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ELITE RECRUITMENT AND POLITICAL STABILITY: THE IMPACT OF THE ABOLITION OF CHINA'S CIVIL SERVICE EXAM

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This paper studies how the abolition of an elite recruitment system—China's civil exam system that lasted over 1,300 years—affects political stability. Employing a panel data set across 262 prefectures and exploring the variations in the quotas on the entry-level exam candidates, we find that higher quotas per capita were associated with a higher probability of revolution participation after the abolition and a higher incidence of uprisings in 1911 that marked the end of the 2,000 years of imperial rule. This finding is robust to various checks including using the number of small rivers and short-run exam performance before the quota system as instruments. The patterns in the data appear most consistent with the interpretation that in regions with higher quotas per capita under the exam system, more would-be elites were negatively affected by the abolition. In addition, we document that modern human capital in the form of those studying in Japan also contributed to the revolution and that social capital strengthened the effect of quotas on revolution participation.

KEYWORDS: Elite recruitment, mobilization, revolution, social mobility, civil service exam.

1. INTRODUCTION

FOR OVER 1,300 YEARS, China used a civil service exam to recruit its elites, including high-level state bureaucrats and a much larger corpus of non-official gentry.[2] One of the most important institutions in Chinese history, the exam system influenced not only the competence of the bureaucracy but also the circulation of elites, the allocation of talent, and the perception of social mobility among average citizens (Weber (1915), Chang (1962), Qian (1982), Elman (2000)). In September 1905, however, the exam system was abruptly abolished, and the primary method for recruiting elites in the late imperial China

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[2]The gentry in China refers to those who passed the exams, a small share of whom became government officials. The remainder, even though they held no official positions, still enjoyed privileged status. In the mid-19th century, the gentry and their immediate family members accounted for around 2% of the population (Chang (1955)).

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was changed to a less transparent system. Although scholars have long argued that access to elite status plays a crucial role in determining social order (Pareto (1916), Mosca (1939), Acemoglu and Robinson (2008), North, Wallis, and Weingast (2009)), there currently exists little empirical research into this argument.[3] In this paper, we investigate one important political consequence of the change in elite recruitment: how the abolition of the exam contributed to revolution participation in the late 19th and early 20th century across China, thereby hastening the fall of the Qing dynasty in 1911. We also provide evidence to understand the mechanism underlying the link between the abolition of the exam and political instability.

One feature that facilitates analysis of the exam system is that it was governed by long-standing quotas. For the entry level of the exam, all 262 prefectures were assigned specific quotas that determined the number of candidates succeeding in the exam and entering the elite class.[4] The quota assignment did not change over time and hence did not respond to change in the population, development, or conflict propensity in a prefecture.[5] The persistence of these quotas is an example of "state simplifications" where the state lacked the capacity to implement a more complicated proportional system (Scott (1998)). This simplification was also reflected in the stepwise nature of the quota assignment for each prefecture, which comprised one quota for each county in the prefecture and an additional quota that could be shared among counties.[6] The values of these two parts followed a stepwise rule—8, 12, 15, and 20 accounted for over 70% of the cases, roughly according to the administrative scale. The persistence and lumpiness of these quota values provide significant regional variations, allowing us to use quotas per capita at the prefecture level to measure the exam system's influence and examine the impact of exam influence on participation in revolutionary organizations before and after exam abolition in 1905.

The abolition of the exam was driven by a combination of internal demand and external shocks. First, in the late Qing period, China was defeated in a series of wars against the West and forced to open its doors to foreign trade.

[3]One challenge in studying the role of elite recruitment is that such systems usually evolve gradually. For instance, in traditional European societies, elite status was mainly hereditary and was gradually changed to be based on meritocratic exams in the 19th century. This shift was underlain by many major social changes, including the Industrial and the French Revolutions, economic development, and political democratization. In the United States, the civil service system was gradually reformed across 100 years (Ujhelyi (2014)). This challenge is less critical in the case of the abrupt abolition of the exam system in China.

[4]A prefecture is the administrative level below the province. During the Qing dynasty, 18 provinces were located in the traditional agricultural area, with 10 to 20 prefectures under each province and 10 to 20 counties within each prefecture.

[5]The quota assignment only changed once in the Qing dynasty due to the fighting of the Taiping Rebellion (see Section 2.1 for more discussions on the quotas).

[6]At the county level, individuals could compete for the additional quota provided for the entire prefecture, meaning that the quota was binding at the prefecture level.

At this time, the traditional exam system, in contrast to modern and Western education systems, was considered by many intellectuals to be one of the roots of China's underdevelopment. For example, the exam sought out men who were obedient to their elders and able to recite the classics rather than those with political ability or technical knowledge of modern topics like engineering and science (Castrillon (2012)). The demand for the abolition of the exam became much stronger in 1905 when Japan won the Russo-Japanese War and gained control over Manchuria, a success attributed to the Meiji Restoration and Modernization. This success set an example for the Qing dynasty and facilitated the abolition of the exam (The Imperial Edict (1905), Franke (1960)). Along with the abolition, the dynasty attempted to switch to a Western-style education system. Government resources, however, were inadequate to provide an alternative for a large population that was investing or had invested in the exam (see Section 2.2 for more discussion).

Both historians and sociologists have conjectured that the abolition of the civil service exam hastened the dynastic collapse that soon followed (Franke (1970), Rozman (1982), Elman (2009)).[7] We focus on how the abolition contributed to participation in the revolutionary organizations that emerged in the late 19th and early 20th century. We choose this focus for two reasons: first, the revolution that transformed China from an imperial system into a republic was the most significant political change in China for over 2,000 years, and second, in such a non-democratic society, rebellions and revolutions are the main means by which average citizens can express discontent or demand redistribution. To measure revolution participation, we construct a data set of the prefectures of origin for 1,304 registered revolutionaries between 1900 and 1906 from the major revolutionary organizations.[8] The revolutionaries for whom we can get information were members of major revolutionary groups at the national level, who could motivate more participants at different local levels. We are concerned about whether the missing of information on revolutionaries is random. This concern matters only if the missing is correlated with quotas and varies before and after the abolition, which is not very likely. Nevertheless, as a further check for this concern, we collect a second data set to measure revolutionary activities. Based on reports from a major Japanese newspaper in 1911, we code the spatial distribution of the early uprisings in 1911 across China. The strong positive correlation between the cumulative number of revolutionaries

[7]Section 2.5 discusses qualitative evidence based on historical studies and diaries of individuals living in this period.

[8]We choose 1900–1906 as the study period due to the nature of the data. Before 1900, the majority of revolutionaries came from one province (Guangdong). In 1907, the largest revolutionary group divided into many groups, meaning that systematic data for the major revolutionary groups are only available until 1906. To have a longer study period, we also construct a county-level data set for Guangdong province in which we trace revolutionaries back to 1894, while our secondary revolutionary activity data set allows us to examine the incidence of revolutionary activities in 1911.

and the incidence of uprisings in a prefecture suggests that it is reasonable to examine both measures as outcomes. We also collect a set of observable prefecture characteristics to control for geographical characteristics, economic development, as well as for urbanization.

Using difference-in-differences as our baseline estimation strategy and comparing the impact of quotas on revolution participation before and after the abolition of the exam system, we find that a one standard deviation increase in the logged quotas (0.57 after controlling for the logged population) implies a six percentage point higher probability of revolution participation in the prefecture-level data between 1900 and 1906, about 40% of the mean participation probability in this period.[9] Combining yearly and monthly data, we find no pre-trends before September 1905. When examining the correlation between quotas and the 1911 Revolution, we find that a one standard deviation increase in the logged quotas (after controlling for the logged population) is associated with a four percentage point higher probability of early uprisings in 1911, about 25% of the mean uprising incidence.

Two major concerns about our baseline findings are whether the results actually reflect the part played by quotas and whether they are specific to exam abolition. For instance, prefectures with higher quotas might be more pro-conflict or different in political sentiment. Another possibility is that our findings may be capturing the impact of a general revolutionary upheaval in 1905 rather than the exam abolition. We address the first concern using the incidence of the 1899–1901 Boxer Rebellion as a placebo. Since the Boxer Rebellion was motivated by proto-nationalist sentiments and opposition to foreign imperialism, it was correlated with ideology and conflict propensity in a prefecture. However, it was uncorrelated with the exam system and we would not expect quotas to affect its incidence. In fact, we find no impact of quotas on the incidence of the Boxer Rebellion, indicating that our finding is unlikely to be driven by pro-conflict sentiment correlated with quotas. To check the second concern, we examine the impact of other historical events in this period and find that they did not exhibit a similar effect on revolution participation.

Even though the prefecture-level quota was persistent, the quotas could still conceivably be correlated with omitted variables whose impact differed before and after exam abolition. In particular, political networks correlated with the quota system might have more greatly influenced the selection of bureaucrats post abolition. To deal with this possibility, we employ two instruments for quotas based on geographical and historical features. First, exploring the stepwise feature, we use the number of small rivers (given the length of rivers) in a prefecture as an instrument. Given the same population, the number of small rivers in a prefecture affected the number of counties under its administration.[10] Since each county was usually assigned a positive quota according to the

[9]Throughout this paper, per capita refers to per 1 million population.

[10]Although this assumption that the number of small rivers affected the number of counties initially looks similar to that in Hoxby (2000), in fact, the channel is different. That is, rivers did

stepwise rule, the number of counties was positively correlated with the quota. These two links are supported by the data. We also conduct various placebo tests to make sure that the instrument is not correlated with other dimensions such as transportation, agriculture, or climate. Our historical instrument is the short-run change in exam performance before the quota system. Compared with the level of performance, short-run change was more likely to be driven by some random factors such as the emperor's idiosyncratic preference. It affected the assignment of quotas but did not affect human capital in the long run. The main finding using the difference-in-differences method still holds using the two instruments.

Why would the abolition of the exam contribute to revolution participation? We lay out four hypotheses around the major players in society. The first, "*state capacity*," assumes that exam abolition might signal or be driven by the deterioration in state capacity. The second, "*elite eligibility*," addresses the potential dissatisfaction of commoners who had hoped to become elites through the exam, while the third, "*elite resistance*," refers to the possible negative effects of abolition on existing elites who had already succeeded in the exam system. Finally, the "*modern human capital*" hypothesis considers the role of those who had switched to the modern Western education system and might be exposed to revolutionary and modernization ideology.

After providing direct and indirect evidence for or against each hypothesis, we find that the elite eligibility hypothesis appears most consistent with the data patterns. These would-be elites could have been negatively affected in two major ways: abolition changed their prospect of upward mobility and/or gave them more time to rebel (i.e., decreased their opportunity costs).[11] Although both channels could be at work, our analysis of the variation in prefecture-specific returns from the exam suggests that the impact of quotas post abolition was amplified when these returns were higher, which is more consistent with the prospects channel. We also provide quantitative evidence for the change in prospects and formalize this channel in a simple model, which predicts that social capital, by combating the collective action problem, strengthens the part played by prospects. We provide further evidence for this additional prediction.

We find no evidence, however, that the other hypotheses explain the role of quotas before and after exam abolition.[12] Nevertheless, in testing these hypotheses, we do uncover relevant new insights into revolutionary dynamics; for example, consistent with Huntington's (1968) modernization hypothesis, mod-

not usually serve as county boundaries in China and other Asian countries in which agriculture has been the dominant sector. Rather, even though every county would have liked to include rivers within its administrative boundaries, the number of rivers increased administrative costs, so in equilibrium, each county tended to include one or two small rivers within its boundary.

[11] On top of these incentives, the change in prospect or opportunity costs is likely to be correlated with dissatisfaction with the government. Disentangling such psychological factors from incentives, however, is beyond the scope of this paper.

[12] Our approach is not to cherry-pick evidence but to present as much evidence as possible.

ern human capital in the form of those studying in Japan contributed to the revolution.

Our study thus contributes to the growing literature on the importance of political institutions for development and stability (Acemoglu, Johnson, and Robinson (2001), Acemoglu and Robinson (2001), Besley and Persson (2011)). In particular, it provides empirical evidence for the importance of elite recruitment, which has been argued to affect political development (Seligman (1964), Acemoglu and Robinson (2008), North, Wallis, and Weingast (2009)).

We find that it was the would-be elites who were most affected by the abolition of the old system and contributed to the revolution. One reason for such a response is that the abrupt abolition changed their prospect of moving upward, a channel closely related to the prospect of upward mobility (POUM) hypothesis formalized in Benabou and Ok (2001) where the poor do not support high levels of redistribution because of the hope that they or their offspring may make it up the income ladder.[13] Little evidence on this hypothesis comes from authoritarian regimes, as it is unclear how redistribution can be realized without a democracy.[14] In this perspective, revolution can be thought of as a way of achieving redistribution. Our study thus sheds new light on the understanding of revolution participation.

To the best of our knowledge, ours is the first paper to systematically study the effects of China's historic exam system on the republican revolution, adding evidence to historical arguments such as those of Franke (1970) and Rozman (1982). Without doubt, the exam system is a key element in understanding the political and social order of China, on which there have been many historical but few economic studies. Our approach could therefore be usefully applied to other short- or long-run consequences of the traditional exam system.

The rest of the paper is organized as follows. Section 2 discusses the historical background, data, and qualitative historical evidence, after which Section 3 reports the empirical results. Section 4 presents further evidence to understand the mechanism, and Section 5 concludes the paper.

2. HISTORICAL BACKGROUND AND DATA

As background to our analysis, we first describe the exam system and explain its abolition and then discuss the revolution participation and uprisings in the critical year of 1911. This discussion includes not only a description of the quota and related data but also qualitative historical evidence on the link between exam abolition and revolution participation.

[13]Recently, Acemoglu, Egorov, and Sonin (2015) provided a dynamic model on the relationship between social mobility and political stability and demonstrated that there can be multiple equilibria.

[14]Existing empirical evidence for the hypothesis includes findings for the U.S. (Alesina and La Ferrara (2005a, 2005b)), Russia (Ravallion and Lokshin (2000)), and a set of OECD countries (Corneo and Gruner (2002)).

2.1. *The Civil Service Exam and the Assignment of Quotas*

The Structure of the Exam

Established in 605 AD during the Sui Dynasty (581–618), the civil service exam system was initially used on a small scale but subsequently expanded under the Song dynasty (960–1276). After being interrupted during the Mongol Yuan dynasty (1276–1368), the system became the primary channel for recruiting elites in the late imperial period (the Ming (1368–1644) and Qing (1644–1911) dynasties). The contents of the examinations were dominated by the Confucian texts (Elman (2000)).

Figure 1 illustrates the basic structure of this system in the Qing dynasty, which consists of three stages of exams. The entry exam was a prefecture-level licensing test (*yuankao*), held twice every three years in the prefecture's capital following annual primary testing in the county seat. The candidates who passed this exam, labeled "the literati" (*xiucai*), entered the lower class of gentry and were exempt from taxes and corporal punishment. This political status also offered them the opportunity to manage local affairs, become secretarial assistants to officials, or teach, three important income sources for Chinese gentry (Chang (1962)). The second level was a triennial provincial-level qualifying examination in the provincial capital, whose successful candidates were labeled

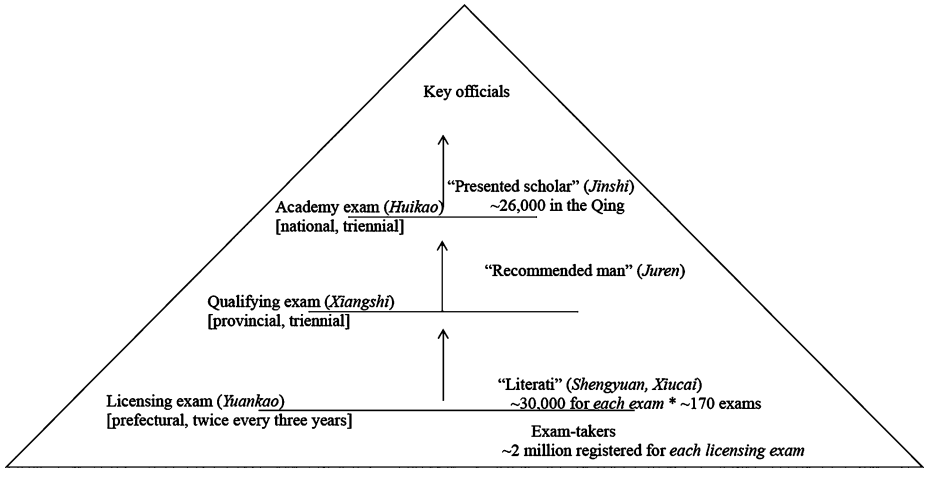


FIGURE 1.—The structure of the civil service exam. *Notes*: The number of prefecture-level exam graduates was governed by a prefecture-level quota. We code the prefecture-level data based on Kun et al. (1899). The number of national-level exam graduates was governed by a province-level quota. We code their prefectures of origin based on the lists of names and origins in Zhu and Xie (1980). The number of provincial-level exam graduates was governed by provincial-level quota. We do not have prefecture-level information on these graduates. The number of exam-takers comes from Elman (2013). Note that there was no limit on age or the number of attempts to take the exam.

"recommended men" (*juren*) and became eligible for positions as lower-level officials. The third was a national exam held in the capital, with re-examination to rank the candidates in the imperial palace. Many of these candidates, known as "presented scholars" (*jinshi*), were selected for high-level government positions. Because the entry exam, administered on a prefecture level, determined entry into the gentry class, it constitutes our analytical focus.

Eligibility and Entry Costs

The exam was, in principle, open to men from all socio-economic backgrounds and hence every male could in theory take the exam regardless of his background. The degree could not be inherited, so that children of the existing elites still needed to compete with the commoners in the exam. There was no limit on age or the number of attempts to pass the exam. In practice, exam-takers needed to invest time and money in preparing for the exam and forgo the opportunities to work, which hindered men from poor families from taking the exam. Wang (1989) estimated that an average family (of five individuals) needed land holdings of around 1.5 acres to afford the entry-level exam for one individual, while 30–40% of households in the late Qing period owned land above this threshold.[15] In addition, clans often pooled resources to support promising individuals from poor families. The population influenced by the exam system was further amplified by the fact that family members of the exam-takers also became involved in the investment (Wang (2013)).

Given the exam's importance in political selection, systemic institutions were set up to keep the system as fair as possible. For instance, all essays were transcribed before marking to prevent examiners from identifying students by their calligraphy, and the Emperor personally supervised the exam's final stage. In addition, the children of officials working in the central government were required to take the exam in their parents' home prefecture. Despite these institutions, however, corruption still happened, as illustrated by the Emperor's 1781 execution of 56 officials in Gansu province for selling examination degrees.[16] These facts partly explain why commoners perceived the exam as a relatively fair system for allocating elite status ex ante (Elman (2000)).

As a result of the exam system's openness and positive public perceptions, at least two million men—about 2.5% of the male population aged between 15 and 49 in the mid-Qing era—registered for each prefecture-level exam, with

[15]He only had information from one region in China. Naturally, the land concentration varied significantly across regions.

[16]Selling lower-level exam degrees to raise money for the government became a more important issue in the last decades of the dynasty but still did not stop commoners from investing in the exam system (Chang (1955)). In fact, the abolition was primarily intended to stop such investment. As stated in the Imperial Edict on the abolition in 1905, "as long as the system of examinations is not abolished, students will trust their good fortune for obtaining the highest degrees, and their desire for the difficult work of real betterment will be diverted. The people will follow their example, and the hope for the establishment of private schools will be small."

a total quota for each exam of around 30,000 (Elman (2000)).[17] Because of the large number of repeat candidates, we have no regional data on exam-takers. Conceptually, exploring the number of exam-takers is not helpful as it is endogenous to the probability of success governed by quotas per capita.

Quota Assignment and Related Data

The numbers of successful candidates at each exam level were controlled by a quota system, with quotas for the prefecture-level examination assigned at the prefecture level and those for higher-level exams assigned at the province level. Thus, in addition to allowing central authorities to recruit officials from different parts of the country, the quota system worked as an institutional means of confining and regulating the power of the elites (Elman (2000)).

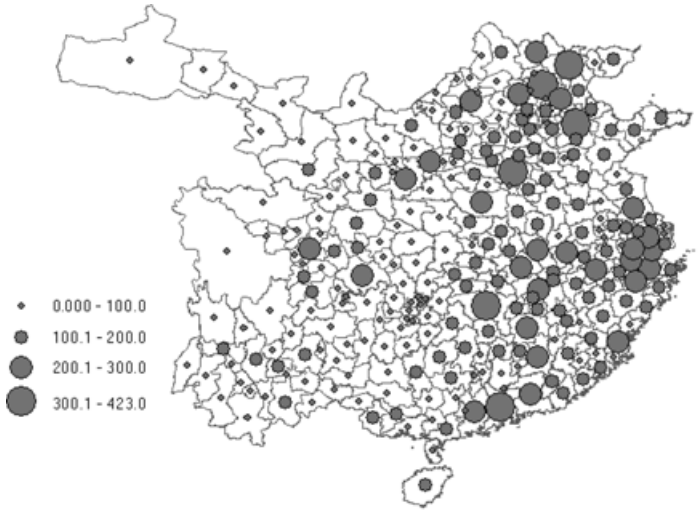
The quota for a prefecture consisted of two parts: each county in the prefecture received a quota and the prefecture as a whole received an additional quota that could be shared among counties. The quota was therefore binding at the prefecture level. The data on quotas come from the Imperially Established Institutes and Laws of the Great Qing Dynasty (Kun et al. (1899)). Figure A.1 of the Supplemental Material (Bai and Jia (2016)) gives an example of how the quotas were recorded.

There was no standard formula for the regional quota, but two features of the assignment deserve emphasis. First, the quota assignment was very stable during the Qing dynasty. The prefecture-level quota assigned in 1724 persisted until 1851, when the civil war (the Taiping Rebellion) started and the government increased quotas to encourage contribution to the fight. After the war, the revised quota assigned in 1873 persisted until the abolition of the exam. We collect the quota data for both the early Qing dynasty (1724–1851) and the late Qing dynasty (1873–1904). Our main analysis focuses on quotas in the latter period, and the results are robust to using quotas in the early period. Second, quotas assigned to each county and the additional quota for each prefecture followed a stepwise rule: the most common quota values are 8, 12, 15, and 20, roughly based on the administrative scale. As shown in Figure A.2 of the Supplemental Material, these four values account for over 70% of the observations. Both features are driven by the fact that the state lacked the capacity to implement a more complicated proportional system and had to simplify and regularize local practices.

Our empirical analysis focuses on the variations in quotas per capita at the prefecture level, while controlling for province fixed effects. Variations come from the lumpiness of quota values, the persistence over time, and variation in prefecture population. Province fixed effects only explain 30% of the variations in the quotas, leaving a large chunk for our exploration within provinces. Figure 2 maps the spatial distribution of quotas as well as quotas per million

[17]The total population size in the mid-Qing dynasty was around 300 million, 80 million of whom were the males aged between 15 and 50.

(a) Spatial Distribution of Quotas



(b) Spatial Distribution of Quotas per Million People

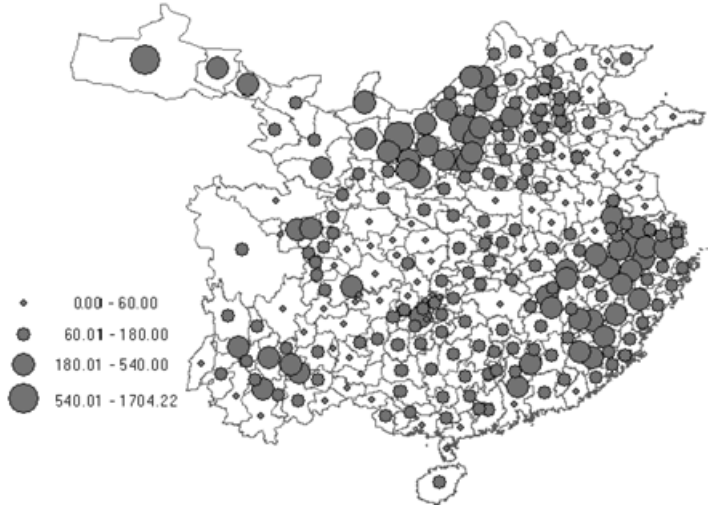


FIGURE 2.—Distribution of quotas across prefectures. *Notes*: This figure shows that there are great regional variations in quotas and quotas per capita. For example, province fixed effects only explain 30% of the variations in the quotas across prefectures.

individuals (based on the population in 1880), while Table I presents the data sources and summary statistics for these variables. For *each* prefecture-level exam, the mean and standard deviation of quotas are 114 and 76, whereas the mean and standard deviation of quotas per million individuals are 136 and 141.[18]

In addition, to measure the prospect for the lower-level elites, we collect information on the origin of candidates who succeeded in the highest-level exams (i.e., the presented scholars) and the prefecture origin of all the key officials. The number of presented scholars was controlled by a province-level quota. The data come from Zhu and Xie (1980) and Qian (2005) who listed the name and county of origin of all presented scholars and all key officials (higher than or equal to the level of vice-provincial governors). During the Qing dynasty, all 112 national exams produced around 26,000 presented scholars, whose number was inherently controlled by the province-level quotas. At the same time, over 90 percent of the 4,200 key officials held a presented scholar degree.

2.2. *The Abolition of the Exam*

As mentioned in the Introduction, after China's defeat in a series of wars against the West led to criticism of the civil service exam's outdated content and purpose, the government tried to reform the exam system (see Franke (1960) for a detailed discussion). First, in 1901, the format of the eight-legged (eight section) exam essay was relaxed, although the three-level exam system structure was retained. In late 1903 and early 1904, however, the Committee on Education submitted a memorandum urging abolition of the exam system, which received imperial support on January 13, 1904, with the understanding that the exam would be gradually abolished within the next decade.

However, on September 2, 1905, the Empress Dowager Cixi abruptly endorsed a memorandum ordering the abolition of the examination system at all levels. One important external factor behind the abolition was Japan's defeat of Russia in the Russo-Japanese War of 1904–1905, which was fought over the question of which country should have dominant influence in Manchuria. This war was the first major military victory of an Asian power over a European one in the modern era. Japan's success was attributed to the Meiji Restoration and Modernization, which set an example for the Qing dynasty and facilitated the abolition of the exam (Franke (1960)). As stated in the memorandum of the abolition, "those who have carefully studied the situation know that the secret of Prussia's victory over France and of Japan's over Russia lay in their primary

[18]Since there was no limit on the number of attempts, one way to understand the probability of success is to calculate $1-(1-2q)^n$, where q indicates quotas per capita and n indicates the times a man could attempt to take the exam in his life. If $n=10$, the probability is 0.27% for a man in the prefecture with the mean quotas per capita. At the household level, the probability of success depends on the number of male members.

TABLE I

SUMMARY STATISTICS FOR THE MAIN VARIABLES

Variables	Variables Definition	Data Sources[a]	Observations	Mean	S.D.
Revolutionaries	Having or not	1, 2	1,834	0.157	0.364
	# revolutionaries	1, 2	1,834	0.711	3.276
Early uprisings in 1911	Incidence of early uprisings in 1911	3	262	0.160	0.367
Measures of the exam	Quotas	4	262	113.771	75.728
	ln(Quotas)	4	262	4.441	0.891
	# presented scholars	5	262	95.977	146.596
	# key officials (pre-1905)	6	262	15.130	29.605
Baseline controls	ln(Population in 1880)	7	262	13.620	1.075
	ln(Area)	8	262	9.336	0.770
	Treaty port	9	262	0.115	0.319
	Small city	10	262	0.198	0.400
	Middle city	10	262	0.122	0.328
	Large city	10	262	0.038	0.192
	Major river	8	262	0.618	0.487
	Coast	8	262	0.134	0.341
Instrumental variables	# small rivers/river length	8	262	0.886	0.436
	River length	8	262	6.847	0.713
	Δ(Presented scholars before 1425)	5	262	0.377	0.727
Placebo tests	Incidence of the Boxer Rebellion	11	262	0.099	0.300
	Important in transportation (prefecture)	12	262	0.615	0.488
	Important in transportation (county)	12	262	0.380	0.300
	Fox millet suitability	13	262	2.877	1.334
	Wetland rice suitability	13	262	1.991	1.075
	Sweet potato suitability	13	262	2.622	0.992
	Climate shocks	14	262	0.063	0.092
	Basin HHI	8	262	0.608	0.243

(*Continues*)

TABLE I—*Continued*

Variables	Variables Definition	Data Sources[a]	Observations	Mean	S.D.
Modern human capital	# domestic modern firms	15	1,834	0.097	0.573
	# oversea students in Japan	16	1,834	0.793	2.725
Social capital	Language fragmentation index	17	262	0.086	0.164
	Language polarization index	17	262	0.162	0.299
Measures of importance	Land tax per capita in 1820	18	257	0.080	0.083
	Provincial capital	8	262	0.069	0.253
	Important in business	12	262	0.760	0.428
	Difficult to tax	12	262	0.267	0.443
	High in crime rates	12	262	0.676	0.469
Information/foreign influence	Newspapers per capita in 1911	19	262	0.002	0.011
	Foreign enclaves	9	262	0.065	0.247

[a]*Data Sources*:
1: Chang (1982).
2: Luo (1958).
3: *Tokyo Daily News* (Tokyo Nichi Nichi Shimbun), November 3, 1911.
4: Kun (1899).
5: Zhu and Xie (1980).
6: Qian (2005).
7: Ge (2000).
8: Harvard Yenching Institution (2007).
9: Yan (1955).
10: Rozman (1973).
11: The Boxer Protocol (1901).
12: Liu (1993).
13: FAO (2012).
14: The State Meteorological Society (1981).
15: Chang (1989).
16: Shen (1978).
17: Wurm, Li, Baumann, and Lee (1987).
18: Liang (1981).
19: Shi, Yao, and Ye (1991).

schools; in fact, the root of prosperity and strength is in the establishment of schools, and in this respect it is now only China that lags in the rear" (The Imperial Edict (1905)).[19]

As also pointed out in the memorandum of the abolition, the resources of the government were not adequate for the opening of enough modern schools and there were no teachers that could teach modern subjects. Even with universal modern schools, it required more than 10 years before men of talents "of the modern sort" could be produced. Yet any postponement in the abolition would simply delay the availability of talent even more, and pressure from strong neighbors, especially Japan, made any delay even more costly (The Imperial Edict (1905)).

Along with the abolition, the dynasty attempted to switch to a Western-style education system.[20] The intention of the reform was to modernize China. However, the modern system favored the existing elite more than the previous exam system. First, without an open exam to compete for the elite status, the incumbents could have more influence on elite recruitment (Spence (1990)). Second, it was also easier for existing elites to adapt to the new system. The modern schools were more expensive and it was difficult for commoners to afford modern education. For instance, Wang (1960), in a study of mobility changes during this transition, pointed out that the new system limited the higher education opportunity to a small group of men from official and professional families, while under the old scheme, a scholar with limited financial resources had a good chance of succeeding. For commoners, the abolition implied an interruption in the link between their exam performance and political status.

Also important for our study is the question of whether exam abolition reflected a revolutionary upheaval in China at the time. For example, the Russo-Japanese War might have had a greater effect on regions with higher quotas. Hence, in Section 3.3, we conduct several placebo tests to show that our findings are driven by exam abolition rather than other events in this period.

2.3. *Revolutionaries and the Early Uprisings in 1911*

In the 1890s, underground anti-Qing groups, with the support of Chinese revolutionaries in exile, tried to overthrow the Qing dynasty. These groups arose mainly in response to the decline of the Qing state, which had proven ineffective in its efforts to modernize China and confront foreign aggression. The opposition was exacerbated by ethnic resentment against the ruling Manchu

[19]See Ferguson (1906) for an English translation of the memorandum.

[20]The government also tried to provide alternative channels for the existing elites. These options were very limited and restricted to those who got degrees at the provincial level and national level. The related government document on these options, "Regulations on the Options for the Degree-Holders," was issued in 1906. Tsing (2007) provided a useful explanation of why these regulations did not mitigate the shock of the abolition.

minority. The earliest revolutionary organizations were founded outside of China, including Sun Yat-sen's *Xingzhonghui* (Revive China Society), which was established in Honolulu in 1894 and spread to Hong Kong and Guangzhou in Guangdong province. Chang (1982), drawing on member rosters disclosed after the revolution together with biographies and memoirs, provided useful background information on six major groups in existence from 1900 to 1906, as well as lists of registered revolutionaries. Panel (a) of Figure A.3 of the Supplemental Material describes the establishment timing of the six groups. Because the largest group, the Chinese Revolutionary Alliance, splintered into many groups in 1907, systematic data on these major revolutionary groups are only available until 1906.

Although these revolutionaries launched many small-scale uprisings, most were repressed by the government, until the successful Wuchang Uprising in Hubei Province on October 10, 1911, turned the tide and many other uprisings broke out across China. The revolution ended with the abdication of the Last Emperor, Puyi, on February 12, 1912, which marked the end of over 2,000 years of imperial rule and the beginning of China's republican era.[21]

We are not assuming that the main aim of the revolution was to restore the exam system. It is more reasonable to think that the exam hopefuls were mobilized to join the existing anti-Qing revolution groups to pursue a different career path once the exam channel was destroyed.[22] However, the importance of the exam system in political selection was well understood by the revolutionary leaders. In 1906, Sun Yet-sen unveiled his "five-power" constitution for his government if the revolution succeeded, adding two Chinese institutions—the recently scrapped examination system and the censorate—to the legislative, executive, and judicial powers (Schiffrin (1980)). The examination system was later institutionalized as the "Examination Branch" in the Republic of China including Taiwan today.

Data on the Origins of the Revolutionaries

Based on Chang's (1982) listings of the revolutionaries' names, counties of origin, and year of joining the organization, we track the origins of 1,304 reg-

[21]The new republic was by no means a well-functioning democracy. According to the Polity score, although the republican period had better scores than the previous (pre-1911) dynasty and the subsequent People's Republic of China (post-1949), between 1980 and 2000, China only earned a positive score once, in 1912.

[22]One might wonder why these people indoctrinated in Confucian values (such as the emphasis of obedience and loyalty) could turn to anti-government revolutionaries. One factor is that the Qing dynasty was ruled by Manchus who were considered non-Chinese. The anti-Manchu attitude did not necessarily contradict with the Confucian values. More importantly, the success of the indoctrination was closely related to the career opportunity provided by the exam system. Harrison (2001) discussed more on the importance of employment opportunity in shaping the ideology in this period. In fact, even revolutionaries like Sun Yat-sen hoped to get a career within the government first (Schiffrin (1980), Harrison (2001)).

istered participants and construct a data set of prefecture-level revolutionaries across China between 1900 and 1906.[23] The records for the Chinese Revolutionary Alliance include date of enrollment (see panel (b) of Figure A.1 of the Supplemental Material for an example) whereas those for the other five groups include only yearly information. Because, at this time, the revolution was spreading across the country, the participants with identifiable origins came from 152 prefectures (in 17 out of the 18 provinces), with the lowest share (1.4%) from Shaanxi in the west and the highest share (11.93%) from Hubei in the south. We report related summary statistics in Table I.

In our empirical analysis, we test for both the presence of participants and their number. However, one obvious concern with the quality of our data is whether the earlier records are less precise than the later ones. To address this issue, we complement the prefecture-level data set with a 1894–1906 county-level data set for Guangdong province, from which the majority of pre-1900 revolutionaries originated. Another important concern is the selection of registered revolutionaries, even though such selection would only matter to our analysis if it were systematically correlated with quotas and differed before and after exam abolition, which seems unlikely. As a validity check of the data, we collect a second data set on the uprisings in 1911 from a Japanese source.

Data on the Early Uprisings in 1911

The country-wide insurrection that quickly followed the successful Wuchang Uprising was closely followed by Japanese newspapers, and on November 3, 1911, the *Tokyo Daily News* (Tokyo Nichi Nichi Shimbun) provided a detailed map on the incidence across China. We code this information as a dummy variable indicating whether any uprisings occurred in a given prefecture in 1911. Because this information only includes the early uprisings, it is reasonable to think of them as the immediate echo of the Wuchang Uprising; a short burst by existing revolutionaries rather than a slow diffusion of a movement. Hence, if there were more revolutionaries in a prefecture, the probability of echoing was likely to be higher.

These data also help us to further check the reliability of the revolutionary data. As the spatial distribution in Figure A.4 of the Supplemental Material shows, the incidence of insurrection is highly correlated with the number of revolutionaries originating from each prefecture. The correlation between the cumulative number of revolutionaries during 1900–1906 and the incidence of uprisings is 0.32 (significant at the one percent level). With this information, we can also link quotas to the incidence of uprisings.

[23]As the data on the members in the Chinese Revolutionary Alliance were already compiled in Luo (1958), Chang (1982) only provided information on the founders. We add the information in Luo (1958).

This analysis, however, is still subject to the data quality concern that the Japanese newspaper might have drawn on news reports across China, meaning that the reporting could reflect the number of newspapers in the various regions. We thus ascertain the number of newspapers for each prefecture in 1911 but find that the correlation between the incidence of early uprisings and the number of newspapers is very weak (a p-value of 0.325).[24] Nevertheless, when analyzing the impact of quotas on the incidence of early uprisings, we also include the number of newspapers per capita as a control.

Additional Information on the Revolutionaries

For the largest group (the Chinese Revolutionary Alliance), we know the exact date of participation (see panel (b) of Figure A.1 of the Supplemental Material), which allows us to check for pre-trends using monthly data (discussed in Section 3.1). Some records also give the revolutionaries' ages, whose distribution (plotted in Figure A.5 of the Supplemental Material), with a median age of 24, suggests that the majority were young.[25]

Although it would also be ideal to know their family and educational backgrounds, the records provide only their geographic origins. To glean further qualitative information, we examine the background of the 86 revolutionaries killed in one well-documented uprising in April 1911, known as the Yellow Flower Mound revolt. Of these 86 "Yellow Flower Mound Martyrs," we have educational background and (some) family background for 32. Among these, 30 had received traditional education, but only 7 (versus 23) had family members (grandfathers or uncles) who were officials at some level, which suggests that the majority were from families that could afford a certain amount of education but were not among the upper gentry. Twelve had received some education in Japan, 6 of them among the 7 with an official in the family. This information implies that those who had studied in Japan were from relatively richer families and also contributed to the revolution. Since the sample is very limited, however, these numbers are merely suggestive.

2.4. *Additional Prefecture Characteristics*

As our baseline controls, we include four sets of prefecture characteristics (see Table I for the data sources and summary statistics). First, having no prior on the effect of population size, we control for a flexible third-degree polynomial of logged population size in 1880 and also include the logged area of the prefectures. Second, because of the potential importance of geography, we include two dummy variables for whether a prefecture is situated on the coast or includes one or more major rivers (those ranked as first- and second-order

[24]The data on the number of newspapers come from Shi, Yao, and Ye (1991).

[25]During the Qing dynasty, the average ages of the literati, recommended men, and presented scholars were around 24, 30, and 35, respectively (Chang (1955)).

streams in the Chinese river hierarchy). Third, because China's forced opening of designated foreign trading areas (treaty ports) after the first Opium War (1839–1942) introduced new knowledge and economic forces that could be correlated with the diffusion of revolutionary thought, we use treaty port indicators to control for possible foreign influence. Lastly, because revolutionary participation might also be correlated with economic conditions, we control for urbanization by adopting Rozman's (1973) three-part classification of Chinese cities: large (population of 300,000 and above), mid-level (population between 70,000 and 300,000), and small (population between 30,000 and 70,000).

In the analysis of understanding the mechanism, we also measure the importance of a prefecture by compiling a data set containing the following measures: (i) whether a prefecture is a provincial capital, (ii) the land tax per capita in 1820 (Liang (1981)), and (iii) government designations of whether a region belonged to any of four specialized groups: *chong* (important in transportation/communication), *fan* (important in business), *pi* (difficult to gather taxes in), and *nan* (high in crimes). These designations are available for both counties and prefectures and coded based on Liu (1993).

Table A.I of the Supplemental Material presents the correlations between these prefecture characteristics and quotas per capita. As expected, quotas per capita is positively correlated with the prosperity and importance of a prefecture. We will examine whether the prosperity and importance mattered for revolution before and after the abolition of the exam.

2.5. *Qualitative Evidence*

The abolition decision reached average citizens soon, as illustrated by both newspaper reports and individual diaries from 1905. For example, discussions by *Shanghai News* (*Shen bao*) and *Eastern Times* (*Shi bao*) on September 6 reflected mixed opinions toward the abolition: while echoing the intention of the abolition, the commentators were concerned how a totally different education system could get implemented. On September 10, *Sino-Foreign Daily* (*Zhongwai ribao*) discussed whether the consequences of the hasty abolition were remediable. These events were reflected in personal records of the time. For instance, on September 18, Zhu Zhisan, a member of the gentry in Hubei province (along the Yangtze River), recorded in his diary that many exam hopefuls had cried after abolition. In early October, Liu Dapeng, another member of the gentry in a relatively isolated village in Shanxi province (in the northwest), remarked on the abolition in his diary several times. He soon learned from his friend in the provincial capital that many students joined recently established organizations and was worried that "disasters and calamities will arise from them." Based on his diaries, Harrison (2005) depicted the shock of the abolition to a typical village:

> "Then in 1905, the examination system itself was abolished. Liu woke up a few days later 'with a heart like ashes' as he realized that his hopes for an official career had now completely vanished. When the sun came up he went out into the village street and found that

> everyone he met was talking about the end of the examinations. He talked with them about what a disaster it would be for the country, especially since no one knew what the graduates of the modern schools would be like. Together they wondered about what other changes there would be in the next few years. People realized that the ending of the examination system would transform channels for social mobility and that many other changes would inevitably follow."

The importance of the abolition of the exam in revolution was also recognized by revolutionary leaders. Hu Hanmin, who became a key leader of the KMT party after the success of revolution, once remarked, "who would have followed the revolution if the exam had not been abolished?".[26] Even Zhang Zhidong, the main pro-abolition politician in the Qing government, later expressed regretful second thoughts over the passing of the exam system.[27]

Given this anecdotal evidence, it is not surprising that scholars in the China field have conjectured that the abolition of the exam had important consequences on the collapse of the last dynasty. Franke (1970) stated that the abolition of the examination system resulted in "the dissolution of existing political and social order," and hence the importance of this measure for the final collapse of the traditional system which soon followed cannot be overestimated. Rozman (1982) emphasized that the abolition unlocked changes in what must be the main institutional base of any government: "the means of awarding status to society's elites and staffing the administration." Elman (2009) remarked that the demise of the Qing was already assured when the Qing state "lost control of the education system." Our main contribution to this discourse is to construct systematic data sets that not only permit investigation of the link between exam abolition and revolutionary participation, but shed light on the mechanism underlying this link.

3. EMPIRICAL RESULTS

In Section 3.1, we estimate the impact of quotas per capita on revolution participation before and after the abolition of the exam. We also present the link between quotas and the incidence of early uprisings in the 1911 Revolution. In Section 3.2 and Section 3.3, we present two sets of placebo tests to ensure that the baseline estimates are specific to the role of quotas and the abolition of the exam. Finally, to further deal with omitted variables, we present results using two instrumental variables in Section 3.4.

[26]The website in memory of the 100th anniversary of the 1911 Revolution provides more anecdotal evidence on the importance of the abolition of the exam: http://news.ifeng.com/mainland/special/xinhaigeming100/.

[27]Ayers (1971) described the role of Zhang in the abolition decision and his effort after 1905 to restore the traditional education.

3.1. *Linking Quotas to Revolution Participation*

Results Using Yearly Data

Panel (a) of Figure A.3 of the Supplemental Material presents the overall trends of revolution participation at the national level. In general, there tends to be an increase of revolutionary activity when a new organization got established, as a new organization could attract individuals who already had a revolutionary tendency—for example, when the Revive China Society got established in November 1894 and the Society of National Military Education got established in May 1903.[28] In 1905–1906, we see a notable increase in revolution participation, which is correlated with the timing of the establishment of the Revolutionary Alliance (in August 1905) and the abolition of the exam. To identify whether revolution participation was affected by the abolition of the exam, we examine the effect of exam quotas before and after the abolition. Our baseline estimations are based on data across 262 prefectures between 1900 and 1906. We complement the prefecture-year analysis with county-level data within one province between 1894 and 1906 and prefecture-month data for the Revolutionary Alliance.

In our prefecture-year analysis, we examine the link between quotas per capita and revolution participation before and after the abolition using the following specification:

$$
\begin{aligned}
(1) \qquad \mathrm{R}_{p,t} &= \beta \ln\left(\frac{\mathrm{Quota}}{\mathrm{Pop}}\right)_p \times \mathrm{Post}_t + \vartheta \ln \mathrm{Pop}_p \times \mathrm{Post}_t + \theta \mathrm{X}_p \times \mathrm{Post}_t \\
&\quad + \lambda_p + \gamma_t + \delta_{\mathrm{prov}} \times \gamma_t + \varepsilon_{p,t} \\
&= \beta \ln \mathrm{Quota}_p \times \mathrm{Post}_t + \nu \ln \mathrm{Pop}_p \times \mathrm{Post}_t + \theta \mathrm{X}_p \times \mathrm{Post}_t \\
&\quad + \lambda_p + \gamma_t + \delta_{\mathrm{prov}} \times \gamma_t + \varepsilon_{p,t},
\end{aligned}
$$

where $\nu = \vartheta - \beta$.

The dummy variable Post_t is 0 for the years before 1905 and 1 for 1905 and 1906. $\mathrm{R}_{p,t}$ is a dummy indicating whether there is any revolutionary in prefecture p and year t. $\ln \mathrm{Quota}_p$ is the logged quotas for the entry-level exam and $\ln \mathrm{Pop}_p$ is the logged population size in 1880. To allow for possible nonlinear effects of population size, we also include $(\ln \mathrm{Pop}_p)^2$ and $(\ln \mathrm{Pop}_p)^3$ in our analysis.

λ_p and γ_t indicate prefecture and year fixed effects to control for all time-invariant differences between prefectures and changes over time that affect all prefectures similarly. In addition, we also include very flexible provincial-specific trends: $\delta_{\mathrm{prov}} \times \gamma_t$. This will help us deal with potential confounding

[28]The two groups established in 1904 were relatively small and account for 4% (for the China Arise Society) and 3% (for the Revive the Light Society) of all revolutionaries during 1900–1906 in our data. This explains why there is no surge in revolution participation in 1904.

factors at the province level such as the quotas at the province level for candidates in higher-level exams. This is feasible because within-province variations account for the major part of the variations in quotas.

In addition to $(\ln \text{Pop}_p)^2$ and $(\ln \text{Pop}_p)^3$, X_p includes other prefecture-level characteristics discussed in Section 2.4. To further control for size effect, we control for logged area size. Additionally, we include a set of dummy variables indicating whether the prefecture is located on the coast, the Yangtze River, or any major river, whether the prefecture is a treaty port, and dummies for city ranks (to measure urbanization).

The results are presented in columns (1)–(5) of Table II. Column (1) presents the results including prefecture and year fixed effects and column (2) further includes provincial-specific trends. They show that prefectures with higher quotas had a higher probability of revolution participation following the abolition of the exam. Column (3) includes the interactions of the post dummy and the control variables in X_p (except for the nonlinear forms of population size). Now, the coefficient of $\ln \text{Quota}_p \times \text{Post}_t$ can be interpreted as the effect of quotas per capita. On average, a one standard deviation increase in the logged quota (0.57 after controlling for logged population size) implies about a six percentage point higher probability of revolution participation, which is about 40% of the mean probability (16 percentage points). Column (4) adds the interactions between the nonlinear terms of population and the post dummy and shows that the effect of quotas per capita varies little. Column (5) reports the results weighted by population sizes where the coefficient of $\ln \text{Quota}_p \times \text{Post}_t$ is still close to those from the unweighted specifications.

Table A.II of the Supplemental Material reports the results on the number of revolutionaries. Column (1) presents the impact on the number of revolutionaries using the same specification as that in column (5) of Table II. It shows that higher quotas per capita also increased the number of revolutionaries after the abolition of the exam. Column (2) shows that the results are also robust to using the number of revolutionaries per 100,000 inhabitants as the dependent variable. Column (3) employs a Poisson regression on the number of revolutionaries and shows that the baseline pattern still holds.

We can also examine the link between the quotas and revolution participation year by year. This way, we can examine whether there were already different trends for prefectures with different quotas before the abolition. The specification is as follows:

$$\text{R}_{p,t} = \sum_{\tau=1901}^{1906} \beta_\tau \ln \text{Quota}_p \times \text{Year}_\tau + \sum_{\tau=1901}^{1906} \upsilon_\tau \ln \text{Pop}_p \times \text{Year}_\tau + \sum_{\tau=1901}^{1906} \theta_\tau \text{X}_p \times \text{Year}_\tau + \lambda_p + \gamma_t + \delta_{\text{prov}} \times \gamma_t + \varepsilon_{p,t}, \tag{2}$$

where the year of 1900 is left as a comparison.

TABLE II

THE IMPACT OF QUOTAS I: ON REVOLUTION PARTICIPATION (D.V.: REVOLUTIONARY = 0/1)[a]

	Yearly Data for All Six Groups 1900–1906 (Mean: 0.16)					Monthly Data for the Alliance July–December 1905 (Mean: 0.12)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ln Quota × Post	0.145***	0.211***	0.112**	0.109**	0.132**	0.034**	0.042***	0.061**	0.068**	0.057**
	(0.020)	(0.025)	(0.045)	(0.045)	(0.057)	(0.015)	(0.016)	(0.028)	(0.027)	(0.028)
ln Pop × Post			0.115***	0.177***	0.082			−0.019	−0.035	−0.023
			(0.043)	(0.063)	(0.076)			(0.025)	(0.036)	(0.040)
ln Area × Post			−0.055	−0.074**	−0.020			0.029	0.025	0.001
			(0.034)	(0.034)	(0.023)			(0.024)	(0.025)	(0.011)
Coastal × Post			−0.098	−0.127	−0.086			−0.110*	−0.120*	−0.079
			(0.085)	(0.087)	(0.090)			(0.062)	(0.063)	(0.054)
Main river × Post			0.083*	0.079*	0.087*			−0.050*	−0.050*	−0.021
			(0.048)	(0.048)	(0.045)			(0.030)	(0.029)	(0.020)
Treaty port × Post			0.114	0.116	0.132*			0.049	0.052	−0.016
			(0.075)	(0.075)	(0.077)			(0.073)	(0.072)	(0.057)
Small city × Post			−0.025	−0.028	0.011			0.034	0.033	0.070
			(0.056)	(0.056)	(0.088)			(0.042)	(0.042)	(0.047)
Middle city × Post			−0.007	−0.024	−0.031			−0.103	−0.112*	−0.102
			(0.080)	(0.080)	(0.082)			(0.068)	(0.067)	(0.069)
Large city × Post			0.107	0.082	0.203			0.003	−0.041	0.070
			(0.137)	(0.137)	(0.140)			(0.102)	(0.109)	(0.101)
$(\ln \text{Pop})^2$ × Post				0.012	0.025				0.026*	0.013
				(0.023)	(0.022)				(0.014)	(0.012)
$(\ln \text{Pop})^3$ × Post				−0.009	0.006				0.009	0.005
				(0.010)	(0.008)				(0.006)	(0.004)

(*Continues*)

TABLE II—*Continued*

	Yearly Data for All Six Groups 1900–1906 (Mean: 0.16)					Monthly Data for the Alliance July–December 1905 (Mean: 0.12)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Prefecture FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y					
Province FE × Year FE		Y	Y	Y	Y					
Month FE						Y	Y	Y	Y	Y
Province FE × Month FE							Y	Y	Y	Y
Weighted by population					Y					Y
Observations	1,834	1,834	1,834	1,834	1,834	1,572	1,572	1,572	1,572	1,572
R-squared	0.286	0.448	0.462	0.464	0.397	0.033	0.148	0.158	0.161	0.098

[a]This table reports the impact of quotas on revolution participation after the abolition of the exam, compared with that before the abolition. Columns (1)–(5) employ yearly data for all six groups whereas columns (6)–(10) use monthly data for the Revolutionary Alliance only. The results in columns (5) and (10) are weighted by population size. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

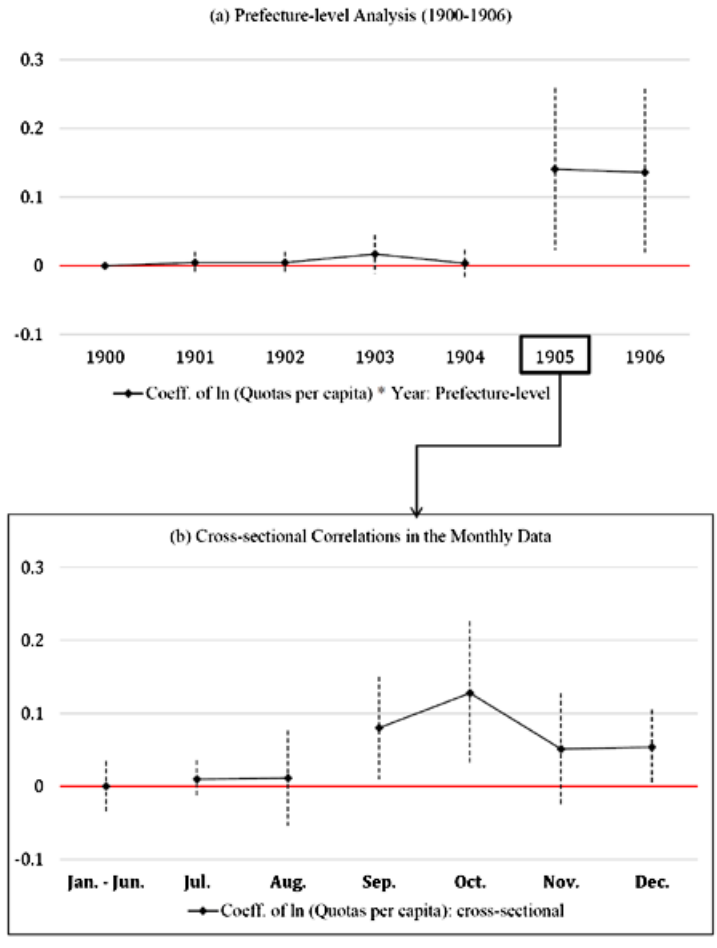


FIGURE 3.—The dynamic impacts of the quotas on revolution participation. *Notes*: Panel (a) visualizes the dynamic effects of quotas per capita on revolution participation, using the year 1900 as the reference, where the solid line connects the estimates and the dashed line indicates the 95% confidence intervals. Panel (b) presents the correlations between quotas per capita and revolution participation in the monthly data in 1905. The data between July and December are from the records of the Revolutionary Alliance while those between January and June are from the records of the earlier groups.

We leave the estimates across different specifications in Table A.III of the Supplemental Material and visualize the main results in column (4) in panel (a) of Figure 3, where the solid line connects the estimates and the dashed lines indicate the 95% confidence intervals with standard errors clustered at the prefecture level.

The figure shows that there were no significant differences in the pre-trends for the prefectures with high and low quotas while the positive impact of quotas

occurred in 1905, when the exam system was abolished. The magnitudes of the impacts are around 0.13 for 1905 and 1906, similar to the baseline estimates in Table II.

We focus on the period between 1900 and 1906 because the majority of revolutionaries came from one province (Guangdong) before 1900. This limitation shortens the number of years for our estimations on the dynamic impacts. In addition, the number of revolutionaries is smaller before the abolition, which might mechanically affect the finding of no pre-trends. To deal with this concern, we construct a county-level panel for 92 counties in Guangdong, where we can trace revolutionaries back to 1894. As shown in panel (a) of Figure A.6 of the Supplemental Material, quite a few counties already had revolutionary participation before 1900. Considering that counties could still compete for some quotas at the prefecture capital, we also control for prefecture-specific trends ($\delta_{\text{pref}} \times \gamma_t$) in our analysis.

Replacing the variables in the prefecture-level analysis with county-level information, the estimates on the dynamic impacts are presented in Table A.IV of the Supplemental Material. Panel (b) of Figure A.6 of the Supplemental Material visualizes the results in column (4) of Table A.IV of the Supplemental Material. Once again, the effect of quotas only took place after the abolition of the exam. Moreover, it shows that our no pre-trend finding is not due to the lack of revolutionaries before 1905.

Results Using Monthly Data

In our yearly analysis, we treat 1905 as a post-abolition year because of the timing of the revolutionary groups. The majority of the new members in 1905 belonged to the Chinese Revolutionary Alliance that officially allied the earlier revolutionary groups in August 1905. The Revolutionary Alliance was the largest revolution group in this period and later became the core of the KMT.[29] Among the new members in 1905, 35% of them joined the earlier groups and the Alliance before September and 65% joined the Alliance between September and December. In other words, the revolution participation in 1905 was mainly driven by the activities in the last few months.

We have information on the exact date of participation for the Chinese Revolutionary Alliance. The earliest members joined in July 1905 (who were involved in the founding process). Thus, for 1905, we have monthly information on those who joined between July and December. Panel (b) of Figure A.3 of the Supplemental Material plots the overall trends of revolution participation in the monthly data. We see an increase of prefectures with revolutionaries from August onward. The increase in August is due to the fact that the Revolutionary Alliance attracted quite a few participants when it was officially established in August. This pattern is the same as that in the yearly data when

[29]In our data, the members in the Revolutionary Alliance account for 67% of all revolutionaries between 1900 and 1906.

other influential groups were initially established. Similarly to the yearly analysis, we explore prefecture-level variation in exam quotas to investigate whether the participation is affected by the abolition of the exam.

To incorporate the data (from the earlier groups) during January–June of 1905, we visualize the cross-sectional correlation between quotas per capita and revolution participation month by month in panel (b) of Figure 3, while controlling for all the variables in X_p. The solid line connects the monthly correlations and the dashed lines indicate the 95% confidence intervals where the standard errors are clustered at the prefecture level. As shown, quotas per capita were not significantly correlated with the participation before the abolition but mattered since September. We do not have a structural theory to explain the effect month by month, but it is reasonable to see an immediate response after the abolition: the marginal individuals who were deciding to participate or not were likely to be mobilized quickly.

Using the same specifications in columns (1)–(5) of Table II, columns (6)–(10) of Table II report the impact of logged quotas per capita and the post-September dummy from difference-in-differences analysis, based on the data on the Revolutionary Alliance only. Once again, they show that prefectures with higher quotas per capita had a significantly higher probability of revolution participation after the abolition of the exam. A one standard deviation increase in the logged quota implies about a three percentage point higher probability of revolution participation in the monthly data, which is about 28% of the mean probability (12 percentage points).

Correlation Between Quotas and the Incidence of Uprisings in 1911

The baseline results on revolution participation are obtained from difference-in-differences analysis. The incidence of early uprising in 1911 is cross-sectional information by definition. Hence, we can only examine a cross-sectional correlation as follows:

$$\mathrm{I}_{p,1911} = \alpha \ln \mathrm{Quota}_p + \nu \ln \mathrm{Pop}_p + \theta \mathrm{X}_p + \delta_{\mathrm{prov}} + \varepsilon_p,$$

where $\mathrm{I}_{p,1911}$ indicates whether there was an early uprising in prefecture p in 1911.

The results are presented in columns (1)–(4) of Table III. Column (1) only controls for logged population and column (2) also controls for other prefecture characteristics. Column (3) adds the nonlinear forms of logged population. Since our data source (the Japanese newspaper) might consult news reports in China, there is a concern whether the reporting might reflect the number of newspapers across regions. To deal with this issue, we include newspapers per capita in our analysis in column (4), which shows no significant correlation between newspapers per capita and the incidence of uprisings.

The estimates on logged quotas per capita are stable over these specifications. Quotas were positively correlated with the incidence of the early upris-

TABLE III

THE IMPACT OF QUOTAS II: ON EARLY UPRISINGS IN 1911 AND THE BOXER REBELLION[a]

	Incidence of the Revolution 1911					Incidence of the Boxer Rebellion 1899–1901			
	All	All	All	All	Northern	All	All	All	Northern
Mean of D.V.	0.16	0.16	0.16	0.16	0.17	0.10	0.10	0.10	0.14
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ln Quota	0.094**	0.075*	0.083*	0.085*	0.136**	0.023	0.000	−0.003	0.008
	(0.040)	(0.044)	(0.046)	(0.046)	(0.053)	(0.023)	(0.024)	(0.025)	(0.037)
ln Pop	0.015	0.048	0.032	0.031	−0.024	0.039*	0.025	0.062*	0.084*
	(0.033)	(0.040)	(0.066)	(0.066)	(0.073)	(0.023)	(0.025)	(0.035)	(0.046)
ln Area		−0.039	−0.046	−0.047	−0.050		0.058***	0.049**	0.060**
		(0.041)	(0.045)	(0.045)	(0.058)		(0.022)	(0.022)	(0.029)
Coastal		−0.130	−0.147	−0.145	−0.200*		−0.046	−0.059	−0.044
		(0.088)	(0.093)	(0.093)	(0.119)		(0.056)	(0.057)	(0.095)
Main river		0.038	0.038	0.037	0.004		−0.046	−0.048	−0.063
		(0.050)	(0.050)	(0.050)	(0.059)		(0.037)	(0.037)	(0.049)
Treaty ports		0.046	0.051	0.056	0.196		−0.062	−0.061	−0.061
		(0.087)	(0.088)	(0.091)	(0.125)		(0.039)	(0.040)	(0.062)
Small city		−0.004	−0.004	−0.003	−0.054		−0.013	−0.015	−0.016
		(0.071)	(0.071)	(0.072)	(0.076)		(0.035)	(0.035)	(0.048)
Middle city		−0.016	−0.030	−0.029	−0.041		0.156**	0.149**	0.192**
		(0.084)	(0.084)	(0.084)	(0.098)		(0.064)	(0.065)	(0.080)
Large city		0.077	0.015	0.022	−0.085		0.064	0.060	0.065
		(0.150)	(0.146)	(0.142)	(0.171)		(0.084)	(0.087)	(0.114)
$(\ln \text{Pop})^2$			0.036**	0.036**	0.031			0.000	−0.008
			(0.018)	(0.018)	(0.019)			(0.015)	(0.020)
$(\ln \text{Pop})^3$			0.012	0.012	0.013			−0.007	−0.012
			(0.008)	(0.008)	(0.009)			(0.007)	(0.009)
Newspaper density				−0.668	−1.485				
				(1.175)	(1.381)				
Province FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	262	262	262	262	191	262	262	262	191
R-squared	0.231	0.248	0.258	0.258	0.336	0.385	0.422	0.425	0.424

[a]Columns (1)–(5) show that higher quotas per capita were associated with a higher probability of early uprisings in 1911. Columns (6)–(9) show that quotas did not affect the Boxer Rebellion. The Boxer Rebellion was motivated by proto-nationalist sentiments and opposition to foreign imperialism but was unlikely to be correlated with the exam system. Columns (5) and (9) limit the sample to the prefectures with latitude higher than that of the southernmost prefecture with the Boxer Rebellion. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

ings in 1911: a one standard deviation increase in the logged quotas (after controlling for the logged population) was associated with about a four percentage point higher probability of early uprisings, which is 25% of the mean of the dependent variable (0.16). These results show a sizable correlation between quotas and the incidence of uprisings in the critical year of the revolution.

Two main concerns about our baseline findings are whether the results are driven by the role of quotas and whether they are specific to the abolition of the exam. We examine these two issues in Section 3.2 and Section 3.3.

3.2. *Using the Boxer Rebellion as a Placebo*

To test our concern that quotas may reflect the impact of other prefectural characteristics such as differences in political sentiment or pro-conflict feelings in higher quota districts, we use the 1899–1901 Boxer Rebellion as a placebo. Because most of the Boxer rebels were landless peasants (Esherick (1987)), this rebellion, despite the correlation between its proto-nationalist and anti-foreign motivations and ideology and conflict propensity in a prefecture, had little to do with the exam system. It thus serves as a useful check that prefectures with higher quotas were not necessarily pro-conflict or more motivated by proto-nationalist sentiments.

As our data source, we use the appendix to the Boxer Protocol (1901), which lists the prefectures and counties in which the Boxers operated and killed foreigners, used in the analysis as a dummy variable that measures the incidence of rebellion. We find that in contrast to their impact on the 1911 Revolution, quotas are not at all correlated with the Boxer Rebellion (Table III, columns (6)–(9)). This observation implies that the above findings are specific to quotas rather than other omitted variables such as whether certain prefectures were more pro-conflict. Additionally, because the Boxer Rebellion affected the northern China more, we also limit the sample to the prefectures with a latitude higher than that of the southernmost prefecture involved in the rebellion. The results, given in columns (5) and (9), are similar to those for the whole sample.

Another related test that we conduct on potential ideological difference is to examine party identification after the success of the revolution. In particular, compared with the other parties (the Republican Party and the Democratic Party), the KMT (which literally means "Chinese Nationalist Party") was known to be more radical. We do not find that quotas were of importance for party identification, which is also consistent with the placebo test using the Boxer Rebellion. The results and discussions are presented in Table A.V of the Supplemental Material and discussed in Section A.1 of the Supplemental Material.

3.3. *Investigating the Impacts of Other Historical Events*

Another major concern with our main findings is that in addition to exam abolition, they might be capturing other concurrent policy shocks that increased the general revolutionary upheaval. To address this concern, we examine whether other important historical events that might also trigger some revolutionary upheavals exhibited any impacts similar to that of the abolition. Specifically, we consider three sets of variables:

(i) Other Domestic Events

The three other important events during the 1895–1905 period were the failure of the Qing government in the Sino-Japanese War of 1895, the Hundred Days' Reform of 1898,[30] and the Boxer Rebellion and Boxer Protocol agreement of 1901 (Cameron (1931)). The treaty following the Sino-Japanese War and the Boxer Protocol, in particular, required the Qing government to pay large indemnities that could indirectly increase the tax burden of citizens. Thus, even though these events did not change the elite recruitment system, they may have triggered some revolutionary upheaval. We thus use the county-level information between 1894 and 1906 to check their impacts. As Figure A.6 of the Supplemental Material shows, quotas appear to have no significant effect in either 1895, 1898, or 1901, confirming that our finding is specific to the year of exam abolition.

(ii) Access to Information and Foreign Influence

Certain international events might also have added to the upheaval in China. If their role was significant, we would expect the effects to be larger in regions with more information exposure or higher foreign penetration. To test this prediction, we employ newspaper density and a dummy for foreign enclaves as the relevant proxies and examine whether their impacts changed before and after abolition.[31] We also allow for a triple quota effect of abolition timing (dummy) and the two proxies. As columns (1)–(2) of Table A.VI of the Supplemental Material show, neither information exposure nor foreign penetration explains our findings, and we find no significant heterogeneous effects either.

(iii) The Russo-Japan War

The Russo-Japan war, which preceded exam abolition, was one of the most influential international events of 1905, and the resulting exposure to Japanese influence provides useful insights into the war's potential heterogeneous impacts. We measure this influence using two variables, one for whether a prefecture was a Japanese enclave and the other for how many students studying in Japan originated from that prefecture. Once again, we allow for a triple quota effect based on the post-abolition dummy and these two measures. The results for the Japanese enclaves, reported in columns (3)–(4) of Table A.VI of the Supplemental Material, show little variation from our main finding. Those for the number of students studying in Japan (discussed in more detail in Section 4.3 on the role of modern human capital) indicate that this variable cannot explain our baseline finding either. Column (5) in this same table further

[30]The Hundred Days' Reform was a failed 104-day national cultural, political, and educational reform movement from 11 June to 21 September 1898. The reform touched the economic and political system, the education system as well as the military. But none of them really succeeded.

[31]The data on foreign enclaves come from Yan (1955).

demonstrates that the impact of quotas is still close to our baseline estimate even after these factors are incorporated.

Taken together, the above results confirm that our baseline findings are unlikely to be driven by historical events other than exam abolition. Additionally, we also find that the impact of quotas post abolition is amplified by the prefecture-specific returns from the exam system (see Section 4.1 for more discussions). This further suggests that our finding is specific to the exam system rather than other historical events.

3.4. *Results From Instrumental Variables*

Using the difference-in-differences strategy, we can rule out the effects of omitted variables only when their effects did not change before and after the abolition of the exam. Given that the government still selected bureaucrats after the abolition, the selection was likely to be affected by omitted variables (OV) positively correlated with quotas (such as political networks). The role of such omitted variables was likely to become more important in elite recruitment after the abolition and hence decreased the probability of revolution participation, that is, $\beta^{\text{OV}}_{\text{post}} - \beta^{\text{OV}}_{\text{pre}} < 0$. Given that $\beta = (\beta_{\text{post}} - \beta_{\text{pre}}) + \frac{\text{cov(Quota,OV)}}{\text{var(Quota)}} \times (\beta^{\text{OV}}_{\text{post}} - \beta^{\text{OV}}_{\text{pre}})$, the DID estimate is likely to be biased downwards. For this concern, we further employ two instruments for quotas.

Instrument I: The Number of Small Rivers (Given River Lengths)

Our first instrument stems from geographical characteristics. Conceptually, for two prefectures with the same population size, the prefecture with more counties tended to have higher quotas per capita, because the quota assignment followed the stepwise role and each county would get some quota. Therefore, we would like to find an instrument that affected the formation of counties in a prefecture but did not affect revolution through any other channels such as economic development. The number of rivers is a reasonable candidate for two reasons. First, counties were usually formed around rivers. As shown in Figure A.7 of the Supplemental Material, county seats were generally located on rivers. Second, it is not efficient to have many rivers within a county due to high administrative costs (e.g., tax collection costs). These two reasons lead to a positive link between the number of rivers and the number of counties.

However, the number of rivers might affect other dimensions besides the number of counties (e.g., economic development). To take into consideration potential confounding impacts of rivers on development, we exclude major rivers and use the number of small rivers normalized by the total length of rivers. The idea is that the shape of rivers affected the number of counties, given their length.

In sum, the channel through which the number of small rivers affects the prefecture-level is as follows:

$$\left(\frac{\#\text{ small rivers}}{\text{River length}}\right)_p \xrightarrow{\text{(a)}} \left(\frac{\#\text{ counties}}{\text{River length}}\right)_p \xrightarrow{\text{(b)}} \left(\frac{\text{Quota}}{\text{River length}}\right)_p.$$

Section A.2 of the Supplemental Material gives an example in the data to illustrate the channel. Figure 4 visualizes this channel. Panel (a) visualizes the positive correlation between the number of counties and the number of small rivers, given the length of rivers. Panel (b) visualizes the positive correlation between quotas and the number of counties, given the length of rivers. The two links together imply that the number of small rivers has a strong positive impact on quotas, as presented in columns (1)–(2) of panel (a) in Table IV. Column (3) reveals no significant correlation between the instrument and the change in quotas, which happened once due to the fighting of the Taiping Rebellion and should not be explained by geography.

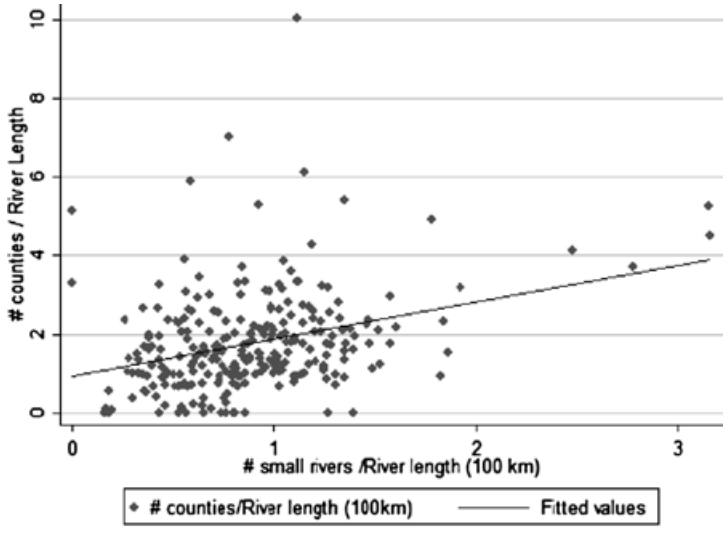
We also conduct four sets of tests to check whether our instrument might affect other dimensions besides quotas. First, using the official designation of transportation centers (Liu (1993)), we examine whether the river feature affects transportation conditions. As shown in columns (4) and (5) of Table IV, our instrument is uncorrelated with importance for transportation regardless of whether we use a dummy for prefecture or the average number of counties it contains. As expected, however, transportation importance is correlated with being located on a major river. Second, premised on the notion that the number of small rivers might be correlated with agricultural suitability, our analysis includes suitability for three crops: wetland rice, a staple food highly dependent on water; foxmillet, a traditional Chinese drought-resistant crop; and sweet potato, the main New World crop adopted in China. We observe no significant correlation between our instrument and crop suitability (columns (6)–(8)).[32] Third, to assess whether the incidence of climate disasters might systematically differ between two prefectures with different small river density, we construct a measure on the drought/flood index in 1800–1999.[33] Yet again, the measure shows no significant correlation with our instrument (column (9)). Finally, we check whether the number of small rivers affects the fragmentation of basins, which may also affect the suitability of agriculture. We find no significant impact on basin fragmentation as measured by the Herfindahl–Hirschman index (column (10)). Even though our tests may not capture all dimensions, they suggest that the major omitted variables are unlikely to be of critical concern.

These placebo tests, in conjunction with the relevance tests, suggest that the number of small rivers is a reasonable instrument. Additionally, we define

[32]The suitability data are from FAO (2012).

[33]The weather data come from the State Meteorological Society (1981). Weather conditions are coded into five indicators: −2 (extreme drought), −1 (drought), 0 (normal), 1 (flood), and 2 (extreme flood).

(a) #Small Rivers/Riv.Length and #Counties



(b) #Counties and the Quota

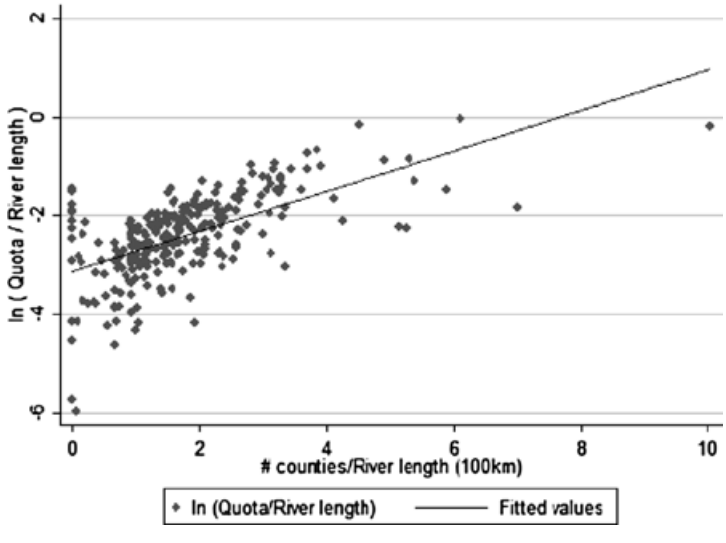


FIGURE 4.—Small rivers, counties, and quotas. *Notes*: Panel (a) shows that the number of small rivers (given the length of river) in a prefecture is positively correlated with the number of counties. Panel (b) shows that the number of counties is positively correlated with quotas.

TABLE IV

RELEVANCE AND PLACEBO TESTS OF THE TWO INSTRUMENTS[a]

(a) IV1: # small rivers/River length

	Relevance Tests			Placebo Tests						
	ln Quota			Transportation		Suitability			Climate	Basin
	Late Qing	Early Qing	Change	Prefecture	County Average	Rice	Foxmillet	Sweet Potato	Drought/Flood	HH Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
# small rivers/River length	0.262**	0.265*	−0.003	−0.114	−0.064	0.166	0.036	0.140	0.009	0.038
	(0.122)	(0.140)	(0.029)	(0.085)	(0.060)	(0.126)	(0.184)	(0.133)	(0.014)	(0.051)
ln(River length)	0.242*	0.252*	−0.010	0.032	0.033	0.061	−0.088	−0.274*	0.021*	−0.063
	(0.128)	(0.142)	(0.032)	(0.088)	(0.072)	(0.144)	(0.200)	(0.145)	(0.012)	(0.043)
Major river	0.123*	0.106	0.016	0.150**	0.131***	0.007	−0.068	0.105	0.010	−0.019
	(0.069)	(0.068)	(0.015)	(0.071)	(0.046)	(0.104)	(0.123)	(0.117)	(0.010)	(0.035)
Baseline controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	262	262	262	262	262	262	262	262	262	262
R-squared	0.777	0.755	0.704	0.289	0.248	0.690	0.722	0.542	0.403	0.382

(*Continues*)

TABLE IV—*Continued*

(b) IV2: Short-Run Change in Performance

	Relevance Tests			Placebo Tests: Changes in Presented Scholars in the Long Run					
	ln Quota								
	Late Qing	Early Qing	Change	1436–1505 vs. 1368–1435	1506–1572 vs. 1436–1505	1573–1643 vs. 1506–1572	1644–1722 vs. 1573–1643	1723–1795 vs. 1644–1722	1796–1861 vs. 1723–1795
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Δ ln(PresentedScholar)	0.221***	0.212***	0.009	−0.029	−0.101	−0.086	−0.127	0.041	−0.065
	(0.044)	(0.045)	(0.013)	(0.088)	(0.079)	(0.063)	(0.080)	(0.096)	(0.072)
ln(PresentedScholar$_0$)	Y	Y	Y	Y	Y	Y	Y	Y	Y
Baseline controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	262	262	262	262	262	262	262	262	262
R-squared	0.788	0.764	0.705	0.429	0.140	0.173	0.281	0.473	0.195

[a]Columns (1)–(3) show that the instrument is correlated with the level of quotas but not the change. The other columns present different sets of placebo tests. Columns (4)–(10) in panel (a) show that the instrument was not significantly correlated with transportation importance, agricultural suitability, climate shocks, or basin fragmentation. Columns (4)–(9) in panel (b) show that the instrument did not affect the growth of successful candidates in the long run. The baseline controls include (i) third-degree polynomials of logged population in 1880, and logged area; (ii) whether a prefecture is located on the coast and whether it is located on a major river; (iii) whether a prefecture has a treaty port; and (iv) whether a prefecture was counted as a big city, a middle-size city, or a small city. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

small rivers as rivers of less than 70, 80, . . . , 120 km and show that the results are robust to such variations. These checks are presented in Table A.VII and discussed in Section A.3 of the Supplemental Material.

Instrument II: Exam Performance Before the Quota System

Our alternative instrument stems from historical roots of the quota system. The regional quota system was initially employed during the Ming dynasty (1368–1644 AD). In 1425, a provincial-level quota system was introduced to balance the opportunity to pass the national exam. In 1436, the central government began to appoint government officials to each prefecture to select candidates for the province-level exam (Li (1989)). In short, the quota system was initially introduced during 1425–1436.

The performance in the exam before 1425, measured by the number of presented scholars (those succeeding in the national-level exam), is another possible instrument as it affected the subsequent quota assignment. However, using the level of the past performance as an instrument suffers from the concern that it reflects prefecture-specific factors that can have long-run impacts. To minimize this concern, we employ changes in the performance in the very short run before the quota system as a second instrument. This instrument has two advantages over the level of performance. First, the short-run change rules out the time-invariant prefecture-specific factors that can have long-run impacts. Unlike the level of performance, the short-run change is more likely to be driven by random factors. For instance, it could capture the supply of exceptional students that did not persist in the long run. Another example is that the idiosyncratic preference of an emperor could also affect the short-run performance of a prefecture but did not have a persistent effect. Second, the change also captures the fact that the performance in more recent years played a more important role than that in more distant years when the quotas got assigned.

Following this thought experiment, we divide the pre-1425 period into two sub-periods of similar length (1368–1398 and 1399–1425). Denote the logged number of presented scholars in the first three decades as $\ln(1 + \text{PresentedScholar}_0)$ and that during the second three decades $\ln(1 + \text{PresentedScholar}_1)$. We employ the first difference of exam performance $\Delta \ln \text{PresentedScholar}$ as our alternative instrument. To further control for potential level effects, we also include the initial level in our analysis.

As with our first instrument, we examine the relevance of this instrument and whether it affects other factors besides the quota. Columns (1)–(3) of panel (b) in Table IV present the results from the relevance tests. As placebo tests, we examine whether this instrument affects changes in the number of presented scholars in longer periods. We look at seven periods (defined by the tenures of emperors) and take the first difference in the number of presented scholars. By regressing these differences on our instrument, we do not find any significant correlations, as shown in columns (4)–(9). Thus, the relevance and placebo

tests in panel (b) suggest that the short-run performance before the quota system is another reasonable instrument.

Estimation Results From Two Instruments

Given the relevance tests as well as the placebo tests, we perform the instrument variable estimations. The first-stage and second-stage estimations are as follows:

$$\begin{aligned}
&\ln \text{Quota}_p \times \text{Post}_t \\
&= \rho_1 \left(\frac{\#\text{ small rivers}}{\text{River length}} \right)_p \times \text{Post04}_t + \delta \ln \text{River length}_p \times \text{Post}_t \\
&\quad + \rho_2 \Delta \ln \text{PresentedScholar}_p \times \text{Post04}_t \\
&\quad + \sigma \ln(1 + \text{PresentedScholar}_0)_p \times \text{Post04}_t + \nu \ln \text{Pop}_p \times \text{Post}_t \\
&\quad + \theta \text{X}_p \times \text{Post}_t + \lambda_p + \gamma_t + \delta_{\text{prov}} \times \gamma_t + \varepsilon_{p,t},
\end{aligned}$$

and

$$\begin{aligned}
\text{R}_{p,t} = {} & \beta \widehat{\ln \text{Quota}_p \times \text{Post04}_t} + \delta \ln \text{River length}_p \times \text{Post}_t \\
& + \sigma \ln(1 + \text{PresentedScholar}_0)_p \times \text{Post04}_t + \nu \ln \text{Pop}_p \times \text{Post}_t \\
& + \theta \text{X}_p \times \text{Post}_t + \lambda_p + \gamma_t + \delta_{\text{prov}} \times \gamma_t + \varepsilon_{p,t}.
\end{aligned}$$

The results are presented in Table V. Before presenting the results using both instruments, columns (1)–(3) report the results using the river instrument. Column (1) reports the reduced-form result and shows that this instrument is significantly correlated with revolution participation. We control for the impacts of both the baseline controls and the variables used in the placebo tests (transportation, crop suitability, climate shocks, and basin fragmentation index). Column (2) reports the IV estimate (0.30). Column (3) includes the second instrument as a regressor. The insignificant effect of the second instrument suggests that it does not have any direct effect on revolutionaries besides the channel of quotas—this method can be regarded as an easy-to-interpret version of the over-identification test. The first stage F-statistics are above 45, implying that this instrument is not weak. Similarly, columns (4)–(6) report the corresponding results using the second instrument. The estimate from the instrument is around 0.22.

Columns (7)–(8) combine the two instruments. Column (7) reports the reduced-form result while column (8) presents the IV estimate. Consistent with the tests in columns (3) and (6), the p-value of the over-id test is around 0.68.

The estimate using the two instruments is about twice that of the estimate using the difference-in-differences method. This difference is consistent with

TABLE V

THE IMPACT OF QUOTAS III: RESULTS FROM INSTRUMENTAL VARIABLES[a]

	IV1: # small rivers/River lenght × Post			IV2: Δ ln(PresentedScholar) × Post			Both	
	Reduced Form	IV	IV	Reduced Form	IV	IV	Reduced Form	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln Quota × Post		0.289**	0.300**		0.223**	0.220*		0.249***
		(0.140)	(0.153)		(0.108)	(0.114)		(0.090)
# small rivers/River lenght × Post	0.090**					0.023	0.086*	
	(0.045)					(0.055)	(0.045)	
Δ ln(PresentedScholar) × Post			−0.018	0.053**			0.049*	
			(0.044)	(0.027)			(0.027)	
		First Stage			First Stage			First Stage
# small rivers/River lenght × Post		0.311***	0.286***			0.286***		0.286***
		(0.035)	(0.033)			(0.033)		(0.033)
Δ ln(PresentedScholar) × Post			0.223***		0.236***	0.223***		0.223***
			(0.019)		(0.020)	(0.019)		(0.019)
Baseline controls × Post	Y	Y	Y	Y	Y	Y	Y	Y
ln(River length) × Post	Y	Y	Y			Y	Y	Y
ln(PresentedScholar$_0$) × Post			Y	Y	Y	Y	Y	Y
Placebo variables × Post	Y	Y	Y	Y	Y	Y	Y	Y
Prefecture FE, Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Province FE × Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,834	1,834	1,834	1,834	1,834	1,834	1,834	1,834
R-squared	0.464	0.454	0.454	0.464	0.461	0.462	0.465	0.459
F-statistics		53.53	47.66		116.46	109.62		91.94
p-value of the over-id test								0.679

[a]Columns (3) and (6) show that the effect of one instrument is not significant once the other is employed, suggesting that the instrument did not affect revolutionaries beyond the quota channel. This is also confirmed by the *p*-value of the over-id test reported in the last row. The baseline controls include (i) third-degree polynomials of logged population in 1880, and logged area; (ii) whether a prefecture is located on the coast and whether it is located on a major river; (iii) whether a prefecture has a treaty port; and (iv) whether a prefecture was counted as a big city, a middle-size city, or a small city. The placebo variables are the transportation importance, crop suitability, climate shocks, and basin fragmentation. Standard errors in parentheses: *significant at 10%; **significant at 5%; ***significant at 1%.

the concern of omitted variables discussed in the beginning of this section. Naturally, another explanation for the difference is that the IV estimates provide local average treatment effects specific to the two instruments.

4. UNDERSTANDING THE MECHANISM

Having used several methods to establish the link between quotas and revolutionary participation following abolition of the civil service exam, we now turn to understanding the mechanism. It is worthwhile emphasizing that for any factor to account for our main findings, it needs to satisfy two criteria: (1) its effect should be correlated with quotas per capita, and (2) its effect changed discontinuously before and after the abolition of the exam.

Based on the main players of society in the period, we lay out four hypotheses. The first hypothesis is the role of "*state capacity*": the abolition of the exam might signal or was driven by the deterioration in state capacity and the impact might be larger in regions with higher quotas per capita. A second hypothesis is "*elite eligibility*": the abolition of the exam negatively affected a large group of commoners who could become elites via the exam system and the impact is larger in prefectures with higher quotas per capita. A third hypothesis is "*elite resistance*": the existing elites might dislike the abolition and the number of them was also positively correlated with quotas. A final hypothesis is "*modern human capital*": there might also be more modern human capital in prefectures with higher quotas per capita and modern education facilitated revolution.

We present multiple pieces of evidence for or against each hypothesis and find that the empirical patterns appear most consistent with the elite eligibility hypothesis. So we first present evidence for this hypothesis and formalize it in a simple model. Then, we come to the hypotheses regarding other groups of citizens. Finally, we discuss the role of state capacity.

4.1. *Elite Eligibility*

The would-be elites could be affected by the abolition of the exam in two major ways: (i) their prospect of becoming elites was negatively affected and/or (ii) their opportunity costs of participating in revolution were lowered as they stopped studying and had more time to rebel. We try to understand the importance of them in two ways. First, we present some evidence for the change in prospect. It is more challenging to measure opportunity costs. Exploiting heterogeneity in the prefecture-specific returns from the exam system, we expect to see an amplification effect if the prospect channel dominates but no such effect if the opportunity costs channel dominates.

Evidence for the Change in Prospect

To examine whether quotas mattered for the prospect of upward mobility, we link quotas for a prefecture to the number of presented scholars and key

officials from each prefecture during the Qing dynasty (1644–1904), while controlling for province fixed effects and population sizes in the mid-Qing period. The results are presented in columns (1)–(4) of Table VIA. To facilitate the comparison across regimes, beta coefficients are reported so that the coefficients should be interpreted as how many standard deviations the dependent variable will change, per standard deviation increase in logged quotas after controlling for logged population. *p*-values are reported in squared brackets.

To examine the change in the influence of quotas before and after the abolition of the exam, we examine the link between quotas and the origins of political newcomers. For the pre-abolition period, the political newcomers were the presented scholars that succeeded in the national exam who could be selected to become top officials. Post abolition, the government selected people with a foreign-educational background and awarded them a degree of quasi-presented scholars. Columns (5)–(6) of Table VIA present the correlations between quotas and the number of newcomers in 1904 (pre abolition), while columns (7)–(8) present that for 1907 (post abolition). They show that the importance of quotas in determining the number of newcomers decreased significantly after the abolition. Given the same population, a one standard deviation in logged quotas explains about a 0.3 standard deviation of political newcomers and the correlation is significant at the 1% level in 1904, while the correlation between quotas and political newcomers is halved and even becomes insignificant in 1907. The difference between the impacts of quotas in 1904 and 1907 is significantly different, with *p*-values of 0.034 and 0.003 for the newcomer dummy and the number of newcomers, respectively.

This quantitative evidence is consistent with a large body of historical literature on the role of the civil service exam in social mobility, which tends to suggest that 40–60 percent of successful candidates had no officials in their family background and that 80 percent of their descendants beyond the grandson generation faded into obscurity.[34] Although this line of historical research is sometimes criticized for not considering larger kin networks or lineages (Hymes (1986), Hao and Clark (2012)), which could decrease the estimates and reveal more advantages, the detailed intergenerational data used by Campbell and Lee (2003) reveal that kin networks do not monopolize opportunities, even though distant kin influence the chances of obtaining a title.

[34]Kracke (1947) used the candidate lists from the Song dynasty to show that approximately 60 percent of all successful candidates came from non-official backgrounds, while Chang (1955) indicated that at least 35 percent of the gentry class in the 19th century were "newcomers" (neither their fathers nor their grandfathers had held gentry status). Likewise, Ho (1962), in a study of candidate biographies in the Ming and Qing dynasties, found that over 40 percent of those succeeding at the highest level (i.e., presented scholars) also came from non-official backgrounds. Although Hsu (1949) used a different method to examine the background of prominent individuals mentioned in the gazetteers of four widely separated regions in China, he also found that roughly 50 percent of the local prominent individuals came from unknown origins and that roughly 80 percent of their descendants beyond the grandson generation became unknown. He also identified the civil service exam as the primary driver of the fairly high degree of mobility.

TABLE VIA

THE ROLE OF PROSPECT I: QUOTAS AND POLITICAL NEWCOMERS UNDER DIFFERENT REGIMES[a]

	Quotas and Elite Recruitment 1644–1904				Before the Abolition 1904		After the Abolition 1907	
D.V.:	Presented Scholar		Key Official		New Presented Scholar		Quasi-Presented Scholar	
Measure:	0/1	Number	0/1	Number	0/1	Number	0/1	Number
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln Quota	0.423***	0.491***	0.324***	0.439***	0.292***	0.357***	0.146	0.184*
	[0.000]	[0.000]	[0.001]	[0.000]	[0.007]	[0.001]	[0.140]	[0.083]
$(\ln \text{Pop})^{1,2,3}$	Y	Y	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	262	262	262	262	262	262	262	262
R-squared	0.380	0.425	0.373	0.392	0.270	0.299	0.376	0.283
p-value							0.034	0.003

[a]Beta coefficients are reported. p-values are reported in squared brackets. The table shows the link between quotas and the number of political newcomers across prefectures under different regimes: there was a strong link between quotas and the number of political newcomers (shown in columns (1)–(6)). This link was very much weakened with the abolition of the exam (shown in columns (7)–(8)). In the last row, the number in column (7) reports the p-value from the test on the difference between the impacts of quotas in 1904 and 1907 on the newcomer dummy (0.292 vs. 0.146). Column (8) reports the test on the number of newcomers (0.357 vs. 0.184). *significant at 10%; **significant at 5%; ***significant at 1%.

One implication of the weakening role of quotas in political selection is that the prospect of becoming elites via the exam quota system was negatively affected. To further examine the role of prospect, we exploit variations in the returns from the exam system.

Further Evidence From Heterogeneous Exam Returns

One natural proxy for the prefecture-specific returns of the exam is the number of (pre-abolition) key officials from that prefecture who enjoyed great prestige and power. For two prefectures with the same quotas per capita, more officials from one prefecture via the exam system could signal higher returns from the system. If prospect plays a dominant role in our main finding, we expect a positive effect of $\ln(1+\text{Official})_p \times \ln \text{Quota}_p \times \text{Post}_t$, where Official indicates the key number of officials from a prefecture before 1905. Instead, if our finding were mainly driven by the fact that those studying for the exam simply had more time to revolt, we expect no heterogeneous effect with respect to higher returns from the exam system.

The results are presented in Table VIB. Column (1) shows that the role of pre-1905 officials did not change before and after the abolition of the exam. However, as column (2) shows, there is a weakly positive effect of $\ln(1+\text{Official})_p \times \ln \text{Quota}_p \times \text{Post}_t$. Column (3) shows that the pattern is similar after including the triple interaction of officials, the post dummy, and population size.

In addition, columns (4) and (5) control for the possible effect of officials in office in 1905. If anything, the number of key officials in office in 1905 weakly dampens the effect of quotas after the abolition. This finding is consistent with our discussion of historical background that existing top elites may not necessarily have lost out from exam abolition and may even have benefited from its hindrance of competition from commoners.

A Simple Model on Prospect and Revolution

Given the two sets of evidence for prospect, we construct a simple model of revolution participation to highlight the role of prospect of upward mobility (or perceived mobility). Borrowing several elements from the riots model of Passarelli and Tabellini (2013), our model provides a reduced-form means of capturing the change in prospects for upward mobility, which can also be interpreted as a change in the expected returns on exam investment.[35] This formalization also delivers additional predictions that can be further tested.

There are two types of agents in the economy: the commoners with income w_0 and the small group of rich elites with income w_1, where $w_1 > w_0$. Under the status quo without revolution, a commoner perceives that he will become a rich

[35]Clearly, this is not the only model to formalize this interpretation. For instance, Leventoglu (2005) introduced mobility to the political transition framework in Acemoglu and Robinson (2001), which can also be interpreted as the role of elite eligibility.

TABLE VIB

THE ROLE OF PROSPECT II: THE IMPACT OF HETEROGENEOUS RETURNS FROM THE EXAM (D.V.: REVOLUTIONARY = 0/1)[a]

	(1)	(2)	(3)	(4)	(5)
ln(1 + Official pre-1905) × ln Quota × Post		0.044*	0.047	0.059*	0.076**
		(0.025)	(0.032)	(0.036)	(0.036)
ln(1 + Official in 1905) × ln Quota × Post				−0.064	−0.155*
				(0.076)	(0.093)
ln Quota × Post	0.100**	0.148***	0.150**	0.151**	0.152**
	(0.045)	(0.055)	(0.061)	(0.062)	(0.061)
ln(1 + Official pre-1905) × Post	0.014	−0.005	−0.006	−0.006	−0.005
	(0.025)	(0.027)	(0.028)	(0.032)	(0.032)
ln(1 + Official in 1905) × Post				0.018	−0.005
				(0.060)	(0.061)
ln(1 + Official pre-1905) × ln Pop × Post			−0.004	−0.004	−0.022
			(0.034)	(0.034)	(0.038)
ln(1 + Official in 1905) × ln Pop × Post					0.106
					(0.082)
Prefecture FE, Year FE	Y	Y	Y	Y	Y
Province FE × Year FE	Y	Y	Y	Y	Y
Baseline controls × Post	Y	Y	Y	Y	Y
Observations	1,834	1,834	1,834	1,834	1,834
R-squared	0.464	0.466	0.466	0.467	0.468

[a]This table shows that the impact of quotas after the abolition is amplified by higher returns from the exam system. The baseline controls include (i) third-degree polynomials of logged population in 1880, and logged area; (ii) whether a prefecture is located on the coast and whether it is located on a major river; (iii) whether a prefecture has a treaty port; and (iv) whether a prefecture was counted as a big city, a middle-size city, or a small city. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

elite in the next period with probability $\eta_0(q)$ and stay poor with probability $1-\eta_0(q)$, where q indicates quotas per capita and η_0 is increasing in q.[36] The abolition of the exam can be considered as a decease in $\frac{\partial \eta_0(q)}{\partial q}$.

The commoner decides whether or not to participate in the revolution. If the revolution succeeds, the probability of becoming rich becomes $\eta_1(q)$ instead of $\eta_0(q)$. Joining a revolution is costly. The cost is the sum of two components: $\mu+\varepsilon^i$, where μ is known and common to all agents and ε^i reflects individual heterogeneity. ε^i follows a distribution $G(\varepsilon)$, which is continuous and has density $g(\varepsilon)$.

Following Passarelli and Tabellini (2013), we use a simplified way of capturing the complementarity in participation, namely, that the benefit of participation grows proportionately with the number of other members also partici-

[36]Similarly, a rich agent perceives that he will become poor in the next period with probability η_0^r and stay as rich with probability $1-\eta_0^r$. The participation of such agents in the revolution is trivial.

pating in the revolution, p. λ captures how easy it is to overcome the collective action problem. One natural measure of λ is social capital: in prefectures with more social capital, it is easier for people to coordinate and overcome the collective action problem. Another way of interpreting λ is that the value of revolution is higher or the probability of being reported to the government is lower for people with stronger group identity.

Given the cost and benefit of revolution, a poor agent i participates in the revolution if

$$p\lambda\big[\eta_1(q)-\eta_0(q)\big](w_1-w_0)-\mu-\varepsilon^i\geq 0.$$

The probability of participation becomes a fixed point of the following condition:

$$p=G\big(p\lambda\big[\eta_1(q)-\eta_0(q)\big](w_1-w_0)-\mu\big). \tag{3}$$

We focus on the interior solution.[37] The revolution condition gives the following comparative statics:

$$\frac{\partial p}{\partial q}=\frac{gp^*\lambda(w_1-w_0)\left[\dfrac{\partial\eta_1(q)}{\partial q}-\dfrac{\partial\eta_0(q)}{\partial q}\right]}{1-g\lambda(\eta_1-\eta_0)(w_1-w_0)}. \tag{4}$$

Under the exam system, equation (4) captures the impact of quotas per capita on revolution participation. After the abolition of the exam, the link between quotas per capita and upward mobility gets changed: $\frac{\partial\eta_0(q)}{\partial q}$ is changed to be $\frac{\partial\eta_0'(q)}{\partial q}$. As a result, the comparative statics after the abolition of the exam becomes

$$\frac{\partial p'}{\partial q}=\frac{gp^*\lambda(w_1-w_0)\left[\dfrac{\partial\eta_1(q)}{\partial q}-\dfrac{\partial\eta_0'(q)}{\partial q}\right]}{1-g\lambda(\eta_1-\eta_0)(w_1-w_0)}. \tag{5}$$

The difference between equations (5) and (4) gives the impact of quotas on the participation before and after the abolition of the exam:

$$\frac{\partial p'}{\partial q}-\frac{\partial p}{\partial q}=\frac{gp^*\lambda(w_1-w_0)}{1-g\lambda(\eta_1-\eta_0)(w_1-w_0)}\left[\frac{\partial\eta_0(q)}{\partial q}-\frac{\partial\eta_0'(q)}{\partial q}\right]>0. \tag{6}$$

As discussed in the previous subsection, $\frac{\partial\eta_0'(q)}{\partial q}<\frac{\partial\eta_0(q)}{\partial q}$. Thus, equation (6) implies that individuals of status w_0 are more likely to participate in the revolution in prefectures with a higher q after the abolition of the exam. This prediction is consistent with the difference-in-differences strategy in our base-

[37]Similarly to Passarelli and Tabellini (2013), we assume $\lambda(\eta_1-\eta_0)(w_1-w_0)g(p\lambda(\eta_1-\eta_0)\times(w_1-w_0)-\mu)<1$ to rule out the case of multiple equilibria.

line analysis (the results in Table II): the first difference is with respect to q and the second difference is with respect to the abolition of the exam.

This simple model also delivers a few other predictions. For example, the impact of q is strengthened by inequality $(w_1 - w_0)$. We do not have information on inequality in this period. But in a broad sense, the finding on the amplification effect of higher returns from the exam (the results in Table VIB) is consistent with the role of $(w_1 - w_0)$.

In addition, the model predicts that the impact of q is strengthened by social capital (λ). Next, we explore variations in social capital across prefectures to test whether the impact of quotas was strengthened by social capital.

Evidence on the Role of Social Capital

λ in the model captures the role of social capital and can be interpreted to measure how easy it is to coordinate in revolution participation or how strong group identity is. A higher λ can increase the expected benefits of revolution or decrease the probability of being reported to the government. One inverse measure for λ is language diversity explored in the ethno-linguistic fragmentation literature (Alesina and La Ferrara (2000)). Specifically, we calculate an ethno-linguistic fragmentation (ELF) measure proposed by Alesina and La Ferrara (2005b): $ELF = 1 - \sum_{i=1}^{N} s_i^2$, where s_i represents the share of dialect i over the total area in a prefecture. The information on dialects comes from the Language Atlas of China (Wurm et al. (1987)). Moreover, considering that it may be riskier to participate when there are two large distinct groups than when there are many small groups, we borrow the polarization index (PI) in the ethnic conflict literature (Garcia-Montalvo and Reynal-Querol (2002)), where $PI = 1 - \sum_{i=1}^{N} s_i(\frac{1/2 - s_i}{1/2})^2$.

To test whether the impact of quotas was strengthened by social capital, we examine the triple effect of logged quotas per capita, the abolition timing dummy, and the measures of linguistic fragmentation or polarization. The results are presented in Table VII. Columns (1)–(4) report the results using the fragmentation index. One standard deviation increase in the fragmentation index (0.09) decreases the effect of $\ln \text{Quota}_p \times \text{Post}_t$ by about one third. Columns (5)–(8) present the results using the polarization measure. One standard deviation increase in the polarization index (0.16) also decreases the effect of $\ln \text{Quota}_p \times \text{Post}_t$ by about one third.

Both groups of results show a smaller effect of quotas following exam abolition in prefectures with lower social capital, consistent with an ability of social capital to strengthen the impact of quotas. This finding contributes to a large literature on the economic and political impacts of social capital.[38]

[38]We will not give an overview of the vast literature here, but would like to point out that this role of social capital in revolution can be related to Satyanath, Voigtlander, and Voth (2013) who showed that social capital facilitated the rise of the Nazi Party. Naturally, the impact of the republic revolution was very different from that of the Nazi Party.

TABLE VII

EXAMINING THE IMPACT OF SOCIAL CAPITAL (D.V.: REVOLUTIONARY = 0/1)[a]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fragmentation index × ln Quota × Post			−0.520**	−0.447**				
			(0.213)	(0.216)				
Polarization index × ln Quota × Post							−0.278**	−0.247**
							(0.115)	(0.114)
ln Quota × Post	0.135***	0.110**	0.133***	0.110**	0.134***	0.110**	0.134***	0.111**
	(0.044)	(0.046)	(0.043)	(0.046)	(0.044)	(0.046)	(0.043)	(0.046)
Fragmentation index × Post	−0.082	−0.053	0.021	0.024				
	(0.134)	(0.141)	(0.151)	(0.153)				
Polarization index × Post					−0.047	−0.045	−0.002	−0.011
					(0.076)	(0.080)	(0.082)	(0.084)
Fragmentation index × ln Pop × Post			0.190	0.147				
			(0.143)	(0.155)				
Polarization index × ln Pop × Post							0.121	0.103
							(0.087)	(0.091)
Prefecture FE, Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Province FE × Year FE	Y	Y	Y	Y	Y	Y	Y	Y
$(\ln \text{Pop})^{1,2,3}$ × Post	Y	Y	Y	Y	Y	Y	Y	Y
Other baseline controls × Post		Y		Y		Y		Y
Observations	1,834	1,834	1,834	1,834	1,834	1,834	1,834	1,834
R-squared	0.454	0.464	0.458	0.467	0.454	0.464	0.458	0.467

[a]The table shows that the impact of quotas was smaller in regions with lower social capital. The other baseline controls include (i) logged area; (ii) whether a prefecture is located on the coast and whether it is located on a major river; (iii) whether a prefecture has a treaty port; and (iv) whether a prefecture was counted as a big city, a middle-size city, or a small city. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

4.2. *Elite Resistance*

Besides the would-be elites, existing elites could also be affected by the abolition and contribute to revolution in two ways. First, they could participate in revolution because their prospect of upward mobility was negatively affected. Second, they played an important role in organizing local affairs including providing public goods. Even if they did not participate in the revolution themselves, they might have become less willing to contribute to local affairs so that other individuals became more likely to participate in the revolution. We examine the two possibilities.

The Prospect for the Existing Elites

In our baseline, we use $\frac{\text{Quota}}{\text{Pop}}$ to measure the prospect for the would-be elites. Similarly, we use $\frac{\text{PresentedScholar}}{\text{Quota}}$ and $\frac{\text{Official}}{\text{PresentedScholar}}$ to measure the prospect for the existing elites. Replace logged quotas per capita in the baseline with these measures; we present the results in Table VIIIA.

Column (1) shows a positive and significant impact of $\frac{\text{Quota}}{\text{Pop}}$ after the abolition. The standard deviation of this measure (multiplied by 100 to facilitate the reading of the coefficient) is 2.39. Thus, a one standard deviation increase in this measure implies a seven percentage point higher probability of revolution participation, comparable to our baseline estimates using logged quotas per

TABLE VIIIA

THE ROLE OF EXISTING ELITES I: IMPORTANCE OF PROSPECT (D.V.: REVOLUTIONARY = 0/1)[a]

	(1)	(2)	(3)	(4)
(100 × Quota/Pop) × Post	0.031***			0.038***
	(0.011)			(0.013)
(100 × PresentedScholar/Quota) × Post		0.040		0.022
		(0.073)		(0.073)
(Official/PresentedScholar) × Post			−0.016	0.014
			(0.068)	(0.071)
Prefecture FE, Year FE	Y	Y	Y	Y
Province FE × Year FE	Y	Y	Y	Y
Baseline controls × Post	Y	Y	Y	Y
Observations	1,834	1,834	1,778	1,778
R-squared	0.465	0.460	0.461	0.467

[a]This table shows that what mattered for revolution participation is the prospect for those who had not become elites rather than that for the existing elites. The baseline controls include (i) third-degree polynomials of logged population in 1880, and logged area; (ii) whether a prefecture is located on the coast and whether it is located on a major river; (iii) whether a prefecture has a treaty port; and (iv) whether a prefecture was counted as a big city, a middle-size city, or a small city. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

capita. This result shows that our baseline estimate is also robust to using this alternative functional form of quotas per capita.

Columns (2) and (3) show that neither $\frac{\text{PresentedScholar}}{\text{Quota}}$ nor $\frac{\text{Official}}{\text{PresentedScholar}}$ has a similar impact to that of $\frac{\text{Quota}}{\text{Pop}}$. Column (4) includes all three measures. Once again, the positive impact is driven by $\frac{\text{Quota}}{\text{Pop}}$.

These results reveal that what mattered for our baselines results is the entry-level quotas. This finding is not surprising, considering that the existing elites were limited in number and could easily adapt to the new system. However, as documented in Table VIB, the prospect at the upper levels can affect the expected returns for the would-be elites and amplify the impact of quotas at the entry level.

The Importance of Public Goods Provision by the Elites

It could also be hypothesized that the gentry's important role in providing such public goods as schooling, irrigation projects, and disaster relief (Chang (1955)) would have been more important in prefectures with higher quotas, leading these to respond more aggressively to exam abolition through revolutionary participation.

To test whether this hypothesis explains our main finding, we need to measure the importance of public goods, but historical data on this aspect are rare. Since historical China was an agrarian economy vulnerable to weather shocks, it is reasonable to assume that the contribution of public goods by the local gentry was more important in regions that suffered more frequently from droughts and floods. We thus employ a long-term measure of weather volatility to proxy how vulnerable a prefecture is, namely, the standard deviation of weather indicators during the 19th century (1800–1899).[39] As shown in columns (1)–(2) of Table VIIIB, the importance of public goods proxied by weather volatility cannot explain our main findings. For completeness, we also include weather shocks in the short run. Columns (3)–(4) further show that short-run weather shocks cannot explain our main findings.

Therefore, neither the prospect of the existing elite nor the importance of public goods proxied by weather volatility can explain our main findings.

4.3. *Modern Human Capital*

Another hypothesis to explain our findings is that individuals were exposed to modernization ideology and participated in the revolution to modernize China. Since prefectures with higher quotas per capita might also be rich in modern human capital, we would like to check whether the effect of quotas per capita mainly reflects the role of modern human capital.

[39]The data come from the State Meteorological Society (1981) and are also used in the placebo tests for the instrument.

TABLE VIIIB

THE ROLE OF EXISTING ELITES II: IMPORTANCE OF PUBLIC GOODS
(D.V.: REVOLUTIONARY = 0/1)[a]

	S.D. of Indicators 1800–1899		Drought/Flood in Year t	
	(1)	(2)	(3)	(4)
ln Quota × Post	0.109**	0.116**	0.102**	0.108**
	(0.046)	(0.050)	(0.046)	(0.047)
Weather × Post	0.007	−0.018	0.120	0.081
	(0.086)	(0.093)	(0.096)	(0.103)
Weather × ln Quota × Post		−0.047		0.163
		(0.151)		(0.180)
Weather × ln Pop × Post		0.107		−0.080
		(0.113)		(0.123)
Weather			0.026	0.050
			(0.026)	(0.031)
Weather × ln Quota				−0.092*
				(0.047)
Weather × ln Pop				0.054
				(0.049)
Prefecture FE, Year FE	Y	Y	Y	Y
Province FE × Year FE	Y	Y	Y	Y
Baseline controls × Post	Y	Y	Y	Y
Observations	1,834	1,834	1,834	1,834
R-squared	0.464	0.465	0.466	0.467

[a]The table shows that the demand for public goods provision by the elites (proxied by the volatility of weather conditions) did not have any significant impact on revolution participation. The baseline controls include (i) third-degree polynomials of logged population in 1880, and logged area; (ii) whether a prefecture is located on the coast and whether it is located on a major river; (iii) whether a prefecture has a treaty port; and (iv) whether a prefecture was counted as a big city, a middle-size city, or a small city. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

We construct two measures on modern human capital: the number of students studying in Japan between 1900 and 1906, and the number of modern firms between 1900 and 1906. Conceptually, these measures could also mitigate the impact of the abolition on revolution because they capture alternative mobility channels. What we can evaluate is only the net effect.

In Table IX, we first examine whether these two proxies change in response to quotas and exam abolition and then check whether they can explain our main findings. Columns (1)–(4) report the results using a dummy for the presence of a new modern firm (individual studying in Japan) and the logged number of new firms (individuals studying in Japan). Although often not significant, the estimates all have positive signs. In fact, in unreported estimates based on data in the 1910s, the effect actually becomes significant. These results suggest that quotas did indeed have an impact on modern human capital after exam abolition but that the effect was minimal in the short run.

TABLE IX

EVALUATING THE IMPACT OF MODERN HUMAN CAPITAL[a]

D.V.:	Modern Firms		To Study in Japan		Revolution				
Measure:	0/1	ln(1 + Firm)	0/1	ln(1 + Japan Student)	0/1				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ln Quota × Post	0.023	0.032	0.066	0.103*		0.111**		0.097**	0.099**
	(0.025)	(0.024)	(0.044)	(0.055)		(0.046)		(0.043)	(0.044)
ln(1 + Firm)					0.067	0.073			0.045
					(0.085)	(0.084)			(0.084)
ln(1 + Firm) × Post					−0.073	−0.095			−0.071
					(0.126)	(0.122)			(0.122)
ln(1 + Japan Student)							0.119***	0.121***	0.118***
							(0.035)	(0.035)	(0.035)
ln(1 + Japan Student) × Post							0.009	−0.004	−0.000
							(0.054)	(0.053)	(0.052)
Prefecture FE, Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Province FE × Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Baseline controls × Post	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,834	1,834	1,834	1,834	1,834	1,834	1,834	1,834	1,834
R-squared	0.187	0.244	0.455	0.588	0.460	0.465	0.473	0.477	0.477

[a]The table shows that modern human capital contributed to revolution but its effect cannot explain the role of quotas. The baseline controls include (i) third-degree polynomials of logged population in 1880, and logged area; (ii) whether a prefecture is located on the coast and whether it is located on a major river; (iii) whether a prefecture has a treaty port; and (iv) whether a prefecture was counted as a big city, a middle-size city, or a small city. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

To further examine whether our baseline results are driven by the role of modern capital, we include the interactions of these modern human capital proxies and the post-abolition dummy in our analysis of quotas and revolution participation. As shown in columns (5) and (7), the effect of the modern human capital did not change before and after the abolition. Columns (6), (8), and (9) further show that our baseline finding on $\ln \text{Quota} \times \text{Post}$ changes little by including these factors, implying that they cannot explain the effect of quotas documented. Across specifications, the number of students studying in Japan displays a strong and positive effect on revolution participation. This finding provides some quantitative evidence for the modernization hypothesis in Huntington (1968).

In sum, we find that modern human capital proxied by the number of students studying in Japan contributed to the revolution but it cannot explain the role of quotas before and after the abolition in our short-run analysis. However, it is possible that the effect of modern human capital became more relevant in the later years for which there are little systematic data on revolution participation.

4.4. *State Capacity*

A different interpretation of our findings is that they reflect the role of the state during the period studied rather than any group of the citizens; in particular, a deterioration in state capacity that exam abolition could either signal or be driven by a dramatic deterioration in state capacity. If signaling were the main dynamic, then other historical events of the period capable of signaling such deterioration would have had a similar impact on revolutionary participation. As shown in Section 3.3, however, these events had no such impact. We further examine whether two other measures related to the role of the state—tax revenues and the importance of political control—could explain our results.

Using Information on Tax Revenues

One measure of state capacity is tax revenues. Land tax has been the mainstay of government finance in imperial China. In the late Qing period, the government also drew a large share of revenues from indirect taxes levied on nonagricultural activities, particularly internal tariffs (*likin*) and customs duties. In this period, land tax accounted for around 35% of the total tax revenues while the other two indirect taxes accounted for about 45%. This reliance on indirect taxes can be considered indicative of a deterioration in state capacity (Besley and Persson (2011)). Although information on prefecture-level taxes is very limited, we are able to examine the correlation between quotas and changes in the different tax revenues before and after exam abolition at the provincial level.

TABLE XA

CHECKING CHANGES IN STATE CAPACITY I: EXAMINING TAXES[a]

D.V.:	ln(Land Tax) 1893, 1903, 1908			ln(Non-Agricultural Tax) 1891–1908		
	(1)	(2)	(3)	(4)	(5)	(6)
ln Quota × Post	−0.067	−0.051	−0.045	−0.333	−0.088	0.061
	(0.170)	(0.138)	(0.096)	(0.486)	(0.417)	(0.267)
Province FE, Year FE	Y	Y	Y	Y	Y	Y
ln Pop × Post	Y	Y	Y	Y	Y	Y
Other controls × Post		Y	Y		Y	Y
$(\ln \text{Pop})^{2,3}$ × Post			Y			Y
Observations	52	52	52	252	252	252
R-squared	0.823	0.961	0.966	0.362	0.525	0.558

[a]This table shows that provincial-level taxes did not respond to the interaction of quotas and the abolition. Other controls include (i) logged area of a province; (ii) the number of prefectures located on the coast or a major river in a province; (iii) the number of prefectures being treaty ports; and (iv) the number of prefectures counted as big cities, middle-size cities, or small cities. Standard errors in parentheses are clustered at the province level: *significant at 10%; **significant at 5%; ***significant at 1%.

For land taxes, we collect province-level information in 1893, 1903, and 1908.[40] With this information, we replace the dependent variable of revolution participation in the baseline estimation with logged land taxes. The results are reported in columns (1)–(3) of Table XA. They show that the correlation between land taxes and quotas did not change discontinuously with the abolition of the exam.

For internal tariffs and customs duties, we collect yearly information at the provincial level during 1891–1908.[41] Similarly to land taxes, we examine how the interaction of quotas and the post dummy affects the revenues from non-agricultural activities. As presented in columns (4)–(6) of Table XA, the correlation between non-agricultural taxes and quotas did not change discontinuously with the abolition of the exam either. Table A.VIII of the Supplemental Material reports the estimates year by year. The pattern is similar to that in Table XA, thereby confirming the lack of any close relation between quotas and the change in tax revenues.

Employing Importance of Political Control

If a deterioration in state capacity were the main driver of our findings, we would expect to see a larger impact in regions where political control was more

[40]The data come from Wang (1973) and Liang (1981).

[41]The data on tariffs come from Luo (1936) and the data on customs from Tang (1992). For customs duties, we match yearly data on the revenues for each customs with the province-level information.

important. Thanks to the availability of rich historical information, we can measure the importance of political control in various ways: (i) the land tax per capita in 1820, (ii) whether a prefecture is a provincial capital, and (iii) the designations by the government indicating the characteristics of a prefecture: *chong* (important in transportation/communication), *fan* (important in business), *pi* (difficult to gather taxes), and *nan* (high in crimes).

These results are presented in Table XB. Among these characteristics, only the difficulty in taxing had a weak and positive impact on revolution participation following the abolition of the exam. As they show, the impact of quotas varies little after controlling for these factors and their interactions with the post dummy. Therefore, these measures cannot explain the impact of quotas. If our assumption that the decline in state capacity had a larger impact on the regions of more importance holds, this finding suggests no dramatic change in state control just before and after exam abolition.

Hence, even though state capacity was deteriorating in the last decade of the dynasty, these results suggest that our main findings are unlikely to be explained by a sudden change in state capacity in 1905.

5. CONCLUSION

By examining the abolition of China's long-lived civil service exam system, whose role as an elite recruitment system was crucial to political stability, we show that higher exam quotas per capita are associated with a higher probability of revolutionary participation following the abolition of the exam. This empirical pattern appears most consistent with the interpretation that abolition negatively impacted the incentives of the would-be elites and thus their decision to participate in revolutionary activities.

These findings make a useful contribution to the growing literature on the impact of political institutions on development. More specifically, they provide new evidence on the importance of elite recruitment in determining political stability. Even though the abolition of the historical civil service exam was unique to China, the implications of our study might be relevant for other contexts of riot and revolution. For instance, limitation of elite recruitment mechanisms may well have contributed to the French Revolution (Brinton (1938), Goldstone (1991)).

Admittedly, the abolition of the exam was only one factor in the 1911 Revolution, which, like all such revolutions, "necessarily involve[d] the alienation of many groups from the existing order" (Huntington (1968)). Nevertheless, in the process of understanding the role of elite recruitment, we also identify the impact of other relevant factors such as modern human capital and social capital, thereby opening new avenues for further research.

TABLE XB

CHECKING THE ROLE OF STATE CAPACITY II: RESULTS USING IMPORTANCE MEASURES (D.V.: REVOLUTIONARY = 0/1)[a]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ln Quota × Post	0.126**	0.107**	0.103**	0.109**	0.101**	0.107**	0.111**
	(0.052)	(0.045)	(0.046)	(0.048)	(0.046)	(0.044)	(0.053)
Taxes per capita in 1820 × Post	−0.184						−0.150
	(0.282)						(0.251)
Province capital × Post		0.099					0.118
		(0.117)					(0.114)
Communication (*chong*) × Post			0.031				0.054
			(0.049)				(0.049)
Business (*fan*) × Post				−0.003			−0.049
				(0.051)			(0.059)
Difficulty of taxing (*pi*) × Post					0.099*		0.110**
					(0.052)		(0.053)
Crime (*nan*) × Post						0.056	0.049
						(0.046)	(0.053)
Prefecture FE, Year FE	Y	Y	Y	Y	Y	Y	Y
Province FE × Year FE	Y	Y	Y	Y	Y	Y	Y
Baseline controls × Post	Y	Y	Y	Y	Y	Y	Y
Observations	1,799	1,834	1,834	1,834	1,834	1,834	1,799
R-squared	0.466	0.464	0.464	0.464	0.467	0.465	0.471

[a]This table shows that the impact of quotas cannot be explained by the importance measures. The baseline controls include (i) third-degree polynomials of logged population in 1880, and logged area; (ii) whether a prefecture is located on the coast and whether it is located on a major river; (iii) whether a prefecture has a treaty port; and (iv) whether a prefecture was counted as a big city, a middle-size city, or a small city. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

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Econometrica Supplementary Material

SUPPLEMENT TO "ELITE RECRUITMENT AND POLITICAL STABILITY: THE IMPACT OF THE ABOLITION OF CHINA'S CIVIL SERVICE EXAM"

(*Econometrica*, Vol. 84, No. 2, March 2016, 677–733)

BY YING BAI AND RUIXUE JIA

APPENDIX

A.1. *Results on the Party Identification*

WE EXPLORE PARTY IDENTIFICATION of the parliament members in 1912 as another check on ideology. Compared with the other parties—the Republican Party (*Kunghotang*) and the Democratic Party (*Minzhutang*)—the KMT (*Kuomintang* literally means "Chinese Nationalist Party") was known to be more radical and more pro-redistribution (Chang (1985)). In contrast, the party ideology of the *Kunghotang* was based on Jean-Jacques Rousseau's The Social Contract, whereas the *Minzhutang* emphasized that stability was their primary goal.

We link quotas per capita to the parliament members' party identification in 1912, using the following cross-sectional specification:

$$\mathrm{KMT}_{i,p} = \varphi \ln \mathrm{Quota}_p + \nu \ln \mathrm{Pop}_p + \theta \mathrm{X}_p + \delta_{\mathrm{prov}} + \varepsilon_{i,p},$$

where $\mathrm{KMT}_{i,p}$ is a dummy indicating whether a party member i belongs to the *Kuomintang* or not.

The results are presented in columns (1)–(2) in Table A.V. Among the 703 party members for whom we can identify the origins and ages, 434 were identified with the KMT. Based on the individual-level information, we do not find any significant impact of quotas per capita on party identification. However, consistent with the hypothesis that the KMT was more radical, we find that younger people were more likely to identify themselves as KMT members.

Columns (3)–(8) report the results using prefecture-level information to examine the link between the quotas and the number of party members. As is shown, quotas per capita increased the party member probability for both the KMT and the other parties. This finding is expected because more revolutionaries should be associated with a higher probability of party members after the success of the revolution. However, the magnitudes of the impacts for the KMT and the other parties are not significantly different. Consistent with the placebo test using the Boxer Rebellion, the finding on party identification once

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again shows that potential ideological differences are unlikely to explain the impact of quotas.

A.2. *An Example for the Instrument*

To illustrate why the number of small rivers can be an instrument for quota per capita, one can consider two prefectures with similar population sizes but different numbers of small rivers.

Qingzhou and Xuzhou were two prefectures of similar population size and area size. Qingzhou had a population of 3.8 million and an area of 17,000 km^2, whereas Xuzhou had a population of 4 million and an area of 17,000 km^2. There were 21 small rivers in Qingzhou and 18 small rivers in Xuzhou. As a result, Qingzhou was divided into 11 counties while Xuzhou was divided into 8 counties. Due to the stepwise rule, each county was assigned a positive quota. Adding the additional quota for the whole prefecture, Qingzhou had a quota of 195, higher than that of Xuzhou (167). Consequently, the quotas per capita at the prefecture level was higher in Qingzhou (51.3 per million) than that in the Xuzhou (41.8 per million). This is the variation we explore in our analysis.

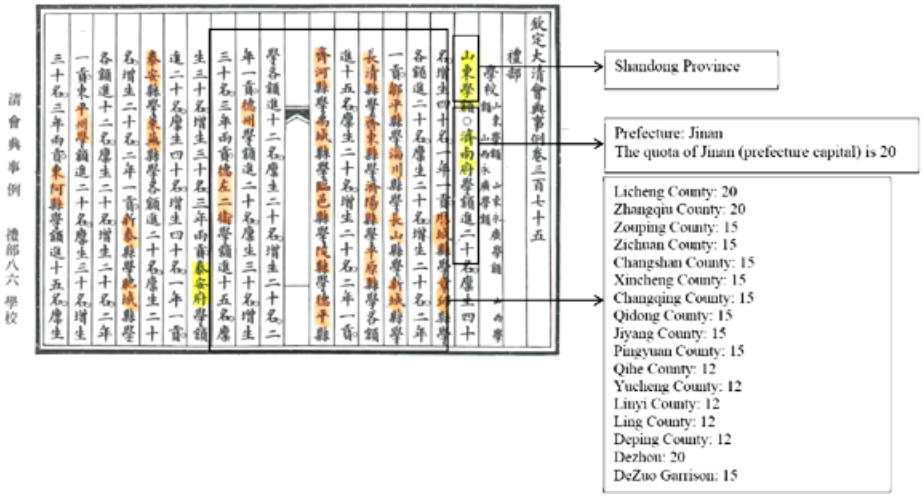
Note that this difference also holds at the county level due to the fact that county-level population sizes were smaller in Qingzhou (with more counties). There was no systematic information on the county-level population, but we can rely on average population sizes. On average, a county in Qingzhou had a population of 0.35 million while a county in Xuzhou had a population of 0.5 million. Depending on the scale and importance, counties in Qingzhou got assigned quota values of 12, 15, and 20. Thus, quotas per capita for a country in Qingzhou ranged between 34.3 per million and 57 per million. In contrast, counties in Xuzhou got assigned quota values of 16 and 20. Hence, quotas per capita for a county in Xuzhou ranged between 32 per million and 40 per million, which are likely to be lower than quotas per capita for an average county in Qingzhou.

A.3. *Varying the Definition of Small Rivers*

For robustness checks of using small rivers as an instrument, we vary the definition of small rivers to be those under the length of X km ($X = 70, 80, 90, \ldots, 120$), while controlling for the interaction of the post dummy and those above X km. These results are presented in Table A.VII. They show that the results are robust to these variations.

Moreover, we find no similar impact of the number of big rivers per se, which once again confirms that our river instrument is reasonable.

(a) Records of Quotas



(b) Roster of the Chinese Revolutionary Alliance

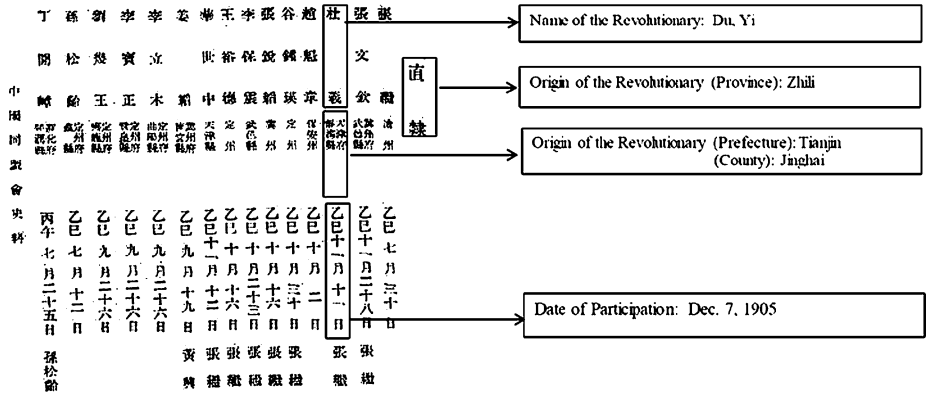


FIGURE A.1.—The data on quotas and revolutionaries. *Notes*: Panel (a) gives an example for the records of quotas for one prefecture (Jinan in Shandong Province). The quota for the prefecture capital is 20 and the total quota of the counties is 255. Thus, the total quota for the Jinan prefecture is 275. Panel (b) gives an example for the records of revolutionaries in the Chinese Revolutionary Alliance. There is information on the date of participation for the members of this group but only the year of participation for members of the other five groups. The dates were recorded in Chinese lunar calendar and are converted to dates in Gregorian calendar in our analysis.

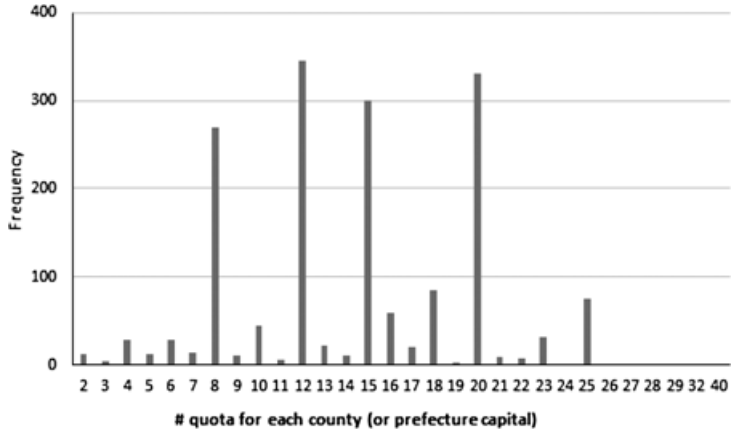


FIGURE A.2.—Distribution of quotas for each county. *Notes*: This figure shows that the quota values assigned to counties within a prefecture follow a stepwise rule: the most frequent values are 8, 12, 15, and 20. This is because the government did not have the capacity to implement a complicated proportional system and needed a simplified way of implementing the quota system.

(a) Overall Trends in the Yearly Data

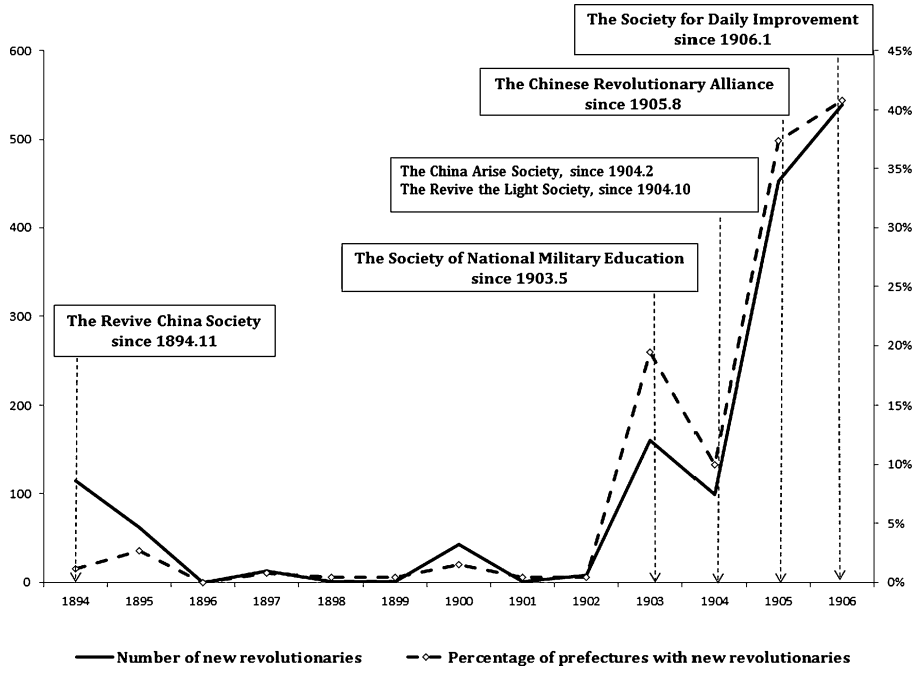


FIGURE A.3.—Aggregate trends of *new* revolutionaries. *Notes*: This figure plots the overall trends in revolution participation in the data. There tends to be an increase in revolution participation when a new group got established, such as in 1894, 1903, and 1905. The two groups established in 1904 were relatively small and only account for 4% (for the China Arise Society) and 3% (for the Revive the Light Society) of all revolutionaries between 1900 and 1906. (*Continues*.)

(b) Overall Trends in the Monthly Data

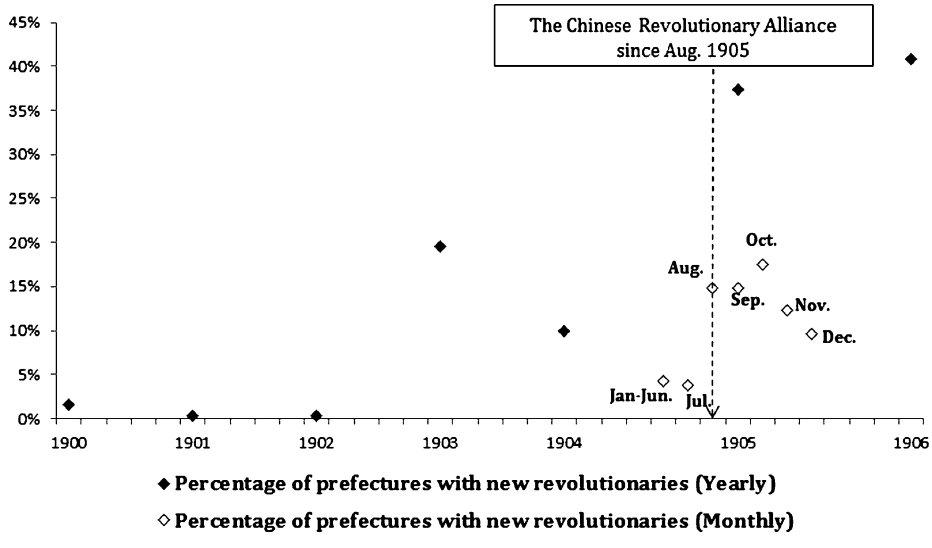


FIGURE A.3.—*Continued.*

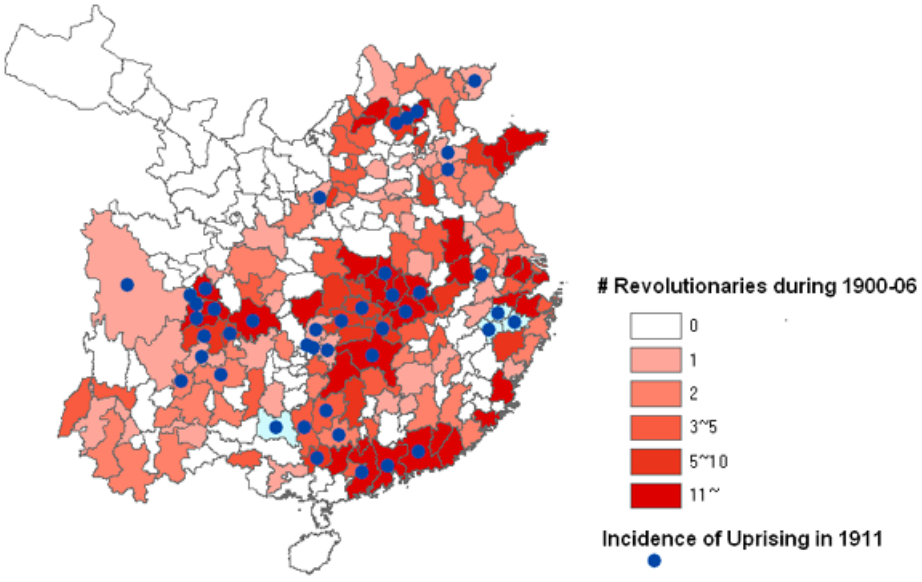


FIGURE A.4.—Revolutionaries and the 1911 Revolution. *Notes*: This map shows that the origins of revolutionaries are correlated with the incidence of uprisings in 1911.

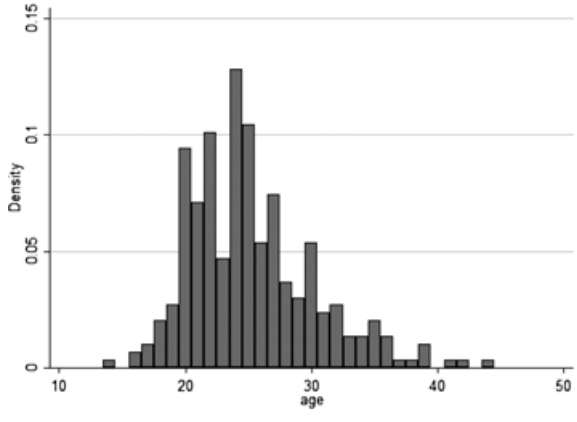
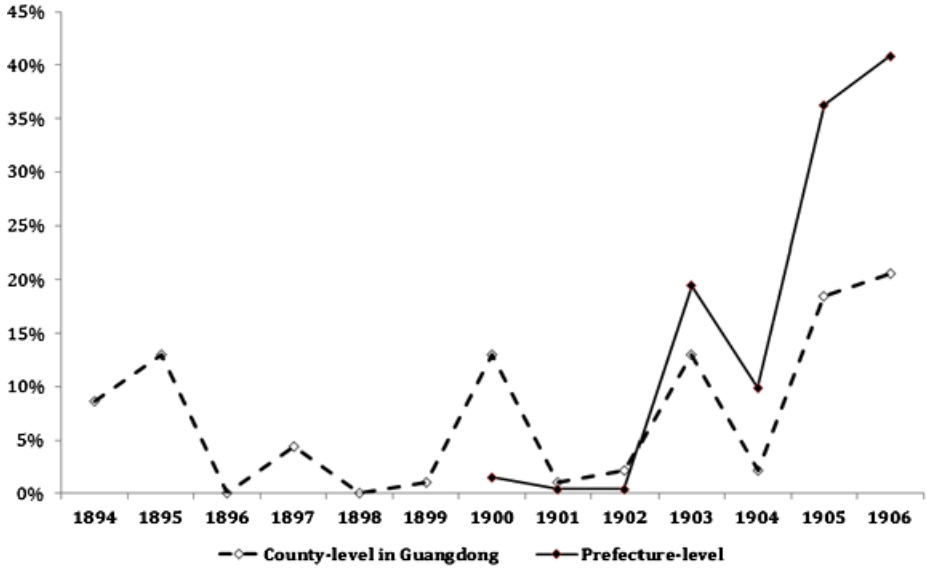


FIGURE A.5.—Age distribution of the revolutionaries in the Chinese Revolutionary Alliance. *Notes*: This figure plots the age distribution of the revolutionaries in the Chinese Revolutionary Alliance. The median is 24.

(a) Share of Counties/Prefectures With Revolutionaries



(b) Estimates from Differences-in-Differences (1894–1906)

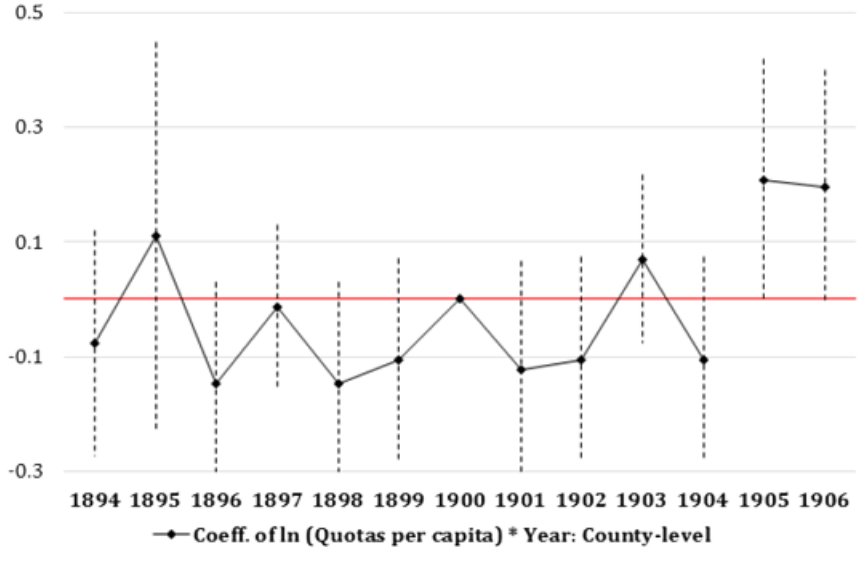


FIGURE A.6.—The results using county-year data (1894–1906). *Notes*: These figures present the pattern in the county-level data. Panel (a) plots the mean probability of revolution participation over years. Panel (b) visualizes the dynamic effects of quotas per capita on revolution participation, using the year 1900 as the reference. The solid line connects the estimates and the dashed line indicates the 95% confidence intervals.

FIGURE A.7.—Rivers and county seats. *Notes*: This map shows that county seats (indicated by the dots) are generally located on rivers. The bold rivers indicate the major ones (those ranked as the first- and second-order streams in the Chinese river hierarchy).

TABLE A.I

QUOTAS PER CAPITA AND PREFECTURE CHARACTERISTICS[a]

D.V.:	ln Quota				
	(1)	(2)	(3)	(4)	(5)
ln Pop	0.630***	0.665***	0.741***	0.667***	0.561***
	(0.042)	(0.042)	(0.092)	(0.096)	(0.110)
$(\ln \text{Pop})^2$			−0.013	−0.040	−0.012
			(0.028)	(0.026)	(0.024)
$(\ln \text{Pop})^3$			−0.023	−0.027*	−0.014
			(0.016)	(0.015)	(0.015)
ln Area			0.086	0.089	0.122
			(0.074)	(0.076)	(0.083)
Coastal			−0.364***	−0.282**	−0.166
			(0.114)	(0.120)	(0.111)
Main river				0.075	0.023
				(0.070)	(0.064)
Treaty port				−0.079	−0.062
				(0.086)	(0.087)
Small city				0.092	0.031
				(0.085)	(0.073)
Middle city				0.226**	0.123
				(0.088)	(0.085)
Large city				0.620***	0.430***
				(0.103)	(0.148)
Province capital					0.131
					(0.112)
Tax per capita in 1820					1.640**
					(0.768)
Transportation (*chong*)					0.171**
					(0.072)
Business (*fan*)					0.178**
					(0.087)
Difficulty of taxing (*pi*)					0.099
					(0.063)
Crime (*nan*)					−0.043
					(0.072)
Province FE		Y	Y	Y	Y
Observations	262	262	262	262	257
R-squared	0.579	0.728	0.747	0.765	0.794

[a]This table reports the correlations between quotas and other prefecture characteristics. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

TABLE A.II

USING THE NUMBER OF REVOLUTIONARIES AS AN ALTERNATIVE OUTCOME[a]

D.V.:	# Revolutionaries Linear	Revolutionaries per 100,000 Linear	# Revolutionaries Poisson
	(1)	(2)	(3)
ln Quota × Post	0.509**	2.912*	0.849**
	(0.242)	(1.722)	(0.422)
ln Pop × Post	1.130***	−3.238	0.698
	(0.422)	(2.265)	(0.450)
ln Area × Post	−0.145	−0.355	−1.550***
	(0.089)	(0.392)	(0.286)
Coastal × Post	−0.857	0.938	−0.554
	(0.568)	(1.191)	(0.347)
Main river × Post	0.073	0.547	−0.837***
	(0.183)	(0.648)	(0.249)
Treaty port × Post	0.409	0.895	−0.305
	(0.374)	(0.704)	(0.243)
Small city × Post	−0.142	3.196	0.440
	(0.423)	(2.713)	(0.299)
Middle city × Post	0.317	0.366	0.580**
	(0.593)	(0.862)	(0.286)
Large city × Post	0.265	−0.380	−0.277
	(1.160)	(0.940)	(0.344)
$(\ln \text{Pop})^2 \times$ City	0.868***	0.024	−0.318
	(0.231)	(0.288)	(0.223)
$(\ln \text{Pop})^3 \times$ City	0.181***	0.255	0.083
	(0.057)	(0.181)	(0.108)
Prefecture FE, Year FE	Y	Y	Y
Province FE × Year FE	Y	Y	Y
Weighted by population	Y	Y	
Observations	1,834	1,834	1,064
R-squared	0.284	0.309	

[a]This table shows that quotas also affected the number of revolutionaries after the abolition. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

TABLE A.III

THE DYNAMIC EFFECTS OF QUOTAS ON THE REVOLUTIONARY INDICATOR
(D.V.: REVOLUTIONARY = 0/1)[a]

	(1)	(2)	(3)	(4)
ln Quota × 1901	−0.003	−0.002	−0.004	0.005
	(0.011)	(0.014)	(0.015)	(0.008)
ln Quota × 1902	−0.003	−0.002	−0.004	0.005
	(0.011)	(0.014)	(0.015)	(0.008)
ln Quota × 1903	0.048	0.057	0.042	0.017
	(0.039)	(0.039)	(0.039)	(0.014)
ln Quota × 1904	0.004	0.008	−0.011	0.003
	(0.028)	(0.027)	(0.028)	(0.010)
ln Quota × 1905	0.157***	0.150***	0.136**	0.141**
	(0.051)	(0.054)	(0.055)	(0.060)
ln Quota × 1906	0.124**	0.106*	0.091	0.136**
	(0.053)	(0.056)	(0.059)	(0.062)
Prefecture FE, Year FE	Y	Y	Y	Y
Province FE × Year FE	Y	Y	Y	Y
$(\ln \text{Pop})^{1,2,3}$ × Year FE	Y	Y	Y	Y
(ln Size, Coastal, River) × Year FE		Y	Y	Y
(Urbanization, Treaty port) × Year FE			Y	Y
Weighted by population				Y
Observations	1,834	1,834	1,834	1,834
R-squared	0.460	0.469	0.479	0.411

[a]This table reports the dynamic effects of quotas per capita on the revolutionary indicator, using the year 1900 as the reference group. It shows that the effect of quotas only took place after the abolition. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

TABLE A.IV

YEAR-BY-YEAR IMPACTS ACROSS COUNTIES IN GUANGDONG (D.V.: REVOLUTIONARY = 0/1)[a]

	(1)	(2)	(3)	(4)
ln Quota × 1894	−0.087	−0.120	−0.093	−0.077
	(0.109)	(0.115)	(0.133)	(0.099)
ln Quota × 1895	0.028	0.071	0.100	0.109
	(0.137)	(0.151)	(0.178)	(0.171)
ln Quota × 1896	−0.188*	−0.211*	−0.182	−0.146
	(0.106)	(0.111)	(0.128)	(0.089)
ln Quota × 1897	−0.067	−0.056	0.015	−0.015
	(0.121)	(0.128)	(0.151)	(0.073)
ln Quota × 1898	−0.188*	−0.211*	−0.182	−0.146
	(0.106)	(0.111)	(0.128)	(0.089)
ln Quota × 1899	−0.138	−0.164	−0.118	−0.106
	(0.108)	(0.111)	(0.126)	(0.089)
ln Quota × 1901	−0.149	−0.160	−0.116	−0.124
	(0.124)	(0.138)	(0.166)	(0.096)
ln Quota × 1902	−0.149	−0.172	−0.125	−0.106
	(0.111)	(0.113)	(0.131)	(0.090)
ln Quota × 1903	0.153	0.151	0.182	0.070
	(0.108)	(0.113)	(0.136)	(0.074)
ln Quota × 1904	−0.149	−0.172	−0.125	−0.106
	(0.111)	(0.113)	(0.131)	(0.090)
ln Quota × 1905	0.224*	0.243*	0.229	0.207*
	(0.116)	(0.127)	(0.154)	(0.107)
ln Quota × 1906	0.175	0.174	0.242	0.196*
	(0.128)	(0.134)	(0.152)	(0.103)
Post- vs. Pre-	0.293***	0.313***	0.300***	0.266***
	(0.074)	(0.078)	(0.083)	(0.092)
Year FE, County FE	Y	Y	Y	Y
Prefecture FE × Year FE	Y	Y	Y	Y
$(\ln \text{Pop})^{1,2,3}$ × Year FE	Y	Y	Y	Y
(ln Size, Coastal, River) × Year FE		Y	Y	Y
(Urbanization, Treaty port) × Year FE			Y	Y
Weighted by population				Y
Observations	1,196	1,196	1,196	1,196
R-squared	0.328	0.348	0.367	0.345

[a]This table reports the dynamic effects of quotas per capita using data from 92 counties in Guangdong between 1894 and 1906, using the year of 1900 as the reference. It shows that the quotas only had an effect after the abolition. Standard errors in parentheses are clustered at the county level: *significant at 10%; **significant at 5%; ***significant at 1%.

TABLE A.V

QUOTAS AND PARTY IDENTIFICATION[a]

	Individual-Level		Prefecture-Level			Prefecture-Level		
	KMT = 0/1		KMT (0/1)	Other (0/1)	Diff.	# KMT	# Other	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln Quota	−0.027	−0.021	0.134**	0.193***	−0.059	0.391**	0.334**	0.057
	(0.044)	(0.052)	(0.054)	(0.055)	(0.076)	(0.181)	(0.131)	(0.238)
Age in 1912		−0.005*						
		(0.003)						
Province FE	Y	Y	Y	Y	Y	Y	Y	Y
$(\ln \text{Pop})^{1,2,3}$	Y	Y	Y	Y	Y	Y	Y	Y
Other baseline controls		Y	Y	Y	Y	Y	Y	Y
Observations	701	699	262	262	262	262	262	262
R-squared	0.184	0.191	0.426	0.433	0.238	0.555	0.494	0.347

[a]This table shows that quotas per capita did not affect party identification, although younger people tend to join the more radical party (the KMT), as shown in the individual-level analysis in columns (1)–(2). Columns (3)–(8) report the results using prefecture-level data: quotas per capita increase the party member probability for both the KMT and the other parties, but the impacts of quotas are not significantly different in terms of party identification. Other baseline controls include (i) logged area; (ii) whether a prefecture is located on the coast and whether it is located on a major river; (iii) whether a prefecture has a treaty port; and (iv) whether a prefecture was counted as a big city, a middle-size city, or a small city. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

TABLE A.VI

EXAMINING THE IMPACT OF INTERNATIONAL INFLUENCE[a]

	(1)	(2)	(3)	(4)	(5)
ln Quota × Post	0.101**	0.113**	0.108**	0.118**	0.109**
	(0.046)	(0.045)	(0.046)	(0.046)	(0.046)
Newspaper per capita × Post	4.419				2.720
	(14.931)				(15.525)
Newspaper per capita × ln Quota × Post	−0.485				0.824
	(14.403)				(17.579)
Newspaper per capita × ln Pop × Post	−1.176				1.426
	(5.387)				(6.834)
Enclave × Post		−0.137		0.097	0.004
		(0.244)		(0.425)	(0.468)
Enclave × ln Quota × Post		0.030		−0.602	−0.567
		(0.304)		(0.669)	(0.724)
Enclave × ln Pop × Post		−0.048		0.133	0.024
		(0.118)		(0.212)	(0.280)
Japanese enclave × Post			−0.262	−0.375	−0.228
			(0.322)	(0.510)	(0.506)
Japanese enclave × ln Quota × Post			0.347	0.903	0.680
			(0.348)	(0.719)	(0.742)
Japanese enclave × ln Pop × Post			−0.104	−0.241	−0.080
			(0.150)	(0.244)	(0.359)
Prefecture FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Province FE × Year FE	Y	Y	Y	Y	Y
Baseline controls × Post	Y	Y	Y	Y	Y
Observations	1,834	1,834	1,834	1,834	1,834
R-squared	0.465	0.466	0.465	0.467	0.471

[a]The table shows that foreign penetration cannot explain our main findings. The baseline controls include (i) third-degree polynomials of logged population in 1880, and logged area; (ii) whether a prefecture is located on the coast and whether it is located on a major river; (iii) whether a prefecture has a treaty port; and (iv) whether a prefecture was counted as a big city, a middle-size city, or a small city. Standard errors in parentheses are clustered at the prefecture level: *significant at 10%; **significant at 5%; ***significant at 1%.

TABLE A.VII

ROBUSTNESS CHECKS OF USING SMALL RIVERS (D.V.: REVOLUTIONARY = 0/1)[a]

Small Rivers:	≤70 km	≤80 km	≤90 km	≤100 km	≤110 km	≤120 km
	(1)	(2)	(3)	(4)	(5)	(6)
ln Quota × Post	0.253***	0.252***	0.243***	0.241***	0.246**	0.237**
	(0.090)	(0.089)	(0.092)	(0.093)	(0.096)	(0.098)
# rivers (>70 km)/River length × Post	−0.081					
	(0.075)					
# rivers (>80 km)/River length × Post		−0.067				
		(0.091)				
# rivers (>90 km)/River length × Post			−0.116			
			(0.102)			
# rivers (>100 km)/River length × Post				−0.094		
				(0.103)		
# rivers (>110 km)/River length × Post					−0.012	
					(0.111)	
# rivers (>120 km)/River length × Post						−0.068
						(0.124)
Baseline controls × Post	Y	Y	Y	Y	Y	Y
ln(River length) × Post	Y	Y	Y	Y	Y	Y
Placebo variables × Post	Y	Y	Y	Y	Y	Y
Prefecture FE, Year FE	Y	Y	Y	Y	Y	Y
Province FE × Year FE	Y	Y	Y	Y	Y	Y
Observations	1,834	1,834	1,834	1,834	1,834	1,834
R-squared	0.459	0.459	0.460	0.460	0.459	0.460

[a]This table shows that the results using the number of smaller rivers as an instrument are robust to variations in defining smaller rivers. The baseline controls include (i) third-degree polynomials of logged population in 1880, and logged area; (ii) whether a prefecture is located on the coast and whether it is located on a major river; (iii) whether a prefecture has a treaty port; and (iv) whether a prefecture was counted as a big city, a middle-size city, or a small city. The placebo variables are the transportation importance, crop suitability, climate shocks, and basin fragmentation. Standard errors in parentheses: *significant at 10%; **significant at 5%; ***significant at 1%.

TABLE A.VIII

EXAMINING TAXES YEAR BY YEAR[a]

	ln(Land Tax)		ln(Non-Agricultural Tax)	
Reference Year:	1893	1893	1891	1891
	(1)	(2)	(3)	(4)
ln Quota × 1892			−0.113*	−0.098
			(0.052)	(0.167)
ln Quota × 1893			0.077	0.072
			(0.149)	(0.344)
ln Quota × 1894			−0.047	−0.235
			(0.141)	(0.248)
ln Quota × 1895			0.405*	0.365
			(0.202)	(0.367)
ln Quota × 1896			0.185	−0.102
			(0.234)	(0.383)
ln Quota × 1897			0.178	−0.054
			(0.242)	(0.463)
ln Quota × 1898			0.052	−0.103
			(0.232)	(0.639)
ln Quota × 1899			−0.146	−0.679*
			(0.223)	(0.347)
ln Quota × 1900			−0.507*	−1.104**
			(0.250)	(0.421)
ln Quota × 1901			−0.348	−0.749
			(0.409)	(0.499)
ln Quota × 1902			0.381	0.251
			(0.826)	(0.873)
ln Quota × 1903	−0.233**	−0.067	0.378	0.382
	(0.110)	(0.145)	(0.766)	(0.930)
ln Quota × 1904			0.273	−0.079
			(0.819)	(0.601)
ln Quota × 1905			0.047	−0.505
			(0.838)	(0.669)
ln Quota × 1906			−0.271	−0.167
			(0.722)	(0.989)
ln Quota × 1907			−0.413	−0.176
			(0.686)	(0.899)
ln Quota × 1908	−0.172	−0.088	−0.474	−0.112
	(0.147)	(0.218)	(0.761)	(1.069)
Province FE, Year FE	Y	Y	Y	Y
ln Pop × Year FE	Y	Y	Y	Y
Other baseline controls × Year FE		Y		Y
Observations	52	52	252	252
R-squared	0.829	0.982	0.402	0.844

[a]This table shows that provincial-level taxes did not respond to the interaction of quotas and the abolition. Other baseline controls include (i) logged area of a province; (ii) the number of prefectures located on the coast or a major river in a province; (iii) the number of prefectures being treaty ports; and (iv) the number of prefectures counted as big cities, middle-size cities, or small cities. Standard errors in parentheses are clustered at the province level: *significant at 10%; **significant at 5%; ***significant at 1%.

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