increased, prefectures with greater pre-existing human capital experienced larger gains. Within provinces whose share declined, historically stronger prefectures were less adversely affected. The reform therefore reduced disparities across provinces without producing comparable convergence among prefectures within them.

Overall, the reform changed the geographic structure of inequality. Differences in *jinshi* production across provinces narrowed, while disparities among prefectures within the same province remained persistent.

VII The Policy Reversal in 1905

This section examines how the gains associated with the 1712 reform changed after the province-specific admission rule ended in 1905. We ask whether provinces that gained more under the reform maintained their relative advantage in educational and professional attainment during the following four decades.

Because the *jinshi* degree was no longer awarded after 1905, we use the closest available measures of educational and professional attainment in the subsequent period. These include notable

figures with documented advanced education, graduates of Tsinghua and Peking University, and students who studied overseas. The measures allow us to examine whether the geographic advantage associated with reform intensity remained visible after the province-specific policy ended.

We construct a panel spanning the periods immediately before and after 1905. Before 1905, the dependent variable is *jinshi* per capita in 1875–1885, 1885–1895, and 1895–1905. After 1905, the dependent variable is the per-capita number of notable figures, Tsinghua graduates, Peking University graduates, or overseas students in 1905–1915, 1915–1925, 1925–1935, and 1935–1945. Because these outcomes differ in scale, each is standardized within period.

$$Y_{pref,prov,t} = \beta_0 + \beta_1 ReformIntensity_{prov} \cdot Post_t + \gamma X_{pref} \cdot \eta_t + \theta_{pref} + \theta_r \cdot \eta_t + \varepsilon_{pref,prov,t} \quad (6)$$

The dependent variable $Y_{pref,prov,t}$ is the period-standardized outcome for prefecture $pref$ in province $prov$ and period t. $ReformIntensityprov$ is the post-minus-pre change in the province's share of *jinshi* within its historical examination region, and $Postt$ equals one for periods after 1905. The coefficient β_1 captures whether prefectures in provinces with greater reform intensity experienced different changes after the province-specific policy ended.

The vector X_{pref} includes population density around 1910, log distance to Beijing, lower-tier examination quotas, caloric suitability, ruggedness, river access, treaty-port status, and Republican-era Christian presence, each interacted with period fixed effects. The specification also includes prefecture fixed effects, period fixed effects, and examination-region-by-period fixed effects. Standard errors are clustered at the province level.

Table V reports negative coefficients on $ReformIntensityprov \times Post1905$ for all four outcomes. The estimates are statistically significant for notable figures and overseas students. The estimates for Tsinghua and Peking University graduates are also negative but less precise. Because the outcomes are standardized within period, the coefficients capture relative changes rather than absolute declines. Prefectures in provinces with greater reform intensity experienced less favorable changes after 1905 than prefectures in provinces with lower reform intensity.

The magnitudes are meaningful for the two statistically significant outcomes. Given that one standard deviation in reform intensity is 0.046, the estimates imply that a one-standard-deviation increase in reform intensity is associated with a $3.598 \times 0.046 \approx 0.17$ standard-deviation less favorable post-1905 change in notable figures and a $3.867 \times 0.046 \approx 0.18$ standard-deviation less favorable change in overseas students.

These results indicate a partial reversal of the gains associated with the 1712 reform. Prefectures in provinces with greater reform intensity experienced less favorable relative changes after the province-specific policy ended. The negative coefficients across all four measures suggest that this pattern was not confined to a single route of educational or professional advancement.

Dynamic Path. Figure II presents an event-study version of Equation 6, replacing the single

Table V: The Impact of the 1712 Reform on Highly Educated Individuals Upon Policy Reversal in 1905

	(1) Notable Figures	(2) Tsinghua Graduates	(3) Peking Graduates	(4) Oversea Students
1712 Reform Intensity $\times$Post$_{1905}$	-3.598**	-1.740	-0.562	-3.867***
	(1.676)	(1.613)	(1.739)	(1.870)
Decade FE	✓	✓	✓	✓
Region FE $\times$ Decade FE	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓
Controls $\times$ Decade FE	✓	✓	✓	✓
Mean of Dep. Var.	-0.000	-0.000	-0.000	-0.000
R-squared	0.483	0.465	0.606	0.589
Observations	1869	1869	1869	1869

Notes: This table examines how outcomes associated with the 1712 reform changed after the province-specific admission rule ended in 1905. Each observation is a prefecture–period from 1875 to 1945. Before 1905, the dependent variable is *jinshi* per capita. After 1905, the dependent variable is the per-capita number of notable figures, Tsinghua University graduates, Peking University graduates, or overseas students. Each outcome is standardized within period. All specifications control for population density around 1910, log distance to Beijing, caloric suitability, predetermined lower-tier examination quotas, ruggedness, river access, treaty-port status, and Republican-era Christian presence, each interacted with decade fixed effects. All specifications include decade fixed effects, prefecture fixed effects, and examination-region fixed effects interacted with decade fixed effects. Standard errors, clustered at the province level, are reported in parentheses. Stars use wild cluster bootstrap inference with Webb weights. $p < 0.10$, $p < 0.05$, $p < 0.01$.

post-1905 interaction with interactions between reform intensity and indicators for each period. The pre-1905 estimates are close to zero. After 1905, the coefficients become negative for notable figures, Tsinghua graduates, and overseas students, consistent with the estimates in Table V. The pattern for Peking University graduates is weaker.

The table and event-study evidence therefore point to a weakening of the relative advantage of higher-intensity provinces during the decades after 1905. This need not mean that the reform left no durable legacy. The next section asks whether any effects remained visible over a much longer horizon.

VIII Persistence and Spillovers

The preceding section shows that the relative advantage of higher-intensity provinces weakened during the four decades after 1905. This section extends the time horizon to 1982 and asks whether effects of the 1712 reform nevertheless remained visible in educational and occupational outcomes. These outcomes extend well beyond the small number of candidates who obtained the *jinshi* degree, while college education provides a comparably selective measure of educational achievement in a much later period.

Contemporaneous administrative records provide limited evidence on educational attainment beyond successful examination candidates. Because the number permitted to pass each level was restricted and the surviving records primarily capture degree holders, these sources cannot reveal

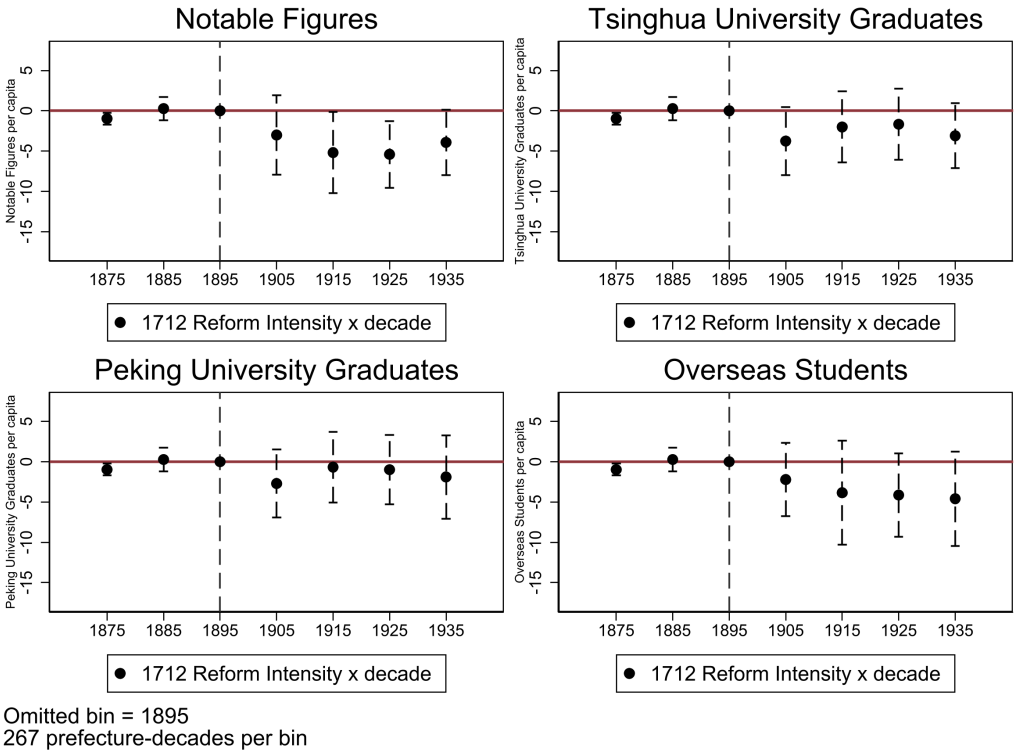


Figure II: The Impact of the 1712 Reform upon Policy Reversal in 1905

Notes: This figure presents an event-study version of Equation 6, showing how the relationship between reform intensity and educational and professional attainment evolved from 1875 to 1945. The dependent variable is period-standardized *jinshi* per capita for 1875–1885, 1885–1895, and 1895–1905. For 1905–1915, 1915–1925, 1925–1935, and 1935–1945, it is the period-standardized per-capita number of notable figures, Tsinghua University graduates, Peking University graduates, or overseas students. Each point represents the coefficient on reform intensity interacted with the corresponding period, relative to the omitted reference period. Dashed lines show conventional 90% confidence intervals. Statistical significance for the displayed reform-intensity coefficients is assessed using wild cluster bootstrap p-values with Webb weights.

whether the reform also affected educational investment among the much larger population that studied for the examinations without obtaining the highest degree. We therefore turn to the 1982 census to test whether reform intensity remained associated with educational and occupational outcomes in the broader population nearly three centuries later.

The baseline controls include population density in 1820, log distance to Beijing, caloric suitability, predetermined lower-tier examination quotas, ruggedness, access to major navigable rivers, and the 1982 shares of residents aged 40 and older, 60 and older, and 80 and older. The additional controls capture other historical processes that may have shaped long-run educational and occupational outcomes: distance to the coast, exposure to the Taiping Rebellion, treaty-port status, Ming-era urbanization, and Republican-era Christian institutional presence. The final specification for each outcome also controls for *jinshi* per capita before 1712.

Table VI reports the results. Panel A examines the shares of the population with primary, secondary, and college education. Panel B reports occupational status using the International Socio-

Table VI: The Long-Term Impact of the 1712 Reform on Educational and Occupational Outcomes

Panel A. Educational Attainment (1982)

	Primary			Secondary			College		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1712 Reform Intensity	0.125	0.114	0.124	0.108**	0.104***	0.104***	0.0149**	0.0149***	0.0151***
	(0.131)	(0.096)	(0.105)	(0.036)	(0.026)	(0.026)	(0.004)	(0.004)	(0.004)
Jinshi Per Capita$^{1368–1712}$			✓			✓			✓
Additional Controls		✓	✓		✓	✓		✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	0.573	0.576	0.576	0.061	0.062	0.062	0.004	0.004	0.004
Observations	266	251	251	266	251	251	266	251	251
R-squared	0.390	0.439	0.449	0.462	0.520	0.520	0.561	0.638	0.641

Panel B. Occupational Prestige (1982)

	ISEI			SIOPS			ISEI (Non-agri)			SIOPS (Non-agri)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1712 Reform Intensity	2.409	2.332	2.324	1.303	1.138	1.136	19.67**	16.89***	16.65***	12.33**	10.00*	9.873*
	(3.987)	(4.404)	(4.446)	(2.539)	(2.916)	(2.950)	(7.550)	(6.597)	(6.791)	(6.089)	(5.284)	(5.394)
Jinshi Per Capita$^{1368–1712}$			✓			✓			✓			✓
Additional Controls		✓	✓		✓	✓		✓	✓		✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	22.603	22.624	22.624	25.580	25.593	25.593	44.805	44.717	44.717	40.841	40.766	40.766
Observations	266	251	251	266	251	251	266	251	251	266	251	251
R-squared	0.423	0.545	0.545	0.421	0.538	0.538	0.453	0.557	0.562	0.451	0.565	0.568

Notes: This table reports the relationship between the 1712 reform and educational and occupational outcomes in 1982. Each observation is a prefecture. Panel A reports the share of the population with primary, secondary, or college education. Panel B reports occupational prestige using ISEI and SIOPS, both for the full workforce and among non-agricultural workers. The baseline controls include population density in 1820, log distance to Beijing, caloric suitability, predetermined lower-tier examination quotas, ruggedness, access to major navigable rivers, and the shares of residents aged 40 and older, 60 and older, and 80 and older in 1982. Additional controls include distance to the coast, exposure to the Taiping Rebellion, treaty-port status, Ming-era urbanization, churches per capita, and Christian adherents per capita during the Republican period. *Jinshi* per capita in 1368–1712 is included selectively as shown. All specifications include historical examination-region fixed effects. Standard errors, clustered at the province level, are reported in parentheses. Stars use wild cluster bootstrap inference with Webb weights. $p < 0.10$, $p < 0.05$, * $p < 0.01$.

Economic Index (ISEI) and the Standard International Occupational Prestige Scale (SIOPS), both for the full workforce and among non-agricultural workers.

Panel A shows a positive and statistically significant relationship between reform intensity and both secondary and college education. The estimates remain nearly unchanged after adding the historical controls and pre-1712 *jinshi* production. The coefficients for primary education are also positive but imprecisely estimated.

The magnitudes are meaningful. Using the fully controlled specifications, a one-standard-deviation increase in reform intensity, equal to 0.046, is associated with a $0.104 \times 0.046 \approx 0.0048$ increase in the share with secondary education, or approximately 0.48 percentage points. This is

about 8% of the sample mean. The corresponding increase in the share with college education is $0.0151 \times 0.046 \approx 0.00069$, or approximately 0.07 percentage points. Although small in percentage-point terms, this amounts to about 17% of the sample mean because only 0.4% of the population had college education in 1982.

In Panel B, the estimates for the full workforce are positive but imprecise. Among non-agricultural workers, however, reform intensity is positively associated with both ISEI and SIOPS scores across specifications. In the fully controlled specifications, a one-standard-deviation increase in reform intensity corresponds to approximately $16.65 \times 0.046 \approx 0.77$ additional ISEI points and $9.873 \times 0.046 \approx 0.45$ additional SIOPS points. The long-run relationship between reform intensity and occupational prestige is concentrated among non-agricultural workers.

One explanation for these patterns is that the reform raised the expected return to education in provinces that gained more. This could have increased preparation and persistence not only among candidates who eventually became *jinshi*, but also among the much larger number who studied without reaching the final degree. Their educational investment could still have yielded literacy, numeracy, and other skills useful in teaching, clerical work, local administration, and other non-agricultural occupations. Greater demand for preparation may also have supported local teachers and schools, allowing the effects to spread beyond the candidates directly affected by the final-stage rule.[42]

Using outcomes from 1982 also limits the extent to which the estimates can be attributed to the rapid economic transformation and urbanization that occurred later in the reform era. China was still predominantly rural, and college education remained rare. The positive association with secondary and college attainment therefore appears before the large expansion of education and structural change in subsequent decades.

The results show that the 1712 reform remained associated with educational attainment and occupational status nearly three centuries later. The educational relationships are strongest at the secondary and college levels, while the occupational relationship is concentrated among non-agricultural workers. The partial reversal observed after 1905 therefore did not amount to the disappearance of the reform's effects. Instead, the evidence suggests that the policy generated longer-lasting educational responses extending beyond the small number of candidates who directly received the *jinshi* degree.

IX Discussion

While our main results show that the 1712 reform increased the number of *jinshi* in under-represented provinces and improved their relative performance, the mechanisms through which

[42]The quality and quantity of schooling can have lasting effects on earnings and other adult outcomes (Card and Krueger, 1992; Chetty et al., 2011).

these changes translated into broader outcomes warrant further discussion.

Our leading interpretation is that the reform changed the perceived returns to education. By increasing the likelihood of success in the highest-level exam for candidates from previously underrepresented provinces, the reform raised the expected payoff from investment in education. This shift likely strengthened educational investment at the household and community level, particularly in regions that had historically faced low probabilities of success. Even if the number of successful candidates remained small, the signal that success was now more attainable could have shifted marginal cost–benefit calculations in favor of educational effort. At the same time, higher expected returns plausibly changed who entered and persisted on the exam track, drawing in academically prepared candidates who might otherwise have chosen not to begin exam preparation or not to continue through earlier stages. We do not observe these private responses directly. We therefore use several additional tests to assess whether more institutional or elite-based channels account for the results.

Because the reform targeted only the final stage of the exam system, earlier selection hurdles remained intact. This structure preserved incentives for sustained preparation and continued to screen candidates, while expanding access at the top and limiting the risk of shirking or strategic underinvestment.

These incentive effects were likely magnified by the tournament-like structure of the imperial examination system. Although only around 300 *jinshi* were awarded every three years, the exams functioned as a high-stakes contest with outsized symbolic and material rewards. In such settings, changes in perceived odds can affect both effort among incumbents and entry or persistence decisions by high-potential candidates at the margin. A rich literature in economics shows that tournament settings amplify responses to rank-based incentives (e.g., Lazear and Rosen, 1981; Schotter and Weigelt, 1992). In the context of imperial China, even modest improvements in the probability of winning the tournament could generate large behavioral responses across generations, as families invested more in their sons' schooling in hopes of eventual success.

Beyond these direct incentives, the reform likely triggered educational spillovers and shaped broader cultural norms. The increased visibility of successful candidates from reform-beneficiary provinces may have reshaped beliefs about who could aspire to elite status and made sustained exam preparation a more attractive path for capable students who would otherwise have sorted out. Success in the exam system carried profound symbolic significance, and its expansion into new regions may have transformed local educational cultures, normalizing aspiration and reinforcing ambition. Households might have increased investment in education not only in anticipation of bureaucratic appointment but also in response to shifting norms and expectations regarding social mobility. In this way, the reform's consequences may reflect both instrumental responses to increased returns and deeper changes in how success and effort were socially framed.

Two institutional features make persistent spillovers plausible despite extreme selectivity.

First, unsuccessful candidates commonly moved into literate occupations-private-school teaching, lineage tutoring, clerical and legal-scribal work, and informal administrative posts-so exam preparation created marketable skills rather than purely sunk costs (Appendix C.3.3; Ho, 1962; Elman, 2000, 2013; Rawski, 1979). Second, lineage institutions pooled resources to back promising boys (stipends, lodging, lineage schools), lowering participation thresholds and widening the pool of potential entrants (Ho, 1962; Elman, 2000). Together, these margins can amplify small, rules-based increases in *perceived* final-stage success into broader and longer-run gains in local human capital, even when only a tiny fraction ultimately obtains the *jinshi* degree.

To distinguish private educational responses from observable institutional alternatives, the appendix examines two channels that could otherwise account for the results. First, places that had already produced senior officials may have benefited from information, mentoring, or political connections that assisted later candidates. Controlling for exposure to prior top officials over several earlier periods leaves the estimated reform effect close to the baseline (Appendix Table A.15). Second, the reform may have encouraged academy construction and thereby expanded the local supply of education. We find no systematic relationship between reform intensity and subsequent academy construction, and controlling for academy expansion again leaves the reform coefficient largely unchanged (Appendix B.6.2). The persistence of the main effect after accounting for these channels makes it less likely that the results were driven by prior-elite networks or formal educational expansion, and strengthens the interpretation that the reform operated through greater private investment in preparation and persistence by candidates and their families.

Other elite-based channels were also limited by the institutional setting. Most gentry status derived from the lower-tier *shengyuan* and *juren* degrees, whose quotas were unchanged by the reform. The policy therefore had only a limited direct effect on the overall size of the local degree-holding elite. Assignment rules also generally prevented officials from serving in their native provinces, constraining direct hometown patronage as a transmission mechanism.[43]

The responses of provinces that lost from the reform may have differed from those of provinces that gained. The policy reduced their probability of success at the final stage, but educational investment may have adjusted slowly because examination preparation rested on established schools, lineage institutions, family strategies, and expectations. Candidates in these provinces may therefore have continued to prepare at similar rates for some time despite facing greater competition. Such adjustment frictions could produce an asymmetric response in which gains in higher-intensity provinces emerged more quickly than declines elsewhere.

Candidates in provinces that lost may also have redirected effort toward other routes of advancement, including commerce, teaching, local administration, or informal leadership. To the

[43]Among 2,904 successful candidates with known posting locations in our data, only four ever served in their province of birth. Such exceptions generally arose from special circumstances, including obligations to care for ill parents.

extent that these alternatives became more attractive when the probability of final-stage success declined, such substitution would narrow observed differences in exam-related outcomes between gaining and losing provinces. The estimated effects may therefore understate the full behavioral response to the reform along the examination margin, although we do not observe these alternative pathways directly.

The reform may also have affected perceptions of fairness and inclusion within the imperial system, particularly in frontier and minority regions.[44] By acknowledging regional disparities within a centrally administered selection system, the policy may have strengthened attachment to state institutions and Confucian educational culture.[45]

Finally, the findings relate to research on bureaucratic recruitment and performance. Candidates from provinces with greater reform intensity did not experience weaker subsequent career outcomes. Conflict and disaster-relief activity at their posting locations also show no systematic deterioration. Evidence from other settings indicates that recruitment rules and bureaucratic composition can shape government performance. Aneja and Xu (2024) find that U.S. civil-service reform during the Gilded Age improved public-sector performance by changing the composition of the bureaucracy, while Rasul and Rogger (2015) show that ethnic diversity within Nigerian bureaucracies affected project outcomes. The results therefore suggest that broader geographic access to public office need not reduce the performance of those selected. Future research could examine whether it also affected political legitimacy, state reach, or institutional resilience.[46]

X Conclusion

Imperial China relied on a competitive examination system to select officials for the bureaucracy, but success rates differed sharply across provinces. In 1712, the Qing state introduced province-specific final-stage quotas tied to candidate pools, substantially narrowing these differences. The reform provides an unusual setting in which to study the short- and long-run effects of affirmative action within a highly selective, multi-stage admissions system.

The reform increased access to the *jinshi* degree for provinces that had previously produced relatively few successful candidates. This expansion was not accompanied by weaker outcomes among those selected. Candidates from provinces with greater reform intensity received higher first assignments and attained higher peak and career-average ranks. Additional evidence on career duration, posting characteristics, conflict, and disaster relief likewise provides no systematic indication of poorer subsequent performance.

[44] Affirmative action in autocratic settings has often served goals of territorial integration and political legitimation; see Martin et al. (2001) on the Soviet Union and and (1998) on contemporary China.

[45] For a recent overview of the cultural and institutional consequences of Confucianism, see Jia and Kung (2025).

[46] For historical perspectives on recruitment balance and provincial origins within the Qing bureaucracy, see Kuhn (2002) and Elman (2013).

The benefits were distributed unevenly within provinces. Prefectures with stronger pre-existing human capital experienced more favorable post-reform changes, both in provinces that gained and in those that lost from the new allocation. Inequality between provinces declined, while disparities among prefectures within the same province remained persistent. The reform therefore reduced one dimension of inequality while reinforcing the importance of prior educational capacity in determining who benefited.

The effects of the policy also changed after the province-specific rule ended in 1905. During the following four decades, prefectures in higher-intensity provinces experienced less favorable relative changes in several measures of educational and professional attainment. This partial reversal did not amount to the disappearance of the reform's effects. Reform intensity remained positively associated with secondary and college education in 1982, as well as with occupational prestige among non-agricultural workers. These later relationships suggest that the reform generated educational responses extending beyond the small number of candidates who directly obtained the *jinshi* degree.

The evidence shows that affirmative action can expand access without reducing the subsequent performance of those admitted. Its benefits may nevertheless be concentrated among individuals and localities with greater capacity to respond to the opportunities it creates. Some effects may also persist long after the policy ends, even when the immediate advantage created by preferential treatment weakens after its removal.

Supplementary Figures

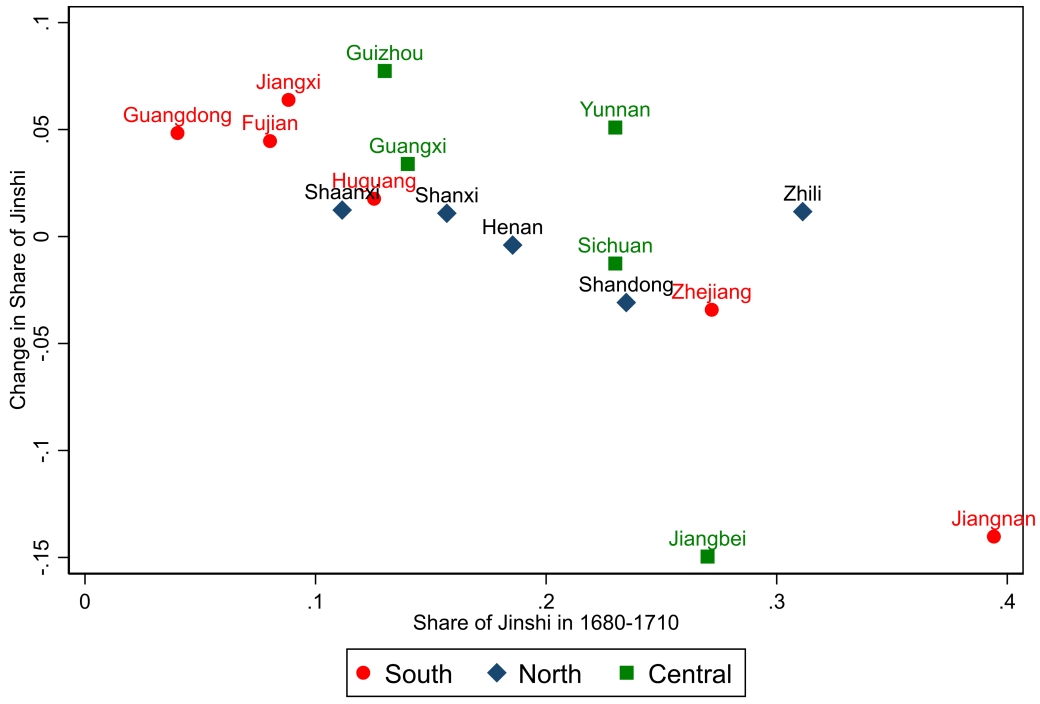


Figure S.1: Who Benefited From the Reform?

Notes: The vertical axis shows the change in a province's share of *jinshi* within its region, calculated as the difference between the share in 1710–1730 and in 1680–1700, with the 1712 cohort excluded from the post-reform window. The horizontal axis plots the province's baseline share in 1680–1700. There is a strong inverse relationship: provinces with smaller initial shares within their region saw the largest gains following the 1712 reform.

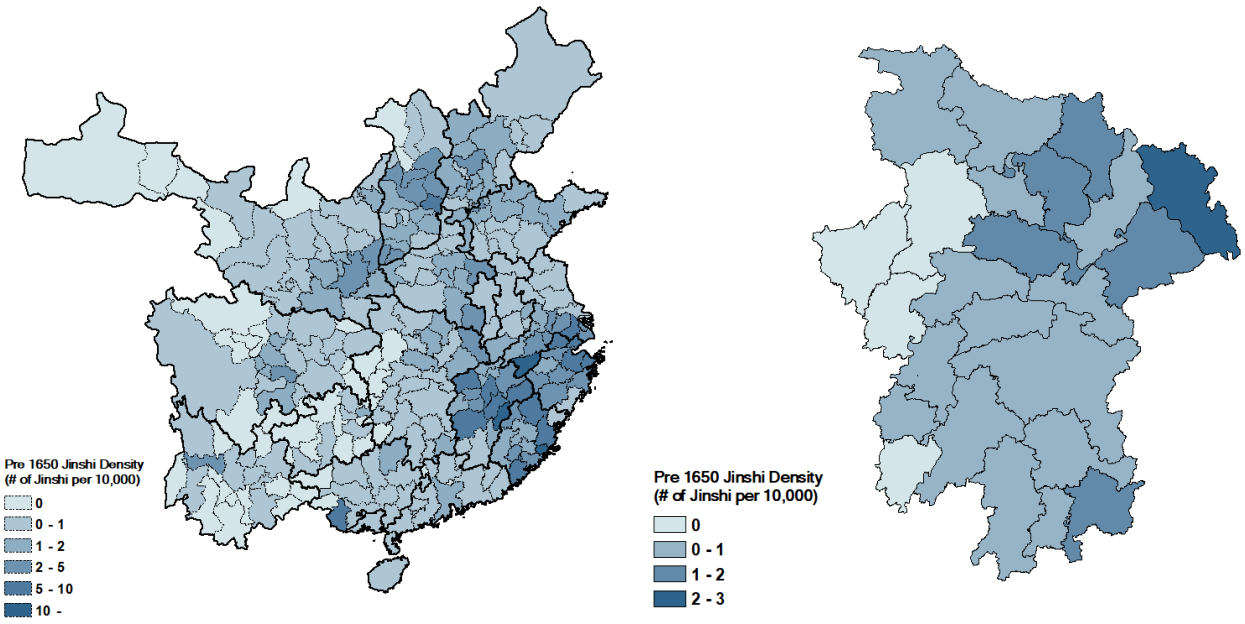
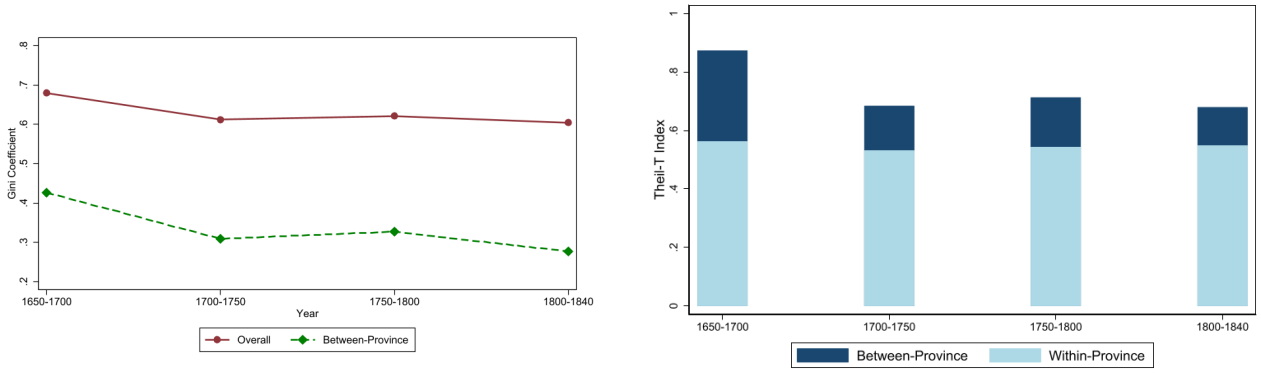


Figure S.2: Heterogeneity in Pre-Existing Human Capital

Notes: The left map depicts the spatial variation in pre-1650 human capital across the country. The right map depicts spatial variation in pre-existing human capital within Huguang.



(a) Overall vs. Between-Province Inequality, Gini Coefficient

(b) Overall vs. Between-Province Inequality, Theil-T Index

Figure S.3: Trends in Inter- and Intra-Provincial Inequality

Notes: The figures show the Gini coefficient and Theil-T index for jinshi per capita across the country and between provinces. The unit of analysis is a prefecture.

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For Online Appendix

A Additional Figures and Tables

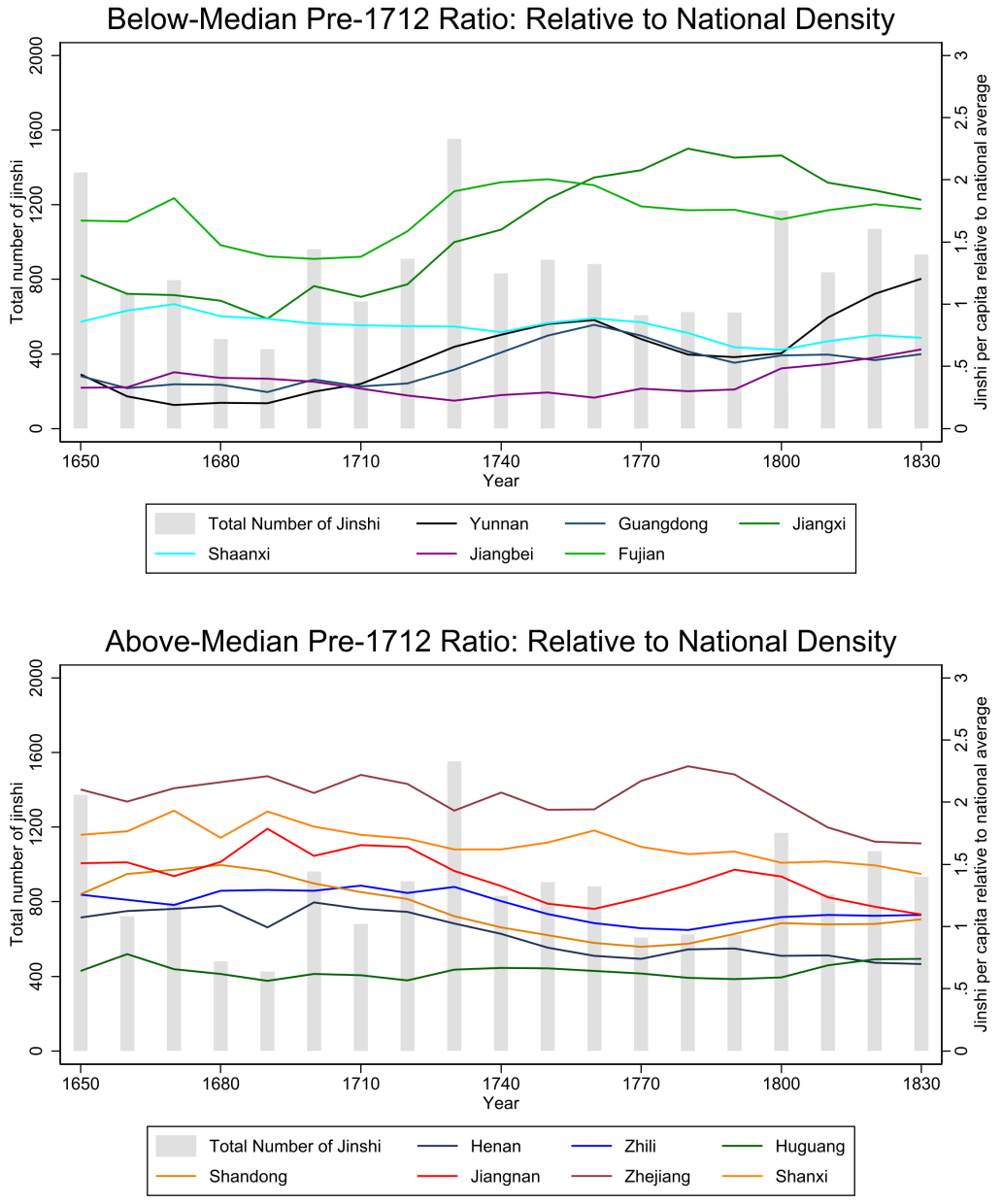


Figure A.1: Jinshi Per Capita Relative to the National Average, by the Pre-1712 Within-Region Jinshi-to-Juren Ratio

Notes: Each panel plots a province's *jinshi*-per-capita value relative to the empire-wide *jinshi*-per-capita average in the same decade. Provinces are grouped by the median pre-1712 ratio of their within-region *jinshi* share to their within-region *juren* share. The top panel shows provinces below the median ratio; the bottom panel shows provinces above the median ratio. Gray bars denote the total number of *jinshi* nationwide in each decade.

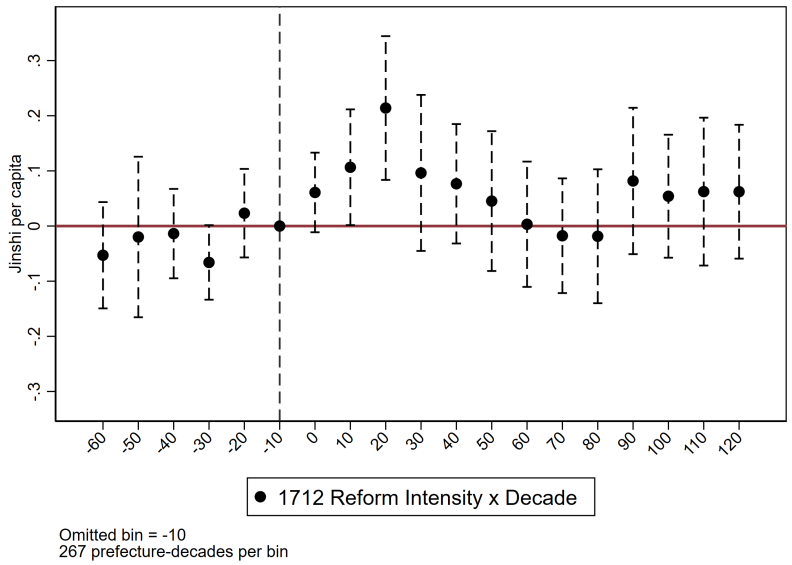


Figure A.2: Access to the *Jinshi* Degree: Event-Study Estimates for *Jinshi* per 10,000 Around 1712

Notes: Coefficients show event-time effects from column 2 of Table I. Each point is the interaction of decade bins relative to 1712 with reform intensity. Dashed lines show conventional 90% confidence intervals. For the displayed reform-intensity coefficients, statistical significance is assessed using wild cluster bootstrap p-values with Webb weights. Fixed effects, controls, and clustering match Table I column 2.

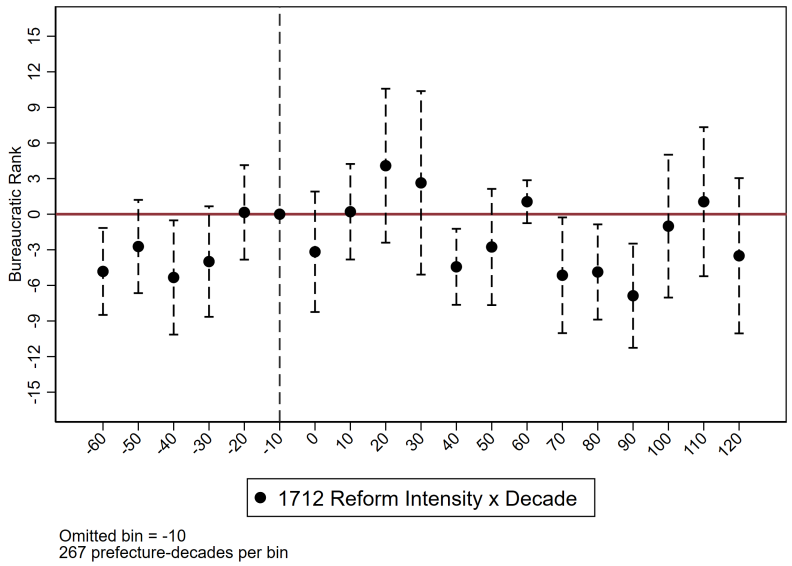


Figure A.3: Post-Admission Outcomes: Event-Study Estimates for Highest Position Rank Around 1712

Notes: Coefficients show event-time effects from column 4 of Table II. Each point is the interaction of decade bins relative to 1712 with reform intensity. Dashed lines show conventional 90% confidence intervals. For the displayed reform-intensity coefficients, statistical significance is assessed using wild cluster bootstrap p-values with Webb weights. Fixed effects, controls, and clustering match Table II column 4.

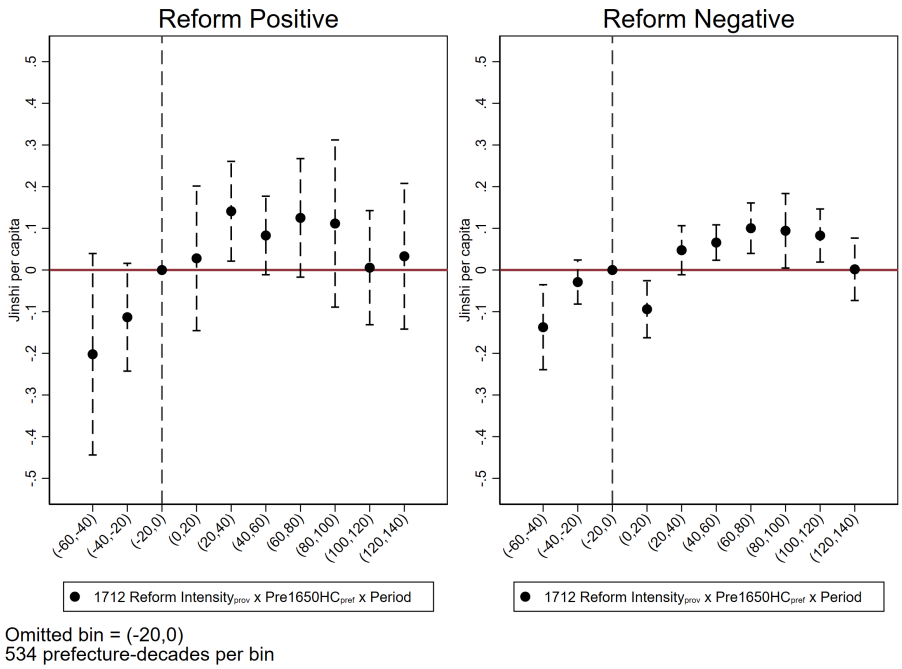


Figure A.4: The Distributional Consequences of the 1712 Reform: Event-Study Estimates

Notes: Coefficients show event-time effects from column 2 of Table IV, estimated separately for reform-positive and reform-negative provinces. Dashed lines show conventional 90% confidence intervals. For the displayed reform-intensity coefficients, statistical significance is assessed using wild cluster bootstrap p-values with Webb weights. Axes are harmonized across panels. Fixed effects, controls, and clustering match Table IV column 2.

Table A.1: Summary Statistics

	Obs.	Mean	S.D.
A. Prefecture–Decade Data, 1650–1840			
Jinshi per capita	4806	0.024	0.042
Bureaucratic Rank (Highest Position)	4806	1.591	2.601
Bureaucratic Rank (First Assignment)	4806	0.926	1.493
Career-Average Bureaucratic Rank	4806	1.497	2.096
New Academies (Total)	4806	0.004	0.018
New Academies (Gentry)	4806	0.001	0.010
New Academies (Official)	4806	0.003	0.014
B. Prefecture-level Historical Controls			
Pre-Existing Human Capital	266	1.330	2.101
Population Density (1680)	266	64.148	77.553
Population Density (1820)	266	129.343	136.013
Population Density (1910)	266	131.197	116.407
Log Distance to Beijing	266	6.905	0.759
Quota for Shengyuan	266	1.054	0.984
Caloric Suitability Index	266	3628.844	949.729
Ruggedness	266	409.591	306.632
Has Navigable River	266	0.346	0.477
Treaty Port	266	0.180	0.385
Distance to Coast	266	439.976	354.140
Taiping Rebellion Presence	266	0.406	0.492
Urbanization Rate	250	0.082	0.052
Churches per capita	266	0.166	0.294
Christian Adherents per capita	266	0.234	0.292
C. Individual-level Data in 1712–1840			
Exam Rank	15396	0.499	0.288
Bureaucratic Rank (Highest Position)	4477	5.226	1.570
Bureaucratic Rank (First Assignment)	4540	3.228	0.558
D. Individual-Position-level Data in 1712–1840			
Distance to Border	2833	878.932	465.973
Distance to Capital	2851	1018.349	638.845
Whether in Capital	2851	0.124	0.329
Whether Important	2851	0.979	0.145
Tax per Capita	2805	6.116	7.448
Urban Pop	2650	7.396	7.856
Conflict	1244	0.043	0.204
Disaster Relief	1264	0.179	0.383
Drought and Flood Frequency	1394	0.366	0.219
E. Prefecture-level Data in 1875–1945			
Notable Figures per capita (z-score)	1068	-0.000	1.000
Tsinghua University Graduates per capita (z-score)	1068	-0.000	1.000
Peking University Graduates per capita (z-score)	1068	0.000	1.000
Oversea Students per capita (z-score)	1068	-0.000	1.000
Notable Figures per capita	1068	0.043	0.069
Tsinghua University Graduates per capita	1068	0.012	0.021
Peking University Graduates per capita	1068	0.056	0.076
Oversea Students per capita	1068	0.013	0.025
F. Prefecture-level Data in 1982			
Share of Population with Primary Education	265	0.573	0.101
Share of Population with Secondary Education	265	0.061	0.029
Share of Population with Tertiary Education	265	0.004	0.004
Average ISEI	265	22.582	3.032
Average SIOPS	265	25.566	2.056
Average ISEI (Non-agri)	265	44.820	3.911

Continued on next page

Table A.1 continued

	Obs.	Mean	S.D.
Average SIOPS (Non-agri)	265	40.853	2.876
Share Aged 40	265	0.244	0.021
Share Aged 60	265	0.076	0.012
Share Aged 80+	265	0.005	0.006

Notes: (1) *Exam Rank* is an examinee's standardized percentile rank (0–1) in the metropolitan exam. (2) *Bureaucratic Rank* is an 18-level scale from 0.5 (lowest) to 9 (highest). (3) *Jinshi per capita* is the number of successful candidates per 10,000 population in a 10-year period. (4) *Pre-Existing Human Capital* is the total jinshi count from 1371–1650, normalized by population circa 1600. (5) *New Academies* is the number of newly established academies per 10,000 population in a decade, reported for all academies, gentry-sponsored academies, and state-sponsored academies. (6) *Population Density* is persons per km^2 in 1680, 1820, or 1910. (7) *Log Distance to Beijing* is the log distance from the prefectural seat to Beijing. (8) *Quota for Shengyuan* is the assigned number of lower-tier exam pass slots per 10,000 people. (9) *Caloric Suitability Index* measures potential caloric yield from Galor and Özak (2016). (10) *Ruggedness* is derived from elevation data in Danielson and Gesch (2011). (11) *Has Navigable River* is a dummy indicating major rivers identified by Matsuura (2009). (12) *Treaty Port*, *Distance to Coast*, *Taiping Rebellion Presence*, *Urbanization Rate*, *Churches per capita*, and *Christian Adherents per capita* are historical controls used in the long-run analyses. (13) *Distance to Border* is the distance between the location of a successful candidate's highest position and the border (km). (14) *Distance to Capital* is the distance between the location of a successful candidate's highest position and Beijing (km). (15) *Whether in Capital* is a dummy indicating whether the location of a successful candidate's highest position is Beijing. (16) *Whether Important* is constructed from Liu (1993), indicating whether the location of a successful candidate's highest position belongs to any of the four categories: *chong* (important in transportation/communication), *fan* (important in public affairs), *pi* (difficult to gather taxes), and *nan* (high crime rate). (17) *Ming Tax per Capita* is the conservative Ming-tax allocation to the county of a successful candidate's highest position divided by 1600 county population from HYDE. (18) *Ming Urban Population* is the pre-treatment Ming-era urban population in the location of a successful candidate's highest position. (19) *Conflict* is a dummy of whether conflict occurred at the location of a successful candidate's highest position. (20) *Disaster Relief* is a dummy of whether disaster-relief activity occurred at the location of a successful candidate's highest position. (21) *Drought and Flood Frequency* is the share of years within a decade that experienced severe droughts or famines at the relevant posting location. (22) *Notable Figures per capita*, *Tsinghua University Graduates per capita*, *Peking University Graduates per capita*, and *Oversea Students per capita* are reported per 10,000 population in a 10-year period; the corresponding z-scores standardize each series. (23) *Share of Population with Primary Education*, *Share of Population with Secondary Education*, and *Share of Population with Tertiary Education* are from the 1982 census. (24) *Average ISEI* and *Average SIOPS* are the average values of the International Socio-Economic Index and the Standard International Occupational Prestige Scale in the 1982 census; the non-agricultural versions restrict the sample to non-agricultural workers. (25) *Share Aged 40*, *Share Aged 60*, and *Share Aged 80+* are age-structure measures from the 1982 census.

Table A.2: Post-Admission Outcomes: Bureaucratic Rank, Controlling for Missing Rank Coverage

	First Assignment		Highest Position		Career Average	
	(1)	(2)	(3)	(4)	(5)	(6)
1712 Reform Intensity × Post	2.845**	2.516**	5.362**	4.019**	4.175**	3.507**
	(0.950)	(0.996)	(1.712)	(1.853)	(1.430)	(1.468)
Decade FE	✓	✓	✓	✓	✓	✓
Decade FE × Region FE	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓
Decade FE × Controls		✓		✓		✓
Missing-Rank Share Control	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	1.021	1.021	1.701	1.701	1.497	1.497
Observations	4806	4806	4806	4806	4806	4806
R-squared	0.461	0.462	0.423	0.424	0.447	0.448

Notes: This table re-estimates the bureaucratic-rank specifications from Table II after adding controls for missing bureaucratic-rank coverage. Columns 1 and 2 control for the prefecture–decade share of successful candidates with missing first-assignment rank; columns 3 and 4 control for the corresponding share with missing highest-position rank; columns 5 and 6 control for the corresponding share with missing career-average rank. Each observation is a prefecture–decade from 1650 to 1830, excluding 1710. Columns 2, 4, and 6 include controls: pre-reform population density, log distance to Beijing, agricultural suitability, predetermined quotas at lower-tier exams, ruggedness, and access to navigable rivers, all interacted with decade fixed effects. All specifications include decade fixed effects, prefecture fixed effects, and interactions between decade fixed effects and region fixed effects. Standard errors, clustered at the province level, are reported in parentheses. Stars use wild cluster bootstrap inference with Webb weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Post-Admission Outcomes: Quality Dilution Check by Exam-Rank Bin

	First Assignment		Highest Position	
	(1)	(2)	(3)	(4)
Exam-rank bin 0–0.2	-0.101**	-0.108**	-1.005***	-1.023***
	(0.041)	(0.038)	(0.046)	(0.048)
Exam-rank bin 0.2–0.4	-0.0826**	-0.0904***	-0.824***	-0.859***
	(0.029)	(0.029)	(0.069)	(0.065)
Exam-rank bin 0.4–0.6	-0.0588*	-0.0683**	-0.620***	-0.626***
	(0.029)	(0.030)	(0.072)	(0.073)
Exam-rank bin 0.6–0.8	-0.0285	-0.0386	-0.412***	-0.431***
	(0.030)	(0.032)	(0.046)	(0.046)
1712 Reform Intensity × (Rank bin 0–0.2)	0.126	0.0572	2.572	2.668
	(0.373)	(0.407)	(0.958)	(1.106)
1712 Reform Intensity × (Rank bin 0.2–0.4)	-0.599	-0.757	1.060	1.009
	(0.436)	(0.463)	(1.371)	(1.355)
1712 Reform Intensity × (Rank bin 0.4–0.6)	-0.269	-0.427	1.396	1.640
	(0.329)	(0.306)	(1.237)	(1.211)
1712 Reform Intensity × (Rank bin 0.6–0.8)	-0.133	-0.273	1.614^{+}	1.695**
	(0.295)	(0.350)	(0.492)	(0.429)
1712 Reform Intensity × Post	0.189	0.209	-1.514	-1.137
	(0.132)	(0.382)	(0.784)	(1.104)
Decade FE	✓	✓	✓	✓
Decade FE × Region FE	✓	✓	✓	✓
Province FE	✓	✓	✓	✓
Decade FE × Controls		✓		✓
Mean of Dep. Var.	3.228	3.228	5.226	5.226
Observations	4540	4540	4477	4477
R^2	0.044	0.068	0.087	0.107

Notes: This table estimates whether career outcomes by exam-rank bin changed after the reform. Each observation is a *jinshi*. The sample drops *jinshi* in the 1710s. Outcomes are the rank of the first assignment (columns 1–2) and the highest position attained (columns 3–4), measured on a 0–10 scale with higher values indicating higher rank. "1712 Reform Intensity" denotes $ReformIntensity_{prov} \times Post_t$. Exam rank is standardized to lie in $[0, 1]$ (higher is better). The omitted category is the top exam-rank bin 0.8–1.0, so coefficients on the rank-bin indicators are relative to top-ranked candidates, and coefficients on the interactions capture differential post-1712 effects by exam-rank bin relative to that top bin. "Controls" are the same time-invariant covariates used elsewhere in the paper, interacted with decade fixed effects. Standard errors, clustered at the province level, are reported in parentheses. For the overall reform-intensity coefficient and the reform-intensity-by-rank-bin interaction coefficients, stars are based on wild cluster bootstrap p-values with Webb weights. The rank-bin indicators themselves use conventional province-clustered inference. $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Post-Admission Outcomes: Bureaucratic Rank, Accounting for Exam Rank and First Assignment

	Dependent Variable		
	(1) Highest Position + Exam Rank	(2) Highest Position + First Assignment	(3) Highest − First (Promotion Gap)
1712 Reform Intensity × Post	-0.0848	-0.625	0.806
	(0.343)	(0.473)	(0.881)
Exam Rank	7.350***		
	(0.175)		
First Assignment		1.648***	
		(0.017)	
Decade FE	✓	✓	✓
Decade FE × Region FE	✓	✓	✓
Prefecture FE	✓	✓	✓
Decade FE × Controls	✓	✓	✓
Mean of Dep. Var.	1.591	1.591	0.665
Observations	4806	4806	4806
R-squared	0.827	0.920	0.298

Notes: This table estimates how the reform effect on career rank changes after accounting for exam rank and first assignment. Each observation is a prefecture–decade from 1650 to 1830, excluding 1710. In columns 1 and 2, the dependent variable is bureaucratic rank measured at the highest position attained. Column 1 controls for exam rank and column 2 controls for the rank at the first assignment. Column 3 uses the difference between highest-position rank and first-assignment rank as the dependent variable. All specifications include decade fixed effects, region-by-decade fixed effects, prefecture fixed effects, and decade-by-control interactions. Standard errors, clustered at the province level, are reported in parentheses. For the reform-intensity coefficient in columns 1–3, and for the added exam-rank and first-assignment coefficients in columns 1–2, stars are based on wild cluster bootstrap p-values with Webb weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Post-Admission Outcomes: Characteristics of the Posting Location

	(1) Distance to Border	(2) Distance to Capital	(3) Whether in Capital	(4) Whether Important	(5) Ming Tax Per Capita	(6) Ming Urban Population
1712 Reform Intensity × Post	-708.7**	563.6	0.0458	-0.00901	0.353	3.565
	(273.716)	(911.157)	(0.314)	(0.068)	(0.403)	(6.633)
Decade FE	✓	✓	✓	✓	✓	✓
Decade FE × Region FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Decade FE × Controls	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	878.459	1018.974	0.124	0.979	0.970	7.377
Observations	2795	2814	2814	2814	4155	2610
R-squared	-0.003	-0.005	0.014	0.010	0.853	0.008

Notes: This table estimates the impact of the 1712 reform on characteristics of the highest position held by a successful candidate. Each observation represents a successful candidate from prefecture $pref$ in period t. The posting-location measures are matched at the county level. From column 1 to column 6, the dependent variable is a position's distance to the border, distance to Beijing, whether a position is in the central government, the importance of the position, Ming tax per capita at the posting county, and Ming urban population in the location of the position, respectively. Ming tax per capita divides the conservative county allocation of Ming tax by 1600 county population from HYDE. All models include decade fixed effects, region-by-decade fixed effects, province fixed effects, and decade-by-control interactions. Coefficients are reported with standard errors, clustered at the province level, in parentheses. For the reform-intensity coefficient in each column, stars are based on wild cluster bootstrap p-values with Webb weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Post-Admission Outcomes: Conflict at Jinshi Posting Locations

	(1) 40–30 yrs before	(2) 30–20 yrs before	(3) 20–10 yrs before	(4) 10–0 yrs before	(5) 0–10 yrs after	(6) 10–20 yrs after	(7) 20–30 yrs after	(8) 30–40 yrs after
Panel A. Highest Position								
1712 Reform Intensity × Post	-0.0962	-0.201	-0.730**	-0.456	0.535**	-0.136	-0.0416	-0.0392
	(0.194)	(0.320)	(0.257)	(0.376)	(0.169)	(0.167)	(0.116)	(0.392)
Exam Decade FE	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE × Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE × Post Location Controls	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE × Origin Prefecture Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	0.030	0.035	0.043	0.038	0.046	0.031	0.042	0.087
Observations	1157	1157	1157	1157	1157	1157	1157	1157
R-squared	0.171	0.074	0.216	0.147	0.110	0.241	0.356	0.321
Panel B. First Assignment								
1712 Reform Intensity × Post	-0.224+	-0.00763	0.330	0.540+	-0.630	0.257+	0.0954	-0.547*
	(0.177)	(0.217)	(0.200)	(0.344)	(0.503)	(0.167)	(0.251)	(0.396)
Exam Decade FE	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE × Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE × Post Location Controls	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE × Origin Prefecture Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	0.015	0.027	0.023	0.044	0.047	0.020	0.020	0.059
Observations	596	596	596	596	596	596	596	596
R-squared	0.149	0.301	0.407	-0.090	0.151	0.127	0.390	0.606

Notes: This table reports the likelihood of conflict occurring at the location of a *jinshi* post within the decade window listed above each column, measured relative to the exam year. Panel A uses the location of the *highest* bureaucratic post attained; Panel B uses the *first* posting. *Interpretation for Panel A:* because peak rank is typically reached about twenty years after *jinshi* success (Appendix C.3.2), the 0–10 and 10–20 years–after columns may precede service in the highest post; the 20–30 and 30–40 years–after columns align more closely with outcomes during or soon after that tenure. The sample excludes *jinshi* from the 1710s and those posted to border or capital locations. The key regressor is reform exposure: the within–exam-region change in provincial *jinshi* share from 1680–1700 to 1710–1730, with the 1712 cohort excluded from the post-reform window, interacted with a post-1712 indicator. All specifications include exam-decade fixed effects, exam-decade×region fixed effects, prefecture fixed effects, and exam-decade interactions with birthplace and post-location controls. Standard errors are clustered at the province level and reported in parentheses. Stars use wild cluster bootstrap inference with Webb weights. $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Post-Admission Outcomes: Disaster Relief at Jinshi Posting Locations

	(1) 40–30 yrs before	(2) 30–20 yrs before	(3) 20–10 yrs before	(4) 10–0 yrs before	(5) 0–10 yrs after	(6) 10–20 yrs after	(7) 20–30 yrs after	(8) 30–40 yrs after
Panel A. Highest Position								
1712 Reform Intensity × Post	0.181	-0.101	0.445	0.330	-0.211	-0.649	-0.0703	-0.242
	(0.190)	(0.323)	(0.392)	(0.286)	(0.423)	(0.513)	(0.252)	(0.188)
Weather Controls	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE × Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE × Post Location Controls	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE × Origin Prefecture Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	0.118	0.118	0.137	0.172	0.173	0.165	0.132	0.122
Observations	1173	1173	1173	1173	1173	1173	1173	1173
R-squared	0.334	0.330	0.279	0.401	0.326	0.330	0.345	0.397
Panel B. First Assignment								
1712 Reform Intensity × Post	0.259	0.0905	1.098^{+}	0.295	0.121	-1.897^{+}	-0.884	-0.961
	(0.424)	(0.451)	(0.851)	(0.657)	(0.643)	(0.944)	(0.705)	(1.232)
Weather Controls	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE × Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE × Post Location Controls	✓	✓	✓	✓	✓	✓	✓	✓
Exam Decade FE × Origin Prefecture Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	0.119	0.108	0.141	0.169	0.180	0.177	0.131	0.155
Observations	611	611	611	611	611	611	611	611
R-squared	0.507	0.433	0.460	0.415	0.434	0.417	0.478	0.362

Notes: This table estimates disaster-relief activity at *jinshi* posting locations around the exam year. The dependent variable equals one if any disaster-relief activity is recorded at the *jinshi* posting location within the decade window listed above each column, measured relative to the exam year. Panel A uses the location of the *highest* bureaucratic post attained; Panel B uses the *first* posting. *Interpretation for Panel A:* because peak rank is typically reached about twenty years after *jinshi* success (Appendix C.3.2), the 0–10 and 10–20 years–after columns may precede service in the highest post; the 20–30 and 30–40 years–after columns align more closely with outcomes during or soon after that tenure. The sample excludes *jinshi* who passed in the 1710s and those holding *jinguan* (capital) posts. The key regressor is reform exposure: the within–exam-region change in provincial *jinshi* share from 1680–1700 to 1710–1730, with the 1712 cohort excluded from the post-reform window, interacted with a post-1712 indicator. All specifications include exam-decade fixed effects, exam-decade×region fixed effects, prefecture fixed effects, exam-decade interactions with birthplace and post-location controls, and weather controls. Weather controls are time varying and measured as the share of years within each decade that experienced severe droughts or famines in the relevant location. Standard errors are clustered at the province level and reported in parentheses. Stars use wild cluster bootstrap inference with Webb weights. $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Empirical Appendix

B.1 Additional Variables and Data Sources

(a) Pre-Existing Human Capital

To proxy for baseline educational strength, we measure *jinshi* per capita prior to 1650. Specifically, we count the total number of successful candidates between 1371 and 1650 in each locality and divide by population in 1600.[1] This captures long-run differences in educational infrastructure prior to the Qing era.

(b) Lower-Tier Exams and Quotas

Unlike the final metropolitan exam (which lacked province-specific quotas prior to 1712), lower-tier exams allocated fixed quotas across counties and prefectures. We collect these quotas from the *Imperially Established Institutes and Laws of the Great Qing Dynasty* for 1724–1851 and normalize by population at each level. We also incorporate historical adjustments to provincial exam quotas from 1645, 1660, 1696, 1711, and 1744. Further details on these lower-level exams and how they enter our regressions appear in Appendix C.2.

(c) Historical Population

We rely primarily on prefecture-level population estimates from the *Population History of China* by Ge and Cao (2001). For the period of interest (1650–1830), these estimates are available in 1776, 1820, 1850, and 1910. To fill intermediate years and cross-check coverage, we also incorporate the Historical Database of the Global Environment (HYDE) by Klein Goldewijk et al. (2017), which provides modeled population and land-use data at a 5-arc-minute resolution. In the current regressions, the core 1650–1830 prefecture panel uses a pre-reform population-density measure based on 1680, the 1905 specifications use a pre-1905 measure around 1910, and the 1982 specifications use a pre-1982 measure from 1820. Combining these sources allows us to derive consistent population densities at the province, prefecture, and county levels.[2]

(d) Agricultural Suitability

To approximate agricultural productivity, we use the *Caloric Suitability Index* (CSI) from Galor and Özak (2016), which measures the maximum caloric yield (kilocalories per hectare) attainable in 5-arc-minute grid cells. We focus on the post-1500 period to capture late-imperial agricultural conditions. For each province, prefecture, or county, we compute the mean CSI value across all relevant cells within its boundaries.

(e) Terrain Ruggedness

To measure topographical roughness, we use the Global Multi-Resolution Terrain Elevation Data 2010 (*GMTED2010*) from Danielson and Gesch (2011). Each cell covers 7.5 arc-seconds of longitude and latitude. For each cell, we take the difference between its elevation and that of the eight surrounding cells (squared), and then compute the square root of the sum of these

[1] Our results are robust to denominators in 1400 or 1500.

[2] See CHGIS (2016b) for GIS-based boundaries and Klein Goldewijk et al. (2017) for spatial population estimates.

differences. We average this ruggedness measure across all grid cells in a given administrative unit (province, prefecture, or county).

(f) Navigable Rivers

We construct a binary indicator for whether an administrative unit has access to any major navigable river. River data are drawn from the China Historical Geographic Information System (CHGIS, 2016b), and navigability is defined based on the classification by Matsuura (2009). The dummy takes a value of 1 if at least one major navigable river passes through (or borders) the province, prefecture, or county.

(g) Extreme Weather

Historical climate data are drawn from the *Distribution Gallery of Droughts and Floods in the Past Five Hundred Years of China* (China Meteorological Administration, 1981). The original sources are county gazetteers and official archives, which record droughts and floods for 120 meteorological stations from 1470 to 2000. For each year, the data assign a five-point Drought/Flood Index (DF-index) at each station, where 1 is severe flood, 2 is light flood, 3 is moderate conditions, 4 is light drought, and 5 is severe drought. We measure extreme weather as the share of years within each decade in which DF-index equals 1, 2, 4, or 5. To convert station-level information into prefecture-level variables, we use inverse distance weighting so that a prefecture-level measure is a distance-weighted average of nearby stations. We then obtain a prefecture-level decade measure of the share of extreme-weather years.

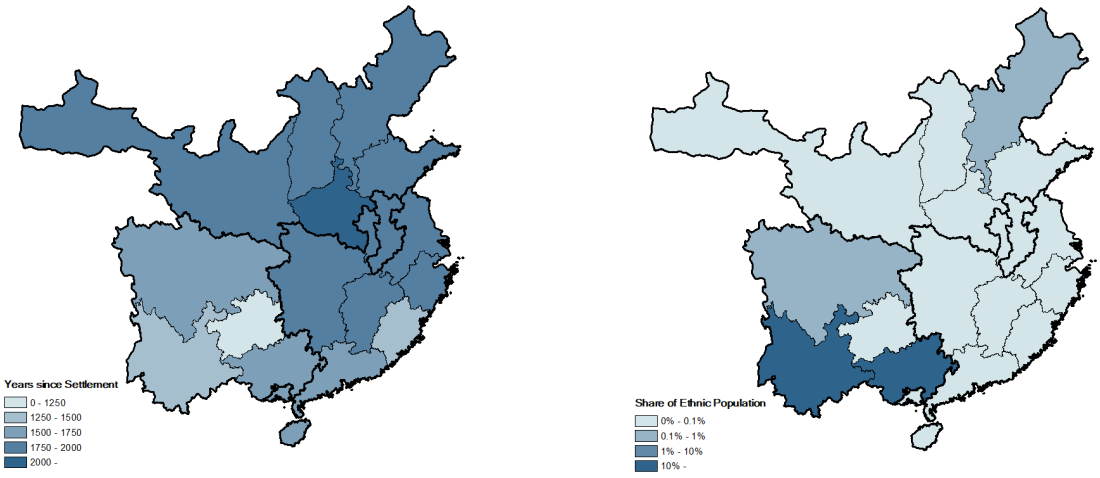
B.2 Measuring Reform Intensity: Additional Details

B.2.1 Ethnic and Frontier Regions

The 1712 reform favored provinces with a lower success rate in the metropolitan exam by construction. We find that reform-positive provinces, as shown in Figure I, tended to be in the hinterland with a high share of ethnic populations (Figure A.5b). Aside from the ethnic populations, these provinces were also home to a large number of Han Chinese who had only recently settled the frontier (Figure A.5a). After the reform, these provinces saw an increase in jinshi per capita in provinces with less exam success before (Figure A.1).

B.2.2 Artificial Provinces

To define a pre-1712 counterfactual within the three exam regions set in 1454, we compute reform intensity on units that are time invariant and contained within a single exam region (Kracke, 1957). These units follow historical examination administration. Jiangbei was treated separately and belonged to the Central exam region; accordingly, we construct an artificial province for Jiangbei and keep Jiangnan within the South exam region. In the northwest, although Gansu became a separate province in the late Ming, its examination administration remained subordinated to Shaanxi for much of the period, so we treat Shaanxi-Gansu as a single unit within the North exam region (Elman, 2000, 2013). In the middle Yangzi, Hunan and Hubei were administered together for examinations (the "Huguang" arrangement) well beyond the 1664 separation under the Qing; we therefore treat Hunan-Hubei as a single unit within the South exam region. Table A.8 provides the crosswalk.



(a) Years since settlement (b) Share of ethnic populations

Figure A.5

Table A.8: Crosswalk Between Historical Provinces and Artificial Provinces

Historical province (late Ming-early Qing)	Exam region (1454)	Artificial province used in baseline
Jiangbei[a]	Central	Jiangbei (artificial unit)
Jiangnan	South	Jiangnan
Shaanxi; Gansu[b]	North	Shaan-Gansu (merged unit)
Hunan; Hubei[c]	South	Hunan-Hubei (merged unit)
All others	–	One-for-one with historical province, constrained to lie within a single exam region

[a] Treated separately from Jiangnan in pre-1712 examination administration and belonging to the Central exam region. [b] Gansu became a province in the late Ming, but examination administration remained subordinated to Shaanxi for much of the period; we therefore use a combined Shaan-Gansu unit within the North region (Elman, 2000, 2013). [c] Although Hunan and Hubei became separate provinces under the Qing (1664), examination administration continued jointly as "Huguang" for an extended period; we use a combined Hunan-Hubei unit within the South region.

B.2.3 National-Share Measure

This subsection constructs a national-share analogue of the baseline measure of reform intensity.

Let $Share_{p,\text{nation}}^{1710\text{–}1730}$ denote province p's share of all national *jinshi* in the early post-reform period, and let $Share_{p,\text{nation}}^{1680\text{–}1700}$ denote its corresponding share in the pre-reform period. We define

$$\text{NationalShareGap}_p = Share_{p,\text{nation}}^{1710\text{–}1730} - Share_{p,\text{nation}}^{1680\text{–}1700}.$$

The measure captures the change in each province's share of national *jinshi* around the reform. As in the baseline construction, the post-reform share excludes the 1712 cohort from the total for the 1710s. The national-share measure retains the same short-run post-minus-pre structure as the baseline treatment but calculates each province's share relative to the national total rather

than relative to the total for its historical examination region.

B.2.4 Alternative Treatment: Pre-1712 Juren *Share Gap*

Figure A.1 provides the intuition for the alternative treatment in this appendix. The figure groups provinces by their pre-1712 within-region ratio of *jinshi* shares to *juren* shares and shows that provinces with relatively low final-stage shares before 1712 tended to gain after the reform. The appendix turns that intuition into a regression treatment by measuring the pre-reform within-region gap between lower-tier *juren* shares and final-stage *jinshi* shares.

The main reform-intensity measure uses the realized change in a province's share of *jinshi* success within its historical exam region before and after 1712. This measure directly captures how provincial *jinshi* shares changed after the reform, but it is not purely ex ante. After 1712, the number of *jinshi* from province p in exam e was closely tied to the number of candidates from that province who sat for the exam:

$$J_{p,e}^{post} \simeq \tau_e C_{p,e}^{post},$$

where τ_e is the exam-level pass rate. Thus the post-reform share of *jinshi* from a province is approximately its post-reform share of exam candidates:

$$Share_{p|r,J}^{post} \simeq Share_{p|r,C}^{post}.$$

The realized post-minus-pre *jinshi* share treatment can therefore be written as

$$\Delta Share_{p|r}^{realized} \simeq \underbrace{Share_{p|r,C}^{pre} - Share_{p|r,J}^{pre}}_{\text{ex ante candidate-pool share gap}} + \underbrace{Share_{p|r,C}^{post} - Share_{p|r,C}^{pre}}_{\text{candidate-share change}}.$$

The first term is the predetermined component: before 1712, did the province have many potential metropolitan candidates relative to its *jinshi* success? The second term captures changes in who sat for the exam after the reform.

Because actual pre-1712 metropolitan candidate counts are not available consistently, this appendix proxies the pre-reform candidate pool using realized pre-1712 *juren*. For province p in exam region r, the measure is

$$\text{JurenGap}_{p|r} = \frac{Juren_p^{pre}}{\sum_{q \in r} Juren_q^{pre}} - \frac{Jinshi_p^{pre}}{\sum_{q \in r} Jinshi_q^{pre}}.$$

The purpose is to capture ex ante pressure for reallocation: before 1712, did a province have a larger lower-tier candidate base than its final-stage *jinshi* share within the same regional pool? In that sense, this is the alternative treatment in the appendix, whereas the national-share exercise is a robustness check on how the short-run shift looks when written in national-share terms. Results for both are reported in Appendix B.3.1.

B.3 Access and Performance Under the 1712 Reform, 1650–1840: Additional Results

B.3.1 Admissions Results Using the National-Share Measure

Panel A of Table A.9 reports the admissions specification using the national-share measure. This measure compares a province's national *jinshi* share in 1710–1730 with its share in 1680–

1700, excluding the 1712 cohort from the post-reform side.

The estimated coefficients range from 0.205 to 0.225 and remain positive across specifications, although they are less precisely estimated once the time-varying controls are included. One reason may be that changes in national shares do not map exactly onto the institutional margin affected by the reform. Whereas the baseline measure captures a province's changing share relative to other provinces in the same historical examination region, the national-share measure also reflects changes in that examination region's share of all national *jinshi*. It therefore combines the within-region redistribution associated with the reform with an additional region-level component. This mismatch may introduce noise into the treatment measure and reduce precision. Even so, the estimates continue to indicate larger post-1712 increases in *jinshi* per capita in provinces that gained national share around the reform.

B.3.2 Admissions Results Using the Pre-1712 Juren *Share Gap*

Panel B of Table A.9 reports the same admissions specification using the pre-1712 *juren* share gap. This measure captures whether a province's share of the lower-tier candidate pool within its examination region exceeded its share of final-stage *jinshi* before the reform.

The estimated coefficients range from 0.0197 to 0.0203 and remain positive across specifications. Provinces with a larger predetermined gap between their lower-tier candidate base and final-stage success experienced larger increases in *jinshi* per capita after 1712. This pattern is consistent with the reform benefiting provinces whose supply of eligible candidates was high relative to their pre-reform success at the metropolitan stage.

B.3.3 Treatment of Shuntian Registrations

Shuntian Prefecture (which included the capital, Beijing) presents complications for our analysis. Many jinshi registered under Shuntian were not local residents, but sons or relatives of officials temporarily stationed in the capital. Because these registrations do not reliably reflect place of origin, we re-estimate our main regressions excluding all candidates with Shuntian registrations. The results are unchanged, confirming that these observations do not drive our findings.

B.3.4 Reform Exposure and Exam Rank at Admission

For transparency, we also report how reform exposure correlates with the exam rank of admitted *jinshi*. Interpreting this relationship is inherently difficult. Exam rank is observed only for candidates who clear the final-stage cutoff, and the 1712 quotas directly affect which candidates appear in that ranked pool. As a result, the coefficient captures a reduced-form shift in the observed rank distribution of admitted cohorts, combining any changes in preparation among candidates who reach the metropolitan exam with changes in selection into the ranked list induced by the quota rule.[3]

Table A.10 reports the corresponding prefecture-level difference-in-differences estimates for exam rank. Across specifications, the interaction between reform exposure and the post-reform indicator is positive. While we emphasize the interpretational limits above, the consistently positive sign is in line with the broader finding that expanded access in reform-beneficiary areas does not show up as weaker observable performance among admitted cohorts.

[3]In particular, the post-1712 ranking is constructed from a pool that is mechanically shaped by province-level quotas. The estimates therefore should not be read as identifying the reform's effect on underlying ability or on the score distribution of all examinees.

Table A.9: Access to the *Jinshi* Degree: National-Share Robustness and an Alternative Treatment

	Dependent Variable: Jinshi Per Capita		
	(1)	(2)	(3)
Panel A. National-share robustness check			
National-share measure × Post	0.2248**	0.2052+	0.2052+
	(0.088)	(0.120)	(0.118)
Decade FE	✓	✓	✓
Decade FE × Region FE	✓	✓	✓
Prefecture FE	✓	✓	✓
Decade FE × Controls		✓	✓
Mean of Dep. Var.	0.024	0.024	0.024
Observations	4806	4806	4806
R-squared	0.564	0.606	0.606
Panel B. Pre-1712 *juren* share gap (alternative treatment)			
Juren Gap × Post	0.0203**	0.0197**	0.0197*
	(0.009)	(0.009)	(0.009)
Decade FE	✓	✓	✓
Decade FE × Region FE	✓	✓	✓
Prefecture FE	✓	✓	✓
Decade FE × Controls		✓	✓
Mean of Dep. Var.	0.024	0.024	0.024
Observations	4806	4806	4806
R-squared	0.561	0.605	0.605

Notes: This table estimates admissions effects for two supplementary exercises. Each observation is a prefecture–decade from 1650 to 1830, excluding 1710. The dependent variable is the number of *jinshi* per 10,000 population. Panel A reports a national-share robustness check: the measure is a province's national *jinshi* share in 1710–1730 minus its national *jinshi* share in 1680–1700, with the 1712 cohort excluded from the post-reform side. Panel B reports the appendix's alternative treatment: "Juren Gap" is a province's pre-1712 share of realized *juren* within its exam region minus its pre-1712 share of *jinshi* within the same region. Column 2 adds population density, log distance to Beijing, agricultural suitability, lower-tier exam quotas, ruggedness, and navigable river access, all interacted with decade fixed effects. Column 3 repeats column 2 and uses wild cluster bootstrap p-values with Webb weights for significance. Standard errors, clustered at the province level, are reported in parentheses. $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.3.5 The Relationship Between Exam Rank and Career Outcomes

We document a clear link between exam performance and subsequent professional attainment. Table A.11 shows that a successful candidate's standardized exam rank is strongly correlated with the bureaucratic positions he ends up holding. Higher rank in the final-round examination predicts both first assignment and the highest position achieved over the course of a career. The table also reports specifications using indicators for Palace Examination class and specifications that condition on first assignment. The full ordinal ranking was organized into three broad classes, with Exam Class 1 the highest and Exam Class 3 the lowest. Figure A.6 reinforces this point visually. It plots a binned scatter of exam rank versus bureaucratic rank, fitted with a quadratic curve. The strong positive slope indicates that individuals who scored higher on the final exam

Table A.10: Exam Outcomes: Exam Rank

	Dependent Variable: Exam Rank		
	(1)	(2)	(3)
1712 Reform Intensity × Post	0.736***	0.579**	0.579**
	(0.175)	(0.206)	(0.206)
Decade FE	✓	✓	✓
Decade FE × Region FE	✓	✓	✓
Prefecture FE	✓	✓	✓
Decade FE × Controls		✓	✓
Mean of Dep. Var.	0.254	0.254	0.254
Observations	4806	4806	4806
R-squared	0.448	0.447	0.447

Notes: This table estimates the effect of the 1712 reform on exam rank. Each observation is a prefecture–decade from 1650 to 1830, excluding 1710. The dependent variable is the prefecture–decade average of standardized exam rank among successful candidates, scaled from 0 to 1, where higher values indicate better performance. Columns 2–3 add the full set of decade-by-control interactions; column 3 uses wild cluster bootstrap p-values with Webb weights for significance. Standard errors are clustered at the province level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

tended to secure more prestigious posts.

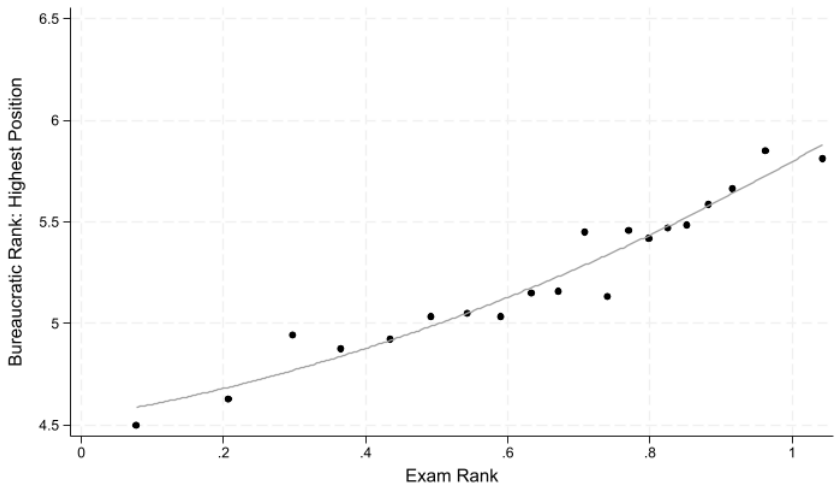


Figure A.6: The Relationship Between Exam Performance and Career Outcome

Notes: This figure shows a binned scatterplot of Exam Rank vs. Bureaucratic Rank, with a quadratic fit. Each point represents an individual. Controls correspond to column 4 of Table A.11. Standard errors are clustered by province.

There is, however, an important exception in the interpretation of first assignments by broad exam class. The Hanlin Academy was a highly prestigious track reserved disproportionately for the best-performing candidates, so a much larger share of Exam Class 1 *jinshi* entered Hanlin shortly after obtaining the degree. Some Exam Class 2 and 3 candidates also entered Hanlin, but a

Table A.11: Exam Performance and Career Outcomes

	Bureaucratic Rank							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	First Assignment		Highest Position		First Assignment	Highest Position		
Exam Rank	0.104**	0.110***	1.343***	1.380***			1.153***	
	(0.038)	(0.035)	(0.073)	(0.083)			(0.104)	
Exam Class 1					-0.0338	1.427***		1.244***
					(0.041)	(0.085)		(0.158)
Exam Class 2					0.111***	0.588***		0.471***
					(0.028)	(0.040)		(0.050)
First Assignment							0.0979*	0.101*
							(0.047)	(0.051)
Decade FE	✓	✓	✓	✓	✓	✓	✓	✓
Decade FE × Region FE	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Decade FE × Controls		✓		✓	✓	✓	✓	✓
Mean of Dep. Var.	3.228	3.228	5.226	5.226	3.228	5.226	5.570	5.570
Observations	4540	4540	4477	4477	4540	4477	3501	3501
R-squared	0.043	0.067	0.088	0.108	0.073	0.102	0.105	0.104

Notes: This table estimates the relationship between exam performance and career outcomes. Each observation is a successful candidate from province p in period t, excluding candidates in the 1710s. Exam Rank is a candidate's exact ordinal position in the Palace Examination, standardized from 0 (lowest) to 1 (highest). The full ordinal ranking was organized into three broad classes. Exam Class 1 is the highest class, Exam Class 2 is the middle class, and the omitted category, Exam Class 3, is the lowest class. Bureaucratic Rank is measured on a 0.5–9 scale, where higher values indicate higher rank. All specifications include decade fixed effects, decade-by-region fixed effects, and province fixed effects; the indicated columns add decade-by-control interactions. Standard errors are clustered at the province level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

larger share of them moved into ordinary bureaucratic posts instead. Because our first-assignment outcome is coded from the formal rank of the initial post, this can compress differences across classes. For example, Exam Class 1 candidates often began in Hanlin positions around the sixth or seventh *pin*, while a common non-Hanlin post for newly appointed *jinshi*, county magistrate (*zhixian*), was also a seventh-*pin* office. On this narrow formal-rank metric, the two tracks can therefore look similar at entry even though Hanlin appointments carried much greater prestige and better long-run prospects. Exam Class 1 therefore need not dominate Exam Class 3 on the narrow metric of formal first-assignment rank, even though it was more strongly associated with selection into the elite Hanlin track. Interested readers can find more detail in Appendix C.2.4 on appointment procedures, Appendix C.2.5 on the relationship between credentials and formal rank, and Appendix C.3.2 on how early placements shaped later careers.

B.3.6 Heterogeneity by Aggregate Exam Supply

A natural prediction is that the reform's effects should be larger in decades when the central government awarded more *jinshi* overall, since the post-1712 rule allocates *jinshi* proportionally to a province's exam takers and any reallocation therefore scales with the size of the nationwide pool. To assess this, Table A.12 interacts reform exposure with the nationwide total number of *jinshi* awarded in a decade (a raw total). The interaction term is positive and statistically significant in both prefecture–decade specifications, including in the specification that adds the

full set of decade-by-control interactions. Taken together, these results are consistent with reform effects that are larger in high-supply decades.

Table A.12: Access to the *Jinshi* Degree: Heterogeneity by Aggregate Exam Supply

	Dependent Variable: Jinshi Per Capita		
	(1)	(2)	(3)
1712 Reform Intensity × Post	0.0290	-0.109**	-0.109*
	(0.031)	(0.050)	(0.050)
1712 Reform Intensity × Post × Total *jinshi*	0.0000747**	0.000213***	0.000213***
	(0.000)	(0.000)	(0.000)
Decade FE	✓	✓	✓
Decade FE × Region FE	✓	✓	✓
Prefecture FE	✓	✓	✓
Decade FE × Controls		✓	✓
Mean of Dep. Var.	0.024	0.024	0.024
Observations	4806	4806	4806
R-squared	0.564	0.608	0.608

Notes: This table estimates whether the admissions response varies with aggregate exam supply. Each observation is a prefecture–decade from 1650 to 1830, excluding 1710. The dependent variable is *jinshi* per 10,000 population. Total *jinshi* is the nationwide number of *jinshi* awarded in a decade. Columns 2–3 add the full set of decade-by-control interactions; column 3 uses wild cluster bootstrap p-values with Webb weights for significance. Standard errors are clustered at the province level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.4 Characteristics of Admitted Candidates: Additional Results

The results above rely on the existing biographical sources and a cross-sectional post-1712 design. We also examine admitted candidates using a novel linked genealogy source constructed from FamilySearch records. For the subset of successful *jinshi* that can be linked to these genealogies, we observe age at exam success and a lineage-based family measure: the number of same-lineage relatives known to be alive at the exam year. The main advantage of this source is that it permits a candidate-level difference-in-differences design with pre-1712 observations. More broadly, it follows the approach in Xue (2025), which shows how crowd-sourced FamilySearch genealogies can be processed into structured data for quantitative historical research. The linked sample here is extremely small, so the results are best treated as suggestive.

To use the pre-1712 observations in this linked sample, we estimate a candidate-level difference-in-differences specification with region fixed effects, decade fixed effects, and the same geographic controls used in the admitted-candidates regressions. Table A.13 reports two versions. Columns 1–2 use all available pre-1712 linked observations. Columns 3–4 restrict the pre-period to 1650–1712 to mirror the standard panel used elsewhere. The restricted-preperiod design is especially thin, with only 11 pre-reform observations.

Compared with the post-1712 cross-sectional estimates reported above, the coefficient on age at exam success remains negative in both specifications but is no longer statistically significant. By contrast, the interaction between reform intensity and the post-reform indicator is positive for the number of relatives alive at the exam year in both specifications. This pattern provides

Table A.13: Genealogy-Based Difference-in-Differences Evidence on Admitted Candidates

	All Pre-1712 Observations		1650–1712 Pre-Period	
	Age at exam (1)	Alive relatives at exam (2)	Age at exam (3)	Alive relatives at exam (4)
1712 Reform Intensity × Post	-35.469	14.890***	-169.709	14.618**
	(36.058)	(3.496)	(158.179)	(6.387)
1712 Reform Intensity	7.038	-6.448	133.653	-3.255
	(30.660)	(4.705)	(163.034)	(9.210)
Region FE	✓	✓	✓	✓
Decade FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Mean of Dep. Var.	34.095	0.637	34.074	0.820
Observations	169	169	68	68
R-squared	0.130	0.218	0.128	0.368

Notes: This table reports candidate-level difference-in-differences estimates using the linked FamilySearch genealogy sample. The dependent variables are age at exam success and the inverse-hyperbolic-sine transform of the number of same-lineage relatives alive at the exam year. Columns 1–2 use all available pre-1712 observations, while columns 3–4 restrict the pre-reform period to 1650–1712. Standard errors are clustered at the province level and reported in parentheses. For the reform-intensity coefficient, stars use wild cluster bootstrap inference with Webb weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

suggestive evidence that candidates from larger or more extensively documented lineages may have benefited more from the reform. The linked sample is very small, however, so these estimates should be interpreted with caution.

B.5 The Distributional Consequences of The 1712 Reform: Additional Results

B.5.1 Competing Hypothesis: Wealth vs. Human Capital

One alternative explanation for the within-province pattern is that pre-1650 *jinshi* density captures local prosperity rather than educational capacity. The reform changed admissions at the province level, while our heterogeneity analysis asks which prefectures within a province were better positioned to respond to that change. Wealthier prefectures may have been able to support more extensive schooling, exam preparation, and travel to the capital, allowing them to benefit more when their province gained quota space or to withstand the loss of quota space more effectively. The estimated interaction with preexisting human capital could therefore partly reflect differences in local economic resources.

To examine this possibility, we use the caloric suitability index (CSI) as a proxy for long-run economic potential (Galor and Özak, 2016). We add interactions between CSI, reform exposure, and the post-reform indicator alongside the corresponding interactions with pre-1650 *jinshi* density. If the heterogeneous effects attributed to preexisting human capital primarily reflect local prosperity, including these CSI interactions should attenuate the human-capital coefficients.

Table A.14 reports the results. Across the full sequence of controls, the human-capital interactions remain positive and statistically significant. In the full-controls specification, the coefficient on *Gain* × *Pre1650HC* × *Post* is 0.0942, and the coefficient on *Loss* × *Pre1650HC* × *Post* is 0.0683. Adding the CSI interactions does not materially attenuate these estimates. The results therefore suggest that the stronger response of high-human-capital prefectures cannot be

Table A.14: The Distributional Consequences of the 1712 Reform: Controlling for Prosperity

	Dependent Variable: Jinshi Per Capita							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gain × Pre1650HC × Post	0.112**	0.0946**	0.102**	0.0941**	0.0919**	0.0941**	0.0942**	0.0942*
	(0.040)	(0.040)	(0.042)	(0.037)	(0.036)	(0.036)	(0.036)	(0.036)
Loss × Pre1650HC × Post	0.0694***	0.0681***	0.0723***	0.0687***	0.0685***	0.0695***	0.0683***	0.0683***
	(0.023)	(0.022)	(0.023)	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)
Gain × CSI × Post	-0.000122	-0.000118	-0.000147	-0.000210**	-0.000169	-0.000167	-0.000169	-0.000169
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loss × CSI × Post	0.000140	0.000150	0.000137	0.000182	0.000200	0.000194	0.000190	0.000190
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Decade FE	✓	✓	✓	✓	✓	✓	✓	✓
Decade FE × Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓	✓	✓
Decade FE × Controls	None	Pop. density	Distance	Quota	CSI	Ruggedness	Full	Full
Mean of Dep. Var.	0.024	0.024	0.024	0.024	0.024	0.024	0.024	0.024
Observations	4806	4806	4806	4806	4806	4806	4806	4806
R-squared	0.573	0.577	0.577	0.614	0.615	0.614	0.614	0.614

Notes: This table estimates distributional consequences while controlling for prosperity. Each observation is a prefecture–decade from 1650 to 1830, excluding 1710. The table adds interactions between caloric suitability and reform exposure to the distributional specification. All specifications include prefecture fixed effects, decade fixed effects, province-by-decade fixed effects, and lower-order terms. Columns 2–7 add controls sequentially; column 8 uses wild cluster bootstrap p-values with Webb weights for the human-capital interaction terms. Standard errors are clustered at the province level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

explained solely by differences in long-run agricultural potential.

B.6 Assessing Potential Channels

The discussion in Section IX interprets the results as being consistent with the reform raising the perceived returns to education and thereby encouraging greater investment by candidates and their families. Two alternative channels may also appear plausible: spillovers from previously successful officials and an expansion of formal educational infrastructure. This section examines whether either channel can account for the main results, with the aim of determining whether these alternative explanations can be ruled out.

B.6.1 Spillovers from Prior Top Officials

One possibility is that places that had previously produced high-ranking officials benefited from information, recommendations, patronage, or other connections within the bureaucracy. Such officials could have helped later candidates from their home regions succeed in the metropolitan examination or advance after entering government service.

To examine this possibility, Table A.15 augments the baseline specification with measures indicating whether a province or prefecture had produced a "top official," defined as an official who reached junior second rank or above, whose *jinshi* degree was obtained 10, 20, or 30 years earlier. These variables are dated by the official's examination year rather than the year in which the official attained senior rank. They therefore capture earlier examination cohorts that may have generated senior bureaucratic representation in subsequent decades. In the imperial ranking system, lower *pin* values denote more senior positions; our constructed rank score reverses this ordering so that higher values correspond to higher office.

Panel A examines subsequent *jinshi* per capita. The top-official measures, particularly those defined at the prefecture level, are positively associated with later *jinshi* production. Places associated with earlier top officials therefore tended to produce more successful candidates in subsequent decades. Conditioning on these measures, however, produces little change in the estimated coefficient on 1712 reform intensity $\times$ post.

Panel B examines subsequent bureaucratic rank. Here, the relationship is weaker and less systematic than in Panel A, but not entirely absent. Some of the lagged top-official measures, especially at longer lags, are positively associated with later career rank for candidates from the same place, although the estimates are only weakly significant. The estimated reform coefficient again remains stable in magnitude. Taken together, the results suggest that earlier top officials may have generated some local spillovers in admissions and perhaps modest spillovers in later career advancement, but they do not account for the reform's estimated effects on either *jinshi* production or bureaucratic rank.

B.6.2 Expansion of Educational Institutions

A second possibility is that the reform increased the local supply of education. Areas receiving larger gains from the reform may have responded by constructing additional academies, thereby expanding access to instruction and increasing the number or quality of candidates entering the examination system. We examine this possibility by testing whether reform intensity predicts the subsequent construction of new academies.

The Qing state did not operate a large-scale public schooling system comparable to those of modern states. Academies nevertheless constituted an important part of the educational infrastructure. Although many academies had historically been founded by private individuals or

Table A.15: The 1712 Reform with Lagged Top-Official Measures: *Jinshi* per 10,000 and Career Rank

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Jinshi per 10,000							
1712 Reform Intensity × Post	0.0854*	0.0872*	0.0865*	0.0856*	0.0856*	0.0851*	0.0850*
	(0.042)	(0.042)	(0.042)	(0.042)	(0.041)	(0.041)	(0.042)
Top officials in province, $t-10$		0.000888**					
		(0.000)					
Top officials in province, $t-20$			0.000808**				
			(0.000)				
Top officials in province, $t-30$				0.000283			
				(0.000)			
Top officials in prefecture, $t-10$					0.00407***		
					(0.001)		
Top officials in prefecture, $t-20$						0.00359*	
						(0.002)	
Top officials in prefecture, $t-30$							0.00202
							(0.001)
Decade FE	✓	✓	✓	✓	✓	✓	✓
Decade FE × Region FE	✓	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓	✓
Decade FE × Controls	✓	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	0.024	0.024	0.024	0.024	0.024	0.024	0.024
Observations	4806	4806	4806	4806	4806	4806	4806
R-squared	0.606	0.607	0.606	0.606	0.607	0.607	0.606
Panel B. Highest Position							
1712 Reform Intensity × Post	3.279*	3.301*	3.313*	3.306*	3.283*	3.274*	3.264*
	(1.757)	(1.771)	(1.721)	(1.737)	(1.740)	(1.742)	(1.736)
Top officials in province, $t-10$		0.0109					
		(0.022)					
Top officials in province, $t-20$			0.0250				
			(0.023)				
Top officials in province, $t-30$				0.0449^{+}			
				(0.032)			
Top officials in prefecture, $t-10$					0.0842^{+}		
					(0.051)		
Top officials in prefecture, $t-20$						0.0800	
						(0.074)	
Top officials in prefecture, $t-30$							0.0851^{+}
							(0.057)
Decade FE	✓	✓	✓	✓	✓	✓	✓
Decade FE × Region FE	✓	✓	✓	✓	✓	✓	✓
Prefecture FE	✓	✓	✓	✓	✓	✓	✓
Decade FE × Controls	✓	✓	✓	✓	✓	✓	✓
Mean of Dep. Var.	1.701	1.701	1.701	1.701	1.701	1.701	1.701
Observations	4806	4806	4806	4806	4806	4806	4806
R-squared	0.412	0.412	0.412	0.413	0.412	0.412	0.412

Notes: This table estimates whether lagged senior-official presence is related to admissions and career rank. Each observation is a prefecture–decade from 1650 to 1830, excluding 1710. Panel A uses *jinshi* per 10,000 as the dependent variable. Panel B uses the average rank of the highest position attained (0–10, with 0 the lowest). "Top officials" are officials at or above the second rank, measured 10, 20, or 30 years earlier at either the province or prefecture level. All specifications include decade fixed effects, region-by-decade fixed effects, prefecture fixed effects, and decade-by-control interactions. Standard errors, clustered at the province level, are reported in parentheses. For the reform-intensity coefficient in every column, and for the lagged top-official coefficient introduced in columns 2–7, stars are based on wild cluster bootstrap p-values with Webb weights. $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

local elites, the Qing government became more involved in their establishment, funding, and administration. The data allow us to distinguish among all newly constructed academies, gentry-sponsored academies, and academies sponsored by state officials.

Table A.16 reports the results. Across all three measures, the estimated coefficient on 1712 reform intensity × post is negative and statistically indistinguishable from zero. We therefore find no evidence that areas receiving larger gains from the reform experienced greater academy construction after 1712. The result holds both for academies associated with the gentry and for those associated with state officials.

These estimates provide little support for the view that the reform operated primarily through an expansion of formal educational infrastructure. They do not rule out changes in the quality or use of existing academies, private tutoring, lineage schools, or other forms of educational provision that are not captured by the number of newly constructed academies. They do, however, suggest that extensive-margin growth in academy construction was not an important response to the reform.

Table A.16: The Impact of the 1712 Reform on the Construction of New Academies

	Dependent Variable: Number of New Academies Per 10,000		
	(1)	(2)	(3)
	All	Gentry	State Official
1712 Reform Intensity × Post	-0.0141	-0.00654	-0.0109
	(0.011)	(0.005)	(0.008)
Decade FE	✓	✓	✓
Decade FE × Region FE	✓	✓	✓
Province FE	✓	✓	✓
Decade FE × Controls	✓	✓	✓
R-squared	0.093	0.083	0.079
Observations	4806	4806	4806
Mean of Dep. Var.	0.004	0.001	0.003

Notes: This table examines whether the 1712 reform influenced the construction of new academies across prefectures. Each observation is a prefecture-decade from 1650 to 1830, excluding 1710. The dependent variable is the number of newly constructed academies per 10,000 population. Column (1) reports results for all academies, column (2) for gentry-sponsored academies, and column (3) for state-sponsored academies. The main explanatory variable measures the interaction of the change in jinshi share between 1710–740 and 1680–710 (within region) with a post-reform dummy (after 1712). All models include decade fixed effects, region-by-decade fixed effects, province fixed effects, and decade-by-control interactions. Standard errors are clustered at the province level and reported in parentheses; significance is based on wild cluster bootstrap p-values with Webb weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Taken together, the two exercises provide little evidence that the main results are accounted for by prior-official spillovers or by the construction of new academies. This pattern is more consistent with the interpretation developed in Section IX, in which the reform changed the perceived returns to examination preparation and induced responses by candidates and their families. Because those private responses are not directly observed, however, the evidence should be interpreted as supporting rather than identifying that channel.

C Historical Appendix

C.1 The 1712 Reform

C.1.1 Spatial Inequalities in Exam Performance

Examination performance varied substantially across Qing provinces. The lower Yangtze, especially Jiangsu and Zhejiang, produced a disproportionate share of successful candidates. As one contemporary observed:

> Since the Southern Song established its capital at Hangzhou, the lower Yangtze valley has become a center of politics, culture, and commerce. Although Qing scholars surpassed those of earlier dynasties in intellectual achievement, most of the leading scholars came from Jiangsu and Zhejiang, the two provinces of the lower Yangtze.[4]

The lower Yangtze's economic prosperity supported dense educational networks and gave its candidates substantial advantages in examination preparation. Regional differences in economic and educational resources were already widely recognized by contemporaries. One writer described northern China as facing two related problems:

> Northern China now suffers from two problems: deserted land and a shortage of talent.[5]

Language barriers also contributed to poor examination performance in frontier provinces, particularly in the southwest. The southwestern provinces contained more ethnically and linguistically diverse populations than much of interior China, and many areas had been incorporated into the Qing administrative and educational system comparatively recently. Figure A.7 shows the distribution of languages across the eighteen inner provinces in 1820. Southwestern provinces contained substantial non-Han populations, including many Miao communities. According to one local gazetteer:

> The Miao people were initially unable to read Chinese. More recently, they have begun to recognize the value of schooling. Those of greater ability who became proficient in the language entered the imperial examinations under Miao registration.[6]

The introduction of province-specific quotas in 1712 created differences in effective admission thresholds across provinces. In principle, this could have encouraged candidates to seek registration in a less competitive province. Such strategic relocation was difficult, however. The government imposed strict rules governing a candidate's registered place of origin, and local residents frequently challenged the eligibility of newcomers. Between 1719 and 1804, for example, residents of Tongdao County in Jingzhou Prefecture repeatedly brought legal disputes against

[4] Shengmu Liu. 1998. *Five Books of Essays in Changchuzhai (Changchuzhai Suibi, Xubi, Erbi, Sanbi, Sibi, Wubi).* Vol. 5. Zhonghua Book Company, 104

[5] Yanwu Gu. 1994. *Compiled Annotations of Rizhilu (Ri Zhi Lu Ji Shi).* Vol. 17. Yuelu Press, 615

[6] *Gazetteers of Longshan County (Longshan Xianzhi).* 1818. Chap. Culture (Fengsu)

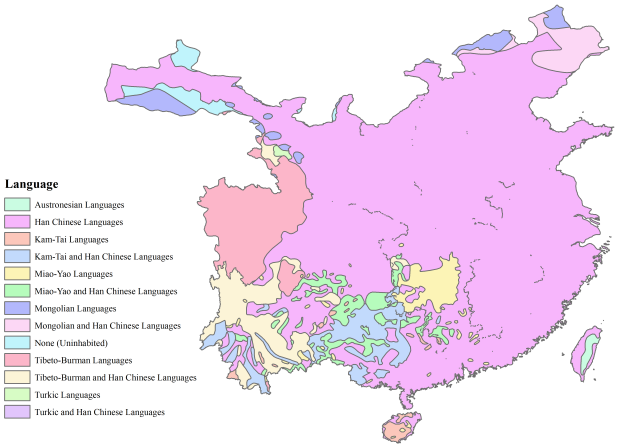


Figure A.7: Language Distribution across Imperial China's Eighteen Inner Provinces

Data Sources: Lawrence W. Crissman. 2012. *Digital Language Atlas of China.* V. V6; CHGIS. 2016a. *1820 Layers GBK Encoding.* V. V1.

eighteen families that had migrated there before 1644, alleging that they were improperly registered as local examination candidates.[7] In Xinning County, Guangdong Province, local residents similarly prevented members of more than 2,000 migrant families living along the coast from taking the examinations under Xinning registration, leading to years of litigation.[8] These cases illustrate both the incentives to obtain examination eligibility in a less competitive locality and the institutional and social barriers to doing so.

C.1.2 The Equity Doctrine in the Imperial Examination System

The equity doctrine has a long history in China and appears frequently in traditional political thought. Equity was treated as a core ingredient of social order alongside material prosperity. In the Analects, Confucius (551 B.C. to 479 B.C.) stated that rulers should not worry about having few people, but about unequal distribution, and should not worry about poverty, but about insecurity within their borders.[9] This emphasis on equity was echoed by later writers, who argued that even a large territory and population cannot sustain prosperity if resources are not distributed fairly.[10]

The equity doctrine also shaped debates over the imperial examination system. Degree holders were celebrated as representatives of local society, and imbalances in degree allocation were often

[7] Xiwei Liu. 2012. *On Imperial Examination Migrants in the Qing Dynasty (Qingdai Keju Maoji Yanjiu).* Central China Normal University Press, 243–246

[8] Xun Zhu. 2000. "Shaanxi Governor Zhuxun's Memorial to the Throne on Regulations of Immigrants and Aborigines to Take Exams in Shangzhou (Shaanxi Xunfu Zhuxun Wei Zhuoding Shangzhou Jiji Tuzhu Yingshi Zhangcheng Shi Zouzhe)." In *Historical Materials on Fake Registered Residence in the Imperial Examinations at Qianlong Emperor and Jiaqing Emperor (Qianjia Shiqi Keju Maoji Shiliao),* edited by Che Wang. The First Historical Archives of China

[9] Confucius. 2016. "Jishi." In *The Analecta of Confucius (Lunyu).* Beijing: Zhonghua Book Company

[10] Juyi Bai. 1994. "On Division of Cultivated (Yi Jingtian Qianmo)." In *The Compilation of Bai Juyi (Baishi Changqing Ji).* Shanghai Classics Publishing House

viewed as politically costly. As the examination system became more institutionalized in the Song period, officials proposed explicit geographic accommodation. In 1066, Sima Guang, a senior Northern Song official, argued for setting aside degrees for provinces in Northwest China:

> A total of roughly 2,000 people have obtained jinshi degrees in recent years, with 200 coming from Southern China and about half from the Capital. The average number of new jinshi degree holders per province is less than 100. Especially, only one or two people have received the degrees in recent years in the provinces of Shaanxi, Hedong, Hebei, Jinghubei, and Guangnandongxi...In certain provinces today, not even one person holds a degree. The implementation of a province-degree quota system is necessary. One jinshi degree should be given to every ten examinees from a province.[11]

This logic of geographic representation resurfaced in Qing-era discussions of the metropolitan exam. Before 1712, some provinces repeatedly recorded exam years with zero *jinshi*. Table A.17 documents these pre-reform episodes.

Table A.17: Pre-1712 Exams in Which a Province Produced Zero *jinshi*

	Fujian	Guangdong	Guangxi	Guizhou	Jiangbei	Sichuan	Yunnan
1652			×	×		×	×
1655			×	×			×
1658				×			×
1659			×	×			×
1661				×			×
1664				×			×
1667				×			×
1676	×		×			×	×
1679			×			×	×
1682			×	×		×	×
1685				×			
1688			×	×			
1691			×	×			×
1694					×		
1697			×				
1703		×					
Total	1	1	9	11	1	4	11

Notes: A shaded cell marked with a cross indicates that the province produced zero *jinshi* in that exam year. Blank cells indicate that the province produced at least one *jinshi*. The "Total" row counts the number of exam years in which each province produced zero *jinshi*.

The importance of the imperial examination system for political equilibrium should not be understated; some historians argue that the territorial integrity of the Ming and Qing empires benefited from institutionalized mechanisms that balanced representation across regions, whereas

[11] Guang Sima. 2009. "Proposal for a Province-Degree-Quota System in the Imperial Examinations (Qi Gongyuan Zhulu Quren Zhuang)." In *Comments on the Compilation of Essays by Sima Guang in Chronological Order (Sima Wengong Ji Biannian Jianzhu).* Ba Shu Press.

the Northern Song's military defeat against nomads can be related to the failure to do so.[12]

In a few instances, attempts to equalize access to *jinshi* degrees were supported directly by emperors. The emperor believed there was bias among officials in charge of the Imperial Exam because they were also from Southern China in 1397 when all of the *jinshi* degrees were awarded to examinees from that region. As a result, the emperor executed criminal suspects and only conferred *jinshi* degrees on Northern Chinese examinees that year.[13] These feelings are also expressed in Qianlong Emperor's comments on affirmative action policies. In 1788, an official named *Qian Feng* suggested that Qianlong Emperor abandon the 1712 reform. Qianlong's reply was:

> Some recurring issues might be resolved by Qian's suggestion. But, the government should choose its officials from a diverse pool that includes not just brilliant academics but also candidates from a vast geographic area. As the academic performance of the provinces varies, if Qian's proposal were to be implemented, the provinces of Jiangsu and Zhejiang would obtain more than half of the degrees, while the remote provinces at the border might not even obtain one. Qian should be blameless because he is from the province of Yunnan. He must have selfish intentions if he was from the provinces of Jiangsu or Zhejiang.[14]

C.2 An Overview of the Imperial Examination System

Candidates in the Qing Empire progressed through a sequence of examinations leading to several degrees. This section describes the main stages of the examination system and the credentials awarded at each stage.

C.2.1 Three-Tier System

Shengyuan Degrees The *shengyuan* was the first formal degree in the imperial examination system. To obtain it, a candidate generally had to pass three examinations: the County Examination (*xianshi*), followed by the Prefectural Examination (*fushi*) and the Academy Examination (*yuanshi*). Candidates who passed all three were admitted to the state-sponsored Confucian school associated with their county or prefecture and received the *shengyuan* degree. Before obtaining the degree, candidates were known as *tongsheng*, regardless of their age. Collectively, the three examinations were known as the Child Examination (*tongshi*) and were generally held twice within each three-year cycle.[15]

Juren Degrees Holders of the *shengyuan* degree could proceed to the Provincial Examination (*xiangshi*), which was held in the provincial capital once every three years. Successful candidates received the *juren* degree.[16] In principle, *juren* degree holders were eligible for government office, although their opportunities were more limited than those of *jinshi*.[17]

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[12] Zheng Jin. 1990. *The Imperial Examination System Institution and Chinese Culture (Keju Zhidu Yu Zhongguo Wenhua).* Shanghai People's Publishing House, 178

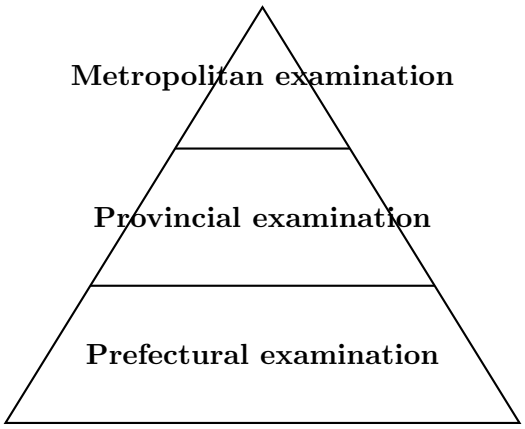
[13] Tingyu Zhang. 1974. *The History of Ming Dynasty (Ming Shi).* Zhonghua Book Company, 1697

[14] "Qianlong Wushisan Nian Sanyue Yiyou." n.d. In *Historical Records of Qianlong Emperor in Qing Dynasty (Qing Gaozong Shilu),* vol. 1301

[15] Yanliu Shang. 2003. *The Memoirs of Qing's Imperial Examination and Other Relevant Books (Qingdai Keju Kaoshi Shulu Ji Youguan Zhuzuo)* , 1–33

[16] *id.*, 48–56

[17] *id.*, 94–96



Metropolitan	**Venue** National capital **Degree holder** *Jinshi* **Competition** Within exam regions before 1712; within provinces after 1712 **Selected per cycle** About 250
Provincial	**Venue** Provincial capitals **Degree holder** *Juren* **Competition** Province-level quotas **Selected per cycle** About 1,100
Prefectural	**Venue** Prefectural capitals **Degree holder** *Shengyuan* **Competition** County- and prefecture-level quotas **Selected per cycle** About 51,000 through two examinations

Figure A.8: The Three-Tier Imperial Examination System

Notes: (1) The *juren* quota changed several times before 1712, fluctuating between 670 and 1,400. After 1712, it remained stable at approximately 1,100. (2) The number of *jinshi* selected varied across examinations. Between 1650 and 1840, the smallest cohort had 77 candidates and the largest had 372. On average, approximately 250 *jinshi* were selected every three years.

Jinshi Degrees Holders of the *juren* degree were eligible to sit for the Metropolitan Examination (*huishi*), which was normally held in the national capital once every three years, in the year following the Provincial Examination. Candidates who passed the Metropolitan Examination then proceeded to the Palace Examination, where they were ranked and awarded the *jinshi* degree, the highest regular credential in the imperial examination system.[18]

C.2.2 Verification of Residency

Candidates were generally required to take the imperial examinations in their registered place of residence. Before sitting for an examination, they had to establish their local eligibility through several forms of verification.

Documentary Proof of Registered Residence To prevent candidates from claiming a false place of registration, the Qing required several forms of documentary evidence. Two registration systems operated concurrently: one for taxation and another for local administration and public security. A candidate's examination residence was based primarily on his tax registration, but he was also required to provide records from the public-security registration system. Candidates could additionally be asked to document the location of their homes and ancestral graves.[19] A 1733 imperial regulation concerning candidates from Chongming and Zhaowen illustrates the importance attached to such evidence:

> Candidates from Chongming and Zhaowen in Jiangnan Province shall not be permitted to take the imperial examinations in Tongzhou until their residence in Waisha has been verified through evidence of houses and ancestral graves. Proof that they have merely arrived there is insufficient. If candidates take the examinations in Tongzhou without possessing houses or ancestral graves in Waisha, both their degrees and those of their guarantors shall be revoked.[20]

[18] Shang, 2003, 102–107

[19] Liu, 2012, 257–260

[20] Gang Kun et al. 1995. *Changes in Qing's Political System (Qinding Daqing Huidian Shili).* Shanghai Classics Publishing House, 239

Guarantors of Registered Residence Candidates were also required to secure guarantors who would attest to their local residence. Five candidates formed a group in which each member guaranteed the eligibility of the other four.[21] Each candidate also required the guarantee of a *shengyuan* degree holder, generally a stipend-receiving *linsheng*. A 1652 imperial regulation stated:

> Five candidates shall form a group, and a *shengyuan* degree holder of good standing shall sign as guarantor. At the roll call, the *linsheng* guarantor and the five members of the group shall verify one another and immediately report any fraud. If the five candidates conceal the deception, they shall be punished and the guarantor's degree shall be revoked.[22]

After 1792, county officials added a second *shengyuan* degree holder as an additional guarantor, reducing the risk that the first had been bribed or otherwise compromised. One contemporary account described the procedure as follows:

> At the prefectural examination, a second guarantor was required in addition to the first. Candidates invited them to serve as additional guarantors, and county education officials posted the list of designated guarantors outside their offices.[23]

County and prefectural officials were themselves responsible for verifying candidates' actual residences and could be held accountable when fraudulent registrations went undetected.[24] The examination regulations stated:

> Candidates must take the examination at their registered place of residence. If a false registration is discovered, the candidate's degree and that of his guarantor shall be revoked. The local officials responsible for the examination, education, and administration shall also be brought to trial.[25]

Accent Verification Officials could also use a candidate's accent to verify his claimed place of origin. Accent checks were first introduced in the *shengyuan* examinations of Wanping and Daxing Counties and were subsequently extended to other areas.[26] An 1844 regulation instructed:

> Following the established practice in Wanping and Daxing Counties, every county under Shuntian and Tianjin Prefectures shall rigorously examine candidates' accents and maintain records with the provincial education commissioner.[27]

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[21] 2012, 262–263

[22] Youming Huo and Haiwen Guo. 2009. *Collation and Annotation on the Imperial School Regulations (Qinding Xuezheng Quanshu Jiaozhu)*. Wuhan University Press, 77

[23] Shang, 2003, 10

[24] Liu, 2012, 271–277

[25] Dingbao Wang, Dong Qichang, et al. 2006. *Compilation of Each Dynasty's Literature on the Imperial Examination System (Lidai Keju Wenxian Jicheng)* , 2672

[26] 2012, 278–279

[27] Tedeng'e. 1844. *Regulations on the Ministry of Rites (Libu Zeli)*

A special censor responsible for examining candidates' accents was established in 1745.[28] The censor could also be punished for failing to identify fraudulent registrations:

> Candidates who falsely claim residence in Daxing or Wanping County shall have their guarantors' degrees revoked, be investigated and tried for bribery, and lose their eligibility to sit for the imperial examinations. The censor responsible for verifying accents shall also be brought to trial.[29]

C.2.3 Curriculum and Examination Preparation

Preparation for the imperial examinations can be divided into three broad stages: elementary literacy and moral instruction, study of the Confucian classics, and direct preparation for examination writing.

Children often began their education around age eight. This initial stage, known as *mengxue*, emphasized learning to read and write Chinese characters together with basic moral instruction. Teachers selected from a variety of elementary texts, among the best known of which were the *Three Character Classic*, the *Hundred Family Surnames*, and the *Thousand Character Classic*. According to the rules of one Qing private school, this stage generally lasted one or two years.[30]

The next stage centered on reading and memorizing the Confucian texts from which examination questions were drawn. These included the Four Books—the *Great Learning*, the *Doctrine of the Mean*, the *Analects*, and *Mencius*—and the Five Classics—the *Book of Songs*, the *Book of Documents*, the *Book of Changes*, the *Book of Rites*, and the *Spring and Autumn Annals*.[31] Additional reading varied across schools, teachers, and students, and could include works of literature, philosophy, and history.

The final stage focused on the writing conventions required by the examinations. Candidates learned to compose the highly structured eight-legged essay, a form that required extensive practice. There was no single standardized preparatory textbook. Instead, commercial publishers compiled and sold large collections of model examination essays. Because these collections offered direct preparation for the examinations, some candidates relied heavily on them at the expense of closer study of the Confucian classics.[32]

C.2.4 Appointment Procedures

Although the *jinshi* was the highest degree awarded through the imperial examination system, obtaining it did not lead all successful candidates directly into the same type of official appointment. Subsequent examinations and selection procedures divided *jinshi* into different career tracks. Candidates who performed well could enter the Hanlin Academy and advance relatively quickly, while others joined the Ministry of Personnel's appointment queue and sometimes waited years for an available post.

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[28] 2012, 278–279

[29] Kun et al., 1995, 257

[30] Chang Wang. n.d. *Regulations of the Free Private School (Yi Shu Gui Tiao)*

[31] Chang Wang. 2011. "Regulations of Youjiao School (You Jiao Shu Yuan Gui Tiao)," edited by Hongbo Deng. Zhongxi Book Company, 624

[32] Zongxi Huang. 1972. "The Imperial Examinations (Keju)." In *The Compilation of Modern China's Historical Materials (Jindai Zhongguo Shiliao Congkan),* edited by Yunlong Shen. Taibei Wenhai Press, 2108–2109

After receiving the *jinshi* degree, candidates took an additional court examination for selection into the Hanlin Academy.[33] Those selected entered the Academy as *shujishi*, while the remaining *jinshi* entered other appointment tracks administered by the Ministry of Personnel. This was the first major division in their subsequent careers.

The *shujishi* underwent approximately three years of further training in the Hanlin Academy before taking the Academy's graduation examination. Performance in this examination determined whether they remained in Hanlin posts or entered other branches of the bureaucracy. Those who left the Academy nevertheless generally received priority over candidates waiting in the ordinary Ministry of Personnel queue. As one contemporary account observed:

> When candidates from the Hanlin Academy or the Household Administration of the Heir Apparent were recommended for office, they received priority over those already waiting in the appointment queue. They were known as the "Tiger Class" because candidates recommended for posts such as county magistrate could be appointed ahead of the ordinary sequence.[34]

Selection into the Hanlin Academy was highly competitive. Across the Qing period, 6,065 of 26,848 *jinshi*, or about one-quarter, entered the Academy.[35] Hanlin service was also strongly associated with advancement to the highest offices. Contemporaries regarded Hanlin scholars as leading candidates for appointment as Grand Ministers of State,[36] and 64 of 138 Grand Ministers, or 46%, had previously served in the Hanlin Academy.[37]

Formally, official appointments were made by the emperor. Candidates recommended by the Ministry of Personnel therefore underwent an imperial audience before their appointments took effect.[38] The emperor could also make appointments directly. Hanlin officials enjoyed particular visibility because they assisted with court writing, scholarship, and consultation and therefore had more frequent contact with the emperor. This gave Hanlin service considerable prestige and could facilitate later advancement. As a contemporary observed:

> Although Hanlin officials held relatively low formal ranks, they were treated with the respect accorded to senior officials because they served the emperor directly in matters of administration.[39]

Entry into the Hanlin Academy thus provided an important fast track within the bureaucracy. *Jinshi* consequently regarded selection as a *shujishi* as a major distinction, even though those who were not retained after the graduation examination could still obtain appointments through other channels.

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[33]The three First Class candidates were appointed directly to Hanlin posts and did not need to compete for admission through this examination.

[34]Kejing Zhu. 1983. "Notes in Hanlin Academy (Hanlin Yipin Ji)." In *Two Volumes in Ming'an House (Ming An Er Zhi).* Yuelu Press

[35]Runqiang Li. 2007. *Qing's Jinshi Degree Holders and their Academic Culture (Qingdai Jinshi Qunti Yu Xueshu Wenhua).* China Social Science Press, 79

[36]Dezhao Wang. 1984. *Research on the Imperial Examination Institutions in Qing Dynasty (Qingdai Keju Zhidu Yanjiu).* Zhonghua Book Company, 56–57

[37]Jianhua Wu. 1994. "A Brief Analysis of Hanlin Bachelors in Qing Dynasty (Qingdai Shujishi Qunti Jianxi)." *Social Science Journal (Shehui Kexue Jikan),* no. 4, 107–115

[38]Zhenguo Zhang. 2010. "Researches on Qing's Civil Official Selection (Qingdai Wenguan Xuanren Zhidu Yanjiu)." PhD diss., Nankai University, 58

[39]Zhu, 1983

C.2.5 Examination Credentials and Official Rank

Examination credentials were closely associated with officials' subsequent rank. Table A.18 reports the degree composition of officials appearing in imperial interview records. *Jinshi* accounted for 34.5% of middle- and high-ranking officials, compared with 21.1% of lower-ranking officials. By contrast, *juren* accounted for 13.4% of middle- and high-ranking officials but 34.8% of lower-ranking officials. Here, lower-ranking positions refer to offices of the fifth rank or below.

Table A.18: Degree Distribution Across Official Ranks

Emperor	**Middle- and High-Level Officials (%)**			**Low-Level Officials (%)**		
	Jinshi	*Juren*	*Others*	*Jinshi*	*Juren*	*Others*
Yongzheng	33.9	9.6	56.5	16.2	31.2	52.6
Qianlong	30.9	14.9	54.2	25.1	44.5	30.4
Jiaqing	30.2	15.4	54.4	24.3	35.5	40.2
Guangxu	41.3	12.2	46.5	19.0	20.2	60.8
Total	34.5	13.4	52.1	21.1	34.8	44.1

Source: Zhiming Wang. 2016. *Research on Qing Officials' Career Path: Analysis based on Emperor's Interviewees' Resumes (Qingdai Zhiguan Renshi Yanjiu: Jiyu Yinjian Guanyuan Lvli Dangan De Kaozheng Fenxi)*. Shanghai Bookstore Publishing House, 168

Table A.19: The Proportion of *Jinshi* in Qing Crucial Positions

Position	**Total Number**	***Jinshi***	**Percentage**
Minister (*Shang Shu*)	744	339	46%
Senior Censors-in-Chief (*Zuo Du Yu Shi*)	430	221	51%
Governors-General (*Zong Du*)	585	181	31%
Governors (*Xun Fu*)	989	390	39%

Data Source: Dezhao Wang. 1984. *Research on the Imperial Examination Institutions in Qing Dynasty (Qingdai Keju Zhidu Yanjiu)*. Zhonghua Book Company, 58

Table A.19 further shows that *jinshi* were strongly represented in senior offices. They held 46% of ministerial posts and 51% of Senior Censor-in-Chief positions in the central government, as well as 31% of Governors-General and 39% of Governors in the provinces. These figures confirm that the *jinshi* degree was an important route to the upper levels of the Qing bureaucracy.

C.2.6 Superstitious Beliefs and Practices

The high stakes of the imperial examinations generated considerable anxiety among candidates and their families. Many sought reassurance or assistance through fortune-telling, temple worship, dream divination, and other religious practices. Examinees were therefore an important source of business for fortune-tellers, especially during examination periods. One contemporary account observed:

Fortune-tellers in the imperial capital profit most when the examinations are under

> way. Some seek immediate gains and tell every candidate, "You will pass." Examinees are pleased by such predictions, and many return to consult them. Others are more concerned with their long-term reputations and routinely predict failure. Because roughly 70% of candidates do not pass, these fortune-tellers are more often correct and come to be regarded as more skillful and honest. Some thereby acquire considerable fame and prosper from their trade.[40]

Candidates also sought relief from examination anxiety through temple worship and dream oracles:

> At the western end of the inner city in the imperial capital stands the Temple of the Two Gentlemen. Before the metropolitan examination, *juren* travel to the capital and pray there for oracles that appear in dreams. They place money in the hands of the two attendant statues. The temple's oracles are said to be especially accurate.[41]

Religious and moral texts also linked examination success to virtuous conduct. One Qing Buddhist collection, for example, contained eight stories in which the principal characters were rewarded with examination degrees for practicing sexual restraint.[42] Worship of the God of Literature was similarly widespread:

> Candidates in Taizhou County worship the God of Literature with great devotion. In addition to the two public schools maintained by the prefecture and county, more than ten halls are dedicated to him. Beginning on the eve of the third day of the second month, each site raises funds in its own way and stages music and song continuously for three days.[43]

C.3 The Life of a Qing Intellectual

C.3.1 Preparation for Imperial Exams

A Qing intellectual typically began preparing for the imperial examinations at a young age. Because only holders of the *shengyuan* degree could proceed to the Provincial Examination, the entry-level examinations known collectively as the *tongshi* were an important stage in the process. Preparation often lasted many years. Zhang (1955) estimates that candidates obtained the *shengyuan* degree at an average age of approximately 24, implying that many had already devoted more than a decade to examination study by that point.[44]

After obtaining the *shengyuan* degree, candidates could prepare for the Provincial Examination and the *juren* degree. This stage was widely regarded as especially difficult.[45] Zhang (1955)

[40]Kuo Shen. 2016. "Deception Tricks (Miu Wu Jue Zha Fu)." In *Brush Talks From Dream Book (Meng Xi Bi Tan).* Zhonghua Book Company

[41]Mai Hong. 1981. "The Temple of Two Gentlemen (Er Xiang Gong Miao)." In *Records by Yijian (Yi Jian Zhi).* Zhonghua Book Company

[42]Anshi Zhou. 2013. "For the Degree Seekers (Quan Qiu Gong Ming Zhe)." In *Compilation of Zhou Anshi's Essays (An Shi Quan Shu).* Unity Press

[43]Yitian Lu. 1984. "Notes in a Cold House (Leng Lu Za Zhi)." In *Collection of Qing Notes as Historical Materials (Qingdai Shiliao Biji Congkan),* edited by Zhonghua Book Company. Zhonghua Book Company, 120

[44]1955, 95

[45]Wang, 1984, 35

estimates that successful candidates obtained the *juren* degree at an average age of approximately 31.[46] Although *juren* were formally eligible for official appointment, opportunities were limited, and many continued to prepare for the Metropolitan Examination in hopes of obtaining the *jinshi* degree.

According to Zhang's estimates, successful candidates typically obtained the *jinshi* degree between ages 33 and 36.[47] He also estimates that *shengyuan* degree holders lived, on average, to around age 60.[48] These figures indicate that examination preparation occupied a substantial portion of an intellectual's life, with the longest period generally spent reaching the initial *shengyuan* degree.

C.3.2 Becoming a Professional Bureaucrat

After obtaining the *jinshi* degree, a candidate typically waited for an official assignment. Examination performance influenced both the type of initial posting and the speed of appointment. Candidates with higher Palace Examination ranks had better prospects of entering the Hanlin Academy or receiving posts in the central government, while lower-ranked candidates more often began in provincial or county offices.[49] Postings were also governed by avoidance rules (*huibi*) intended to limit conflicts of interest. These included restrictions on service near an official's native place, including an early eighteenth-century buffer of approximately 500 *li*; prohibitions on close relatives serving in connected offices; recusals from particular administrative or judicial matters; and regular rotation to prevent officials from becoming entrenched locally (Ch'ü, 1962; Hucker, 1985; Jones, 1994; Man-Cheong, 2004).

Once in office, officials were subject to regular assessments and time-in-rank requirements. Seniority structured the ordinary pace of advancement, while favorable evaluations could accelerate promotion and poor evaluations could delay or end a career. Three procedures governed official assessment during the Qing:

1. **Completion-of-Term Review (*kaoman*).** Officials were reviewed every three years within a nominal nine-year term. Responsibility for evaluation varied according to the official's rank and place of service. This system was phased out during the early Qing.[50]

2. **Capital Evaluation (*jingcha*).** Officials serving in Beijing underwent evaluations every three years. The emperor and senior ministers oversaw the assessment of higher-ranking officials.[51]

3. **Great Reckoning (*daji*).** Provincial and local officials were also evaluated every three years. Responsibility gradually shifted from direct imperial interviews to assessments conducted by provincial governors.[52]

[46] 1955, 126
[47] *id.*, 122
[48] *id.*, 96
[49] Elman (2000, chap. 8). See also Ho (1962) on the relationship between examination rank and initial appointments.
[50] Xue (2020, chap. 2).
[51] Xue (2020, chap. 3).
[52] Xue (2020, chap. 2).

Eligibility for promotion depended partly on time spent at a given rank, while an official's evaluation record determined whether he advanced at the normal pace, was held back, or, less commonly, received accelerated promotion. Service in difficult postings could count more heavily toward promotion eligibility, and service at court sometimes generated seniority more quickly than comparable provincial service.[53]

These formal procedures coexisted with personal connections and alternative routes into office. Patronage could influence transfers and appointments, particularly for senior positions. During the nineteenth century, the donation or purchase system (*juanna*) also expanded entry into lower- and middle-ranking offices. Advancement to the highest levels of the bureaucracy, however, generally required subsequent performance and sponsorship beyond the possession of a purchased title.[54]

Retirement and Local Standing Retirement rules changed over the eighteenth century. The formal age threshold was set at 55 in 1757, raised to 65 in 1768, and raised again to 70 in 1798, although enforcement retained some flexibility.[55] Retired officials commonly returned to their home localities as members of the gentry. Their status, administrative experience, and political networks allowed them to mediate between local communities and the state and to participate in the provision of local public goods.[56]

C.3.3 Non-Official Careers

Many candidates who did not advance through the higher examinations pursued occupations outside the formal bureaucracy. Their literacy, writing ability, and familiarity with official documents gave them access to several alternative careers, including informal government service, teaching, legal work, commerce, medicine, and the sale of calligraphy or paintings.

Informal Government Service The Qing maintained a small formal bureaucracy relative to the size of its population. One contemporary estimate placed the total number of officials in 1850 at only 26,408.[57] Educated men who failed the examinations could nevertheless work as clerks, secretaries, or other informal members of government offices. As one contemporary explained:

> Having failed the imperial examinations, we must pursue other occupations. Among them, informal government service is closest to the work of reading and writing, and many therefore choose it.[58]

Teaching and Tutoring Teaching was a common occupation for men whose examination preparation did not lead to higher degrees or official service. Their literacy, knowledge of the classical curriculum, and writing skills allowed them to work as teachers in private schools or as tutors for families and lineages. One biographical account describes a scholar who combined teaching with the sale of calligraphy:

[53] Xue (2020, chap. 2–3). On the broader combination of examination performance at entry and seniority in routine advancement, see Ho (1962) and Elman (2000, chap. 8).

[54] On donation practices and their limits as a route to senior office, see Zhang (2022) and the quantitative discussion in Chen et al. (2020).

[55] Erxun Zhao et al. 2020. "Regulations on Official Selection (Xuan Ju Zhi)." In *Draft of Qing History (Qing Shi Gao)*. Zhonghua Book Company.

[56] Zhang (1955, pp. 3–71).

[57] Qi Zhong. 1897. *Records of Trivial Matters in Qing Dynasty (Huang Chao Suo Xie Lu)*

[58] Huizu Wang. 1786. *Advice on Governance (Zuo Zhi Yao Yan)*

> He taught children and sold calligraphy to support his family in straitened circumstances.[59]

Legal Work Some unsuccessful candidates used their literacy and knowledge of official procedures to assist others with litigation. Such work required considerable writing ability, but it carried low social status because legal specialists were often portrayed as encouraging conflict and vexatious lawsuits. One contemporary described efforts to persuade impoverished scholars to leave this occupation:

> Some scholars were so poor that they made a living from litigation. I took pity on them and gave them money to support their families. I also arranged for them to continue their studies, paid their expenses, and urged them to change occupations.[60]

Other Occupations Unsuccessful candidates also entered commerce and other professions. In merchant families from Huizhou Prefecture, for example, sons who failed to advance through the examinations sometimes returned to the family business. One family record stated:

> After failing in his examination studies while young, he abandoned the scholarly path and entered commerce. The family business was subsequently entrusted to him.[61]

Others entered medicine:

> When young, he studied for the imperial examinations and became skilled at writing essays. At first, he did not appreciate how difficult the examinations were and failed repeatedly. He then inherited the family occupation and pursued a career in medicine.[62]

Some teachers supplemented their income by selling calligraphy or paintings. One local account describes a teacher whose earnings were insufficient to support his household:

> He worked as a teacher and supported a poor household. Because his teaching income was insufficient to cover daily expenses, he also sold paintings.[63]

Others supplemented teaching income by selling paintings:

> He worked as a teacher and supported a poor household. Because his teaching income was insufficient to cover daily expenses, he also sold paintings.[64]

[59]Shao Liang. 2002. "Essays in Liang Ban Qiu Yu An (Liangban Qiuyu An Suibi)." In *The Continued Imperial Collection of Four Divisions (Xu Xiu Siku Quanshu),* edited by Yun Ji et al., vol. 1263. Shanghai Classics Publishing House, 142

[60]Tingyao Gao. 1859. *My Life as an Official (Huan You Ji Lue)*

[61]Runzhuang Shi. n.d. "The Biography of Mr. Gongshu Cheng in the West of Xi County (Xi Xi Gong Shu Cheng Jun Zhuan)." In *Bibliographies of Cheng Family Southern Branch's Historical Records in Xin'an County,* edited by Mihong Cheng

[62]Letao Wang. 1999. *Doctors in Xin'an County (Xin An Yi Ji Kao).* Anhui Science & Technology Press, 111

[63]Tingji Zhong. 1992. "The Local Gazetteer of Sheng Lake (Shenghu Zhi)." In *The Compilation of China's Local Gazetteers: Villages and Towns (Zhongguo Difangzhi Jicheng: Xiangzhen Zhi Zhuanji),* vol. 11. Shanghai Bookstore Publishing House, 528

[64]*id.*, 528

Some also practiced divination:

> In addition to teaching students for a living, he studied divination. After closing his school, he traveled to Lizhou Prefecture with his divination books and stayed at the home of Liu Qingli, a *shengyuan* degree holder who worked as a fortune-teller.[65]

These examples show that examination preparation generated skills and occupational options extending beyond formal government service.

C.4 University Admissions in Republican China

This section surveys university admission policies in the Republican China. These policies can be divided into two time periods: (1) before 1938, the admission process was decentralized, and universities enjoyed considerable freedom in deciding how to conduct their own entrance exams; and (2) after 1938, as a result of the Second Sino-Japanese War, the admission process was largely under the control of the government.

C.4.1 University Admission Before 1938

C.5 University Admissions in Republican China

This section describes university admission policies during the Republican period. Before 1938, admissions were decentralized, and universities had considerable autonomy over their entrance examinations and admission decisions. After the outbreak of the Second Sino-Japanese War, the central government assumed a larger coordinating role, universities moved inland, and examination sites became more geographically dispersed.

C.5.1 University Admission Before 1938

Before 1938, universities exercised substantial autonomy over admissions. Each university could set its own examination questions, determine the dates and locations of its examinations, and decide how many students to admit. Government intervention was limited. As one contemporary observer explained:

> It is difficult to reach a common arrangement because each university has a different number of lecture halls, laboratories, and instructors. A uniform admissions process is therefore impractical. It would be easier to allow each university to determine how many students it could admit.[66]

Performance on entrance examinations was the principal academic criterion for admission. Some especially strong students could be admitted without sitting a university entrance examination, but they were still required to perform well on their middle-school graduation exam-

[65] The Palace Museum (Gugong Bowuyuan). 2011. *The Records of Qing's Literacy Inquisition: Amplified Version (Qingdai Wenziyu Dang: Zengding Ben).* Shanghai Bookstore Publishing House, 237

[66] Shuangqiu Tai et al., eds. 1936. "Proposition that University Admission in Each Department Should be Allocated According to Need (Daxue Geke Xuee Yiongfou Shi Xuyao Yufou Er Zhuojia Fenpei An)." In *Compilation of Propositions and Resolutions in Each Education Conference (Lijie Jiaoyu Huiyi Yijuean Huibian).* Institute for Educational Compilation / Translation (Jiaoyu Bianyi Guan), 18-19

inations.[67] Requirements concerning moral conduct and physical fitness were generally broad. Tsinghua University, for example, required applicants to be honest and not to have previously been expelled from school.[68] Applicants without serious health problems were ordinarily considered physically eligible for admission.[69]

These decentralized arrangements placed students from inland provinces at a disadvantage. Universities generally held entrance examinations near their campuses, making participation costly for students who had to travel long distances. Large differences in the quality of middle-school education also meant that students from inland provinces were often less well prepared for a uniform examination. At the Tenth Education Union Congress, the Ministry of Education proposed reserving university places for applicants from particular provinces, but the government did not adopt the proposal. The proposal stated:

> Several provinces had few, if any, newly admitted students at national universities. Most admitted students came from places with convenient transportation or from locations in which universities chose to hold their entrance examinations. Universities relied exclusively on examination scores, without recognizing that students from distant areas did not necessarily lack ability but had often received an inferior education and therefore performed less well. This educational disadvantage arose from a shortage of qualified teachers. Without immediate preferential treatment, educational quality in these areas would not improve, and their students would continue to have little chance of entering national universities. The Ministry of Education should therefore instruct national universities and colleges to reserve some admission places for designated provinces. Provincial governments should also provide stipends for students admitted from those provinces.[70]

C.5.2 University Admission After 1938

After the Second Sino-Japanese War broke out in 1937, universities moved inland and the central government assumed a larger role in coordinating admissions. Between 1938 and 1940, the Ministry of Education appointed committees to coordinate student selection and prepare examination questions.[71] After 1940, universities regained some authority to determine examination content and admission standards, although the government continued to oversee both.[72]

The government also divided the country into examination districts and established examination sites across a wider geographic area. This reduced the need for students from inland provinces

[67] "The 1938 Regulations on Recommendation for University Admission of Distinct Students in the Graduation Examination in Each Province and Prefecture and Graduates from National Senior High Schools (Ershiqi Niandu Ge Shengshi Gaozhong Huikao Chengji Youxiu Xuesheng Ji Guoli Ge Zhongxue Gaozhong Biyesheng Baosong Mianshi Shengxue Banfa)." 1938. In *Communique from the Ministry of Education (Jiaoyu Bu Gonggao),* vol. 10, 19-20

[68] Xuewei Yang, Xin Liu, et al., eds. 2003. *Compilation of Literature on China's Examination History: The Republic Era (Zhongguo Kaoshishi Wenxian Jicheng: Minguo).* Vol. 7. Higher Education Press, 42-43

[69] Tao Li. 2014. "A Study on Admission System of National Universities in Republic Era (Minguo Shiqi Guoli Daxue Zhaosheng Yanjiu)." PhD diss., Southwest University, 51

[70] Yang, Liu, et al., 2003, 43

[71] "The 1938 Regulations on Unified Admission of National Universities (Ershiqi Niandu Guoli Ge Yuanxiao Tongyi Zhaosheng Banfa Dagang)." 1938. In *Communique from the Ministry of Education (Jiaoyu Bu Gonggao),* vol. 10, 9-10

[72] Li, 2014, 40

to travel to universities located far from their homes. In 1942, the ten examination districts covered Chongqing, Chengdu, Kunming, Guiyang, northwestern China, Guangdong and Guangxi, Zhejiang and Jiangxi, Fujian, Hunan, and Hubei. Each district had a committee responsible for preparing examination questions and grading examination papers. A district committee could also administer admissions on behalf of universities located outside that district.[73] These arrangements made university entrance examinations more accessible to students in inland provinces and introduced some regional variation into the administration of admissions.

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