ESSAYS ON CULTURE, HUMAN CAPITAL AND DEVELOPMENT IN CHINA

by

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Essays on Culture, Human Capital and Development in China

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Abstract

ESSAYS ON CULTURE, HUMAN CAPITAL AND DEVELOPMENT IN CHINA

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George Mason University, 2015

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My dissertation focuses on the history and culture of modern and imperial China. It comprises three distinct chapters each focusing on a different subject. Chapter 1 examines how textile production shaped and continues to shape attitudes towards women as reflected in the sex ratio and in attitudes towards female labor market participation in today's China. Chapter 2 studies the Imperial Examination System and the effect of the Literary Inquisition during the Qing period. Chapter 3 studies the phenomenon of 'Dragon Babies' documenting the factors that have shaped this modern manifestation of tradition cultural beliefs.

In Chapter 1, I test the hypothesis that emergence of historical textile production led to an increase in gender equality. I exploit exogenous variation in historical textile production

at the county level to casually identify the effect of textiles on gender equality past and present. I find that historical textile production is positively correlated with female labor participation, and negatively correlated with sex ratio imbalances and sex-specific parental investment. My results are robust to various robustness checks, propensity score matching, instrumental variable analyses, and micro-level analyses. I identify cultural transmission as a possible channel of the persistence effect of historical textile production on today's gender equality and gender norms.

In Chapter 2, I use a semiparametric matching-based difference-in-differences estimator to 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 I 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 I find that literary inquisitions reduced literacy by between 2.25 and 4 percentage points at a prefectural level in the early 20th century. Prefectures affected by the literary inquisition had a higher proportion of workers in agriculture until the 1990s.

In Chapter 3, I study why China suddenly exhibited a large surge in births–a 50% increase in 2000 relative to 1999–in the 2000 Year of the Dragon by disaggregating birth rates at the city level. I define the dragon effect as a relative jump in birth rates compared to the trend. Prior to 2000, Asian nations with large numbers of ethnic Chinese — but not China — exhibited strong dragon effects. I exploit the uneven economic growth of regions in China to understand the emergence of the dragon effect. I find the dragon effect was most pronounced in rapidly developing cities with higher incomes, higher average education, and greater employment prospects. A micro-level analysis also suggests the dragon effect is positively correlated with educational attainment and income.

Chapter 1: Textiles and the Historical Emergence of Gender Equality in China

1.1 Introduction

This study examines an important deeply held belief across many societies that sons are superior to daughters, men superior to women. China is a country with large variation in the desirability of daughters. Through sex-selective abortions, Chinese people often choose the gender for their newborns. Due to varying son preferences, some parts of China have close to normal sex ratio at birth, whereas in other parts, sex ratio imbalances have long reached a alarming level. According to the fifth Chinese Population Census conducted in 2000, in Jincheng, 102 boys were born to every 100 girls; whereas in Erzhou, 170 boys were born per 100 girls.

My interest is in investigating the historical origins of these differences in son preference. Similar to other East Asian countries, China has a long history of Confucianism. In a traditional Confucian society, women are assigned different roles and take on different responsibilities in family and society. Daughters are generally valued less than sons because they do not carry on family names and they do not provide for parents once they grow up. However, despite the general preference for sons, the degree of son preference varies widely across regions. In some parts of China, gender equality still emerged in spite of a hostile ideological environment. To explain this phenomenon, I examine cotton textile production in Imperial China and its role in shaping gender norms and gender equality.

This study is related to Alesina et al. (2013), who explore how traditional agricultural practices influenced the historical gender division of labor and the modern levels of gender

equality across countries.[1] Alesina et al. (2013) test Boserup's hypothesis that differences in gender roles have their origins in traditional agriculture practices in the pre-industrial period. They find that plough cultivation in pre-industrial periods led to an important belief that the natural place for women is within the home. However, as the authors note, the plough was also a highly productive technology. It is possible that this productive technology affected the construction of gender identities.

My paper adds to our understanding how gender norms are shaped because it can distinguish between these two channels through which pre-modern production could have shaped gender norms and beliefs. Imperial China offers an ideal testing group because the plough was widely used and because cotton textile production took place at home and was performed mostly by women.

Textile production was revolutionized following major technological innovations in the 13th century which greatly increased women's productivity. Textile production required adroitness and patience, but not as much upper body strength as animal husbandry. By increasing women's productivity relative to men's, the mechanization of textile production raised the relative benefit of having daughters. The technology shock in textile production I exploit was plausibly exogenous as it borrowed from outside of the country. In the Yuan Dynasty (1271–1368), Huang Dao Po, a Shanghai native (1245–1330), learned new technologies in both spinning and weaving from Hannan Island. Specifically, she developed a pedal spinning wheel with three spindles, a piece of equipment similar to the Spinning Jenny. This new technology was then promoted to the rest of China, but was only able to set roots where geo-climatic conditions were favorable to textile production. Women in textile-suitable regions reaped the greatest benefit from this technological innovation.

Bray (1997) demonstrates how after 1300, the textile sector increasingly commercialized and specialized. Based on Bray's work, I hypothesize that women's use of a productive technology increases their social status and the desirability of daughters. Women's increasing relative contribution to household income in late imperial China generated

[1] For surveys of field see Guiso et al. (2006); Bisin et al. (2011a) and Nunn (2012).

new norms about women's role in the household as a main breadwinner. In textile regions people developed the belief that women can protected families from destitution and allowed them to pay their taxes just as effectively as men. The newly generated gender norms and beliefs from the period of 1300 and 1800 tend to persist even though the economy has moved out of traditional cotton textile production, playing a crucial role in gender equality, women's well-being in the present.

Other scholars have posited that the shift from flax to wool in the ancient Middle East led to a decline in the status of women as linen was cultivated on a small scale by women and children while wool production relied on male management of sheep herds(McCorriston, 1997).

To identify the causal impact of historical textile production on modern outcomes, I employ a variety of empirical strategies. Data on historical textile production obtained from local gazetteers, enables me to identify whether a place had textile production in the past. I link this data to contemporary measures of son preference and gender inequality. My outcome variables include sex ratio at birth, education gap between sons and daughters within a family, attitudes towards remarried women owning property, and female labor force participation. My analysis examines variation across counties and individuals. I find a strong negative relationship between historical textile production and son preference today. The baseline estimates suggest that the presence of cotton textile industry is associated with a reduction of 15% in the deviation of the sex ratio at birth from the upper bound of normal sex ratios at birth in the population.[2] Historical textile production is also positively correlated with female labor participation and positive attitudes towards remarried women owning property, and negatively correlated with sex ratios, as well as the education gap between sons and daughters within a family.

I use a sample of 1243 counties in my main analysis. I include controls for a number of historical characteristics of each county, such as the suitability of its environment for agriculture, share of rice paddies, its distance to the Grand Canal or Yangtze River, number

[2]According to the CIA Fact Book, sex ratios at birth normally range from 103 to 107. Compared with sex ratio, the deviation from the normal sex ratio is closer to having a normal distribution.

of courier routes passing the county, its level of economic activity before the adoption of textile technologies, and whether it became a treaty port in the 19th century, as well as set of geographic controls, such as latitude, longitude, change in elevation and distance to coast. I also control for current per capita GDP, scale of current textile production, share of agricultural workforce, share of non-agricultural household registration, share of ethnic population, total fertility rate, and whether a county is located in a provincial capital. Region, province or prefecture fixed effects are included in all specifications.

My results are robust to an instrumental variable approach. Farnie (1979) points out that humidity played a key role in textile production. Humidity makes cotton fibers more pliable and reduces the chance of breakages in the yarn. This motivates the use of county-level relative humidity as an instrument for historical textile production. To account for the correlations between relative humidity and other geographic characteristics, I include as controls overall agricultural suitability, share of rice paddies, distance to coast, longitude and latitude. My IV estimates are comparable to OLS estimates, and are robust to inclusion of other geographic covariates.

Previous studies have showed women's earnings and empowerment increases female autonomy (Anderson and Eswaran, 2009; Ashraf et al., 2010; Deininger et al., 2010). Anderson and Eswaran (2009) examines the determinants of female autonomy. They find earned income contributes more to women's autonomy relative to unearned income, and that only employment outside their husbands' farms contributes to women's autonomy. The relationship between sex-specific income and the desirability of daughters in East Asia has been documented by Qian (2008). Qian finds a short-term relationship between prenatal sex selection and women's value realized through tea production. My paper shows this relationship continues to hold in the long run. Women enjoyed higher social status in areas with historical textile production; and they continue to do so today even when proto-textile production is long out of the picture. One explanation is that historical textile production has shaped cultural beliefs about the desirability of daughters and cultural attitudes towards women.

However, there are also alternative arguments that suggest that part of the long-term impact arises because historical textile production promoted the development of formal institutions, gender-specific economic opportunities and overall wealth that may favor the decision to raise daughters. To rule out the first channel, I rely on within-country, within-region, within-province and even within-prefecture variation, where formal institutions are identical and policies are enforced to a similar extent.[3] Due to pervasive communist reforms in favor of gender equality, official sex discrimination or labor laws hardly vary at a local level, aiding my identification of the role of historical determinants in today's gender norms. To account for the second channel, I control for sectoral composition today, including scale of textile production and agricultural workforce. To best account for the third channel, I control for both current per capita GDP and past levels of economic activity. I show that overall wealth alone hardly explains the large and systematic differences in sex ratios, attitudes, sex-specific education investment, and female labor force participation documented in this paper.

More broadly, this paper contributes to the literature on historical determinants of cultural norms and beliefs. Many of these document the persistent impact of a negative shock on current cultural values such as Nunn and Wantchekon (2011)'s work on the effects of the trans-Atlantic slave trade on corruption and trust today and Voigtländer and Voth (2012)'s study of the persistence of antisemitic beliefs in Germany. My study is most closely related to those papers that study how economic factors have shaped contemporary gender norms such Grosjean and Khattar (2014) who examine conservative gender norms and its origins in historical marriage market conditions in Australia.[4]

This paper also contributes to the literature on parental gender bias and sex ratio imbalances, by identifying an important source of differentials in sex ratios. Edlund (1999)

[3]Despite the highly centralized law making process, policies can be implemented by local governments with greater latitude.

[4]Other relevant studies include Jha (2013) who shows that a cities in India that were medieval trading ports experienced significantly less religious riots between Muslims and Hindus in the period after 1850. Grosjean (2011) examines the persistence of a culture of honor among Americans of Scots-Irish descent. She finds that this culture of honor results in higher homicide rates among Scots-Irish in the US South and Mountain West but not elsewhere and argues that this culture has only persisted where formal institutions are comparatively weak.

explicitly models sex ratios in relation to son preference, indicating several factors that contribute to unbalanced sex ratios. Daul and Moretti (2008) finds evidence for parental gender bias in the U.S. that parents favor boys over girls. Others have studied son preference, sex-selective abortions, and changes in sex ratios in non-western countries (Gupta, 2014; Li and Lavely, 2003). In particular, Chung and Gupta (2007) suggests income levels play a key role in unbalanced sex ratios and that sex ratios can change in nonlinearity through different stages of development.[5]

The third literature this paper builds on is the economic history literature studying the impact of textile production on the pre-modern Chinese economy in the context of the Great Divergence (Huang, 1990; Goldstone, 1996; Li and Li, 1998; Ma, 2005; Pomeranz, 2009; Wong, 2002). Several scholars have argued that the 17th and 18th centuries were a comparatively golden period for the Yangtze Delta, one of the major textile regions. Pomeranz and Li, in particular, have argued that China's textile industry remained highly productive and profitable through to the 19th century (Li and Li, 1998; Pomeranz, 2009). These claims motivate my focus on the textile industry.

This paper is organized as follows. The second section lays out the conceptual framework. Section 1.3 lists data sources and discuss historical context for the paper. Section 1.4 summarizes my baseline results and subjects them to a series of robustness tests. Section 1.5 demonstrates that similar effects can be found using an alternative micro-database. My instrumental variable analysis is contained in Section 1.6. I explore how the effects of textile production persisted after the demise of the traditional textile industry in the late nineteenth century in Section 1.7. In Section 1.8 I discuss some possible channels that could be responsible for the persistent effect of textile production on son preference in China. Section 1.9 concludes the paper.

[5] Almond et al. (2013) find positive incomes shocks from land reforms increased sex ratios. Finally, the economic consequences of sex ratio imbalances have also attracted scholarly attention in recent years. Wei and Zhang (2011) links sex ratio imbalances to differential saving rates across China.

1.2 Conceptual Framework

The emergence of the textile industry in Ming China constituted a significant shock to the level of women's participation in market work. I argue that historical textile production has a strong, persistent impact on gender roles, gender norms and gender equality today. In particular, I hypothesize a relationship between historical textile production and son preference in contemporary China.

In this section I discuss the channels that can potentially account for this persistence. First, I discuss a variety of approaches economists have employed to explain the determination and persistence of cultural values. Second, I note that sex ratio imbalances are an important indicator for differential values being assigned to each sex and thus a good proxy for contemporary gender attitudes. Finally, I show that the emergence of the textile industry did constitute a large enough shock to women's economic opportunities that could give rise to new gender norms which persist to this day.

A number of recent papers have studied how attitudes to women have changed in recent decades (Doepke and Tertilt, 2009; Doepke et al., 2012; Jensen and Oster, 2009). In this paper I emphasize how a set of gender norms that emerged historically continue to shape the sex ratio in modern China.

Economists have recently devoted considerable attention to the determination and persistence of cultural beliefs (for a survey, see Bisin et al., 2011a). However, there is no single dominant approach to modeling cultural values. One approach is to treat cultural values as preferences (Weber, 1930) or as a form of consumption capital (Becker and Murphy, 2000).[6] Cultural values have also been modeled as deriving from the value individuals attach to a particular identity (Akerlof and Kranton, 2000) or as shaping social norms (Greif et al., 1994; Greif, 2006).

Viewing general norms as a complex of nexus of different beliefs and attitudes concerning

[6] For an example of the culture as preferences view see Ichimrua (2000, 23) who argues that '[c]ulture is a major determinant of [an individual's] utility function . . . Culture determines the tastes, preferences, beliefs shared by a majority of people in a country.'

the relative status of woman, it is sufficient to note that gender norms are both perpetuated from generation to generation (as shown theoretically by Bisin and Verdier (2001) and as discussed empirically by numerous studies in sociology and economics (Moen et al., 1997; Vella and Farré, 2007) and also shaped by the attitudes of others in society (Burda et al., 2007). Both mechanisms generate cultural persistence and can explain why cultural values, once established, can be difficult to dislodge.

Inherited gender attitudes shape a wide range of outcomes today. The most important one I focus on is the sex ratio. When parents today make a decision as to whether to have a boy or a girl, they do not have complete information on the future prospects of a boy or a girl in contemporary world. They instead resort to general beliefs about whether boys or girls are morel likely to thrive in society and to favor the family.[7] These cultural beliefs are particularly important under a one-child policy regime and can be exercised at low costs given the availability of sex-selection technology.

In pre-modern China, it was folk wisdom that a family would suffer economically from the birth of a daughter.[8] This cultural belief was consistent with economic reality prior to the emergence of the textile industry: daughters could not work outside home due to "chastity" concerns, and had to rely on family resources to survive.[9] And unlike sons, a daughter would not provide for her own parents once she was married. Coupled with the cost of dowries, the birth of additional daughters could mean poverty for an entire household (Harrell, 1995; Watson and Ebrey, 1991). Excess female mortality during infancy and childhood was widely observed.[10] After the rise of the textile industry in the fourteenth century, however, women in textile regions became able to earn enough to support a household independently for the first time. This shock led to the breakdown of

[7]Altruistic parents who care about whether or not their children have fruitful lives will prefer to have boys if they live in a society where women are treated less well.

[8]In an early Yuan era play *Qujiang Chi*, the heroine refers to herself as *pei qian huo*, which literally means a money-losing proposition. The term is still used in Mainland China, Singapore, Malaysia, Taiwan, Macau and Hong Kong today. In 2007, the Yahoo dictionary in Taiwan was caught giving the English-language translation of the Chinese term *pei qian huo* as (1) "a money-losing proposition" and (2) "a girl; a daughter" (http://news.tvbs.com.tw/entry/305992).

[9]Chow (1991) regards non- western women's "purity" or "chastity" as both sexual and nationalistic.

[10]Historian James Z. Lee and sociologist Cameron D. Campbell (2007) discovered that girls between ages one and five had a 20 percent higher mortality than boys.

prior cultural beliefs concerning women's productivity.

Historians have argued that as new textile production technologies were introduced to mainland China in the 14th century, cotton textile production was made economically viable for the first time in history (Bray, 1997; Kang, 1977). In the areas where textiles were produced, women were able to produce a surplus of cotton textiles for market exchange—the fact that this work was for market is significant as it meant that the women (and not male household heads) were residual claimants.

Importantly, textiles were sold on the market. This meant that textile production shared many similarities with work in the proto-industry in other advanced premodern economies such as 18h century England.[11] Textile production represented an new opportunity for women to earn monetary income and thereby contribute to household income.[12] As the payoff to producing textiles was sufficiently high, women were induced into entering the labor market and switched away from performing non-market work or producing other fabrics at low quantities mainly for home use. Although Chinese women had been doing productive work prior to textile production, textile production provided women with a new role as major income earners. By the late Ming period, both unmarried and married women became prominent working directly for the market, and in many cases their production became the main source of family incomes. In the High Qing period, diligent productive manual labor was seen as the virtue for all women, regardless of class (Mann, 1997). To summarize this change, using Pomeranz's terms, women became more respectable; to describe the change from the perspective of parents, it became less worrisome to have a daughter born to the family. Women had a potential role as productive members of the economy in their own right.[13]

[11]China had a well developed market economy in the late Ming period. Shiue and Keller (2007) shows the performance of markets in China and Western Europe overall was comparable in the late 18th century

[12]Pomeranz in his research on economics of respectability describes the role of daughters in a family: a family's capacity to survive and to profit from its work relied upon "an optimal mix of family members of particular ages and sexes" (Pomeranz, 2005).

[13]For example, due to a strong emphasis on a women's fidelity to her husband, even after he passed away, women would rarely remarry, which often meant deep poverty or suicide for the widowed. Ability to produce textiles helped women to support themselves in more situations.

My conceptual framework therefore indicates that a sufficiently large productivity shock to women's income could cause the breakdown of traditional gender norms in some parts of Ming China. Further, it suggest that his breakdown in traditional gender norms could lead to the emergence of more favorable attitudes towards woman and a more optimistic assessment of the prospective fates of potential daughters.

My argument relies on the effects of textiles not being confounded by other factors such as labor mobility. I discuss below, this assumption holds as labor mobility was limited for most parts of the Ming and Qing Dynasty, due to the strong influence of clans (as discussed in Greif and Tabellini, Greif and Tabellini). This constrained women's ability to relocate to textile-suitable areas for jobs. Consequently, cotton textiles continued to generate enough incomes for women to be a major contributor to the household income through to the end of the 19th century, when manufactured textile products from the west began to dominate the Chinese market. During that period, cultural beliefs about women's role in a family and the prospects of having a daughter continued to evolve in favor of women. Because of the persistent nature of cultural beliefs, I hypothesize that these beliefs may have persisted even after the Chinese economy moved out of proto-industries, and could still affect women's participation in market work and sex ratios today.

1.3 Data and Historical Context

I use data from several periods regarding historical textile production. I also construct contemporary measures of son preference and gender equality, and historical and contemporary county characteristics. For modern outcome variables, I use the county-level Fifth National Population Census (2000) from China Geo Explorer, Chinese City Statistical Yearbooks and individual-level census data (1990) from IPUMS-International. A total of 1243 counties, 160 prefectures, 14 provinces, and 8 regions are used in the main analysis.

1.3.1 Textile production from 1300 to 1800

Following the technological breakthrough in cotton textile production around 1300, the industry went through a period of rapid expansion. Cotton quickly gained popularity for many of its attractive properties, compared with silk and linen. Due to strict geo-climatic conditions required for textile production, only relatively humid areas could produce textiles for most periods of the Ming Dynasty. Dry climate impeded weaving more than spinning. Places that were semi-humid could end up having only spinning but not weaving. As weaving was much more value-added activity than spinning, places specializing in spinning did not see as big an effect of textiles on women's ability to generate incomes.

To account for the location of textile production within China I use climate data. The Climate Research Unit of University of East Anglia, UK provides 30-year monthly average relatively humidity data across 10 arc-minute by 10 arc-minute grid cells globally. I extract relative humidity values on the basis of x, y coordinates. I construct a relative humidity variable at the county level by averaging over all relative humidity values within a polygon that represents a county.

Due to lower transportation costs, a higher percentage of counties located near the Yangtze River and Grand Canal produced textiles historically. Huang (1964) paints a vivid picture of the importance of the Grand Canal in Ming Dynasty China, confirming that many counties famous for textile production were located in the great canal area, and the size of trade was considerable.[14] This could pose a challenge to my identification strategy, which I will discuss in the next section.

From Harvard University's digital world map collection, I obtain shape files that contain historical characteristics for the counties within China. I calculate the distance to the Grand Canal or Yangtze River at the county level by matching the shape file for the Grand Canal, the shape file for the Yangtze River and shape file for contemporary China .

To best control for economic activity prior to 1300, I match the shape file for commercial

[14] Huang estimates the size of trade in cotton cloth to be one million teals around 1600.

tax quota in 1077 and the shape file for contemporary China to obtain an estimate for historical level of economic activity.

Prior to 1800, only about 10% of all counties produced textiles. Despite later improvement in humidification, textile producers from less humid areas struggled to compete with their counterparts from more humid areas, particularly in the high-ended market, because top quality cotton cloths had to be weaved in a more humid environment than those less humid areas could ever provide. Thus the prices of textiles stayed at a level that generated enough income for a skilled textile worker to support a family of four.[15]

Textile production was predominantly performed by women. Weaving was defined as a womanly skill and women were far more productive in producing textiles than they were in agriculture. Such division of labor was first established under the state tax system dating back to 300 AD–each household was required to pay in-kind taxes in both grain and textile products. The earnings of women from cotton textile production were significant. Allen's (2011) wage regressions indicate that textile workers earned a wage premium compared with workers in construction or agriculture. Women who were able to weave artisan cloth, the more skilled workers, managed to earn an even higher income.[16]

Using data from local gazetteers between 1368 and 1800, I construct an indicator variable on historical textile production at a county level. Local gazetteers were published by prefecture governments and county governments, containing information on local produces and manufactured products. A total of 1243 counties are included in the county-level analysis, within which a smaller set of counties are known to produce textiles. A prefecture-level indicator is constructed separately for analyses conducted with IPUMS-International census micro-data.

It is possible a county that started textile production first would see a larger impact of textile productions in shaping values and beliefs. As timings of starting textile production

[15]Allen (2009a) shows one day's work by a weaver in the late 17th century produced 7,684 calories, which was adequate to support a family.

[16]The production of artisan cloth was backed up popular demand of weddings and funerals in pre-modern China.

are relatively clustered, this particular source of heterogeneity is limited. Unfortunately, due to data limitations, I cannot test the differential impact of quantity of total production by each county, and quality. As quantity produced and quality can be potential sources of heterogeneity in the treatment effect, the estimates should be interpreted as average effect of having historical textile production.

To obtain an estimate of the distribution of then textile-producing counties across China today. I match county names with county names in a point shape file that codes all counties in 1305, a polygon shape file that codes all prefectures in 1305, a point shape file that codes all counties in 1820 and a polygon shape file that codes all prefectures in 1820. Then I spatially join all two county shape files with the county shape file corresponding with the 2000 population census to obtain a county-level estimate of cotton textile production.[17]

1.3.2 Sex ratio imbalances in contemporary China

Communist China has been promoting gender equality through laws, policies and institutions for over half a century (Johnson, 2009). However, even during the period that the most strict measures were imposed to eliminate gender-based differences, women's reported earning were less than men. As the economy opened up in 1979, and the state relaxed its control on the economy, hidden gender inequality has surfaced (Li and Lavely, 2003).

Sex ratio imbalances reflect gender inequality from another angle. China has had the most unbalanced sex ratios in East Asia for the past decade. In the 2000 Census, the national average sex ratio for Age 0 is 118:100, i.e. every 118 boys were born to every 100 girls. Ethnic Han Chinese have a particularly strong son preference.

Prior to the one-child policy, most families went for higher-parity births, if they were unable to have male births in first few attempts. A major problem for identifying the

[17]Central districts within the urban core of a particular prefecture are treated as one polygon during the matching process, as they have the same administrative level as counties, but are of much smaller geographic sizes.

magnitude of son preference in this setting is the characteristics associated with low fertility are often correlated with characteristics associated with gender equality. Though stopping rules can distort sex ratios, the distortions become smaller as number of children increases. As a result, places with high fertility and high levels of gender inequality does not necessarily have a more distorted sex ratio.

When levels of fertility are imposed rather than chosen, the relationship between son preference and sex ratio not only becomes more pronounced, but also more comparable across China. In the 1980s, the state initiated its well-known one-child policy. Since then, families have mostly lost their ability to pursue sons by going for higher-parity births. As sex-selective technology improved, families started to rely on ultrasound and other technology to aim for a son in their first or second attempt, depending on their household registration status. The strategy of sex selection at a lower-parity birth causes sex ratios within a family to be artificially chosen, contributing to sex ratio imbalances on a much larger scale at the aggregate level (Ebenstein, 2010).

There is considerable variation in the extent of sex ratios. At the county level, sex ratios range from 81:100 to 196:100. With the exclusion of five autonomous regions, where ethnic minorities account for a much higher proportion of the total population, I still find a wide range of sex ratios (94:100 to 196:100) across counties.

For the outcome variables, I include deviation of sex ratios at birth from the upper bound of normal range of sex ratios to proxy son preference. Data on sex ratios at birth are available at the county level. The distribution of the raw sex ratio is left-skewed. To obtain a normally distributed outcome variable I focus on deviations from a normal sex ratio. In main my specification I examine deviations from a sex ratio of 107. I have also experimented with a range of cut-off values between 94 and 107. My results remain unchanged.

I also use other measures of gender equality, such as attitudes towards remarried women owning property, female labor participation and education gap between daughters and sons within a family. Those measures are taken at the household level. Micro data allows

me to produce more precise county or prefecture estimates by taking out individual and household-level variations.

1.3.3 Descriptive Statistics

I construct my data set as follows. I limit the sample to areas historically populated by Han Chinese people, since they have pronounced son preference. I exclude five autonomous regions, as well as autonomous counties in other provinces, that historically comprise ethnic minorities. Next, I collect information on all the counties that are in provinces having local gazetteers prior to 1800. A few historic prefectures are excluded that are known to have had textile production but lacking in data sources on county-level production.

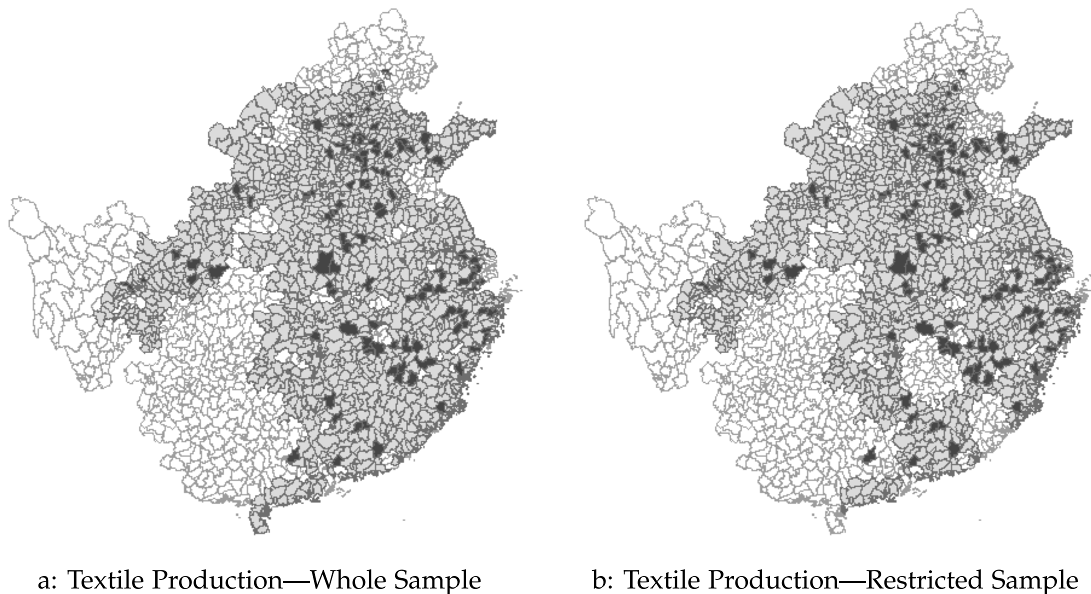
Table 1.1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Textile production by 1800	0.095	0.293	0	1	1243
Sex ratio at birth	118.53	13.318	94.524	176.888	1243
Deviation in sex ratio at birth	12.001	12.799	0	69.888	1243
Log per capita GDP	13.23	1.092	3.472	16.251	1243
Total fertility rate	1.241	0.296	0.59	2.51	1243
Number of textile plants	81.432	237.344	0	2617	1243
Share of urban residents	22.866	19.972	3.02	92.81	1243
% Agriculture workforce	63.254	26.005	0.09	96.59	1243
Provincial capital	0.117	0.321	0	1	1243
% Ethnic population	1.443	6.413	0.01	94.05	1243
Agricultural suitability	-4.108	1.894	-8	-1	1243
Treaty port	0.131	0.338	0	1	1243
Number of courier routes	0.474	0.872	0	6	1243
Dist. to Great Canal or Yangtze River	1.925	1.727	0	8.423	1243
Commercial tax quota in 1077	274.91	217343.172	1243		
Share of rice paddies	34.126	31.226	0.1	90.900	1243
Change in elevation (slope)	3.318	3.428	0.081	20.823	1243
Dist. to coast	415.235	324.378	1.423	1175.69	1243
Latitude	32.123	4.186	21.274	39.96	1243
Longitude	114.203	4.375	102.284	122.391	1243
Relative humidity index	-30.788	16.276	-64.004	-12	1243

Table 1.1 gives an overview of the key variables in the main sample, where a total of 1243 counties are included. Only a small percentage of the counties were able to produce textiles. Average distance to the Great Canal or Yangtze River is 191.3km. Agricultural

suitability ranges from -8 (worst) to -1 (best). An average county has a suitability level of -4.1. In 2000, average sex ratio at age 0 in 2000 was 118.5 boys per 100 girls, with a standard deviation of 13. Roughly 23% of the population had non-agricultural household registration. I refer to these households as “urban residents" for convenience.

Figure 1.0a shows textile production at the county level including historic prefectures with no local production details. Figure 1.0b shows the main sample used in the following analysis. Counties shaded in black had textile production historically, counties shaded in grey had no records for historical textile production, and counties shaded in white represent missing values.



a: Textile Production—Whole Sample b: Textile Production—Restricted Sample

Figure 1.1: The Distribution of Textile Production by 1800

1.4 County-level OLS estimates

Having constructed county-level measures of historical textile production, I can examine the relationship between historical textile use and son preference or gender equality in modern day China. I begin by examining variation at the county level. My main outcome of interest at this stage of analysis is sex ratio imbalances, which is intended to reflect son

preference. My measure is each county's sex ratio in 2000:

$$\text{Dev. sex ratio} = \begin{cases} \log(\text{sex ratio at birth} - 107 + 1)_c & \text{if sex ratio at birth} >= 107, \\ 0, & \text{if sex ratio at birth} < 107 \end{cases} \quad (1.1)$$

I first examine the unconditional relationship between sex ratio and historical textile production. I find the bivariate relationship is both consistent with the hypothesis and statistically significant. Moving from a country that did not produce textiles to one that did produce textiles can reduce the deviation in the sex ratio at birth by 16%. The relationship continues to hold when I add controls and region fixed effects. Specifically, I test my hypothesis by estimating the following equation:

$$\text{Dev. sex ratio} = \alpha + \beta \text{Textile}_c + \mathbf{X}_c^H \mathbf{\Omega} + \mathbf{X}_c^G \mathbf{\Lambda} + \mathbf{X}_c^C \mathbf{\Pi} + \epsilon_p \ , \quad (1.2)$$

where c denotes a county. Textile_c is my measure of historical textile production at a county level. $\mathbf{X}_c^H$ is a vector of historical controls, and $\mathbf{X}_c^G$ and $\mathbf{X}_c^C$ are vectors of geographical and contemporary controls respectively, each measured at the county level.

$\mathbf{X}_c^G$ and $\mathbf{X}_c^H$ are intended to capture geographic and historical characteristics that may have been correlated with historical textile production and may still affect present-day outcomes. In particular, how textile production was located was likely influenced by access to market. I control for distance to the Grand Canal or Yangtze River, number of courier routes passing the county in all specifications and pre-adoption levels of commercialization measured by commercial tax quota in 1077. To account for geographic differences across counties that may be correlated with access to market, I include in $\mathbf{X}_c^G$ distance to coast[18] and change in elevation (slope). I also control for the county historical economic specialization, by including in $\mathbf{X}_c^H$ share of rice paddies. As overall levels of economic development in the past might have affected both the adoption of textile production and attitudes towards

[18]Distance to the nearest coast data are taken from NASA (http://oceancolor.gsfc.nasa.gov/DOCS/DistFromCoast

women, and China was an agrarian country between 1300 and 1800, I include in $\mathbf{X}_c^H$ agricultural suitability as a proxy for the county earning potential, and include in $\mathbf{X}_c^G$ latitude and longitude.

The contemporary control variables $\mathbf{X}_c^C$ include the natural log of a county's per capita GDP measured in 2000 and its squared term, number of textile plants, share of agriculture workforce, share of non-agricultural household registration, share of ethnic population, and whether a county is located in a provincial capital. I use share of non-agricultural household registration to capture the variation in the details of one-child policy.[19] Whether a county is located in a provincial capital is intended to capture different levels of enforcement in one-child policy, with the assumption that provincial capital has the strictest control on its residents. Both could have had an effect on sex ratios through one-child policy (Ebenstein, 2010). In addition, as son preference is the most predominant among Han Chinese, I control for share of ethnic population to reduce composition bias.

OLS estimates of equation (1) including above controls are reported in Table 1.2. Column 1-3 reports estimates with prefecture, province or region fixed effects respectively.[20] Column 4-5 reports specifications with two potentially endogenous variables excluded. Column 4 shows estimates when per capita GDP is excluded from the specification, and Column 5 shows estimates when total fertility rate is included.

The estimates show that in counties with the presence of historical textile production, fewer girls are missing today. The coefficient estimates are both statistically significant and economically meaningful. Based on the estimates from Column 1, one unit increase in historical textile production is associated with a decrease of 16% (exp (0.15)) in deviation in sex ratios. The size of the effect can range from reducing 0 to 10.5 boys ($70 * 16\%$) per 100 girls, depending on the size of the "boy surplus". At the mean sex ratio (119:100), historical textile production can reduce the "boy surplus" by 2 boys($12 * 15\%$) per 100 girls. The

[19]Though the one-child policy is strictly enforced among Chinese citizens on non-agricultural registration status, a more relaxed version of the one-child policy is enforced among those on agricultural household registration status.

[20]Region dummies are taken from the Skinner Regional Systems Analysis Dataverse - Skinner socioeconomic macroregions (http://worldmap.harvard.edu/maps/skinner).

coefficients are slighter smaller in Column 2 and somewhat larger in Column 3, ranging from reducing deviation in sex ratio by 16% to 21 %. The coefficient estimates for per capita GDP and its squared term are sizable, but not significant. Column 4 suggests the strength of the coefficient for textile production does not depend on whether I control for per capita GDP.[21] Across Columns 1 to 4, total fertility rate is positively associated with deviation in sex ratios. This is not a surprising result, given that higher parity births remain as a strategy for parents to have male offspring under many circumstances. Column 5 shows the coefficient of historical textile production does not change as a result of including total fertility rate.

1.4.1 Robustness checks

Subsamples

I first check the robustness of my results to the use of alternative samples. Motivated by the fact that the Yangtze Delta is of special importance to Chinese economy both historically and contemporarily, I test to see if my results are robust to the omission of three provinces (Jiangsu, Zhejiang & Shanghai) from the sample. Table A- 1 in the appendix summarizes the results. The coefficient estimates are relatively stable (-0.142 to -0.189) across the columns.

The second set of robustness checks look at counties with different rates of migration. Historically, labor mobility was low due to the control of the clan system. In modern China, the speed of migration has picked up. Gender norms in the less developed regions of China could have been strengthened if individuals with more progressive gender norms are more likely to move to more developed areas for a better life. Hence my results could be biased if textile locations are correlated with unobservable characteristics of counties that attract many modern migrants. I omit counties with high rates of in-flow

[21] Though large sex ratio imbalances are a relatively new phenomenon in China, per capita GDP could still have already been affected that the sex ration, i.e. it is possible that per capita GDP is partly endogenous to sex ratio imbalances (Wei and Zhang, 2011).

migration, and focus solely on counties that are mostly dominated by locals. Table A- 2 in the appendix shows counties with mostly local inhabitants display a slightly stronger effect of textile production. The effect of historical textile production does not go away if I omit high-migration counties. This shows the textile effect is unlikely to be driven by urban-rural differences, or by different characteristics between migrants and locals.

The third set of robustness checks try to address potentially uneven effects of the industrialization process across counties with or without textile production. China began to industrialize from the 19th century onwards, first in treaty ports. Jia (2014a) shows that treaty ports had a long-lasting impact on local economies.

This is a potential source of bias if textile locations overlapped with areas that experienced early industrialization, as gender norms might be affected by such drastic economic and social change. Historical evidence suggests that this should not be a major concern as industrialization in China was gradual and highly isolated.[22] Hence to control for this I drop counties that used to be treaty ports, to see if the textile effect remains in the remaining sample. Table A- 3 in the appendix shows coefficient estimates of textile production are robust to omitting all or some of the treaty ports.

Notes: The table reports OLS estimates. The unit of observation is a county in 2000 Census. The dependent variable is log of deviation from normal sex ratios plus 1, when sex ratio at birth is greater than 107; 0 otherwise. 107 is the upper bound of normal sex ratio ranges (103 - 107) in the population. Column 1- 3 reports estimates with prefecture, province or region fixed effects respectively. "Contemporary controls" are log of current per capita GDP, log of number of textile plants plus 1, log of share of non-agricultural household registration, share of agriculture workforce, log of share of ethnic population, and whether a county is located in a provincial capital. "Historical controls" are agriculture suitability, log of share of rice paddies, log of distance to the Grand Canal or Yangtze River plus 1, number of courier routes passing the county, log of commercial tax quotas in 1077, and whether a county was located in a treaty port in 19th century. "Geographic controls" are latitude, longitude, log of slope plus 1 and log of distance to coast. Column 4 shows estimates

[22] During late Qing and Republican China era, many parts of rural and hinterland China continued to maintain autonomous economies, as well as traditional lifestyles. cite industrial sites in 1930s; rural China in early 20th century.

Table 1.2: Historical Textile Production and Sex Ratio Imbalances: OLS Results

	Dev. sex ratio)				
	(1)	(2)	(3)	(4)	(5)
Textile production by 1800	-0.157**	-0.146*	-0.201**	-0.156*	-0.150*
	(0.072)	(0.074)	(0.071)	(0.072)	(0.071)
Log per capita GDP	0.023	0.205	0.175		0.065
	(0.184)	(0.182)	(0.159)		(0.182)
Log per capita GDP^2	-0.0019	-0.012	-0.011		-0.0033
	(0.008)	(0.009)	(0.008)		(0.008)
Total Fertility Rate					0.518**
					(0.230)
Contemporary controls	Yes	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes	Yes
Fixed effects	Prefecture	Province	Region	Prefecture	Prefecture
Observations	1243	1243	1243	1246	1243
Adjusted R^2	0.507	0.277	0.297	0.507	0.511

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

when per capita GDP is excluded from the specification, and Column 5 shows estimates when total fertility rate is included. Robust standard errors are clustered at the province level.

Propensity Score Analysis

To further demonstrate the strength of my results, I also use propensity score matching estimation on pre-adoption covariates. Pre-adoption covariates includes pre-1300 historical characteristics agricultural suitability, share of rice paddies, distance to Grand Canal or Yangtze River, number of courier routes passing the county, commercial tax quota in 1077, as well as geographic characteristics, such as latitude, longitude, slope and distance to coast. This exercise yields an even larger coefficient estimate. With the nearest neighbor matching method, textile production, on average, reduces deviation in sex ratios by 33% (exp (0.29)) to 40% (exp(0.34)). Further details are available in the Appendix.

1.5 Micro-level analysis

1.5.1 Evidence from CGSS

I now begin a micro-level analysis that examines variation in attitudes towards women owning property across survey respondents in the CGSS 2005 (Chinese General Social Surveys). The CGSS includes questions on individual attitudes and preferences across a wide range of topics. The CGSS also includes information on age group, gender, marital status, education attainment, party member status, socioeconomic status, and whether a daughter-in-law has joined the family since 1998. The measure of attitudes towards women owning property is constructed from each respondent's view of the following question: "Do you think a woman should be allowed to keep her land when she marries a second time,?" The respondent can choose from *a. the village reclaims the land b. Woman's first husband keeps the land c. if the woman marries a man in you village, she can keep the land d. if a woman marries a man outside of your village, she cannot keep the land e. she can take the land with her to wherever she is going to be registered f. she can keep her land g. her husband can keep the land for now before next round of land distribution by the village committee h. don't know i. others*. I code answer e and f as "1", meaning positive attitude towards remarried women owning land. All other answers are coded as "0". h and i are recorded as missing values.

Other than attitudes and values, CGSS also reports a woman's work status. I construct an indicator variable that equals one if a woman has ever been in the workforce which is defined as full-time, part time, temporary employment, retired, unemployment, part-time farming, or full-time farming. The indicator variable takes the value of 0 if a woman reports "has never worked", and she is older than 30.

Examining the two outcomes attitudes about remarried women owning property and "a woman has never worked," I estimate the following individual level equation:

My estimation equation is

$$y_{i,c} = \alpha + \beta \text{Textile}_c + \mathbf{X}_c^H \mathbf{\Omega} + \mathbf{X}_c^G \mathbf{\Lambda} + \mathbf{X}_c^C \mathbf{\Pi} + \mathbf{X}_i^I \mathbf{\Gamma} + \epsilon_{i,c} \ , \tag{1.3}$$

where c denotes a county. Textile_c is my measure of historical textile production at the county level. $\mathbf{X}_c^H$ includes agriculture suitability, log of distance to the Grand Canal or Yangtze River plus 1, number of courier routes passing the county in all specifications and log of commercial tax quotas in 1077. $\mathbf{X}_c^G$ includes latitude, longitude, log of slope plus 1. $\mathbf{X}_c^C$ includes log of current per capita GDP, log of share of non-agricultural household registration, share of agriculture workforce, log of share of ethnic population, total fertility rate, and whether a county is located in a provincial capital. $\mathbf{X}_i^I$ denotes current individual-level controls: sex, age group, urban/rural, marital status, education attainment, father's education, mother's education, communist party member, socioeconomic status, and whether the respondent has a daughter-in-law. Standard errors are clustered at the county level for all specifications.

Table 1.3 summarizes the estimates of equation 1.3 with outcome variable being attitudes towards remarried women owning property, based on logit estimation. Column 1 reports estimates of equation 1.3 without including $\mathbf{X}_I^i$. Estimates with the full set of controls are reported in Column 3. Estimates with a nonlinear term of distance to the Great Canal or Yangtze River, in the addition to the full set of controls, are reported in Column 4. I find a positive relationship between historical textile production and attitudes towards remarried women owning property. All four relationships are qualitatively and quantitatively similar. The coefficient estimates range from 18.1 to 19.5. In average marginal effects terms, historical textile production increases the probability of supporting remarried women owning property by 37% in Column 4.

Table 1.3 summarizes the estimates of Equation 1.3 using the outcome variable "has never worked" based on logit estimation. Column 1 reports estimates of equation 1.3 without including $\mathbf{X}_I^i$. I include $\mathbf{X}_I^i$ in the specification for Column 2, with exception of education

attainment. Column 3 contains estimates with the full set of control. Column 4 controls for nonlinearities in the effect of distance to the Great Canal or Yangtze River. I find a negative relationship between historical textile production and the probability that a woman has never worked. The coefficient estimates are between -1.6 and -2.4. The size of the coefficient is greater as more individual characteristics are controlled for. In Column 4, historical textile production reduces the probability that a woman has never worked by 12% (marginal effects).

1.5.2 Evidence from IPUMS-International

I turn to a micro-level analysis that examines variation in parental investment across individuals and households. The 1982 and 1990 Population Census are available via IPUMS - International. These two censuses are considered to have the best quality data, due to limited migration occurring during that period. I use the 1990 population census to construct my outcomes of interest: education gap between the best educated daughter and the best educated son within a family. I select all individuals who were at least 22 by 1990, and were the children of the head to the household. I compare educational attainment of the most educated daughter and the most educated son within a household. With educational and financial resources being scarce in rural China during the sample period, family sources of educational gender inequality is known to exist (Brown and Park, 2002; Hannum, 2003; Hannum et al., 2009; Wang, 2005). In rural China, it is not uncommon for female children to work to support their brothers through school. I interpret differences in educational attainment between the best educated daughter versus son as driven by gender-specific parental investment. Compared with the CGSS, the 1990 Census has more geographic coverage and a much greater sample size.

Table A-2 in the appendix describes my micro analysis sample. 37% of prefectures engaged in textile production in the past. As high as 94.8% of the female in my sample are part of the workforce. Educational attainment takes the value of 0 for "illiterate or semi-illiterate", 1 for "primary education", 2 for "middle school education, 3 for "high school education",

Table 1.3: Historical Textile Production and Attitudes towards Remarried Women

	Attitudes towards remarried women owning property			
	(1)	(2)	(3)	(4)
Textile production until 1800	18.12**	19.23***	19.54***	18.91**
	(7.493)	(7.261)	(7.240)	(8.016)
Socioeconomic status - middle			0.737	0.527
			(0.807)	(0.751)
Socioeconomic status - low			0.617	0.423
			(0.864)	(0.769)
Daughter-in-law since 1998			1.066*	1.098*
			(0.592)	(0.648)
Log (dist. to Great Canal or Yangtze +1)	0.314	0.443	0.427	1.806
	(0.244)	(0.349)	(0.352)	(1.861)
Log (dist. to Great Canal or Yangtze+1) ^{2}				-0.274
				(0.367)
Individual controls	No	Yes	Yes	Yes
Contemporary controls	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes
Observations	1807	1693	1672	1672

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table presents results of logit estimation when the dependent variable is an individual's response to "In you village, if a woman gets married a second time, what's your view on her rights to land should be handled?". I code answers e. she can take the land with her to wherever she is going to be registered and f. she can keep her land others as "1". "1" means positive attitudes towards remarried women owning land. "0" means negative attitudes. The unit of observation is an individual in a Census 2000 county. "individual controls" are sex, age group, urban/rural, marital status, education attainment, father's education, mother's education, communist party member, socioeconomic status, and whether the respondent has a daughter-in-law. "Contemporary controls" are log of current per capita GDP, log of share of non-agricultural household registration, share of agriculture workforce, log of share of ethnic population, total fertility rate, and whether a county is located in a provincial capital. "Historical controls" are agriculture suitability, log of distance to the Grand Canal or Yangtze River plus1, number of courier routes passing the county, and log of commercial tax quotas in 1077. "Geographic controls" are latitude, longitude, log of slope plus 1. Column 1 includes all but individuals controls. Column 2 includes all controls except for socioeconomic status and whether to have a daughter-in-law. Column 3 contains full set of controls. Column 4 includes the squared term of in addition to the full set of controls. Regional fixed effects are included in all specifications. Robust standard errors are clustered at the county level.

Table 1.4: Historical Textile Production and Women's Decision to Work

	Women who never worked			
	(1)	(2)	(3)	(4)
Textile production until 1800	-2.120***	-1.625***	-1.601***	-2.463**
	(0.807)	(0.593)	(0.600)	(0.849)
Education attainment			-0.165***	-0.162***
			(0.0242)	(0.0246)
Log (dist. to Great Canal or Yangtze+1)	0.174	0.221	0.217	-0.621
	(0.236)	(0.204)	(0.199)	(0.456)
Log (dist. to Great Canal or Yangtze+1)^2				0.141*
				(0.0720)
Individual controls	No	Yes	Yes	Yes
Contemporary controls	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes
Observations	2155	2313	2313	2131

Standard errors in parentheses

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

Notes: The table presents results of logit estimation when the dependent variable is whether a woman has ever worked. The unit of observation is a woman older than 30 in a Census 2000 county. The sample is restricted to Han Chinese women. "individual controls" are age group, urban/rural, marital status, education attainment, father's education, mother's education and household size. "Contemporary controls" are log of current per capita GDP, log of share of non-agricultural household registration, share of agricultural workforce, log of share of ethnic population, and whether a county is located in a provincial capital. "Historical controls" are log of share of rice paddies, log of distance to the Grand Canal or Yangtze River plus1, number of courier routes passing the county, and whether a county was located in a treaty port in 19th century."Geographic controls" are latitude and longitude, log of slope plus 1, and log of distance to coast. Column 1 includes all but individual controls. Column 2 includes all controls except for education attainment. Column 3 contains full set of controls. Column 4 includes the squared term of in addition to the full set of controls. Regional fixed effects are included in all specifications. Robust standard errors are clustered at the county level.

and 4 for "university education or above". My outcome variable education gap has a mean of 0.395, which is quite substantial given that the best educated daughter receives less than middle school level (1.662) education on average.

My estimation equation is

$$y_{i,p} = \alpha + \beta \text{Textile}_p + \mathbf{X}_p^H \boldsymbol{\Omega} + \mathbf{X}_p^G \boldsymbol{\Lambda} + \mathbf{X}_p^C \boldsymbol{\Pi} + \mathbf{X}_i^I \boldsymbol{\Gamma} + \epsilon_{i,p} \,, \tag{1.4}$$

where p denotes a prefecture.[23] Textile_p is my measure of historical textile production aggregated to the prefecture level. $\mathbf{X}_p^H$ includes agriculture suitability, log of distance to the Grand Canal or Yangtze River and log of commercial tax quotas in 1077. $\mathbf{X}_p^G$ and $\mathbf{X}_p^C$ are vectors of geographical and contemporary controls respectively, each measured at the prefecture level. $\mathbf{X}_p^G$ includes latitude, longitude and distance to coast. $\mathbf{X}_p^C$ includes non-agricultural household registration, share of primary industry and share of ethnic population.[24] $\mathbf{X}_p^C$ denotes current individual-level controls: age group.[25] father's education and mother's education.[26] Standard errors are clustered at the prefecture level for all specifications.

Estimation results are reported in Table 1.6. Coefficient estimates are statistically significant for all columns, and economically meaningful. The presence of historical textile production is associated with a reduction in education gap of 0.03 to 0.06, or 2 to 4 months of education, which is equal to 8% to 16% of the sample mean. Column 1 reports estimates without contemporary controls. Contemporary controls are included in the specification in Column 2, reducing the size of the coefficient estimate by about half. I include total number of children in Column 3. As expected, total number of children is positively associated with

[23]In the IPUMS 1990 census data, individual residence is only recorded at the prefecture level.

[24]The log of share of non-agricultural household registration and share of primary industry are taken from 2000 China City Yearbook, in which the jurisdiction of a prefecture is more restricted. A prefecture in the Yearbook is smaller and more urbanized.

[25]Age group is computed from the mean age of the best educated son and the best educated daughter. Three age groups are Age Group 23-30, Age Group 31-40, and Age Group 41-50.

[26]Household income would be a crucial budget constraint on the decision to educate children, but is unfortunately not available in the census data.

Table 1.6: Historical Textile Production and Within-Household Education Gap: OLS Results

	(1)	(2)	(3)	(4)	(5)
			Education gap		
Textile production by 1800	-0.0627*	-0.0376*	-0.0333*	-0.0365*	-0.0356*
	(0.0352)	(0.0176)	(0.0164)	(0.0202)	(0.0178)
Number of children			0.0316**		
			(0.00793)		
Log population density in 1820				-0.0121	
				(0.0547)	
Log (dist. to Great Canal or Yangtze+1)	-0.0250*	-0.0245**	-0.0239**	-0.0251**	0.0138
	(0.0127)	(0.0104)	(0.00985)	(0.00915)	(0.0371)
Log commercial tax quota in 1077	0.00206	0.0144	0.0146*	0.0149	0.165
	(0.0146)	(0.00863)	(0.00813)	(0.00919)	(0.105)
Log (dist. to Great Canal or Yangtze+1)^2					-0.00726
					(0.00794)
Log commercial tax quota in 1077^2					-0.00784
					(0.00563)
Individual controls	Yes	Yes	Yes	Yes	Yes
Contemporary controls	No	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes	Yes
Province fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	53909	53909	53909	53909	53909
Adjusted R^2	0.038	0.043	0.045	0.043	0.043

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports OLS estimates. The dependent variable is the difference in education attainment between the best educated daughter and the best educated son within a household. Educational attainment ranges from 0 (illiterate to semi-illiterate) to 4 for (university education or above). The unit of observation is a prefecture in the 2000 Census. Individuals aged between 22 and 50 in 1990 are included in the sample. In Column 1, “Individual controls" are age group, father’s education and mother’s education. Three age groups are Age Group 23-30, Age Group 31-40, and Age Group 41-50 “Historical controls" are agriculture suitability, log of distance to the Grand Canal or Yangtze River plus 1 and log of commercial tax quotas in 1077. “Geographic controls" are latitude, longitude and log of distance to coast. Robust standard errors are clustered at the prefecture level. Column 2 contains the full set of controls, including contemporary controls: log of share of non-agricultural household registration, share of primary industry and log of share of ethnic population. In addition to the full set of controls in column 2, total number of children is included as a control in Column 3, population density in 1820 is included as a control in Column 4, and squared terms of log of distance to the Grand Canal or Yangtze River plus1 and log of commercial tax quotas in 1077 are included in Column 5. Population density in 1820 is taken from Shujin Cao’s work (Ge and Cao, 2001). Columns 2 through Column 5, robust standard errors are clustered at the province level. Province fixed effects are included in all specifications.

the size of the education gap, but the coefficient estimate does not change much from Column 2. When population density in 1820 is controlled for in Column 4, the coefficient estimate remains similar. This suggests historical textile production does not positive affect modern-day outcomes through higher past levels of commercialization. Column 5 contains estimates with nonlinearities of commercial tax quota in 1077 and distance to the Great Canal or Yangtze river being controlled for. The coefficient estimate stays close to those in Column 2, 3 and 4. The same robustness checks are repeated for the IPUMS sample. The results remain robust.

1.6 IV Estimation

A potential concern with the OLS estimates is that the counties that were textile producers may have a higher likelihood of adopting textile technologies. It is possible that counties that were economically more developed were more likely to have adopted textile technologies, and counties that were closer to the market or transportation routes were more likely to sustain its production and make greater profits. If these counties are richer and have less son preference and more gender equality, this would bias the OLS estimates away from zero. Though a set of variables (mainly overall agricultural suitability, commercial tax quota in 1077, and distance to the Grand Canal or Yangtze River) have been included in the main specification and its variants, I am unable to address likely issues caused by unobservable characteristics, such as attitudes towards women prior to textile production. Besides, due to imperfect data on historical textile production, some of the coefficient estimates can suffer attenuation bias due to measurement error.

An important determinant for the location of textile industry is geo-climatic conditions. Among all contributing factors, scientists, engineers and industry experts highlight the importance of relative humidity in producing textiles. In a report on the textile industry in China (1909), the word "humidity" occurs more than 100 times, suggesting the pivotal role of humidity in the textile industry. The role of relative humidity was even more crucial for the earlier period (1300-1600), when humidification technologies remained

underdeveloped.

Textiles could be produced much more efficiently during parts of the day, and parts of the year that were comparatively humid. For places that experience greater variance in humidity within the day, the number of hours available for textile production could be limited, regardless of the average relative humidity. A textile machine represented a large fixed cost. Thus for a family the decision to own textile machinery the total number of hours possible for textile production was a key consideration. In addition, it is widely acknowledged that hardly any textiles can be produced when relative humidity drops 60%, and that the benefit of moisture is offset by stickiness of the fiber once relative humidity exceeds 80%, i.e. there is a non-linear relationship between relative humidity and suitability for textile production.

Using high resolution gridded datasets from the Climate Research Unit, University of East Anglia, I can identify the monthly average relative humidity of each county and construct a relative humidity index variable in the procedure below.[27] Every county receives a score ranging from 1 to 5 for each month, based on its average relative humidity for the month.

Ideally, this variable would represent the hours available for textile production. In practice, data do not exist on the relative humidity for any particular day, let alone variance within a day. Hence I restrict my focus on the number of months humid enough for textile production. Every county receives a score ranging from 1 to 5 for each month, based on its average relative humidity for the month. To account for non-linearity in the impact of relative humidity on historical textile production, I set the lower-bound relative humidity for feasible production to be 60%, and make it take a value of "5" if actual relative humidity is below that level. Once above 60%, a county will be scored on a lower number as its relative humidity level increases.[28] When I add up monthly scores, I get a number ranging from 12 to 60 for each county, with 12 being the most suitable, and 60 being the least suitable. I take the negative of the total score to build an suitability index where suitability

[27] This data is downloaded from http://www.cru.uea.ac.uk/cru/data/hrg/. The dataset contains information on 10 arc-minute by 10 arc-minute grid-cells globally.

[28] "4" for 61%-65%, "3" for 66%-70%, "2" for 71%-75%, "1" for 76% or above

increases in its value. This index can be seen as approximating the number of months available for production with a gradient to quality and efficiency. Figure 1.7 shows the distribution of relative humidity index at a county level. Darker shades represent higher relative humidity and hence, higher weaving suitability. Missing values are shaded white.

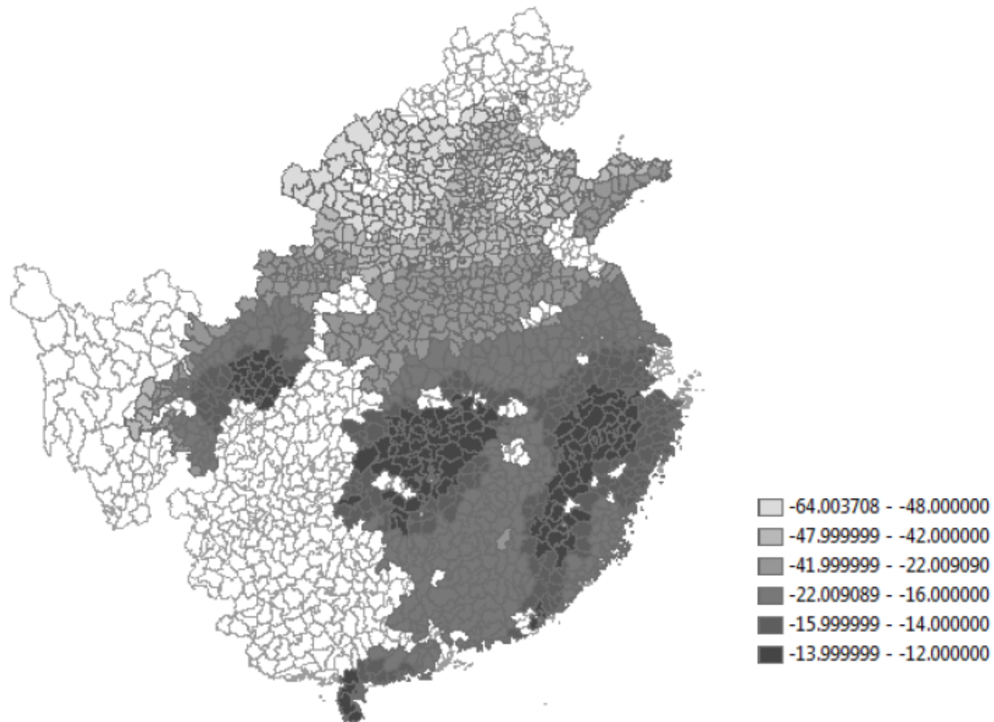


Figure 1.2: IV: Relative Humidity Index

I begin my IV estimation by testing the relationship between my relative humidity index and historical textile production excluding treaty ports. Due to the special properties of a binary regressor, I opt for a treatment-effect model for IV estimation that has the first stage being a Probit model. In the first stage of a treatment-effect model, I include the excluded instrument, which is relative humidity index (in six quantiles), and agricultural suitability, number of courier routes, commercial tax quotas in 1077 and its squared term, change in elevation (slope), distance to the coast, longitude, latitude and province effects. Panel A of Table 1.7 shows the estimates from the first stage: relative humidity index is positively correlated with historical textile production. Wald test rejects the null of independent equations in all specifications. Values of athrho are recorded at the bottom

Table 1.7: Historical Textile Production and Sex Ratio Imbalances: Instrumental Variable Analysis

	(1)	(2)	(3)
		First Stage	
Dependent variable: Textile production by 1800			
Relative humidity index		0.129 **	
Relative humidity * Log (dist. to Great Canal or Yangtze+1)			0.0006
Contemporary controls	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes
Province Fixed effects	Yes	Yes	Yes
Athrho		0.282*	0.239*
		Second Stage	
Dependent variable: Sex ratio at birth			
Textile production by 1800	-2.147 **	-7.784 **	-6.858**
All controls	Yes	Yes	Yes
Observations	1080	1080	1080

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports IV estimates. The dependent variable is sex ratio at birth. The unit of observation is a 2000 Census county. The same controls as in Column 1 of Table 1.2 are included. Column 1 contains OLS estimates. Column 2 report IV estimates with relative humidity index being the instrument. Column 3 reports estimates of a specification with relative humidity index * log of distance to the Great Canal or Yangtze plus 1 being the instrument. Province fixed effects are included in all specifications. Robust standard errors are clustered at the province level.

of the table. Second-stage results are reported in Panel B. Column 1 contains my OLS estimates. Column 2 report IV estimates with relative humidity index being the instrument. Column 3 reports estimates of a specification with relative humidity index * log of distance to the Great Canal or Yangtze plus 1 being the instrument. This allows the effect of relative humidity to vary at the distance to Great Canal or Yangtze. IV estimates range from -7.3 to -8.6, meaning the surplus of boys is reduced by roughly seven to eight boys per a hundred girls, if a county produced textiles historically versus not. The increase in coefficient estimates is likely explained by a removal of attenuation bias due to the use of better measured data.

While these results are highly suggestive, I am aware that relative humidity is closely correlated with other geographic characteristics that can have an independent role on gender equality, such as agricultural suitability, distance to coast, and in the case of China, latitude. Distance to coast is certainly economically important, and it can affect gender equality through openness to trade, or economic development in general. Likewise, distance to the Grand Canal had an effect on past economic development, which could in turn affect local conditions for women's well being in the past.[29] However, it should be noted that in previous estimations, agricultural suitability, distance to coast, latitude and longitude are already controlled for, and commercial taxes collected in 1077 are used to account for differential economic conditions and commercial activities prior to the introduction of textile technologies.

I acknowledge it is possible that relative humidity and distance to the Grand Canal can affect son preference and gender equality conditional on covariates through channels other than historical textile production (for instance, perhaps through the channel of women's appearance which might plausibly give women from certain parts of China a competitive edge in an open marriage market), so the results have to be interpreted with caution.

[29]The Grand Canal was first built during the Sui Dynasty (581 - 618 AD.

1.7 The Emergence and Persistence of Gender Equality

1.7.1 Changing notions of women's possible role in society, 1300 - 1800

Differing from the Europe Marriage Pattern (De Moor and Van Zanden, 2010; Voigtländer and Voth, 2013), pre-modern China had both universal marriage and early marriage. Women had limited opportunities to participate in society on their own. Despite the overall conservative gender norms in the Ming Dynasty, widows were given a certain amount of autonomy in making economic decisions for the household (Afeng, 2002). From 1300 to 1800, Ming and Qing China witnessed an unprecedented number of widows who participated in a wide range of economic and social activities. Relying on textile incomes, widows continued to support their children and in-laws (Zurndorfer, 1998; Sommer, 2000; Elvin, 1984). Textiles played a conducive role in broadening the understanding of women's possible role in society (Pomeranz, 2004; Bray, 1997; Pomeranz, 2005).

Prior to 1300, ordinary women typically lacked the means to support themselves after their husband's death. Most would get remarried when their husband passed away before their own death. Between 1000 and 1300, inheritance laws became more stringent, and it became increasingly more difficult for women to inherit wealth from their deceased husband, further limiting women's choices as a widow. Under Neo-Confucianism, which developed during Song Dynasty (960–1279), women were praised for maintaining female chastity after their husband's death. Those women were called "virtuous" women and often documented in local gazetteers for their deeds.

After 1300, textiles began to provide women with a means of living. Before the Ming period, among "virtuous" women, half stayed widowed to provide for her in-laws and children for an extended period of time, the other half committed suicide upon their husband's death to demonstrate their exemplary character (Jiazun, 1979).[30] The percentage

[30]According to Qing regulations, to be eligible to the title of " chase widow", a woman either had to remain chaste since before the age of 30 years old to the age of 50 years old, or had been chaste ten years or more but died before reaching 50 (Mann, 1987).

Table 1.8: Summary Statistics

Non-Textile Counties				
Variable	Mean	Std. Dev.	Min.	Max.
Widow	2.345	2.663	0	14
Suicide	0.655	1.396	0	6
N		29		
Textile Counties				
Variable	Mean	Std. Dev.	Min.	Max.
Widow	2.25	2.927	0	11
Suicide	0.083	0.289	0	1
N		12		

Table 1.9: *Jingbiao*: Suicidal and Long-Widowed Women

	Difference in Means	Standard Error
Suicides	0.572**	(0.272)
Widows	0.0948	(0.979)
Observations	41	

Standard errors in parentheses

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

Notes: The table reports a two-group mean-comparison test on number of records on suicidal women and chaste widows. The unit of observation is a historic county. The sample is restricted to counties that could be identified on the map of Year 1305.

of women who chose chaste widowhood over suicide dramatically increased in the Ming Dynasty. Though many factors were at play (Theiss, 2005; Ropp et al., 2001), textiles likely affected women's decision between suicide and chaste widowhood, since availability of financial means was key to surviving as a chaste widow. All else equal, women with no financial means were at a higher risk to commit suicide. To test the relationship between textile production and suicide, I search county gazetteers for evidence on "virtuous" women. I focus on women awarded imperial testimonials of merit (*jingbiao*) by the state. Table 1.8 suggests that from 1424 to 1644, among "virtuous" women with *jingbiao*, only about 3.5% committed suicide in the twelve textile counties, as opposed to 21.8% in the twenty-nine non-textile counties. A two-group mean-comparison test shows numbers of suicidal women are statistically different between textile counties and non-textile counties. Women in textile counties were far less likely to commit suicide upon the death of their husband.

These numbers supply qualitative evidence that suggests that women in regions where textile production was more important had a greater chance of avoiding the fate of suicide. Textiles enabled women to maintain a livelihood in the absence of their husband, contributing to a new notion that women could be productive and independent members of society. From the perspective of parents, daughter's ability to support herself under adverse circumstances reduced their mental and financial exposure to an ill-fated daughter.[31]

1.7.2 Persistence of gender norms after 1800

From the late 19th century onwards, China began to industrialize, starting from the treaty ports. The 1916 Economic Census documents the number of male and female workers in various industries at a province level. In provinces where textiles had been produced for commercial purposes, women were much more likely to work outside the home. The presence of women in industrial plants was much more common in Jiangsu, Zhejiang and Shanghai, where women often outnumbered men. Women working outside the home was extremely rare in Zhili, Shanxi and Shaanxi, where women had little experience with market work. A higher representation of women was noticed in a wide range of industries, including textile manufacturing plants. Table 1.10 summarizes the results.

I do a simple correlation test between share of female workers and textile population. Textile population is computed as share of population in a province residing in prefectures with historical textile production. Prefecture-level population in 1820, taken from Shuji Cao's work (Ge and Cao, 2001), is used in the calculation. All industries with at least 5% of the workers being women, and present in more than half of provinces, are included in the sample. Table 1.10 shows historical textile production is positively correlated with the share of female workers in most industries, with the exception of fur making. No similar correlations are observed in traditional family production.

[31] Bossler (2000) finds evidence for a continued relationship between a married woman and her natal family. While a woman became a member of her husband's extended family upon marriage, her natal family could still be involved in times of crisis. This includes cases in which a widowed woman in poverty imposed a financial burden on her natal family.

Table 1.10: Historical Textile Production and Share of Female Workers

	(1) Cotton	(2) Fur	(3) Knitting	(4) Attire	(5) Ceramics	(6) Match	(7) Gunpowder	(8) Medicine
Textile Pop.	0.483	-0.207	0.498	0.063	0.344	0.056	0.352	0.086

Notes: The table summarizes correlations between share of female workers in 1916 (by industry) and share of population residing in prefectures with historical textile production. The unit of observation is a province in 1916. All industries with at least 5% of the workers being women, and present in more than half of provinces, are included in the sample.

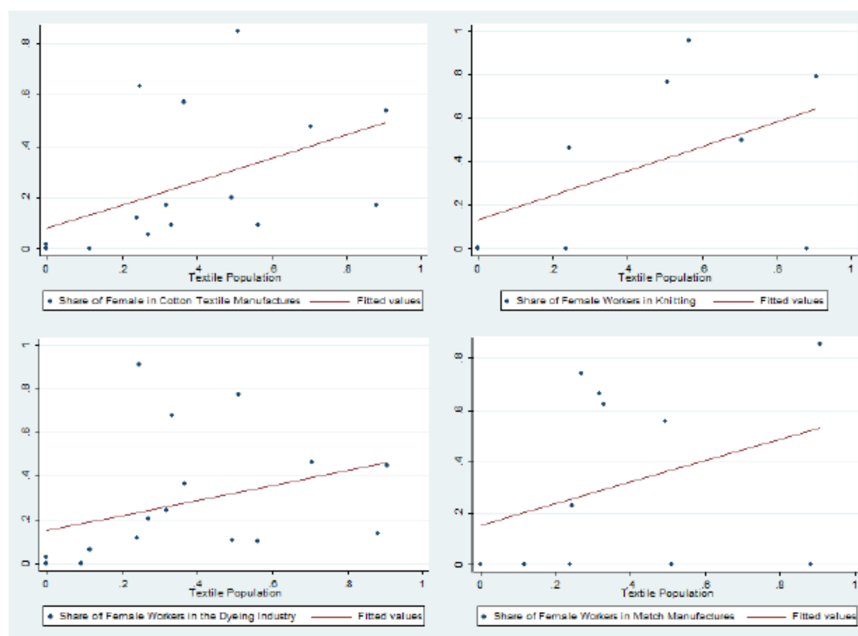


Figure 1.3: Share of Female Workers in 1916

The results reported in Table 1.10 provide suggestive evidence that when China started to industrialize, textile regions saw a higher level of female labor force participation. The effect is present in, but not limited to, the textile manufacturing. Figure 1.3 illustrates this point with a scatter plot and a fitted regression line for each of cotton textile manufacturing, knitting, dyeing, and match manufacturing. One interpretation of these results is in textile regions, women continued to do market-oriented work when Chinese economy began to industrialize. Families that were used to incomes provided by women quickly adapted to new economic realities, and began to let women work in manufacturing jobs to support a family. Notably, these results do suggest an alternative mechanism for historical textile production to affect modern-day outcomes: places that had more equal gender norms may have had more rapid industrialization due to abundant female labor, and early industrialization became proximate causes of better later outcomes for women.[32]

1.8 Channels for Persistence

My results indicate that textile production improved the status of women in premodern China and that these effects continue to shape attitudes to women in China today. I now take a closer look at the causal mechanisms underlying my results. The long-term impacts of historical textile production could have directly affected how daughters are valued as manifested in sex ratios, as well as indirectly affected sex ratios through development of institutions, policies, laws and industries that are more complementary with women in more recent periods. My micro-level analyses based on CGSS and IPUMS provide evidence consistent with historical textile production shaping values and beliefs about women. In this section, I discuss uniformity in formal institutions in greater details, and explore cultural transmission of values as one possible mechanism for the effect of historical textile production on modern-day outcomes.

[32]Previous studies have shown the effect of proto-industry on the locations of modern industries (Farnie, 1979). If places with more equal gender norms had better labor market conditions, and therefore, had more success in the process of industrialization, those places might continue to have more favorable labor market conditions for reasons less related to gender norms.

1.8.1 Uniformity in Formal Institutions since 1949

Due to communist efforts to impose gender equality, modern China has a range of formal institutions designed to promote gender equality. In the communist period, Chinese women's status changed from a "family private person" of traditional society to a "social person", and Chinese women gained the same legal status as men. The Constitution of the People's Republic of China enacted in 1954 expressly stated that women and men enjoy equal rights. In the era of planned economy, China was able to realize the ideal of equal pay for equal work for men and women in legal terms (Entwisle and Henderson, 2000; Hannum and Xie, 1994; Johnson, 2009; Yang, 1999). Moreover, there is very little local variation in these formal institutions because of its high level of political centralization, as urban authorities in China have little or no ability to shape labors laws and policies at a local level.[33] This history of both radical reforms and institutional uniformity makes formal institutions an unlikely candidate for a channel of persistence.

Despite a lack of variation in either labor laws or maternity leave law at a local level, one could still argue that the recent growth in China has led some regions to develop informal institutions that indirectly encourage or discourage gender equality. And it could be similarly argued that as economy grew, policies and laws have not been created accordingly to maintain gender equality, implying a "deficit" of formal institutions in more developed areas. In both cases, per capita income plays a central role in differential gender equality in the post-reform era. To account for the effect of newly emerging institutional differences, I control for per capita income in my specifications, as well as including prefecture, province or region fixed effects.

1.8.2 Cultural Transmission of Values

In the robustness section, I show that places with more local residents see a greater textile effect. Low-migration areas seeing a greater textile effect could be related to a higher

[33] Rural China has more policies generated through democratic processes at the local level (O'brien and Li, 2000).

percentage of residents descending from ancestors who lived in the same area. This implies cultural norms have been passed down from generation to generation. An alternative interpretation is, values spread horizontally, and low-migration areas are more effective in horizontal transmission of values.

To separate these two mechanisms, I conduct an additional analysis with the CGSS data. My dependent variable is female labor force participation. I consider women listed as “homemakers", “unemployed or retired", “never worked or still at school" as out of workforce. In CGSS, “never worked" and “still at school" are lumped into the same category. As I am mostly interested in out-of-labor-force status due to reasons other than additional years of education, I primarily focus on women older than 30. I use place of household registration as a proxy for place of birth. As part of the “internal passport" system, household registration identifies the legal residence of a Chinese citizen. Due to difficulty in changing one's place of registration, a person is sometimes registered in one place, but lives in another place. Place of registration often lags behind actual residence. I infer from household registration status that when a person's registration status matches his/her actual residence, he/she is less likely to be a migrant, and that when a person has the same place of registration as his/her parents, he/she should possess stronger values identified in the locality, if there is vertical transmission of values.

Table 1.11 summarizes the results of this logit estimation. Column 1 contains estimates of the full sample where mother's household registration is known. Column 2 contains estimates on a subsample of respondents who themselves are registered locally, but whose mother is registered elsewhere. In Column 3, both respondents and mothers are registered locally. Column 4 - Column 6 repeat Column 1 - Column 3, with "mother" being replaced by "father".

Table 1.11 illustrates how the textile effect varies from respondents with the same place of registration as their parents, to respondents with different places of registration. Overall, more people have a known mother's registration than have a known father's registration. The difference is mostly driven by differences in female and male life expectancy. Among those who have a living father (Column 4 - Column 6), the textile effect is slightly weaker, compared with those with a living mother but not necessarily a living father (Column 1 - Column 3). Column 1 and Column 4 include migrants who are registered elsewhere. The textile effect is statistically significant in Column 1, but not in Column 4. Column 2 and Column 5 only include respondents whose actual residence of household registration match. Column 3 and Column 6 further restrict the sample to those who have the same household registration as their mother or father. This considerably increases the size of coefficient and statistical significance of estimates in both cases. The textile effect is the strongest in respondents whose parents are likely to have been born and raised in the town they currently reside. In other words, parents can have an effect on respondents' gender norms, weakening the effect of current residence on a respondent. Results from Table 1.11 support the hypothesis that at least some of the cultural values are transmitted vertically from parents to children.

1.9 Conclusion

This paper provides evidence that a portion of the variation in son preference in modern day China can be accounted for by the historical production of textiles in a location. It suggests that gender norms can be shaped by long-lasting relative productivity shocks.

Table 1.11: Persistence: Transmission by Parents

	Female Labor Force Participation					
	Mother's reg. known	Mother reg. elsewhere	Mother reg. locally	Father's reg. known	Father reg. elsewhere	Father reg. locally
Textile production 1800	-0.971**	-0.928**	-1.289***	-0.444	-0.454	-0.683*
	(0.417)	(0.412)	(0.395)	(0.348)	(0.340)	(0.307)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Contemporary controls	Yes	Yes	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1183	1168	1046	950	937	835

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table presents results of logit estimation when the dependent variable is women's labor-force status: "homemakers", "unemployed or retired", "never worked or still at school" are coded as out of workforce. The unit of observation is a woman older than 30 in a Census 2000 county. The sample is restricted to Han Chinese women. Same controls as in Column 3 of Table 1.4 are included in all specifications. Column 1 contains estimates of the full sample where mother's household registration is known. Column 2 contains estimates on a subsample of respondents who themselves are registered locally, but whose mother is registered elsewhere. In Column 3, both respondents and mothers are registered locally. Column 4 - Column 6 repeat Column 1—Column 3, with "mother" being replaced by "father". Robust standard errors are clustered at the county level.

I use both OLS and IV to estimate the impact of historical textile production on today's son preference and gender equality. The results are robust to the exclusion of regions famous for historical textile production, such as the Yangtze Delta, and regions that barely had any textile production at all. My micro-level analysis lends support to my county-level analysis, and generates additional insights that allow me to extend my analysis to include other variables more commonly discussed in the context of gender equality. I find that historical textile production also helps to eliminate gender-specific parental investment and to increase female labor workforce participation.

Finally, I am able to explore historical trajectories of gender equality and gender norms in China. I find evidence for quick adaptation in gender norms in face of the "textile revolution", and persistence of the new norms. My analysis suggests that an important channel of persistence is through cultural transmission from parents to children.

Appendix

Robustness Checks: Subsamples

Table A- 1: Robustness Check: Yangtze Delta

	Dev. sex ratio			
	(1)	(2)	(3)	(4)
Textile production by 1800	-0.178**	-0.189**	-0.142*	-0.154*
	(0.0783)	(0.0712)	(0.0797)	(0.0742)
Log per capita GDP	-0.0287	0.0339	0.00145	0.0578
	(0.180)	(0.187)	(0.180)	(0.186)
Log per capita GDP^2	0.00163	-0.00182	0.000150	-0.00300
	(0.00833)	(0.00869)	(0.00829)	(0.00856)
Total Fertility Rate	0.574**	0.549**	0.546**	0.523**
	(0.234)	(0.235)	(0.229)	(0.228)
Individual controls	Yes	Yes	Yes	Yes
Contemporary controls	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes
Prefecture fixed effects	Yes	Yes	Yes	Yes
Observations	1079	1164	1158	1243
Adjusted R^2	0.511	0.509	0.513	0.510

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: See Table 1.2. All controls are the same as in Column 1 of Table1.2. In Column 1, all three provinces (Zhengjiang, Jiangsu & Shanghai) are omitted. Zhejiang is omitted in Column 2, Jiangsu is omitted in Column 3, and Shanghai is omitted in Column 4. Robust Standard errors are clustered at the province level.

Table A- 2: Robustness Check: Migration

	Dev. sex ratio			
	(1)	(2)	(3)	(4)
Textile production by 1800	-0.218**	-0.250***	-0.193**	-0.196**
	(0.0771)	(0.0730)	(0.0656)	(0.0673)
	(0.147)	(0.156)	(0.154)	(0.144)
	(0.00680)	(0.00688)	(0.00684)	(0.00628)
	(0.238)	(0.271)	(0.275)	(0.275)
Individual controls	Yes	Yes	Yes	Yes
Contemporary controls	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes
Prefecture fixed effects	Yes	Yes	Yes	Yes
Observations	853	942	973	1009
Adjusted R^2	0.494	0.475	0.477	0.488

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: See Table 1.2. All controls are the same as in Column 1 of Table1.2. Counties above the threshold of migration rates are omitted. From Column 1 to Column 4, the threshold is that total population is 101%, 102%, 103%, and 104% of the local population respectively. Robust standard errors are clustered at the province levels.

Table A- 3: Robustness Check: Treaty Port

	Dev. sex ratio			
	(1)	(2)	(3)	(4)
Textile production by 1800	-0.160**	-0.199***	-0.170**	-0.181**
	(0.0727)	(0.0593)	(0.0646)	(0.0654)
Log per capita GDP	0.164	0.0517	0.0740	0.139
	(0.178)	(0.187)	(0.178)	(0.177)
Log per capita GDP^2	-0.00840	-0.00267	-0.00386	-0.00725
	(0.00763)	(0.00870)	(0.00810)	(0.00766)
Total Fertility Rate	0.395	0.476*	0.390	0.411
	(0.228)	(0.245)	(0.243)	(0.247)
Individual controls	Yes	Yes	Yes	Yes
Contemporary controls	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes
Prefecture fixed effects	Yes	Yes	Yes	Yes
Observations	1080	1216	1159	1136
Adjusted R^2	0.528	0.512	0.513	0.518

Standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: See Table 1.2. All controls are the same as in Column 1 of Table1.2. All treaty ports are omitted in Column 1. In Column 2, Wave 1 treaty ports (treated ports set up in 1842) are omitted. In Column 3, Wave 1 & 2 treaty ports (treaty ports set up before 1864) are omitted. In Column 4, Wave 1, 2 & 3 (treaty ports set up before 1890) are omitted. Robust standard errors are clustered at the province level.

Robustness Checks: Propensity Score Matching

I estimate propensity scores using a logit model. Table A- 4 presents estimates from an OLS and logit regressions. It show adoption of textile technologies was positively correlated with commercial tax in 1077 and longitude, and negatively correlated with change in and distance to Grand Canal or Yangtze River. I use the estimates from Table A- 4 to compute propensity scores. Figure A- 1 plots densities of propensity score for textile counties and non-textile counties. It shows that there is substantial overlap in the entire distributions for textile and non-textile counties propensity scores.

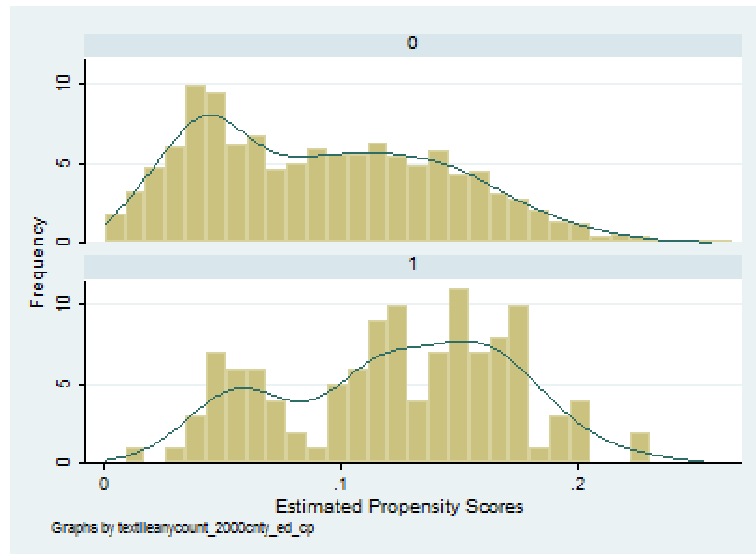


Figure A- 1: Propensity Score Densities

Table A- 5 summarizes matching results using propensity scores calculated in Table A- 4. I use nearest neighbor matching with no replacement and a small caliper in all models.[34] Following the literature (Frölich, 2004; Austin, 2009), I use a caliper size of 0.001 in most of my models, except for Column 2. Figure A- 2 and Figure A- 3 provide a basic summary of matched observations in the treatment group (textile counties) and the control group (non-textile counties) .

[34]Caliendo and Kopeinig (2008) summarize the bias-efficiency trade-off associated with the width of a caliper and various matching methods. A decrease in caliper width reduces the bias of the estimator. 1:1 nearest neighbor matching, relative to 1:n nearest neighbor, radius or kernel matching, reduces the most bias but decreases precision.

Table A- 4: Regression Analysis of the Adoption of Textile Techonologies

	(1) Logit	(2) OLS
Agricultural suitability	-0.0985	-0.0101
	(0.0741)	(0.00695)
No. courier routes	0.0241	0.000180
	(0.0782)	(0.00719)
Log (dist. to Great Canal or Yangtze+1)	0.0544	0.00263
	(0.0850)	(0.00854)
Log (dist. to Great Canal or Yangtze+1) ^{2}	-0.0000153**	-0.000000665**
	(0.00000632)	(0.000000252)
Log commercial tax quota in 1077	0.192**	0.0142*
	(0.0954)	(0.00694)
Log (slope+1)	-0.397**	-0.0341**
	(0.176)	(0.0137)
Log distance to coast	0.169	0.0152
	(0.157)	(0.0147)
Latitude	-0.0767	-0.00564
	(0.0536)	(0.00412)
Longitude	0.167**	0.0137**
	(0.0751)	(0.00614)
Region Effects	Yes	Yes
Observations	1238	1246
Adjusted R^2		0.017

Standard errors in parentheses

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

Notes: The table reports logit and OLS estimates. The unit of observation is a county in 2000 Census. The dependent variable is historical textile production. Regressions include regions effects and controls agriculture suitability,number of courier routes passing the county, log of distance to the Grand Canal or Yangtze River plus 1 and its squared term, log of commercial tax quotas in 1077, log of slope plus 1 and log of distance to coast, latitude and longitude. Robust standard errors are clustered at the province level.

Column 1 of Table A- 5 reports baseline results. The common support [0.0178, 0.227] is determined by the "minima and maxima criterion", i.e. to delete all observations whose propensity score is smaller than the minimum and larger than the maximum in the opposite group. Column 2 reports estimates with a wider caliper, 0.01. Column 3 and Column 4 presents estimates on trimmed samples.[35] Column 5 reports estimates

[35]Crump et al. (2009) develops optimal bounds for common support, and recommends [0.1, 0.9] as a rule of thumb. Given the range of scores I have [0.0007, 0.2566], I apply a common support [0.025, 0.227] in Column 3. In Column 4, in addition to the "minima and maxima criterion", I impose common support by dropping 6 percent of the treatment observations at which the propensity score density of the control observations is the lowest. The cut-off point, 6 percent, is also used by (Li and Zhao, 2006).

Table A- 5: Deviation in Sex Ratio at Birth in Textile vs. Non-Textile Counties

	Difference in Dev. in sex ratio at birth				
	Baseline	Caliper=0.01	Trimmed - 1	Trimmed - 2	ATT
Unmatched	-0.164	-0.165	-0.132	-0.130	-0.165
Matched (ATT)	-0.302**	-0.296*	-0.323**	-0.343**	-0.313**
Matched (ATE)	-0.217	-0.219*	-0.241	-0.256*	
On-support observations	232	236	222	218	1244

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

Notes: The table summarizes estimated average treatment effects of historical textile production on deviation in sex ratio at birth based on propensity scores. Nearest neighbor matching with no replacement is used in all models. A caliper width of 0.001 is used in Column 1, 2 and 4. Column 1 reports baseline results, with a common support [0.0178, 0.227]. Column 2 reports estimates with a caliper width of 0.01. Column 3 presents estimates derived from a trimmed sample, with a common support [0.025, 0.227]. Column 4 presents results of an alternative trimmed sample, where a common support is imposed by dropping 6 percent of the treatment observations at which the propensity score density of the control observations is the lowest. Column 5 reports estimates of average treatment effects on the treated (ATT) with all but two off-support observations.

with more on-support observations, when only average treatment effects on the treated (ATT) are estimated. Across all models, average treatment effects on the treated (ATT) are quantatitively similar and statistically significant, ranging from -0.296 to -0.344. Average treatment effects (ATE) are slightly smaller in magnitude, ranging from -0.217 to -0.256. Estimates of ATE are statistically significant in Model 2 and Model 4, when a wider caliper or a trimmed sample is used. Average treatment effects are not significant on unmatched samples.[36]

Covariates balancing is satisfied in all models. Strata analysis produces similar average treatment effects to propensity score analysis performed directly on the entire sample. OLS regressions weighted by propensity score weights, carried on on-support observations, have quantitatively larger effects than OLS regressions on unmatched samples, but those effects are often not significant (Model 2 - Model 4). This is likely due to lower precision caused by shrinking sample size, as well as heterogeneous treatment effect across the

[36]These results are consistent with those of models with change in the raw numbers of deviation in the sex ratio (rather than logged values) being the dependent variables. Those results are not reported here, but are available on request.

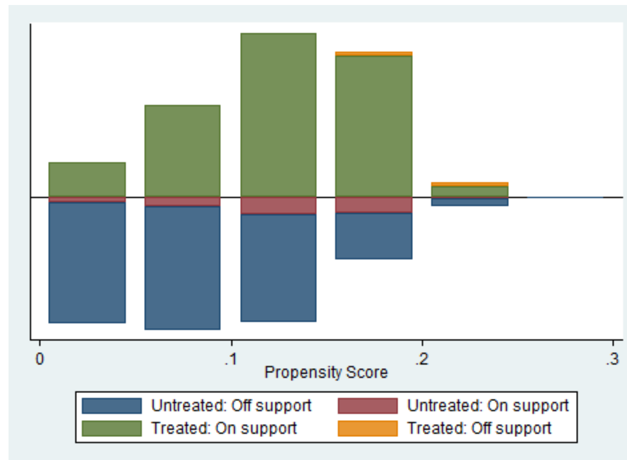


Figure A- 2: ATE

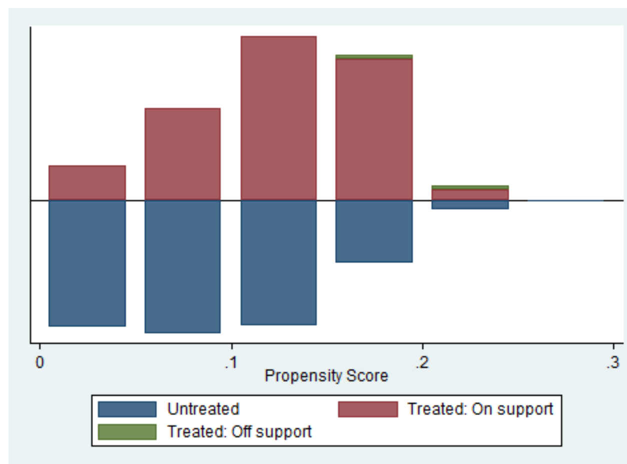


Figure A- 3: ATT

Figure A- 4: Matched Samples

strata.[37]

Propensity score matching estimation supports the main conclusions of the OLS estimation. After correcting for selection bias, historical textile production demonstrates a greater impact on modern-day sex ratio at birth.

[37]These results are not reported here, but are available on request.

Additional Tables

Table A- 6: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Education	1.662	0.895	0	4	53909
Education gap	0.393	0.879	-4	4	53909
Textile production by 1800	0.428	0.494	0	1	53909
Mother's education	0.415	0.702	0	4	53909
Father's education	0.982	0.86	0	4	53909
Age	27.931	4.673	23	49	53909
Total number of children	3.083	1.303	2	14	53909
%Ethnic population	1.034	2.52	0.08	77.19	53909
%Non-agricultural household registration	155.601	94.086	24.97	441.14	53909
%Agriculture in GDP	16.45	9.016	1.1	40.6	53909
Agricultural suitability	-4.023	1.807	-7	-1	53909
Dist. to Great Canal or Yangtze	2.364	2.422	0	8.584	53909
Commercial tax quota in 1077	19328.586	30892.488	407.248	217343.172	53909
Dist. to coast	274.56	308.118	0.087	1175.69	53909
Longitude	115.442	4.441	103.658	121.941	53909
Latitude	30.755	4.703	21.065	39.281	53909
Province	37.995	8.852	13	61	53909

Chapter 2: The Literary Inquisition: The Persecution of Intellectuals and Human Capital Accumulation in China

> 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)

2.1 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

[1]Including Max Weber (1946, 416) and Joseph Needham (1995). Huff (1993, 275-314) explores how the imperial examination system shaped science and innovation. Moykr (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

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 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; furthermore, 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 often the 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

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.

[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).

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 mitigate 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 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

[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.

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,

[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.

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. Wolitzky and Acemoglu (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 outcomes including publications and the placement of PhD students in Germany. In contrast, Hornung (2014) examines the positive effect of the Huguenot

[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.

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 for social mobility of 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]

[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 **?** 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

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 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, 2014b; 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 over quality, thereby further delaying the transition to modern growth.

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environment will likely be hostile to innovation.

[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.

The rest of the paper is organized as follows. Section 2.2 describes our data. In Section 2.3 we explain our empirical strategy and present our main results. In Section 2.4 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 2.5. In Section 2.6 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 2.7 concludes.

2.2 Data and Historical Setting

2.2.1 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

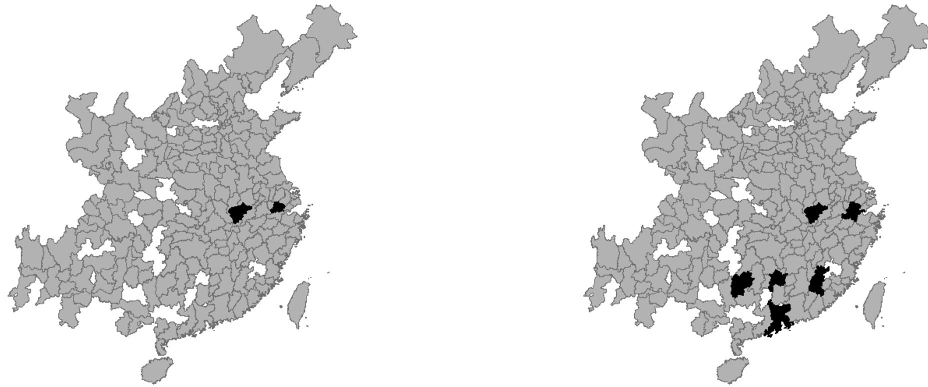
[15]This animosity preceded the Qing dynasty. It was firmly established from the Song dynasty onwards (see Rossabi, 1983; Ebrey, 1991; Ge, 2004). Brook notes: 'The legitimacy of the Manchus, China's Inner Asian rulers during the Qing (1644–1911), depended on their being seen as rightful candidates for 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).

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.

[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 (2014c) 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, 1660–1800.

Figure 2.1: 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 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 2.1 depicts the prefectural boundaries of Qing China and displays the prefectures

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

[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.

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 officials' (Brook, 2005, 178). Particularly during the reign of the Qianlong emperor, the houses of scholar-officials were searched for suspect material; at the 'same

[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); Liang (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).

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 actually 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 on relevant provincial or prefectural characteristics. In Tables 2.1 and 2.2 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]

[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.

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 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 at 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' (Zizhen, 1991).

Table 2.1: 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.

2.2.2 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 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 2.2). Our data contains 1,484 jinshi.[25] Our dependent variable reflects both the number of

[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

[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

Table 2.2: Correlations of inquisitions at a prefectural level

	Inquisition	Earthquake	Conflict	Extreme weather	Natural disasters
	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.

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 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

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 (1989).

who were victims of the literary inquisition was mourned by the local community for decades or more.[27]

2.2.3 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 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]

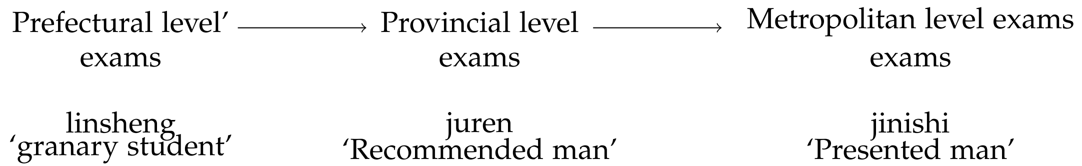
The Imperial examination was characterized by quotas at various administrative levels. The existence of these quotas meant that a given examinee's chance of success varied across prefectures and provinces. Quotas arbitrarily limited the proportion of lower level candidates (*linsheng* and *juren*) who 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.

[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).

[28]Tables 2.1 and 2.2 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.

Figure 2.2: The Imperial Examination System



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.

2.3 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 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.

[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.

2.3.1 Province-level DID

We first estimate the following equation:

$$\begin{aligned} \text{Exam Candidates}_{p,d} = & \beta_0 + \beta_1 \text{Affected by Inquisition}_{p,d} + \mathbf{\Omega}_p + \mathbf{\Lambda}_d \\ & + \epsilon \ , \end{aligned} \tag{2.1}$$

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. Affect by Inquisition$_{p,d}$ denotes a province p of an individual affected by an inquisition for all subsequent decades d. $\mathbf{\Omega}_p$ 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 .

2.3.2 Provincial Level Results

Tables 2.3 and 2.4 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 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 2.3: 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 2.3 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 the 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 2.3 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 2.4 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.

2.3.3 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 have unbalanced covariates before matching. To mitigate concerns over violations of the ‘‘parallel trends" assumption, we restrict our comparison group to only those with similar propensity matching scores to those of the

Table 2.4: 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.

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]

2.3.4 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

[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).

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 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.

2.3.5 Prefecture-level results

Table 2.5 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,

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

[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'.

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 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 2.6 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 2.7 we include the set of time-varying controls we employed in our provincial level estimation which included a measure of the number of conflicts,

[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.

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 individuals on average.

Figure 2.3 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 2.4 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.

2.3.6 Robustness Checks

In this subsection, we demonstrate that our baseline results are robust to allowing for heterogeneous trends, 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 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

Table 2.5: Prefecture Level DID Estimation

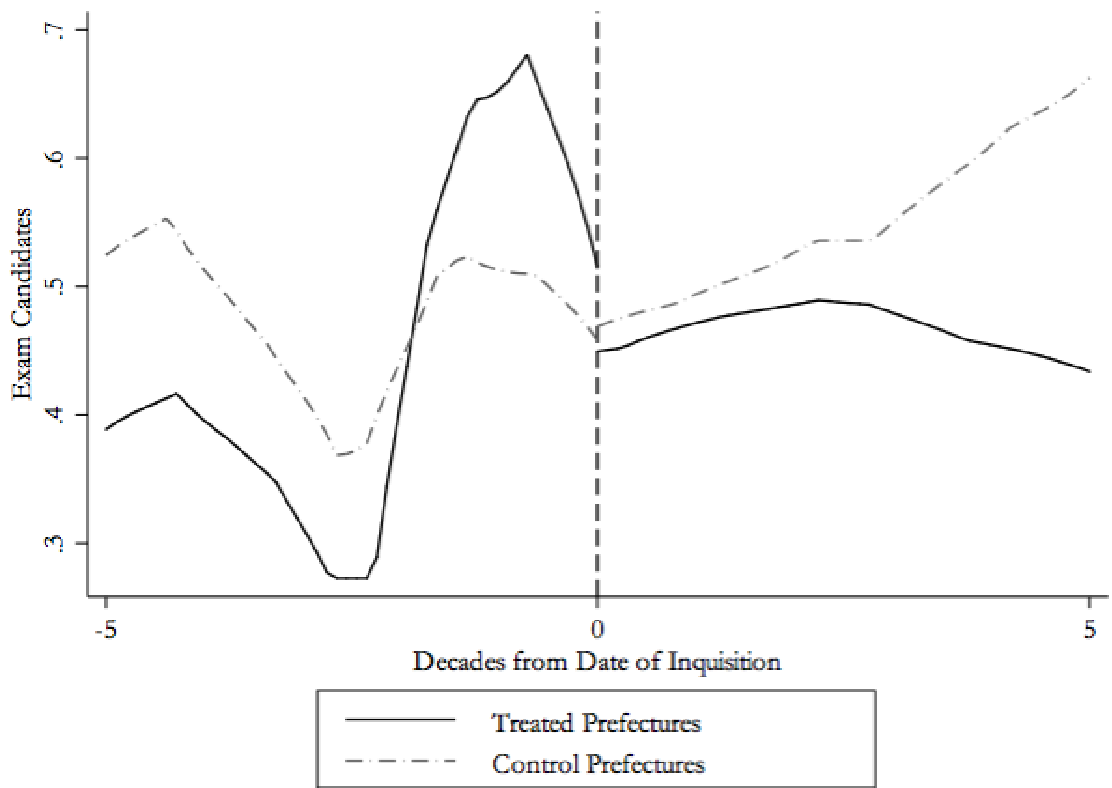
	# Officials			ln (1 + # Officials)
	(1)	(2)	(3)	(4)
Affected by Inquisition	-0.269*	-0.306*	-0.286**	-0.147*
	(0.143)	(0.155)	(0.134)	(0.0829)
Past n. exam candidates	No	No	Yes	No
Ming Exam Region*Time Trend*Trend Break in 1712	Yes	Yes	Yes	Yes
Log Pop in 1600 *Time Trend	Yes	Yes	Yes	Yes
Log Pop in 1600 ^{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

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

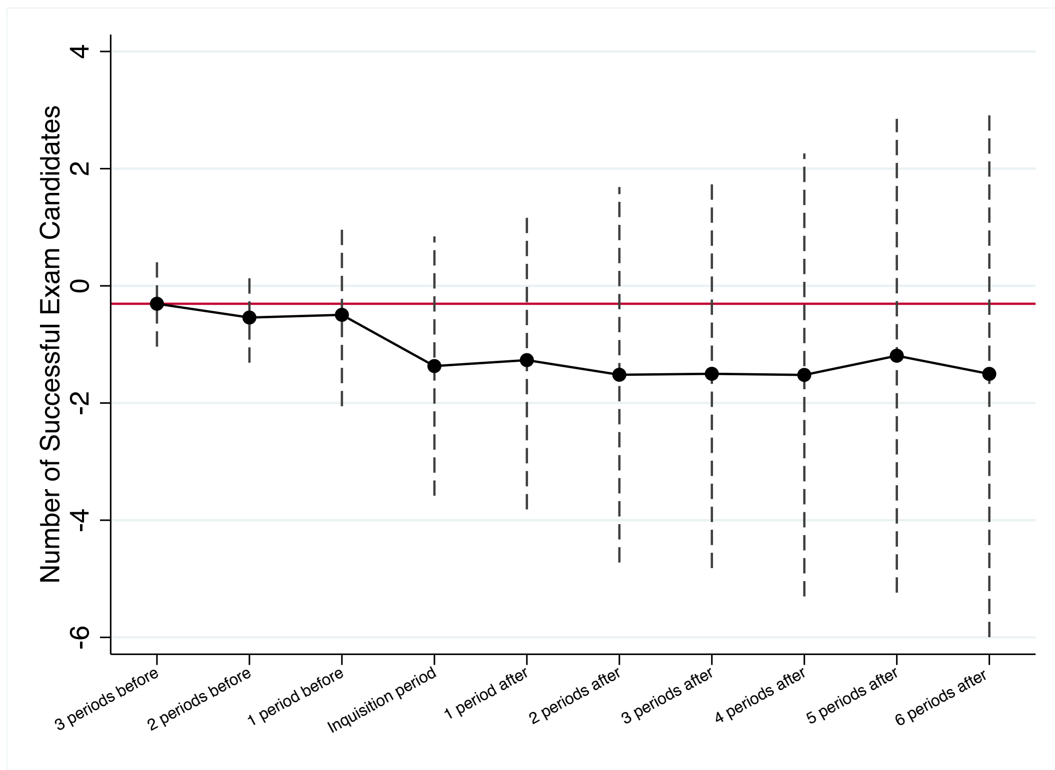
Notes: The table reports DID estimates at a prefectural level. It demonstrates the effect of a literary inquisition on the number of candidates in the next decade. The unit of observation is a prefecture on the 1820 map. The dependent variable is the number of government officials recruited from a particular 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.

Figure 2.3: 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.

Figure 2.4: 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.

yields estimates that are comparable to our baseline.

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 as Pomeranz (2000) as being particularly economically developed—is contained within these provinces. These areas also experienced lower population growth during this period (Allen, 2009b; **?**).[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 period. Table A-8 we show that our results are robust to the exclusion of these prefectures.[37]

[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.

[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.

2.4 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.

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[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.

Examination candidates often worked as teachers prior to passing higher level exams. As they 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.

2.4.1 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

[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

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 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:

$$\max_{e_i,b_i} u(e_i, b_i, p) - b_i p v(\gamma) \quad \text{subject to} \quad \Gamma_i(k(t_j), e_i, c(a_i, e_i)) \leq w_i \, , \tag{2.2}$$

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

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

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).

2.4.2 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:

$$u(e, 1, p) - k(t) - c(e, \hat{a}) = u(e), 0) - k(t) - c(e, \hat{a}) \ . \tag{2.3}$$

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 $b_i = 1$), where $\bar{a} > \hat{a}$ is determined by the following indifference condition:

$$u(e, 1, p) - pv(\gamma) - k(t) - c(e, \bar{a}) = u(e), 0) - k(t) - c(e, \bar{a}) \ . \tag{2.4}$$

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 2.6 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

Figure 2.5: 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.

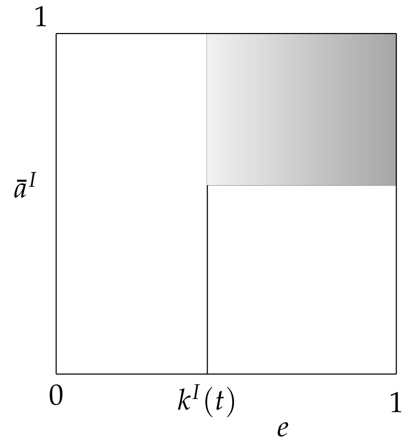

Figure 2.6: Before an inquisition

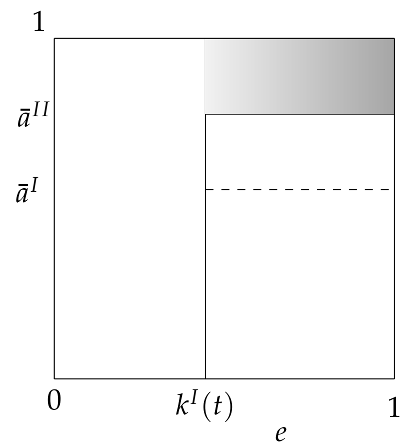

Figure 2.7: The short-run effect

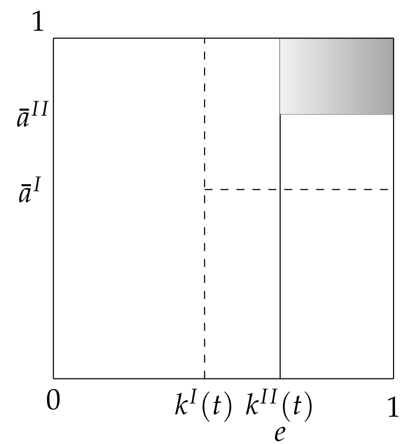

Figure 2.8: The long-run effect.

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 2.7. 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 2.8.

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.

[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.

2.5 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.

2.5.1 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). 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.

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

2.5.2 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 edilliteracy 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 2.8. 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. As 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

[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.

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 for them.[48]

2.5.3 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 2.9 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

[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.

in magnitude and not statistically significant.

Table 2.10 demonstrates that literary inquisitions continued 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 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 yields 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 2.11 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 2.12 depicts the effect of a literary inquisition on illiteracy by cohort. The effect

[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.

that we find 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]

2.5.4 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 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 2.13 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.

[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.

[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 2.14 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 (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, this does not account for why the effect lasted through the 1980s into the1990s. One reason for this is that 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.

2.6 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 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

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

[55]More recently **?** 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]**?**, 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).

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 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

[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).

[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.

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

2.7 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. Employing new data we

[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).

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.

Table 2.6: 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 2.5. 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 2.7: 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 2.5.

Table 2.8: 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 2.9: 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 2.8.

Table 2.10: 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 rec. p.c. (GSU)		0.00227					
		(0.00206)					
Migration rec. p. c. (GSU)			8.92e-09				
			(5.92e-08)				
Any migration rec. (GSU)				0.0140			
				(0.0122)			
Log migration rec. p.c. (all measures)					0.00137		
					(0.00118)		
Migration rec. p.c. (all measures)						5.37e-09	
						(4.23e-09)	
Any migration rec. (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 2.8.

Table 2.11: 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 2.8.

Table 2.12: 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 2.8.

Table 2.13: 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 2.14: 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 .

Supplementary 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:

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

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.

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.

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, 2014b).

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

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

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.

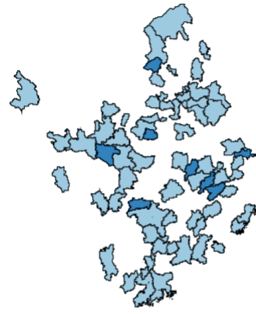
Matching Procedure

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

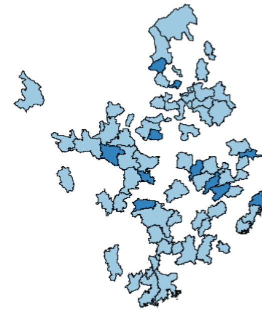
$$P(LQ_i = 1) = F(X_i) \tag{2.5}$$

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 covariates 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.

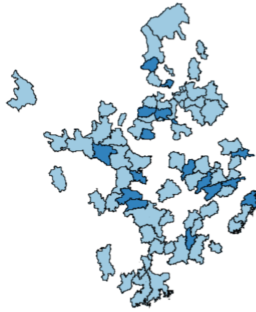
From inspecting the data we find that areas that had inquisition cases were much more likely to be more economically developed. A logit analysis suggests that prefectures affected by literacy inquisition had higher population density and more courier routes, though those coefficients are not significant. 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.



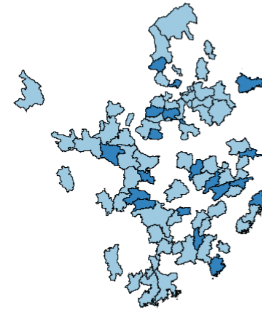
a: Caliper size: 0.001



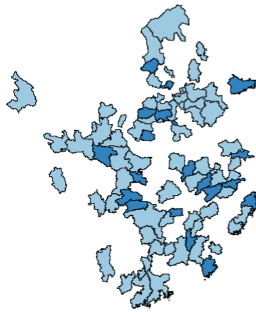
b: Caliper size: 0.002



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

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.

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 economic 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,

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.

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.

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.

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 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

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	Pprefectures with possible migration	Prefectures with $<$ 50% migration	Prefectures with $>$ 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).

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).

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

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/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,

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 2.14. The effect of a literary inquisition loses significance after 1990.

Chapter 3: Raising Dragons

3.1 Introduction

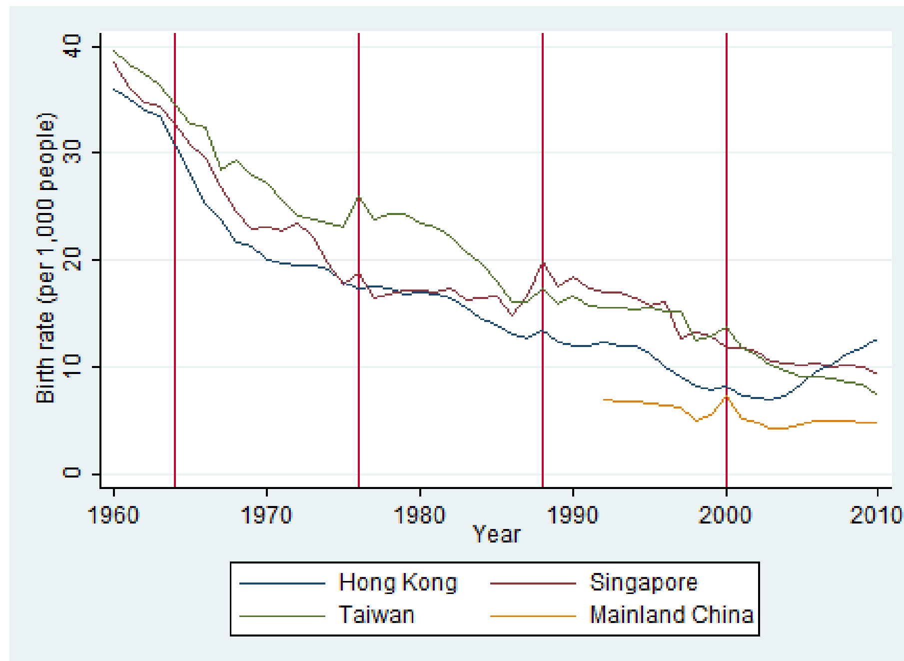
It is a common belief in traditional Chinese culture that children born in certain years of the lunar zodiac are more fortunate than average. The Year of the Dragon is considered to be an especially auspicious year, and in the two most recent Dragon years – 2000 and 2012 – a large increase in births has been observed across Mainland China. We call this the "dragon effect."

East Asians, and especially ethnic Chinese, have long believed that children born in the Year of the Dragon (which comes once every twelve years in the Chinese lunar calendar) would benefit from greater luck, be more intelligent, and would enjoy a better life.[1] Recent academic work has shown that Asian-American immigrants born in the dragon year were likely to have more educational attainment than similar immigrants born in non-dragon years, thus reinforcing this belief (Johnson and Nye, 2011). Goodkind (1991) documented a surge in births among ethnic Chinese populations for recent dragon years, starting from 1976. In 1988, the Chinese community in Malaysia saw a striking 24% increase in births compared to -2.8% in the year before. However, mainland China was an outlier as East Asian countries with large ethnically Chinese populations showed clear dragon year birth effects from 1976 onwards, while no such effect was visible for China until 2000.

Although some aspects of modernization theory have stressed that religious and superstitious beliefs tend to fade or become moderated by industrialization and rising prosperity

[1] In 2011, a newspaper survey of Chinese from the mainland and Hong Kong indicated that over 80% of those questioned believed dragon babies to be smarter. Similar beliefs are common in overseas Chinese communities from Taiwan to Singapore and Malaysia.

Figure 3.1: Birth Rates in Ethnically Chinese Populations, 1960–2010.



Notes: a. red lines represent 1964, 1976, 1988, and 2000 dragon years b. rate of natural increase is used for mainland China c. the sample of "mainland China" includes urban-core districts of prefecture-level Chinese cities

(Weber, 2009; Mason, 1980), the dragon effect seems to have become more pronounced and visible as Asian countries have grown wealthier. In this paper we demonstrate that the dragon effect only emerged in mainland China as the country modernized and grew richer. Both city-level and household-level data suggest that parents of dragon babies are more successful and more educated. Educated and urban couples are dedicating real resources to raising dragon babies and may bear extra costs from raising children in a crowded cohort.

Our findings shed light on several important questions. First, we contribute to the economics of culture and identity (Akerlof and Kranton, 2000, 2002; Bisin and Verdier, 2000, 2001; Bisin et al., 2011b; Carvalho and Koyama, 2014). Many East Asians attach considerable cultural importance to having a dragon baby (Do and Phung, 2010). When incomes are low, due to budget constraints, Asian parents experience difficulty in setting aside resources to raise dragon babies, whereas economic development enables them to indulge this cultural preference. Furthermore, while many believe that globalization

and exposure to western culture would lead to cultural homogenization and a decline in traditional values (Inkeles and Smith, 1974), our evidence does not support this view. Exposure to western culture seems to strengthen East Asians' preference for particularly salient aspects of their traditional cultural identity.

This paper provides evidence that economic growth, education and urbanization have not led to a decline in 'superstitious' belief. Since Max Weber and Emile Durkheim, many sociologists have predicted that economic growth and education would lead to a decline in religiosity and superstitious belief (Aarnio and Lindeman, 2005; Robertson, 1971). However since the 1980s the secularization hypothesis has come under attack. Iannaccone and Franck (2009) find little evidence to support the secularization hypothesis in twentieth century Europe. They suggest that the decline in religiosity was driven by the rise of the welfare state. Carvalho and Koyama (2011) examine how economic development and political liberalism helped give rise to the emergence of ultra-Orthodox Judaism. Binzel and Carvalho (2013) look at the revival of religiosity in the Middle East since the 1970s while Becker and Woessmann (2013) do find some evidence of economic growth and educational spending being associated with secularization in late nineteenth and early twentieth century Germany. Studies find traditional values are highly persistent in Asian countries, as well as folk religions and superstitious beliefs (Chen, 1995; Tan, 1994). In our study, we find no obvious evidence for a decline in the preference for dragon babies among the Chinese. In contrast, we find increasingly more dragon babies were born in the midst of economic growth and urbanization.

Finally, our findings are related to the literature on economic development in China. It is well documented that since 1978 China has experienced uneven economic growth (Bao et al., 2002; Démurger et al., 2002; Yao, 2006; Du et al., 2008; Wei, 2013). Geography, the varying pace of economic reforms and globalization has led to varying levels of economic growth across the country. Hence there is considerable regional variation within China, which allows us to identify the relationship between economic development and the dragon effect.

The remainder of the paper is structured as follows. Section II describes the background for the emergence of the dragon effect among Chinese populations across cities. Section III introduces the data used in this paper. Section IV lays out the empirical strategies used in all levels of analyses. Section V summarizes the main findings.

3.2 The Dragon Puzzle

The long-standing belief that dragon babies are auspicious did not translate into a notable shift in births towards the dragon years until 1976, when demographers began to note that birth rates ticked upwards in regions with large ethnic Chinese populations such as Taiwan, Hong Kong, and Singapore (Goodkind, 1991). This was especially notable in a period when birth rates had been steadily declining throughout East Asia. In 1976, per capita GDP of Hong Kong, Singapore and Taiwan reached $2647, $2,850 and $1,146 respectively.[2] This was repeated in 1988 with even greater effect and indeed the strength of this effect was sufficient to alarm government officials who feared overcrowding in schools and certain public services. This led to calls for mothers not to plan their children for the dragon year (Goodkind, 1991). Yet these effects persisted and have been seen in every dragon year since (2000 and 2012).[3] However, the largest anomaly was mainland China, which did not see anything even vaguely resembling a dragon year birth effect in both 1976 and 1988. It seemed that culturally at least, the People's Republic of China was less traditional in its beliefs than overseas Chinese populations.

But all this changed in 2000. In that year it began to be clear that the dragon superstition was now widespread throughout the worldwide Chinese community and the PRC also began to see rising birth rates. This was especially prevalent in the more developed areas and contemporary newspaper accounts spoke of mothers seeking medical intervention to produce babies in that dragon year (31 Dec 1999).[4] This mirrored the same efforts of

[2]By PPP converted GDP per capita at 2005 constant prices, Hong Kong's per capita GDP was at $9,797, Singapore, $11,126, and $5,505. Source: Penn World Table Version 7.1, World Development Indicators from World Bank and EconStats from IMF.

[3]"Enter the dragons: A baby boom for Chinese across Asia." ¸

[4]"Having a Baby in Year of the Dragon Is Too Lucky to Be Left to Chance." ¸

women in Taiwan, Hong Kong, Singapore, and in the ethnic Chinese areas of Malaysia, to time their children for dragon births. By 2012, this effect was even more pronounced and the widespread phenomenon of dragon year births was as well established on the mainland as it was abroad with both official data and newspaper accounts reflecting this obvious trend.

3.3 Data

A major challenge to our study is the absence of micro-data in the 2000 census, the year in which we suspect that the dragon effect first emerged. We instead use city and province data and a smaller sample survey of household data. We rely on the working hypothesis that city- and province-level variations reflects decisions made at the individual level.

We assemble datasets from four major sources to study the dragon effect at the province-level, city-level, and household-level. The aggregated data of China's 2000 and 2010 population census contain monthly birth rates of each province. The same sources provide a wide range of socioeconomic characteristics of each city on a 10-year interval.

To estimate the dragon effect and study its determinants at the city-level, we use two different data sources to construct a time series of birth rates (1965–1989) or rates of natural increase (1989 –2012). This allows us to estimate the dragon effect for 1988, 2000, and 2012. Prior to 1989, we extrapolate birth rates of each city from the birth records of 1990 population census available through IPUMS. For the rest of the data years, we use China City Statistical Yearbooks.

We further study the characteristics of the dragon effect on a household level. The China Health and Nutrition Survey from Carolina Population Center contains longitudinal data of 959 households from 1989 to 2009. It is one of the most used surveys in socioeconomic research of China. We use CHNS for household-level analysis. In the absence of micro data for 2000 population census, CHNS is a decent source for a micro-level analysis of the dragon effect in 2000.

We consider various socioeconomic variables as potential explanatory variables of the dragon effect. We include the same set of socioeconomic variables, such as income, education, urbanization and fertility in all levels of our analysis. In addition, at the province level, we use per capita GDP to proxy for the level of development in a difference-in-differences analysis; at the city-level, we add per capita GDP, share of local residents, number of surviving children per woman, and percentage of urban household registration to the list of explanatory variables; at the household-level, we further include household registration status and family structure.

We control for the share of local residents in our main specification as a proxy for economic and social prospects in a city. Relative to cities seeing residents leaving the town, cities with an influx of migrants have fewer local residents relative to the total population. In the data, the stock of in-flow migration is indicated by the locally-registered-to-total ratio. The artificially imposed household registration system, which artificially restricted residence in large cities, led to highly constrained mobility in the time of the strictly planned economy.

Pre-1978 developmental policies contributed to phenomenal differences between urban and rural areas, big and small cities; education resources and general access to the social safety net are unevenly distributed across the country often for purely political reasons. Given the more liberal migration policies of the past two decades, hundreds of millions of migrants have flooded into more developed cities, where opportunities are greater and resources are more accessible.[5] Therefore we use the direction of migration as an indicator for economic opportunities and standards of living, and infer that host cities are generally the more developed cities.[6]

Beyond the above controls, we also control for historical and geographic characteristics of

[5]Coinciding with theory of migration, the Chinese have the traditional belief "water goes downhill, and people go uphill."; most migrants have highly practical reasons for moving (Zhao, 1999; Chan and Zhang, 1999).

[6]Notice that despite their long-term stay in the host city, many migrants never get to change their registration status, because they often do not meet the criteria to be admitted to the host city (Zhao, 2002). As popularity of the city is a determinant of the strictness of criteria, the locally-registered-to-total ratio can underestimate the total number of incoming migrants in the less popular cities. But it should not affect the validity of our analysis, as "popularity" is precisely what we are interested in as a latent variable.

a city, such as distance to the Great Canal or the Yangtze River, treaty port, distance to the coast, ruggedness, latitude and longitude. We assemble those controls using GIS files from various sources, among which CHGIS is the most important source that we use for historical controls. We also extract socioeconomic macro-region variables from G. William Skinner's map collection.[7]

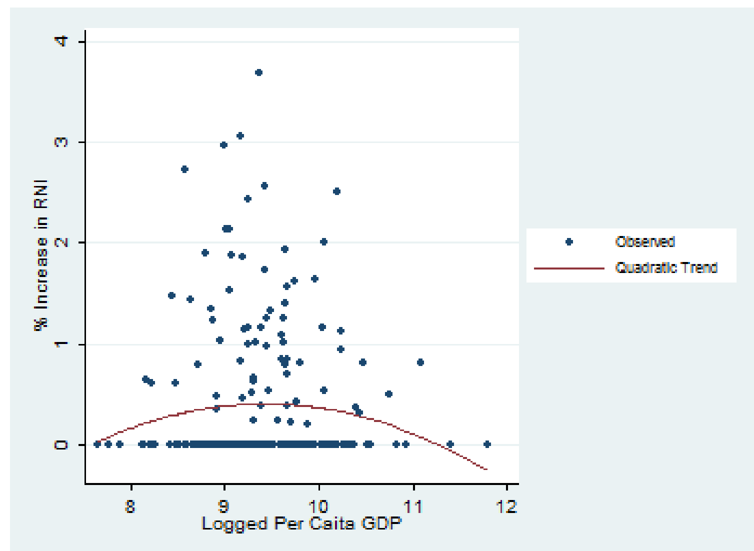
3.3.1 Descriptive Statistics

We construct the main variable, dragon effect, by two methods—the ARIMA method and the dummy variable method (see the Appendix). Table 3.1 gives an overview of the key variables in the main sample with a total of 190 cities included. For better over-the-time consistencies, we focus on the urban core of the 190 cities. On average, there is a 38% increase in RNI in 2000, but with huge variation across the cities. An average resident in an average city in the sample acquires 8.7 years of education, and receives 102 RMB foreign direct investment. An average female resident has 1.05 children. 59.4% of all residents in an average city hold urban household registration status, and 91.4% of all residents are locally registered. Several cities have a larger registered population than the actual population, suggesting they face an outflow of migrants who no longer reside locally despite their local household registration status. 58.9% of all households are two-generation households. An average city has a per capita GDP of 15,180 RMB, and has 3.3% of its residents being ethnic minorities rather than Han Chinese.[8] As culture is highly relevant for beliefs in zodiac signs, we restrict our main sample to culturally and ethnically Han regions.[9]

Table 3.1: Descriptive Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Dragon effect	0.38	0.706	0	3.684	190
Log per cap GDP	9.412	0.646	7.645	11.8	190
Per cap GDP	15180.07	12768.754	2091.041	133304.563	190
Education attainment	8.74	0.841	6.524	10.627	190
Share of local residents	0.914	0.133	0.174	1.194	190
Number of surviving children per woman	1.056	0.234	0.552	1.774	190
Share of urban residents	0.594	0.214	0.128	0.938	190
FDI per capita	102.992	219.472	0	1694.886	190
Minority population	3.299	8.108	0.03	81.914	190
Share of nuclear households	0.589	0.052	0.396	0.688	190
Distance to coast	330.001	313.155	0.087	1175.69	190
Distance to Grand Canal or Yangtze River	2.646	2.38	0	8.584	190
Ruggedness	3.097	2.419	0.094	12.845	190
Treaty port	0.132	0.339	0	1	190
Latitude	31.407	5.489	21.16	42.519	190
Longitude	114.765	4.801	101.729	124.189	190
Region dummy	4.3	2.337	0	9	190

Figure 3.2: Quadratic Trend of Log Per Capita GDP in Dragon Effect



3.4 Baseline Estimates

The main question of the paper is how economic growth affects the exercise of traditional beliefs. Our key finding is that a higher per capita GDP is correlated with a greater dragon effect; however, this effect is not linear. Figure 3.2 shows how the size of the dragon effect changes as per capita GDP changes.

3.4.1 Dragon Years 1988, 2000, 2012: Panel Estimation

To explore the causal role of economic growth in the emergence of dragon effect, we conduct a panel analysis of three dragon years, 1988, 2000 and 2012, to estimate the effect of per capita GDP on the emergence of the dragon effect. The dependent variable is *dragon effect* constructed with the methods discussed in the Appendix.[10] We apply both logit and linear fixed-effect models to the data, as well as pooled OLS regressions.

Table 3.2 summarizes the results. Per capita GDP has a non-linear effect on the occurrence of dragon effect in all specifications. Column 1 of Table 3.2 summarizes the results of the baseline specification, in which we estimate the effect of per capita GDP with a fixed-effect logit model with city fixed effects. We use city fixed effects to control for time invariant city characteristics, such as propensity to superstition (intensity of communist rule, political control, local culture and etc.). In Column 2, we include province specific trends, and the coefficient of log per capita GDP is slightly greater than in Column 1. Province specific trends capture time-varying factors that led to the probability of having dragon babies linearly increasing or decreasing at the province level, such as the changing

[7]Region dummies are taken from the Skinner Regional Systems Analysis Dataverse - Skinner socioeconomic macroregions ()

[8]15,180 in 2000 is equal to 5,454 2005 international dollars/US dollars, based on PPP converted GDP Per Capita

[9]Our main sample contains mostly “China-proper" provinces.“China-proper" refers to the territory of Ming China (1368–1644), which has been predominantly populated by Han Chinese both in the past and in the present. To further alleviate heterogeneity in traditional beliefs in dragon babies, ethnic minority autonomous counties/prefectures/regions are not included in the sample.

[10]We use an ARMIA (0,1,0) model to forecast birth rates or rates of natural increase for each city, with 75% confidence intervals.

role of economic geography and degree of foreign exposure. In Column 3, we include socioeconomic macro-region specific trends, which lead to a smaller coefficient estimate. Column 4 presents results of the same specification as Column 2 estimated by a linear fixed-effect model. While this specification allows us a large sample, it also leads to a negative adjusted R-square. We proceed to include city-specific time trends in Column 5. The size of the coefficient in Column 5 increases relative to that in Column 4. City-specific time trends account for linear trends in the probability of having dragon babies at the city level, such as disposable income, cost of living, and the evolving local one-child policy. We drop fixed effects in Column 6 and 7, based on the results of F-tests in linear fixed-effect estimation.In both F-tests, the null hypothesis that all fixed effects are zero is not rejected, and therefore, the composite error terms (ui+eit) are likely to be uncorrelated. Results of F-tests favor a decision to use pooled OLS regressions. For linear regressions, the coefficient of per capita GDP decreases to 0.636 in Column 6 (pooled OLS) from 0.883 in Column 4 (linear fixed effects). For logit regressions, the coefficient estimate increases slightly to 6.588 in Column 7 from 5.868 in Column 1.

Our panel analysis suggests economic growth is a proximate cause for the emergence of the dragon effect. The relationship between per capita GDP and the dragon effect is non linear. When per capita income initially rises, a city becomes more likely to see a dragon effect; but this effect wears off as a city becomes even richer. One possible explanation for such non linearities is having dragon babies is a form of conspicuous consumption, which rises when living standards exceeds levels of subsistence, and gradually declines as the income of the reference group increases (Charles et al., 2007).[11] While the panel analysis establishes the increasing pattern of dragon effect in relation to economic growth, we remain uncertain why higher incomes cause more dragon babies. To acquire a better understanding of the dragon effect and how it is affected by incomes, we explore correlates of the dragon effect in an OLS analysis presented below.

[11] A similar pattern is observed in the purchase of luxury brands in China. Many famous luxury brands have shifted focus to lower tier cities in recent years, as incomes in top tier cities continue to rise. http://www.nytimes.com/2013/11/21/fashion/Chinas-Smaller-Cities-Thirst-for-the-Luxe-Life.html?pagewanted=all. http://www.businessweek.

Table 3.2: Panel Estimation

	Dragon Effect						
	FE logit	FE logit	FE logit	Linear FE	Linear FE	Pooled OLS	Pooled logit
Log per cap GDP	5.868**	7.045**	3.976 *	0.883*	2.493**	0.636**	6.588**
	(2.140)	(2.637)	(2.282)	(0.360)	(0.799)	(0.234)	(2.418)
Log per cap GDP^2	-0.295*	-0.435**	-0.255**	-0.0514**	-0.146***	-0.0312*	-0.330*
	(0.115)	(0.142)	(0.119)	(0.0188)	(0.0405)	(0.0131)	(0.1299)
City FE	Yes	Yes	Yes	Yes	Yes	No	No
City FE* Time Trend	No	No	No	No	Yes	No	No
Province FE* Time Trend	No	Yes	No	Yes	No	No	No
Region FE* Time Trend	No	No	Yes	No	No	No	No
Observations	255	255	255	473	473	473	473
Adjusted R^2				-0.371	-0.753	0.037	
Pseudo R^2	0.099	0.294	0.159				0.051

Standard errors in parentheses

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

Notes: The table reports panel estimates at a city level. The unit of observation is the urban core of a city in the China City Statistical Yearbooks. The dependent variable is dragon effect. The panel is balanced in all specifications, as only cities with known values for the dragon effect for all three years are included. Standard errors derived from asymptotic theory (oim) are used in Column 1 and 2. Robust standard error are used from Column 3 through Column 7.

3.4.2 Dragon Year 2000: OLS Estimation

The panel analysis allows us to establish the role of per capita income in the emergence of the dragon effect. To know more of the possible mechanism, we first conduct a multivariate analysis with per capita GDP, educational attainment, share of local residents, share of urban residents, and number of surviving children per woman. To best avoid omitted variable biases, we include a number of modern, historical and geographic controls, such as minority population, foreign direct investment, distance to coast, distance to the Great Canal, treaty port, ruggedness, latitude, longitude and region dummies.

To estimate the effect of economic development on the emergence of the dragon effect, we focus our attention on income, education and urbanization. To proxy for the hidden benefits and future economic prospects of a city, we use the share of local residents as a measure of in-flow migration. In-flow migration best reflects the demand for better public amenities and greater economic opportunities in China, an economy moving from a constrained-mobility environment to a more open migratory policy environment.

We first use an OLS regression to estimate the following equation:

$$\begin{aligned}\text{Dragon effect}_i = &\beta_0 + \beta_1 \text{Log Per capita GDP}_i + \beta_2 \text{Log Per capita GDP}^2{}_i \\ &+ \beta_3 \text{Education}_i + \beta_4 \text{Local}_i + \mathbf{X}_i' \Lambda + \gamma + \epsilon_i \ ,\end{aligned} \tag{3.1}$$

where π_i is the probability that city i exhibits a Dragon Effect in the year 2000. Log Per capita GDP$_i$ is log per capita GDP in the year 2000. Education$_i$ is years of education. Local$_i$ is the share of local residents. $\mathbf{X}_i'$ is a vector of modern, historical and geographic controls, and γ are region dummies. Modern controls include share of population holding urban household registration status, total number of surviving children per woman, foreign direct investment per capita, share of minority population, and share of nuclear households. Historical controls include distance to the Grand Canal or the Yangtze River, and whether

com/articles/2014-03-25/foreign-brands-shift-focus-to-chinas-second-tier-cities.

Table 3.3: Baseline Results

Dragon Effect	(1)	(2)	(3)	(4)	(5)
Log per cap GDP	4.225***	4.225***	4.108***	5.160***	3.841**
	(1.433)	(0.711)	(1.129)	(1.459)	(1.349)
Log per cap GDP^2	-0.244***	-0.244***	-0.254***	-0.290***	-0.222***
	(0.0782)	(0.0314)	(0.0564)	(0.0734)	(0.0659)
Education attainment	0.193	0.193	0.223	0.259	-1.407
	(0.186)	(0.292)	(0.295)	(0.290)	(1.656)
Share of local residents	-1.437**	-1.437***	-1.242***	-1.007	-2.203
	(0.587)	(0.281)	(0.309)	(0.667)	(1.912)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes	Yes
Region Effects	Yes	Yes	Yes	Yes	Yes
Observations	190	190	190	156	190
Adjusted R^2	0.089	0.089	0.081	0.176	0.112

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table reports OLS estimates at a city level. The unit of observation is the urban core of a city in the China City Statistical Yearbooks. The dependent variable is dragon effect. All regressions include a set of socioeconomic controls, historical controls and geographic controls. Standard errors are clustered at the regional level in Column 2. Spatially deflated per capita GDP is used in Column 3. A subsample is used in Column 4, including only cities whose actual populations exceed their registered populations. Robust standard errors are used in all specifications.

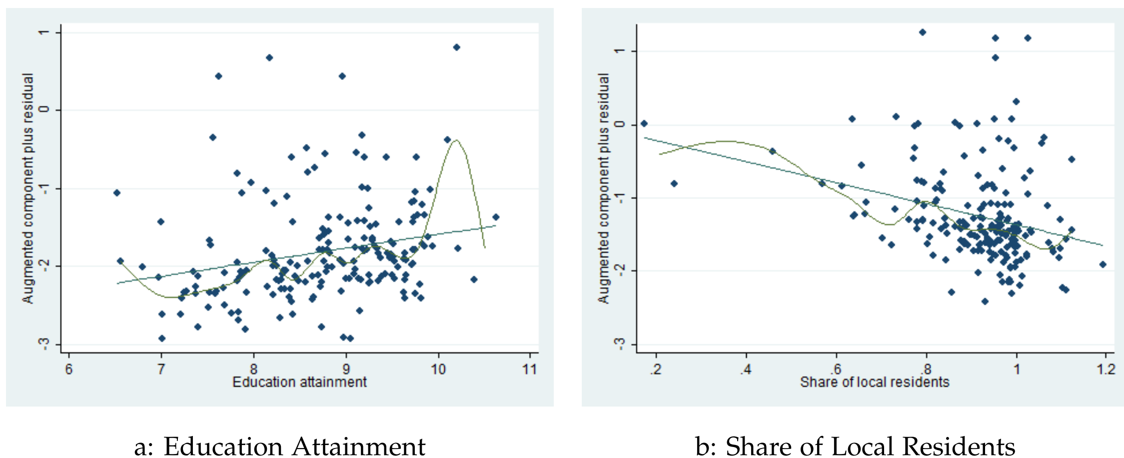
a city was a treaty port in 19th century China. Geographic controls include distance to the coast, ruggedness, latitude, longitude and their interaction. We also use generalized linear models to estimate the equation due to concerns over a large number of zeros in the sample (results are available on request).

Table 3.3 summarizes the results of our baseline model. Column 1 reports baseline estimates with robust standard errors. Column 2 reports estimates with standard errors clustered at the regional level. Column 3 shows estimates using spatially deflated per capita GDP rather than nominal per capita GDP.[12] Column 4 shows the estimates based on a subsample: cities whose actual populations exceeds their registered populations. Column 5 shows estimates with a fuller set of non-linear terms included.

The estimates suggest that beginning per capita GDP has a non-linear effect on making

[12]Spatial deflators are taken from Brandt and Holz (2006).

fertility decisions in favor of dragon babies. Cities with higher per capita GDP tend to have a bigger dragon effect; the effect peaks at 9,000 RMB per capita GDP ($3,240 by PPP converted GDP per capita at 2005 constant prices). In-flow migration is positively correlated with the dragon effect, or the share of local residents is negatively correlated with the dragon effect. Based on the estimates from Column 1 and Column 2, a one-standard deviation increase in share of local residents (0.133) is associated with an increase in births in the dragon year of 0.191 (1.437 * 0.133), i.e. a 19.1% additional increase in rates of natural increase above the base rates. The use of spatially deflated per capita GDP has little effect on the sign and size of various coefficients, whereas the choice of a subsample that focuses more on cities with more in-flow migration has a small increasing effect on the size of coefficients in several cases. In Column 5, as more non-linear terms are included, share of urban residents appears to have a non-linear effect on the dragon effect as well, though the effect is rather small. In most specifications, urban household registration and number of surviving children have a positive effect on the dragon effect, but the effects are not always statistically significant. This can be explained by the fact that a longer reproductive cycle lowers the cost of planning to have at least one of their children born in the Year of the Dragon. Most other variables take on the expected sign but their coefficient estimates are not statistically significant. Figure 3.3 shows augmented partial residual plots of education attainment and share of local residents.



a: Education Attainment

b: Share of Local Residents

Figure 3.3: Augmented partial residual plots

3.4.3 Dragon Effect as a Binary Variable: logit Estimation

In data some cities show evidence of a dragon effect whereas others do not. Our baseline model captures both the presence of a dragon effect and its magnitude. To check the robustness of the results, we propose an alternative model, a logit model, to model the chance a city displays a dragon effect ignoring its magnitude. This allows us to determine what factors affect the chance of a city having a dragon effect.

Tables A-1 and A-2 (see the Appendix) present summary statistics for cities that displayed the dragon effect in 2000 and cities that did not. Overall, dragon cities have better educated residents, and a lower percentage of them being local.

Table A-3 (see the Appendix) shows that the main findings are consistent with OLS estimates. Per capita GDP similarly displays a non-linear effect on the probability of a city having a dragon effect, with a similar cutoff point (around $5,400). A difference is that in the logistic model, education attainment and number of surviving children per woman display a non-linear effect on the dragon effect in more specifications.

3.5 Household evidence from CHNS

In the remainder of our analysis, we use household-level data to further explore the causes of the dragon effect. First, we use household level data, albeit from a less representative sample, to check some of the results we obtain from the city-level analysis. One complication in interpreting our results in the city-level analysis is the potential compositional bias. By examining the effect of income at the household level, we likely distinguish between wealthy households having dragon babies and poor households in a rapidly developing cities having dragon babies. Second, we make an attempt to address whether modern education necessarily weakens traditional beliefs in dragon babies.

Our data are from the China Health and Nutrition Survey. We construct a sample including households with babies born between 1998 and 2002 and examine the characteristics

predicting the birth of a dragon baby in the household "cohort" of 1998–2002. We compare households with a dragon baby born in 2000 with households with no dragon baby, but with babies born between 1998 and 2002. As some households had more than one baby born during the five-year window, we create a third category for households with both dragon and non-dragon babies born between 1998 and 2002. Due to the vastly different characteristics of households having multiple births during a five-year window, we restrict our analysis to households with only one birth. Table A-4 (see the Appendix) provides summary statistics for the households included in the main sample.

Every household in our sample made a discrete choice between having a dragon baby and not having a dragon baby. Hence, we use a logit model to estimate the following equation:

$$\text{logit}(\pi_i) = \beta_0 + \beta_1 \text{Income}_i + \beta_2 \text{Education}_i + \mathbf{X}'_i + \lambda + \zeta + \epsilon_i \ , \tag{3.2}$$

where π_i is the probability of having a dragon baby; Income_i is the total net household income in 1997. Education_i is the highest education level in the household, i.e. the education level of the best educated member in the household;

$\mathbf{X}'_i$ include family structure, household registration status, average age of parents, number of children and household income in 1997.[13] λ captures community level urbanicity variable and its squared term and ζ are region fixed effects.

Table 3.4 shows logit estimates based on our household analysis. Table A-5 (see the Appendix) shows marginal effects at the means (MEMS) of all variables for the convenience of interpretation. All models have standard errors clustered at the region level. To test the relationship between income, education and having a dragon baby, we include education only in Column 1, income only in Column 2, and both education and income in Column 3. From Column 4 to Column 6 we report estimates of models excluding one control variable at a time. Our results suggest dragon babies were born in households with more

[13] When there is a missing value of household registration status, the household registration status of the entire household is imputed by the registration status of the head of the household. In most cases, we know the head of household's household registration status.

educated family members and higher household income; those households are more likely to be multi-generational, and more likely to have urban household registration. Coefficient estimates of the highest education level in the household are positive and significant across all models. Household income is positively associated with having a dragon baby, but is only statistically significant in Column 1. Column 1 through Column 3 suggests education has an independent effect on the chance to have a dragon baby controlling for household income. The size of the coefficient on education decreases when household income is added to the specification, suggesting that household income might be a mediator of the positive effect of education on having a dragon baby. This result confirms the validity of the city-level analysis, i.e. the dragon effect is not driven by a particular portion of the distribution of a population within cities with higher per capita income. Given the positive relationship between education and having a dragon baby, it is highly unlikely that in the city-level analysis, dragon babies are being produced by a marginalized and isolated social group with less-than-average education. This result also indicates education does not necessarily undo traditional beliefs in zodiac signs in the context of China. Highly educated individuals are more willing to produce dragon babies, even when holding constant the ability to exercise their belief, such as household income. This is true even after accounting for more traditional individuals being selected into education, traditionalism being proxied by living in a multi-generational family.

Based on column 3, in average marginal effects' terms, one standard deviation increase (1.133) in education level translates into an increase of 4.1% (0.036 * 1.133) in probability of having a dragon baby. A one standard deviation increase (0.498) in *extended family* is positively associated with an increase of 7.9% (0.160 * 0.498) in probability of having a dragon baby. Urban Household registration status has a small positive effect, average age of parents and number of children have a small negative effect, but none of them are statistically significant in most specifications. The Urbanicity Index of a community has a small positive effect, but is not statistically significant.

With regards to *extended family*, we mainly interpret the positive coefficient as more traditional households having more dragon babies. However we cannot rule out another interpretation of the positive coefficient of *extended family*, which is that on-site grandparents provide consultancy for and share childcare responsibilities with young parents, and thus, encouraging and facilitating births of dragon babies. The presence of grandparents can be important to the decision to produce dragon babies as it expands the choice set faced by young parents. When a dragon year coincides with critical moments of a mother's career, knowing the availability of childcare, a mother is more likely to have a dragon baby than otherwise.

Table 3.4: Household-Level Analysis Based on logit

	Dragon Baby					
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1997 Household income+1)	0.269**		0.230	0.101	0.229	0.230
	(0.136)		(0.142)	(0.150)	(0.147)	(0.142)
Highest household education level		0.262**	0.239*	0.199**	0.239*	0.239*
		(0.121)	(0.128)	(0.0795)	(0.127)	(0.128)
Extended family	1.120***	1.141***	1.063***	1.118**	1.065***	1.063***
	(0.396)	(0.397)	(0.411)	(0.518)	(0.412)	(0.411)
Urban household registration	0.624	0.512	0.497		0.496	0.497
	(0.423)	(0.387)	(0.407)		(0.407)	(0.407)
Average age of parents	-0.0623**	-0.0539**	-0.0555*	-0.0108	-0.0577	-0.0555*
	(0.0249)	(0.0270)	(0.0290)	(0.0326)	(0.0450)	(0.0290)
Number of children	-0.0358	-0.0213	-0.0328	-0.171		-0.0328
	(0.426)	(0.403)	(0.402)	(0.514)		(0.402)
2000 Community Urbanicity Index	0.0443	0.0674	0.0614	0.0826	0.0602	0.0614
	(0.113)	(0.102)	(0.111)	(0.0913)	(0.124)	(0.111)
2000 Community Urbanicity Index^2	-0.0004	-0.0006	-0.0006	-0.0007	-0.0006	-0.0006
	(0.0009)	(0.0008)	(0.0009)	(0.0007)	(0.001)	(0.0009)
Region dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	311	311	311	340	311	311
Pseudo R^2)	0.073	0.076	0.079	0.059	0.079	0.079

Standard errors in parentheses

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

Notes: The table reports logit estimates at a household level. The unit of observation is a household in China Health and Nutrition Survey. The dependent variable is having a dragon baby or not. Omitted category of *urban household registration* is rural/village household registration.All regressions include region dummies. Robust standard errors are used in all specifications.

3.6 Additional Evidence

Our analysis has so far shed light on the link between income, education and dragon effect. Due to a limited number of dragon years in our sample, however, we cannot rule out the possibility that dragon years from 1988 on have coincided with other shocks that have affected high income and low income areas, highly educated and poorly educated individuals differently. To remove those concerns, we concentrate on the transition of the lunar calendar from 1999 to 2000 in February of 2000.

We use the variation in birth rates between dragon months and non-dragon months within the 2000 Census Year and 2010 Census Year. More specifically, we use 24 months of birth rates in 1999, 2000, 2009 and 2010 to construct a minimalistic difference-in-differences

model. The model captures the differences in birth rates right before and during the Year of the Dragon.

The 2000 population census contains monthly birth rates for the first ten months of 2000 and the last two months of 1999.[14] In 2000, the Chinese New Year Eve fell upon February 5. Due to the discrepancy between the lunar calendar and the solar calendar, the last two months of 1999 and the first month of 2000 are "normal months" but in the proximity of the Year of Dragon. Since within the 12 census months of the year only 9 months are liable to comprise a Dragon Year according to the lunar calendar, our treatment group comprises the provinces within the months of March through October of 2000. We call this the Dragon Treatment. We pool monthly data from both 1999–2000 and 2009–2010. For each province in the treatment group we identify the same province in 2010 and use that province to form the control for all 30 provinces.

To account for seasonality in birth rates, we choose 2009–2010 as a base year to provide a reference point for 2000, and control for seasonality with month effects. We argue if it were not for the dragon effect, both years would see similar trends in birth rates, and similar seasonality in birth rates. Our regression design is therefore as follows:

$$
\begin{aligned}
\text{Birth Rate}_{i,m} = {} & \beta_0 + \beta_1 \text{Treatment}_i + \beta_2 \text{Post}_m + \beta_3 \text{y}_i^{2000} + \beta_4 \text{Treatment}_i \cdot Post_m \\
& + \beta_5 \text{Treatment}_i \cdot \text{y}_i^{2000} + \beta_6 \text{Post}_m \cdot \text{y}_i^{2000} \\
& + \beta_7 \text{Treatment}_i \cdot Post_m \cdot \text{y}_i^{2000} + \mathbf{\Gamma}_i + \epsilon_{i,m} \ .
\end{aligned}
\tag{3.3}
$$

The dependent variable, Birth $\text{Rate}_{i,m}$, is the provincial monthly birth rate. Treatment_i represents the province in the 2000 census year. Post_m denotes the months from February to October that constitute the dragon year. Our variable of interest is the triple interaction term

[14] Year 2010 is the only feasible comparison year given data availability.

between the treatment group (Treatment_i), the treatment period (Post_m) and provincial level income per capita in 2000.

Table 3.5 reports the results of this regression. Based on the coefficient estimate of the interaction term, cities with a higher per capita GDP see a greater difference in birth rates between months that comprise a lunar dragon year and the months preceding the lunar dragon year. We interpret this coefficient as higher birth rates in 2000 for the months that comprise a lunar dragon year than for the months preceding the dragon year. This is likely to be a result of active birth planning and a surge in births from February 2000 to October 2000, driven by the dragon treatment.

Figure **??** show for provinces with higher per capita GDP,birth rates in dragon months are also higher, after controlling for base level differences in birth rates between high per capita GDP provinces and low per capita GDP provinces. For dragon months, a province with a 2000 per capita GDP of 30,000 RMB in the treatment group has almost the same birth rates as its control, whereas a province with a 2,000 per capita GDP of 3,000 RMB in the treatment group has a birth rate more than one birth per 1000 higher than its control. Over the non-dragon months, however, the relationship is the opposite: a province with a 2000 per capita GDP of 30,000 RMB in the treatment group has a birth rate that is two births per 1000 lower than its control, whereas a province with a 2,000 per capita GDP of 3,000 RMB in the treatment group has a birth rate more than two births per 1000 higher than its control.

3.7 Conclusion

Until 2000, the People's Republic of China did not exhibit the dragon Year birth increases seen in overseas nations with large Chinese populations. But from 2000 on, the pattern of births observed in mainland China began to mirror the dragon Year increases seen in Hong Kong, Taiwan, or Singapore. Statistical analysis at the municipal and regional level indicates that the most pronounced dragon year effects are in developing cities with higher incomes, higher average education, and a greater share of non-local residents, which implies a strong net inflow most likely due to greater employment prospects. City level data gives analogous results to provincial data and further allows us to see that these effects are associated with more educated families and those living in multi-generational households. Contrary to theories of modernization that postulate increasing secularization as people get richer, the Chinese people seem to have become more attached to the Dragon superstition than they were at lower income levels. Whether this was because they were unable to properly implement a dragon strategy earlier, had higher opportunity costs of doing so, or developed stronger preferences for superstitious beliefs as a marker for identity cannot be gleaned from our data. What is clear is that the pattern observed in China cross-sectionally is consistent with the observation that the dragon effect was observed in East Asian nations just as the so-called Tiger nations industrialized and grew wealthier in the 1970s. There is some limited suggestion in our work that the effect of income on dragon year birth increases may be non-linear with the highest income regions showing slightly weaker effects. But it is not clear whether this is an artifact of lacking accurate measures of price=adjusted, real incomes for areas with high nominal incomes but also much higher living costs. It also remains to be seen what will happen as development continues in the coming years. Will the rest of China soon exhibit the same dragon year increases? Or will the most developed areas taper off just as the newly developing areas begin to exhibit similar dragon year increases? Sorting out these issues are an obvious subject for future research.

Table 3.5: Difference-in-Differences Estimation Relating Dragon Treatment to birth rates

	Monthly Birth Rates			
	(1)	(2)	(3)	(4)
The Year of Dragon	2.652***	2.652***	2.652***	-1.723***
	(0.775)	(0.704)	(0.595)	(0.150)
Feb. - Oct.	-1.814***	-2.014***	-2.014***	-2.014***
	(0.385)	(0.381)	(0.325)	(0.289)
The Year of Dragon * Feb. - Oct.	-1.919**	-1.919***	-1.919***	
	(0.808)	(0.736)	(0.614)	(0.560)
Log Per Cap GDP	-0.430***	-0.430***	-0.705***	0.597***
	(0.0387)	(0.0396)	(0.0407)	(0.0962)
The Year of Dragon * Log Per Cap GDP	-0.280***	-0.280***	-0.280***	
	(0.0839)	(0.0775)	(0.0657)	
Feb. - Oct. Log Per Cap GDP	0.164***	0.164***	0.164***	0.164***
	(0.0419)	(0.0420)	(0.0360)	(0.0323)
The Year of Dragon * Feb. - Oct. * Log Per Cap GDP	0.208**	0.208**	0.208***	0.208***
	(0.0874)	(0.0808)	(0.0677)	(0.0323)
Month Effects	No	Yes	Yes	Yes
Province Effects	No	No	Yes	No
Province-year Effects	No	No	No	Yes
Observations	744	744	744	744
Adjusted R^2	0.535	0.610	0.837	0.881

Standard errors in parentheses

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

Appendix

Construction of the dependent variable

At the city level we have time series data for forty-eight years of birth rates from 1965 to 2011. These data enable us to establish a measure of a possible dragon effect over time and therefore to construct our dependent variable. We use an ARMIA model to forecast birth rates or rates of natural increase for each city.[15]

Based on data availability, we take birth rates from 1965 to 1987 to forecast thebirth rate for 1988; we take rates of natural increase from 2001 to 2011 to backforecast the RNI for 2000, as well as to forecast the RNI for 2012.[16] We use one-step-ahead forecast and a 75% forecast interval to identify the counter-factual birth rates/RNIs for 1988, 2000 and 2012, given that it had not been a dragon year. We apply the following decision rule to identify cities having a dragon effect: when a city has a birth rate or RNI higher than the upper bound of our forecast interval for that year (1988, 2000 or 2012), we conclude it has an abnormal surge in births; therefore, *dragon effect* will be set to be one. In all other cases, the *dragon effect* will take a value of zero. If the forecasting model is invalid due to missing data, or a null value is resulted for the projected birth rate/RNI, the city will be excluded from the sample.

We use an alternative measure to check the robustness of our results. By including dummies for 1988, 2000 and 2012 in a simple regression with a cubic trend, we can estimate the dragon effect based on those dummies. We employ the following time series specification for all cities $j = 1 \ldots N$ where $N = 190$:

$$\mathrm{RNT}_{j,t} = \alpha_0 + \alpha_1 RNT_{t-1} + \alpha_2 t + \alpha_3 t^2 + \alpha t^3 + dI_t + e_t \ , \qquad (3.4)$$

[15]We employ an ARMIA (0,1,0).

[16]China city statistical yearbooks only started to publish the RNI for most cities from 1989 on. Prior to 1989, I construct city-level birth rates from 1990 census microdata in order to estimate the dragon effect for 1988. For 2000, as many cities suffer missing data problems in the 1990s, I use "backforecasting" technique to predict 2000 RNI using RNI from 2001 to 2011 instead of using RNI from 1989 to 1999 to forecast 2000 RNI.

where I_t is an indicator variable which takes on the value of 1 if the year is a Dragon year. RNT_{jt} is the rate of natural increase. We use this in lieu of birth rates for years where birth rates are not available.[17] F-ratio and R square of each regression are recorded to inform the validity of this model for each city.

Our dragon effect variable is a continuous variable: a positive and significant dummy translates into a positive dragon effect, a positive or negative but insignificant dummy suggests no effect, and a negative and significant dummy equals a negative dragon effect; for cities with a positive and significant dummy, the dragon effect is computed as the deviation of RNI in 2000 from the predicted 2000 RNI, i.e. the coefficient of the dummy as a percentage of rate of natural increase in 2000 subtracting the coefficient of the dummy; for other cities, dragon effect variable takes the value of 0. Cities with a dummy that cannot be estimated are excluded from the sample. Cities with a negative predicted 2000 rate of natural increase are also excluded, to ensure that the dragon effect variable is monotonic in the magnitude of the latent variable across cities with positive and negative predicted 2000 rate of natural increase. This enables us to create a dependent variable for the Dragon effect.

Additional Tables

[17] Rate of natural increase, which equals birth rate minus death rate, can be used to infer birth rates, when death rates are stable. From a stylized fact from Notestein (1945), it is known that during demographic transition, death rates first decline, and stabilize at a lower level; when death rates are more or less constant over the time, a change in RNI should correspondence with a change in birth rates. We perform a correlation ratio test between birth rates and RNI to show this stylized fact does hold in our sample.

Table A-1: Summary Statistics for Cities which Displayed Dragon Effect in 2000

Variable	Mean	Std. Dev.	Min.	Max.	N
Per cap GDP	16031.596	10764.445	4663.583	65070.352	50
Log per cap GDP	9.529	0.529	8.448	11.083	50
Education attainment	8.987	0.817	6.524	10.222	50
Share of local residents	0.88	0.137	0.459	1.125	50
Number of surviving children per woman	1.044	0.222	0.766	1.761	50
Share of urban residents	0.652	0.185	0.249	0.92	50
FDI per capita	130.933	264.031	0	1694.886	50
Miniority population	3.747	6.964	0.104	35.992	50
Share of nuclear households	0.596	0.046	0.509	0.688	50
Distance to coast	308.147	263.569	0.224	926.906	50
Distance to Great Canal or Yangtze River	3.126	2.118	0	7.674	50
Ruggedness	3.71	2.579	0.099	12.845	50
Treaty port	0.08	0.274	0	1	50
Latitude	30.796	5.911	21.907	41.122	50
Longitude	114.49	4.297	101.729	122.337	50
Region Dummy	4.4	2.515	0	8	50

Table A-2: Summary Statistics for Cities which did not Display Dragon Effect in 2000

Variable	Mean	Std. Dev.	Min.	Max.	N
Per cap GDP	14875.953	13434.566	2091.041	133304.563	140
Log per cap GDP	9.370	0.679	7.645	11.8	140
Education attainment	8.652	0.834	6.560	10.627	140
Share of local residents	0.926	0.13	0.174	1.194	140
Number of surviving children per woman	1.061	0.238	0.552	1.774	140
Share of urban residents	0.573	0.221	0.128	0.938	140
FDI per capita	93.013	201.342	0	1569.805	140
Minority population	3.139	8.497	0.03	81.914	140
Share of nuclear households	0.586	0.054	0.396	0.678	140
Distance to coast	337.805	329.575	0.087	1175.69	140
Distance to Great Canal or Yangtze River	2.474	2.451	0	8.584	140
Ruggedness	2.878	2.329	0.094	10.579	140
Treaty port	0.15	0.358	0	1	140
Latitude	31.625	5.335	21.16	42.519	140
Longitude	114.863	4.98	103.664	124.189	140
Region dummy	4.264	2.278	0	9	140

Table A-3: City-Level Analysis Based on logit Estimation

	Dragon Effect				
	(1)	(2)	(3)	(4)	(5)
Log per cap GDP	21.40**	21.40**	18.81*	23.51	19.35*
	(10.65)	(8.600)	(10.66)	(16.64)	(10.02)
Log per cap GDP^2	-1.158**	-1.158**	-1.100*	-1.264	-1.051**
	(0.563)	(0.469)	(0.613)	(0.861)	(0.527)
Education attainment	-9.721*	-9.721	-10.07*	-11.34**	-9.148**
	(5.740)	(6.346)	(6.100)	(5.267)	(4.601)
Education attainment^2	0.648*	0.648*	0.674*	0.781***	0.628**
	(0.333)	(0.360)	(0.347)	(0.276)	(0.273)
Share of local residents	-8.837***	-8.837***	-8.111***	-13.06***	-14.10
	(3.210)	(2.581)	(2.245)	(4.684)	(10.93)
Number of surviving children	28.94***	28.94**	28.77**	54.71***	34.50***
per woman	(10.26)	(12.39)	(12.33)	(19.27)	(12.27)
Number of surviving children	-9.393**	-9.393**	-9.311*	-20.02**	-11.06**
per woman^2	(3.954)	(4.703)	(4.804)	(8.754)	(4.680)
Share of urban residents	1.461	1.461	1.398	1.981	5.148
	(1.891)	(1.856)	(1.779)	(2.418)	(6.865)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes
Historical controls	Yes	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes	Yes
Region Effects	Yes	Yes	Yes	Yes	Yes
Observations	189	189	189	155	189
Pseudo R^2	0.302	0.302	0.296	0.414	0.329

Standard errors in parentheses

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

Notes: The table reports logit estimates at a city level. The unit of observation is the urban core of a city in the China City Statistical Yearbooks. The dependent variable is dragon effect. All regressions include a set of socioeconomic controls, historical controls and geographic controls. Standard errors are clustered at the regional level in Column 2. Spatially deflated per capita GDP is used in Column 3. A subsample is used in Column 4, including only cities whose actual populations exceed their registered populations. Robust standard errors are used in all specifications.

Table A-4: Household-level Analysis–Summary Statistics

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Dragon Baby	0.183	0.387	0	1	776
Highest education level in the household	2.341	1.133	0	6	607
Extended family	0.549	0.498	0	1	674
Household income in 1997	11834.168	10475.087	-8000	102770	439
Urban household registration	1.703	0.458	1	2	481
Average age of parents	26.585	4.717	13	43	436
Average age of parents (truncated)	26.564	4.216	18	42.5	435
Number of children	1.611	1.282	1	15	776
Province	39.445	9.381	21	52	776
2000 Urbanization index	54.15	17.816	24.301	90.152	759
2000 Urbanization index^2	3249.18	2024.265	590.519	8127.323	759

Table A-5: Household-Level Analysis–Average Marginal Effects

	Dragon baby	
	Point estimate	Marginal effect
Log (1997 Household income+1)	.2301424	.0347163
	(.1423937)	(.0216699)
Highest education level in the household	.2388799	.0360343
	(.1279172)	(.0190552)
Extended	1.06336	.1604047
	(.4108005)	(.0592294)
Urban household registration	.4974142	.079945
	(.4066483)	(.0693836)
Average age of parents	-.0554965	-.0083715
	(.0289973)	(.0043972)
Number of children	-.0328287	-.0049521
	(.4022893)	(.0607085)
2000 Community Urbanicity Index	.0614071	.0092631
	(.1107887)	(.0166558)
2000 Community Urbanicity Index^2	-.0006154	-.0000928
	(.0009124)	(.0001371)
Observations	311	311

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Curriculum Vitae

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