The Literary Inquisition: The Persecution of Intellectuals and Human Capital Accumulation in China

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Abstract

Imperial China used an empire-wide system of examinations to select civil servants. Using a semiparametric matching-based difference-in-differences estimator, we show that the persecution of scholar-officials led to a decline in the number of examinees at the provincial and prefectural level. To explore the long-run impact of literary inquisitions we employ a model to show that persecutions could reduce the provision of basic education and have a lasting effect on human capital accumulation. Using the 1982 census we find that literary inquisitions reduced literacy by between 2.25 and 4 percentage points at a prefectural level in the early 20th century. This corresponds to a 69% increase in the probability of an individual being illiterate. Prefectures affected by the literary inquisition had a higher proportion of workers in agriculture until the 1990s.

Keywords: China, Human Capital, Institutions, Persecutions, Persistence

JEL Codes: N45, K42, I2

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China had little in the way of religious persecution, but her literary persecutions amounted at times to inquisitions. The worst persecution came under the Manchus

Han Yu-Shan (1947)

1 Introduction

For centuries China was governed by a bureaucracy selected through competitive examinations. These exams shaped the incentive to acquire human capital until the beginning of the twentieth century. Numerous scholars have speculated about how this system affected education, scientific innovation, and intellectual inquiry.[1] This paper studies how the persecution of intellectuals shaped the incentive to acquire elite-level human capital through the examination system and explores how these persecutions affected the provision of basic education in the long-run.

In High Qing period (1660–1794) graduates of examination system faced systematic, large-scale, persecutions for the first time in Chinese history.[2] These persecutions were known as literary inquisitions. Because literary inquisitions specifically targeted examination graduates and punished them for their writing, the risk of being persecuted reduced the perceived net returns to studying for the examination system. We exploit the variation in time and space generated by these persecutions. We find that the repression of intellectual elites led to fewer individuals entering the imperial examinations. In addition to this short-run effect, we show

[1] Including Max Weber (1946, 416) and Joseph Needham (1995). Huff (1993, 275-314) explores how the imperial examination system shaped science and innovation. Mokyr (1990, 234-239) discusses the role the Chinese state and bureaucracy played in retarding innovation after 1500. Davids (2013) studies how scientific knowledge circulated in both Europe and China and how the imperial examination systems shaped the transfer of knowledge. Elman (2000) is the definitive treatment of the examination systems from the perspective of cultural history but it does not address these questions. Elements of the imperial examination system date back to the Han Dynasty (206 BCE–220 CE). However, the policy of systematic examinations to recruit the vast majority of government officials and bureaucrats was instituted in the Song dynasty (960-1279). It was in the Ming period (1368–1644) that exams became the most important form of recruitment and competition for positions became intense (Ropp, 1981, 18). The imperial examination system was abolished in 1905.

[2] One historian writing of these literary inquisitions notes that '[t]he trial and punishment of Galileo (confinement to his village overlooking Florence) is nothing compared to this' (Huff, 1993, 318). In modern China prominent victims of the literary inquisitions are still discussed in their hometowns today (see, for instance, Luo, 2008). The literary inquisitions of the Qing era were different to previous episodes where on occasion scholar-officials were punished for being involved in a conspiracy against the emperor.

that the repression of the 'upper tail' of the human capital distribution had long-run consequences; it reduced the provision of basic education at the beginning of the twentieth century and led to a higher proportion of workers in agriculture at a prefectural level until the 1990s.

To conduct our analysis, we employ several historical datasets, including data on 1,898 successful examination candidates in the imperial bureaucracy—who we refer to as scholar-officials—and 88 literary inquisitions between 1660 and 1840.[3] Scholar-officials were magistrates and administrators who earned a salary and a pension from the central government (Wong, 2012). The exams through which they were selected was meritocratic and extremely competitive; examination candidates came from diverse socio-economic backgrounds. This system of exams aimed to select those best able to govern. Individuals who completed the exams comprised a local elite. In the periods in which they did not hold public offices, scholar-officials were the often main providers of schooling and basic education in their hometowns.[4] This meant that in the absence of state-provided education, examination graduates played an important role in the provision of education.

We study the effect of persecutions on the number of examination candidates from the home province and prefecture of the persecuted individual(s). We employ a difference-in-differences approach (DID) that exploits differences between pre-inquisition and post-inquisition outcomes in the 'treated' provinces and prefectures compared to differences in the control provinces and prefectures. At a prefectural level we employ a semiparametric matching-based difference-in-differences estimator to ensure comparability of our treatment and control groups and to best overcome selection bias.

Literary inquisitions were intended to create a climate of fear among scholar-officials. In this respect, they resembled Stalin-era trials more closely than they did persecutions in early modern Europe, which were often religious in character. We provide evidence that unlike persecutions in Europe, literary inquisitions were not correlated with conflicts and peasant rebellions, natural disasters or extreme weather events.[5] In our empirical analysis we use

[3]Our analysis finishes in 1840 due to the disruptive impact of the Taiping Rebellion which led to the suspension of the examination system in several parts of the country.

[4]Etienne Balazs described the scholar-official as 'omnipotent by reason of their strength, influence, position, and prestige, held all the power and owned the largest amount of land' (Balazs, 1964, 16). Ho observes: '[t]here can be little doubt that traditional Chinese society considered entry into the ruling bureaucracy the final goal of upward social mobility' (Ho, 1962, 92). Scholars like Baumol (1990) indict Chinese scholar-officials as rent-seekers. However, a more recent literature points out the positive contributions that they made to the local economy and society (see Deng, 2000).

[5]In the the context of the Spanish Inquisition Vidal-Robert (2013) argues external wars led to more trials and executions. Anderson et al. (2013) show that weather shocks were associated with Jewish persecutions in pre-modern Europe.

these time varying covariates to directly control for shocks that might have influenced the decision to persecute.

To deal with developments that might have affected the number of potential examination candidates, we interact a range of provincial and prefectural level characteristics with linear and quadratic time trends. To accommodate policy interventions that led to arbitrary changes in the probability of passing the exams and hence to different trends in the number of examination candidates before and after the intervention, we allow for trend breaks in 1712, the year when the policy changed. At the prefectural level we more precisely control for these policy changes by using province-specific decade fixed effects.

We find that a literary inquisition is associated with between a 40 and 50 % decline in the standard deviation of the number of officials recruited from the examination track from the victim's home province relative to the mean in each subsequent decade. At a prefectural level, we find that a literary inquisition is associated with a 27 % decrease in the standard deviation in the number of officials per decade recruited from the examination track from that individual's home prefecture in subsequent decades. These results are robust to varying our matching criteria, employing different starting years, and omitting regions that experienced different levels of migration or are otherwise outliers for a variety of reasons.

We go on to investigate the consequences that these persecutions had on human capital accumulation in the long-run. As historians have shown, before passing the next level of exams, examination candidates frequently worked as teachers in their local area. This highlights a potential mechanism linking the number of examination candidates to long-run educational outcomes: the relationship between the pool of examinees and the number of teachers. We explore this link by developing a simple model. In our model an increase in the perceived probability of persecution reduces the number of individuals who study for the exams. As a result it also reduces the pool of potential teachers in the future and therefore raises the costs of acquiring basic education in the long-run.

A challenge we face in testing the long-run impact of literary inquisitions on human capital accumulation is the absence of disaggregated literacy data for nineteenth or early twentieth century China. We overcome this problem by using a later census that reflects levels of human capital for individuals born in the last decade of the Qing dynasty. We find that individuals aged 80 or older in 1982 in prefectures that experienced literary inquisitions had a 69% higher probability of being illiterate. This corresponds to between a 2.5 and a 4 percentage points' increase in the illiteracy rate; this effect remains when we account for survivorship bias and selective migration. Finally, we show that prefectures affected by the literary inquisition also had a higher proportion of the population working in agriculture into the modern period, and

that this effect has faded away in recent decades.[6]

In studying the effects of the literary inquisition on the accumulation of human capital, our findings relate to research on the role institutions have in shaping the incentive to acquire human capital.[7] In particular we build on work that has shown that the 'upper tail' knowledge of educated elites played a crucial role in transmitting the Industrial Revolution (Squicciarini and Voigtländer, 2014).[8] Our results are in line with several studies that have established that the effects of shocks can persist for decades or even centuries, notably Becker and Woessmann (2009) who study the impact of the Reformation on the literacy and income of Protestants in nineteenth century Prussia.[9] Existing papers have shown that institutions or political events can have a long-run effect on economic outcomes including human capital accumulation. An advantage of our analysis is that we can provide evidence on the channel linking past persecutions to later outcomes. Specifically, the richness of our data on the imperial examination system allows us to study the immediate effect that literary inquisitions had in reducing the number of successful exam candidates which is our measure of elite-level human capital. We can therefore show how persecutions *first* led to a decline in elite-level human capital and then *subsequently* led to a decline in overall literacy at the end of the Qing dynasty.

Our findings are also related to other work that explores the economic effects of persecution. Acemoglu et al. (2011) examine the negative long-run consequence of the Holocaust in Russia. Their main channel is how persecutions led to a reduction in the size of the middle class which permanently changed the economic structure of particularly hard cities. Waldinger (2010, 2012) studies the negative effects of the expulsion of Jewish scientists for scientific out-

[6]In interpreting our results, it should be noted that one alternative career path for individuals who might be deterred from entering the examination system was commerce. To the extent that literary inquisitions reduced the number of examination candidates, it might have increased the number of individuals entering commerce and becoming merchants. This may have had positive economic effects and might plausibly have lead to an increase in literary rates. We cannot measure this effect due to a lack of data on commercial activities in nineteenth century China. What we measure is the total effect of a literacy inquisition on literacy levels. Our findings suggest that the negative effect of a literacy inquisition on the number of literate individuals at the end of the Qing dynasty overwhelms any positive effect on literacy that might have operated through the channel of more individuals entering commerce.

[7]As discussed by Galor et al. (2009); Gallego (2010) and Acemoglu et al. (2014).

[8]Squicciarini and Voigtländer (2014) build on earlier seminal work by Mokyr (2002, 2005b,a). Gennaioli et al. (2013) provide evidence that upper tail of the human capital distribution continues to play a crucial role in driving economic growth today.

[9]Similarly Chaudhary and Rubin (2011) examine how differences in literacy among Muslims in colonial India have persisted to this day. They argue that in parts of India where Muslim political authority collapsed more rapidly, religious authorities were able to establish themselves and these authorities established schools that were less effective in promoting literacy. Similarly, Botticini and Eckstein's (2005; 2012) show how an exogenous shock (the destruction of the Temple in Jerusalem) caused Judaism to become a religion of the literate.

comes including publications and the placement of PhD students in Germany. In contrast, Hornung (2014) examines the positive effect of the Huguenot diaspora on economic outcomes in Prussia.[10]

This paper also contributes to a better understanding of the development of China in the period before the Great Divergence. This is the first paper to study the effects of the imperial examination system during its heyday in pre-modern China. Important recent work studies the imperial examination system during the final years of the Chinese empire. Looking at the late nineteenth and earlier twentieth centuries, Yuchtman (2010) finds that graduates who had studied the classics earned lower wage premiums than those who studied engineering or other western subjects. Building on this finding, Cantoni and Yuchtman (2013) conclude that the imperial examination system became a barrier to economic development in China in the late nineteenth century because it created vested interests that had an incentive to block the growth of western-style modern education. Relatedly, recent work by Bai and Jia (2014) analyze the effects of the abolition of the examination system at the turn of the twentieth century on the expectations of social elites.

This research tells us much about why at the beginning of the twentieth century, Chinese imperial institutions were ill-suited for modern economic growth, but this does not mean that the Chinese education system was always ossified, ill-equipped to provide useful knowledge, and incapable of adaption and change. Indeed had this been the case, it is highly unlikely that China could have been a leading contributor to science in earlier centuries (Needham, 1995; Lin, 1995). By analyzing how the persecution of intellectuals in eighteenth century China shaped human capital formation we illustrate how the Qing state suppressed Chinese elites and provide supportive evidence to claims made by historians that the intellectual climate in China became decisively more hostile to innovative ideas at precisely the same time that it became more open and dynamic in western Europe (Mokyr, 2005b).[11]

Finally, our study has implications for the Great Divergence debate. The study of the institutions that shaped human capital formation takes on particular significance in the Chinese context because growth theorists argue that human capital accumulation is crucial to the emergence of sustained economic growth (Galor and Weil, 2000; Galor, 2011). This suggests that

[10]Our analysis of the effects of the literary inquisition is also related to a wider literature on persecutions. Johnson and Koyama (2013) examine the causes of the persecution of heretics in medieval and early modern Europe while Johnson and Koyama (2014) study how legal fragmentation helped perpetuate large-scale witch-hunts in seventeenth century France.

[11]As discussed by Kuran (1995) and Greif and Tadelis (2010) an atmosphere of persecution can create a climate of intellectual conformity by inducing individuals to falsify their preferences or adhere to a crypto morality. Such an environment will likely be hostile to innovation.

imperial China, with its educated civil service and long tradition of valuing education, should have been well positioned to achieve the transition to sustained economic growth.[12] However, this did not happen: at the same time that growth rates in western Europe began a sustained increase, China experienced stagnation and a series of economic and political crises (Pomeranz, 2000; Maddison, 2003; Chen and Kung, 2012; Broadberry, 2013; Li et al., 2013; Jia, 2014a; Sng, 2014; Sng and Moriguchi, 2014).[13]

Our results provide evidence of a human capital channel through which autocratic institutions effect subsequent economic performance. They also suggest further, much more speculative, channels through which Chinese political institutions may have had an adverse impact on economic growth. For instance, the threat posed by literary inquisitions may have deterred honest individuals from entering government at the expense of those willing to accommodate themselves to the system.[14] Relatedly, because literary inquisitions targeted the writings of scholar-officials, it is likely that they caused communication and cooperation within the imperial bureaucracy to decline. This could have had consequences for the ability of the Chinese state to respond to the challenges posed by the nineteenth century. Finally, because the literary inquisition led to an increase in the cost of basic education it may have shaped the fertility decisions of the population in favor of greater quantity thereby further delaying the transition to modern growth.

The rest of the paper is organized as follows. Section 2 describes our data. In Section 3 we explain our empirical strategy and present our main results. In Section 4 we develop a simple model to explore the channels through which the persecution of intellectuals could generate a large and persistent negative effect on human capital accumulation in the local area for decades and even centuries. We test this channel of persistence using data that reflects literacy levels in the early twentieth century in Section 5. In Section 6 we place our findings in the broader context of Chinese history and the debate on the Great Divergence and the origins of modern economic growth. Section 7 concludes.

[12]The traditional view in the literature on British industrialization downplayed the role of education (see Mitch, 1999). But education played a crucial role in enabling Prussia to catch up to Britain in the late nineteenth century (Becker et al., 2011) and in the economic development of the United States in the twentieth century (Katz and Goldin, 2008).

[13]For an excellent survey of Qing economic history see Brandt et al. (2014). It is important to note that not only did China fall behind relative to Europe, it also fell behind relative to Japan after the Meji Revolution of 1868 as Sng and Moriguchi (2014) emphasize. One speculative reason for this is that while elites in Japan were able to orchestrate a transition to modern political organizations, elites in China were too weak and fragmented to do so.

[14]This mechanism is similar to the one proposed by Hayek (1944) in his account of why 'the worst get to the top' in autocratic regimes.

2 Data and Historical Setting

2.1 Literary Inquisitions

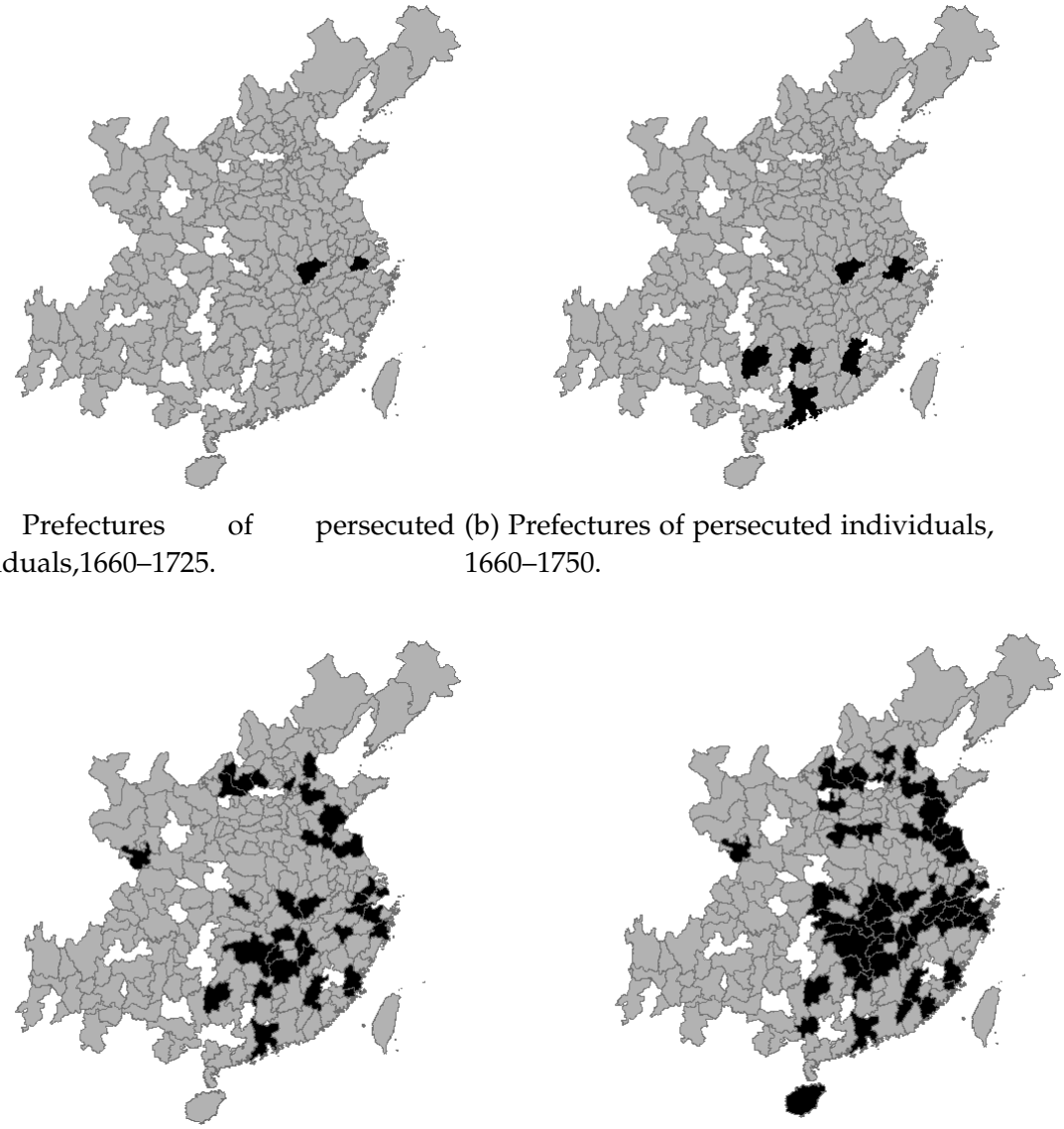
Literary inquisitions—purges of scholar-officials for what they wrote or were suspected of thinking—became a frequent and institutionalized feature of Qing rule over China during the seventeenth and eighteenth centuries. The Qing rulers were not Han Chinese but Manchus. The Manchus conquered China following the collapse of the Ming dynasty in 1644. The transition from Ming to Qing rule was extremely violent—resistance to the Qing continued into the 1680s. This resistance was driven by Han Chinese elites. Hostility to the Qing stemmed largely from the fact that the legitimacy of the Chinese state was built on the role of the Emperor in protecting the sedentary Chinese from nomadic invasion by 'barbarians' like the Manchus (Brook, 1988, 177–178).[15] As a consequence, the relationship between the educated class of scholar-officials and the emperor were fraught. The main issue facing the Qing emperors became how 'to dominate a literate and highly sophisticated Chinese elite?' (Guy, 1987, 18).

To do this the Qing rulers routinely investigated and punished scholar-officials for what they wrote.[16] These purges reduced both the status and the material payoffs of the scholar-official class. Describing the reign of the Yongzheng emperor, one historian notes the 'Literary inquisition was another form of restriction on the Chinese literati ... rigorously carried on, together with the measures to enforce ideological orthodoxy ... literary inquisition was used to intimidate nonconformists' (Huang, 1974, 204).[17] It is the effect of these purges that we seek to identify in our empirical analysis.

[15]This animosity preceded the Qing dynasty. It was firmly established from the Song dynasty onwards (see Rossabi, 1983; Ebrey, 1991; Ge, 2004). Brook notes: 'The legitimacy of the Manchus, China's Inner Asian rulers during the Qing (1644–1911), depended on their being seen as rightful candidates for the "mandate of Heaven," rather than as barbarian interlopers from beyond the pale of civilization. And in Chinese political theory, civilization, a concept consistently phrased in terms of *wen* or literate expression, was everything. Civilization meant, among other things, the recording of knowledge, and those who controlled that record held the keys to state legitimacy. The Manchu leaders realized that they needed to dominate discourse about the past so as to be able to project certain historical interpretations that would justify Inner Asian rule over the Chinese people' (Brook, 1988, 177–178).

[16]Literary inquisitions were solely targeted against the educated elite. We do not consider other persecutions or peasant-led protests or rebellions. Kung and Ma (2014) study how Confucian values mitigated peasant rebellions in imperial China while Jia (2014b) examines how the introduction of the sweet potato weakened the relationship between natural disasters and peasant rebellions.

[17]The Kangxi emperor (r. 1661–1722), the Yongzheng Emperor (r. 1722–1735), and the Qianlong Emperor (r. 1735–1796) adopted a janus faced attitude towards intellectuals. On the one hand, they patronized intellectuals, the Kangxi emperor recruiting large numbers of them, for example, to produce the Kangxi dictionary (1710–1716). On the other had, they ruthlessly suppressed dissent and opposition among scholar-officials.



(a) Prefectures of persecuted individuals,1660–1725.

(b) Prefectures of persecuted individuals, 1660–1750.

(c) Prefectures of persecuted individuals, 1660–1775.

(d) Prefectures of persecuted individuals, 1600–1800.

Figure 1: Prefectures of individuals persecuted as result of a literary inquisition per quarter century: 1725, 1750, 1775, 1800 .

We use data on the persecution of scholar-officials from *Qing chao wen zi yu an* (Qing literary inquisition case). 88 cases are included in the book, dated from 1661 to 1788.[18] We identify the hometown of each examination candidate mentioned as a victim of an inquisition. 75 of 88 cases can be matched to a specific county. And the individuals involved in all 88 cases

[18]We also consult *Qing chao wen zi yu dang* (Archives of Museum of Forbidden City, 1934).

can be matched to a specific province and prefecture.[19] *Qing chao wen zi yu an* (Qing literary inquisition case) adopts a strict definition of literary inquisition. Figure 1 depicts the prefectural boundaries of Qing China and displays the prefectures associated with victims of literary inquisitions per quarter century.[20]

Scholars were investigated for arbitrary reasons, often on the basis of suspicion alone. The definition of what was deemed subversive was not defined and changed over time: 'the ruler was the sole interpreter of these cases, and some accusations were based on suspicion.' (Huang, 1974, 208).[21] Alleged crimes for which individuals were punished included writings that were deemed to either explicitly or implicitly criticize the dynasty; for use of taboo words such as the name of an emperor; for work that was deemed to reflect negatively on the achievements of Qing emperors (such as anything that seemed to critique the Kangxi dictionary); for writing positively of the previous dynasty; for actual or alleged factionalism (see Huang, 1974, 208). Individuals were liable, not only if they wrote suspect literature, but also if they kept silent about the existence of such literature or owned copies themselves. Individuals were punished for distributing and selling books written by those found guilty by the literary inquisition.

From the reign of the Yongzheng emperor onwards, scholar-officials could be investigated on the basis of anonymous denouncements from their peers and, in response, a wide network of informers emerged. Timothy Brook observes that the literary inquisition 'grew into a hydra of suspicion and denunciated because the Chinese (as opposed to the Manchu) elite found in project's hazy guidelines opportunities for pursuing personal vendettas. Scholars began to denounce each other, both to settle old scores and to attract the attention of regional offi-

[19]There were three levels of administration in Imperial China: the province, the prefecture and the county. There were roughly five or six counties per prefecture and seven to thirteen prefectures per province. Rowe notes that 'the county was the lowest level of formal administration, the smallest unit to which a centrally-appointed, examination-certified bureaucrat was assigned. (Rowe, 2009, 37). Therefore the prefecture level is the lowest level of aggregation at which we expect to find a measurable effect of a literary inquisition.

[20]The literary inquisition is mentioned by a large number of historians. But though it was studied by Chinese scholars in the early and mid-twentieth century (e.g. Goodrich (1935); Ch'i-ch'ao (1959); Wiens (1969)), it has not been the subject of a specialized study among modern historians. An exception is Jones (1975) who criticizes earlier historians who argued that '[i]n order to avoid involvement in the purges surrounding the literary inquisition, and in order to preclude some inadvertent criticism of the Manchus or their policies (so the argument goes), scholars shunned current political and social topics' (Jones, 1975, 22). However, this argument is weak. She argues that 'literary inquisition did not arouse a notable outcry; in fact it won approval from one of the most prominent intellectuals' (Jones, 1975, 29). But, of course, this is in fact evidence in favor of the proposition that the persecution of intellectuals *did matter* and *was repressive*. The existing literature comprises either narrative accounts, detailed case studies (Spence, 2001), or comparatively brief mentions in more general accounts of Qing China (see, for example, Gernet (1972, 506), Huang (1974, 204–208), and Guy (1987, 166–179)).

[21]Wakeman refers to this as 'the institutionalization of Imperial subjectivity' (Wakeman, 1998, 168).

cials' (Brook, 2005, 178). Particularly during the reign of the Qianlong emperor, the houses of scholar-officials were searched for suspect material; at the 'same time brutal measures were taken against the authors and their relations—execution, exile, forced labor, confiscation of property, and so on' (Gernet, 1972, 506). Therefore, even though the number of individuals acted persecuted for literary crimes was relatively small, large numbers of scholar-officials saw themselves as potential victims of a literary persecution.

The types of individuals persecuted as a result of the literary inquisition fell into roughly three categories: (i) individuals who were not anti-Qing and who were persecuted for writing or possessing books which were mistakenly interpreted as containing anti-Qing material; (ii) individuals who were indeed anti-Qing but were anti-Qing for idiosyncratic reasons; (iii) individuals who were opposed to the Qing state and whose views were potentially shaped by local sentiment. The case of Fang Bao who was jailed for a preface that he did not in fact write is an example of (i) (Schmidt, 2003, 369). An example of (ii) is provided by the case of Ding Wenbin who had visions of being favored by heaven and referred to himself as "emperor" in his writings. When he was discovered he was put on trial and executed. An example of (iii) is provided by the persecution of the descendants of Lü Liuliang (1629–1683) during a celebrated literary inquisition case in 1728-1730. Lü Liuliang was a Ming era scholar who had opposed the Manchus. His family suffered persecution because of the indirect influence of his writings on Zeng Jing who attempted to suborn a provincial governor (Spence, 2001).

For categories (i) and (ii) the persecution of an individual was plausibly exogenous to characteristics of the individual's home province or prefecture. For categories (iii) our treatment effect is exogenous conditional relevant provincial or prefectural characteristics. In Tables 1 and 2 we report the correlation between the home province or prefecture of the victim of a literary inquisition and a number of time varying observables such as conflicts, earthquakes, extreme weather, and natural disasters. None of these events appears to have prompted literary inquisitions.[22]

This is consistent with the qualitative historical evidence and with the observation that the principal purpose of 'witch-hunts' in general is not to remove a single individual who is suspected of subverting the regime, but to deter other individuals from attempting to do so by creating a climate of fear and mutual suspicion. The aim of the policy was to suppress dissent to 'foster orthodoxy and prevent the rise of factionalism such as had plagued the politics of late Ming period' (Fairbank, 1987, 102). The way in which literary inquisitions were conducted was

[22]The period we study (1660-1794) was one of tremendous political stability in China and there is no indication in the data or secondary literature that inquisitions were employed in direct response to rebellions, wars, or economic shocks.

consistent with a Beckerian model of deterrence (Becker, 1968). As it was costly to investigate every single scholar-official for having potentially subversive views and because the imperial administrative faced severe agency problems, it was preferable to single out a small number of individuals to be investigated and punished.

Gregory et al. (2011) study the Stalin-era purges to examine why a rational dictator has an incentive to persecute individuals who are not genuine enemies in the presence of low quality information. Like the Stalin-era purges, the literary inquisitions of the Qing period were not so much targeted as specific individuals guilty of wrong-doing, but rather aimed at overawing the entire class of scholar-officials into submission by demonstrating that any of them could be persecuted.

For example, consider the case of Wang Xihou—a dictionary maker—who was arrested along with 21 members of his family, for offending the Qianlong emperor (Guy, 1987, 175–6). Wang was found guilty of several crimes, including printing in full the characters of the name of Confucius and of early Qing emperors, both of which was forbidden (Reischauer and Fairbank, 1958, 382). Though he was was guilty of the offenses listed, there was no evidence of subversive intent; rather, as Guy observes, 'the emperor was using the Wang case to make a statement to the literary community about his determination to preserve his dynasty's reputation. The singling out of one offender, repugnant though it may seem today, was not an uncommon means of communicating, in the eighteenth century to a large and diffuse community uncertain of Imperial directions' (Guy, 1987, 176). Consistent with a Beckerian framework that emphasizes the importance of deterrence, individuals found guilty were usually executed by slow slicing in public. Historians agree that this policy was successful in achieving its aims (Huang, 1974; Fairbank, 1987; MacKinnon, 1997; Wakeman, 1998; Schmidt, 2003). Scholars into the nineteenth century noted that the pervasive effect of 'fear of the smell of the inquisition' (Gong, 1991).

2.2 Exam Candidates

To construct our dependent variable, we collect data on government officials from the Chinese Biographical Database (CBDB). The CBDB has 3,531 records on those who obtained government positions between 1651 and 1840.[23] To obtain the number of successful exam candidates, we exclude officials who bought their office and focus on those who came through the exam

[23]Since we aggregate the number of candidates by decade, our analysis begins in 1660. This summarizes the number of officials who passed the exams between 1651 and 1660. The CBDB provide a representative sample of the number of exam graduates. We discuss this data more in the Appendix

Table 1: Correlations of inquisitions at a provincial level

	Inquisition	Earthquakes	Conflict	Extreme weather	Natural disasters
Inquisition (count)	1				
Earthquakes	-0.0517	1			
Conflict	-0.142***	0.0177	1		
Extreme weather	-0.0293	-0.0253	0.0495	1	
Natural disasters	-0.0202	-0.0582	0.0831	0.104	1
	Inquisition (binary)	Earthquakes (binary)	Conflict (binary	Extreme weather (binary)	Natural disasters
Inquisition (binary)	1				
Earthquakes (binary)	-0.0514	1			
Conflict (binary)	-0.130**	-0.0638	1		
Extreme weather	-0.0371	-0.0323	0.0117	1	
Natural disasters	0.00750	-0.0767	0.0831	0.117**	1

* $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$

This table shows that literary inquisitions were not correlated with natural disasters, or periods of extreme weather at a provincial level. Further details on the data are in the Appendix.

track. This gives us 1,898 unique individuals.[24] There were three levels of examinees in Imperial China: prefectural level *linsheng* ('granary student' as they would receive government rations); provincial level *juren*; and metropolitan level *jinshi* (Figure 2). Our data contains 1,484 jinshi.[25] Our dependent variable reflects both the number of successful examination candidates and the percentage of successful examination candidates who decided to become government officials. As both decisions could be affected by a literary inquisition, our dependent variable captures the overall deterrence persecution of persecution.

Importantly, the imperial civil service was highly centralized. To prevent scholar-officials from developing local affiliations, they were typically assigned to locations hundreds of kilometers from their hometown and regularly rotated.[26] As such, they had little connection with

[24]The majority of this decrease is due to the removal of duplicate entries.

[25]In the event of multiple entries, an individual is identified as jinshi by the year he obtained jinshi; if he never achieved the rank of jinshi, he will be identified as juren by the year he obtained juren status; for the rest of individuals, they are identified by the entry with the earliest entry year.

[26]Under the Qing dynasty the system of rotation of officials ensured that officials at a provincial level served 'no longer than three years and those at a local level, no longer than half that time' (Rowe, 2009, 39). Also see Wei

Table 2: Correlations of inquisitions at a prefectural level

	Full Sample				
	Inquisition	Earthquake	Conflict	Extreme weather	Natural disasters
Inquisition	1				
Earthquake	-0.0161	1			
Conflict	-0.0570***	0.00133	1		
Extreme weather	-0.00938	-0.00690	0.0341**	1	
Natural disasters	-0.0160	-0.0358**	0.0441***	0.0633***	1
	Matched Sample				
	Inquisition	Earthquake	Conflict	Extreme weather	Natural disasters
Inquisition	1				
Earthquake	-0.00987	1			
Conflict	-0.0548	0.0443	1		
Extreme weather	-0.00696	-0.00669	0.0176	1	
Natural disasters	-0.0294	0.0706**	0.0370	0.0642**	1

* $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$

This table shows that literary inquisitions were not correlated with natural disasters, or periods of extreme weather at a prefectural level in both the full sample and the matched sample of prefectures that we use in our regression analysis.

the province or prefecture in which they were based. Their familial, kinship, and social connections remained with their home province or prefecture. Therefore it is in the home province or prefecture that we expect to find an effect of a literary inquisition.

Passing the metropolitan exams was a source of great pride for the family and hometown of a successful jinshi candidate. The list of successful examinees in each county was published and circulated in every city (Marsh, 1961, 2). The fate of those scholar officials who were victims of the literary inquisition was mourned by the local community for decades or more.[27]

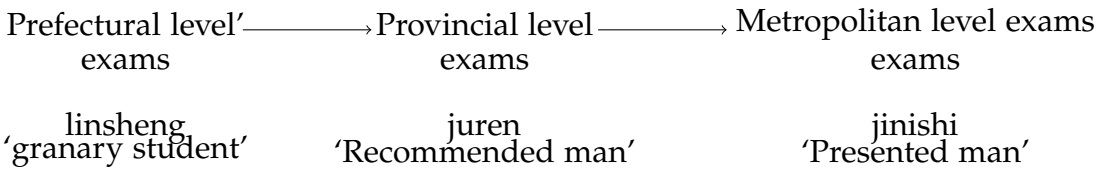
2.3 Other Factors Influencing Number of Successful Exam Candidates

We choose our controls for two purposes. First, we want to ensure that our estimates are not biased by province or prefecture-specific developments that might led the emperor to target an individual from a specific region. At the provincial level we introduce our time invariant

(1989).

[27] In fact in modern China prominent victims of the literary inquisitions are still discussed in their hometowns to this day (see, for instance, Luo, 2008).

Figure 2: The Imperial Examination System



covariates linearly. In our prefectural level analysis we employ a matching approach based on time invariant covariates. In both specifications we also control for time varying covariates including conflicts, extreme temperatures, earthquakes, droughts, and floods.[28]

Second, to identify the effect of a literary inquisition on the subsequent number of exam candidates we need to control for other factors that could affect the number of exam candidates in a province or prefecture. This motivates our choice of economic, geographical, and pre-treatment historical characteristics. Table A.2 presents our summary statistics and provides information on our more important control variables including log population in 1600, log population density in 1600, latitude, longitude, agricultural suitability, the log of the shortest distance to either the Grand Canal or the Yangtze river, the number of courier routes, and number of Buddhist temples.[29]

The Imperial examination was characterized by quotas at various administrative levels. The existence of these quotas meant that a given examinee's chance of success varied across prefectures and provinces. Quotas arbitrarily limited the proportion of lower level candidates (*linsheng* and *juren*) who could go on to the metropolitan exams and achieve *jinishi* status. Prior to 1712 regional level quotas existed that went back to the Ming-era. However, after 1712, metropolitan-level exams start to select jinshi as a percentage of the total examinees by province. Competition became more localized within a province as a consequence.

3 Empirical Strategy and Benchmark Results

We first implement a difference-in-differences strategy to test the impact of a literary inquisition on the subsequent number of government officials recruited from the imperial examination

[28] Tables 1 and 2 shows that there is no correlation between these time-varying covariates and the timing of an inquisition.

[29] As there are no reliable historical estimates for population density in China prior to 1730, we rely on estimates constructed by environmental scientists (Klein Goldewijk et al., 2011). Further details are confined to a data appendix.

system at the provincial level.[30] We go on to focus on prefecture-level variation because after the abolition of the Ming-era exam areas in 1712, the most reasonable approach is to study within province variation.

China proper was divided into 18 provinces during the Qing period. The administrative level below the provincial level was the prefectural level. In total there were 275 prefectures in the 18 provinces of China proper. The CBDB has data on individuals from 199 prefectures in the database. In our provincial level analysis we use 17 out of 18 provinces.[31] For our prefectural analysis we use propensity score matching to ensure that we are comparing like prefectures.

3.1 Province-level DID

We first estimate the following equation:

$$\begin{aligned}\text{Exam Candidates}_{p,d} =& \beta_0 + \beta_1 \text{Affected by Inquisition}_{p,d} + \mathbf{\Omega}_p + \mathbf{\Lambda}_d \\ &+ \epsilon \ ,\end{aligned} \tag{1}$$

where subscript p represents a province; and d a decade. Our dependent variable is the number of successful exam candidates (either juren or jinshi) recruited as government officials. Inquisition_p denotes the home province p of an individual or more being a victim of a literary inquisition between 1660 to 1840. Post_d denotes the decades after the the first individual(s) from a province was or were investigated. $\mathbf{\Omega}_p$ are province fixed effects. $\mathbf{\Lambda}_d$ represents decade fixed effects. We also either include province-specific time trends or interactions between time trends and time-invariant controls .

3.2 Provincial Level Results

Tables 3 and 4 report our provincial level results. Our sample consists of seventeen of the eighteen provinces that comprise China-proper from 1660 to 1840. We employ province fixed effects to control for observables and unobservables including differing levels of urbanization or income that might have led to some provinces having greater resources for individuals to study for the exams. Province fixed effects also control for underlying differences in either

[30] Provincial-level exams took place in an exam venue at the provincial capital. Examinees from the same province would meet each other when taking the exams. Examinees who sat for the metropolitan exam would have passed the provincial exam first.

[31] We exclude Gansu as it remained politically unstable during the Qing period. It produced an extremely low number of government officials from the exam track. In our sample, over the course of 180 years, Gansu had just 2 government officials who held exam degrees.

attitudes to the Manchus or perceived threats to the emperor. Decade fixed effects allow us to control for time varying observables and unobservables such as negative income shocks (which might reduce the number of candidates for the exams). The Qianlong emperor pursued a policy of deliberately weakening Han-elites by reducing the number of official positions open to scholar-officials as this opened up more positions for ethnic Manchus (Elman, 1991). By employing decade fixed effects we can explicitly control for this trend. All specifications allow for a trend break in 1712 as this was the date of the abolition of the Ming-era exam regions.

Table 3: Provincial Level DID Estimation: Baseline

	(1)	(2)	(3)	(4)	(5)
			# Officials		
Affected by Inquisition	-3.473**	-1.780*	-4.401**	-2.344**	-4.045**
	(1.556)	(1.004)	(1.837)	(0.978)	(1.874)
Population 1644*Time Trend	No	No	No	Yes	No
Exam region*Time Trend* Trend break in 1712	Yes	Yes	Yes	Yes	No
Exam region*Decade FE	No	No	No	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes
Province-specific time trends	Yes	No	Yes	No	Yes
Population Weights	No	No	Yes	No	No
Observations	323	323	304	304	323
Adjusted R^2	0.448	0.432	0.565	0.445	0.450

Robust standard errors clustered at the provincial level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports DID estimates at a provincial level. The unit of observation is a province on the 1820 map. The dependent variable is the number of government officials recruited from a particular province. Column 1 is our baseline using a sample including seventeen provinces from 1660 to 1840 and provincial time trends. Column 2 includes no provincial time trends. In Column 3 we weight our estimates by population in 1644. Column 4 interacts population in 1644 with a time trend. Column 5 utilizes exam-region specific decade effects. Province fixed effects and decades fixed effects are included in all specifications. Robust standard errors are clustered at the province level.

The coefficient we obtain in Column 1 of Table 3 implies that a literary inquisition reduces the number of government officials recruited from a province by 3.5 individuals. This effect

is statistically significant and economically meaningful. A one-standard-deviation increase in the probability of an inquisition reduces the number of government officials recruited from a province by 1.47 persons (0.425*3.473), which is equal to 25% of the sample mean for the dependent variable.

This specification includes province-specific time trends to control for different trends in number of successful exam candidates at the provincial level driven by demographic changes or differing trajectories of commercialization or economic development. The coefficient estimate is smaller in Column 2, when provincial time trends are not controlled for. In Column 3 of Table 3 we weight provinces by their population in 1644, which produces a slightly larger coefficient estimate. Column 4 we interact population in 1644 with a time trend in place of provincial time trends. The coefficient estimate (-2.344) is between that in Column 1 (-3.473) and Column 2 (-1.780). We interpret this result as suggesting that the Population 1644*Time Trend partly accounts for differential trends across provinces in the absence of literary inquisition. Finally, Column 5 utilizes exam-region specific decade effects based on pre-1712 exam regions, allowing for a trend break in 1712, as these regions could have experienced differential trends before and after the abolition of the exam regions. In general, our results are consistent across specifications.

Our provincial level results are robust to alternative samples and additional controls. In Column 2 of Table 4 we exclude Zhejiang from the sample, because the first metropolitan exam after the 1727 inquisition was not open to examinees from Zhejiang Province (subsequently exams were open to candidates from Zhejiang). In Column 3 we use 1670 rather than 1660 as the starting point of the analysis due to a concern over lower data accuracy for the beginning of the Qing dynasty, as well as the existence of various local shocks in the area due to anti-Qing campaigns during the 1660s. In Column 4 we add a number of time-varying covariates including a conflict dummy, a measure of natural disasters, drought and flooding, and earthquakes. Across specifications we find that a literary inquisition reduces the number of officials recruited through the exam track by between 2.8 and 3.1 individuals.

3.3 Prefectural-Level DID

We now conduct a sub-provincial level analysis as this has the advantage of being able to more precisely control for time varying observables and unobservables at a provincial level. Furthermore, we use a propensity score matching approach to construct a more comparable control group out of all prefectures with no inquisition. We observe that prefectures with and without inquisitions are unbalanced in covariates before matching. To mitigate concerns over violations of the “parallel trends" assumption, we restrict our comparison group to only

Table 4: Provincial Level DID Estimation: Robustness

	(1)	(2)	(3)	(4)
	# Officials			
Affected by Inquisition	-3.473**	-2.867*	-3.344**	-3.414**
	(1.556)	(1.498)	(1.549)	(1.597)
Conflict			0.305	
				(0.279)
Natural Disaster				4.338*
				(2.205)
Extreme Weather				-0.665
				(0.903)
Earthquake				0.509
				(0.344)
Exam Region*Time Trend* Trend break in 1712	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes
Province-specific time trends	Yes	Yes	Yes	Yes
	Baseline	Omit Zhejiang	1670 start year	
Observations	323	304	306	323
Adjusted R^2	0.448	0.432	0.400	0.445

Robust standard errors clustered at the provincial level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports DID estimates at a provincial level. The unit of observation is a province on the 1820 map. The dependent variable is the number of government officials recruited from a particular province. Column 1 uses a sample including seventeen provinces from 1660 to 1840. Column 2 excludes Zhejiang province. Column 3 uses a different year, 1670, as the starting point of the analysis. Column 4 employs a set of time-varying controls. Province fixed effects, decades fixed effects, and a trend break for 1712 are included in all specifications. Robust standard errors are clustered at the province level.

those with similar propensity matching scores to those of the treatment group. By combining propensity score matching and a DID estimation, we aim to minimize the bias from observable characteristics and to obtain accurate estimates of the 'treatment effect' of an literary inquisition even though our setting is non-experimental.[32]

3.4 Matching Prefectures by Propensity Score

Possible trend confounders arising from differences in the pre-treatment characteristics between prefectures with and without inquisitions could be a source of potential bias in our analysis. Before matching, we observe noticeable differences between the treated and untreated with regards to the existing size of talent pool, as proxied by population size and past successful exam candidates. We expect such initial differences to produce different dynamics governing the number of successful exam candidates over time, and, possibly, generate varying responses to later policy reforms and external shocks. This poses a challenge to estimating the effect of literary inquisition using only linear regressions. To correct for this potential bias, we match our prefectures on a range of covariates including the number of successful exam candidates in the past. "Past" here means the previous Ming dynasty (1368–1644). We also include the number of Buddhist temples as a pre-treatment covariate, in light of the fact that Buddhist priests were not permitted to attend the civil service exams.

Emperors might be more likely to persecute individuals from areas of the country where the perceived threat from elites was greater. This can be proxied by the size of elites in the previous dynasty as measured by the number of successful exam candidates. Alternatively, emperors might be more prone to persecute individuals from parts of the country that were of specific economic or political importance as losing control of these areas would be particularly costly. We can account for this by matching prefectures on covariates such as population size in 1600, agriculture suitability, the number of courier routes, and distance to the Grand Canal or Yangtze river as these are measures of economic importance. As political scientists like Scott (1999) argue that premodern states struggled to enforce conformity in rugged areas, we also match prefectures according to ruggedness.[33] Because Buddhist temples provided a sanctuary for individuals who wished to withdraw from politics—including loyalists to the

[32]For discussion of this point see Heckman et al. (1998); Blundell and Monica (2000); Dehejia and Wahba (2002); Blundell and Dias (2009). A matching approach is appropriate in our context and the data we have on premodern China means that there are a large number of observable covariates to condition on. By conditioning our DID estimates on a set of covariates through matching we further reduce our measurement error. A recent paper in economic history that employs this method is Dittmar (2011). Also see Voigtländer and Voth (2012); Squicciarini and Voigtländer (2014).

[33]This is measured as the mean change in the slope across grid cells in a prefecture.

Ming regime—we also control for Buddhist temples as a covariate.

Potential examinee candidates could choose to become merchants as an alternative career path. Therefore we control for alternative labor market opportunities by including a measure of trading activity as one of our covariates that we match on. As a result we obtain 71 matched prefectures for our main specification. Further details on our matching procedure are confined to the Appendix.

3.5 Prefecture-level results

Table 5 reports our prefecture-level DID analysis on the 71 matched prefectures.[34] All specifications include controls for log population size in 1600, agricultural suitability, distance to either the Great Canal or the Yangtze river, whether or not a region belonged to the Ming trade area, and Ming-era exam region. To further alleviate concerns that different regions were experiencing different economic or political developments over time, or changing importance of economic determinants in number of successful exam candidates, we also directly control for interactions between Ming-era exam region and a time trend, as well as other time invariant characteristics (population in 1600 and its squared term, latitude and longitude) and a time trend. As before we allow time trends to break at 1712 by exam region.

Prefecture fixed effects control for prefecture specific observables and unobservables that are not controlled for by our matching procedure such as differences in income, urbanization or agricultural product mix that might bias our estimates. In particular, they control for differences in attitudes to the Manchus could lead to lower participation in the examination system due to a reluctance to cooperate with the Qing dynasty. Furthermore, province fixed effects allows us to control for prefecture-level quotas that limited the proportion of lower level exam candidates who could obtain *jinishi* status. This ensures that we compare the effects of a literary inquisition on prefectures within a given province.

To further control for temporal trends at the provincial level we employ province-level time trends in Column (1) and provincial-specific decade fixed effects in Columns (2–4). By estimating a flexible model we are able to control for province-level institutional variation such as changes in the provincial-level quota for exam candidates.[35]

Our main specification in Column 1 indicates that a literary inquisition reduces the number

[34]For analysis of the use of matching in conjunction with DID estimation see Blundell and Monica (2000); Blundell and Dias (2009). In particular Blundell and Dias (2009, 609) note that 'matching and DID can be combined to weaken the underlying assumptions of both methods'.

[35]This also controls for variation in the perceived threat posed by a region as this could provide a reason for an emperor to preemptively target a specific prefecture with a persecution.

of officials recruited from the exam track in a prefecture by 0.27 individuals. This coefficient increases slightly when we employ provincial-specific decade fixed effects. In Columns (3) we directly control for the number of exam candidates for a prefecture in the previous decade and obtain a similarly sized coefficient of -0.286. Column (4) estimates the log plus one of the number of officials.

Table 6 introduces more time invariant characteristics interacted with a time trend. Column (1) reports our baseline. In Columns (2-5) we systematically introduce agricultural suitability, whether a prefecture was part of the Ming trade area, whether a prefecture was on the Grand Canal or Yangtze River, and distance to the coast interacted with time trends. All of these specifications are consistent with our baseline results.

One concern with our analysis is that there still might exist some time-varying prefectural level characteristics that may make officials from some prefectures more likely to experience an inquisition and for those same prefectures to produce fewer exam candidates. To explicitly address this in Table 7 we include the set of time-varying controls we employed in our provincial level estimation which included a measure of the number of conflicts, earthquakes, other natural disasters and droughts and floods. The coefficient of interest remains unaffected: a literary inquisition reduces the number of candidates from a prefecture by approximately 0.3 on average.

Figure 3 depicts the difference in the number of examination candidates before and after a literary inquisition in those prefectures that experienced a literary inquisition (the solid line) in comparison with those that did not (the dashed line). Though the data is volatile, we observe that the number of examination candidates from control and treatment prefectures move together prior to the date of an inquisition. After an inquisition there is a clear divergence between the treated and the untreated prefectures. In order to get a better sense of the effect of an inquisition by decade, Figure 4 plots the coefficients that we obtain from a fully flexible DID regression. It is consistent with Table A.9 which depicts the results of placebo regressions that show that if we move the date of an inquisition back either ten, twenty, thirty, or forty years we obtain no effect on the number of examination candidates.

3.6 Robustness Checks

In this subsection, we demonstrate that our baseline results are robust to alternative matching criteria, alternative starting periods and alternative subsamples.

Heterogeneous trends Table A.4 allows for a variety of heterogeneous trends. Our results are unaffected when we interact our measure of past human capital (the number of Ming-era examination candidates) with decade fixed effects. Nor do they change when we include

Table 5: Prefecture Level DID Estimation

	# Officials			ln (1 + # Officials)
	(1)	(2)	(3)	(4)
Affected by Inquisition	-0.269*	-0.306*	-0.286**	-0.147*
	(0.143)	(0.155)	(0.134)	(0.0829)
Past n. exam candidates	No	No	Yes	No
Ming Exam Region*Time Trend*Trend Break in 1712	Yes	Yes	Yes	Yes
Log Pop in 1600 *Time Trend	Yes	Yes	Yes	Yes
Log Pop in 1600 ^{2}*Time Trend	Yes	Yes	Yes	Yes
Latitude*Time Trend	Yes	Yes	Yes	Yes
Longitude* Time Trend	Yes	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes
Provincial Time Trends	Yes	No	No	No
Province FE*Decade FE	No	Yes	Yes	Yes
Observations	1278	1278	1278	1278
R^2	0.208	0.208	0.340	0.259
Adjusted R^2	0.134	0.134	0.109	0.066

Robust standard errors clustered at the prefectural level in parentheses

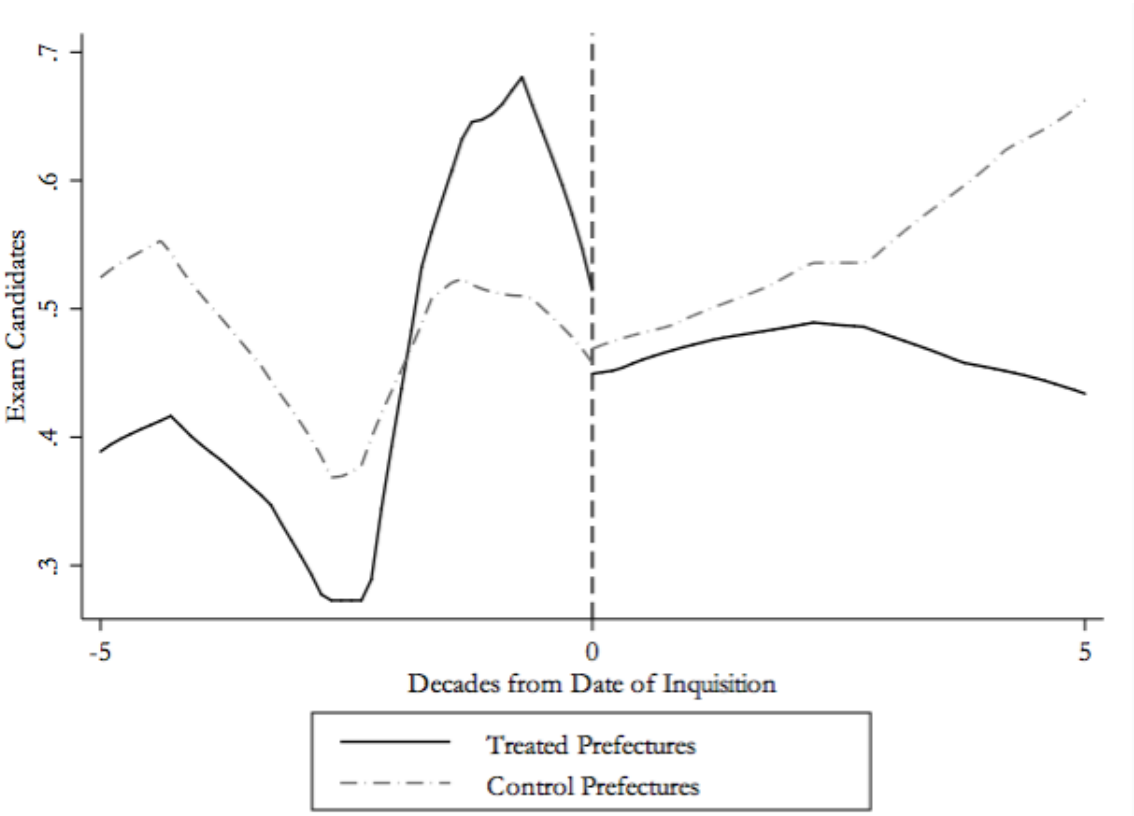
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports DID estimates at a prefectural level. It demonstrates the effect of a literary inquisition on the number of candidates in the next decade. The unit of observation is a prefecture on the 1820 map. The dependent variable is the number of government officials recruited from a particular prefecture. Past n. exam candidates refers to successful exam candidates from the previous decade. A sample including 71 prefectures from 1660 to 1840 is used in all specifications for a total of 1278 (18×71). Robust standard errors are clustered at the prefectural level.

interactions between the propensity score for each prefecture and decade fixed effects or allow for prefectures with post-1750 inquisitions to have a different time trend to those with pre-1750 inquisitions.

Matching procedure Table A.5 demonstrates that our prefecture level DID is robust to alternative matching criteria. If we vary the caliper width from 0.001 to 0.005 we achieve greater precision but at the expense of introducing bias. We find varying the caliper width yields estimates that are comparable to our baseline.

Figure 3: The effect of a literary inquisition in treated prefectures

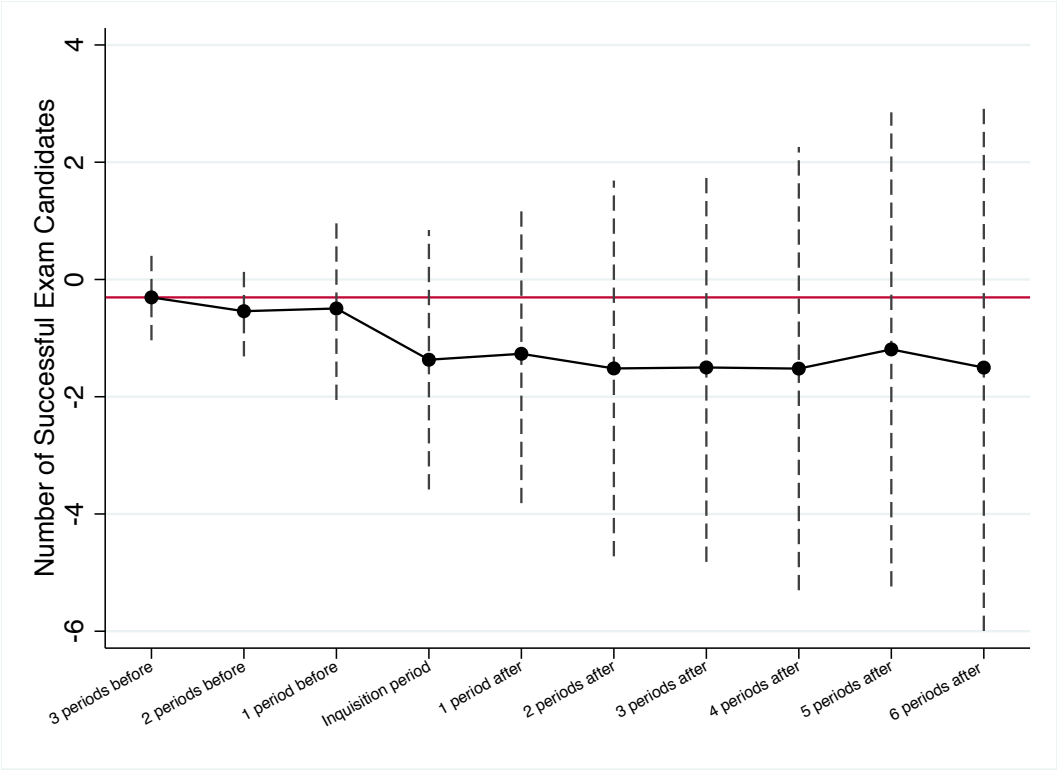


Local polynomial smoothing depiction of DID results. Treated prefectures are prefectures associated with victims of a literary inquisition. Control prefectures are prefectures that did not experience a literary inquisition.

Alternative beginning periods There may be concerns about the early part of the Qing period due to low data quality and conflicts between Ming loyalists and the Qing in the 1660s. In particular, it was only in the 1680s with the defeat of the Revolt of the Three Feudatories that the Kangxi emperor unified all of China. In Table A.6 we show that our estimates remain unchanged as we systematically exclude all years between 1660 and 1690.

Alternative subsamples Another possible concern is that our results are being driven by specific regions within China. Table A.7 provides further evidence that this is not the case. In Column (1) we exclude prefectures that produced no examination candidates. Column 2 omits Zhejiang from the sample, because it is documented that the first metropolitan exam after the 1727 inquisition was not open to examinees from Zhejiang Province. Column (3) leaves out Jiangsu, Anhui, and Zhejiang because the Yangtze Delta–highlighted by historians such

Figure 4: The effect of an inquisition by period



The effects of a literary inquisition on the number of examination candidates by prefecture. This table plots coefficients of a fully flexible DID regression.

as Pomeranz (2000) as being particularly economically developed—is contained within these provinces. These areas also experienced lower population growth during this period (Allen, 2009; Allen et al., 2011).[36] Finally, we drop Guangdong (Canton) as it was an important Qing trading post and the only part of China where European merchants were allowed to trade during this period after 1750. These areas do not drive our results.

Population movements A final concern is that some parts of China saw large population movements during this period. An inflow of migrants could either reduce or increase the number of potential examination candidates. Furthermore, areas that attracted migrants could differ along other characteristics. Ge (2005) provides estimates of the numbers of locals and migrants in all prefectures known to have experienced in-flow migration during the Qing

[36]The Lower Yangtze delta region also had higher levels of urbanization, more academies, and greater economic inequalities, all reasons that could induce a differential response to literary inquisitions than that which characterized the rest of the country.

period. Table A.8 we show that our results are robust to the exclusion of these prefectures.[37]

4 Persecutions and Persistence: Conceptual Framework

Having established that literary inquisitions reduced human capital formation in the home provinces and prefectures of persecuted individuals in the years following an inquisition, we now explore whether or not the persecution of intellectuals in mid-Qing China had a persistent impact on human capital accumulation.

Autocratic states like Qing China maintained political stability and legitimacy by persecuting perceived enemies. Previous research has shown that past institutions can have long-lasting effects through a variety of channels.[38] To see whether these persecutions had long-run consequences we explore a specific mechanism through which persecutions could have affected long-term educational outcomes; this mechanism is based on the provision of basic education.[39]

In our model each region is populated by individuals of varying ability and wealth. They face a fixed cost of providing basic education. This cost is determined by the number of teachers in a region. Individuals who acquire basic education can choose to study further for the exams.[40]

The cost of further education varies according to their ability. If they successfully pass the exams, they become scholar-officials eligible for office; individuals who do not succeed become teachers. The possibility of being investigated or persecuted due to an inquisition reduces the payoff of being a scholar-official and thus the proportion of individuals entering the examination track.[41]

These assumptions keep our analysis simple and are consistent with the historical evidence. Examination candidates often worked as teachers prior to passing higher level exams. As they

[37]These prefectures were predominantly in Jiangxi, south Shaanxi, parts of Zhejiang, Hunan and Sichuan. The fact that many high migration areas are already excluded by our matching exercise gives us additional confidence in the validity of our matching exercise.

[38]Nunn (2008); Nunn and Wantchekon (2011) argues that slave trade has had a negative long-run effect on economic development in sub-Saharan Africa, specifically by increases levels of mistrust both within and between different ethnic groups. Dell (2010) demonstrated that the Peruvian mita has had a persistent effect in retarding economic development. Voigtländer and Voth (2012) study the local persistence of anti-Semitism in Germany from the Black death to the Nazi period.

[39]Acunto (2014) finds that within-county distance from Mainz—the city where the printing press originated—predicts literacy in 1800 and basic education today.

[40]For simplicity we use the same analysis to focus on the metropolitan exams and the lower level exams.

[41]The only way a scholar-official could avoid persecution was by not writing. However, this was all but impossible for someone who held an office.

often came from ordinary backgrounds, they lacked the human capital or social networks to become merchants. Teaching was an 'honorable profession' and many 'took the attitude that when they were accepted by the government, they should step into officialdom, and that if they were not in government service, they should be engaged in teaching' (Chang, 1962).[42] The availability of teachers was the primary determinant of the costs of the education as historians notes that 'those who could afford the financial and labor sacrifices (read "investments") needed to prepare young men for the examinations did so without question' (Elman, 1991, 15).[43] A literary inquisition that deters individuals from being qualified to work as teachers raised the costs of basic education for future generations.

4.1 Model setup

Each region is comprised of a unit mass of individuals i each corresponding to a pair a_i, w_i where $a_i \in [0,1]$ is ability and $w_i \in [0,1]$ wealth. Utility is given by $u(e_i, b_i, p) - v(\gamma)$ where e_i corresponds to education which determines occupation, status, and income and $u_e > 0$; $b_i \in \{0,1\}$ is an indicator variable that takes the value of 1 if i chooses the examination path; $p \in [0,1]$ is the probability of passing the exams; $v(\gamma)$ is the cost of a persecution where $\gamma \in [0,1]$ refers to the probability of a persecution and $v(0) = 0$.

Individuals who choose the examination path, pass with probability p and become scholar-officials while a proportion $1-p$ become teachers.[44] Only individuals who choose to go down the examination path are liable to be persecuted. To simplify our analysis, we also assume that the probability of being persecuted, γ, is separable; that is, among scholar-officials, higher levels of education do not affect the probability of persecution. Our main assumption is that individuals who choose the exam path obtain a higher return to investments in education as

[42] Teaching, though respected, was also understood to bring less income than most of the other kinds of services that the scholar-officials rendered. They worked as teachers as 'teaching was the only satisfactory outlet for the gentry aside from entering officialdom' (Chang, 1962, 89–90). Some estimates suggest that as many as one third of all examination graduates in some regions were employed as teachers (often after they completed one level of the examination system and were preparing to study for higher level exams) (Chang, 1962, 92). Teaching was also the chosen occupation of failed exam candidates because it enabled one to study further in order to take the exam in the future. Although we do not model this explicitly, this consideration strengthens our argument.

[43] Balazs notes: '[t]he actual preparation of scholars was carried on first of all in the home. This fact gave advantages to youths not only from families of wealth which could afford tutors, but specifically from scholar-official families, in which parental example and family tradition provided both incentive and intellectual guidance' (Balazs, 1964, 306).

[44] To simplify the exposition we treat p as a parameter and do not make this probability dependent on e though this can be easily accommodated. Our assumption is consistent with the observation that '[i]n Ming-Qing times it was by no means certain that even the studious, intelligent, and unspoiled members of distinguished families could pass higher examinations, for there was always the element of luck' (Ho, 1962, 148).

either scholar-officials or as teachers:

$$\frac{\partial u(e_i, 1, p)}{\partial e_i} > \frac{\partial u(e_i, 0, p)}{\partial e_i} \, .$$

There is a cost to investing in education. Acquiring education up to $\bar{e}$—basic education—incurs a financial cost $k(t)$ where k depends on the number of teachers in a region t. This cost has to be financed from an individual's endowment w_i. Investing in education beyond $\bar{e}$ involves studying which is more costly for low ability individuals than it is for high ability individuals: $c(e_i, a_i)$ where $c'_{e_i} > 0$, $c'_{a_i} < 0$ and $c''_{e_i,a_i} < 0$. If individuals choose not to invest in education they become peasants. If they invest in education beyond $\bar{e}$ they have the option of studying for the imperial exams or taking up other occupations. Summarizing, each individual faces the following maximization problem:

$$\max_{e_i,b_i} u(e_i, b_i, p) - b_i p v(\gamma) \quad \text{subject to} \quad \Gamma_i(k(t_j), e_i, c(a_i, e_i)) \leq w_i \, , \tag{2}$$

where Γ_i is the cost of education and is given by:

$$\Gamma_i = \begin{cases} 0 & \text{if } e_i = 0 \, , \\ k(t) & \text{if } 0 < e_i \leq \bar{e} \, , \\ k(t) + c(a_i, e_i) & \text{if } e_i > \bar{e} \, . \end{cases}$$

4.2 Equilibrium

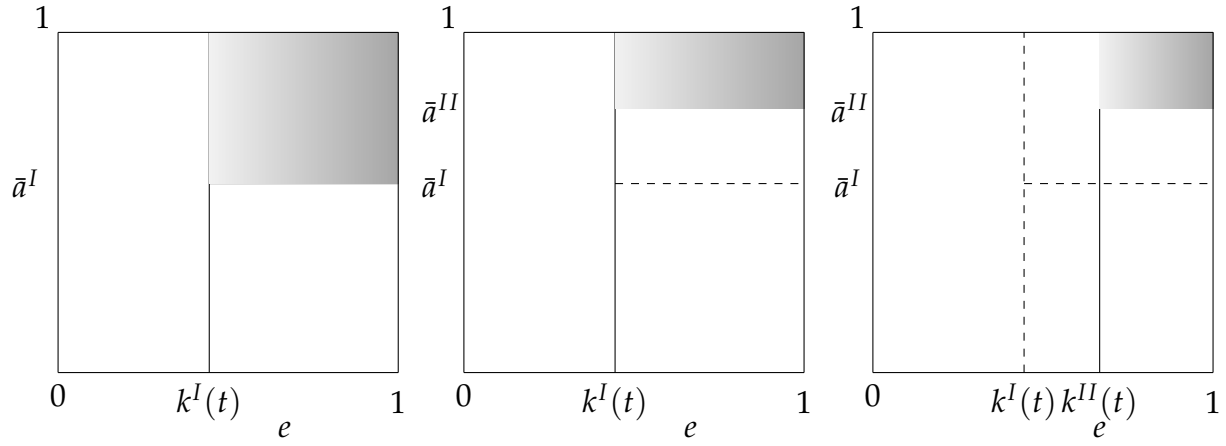
There are three cases to consider: (1) All individuals i such that $w_i < k(t)$ choose $e_i = 0$. This is a corner solution: regardless of their ability, these individuals cannot afford basic education. As there is a unit mass of individuals, the number of people who acquire no education is equal to $k(t)$. (2) Individuals who acquire some basic education but less than $\bar{e}$ and therefore cannot enter the exams and become scholar-officials. These individuals have $w_i > k(t)$ and so long as: $u(e_i, 0) - k(t) \geq 0$), choose an interior value of e_i, which satisfies the first order conditions: $u'(e_i, 0) = c'(e_i, a_i)$ but is less than $\bar{e}$. Our interest is in group (3) in those individuals who acquire enough education to take the exams.

In the absence of the threat posed by literary inquisitions, all individuals with $a_i \geq \hat{a}$ would enter the examination system where $\hat{a}$ is given by:

$$u(e, 1, p) - k(t) - c(e, \hat{a}) = u(e), 0) - k(t) - c(e, \hat{a}) \, . \tag{3}$$

However in the presence of the threat of a literary inquisition fixed at some arbitrary value of γ all individuals with ability $a_i \geq \bar{a}$ enter the imperial examination system (choose $b_i = 1$),

Figure 5: The effect of an increase in the perceived probability of persecution λ. The shaded area represents the proportion of the population who enter the examination system.



(a) Before an inquisition (b) The short-run effect (c) The long-run effect.

where $\bar{a} > \hat{a}$ is determined by the following indifference condition:

$$u(e, 1, p) - pv(\gamma) - k(t) - c(e, \bar{a}) = u(e), 0) - k(t) - c(e, \bar{a}) \ . \tag{4}$$

All individuals with less ability than $\bar{a}$ enter other professions. Given that there is a unit mass of individuals, in equilibrium, $(1 - \bar{a})/p$ individuals become scholar-officials whereas $(1 - \bar{a})/(1 - p)$ proportion of the population become teachers. We can see what happens when the probability of an inquisition γ increases from inspecting equation (4). An increase in γ increases the threshold level of ability at which an individual finds it worthwhile to enter the examination path.

Figure 5a depicts the initial situation prior to a literary inquisition in region. Consider an increase in γ_i from γ_i^I to γ_i^{II}. This induces a fall in the number of individuals who enter the examinations from $1 - \bar{a}^I$ to $1 - \bar{a}^{II}$ and a fall in the number of teachers to $(1 - \bar{a}^{II})/(1 - p)$. We illustrate this short-run impact of a literary inquisition in Figure 5b. The number of examinees declines while the number of individuals who enter other professions rises.

Now consider the long-run effect of a literary inquisition. After an inquisition, the number of teachers in the next period declines from t^I to t^{II}. In the subsequent period, therefore, the cost of basic education increases from $k^I(t)$ to $k^{II}(t)$. This causes more individuals not to invest in basic education. Furthermore, it leads to a further decline in the number of individuals who enter the examination system from $(1 - k^I(t))(1 - \bar{a}^{II})/(1 - p)$. We illustrate this in Figure 5c.

This simple framework elucidates a mechanism through which the literary inquisition has an effect on educational outcomes of future generations because it increases the costs of basic

education. The initial impact of a literary inquisition is most likely to be found among elites—as these individuals were mostly likely to pass through the various levels of examinations and obtain jobs as officials. However, the long-run effects of a literary inquisition are more likely to be discernible among ordinary individuals as they would be more likely to be affected by a shortage of teachers and an increase in the cost of basic education.

Clearly, in a general equilibrium framework, the decline in the number of teachers would also lead to an increase in teachers' wages and in a competitive economy with a fluid labor market this would prevent the decline in teacher numbers from being self-reinforcing. China was characterized by labor immobility and wages adjusted very slowly.[45] We focus on partial equilibrium effects here as our purpose is simply to highlight possible channels through which the persecution of intellectuals could have affected human capital accumulation in China. To the extent that labor mobility did exist, we expect it to attenuate any evidence of persistence.

5 Persecutions and Persistence: Evidence from literacy at the end of the Qing dynasty

We test the hypothesis that literary inquisitions had a long-run impact on human capital accumulation by assessing individual literacy outcomes in the early twentieth century using the literacy rates of 80 year olds surveyed in 1982. This measure reflects levels of literacy during the last decade of the Qing dynasty and allows us to overcome the problem posed by the lack of disaggregated human capital data for the nineteenth or most of the twentieth century.

5.1 Literacy Data

We use the Integrated Public Use Microdata Series census (IPUMS) to obtain individual level literacy data for China in 1982—the earliest date for which reliable microdata containing information about literacy is available. To obtain covariates, we match individual level observations from IPUMS data with prefecture-level data from the Historical China County Population Census (HCCPC) from 1982 and prefectural-level information gleaned from historical GIS data.[46]

We study illiteracy among individuals aged at least 80 in 1982 (i.e. those born before 1902).

[45] Kinship groups, lineages, and clans dominated Chinese society. These groups played an important role in ritual worship of ancestors and provided a range of economic services including social insurance (Brook, 1989). As such they acted as a force that limited geographic mobility and migration (for a survey of these kinship groups see Watson, 1982). Moreover, the Chinese state took action to limit mobility (Davids, 2013, 147). Migration did increase in the later Qing period after the disruptions associated with the Taiping Rebellion.

[46] We describe the process involved in matching different datasets in the Appendix.

This provides a sample of 14,642 individuals who were all at least nine years old at the fall of Qing dynasty and therefore obtained their education during a period influenced by the institutions of Imperial China before the collapse of the Qing dynasty. Summary statistics are presented in Table A.10.

5.2 The effect of literary inquisitions on long-run literacy

Our dependent variable is whether or not an individual was illiterate when surveyed in 1982. We restrict our attention to Han Chinese only and to China proper. In all specifications we control for prefecture-level variables that likely affect illiteracy including whether a prefecture is on the coast, had a historical courier route, agricultural suitability, and log population in 1820. We include a measure of social and economic activity encompassing prefectures which were identified in 1820 as important centers of transport and communication (Chong), important in business (Fan), and areas with high crime (Nan).[47] We also control for individual level characteristics that are known to affect literacy such as gender and martial status. In all specifications region effects correspond to socioeconomic macro-region fixed effects. We also employ province fixed effects in all specifications.

We report our main results in Table 8. Column 1 report results from our Logit estimation for the effects of a literacy inquisition on illiteracy in the early twentieth century. The OLS results in Column 2 indicate that an area which experienced a literary inquisition had 3.7 percentage points more illiterate 80 year olds in 1982. This is consistent with the average marginal effect of about 3 percent points that we obtain from our Logit estimation in Column 1. We use OLS for the rest of the analysis.

Pre-existing levels of human capital in a prefecture could have had a positive impact on more recent literacy outcomes. Alternatively, there could be a 'reversal of fortunes' effect if inquisitions occurred in prefectures with higher initial levels of literacy. Columns 3 and 4 control for the number of Ming era jinshi as this measure of prior human capital in a prefecture is an important determinant for pre-existing literacy levels. Adding in these controls reduces the size of our coefficient slightly.

In Column 5 we include age structure and mortality rates for the population. We include these variables because it is possible that the elasticity of life expectancy with respect to literacy is higher in high mortality environments than in lower mortality environments. Both age structure and death rates are insignificant and our results remain unchanged when we control

[47] In all specifications we use the same sample as in our prefectural level DID and as in those estimations we employ a caliper size of 0.002. We only report coefficients on control variables that are either statistically significant or otherwise of economic interest.

for them.[48]

5.3 Controlling for Selective Migration

We now control for the possibility of selective migration and it effect on illiteracy rates. In the wake of the fall of the Nationalist government in 1949 more educated individuals were more likely to migrate to Taiwan—a source of potential bias in our estimates.

To overcome this concern, we create an estimate of the percentage of the population who migrated to Taiwan (the main destination of migrants fleeing the Communists). We use the Taiwan Family Genealogy Catalogue Database—a database that aggregates information from a range of sources, the most important of which is the Taiwan special collection maintained by the Genealogical Society of Utah (GSU).[49]

The measure we obtain from this database is the number of clans (proxied by number of family trees) by prefecture who migrated to Taiwan in the late 1940s. We normalize our migration measure by the prefecture-level population as measured in the 1953 census.[50] In our regressions we distinguish between the records originally obtained from the GSU as these are more reliable from those records collected from other libraries that are also available in the Taiwan Family Genealogy Catalogue Database.

Table 9 provides evidence for the validity of our migration measure. Columns 1 and 2 is consistent with the claim that more educated individuals did migrate to Taiwan. Places with higher recorded migration are associated with fewer individuals with higher and middle school education in 1982. The effect on basic literacy is negative but much smaller in magnitude and not statistically significant.

Table 10 demonstrates that literary inquisitions continue to have had a strong effect on literary among individuals born at the beginning of the twentieth century when we control for selective migration. A comparison of Column 1, which reports our benchmark analysis without controlling for migration, and Columns 2–7, indicates that our results are robust to the inclusion of various measures of migration.[51] In Column 2 we introduce the log of our

[48] In the Appendix Table A.12 we show that our literacy results are robust when we explore how administrative codes and borders changed between 1982 and 1990.

[49] Further details about the source for this migration data are provided in the Appendix.

[50] We use 1953 data to normalize migration as this data is the closest available to the time at which the majority of migration took place. The size of each family that migrated is unknown. But this is unlikely to be a source of bias as there is no reason to think that there would be systematic differences in the size of families that migrated across prefectures. We also assume that there are no systematic differences across prefectures in the literacy of the migrants who fled to Taiwan.

[51] The number of observations declines from 14,642 to 9,042. This is because we only include prefectures that maintained the same administrative codes between 1953 and 1982.

measure of the number of families or clans who are known to have migrated to Taiwan from a given prefecture as taken from the GSU and normalized on a per capita basis. Column 3 includes an alternative control for the number of migration records per capita. Column 4 employs an indicator variable which takes the value of one if there are any records of a family migrating to Taiwan. In Columns 5, 6 and 7 we use a broader set of data on migration to Taiwan. In Column 5 we use the log per capita number of families who migrated, while Column 6 uses the number of migration records per capita; Column 7 uses a binary measure. Introducing controls for selective migration gives quantitatively similar results to those we obtain in Column 1.

Our theoretical framework predicts that literary inquisitions raised the cost of basic education in the Qing period. It provides a mechanism for why the long-run effects of a literary inquisition should be concentrated among the least educated. Furthermore, it is plausible that elite individuals would have been less likely to have been adversely affected by an increase in the cost of basic education or a shortage of teachers. Table 11 provides evidence consistent with the prediction that the effect of a literary inquisition on 80 year olds in 1982 was concentrated among the least educated. Literary inquisitions in the Qing period do not have a statistically significant effect on the number of 80 year olds reporting middle school education (Columns 1 and 2) or higher education (Columns 3–5) in 1982. This remains the case when we control for the prefecture-level illiteracy rate of 80 year olds.

Table 12 depicts the effect of a literary inquisition on illiteracy by cohort. This effect is confined to those born during the last years of the Qing dynasty. For younger cohorts, although the coefficients are positive, they are not precisely estimated. Both Nationalist and Communist governments invested heavily in public schools from the 1920s onwards. Therefore it is unsurprising that we do not find a strong effect associated with literary inquisitions for later cohorts. The exception to this is the Cultural Revolution generation—those aged 15–20 and 21–30 in 1982. This is perhaps not surprising as Cultural Revolution led to the persecution of teachers and destroyed a lot of educational capital.[52]

5.4 The effect of literary inquisitions on the proportion of the population in agriculture

In premodern China there was a close association between illiteracy and working as a peasant in the agricultural economy. We therefore investigate the effect literary inquisitions had on

[52]The legacies of the Cultural Revolution are studied by (Bai, 2014). The Cultural Revolution might be an additional channel through which literary inquisitions might have affected literacy. Literary inquisition may have lowered the social threshold required to persecute individuals.

the proportion of the population who worked in agriculture at a prefecture level. To further explore the long-run dynamics of the impact of a literary inquisition on the proportion of the population in agriculture we use the IPUMS data for 1982 and more recent census data for 1990, 2000, and 2010.

Table 13 depicts the impact of a literary inquisition on the proportion of the population working in agriculture in the 1982 census at the prefectural level.[53] According to Column 1 a literary inquisition in a prefecture is associated with a 7% increase in the proportion of the population that were agricultural in 1982. In Column 2 we no longer control for Ming-era jinshi. Column 3 controls for those prefectures that are listed as autonomous regions as these have different institutions. Column 4 shows that our results remain robust when we exclude post-treatment controls such as our measures of economic activity in 1820, population in 1820 and whether a prefecture contained a treaty port.

[53]We report results for the same matched sample of prefectures that we used in our DID analysis.

To check if this effect persists to this day, Table 14 shows the long-run impact of a literary inquisition on the proportion of the population that were in agriculture in 1982, 1990, 2000, and 2010 in Columns 1–4 respectively. In all specifications we use the same sample of prefectures as in the main analysis ensuring that we do not include prefectures that had border changes between 1982 and 1990. As a robustness check in Table A.14 we conduct the same analysis using all the prefectures for which we have data. The effect we find persisted into the 1990s but loses statistical significance after 2000.

Our results indicate a degree of persistence in the proportion of the population that remained agricultural at a prefectural level. While the modernization of the school system resulted in convergence in levels of literacy from the 1950s onwards, the proportion of the population that remained agricultural remained lower in prefectures affected by a literary inquisition into the 1990s. One possible reason for why this effect disappeared in the 1990s is that by this date all individuals influenced by Qing-era educational institutions had died out. This means that we should not expect to find a direct effect of Qing-era institutions by the 1990s. Nevertheless, restrictions on economic activity and migration continued to hold back literate rural individuals from moving out of agriculture. Once these were liberalized, those prefectures that had been affected by the literary inquisition rapidly became less agricultural as rural-urban migration reduced the number of workers in agriculture and the policy of gradual liberalization led many rural residents to begin to engage in a range of small-scale businesses.

6 The Consequences of the Literary Inquisition

Earlier research suggests that extractive institutions can have a variety of long-lasting effects on a range of economic outcomes including human capital formation (see, for instance, Acemoglu et al., 2001; Nunn, 2008; Dell, 2010; Vidal-Robert, 2014). The political theorist Leo Strauss (1952) observed that the threat of persecution influenced the behavior and writings of intellectuals throughout European history in ways that modern scholars have often struggled to detect: they shaped what was not said as much as what was said. In premodern Europe scholars wrote esoterically and took care to disguise their words and deeds in order to avoid state repression.[54] Important work by Kuran (1987, 1995) has pointed out how political repression can affect economic and political outcomes. Kuran examined how under autocratic regimes individuals have an incentive to falsify their true preferences in response to the fear of persecution. This can lead to an equilibrium in which individuals believe that a regime has more popular support

[54]See Melzer (2014) for a systematic overview of the prevalence of esoteric writing.

than it does in fact enjoy.[55]

These insights are relevant to the case of Qing China.[56] Education in Imperial China was intimately tied to the political system.[57] Our findings that the literary inquisitions conducted by the Qing state had a lasting impact on the behavior of intellectual elites are consistent with qualitative evidence that attests to the psychological impact of the literary inquisition on Chinese scholar-officials. Schmidt (2003, 369) notes that the 'Chinese poet of the age had to be extremely cautious about what he wrote, since a number of authors and their relatives were subjected to horrendous punishments for seemingly innocent lines in their works'.[58] This is alluded to in the writing and poetry of Yuan Mei who noted that he was 'normally ... able to use my wits for the sake of self-preservation' but that life at court forced him into a situation where he faced a choice between his 'personal integrity' and putting his own life in danger (quoted in Schmidt, 2003).[59]

To better understand the significance of the literary inquisition in China, we can compare it with the persecution of intellectuals in Europe. Numerous intellectuals and scholars were punished in early modern Europe for subversive writings: the most notable included Michael Servetus, Giordano Bruno, and Galileo, while others including Descartes, Hobbes, Locke, Bayle, and Rousseau were forced to flee into exile.[60] Many other less celebrated individuals were executed or imprisoned for blasphemy. As late as 1697 (eight years after the Glorious Revolution) Thomas Aikenhead, a student at the University of Edinburgh was executed for expressing atheist views and rejecting Christ and the Trinity (Hunter, 2004). But the threat of persecution weakened in the eighteenth century; and scholars such as Joel Mokyr have argued that this increased level of intellectual freedom played an important role in the

[55]More recently Greif and Tadelis (2010) show how coercion or the threat of persecution can generate a crypto-morality—that is, the secret adherence to one morality while practicing another in public.

[56] Greif and Tadelis (2010, 241) do briefly discuss the case of China without focusing on the Qing period.

[57]See for example Wakeman (1972). Cheong 'examinations helped generate the link between the throne and the Confucian-educated elites; this linkage was central to the state's authority, and the production of ideology was a crucial function of this nexus' (Man-Cheong, 2004, 8).

[58]See Jones (1975, 28). Also see Wiens (1969, 16).

[59]This attitude is clear in Yuan Mei's poem 'Avoiding the Heat':

> 'There's no other method of avoiding the heat;
> There is a secret recipe for saving your life:
> Just stay far, far away from the crimson sun,
> Then you'll feel how cool the blue sky can be!'

Schmidt (quoted in 2003, 371).

[60]See for an overview Zagorin (2003); Johnson and Koyama (2013). As the French Encyclopédie observed that 'the conditions of the sage is very dangerous: there is hardly a nation that is not soiled with the blood of several of those who have professed it' (quoted in Melzer, 2014, 139).

rise of the Western Europe and the Great Divergence.[61] As Parker (2013) observes: in contrast, '[t]he Qing thus continued to see intellectual innovation and much "useful knowledge" as a potential threat, not a potential asset ... Unlike rulers in northwest Europe, China's new masters refused to allow their leading scholars either freedom of expression or freedom to exchange ideas' (Parker, 2013, 667).[62]

While we have focused on the effect of the literary inquisition on human capital accumulation, the persecution of intellectuals had manifold possible consequences that we can only briefly mention here. To reduce the risk of persecution intellectuals avoided activities that could interpreted and constituting opposition to Qing rule; instead they 'immersed themselves in the non-subversive "sound learning" and engaged in textual criticism, bibliography, epigraphy, and other innocuous purely scholarly pursuits' (Wiens, 1969, 16). Hung (2007, 2011) argues that while in earlier periods in Chinese history, elites drawn from exam graduates played a crucial role in mediating protests and provided a bridge between the government and the peasantry, this ceased to be the case by the early nineteenth century. As scholar-officials no longer played this role, protests became more violent and the later Qing period had to deal with increasingly frequent and destabilizing peasant rebellions, which impeded the dynasty's ability to respond adequately to European imperialism in the late nineteenth century. Furthermore, whereas in Meji-era Japan, the samurai elite were able to both abolish the old feudal system and modernize the economy and the state, historians have suggested that the absence of a cohesive elite in nineteenth century China may have also have played a role in the failure of the Chinese state to successfully industrialize.

In this paper we have provided the first empirical evidence of the effects of the repression of knowledge elites on human capital accumulation. Ideally, it would be informative to investigate whether or not literary inquisitions had an effect on innovation or scientific thought. Unfortunately, we lack data to test this hypothesis directly. The one scholar who could be counted as a scientist was Zhu Fangdan who claimed that the brain rather than the heart enabled thought. This went against the Confucian orthodoxy of the time. This intellectual dissent and his popularity as doctor meant he came to be seen as a threat by the Kangxi emperor. As

[61] See, for example, the discussion in Mokyr (2007, 19–27).

[62] Another important contrast is that in Europe the governing classes and the intellectuals were distinct groups. But in China the literati and scholars were also expected to rule the empire. Etienne Balazs described the scholar-official, who 'omnipotent by reason of their strength, influence, position, and prestige, held all the power and owned the largest amount of land' (Balazs, 1964, 16). This power and responsibility meant that the Qing state could not permit them intellectual freedom: they relied on them to govern. Therefore subversive opinions among the bureaucracy could not be tolerated. Furthermore, China was a vast and unified empire (see, for further analysis Ko et al., 2014). While Descartes could escape to the Netherlands and Sweden and Rousseau to England, Chinese intellectuals who fell foul of the emperor could not escape, but rather had to submit to Imperial authority.

a result, he was executed along with his students and his books destroyed. At the very least, the timing of the literary inquisition is suggestive: just as the intellectual straitjacket holding back thinkers in Europe was loosening, it became tighter and more restrictive in China.[63]

7 Conclusion

This paper provides systematic empirical evidence that the persecution of intellectuals in Imperial China—known as literary inquisitions—had an effect on human capital formation in China at a local level in both the short-run and the long-run. Using new data we show that these persecutions deterred individuals from entering the imperial examinations at both the provincial and the prefectural level. A simple model links persecutions to fewer teachers and higher costs of basic education. Using the earliest available modern data on literacy levels, we find that individuals in prefectures associated with victims of the literary inquisition in the seventeenth and eighteenth centuries had lower levels of literacy at the beginning of the twentieth century controlling for a host of individual, prefectural, and provincial characteristics and correcting for the possibility of selective migration during the Communist takeover. We show that prefectures affected by a literary inquisition also remained more agricultural to this day.

Our findings shed light on one reason why China was unable to transition to sustained economic growth during the pre-industrial period and why it struggled to catch up to Europe in the late nineteenth and early twentieth centuries. Human capital and especially upper tail human capital is a crucial ingredient for both innovation and convergence (Squicciarini and Voigtländer, 2014). In Qing China the persecution of intellectuals caused fewer individuals to acquire upper tail human capital. In the long-run we have provided evidence that this undermined the ability of ordinary people to acquire basic education. Exploring how the policies of the Chinese state affected innovation or governance awaits future research.

[63]Mokyr observes that it is 'highly probable that men and women with novel ideas emerged outside the West and would have been part of an Islamic Enlightenment or Chinese Enlightenment' and cites the example of individuals like Tai Chen (Dai Zhen) (1724-1777). However, as Mokyr himself comments, 'Chen was arrested and deported (twice)' (Mokyr, 2005b, 340).

A Data Appendix

Exam Candidates Data

Our data on exam candidates is from the China Biographical Database Project (CBDB). This on-line database originally began with the work of the historian Robert M. Hartwell (1932–1996) and is an ongoing project. It is the largest electronic database for the study of Chinese historical biographies in the world. China has a tradition of compiling elite biographies going back two thousand years. The CBDB relies on modern sources of biographical data, traditional biographical records, and evidence for office holding from modern and traditional sources. In total the CBDB contains over 328,000 individuals for all Chinese history. We use the 3,531 records pertaining to individuals who obtained government offices between 1660 and 1840 through the exam track. The CBDB provides the geo-coordinates of these individuals which we use to assign each individual to a province and prefecture.

According to the CBDB this data has been collected in a way to ensure that is as representative as possible. In particularly, to avoid selection bias they have not relied on local gazetteers as that would introduce a source of regional bias.

Historical GIS Data and Other Controls

The bulk of our historical GIS data comes from the WorldMap page maintained by Harvard University. The shape-file for Qing-era prefectures is from Bol (2011). Data on distance to the coast is from Stumpf (2009). Agricultural suitability data is from the FAO (Fischer et al., 2002). Rough estimates of Chinese population density in 1600 by geo-scientists are from Klein Goldewijk et al. (2011).

Our data on extreme floods and droughts is from Central Meteorological Bureau of China (1981). Data on extreme temperature is extrapolated from Mann et al. (2009). War and conflict data is from Chen (1939). We are grateful to Tuan-Hwee Sng and Se Yan for sharing their conflict data with us. Data on the location of Buddhist temples is from:

http://www.fas.harvard.edu/ chgis/data/chgis/downloads/v4/.

Literacy Data

As mentioned in the main text, our literacy data are at a prefectural level and come from IPUMS. The IPUMS data provides a series of individual level controls including gender, ethnicity, number of married couples in the household, and marital status. We employ a host of

prefecture-level controls for 1982 from the Historical China County Population Census. These include demographic controls such as the percent of the population over 65 and death rates in 1981.

GIS data enables us to additionally control for the usual geographical covariates that previous research has indicated can influence both economic and educational outcomes such as distance to coast and ruggedness. We use GIS data to construct a range of historical controls which we match to 1982 prefectures. These historical characteristics include agricultural suitability, distance to the Grand Canal and to the Yangtze river, distance to courier routes from 1820 to 1893, and whether a prefecture included a treaty port (as in Jia, 2014a).

Migration Data

For information on selective migration we use the Taiwan Family Genealogy Catalogue Database.[64] This database aggregates information from a range of sources, the most important of which is the Taiwan special collection maintained by the Genealogical Society of Utah (GSU). The GSU was founded in 1894 by members of the Church of Jesus Christ of Latter-Day Saints to preserve historical records for genealogical research and it collects data from across the world. Since 1976, the GSU has collaborated with academic institutions in Taiwan to locate microfilm and other privately owned genealogical sources.

We remove duplicates (i.e., where the same family is recorded by more than one library) and focus on families that migrated during the period of interest. We only include records for families for whom we have information on the their known residence in mainland China. Guangdong and Fujian are excluded from the analysis because they had large-scale migration during the Ming and Qing period as this migration was not selected on literacy and we cannot we distinguish these families from those who migrated specifically in response to the Communist takeover.

Matching Procedure

We generate a propensity score for each prefecture by estimating a logistic regression on a set of pre-treatment covariates. Specifically we estimate:

$$P(LQ_i = 1) = F(X_i) \tag{5}$$

where P is the probability or propensity score that a prefecture produces an official who is persecuted as a result of a literary inquisition and X_i is our vector of covariates. The covari-

[64] This is available at http://rarebook.ncl.edu.tw/rbook.cgi/frameset5.htm.

ates we employ include the number of government officials recruited from each prefecture in the previous (Ming) dynasty from 1368 to 1644. During the Ming period the examinations worked in a particular way. Examinees competed with one another within an exam region, for a fixed percentage of total number of degrees. Therefore we also include “exam region" as a pre-treatment covariate. “Exam region" refers to North, South and Central Exam Region. Then we include variables that contain critical information on economic development and educational resources prior to 1660. Specifically, we include population density in 1600, agricultural suitability, trading activities, number of courier routes and whether they were dash routes, distance to the Grand Canal or Yangtze river and change in elevation (slope). We also include number of Buddhist temples as a pre-treatment covariate, in light of the fact that Buddhist priests were not permitted to attend the civil service exam. A set of socioeconomic macro-region fixed effects are included to capture other socioeconomic differences. Table A.3 presents the results of the regression analysis we employ to match prefectures on pre-treatment characteristics.

From inspecting the data we find that areas that had inquisition cases were much more likely to be more economically developed. A logit analysis suggests that prefectures affected by literacy inquisition had higher population density and more courier routes, though those coefficients are not significant. Less ambiguously, those prefectures had far more jinshi during the Ming period. To match prefectures on their pre-treatment covariates, we employ the nearest neighbor matching method to match each prefecture with a prefecture with the most similar propensity score in the comparison group. We use a tight caliper (0.002). To ensure common support, we further reduce the sample size by trimming out observations with a propensity score lower than 0.1 or higher than 0.6. Hence 71 prefectures are left in the final sample we use for the prefecture-level analysis. We choose a caliper width of 0.002 in our baseline so as to minimize bias while still retaining a sufficient number of observations. We show our results are robust under a range of calipers of different widths (Table A.5). Figure A.1 depicts the matched prefectures by caliper width.

Table 6: Prefecture Level DID Estimation: Interacting Prefecture Characteristics with Time Trends

	# Officials				
	(1)	(2)	(3)	(4)	(5)
Affected by Inquisition	-0.306*	-0.304*	-0.307**	-0.324**	-0.325**
	(0.155)	(0.153)	(0.153)	(0.157)	(0.159)
Ag Suitability*Time Trend	No	Yes	Yes	Yes	Yes
Ming Trade Area*Time Trend	No	No	Yes	Yes	Yes
Grand Canal/Yangtze*Time Trend	No	No	No	Yes	Yes
Distance to Coast*Time Trend	No	No	No	No	Yes
Ming Exam Region*Time Trend*Trend Break in 1712	Yes	Yes	Yes	Yes	Yes
Log Pop in 1600 *Time Trend	Yes	Yes	Yes	Yes	Yes
Log Pop in 1600 ^{2}*Time Trend	Yes	Yes	Yes	Yes	Yes
Latitude*Time Trend	Yes	Yes	Yes	Yes	Yes
Longitude* Time Trend	Yes	Yes	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes
Province-specific decade FE	Yes	Yes	Yes	Yes	Yes
Observations	1278	1278	1278	1278	1278
Adjusted R^2	0.042	0.041	0.040	0.048	0.047

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table demonstrates that our results are stable when we interact time invariant controls with time trends. In our baseline we include all of the controls and interaction effects employed in Column 1 of Table 5. Column 2 interacts agricultural suitability with a time trend. In Column 3 we allow for the effect of being within the Ming trade area to vary over time. The Ming trading area was the richest and most developed coastal part of China that was most involved in foreign trade while the Ming non-trading area was located in north-western China and did not have foreign trade. Both of these regions may have experienced differential economic trends which could have biased our results. In particular the Ming trading area suffered a temporary fall in the number of exam candidates due to the Qing take-over but it recovered rapidly in the eighteenth century. In the mid-eighteenth century, however, the Qing government further restricted all foreign trade. In Column 4 Grand Canal or Yangtze refer to prefectures that border either the Grand Canal or Yangtze river. Column 5 controls for whether or not a prefecture was within 50 km of the coast.

Table 7: Prefecture Level DID Estimation: Controlling for Time Varying factors

	# Officials			
	(1)	(2)	(3)	(4)
Affected by Inquisition	-0.301*	-0.301*	-0.309*	-0.310*
	(0.155)	(0.155)	(0.157)	(0.157)
Extreme Temperature	-0.142	-0.142	-0.137	-0.134
	(0.200)	(0.200)	(0.200)	(0.199)
Conflict		0.0883	0.0741	0.0766
		(0.116)	(0.115)	(0.117)
Earthquakes			-0.428	-0.424
			(0.503)	(0.507)
Natural Disasters				0.161
				(0.398)
Province FE	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes
Province-specific decade FE	Yes	Yes	Yes	Yes
Baseline Controls*Time Trend	Yes	Yes	Yes	Yes
Observations	1278	1278	1278	1278
Adjusted R^2	0.042	0.042	0.044	0.043

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table demonstrates that our results are robust to the inclusion of time-varying factors that might affect the number of exam candidates. In our baseline we include all of the controls and interaction effects employed in Column 1 of Table 5.

Table 8: Illiteracy of 80 year olds in 1982

	Illiteracy				
	(1)	(2)	(3)	(4)	(5)
	Logit	OLS	OLS	OLS	OLS
Inquisition	0.529**	0.0371**	0.0226**	0.0253**	0.0221**
	(0.224)	(0.0155)	(0.0101)	(0.0104)	(0.0101)
Agricultural suitability	-0.146**	-0.00980**	-0.00795**	-0.00856**	-0.00660
	(0.0720)	(0.00469)	(0.00353)	(0.00366)	(0.00450)
Female	3.160***	0.248***	0.248***	0.248***	0.248***
	(0.175)	(0.0155)	(0.0155)	(0.0155)	(0.0155)
Married	-0.927***	-0.128***	-0.131***	-0.130***	-0.131***
	(0.324)	(0.0303)	(0.0297)	(0.0297)	(0.0296)
N. married couples in household	0.162***	0.0115***	0.0132***	0.0130***	0.0132***
	(0.0578)	(0.00365)	(0.00356)	(0.00357)	(0.00356)
Log Ming jinshi			-0.0251***		-0.0263***
			(0.00357)		(0.00428)
Log per capita Ming jinshi				-0.0212***	
				(0.00373)	
% over 65					0.000369
					(0.00944)
Death rate					0.00717
					(0.0108)
Province FE	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes
Observations	14642	14642	14642	14642	14642
Adjusted R^2		0.176	0.181	0.180	0.181
Pseudo R^2	0.273				

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows the effects of a literary inquisition at a prefectural level on the illiteracy rates of 80 years in 1982. Other control variables include: whether a prefecture is on the coast, had a historical courier route, measures of social and economic activity (specifically prefectures were designed by the government as belonging to Chong (important as centers of transport and communication), Fan (important in business), and Nan (areas with high crime)), a measure of ruggedness, log population in 1820, or if a prefecture contained a Treaty Port. We also control for whether an individual is separated or divorced, and widowed or widowers.

Table 9: Selective migration and educational attainment

	(1) Higher Education	(2) Middle School	(3) Literacy
Migration records p. c	-0.0101***	-0.0129***	-0.00207
	(0.00160)	(0.00459)	(0.00205)
Inquisition	-0.0140	-0.0537	-0.0413**
	(0.00978)	(0.0566)	(0.0154)
Controls	Yes	Yes	Yes
Province FE	Yes	Yes	Yes
Region FE	Yes	Yes	Yes
Observations	782	782	9042
R^2	0.092	0.079	0.158
Adjusted R^2	0.055	0.041	0.155

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table provides evidence for the validity of our migration variable. Controls includes all controls used in Table 8.

Table 10: Illiteracy of 80 year olds in 1982 controlling for selective migration

	Illiteracy Rate						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Inquisition	0.0394**	0.0405**	0.0392**	0.0368**	0.0404**	0.0415***	0.0382**
	(0.0155)	(0.0152)	(0.0164)	(0.0153)	(0.0151)	(0.0153)	(0.0146)
Log migration records p.c. (GSU)		0.00227					
		(0.00206)					
Migration records p. c. (GSU)			8.92e-09				
			(5.92e-08)				
Any migration records (GSU)				0.0140			
				(0.0122)			
Log migration records p.c. (all measures)					0.00137		
					(0.00118)		
Migration records p.c. (all measures)						5.37e-09	
						(4.23e-09)	
Any migration records (all measures)							0.0215
							(0.0167)
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9042	9042	9042	9042	9042	9042	9042
Adjusted R^2	0.154	0.154	0.154	0.154	0.154	0.154	0.155

Robust standard errors clustered at the prefectural level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports the effects of a literary inquisition on illiteracy at a prefectural level among 80 years in 1982 controlling for selective migration. Other controls include all controls used in Table 8.

Table 11: Educational attainment of 80 year olds in 1982

	(1)	(2)	(3)	(4)	(5)
	Middle School		Higher Education		
	Logit		Logit	OLS	OLS
Inquisition	-0.318	-0.222	-0.0748	0.00227	0.0113
	(0.240)	(0.244)	(0.621)	(0.0136)	(0.0126)
Prefecture-level illiteracy rate (> 80)		-2.296			-0.273***
		(1.434)			(0.0923)
Controls	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Observations	1428	1428	1238	1435	1435
Adjusted R^2				0.023	0.027
Pseudo R^2	0.066	0.067	0.196		

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The long-run effect of a literary inquisition on the percentage of 80 years in 1982 who attended either middle school or higher education. Controls are identical to those used in Table 8.

Table 12: The effect of a literary inquisition on illiteracy by cohort in 1982.

	Age cohorts							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	81–90	71–80	61–70	51-60	41–50	31–40	21–30	15–20
Odds ratio	1.371	1.192	1.170	1.209	1.252	1.210	1.5018	1.5097
Mean of depvar	0.087	0.212	0.284	0.491	0.668	0.778	0.85	0.902
Inquisition	0.0237**	0.0212	0.0240	0.0312	0.0402	0.0265	0.0544*	0.0318*
	(0.0105)	(0.0145)	(0.0196)	(0.0247)	(0.0285)	(0.0286)	(0.0327)	(0.0181)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14347	80357	176291	275846	345950	444621	624151	507406
Adjusted R^2	0.181	0.204	0.239	0.282	0.271	0.213	0.215	0.082

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Literary inquisitions only have a statistically significant effect on illiteracy rates among those born in the Qing dynasty and the Cultural Revolution generation. Controls include all controls from Table 8.

Table 13: The effect of a literary inquisition on the proportion of the population in agriculture (%)

	Proportion of Population in Agriculture			
	(1)	(2)	(3)	(4)
Inquisition	7.284*	7.901*	6.647*	9.162**
	(3.931)	(3.988)	(3.811)	(3.874)
Log Ming jinshi	-2.767*			
	(1.571)			
Had a "busy" county in 1820	-4.403	-3.751	-1.162	
	(5.478)	(5.567)	(5.366)	
Treaty port	0.128	0.895	2.791	
	(5.086)	(5.162)	(4.945)	
Log population in 1820	6.105**	4.120*	4.405**	
	(2.486)	(2.257)	(2.142)	
Autonomous regions			-13.02***	
			(4.827)	
Other controls	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Observations	86	86	86	87
Adjusted R^2	0.417	0.396	0.456	0.384

Robust standard errors clustered at the prefectural level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The long-run impact of a literary inquisition on the proportion of the population that was agricultural in 1982. Other controls include agricultural suitability, ruggedness, economic activity in 1820, whether a prefecture is on the coast, and whether it had a courier route.

Table 14: The effect of a literary inquisition on the proportion of the population in agriculture

	The Proportion of Population in Agriculture (%)			
	(1)	(2)	(3)	(4)
	1982	1990	2000	2010
Inquisition	8.239***	8.942*	6.828	5.086
	(2.988)	(5.145)	(5.545)	(4.144)
Agricultural suitability	-1.329	-0.547	-1.719	-0.102
	(1.910)	(2.925)	(2.274)	(2.103)
Other Controls	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Observations	57	57	57	57
Adjusted R^2	0.343	0.359	0.454	0.554

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The long-run impact of a literary inquisition on the proportion of the population of a prefecture that was agricultural in 1982, 1990, 2000, and 2010. In this sample we include all prefectures whose administrative codes remained the same from 1982 to 2010. Other controls include whether a prefecture is on the coast, or had a courier route .

Table A.1: Summary statistics for main variables and time invariant controls

Variable	Description/Sources	Mean	Std. Dev.	N.
	Provincial level			
Inquisition	Indicator variable whether a province experienced an inquisition	0.765	0.425	17
Exam Candidates	N. officials entering government through the exam track (jinshi, juren and others) per decade per province	5.827	6.473	
Affected by inquisition	Indicator variable for each decade after a province was affected by an inquisition	0.443	0.497	323
	Prefectural level			
Inquisition	Indicator variable whether a prefecture experienced an inquisition	0.155	0.364	71
Exam Candidates	N. officials entering government through the exam track (jinshi, juren and others) per decade per prefecture	0.455	1.11	
Affected by inquisition	Indicator variable for each decade after a prefecture was affected by an inquisition	0.07	0.255	1278
	Time Invariant Controls			
Log population in 1600	Source: Klein Goldewijk et al. (2011)	13.32	0.83	71
Log population density in 1600	Source: Klein Goldewijk et al. (2011)	3.93	0.88	71
Latitude	Source: Bol (2011)	31.886	4.24	71
Longitude	Source: Bol (2011)	114.90	3.11	71
Agricultural suitability	Source: Fischer et al. (2002)	-4.39	1.65	71
Log (distance to Grand Canal or Yangtze+1)	Source: Bol (2014)	8.28	5.48	71
N. courier routes	Source: Skinner and Yue (2011).	2.89	2.10	71
N. dash routes		0.76	1.247	71
N. Buddhist temples	Source: Berman (2011)	11.90	10.57	71

Table A.2: Summary statistics for time varying controls

Time Varying Controls				
Variable	Description/Sources	Mean	Std. Dev.	N.
Conflict	Count variable that includes rebellions	0.422	0.98	1278
Natural Disasters	Average of years of severe flood or drought (2), less severe flood or drought (1), and no flood or drought (0)	0.772	0.161	1278
Earthquakes	Number of earthquakes greater than 5.5 on the Richter scale.	0.01	0.108	1278
Extreme Temperature	Number of years outside of 3 standard deviation from the mean	0.005	0.068	1278
Conflict (binary)	Dummy variable that includes rebellions	0.226	0.418	1278
Earthquakes (binary)	Dummy variable for earthquakes greater than 5.5 on the Richter scale.	0.009	0.096	1278

Summary statistics for our time varying control variables.

(a) Caliper size: 0.001

(b) Caliper size: 0.002

(c) Caliper size: 0.003

(d) Caliper size: 0.004

(e) Caliper size: 0.005

Figure A.1: Matched prefectures based on different caliper sizes.

Table A.3: Regression Analysis of the Likelihood of an Inquisition

	(1) Logit	(2) OLS
Ming N. successful exam candidates – *jinshi*	0.950***	0.112***
	(0.0627)	(0.00686)
N. courier routes	0.0113	0.0124***
	(0.0251)	(0.00467)
N. dash routes	0.167***	0.0208**
	(0.0593)	(0.00981)
Agricultural suitability	-0.344***	-0.0626***
	(0.0478)	(0.00640)
N. Buddhist temples	-0.0147***	-0.000203
	(0.00466)	(0.000886)
Ming Exam Area — North	-0.0377	-0.0341
	(0.151)	(0.0270)
Ming Exam Area — Central	0.516*	0.0539
	(0.272)	(0.0402)
Log population in 1600	0.229***	0.0298***
	(0.0709)	(0.00895)
Log (distance to Grand Canal or Yangtze+1)	0.406***	0.0636***
	(0.0654)	(0.0100)
Log (distance to Grand Canal or Yangtze+1)^2	-0.0393***	-0.00573***
	(0.00575)	(0.000850)
Ruggedness (quartiles)	Yes	Yes
Region FE	Yes	Yes
Observations	3173	3648
Adjusted R^2		0.243
Pseudo R^2	0.213	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Additional Tables

In this section of the appendix we present additional tables that demonstrate the various robustness checks and exercises that we conducted. In Table A.4 we first interact the number of Ming-era examination candidates (jinshi) with decade fixed effects (Column 1). This shows that our results are unaffected when we allow for the effects of past human capital to vary over time. Second, we interact the propensity score for each prefecture with decade fixed effects

Table A.4: Prefecture Level DID Estimation:

	# Officials		
Affected by Inquisition	-0.309**	-0.321**	-0.847*
	(0.151)	(0.152)	(0.444)
Ming Jinshi*Decade FE	Yes	Yes	Yes
Propensity Score*Decade FE	No	Yes	Yes
Timing of Inquisition*Decade FE	No	No	Yes
Baseline Controls	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes
Province-specific Decade FE	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes
Observations	1278	1278	1278
R^2	0.273	0.280	0.333
Adjusted R^2	0.069	0.061	0.100

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In all specifications we interact decade fixed effects with the log of the number of Ming jinshi. Column 2 includes decade fixed effects interacted with the propensity score of each prefecture. In Column 3 we control for the timing of inquisition variable by using a dummy variable for post-1750 persecutions.

(Column 2). Finally, we allow for post-1750 inquisitions to have a different effect to pre-1750 inquisitions. Our results are unaffected.

Table A.5 shows that our estimates are not affected by employing different caliper sizes in our matching exercise. The coefficient we report in Column 1 where we use a caliper of 0.001 loses statistical significance likely due to the smaller sample size. Column (2) reproduces our benchmark estimates with a caliper size of 0.002. Our results remain economically and statistically significant as we expanded the caliper size to 0.003, 0.004, and 0.005.

Table A.6 demonstrates that our results are robust to varying the starting years of our analysis. Column (1) reports our baseline estimates. We want to show that our results are not driven by the early Qing period when there was widespread anti-Qing sentiment and movements in parts of China. Columns (2), (3), and (4) begin the analysis in 1670, 1680, and 1690 respectively and we can show that the coefficient we obtain for the effect of an inquisition is stable across specifications.

In Table A.7 we systematically exclude regions of China that experienced differential eco-

Table A.5: Prefecture Level DID Estimation: Robustness to Different Caliper Sizes

	# Officials				
	(1)	(2)	(3)	(4)	(5)
Affected by Inquisition	-0.339	-0.306*	-0.252**	-0.211*	-0.211*
	(0.210)	(0.155)	(0.121)	(0.112)	(0.112)
Baseline Controls	Yes	Yes	Yes	Yes	Yes
Baseline Controls*Time Trend	Yes	Yes	Yes	Yes	Yes
Time-varying Controls	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes
Province-specific Decade FE	Yes	Yes	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes
Caliper size	0.001	0.002	0.003	0.004	0.005
Observations	1224	1278	1368	1422	1422
Adjusted R^2	0.052	0.042	0.048	0.050	0.050

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports the sensitivity of our estimates to different caliper sizes in our matching exercise. Column (2) reproduces our benchmark estimates which employ a caliper size of 0.002. Our results remain economically and statistically significant as we expanded the caliper size. The results in column (1) lose statistical significance due to the smaller sample size.

nomic or political developments during the period under study. Column 3 omits prefectures which produced no examination candidates. In Column (2) we exclude Zhejiang from the sample, because it is documented that the first metropolitan exam after the 1727 inquisition was not open to examinees from Zhejiang Province. Column (3) leaves out the Lower Yangtze area, which includes Jiangsu, Anhui, and Zhejiang, because this region is highlighted by historians such as Pomeranz (2000) as being particularly economically developed in the seventeenth and eighteenth centuries. Column (4) drops Guangdong.

In Table A.8 we demonstrate that our results are robust to the exclusion of prefectures which had sizable migration during the period under study. Column (1) reports our baseline specification. In Column (2) we use the data we collected from Ge (2005) to exclude all prefectures mentioned as having migration; in Column (3) we exclude all prefectures listed as having moderate migration—that is, migration levels of up to 50%; finally, in Column (4) we exclude

Table A.6: Prefecture Level DID Robustness: Different Starting Periods

	# Officials			
	(1)	(2)	(3)	(4)
Affected by Inquisition	-0.306*	-0.285*	-0.292*	-0.288*
	(0.155)	(0.150)	(0.152)	(0.163)
Baseline controls	Yes	Yes	Yes	Yes
Baseline Controls*Time Trend	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes
Province-specific Decade FE	Yes	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes
Start Year	1660	1670	1680	1690
Observations	1278	1207	1136	1065
Adjusted R^2	0.042	0.040	0.034	0.026

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table demonstrates that our results are robust to excluding the early Qing period when there was widespread anti-Qing sentiment and rebellions in parts of China. Column (1) reports our baseline estimates. Columns (2)–(4) begin the analysis in 1670, 1680, and 1690 respectively.

only those prefectures that experienced more than 50% migration. The coefficient we obtain for the effect of a literary inquisition on examinee numbers remains more or less unaffected.

For our long-run analysis in Table A.12 we take into account different administrative levels in China in 1982. The IPUMS data and HCCPC data use different administrative codes. Specifically the HCCPC data records data at the level of 1992 administrative boundaries. In the baseline we only include prefectures which did not have boundary changes between 1982 and 1992 because such changes prevent us from accurately matching data from our two datasets. Column 1 reports our baseline. This coefficient is unchanged when in Column 2 we include the main sample plus prefectures that did have boundary changes between 1982 and 1991. We exclude prefectures with either boundary change or administrative code change between 1991 and 2000 in Column 3. To further show that our results are not being driven by selection on unobserved prefectural level characteristics such as different provision public goods, Column 4 allows for a difference in the level of illiteracy between mostly Han prefectures (administrative codes end with 1-20, 20-70) and autonomous prefectures (administrative codes end with 20-50).

Table A.7: Prefecture Level DID Robustness: Omitting Particular Regions

	# Officials			
	(1)	(2)	(3)	(4)
Affected by Inquisition	-0.308*	-0.338**	-0.293*	-0.339**
	(0.173)	(0.164)	(0.148)	(0.168)
Baseline Controls	Yes	Yes	Yes	Yes
Baseline Controls* Time Trend	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes
Province-specific decade FE	Yes	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes
Omitted Region	No Candidates	Zhejiang	Lower Yangtze	Guangdong
Observations	1242	1206	1008	1188
Adjusted R^2	0.039	0.041	0.033	0.042

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows that our results survive excluding regions of China that for historical reasons experienced differential economic or political trends.

In Table A.13 we show that our results still hold when we stop controlling for post-1820 developments. Column 1 reports our baseline estimates. In Column 2 we no longer employ our measure of whether a prefecture had specifically prefectures were identified by the government as Chong (important as centers of transport and communication), Fan (important in business), and Nan (areas with high crime). The reason for doing this is that there may be some concern that post-inquisition controls could themselves be influenced by whether or not a prefecture experienced an inquisition. In Column 3 we also no longer control for population in 1820 as this might be partially affected by the presence of an inquisition in a prefecture. Finally, in Column 4 we no longer control for whether a prefecture contained a treaty port.

Table A.14 reports the persistent effects of a literary inquisition on the proportion of the population that was agricultural in the censuses of 1982, 1990, 2000, and 2010. It reports all prefectures for which we have data. The results we obtain are comparable to those in Table 14. The effect of a literary inquisition loses significance after 1990.

Table A.8: Prefecture Level DID Robustness: Migration

	# Officials			
	(1)	(2)	(3)	(4)
Affected by Inquisition	-0.306*	-0.406**	-0.303*	-0.295*
	(0.155)	(0.184)	(0.171)	(0.167)
Baseline Controls	Yes	Yes	Yes	Yes
Baseline Controls*Time Trend	Yes	Yes	Yes	Yes
Time-Varying Controls	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes
Province-Specific Decade FE	Yes	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes
Excluded Prefectures	None	All prefectures with possible migration possible migration	Prefectures with moderate ($< 50\%$) migration	Prefectures with high ($> 50\%$) migration
Observations	1278	1116	1152	1188
R^2	0.239	0.246	0.247	0.239
Adjusted R^2	0.042	0.036	0.046	0.040

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows that our results are robust to the exclusion of prefectures that had large numbers of migrants during the Qing period according to Ge (2005).

Table A.9: Placebo regressions

	# Officials					
	(1)	(2)	(3)	(4)	(5)	(6)
Affected by Inquisition	-0.847*					
	(0.444)					
Ten year placebo		-0.205				
		(0.467)				
Twenty year placebo			-0.199			
			(0.293)			
Thirty year placebo				-0.268		
				(0.307)		
Forty year placebo					0.0518	
					(0.364)	
Fifty year placebo						0.0998
						(0.359)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls*Time Trend	Yes	Yes	Yes	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-Specific Decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Prefecture FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1278	1278	1278	1278	1278	1278
R^2	0.333	0.331	0.331	0.331	0.331	0.331
Adjusted R^2	0.100	0.097	0.097	0.097	0.097	0.097

Robust standard errors clustered at the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Placebo regressions at a prefectural level.

Table A.10: Summary statistics for persistence analysis

Variable	Mean	Std. Dev.	N
Individual level variables			
Illiteracy	0.902	0.297	14642
Middle School	0.225	0.418	1435
Higher Education	0.023	0.15	1435
Female	0.673	0.469	14642
N. married couples in household	0.756	0.67	14642
Prefectural level variables			
Inquisition	0.256	0.439	86
Ming Jinshi	3.218	1.541	86
Ming Jinshi per capita	3.575	1.452	86
Agricultural suitability	-4.047	1.795	86
Includes Grand Canal or Yangtze	0.314	0.467	86
On the coast	0.081	0.275	86
On a courier route	0.64	0.483	86
'Chong' 1820	0.767	0.425	86
Ruggedness	1.249	0.742	86
Treaty port	0.128	0.336	86
% over 65	5.034	0.864	86
Death rate	6.314	1.017	86
Log population in 1820	4.11	0.836	86
Regular prefectures	0.826	0.382	86
% Population in agriculture	73.34	15.708	86
Birth rate	19.8	2.941	86
College (individuals per 10,000)	46.621	60.629	86
Middle school (individuals per 10,000)	1830.397	473.238	86
% Illiteracy	33.166	9.525	86
Log per capita migration records (GSU)	0.981	3.112	47
Migration records (GSU)	26396.005	124676.371	47
Any migration records p.c. (GSU)	0.319	0.471	47
Log per capita migration records (all measures)	2.364	4.739	47
Migration records (all measures	298864.304	1031967.158	47
ny migration records per capita (all measures)	0.447	0.503	47

Table A.11: For a description of the 1982 IPUMS survey see main text and data appendix.

Table A.12: Literacy regressions robustness: Administrative code or boundary changes

	Illiteracy Rate			
	(1)	(2)	(3)	(4)
Inquisition	0.0385***	0.0382***	0.0410***	0.0369***
	(0.0137)	(0.0133)	(0.0142)	(0.0132)
Autonomous regions				-0.0486***
				(0.0110)
Boundary changes 1982-1991	Yes	No	No	No
Admin. code or boundary changes 1992-2000	Yes	Yes	No	Yes
Region FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Observations	14642	14856	12792	14642
Adjusted R^2	0.177	0.177	0.180	0.178

Robust standard errors clustered on the prefectural level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows that the effects of a literary inquisition of illiteracy rates of 80 years olds in 1982 is unaffected by controlling for the changes in the administrative codes for different prefectures. Column 1 reports the baseline. Column 2 reports our coefficient for the main sample plus prefectures that had boundary changes between 1982 and 1991. Column 3 excludes prefectures with either boundary change or administrative code change between 1991 and 2000. To further show that our results are not being driven by selection on unobserved prefectural level characteristics such as different provision public goods, Column 4 allows for a difference in the level of illiteracy between regular prefectures (administrative codes end with 1-20, 20-70) and autonomous regions, (administrative codes end with 20-50). Other controls include: whether a prefecture is on the coast, agricultural suitability, log Ming jinshi, whether a prefecture had a treaty port, ruggedness, had an historical courier route, measures of social and economic activity and log population in 1820, individuals who are married, separated or divorced, widowed or widowers, number of children ever born, number of married couples in a household, death rates, and percentage of individuals over the age of 65.

Table A.13: Literacy regressions robustness: No post-Inquisition controls

	Illiteracy Rate			
	(1)	(2)	(3)	(4)
Inquisition	0.0371**	0.0385**	0.0369**	0.0276***
	(0.0155)	(0.0154)	(0.0142)	(0.0104)
Had a "busy" county in 1820	Yes	No	No	No
1820 Population	Yes	Yes	No	No
Treaty Port	Yes	Yes	Yes	No
Region FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Observations	14642	14642	14751	14751
Adjusted R^2	0.176	0.176	0.176	0.176

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table shows that our literacy results are robust when we do not include controls that might be related to intermediate effects of an inquisition. Column 1 reports our baseline estimates. Column 2 does not control for our measure of whether a prefecture was identified as Chong, Fan or Nan. Column 3 also does not control for 1820 population. Finally, in Column 4 we no longer control for whether a prefecture contained a Treaty Port. The other controls are the same as before and include whether a prefecture is on the coast, agricultural suitability, log Ming jinshi, ruggedness, had a historical courier route, individuals who are married, separated or divorced, widowed or widowers, gender, number of married couples in a household, death rates, and percentage of individuals over the age of 65.

Table A.14: The effect of a literary inquisition on the proportion of the population in agriculture (%)

	Proportion of Population in Agriculture			
	(1)	(2)	(3)	(4)
	1982	1990	2000	2010
Inquisition	7.584**	10.90*	6.896	5.164
	(3.070)	(5.928)	(5.399)	(4.727)
Controls	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Observations	87	68	75	72
Adjusted R^2	0.376	0.408	0.427	0.502

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The long-run impact of a literary inquisition on the proportion of the population that was agricultural in 1982, 1990, 2000, and 2010. In this sample we include all prefectures for which we can link 1982 prefecture-level data to more recent data based on their administrative code. Other controls include agricultural suitability, whether a prefecture is on the coast, had a courier route,

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